Smoothing consumption across households and time: essays in development economics

by

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Abstract

This thesis studies two strategies that households may use to keep their consumption smooth in the face of fluctuations in income and expenses: credit (borrowing and savings) and insurance (state-contingent transfers between households). The first chapter asks why insurance among households in rural Thai villages is incomplete. The second chapter analyzes the impacts of micro-credit. The third chapter examines the interaction between interpersonal insurance and access to savings.

The first chapter is motivated by the observation that interpersonal insurance within villages is an important source of insurance, yet consumption, while much smoother than income, is not completely smooth. That is, insurance is incomplete. This chapter attempts to identify the cause of this incompleteness. Existing research has suggested three possibilities: limited commitment—the inability of households to commit to remain within an insurance agreement; moral hazard—the need to give households incentives to work hard; and hidden income—the inability of households to verify one another's incomes. I show that the way in which "history" matters can be used to distinguish insurance constrained by hidden income from insurance constrained by limited commitment or moral hazard. This history dependence can be tested with a simple empirical procedure: predicting current marginal utility of consumption with the first lag of marginal utility and the first lag of income, and testing the significance of the lagged income term. This test is implemented using panel data from households in rural Thailand. The results are consistent with insurance constrained by hidden income, rather than limited commitment or moral hazard. I test the robustness of this result to measurement error using instrumental variables and by testing over-identifying restrictions on the reduced form equation for consumption. I test robustness to the specification of the utility function by nonparametrically estimating marginal utility. The results suggest that constraints arising from private information about household income should be taken into account when designing safety net and other policies.

My second chapter (co-authored with Abhijit Banerjee, Esther Duflo and Rachel Glennerster) uses a randomized trial to analyze the impacts of microcredit in urban South India. We find that more new businesses are created in areas where a microcredit branch opens. Existing business owners increase their spending on durable goods but not non-durable consumption. Among households that did not have a business before the program began, those with high estimated propensity to start a business reduce non-durable consumption and increase spending on durables in treated areas. Those with low estimated propensity to start a business increase non-durable consumption and spend no more on durables. This suggests that some households use microcredit to pay part of the fixed cost of starting a business, some expand an existing business, and others pay off more expensive debt or borrow against future income. We find no effects on health, education, or women's

empowerment.

My third dissertation chapter (co-authored with Arun Chandrasekhar and Horacio Larreguy) is motivated by the observation that the ability of community members to insure one another may be significantly reduced when community members also have the ability to privately save some of their income. We conducted a laboratory experiment in rural South India to examine the impact of savings access on informal insurance. We find that transfers between players are reduced when savings is available, but that, on average, players smooth their consumption more with savings than without. We use social network data to compute social distance between pairs, and show that limited commitment constraints significantly limit insurance when risk-sharing partners are socially distant, but not when pairs are closely connected. For distant pairs, access to savings helps to smooth income risk that is not insured interpersonally.

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[1]**This chapter is coauthored with Abhijit Banerjee, Esther Duflo and Rachel Glennerster.**

[2]This chapter is coauthored with Arun Chandrasekhar and Horacio Larreguy.

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Chapter 1

Distinguishing barriers to insurance in Thai villages

1.1 Introduction

Risk to households' incomes is widespread in developing countries—crops and businesses fail, jobs are lost, livestock die, prices fluctuate, family members become ill, etc. If perfect insurance were available, such income risk would not translate into fluctuations in household per capita consumption. In fact, poor households in many developing countries are insured against short-term, idiosyncratic income shocks to a surprising degree, despite absent or imperfect markets for formal insurance, credit, and assets (Rosenzweig (1988), Townsend (1994), Townsend (1995), Udry (1994), Morduch (1995), Suri (2005)). However, households are generally not completely insured—income and consumption are typically found to be positively correlated, and serious income shocks like severe illness translate into reduced household consumption (Gertler and Gruber 2002). Households neither seem to live "hand to mouth," with shocks to income translating one-for-one to fluctuations in consumption, nor to be fully insured, with consumption completely buffered against shocks to income.

Furthermore, households do not smooth consumption only with a borrowing-savings technology. There is direct evidence that households make state-contingent transfers to others in their village (Scott (1976), Cashdan (1985), Platteau and Abraham (1987), Platteau (1991), Udry (1994), Collins et al. (2009)). Transfers which depend on current states—loans forgiven when the borrower's crops fail, money given when a neighbor is ill, etc.—are the hallmark of insurance, since in a pure credit system transfers would depend only on past states (the amount borrowed, etc.). The incidence of state-contingent transfers demonstrates that households obtain insurance from others in their village. A natural question is then, why is this insurance not complete? Among the reasons proposed for the failure of full insurance are: moral hazard—one household's actions are not observable to others; imperfect information about income realizations—households' income realizations are unobservable by others; and limited commitment—households with high incomes, who would be required by full insurance to make transfers to others, may leave the insurance arrangement instead.

Knowing what barrier to full informal risk-sharing is most important in a given community is important for evaluation of policies that may affect the sustainability of informal insurance. Policies that interact with existing informal risk-sharing mechanisms may have very different impacts depending on the nature of incomplete informal insurance. For instance, a work-guarantee program such as India's National Rural Employment Guarantee Act could crowd out insurance constrained by moral hazard (by reducing the penalty for exerting low effort) or limited commitment (by making exclusion from the informal insurance network less painful), but could "crowd in" insurance constrained by imperfect information about households' incomes (which I will refer to as "hidden income"), by ruling out the possibility that a household received a very low income, since households have recourse to the work-guarantee program.

If binding, the participation constraints of the limited commitment model, the truth-telling constraints of the hidden income model and the incentive-compatibility constraints of the moral hazard model all preclude the village from achieving full insurance. All three models predict a positive correlation between income and consumption changes[1], as well as predicting that one household's income realizations will affect the consumption of other households in the village. Therefore, finding such a positive correlation is not sufficient to distinguish between these models. Most of the existing literature on barriers to informal insurance, which I briefly review below, tests one model of incomplete insurance against one or both of the benchmark cases—full insurance and borrowing-saving only. Such tests, while they can reject full insurance, are not able to reject models of incomplete insurance other than the particular insurance friction they consider. It is possible that tests of a particular insurance friction versus borrowing-saving or full insurance will conclude in favor of that incomplete insurance model if the true data-generating process is in fact another

[1] The relationship between income and consumption need not be everywhere positive under a moral hazard model, even if the likelihood ratio is monotone (Milgrom 1981), (Grossman and Hart 1983). However, incentive compatibility requires that consumption be increasing in output on average. Moreover, if agents can costlessly "burn output," monotonicity may be required (Bolton and Dewatripont 2005).

insurance friction. The contribution of this paper is to develop and empirically implement a set of testable predictions which distinguishes *between* the hidden income-, limited commitment- and moral hazard-constrained insurance.

I show that, when insurance is constrained by limited commitment or moral hazard, a household's "history" matters in a specific way in predicting that household's current consumption: conditional on the village's shadow price of resources (a measure of the aggregate shock faced by the village), a household's lagged inverse marginal utility ("LIMU") is a sufficient statistic for forecasting the household's current inverse marginal utility. This implies that no other past information should improve the forecast of current inverse marginal utility made using LIMU. Allowing the distribution of household income to depend on actions taken by the household in the past (investment, for instance) does not overturn this sufficiency result.

On the other hand, when household income is unobserved, a household's LIMU is no longer a sufficient statistic in forecasting consumption. Because low-income households are optimally assigned low consumption, hence high marginal utility, their temptation to claim *even lower* income (resulting in a higher transfer), is highest for these households. Because truthful households value current consumption more than misreporting households, while truthful and misreporting households value promised future consumption equally, incentive compatibility is attained by reducing the expected future surplus promised to low-income households relative to their current consumption. That is, the timing of households' consumption is distorted in the hidden income model, meaning that community's expected cost of providing each household's marginal unit of consumption is not equated across periods. In a world without private information, this distortion would be inefficient: a given level of expected utility can be provided most efficiently if the cost of the marginal unit is equated in expectation across periods. However, in the hidden income model, this distortion in the timing of consumption serves a screening purpose: households truthfully reporting low income value an extra unit of consumption in that period more than households falsely reporting low income, whereas truthful and untruthful households put equal value on expected utility promised in the future.

The tests of limited commitment and moral hazard I derive generalize existing results from the contract economics literature (Kocherlakota 1996), (Rogerson 1985), while the hidden income test is a new result. The second contribution of this paper is to empirically implement these tests, examining the relationship between LIMU and current consumption in rural Thailand using 84 months (7 years, 1999-2005) of the Townsend Thai Monthly Survey. Sufficiency of LIMU is rejected:

lagged income has predictive power in forecasting current inverse marginal utility. Moreover, the prediction errors generated with LIMU alone display a significant, positive correlation with lagged income, as predicted by the hidden income model. This suggests that the ability of household to hide their income plays a role in generating the observed comovement between income and consumption.

An important consideration in implementing these tests is the concern that consumption is measured with error. Measurement error in right-hand variables is usually seen as a threat to power, causing under-rejection of the null hypothesis (in this case, sufficiency of LIMU), but the tests used here, measurement error can distort the size of the test, causing over-rejection of the null. Accounting for measurement error in lagged consumption using instrumental variables techniques and by testing over-identifying restrictions on the reduced form equations for current and lagged consumption does not overturn the rejection of sufficiency of LIMU. This suggests that measurement error is not driving the conclusion that neither limited commitment nor moral hazard can explain the relationship between current consumption, past consumption and past income.

The rest of the paper is organized as follows: Section 2 provides a brief overview of related literature. Section 3 outlines the benchmarks of full insurance and pure borrowing-saving, discusses the three barriers to insurance (moral hazard, limited commitment and hidden income), and explains the theoretical approach for distinguishing among these barriers. Section 4 explains how these theoretical predictions can be empirically tested, accounting for measurement error in consumption and uncertainty about the form of households' utility functions. Section 5 discusses the data used to implement these tests, Section 6 presents the results and Section 7 concludes. Proofs are contained in Appendix A. Tables appear in Appendix B, and figures are in Appendix C.

1.2 Related literature

Several papers have examined whether limited commitment- or moral hazard-constrained insurance explain consumption and income data better than pure borrowing-saving or full insurance models. The contribution of this paper, relative to the existing literature is, first, to propose and implement a test of the hidden income model, which to my knowledge, has not previously been empirically tested. Another novel contribution of this testing procedure is that it can distinguish the hidden income model not only from full insurance and borrowing-lending, but also from limited commitment- and moral hazard-constrained insurance. The third contribution of this paper relative to existing literature is that, unlike maximum likelihood and GMM approaches, the tests proposed here do not rely on a particular specification of the production technology or the utility function.

Ligon (1998) uses a generalized method of moments (GMM) approach to test moral hazard-constrained insurance against full insurance and borrowing-saving (i.e., the permanent income hypothesis) in India using ICRISAT village data, and finds that moral hazard best explains consumption data in 2 of 3 villages; in the third some households' consumptions are better explained by the PIH. Ligon, Thomas and Worrall (2002) use a maximum likelihood approach to test full insurance against limited commitment, also in the ICRISAT villages. They find that limited commitment explains consumption dynamics, but not why high-income households consume as little as they do relative to low-income households. Lim and Townsend (1998) incorporate capital assets and livestock into a moral hazard-constrained insurance model, and find that it fits the ICRISAT consumption data better than the PIH or full insurance using a maximum likelihood approach. Cox et al. (1998) argue that qualitative features of lending in Peruvian villages are inconsistent with full insurance or the PIH, but consistent with limited commitment. Albarran and Attanasio (2003) show that the comparative statics of a limited commitment model are matched by data from Mexico following the introduction of Progresa. Dubois et al. (2008) develop a model with limited commitment and incomplete formal contracts and find, using a maximum likelihood approach, that its predictions are matched in Pakistani data. Kocherlakota and Pistaferri (2009) review the literature which uses the asset-pricing implications of incomplete markets (borrowing-lending only) and private information (moral hazard/adverse selection) economies; they find that the asset-pricing implications of the moral hazard/adverse selection model fit US, UK and Italian data with a "reasonable" coefficient of relative risk aversion (estimated using a GMM approach), while the implications of the borrowing-lending model are rejected. Hayashi et al. (1996) review the literature on full consumption smoothing in the US and find that neither endogeneity of labor nor nonseparability between labor and consumption explains the rejection of full smoothing of food consumption in the PSID. Blundell et al. (2008) document that persistent income shocks are partially insured in the US, and even transitory shocks are not fully insured for low-wealth households.

Several papers have examined whether private information about households' productivity (a Mirrlees-style adverse selection model) can explain incomplete insurance in developed economies. Kaplan (2006) derives quantitative predictions about the amount of risk sharing that would arise, for a given wage distribution, under limited commitment versus a setting with observed income but unobserved productivity. Ai and Yang (2007) find that a model with limited commitment and private information about productivity (but observed income) better fits quantitative features US data than a model with limited commitment alone.

The implications of full consumption insurance have been characterized by Wilson (1968), Cochrane (1991), Mace (1991) and Townsend (1994). The inverse Euler equation implication of moral hazard-constrained insurance was first characterized by Rogerson (1985), and Phelan (1998) developed a recursive formulation of the moral hazard problem. The limited commitment model was first characterized by Kimball (1988) and Coate and Ravallion (1993). The hidden income model was first characterized by Townsend (1982) and Green (1987). The method used in this paper, distinguishing hidden income from limited commitment and moral hazard using the first-order conditions of the social planner's problem, draws on the characterization of efficient limited commitment-constrained insurance in Kocherlakota (1996) (which is described in section 1.3), and on the recursive formulation of the hidden income problem developed in Thomas and Worrall (1990).

The next section presents the benchmark cases of full consumption insurance and pure borrowing-saving, and then shows how the full insurance benchmark is altered by the presence of limited commitment, moral hazard and hidden income.

1.3 Models of optimal consumption smoothing: full insurance, borrowing-saving, moral hazard, limited commitment, hidden income

1.3.1 Setting

As a simplified approximation to the environment in a village, consider N risk-averse households who interact over an infinite time horizon in a mutual insurance network. Let i index households and t index time. Each household evaluates per capita consumption and effort plans according to:

$$U(\mathbf{c}_i, \mathbf{e}_i) = \mathbb{E} \sum_{t=0}^{\infty} \beta^t \left[v(c_{it}) - z(e_{it}) \right]$$

The specification of $U(\mathbf{c}_i, \mathbf{e}_i)$ embodies the assumption of no *ex ante* heterogeneity among households:

Assumption 1 *All households have a common discount factor β, and common, additively separable utility of per capita consumption and disutility of effort functions $v(c)$ and $z(e)$. Utility is increasing and concave in per capita consumption: $v' > 0$ and $v'' < 0$.*

Following Thomas and Worrall (1990), I also make the following assumption:

Assumption 2 *Absolute risk aversion is non-increasing:*

$$d\left(\frac{-v''(c_{it})}{v'(c_{it})}\right)/dc_{it} \leq 0$$

This assumption guarantees the concavity of the value function in the hidden income model (Thomas and Worrall 1990); it is satisfied by the commonly-used constant relative risk aversion and constant absolute risk aversion utility functions. It seems to be a natural assumption since, as pointed out by Arrow (1971), increasing absolute risk aversion implies that higher-wealth individuals would be more averse to a given absolute gamble than lower-wealth individuals; that is, risky assets would be inferior goods.

A key assumption is:

Assumption 3 *As long as any household is participating in the village insurance network, the household's borrowing and savings decisions are contractible.* (As described below, if a household leaves the village insurance network they may have access to a borrowing-savings technology with a weakly lower return.) As a result, savings and borrowing by network member households are determined as if chosen by a welfare-maximizing planner, not to maximize the household's own expected payoff. This may appear to be a strong assumption, but given the prevalence of joint savings groups (ROSCAs) in rural Thailand, and of borrowing from and saving with "village funds," where accounts are overseen by a committee of village members, this assumption is not implausible. Contractibility of borrowing and saving can be implemented as long as other households can observe a household's asset position, since transfers and future utility can be conditioned on the household choosing the recommended level of assets.[2]

Moreover, when insurance is limited by hidden income, if households can privately save at the same interest rate available to the community, no interpersonal insurance is possible, because the household will always find it in their incentive to report whatever income realization yields the

[2] Collins et al. (2009) document that in samples of Indian, Bangladeshi and South African households, ROSCAs and other types of group savings arrangements (saving-up clubs and accumulating savings and credit associations, or ASCAs) are the primary means by which households accumulate sums of savings equal to one month's income or more. A key feature of these clubs and associations is that members know how much one another have contributed and borrowed.

highest present discounted value of current and future transfers (Allen 1985), (Cole and Kocherlakota 2001). Therefore, to the extent that the predictions of the pure borrowing-saving (PIH) model are rejected in the data in favor of the hidden income model, the joint hypothesis of hidden income and hidden savings (at the same interest rate as the community) is also rejected.[3]

I assume that the community-controlled borrowing-saving technology has gross return $R \geq 1$. There is an autarkic technology with gross return $R' < R$. (If no savings is possible in autarky, $R' = 0$.) Because the community-controlled borrowing-saving technology is assumed to have a strictly higher rate of return, the contractibility of savings implies that any net borrowing or saving by agents in the network (such that (1.21) does not hold with equality) will take place via the community-controlled technology.

When specifying the value of autarky below, I will make the following assumption:

Assumption 4 *Agents cannot take savings accumulated while in the insurance network with them into autarky.* Even in this case, potential access to the autarkic borrowing-saving technology after leaving the insurance agreement will reduce the amount of insurance attainable in a limited commitment insurance relationship (Ligon, Thomas, and Worrall 2000). Allowing households to take their savings with them into autarky will further reduce the amount of feasible insurance, but does not change the properties of efficient insurance derived below, because the effect of such savings, which is to raise the value of autarky, will be fully captured in the consumption allocation to households tempted to leave the insurance network.

Finally, the following assumptions are made on the production technology:

Assumption 5a *Output can take on S values, $\{y_1, ..., y_S\}$.* Indices are chosen so that a higher index means more output: $r > q \Rightarrow y_r > y_q$. The number of possible output realizations is restricted to be finite (although potentially very large).[4] This assumption is required for the approaches of Grossman and Hart (1983) characterizing the optimal contract under moral hazard, and the approach of Thomas and Worrall (1990) characterizing the optimal contract under hidden income, to be valid.

[3]Doepke and Townsend (2006) show that when income is hidden, if households can privately borrow and save at a sufficiently different interest rate than the community, some insurance is possible. Although the optimal contract is then difficult to characterize analytically, Doepke and Townsend show numerically that access to private storage at a very low gross return does not distort insurance very much, relative to the no-private-savings case, because the low return dampens the temptation to privately save. This suggests that "saving under the mattress," which likely carries a negative net return due to inflation and risk of theft, may not pose too great a threat to the characterization of the optimal contract derived below. Formally introducing the possibility of hidden savings to the model is left to future work.

[4]For instance, in the context of Thailand, income could take any one-baht increment from zero to one million baht.

Assumption 5b *Effort can take on two values in each period, working* ($e_t = 1$) *or shirking* ($e_t = 0$). This assumption is made for simplicity and allowing for additional levels of effort, including a continuum of effort levels, would not substantially change the results. Effort costs are normalized as:

$$\begin{aligned} z(1) &= 1 \\ z(0) &= 0 \end{aligned}$$

Like the assumption of a finite number of income levels, the following assumption is required for Grossman and Hart's approach to the moral hazard to be valid:

Assumption 5c *For every feasible level of promised utility* u, *there exists a feasible transfer schedule* $\{\tau_{r1}(u)\}$ *that delivers, in expectation, exactly* $u + z(1)$, *gross of effort costs, when high effort is exerted, and a feasible transfer schedule* $\{\tau_{r0}(u)\}$ *that delivers exactly* $u+z(0)$ *in expectation when low effort is exerted. The first schedule satisfies the "promise-keeping" constraint for an agent with promised utility* u *who is assigned high effort* ($e = 1$), *and the second satisfies the promise-keeping constraint for an agent with promised utility* u *who is assigned low effort* ($e = 0$).

Since the main result for the moral hazard and limited commitment models is that a single lag of inverse marginal utility is sufficient to capture the extent to which history has influenced what the household is promised, a natural question is whether this relies on a "memoryless" production process, with income *i.i.d.* across time. In fact, allowing the distribution of income to depend on actions taken by the household in the past does not overturn this result. To make this point, I make the following assumption:

Assumption 5d *The distribution of income at time* t *is affected by household's effort at time* t *and at time* $t-1$:[5]

$$\Pr(y_t = y_r) = \Pr(y_r | e_t, e_{t-1})$$

Define $p_{ree'} \equiv \Pr(y_r | e_t = e, e_{t-1} = e')$, the probability of income realization y_r when an effort level e is exerted in the current period and e' was exerted in the last period. So, p_{r11} is the probability of output level y_r if high effort was exerted in the current period and the previous period, etc. The next assumption (full support of output under high or low effort) rules out schemes that achieve

[5] Allowing more than one lag of effort to influence the distribution of output would further complicate the notation, but would not change the results. Golosov et al. (2003) show that an Inverse Euler equation relationship is obtained in a wide variety of adverse selection economies with very general production functions.

full insurance by punishing the household severely if a level of output occurs that is impossible when the recommended effort is followed:

Assumption 5e *Each of the S income realizations occurs with positive probability under either high or low effort:*

$$p_{ree'} \in (0,1), \forall e, e', r$$

Finally, so that there may be a nontrivial moral hazard problem if effort is not observable, I make the assumption that surplus (expected output less effort costs) is higher when households exert effort than when they do not:

Assumption 5f *Effort raises expected surplus:*

$$\sum_{r=1}^{S} \left[p_{r11} - p_{r01}\right] y_r > \sum_{r=1}^{S} \left[p_{r10} - p_{r00}\right] y_r \geq z(1) - z(0)$$

Having set out the environment, I will briefly characterize the benchmark cases full insurance and pure borrowing-saving before introducing the constraints which may lead to incomplete interpersonal insurance.

1.3.2 Full insurance

We can find the set of first-best allocations by considering the problem of a hypothetical risk-neutral planner who maximizes the utility of villager N such that each villager 1 to $N-1$ gets at least a value u_{it} in period t. Let $\mathbf{u}_t \equiv \{u_{it}\}_{i=1}^{N-1}$ be the vector of time t utility promises and $\mathbf{e}\prime \equiv \{e_{i,t-1}\}_{i=1}^{N-1}$ be the vector efforts that were exerted at time $t-1$. The state variables of the planner's problem are $\mathbf{u}_t, \mathbf{e}', a_t$. The planner chooses effort recommendations e_{it}, transfers τ_{irt}, and future promises $u_{ir,t+1}$ for each villager. Transfers, which are equal to the difference between a household's income and its consumption, $\tau_{irt} \equiv c_{irt} - y_r$; and future promises, which summarize the utility the household can expect from next period onward (Spear and Srivastava 1987); are indexed by r because they may be income-contingent (though the dependence of promised utility $u_{ir,t+1}$ on the income realization y_r will be degenerate in the case of full insurance while the dependence of the transfer τ_{irt} on the income realization will be degenerate in the case of pure borrowing-saving). The planner's value function is:

$$u_N(\mathbf{u}_t, a_t, \mathbf{e}') \equiv \max_{\mathbf{e},\{\tau_{rt}\},\{\mathbf{u}_{r,t+1}\}} \tag{1.1}$$

$$\sum_{r=1}^{S} p_{ree'} v(y_r + \tau_{Nrt}) - z(e_N) + \beta \mathbb{E}_{\{y\}} u_N(\mathbf{u}_{t+1}, a_{t+1}, \mathbf{e})$$

subject to the promise-keeping constraints that each household 1 to $N-1$ must get their promised utility u_{it} (in expectation):

$$\sum_{r=1}^{S} p_{ree'}[v(y_r + \tau_{irt}) - z(e_i) + \beta u_{ir,t+1}] = u_{it}, \forall i < N \tag{1.2}$$

and the law of motion for assets:

$$a_{t+1} = R\left[a_t - \sum_{i=1}^{N} \tau_{irt}\right] \tag{1.3}$$

Let the multiplier on household i's time t promise-keeping constraint be λ_{it} and the multiplier on the village's time t budget constraint be η_{it}.

As is well known, absent problems of commitment or information, every village member's consumption is independent of their own income realization, given aggregate village resources. Therefore we have

Proposition 1 *Under full insurance, (a) realized household income has no effect on household consumption, given village aggregate consumption, and (b) with no preference heterogeneity and a common discount factor, households never change place in the village consumption distribution.*

Proof. *In Appendix A.* ■

In summary, full insurance predicts a complete decoupling of idiosyncratic income shocks and consumption changes. Since this implication fails to hold in virtually every dataset where it has been tested, the next question is how to distinguish among models that do predict a correlation between income shocks and consumption changes. I will first discuss the other benchmark case of no interpersonal insurance (borrowing and saving only) and then the moral hazard, limited commitment and hidden income models.

1.3.3 Borrowing-saving only (PIH)

Hall (1978) showed that, when households discount the future at rate β and can save and borrow at rate R, but have access to no interpersonal or state-contingent assets, marginal utility follows a random walk (even if income is correlated over time):

$$\mathbb{E}_{t-1}u'(c_t) = \beta R u'(c_{t-1}) \tag{1.4}$$

An implication of the Euler equation (1.4) characterizing the path of consumption under a pure borrowing-saving model is that, once lagged marginal utility $u'(c_{t-1})$ is controlled for, no other information dated $t-1$ or before should predict current marginal utility. Borrowing and saving allows the household to smooth its path of consumption independent of the timing of receipt of expected income (appropriately discounted). Unanticipated innovations to income are smoothed optimally over time (but not across households), starting from the time they are realized, so there is no tendency for consumption to revert to its pre-innovation mean: a household that receives a negative income shock with have lower expected consumption (higher expected marginal utility) permanently thereafter.

As discussed below, optimal moral hazard- and limited commitment-constrained insurance lead to the implication that, conditional on last period's *inverse* marginal utility, no other lagged information should predict current inverse marginal utility. These implications (sufficiency of marginal utility vs. sufficiency of inverse marginal utility) will not be distinguishable with isoelastic or nonparametrically estimated utility. With isoelastic utility, in a log specification sufficiency of the proposed statistic under limited commitment and moral hazard, $\ln\left(\frac{1}{u'(c_{i,t-1})}\right) = \rho \ln c_{i,t-1}$, cannot be distinguished from sufficiency of the proposed statistic under under borrowing-saving, $\ln u'(c_{i,t-1}) = -\rho \ln c_{i,t-1}$. With nonparametrically estimated utility, both implications reduce to the requirement that there exists a function $f(c_{i,t-1})$ conditional on which no other lagged information predicts $f(c_{it})$. However, if sufficiency of (inverse) marginal utility is not rejected, it is possible to test among borrowing-saving, moral hazard and limited commitment using other implications, discussed below.

1.3.4 Moral hazard

The moral hazard model has been widely used to explain imperfect insurance in developing and developed countries. Under a moral hazard model, the agent must be given incentives to do something—such as exert effort or invest—which cannot be directly observed or contracted on. The action occurs before output is realized and affects the expected level of output. Introducing incentive compatibility constraints to the optimal insurance setup implies that Proposition 1 no longer necessarily holds. With two effort levels, and a utility function separable in consumption and effort, the incentive-compatibilty constraint will be binding at the optimum (Grossman and Hart 1983). The constraint is:

$$\sum_{r=1}^{S} p_{r11}[v\left(y_r + \tau_{irt}\right) + \beta u_{ir,t+1}] - z(1) = \sum_{r=1}^{S} p_{r01}[v\left(y_r + \tau_{irt}\right) + \beta \hat{u}_{ir,t+1}]$$

i.e. the household must expect the same level of surplus (net of effort costs $z(1)$) if it exerts effort in the current period as the household expects if it shirks (and pays no effort cost).[6]

The inverse Euler equation implication[7] of moral hazard-constrained insurance (Rogerson 1985) has been used to test the moral hazard model against the PIH, which predicts a standard Euler equation. The moral hazard model considered by Rogerson assumed that the distribution of time t output was affected only by the agent's effort at time t. However, Fernandes and Phelan (2000) show that when the distribution of income depends on past as well as current effort, the moral hazard problem still has a recursive formulation, with two[8] additional "threat-keeping" constraints added to the planner's problem. These constraints enforce an upper bound on a household's expected utility from today on if the household disobeyed yesterday's effort recommendation, whether they obey or disobey today. The constraint requiring that, if the household disobeyed (shirked) yesterday but obeys (works) today (so that the relevant probabilities are p_{r10}), it does not expect higher utility than $\hat{u}_{it}$, is:

$$\sum_{r=1}^{S} p_{r10}[v(y_r + \tau_{irt}) - z(1) + \beta u_{ir,t+1}] \leq \hat{u}_{it}$$

The constraint requiring that, if the household disobeyed yesterday *and* disobeys today (shirking in both periods, so that the relevant probabilities are p_{r00}), it does not expect higher utility than

[6]The constraint is written for a household that exerted effort in the previous period (i.e., the household compares the probabilities p_{r11} with the probabilities p_{r01}, both of which reflect having exerted effort in the previous period) since by Assumption 5f effort raises expected surplus and so households will exert effort along the equilibrium path; the constraints which ensure this are discussed below.

[7]The Inverse Euler equation implies that inverse marginal utility follows a random walk: $\frac{1}{v'(c_{i,t-1})} = \beta R \mathbb{E}_{t-1}\left(\frac{1}{v'(c_{it})}\right)$.

[8]If there are N effort levels instead of 2, there are $N(N-1)$ threat-keeping constraints, but the solution method is unchanged.

$\hat{u}_{it}$, is:

$$\sum_{r=1}^{S} p_{r00}[v(y_r + \tau_{irt}) - z(0) + \beta \hat{u}_{ir,t+1}] \leq \hat{u}_{it}$$

Using Fernandes and Phelan's recursive setup, I show in Appendix A that the inverse Euler equation also holds under moral hazard even if the distribution of output depends on actions taken in past periods as well as the current period.[9] Therefore, a single lag of inverse marginal utility (LIMU) is a sufficient statistic in forecasting current inverse marginal utility, even with such technological linkages between periods:

Proposition 2 *When insurance is constrained only by moral hazard, conditional on the time t shadow price of resources η_t, LIMU $\left(\frac{1}{u'(c_{i,t-1})}\right)$ is a sufficient statistic for household i's time t inverse marginal utility.*

Proof. *In Appendix A.* ■

We obtain the result that, conditional on η_t, time $t-1$ inverse marginal utility is a sufficient statistic for all $t-1$ information for forecasting time t consumption because in the moral hazard-constrained model (and in the limited commitment model discussed below), income is observed. As a result, the planner or community directly controls consumption and marginal utility. Moreover, the temptation preventing full insurance (in this case, the temptation to shirk) is evaluated at the same levels of consumption and marginal utility that the household actually realizes in equilibrium. Therefore, expected marginal utility can be expressed as a function of the past only via lagged inverse marginal utility. It will turn out that this property also holds under limited commitment, another workhorse model of incomplete informal insurance.

1.3.5 Limited commitment

If an agent can walk away from the insurance network at any time if he can do better in autarky, Proposition 1 no longer necessarily holds (Coate and Ravallion 1993). Limited commitment imposes further constraints on the planner's problem (1.1), which is now subject to the promise-keeping constraints (whose multipliers are λ_{it}), the budget constraint (with multiplier η_{it}) and the participation constraints that the expected utility an agent gets in the insurance network be at least as great as the expected utility he could achieve in autarky, choosing his own savings and

[9] Golosov et al. (2003) show a similar result for adverse selection economies with very general production functions; see note 5.

effort optimally. That is, a household will only remain in the network if

$$v(y_r + \tau_{irt}) + \beta u_{ir,t+1} \geq u_{aut}(y_r, e), \forall i, r \tag{1.5}$$

where

$$\begin{aligned} u_{aut}(y_r, e) \equiv & \max_{s_t, e_{t+1}} v(y_r - s_t) - \beta z(e_{t+1}) \\ & + \beta \mathbb{E}\left[u_{aut}(y_{t+1} + R' s_t) | e_{t+1}, e\right] \end{aligned}$$

Sufficiency of lagged inverse marginal utility

Kocherlakota (1996) showed that, under limited commitment, the vector of lagged marginal utility ratios for every member of the insurance group,

$$\left\{ \frac{v'(c_{N,t-1})}{v'(c_{i,t-1})} \right\}_{i=1}^{N-1}$$

is a sufficient statistic for history when forecasting any household's consumption. This vector specifies a unique point on the Pareto frontier and therefore captures all relevant information in forecasting any households' future consumption. However, Kocherlakota's result is not directly testable if the econometrician does not have information on all the members of the insurance group. Since consumption and income data generally come from surveys, rather than censuses, the test has limited empirical applicability. In Kocherlakota's setting, the need to keep track of the past consumption of every member of the insurance network in order to forecast any member's current consumption arises due to the assumption that the village as a whole cannot borrow or save. If the village can borrow and save, the shadow price of resources at time t serves as a summary measure of how much consumption must be given to other households in the village. In this case, we have the following result, which is testable with panel data for only a sample of households in a network.

Proposition 3 *With village-level credit access, conditional on the time t shadow price of resources η_t, household i's LIMU $\left(\frac{1}{v'(c_{i,t-1})}\right)$ is a sufficient statistic for household i's time t inverse marginal utility under limited commitment. When i's participation constraint binds, i's current and expected future consumption are increasing in i's income.*

Proof. *In Appendix A.* ■

The intuition for this result is that, when the only barrier to full insurance is the fact that the household can walk away when income is high, the principal can allocate consumption to a household who is tempted to walk away without affecting the incentive of any other household to stay in the network, except through the tightness of the village's budget constraint. The constrained household gets current consumption and a future promise that make it exactly indifferent between staying in or leaving the network. At the optimum, providing a household with utility in the current period (through current consumption c_{it}) should be exactly as effective as providing promised utility in the future (through the utility promise $u_{i,t+1}$). Therefore, the Lagrange multiplier on the household's promise-keeping constraint uniquely describes the efficient combination of $c_{it}, u_{i,t+1}$. Moreover, under limited commitment the household's lagged inverse marginal utility fully captures the Lagrange multiplier on the promise-keeping constraint. So LIMU $\left(\frac{1}{v'(c_{i,t-1})}\right)$ captures all the information from time $t-1$ and earlier that is relevant in predicting household i's time t consumption, c_{it}. The need to control for the time t shadow price of resources, η_t, arises because η_t captures the "size of the pie" at time t, while $\frac{1}{v'(c_{i,t-1})}$ captures the share of that pie that will, in expectation, go to household i.

Since, as discussed above, the same sufficiency result is obtained under moral hazard (with the additional, stronger implication of an Inverse Euler equation), and with isoelastic or nonparametrically estimated utility an indistinguishable result holds under the PIH[10], if we are unable to reject sufficiency of LIMU in a given setting, this does not tell us whether limited commitment, moral hazard, or borrowing-saving is a more plausible alternative. Thus, before moving on to discuss hidden income, I discuss a stronger implication of limited commitment that would allow a researcher to distinguish limited commitment from moral hazard and borrowing-saving in the case that sufficiency of LIMU is not rejected.

Amnesia

A stronger implication of limited commitment, which does not hold under moral hazard or borrowing-saving, is what Kocherlakota calls "amnesia." As noted above, when limited commitment binds for household i, consumption c_{irt} and promised future utility $u_{ir,t+1}$ are pinned down by the require-

[10]As discussed in section 1.3.3, in a log specification with isoelastic utility sufficiency of the proposed statistic under limited commitment and moral hazard, $\ln\left(\frac{1}{u'(c_{i,t-1})}\right) = \rho \ln c_{i,t-1}$, cannot be distinguished from sufficiency of the proposed statistic under under borrowing-saving, $\ln u'(c_{i,t-1}) = -\rho \ln c_{i,t-1}$. With nonparametrically estimated utility, both implications reduce to that there exists a function $f(c_{i,t-1})$ conditional on which no other lagged information predicts $f(c_{it})$.

ment that the household be just indifferent between staying in and leaving the network, and that the utility value of current and future consumption be equated at the margin:

$$v(y_r + \tau_{irt}) + \beta u_{ir,t+1} = u^t_{aut}(y_r)$$

$$v'(y_r + \tau_{irt}) = -\left(\frac{\partial u_N(\mathbf{u}_{r,t+1})}{\partial u_{ir,t+1}}\right)^{-1}$$

independent of the time t promised value u_{it}. Thus the household's old promised value, u_{it}, is "forgotten" when limited commitment binds. Kocherlakota suggests the following procedure to test for amnesia: find the network member(s) with the lowest growth in consumption between periods $t-1$ and t. Ignoring measurement error in consumption for now (see Section 1.6), define

$$B_t \equiv \min_{i=1,\ldots,N} v'(c_{i,t-1})/v'(c_{it})$$

Those for whom $v'(c_{i,t-1})/v'(c_{it}) > B_t$, by construction, had binding limited commitment constraints—otherwise their consumption would have been fully smoothed between periods $t-1$ and t. Those with $v'(c_{i,t-1})/v'(c_{it}) = B_t$ were not constrained, and therefore did achieve full intertemporal consumption smoothing. Define the sets of constrained and unconstrained households

$$\begin{aligned} C_t &\equiv \{i : v'(c_{i,t-1})/v'(c_{it}) > B_t\} \\ U_t &\equiv \{i : v'(c_{i,t-1})/v'(c_{it}) = B_t\} \end{aligned}$$

Amnesia implies that, for any constrained household $i \in C_t$, LIMU $\left(\frac{1}{v'(c_{i,t-1})}\right)$ should not predict current consumption c_{it}, given current income y_{jt}. That is, if we estimate the regression

$$\ln c_{it} = \alpha_1 \ln c_{i,t-1} + \alpha_2 \ln y_{it} + \delta_v + \varepsilon_{it} \tag{1.6}$$

for those households $i \in C_t$, limited commitment implies, since the households are constrained, $\alpha_1 = 0$: the old promises are forgotten. This test is implemented, and the results discussed, in Section 1.6.

The result that, when insurance is constrained by either limited commitment or moral hazard, the village's current shadow cost of resources and a household's LIMU should together be a sufficient statistic for the past in forecasting the household's current inverse marginal utility, arises because in these models (unlike the hidden income model) income is observed, so the community can effectively control consumption by controlling income-contingent transfers. As a result, there is no deviation from the optimal division of promised utility across periods—utility in the current period (via transfers) and utility in future periods (via promised utility) are equally valuable to the household.

1.3.6 Hidden income

As well as issues of *ex ante* information (moral hazard) and of limited commitment, *ex post* informational asymmetries may also restrict the type of (implicit or explicit) contracts that agents can enter into, and thereby restrict insurance. Namely, it may be that income is not observable by the community, and households must be given incentives to report it (Townsend 1982). It turns out that such *ex post* informational asymmetries cause the sufficiency result of limited commitment and moral hazard to break down.

