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Risk Sharing Relations and Enforcement Mechanisms*

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Abstract

We investigate whether the set of available enforcement mechanisms affects the formation of risk sharing relations by applying dyadic regression analysis to data from a specifically designed behavioural experiment, two surveys and a genealogical mapping exercise. During the experiment participants are invited to form risk sharing relations under three institutional environments, each associated with different enforcement mechanisms: external, intrinsic, and endogenous extrinsic, i.e., the threat of (partial) social exclusion. Dyads who are similar in age and gender, genetically related, or who belong to the same organizations with an economic purpose are more likely to share risk. However, the latter are associated with less risk sharing when endogenous extrinsic incentives can be applied, while co-membership in religious congregations and being related by marriage support enforcement through such incentives. We find no evidence of assortative grouping on risk preferences but, ex post, co-group members' risk-taking behavior converges.

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1 Introduction

It is common for households to pool risk, for instance through mutual insurance arrangements (e.g. Foster and Rosenzweig 2001, Fafchamps and Lund 2003, De Weerdt and Fafchamps 2007) and group loans with joint liability (e.g. Besley and Coate 1995, Ghatak 2000, Ghatak 1999, Morduch 1999). The way in which risk pooling arrangements are enforced varies with the institutional and legal environment. Informal arrangements, such as those that exist among friends and relatives, are thought to be enforced through a mix of quid pro quo, altruism, and adherence to social norms (e.g. Coate and Ravallion 1993, Cox and Fafchamps 2007, Platteau 1994). Formal arrangements, such as insurance contracts and funeral societies, combine the above with external enforcement through courts and other adjudication processes (e.g. De Weerdt and Dercon 2006, De Weerdt, Dercon, Bold and Pankhurst 2006). What remains unclear is the extent to which the nature of the available enforcement mechanisms affects who pools risk with whom and whether and how this, in turn, affects subsequent risk taking behavior. We investigate these questions using a behavioral field experiment designed specifically for this purpose.

Since the seminal work of Kimball (1988) and Coate and Ravallion (1993), the theoretical literature has modeled informal risk sharing as a repeated game between self-interested parties (e.g. Kocherlakota 1996, Ligon, Thomas and Worrall 2001, Foster and Rosenzweig 2001). More recently the role of intrinsic incentives such as altruism and guilt has also been investigated (e.g. Altonji, Hayashi and Kotlikoff 1992, Ravallion and Dearden 1988, Cox 1987, Cox, Eser and Jimenez 1998, Cox and Fafchamps 2007). This theoretical literature predicts that external enforcement facilitates risk sharing between relative strangers, i.e., individuals who have little knowledge of each other and have little reason to care about one another's wellbeing, because it overcomes the limitations imposed by intrinsic incentives and enforcement constraints.[1]

[1] The reader is referred to Genicot and Ray (2003) and Bloch, Genicot and Ray (2004) for a detailed discussion

Also relevant is the theoretical literature on joint liability loans Ghatak (1999) as the joint liability agreement is effectively a risk sharing arrangement. Here, we see that individuals may sort into joint liability groups assortatively with respect to risk attitudes: individuals who are highly risk averse, and have safe portfolios as a result, may prefer not to pool risk with individuals who are risk loving and thus have risky portfolios – and vice-versa. Applying the same reasoning, Hoff (1996) shows that heterogeneous risk attitudes may lead to less than full risk pooling, even when enforcement is perfect.

The purpose of this paper is to test these predictions using data from a controlled behavioral field experiment. The advantage of using an experimental approach is that it facilitates causal inference. However, the approach requires that the participating subjects be placed in a somewhat artificial decision-making environment. This may cast doubt on whether the behavior observed during the experiment bears any resemblance to the corresponding behavior as it naturally arises (Bardsley 2005). We minimize this potential drawback in three ways. First, we involve in the experiment villagers who are known to engage in informal risk sharing. Second, each experimental session is conducted in a single village, thereby ensuring that the participants are interacting with people they already know. Third, the experiment is designed to allow face-to-face, rather than anonymous, interaction. A comparison of the experimental results with real risk sharing within a sub-sample of villages indicates that these measures were at least partially successful (Barr and Genicot 2008).