Assume now that agents can commit to the insurance arrangement and that effort is observable. However, household income is not observable by other households. Potentially $S(S-1)$ incentive-compatibility constraints are added to the planner's problem:

$$\begin{aligned} v(y_r + \tau_{irt}) + \beta u_{ir,t+1} &\geq v(y_r + \tau_{ir',t}) + \beta u_{ir',t+1} \\ r' &\in \mathcal{S}\backslash y_r \end{aligned}$$

These constraints require that a household realizing any of the S income levels must not gain by claiming any of the $S-1$ other possible levels. However, Thomas and Worrall (1990) show that only the $S-1$ local downward constraints, which require that an agent getting income y_r not prefer to claim the slightly lower income y_{r-1}, will be binding at the optimum. These constraints are:

$$\begin{aligned} &v(y_r + \tau_{irt}) + \beta u_{ir,t+1} = v(y_r + \tau_{i,r-1,t}) + \beta u_{i,r-1,t+1}, \\ &r = 2, ..., S \end{aligned}$$

The first-order conditions of the problem imply:

Proposition 4 *When agents can commit to the insurance agreement, and effort is contractible, but output is hidden, forecasts using only* $\frac{1}{v'(c_{i,t-1})}$ *and* η_t *will over-predict consumption for households with the lowest time* $t-1$ *income realizations, and the degree of overprediction will decline with the level of time* $t-1$ *income (controlling for an interaction between time* $t-1$ *income and the aggregate shock* η_t*).*

Proof. *In Appendix A.* ■

The intuition for this difference between hidden income on one hand, and limited commitment and moral hazard on the other is that, in the limited commitment and moral hazard cases, the temptation of a household with high output to claim a lower level of output is not a relevant constraint, and as a result there is no deviation from the optimal division of promised utility across periods–utility in the current period (via transfers) and utility in future periods (via promised utility) are equally valuable to the household. As a result, all past information relevant to forecasting current consumption is encoded in last period's consumption. When income is private information, in contrast, consumption is not effectively controlled by the community, and the constrained-optimal schedule of transfers and promised utilities distorts the trade-off between current consumption and future expected utility, with households announcing low incomes being penalized more in terms of future utility, which is equally valuable to truthful and misreporting households, than current consumption, which is more valuable to truthful households, who have lower income than households who are tempted to falsely claim the same level of income.

Aggregate risk may matter because if the network receives a positive income shock, there is a potentially countervailing effect: all agents consume more than would have been predicted using past marginal utility, and the aggregate shock is divided unequally between high- and low-past income households. (In the limited commitment and moral hazard cases, on the other hand, lagged inverse marginal utility is the only past information which determines how the aggregate shock is divided among households. Scheuer (2009) discusses the implications of aggregate risk in the moral hazard case.)

Therefore, under hidden income, estimating (1.10) should lead to $\hat{\zeta} \neq 0$, since $\ln y_{i,t-1}$ has predictive power in forecasting current inverse marginal utility not captured in LIMU. A further implication of the hidden income model is that, if the residuals defined in (1.11) are regressed on lagged income:

$$\hat{\varepsilon}_{it} = \alpha + \beta \ln y_{i,t-1} + u_{it} \tag{1.7}$$

we should find $\alpha < 0, \beta > 0$, because the residuals will be negative at the lowest levels of past income ($\alpha < 0$) and the residuals will be increasing in past income ($\beta > 0$). On the other hand, if we are unable to reject $\alpha = \beta = 0$, this is evidence for either limited commitment, moral hazard or borrowing-saving, which can then be distinguished based on the amnesia test discussed above, the inverse Euler equation implication of moral hazard, and the Euler equation implication of the

PIH. The results of this test are discussed in Section 1.6.

An additional implication of hidden income: insufficiency of LIMU is less when income is less variable

An additional prediction of the hidden income model is that a reduction in the variability of a household's income process will have the effect of making truth-telling constraints less binding, which in turn implies a reduced wedge between LIMU and expected promised utility:

Proposition 5 *A decrease in variability of the income process (in the sense of that the new distribution is second-order stochastically dominated by the old distribution, keeping the probability of each income realization the same) reduces the degree to which LIMU over-predicts current inverse marginal utility for low-lagged income households.*

Proof. *In Appendix A.* ■

The intuition for this result is that, the less uncertainty about a household's income, the less binding are truth-telling constraints. Since the truth-telling constraints are the cause of the wedge between LIMU and expected promised utility, relaxing the constraints reduces the wedge. Therefore, if one household's income process is more predictable than another's, the household with more predictable income should exhibit a reduced degree of overprediction at the bottom. The results of this test are also discussed in Section 1.6.

1.4 Distinguishing barriers to insurance

1.4.1 Testable implication of limited commitment or moral hazard

The fact that, under either limited commitment or moral hazard, all past information relevant to forecasting current consumption is encoded in last period's consumption implies that the prediction errors

$$\hat{\varepsilon}^*_{it} \equiv \frac{1}{v'(c_{it})} - \mathbb{E}\left(\frac{1}{v'(c_{it})} | \frac{\eta_t}{v'(c_{i,t-1})}\right) \tag{1.8}$$

should be uncorrelated with past income, a finding that contrasts with the prediction of the hidden income model discussed below. Of course, implementing this test requires assuming or estimating a functional form for $v()$. A natural starting point is the constant relative risk aversion (CRRA) function. There is some empirical evidence that the CRRA function provides a good fit for actual behavior (Szpiro 1986); moreover Schulhofer-Wohl (2006) shows that CRRA can be viewed as a local approximation to any concave utility function. With CRRA utility with coefficient of relative risk aversion ρ, the utility function is:

$$v(c_{it}) = \begin{cases} \frac{c_{it}^{1-\rho}}{1-\rho} \text{ if } \rho \neq 1 \\ \ln(c_{it}) \text{ if } \rho = 1 \end{cases}.$$

Since the coefficient of relative risk aversion ρ is unknown, ideally the test would be implemented in a way that did not depend on assuming a particular value of ρ. One implication of no correlation between the prediction errors (1.8) and $y_{i,t-1}$ is that the prediction errors are not systematically high for high (low) values of $y_{i,t-1}$ and systematically low for low (high) values of $y_{i,t-1}$, an implication that is preserved by taking a monotonic transformation of (1.8). That is, we can test whether the transformed prediction errors

$$\hat{\varepsilon}_{it}^{**} \equiv \ln \frac{1}{v'(c_{it})} - \mathbb{E}\left(\ln \frac{1}{v'(c_{it})} | \ln \frac{1}{v'(c_{i,t-1})}, \ln \eta_t\right)$$

are uncorrelated with past log income.

When utility is CRRA,

$$\ln\left(\frac{1}{v'(c_{i,t-1})}\right) = \rho \ln c_{i,t-1}$$

so the value of $\rho > 0$ will not affect the sign of $\hat{\varepsilon}_{it}^{**}$. Since $\ln \eta_t$ enters additively, it can be controlled for by adding a village-year effect δ_{vt}. Then, expected inverse marginal utility $\mathbb{E}\left(\frac{1}{v'(c_{it})} | \frac{1}{v'(c_{i,t-1})}, \eta_t\right)$ is proportional to the predicted value from the regression

$$\ln c_{ivt} = \gamma \ln c_{iv,t-1} + \delta_{vt} + \varepsilon_{ivt}. \tag{1.9}$$

Sufficiency of LIMU implies that if we add $\ln y_{i,t-1}$ (or any other variable dated $t-1$ or earlier) to (1.9) and estimate

$$\ln c_{ivt} = \gamma \ln c_{iv,t-1} + \zeta \ln y_{i,t-1} + \delta_{vt} + \varepsilon_{ivt} \tag{1.10}$$

we should be unable to reject $\hat{\zeta} = 0$. Another way to test the sufficiency implication is to test whether the residuals

$$\hat{\varepsilon}_{it} \equiv \ln c_{ivt} - \hat{\gamma} \ln c_{iv,t-1} - \hat{\delta}_{vt} \tag{1.11}$$

are uncorrelated with $\ln y_{i,t-1}$ or any other variable dated $t-1$ or earlier. The results of the regression-based test using (1.10) and the results of the residuals-based test using (1.11) are discussed in Section 1.6.[11]

However, two further empirical issues must be considered in distinguishing among different insurance regimes: agents' utility functions are not known, and consumption is measured with error. Both of these, if not accounted for, can result in biased inference about the nature of the barrier to full insurance.

1.4.2 Measurement Error in Expenditure

Classical measurement error

If expenditure is measured with classical error, the estimated coefficient on LIMU in (1.7) will be attenuated toward zero. This will result in biased predictions of consumption using LIMU. To see what form the bias will take, note that we want to estimate the part of consumption that is unexplained by LIMU and village-year effect:

$$\varepsilon_{ivt} = \ln c_{ivt} - \delta_{vt} - \gamma \ln c_{iv,t-1} \tag{1.12}$$

Assume an error-ridden measure of consumption is observed,

$$\tilde{c}_{iv,t-1} = c_{iv,t-1} \cdot \nu_{iv,t-1}$$

where the measurement error $\nu_{iv,t-1}$ is uncorrelated with true time $t-1$ consumption, $c_{iv,t-1}$, or true time t consumption, c_{ivt}. The estimated prediction error is constructed using observed lagged consumption $\tilde{c}_{iv,t-1}$, and the estimates of γ and δ:

$$\hat{\varepsilon}_{ivt} = \ln c_{ivt} - \hat{\delta}_{vt} - \hat{\gamma} \ln \tilde{c}_{iv,t-1}$$

Assume the true data-generating process is insurance constrained by limited commitment or moral hazard, so that LIMU is in fact a sufficient statistic for forecasting current inverse marginal utility.

[11] Estimating (1.8) for various values of ρ leads to similar conclusions as tests using (1.11); results available on request.

Then, the forecast error (1.12) will be uncorrelated with lagged income:

$$\mathbb{E}\underbrace{(\ln c_{ivt} - \gamma \ln c_{iv,t-1} - \delta_{vt})}_{\text{"true" residual } \varepsilon_{ivt}} y_{iv,t-1} = 0 \tag{1.13}$$

However, if γ is estimated by OLS, the null hypothesis (1.13) may potentially be incorrectly rejected, because $\hat{\gamma}$ is biased downward:

$$p \lim \hat{\gamma} = \gamma \left(1 - \frac{\sigma_\nu^2}{\sigma_c^2 + \sigma_\nu^2}\right)$$

The estimated residual is then positively correlated with lagged income, because fraction $\frac{\sigma_\nu^2}{\sigma_c^2+\sigma_\nu^2}$ of current log consumption is incorrectly not projected onto lagged log consumption, and this term is correlated with lagged income (because under either limited commitment or moral hazard, contemporaneous income and consumption are positively correlated):

$$\begin{aligned}
\hat{\varepsilon}_{ivt} &= \ln c_{ivt} - \hat{\delta}_{vt} - \hat{\gamma} \ln \tilde{c}_{iv,t-1} \\
p \lim \hat{\varepsilon}_{ivt} &= \ln c_{ivt} - \hat{\delta}_{vt} - \gamma \left(1 - \frac{\sigma_\nu^2}{\sigma_c^2 + \sigma_\nu^2}\right) \ln \tilde{c}_{iv,t-1} \\
&= \underbrace{\ln c_{ivt} - \hat{\delta}_{vt} - \gamma \ln \tilde{c}_{iv,t-1}}_{\text{uncorrelated w/ } y_{iv,t-1}} + \underbrace{\frac{\sigma_\nu^2}{\sigma_c^2 + \sigma_\nu^2} \gamma \ln \tilde{c}_{iv,t-1}}_{+ \text{ correlated w/ } y_{iv,t-1}}
\end{aligned}$$

That is, we may conclude wrongly that $corr(\hat{\varepsilon}_{ivt}, y_{iv,t-1}) > 0$, that is, that LIMU is not a sufficient statistic, when consumption is measured with classical error, because lagged income is then in effect a second proxy for true LIMU.

However, for classical error, there is a straightforward solution. If γ is estimated using the second lag of consumption as an instrument for the first lag, we obtain a consistent estimate of γ:

$$\begin{aligned}
p \lim \hat{\gamma}^{IV} &= \frac{cov(\ln \tilde{c}_{iv,t-2}, \ln \tilde{c}_{ivt})}{cov(\ln \tilde{c}_{iv,t-2}, \ln \tilde{c}_{iv,t-1})} \\
&= \gamma \left(1 - \frac{cov(\nu_{t-2}, \nu_{t-1})}{\underbrace{cov(\ln \tilde{c}_{iv,t-2}, \ln \tilde{c}_{iv,t-1})}_{=0}}\right)
\end{aligned}$$

Then, the probability limit of the residual is

$$\begin{aligned} p\lim \hat{\varepsilon}^{IV}_{ivt} &= \ln \tilde{c}_{ivt} - \hat{\delta}_{vt} - \gamma \ln \tilde{c}_{iv,t-1} \\ &= \ln c_{ivt} + \ln \nu_{ivt} - \hat{\delta}_{vt} - \gamma \left(\ln c_{iv,t-1} + \ln \nu_{iv,t-1}\right) \end{aligned}$$

Rearranging,

$$p\lim \hat{\varepsilon}^{IV}_{ivt} = \underbrace{\ln c_{ivt} - \gamma \ln c_{iv,t-1} - \delta_{vt}}_{\text{“true” residual}} + \underbrace{\ln \nu_{ivt}}_{\text{meas. error in } c_{ivt}} - \gamma \underbrace{\ln \nu_{iv,t-1}}_{\text{meas. error in } c_{iv,t-1}}$$

Under the hypothesis that true lagged inverse marginal utility ($\ln c_{iv,t-1}$) is a sufficient statistic, the “true” residual (1.12) is uncorrelated with lagged income. Moreover, if the measurement error in (log) consumption is classical, $\ln \nu_{ivt}$ and $\ln \nu_{iv,t-1}$ are also uncorrelated with lagged income:

$$corr(\ln \nu_{ivt}, y_{iv,t-1}) = corr(\ln \nu_{iv,t-1}, y_{iv,t-1}) = 0$$

Therefore, with classical measurement error and a true data-generating process of limited commitment or moral hazard, instrumenting the first lag of consumption with the second lag of consumption will lead to the correct conclusion:

$$p\lim \hat{\varepsilon}^{IV}_{ivt} y_{iv,t-1} = 0.$$

Non-classical measurement error

Using the second (or longer) lag of consumption as an instrument will not address non-classical measurement error which is correlated over time. A possible solution in this case is to move lagged consumption from the right- to the left-hand side of the equation of interest, and test overidentifying restrictions on the reduced form equations for $\ln c_{it}$ and $\ln c_{i,t-1}$. If lagged income affects current consumption only through lagged consumption, then all components of lagged income, or any other lagged information $x_{i,t-s}$ which predicts lagged income, should satisfy the restriction

$$\frac{d \ln c_{it}}{dx_{i,t-s}} / \frac{d \ln c_{i,t-1}}{dx_{i,t-s}} = K, \forall x_{i,t-s}$$

That is, a unit change in an instrument $x_{i,t-s}$ should have the same relative effect on current versus lagged consumption as a unit change in another instrument $x'_{i,t-s}$.

Under the null of limited commitment/moral hazard, consumption depends on a household's initial Pareto weight and its subsequent income realizations. (Under limited commitment or moral hazard, lagged income does not belong in the structural equation for consumption, but it appears in the reduced form because y_{is} depends on c_{is}.) Three lags of income are significant predictors of c_{it}, so write

$$\ln c_{it} = \sum\nolimits_{s=1}^{3} \alpha_s y_{i,t-s} + \hat{\lambda}_0 + \varepsilon_{it}$$

where $\hat{\lambda}_0$ is a measure of the household's Pareto weight as of 1999: the household's rank in the 1999 per-capita consumption distribution for the village.

Since lagged income appears in the reduced form for consumption, lags of total income cannot be used to generate overidentifying restrictions. Instead, I test whether the *composition* of lagged income matters for predicting current consumption, beyond its effect on lagged consumption. In particular, I test whether income from crop cultivation matters differently than income from raising livestock or fish and shrimp. If crops are more homogenous than animals, less susceptible to difficult-to-verify disease, or simply easier to observe by virtue of growing in a fixed location rather than being mobile, reporting low income from animal cultivation may result in a greater wedge between current and future utility than reporting low income from crop cultivation. That is, animal cultivation income would be associated with high contemporaneous consumption relative to future consumption, while crop cultivation income would be associated with lower contemporaneous consumption relative to future consumption. This would not be the case under the other models of incomplete insurance. While different types of income may convey different information about effort, or different information about the household's prospects in autarky, under limited commitment or moral hazard that information will be completely encoded in consumption. Under hidden income, in contrast, the components of income will also matter through the direct effect of lagged income on current consumption. So in the reduced-form regressions

$$\begin{aligned} \ln c_{it} &= \sum_{s=1}^{3} [\pi_{1Cs} y_{i,t-s}^{crops} + \pi_{1Ls} y_{i,t-s}^{livestock}] + \hat{\lambda}_{i0} + \varepsilon_{it} \\ \ln c_{i,t-1} &= \sum_{s=1}^{3} [\pi_{2Cs} y_{i,t-s}^{crops} + \pi_{2Ls} y_{i,t-s}^{livestock}] + \hat{\lambda}_{i0} + \varepsilon_{i,t-1} \end{aligned}$$

and

$$\begin{aligned}
\ln c_{it} &= \sum_{s=1}^{3}[\pi_{1Cs}y_{i,t-s}^{crops} + \pi_{1Fs}y_{i,t-s}^{fish}] + \hat{\lambda}_{i0} + \varepsilon_{it} \\
\ln c_{i,t-1} &= \sum_{s=1}^{3}[\pi_{2Cs}y_{i,t-s}^{crops} + \pi_{2Fs}y_{i,t-s}^{fish}] + \hat{\lambda}_{i0} + \varepsilon_{i,t-1}
\end{aligned}$$

if the first lag of income does not directly affect current consumption, we should find

$$\frac{\pi_{1C1}}{\pi_{2C1}} = \frac{\pi_{1L1}}{\pi_{2L1}}$$

and

$$\frac{\pi_{1C1}}{\pi_{2C1}} = \frac{\pi_{1F1}}{\pi_{2F1}}$$

These overidentifying restrictions can be used to test whether the rejection of limited commitment is only due to measurement error.

1.4.3 Specification of u()

The test of hidden income proposed above is to test whether $\varepsilon_t \perp y_{t-1}$ in

$$\ln\left(\frac{1}{v'(c_{it})}\right) = \delta_t + \ln\left(\frac{1}{v'(c_{i,t-1})}\right) + \varepsilon_{it} \tag{1.14}$$

However, since the form of $v()$ is unknown, the approach above was to approximate it with the isolelastic function

$$\begin{aligned}
v(c_{it}) &= \frac{c^{1-\rho}}{1-\rho} \\
\ln\left(\frac{1}{v'(c_{it})}\right) &= \rho \ln(c_{it})
\end{aligned}$$

and test $\hat{\varepsilon}_t \perp y_{t-1}$ in

$$\ln(c_{it}) = \delta_{vt} + \ln(c_{i,t-1}) + \hat{\varepsilon}_{it} \tag{1.15}$$

This raises the question, if the true error ε_t satisfies $\varepsilon_t \perp y_{t-1}$ in (1.14), will testing $\hat{\varepsilon}_t \perp y_{t-1}$ in (1.15) yield the correct conclusion? Nonparametrically estimating $\frac{1}{v'(c)}$ avoids the need to make an assumption about the form of the utility function. In order to correct for measurement error as well, a nonparametric IV approach seems most appropriate.

One possible approach would be to use the nonparametric 2SLS approach of Newey and Powell (2003) to estimate

$$f\left(c_{it}\right)=f\left(\tilde{c}_{i,t-1}\right)+\delta_{vt}+\hat{\varepsilon}_{it}$$

where $\tilde{c}_{t-1}$ is estimated using a nonparametric first stage with c_{t-2} as an instrument. However, consistency of this estimator requires that $f()$ and its derivatives are bounded in the tails, if $\tilde{c}_{t-1}$ is not bounded. Since in this context $f()$ is an inverse marginal utility function which may tend to infinity as consumption tends to infinity, this is an unappealing assumption in this context. Newey and Powell's approach also requires the conditional mean zero assumption:

$$\mathbb{E}\left(\varepsilon_{it}|\tilde{c}_{i,t-2}\right)=0$$

which is stronger than the assumption needed for linear IV:

$$corr\left(\varepsilon_{it},\tilde{c}_{i,t-2}\right)=0$$

Fortunately, inspection of the nonparametric first stage between $\ln(\tilde{c}_{t-1})$ and $\ln(\tilde{c}_{t-2})$ shows it to be nearly linear (see Figure 2), suggesting that linear IV may be a suitable approach. Therefore, I nonparametrically estimate $f()$, using a 5-knot spline,[12] in

$$\ln\left(\tilde{c}_{t}\right)=f\left(\hat{c}_{t-1}\right)+\delta_{vt}+\tilde{\varepsilon}_{t}$$

Then, $f\left(\hat{c}_{t-1}\right)$ is linearly instrumented with $f\left(\hat{c}_{t-2}\right)$. The fitted relationship $f\left(\hat{c}_{t-1}\right)$, graphed in Figure 3, is quite similar to the log form implied by CRRA, which is also shown. This is consistent with other empirical evidence suggesting that the CRRA utility function is, in fact, a reasonable approximation to actual utility functions (Szpiro 1986).

1.4.4 Summary: Distinguishing barriers to insurance

The preceding discussion suggests four tests that, in combination, can be used to distinguish among limited commitment, moral hazard, hidden income, and borrowing-saving (PIH):

[12] Results are not sensitive to the number of knots used. (Results using a 7-knot spline available on request.)

1. Sufficiency of $\frac{1}{v'(c_{i,t-1})}$: under limited commitment, moral hazard, or borrowing-saving (PIH):

$$\mathbb{E}\left(\frac{1}{v'(c_{it})}|\frac{1}{v'(c_{i,t-1})},\eta_t,x_{i,t-s}\right)=\mathbb{E}\left(\frac{1}{v'(c_{it})}|\frac{1}{v'(c_{i,t-1})},\eta_t\right),\forall x_{i,t-s},s>0$$

.

2. Amnesia: under limited commitment, if household i is constrained at t:

$$\mathbb{E}\left(\frac{1}{v'(c_{it})}|\frac{1}{v'(c_{i,t-1})},\eta_t,y_{it}\right)=\mathbb{E}\left(\frac{1}{v'(c_{it})}|\eta_t,y_{it}\right)$$

3. Overprediction at the bottom: under hidden income:

$$\begin{aligned}\mathbb{E}\left(\left[\frac{1}{v'(c_{it})}-\mathbb{E}\left(\frac{1}{v'(c_{it})}|\frac{1}{u'(c_{i,t-1})},\eta_t\right)\right]\middle| y_{i,t-1}=0\right) &< 0\\ &\text{and}\\ \frac{d}{dy_{i,t-1}}\left(\frac{1}{v'(c_{it})}-\mathbb{E}\left(\frac{1}{v'(c_{it})}|\frac{1}{u'(c_{i,t-1})},\eta_t\right)\right) &> 0\end{aligned}$$

4. Inverse Euler equation: under moral hazard:

$$\frac{1}{v'(c_{i,t-1})}=\mathbb{E}_{t-1}\left(\frac{1}{v'(c_{it})}\right)$$

These tests are summarized in the following table:

	Autarky/PIH	Limited com.	Moral hazard	Hidden inc.
Sufficiency of $\ln c_{t-1}$	✓	✓	✓	
Amnesia		✓		
Overprediction at the bottom				✓
Inverse Euler			✓	

Ligon (1998) and Attanasio and Pavoni (2009) test for asymmetric information regarding agents' choice of actions (moral hazard) using GMM approaches, while Karaivanov and Townsend (2008) test across several moral hazard models as well as the PIH using an MLE approach. The test proposed here has the advantage of accommodating nonparametric estimates of the utility function, rather than requiring the specification of a parametric form, and requiring no assumptions on the form of the production function. Of course, in the event that the assumptions imposed by

GMM/MLE methods are correct, they may provide more powerful tests, but such assumptions are difficult to test and may result in incorrect conclusions if the assumptions are incorrect.

1.5 Data

Data are from the 1999-2005 waves of the Townsend Thai Monthly Survey, which covers 16 villages in central and northeastern Thailand, 4 each in four provinces, two in the central region near Bangkok and two in the northeast. In each village, 45 households were initially selected at random and reinterviewed each month. (See Townsend et al. (1997) for details.) Detailed data were collected on households' demographic composition and their income, including farms, businesses, and wage employment. Information was also collected on household expenditure, using detailed bi-weekly and monthly surveys. Thus expenditure is likely to be quite well-measured in this dataset, relative to datasets which measure expenditure over a longer recall period and/or which collect information on only a subset of expenditures, such as only food (as in the Panel Survey of Income Dynamics in the US).

A total of 531 households appear in all 84 months of the survey period used here, out of an original 670 who were interviewed in January 1999. I focus on the continuously-observed sample so that changes in a household's rank in the PCE distribution are not due to migration in and out of the survey. Differences between the continuously-observed sample and the initial sample are reported in Table 3. Smaller households and those whose head is engaged in rice farming or construction are most likely not to be continuously observed, while corn and livestock farmers are more likely to be continuously observed. This degree of missing data is a concern; however, residuals of income and consumption (partialing out demographic, village, year and occupation variables) do not differ across the two samples. Imputing income and expenditure data for missing household-months based on village, year, occupation and baseline demographic variables and running the analysis on this sample, yields results similar to the results for the continuously-observed sample.[13]

Summary statistics are reported in Table 1. Average household size is 4.5, or 3.8 adult equivalents. Average reported monthly per capita expenditure was 5,213 2002 baht (approximately 124 2002 US dollars.[14]). Average reported monthly income per capita is higher than expenditure at 8,981 baht, due to investment.

[13] Results available on request.

[14] The exchange rate in 2002 was approximately 42 baht=\$1. All following references to baht refer to 2002 baht.

Households are classified into occupations based on the primary occupation reported by the household head in the initial wave of the survey. The most common occupation in the sample is rice farming (35% of household heads), followed by non-agricultural labor (including owning a non-agricultural business) (12% of household heads), growing corn (10%), raising livestock (9%), and agricultural wage labor (5%). Growing other crops, raising fish or shrimp, growing orchard crops, and construction each account for less than 5%. Seven percent report an occupation classified as "other."

Another strength of the Townsend Thai Monthly Survey data is that households are asked separately about gifts and transfers (both in money and in-kind) from organizations, from households in the village, and from households outside of the village. All of these types of transfers are prevalent: gifts given to other households in the same village equal 5.4% of average expenditure, while gifts from others in the same village equal 9% of average expenditure. Gifts/remittances given to those outside the household's village equal 17.5% of average expenditure, and gifts/remittances received from those outside the village equal 27.7% of average expenditure. Moreover, these numbers exclude transfers embodied in interest-free, low-cost and flexible loans, which are prevalent in these villages, as well as in other settings ((Platteau and Abraham 1987), (Udry 1994), (Fafchamps and Lund 2003)) The significant magnitude of intra-village transfers is direct evidence that within-village insurance is important, while transfers made with those outside the village may constitute a source of unobserved income.

Finally, using data from rain gauges located in each village, yielding a measure of total rainfall in each village in each month between 1999 and 2003, quarterly rainfall variables (deviations from the provincial average in that quarter over the entire period) were constructed following Paxson (1992):

$$
\begin{aligned}
& R_{qvt} - \bar{R}_{qp}, \left(R_{qvt} - \bar{R}_{qp}\right)^2, \qquad (1.16) \\
& q = 1, 2, 3, 4
\end{aligned}
$$

The rainfall variables are used to construct instruments for income in the tests of full insurance, and for tests of the hidden income model. The next section presents the empirical results.

1.6 Results

1.6.1 Insurance is imperfect...

If households were perfectly insured, there would be no need to look for evidence of a particular insurance friction—if household consumption did not move with contemporaneous household income, and all villagers' consumptions moved one-for-one with average village consumption, this would mean that none of hidden income, moral hazard, or limited commitment was a significant impediment to full insurance. This is not the case for rural Thailand. To see this, I estimate the standard omnibus test of full insurance (Townsend 1994) using the January 1999-December 2005 waves of the Townsend Thai Monthly Survey:[15]

$$\ln c_{it} = \alpha \ln y_{it} + \beta_i + \varepsilon_{it} \tag{1.17}$$

where c_{it} is household i's per-capita consumption at time t, y_{ivt} is household i's income at time t and β_i is a household-fixed effect, yields $\hat{\alpha} = .078$ ($t = 10.5$). (See Table 2, column 1.) That is, a 10% change in household income is associated with a .78% change in contemporaneous per capita consumption.[16]

Adding village-year dummy variables δ_{vt} to capture common changes in villagers' consumption due to change in aggregate resources (indexing households by v to denote their village) and estimating

$$\ln c_{ivt} = \alpha \ln y_{ivt} + \beta_{iv} + \delta_{vt} + \varepsilon_{ivt} \tag{1.18}$$

reduces the correlation between income and consumption deviations (from the household means) to $\hat{\alpha} = .067$ ($t = 9.2$). (See Table 2, column 2.[17]) The significance of the village-year indicators is direct evidence that village-level networks are providing insurance, as discussed below, but the continued significant correlation between income and consumption changes demonstrates that this

[15]As detailed in Section 4, income and expenditure data are collected monthly. However, throughout the paper I aggregate the 84 months of data to the annual level because the correspondence between expenditure and consumption is likely to be higher at annual frequencies than monthly frequencies. Aggregating to the annual level will also reduce the importance of measurement error if recall errors are uncorrelated across months.

[16]Consumption is measured as expenditure and converted to per capita terms using the equivalence scale used by Townsend (1994) for Indian villages. The weights are: for adult males, 1.0; for adult females, 0.9. For males and females aged 13-18, 0.94, and 0.83, respectively; for children aged 7-12, 0.67 regardless of gender; for children 4-6, 0.52; for toddlers 1-3, 0.32; and for infants 0.05. Using an equivalence scale that accounts for within-household economies of scale (Olken 2005) does not significantly affect any reported results (results available on request).

[17]A first-differenced specification with a village-year effect yields a correlation of .04 ($t = 4.30$), the same point estimate found by Chiappori et al. (2008) for the same dataset.

insurance is incomplete.[18]

Measurement error in income is a concern in interpreting the individual and village results. Classical measurement error in income (uncorrelated with the true values of income changes and with the error terms ε), will attenuate $\hat{\alpha}$ toward zero. This would make the extent to which income changes predict consumption changes in the data a lower bound on the true sensitivity of consumption to income. In this case, instrumenting income with variables correlated with true income but uncorrelated with the measurement error should then result in a higher estimate of α. Because many households in these villages work in agriculture, rainfall is a possible instrument. As discussed above, village-level monthly rainfall data is available for the years 1999-2003. Following the strategy of Paxson (1992), I instrument income changes with the interactions between occupation indicators[19] and deviations of quarterly income from the province-wide quarterly average defined in (1.16), and occupation interactions with squared deviations:

$$\begin{aligned}
&\mathbf{1}(occ_i = o) \times R_{qvt} - \bar{R}_{qp}, \\
&\mathbf{1}(occ_i = o) \times \left(R_{qvt} - \bar{R}_{qp}\right)^2, \\
&q = 1, 2, 3, 4; o \in \{1, 10\}
\end{aligned}$$

Using the occupation-rainfall variables as instruments for income raises the coefficient on income changes significantly, to $\hat{\alpha}^{IV} = .21$ ($t = 5.4$) without the inclusion of village-year dummy variables (Table 2, column 4), and $\hat{\alpha}^{IV} = .17$ ($t = 3.9$) when the village-year dummies are added. Once measurement error in income is addressed, the evidence is even stronger that households bear a substantial fraction of their idiosyncratic income risk, although village-level insurance does smooth a significant portion of income risk, as discussed below.

Another telling feature of the data is a large amount of movement in the village per capita expenditure (PCE) distribution: the correlations between household PCE rankings in adjacent years range from .824 (1999-2000) to .539 (2000-2001). (See Table 3, Panel A.) Moreover, PCE rank changes are not random, as they would be if driven by classical error in expenditure, but are predicted by income changes, with a +10% change in income associated with an increase in the

[18] Townsend (1995) also finds imperfect insurance in northern Thai villages in the years 1989-1991.

[19] Households were asked in the initial wave of the survey about the primary occupation of each adult household member. The response of the household head was used to classify the household, with responses grouped into 10 categories: farm rice, farm corn, farm orchard crops, farm other crops, raise livestock, raise fish/shrimp, agricultural wage labor, non-agricultural wage labor, construction, and other.

PCE distribution of about one-half of a ranking. An ordered probit regression shows that, at the mean income level, a +10% change in income is associated with a 5.9% increase in the probability of moving up in the consumption distribution. (See Table 3, Panel B.)

Absent taste shocks and with no heterogeneity in risk aversion, churn in the consumption distribution is incompatible with full insurance, as discussed above, as is $\alpha \neq 0$ in (1.17). However, insurance constrained by either limited commitment, hidden income, or moral hazard would predict both $\alpha > 0$ and $corr(rank_{it}, rank_{it'}) < 1$.

1.6.2 ...but villages do provide insurance

Finding $\alpha < 1$ in equation (1.17) does not establish that villages provide insurance: households could smooth consumption using borrowing and saving (Hall 1978), (Deaton 1991), or the relevant risk-sharing network might be a different group, such as kinship groups. The presence of intravillage insurance can be established by testing the hypothesis that the village-year effects in (1.17) are jointly insignificant in explaining household consumption changes. If these village-year effects play a significant role in explaining consumption changes, this implies that villagers' consumptions move together, evidence of the spillover implied by inter-village insurance. The hypothesis of no common component to within-village consumption changes is strongly rejected: $F(111, 3210) = 5.256, p = 0.000$ in the OLS regression (table 1, column 2) and $F(63, 1814) = 3.471, p = 0.000$ in the IV regression (table 2, column 5), indicating that there is a highly significant tendency for the consumption of households in the same village to move together.

To get a quantitative estimate of the extent of within-village insurance, Suri (2005) notes that an additional implication of a set of households belonging to an insurance group is that household consumption is less correlated with household income, conditional on total group consumption, than group average consumption is correlated with group average income. If we estimate the village-fixed effects specification

$$\ln c_{PCvt} = \alpha^W \ln y_{ivt} + \beta_i + \delta_{vt} + \varepsilon_{ivt} \tag{1.19}$$

and the between-village (or village average) specification

$$\overline{\ln c_{vt}} = \overline{\ln y_{vt}} \alpha^B + \bar{\varepsilon}_{vt} \tag{1.20}$$

where $\ln c_{ivt}$ and $\ln y_{ivt}$ are the log-per capita consumption and log-income of household i in village v at time t, and $\overline{\ln c_{vt}}$ and $\overline{\ln y_{vt}}$, are the time t averages of log-consumption and log-income for village v, insurance at the village level implies $\frac{\alpha^W}{\alpha^B} < 1$. Suri (2005) shows that the "contrast estimator"

$$\hat{\beta} = 1 - \frac{\alpha^W}{\alpha^B}$$

is a measure of the extent of insurance provided by village-level networks. (Under the null hypothesis that villages do not provide insurance, household consumption would be no less correlated with household income, conditional on total group consumption, than group average consumption is correlated with group average income, implying $\alpha^W = \alpha^B$ and $\hat{\beta} = 0$.)

Estimating (1.20) by OLS yields $\alpha^{B,OLS} = .172$, while $\alpha^{W,OLS} = .0669$. (See table 2, columns 2 and 3.) This implies $\hat{\beta}^{OLS} = .61$. Estimating (1.20) by IV, using quarterly rainfall deviations and squared deviations as instruments for average village income yields $\alpha^{B,IV} = .300$, while $\alpha^{W,IV} = .174$ (see table 2, columns 5 and 6), implying $\hat{\beta}^{IV} = .421$.

Whether estimated by OLS or IV, $\hat{\beta}$ is well below one: belonging to a village network does not remove all idiosyncratic risk, but village networks do manage to reduce dependence of household consumption on household income by between 40 and 60 percent. Section 1.3 discussed three models that attempt to rationalize this finding of partial insurance: limited commitment, hidden income, and moral hazard.

1.6.3 Credit is available

The form of the contract that the hypothetical village social planner can offer to a household depends on whether the village's budget must balance each period. If so, a constraint on the planner's problem is that, at each date and state of the world, total consumption among the villagers ($i \in V$) cannot exceed their total income:

$$\sum_{i \in V} c_{it} \leq \sum_{i \in V} y_{it}, \forall t. \tag{1.21}$$

Alternatively, if borrowing and savings are possible, subject only to a terminal condition,[20] village assets a_{vt} evolve according to

$$a_{v,t+1} = R\left[a_{vt} + \sum_{i \in V}(y_{it} - c_{it}^T)\right] \tag{1.22}$$

[20] $a_{v,T+1} = 0$ if T is finite or, if T is infinite, $\sum_{t=1}^{\infty} R^{-t}(y_{it} - c_{it}^T) \leq a_{v0}$.

where R is the gross interest rate and y_{it} and c_{it}^T are the income and total (not per capita) consumption of household $i \in V$.

Dependence of village consumption at time t on village income at t can be tested with with between-village estimator (1.20). As noted above, $\alpha^{B,OLS} = .172$ (table 1, col 3) and $\alpha^{B,IV} = .300$ (table 2, col 6). Therefore, even correcting for measurement error in income, villages are far from living "hand to mouth," consuming total village income period-by-period. This suggests that village institutions (banks, moneylenders, local government, etc.) have access to a national-level credit market or a set of equivalent institutions.

1.6.4 Testing sufficiency of lagged inverse marginal utility

Under limited commitment, moral hazard, or autarky, current inverse marginal utility should only depend on the past through $\frac{1}{v'(c_{i,t-1})}$. If households' consumptions are described by efficient insurance constrained by limited commitment or moral hazard, we should find $\gamma \neq 0, \zeta = 0$ in

$$\ln c_{it} = \gamma \ln c_{i,t-1} + \zeta' X_{i,t-1} + \delta_{vt} + \varepsilon_{it} \tag{1.23}$$

where $X_{i,t-s}$ is any information dated $t-1$ or before. Table 4 presents the results of this test. While lagged inverse marginal utility is significantly predictive of current inverse marginal utility (column 1), lagged log income is also a significant predictor of current inverse marginal utility ($p < .001$) in the full sample (column 2). The result is unchanged when the top and bottom 5% of per capita expenditure (by year) are dropped, to address the concern that very high or low observed consumption may be due to measurement error. (See columns 3 and 4.) This suggests that neither limited commitment or moral hazard alone can explain the failure of full insurance in these villages.

1.6.5 Testing amnesia

Table 5 presents tests of the amnesia prediction of the limited commitment model. If there is measurement error in expenditure, exactly following Kocherlakota's proposed procedure for implementing this test—classifying as constrained every household in a village who had consumption growth above the village minimum—would result in every household but one in each village appearing constrained. In fact, many of these households will be unconstrained, and including them in the set of households for whom amnesia is predicted will introduce bias toward rejecting the predictions of limited commitment. To address this, in columns 1 through 4, interaction terms between $\ln \frac{1}{v'(c_{i,t-1})}$ and indicators for the quartile of the village distribution of consumption growth between $t-1$ and t into which the household fell ($\mathbf{1}_q$); and similar interaction terms with $\ln(y_{i,t})$ are added to (1.6). That is, estimate

$$\ln c_{it} = \alpha + \beta_1 \ln c_{i,t-1} + \sum_{q=2}^{4} \beta_q \ln c_{i,t-1} \times \mathbf{1}_q + \gamma_1 \ln y_{it} + \sum_{q=2}^{4} \gamma_q \ln y_{i,t} \times \mathbf{1}_q + \delta_{vt} + \varepsilon_{it}$$

If past promises are forgotten, conditional on current income, for those who had highest consumption growth due to binding participation constraints, the sum of the coefficients on the LIMU terms $\beta_1 + \beta_q$ should be low and insignificant for higher quartiles of consumption growth and, since the main effect of $\ln \frac{1}{v'(c_{i,t-1})}$ is positive and significant, β_4 should be negative. In fact, these predictions are rejected. The pattern of coefficients β_q is the opposite of that predicted by amnesia—LIMU is *more* strongly (positively), predictive of current consumption, conditional on current income, for households with higher consumption growth: β_4 is larger than β_3, which in turn is larger than β_2 ($\beta_4 = .201 > \beta_3 = .152 > \beta_2 = .134$). For those in the highest quartile of consumption growth, the hypothesis that $\beta_1 + \beta_4$ equals zero is overwhelmingly rejected (point estimate $.057, p < .001$), suggesting again that limited commitment is not the (entire) explanation for incomplete insurance in these villages.

As a second test, columns 5 and 6 estimate (1.6) for households with above-median consumption growth, separately for villages where the variability of rainfall from year to year is high and villages where rainfall variability is low, based on monthly rainfall data from 1999-2003. Villages with high rainfall variance also had higher average income variance in every year but 2004, when the opposite is true—see Figure 1. If measurement error in expenditure is independent of the variance in incomes, then when high consumption growth is observed in high-rainfall-variance (HRV) villages, it is more likely to be due to a high income realization resulting in a binding participation constraint. In low-rainfall-variance (LRV) villages, high consumption growth is more likely to be due to measurement error. This suggests that, if limited commitment is the true model, the amnesia prediction should do better in HRV villages, i.e. the coefficient on $\ln \frac{1}{v'(c_{i,t-1})}$ in column 6 should be less than in column 5. Indeed, the point estimate for HRV villages is lower than for LRV villages, but the two estimates are not statistically different ($p = .66$). Therefore, both the sufficiency and amnesia predictions of the limited commitment model are strongly rejected.

1.6.6 Testing hidden income: insufficiency of LIMU and predictive power of lagged income

Table 6, Panel A presents the results of the tests that under hidden income LIMU will overpredict consumption for those households whose promises decreased, i.e. who had low income in the previous period, while under moral hazard or limited commitment, the prediction errors will be uncorrelated with last-period income because LIMU is a sufficient statistic for history, hence no additional lagged information will contain predictive power. Consistent with the hidden income prediction, when the prediction errors (1.11) are regressed on lagged income (and lagged income and lagged income squared interacted with the aggregate shock measure η_t) the slope is positive and significant while the intercept is significantly negative (column 1). Since the dependent variable is a regression residual, which has mean zero by construction, the slope and intercept are not independent. The joint hypothesis that $\alpha = 0, \beta = 0$ is rejected at the .0001 level. Column 2 repeats this test without the aggregate shock interaction terms, showing that the overprediction result holds unconditionally; i.e., the potential countervailing effect of increased aggregate resources does not undo the overprediction result. Again, the joint hypothesis that $\alpha = 0, \beta = 0$ is rejected at the .0001 level.

Columns 3 and 4 of table 6 show that instrumenting $\ln c_{iv,t-1}$ with $\ln c_{iv,t-2}$ does not overturn the finding that the prediction residuals are negative at low levels of lagged income: the null that the slope and the intercept in (1.7) are both 0 is rejected at the 1% level. This suggests that the rejection of sufficiency of LIMU is not driven by classical measurement error.

To check the robustness of the insufficiency of LIMU to non-classical measurement error, the tests of overidentifying restrictions on the reduced forms for current consumption and lagged consumptions are presented in Table 7. Columns 1 and 2 present the results of comparing the reduced forms of $\ln c_{it}$ and $\ln c_{i,t-1}$ using crop and livestock income as "instruments" for consumption. Time $t-1$ crop income is associated with higher consumption at time t than at $t-1$, while the opposite is true for time $t-1$ livestock income, consistent with what would be expected if crop income were easier to observe than livestock income. The hypothesis that $\frac{\pi_{1C1}}{\pi_{2C1}} = \frac{\pi_{1L1}}{\pi_{2L1}}$ is rejected at the 5% level (p=.0422). Columns 3 and 4 present the results of comparing the reduced forms of $\ln c_{it}$ and $\ln c_{i,t-1}$ using crop and fish income as instruments, and the results are similar, again consistent with what would be expected if crop income were easier to observe than income from aquaculture, although in this case the hypothesis that $\frac{\pi_{1C1}}{\pi_{2C1}} = \frac{\pi_{1F1}}{\pi_{2F1}}$ is rejected at the 10% level (p=.0535). This suggests that the rejection of sufficiency of LIMU is not due to measurement error in lagged consumption, but in fact arises because reporting low levels of difficult-to-observe income is associated with a greater penalty in terms of future consumption than contemporaneous consumption.

Finally, to check the robustness of this finding to allowing for a utility function that is not CRRA, table 8 shows that when $\frac{1}{v'(c)}$ is estimated nonparametrically, sufficiency of LIMU is still rejected. Panel A shows that there is still a significant positive association between the prediction errors $\hat{\varepsilon}_t$ (formed using a nonparametric estimate of LIMU) and lagged income. When the forecast of inverse marginal utility based on LIMU is estimated by OLS, sufficiency of LIMU is once again rejected, at the 1% level (column 1). Because measurement error is still a concern, column 2 presents results instrumenting nonparametrically estimated LIMU with the second lag of nonparametric inverse marginal utility. Sufficiency of LIMU is still rejected, now at the 5% level. Table 8, Panel B presents the results of an alternative specification of the test of sufficiency of LIMU. Analogously to equation (1.23), results for which are shown in table 4, Panel B estimates

$$\ln c_{it} = f\left(c_{i,t-1}\right) + \zeta \ln y_{i,t-1} + \delta_{vt} + \varepsilon_{it}$$

where $f\left(c_{i,t-1}\right)$ is the nonparametric estimate of LIMU. Sufficiency of LIMU implies $\zeta = 0$—lagged income should contain no additional information relevant to forecasting current inverse marginal utility once $f\left(c_{i,t-1}\right)$ is controlled for. The hidden income model, in contrast, predicts $\zeta > 0$, since higher lagged income implies a higher forecast of current inverse marginal utility. In fact, as in the CRRA formulation in table 4, ζ is significantly positive, significant at the 1% level in the OLS specification and at the 5% level in the IV specification. Given that the nonparametric estimate of $f\left(\hat{c}_{t-1}\right)$ is quite similar to the CRRA form, it is not surprising that the two methods yield similar conclusions about the (in)sufficiency of LIMU.

To summarize, a wide variety of evidence suggests that hidden income constraints cause those with low past income to receive less current consumption (i.e. lower current inverse marginal utility) than predicted by LIMU, while those with high past income receive more consumption and higher current inverse marginal utility. This suggests that insurance is constrained by the need to provide incentives to high-income households to truthfully reveal that income. This finding does not appear to be driven by measurement error or misspecification of the utility function. Next, I present two tests of the prediction that households with easier-to-predict income processes should display less departure from sufficiency of LIMU.

1.6.7 Testing hidden income: departure from sufficiency and predictive power of rainfall

If the primary barrier to insurance is the inability of the community to directly observe households' incomes, and this barrier is manifested through insufficiency of LIMU, households whose income processes are less uncertain, because they are predicted by observed factors, or are unconditionally less variable, should display less insufficiency of LIMU.

As a first test of this prediction, I regress income on the rainfall variables $R_{qvt} - \bar{R}_{qp}$ and $\left(R_{qvt} - \bar{R}_{qp}\right)^2$ separately for households in each of 10 occupational categories. The R^2 from this regression was interacted with lagged income. (The R^2s are shown in Table 10.) Table 9a shows the results of regressing the prediction errors (1.11) on lagged income, separately for the occupations with above- and below-median R^2s of income on the rainfall variables:

$$\hat{\varepsilon}_{it} = \alpha + \beta y_{i,t-1} + u_{it}$$

If insufficiency of LIMU is reduced when a household's income is easier to forecast, we should find $\alpha^{highR^2} > \alpha^{lowR^2}, \beta^{highR^2} > \beta^{low}$, and $\chi^2_{highR^2} < \chi^2_{lowR^2}$.In fact, this is the case: there is less insufficiency of LIMU (in the sense of a less significant correlation of the residuals with lagged income), when rainfall R^2 is high than when it is low.

As a second test, for each household, I calculate variance of income, after removing the component of income predicted by the rainfall variables and occupation-year dummies; i.e. that part which should be difficult to forecast. I split the sample according to whether this variance is above or below the median. The prediction of the hidden income model is that there should be less insufficiency of LIMU for the low-variance sample. Table 9b shows the results. Both in terms of the point estimates and the chi-squared test of joint significance, the high-variance sample displays greater insufficiency of LIMU: $\alpha^{high} < \alpha^{low}, \beta^{high} < \beta^{low}$, and $\chi^2_{high} > \chi^2_{low}$.

1.7 Conclusion

Knowing what barrier to full informal risk-sharing is most important in a given community is important for evaluation of policies that may affect the sustainability of informal insurance. One such group of policies is those that aim to increase individuals' access to savings, such as rural bank expansion, cell phone banking and microsavings accounts. Access to savings can crowd out limited commitment-constrained insurance if savings can be used after individuals renege on their informal insurance obligations (Ligon, Thomas, and Worrall 2000). On the other hand, savings access may crowd out insurance subject to hidden income if individuals' savings are not observable by the community, and the degree of crowdout will be complete if hidden savings offers the same rate of return as community-controlled savings (Cole and Kocherlakota 2001), (Doepke and Townsend 2006). Technologies that make observing others' incomes easier (such as crop price information dissemination) or harder (such as taking individual deposits rather than collecting savings at a group meeting; or access to larger, more anonymous markets) may affect informal insurance constrained by hidden income, but not if the only barrier to insurance is limited commitment or moral hazard.[21] Weather insurance which makes leaving community insurance more palatable will crowd out insurance under limited commitment (Attanasio and Rios-Rull 2000), but not under hidden income or moral hazard. Policies that expand communities' sanctioning ability (such as community-allocated aid; see Olken (2005)), or restrict it (such as road access; see Townsend 1995) will also affect limited commitment constraints, while community-allocated aid may reduce problems of hidden income, since the community knows the amount of aid each household is getting. Conditional cash transfer programs may also have differing effects on insurance constrained by limited commitment, moral hazard or hidden income.[22]

This paper suggested a set of tests that can be used to determine whether any of three models of endogenously incomplete insurance—limited commitment, moral hazard or hidden income—is consistent with the relationship between current consumption, lagged consumption and other lagged information. If information from "the past" helps to forecast current consumption, conditional on one lag of inverse marginal utility, neither limited commitment or moral hazard can fully explain incomplete insurance. However, if a household's past income helps to forecast current consumption, in the particular sense that prediction errors ignoring past income are positive when past income was low, this is consistent with a model in which households cannot directly observe one another's income and must be given incentives to truthfully report it.

Measurement error in right-hand side variables, which is commonly seen as a threat to power (causing underrejection of the null), is a particular concern with tests of this type, because mismeasurement of the proposed sufficient statistic (here, lagged inverse marginal utility) can distort

[21] Of course, a technology that made observing others' incomes harder could also *create* a hidden income problem where none had existed previously.

[22] Angelucci and De Giorgi (2009) discuss partial insurance of income transfers under Mexico's Progresa program.

the size of the test, causing *over*rejection of the null, if those variables which are excluded under the null hypothesis are correlated with the true value of the proposed sufficient statistic. This concern is addressed here with instrumental variables and by testing overidentifying restrictions on the reduced forms for the left- and right-hand-side variables.

Results from an 84-month (7-year) panel of households in rural Thailand are inconsistent with pure moral hazard or limited commitment, and suggest that hidden income plays a role in constraining households from achieving full risk sharing. This suggests that policies which make it easier (harder) for villagers to infer one another's incomes may improve (worsen) risk sharing. Changes that improve observability of income could include dissemination of crop or other price information; changes that worsen observability could include access to larger, anonymous markets; diversification of occupations within a village; electronic payments of remittances or for business transactions; seasonal migration; and private rather than group banking. Since policies that have the potential to worsen observability of income may also raise the average *level* of income, this is not to suggest that such policies be avoided. However, when possible they should be designed with consideration of the consequences for informal insurance.

1.A Appendix: Proofs

Define the N-dimensional vector of household incomes at t, $h_t = \{y_{it}\}_{i=1}^{N}$, and the history $(h_1, ..., h_t) \equiv h^t$.

1.A.1 Proof of Proposition 1: Full insurance rules out rank-reversals and dependence of consumption on income

Let λ_{it} be the multiplier on household i's time t promise-keeping constraint, and η_t be the multiplier on the village's time t budget constraint. Solving (1.1) subject to the promise-keeping constraints (1.2) and the village's budget constraint (1.3) yields the following first-order conditions for transfers, promised utility, and assets:

Proof. The FOCs are ■

$\tau_{it}(h^t)$:

$$\eta_t(h^t) = \lambda_{it} \Pr(h^t) v'(y_{it} + \tau_{it}(h^t)) \tag{1.24}$$

$u_{i,t+1}(h_t)$:

$$\Pr(h^t) \frac{\partial u_N(\mathbf{u}_{t+1}(h^t), a_{t+1}(h^t), \mathbf{e})}{\partial u_{i,t+1}(h^t)} = -\Pr(h^t)\lambda_{it}, \forall h^t, i < N \tag{1.25}$$

a_{t+1} :

$$\Pr(h^t) \frac{\partial u_N(\mathbf{u}_{t+1}(h^t), a_{t+1}(h^t), \mathbf{e})}{\partial a_{t+1}(h^t)} = \eta_t(h^t) \tag{1.26}$$

and the envelope conditions:

$$\frac{\partial u_N(\mathbf{u}_t(h^{t-1}), a_t(h^{t-1}), \mathbf{e}')}{\partial u_{it}(h^{t-1})} = -\lambda_{it}, \forall i < N \tag{1.27}$$

$$\frac{\partial u_N(\mathbf{u}_t(h^{t-1}), a_t(h^{t-1}), \mathbf{e}')}{\partial a_t(h^{t-1})} = \eta_{t-1}(h^{t-1}) \tag{1.28}$$

The FOCs for transfers for households i and N imply

$$\frac{\lambda_{Nt}}{\lambda_{it}} = \frac{v'(y_{it} + \tau_{it}(h^t))}{v'(y_{Nt} + \tau_{Nt}(h^t))}$$

So that

$$c_{it} \equiv y_{it} + \tau_{it}(h^t) = v'^{-1}\left(\frac{\lambda_{Nt}}{\lambda_{it}} v'(y_{Nt} + \tau_{Nt}(h^t))\right) \tag{1.29}$$

Substituting into the law of motion for assets,

$$R^{-1}a_{t+1} = a_t + \sum_{i=1}^{N} y_{it} - \sum_{i=1}^{N} v'^{-1}\left(\frac{\lambda_{Nt}}{\lambda_{it}} v'(y_{Nt} + \tau_{Nt}(h^t))\right) \tag{1.30}$$

which is a single equation in c_{Nt}, i.e. c_{Nt} depends only on the aggregate endowment, and not on h^t or $\{y_{it}\}$. Then (1.29) implies that for all households, c_{it} depends only on the aggregate endowment.

Using (1.25) and (1.27), $\lambda_{it} = \lambda_{it+1} = \lambda_i, \forall i, t$.

Further, for all i, j in the network:

$$\begin{aligned}\frac{v'(y_r + \tau_{Nrt})}{v'(y_r + \tau_{irt})} &= \lambda_i, \forall r, t, i < N \\ \frac{v'(y_r + \tau_{jrt})}{v'(y_r + \tau_{irt})} &= \frac{\lambda_j}{\lambda_i}\end{aligned}$$

So if in the first period, household i consumes more than household j, this will be the case in all subsequent periods, and vice versa. Therefore under full insurance the ordering of initial multipliers λ_{i0} or equivalently initial promises u_{i0} will determine the ordering of household i in the consumption distribution in all periods.■

1.A.2 Proof of Proposition 2: Under moral hazard, lagged inverse marginal utility is a sufficient statistic for current consumption

The proof proceeds in two steps. First, to show that the difference between the multipliers on the household's time t promise- and threat-keeping constraints equals expected time t inverse marginal utility. Second, that the expected difference between the multipliers on the household's time t promise- and threat-keeping constraints equals time t inverse marginal utility; the difference is a random walk (conditional on the time t budget multiplier, η_t).

Again let λ_{it} be the multiplier on household i's promise-keeping constraint, and η_t be the multiplier on the village's time t budget constraint. Let ζ_{it} be the multiplier on household i's incentive-compatibility constraint. (Since there are only two possible effort levels and utility is separable in consumption and effort, the incentive-compatibilty constraint will be binding at the optimum (Grossman and Hart 1983).)

The planner's problem is now

$$u_N(\mathbf{u}_t, \hat{\mathbf{u}}_t, a_t|\mathbf{e}') \equiv \max_{\{\tau_{rt}\},\{\mathbf{u}_{r,t+1}\},\{\hat{\mathbf{u}}_{r,t+1}\}}$$

$$\sum_{r=1}^{S} p_{r11} v(y_r + \tau_{Nrt}) - c(1) + \beta \mathbb{E}_{\{y\}} u_N(\mathbf{u}_{t+1}, \hat{\mathbf{u}}_{t+1}, a_{t+1}|\mathbf{e})$$

subject to the promise-keeping constraints:

$$\sum_{r=1}^{S} p_{r11}[v(y_r + \tau_{irt}) - c(1) + \beta u_{ir,t+1}] \geq u_{it}, i < N \qquad (\lambda_{it})$$

the law of motion for assets:

$$R^{-1} a_{t+1} = a_t - \sum_{i=1}^{N} \tau_{irt} \qquad (\eta_t)$$

the incentive-compatibility constraints:

$$\begin{aligned}&\sum_{r=1}^{S} p_{r11}[v\left(y_r+\tau_{irt}\right)+\beta u_{ir,t+1}]-c(1) \\ =\ &\sum_{r=1}^{S} p_{r10}[v\left(y_r+\tau_{irt}\right)+\beta \hat{u}_{ir,t+1}]-c(0)\end{aligned} \qquad (\zeta_{it})$$

threat-keeping 1: if the household disobeyed yesterday but obeys today, they don't get more than $\hat{u}_{it}$:

$$\sum_{r=1}^{S} p_{r10}[v(y_r+\tau_{irt})-c(1)+\beta u_{ir,t+1}] \leq \hat{u}_{it}, i<N \qquad (\psi_{1it})$$

threat-keeping 2: if the household disobeyed yesterday and disobeys today, they don't get more than $\hat{u}_{it}$:

$$\sum_{r=1}^{S} p_{r00}[v(y_r+\tau_{irt})-c(0)+\beta \hat{u}_{ir,t+1}] \leq \hat{u}_{it}, i<N \qquad (\psi_{2it})$$

The FOCs are:

τ_{irt} :

$$\frac{\eta_t p_{r11}^{-1}}{v'(y_r+\tau_{irt})}=\lambda_{it}+\frac{p_{r11}-p_{r01}}{p_{r11}}\zeta_{it}-\frac{p_{r10}}{p_{r11}}\psi_{1it}-\frac{p_{r00}}{p_{r11}}\psi_{2it}$$

$u_{ir,t+1}$:

$$-\mathbb{E}_{\{y_{-i}|y_i\}}\frac{\partial u_N(\cdot,\cdot,\cdot|\mathbf{e})}{\partial u_{ir,t+1}}=\lambda_{it}+\zeta_{it}-\frac{p_{r10}}{p_{r11}}\psi_{1it}$$

$\hat{u}_{ir,t+1}$:

$$-\mathbb{E}_{\{y_{-i}|y_i\}}\frac{\partial u_N(\cdot,\cdot,\cdot|\mathbf{e})}{\partial \hat{u}_{ir,t+1}}=-\frac{p_{r01}}{p_{r11}}\zeta_{it}-\frac{p_{r00}}{p_{r11}}\psi_{2it}$$

a_{t+1} :

$$\mathbb{E}_{\{y\}}\frac{\partial u_N(\cdot,\cdot,\cdot|\mathbf{e})}{\partial a_{t+1}}=\eta_t$$

and the envelope conditions:

$$\begin{aligned}-\frac{\partial u_{Nt}(\mathbf{u}_t,\hat{\mathbf{u}}_t,a_t|\mathbf{e}')}{\partial u_{irt}} &= \lambda_{it} \\ -\frac{\partial u_{Nt}(\mathbf{u}_t,\hat{\mathbf{u}}_t,a_t|\mathbf{e}')}{\partial \hat{u}_{irt}} &= \psi_{1it}+\psi_{2it} \\ \frac{\partial u_{Nt}(\mathbf{u}_t,\hat{\mathbf{u}}_t,a_t|\mathbf{e}')}{\partial a_t} &= \eta_t\end{aligned}$$

Multiplying the FOC for each τ_{irt} by p_{r11} and summing gives

$$\eta_t\mathbb{E}\left(\frac{1}{v'(y_r+\tau_{irt})}|\eta_t\right)=\lambda_{it}-(\psi_{1it}+\psi_{2it})$$

Expected inverse marginal utility at t equals the difference $\lambda_{it}-(\psi_{1it}+\psi_{2it})$ (Step 1)

Adding the FOCs for $u_{ir,t+1}$ and $\hat{u}_{ir,t+1}$ gives:

$$\mathbb{E}_{\{y_{-i}|y_i\}}\left(\frac{-\partial u_N(\mathbf{u}_{t+1},\hat{\mathbf{u}}_{t+1},a_{t+1}|\mathbf{e})}{\partial u_{ir,t+1}}-\frac{\partial u_N(\mathbf{u}_{t+1},\hat{\mathbf{u}}_{t+1},a_{t+1}|\mathbf{e})}{\partial \hat{u}_{ir,t+1}}\right)$$

$$=\underbrace{\lambda_{it}+\zeta_{it}-\frac{p_{r10}}{p_{r11}}\psi_{1it}}_{u_{ir,t+1}}+\underbrace{\left(-\frac{p_{r01}}{p_{r11}}\zeta_{it}-\frac{p_{r00}}{p_{r11}}\psi_{2it}\right)}_{\hat{u}_{ir,t+1}}$$

$$\begin{aligned} &= \lambda_{it}+\frac{p_{r11}-p_{r01}}{p_{r11}}\zeta_{it}-\frac{p_{r10}}{p_{r11}}\psi_{1it}-\frac{p_{r00}}{p_{r11}}\psi_{2it} \\ &= \frac{\eta_t}{v'(y_r+\tau_{irt})} \end{aligned}$$

Lagging this by one period,

$$\frac{\eta_{t-1}}{v'(y_{i,t-1}+\tau_{i,t-1})}=\mathbb{E}_{\{y\}}\frac{-\partial u_N(\mathbf{u}_t,\hat{\mathbf{u}}_t,a_t|\mathbf{e}')}{\partial u_{it}}-\frac{\partial u_N(\mathbf{u}_t,\hat{\mathbf{u}}_t,a_t|\mathbf{e}')}{\partial \hat{u}_{it}}$$

So that, using the time t envelope conditions for u_{it} and $\hat{u}_{it}$:

$$\frac{\eta_{t-1}}{v'(y_{i,t-1}+\tau_{i,t-1})}=\lambda_{it}-(\psi_{1it}+\psi_{2it})$$

Using Step 1, this implies

$$\frac{1}{v'(y_{i,t-1}+\tau_{i,t-1})}=\frac{\eta_t}{\eta_{t-1}}\mathbb{E}\left(\frac{1}{v'(y_{it}+\tau_{it})}|\eta_t\right)$$

Inverse marginal utility times the budget multiplier is a random walk (given the time t budget multiplier).

LIMU is a sufficient statistic for past information in forecasting consumption.■

1.A.3 Proof of Proposition 3: Lagged inverse marginal utility is a sufficient statistic under limited commitment

Let $\frac{\lambda_{it}}{\Pr(h^t)}$ be the multiplier on household i's promise-keeping constraint, and $\frac{\eta_t(h^t)}{\Pr(h^t)}$ be the multiplier on the village's time t budget constraint after history h^t. Using the stationarity of the problem,

$$\Pr(h^t|\mathbf{u}(h^{t-1}),a(h^{t-1}),\mathbf{e})=\Pr(h^t|h^{t-1})=\Pr(h_t)$$

so probabilities are written conditional only on the time t realization h_t. Let $\phi_{it}(h^t)$ be the multiplier on household i's participation constraint after history h^t.

Assume that there is at least one realization h_t such that no household's participation constraint is binding: this guarantees differentiability of the planner's value function (Koeppl 2006). Solving (1.1) subject to the promise-keeping constraints (1.2), the participation constraints (1.5) and the village's budget constraint (1.3) yields the following first-order conditions for transfers, promised utility, and assets:

$\tau_{it}(h^t)$:

$$\eta_t(h^t)=(\lambda_{it}+\phi_{it}(h^t))v'(y_{it}+\tau_{it}(h^t)) \tag{1.31}$$

$u_{i,t+1}(h^t)$:

$$\Pr(h^t)\frac{\partial u_N(\mathbf{u}_{t+1}(h^t), a_{t+1}(h^t), \mathbf{e})}{\partial u_{i,t+1}(h^t)} = -\Pr(h^t)\lambda_{it} - \phi_{it}(h^t), \forall h^t, i < N \tag{1.32}$$

$a_{t+1}(h^t)$:

$$\Pr(h^t)\frac{\partial u_N(\mathbf{u}_{t+1}(h^t), a_{t+1}(h^t), \mathbf{e})}{\partial a_{t+1}(h^t)} = \eta_t(h^t) \tag{1.33}$$

and the envelope conditions for current promises (1.27) and assets (1.28):

$$\frac{\partial u_N(\mathbf{u}_t(h^{t-1}), a_t(h^{t-1}), \mathbf{e}')}{\partial u_{it}(h^{t-1})} = -\lambda_{it}, \forall i < N$$

$$\frac{\partial u_N(\mathbf{u}_t(h^{t-1}), a_t(h^{t-1}), \mathbf{e}')}{\partial a_t(h^{t-1})} = \eta_{t-1}(h^{t-1})$$

It will be helpful to use the following result:

Lemma 6 *The double (y_{it}, η_t) is a sufficient statistic for the N-vector of income realizations h^t in determining household i's transfer: $\tau_{it}(h^t) = \tau_{it}(y_{it}, \eta_t)$*

Proof. Note that, when $\phi_{it}(h^t) > 0$, i.e. household i's participation constraint is binding, (1.31) and (1.27) imply that the household's transfer and future promise are set to make the household exactly indifferent between staying in the network or defaulting, and to equate the cost of providing the current transfer τ and future promise u, irrespective of the income realizations of other households in the network:

$$v(y_r + \tau_{it}(h^t)) + \beta u_{i,t+1}(h^t) = u^t_{aut}(y_r)$$

$$v'(y_r + \tau_{it}(h^t)) = -\left(\frac{\partial u_N(\mathbf{u}_{t+1}(h^t), a_{t+1}(h^t), \mathbf{e})}{\partial u_{i,t+1}(h^t)}\right)^{-1}$$

so $\tau_{it}(h^t|\phi_{it}(h^t) > 0) = \tau_{it}(y_{it}, \eta_t)$. And, when $\phi_{it}(h^t) = 0$, i.e. household i's participation constraint is not binding, (1.31) and (1.27) imply that $\frac{1}{v'(y_{it}+\tau_{it}(h^t))} = \frac{\eta_t(h^t)}{\lambda_{it}}$, so, again, $\tau_{it}(h^t|\phi_{it}(h^t) = 0) = \tau_{it}(y_{it}, \eta_t)$. ■

This lemma allows us to write $\tau_{it}(y_{it}, \eta_t)$ for $\tau_{it}(h^t)$. Using the FOCs for $\tau_{it}(y_{it}, \eta_t)$ and $u_{i,t+1}(h^t)$:

$$\begin{aligned}\eta_t(h^t) &= \Pr(h_t)\frac{\partial u_N(\mathbf{u}_{t+1}(h^t), a_{t+1}(h^t), \mathbf{e})}{\partial u_{i,t+1}(h^t)}v'(y_{it} + \tau_{it}(y_{it}, \eta_t)) \\ &= \Pr(y_{it}, \eta_t)v'(y_{it} + \tau_{it}(y_{it}, \eta_t))\Pr(h_t|y_{it}, \eta_t)\frac{\partial u_N(\mathbf{u}_{t+1}(h^t), a_{t+1}(h^t), \mathbf{e})}{\partial u_{i,t+1}(h^t)}\end{aligned}$$

since $\Pr(y_{it}, \eta_t)\Pr(h_t|y_{it}, \eta_t) = \Pr(h_t \cap (y_{it}, \eta_t)) = \Pr(h_t \cap (\eta_t(h^t)))$. This says that inverse marginal utility, weighted by the shadow price of resources scaled by the probability of (y_{it}, η_t), is equal to the gradient of the planner's value function with respect to household i's time $t+1$ promised utility weighted by the probability of the N-vector of income realizations h_t, given (y_{it}, η_t):

$$\frac{\eta_t(h^t)}{\Pr(y_{it}, \eta_t)v'(y_{it} + \tau_{it}(y_{it}, \eta_t))} = \Pr(h_t|y_{it}, \eta_t)\frac{\partial u_N(\mathbf{u}_{t+1}(h^t), a_{t+1}(h^t), \mathbf{e})}{\partial u_{i,t+1}(h^t)} \tag{1.34}$$

Note that

$$\sum_{h_t|\Pr(h_t|y_{it},\eta_t)>0}\left(\frac{\eta_t(h^t)}{\Pr(y_{it},\eta_t)v'(y_{it}+\tau_{it}(y_{it},\eta_t))}\right)=\frac{\Pr(y_{it},\eta_t)^{-1}}{v'(y_{it}+\tau_{it}(y_{it},\eta_t))}\sum_{h_t|\Pr(h_t|y_{it},\eta_t)>0}\eta_t(h^t)$$

since the term $\frac{\Pr(y_{it},\eta_t)^{-1}}{v'(y_{it}+\tau_{it}(y_{it},\eta_t))}$ does not depend on h^t: $\Pr(y_{it},\eta_t)$ is the unconditional probability that (y_{it},η_t) occurs.

Summing (1.34) over all time t realizations h_t such that $\Pr(h_t|y_{it},\eta_t)>0$ gives

$$\begin{aligned}
&\frac{\Pr(y_{it},\eta_t)^{-1}}{v'(y_{it}+\tau_{it}(y_{it},\eta_t))}\sum_{h_t|\Pr(h_t|y_{it},\eta_t)>0}\eta_t(h^t)\\
&= \sum_{h_t|\Pr(h_t|y_{it},\eta_t)>0}\Pr(h_t|y_{it},\eta_t)\frac{\partial u_N(\mathbf{u}_{t+1}(h^t),a_{t+1}(h^t),\mathbf{e})}{\partial u_{i,t+1}(h^t)}\\
&= \mathbb{E}\left(\frac{\partial u_N(\mathbf{u}_{t+1}(h^t),a_{t+1}(h^t),\mathbf{e})}{\partial u_{i,t+1}(h^t)}\middle| y_{it},\eta_t\right)
\end{aligned}$$

So that

$$\frac{1}{v'(y_{it}+\tau_{it}(y_{it},\eta_t))}\sum_{h_t|\Pr(h_t|y_{it},\eta_t)>0}\eta_t(h^t)=\Pr(y_{it},\eta_t)\mathbb{E}\left(\frac{\partial u_N(\mathbf{u}_{t+1}(h^t),a_{t+1}(h^t),\mathbf{e})}{\partial u_{i,t+1}(h^t)}\middle| y_{it},\eta_t\right)$$

Summing over all realizations of (y_{it},η_t) gives

$$\sum_{y_{it},\eta_t}\frac{1}{v'(y_{it}+\tau_{it}(y_{it},\eta_t))}\sum_{h_t|\Pr(h_t|y_{it},\eta_t)>0}\eta_t(h^t)=\mathbb{E}\left(\frac{\partial u_N(\mathbf{u}_{t+1}(h^t),a_{t+1}(h^t),\mathbf{e})}{\partial u_{i,t+1}(h^t)}\right)$$

or

$$\sum_{h_t}\left(\frac{\eta_t(h^t)}{v'(y_{it}+\tau_{it}(y_{it},\eta_t))}\right)=\mathbb{E}\left(\frac{\partial u_N(\mathbf{u}_{t+1}(h^t),a_{t+1}(h^t),\mathbf{e})}{\partial u_{i,t+1}(h^t)}\right)$$

So, using the time $t+1$ envelope condition for $u_{i,t+1}(h^t)$, (1.27):

$$\frac{\eta_t(h^t)}{v'(y_{it}+\tau_{it}(h^t))}=\Pr(h_t)\frac{\partial u_N(\mathbf{u}_{t+1}(h^t),a_{t+1}(h^t),\mathbf{e})}{\partial u_{i,t+1}(h^t)}=\mathbb{E}_{h^{t+1}}\left(\lambda_{i,t+1}|h^t\right)$$

lagging by one period and using the FOC for $\tau_{it}(h^t)=\tau_{it}(y_{it},\eta_t)$,

$$\mathbb{E}_{h^t}\left(\frac{\lambda_{it}}{\eta_t(h^t)}|h^{t-1},\eta_t(h^t)\right)=\frac{1}{v'(c_{i,t-1}(h^{t-1}))}=\frac{1}{\eta_t(h^t)}\left(\lambda_{i,t-1}+\frac{\phi_{ir,t-1}(y_{i,t-1})}{\Pr(h^t|h^{t-1})}\right).$$

Starting from the multiplier on the initial promise-keeping constraint, λ_{i0},

$$\begin{aligned}
&\mathbb{E}_{h^t}\left(\frac{1}{v'(c_{it}(h^t))}|\eta_t(h^t)\right)\\
&= \frac{\lambda_{i,t-1}(h^{t-1})}{\eta_t(h^t)}=\lambda_{i0}+\sum_{q=1}^{t-1}\frac{\phi_{ir,t-q}(y_{i,t-q})}{p(y_q)\eta_q}
\end{aligned}$$

Lagged inverse marginal utility, conditional on the current shadow price of resources $\eta_t(h^t)$, captures all past information relevant to forecasting current marginal utility of consumption.■

1.A.4 Proof of proposition 4: With hidden income, lagged inverse marginal utility over-predicts consumption for low-lagged income households

Let λ_{it} be the multiplier on household i's promise-keeping constraint, η_t the multiplier on the budget constraint, and ζ_{irt} the multiplier on the truth-telling constraint when $y_t = y_r$. The FOCs are:

τ_{irt} :

$$\eta_t = \left(p_{ree'}\lambda_t + \xi_{irt}\right) v'(y_r + \tau_{rt}) - \xi_{i,r+1,t}v'(y_{r+1} + \tau_{irt})$$

$u_{ir,t+1}$:

$$p_{ree'}\mathbb{E}_{\{y_{-i}|y_i\}}\frac{-\partial u_N(\mathbf{u}_{t+1}, a_{t+1}, \mathbf{e})}{\partial u_{ir,t+1}} = p_{ree'}\lambda_t + \xi_{irt} - \xi_{i,r+1,t}$$

a_{t+1} :

$$-\mathbb{E}_{\{y\}}\frac{\partial u_N(\mathbf{u}_{t+1}, a_{t+1}, \mathbf{e})}{\partial a_{t+1}} = -\eta_t$$

envelope conditions:

$$\begin{aligned} \frac{\partial u_N(\mathbf{u}_t, a_t, \mathbf{e}')}{\partial u_{it}} &= -\lambda_{it} \\ \frac{\partial u_N(\mathbf{u}_t, a_t, \mathbf{e}')}{\partial a_t} &= \eta_t \end{aligned}$$

The lagged promise-keeping multiplier, λ_{t-1}, is a sufficient statistic for history, since the FOC for $u_{ir,t+1}$ and the envelope condition for u_{it} imply

$$\mathbb{E}\left(\lambda_{i,t+1}|\eta_{t+1}\right) = \lambda_{it} + \frac{\xi_{irt} - \xi_{i,r+1,t}}{p(y_t)}$$

lagging one period,

$$\mathbb{E}\left(\lambda_{it}|\eta_t\right) = \lambda_{i,t-1} + \frac{\xi_{ir,t-1} - \xi_{i,r+1,t-1}}{p(y_{t-1})}.$$

The FOC for transfers at $t-1$ implies that

$$\lambda_{i,t-1} = \frac{1}{v'(y_r + \tau_{ir,t-1})} \times \left(1 - \frac{\xi_{ir,t-1}v'(y_r + \tau_{irt}) - \xi_{i,r+1,t-1}v'(y_{r+1} + \tau_{irt})}{\eta_{t-1}p(y_{t-1})}\right) \tag{1.35}$$

Since $\mathbb{E}(\lambda_{it}|\eta_t) = \lambda_{i,t-1}$,

$$\mathbb{E}(\lambda_{it}|\eta_t) = \frac{1}{v'(y_r + \tau_{r,t-1})} \times \left(1 - \frac{\xi_{ir,t-1}v'(y_r + \tau_{rt}) - \xi_{i,r+1,t-1}v'(y_{r+1} + \tau_{rt})}{\eta_{t-1}p(y_{t-1})}\right)$$

Using the envelope condition for u_{it}, the time $t-1$ FOC for u_{it} can be written

$$\frac{\partial u_N(\mathbf{u}_t, a_t, \mathbf{e})}{\partial u_{it}} - \frac{\partial u_N(\mathbf{u}_{t-1}, a_{t-1}, \mathbf{e})}{\partial u_{i,t-1}} = \frac{\xi_{i,r,t-1} - \xi_{i,r+1,t-1}}{p_{ree'}}$$

First, assume no aggregate uncertainty: $a_t = a_{t-1}$
Since $u_N(\mathbf{u}_t, a_t, \mathbf{e})$ is concave in each u_{it}, when a household's promise decreases ($u_{it} < u_{i,t-1}$), then

$$\frac{\partial u_N(\mathbf{u}_t, a_t, \mathbf{e})}{\partial u_{it}} > \frac{\partial u_N(\mathbf{u}_{t-1}, a_t, \mathbf{e})}{\partial u_{i,t-1}},$$

so $\xi_{ir,t-1} > \xi_{i,r+1,t-1}$: truth-telling constraints bind more at lower than higher output levels. Then, since $v'(y_r + \tau_{rt}) > v'(y_{r+1} + \tau_{rt})$,

$$\xi_{ir,t-1} v'(y_{ir} + \tau_{ir,t-1}) > \xi_{i,r+1,t-1} v'(y_{ir+1} + \tau_{ir,t-1})$$

so

$$\mathbb{E}(\lambda_{it} | h^{t-1}) < \frac{1}{v'(y_{ir} + \tau_{ir,t-1})}$$

LIMU over-predicts λ_{it} when the household's promise decreased between $t-1$ and t. Promises are unobserved, but truth-telling implies that promises are an increasing function of income, so low-y_{t-1} households will get less consumption at t than predicted using lagged inverse marginal utility.