The experiment is divided into two rounds, played over two consecutive days. In the first round, participants independently play a version of Binswanger's risk preference elicitation game (Binswanger 1980). In the second round, participants play the game a second time but are offered, ex ante, the opportunity to form risk sharing groups, i.e., groups within which the

of the limitations imposed by self-enforcement constraints on the formation of risk sharing networks.

proceeds of all members' gambles are shared equally. The institutional environment and, hence the set of available enforcement mechanisms is varied across three treatments applied during the second round. In one treatment the agreements are perfectly exogenously enforced by the experimenter. In another, agreements are enforced only by intrinsic motivations (e.g., altruism and guilt) because people can secretly defect from the agreement after observing the outcome of their gamble. And in the third, people can defect from their groups after realizing their gamble only if they are willing to announce their defection to everyone in the session. So, in this treatment agreements can be enforced by extrinsic incentives in the form of social sanctions (e.g., shaming, or some form of threatened exclusion).

Using the resulting data, Barr and Genicot (2008) have shown that when the risk sharing agreements are externally enforced by the experimenter individuals are most likely to enter into such agreements, although even then many potentially beneficial agreements are not entered into. They also show that it is only under this treatment that risk sharing group members engage in more risk taking. And finally they find that, in disagreement with the theoretical literature cited above, individuals are least likely to enter into risk sharing arrangements when enforcement through some form of social sanctioning is possible. They then go on to show that this third finding is consistent with two theoretical explanations: either social sanctions, especially in the form of full or partial exclusion, are costly to inflict; or individuals suffer from time-inconsistent preferences. However, the experimental data alone did not allow them to test these theories.

In this paper our primary focus is who chooses to enter into a risk sharing arrangement with whom under each of the three different institutional environments. We investigate this by combining the experimental data with data from a survey and a genealogical mapping exercise conducted on the same households. The two additional sources of information provide a rich

description of the economic and social backgrounds of the experimental participants, including data on kinship and marriage ties between them, and on the various religious congregations, groups and societies to which they belong. We apply dyadic regression analysis to this data.

Our findings can be summarized as follows. Individuals are more likely to group together the more similar they are in terms of age and gender, the more closely related they are genetically, and the more memberships in organizations with an economic purpose they have in common. However, genetically related individuals tend to distrust one another and so do not group when enforcement depends on intrinsic motivations alone. When endogenous intrinsic incentives can also provide a basis for enforcement, the relatively robust relationships associated with belonging to the same religious congregation and being related by marriage come to the fore, while co-members in economic organizations tend not to group, possibly because they wish to protect their relatively vulnerable but highly valued relationships. We found no evidence of assortative grouping on risk preferences, possibly because other concerns such as the costs of group formation and issues of enforcement prevailed. However, ex post, co-group members' gamble choices converge.

The paper is organized as follows. We begin in Section 2 by explaining the experimental design, conceptual framework and testing strategy. Data sources and variable construction are discussed in Section 3. In Section 4, we present results from the group formation regressions. Section 5 investigates convergence in risk taking behaviour within groups. Section 6 concludes.

2 Empirical strategy

2.1 Experimental design

The experiment was organized around a risk preference elicitation game. One series of two experimental rounds was played in each village, the rounds each taking between one and two

hours and being held on consecutive days. In every village, each household was invited to send one adult, preferably the household head or his/her spouse, to the series in their village. In the first round, played on day 1, each participant was interviewed privately and asked to select one of six possible gambles g, ranked from the least (1) to the most risky (6). The gamble choice set was the same for all participants. Riskier gambles had higher expected returns. This game structure was originally used by Binswanger (1980) to elicit risk preferences: the choice of gamble implies a range of possible values for the individual's coefficient of relative risk aversion. The gambles used in our experiment are presented in Table 1 together with the implied ranges of the risk aversion coefficient.[2]