However, if $a_t > a_{t-1}$, there is an offsetting effect:

$$\begin{aligned}
\frac{\partial^2 u_N(\mathbf{u}_t, a_t, \mathbf{e})}{\partial u_{it} \partial a_t} &\neq 0 \Rightarrow \\
\frac{\partial u_N(\mathbf{u}_t, a_t, \mathbf{e})}{\partial u_{it}} &\neq \frac{\partial u_N(\mathbf{u}_t, a_{t-1}, \mathbf{e})}{\partial u_{i,t-1}}
\end{aligned}$$

However, we can sign this effect: by the envelope condition for u_{it}:

$$\frac{\partial u_N(\mathbf{u}_t, a_t, \mathbf{e})}{\partial u_{it}} = -\lambda_{it}$$

So

$$\begin{aligned}
\frac{\partial^2 u_N(\mathbf{u}_t, a_t, \mathbf{e})}{\partial u_{it} \partial a_t} &= -\frac{\partial \lambda_{it}}{\partial a_t} \\
sgn\left(-\frac{\partial \lambda_{it}}{\partial a_t}\right) &= sgn\left(\frac{\partial \lambda_{it}}{\partial \eta_t}\right)
\end{aligned}$$

Using the formula for λ_{it}:

$$\begin{aligned}
\frac{\partial \lambda_{it}}{\partial \eta_t} &= \frac{1}{v'(y_r + \tau_{irt})} \times \\
&\frac{\partial}{\partial \eta_t}\left(1 - \frac{\xi_{irt} v'(y_r + \tau_{irt}) - \xi_{i,r+1,t} v'(y_{r+1} + \tau_{irt})}{\eta_t p(y_r)}\right)
\end{aligned}$$

$$sgn\left(\frac{\partial \lambda_{it}}{\partial \eta_t}\right) = sgn\left(\xi_{irt} v'(y_r + \tau_{irt}) - \xi_{i,r+1,t} v'(y_{r+1} + \tau_{irt})\right)$$

That is, when $u_{it} < u_{i,t-1}$,

$$\frac{\partial^2 u_N(\mathbf{u}_t, a_t, \mathbf{e})}{\partial u_{it} \partial a_t} > 0$$

so the extent of "overprediction at the bottom" is reduced the greater is $\Delta a_t \equiv a_t - a_{t-1}$.■

1.A.5 Proof of proposition 5: Less variable income processes display a reduced wedge between LIMU and current inverse marginal utility:

Using (1.35):

$$\mathbb{E}(\lambda_{it}|\eta_t) = \frac{1}{v'(y_q + \tau_{iq,t-1})} \times \\ \left(1 - \frac{\xi_{iqt-1} v'(y_q + \tau_{iqt-1}) - \xi_{iq+1t-1} v'(y_{q+1} + \tau_{iqt+1})}{\eta_{t-1} p_{qee'}}\right)$$

Define

$$\theta(y_q) \equiv 1 - \frac{\xi_{iqt-1} v'(y_q + \tau_{iqt-1}) - \xi_{iq+1t-1} v'(y_{q+1} + \tau_{iqt+1})}{\eta_{t-1} p_{qee'}}.$$

$\theta(y_q)$ measures the "wedge" between λ_{it} and $\frac{1}{v'(y_q+\tau_{iq,t-1})}$. Take the expectation of $\theta(y_q)$, given that y_q was below the average level of income $\bar{y}$:

$$\mathbb{E}[\theta(y_q)|y_q < \bar{y}] = \\ \sum_{q:y_q<\bar{y}} p_{qee'} \left[1 - \frac{\xi_{iqt-1} v'(y_q + \tau_{iqt-1}) - \xi_{iq+1t-1} v'(y_{q+1} + \tau_{iqt+1})}{\eta_{t-1} p_{qee'}}\right]$$

Fixing the probability of each income realization, $p_{qee'}$, a SOSD reduction in variability will reduce

$$\mathbb{E}\left[v'(y_q + \tau_{iq,t-1}) - v'(y_{q+1} + \tau_{iq,t+1})\right]$$

since income levels are closer together (note these differences remain negative since $y_q < y_{q+1}$), and will reduce

$$\mathbb{E}\left|\xi_{ir,t-1} - \xi_{i,r+1,t-1}\right|$$

since

$$\frac{\partial u_N(\mathbf{u}_t, a_t, \mathbf{e})}{\partial u_{it}} - \frac{\partial u_N(\mathbf{u}_{t-1}, a_{t-1}, \mathbf{e})}{\partial u_{i,t-1}} = \frac{\xi_{i,r,t-1} - \xi_{i,r+1,t-1}}{p_{ree'}}$$

and a reduction in the amount of uncertainty about the household's income moves u_{it} and $u_{i,t-1}$closer together, on average (insurance improves). By the concavity of the planner's value function, this in turn reduces the gap $\frac{\partial u_N(\mathbf{u}_t,a_t,\mathbf{e})}{\partial u_{it}} - \frac{\partial u_N(\mathbf{u}_{t-1},a_{t-1},\mathbf{e})}{\partial u_{i,t-1}}$ (which remains negative since the household's promise is falling).

Therefore, $\mathbb{E}[\theta(y_q)|y_q < \bar{y}] \to 1$ as the variability of y decreases, so that the amount of additional information contained in y_{t-1} falls.■

1.A Appendix: Tables

Table 1: Summary statistics

Panel A: Income, demographics and occupation				
		531-HH panel mean	Non-continuously observed HH difference	N
Income				
	Monthly income	8981.224	-2624.627	670
	Monthly expenditure	5213.472	-1108.721***	670
	Monthly income, resids	32.443	-163.756	670
	Monthly expenditure, resids	67.416	-570.84	670
Household	composition			
	Household size	4.525	-0.663***	669
	Adult equivalents	3.786	-0.638***	669
	Adult men	1.382	-0.324***	669
	Adult women	1.552	-0.247***	669
Occupation	(household head, baseline)			
	Rice farmer	0.355	0.116*	667
	Non-ag labor	0.119	0.033	667
	Corn farmer	0.098	-0.062*	667
	Livestock farmer	0.089	-0.082***	667
	Ag wage labor	0.051	0.007	667
	Other crop farmer	0.043	-0.036*	667
	Shrimp/fish farmer	0.036	-0.021	667
	Orchard farmer	0.017	0.005	667
	Construction	0.015	0.036*	667
	Other	0.074	0.013	667

Notes: All baht-denominated variables were converted to 2002 baht using the Thai Ministry of Trade's Rural Consumer Price Index for Thailand. In 2002, approximately 42 Thai baht were equal to US$1. Income and expenditure residuals are residuals from regression on village, year, occupation and demographic variables.

Table 1: Summary statistics

Panel B: Gifts				
		531-HH panel mean	Non-continuously observed HH difference	N
Gifts given				
	Gifts to orgs in village	33.714	-9.813	670
	Gifts to orgs not in village	53.749	-29.063**	670
	Gifts given for events in village	103.219	-35.550***	670
	Gifts given for events not in village	220.117	-140.576***	670
	Other gifts to HHs in village	147.317	-29.854	670
	Other gifts to HHs not in village	637.198	-96.868	670
Gifts received				
	Gifts from orgs in village	36.105	-20.002**	670
	Gifts from orgs not in village	38.963	10.82	670
	Gifts rec'd for events in village	316.862	-213.653***	670
	Gifts rec'd for events not in village	80.068	9.976	670
	Other gifts from HHs in village	118.129	-20.575	670
	Other gifts from HHs not in village	1327.131	-253.376	670

Notes: All baht-denominated variables were converted to 2002 baht using the Thai Ministry of Trade's Rural Consumer Price Index for Thailand. In 2002, approximately 42 Thai baht were equal to US$1. Income and expenditure resids are residuals from regression on village, year, occupation and demographic variables.

Table 2: Consumption smoothing at the individual and village level

	log household PCE	log household PCE OLS	log avg household PCE	log household PCE	log household PCE IV	log avg household PCE
	(1)	(2)	(3)	(4)	(5)	(6)
log household income	.0778***	.0669***		.2113***	.1737***	
	[.0074]	[.0073]		[.0394]	[.0444]	
avg log household income			.1722***			.3002***
			[.0499]			[.1164]
Village-year fixed effect?	No	Yes	No	No	Yes	No
Village-year F statistic	-	5.256	-	-	3.471	-
P value	-	0.0000	-	-	0.0000	-
Observations	3323	3323	112	1879	1879	64
R-squared	0.0318	0.1807	0.8763			

Notes: Household-level variables in columns (1), (2), (4) and (5) are deviations from individual means. Standard errors in brackets. All variables are in 2002 Thai baht. F-statistic tests the joint significance of the village-year effects. In columns (4) and (5) income is instrumented with quarterly rainfall deviations from average province-level quarterly rainfall, and deviations, and deviations and squared deviations interacted with 11 occupation dummies. In column (6) income is instrumented with quarterly rainfall deviations and squared deviations. Rainfall data is available for 1999-2003. *p<.1, ** p<.05, *** p<.01

Table 3: Movement in the consumption distribution

Panel A: Correlations in per capita expenditure rank over time

Rank in village PCE distribution

	2005	2004	2003	2002	2001	2000	1999
2005	1.000						
2004	0.643	1.000					
2003	0.645	0.658	1.000				
2002	0.565	0.681	0.680	1.000			
2001	0.453	0.549	0.591	0.589	1.000		
2000	0.354	0.409	0.436	0.437	0.539	1.000	
1999	0.375	0.442	0.466	0.459	0.525	0.824	1.000

Notes: PCE is household expenditure divided by adult equivalents.

Table 3: Movement in the consumption distribution

Panel B: Changes in PCE rank vs. changes in income

	OLS (LHS var: change in PCE rank)	Ordered probit* (LHS var: direction of change)
Change in ln(income)	.527	.0586
	[.1414]	[.0089]
	3.73	*6.56*
R-squared	0.0052	
N	2674	2674

Notes: Standard errors in brackets, t-statistics in italics.
*Marginal effect on probability of positive change in income rank, evaluated at mean income.

Table 4: Testing sufficiency of lagged inverse marginal utility

	Full sample		Drop top and bottom 5% of PCE	
	(1)	(2)	(3)	(4)
ln(LIMU)	.7386***	.7126***	.6215***	.5952***
	[.0208]	[.023]	[.0212]	[.0233]
Lagged log income		.0424***		.0378***
		[.007]		[.0068]
Village-year fixed efftects?	Yes	Yes	Yes	Yes
R-squared	0.6645	0.6687	0.6200	0.6299
Observations	3186	2845	2874	2573

Notes: Robust standard errors in brackets. Ln(LIMU) is proportional to $ln(c_{t-1})$. LIMU is lagged inverse marginal utility.

Table 5: Testing Amnesia

	Full Sample		Drop top and bottom 5% of PCE		Low rainfall variance	High rainfall variance
	(1)	(2)	(3)	(4)	(5)	(6)
ln(LIMU)	0.846***	0.756***	0.790***	0.714***	0.949***	0.933***
	[0.011]	[0.019]	[0.014]	[0.021]	[0.027]	[0.024]
ln(LIMU)X25	0.041***	0.134***	0.038***	0.120***		
	[0.001]	[0.015]	[0.001]	[0.016]		
ln(LIMU)X50	0.059***	0.152***	0.054***	0.139***		
	[0.001]	[0.015]	[0.001]	[0.015]		
ln(LIMU)X75	0.099***	0.201***	0.088***	0.166***		
	[0.002]	[0.022]	[0.002]	[0.020]		
ln(income)		0.093***		0.083***	0.030*	-0.004
		[0.013]		[0.013]	[0.012]	[0.012]
ln(income)X25		-0.084***		-0.074***		
		[0.013]		[0.013]		
ln(income)X50		-0.085***		-0.076***		
		[0.013]		[0.013]		
ln(income)X75		-0.092***		-0.071***		
		[0.018]		[0.017]		
ln(LIMU)+ln(LIMU)X75		0.957		0.880		
F-statistic		3576.2		2807.3		
p-value		0.000		0.000		
Chi-squared (High=Low)					0.20	
p-value					(0.658)	
Fixed effects	Village	Village	Village	Village	Village	Village
Sample	Full	Full	Middle 90% by PCE	Middle 90% by PCE	HHs w/ above median growth in PCE, low var. villages	HHs w/ above median growth in PCE, high var. villages
R-squared	0.85	0.86	0.82	0.83	0.70	0.74
N	3186	2860	2874	2589	665	811

Note: High-rainfall variance villages are those with above-median standard deviation of annual rainfall. Robust standard errors in brackets (clustered at the household level). Ln(LIMU) is proportional to $ln(c_{t-1})$. LIMU is lagged inverse marginal utility. *p<.1, ** p<.05, *** p<.01

Table 6: Testing the hidden income model (CRRA utility)

LHS=Prediction residuals from a regression of $ln(c_t)$ on $ln(c_{t-1})$ and a village-year effect.	OLS		IV	
	(1)	(2)	(3)	(4)
Constant (α)	-.5406	-.4839	-.2301	-.2123
	[.0691]	[.0694]	[.0668]	[.0576]
Lagged log income (β)	.0509	.0453	.0224	.0205
	[.0061]	[.0063]	[.0059]	[.0052]
Control for aggregate shock interactions?	Yes	No	Yes	No
Chi-square stat (α<0, β>0)	81.47	54.84	19.11	19.40
p value	(0.000)	(0.000)	(0.000)	(0.000)
Observations	2781	2781	2322	2322

Notes: Bootstrapped standard errors in brackets. All regressions include a village-year fixed effect. Chi-square stat is the statistic for the test that the slope>0, intercept<0. P-value in parentheses.

Table 7: Test overidentifying restrictions on reduced form for consumption

	(1) $ln(c_t)$	(2) $ln(c_{t-1})$	(1)/(2)	(3) $ln(c_t)$	(4) $ln(c_{t-1})$	(3)/(4)
$Cultivation_{t-1}$	0.1033	0.0656	1.575	0.1029	0.0652	1.578
	[0.0235]	[0.0203]		[0.0236]	[0.0204]	
$Cultivation_{t-2}$	0.0112	0.047		0.0135	0.0498	
	[0.0207]	[0.0164]		[0.0204]	[0.0166]	
$Cultivation_{t-3}$	-0.0283	-0.0295		-0.0376	-0.0396	
	[0.0318]	[0.0326]		[0.0325]	[0.0334]	
$Livestock_{t-1}$	0.0141	0.0223	0.632			
	[0.0147]	[0.0120]				
$Livestock_{t-2}$	0.0104	0.0085				
	[0.0057]	[0.0073]				
$Livestock_{t-3}$	0.0039	0.002				
	[0.0105]	[0.0092]				
$Fish_{t-1}$				0.0396	0.0516	0.767
				[0.0166]	[0.0142]	
$Fish_{t-2}$				0.0121	0.0077	
				[0.0083]	[0.0091]	
$Fish_{t-3}$				0.012	0.0129	
				[0.0094]	[0.0092]	
Rank in 1999	0.027	0.0273		0.0274	0.0278	
	[0.0027]	[0.0027]		[0.0027]	[0.0026]	
Constant	9.057	8.9959		9.0472	8.9854	
	[0.0500]	[0.0487]		[0.0496]	[0.0483]	
N	2124	2124		2124	2124	
Chi-sqared statistic (p-value) on ratios of $t-1$ coefficients equal	4.1286	(0.0422)		3.7292	(0.0535)	

Notes: Standard errors clustered at the household level in brackets. Coefficients and standard errors on income variables (in levels) are multiplied by 100,000. "Cultivation" is income from growing crops (rice, corn, orchard crops, etc.). "Livestock" is income from raising cows, pigs, ducks, etc.
"Fish" is income from raising fish and shrimp. "Rank in 1999" is the household's rank in the 1999 distribution of per capita consumption.

Table 8: Testing the hidden income model, nonparametric $u()$

Panel A: LHS=Prediction residuals from a regression of $ln(c_t)$ on $f(c_{t-1})$ and a village-year effect.		
	OLS	IV
	(1)	(2)
Constant (α)	-0.370	-0.141
	[0.0643]	[0.0668]
Lagged log income (β)	0.034	0.014
	[0.0059]	[0.0060]
Control for aggregate shock interactions?	Yes	Yes
Chi-square stat (α<0, β>0)	33.86	7.30
p value	(0.000)	(0.026)
Observations	2781	2322

Panel B: LHS=ln(c_t)		
	OLS	IV
LIMU ($f\ (c_{t-1})$)	0.906***	1.140***
	[0.0178]	[0.0286]
Lagged log income	0.0446***	0.0209**
	[0.0066]	[0.0079]
Village-year effect?	Yes	Yes
N	2781	2322

Notes: In Panel A, standard errors bootstrapped (50 replications) to account for the generated regressor. LHS variable is prediction residuals from OLS or IV regression of $ln(c_t)$ on $f(c_{t-1})$ and a village-year effect. Column (1) uses the nonparametric spline estimate of $f(c_{t-1})$ as an explanatory variable to form the predicted value of $ln(c_t)$; column (2) instruments this nonparametric estimate with its lag, $f(c_{t-2})$. Chi-square stat is the statistic for the test that the slope>0, intercept<0. p-values in parentheses.

Table 9a: Testing the hidden income model:
Split by predictive power of rainfall

LHS=Prediction residuals from a regression of $ln(c_t)$ on $ln(c_{t-1})$ and a village-year effect.

	High rainfall R^2	Low rainfall R^2
	(1)	(2)
Constant (α)	-0.421	-0.621
	[0.088]	[0.090]
Lagged log income (β)	0.047	0.056
	[0.008]	[0.008]
Control for aggregate shock interactions?	Yes	Yes
Chi-square stat (α<0, β>0)	28.581	54.156
p value	(0.000)	(0.000)
Observations	1173	1326

Notes: Bootstrapped standard errors in brackets. Chi-square stat is the statistic for the test that the slope>0, intercept<0. p-value in parentheses.

Table 9b: Testing the hidden income model:
Split by variance of income

LHS=Prediction residuals from a regression of $ln(c_t)$ on $ln(c_{t-1})$ and a village-year effect.

	High variance	Low variance
	(1)	(2)
Constant (α)	-0.49	-0.406
	[0.087]	[0.089]
Lagged log income (β)	0.047	0.037
	[0.008]	[0.008]
Control for aggregate shock interactions?	Yes	Yes
Chi-square stat (α<0, β>0)	56.96	22.03
p value	(0.000)	(0.000)
Observations	1387	1394

Notes: Bootstrapped standard errors in brackets. Chi-square stat is the statistic for the test that the slope>0, intercept<0. p-value in parentheses.

Table 10: Predicting income with rainfall

Occupation	R^2	N
Rice farmer	0.386	752
Construction	0.292	32
Orchard farmer	0.222	36
Shrimp/fish farmer	0.195	76
Agricultural wage labor	0.143	108
Livestock	0.142	188
Other crop farmer	0.120	92
Non-agricultural wage labor	0.116	252
Other	0.100	156
Corn farmer	0.088	208

Notes: R^2 is the R-squared of annual income on quarterly income deviations and squared deviations, plus province-fixed effects. N is the number of household-year observations.

1.A Appendix: Figures

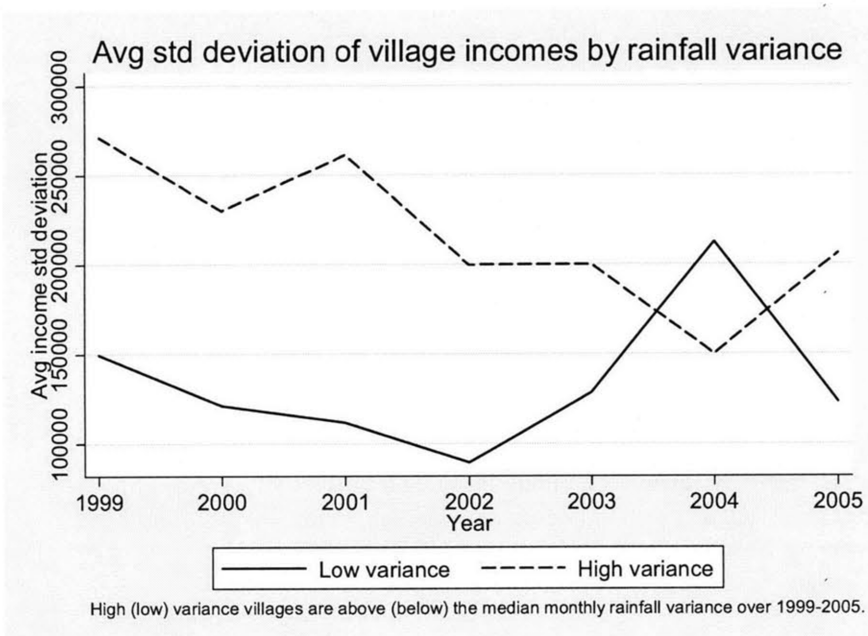


Figure 1: Standard deviation of incomes by rainfall variance

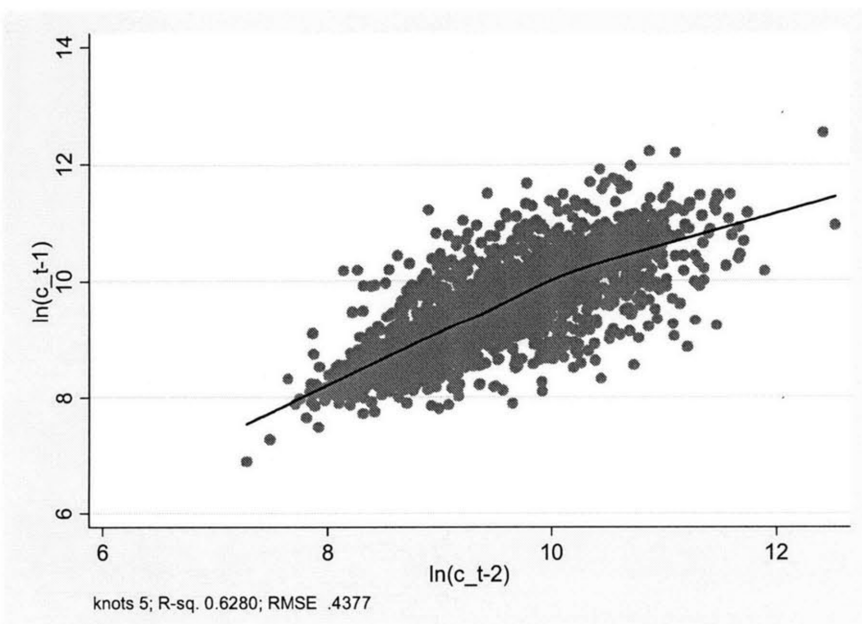


Figure 2: Spline regression of $\ln(c_{t-1})$ on $\ln(c_{t-2})$

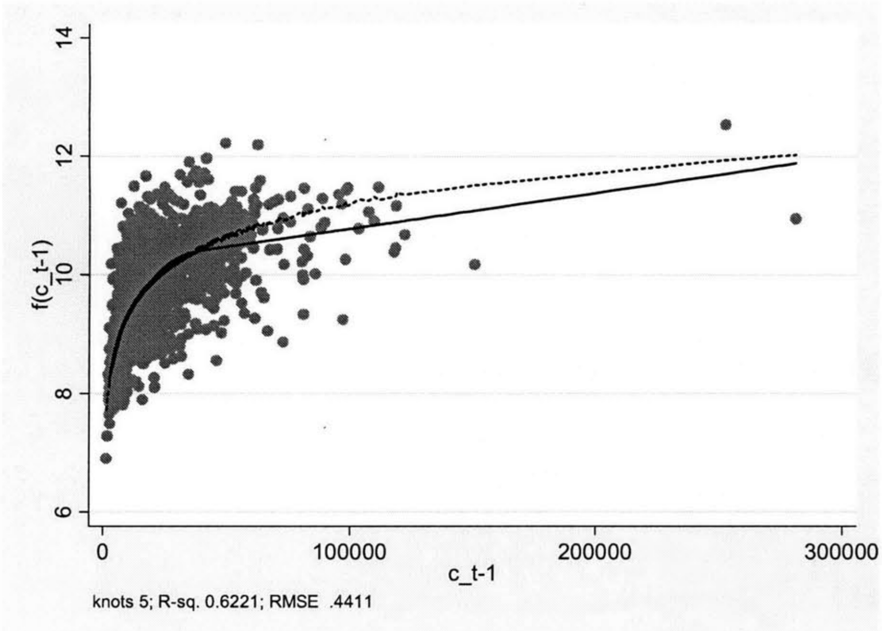


Figure 3: Spline regression of c_t on $f\left(c_{t-1}\right)$

Chapter 2

The miracle of microfinance? Evidence from a randomized evaluation[1]

2.1 Introduction

Microfinance institutions (MFIs) have expanded rapidly in recent years: According to the Microcredit Summit Campaign, microfinance institutions had 154,825,825 clients, more than 100 million of them women, as of December 2007. In 2006, Mohammad Yunus and the Grameen Bank were awarded the Nobel Prize for Peace, for their contribution to the reduction in World Poverty.

CGAP, a branch of the World Bank dedicated towards promoting micro-credit, reports in the FAQ section of its web-site that "There is mounting evidence to show that the availability of financial services for poor households – microfinance – can help achieve the MDGs." Specifically to answer the question "What Do We Know about the Impact of Microfinance?" it lists eradication of poverty and hunger, universal primary education, the promotion of gender equality and empowerment of women, reduction in child mortality and improvement in maternal health as contributions of microfinance for which there is already evidence.

However evidence such as presented by CGAP is unlikely to satisfy the critics of microfinance who fear that it is displacing more effective anti-poverty measures or even contributing to over-borrowing and therefore even greater long term poverty. For instance, an August 2009 article in

[1]This chapter is coauthored with Abhijit Banerjee, Esther Duflo and Rachel Glennerster.

The Wall Street Journal states that Indian households are being "carpet bombed" by loans, and quotes a woman who borrowed from multiple MFIs saying, "I took from one bank to pay the previous one. And I did it again.... [Microfinance] increased our desires for things we didn't have." Another overindebted borrower is quoted saying she would like to see microlenders kicked out of her community "[n]ot just now, but forever" (Gokhale 2009).

The problem is with comparing microfinance clients to non-clients is that clients are self-selected and therefore not comparable to non-clients. Microfinance organizations also purposively chose some villages and not others. Difference in difference estimates can control for fixed differences between clients and non-clients, but it is likely that those who choose join MFIs would be on different trajectories even absent microfinance. This invalidates comparisons over time between clients and non clients (see Alexander-Tedeschi and Karlan (2007)). Moreover, anecdotes about highly successful entrepreneurs or deeply indebted borrowers tell us nothing about the effect of microfinance for the average borrower, much less the average household.

These issues make the evaluation of the impact of microcredit a particularly difficult problem. Thus, there is so far no consensus among academics on the impact of microcredit. For example, Pitt and Khandker (1998) use the eligibility threshold for getting a loan from Grameen bank as a source of identifying variation in a structural model of the impact of microcredit, and find large positive effects, especially for women. However, Morduch (1998) criticizes the approach, pointing out that there is in fact no discontinuity in the probability to borrow at that threshold.[2]

In 1999, Jonathan Morduch wrote that "the 'win-win' rhetoric promising poverty alleviation with profits has moved far ahead of the evidence, and even the most fundamental claims remain unsubstantiated" . In 2005, Beatriz Armendáriz and Morduch reiterated the same uncertainty in their book *The Economics of Microfinance*, noting that the relatively few carefully conducted longitudinal or cross-sectional impact studies yielded conclusions much more measured than MFIs' anecdotes would suggest, reflecting the difficulty of distinguishing the causal effect of microcredit from selection effects. They repeated these cautions in the book's second edition in 2010.

Given the complexity of this identification problem, the ideal experiment to estimate the effect of microcredit appears to be to randomly assign microcredit to some areas, and not some others, and compare outcomes in both sets of areas: randomization would ensure that the only difference between residents of these areas is the greater ease of access to microcredit in the treatment area.

[2]Kaboski and Townsend (2005) use a natural experiment (the introduction of a village fund whose size is fixed by village) to estimate the impact of the amount borrowed and find impacts on consumption, but not investment.

Another possibility would to randomly assign *individuals* to treatment and comparison groups, for example by randomly selecting clients among eligible applicants: the difficulty may then be that in the presence of spillovers, the comparison between treatment and comparison would be biased.

Randomized designs have been used to explore the impact of number of microfinance product design such as group lending and repayment schedules (e.g. Giné and Karlan (2006, 2009), Field and Pande (2008) and Fischer (2010)), while Kaboski and Townsend (2009a, 2009b) use a natural experiment in Thailand to study the intensive-margin impact of a village credit program in Thailand. In work close in spirit to ours, Karlan and Zinman (2009) use individual randomization of the "marginal" clients in a credit scoring model to evaluate the impact of consumer lending in South Africa, and find that access to microcredit increases the probability of employment, and Karlan and Zinman (2010) use a similar random assignment procedure in Manila to study the impacts of "second generation" individual-liability microfinance on male and female borrowers. However, to date, to the best of our knowledge, there have not been any large-scale randomized trials with the potential to examine what happens when "first generation" microcredit (i.e., very small, joint-liability, female-directed loans) becomes available in a new market.[3]

In this paper we report on the first randomized evaluation of the effect of the canonical group-lending microcredit model. In 2005, 52 of 104 neighborhood in Hyderabad (the fifth largest city in India, and the capital of Andhra Pradesh, the Indian state were microcredit has expended the fastest) were randomly selected for opening of an MFI branch by one of the fastest growing MFIs in the area, Spandana, while the remainder were not. Fifteen to 18 months after the introduction of microfinance in each area, a comprehensive household survey was conducted in an average of 65 households in each slum, for a total of 6,850 households. In the mean time, other MFIs had also started their operations in both treatment and comparison households, but the probability to receive an MFI loans was still 8.3 percentage points (44%) higher in treatment areas than in comparison areas (27% borrowers in treated areas vs. 18.7% borrowers in comparison areas).

Inspired by claims similar to those on the CGAP website and in the *The Wall Street Journal,* we examine the effect on both outcomes that directly relate to poverty like consumption, new business creation, business income, etc. as well as measures of other human development outcomes like education, health and women's empowerment.

[3]Karlan and Zinman (2009) use individual randomization of the "marginal" clients in a credit scoring model to evaluate the impact of consumer lending in South Africa, and find that access to microcredit increases the probability of employment down the road. Karlan and Zinman (2010) use a similar random assignment to study the impacts of "second generation" individual-liability microfinance on male and female borrowers in Manila.

On balance our results show significant and not insubstantial impact on both how many new businesses get started and the profitability of pre-existing businesses. We also do see significant impacts on the purchase of durables, and especially business durables. However there is no impact on average consumption, although the effects are heterogenous, and as we will argue later, there may well be a delayed positive effect on consumption. Nor is there any discernible effect on any of the human development outcomes, though, once again, it is possible that things will be different in the long run.

2.2 Experimental Design and Background

2.2.1 The Product

Spandana is one of the largest and fastest growing microfinance organizations in India, with 1.2 million active borrowers in March 2008, up from 520 borrowers in 1998-9, its first year of operation (MIX Market 2009). From its birth place in Guntur, a dynamic city in Andhra Pradesh, it has expanded in the State of Andhra Pradesh, and several others.

The basic Spandana product is the canonical group loan product, first introduced by the Grameen Bank. A group is comprised of six to 10 women, and 25-45 groups form a "center". Women are jointly responsible for the loans of their group. The first loan is Rs. 10,000 (about \$200 at market exchange rates, or \$1,000 at PPP-adjusted exchange rates). It takes 50 weeks to reimburse principal and interest rate; the interest rate is 12% (non-declining balance; equivalent to a 24% APR). If all members of a group repay their loans, they are eligible for second loans of Rs. 10,000-12,000; loan amounts increase up to Rs. 20,000.

Unlike other microfinance organizations, Spandana does not require its clients to borrow to start a business: the organization recognizes that money is fungible, and clients are left entirely free to chose the best use of the money, as long as they repay their loan.

Eligibility is determined using the following criteria: (a) female,[4] (b) aged 18 to 59, (c) residence in the same area for at least one year, (d) possesion of valid identification and residential proof (ration card, voter card, or electricity bill), (e) at least 80% of women in a group must own their home. Groups are formed by women themselves, not by Spandana. Spandana does not determine

[4]Spandana also offers an individual-liability loan. Men are also eligible for individual-liability loans, and individual borrowers must document a monthly source of income, but the other criteria are the same as for joint-liability loans. 96.5% of Spandana borrowers were female in 2008 (Mix Market 2009). Spandana introduced the individual-liability loan in 2007; very few borrowers in our sample have individual-liability loans.

loan eligibility by the expected productivity of the investment (although selection into groups may screen out women who cannot convince fellow group-members that they are likely to repay).

Also, Spandana does not insist on "transformation" in the household (unlike Grameen). Spandana is primarily a lending organization, not directly involved in business training, financial literacy promotion, etc. (Though of course business and financial skills may increase as a result of getting a loan.)

2.2.2 Experimental Design

Spandana selected 120 areas (identifiable neighborhoods, or *bastis*) in Hyderabad as places in which they were interested in opening branches. These areas were selected based on having no pre-existing microfinance presence, and having residents who were desirable potential borrowers: poor, but not "the poorest of the poor." Areas with high concentrations of construction workers were avoided because people who move frequently are not desirable microfinance clients. While those areas are commonly referred to as "slums", these are permanent settlements, with concrete houses, and some public amenities (electricity, water, etc.). Within eligible neighborhoods, the largest areas were not selected for the study, since Spandana was keen to start operations in the largest areas. The population in the neighborhoods selected for the study ranges from 46 to 555 households.

In each area, a baseline survey was conducted in 2005. Households were selected for the baseline survey conditional on having a woman between the ages of 18-55 in the household. Information was collected on household composition, education, employment, asset ownership, decision-making, expenditure, borrowing, saving, and any businesses currently operated by the household or stopped within the last year. A total of 2,800 households were surveyed in the baseline.[5]

After the baseline survey, sixteen areas were dropped from the study prior to randomization. These areas were dropped because they were found to contain large numbers of migrant-worker households. Spandana (like other microfinance agencies) has a rule that loans should only be made to households who have lived in the same community for at least three years because dynamic incentives (the promise of more credit in the future) are more effective in motivating repayment for these households. The remaining 104 areas were paired based on minimum distance according to per capita consumption, fraction of households with debt, and fraction of households who had

[5]Unfortunately, the baseline sample survey was not a random survey of the entire area. In the absence of a census, the first step to draw the sample was to perform a census of the area. The survey company did not survey a comprehensive sample, but a sample of the houses located fairly close to the area center. This was rectified before the endline survey, by conducting a census in early 2007.

a business, and one of each pair was randomly assigned to the treatment group. Spandana then progressively began operating in the 52 treatment areas, between 2006 and 2007. Note that in the intervening periods, other MFIs also started their operations, both in treatment and comparison areas. We will show below that there is still a significant difference between MFI borrowing in treatment and comparison groups.

A comprehensive census of each area was undertaken in early 2007 to establish a sampling frame for the followup study, and to determine MFI takeup (to estimate the required sample size at endline). The endline survey began in August 2007 and ended in April 2008. The endline survey in each area was conducted at least 12 months after Spandana began disbursing loans, and generally 15 to 18 months after. The census revealed low rates of MFI borrowing even in treatment areas, so the endline sample consisted of households whose characteristics suggested high propensity to borrow: households who had resided in the area for at least 3 years and contained at least one woman aged 18 to 55. Spandana borrowers identified in the census were oversampled, and the results presented below correct for this oversampling so that the results are representative of the population as a whole. In general, baseline households were not purposely resurveyed in the followup.[6]

Table 1, Panel A shows that treatment and comparison areas did not differ in their baseline levels of population, household indebtedness, businesses per capita, expenditure per capita, or literacy levels. This is not surprising, since the sample was stratified according to per capita consumption, fraction of households with debt, and fraction of households who had a business.

Table 1, Panel B shows that households in the followup survey do not systematically differ between treatment and comparison in terms of literacy, the likelihood that the wife of the household head works for a wage, the adult-equivalent size of the household,[7] the number of "prime-aged" women (aged 18 - 45), in the presence of teenagers (aged 13-18) in the household, the percentage who operate a business opened a year or more ago, or the likelihood of owning land, either in Hyderabad or in the family's native village.

[6]Baseline households were not deliberately resurveyed, since they were not a random sample to start with. Furthermore, the baseline sample was too small to detect plausible treatment effects, given the low takeup of MFI loans. These problems were both corrected in the followup survey, at the cost of not having a panel. The exception to the non-resurveying of baseline households is a small sample of households (about 500 households) who indicated they had loans at the baseline, who were surveyed with the goal of understanding the impact of an increase in credit availability for those households who were already borrowing (though not from MFIs). This analysis is ongoing.

[7]Following the conversion to adult equivalents used by Townsend (1994) for rural Andhra Pradesh and Maharastra, the weights are: for adult males, 1.0; for adult females, 0.9; for males and females aged 13-18, 0.94 and 0.83, respectively; for children aged 7-12, 0.67 regardless of gender; for children 4-6, 0.52; for toddlers 1-3, 0.32; and for infants, 0.05. Using a weighting that accounts for within-household economies of scale does not affect the results (results available on request).

2.2.3 The context: Findings from the Baseline

The average baseline household is a family of 5, with monthly expenditure of Rs 5,000, \$540 at PPP-adjusted exchange rates (World Bank 2006).[8] A majority of households (70%) lived in a house they owned, and the remaining 30% in a house they rented. Almost all of the 7 to 11 year olds (98%), and 84% of the 12 to 15 year olds, were in school.

There was almost no MFI borrowing in the sample areas at baseline. However, 69% of the households had at least one outstanding loan. The average loan was Rs. 20,000 (median Rs 10,000), and the average interest rate was 3.85% per month. Loans were taken from moneylenders (49%), family members (13%), friends or neighbors (28%). Commercial bank loans were very rare.

Although business investment was not commonly named as a motive for borrowing, 31% of households ran at least one small business at the baseline, compared to an OECD-country average of 12%. However, these businesses were *very* small: only 10% had any employees, and typical assets employed were sewing machines, tables and chairs, balances and pushcarts; 20% of businesses had no assets whatsoever. Average profits were Rs. 3,040 (\$340) per month on average.

Baseline data revealed limited use of consumption smoothing strategies other than borrowing: 34% of the households had a savings account, and only 26% had a life insurance policy. Almost none had any health insurance. Forty percent of households reported spending Rs. 500 (\$54) or more on a health shock in the last year; 60% of households who had a sick member had to borrow.

2.2.4 Did the intervention increase MFI borrowing?

Treatment communities were randomly selected to receive Spandana branches, but other MFIs also started operating both in treatment and comparison areas. We are interested in testing the impact of *microcredit*, not just Spandana branches. In order to interpret differences between treatment and comparison areas as due to microcredit, it must be the case that MFI borrowing is higher in treatment than in comparison. Table 2 shows that this is the case. Households in treatment areas are 13.3 percentage points more likely to report being Spandana borrowers–18.6% vs. 5.3% (table 2, column 1). The difference in the percentage of households saying that they borrow from any MFI is 8.3 percentage points (table 2, column 2), so some households borrowing from Spandana in treatment areas would have borrowed from another MFI in the absence of the intervention. While the absolute level of total MFI borrowing is not very high, it is almost 50% higher in treatment than

[8]PPP exchange rate: \$1=Rs. 9.2. All following references to dollar amounts are in PPP terms unless noted otherwise.

in comparison areas–27% vs. 18.7%. Columns 3 and 4 show that treatment households also report significantly more borrowing from MFIs than comparison households. Averaged over borrowers and non-borrowers, treatment households report Rs 1,408 more borrowing from Spandana, and Rs. 1,257 more from all MFIs.

2.3 The Impacts of Microfinance: Conceptual Framework

The purpose that the borrower reports for borrowing from Spandana is instructive about the kinds of effects of microcredit access that we might expect. Recall that Spandana does not insist that the loan be used for business purpose; nevertheless, these responses come from the survey, not what was reported to Spandana. In the case of 30% of Spandana loans the reported purpose was starting a new business; 22% were supposed to be used to buy stock for existing business, 30% to repay an existing loan, 15% to buy a durable for household use, and 15% to smooth household consumption. (Respondents could list more than one purpose, so purposes add up to more than 100%.) In other words, while some households plan to use their loans to start a business and others use a loan to expand a business they already have, many others use the loan for a non-business purpose, such as repaying another loan, buying a television or meeting day-to-day household expenses.

A feature of starting a business is that there are some costs that must be paid before any revenue is earned. While a small business like those operated by households in our sample may not have a lot of durable assets (machinery, property, etc.), they typically need working capital, such as stock for a store, fabric to make saris, etc. And since there is always a fixed minimum time commitment in any of these businesses (someone has to sit in the shop, go out to hawk the saris, etc.), it makes no sense to operate them below a certain scale and hence it is hard to imagine operating even these businesses without a minimum commitment of working capital. Many businesses also have some assets, such as a pushcart, dosa tawa, sewing machine, stove, etc. The need to purchase assets and working capital constitutes a fixed cost of starting a business, and one impact of microfinance may be that it enables households who would not or could not pay this fixed cost without borrowing, to become entrepreneurs.

2.3.1 A simple model of occupational choice

No MFI

As a simple model of the decision to become an entrepreneur, consider households who live for two periods ($t = 1, 2$) and have endowment income y_1^i, y_2^i. Households maximize the utility function:

$$U(c_1^i) + \delta_i U(c_2^i) \tag{2.1}$$

They can simply consume their endowment in each period ($c_1^i = y_1^i, c_2^i = y_2^i$), or they can make several intertemporal decisions. In the first period they can invest in a business with a constant-returns production function that generates second period income:

$$y = A_i(K - \underline{K})$$

Households differ in their return to entrepreneurship: some households are high-return: $A_i = A_H$. Other households have a low return to entrepreneurship: $A_i = A_L < A_H$. Households also differ in their patience (that is, in their relative preference for consumption in period 1 versus period 2). Patient households have $\delta_i = \delta_H$, while impatient households have $\delta_i = \delta_L < \delta_H$.

In addition to the option of starting a business, households can also borrow and save. Prior to the entry of the MFI, they can borrow up to an amount M from a money-lender at interest rate $R(m) > A_H$. Alternatively, they can lend at net interest rate $R(I) < A_L < A_H < R(m)$. (Therefore, in the absence of the fixed cost, households with a sufficiently strong desire to shift consumption from period 1 to period 2 would invest in a business, rather than lend, since entrepreneurship has a higher rate of return. However, households who do not want to shift consumption from period 1 to period 2 will not borrow to start a business since $A_H < R(m)$.)

Households make decisions regarding first-period saving/borrowing s_1^i, and whether to become entrepreneurs, in the first period. Let $\mathbf{1}_E$ be an indicator for a household entering entrepreneurship; $\mathbf{1}_S$ be an indicator for being a period-1 saver ($s_1^i > 0$), and $\mathbf{1}_B$ be an indicator for being a period-1 borrower ($s_1^i < 0$). Households maximize utility (2.1) subject to the constraints that first-period consumption plus any net savings or investment not exceed first-period endowment income, and that second-period consumption not exceed second-period endowment income, plus the net return from any borrowing/saving or investment .

$$
\begin{aligned}
c_1^i + s_1^i + K_i &\leq y_1^i \\
c_2^i &\leq y_2^i + \mathbf{1}_E A_i(K - \underline{K}) + \mathbf{1}_S R(I) s_1^i - \mathbf{1}_B R(m) s_1^i
\end{aligned}
\tag{2.2}
$$

where $s_1^i \equiv y_i^i - c_i^1 - \mathbf{1}_E K$.

Figure 1a shows the intertemporal choice problem of a household with a relatively low discount factor ($\delta_i = \delta_L$) and/or low return to entrepreneurship ($A_i = A_L$). The indifference curve (solid curve) is the locus of points that give equal utility, and the budget line (dashed line) is the locus of points satisfying (2.2). This household will not choose to start a business in the absence of an MFI. To do so would require borrowing at rate $R(m)$ and/or choosing very low first-period consumption, which is too painful for an impatient household or a household that realizes that its period 2 returns from entrepreneurship will be low. Due to the wedge between borrowing and lending rates ($R(I) < R(m)$), the household optimally consumes its endowment (y_1^i, y_2^i).

Figure 1b shows a the indifference curve and budget line of a household with high discount factor ($\delta_i = \delta_H$) and high return to entrepreneurship ($A_i = A_H$), who will choose to start a business, borrowing from the moneylender to do so, because for this household cutting first-period consumption is not too painful relative to the second-period returns.

Therefore, even when borrowing is expensive, the households with the highest incentives to move consumption into the future will choose to become entrepreneurs, by borrowing or cutting consumption. Other households will not start businesses in the high-interest regime, although some of these households may opt to do so when they get access to a cheaper source of credit.

MFI enters

Now, an MFI enters. Households can now borrow at rate $R(I) < R(s) < R(m)$ up to an amount L. We assume that $A_L < R(s) < A_H$; the MFI lends at rates that are lower than the high return to entrepreneurship, but lower than the low return to entrepreneurship. For simplicity, we assume $L \leq \underline{K}$: the MFI will lend up to the amount needed to finance the fixed cost of entrepreneurship. Now, for some households, it may pay to borrow to go into business. Figure 2 shows two households, both of whom are relatively impatient ($\delta_i = \delta_L$). Because they are impatient, neither household had started a business before the MFI entered. However, household 1 has high return to entrepreneurship ($A_i = A_H$), while household 2 has low return to entrepreneurship

($A_i = A_L$).

The higher-return household, Household 1, now decides to start a business, borrowing from the MFI at rate $R(s)$ to finance the fixed cost. Due to the nonconvexity in the budget set, Household 1's current consumption may actually fall when they get access to microfinance, because they pay for part of the fixed cost with borrowing, and part by cutting consumption, rather than borrowing the full amount.[9] Because of the fixed cost, households who did not have a business before they gained access to microfinance, but are have a high return to starting a business, may see their consumption *decrease* due to treatment.

The other indifference curve in Figure 2 shows the case of a household with low return to entrepreneurship, Household 2. This household does not choose to start a business even when MFI loans are available. However, because the household is impatient ($\delta_i = \delta_L$), the household takes advantage of less-expensive credit to borrow against future income, and sees an immediate increase in consumption when MFI credit becomes available.

Note that it is not necessary that $A_L << A_H$ in order to see households with high and low returns behaving differently. Because of the nonconvexity due to the fixed cost of entrepreneurship, even quite similar households may make very different decisions.

A third group of households is those that already had a business when they gained access to microfinance. Unlike new entrepreneurs, these households have already paid the cost of starting a business, before the MFI entered. For such households, microfinance can allow them to scale up their business. Because they do not need to pay a fixed cost at the time they start to borrow from the MFI, their consumption should not decrease. Figure 3 shows that for a household that expands an existing business with an MFI loan, investment in the business increases when they get access to microfinance since $R(s) < A_H$; current consumption may or may not increase significantly, but will not fall as it may for households who are starting new businesses.

The final group of households is those who have $A_i = A_L$ and $\delta_i = \delta_H$: they have low returns to entrepreneurship, and they are patient. For these households, since $A_L < R(s)$, it does not pay to borrow to become an entrepreneur, and since they are patient, they do not want to borrow to increase their current consumption. These households do not borrow from the MFI and, since $R(I) < R(s)$, the may continue to consume their endowment. Figure 4 shows such a household.

[9]Alternatively, the household may borrow the full amount, but use part of the loan principal to make the initial repayments, since MFI loans typically require that the borrower begin to make repayments just 1 week after the loan is disbursed.

2.3.2 Summary of predictions

The presence of a fixed cost that must be paid to start a business suggests that we should see the following when credit access increases:

- Of those without an existing business:
  - Households with high returns to becoming an entrepreneur will pay the fixed cost and become entrepreneurs: investment will rise, and consumption may fall.
  - Households with low returns to becoming an entrepreneur will borrow to increase consumption.
- Existing business owners do not face a nonconvexity: they can borrow to increase investment (and perhaps consumption).

Before testing these predictions, we will summarize the overall treatment-comparison differences in business outcomes and in household spending, averaged over existing business owners, those with low propensity to become business owners, and those with high propensity to become business owners.

2.4 Results: Entire Sample

2.4.1 New businesses and business outcomes

To estimate the impact of microfinance becoming available in an area, we examine intent to treat (ITT) estimates; that is, simple comparisons of averages in treatment and comparison areas, averaged over borrowers and non-borrowers. Table 3 shows ITT estimates of the effect of microfinance on businesses operated by the household, and, for those who own businesses, we examine business profits, revenue, business inputs, and the number of workers employed by the business. (The construction of these variables is described in the Data appendix.) Each column reports the results of a regression of the form

$$y_i = \alpha + \beta \times Treat_i + \varepsilon_i$$

where $Treat_i$ is an indicator for living in a treated area; β is the intent to treat effect. Standard errors are adjusted for clustering at the area level and all results are weighted to correct for oversampling of Spandana borrowers.

Column 1 of table 3 indicates that households in treated areas are 1.7 percentage points more likely to report operating a business opened in the past year. In comparison areas, 5.3% of households opened a business in the year prior to the survey, compared to 7% in treated areas, so this represents 32% more new businesses in treatment than in comparison. Another way to think about the economic significance of this figure is that approximately 1 in 5 of the additional MFI loans in treatment areas is associated with the opening of a new business: 1.7pp more new businesses due to 8.3pp more MFI loans.[10]

We also examine the impact of microcredit access on business profits. While the point estimate in column 2 indicates that average profits in treated areas are higher than in nontreated areas, this effect is not significant. The difficulty in measuring business profits means that we cannot rule out either a large positive or a negative treatment effect on business profits. The effects on monthly business revenues and monthly spending on business inputs are both positive, but not significant (Table 3, columns 3 and 4).[11] Business owners in treatment areas do not report having more employees (column 5).

2.4.2 Expenditure

Table 4 gives intent to treat estimates of the effect of microfinance on household spending. (The construction of the expenditure variables is described in the Data appendix.) Column 1 shows that, averaged over old business owners, new entrepreneurs, and non-entrepreneurs, there is no significant difference in total household expenditure per adult equivalent between treatment and comparison households. The average household in a comparison area has expenditure of Rs 1,420 per adult equivalent per month; in treatment areas the number is 1,453, not statistically different. About Rs 1,300 of this is nondurable expenditure, in both treatment and comparison areas (column 2). However, there are shifts in the composition of expenditure: column 3 shows that households in treatment areas spend a statistically significant Rs 22 more per capita per month on durables than do households in comparison areas–Rs 138 vs. Rs 116. Further, when focusing on spending on durable goods used in a household business (column 4), the difference is even more striking:

[10] If we were confident that there were no spillovers of microfinance that affected the outcomes of nonborrowers in treated areas, this would be the local average treatment effect (LATE) of borrowing on those induced to borrow because of treatment. Although we are unable to conclusively estimate the extent of spillovers, this is nevertheless the per-loan impact of microcredit access.

[11] A second survey of the households is planned for late 2009-early 2010; we hope that when panel data on households with businesses is available, we may be able to estimate the effect of microcredit access on business outcomes with more precision.

households in treatment areas on average spend more than twice as much on durables used in a household business, Rs 12 per capita per month in treatment vs. Rs. 5 in comparison.

Column 5 shows that the increase in durables spending by treatment households was partially offset by reduced spending on "temptation goods": alcohol, tobacco, betel leaves, gambling, and food consumed outside the home. Spending on temptation goods is reduced by Rs 9 per capita per month.

The absolute magnitude of these changes is relatively small: for instance, the Rs 22 of increased durables spending is approximately $2.50 at PPP exchange rates. However, this represents an increase of almost 20% relative to total spending on durable goods in comparison areas (Rs 116). Furthermore, this figure averages over nonborrowers and borrowers. *If* all of this additional spending were coming from those who do borrow (that is, if there were no spillover effects to non-borrowers), the implied increase per new borrower would be Rs 265, more than twice the level of durable goods spending in comparison areas. However, since it is entirely possible that there are spillover effects, we will focus here on reduced-form/intent to treat estimates.

2.4.3 Does microfinance affect education, health, or women's "empowerment"?

The evidence so far suggest that, on average, after 15 to 18 months, microcredit allowed some households to start a new business. While we see no impact on overall expenditures, there is a significant impact on durable expenditures, and a significant decrease in goods that individuals had reported most frequently in the baseline as being "temptation goods".

The increase in durable expenditure, and the decrease with spending on temptation goods fits with the claims often made regarding microcredit, that microcredit changes lives. According to these claims, microcredit can also empower women or allow families to keep children in school (e.g. CGAP 2009). To examine these questions, Table 8 examines ITT effects on measure of women's decision-making, children's health, and education spending. Columns 1-3 show that women in treatment areas were no more likely to be make decisions about household spending, investment, savings, or education. Column 2 shows that even focusing on non-food decisions, which might be more sensitive to changes in empowerment, does not change the finding.

A finding of many studies of women's vs. men's decisions is that women spend more on child health and education (e.g. Lundberg et al. 1997). These are interesting outcomes in their own right, and increased spending in these areas might also demonstrate greater decision-making or bargaining power for women. However, there is no effect on health or education outcomes, either.

Column 3 shows that households in treatment areas spend no more on medical and sanitation (e.g. soap) than do comparison households, and column 4 shows that, among households with children, households in treatment areas were no less likely to report that a child had a major illness in the past year. Columns 5-7 examine educational outcomes. Among households with school-aged children, households in treatment areas are not more likely to have children in school. Looking just at girls' school enrollment gives the same conclusion (column 6). While the enrollment results are unsurprising since the majority of children are enrolled in school even in treatment areas, schooling expenditures vary widely from household to households, and treatment households do not spend more on schooling, either: spending on tuition, school fees and uniforms is the same in treatment and comparison areas. For decision-making, health, and education, the standard errors of the treatment effects are reasonably small: with 95% confidence we can rule out an effect on any of these outcomes of more than about 10% of the standard deviation in comparison areas.

This suggest that, at least in the relatively short run, there is no prima facie evidence that microcredit changes the way the household functions.

2.5 Testing the model: Impact Heterogeneity

As discussed above, the fact that starting a new business requires a fixed, up-front expenditure on assets and working capital, while expanding an existing business does not require such a fixed cost, means that we predict different impacts of MFI access for 3 groups of households:

1. those who had a business one year before the survey
2. among who did not have a business one year before the survey, those who **are not** likely to become entrepreneurs
3. among who did not have a business one year before the survey, those who **are** likely to become entrepreneurs.

This section investigates those predictions.

2.5.1 Predicting who is a likely entrepreneur

Because starting a new business is an outcome that is itself affected by the presence of microcredit (as shown in Table 3, column 1) we cannot just compare those who become new entrepreneurs in treatment areas to those who become in comparison areas. We need to identify characteristics that are not themselves affected by treatment, and which make some households more likely to become entrepreneurs, so that we can compare their outcomes with those in comparison areas who would have stated businesses if they had gotten access to microcredit. It also allows us to compare the impact of microcredit on those likely to use microcredit to become entrepreneurs, to those who are unlikely to use microcredit for this purpose.

Among those who did not already own a business a year ago, the following characteristics predict the decision to become an entrepreneur: whether the wife of the household head is literate, whether the wife of the household head works for a wage, the number of prime-aged women in the household, and whether the household owns land in Hyderabad or in their native village. In the context of the model in Section 6, education and number of women may proxy for time preference, since Indian women have been found to be more patient than Indian men, and more educated individuals have been found to be more patient (Bauer and Chytilová 2008). If the wife of the household head works for a wage, this will reduce the return to opening a business; land ownership is a proxy for initial wealth.

Data on treatment-area households who do not own an old business is used to identify the relationship between these predictors and entrepreneurship: the "first stage" is shown in Table 9. Fitted values, "Biz hat" are generated for all households, treatment and comparison, who do not own an old business.[12] Literacy of the women in the family, the presence of women who do not work for a wage in the family, and the number of prime-aged women and the presence of teenagers in the household all positively predict the family starting a new business. This is as it should be: They all predict mean that the family has a larger pool of labor who have the ability to run a business, labor whose outside wage is likely quite low. These households correspond to "A_H households" in the model. Land ownership, a proxy for wealth that is unlikely to be affected by treatment (and is balanced across treatment and control, as shown in Table 1B, columns 7 and 8). also positively predicts starting a business.[13]

[12]The number of observations in these regressions is lower because 10% of the sample is missing information for at least one predictor. Adding dummies for missing values and including these households does not substantially change the results (available on request).

[13]Results dropping land ownership as a predictor are very similar and are available on request.

2.5.2 Relative consumption of old vs. likely vs. unlikely entrepreneurs

To interpret the findings below, which demonstrate significantly different treatment effects on the families of current business owners, compared to non-business owners who we predict to be likely to start a business as well as non-business owners who we predict to be unlikely to start a business, it may be helpful to have in mind what these groups look like in terms of average per capita expenditure in the absence of treatment. Due to randomization, the comparison group constitutes a reliable source of this information. Table 5 shows, for households in comparison areas only, the total per capita monthly consumption of old entrepreneurs (group 1 above), and, among those without a business 1 year prior to the survey, those with below-median predicted probability of starting a business (group 2 above), and those with median or above predicted probability of starting a business (group 3 above). Approximately one third, 31%, of comparison households are old business owners (Table 1b, col 5). Because all of the predictors of business propensity are binary, a significant number of households are exactly at the median level of business propensity, so group 2 includes 1,525 households and group 3 includes 2,571 households. Both those who own a business and those with median-or-above propensity of starting a business have nondurable monthly per capita expenditure approximately Rs 100 greater than low-propensity household: Rs 1,336 for old owners, Rs 1,337 for high-propensity households, and Rs 1,237 for low-propensity households. When durables purchases are included, the gap between old business owners and low-propensity households widens to Rs. 132 (Rs 1,480 vs. Rs 1,348) and the gap between high- and low-propensity households narrows slightly to Rs 82 (Rs 1,430 vs Rs 1,348). All 3 groups are quite poor in absolute terms: average nondurable consumption of old business owners and high-propensity households, the better-off groups, is less than $5 per person per day at PPP exchange rates: hardly prosperous. So, the impacts of microfinance discussed below are impacts for poor households, although old business owners and likely new entrepreneurs are slightly better off than those unlikely to become new entrepreneurs.

2.5.3 Measuring impacts for different groups

Table 6 presents the results of ITT regressions of the following form:

$$\begin{aligned} y_i &= \alpha_0 + \alpha_1 Old\_biz_i + \alpha_2 Biz\_hat_i + \\ &\quad \beta_1 Treat_i \times Old\_biz_i + \beta_2 Treat_i \times No\_old\_biz_i + \beta_3 Treat_i \times Biz\_hat_i + \varepsilon_i \end{aligned}$$

The β's are the intent to treat effects for the different groups for whom we expect different effects. β_1 measures the treatment effect for households who have an old business, and therefore did not have to pay a fixed cost, but could expand their business with an MFI loan. β_2 measures the treatment effect for households who do not own an old business, and have the lowest propensity to become new entrepreneurs. β_3 measures the *additional* treatment effect for households who do not own an old business, and are at the 75th percentile of propensity to become new entrepreneurs.[14]

Column 1, where the outcome variable is an indicator for being an MFI borrower, shows that all 3 groups take out MFI loans at very similar rates: households who have an old business increase their rate of MFI borrowing by 8.5 percentage points in treatment vs. comparison, and households who do not have an old business increase their rate of MFI borrowing by 9.6 percentage points; a higher propensity to become a new entrepreneur does not imply a higher chance of borrowing from an MFI. Therefore the results in columns 2 - 5 in Table 6 reflect different *uses* of MFI credit among these groups, not different rates of takeup.

Column 2 of Table 6 shows that, indeed, it is those with high business propensity who start more businesses in treatment than in comparison. Households with an old business are neither more nor less likely to start new businesses in treatment areas than comparison areas.

2.5.4 Differing patterns of changes in spending

In column 3 of Table 6, the outcome variable is monthly per capita spending on durable goods. Households who have an old business significantly increase durables spending, by 55 Rs in treatment vs. comparison areas, averaged over borrowers and nonborrowers. Households who do not have an old business, and have the lowest propensity to start a business, do not increase durables spending at all. However, moving from the lowest propensity to become a new entrepreneur to the 75th percentile of propensity is associated with an 54.9 Rs. per capita per month increase in the effect on durables spending. Therefore, consistent with the predictions above, those households who already own a business, or who are likely to start a new business, show a significant positive treatment effect on durables spending, while those who are least likely to start a new business do not use MFI credit for durable goods.

In column 4 of Table 6, the outcome variable is monthly per capita spending on nondurables

[14]The business propensity variable is scaled to have a minimum of zero and to be equal to 1 at the 75th percentile. Because this is a generated regressor, all regressions with the business propensity variable are reported with bootstrapped standard errors. The regressions are weighted to correct for oversampling of Spandana borrowers.

(food, entertainment, transportation, etc.). Households who have an old business show no significant treatment effect on nondurable spending. Households who do not have an old business, and have the lowest propensity to start a business, on the other hand, show a large and significant increase in nondurable spending: 212 Rs per capita per month. Moving from the lowest propensity to become a new entrepreneur, to the 75th percentile of propensity is associated with 258 Rs. per capita per month decrease in the effect on nondurable spending so that, at the 75th percentile, households are *reducing* spending by 46 Rs. per capita per month. So, again consistent with the predictions above, those households who are least likely to start a new business show a significant positive treatment effect on nondurable spending (they do not pay the fixed cost to start a business, and instead use the loan to pay off more expensive debt or borrow against future income), while those who are highly likely to start a new business decrease spending on nondurables, in order to finance the fixed cost of becoming entrepreneurs.

In column 5 of Table 6, the outcome variable is monthly per capita spending on "temptation goods" (alcohol, tobacco, betel leaves, gambling, and food and tea outside the home). Microfinance clients sometimes report, and MFIs sometimes claim, that access to MFI credit can act as a "disciplining device" to help households reduce spending that they would like to reduce, but find difficult to reduce in practice. The pattern of effects for temptation goods is similar to the pattern for overall nondurable spending, but the effect for those with a high propensity to become entrepreneurs is much larger relative to spending on this category (temptation goods spending accounts for 6.5% of nondurables spending by comparison households). Households who do not have an old business, and have the lowest propensity to start a business, increase spending on temptation goods, roughly proportionally with the increase in other nondurables spending. However, moving from the lowest propensity to become a new entrepreneur, to the 75th percentile of propensity is associated with Rs. 40 per capita per month decrease in the effect on temptation goods spending so that, at the 75th percentile, households are *reducing* spending on temptation goods by Rs. 14 per capita per month. In other words, those with high entrepreneurship propensity households are cutting back temptation goods by 17%. If all of this effect were concentrated on those who become borrowers due to treatment, it would suggest a decrease of Rs. 168 per capita per month, for high entrepreneurship propensity households who become MFI borrowers due to treatment.

2.5.5 Business outcomes for existing businesses

Because new entrepreneurs (those who open businesses as a result of treatment) are a selected sample, we analyze business profits separately for businesses that existed before the start of the program. Table 7 shows treatment effects on business profits for these existing entrepreneurs. Because month-to-month profits for small businesses are extremely variable, and we are concerned that profits results may be driven by businesses who accidentally report no inputs or no income, we report results for all existing entrepreneurs and results dropping businesses reporting no inputs or no income.

Using both measures, we find impacts on business profits that, while uniformly positive, are not significant. Column 1 looks at business profits for all existing entrepreneurs. Existing business owners see an insignificant increase in business profits of Rs. 785 per month. Dropping businesses reporting no inputs or no income reduces this estimate to Rs. 143, also insignificant (column 2). Column 3 shows that the estimated effect on the 95th percentile of business profits is large in magnitude (Rs 2095), but insignificant, while column 4 shows that the estimated effect on median (50th percentile) business profits is an insignificant Rs 80.

In short, profits data for small businesses are extremely noisy, due in part to some businesses with very high or very low profits, and unfortunately we cannot rule out either a large positive or negative average impact on business profits. However, for the median business, we can rule out a positive impact of more than roughly Rs 500 per month (one third of median profits in the control group), or a negative effect of more than roughly Rs 300 per month, one sixth of median profits in the control group. A second survey of our sample is planned for late 2009-early 2010; we hope that when panel data on households with businesses is available, we may be able to estimate the effect of microcredit access on outcomes for existing businesses with more precision.

2.6 Conclusion

These findings suggest that microcredit does have important effects on business outcomes and the composition of household expenditure. Moreover, these effects differ for different households, in a way consistent with the fact that a household wishing to start a new business must pay a fixed cost to do so. Existing business owners appear to use microcredit to expand their businesses: durables spending (i.e. investment) increases. Among households who did not own a business when the program began, those households with low predicted propensity to start a business do not increase durables spending, but do increase nondurable (e.g. food) consumption, consistent with using microcredit to pay down more expensive debt or borrow against future income. Those households with high predicted propensity to start a business, on the other hand, reduce nondurable spending, and in particular appear to cut back on "temptation goods," such as alcohol, tobacco, lottery tickets and snacks eaten outside the home, presumably in order to finance an even bigger initial investment than could be paid for with just the loan.

This makes it somewhat hard to assess the long run impact of the program. For example, it is possible that in the longer run these people who are currently cutting back consumption to enable greater investment will become significantly richer and increase their consumption. On the other hand, the segment of the population that increased its consumption when it got the loan without starting a business may eventually become poorer because it is borrowing against is future, though it is also possible that they are just enjoying the "income effect" of having paid down their debt to the money-lender (in which case they are richer now and perhaps will continue to be richer in the future).

While microcredit "succeeds" in affecting household expenditure and creating and expanding businesses, it appears to have no discernible effect on education, health, or womens' empowerment. Of course, after a longer time, when the investment impacts (may) have translated into higher total expenditure for more households, it is possible that impacts on education, health, or womens' empowerment would emerge. However, at least in the short-term (within 15-18 months), microcredit does not appear to be a recipe for changing education, health, or womens' decision-making. Microcredit therefore may not be the "miracle" that is sometimes claimed on its behalf, but it does allow households to borrow, invest, and create and expand businesses.

2.A Appendix: Tables

Table 1: Treatment-Control balance

Panel A: Slum-level characteristics (baseline sample)

	(1) Population (census)	(2) Avg debt outstanding (Rs)	(3) Avg debt outstanding (Rs), no outliers	(4) Businesses per capita	(5) Per capita expenditure (Rs/mo)	(6) Literacy
Treatment	-16.258	-4815.276	-2109.195	-0.014	24.777	0.002
	[31.091]	[4812.666]	[2551.356]	[0.035]	[35.694]	[0.018]
Control Mean	316.564	36567.56	28820.718	0.299	981.315	0.68
Control Std Dev	162.89	35319.929	12639.611	0.152	163.19	0.094
N	104	104	104	104	104	104

Note: Cluster-robust standard errors in brackets. Results are weighted to account for oversampling of Spandana borrowers. * means statistically significant at .1, ** means statistically significant at .05, *** means statistically significant at .01.

Table 1: Treatment-Control balance

Panel B: Household-level characteristics (followup sample)

	(1) Spouse is literate	(2) Spouse works for a wage	(3) Adult equivalents	(4) Prime-aged women (18-45)	(5) Any teen (13-18) in HH	(6) Old businesses owned	(7) Own land in Hyderabad	(8) Own land in village
Treatment	-0.001	-0.013	-0.01	-0.021	0.018	0.002	-0.002	0.005
	[0.027]	[0.026]	[0.066]	[0.028]	[0.016]	[0.022]	[0.007]	[0.028]
Control Mean	0.544	0.226	4.686	1.456	0.495	0.306	0.061	0.195
Control Std Dev	0.498	0.418	1.781	0.82	0.5	0.461	0.239	0.396
N	6133	6223	6821	6856	6856	6733	6824	6813

Note: Cluster-robust standard errors in brackets. Results are weighted to account for oversampling of Spandana borrowers. Spouse is the wife of the household head, if the head is male, or the household head if female. An old business is a business started at least 1 year before the survey. * means statistically significant at .1, ** means statistically significant at .05, *** means statistically significant at .01.

Table 2: First stage

	(1) Borrows from Spandana	(2) Borrows from any MFI	(3) Spandana borrowing (Rs.)	(4) MFI borrowing (Rs.)
Treatment	0.133***	0.083***	1406.814***	1250.504**
	[0.023]	[0.030]	[261.568]	[477.956]
Control Mean	0.052	0.186	592.467	2404.742
Control Std Dev	0.222	0.389	2826.855	6698.216
N	6651	6651	6651	6651

Note: Cluster-robust standard errors in brackets. Results are weighted to account for oversampling of Spandana borrowers. * means statistically significant at .1, ** means statistically significant at .05, *** means statistically significant at .01.

Table 3: Impacts on business creation and business outcomes

	All households		Business owners					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	New business	Stopped a business	Profit	Inputs	Revenues	Employees	Wages (Rs per month)	Value of assets used in businesses
Treatment	0.016**	-0.003	475.15	2391.534	2866.683	-0.028	-100.937	857.876
	[0.008]	[0.004]	[2326.340]	[4441.696]	[3187.618]	[0.084]	[136.518]	[979.533]
Control Mean	0.054	0.031	550.494	13193.81	13744.304	0.384	411.477	6675.911
Control Std Dev	0.252	0.173	46604.8	59769.3	47025.5	1.656	2977.457	16935.123
N	6735	6650	2362	2362	2362	2365	2365	2360

Note: Cluster-robust standard errors in brackets. Profits, inputs and revenues are monthly, measured in Rs. Results are weighted to account for oversampling of Spandana borrowers. * means statistically significant at .10, ** means statistically significant at .05, *** means statistically significant at .01.

Table 4: Impacts on monthly household expenditure

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
				Rs per capita per month					
	Total PCE	Nondurable PCE	Food PCE	Durable PCE	Durables used in a business	"Temptation goods"	Festivals (not weddings)	Any home repair>Rs 500 last year	75th percentile of home repair value (Rs)
Treatment	9.863	-6.689	-12.674	19.575*	6.832*	-8.859*	-22.217**	0.03	-1000
	[37.231]	[31.857]	[11.618]	[11.308]	[3.519]	[4.885]	[10.620]	[0.020]	[1320.07]
									75th percentile
Control Mean	6821	6775	6821	6775	6817	6857	6857	0.495	in control is
Control Std Dev	1419.229	1304.786	520.51	116.174	5.335	83.88	119.489	0.501	8000
N	978.299	852.4	263.099	332.563	89.524	130.213	161.522	2189	2189

Note: Cluster-robust standard errors in brackets. "Temptation goods" include alcohol, tobacco, gambling, and food and tea outside the home. Durables include assets for household or business use. Results are weighted to account for oversampling of Spandana borrowers. * means statistically significant at .10, ** means statistically significant at .05, *** means statistically significant at .01.

Table 5: Expenditure for control households, by business status

	Old business owners (1)	Did not have a business 1 yr ago: High-business propensity (2)	Did not have a business 1 yr ago: Low-business propensity (3)	P value: (1)=(3)	P value: (2)=(3)
Total PCE (Rs/mo)	1,479.56	1,430.31	1,347.56	0.014	0.011
Nondurable PCE) (Rs/mo)	1,335.57	1,336.81	1,237.32	0.006	0.051
Number of control households	979	2,571	1,525		

Note: P-values computed using cluster-robust standard errors. Old business owners are those who own a business started at least 1 year before the survey. High-business propensity households are those (who did not have a business 1 year before the survey) with median or above predicted propensity to start a new business; low-business propensity households are those with below-median propensity who did not have a business 1 year before the survey. New business propensity estimated using spouse's literacy, spouse working for a wage, number of prime-aged women, presence of any teens in household, and land ownership. PCE is per capital expenditure (Rs per month). Nondurable PCE excludes purchases of home and business durable assets.

Table 6: Effects by business status: borrowing and expenditure

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Borrowing		Monthly PCE				Business outcomes		Social index
	Borrows from any MFI	Non-MFI loan age (years)	Durable expenditure	Business durables	Nondurable expenditure	"Temptation goods"	Started new business	Stopped business	
Main effects									
New biz propensity	0.00	-.281**	4.49	-7.58	201.94***	-25.03***	.046**	-0.08	.127***
(no old biz)	(0.03)	(0.13)	(19.68)	(7.62)	(57.56)	(8.10)	(0.02)	(0.11)	(0.039)
Any old biz	.125***	-.309**	50.13**	1.74	202.42***	-10.58	.0395**	-0.15	.158***
	(0.03)	(0.14)	(22.08)	(9.20)	(51.13)	(7.97)	(0.02)	(0.09)	(0.038)
Interaction w treatment									
No old biz	.095**	-0.31	-46.72**	-5.10	213.30**	19.90*	-0.02	0.02	0.065
	(0.05)	(0.20)	(23.10)	(9.33)	(99.12)	(12.06)	(0.02)	(0.16)	(0.057)
New biz propensity	-0.02	0.24	67.40**	7.45	-260.24**	-32.87***	.0424*	0.04	-0.064
	(0.04)	(0.20)	(29.17)	(8.63)	(102.29)	(12.35)	(0.02)	(0.18)	(0.053)
Any old biz	.085*	-0.09	55.42**	18.90**	65.12	-14.71*	0.01	0.00	0.001
	(0.05)	(0.12)	(24.53)	(8.86)	(56.03)	(8.86)	(0.01)	(0.01)	(0.028)
Control mean of LHS var	0.19	0.85	116.17	5.34	1,304.79	83.88	0.05	0.04	-0.001
Control Std Dev	0.39	1.41	332.56	89.52	852.40	130.21	0.25	0.19	0.456
N	5996	6037	6141	6179	6141	6183	6183	2299	6183

Note: New business propensity estimated in treatment using spouse's literacy, spouse working for a wage, number of prime-aged women, indicator for any teens in household, and land ownership (HHs with missing predictors dropped). New business propensity scaled to equal one at 75th percentile. Loan age in column 2 is the average age of a household's loans (i.e., the time since the loans were taken), weighted by the size of the loan principal. "Temptation goods" include alcohol, tobacco, paan, gambling, and food and tea outside the home. Durables include assets for household or business use. Index of social outcomes is an equally-weighted average of z-scores for outcomes including: indicators for women making decisions on food, clothing, health, home purchase and repair, education, durable goods, gold and silver, investment; levels of spending on tuition, fees, and other education expenses; medical expenditure; teenage girls' and teenage boys' school enrolment; and counts of female children under 1 and 1-2 years old. Cluster-robust standard errors in parentheses bootstrapped (200 repetitions) to account for generated regressor; regressions are weighed to account for oversampling of Spandana borrowers. * means statistically significant at .10, ** means statistically significant at .05, *** means statistically significant at .01.

Table 7: Business effects on existing business owners

	OLS		95th quantile regression	Median regression
	(1)	(2)	(3)	(4)
	Profits	Drop businesses with zero inputs or zero income	Drop businesses with zero inputs or zero income	Drop businesses with zero inputs or zero income
Treatment effect	784.967	143.27	2095	80
	[2,561.379]	[2,516.557]	[2,120.626]	[221.443]
Control mean for existing businesses	35.829	1,432.80	95th percentile in treatment is Rs. 14,473	Median in treatment is Rs. 1,768
Control Std Dev	47055.357	27,446.82		
N	2084	1968	1968	1968

Note: Existing businesses are those started at least 1 year prior to the survey. Cluster-robust standard errors in brackets; regressions weighted to account for oversampling of Spandana borrowers. * means statistically significant at .10, ** means statistically significant at .05, *** means statistically significant at .01.

Table 8: Treatment effects on empowerment, health, education

	Women's empowerment: All households				HHs with loans	Health: HHs w/ kids 0-18
	(1)	(2)	(3)	(4)	(5)	(6)
	Woman primary decision-maker	Woman primary decision-maker (non-food spending)	Health expenditure (Rs per capita/mo)	Index of social outcomes	Woman primary decision-maker on loans	Child's major illness
Treatment	0.014	0.024	-2.608	0.008	0.009	0.017
	[0.035]	[0.032]	[12.431]	[0.023]	[0.017]	[0.032]
Control Mean	0.662	0.516	140.253	-0.002	0.281	0.420
Control Std Dev	0.473	0.500	455.740	0.457	0.396	0.659
N	6849	6849	6821	6856	6028	5871

Note: Cluster-robust standard errors in brackets. Decisions in columns 1 and 2 include household spending, investment, savings, and education. Health expenditure (col 3) includes medical and cleaning products spending. Index of social outcomes (col 4) is an equally-weighted average of z-scores for outcomes including: indicators for women making decisions on food, clothing, health, home purchase and repair, education, durable goods, gold and silver, investment; levels of spending on tuition, fees, and other education expenses; medical expenditure; teenage girls' and teenage boys' school enrolment; and counts of female children under 1 and 1-2 years old. Decisions in cols 5 and 6 indicate women being the primary decision-maker in taking out household loans. Child's major illness in col 7 is a child's lillness in the past year on which the household spent more than Rs. 500. Results are weighted to account for oversampling of Spandana borrowers. * means statistically significant at .10, ** means statistically significant at .05, *** means statistically significant at .01.

Table 9: Predicting business propensity

RHS variable: Household opened new business	
Spouse is literate	0.017
	0.014
Spouse works for wage	-0.048***
	0.016
Number prime-aged women	0.009
	0.009
Own land in Hyderabad	0.019
	0.032
Own land in village	-0.018
	0.017
Any teenagers in household	0.025*
	0.014
Constant	0.049***
	0.018
N	2134

Note: Regression estimated using treatment-area households who did not own a business one year prior to the survey. "Spouse" is the wife of the household head, if the head is male, or the household head if female. Teenagers are household members aged 13-18. * means statistically significant at .10, ** means statistically significant at .05, *** means statistically significant at .01.

2.A Appendix: Figures

Figure 1a: No MFI, non-entrepreneur
(A_L or δ_L)

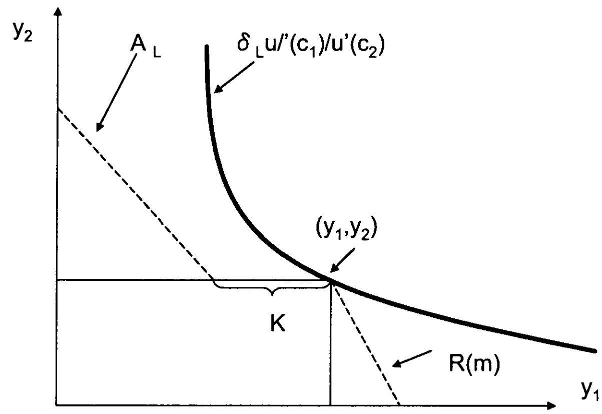


Figure 1b: No MFI, entrepreneur
(A_H and δ_H)

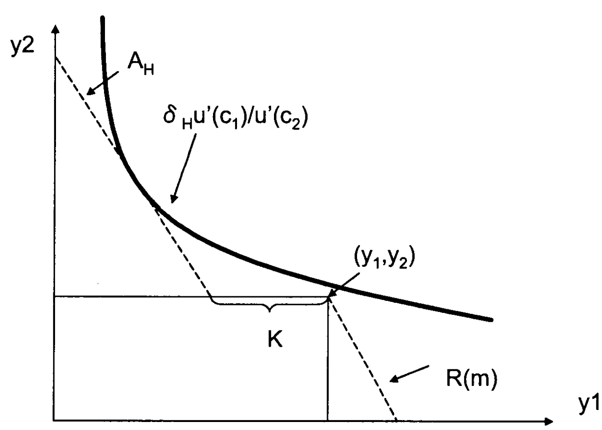


Figure 2: MFI enters:
2 impatient households (no existing business)

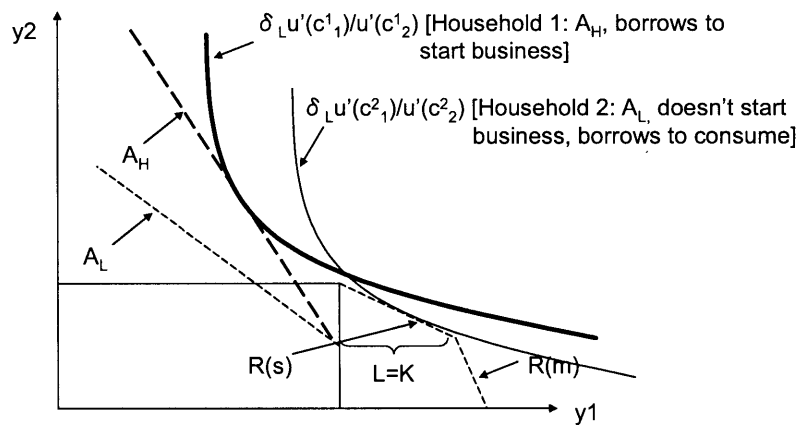


Figure 3: MFI enters:
household w/ existing business
patient, high business propensity (A_H and δ_H)

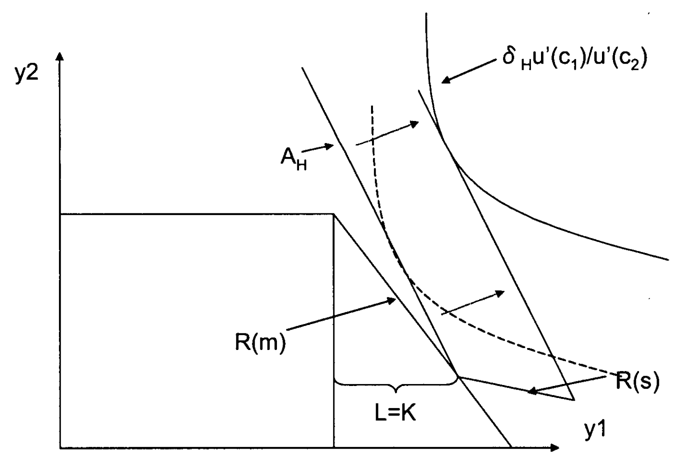
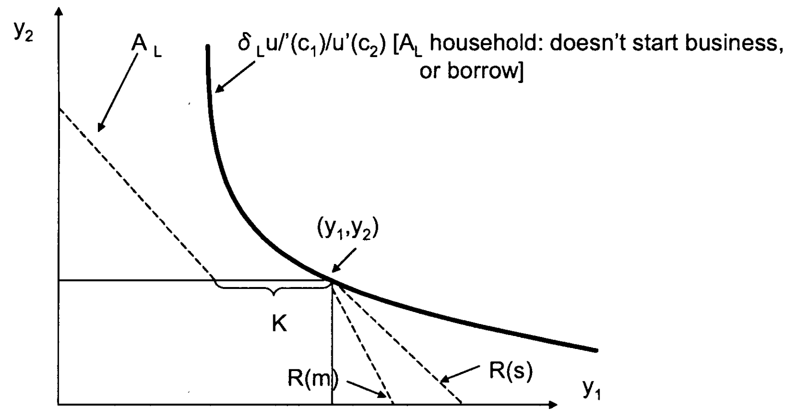


Figure 4: MFI enters: patient, low-business propensity (A_L and δ_H)



Chapter 3

Does Savings Crowd Out Informal Insurance? Evidence from a lab experiment in the field[1]

3.1 Introduction

Village economies have been found to do a surprisingly good job of insuring idiosyncratic risk, as documented by Rosenzweig (1988), Townsend (1994, 1995), Udry (1994), Morduch (1995), Suri (2005) and others. However, households are not completely insured – income and consumption are positively correlated, and serious income shocks like severe illness translate into sharply reduced household consumption (Gertler and Gruber 2002). One proposed explanation for the failure of village economies to achieve full consumption smoothing is the need for insurance relationships to be self-sustaining because households cannot bind themselves to participate in the future ("limited commitment"). The predictions of limited commitment have been found to fit consumption and income data from village economies by Ligon et al. (2002), Dubois et al. (2008), Ligon and Schecter (2009) and others.

Moreover, access to formal savings is low, but growing, in poor countries (Banerjee and Duflo 2007). Therefore, we are interested in analyzing the welfare effect of financial development in poor countries in settings where individuals cannot commit to remain in insurance networks. Furthermore, social networks play an important role in consumption smoothing, as documented by

[1] This chapter is coauthored with Arun Chandrasekhar and Horacio Larreguy.

Fafchamps and Lund (2003), Bloch et al. (2008), Karlan et al. (2008) and Angelucci et. al (2009), among others, so when analyzing the impact of financial development in a limited commitment framework, it is crucial to consider the role of social networks.

Access to savings affects the welfare attained in a limited commitment relationship in two countervailing ways. First, savings provides a technology by which individuals can smooth out risk that is not insured interpersonally. This effect suggests that welfare may increase when savings becomes available. However, since access to savings increases the value of an individual's autarky option, sustaining the insurance relationship becomes more difficult. In turn, informal transfers may be crowded out, thereby reducing the welfare benefit of savings. The interaction of savings and informal insurance suggests a possible negative ramification of financial development: increased access to savings technologies may undermine traditional mutual-insurance arrangements.

Moreover, there are distributional consequences within a village. When inter-household risk sharing is augmented by the ability to smooth risk across time, the average household may be better off, but households that suffer large setbacks may suffer much more than they would under a mutual-insurance-only system (Platteau 2000). It is also theoretically possible that, on average, individuals will be worse off with savings access (Ligon, Thomas, and Worrall 2000). On the other hand, savings access can increase welfare if mutual-insurance arrangements are not undermined by savings, or if informal insurance leaves significant amounts of idiosyncratic risk uninsured, and this risk can be smoothed over time with savings.[2] Given the spread of cellphone banking, microsavings accounts, and other initiatives to increase access to savings in developing countries, understanding how savings interacts with informal insurance is particularly important.

However, there is little empirical evidence addressing the interaction of risk-sharing and savings because this question is difficult, if not impossible, to answer with non-experimental data. Access to savings is likely to be correlated with many other factors which affect the sustainability of informal insurance: communities with banking access may differ from those without in ways that directly affect the sustainability of informal insurance, such as migration opportunities, wealth, or the nature of the income process. Further, even exogenous variation in availability of banks (such as that used by Burgess and Pande 2005) would not be sufficient to answer our question: banks do more than offer household savings (credit, business finance, information, etc.), and savings/credit access allows investment, changing the income process (Giné and Townsend 2004, Dupas and Robinson 2009).

[2]Savings access can also be welfare-enhancing when aggregate risk is important. However we do not consider aggregate risk here, to focus on the interaction of savings and (in)ability to commit to stay in the insurance network.

Therefore, even a field experiment such as Dupas and Robinson (2009) will not isolate the direct effect of savings access on informal insurance from the effect of changing the income process.

We study the interaction of informal insurance with access to savings using a unique field lab experiment in Karnataka, India. In particular, we set out to investigate the effect of savings access (as opposed to an environment where interpersonal transfers are possible but intertemporal storage is not) on welfare, both for the average individual in our experiment and at different quantiles of the experimental income distribution. We are also able to use detailed data on the social linkages between the households in the villages where we conducted our experiment to show how our results interact with the effect of individuals' social ties.

A drawback of analyzing field data on exogenously-formed risk sharing groups, which our approach avoids, is that individuals within a village may have numerous "games" that they are playing outside the game that we are attempting to study, and access to punishment mechanisms other than exclusion from future mutual insurance. Therefore, ideally we would control for the surplus that paired individuals derive from their relationship outside the game we conduct. Our design addresses this in two ways. First, we have information on a wide variety of interactions between individuals (discussed below), which allows us to construct a measure of the social distance between paired individuals; and second, we randomly assign pairings, oversampling the right tail of the social distance distribution. Therefore, pairs' social distance is uncorrelated with other, unobserved characteristics of their relationship.

Finally, we conducted two versions of our experiment in each village to examine whether the post-defection strategy assumed in most theoretical treatments of informal risk sharing, reversion to permanent autarky if promised transfers are not made, is a realistic approximation of the post-defection strategy employed by the individuals in our experiment when this strategy (which we refer to as grim trigger) is not imposed. While some players were randomly assigned to play risk-sharing games in which the grim trigger (GT) post-defection strategy was imposed, others played the same risk-sharing games, but were not constrained to use any particular post-defection strategy. (We will refer to this as the "sequential dictator game" or SDG treatment.) Comparing the experimental outcomes under SDG and GT allows us to examine whether individuals indeed play GT and the interaction of individuals' strategies with the risk-sharing. On a methodological level, it allows us to investigate whether comparative statics and other predictions derived from a model imposing the GT post-defection strategy hold when individuals may choose a different post-defection strategy (both in terms of magnitudes and signs).

We must note that answering this question is not our goal in this paper. We address this methodological point more deeply in the sequel, Chandrasekhar, Kinnan, and Larreguy (2009b). This paper will focus on the questions of whether limited commitment constrains informal insurance, whether savings access crowds out such insurance, and how the interaction of limited commitment and savings access is affected by social ties between risk-sharing partners.

To briefly preview the results presented in this paper, when players are free to choose their own response to defection by their partner, limited commitment binds significantly, and savings does not crowd out informal insurance. Instead, savings access allows individuals to smooth intertemporally some of the income risk that is not insured interpersonally. Limited commitment binds most when players assigned to risk-sharing groups are socially distant, and when one member of the pair gets high income most of the time, while the other gets low income most of the time. Savings access is most beneficial when partners are socially distant. Even for unlucky individuals, savings access is beneficial relative to the limited commitment no savings case—transfers are not increased, but consumption variability is reduced.

The rest of the paper is organized as follows: Section 2 briefly reviews the theory of informal insurance with and without access to an intertemporal technology. Section 3 describes our experimental protocol and data. Section 4 presents the results of our experiment and discusses internal and external validity. Section 5 concludes.

3.2 Insurance without commitment: Theory

The theory of interpersonal consumption insurance without commitment (and without a savings technology) was developed by Kimball (1988), Coate and Ravallion (1993) and Kocherlakota (1996), among others. Ligon et al. (2000) show that access to savings may possibly make the village as a whole better off, by allowing better smoothing of originally uninsured individual and aggregate risk; or worse off, by increasing the temptation of lucky households to walk away. Here we revisit three models—limited commitment without savings, limited commitment with savings which are retained after defection, and limited commitment with savings that are forfeited after defection—to highlight the comparative statics that are predicted by each model of informal insurance, and the comparisons that will allow us to study the interaction of insurance and savings access. We solve these insurance problems from a planning perspective since this allows us to characterize the set of

Pareto optima.[3] This subsection concludes by discussing the effect of imposing an equilibrium on the model characterization.

3.2.1 Setup

Suppose that there are two individuals $i = 1, 2$. Each period $t = 1, 2, ...$ an individual i receives an income $y^i(s) \geq 0$ of a single good, where s is an i.i.d. state of nature drawn from the set $S = \{1, 2\}$. $y^i(s)$ is assumed to follow the process:

$$y^i(s) = \begin{cases} y & \text{if } i = s \\ 0 & \text{otherwise} \end{cases}.$$

The income process is i.i.d. across time, and perfectly negatively correlated ($\rho = -1$) across individuals. This results in an average per period income of $y/2$. In other words, in each period, one individual will earn positive income y while the other individual will earn no income, with each player equally likely to be lucky.

Individuals have a per-period von Neumann-Morgenstern utility of consumption functions $u\left(c^i\right)$, where c^i is the consumption of household i. It is assumed that $c^i \geq 0$. Individuals are assumed to be risk averse, with $u'\left(c^i\right)$, and $u''\left(c^i\right)$ for all $c^i > 0$. Individuals are infinitely lived and discount the future with a common discount factor β.[4]

Individuals may enter into risk sharing agreements. A contract $\tau(.)$ will specify for every date t and for each history of states, $h_t = (s_1, s_2, ..., s_t)$, a transfer $\tau^1(h_t)$ to be made from individual 1 to individual 2, and correspondingly a transfer $\tau^2(h_t)$ to be made from individual 2 to individual 1. For simplicity we then denote $\tau(h_t) \equiv \tau^1(h_t)$, that is, the (positive or negative) transfer that individual 1 makes to individual 2 after history h_t.

Denote $V^i(h_t)$ to be the continuation value of remaining in the insurance agreement, that is, the expected utility of an individual i from a contract from period t onwards, discounted to period t, if history $h_t = (h_{t-1}, s_t)$ occurs up to period t and s_t is already known:

$$V^i(h_t) = u\left(y^i(s_t) - \tau^i(h_t)\right) + \beta \mathbb{E} \sum_{j=t+1}^{\infty} u\left(y^i(s_j) - \tau^i(h_j)\right) \tag{3.1}$$

[3] This will also be the set of decentralizable equilibrium allocations since the conditions of the 2nd welfare theorem are satisfied.

[4] In our experiment the $\beta = \frac{5}{6}$, the chance the game will continue after each period, as explained in Section 3.

In some of the cases we consider below, individuals have access to a savings technology. The gross return on savings is assume to be

$$R = \begin{cases} 1 \text{ when saving is available} \\ 0 \text{ otherwise} \end{cases}.$$

That is, when saving is available, one unit of the consumption good saved today delivers one unit in the next period. Note that the feasible set is given by $\mathbb{R}^{+}$.

In the case that individuals have access to a savings technology, a risk-sharing contract will not only determine transfers $\tau(h_t)$ to be made from individual 1 to individual 2 but also an amount $z^i(h_t)$ that an individual i, for $i = 1, 2$, saves from period t to period $t+1$. For simplicity we then denote as a sharing agreement $(\tau(h_t), z(h_t)) = (\tau^i(h_t), z^i(h_t))$ for $i = 1, 2$.

For the case that individuals have access to a savings technology $V^i(h_t)$ is denoted as

$$V^i(h_t) = u\left(z^i(h_{t-1}) + y^i(s_t) - \tau^i(h_t) - z^i(h_t)\right) + \beta\mathbb{E}\sum_{j=t+1}^{\infty} u\left(z^i(h_{j-1}) + y^i(s_j) - \tau^i(h_j) - z^i(h_j)\right) \tag{3.2}$$

3.2.2 No commitment, no savings

We will now characterize the set of constrained efficient risk-sharing contracts for the case where there is no access to savings. For this characterization we assume that, if either party reneges upon the contract, both individuals consume autarky levels thereafter.[5] In other words, after the violation of a contract, both individuals consume their income in every period.

Denote $V_A^i(s_t)$ to be the expected utility of autarky for an individual i in period t once s_t is already known:

$$V_A^i(s_t) = u\left(y^i(s_t)\right) + \beta\mathbb{E}\sum_{j=t+1}^{\infty} u\left(y_i(s_j)\right)$$

As a risk-sharing contract can be seen as non-cooperative equilibrium of a repeated game, since reversion to autarky is the most severe sub-perfect punishment, this assumption allows us to characterize the most efficient set of non-cooperative sub-perfect equilibria (Abreu (1988)).

The set of efficient risk-sharing contracts for the considered case solves the following dynamic

[5]The "autarky forever after defection" case is used for expositional clarity and because it supports the most on-equilibrium risk-sharing. In our experimental setup, some pairs are constrained to play this strategy, while other pairs are free to choose any post-defection strategy.

programming problem:

$$V_t^1\left(V_t^2(s_t)\right) = \max_{\tau^1(s_t),\left\{V_j^2(s_j)\right\}_{t+1}^{\infty}} \left\{ u\left(y^1(s_t)-\tau^1(s_t)\right)+\beta\mathbb{E}\sum_{j=t+1}^{\infty} V_j^1\left(V_j^2(s_j)\right)\right\} \tag{3.3}$$

s.t.

$$\lambda \; : \; u\left(y^2(s_t)+\tau_t^1(s_t)\right)+\beta\mathbb{E}\sum_{j=t+1}^{\infty} V_j^2(s_j) \geq V_t^2(s_t), \; \forall\, s_t \in S \tag{3.4}$$

$$\beta\phi_j \; : \; V_j^2(s_j) \geq V_A^2(s_j), \; \forall\, j \geq t+1, \; \forall\, s_j \in S \tag{3.5}$$

$$\beta\mu_j \; : \; V_j^1\left(V_j^2(s_j)\right) \geq V_A^2(s_j), \; \forall\, j \geq t+1, \; \forall\, s_j \in S \tag{3.6}$$

$$\psi_1 \; : \; y^1(s_t)-\tau_t^1(s_t) \geq 0, \; \forall\, s_t \in S \tag{3.7}$$

$$\psi_2 \; : \; y^2(s_t)+\tau_t^1(s_t) \geq 0, \; \forall\, s_t \in S \tag{3.8}$$

where we have written $\tau^1(s_t)$ and $V_j^2(s_j)$ instead of $\tau^1(h_t)$ and $V_j^2(h_j)$ because, due to the recursiveness of the problem, all previous history of the efficient risk-sharing contract, is respectively encoded in $V_t^2(s_t)$ and $V_t^2(s_{j-1})$ (Ligon, Thomas, and Worrall 2002).

Due to the strict concavity of $u\left(c^i\right)$ for $c^i > 0$, it follows that $V_t^i(.)$ is also strictly concave for $i = 1, 2$. What is more the set of constraints is convex (this follows from the the concavity of $u(.)$ and the linearity in $V^i(.)$). Consequently, the problem is concave, and the first-order conditions are both necessary and sufficient.

The first-order conditions for this problem are the following:

$$\tau_t^1(s_t) \; : \; \frac{u'\left(y^1(s_t)-\tau_t^1(s_t)\right)}{u'\left(y^2(s_t)+\tau_t^1(s_t)\right)} = \lambda - \frac{\psi_1-\psi_2}{u'\left(y^2(s_t)-\tau(s_t)\right)}, \; \forall\, s_t \in S, \tag{3.9}$$

$$V_j^2 \; : \; -V_j^{1\prime}\left(V_j^2(s_j)\right) = \frac{\lambda+\phi_j}{(1+\mu_j)} \; \forall\, j \geq t+1, \; \forall\, s_j \in S. \tag{3.10}$$

Further, the envelope condition is given by

$$V_t^{1\prime}\left(V_t^2(s_t)\right) = -\lambda, \; \forall\, s_t \in S. \tag{3.11}$$

Ligon et al. (2002) note that a constrained efficient risk-sharing contract can be characterized in terms of the evolution of λ, the multiplier on individual 2's promise-keeping constraint, which from (3.11) measures the rate at which individual 1's expected utility can be traded off once the current state is known against that of individual 2. Once the state of nature for the following period s_j is known, the new value for λ is determined by equation (3.10). Furthermore, λ completely determines the current transfers (3.9) once the state of nature s_t has been realized.

The intuition for this result is the following. For the sake of simplicity assume that the non-negativity constraints never bind, and hence that $\psi_1 = \psi_2 = 0$. Then, we can rewrite (3.9) as

$$\lambda = \frac{u'\left(y^1\left(s_t\right) - \tau\left(s_t\right)\right)}{u'\left(y^2\left(s_t\right) - \tau\left(s_t\right)\right)}$$

The first-best risk sharing contract keeps the ratio of individuals' marginal utilities constant across states and over time. Then, if (3.5) and (3.6) never bind, λ never updates, and hence full insurance can be achieved. Then individuals each consume a constant share of the endowment y where the share is given by the initial value of λ, λ_0. However, if either (3.5) or (3.6) ever bind, λ is no longer constant and hence full insurance is no longer achievable. Because the only player who may be constrained is the player with the high income realization, who would be required to make a transfer to the other under full insurance, binding continuation constraints will cause consumption to be positively correlated with income.

3.2.3 No commitment, private savings

We will now characterize the set of constrained efficient risk-sharing contracts for the case where there savings are available. Again, for the sake of simplicity, we assume that, if either party reneges upon the contract, both individuals consume autarky levels thereafter. An important change with respect to the above case is that, after the violation of a contract, individuals are not constrained to consume their income as now they can make use of the storage technology. What is more for the case analyzed in this section, after the violation of a contract, both individuals keep their previous period savings. This is what we will denote as "private savings."

Then, we denote $V_A^i\left(s_t, z_{t-1}^1\right)$ to be the expected utility of autarky for an individual i in period t with savings z_{t-1}^1 once that s_t is already known:

$$V_A^i\left(s_t, z_{t-1}^1\right) = u\left(z_{t-1}^i + y^i\left(s_t\right) - z_t^i\left(s_t\right)\right) + \beta \mathbb{E} \sum_{j=t+1}^{\infty} u\left(z_{j-1}^i + y^i\left(s_j\right) - z_j^i\left(s_j\right)\right)$$

The set of efficient risk-sharing contracts for the considered case solves the following dynamic programming problem:

$$V_t^1\left(V_t^2\left(s_t, z_{t\text{-}1}^2\right), z_{t\text{-}1}^1\right) = \tag{3.12}$$

$$\max_{\substack{\tau_t^1(s_t), z_{t\text{-}1}^1(s_t)\in\mathbb{R}^+,\\ \left\{V_j^2(s_j, z_{j\text{-}1}^2)\right\}_{t+1}^{\infty}\in\mathbb{R}^+}} \left\{ \begin{array}{c} u\left(z_{t-1}^1 + y^1\left(s_t\right) - \tau_t^1\left(s_t\right) - z_t^1\left(s_t\right)\right) + \\ \beta\mathbb{E}\sum_{j=t+1}^{\infty} u\left(z_{j-1}^1 + y^1\left(s_j\right) \text{-} \tau_j^1\left(h_j\right) \text{-} z_j^1\left(s_j\right)\right) \end{array} \right\}$$

$s.t.$

$$\lambda \ : \ u\left(z_{t-1}^2 + y^2\left(s_t\right) + \tau_t^1\left(s_t\right) \text{-} z_t^2\left(s_t\right)\right) + \beta\mathbb{E}\sum_{j=t+1}^{\infty} V_j^2\left(s_j, z_j^2\right) \geq V_t^2\left(s_t, z_{t-1}^2\right), \ \forall\ s_t \in S \tag{3.13}$$

$$\beta\phi_j \ : \ V_j^2\left(s_j, z_{t-1}^2\right) \geq V_A^2\left(s_j, z_{t-1}^2\right), \ \forall\ j \geq t\ +1, \forall\ s_j \in S \tag{3.14}$$

$$\beta\mu_j \ : \ V_j^1\left(V_j^2\left(s_j, z_{j-1}^2\right)\right) \geq V_A^2\left(s_j, z_{j-1}^2\right), \ \forall\ j \geq t\ +1, \forall\ s_j \in S \tag{3.15}$$

$$\psi_1 \ : \ z_{t-1}^1 + y^1\left(s_t\right) - \tau_t^1\left(s_t\right) - z_t^1\left(s_t\right) \geq 0, \ \forall\ s_t \in S \tag{3.16}$$

$$\psi_2 \ : \ z_{t-1}^2 + y^2\left(s_t\right) + \tau_t^1\left(s_t\right) - z_t^2\left(s_t\right) \geq 0, \ \forall\ s_t \in S \tag{3.17}$$

where as before the problem has been characterized recursively. Note that now the constraint set is not convex and consequently the problem might not be concave. To avoid such issues, lotteries can be used to convexify the problem, as in Ligon et al. (2000).

The qualitative predictions of informal insurance with private savings are similar to those without savings that we characterized above. Therefore, we omit the characterization of (3.12). Our main interest is analyzing the welfare impact of the access to a savings technology. Ligon et al. (2000) note that access to a savings technology has a twofold impact on the optimal constrained efficient risk-sharing contract. On the one hand, access to a savings technology increases the autarky value that individuals enjoy after the violation of a contract. Intuitively, this reduces the amount of interpersonal insurance which can be achieved. On the other hand, if full insurance is not feasible without access to a savings technology, savings can help to smooth over time the risk that cannot be spread interpersonally. Overall, the effect on individuals' risk-sharing and welfare is then ambiguous and depends on the initial level of risk-sharing. In order to see this let us consider two extreme examples. First, let us assume that without savings full risk-sharing is possible. Then, it could be the case that when the possibility of savings is introduced full risk-sharing is no longer possible, and savings access would reduce welfare. Second, let us consider the opposite case where without savings little risk-sharing is achieved. Then, clearly, access to savings allows individuals to smooth intertemporally some amount of risk that they initially could not insure interpersonally.

Therefore, individuals may be better or worse off on average, or equivalently, from an ex ante perspective, before income uncertainty is realized. However, we could also expect distributional effects, i.e. ex post unlucky (lucky) individuals might be better or worse off. Distributional effects might be relevant in terms of policy recommendations.

Additionally, with no aggregate risk and if savings generate no net return (so that $\beta R < 1$), we have the further prediction that if participation constraints do not bind, savings should not be used. Then, the first best involves consuming the entire endowment in each period. Therefore, since our experiment replicates such conditions, any use of savings is direct evidence that participation constraints bind.

3.2.4 No commitment, joint savings

We will now characterize the set of constrained efficient risk-sharing contracts for the case where only joint savings are available. Again, for the sake of simplicity, we assume that, if either party reneges upon the contract, both individuals obtain their autarky utility levels thereafter. An important change with respect to the above case of "private savings" is that, after the violation of a contract, the individual that reneges upon the contract loses her access to savings. This is what we will denote as "joint savings" because it replicates the payoffs of a joint savings account.

Then, as before, denote $V_A^i(s_t)$ to be the expected utility of autarky for an individual i in period t once that s_t is already known:

$$V_A^i(s_t) = u\left(y^i(s_t)\right) + \beta \mathbb{E} \sum_{j=t+1}^{\infty} u\left(y_i(s_j)\right)$$

The set of efficient risk-sharing contracts for the considered case solves the following dynamic programming problem:

$$V_t^1\left(V_t^2\left(s_t, z_{t\text{-}1}^2\right), z_{t\text{-}1}^1\right) = \tag{3.18}$$
$$\max_{\substack{\tau_t^1(s_t), z_{t\text{-}1}^1(s_t)\in\mathbb{R}^+, \\ \left\{V_j^2\left(s_j, z_{j\text{-}1}^2\right)\right\}_{t+1}^{\infty}\in\mathbb{R}^+}} \left\{ \begin{array}{c} u\left(z_{t-1}^1 + y^1(s_t) - \tau_t^1(s_t) - z_t^1(s_t)\right) + \\ \beta\mathbb{E}\sum_{j=t+1}^{\infty} u\left(z_{j-1}^1 + y^1(s_j) \text{-} \tau_j^1(h_j) \text{-} z_j^1(s_j)\right) \end{array} \right\}$$
$s.t.$

$$\lambda \;:\; u\left(z_{t-1}^{2}+y^{2}\left(s_{t}\right)+\tau_{t}^{1}\left(s_{t}\right)-z_{t}^{2}\left(s_{t}\right)\right)+\beta \mathbb{E} \sum_{j=t+1}^{\infty} V_{j}^{2}\left(s_{j}, z_{j}^{2}\right) \geq V_{t}^{2}\left(s_{t}, z_{t-1}^{2}\right), \forall\, s_{t} \in S \quad (3.19)$$

$$\beta \phi_{j} \;:\; V_{j}^{2}\left(s_{j}, z_{t-1}^{2}\right) \geq V_{A}^{2}\left(s_{j}\right), \forall\, j \geq t+1, \forall\, s_{j} \in S \quad (3.20)$$

$$\beta \mu_{j} \;:\; V_{j}^{1}\left(V_{j}^{2}\left(s_{j}, z_{j-1}^{2}\right)\right) \geq V_{A}^{2}\left(s_{j}\right), \forall\, j \geq t+1, \forall\, s_{j} \in S \quad (3.21)$$

$$\psi_{1} \;:\; z_{t-1}^{1}+y^{1}\left(s_{t}\right)-\tau_{t}^{1}\left(s_{t}\right)-z_{t}^{1}\left(s_{t}\right) \geq 0, \forall\, s_{t} \in S \quad (3.22)$$

$$\psi_{2} \;:\; z_{t-1}^{2}+y^{2}\left(s_{t}\right)+\tau_{t}^{1}\left(s_{t}\right)-z_{t}^{2}\left(s_{t}\right) \geq 0, \forall\, s_{t} \in S \quad (3.23)$$

where as before the problem has been characterized recursively. The only difference between (3.18) and (3.12) is that we have replaced $V_A^i\left(s_t, z_{t-1}^i\right)$ by $V_A^i\left(s_t\right)$.

All above mentioned problems and their solutions for the characterization of (3.12) also apply to (3.18). Further, the qualitative predictions of informal insurance with joint savings are similar to those with private savings that we characterized above. Therefore, we omit the characterization of (3.18).

Our main interest of this section is analyzing the differential welfare impact of access to a joint savings technology versus access to a private savings technology. Ligon et al. (2000) note that there is an important difference between the two. In contrast to private savings, where individuals retain access to savings in the event that they default on their insurance obligations, joint savings, which are forfeited in the event of default, should unambiguously increase welfare. The reason is that joint savings, like private savings, allow intertemporal smoothing of risk that cannot be insured interpersonally, but, unlike private savings, do not tighten participation constraints.

3.3 Tests

The theoretical results for the three models presented above, together with the result that in a risk-sharing model with full commitment and no savings, individuals achieve full risk-sharing, allow us to test whether these models predict players' behavior in our experimental setting. Furthermore, our experimental setting will allow us to analyze the theoretically ambiguous welfare implications of introducing private savings when insurance in constrained by limited commitment. For this analysis we can compare the use of transfers and savings, and the degree of consumption smoothing, across the experimental settings which replicate, respectively, full commitment, limited commitment without savings, limited commitment with private savings, and limited commitment with joint savings. The comparison of the use of transfers and savings across different treatments is motivated directly by the predictions of the different models. On the other hand, the models are silent on the degree of consumption smoothing *per se* but have predictions in terms of individual welfare. However, due to the fact that our experimental setup keeps expected individual consumption constant across models, consumption smoothing can be used as a measure of individual welfare, a point we discuss further below.

Hence, to test the validity of the models as a description of experimental subjects' behavior, we can check whether the following comparative statics hold: When comparing the Full Commitment No Savings (FCNS) treatment to the vs. Limited Commitment No Savings (LCNS) treatment, we should see lower transfers and less consumption smoothing if participation constraints bind.

When comparing LCNS and Limited Commitment with Private Savings (LCPS), the comparison is theoretically ambiguous. If limited commitment (participation) constraints were binding in the LCNS treatment, when savings were not available, then access to savings will tighten participation constraints, since the defection continuation value in (3.6) and (3.5) is higher than that in (3.15) and (3.14) but, due to the absence of aggregate risk, access to savings does not increase the total amount of possible consumption smoothing, and hence the cooperation continuation value is no higher. In this case, interpersonal transfers will be reduced ("crowded out"). However, the impact on aggregate consumption smoothing is ambiguous. If tightening of participation constraints reduces interpersonal insurance by more than savings access increases intertemporal smoothing, aggregate consumption smoothing will worsen and the variance of consumption will increase. On the other hand, if interpersonal insurance is reduced by less than intertemporal smoothing is increased, the variance of consumption will decrease, reflecting improved aggregate consumption smoothing. If participation constraints were non-binding when savings were not available, and continue not to bind when savings is available, the savings technology should not be used (since there is no aggregate risk and $\beta R < 1$), and the variance of consumption would remain unchanged. Empirically estimating which of these effects dominates, tightening of participation constraints or smoothing of uninsured risk, is one of the key questions of this paper.

When comparing LCPS versus Limited Commitment with Joint Savings (LCJS), the predictions are more clear-cut. If pairs were fully insuring each other under private savings (LCPS), because participation constraints were not binding, then we would expect to see no change in the variance of consumption under LCJS, because the move from private savings to joint savings further relaxes the (already non-binding) participation constraints. It could be that in this case, the variance of consumption would be zero—because pairs achieve full, first-best insurance. However, if there is a psychic cost to making interpersonal transfers (owing to the contemplation cost of calculating the appropriate transfer, an endowment effect which makes it unpleasant to surrender money one has won, etc.), then there may be less-than-full insurance even when participation constraints *per se* do not bind. We are able to estimate the extent of such psychic costs of engaging in full risk sharing using the Full Commitment, No Savings (FCNS) case. In the case that we see positive variance of consumption under FCNS, if we see no change in the variance of consumption when moving from LCPS to LCJS, it suggests that participation constraints *per se* were not binding under LCPS.

In the case that participation constraints were binding under private savings, then the move to joint savings should relax participation constraints. In this case, we would see increased consumption smoothing and increased transfers. We would expect weakly decreasing use of savings, unless the players use savings as a form of "bond-posting," wherein they put money in savings as a way to commit not to defect, since the more money they have in savings, the higher the penalty for defection.

An important point is that these comparisons were derived assuming that individuals are on the Pareto frontier (albeit possibly a frontier that reflects costs to risk-sharing other than participation constraints). However, we consider these comparisons to be a natural starting point for our analysis even if individuals are not on the Pareto frontier. What is more, even if individuals are not on the Pareto frontier, while we would then not be able to neatly map our empirical findings into statements about quantities in a limited commitment problem such as the magnitudes of Lagrange multipliers on particular constraints, the comparison of the LCNS treatment versus the LCPS treatment will still help us to address the empirical question that this paper proposes: namely, do individuals achieve better overall consumption smoothing with or without access to savings? However, we will argue that the comparative statics we observe are surprisingly consistent with the hypothesis that individuals are on the Pareto frontier, subject to a psychic cost of engaging in full risk sharing (i.e., a cost not derived from the participation constraints of the limited commitment model) .

3.3.1 Equilibrium selection

Models of limited commitment-constrained insurance typically assume that individuals play an equilibrium where they use grim trigger (GT) strategies of "autarky forever after defection". As shown by Abreu (1988), in the absence of direct punishments for defection, shutting down interpersonal trade permanently is the worst possible subgame-perfect punishment, and as such, its use as an off-equilibrium punishment sustains the maximum degree of on-equilibrium-path cooperation. However, individuals may not actually use grim trigger strategies. One reason why they might not use them is the fact that grim trigger strategies are not renegotiation proof. That is, once someone has defected from a risk-sharing contract, his or her partner has incentives not to implement the grim trigger strategy but to renegotiate their risk-sharing contract. The reason is that the use of a grim trigger strategy does not only punish the partner but also the individual who is punishing. Farrel and Maskin (1989) propose a weakly renegotiation proof equilibrium concept. In such an equilibrium, the only feasible punishments are those located on the Pareto frontier. They essentially show that renegotiation proofness limits the scope of the payoffs that can be sustained. Ligon et al. (2000) show that allowing for less-extreme responses to defection does not fundamentally change the shape of the frontier of efficient allocations, although it must weakly reduce the scope for risk-sharing by Abreu's argument.

Moreover, this leaves open the empirical question of what type of post-defection strategies individuals actually use, and what consequences they have for consumption smoothing. To examine what post-defection strategies arise naturally and how consumption smoothing is affected in consequence, some individuals playing our risk-sharing games are not restricted in the way they respond to defection. We call this the sequential dictator game (SDG) treatment, because in each round the lucky individual is essentially playing a dictator game–deciding how much to offer the other player.

To examine the effect of imposing a grim trigger post-defection strategy, we compare the degree of consumption smoothing achieved when we impose this strategy to the degree of consumption smoothing achieved when we do not impose a particular post-defection strategy. If consumption smoothing is worse when we do not impose a particular strategy, this suggests that empirical risk sharing is limited by elements that are not captured in models that impose an equilibrium where individuals use grim trigger strategies after defection.

3.4 Experimental Setup

3.4.1 Setting

To understand how savings access interacts with interpersonal risk sharing ("crowdout"), how punishment strategies affect the nature of risk sharing, and how crowdout and the choice of response to defection are affected by individuals' social ties, we conducted a field lab experiment designed to mimic as closely as possible the risk-sharing opportunities and constraints individuals face in their lives. However, we deliberately shut down certain barriers to trade, such as moral hazard, asymmetric information, and endogenous group formation, in order to understand how participation constraints are affected by savings access and by imposing particular post-defection strategies.

Our experiment was conducted in a total of 34 villages in Karnataka, India. The villages range from 1.5 to 3 hours' drive from Bangalore. South India was chosen as the setting for our experiment because rural, periurban villages in South India have historically been characterized by a high degree of interpersonal risk-sharing, as demonstrated by Townsend (1994) and others for the ICRISAT villages, and because rural South India is currently experiencing rapid growth in the availability of savings, but from a low base (Banerjee and Duflo 2007). These particular villages were chosen because village censuses and social network data were previously collected in these villages, giving us uniquely detailed data, not just on our experimental participants and their direct connections to their partners, but also on indirect linkages between partners, e.g. through mutual friends.

In each village, 40 individuals, aged 18 to 50, were recruited to take part in the experiment. In total, 1,358 individuals and 4,251 pairs participated in the experiment. (Each individual assigned to SDG played 3 games, each with a different partner, and each individual assigned to GT played 4 games, as we explain below.) The average age was 30, 56% of players were female, and the average education was 7th standard. Over 97% of pairs in our sample could reach each other through the social network and the average social distance was 3.5; the median was 4, meaning that the members of a median pair were "friends of a friend of a friend of a friend."

We randomly assigned 20 of the individuals to GT and 20 to SDG. Table 0, Panels a to c, show summary statistics for the 1,358 individuals and 4,251 pairs that participated in the experiment. Groups are well-balanced in terms of demographic and network characteristics. Pairs in the GT group had slightly lower social distance on average because these individuals are paired 4 times while SDG individuals are paired 3 times, so some less-distant pairs are used. We control for network covariates in what follows.

Based on the village census and network data, individuals were assigned a partner. Our randomization was unique in that it stratified against the social network. We computed the social distance between each pair of individuals and then plotted the distance distribution. Since most social networks exhibit small-world phenomena, even if a random subset of villagers took part in our experiments, randomly chosen *pairs* would tend to be fairly close in social distance. This tendency would be exaggerated if people tend to come to the experiment with their friends or relatives, which was the case for many people who took part in our experiment. Therefore, the distribution of social distances will be right-skewed, and simply randomly assigning partners would mean that more often than not, people would be paired with near-kin. To make the distribution of social distances between our pairs more uniform in our sample, we used the network data by oversampling the right tail of the distance distribution. Figure 1 shows the distribution of social distances for 3 villages; both the actual distribution and the distribution of assigning pairings in the experiment. The pairings used in the experiment have more mass at greater distances, particularly distances of 5 and 6.

3.4.2 Overall game structure

The purpose of our games is to replicate the incentives to share income risk that exist in real life, but to do so in a way that can be implemented in an experimental session lasting a few hours. For external validity, individuals should have strong incentives to smooth risk (we explain what that risk is below) and to think carefully about their choices.

Empirical consumption-smoothing has both intertemporal and interpersonal components. We create an interpersonal component by pairing individuals into groups of two. In all games, the members of a pair can make transfers between them. To simulate the intertemporal smoothing motive, individuals play many rounds during the experiment (18 rounds on average for SDG and 24 for GT, as explained below), but are paid their "consumption" for one randomly-selected round. To make this salient, income is represented by tokens that represent Rs 10[6] each, and each consumption realization is written on a chip and placed in a bag that the player keeps with him or her during the entire experiment. At the end of the experiment, a field staff member draws one chip from the bag, and the individual is paid that amount.

Incomes are risky: as in our theoretical setup, there is a high income level (Rs 250), and a low income level (Rs 0). Moreover, to simulate past wealth, which is not equal across individuals, in round 1 of each game, one partner is randomly given an endowment of Rs 60 and the other is given randomly Rs 30. The games are described in the context of a farmer who may receive high income in a round because of good rains this season or low income in a round because of drought. (An excerpt of the experimental protocol, translated into English, appears in Appendix D.) Discussions

[6]Rs 10 is approximately \$0.20 at market exchange rates, or \$1 at PPP-adjusted exchange rates.

with participants indicate that they understand the risk they face and the data show that both transfers and savings are used to smooth this risk.

To replicate an interaction that may extend indefinitely into the future, induce discounting and avoid a known terminal round, the game ends with $\frac{1}{6}$ probability at the end of each period. Therefore at any point when they game has not ended, it is expected to continue for 6 more rounds. Once a game ends, individuals are repaired. The order of the games is randomized, and we control for game order in our regressions.

The options players have to decouple consumption from income vary by game, and also by treatment (SDG vs. GT). However, in all treatments, at the beginning of each round before incomes are realized (but after the endowment is realized in round 1), partners may decide on an income sharing agreement. That is, partner 1 chooses how much 1 will give 2, if 1 gets Rs 250 and 2 gets 0 (τ_t^1), and 1 chooses how much 2 will give 1, if 2 gets Rs 250 and 1 gets 0 (τ_t^2). This agreement may be asymmetric ($\tau_t^1 \neq \tau_t^2$) and time-varying ($\tau_t^1 \neq \tau_{t'}^1$).

The details of each treatment are as follows:

1. **Full commitment, no savings:** each player tells the experimenter their choice of transfer they will make if they get high income, τ_t^i. Once incomes are realized, the experimenter implements the transfer that the lucky player agreed to ex ante. (There is no opportunity for the lucky player to change her mind.) Each individual then "consumes" by placing all of their tokens, net of any transfers, into a consumption cup. The experimenter removes the tokens, writes the amount on a chip, and the chip is placed in the consumption bag. The game is the same in the GT and SDG treatments.

2. **Limited commitment, no savings:** Partners may agree on an income sharing rule as before. However, after seeing their income, the lucky individual may change her mind and transfer a different amount (including transferring nothing). Each individual then consumes by placing all of their tokens, net of any transfers, into the consumption cup. The experimenter takes the tokens, writes the amount on a chip, and the chip is placed in the consumption bag. In the GT treatment, if either opts out of the transfer agreement, each partner consumes her income in that period and in all remaining periods. In the SDG treatment, players are free to continue making transfers after defection, or not.

3. **Limited commitment, private savings:** as in (2), each individual may renege on their promised transfer after seeing their income. Further, each has access to a "savings cup." Once

transfers are made, players can consume tokens by placing them in the consumption cup, or save them by placing them in the savings cup. Saved tokens are available to consume in later rounds, but are lost when the game ends. If an individual reneges on transfers she promised to make, she keeps her savings. In the GT treatment, if either opts out, each can use savings in that period and in all remaining periods, but no further transfers are allowed. In the SDG treatment, players are free to continue making transfers after a defection, or not.

4. **Limited commitment, joint savings:** this game is similar to (3), except that if an individual reneges on transfers she promised to make, her savings will go to the other partner. Thereafter, the defecting partner may not save, but the other partner may continue to use savings. This game is only played in the GT treatment group. Ideally, in the SDG group, players would have been able to *choose* whether, as a pair, they wanted to commit to forfeit their savings in the event of not making their agreed transfers, as they could choose how to punish one another after defection. However, this proved difficult to implement and so the LCJS treatment was only administered in the GT group.

In thinking about the external validity of the findings of this experiment, two points are worth noting. First, the amounts of money involved are substantial. Average expected earnings in the experiment are Rs 130. To put this into perspective, the NREGA (National Rural Employment Guarantee Act) has a wage rate of about Rs 60 for a day's work and the prevailing wage rate in southern Karnataka is about Rs 80 for a day's work.

Second, great care was taken in designing the physicality of the games (consumption bags, income tokens, consumption and savings cups, etc.) and the framing with which we presented them, in order to make them both easy to understand and similar to real life. In explaining the games to the participants, it was explained that the games that they play are much like the decisions they take in every day life. In each round they receive some income and (depending on the game) they are able to make decisions to consume, save for the future, or transfer money to their partner. Many players spontaneously noted the parallels between the games and real-life decisions.[7]

While individuals were registered for the experiment and matched to the social networks data (explained below), we administered a short questionnaire to measure risk aversion, time preference,

[7] One player told us "The games were not boring... They were very interesting, especially for those who have some education... They help us think about how much we really should save and give to our friends in times of hardship." Furthermore, in two villages, after the experiment village leaders inquired about the possibility of having an MFI come to their village, because they saw links between the games and the possibility to have actual savings.

and hyperbolic discounting. We also collected information on education and financial decision-making in the household.[8]

3.4.3 Network data

We make use of a unique dataset containing information on all 34 villages in which our experiment was conducted. We have complete censuses of each of the villages as well as detailed social network data. With the network data, we are able to examine two conceptually distinct concerns with two different types of network statistics. First, we can use vertex level measures of popularity and importance as coarse controls for the relative bargaining power between partners. So long as we can think of bargaining power as, in part, being a function of an individual's prominence in a village, we are able to partial the effect out with such controls. Second, we can use the geodesic distance (shortest path length) between partners to make a methodological point. In lab experiments in the field, one runs the risk of having partners whose relationship extends beyond the game at hand, thereby threatening internal and external validity. We can use the geodesic distance in order to measure such effects and control for them. Moreover, because we created our sample by stratifying against the geodesic distance distribution by over-sampling the right tail, we can use the subset of the data which pairs virtual strangers to see whether our findings are robust. In this manner, we are able to bolster both our internal and external validity.

The network data was collected for Banerjee et al. (2009) in which they conducted a survey about social linkages for a random subset of the population. For a village, the network graph (or multi-graph), represents individuals as nodes with thirteen dimensions of possible links between pairs of vertices. These dimensions include relatives, friends, creditors, debtors, advisors, and co-workers. For our purposes, we work with an undirected, unweighted graph which takes the union of these dimensions. As explained in Banerjee et al. (2009), for these villages, the union graph is the right object of study. For these villages, the multiple dimensions are highly correlated so the union network allows the researchers to capture latent information. Moreover, any weighting method would be rather ad hoc in nature. Henceforth, we will simply refer to this object as the social network of the village.

We include degree, betweenness centrality, eigenvector centrality, and path length in our regression analyses. We provide a more technical description in Appendix C. Degree, the number of

[8] The risk aversion, time preference and financial decision-making data will be incorporated into our analysis once data entry is complete.

edges that a vertex has, is a measure of popularity. Betweenness centrality is a measure of how crucial an individual is in terms of conveying information. To compute the betweenness centrality of individual i, we first find all the geodesics (shortest paths) between all other individuals in the network and then count how many of those pass through vertex i. Eigenvector centrality allows us to rate the importance of a vertex as a function of the importance of a vertex's neighbors. The eigenvector centrality assigns to vertex i the i^{th} entry of the eigenvector corresponding to the maximal eigenvalue of the adjacency matrix. The centrality measure is recursive in the sense that a vertex's eigenvector centrality is higher if it is connected to higher eigenvector centrality nodes. Solving this recursive system produces a measure that corresponds to the eigenvector associated with the maximal eigenvalue. This also has a natural interpretation from an operator perspective which is one way to understand the adjacency matrix.

This data allows us to address two issues. First, by controlling for degree, betweenness centrality, and eigenvector centrality, we are able to partial out whatever effect that network importance may have in the experiment. We interpret these as coarse controls for the relative Pareto weights between partners. That is, insofar as Pareto weights may be a function of an individual's popularity in a village, we can control for it through node-level importance metrics.

Second, we can address a problem that would be faced by similar experiments if the researchers lacked the social network data. One might imagine that lab experiments in the field face severe internal validity problems if the games spillover into real life. That is, if two individuals treat the game as a subgame of their real life interactions, then decisions taken within the game may be polluted by their real world relationships. To address such a concern, ideally one would pair individuals with no prior nor future contact. Of course, this is impossible to do in a village. Our network data enables us to measure precisely such an effect. The distance between a pair is given by the geodesic or the length of the shortest path between the two vertices. We use this to measure the potential out-of-game spillover effect that may occur due to closely connected people playing together in the experiment. By creating a near uniform distribution of the path length between pairs, we are able to actually compute and partial out the effect due to distance. We can interpret this as a measure of internal invalidity due to out-of-game spillovers. We are also able to study our results on a support of pairs with infinite or near-infinite social distance (i.e. no path or very long geodesics from i to j). Because our findings are robust to such perturbations, it is safe to say that we do not have problems of internal validity.

In controlling for network effects, we use degree, betweenness centrality, eigenvector centrality, and path length as well as their interaction with whether or not an individual was administered the follow-up network survey module. The interaction of these factors with whether or not the individual was surveyed is an essential term in our analysis because individuals who were not randomly surveyed mechanically are assigned different degree, betweenness centrality, eigenvector centrality, and geodesic distance. Because of the random sampling of networks induce non-classical measurement error (see Chandrasekhar and Lewis (2009)), we control for whether or not an individual was surveyed.

3.4.4 Methodology and Network Effects

Distance between partners enters significantly in most regressions that we have presented, even though we have taken care to oversample the right-tail of the path length distribution. The regularities that emerge are that individuals transfer more on average to partners whom they can reach through the network, and that conditional on being able to reach their partner through the network, the amount that they transfer to their partner is decreasing in social distance, as discussed below. Moreover, an individual's consumption is more variable if her partner is not reachable and the variability of consumption is increasing in social distance.

This is unsurprising since individuals who are socially close may have numerous "games" that they are playing outside the game that we are attempting to study. Therefore, the gold standard for analysis of risk-sharing games in a village would be to perfectly control for the surplus that paired individuals derive from their relationship outside the game we conduct. We feel that our design approaches this ideal in two ways. First, we have information on a wide variety of interactions between individuals (discussed above), which allows us to construct a measure of distance, and second we have randomly assigned pairings, oversampling the right tail of the social distance distribution. To that end, a natural question to ask is whether our conclusions would have changed had we not used such procedures. That is, we want to see if researchers repeat our experiment in villages without having access to network data and simply use 40 randomly chosen individuals, how would their conclusions change?

Though one natural approach would be to undo our oversampling of the tail and reweight observations, we refrain from this because even the oversampling was not sufficient to get a large enough distributional shift to make this approach meaningful. However, the oversampling enabled us to efficiently estimate a subset of the data in which we restrict ourselves to looking at partners who were far from each other ($\gamma_{ij} \geq 4$, where 4 is the median distance in our sample). Though reweighting requires a sufficiently significant change in the path length distribution for power, the power in a split-sample regression depends on the number of high distance pairs, and not the share. Therefore, by simply sampling ψ more high distance pairs, we can converge at a $\sqrt{(1+\psi)T}$ rate instead of a $\sqrt{T}$ rate, where T is the number of observations in the tail (individual-game-round or pair-game-round).

Motivated by this argument, we split the sample and study our results for the cases of high distance pairings, $\gamma_{ij} \geq 4$, and low distance pairings, $\gamma_{ij} < 4$; we discuss the results below.

It is worth emphasizing that, because we have exogenous variation in social distance, our results are informative for studying how limited commitment relationships and the insurance that they can support are affected by social distance. This would not be possible with data on endogenously-formed risk-sharing groups, where social distance will be an omitted variable. With our data, because we construct and randomly assign a measure of social distance, which will therefore be uncorrelated with other, omitted components of the true value of a pair's relationship outside the game, we obtain consistent estimates of the effect of social distance in changing how limited commitment binds.

Now, we turn to the results of the experiment.

3.5 Results

Our experiment was designed so that many of our hypotheses of interest can be answered by simple comparisons of the mean of a particular outcome across games. We want to measure the effect of different treatments on the magnitude of interpersonal insurance, and on welfare. Before presenting our results, we discuss how we measure these quantities.

3.5.1 Measuring the degree of insurance

To examine the magnitude of interpersonal insurance, we examine average transfers made by individuals with high income realizations to those with low income realizations. This gives us a measure of the amount of interpersonal risk-sharing which does not rely on knowing the relative bargaining power or Pareto weights.

To see this, note that if players 1 and 2 fully insure their idiosyncratic risk, and 1 has a Pareto weight/bargaining power factor of λ, 1 transfers an amount

$$\tau^1_{FI} = (1 - \lambda)\, 250$$

to 2 when 1 is lucky, and 2 transfers an amount

$$\tau^2_{FI} = \lambda 250$$

to 1 when 2 is lucky. Since each player is lucky 50% of the time on average, average transfers will be

$$.5\tau^A_{FI} + .5\tau^2_{FI} = .5\,(\lambda + 1 - \lambda)\, 250 = 125$$

regardless of λ.

Similarly, if players 1 and 2 insure fraction α of their idiosyncratic risk, $\tau^1_\alpha = \alpha\,(1 - \lambda)\, 250$ and $\tau^2_\alpha = \alpha\lambda 250$, and average transfers will be

$$.5\tau^1_\alpha + .5\tau^2_\alpha = \alpha 125$$

Even if transfers change over the course of the game in response to binding participation constraints, as we expect to happen in a limited commitment setting, average transfers will be $\alpha 125$, where α is the fraction of risk that is insured, averaging across rounds. Note that the independence of average transfers and bargaining weights relies on the fact that the income process is independent of bargaining weights. This holds in our setting because each player has a 50% chance of being lucky or unlucky in each round. However, in non-experimental data, bargaining weights would typically be correlated with the individuals' income processes, and it would not be possible to map average transfers into the degree of insurance without knowledge of bargaining weights. Therefore, we can interpret changes in transfers when moving from full commitment (the LCNS game) to limited commitment without savings (LCNS) as the change in interpersonal insurance due to binding participation constraints; and we can interpret changes in transfers when moving from limited commitment without savings (LCNS) to limited commitment with private savings (LCPS) as the change in interpersonal insurance due to savings access affecting participation constraints.

3.5.2 Measuring welfare

Examining transfers as an outcome tells us about the degree of interpersonal insurance. However, we are also interested in the questions: Is welfare higher (or lower) with savings access than without (and by how much)? How much do binding participation constraints reduce welfare, relative to the full commitment case?

In general, the effect of different treatments on welfare would be comprised of an effect of the level of consumption, and an effect on the variability of consumption. However, because the income process was fixed across treatments, there will mechanically be no difference in average consumption between the full commitment (FCNS) and limited commitment (LCNS) games. Table 0d shows that this is indeed the case–average consumption is Rs.132 in both games.[9] Because savings are lost when the savings games end, consumption is slightly lower in the games with savings: between Rs 2 and Rs 3 lower, depending on the game and treatment. With the caveat that average consumption is slightly lower in the savings games, we will think of the variability of consumption as a measure of relative welfare. While making cardinal comparisons (e.g., utility under full commitment is $x\%$ higher than under limited commitment) would require assumptions about the form of players' utility functions, as long as players are risk averse–and their use of smoothing mechanisms shows that they are–more variable consumption implies lower expected utility, holding expected consumption constant.

3.5.3 Regression specifications

Our main estimation specification take the following form for outcomes defined at the individual-by-game-by-round level:

$$\omega_{igr} = \alpha + \beta_g + X_g'\gamma + \delta_i + Z_{ig}'\phi + \varepsilon_{igr}$$

where ω_{igr} is the outcome for i in game g, round r; β_g is a game indicator (commitment, no commitment without savings, etc.); X_g includes characteristics of the game (order-of-play and surveyor effects). δ_i is an individual-fixed effect,[10] and Z_{ig} includes an indicator for whether i and

[9] Consumption is higher in round 1 of each game, where players receive Rs 30 or Rs 60 as an initial endowment. Because there were random variations in how long each game lasted, consumption is an insignificant Rs .31 higher in the LCNS game than in FCNS in the SDG treatment, and an insignificant Rs .11 higher in the LCNS game in the grim trigger treatment.

[10] We have also omitted individual-fixed effects and controlled for characteristics of the individual (degree, betweenness, eigenvector centrality, wealth, etc.) Individual characteristics enter with the expected signs and do not change

i's partner in game g are connected in the village social network, and, if connected, the distance between i and i's partner. Outcomes defined at the individual-by-game-by-round level are absolute deviations of consumption from the overall average for that game, $|c_{igr} - \bar{c}_g|$, consumption squared deviations $|c_{igr} - \bar{c}_g|^2$, and savings s_{igr}.

We also examine consumption variances $var(c_{ig}) \equiv \frac{1}{N_g}\sum_{r=1}^{N_g}(c_{igr} - \overline{c_{ig}})^2$ and standard deviations $var(c_{ig})^{.5}$, which are defined at the individual-by-game level. The specification takes the following form:

$$var(c_{ig}) = \alpha + \beta_g + X_g'\gamma + \delta_i + Z_{ig}'\phi + \varepsilon_{ig}$$

Some outcomes, namely defection (that is, the individual with the high income realization transferring a different amount than stated, including zero)[11] and the magnitude of realized transfers τ_{pgr}, are defined at the pair-by-game-by-round level, since there is only one transfer and one defection decision per pair per round, namely the decision made by the lucky individual. Then we run specifications of the following form:

$$\omega_{pgr} = \alpha + \beta_g + X_g'\gamma + W_{pg}'\zeta + \varepsilon_{pgr}$$

where ω_{pgr} is the outcome for pair p in game g, round r; β_g is a game indicator; X_p includes characteristics of the game (order-of-play and village effects). W_{pg}' includes characteristics of the individuals in the pair (reachability and distance).

The estimation errors (the ε) in our regressions may be correlated across individuals or pairs within a given game in a particular village, due, for instance, to slight idiosyncrasies of game explanation, disruptions in the experiment venue, etc. Therefore all regression standard errors are clustered at the game-times-village level.

3.5.4 Use of smoothing mechanisms

Because we want to use the results of our experiment to study how interpersonal and intertemporal consumption smoothing interact, we need to show that the players understand and are willing to use interpersonal transfers, and, when available, savings. Table 0d shows average transfers and savings by game. Overall transfers are approximately Rs 93 in the full-commitment treatment, 70%

the between-game comparisons we find in the individual-fixed effect regressions. (Results available on request.)

[11] Below we distinguish between "downward defection," i.e. transferring less than promised and "upward defection," i.e. transferring *more* than promised, although both constitute defection in the GT treatment.

of the Rs 131 that would be associated with full insurance. (As noted above, even if one individual always consumed more than the other due to a higher bargaining weight, average transfers would still equal half of aggregate income, or Rs 170 in the first round and Rs 125 in all other rounds. Our games have 7 rounds on average.)

Table 1 shows that average savings levels in the private savings game are Rs 22.6 in the SDG treatment, and Rs 20.1 in the GT treatment. In the joint savings game (GT treatment), average savings are Rs 23.8.

Significant levels of transfers in savings and non-savings treatments, and use of savings when savings are available, suggest that meaningful consumption smoothing is occurring. Figure 2 shows consumption, income and transfers for a typical individual in the "no commitment without savings" game (SDG treatment). Consumption is noticeably smoother than income, due to the use of transfers (defined as positive when she has high income, and negative when she has low income). Figure 3 shows consumption, income and savings for a typical individual in the "no commitment with private savings" game (SDG treatment). Again, consumption is noticeably smoother than income, now due to the use of savings as well as transfers.

Recall, too, that use of savings in our experiment (with $\beta R < 1$ and no aggregate risk) is direct evidence that participation constraints are limiting interpersonal risk-sharing.

3.5.5 Does limited commitment bind?

An implication of binding participation constraints is that transfers are reduced when individuals cannot commit, relative to when they can. Table 1, presents the results of regression-adjusted comparisons of levels of transfers, by game. The first two columns show results for the SDG treatment. The first column shows results for all rounds in SDG games. Transfers are significantly lower in the two no-commitment treatments. Relative to the full commitment treatment, transfers are Rs 9 (10%) lower with limited commitment-no savings, and Rs 11 (12%) lower with no commitment and private savings, indicating reduced interpersonal consumption smoothing when players are not required to use the GT punishment strategy. The reduction in transfers under LCNS is not significantly different than under LCPS, suggesting that savings access does not crowd out interpersonal insurance. The second column shows results for rounds in SDG games where a defection has not previously occurred. (Defection occurs in roughly one-third of SDG game, as we discuss below.) In this sample, transfers are not significantly reduced in the limited commitment-no savings game, indicating that the reduction in transfers in this case is due to players reneging on their promises, rather than promising less in anticipation of binding constraints.

The coefficient on the measure of reachability shows that players at distance zero (a pairing which is never observed in our data) would transfer Rs 15 more than a pairing not connected through the social network in the full sample; Rs 23 more in the no-defection sample. The coefficient on distance shows that a one-unit increase in the path length between the pair reduces transfers by Rs 3, in the full sample or the no-defection sample. These figures tell us, that socially close individuals share more with each other than distant individuals. Below we examine whether the degree of crowdout moving from commitment to no commitment, and no savings to savings, is different when pairs are close versus when they are distant.

The third through sixth columns show limited commitment does not appear to bind when the GT strategy is imposed. Levels of transfers are not significantly lower when individuals cannot commit to transfers than when they are able to do so, suggesting that participation constraints do not bind when the punishment for defection is autarky for the rest of the game. In what follows, we focus on the SDG results, since the SDG results reflect endogenously-chosen punishment behavior and are more likely to reflect the real-world impact of limited commitment and savings access.

3.5.6 Does limited commitment affect consumption smoothing?

Consumption smoothing for the average player

Table 2 shows results for consumption smoothing. Again, the omitted category is FCNS, so coefficients on the indicators for other games give the regression-adjusted difference between that game and the full commitment benchmark. We present results for consumption absolute deviations, squared deviations, variances and standard deviations; all yield similar results. We focus on the regressions with the absolute deviation of consumption and the standard deviation of consumption since these are in units of rupees.

We find that going from FCNS to LCNS leads to a Rs 9 increase in the absolute deviation of consumption (or a Rs 9 increase in standard deviation), significant at the .01 level. This effect is equal to almost 20% of the average absolute deviation in the FCNS game, an economically as well as statistically significant increase.

However, the coefficient on LCPS is only Rs 5. While this is significantly different from zero, indicating that FCNS induces significantly more smooth consumption patterns than LCPS, a simple F-test also demonstrates that LCPS results in significantly more smooth consumption relative to

LCNS ($p < .01$). That is, individuals use savings to smooth some risk intertemporally that they cannot smooth using interpersonal transfers.

In order to make a statement about welfare, we must first look at the levels of consumption. Table 0d shows that the level of consumption is not significantly different between LCNS and LCPS. Conditional on sustaining the same level of consumption, variability is a sufficient statistic for welfare implications.

Therefore, we can interpret our results as saying that limited commitment with no savings induces a welfare loss relative to the full commitment no savings case. However, the introduction of savings to the limited commitment game improves the situation by significantly reducing consumption variability.

Consumption smoothing at different levels of income

As noted above, it is theoretically possible for savings access to reduce the welfare of the average member of a risk-sharing group (Ligon, Thomas, and Worrall 2000). However, our results show that, on average, savings access is beneficial because it allows individuals to smooth some risk intertemporally that they cannot smooth using interpersonal transfers. The limited commitment model also predicts that participation constraints will bind more when the members of a pair have very unequal luck–one is lucky most of the time, so the other is unlucky most of the time. Table 3a shows that this prediction is borne out in our data. In games where one player has a realized income in the lowest tercile of the income distribution, that player's consumption smoothing is much worse in LCNS relative to FCNS–the absolute deviation of consumption increases by Rs. 16. When both players' income realizations are in the middle tercile, the increase in absolute deviation of consumption is only Rs 5.

Comparing the coefficients on LCNS and LCPS shows that the benefit of savings (in terms of consumption smoothing) is greatest in games where one player has a realized income in the lowest tercile of the income distribution: the unlucky individual's consumption is smoother in LCPS than LCNS ($p=.07$), as is the lucky individual ($p<.01$).

Moreover, table 3b shows that, while transfers fall by approximately Rs 20 when moving from FCNS to LCNS and LCPS, the LCNS-vs-LCPS difference in transfers is not significant at the extreme terciles of the income distribution, while it is significant when luck is distributed relatively evenly. That is, savings crowds out transfers most when luck is distributed evenly.

These results seem counter to the hypothesis that those individuals with the worst series of income realizations (i.e., "bad luck") would do worse when their partners have access to savings, because their more fortunate partners would prefer to save their income than repeatedly make transfers to the unlucky partner. Transfers from the lucky to the unlucky member of the pair are not reduced by savings access when luck is uneven (so that average consumption for the unlucky partner is unchanged), and consumption variability falls because the unlucky (and lucky) partners use savings to smooth risk over time. Of course, in a setting where individuals have heterogeneous income process which are initially private information, so that individuals are learning about their partners' income process, it is possible that individuals with a series of low income realizations would see a larger drop in insurance going from the no savings to savings treatments than in the full information, i.i.d. income setting we consider.

Consumption smoothing for close vs. distant pairs

Given our data, a natural question to ask is how informal insurance relationships between pairs who are socially close and pairs who are socially far differ. To that end, we split the sample by high and low social distance: pairs with a social distance of at least the median value of 4 (including unconnected/unreachable pairs), vs. pairs with a social distance of 3 or fewer. The results are presented in Table 7.

The results for the Low Distance sample indicate that limited commitment appears not to bind significantly for low distance pairs, in the sense that consumption variation under LCNS and LSPS is not significantly different from FCNS. On the other hand, limited commitment binds greatly for high distance pairs in the sense that consumption variation under LCNS is significantly different from FCNS.

For Low distance pairs, moving from LCNS to LCPS does not lead to a significant change in consumption smoothing; that is, there is no evidence that access to savings affects welfare. On the other hand, for High distance pairs, moving from LCNS to LCPS leads to significant decrease in the variability of consumption, that is, an increase in consumption smoothing (the LCNS to LCPS effects are significantly different at $p<.01$). Savings allows high distance pairs to smooth some of the risk that they do not insure through transfers.

3.5.7 Crime (defection)...

The results on consumption smoothing and transfers show that participation constraints significantly bind in the SDG treatment, and that socially distant pairs are more affected by participation constraints than socially close pairs. While models of limited commitment feature no defection in equilibrium, because every subgame has an efficient continuation path (Ligon, Thomas, and Worrall 2002), the experimental participants in our games, particularly in the SDG treatment, mention changing their minds (i.e., defecting) in response to binding participation constraints. Table 4, which presents results on defection probabilities, shows that binding participation constraints manifest themselves through defection, i.e. players transferring a different (usually lower) amount than they promised. Defection occurs in 30% of rounds under the SDG treatment. The significant negative coefficient on the reachable indicator shows that individuals defect less often when they can reach their partner through the network. Conditional on being reachable, a greater distance is associated with more defection.

3.6 ...and punishment

If the data featured no actual defection, we would be unable to learn directly what post-defection strategies players used when the experiment did not impose the GT strategy, because defection would never actually occur. However, our data do allow us to study what happens after defection in the SDG treatment. Figures 4 and 5 show how transfers are affected in the rounds following a defection. Transfers are significantly reduced by about Rs. 12, though after 4 rounds they return to the level that prevailed before defection occurred. Figure 4 emphasizes the transiency of the punishment phase.

Moreover, during the punishment phase, transfers are not completely ceased, but only reduced. Even during the maximal punishment phase, transfers fall by about Rs. 12, or roughly 15%, a far cry from permanent reversion to autarky. If the players were endogenously imposing severe punishments post-defection, something in the flavor of a grim trigger, one would see a drop on the order of Rs. 80. Therefore, when not required to follow a GT strategy, players appear to inflict moderate punishments for about 2-3 rounds, or a third to a half of the expected duration of the game at the time the defection occurred (since the game can always be expected to last 6 rounds, conditional on not having ended yet.)

Perhaps surprisingly, Table 5 also shows that individuals do not punish those that are socially close to them any less (or more) than those who are far from them. This is in contrast to Table 4 which shows that distance is proportional to defection rates. That is, while individuals defect less the closer they are to their partner, conditional on defection they do not punish closer partners any less.

3.7 Conclusion and future directions

The results of a unique lab experiment, conducted in the field, show that under SDG (that is, when players are free to choose their own response to defection by their partner), limited commitment binds, and savings does not crowd out informal insurance. Private savings access does not appear to reduce welfare relative to limited commitment without savings, even at low quantiles of the income distribution. Instead, private savings access allows individuals to smooth intertemporally some of the income risk that is not insured interpersonally.

Consistent with the predictions of the limited commitment model, participation constraints are most binding when one member of the pair is very fortunate (gets high income most of the time), while the other in unfortunate and gets low income most of the time. But, even in such cases, savings access does not crowd out interpersonal transfers. Thus, we do not find evidence that savings access has negative distributional consequences, such as benefitting the most fortunate but harming the least fortunate.

When players are free to choose their own response to defection, defection is common (occurring in 30% of rounds), and the punishments are small in magnitude (a roughly 15% reduction in transfers the following period), and short in duration, with the response decaying to zero in 4 periods. The fact that in this case, consumption smoothing is worse than when a "grim trigger" (permanent reversion to autarky) punishment is imposed, suggests that some friction is preventing households from adopting the GT strategy–GT may be socially unacceptable, susceptible to renegotiation, too fragile to accidental lapses in risk-sharing, etc. Modelling these frictions is an interesting avenue for future work, and one we hope to pursue.

Using detailed data on how individuals within a village are connected socially, we find that limited commitment binds significantly when individuals are socially distant, and does not appear to bind when they are socially close. Players are less likely to renege on the transfers they promised their partner when they and their partner are close. However, if defection does occur, players do not punish (i.e., reduce transfers) differently when their partner is socially distant or socially close. While it is perhaps not surprising that limited commitment binds less when those engaged in risk sharing know each other, it illustrates the advantage of using a lab experiment, where we are able to randomly assign pairs. Using endogenously-formed, socially close pairs might result in concluding that limited commitment does not bind and that savings access does not improve consumption smoothing. If economic development weakens social ties between individuals, our results for socially distant pairs may be relevant in forecasting how well income risk can be insured and what role financial access might play in improving consumption smoothing.

Finally, we hope that our experimental strategy—a lab experiment, conducted in field settings in a developing country, carefully designed to test theoretical predictions—is of interest as a way to test other theoretical predictions which are difficult to test with non-experimental data. We feel this method can achieve high external validity by closely mimicking real-life decisions while controlling possibly confounding influences (endogenous network formation, etc.).

3.A Appendix: Figures

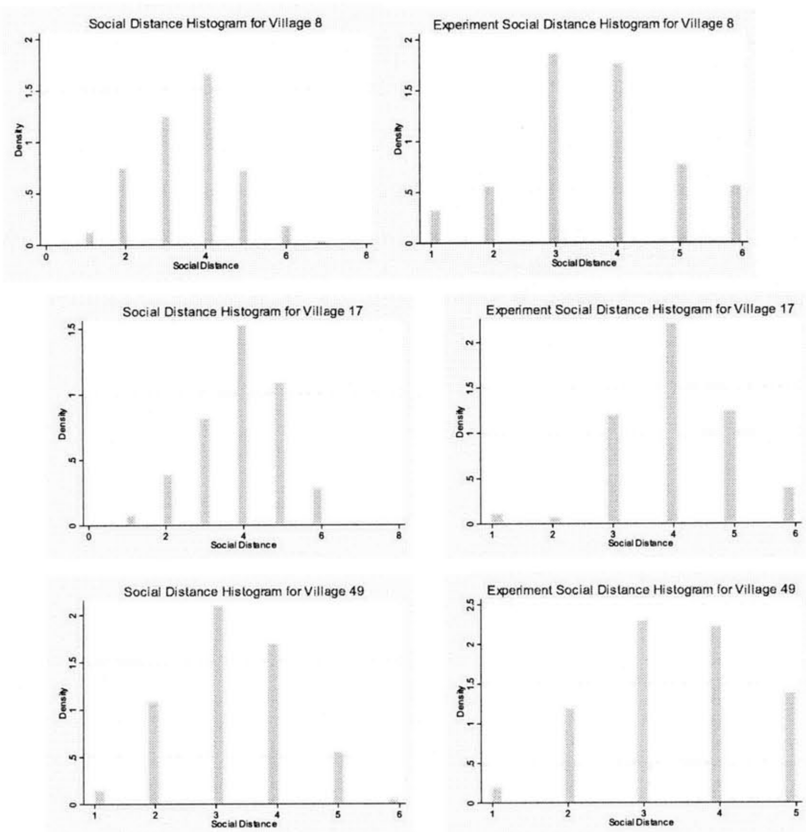


Figure 1: Sampling from the tail of the distribution

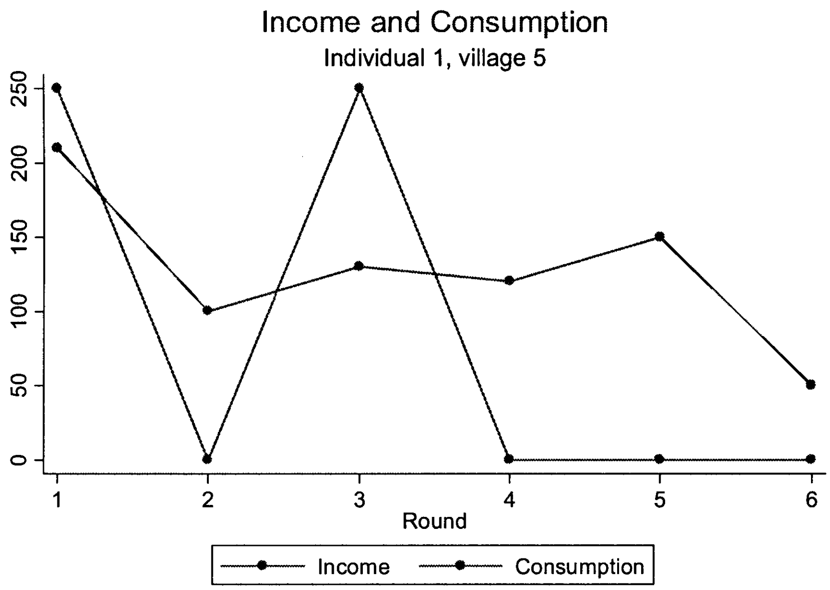


Figure 2a: Income and Consumption

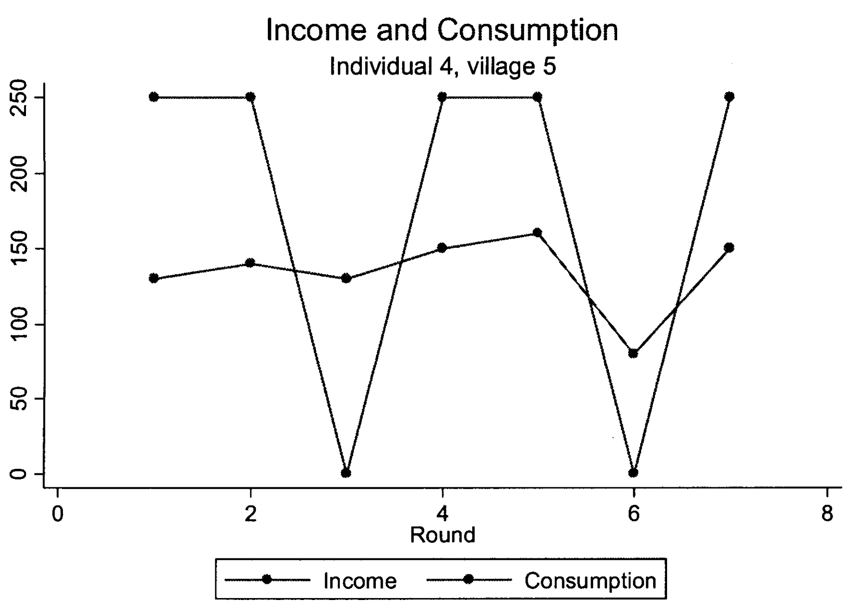


Figure 3: Income and Consumption

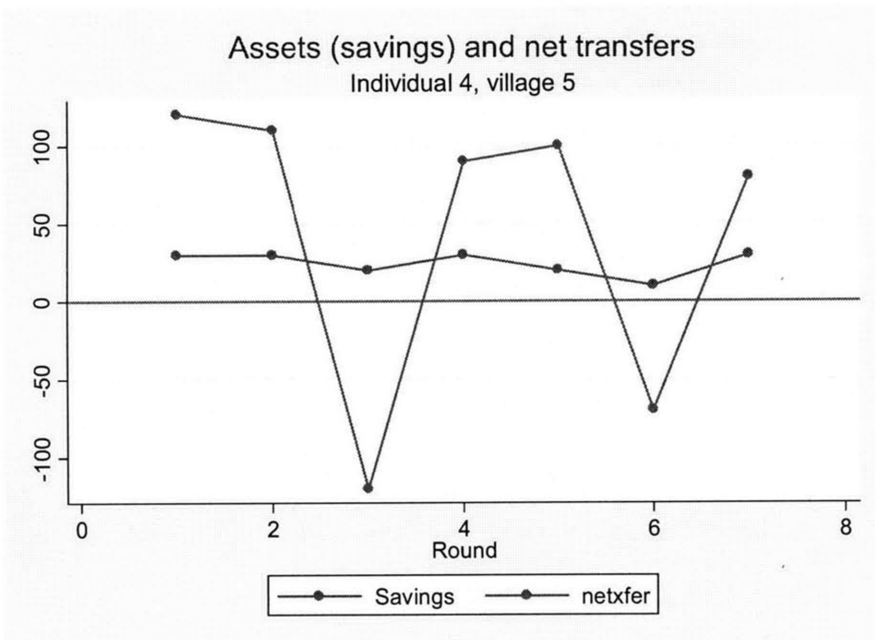


Figure 3b: Savings and Transfers

Figure 3 shows that consumption is smoother than income, net transfers to an individual's partner covaries positively with income, savings covaries with income, and that savings is considerably smoother than transfers.

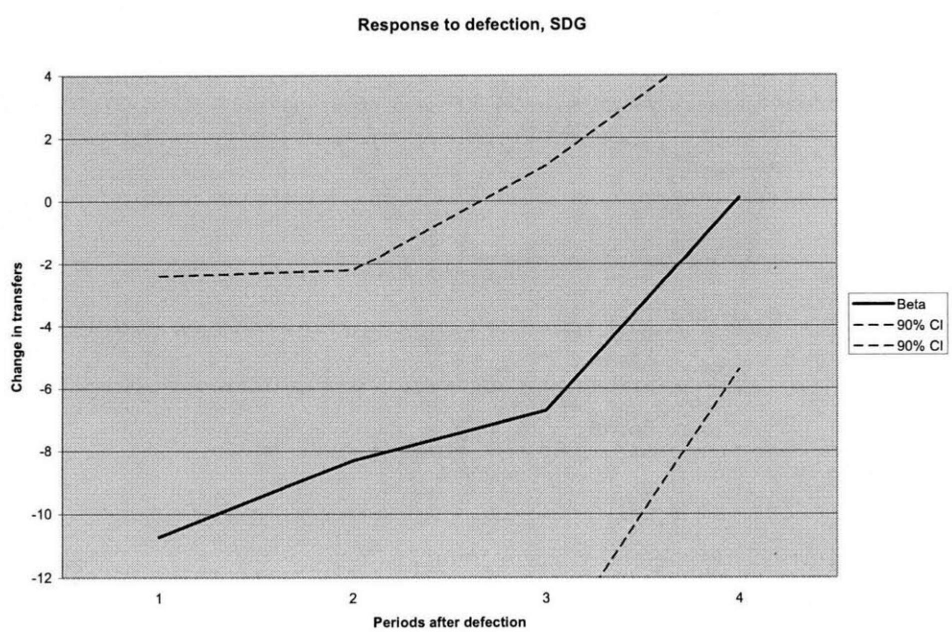


Figure 4a: Response to defection, SDG

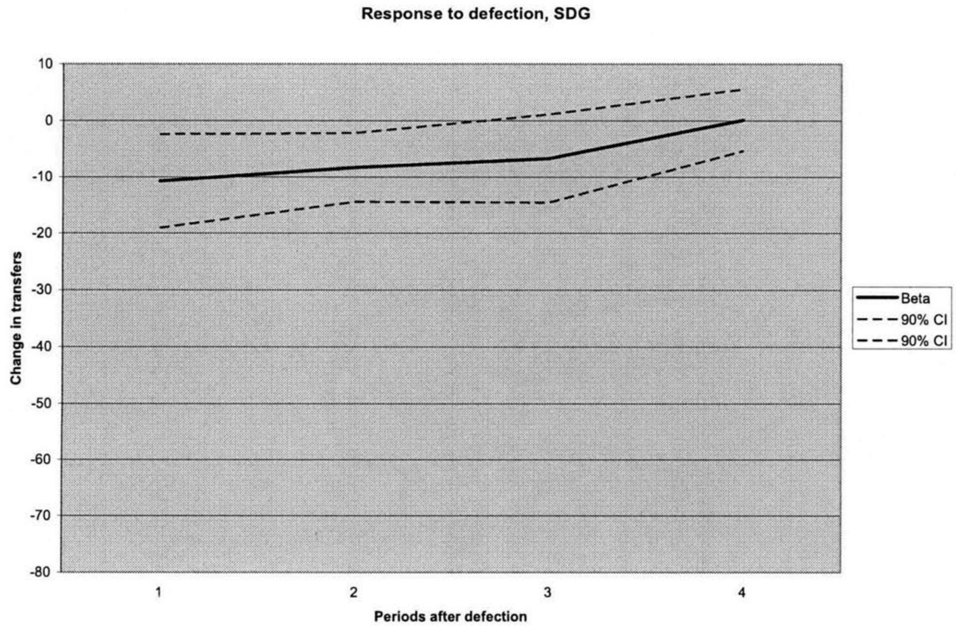


Figure 4b: Response to defection, scaled relative to FCNS transfers

3.A Appendix: Tables

Table 0a: Comparison of Caste Variables from Individual Survey

	Surveyed	SC	ST	OBC	General
SDG	0.0185	-0.0117	0.0269	-0.0362	0.0124
	[0.0265]	[0.0345]	[0.0172]	[0.0388]	[0.0264]
Constant	0.3431***	0.2727***	0.0376**	0.5611***	0.1254***
	[0.0187]	[0.0248]	[0.0124]	[0.0279]	[0.0190]
N	1300	660	660	660	660

Table 0b: Comparison of Wealth Variables from Individual Survey

	Surveyed	Roof						Number	Number	Electricity	Latrine	Owner
		Thatch	Title	Stone	Sheet	RCC	Other	of Rooms	of Beds			of house
SDG	0.0185	-0.0015	-0.0102	0.0203	-0.0058	-0.0013	-0.0095	-0.0304	0.0237	-0.0178	-0.0222	-0.0005
	[0.0265]	[0.0059]	[0.0263]	[0.0271]	[0.0218]	[0.0171]	[0.0117]	[0.0709]	[0.0722]	[0.0337]	[0.0469]	[0.0174]
C	0.3431***	0.0122**	0.3190***	0.3445***	0.1834***	0.1021***	0.0494***	2.5040***	0.9155***	1.4361***	2.5678***	0.8982***
	[0.0187]	[0.0042]	[0.0185]	[0.0191]	[0.0154]	[0.0121]	[0.0083]	[0.0501]	[0.0510]	[0.0238]	[0.0331]	[0.0123]
N	1300	1310	1252	1252	1252	1252	1253	1252	1252	1250	1252	1215

Table 0c: Comparison of Variables from Collected During the Experiment

	Male	Married	Age	Education	Not found in Census	Betweenness	Degree	Eigenvector Centrality	Distance if Reachable	Reachable
SDG	0.0296	-0.0036	-0.2992	0.0162	0.0015	-5.7124	0.2708	0.0025	0.0883*	0.0097
	[0.0270]	[0.0240]	[0.4672]	[0.2464]	[0.0104]	[287.2915]	[0.3571]	[0.0018]	[0.0365]	[0.0071]
C	0.4410***	0.7360***	30.2419***	7.4786***	0.0354***	3285.7023***	9.8400***	0.0198***	3.5454***	0.9792***
	[0.0191]	[0.0170]	[0.3306]	[0.1742]	[0.0073]	[203.1458]	[0.2525]	[0.0013]	[0.0240]	[0.0050]
N	1358	1358	1358	1354	1300	1300	1300	1300	4251	1253

Table 0d: Average transfers and consumption, by game and treatment

	Transfers		Consumption	
	G	SDG	G	SDG
No Savings (LC)	-0.2097	-9.621***	0.1063	0.3142
	[3.546]	[3.551]	[.4407]	[.516]
Private Savings (LC)	2.238	-10.11***	-3.223***	-2.079***
	[3.551]	[3.544]	[.5521]	[.5861]
Joint Savings	1.269		-2.453***	
	[3.575]		[.4346]	
Full commitment Mean	93.18	92.35	132.25	131.04
Standard deviation	33.47	36.31	49.26	51.97
N	8406	7025	16822	14070

Robust standard errors, clustered at the village by game level, in brackets. Transfer is the actual amount given to the unlucky individual (who earned Rs 0) by the lucky individual (who earned Rs 250). Consumption is the amount the individual chose to place in their consumption cup. Individuals were paid one randomly chosen consumption value at the end of the game.
* p<.1, ** p<.05, *** p<.01

Table 0e: Average consumption, by game and in-game income

	Transfers	
	G	SDG
Middle tercile	10.99***	11.6***
	[.832]	[.9694]
Top tercile	20.09***	23.77***
	[1.345]	[1.549]
Lowest tercile mean	119.18	117.12
Standard deviation	44.77	55.89
N	16800	14048

Robust standard errors, clustered at the village by game level, in brackets. Consumption is the amount the individual chose to place in their consumption cup. Individuals were paid one randomly chosen consumption value at the end of the game. * p<.1, ** p<.05, *** p<.01

Table 1: Use of smoothing mechanisms

	SDG			Grim		
	Transfers		Savings	Transfers		Savings
	Unconditional	Conditional		Unconditional	Conditional	
No S (LC)	-8.899***	-3.61		1.423	2.424	
	[1.937]	[2.545]		[1.698]	[1.816]	
Private S (LC)	-11.02***	-6.988**		0.5778	2.894	0.84
	[2.23]	[2.669]		[2.025]	[1.977]	[.8733]
Joint S (LC)				0.9233	1.434	0
				[1.825]	[1.897]	[.]
Reachable	15.37**	23.06**		-4.032	-3.104	-4.159
	[7.598]	[10.58]		[4.727]	[4.552]	[3.693]
Reach	-2.751***	-2.697**		1.363*	1.162	0.039
× Distance	[.8611]	[1.153]		[.7124]	[.733]	[.6022]
Constant	86.32***	66.9***	40.42***	83.46***	83.39***	37.31***
	[8.117]	[11.01]	[2.443]	[6.263]	[6.369]	[4.734]
LCNS=LCPS						
F-stat	1.111	1.325		0.18	0.0502	
p-value	0.2945	0.2523		0.6721	0.8231	
LCNS=LCJS						
F-stat				0.0899	0.2863	
p-value				0.7648	0.5935	
Full Com. Mean	92.3512	93.0808	22.6453	93.1755	92.8645	20.0956
St. Dev.	36.3129	36.6006	28.6262	33.4701	34.2887	26.9258
N	3180	1938	4267	3899	3208	7848
R^2	0.4613	0.5168	0.6296	0.4117	0.4284	0.5387
Adjusted R^2	0.3617	0.3705	0.5662	0.3197	0.3166	0.4962

Regressions at the pair-game-round level. Regressions include individual and village-fixed effects, indicator for whether individuals were directly surveyed about social relationships, network betweenness, degree and eigenvector centrality for both pair members, reachability and distance between partners, village order, surveyor and team effects, and controls for order and round of play. Robust standard errors, clustered at the village by game level, in brackets.
* p<.1, ** p<.05, *** p<.01

Table 2: Consumption smoothing

	SDG - Consumption				Grim - Consumption			
	Sq. Dev.	Abs. Dev.	Variance	St. Dev.	Sq. Dev.	Abs. Dev.	Variance	St. Dev.
No Savings	10.79***	8.865***	1141***	9.249***	1.093	-0.1299	150.7	0.2553
(LC)	[1.656]	[1.348]	[231.5]	[1.801]	[1.546]	[1.274]	[196.9]	[1.465]
Private S	4.427***	4.903***	453*	4**	-3.755**	-3.887***	-489.4**	-5.631***
(LC)	[1.622]	[1.368]	[234.9]	[1.851]	[1.594]	[1.274]	[202.8]	[1.582]
Joint S.					-2.792*	-3.221***	-237.8	-3.892**
(LC)					[1.482]	[1.22]	[192.1]	[1.558]
Reachable	-8.205	-6.249	-951.3	-6.921	-1.488	-1.979	-403	-5.033
	[5.203]	[4.951]	[649.9]	[6.333]	[3.834]	[3.429]	[513.3]	[4.755]
Reach	1.122**	.8942*	168**	1.17*	0.0814	0.0847	2.389	0.2441
Distance	[.5374]	[.4666]	[78.06]	[.6585]	[.5405]	[.4614]	[68.05]	[.5693]
Constant	39.31***	52.24***	3735***	57.5***	26.76***	43.07***	3211***	53.21***
	[6.007]	[5.539]	[876.4]	[7.6]	[.]	[3.253]	[500]	[4.252]
LCNS=LCPS								
F-stat	16.26	10.17	9.765	10.33	8.572	7.944	8.843	13.45
p-value	0.00011	0.0019	0.0023	0.0018	0.004	0.0056	0.0035	0.00035
LCNS=LCJS								
F-stat					5.717	5.404	3.461	6.845
p-value					0.0182	0.0216	0.065	0.0099
FC Mean	27.0085	40.912	2898.8811	48.5979	24.2837	38.7713	2568.1424	45.4687
St. Dev.	37.3173	32.0513	2641.2137	23.1931	33.7581	30.42	2521.5301	22.3938
N	12752	12752	1848	1848	15371	15371	2465	2465
R^2	0.2657	0.2923	0.5612	0.5818	0.28	0.2965	0.5113	0.5539
Adj. R^2	0.2253	0.2533	0.3144	0.3465	0.2473	0.2645	0.3296	0.3879

Notes as in previous table.

Table 3a: Consumption smoothing (Absolute Deviation of Consumption) at income percentiles

	SDG -Percentile			Grim - Percentile		
	33rd	66th	100th	33rd	66th	100th
No Savings (LC)	15.53***	4.004**	14.5***	-1.193	-0.6693	0.2994
	[3.163]	[1.907]	[2.439]	[2.713]	[1.516]	[1.948]
Private Savings (LC)	9.968***	4.129**	5.564**	-4.004*	-2.842*	-3.414
	[3.744]	[1.77]	[2.522]	[2.411]	[1.501]	[2.096]
Joint Savings (LC)				-2.429	-3.966***	-0.2936
				[3.177]	[1.426]	[1.743]
Reachable	-45.48***	-6.033	16.38*	-33.94***	1.971	-6.882
	[13.34]	[10]	[8.812]	[10.03]	[6.013]	[4.656]
Reach * Distance	0.9573	-0.3695	1.818	1.553	-1.065	1.243
	[2.126]	[.7243]	[1.316]	[1.456]	[.6952]	[.8257]
Constant	81.87***	56.41***	27.94***	69.06***	45.79***	48.09***
	[14.28]	[10.92]	[8.873]	[7.658]	[5.204]	[6.786]
LCNS=LCPS						
F-stat	3.255	0.0052	14.3	1.5	1.878	3.543
p-value	0.0743	0.9428	0.00026	0.2228	0.173	0.062
LCNS=LCJS						
F-stat				0.1851	4.144	0.1408
p-value				0.6677	0.0438	0.708
Full. Com. Mean	39.7506	40.8573	40.7789	39.6518	36.3	41.903
Standard Deviation	31.2281	31.8222	31.7478	31.1378	28.3459	32.5682
N	2562	5646	4522	3296	6935	5130
R^2	0.4687	0.35	0.3732	0.4398	0.3691	0.4076
Adjusted R^2	0.3912	0.2842	0.2993	0.3599	0.3093	0.3406

Notes as in previous table.

Table 3b: Transfers at income percentiles

	SDG - Percentile			Grim -Percentile		
	33rd	66th	100th	33rd	66th	100th
No Savings (LC)	-20.14***	-2.395	-13.25***	-0.2475	2.146	-2.725
	[5.562]	[1.977]	[2.685]	[3.058]	[1.456]	[2.126]
Private Savings (LC)	-19.05***	-8.352***	-12.62***	0.1657	0.005	-2.079
	[4.921]	[1.825]	[3.139]	[2.883]	[1.969]	[2.668]
Joint Savings (LC)				1.285	1.462	-3.925*
				[3.672]	[1.717]	[2.358]
Reachable	33.56**	3.76	-19.65*	32.98***	-0.9795	10.46*
	[14.93]	[12.73]	[10.72]	[8.378]	[6.851]	[6.265]
Reach * Distance	0.2191	-0.8215	-3.543**	-1.732	0.7763	-1.794*
	[2.277]	[.9068]	[1.44]	[1.614]	[.9499]	[1.035]
Constant	68.43***	83.43***	116.1***	54.77***	88.76***	94.85***
	[16.44]	[13.97]	[11.13]	[8.347]	[5.299]	[7.985]
LCNS=LCPS						
F-stat	0.0693	11.09	0.0567	0.018	1.321	0.0733
p-value	0.793	0.0012	0.8122	0.8936	0.2524	0.787
LCNS=LCJS						
F-stat				0.2095	0.1431	0.348
p-value				0.648	0.7059	0.5563
Full Com. Mean	93.8895	91.3642	92.8067	91.8027	95.8978	90.0227
Standard Deviation	36.7522	36.0272	36.4272	34.0633	31.4111	35.6621
N	2562	5646	4522	3296	6935	5136
R^2	0.5139	0.4317	0.4407	0.476	0.3858	0.4362
Adjusted R^2	0.443	0.3742	0.3747	0.4014	0.3275	0.3726

Notes as in previous table.

Table 3c: Savings income percentiles

	SDG			Grim		
	33rd Percentile	66th Percentile	100th Percentile	33rd Percentile	66th Percentile	100th Percentile
Joint Savings (LC)				3.162**	1.419	-0.3136
				[1.327]	[1.676]	[1.181]
Reachable					11.32	-13.64
					[12.49]	[9.518]
Reach * Distance				7.412***	0.5883	4.605**
				[2.32]	[1.455]	[1.739]
Constant	37.36***	41.78***	40.1***	-9.342	31.76***	41.39***
	[6.352]	[3.868]	[2.997]	[8.466]	[9.418]	[5.547]
Full Com. Mean	25.698	19.6684	24.6324	19.5562	18.0719	22.6544
St. Dev.	35.1322	22.8611	30.5376	27.5537	21.9806	31.161
N	875	1868	1524	1720	3397	2721
R^2	0.6586	0.597	0.6239	0.6503	0.5207	0.6841
Adjusted R^2	0.5968	0.5312	0.5581	0.5888	0.4509	0.6349

Notes as in previous table.

Table 4: Defection and defection rates of Grim vs SDG, by game

	Total Defection	Total Defection
SDG		
No Savings (LC)		-0.0031
		[.0061]
Private Savings (LC)		
Joint Savings (LC)		
SDG * No Savings (LC)		
SDG * Private Savings (LC)		-0.0061
		[.0173]
SDG * Joint Savings (LC)		
Reachable	-.3104***	-.3105***
	[.0764]	[.077]
Reach * Distance	.0255***	.0254***
	[.0066]	[.0067]
Constant	.2936***	.2971***
	[.0802]	[.0821]
Full Commitment Mean	0.0095	0.0098
N	8059	8059
R^2	0.4166	0.4166
Adjusted R^2	0.3477	0.3475

Notes as in previous table.

Table 5: Response to Defection for SDG

	Transfers	Transfers	Transfers	Transfers	Transfers
Defection	-6.999**				-10.73**
1 Period Ago	[2.805]				[5.075]
Defection		-5.39*			-8.315**
2 Periods Ago		[2.869]			[3.727]
Defection			-6.579*		-6.714
3 Periods Ago			[3.773]		[4.778]
Defection				-0.6261	0.0999
4 Periods Ago				[3.355]	[3.34]
Reachable	11.61	14.94	20.7	17.7	-0.0368
	[8.858]	[15.27]	[17.33]	[20.04]	[18]
Reach * Distance	-1.707	-1.887	-1.719	-0.3321	0.1502
	[1.42]	[1.838]	[1.918]	[2.006]	[2.052]
Constant	74.87***	69.94***	63.76***	62.39***	72.08***
	[9.607]	[13.63]	[15.01]	[21.87]	[17.7]
N	1795	1500	1192	884	884
R^2	0.5716	0.6113	0.6729	0.7035	0.714
Adjusted R^2	0.4344	0.4529	0.4873	0.4476	0.4638

Notes as in previous table.

Table 6: Use of smoothing mechanisms for SDG

	All			Low Distance			High Distance		
	Transfers		Savings	Transfers		Savings	Transfers		Savings
	Unconditional	Conditional		Unconditional	Conditional		Unconditional	Conditional	
No Savings (LC)	-8.899***	-3.61		-6.603*	1.982		-11.1***	-9.512***	
	[1.937]	[2.545]		[3.796]	[5.325]		[2.981]	[3.581]	
Private Savings (LC)	-11.02***	-6.988**		-11.76***	-4.17		-13.53***	-12.72***	
	[2.23]	[2.669]		[3.962]	[4.721]		[2.984]	[3.625]	
Reachable	15.37**	23.06**					-7.544	5.018	
	[7.598]	[10.58]					[16.03]	[18.51]	
Reach * Distance	-2.751***	-2.697**		-4.362*	-0.3183		-0.1733	-1.672	
	[.8611]	[1.153]		[2.479]	[3.552]		[2.586]	[3.878]	
Constant	86.32***	66.9***	40.42***	104.5***	83.97***	39.11***	95.07***	95.07***	41.05***
	[8.117]	[11.01]	[2.443]	[8.924]	[12.26]	[3.814]	[12.21]	[13.71]	[2.979]
LCNS=LCPS									
F-stat	1.111	1.325	0	2.069	1.106	0	0.6468	0.6435	0
p-value	0.2945	0.2523	0	0.1534	0.2956	0	0.4229	0.4241	0
Full Commitment Mean	92.35	93.08	22.65	92.35	95.24	22.65	92.35	90.96	22.65
St. Dev.	36.31	36.6	28.63	36.31	37.94	28.63	36.31	35.19	28.63
N	3180	1938	4267	1459	936	2033	1925	1221	2354
R^2	0.46	0.52	0.63	0.52	0.59	0.52	0.49	0.54	0.67
Adjusted R^2	0.36	0.37	0.57	0.39	0.42	0.44	0.37	0.36	0.62

Notes as in previous table.

Table 7: Consumption smoothing for SDG

	All				Low Distance				High Distance			
	Squared Deviation	Absolute Deviation	Variance	Standard Deviation	Squared Deviation	Absolute Deviation	Variance	Standard Deviation	Squared Deviation	Absolute Deviation	Variance	Standard Deviation
	b/se	b/se	b/se	b/se	b/se	b/se	b/se	b/se	b/se	b/se	b/se	b/se
No S	10.79***	8.865***	1141***	9.249***	4.14*	3.02	541.4	4.099	14.58***	11.9***	1447***	11.41***
(LC)	[1.656]	[1.348]	[231.5]	[1.801]	[2.338]	[2.065]	[383.5]	[3.309]	[2.058]	[1.635]	[341.6]	[2.518]
Private	4.427***	4.903***	453*	4**	0.3669	1.385	223.6	2.344	6.717***	6.946***	589.9*	4.659*
S (LC)	[1.622]	[1.368]	[234.9]	[1.851]	[2.606]	[2.275]	[393.4]	[3.338]	[1.895]	[1.527]	[330.5]	[2.493]
Reach.	-8.205	-6.249	-951.3	-6.921					-9.086	-8.568	-1379	-12.7
	[5.203]	[4.951]	[649.9]	[6.333]					[8.529]	[7.845]	[1265]	[11.27]
Reach.	1.122**	.8942*	168**	1.17*	-0.4166	-0.2858	133.2	0.6725	1.706	1.563*	320.1*	2.591*
× Dist.	[.5374]	[.4666]	[78.06]	[.6585]	[1.577]	[1.366]	[268.5]	[2.316]	[1.042]	[.9219]	[180.6]	[1.492]
Const.	39.31***	52.24***	3286***	52.89***	38.19***	51.17***	3383***	55.06***	32.86***	46.94***	2885**	52.82***
	[6.007]	[5.539]	[839.8]	[7.358]	[5.218]	[4.376]	[1239]	[8.987]	[9.124]	[8.091]	[1379]	[11.97]
LCNS=LCPS												
F-stat	16.26	10.17	9.77	10.33	2.31	0.61	0.87	0.38	15.04	10.11	6.27	7.48
p-value	0.0001	0.0019	0.0023	0.0018	0.1321	0.4355	0.3526	0.538	0.0002	0.0019	0.0136	0.0072
FC Mean	27.01	40.91	2898	48.6	27.01	40.91	2898	48.6	27.01	40.91	2898	48.6
St. Dev.	37.32	32.05	2641	23.19	37.32	32.05	2641	23.19	37.32	32.05	2641	23.19
N	12752	12752	1848	1848	5939	5939	860	860	7137	7137	1040	1040
R^2	0.27	0.29	0.56	0.58	0.32	0.34	0.66	0.68	0.39	0.33	0.66	0.69
Adj. R^2	0.23	0.25	0.31	0.35	0.26	0.29	0.24	0.28	0.24	0.28	0.28	0.33

Notes as in previous table.

Table 8a: Consumption Absolute Deviation by income percentiles

	All - Percentile			Low Distance- Percentile			High Distance - Percentile		
	33rd	66th	100th	33rd	66th	100th	33rd	66th	100th
No Savings (LC)	15.53***	4.004**	14.5***	17.43***	-7.587**	15.96***	18.05***	11.2***	15.51***
	[3.163]	[1.907]	[2.439]	[5.996]	[3.128]	[4.663]	[4.898]	[3.188]	[3.18]
Private Savings (LC)	9.968***	4.129**	5.564**	7.607	-1.864	7.051	5.954	5.302**	10.87***
	[3.744]	[1.77]	[2.522]	[7.131]	[2.875]	[5.058]	[4.16]	[2.386]	[3.165]
Reachable	-45.48***	-6.033	16.38*				-42.07**	10.78	20.55
	[13.34]	[10]	[8.812]				[16.87]	[8.754]	[18.38]
Reach * Distance	0.9573	-0.3695	1.818	-11.77	1.628	-0.4731	6.083*	-2.24	2.158
	[2.126]	[.7243]	[1.316]	[8.698]	[2.316]	[3.824]	[3.62]	[1.65]	[2.505]
Constant	76.75***	53.67***	21.75**	92.03***	53.64***	39.52***	41.48***	54.79***	27.69**
	[14.84]	[10.95]	[8.823]	[32.92]	[7.951]	[12.2]	[8.711]	[6.74]	[12.48]
LCNS=LCPS									
F-stat	3.26	0.01	14.3	3.92	3.54	4.86	5.36	5.68	3.19
p-value	0.0743	0.9428	0.0003	0.0511	0.0632	0.0298	0.0226	0.0188	0.077
Full Commitment Mean	39.75	40.86	40.78	39.44	39.16	41.92	39.29	40.82	39.48
St. Dev.	31.23	31.82	31.75	31.75	31.27	31.00	30.30	31.38	31.15
N	2562	5646	4522	1182	2582	2153	1835	3483	2803
R^2	0.47	0.35	0.37	0.53	0.37	0.41	0.43	0.40	0.41
Adjusted R^2	0.39	0.28	0.30	0.45	0.29	0.32	0.35	0.32	0.33

Notes as in previous table.

Table 8b: Consumption Variance by income percentiles

	All - Percentile			Low Distance- Percentile			High Distance - Percentile		
	33rd	66th	100th	33rd	66th	100th	33rd	66th	100th
No Savings (LC)	2024*	561.6	1920***	2607**	-878.1	2457*	1827	1331*	1859*
	[1103]	[405.2]	[654.1]	[1111]	[803]	[1401]	[1544]	[727.9]	[995.8]
Private Savings (LC)	1127	359.2	802.4	1430	-464.4	1313	740.5	525.5	1293
	[1269]	[382.7]	[677.8]	[1486]	[789.6]	[1584]	[1303]	[558.9]	[1197]
Reachable	-5005	-582.3	1252				-4879	1332	1856
	[4229]	[1884]	[1966]				[5883]	[2031]	[4860]
Reach * Distance	203.4	-21.4	313	-253.2	316.8	30.51	624	-196	374.5
	[757.3]	[148.5]	[313.5]	[2323]	[572.3]	[1258]	[1347]	[408.4]	[664.7]
Constant	5418	3660*	2.639	2120	3691	3838	3868	3516**	-2376
	[5227]	[2141]	[2341]	[7337]	[3418]	[3729]	[2987]	[1625]	[3292]
LCNS=LCPS									
F-stat	0.44	0.27	2.86	1.25	0.22	0.85	0.41	1.78	0.47
p-value	0.507	0.604	0.0937	0.266	0.6428	0.3581	0.5253	0.1853	0.4936
Full Commitment Mean	2677.91	3052.37	2830.11	2566.81	2803.18	2847.8	2682.07	3018.85	2738.32
St. Dev.	2387.62	2941.5	2274.67	2320.85	2525.7	2275.41	2373	2961.24	2297.32
N	376	813	656	173	372	312	274	506	412
R^2	0.90	0.77	0.77	0.85	0.85	0.87	0.89	0.84	0.86
Adjusted R^2	0.21	0.36	0.15	0.28	0.28	0.12	0.22	0.36	0.21

Notes as in previous table.

Table 8c: Transfers, by income percentiles for SDG

	All - Percentile			Low Distance- Percentile			High Distance - Percentile		
	33rd	66th	100th	33rd	66th	100th	33rd	66th	100th
No Savings (LC)	-20.14***	-2.395	-13.25***	8.281	9.969**	-16.97***	-22.04***	-12.89***	-11.59***
	[5.562]	[1.977]	[2.685]	[10.92]	[4.287]	[5.629]	[5.334]	[3.282]	[3.949]
Private Savings (LC)	-19.05***	-8.352***	-12.62***	17.45	-4.67	-16.38***	-12.75***	-11.56***	-16.43***
	[4.921]	[1.825]	[3.139]	[12.18]	[3.643]	[5.682]	[4.713]	[2.637]	[3.902]
Reachable	33.56**	3.76	-19.65*				9.329	-30.97***	-19.41
	[14.93]	[12.73]	[10.72]				[17.14]	[9.383]	[19.29]
Reach * Distance	0.2191	-0.8215	-3.543**	18.01**	-6.182**	-2.335	0.287	4.463***	-2.208
	[2.277]	[.9068]	[1.44]	[8.227]	[3.007]	[4.772]	[3.877]	[1.646]	[3.122]
Constant	68.43***	88.31***	128.4***	-29.46	89.95***	83.15***	96.04***	82.74***	99.69***
	[16.44]	[14.02]	[10.84]	[37.04]	[9.36]	[14.89]	[9.094]	[7.873]	[12.65]
LCNS=LCPS									
F-stat	0.07	11.09	0.06	1.53	17.64	0.02	3.6	0.26	1.95
p-value	0.793	0.0012	0.8122	0.2192	0.0001	0.8944	0.0607	0.6117	0.1658
Full Commitment Mean	93.89	91.36	92.81	95.27	94.51	93.63	91.13	89.34	90.92
St. Dev.	36.75	36.03	36.43	37.92	36.22	38.91	34.79	35.00	34.07
N	2562	5646	4522	1182	2582	2153	1835	3483	2803
R^2	0.51	0.43	0.44	0.58	0.45	0.50	0.48	0.47	0.46
Adjusted R^2	0.44	0.37	0.37	0.50	0.38	0.43	0.41	0.41	0.38

Notes as in previous table.

3.A Appendix: Networks

3.A.1 Introduction

Here we introduce basic social network terminology.[12] A graph or network, Γ, is defined as a pair of a set of vertices, V and edges E, $\Gamma := (V, E)$. We represent Γ by its adjacency matrix $A := A(\Gamma)$, where $A_{ij} = \mathbf{1}\{ij \in E\}$. However, as our data depicts connections on multiple levels (friendship, family, coworkers, borrowing/lending relationships, etc.), we begin with $\{\Gamma^r\}_{r \in R}$, where R is a set of relationships.

Specifically, in our survey, we have the following connections between vertices: (1) Visitors who come to the household, (2) Households that a person visits, (3) Relatives, (4) Non-relatives, (5) Medical aid, (6) Temple company, (7) Borrows material goods, (8) Lends material goods, (9) Borrows money, (10) Lends money, (11) Whom the person gives advice to, (12) Whom the person asks for advice, and (13) Whom the person identifies as a local leader.

Taking this literally we have $|R| = 13$ and therefore while $A_{ij}^r \in \{0,1\}$, $A_{ij} \in \{0,1\}^{13}$. In order to deal with this excess of information, we can consider restricted graphs where we look at networks built upon particular types of links. Alternatively, we can weight the edges via some criterion function which we minimize to get "optimal weights" and get one relationship.[13]

One simple way to collapse the information is to create the "all" network. Here we define $\Gamma^{all} := (V, E^{all})$ where

$$A_{ij}^{all} = \prod_{r \in \{1,..12\}} A_{ij}^r$$

We omit A_{ij}^{13}, the entry for the local leader network, since this is not really a social network but rather a network built upon people identifying their local leader. Henceforth, we drop the *all* superscript and simply refer to $A := A(\Gamma^{all})$ as the social network of the village.

3.A.2 Relevant Statistics

Recall that we want to include measures of an individual's prominence in a village as well as the closeness between partners. Therefore we introduce degree, betweenness centrality, eigenvector centrality, and geodesic distance.

The degree of a vertex is the number of edges emanating from that vertex

$$d_i(\Gamma) := \#\{j : A_{ij} > 0\} = 1_N' A e_i.$$

Associated with degree is the degree distribution. This is simply the empirical cumulative distribution function of the degree function. However, in an abuse of terminology, the distribution typically refers to a density function denoted as $f(d)$, which describes the fraction of nodes that have degree d.

[12] The discussion follows Jackson (2008).

[13] This would involve generating an optimal weighting function

$$\omega(R_e) \in [0,1]$$

which would then give us the weighted, undirected graph

$$\hat{\Gamma} = (V, \Omega)$$

The betweenness centrality of a node measures how well situated a node is in terms of the paths that it lies on. We let $g_i(kj)$ denote the number of geodesics connecting k and j that go through i. Then let $g(kj)$ denote the total number of geodesics connecting k and j. The idea is to study the ratio of these to get a sense of how critical i is to connecting k and j. To that end, we define betweenness centrality as

$$\mathcal{B}_i := \sum_{k \neq j \,:\, i \notin \{k,j\}} \frac{\frac{g_i(kj)}{g(kj)}}{\frac{(n-1)(n-2)}{2}} = \sum_{k \neq j \,:\, i \notin \{k,j\}} 2 \frac{g_i(kj)}{(n-1)(n-2)\,g(kj)}.$$

Eigenvector centrality is a recursive measure which defines the importance of a node as a function of the importance of its neighbors. $\mathcal{E}(\Gamma)$ denote the eigenvector centrality of the graph Γ. That is, it is the vector of eigenvector centralities for each of the N nodes in the graph. We will define eigenvector centrality as the eigenvector associated with the eigenvalue that is the spectral radius of A:

$$A\mathcal{E} = \lambda A \text{ s.t. } \lambda = \max \sigma(A).$$

This can easily be rationalized in the following setting. We can construct a recursive metric as follows. Let us define the centrality of i to be a positive metric which is proportional to the sum of the centrality of i's neighbors

$$\mathcal{E}_i \propto \sum_{j:\mathbf{1}\{ij \in E\}} \mathcal{E}_j.$$

Then we can write $\forall i \in V$

$$\mathcal{E}_i \propto \sum_{j:\mathbf{1}\{ij \in E\}} \mathcal{E}_j = \sum_{j \in V} \mathbf{1}\{ij \in E\}\,\mathcal{E}_j = \sum_{j \in V} A_{ij}\mathcal{E}_j.$$

This completes the demonstration since $\mathcal{E} \propto A\mathcal{E}$ implying that $\mathcal{E}$ is an eigenvector. But for $\mathcal{E}_i > 0$ $\forall i \in V$, the eigenvector must be associated with the maximal eigenvalue.

Reachability and geodesic distance are self-explanatory. We define geodesic distance as

$$\gamma(ij) = \min_{k \in \mathbb{N}} \left[A^k\right]_{ij} > 0$$

and reachability as

$$R_{ij} = \mathbf{1}\{\gamma(ij) < \infty\}.$$

Accordingly we can define the reachability matrix $R = [R_{ij}]$ and the distance matrix $D = [\gamma(ij)]$.

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