At the end of the first round, participants were invited to return and play the gamble choice game again the next day. Participants were then given the opportunity to form 'sharing groups' with other participants from the same village. Within 'sharing groups', second round winnings were pooled and shared equally. The verbal framing of the game was kept to a minimum and, as a consequence, can be likened to a variety of real life situations, including risk sharing, which has been extensively studied in village communities (e.g. Udry 1994, Ligon et al. 2001, Fafchamps and Lund 2003), and group lending with joint liability, a topic that has attracted significant attention recently (e.g. Besley and Coate 1995, Ghatak 2000, Ghatak 1999). There is no lending in our experiment, but participants de facto invest a sure amount (gamble 1) into various risky investments (gambles 2 to 6) – see Table 1.[3]

We are interested in the effect of the institutional environment on the composition of sharing groups and the subsequent risk taking behavior of their members. So, we randomly assigned

[2]The gambles are expressed in Zimbabwean $. The official exchange rate at the time of the experiment was around Z$55 for US$1 while the black market rate was around 2.5 times that amount. In the areas where the experiment was conducted and at the time of the experiments, the daily wage for a farm labourer was around Z$200, similar in magnitude to the average winnings per round from the experiment of Z$158 in round 1 and Z$172 in round 2.

[3]The experiment is also akin to a business partnership within which the returns from a series of risky investments are shared.

villages to a control or one of three treatments during the second round. In the control villages, the possibility of forming sharing groups was not offered.[4] The three treatments vary with respect to the enforcement mechanisms available to keep people in their risk sharing groups ex post.

In the first treatment, risk sharing was exogenously enforced by the experimenter: having joined a sharing group, the members could not subsequently change their mind. So, regardless of the outcomes of all their gambles, their winnings were pooled and shared equally.

In the second treatment, members of sharing groups could secretly leave their groups, taking their second round gamble winnings with them, after the outcome of their own gamble had been realized. In that case, they did not receive a share of the winnings of others in their group. Since defection is unobserved by other villagers, enforcement in this treatment can only be intrinsic, i.e., it relies on individuals' preferences for altruism, reciprocity, and avoiding feelings of guilt.

The third treatment differs from the second in that members of sharing groups who chose to leave had to confirm their decision in front of everyone participating in the same experimental session. In this treatment, enforcement is extrinsic but endogenous in the sense that it relates to the importance that individuals place on their repeated economic and social interactions within the village and whether and how they think these interactions might be damaged should they defect publicly during the experiment.

Under each treatment, the consequences of and rules relating to sharing group formation and defection were explained to the participants at the end of the session on day 1. The second round gamble choices were, once again, made during private interviews and no rules were applied to or recommendations made concerning gamble choices within groups.

[4]Participants in control villages could have agreed to share the proceeds from the game amongst themselves even though no mechanism to do so was provided as part of the game. However, ex post interviews with key informants in each control village indicated that this did not happen.

2.2 Conceptual framework

The principle objective of the paper is to explore the factors affecting the formation of risk sharing relationships. To this effect we need a conceptual framework that will help us make sense of different group formation decisions. Here, we first discuss group formation under the assumption that risk sharing arrangements are perfectly enforced. We then explore the implications of different enforcement mechanisms.

There are two sources of efficiency gain from risk sharing: the transfer of risk from more risk averse to less risk averse agents; and the reduction in risk resulting from the pooling of uncorrelated shocks. Our focus is the latter: the transfer of risk requires asymmetric contracts in which the insured pays a premium to the insurer in exchange for insurance,[5] whereas in our experiment the sharing rules are symmetrical – all participants face the same choice of gambles and winnings are shared equally.

If there are no restrictions on how winnings are shared, efficiency gains from risk pooling are maximized when all risk averse participants join a single group. Pooling risk enables them to take more risky gambles yielding a higher expected return. However, when group members are forced to share equally, individuals with different risk preferences may prefer not to pool risk with one another (e.g. Hoff 1996, Ghatak 2000).

The rationale for such equilibria can be understood with a simple example. Say there are two possible gambles, one safe, one risky, and two types of agent, one moderately risk averse and one very risk averse. Further assume that very risk averse agents prefer to choose the safe gamble and pool risk, and moderately risk averse agents prefer the risky gamble but also derive some benefit from pooling. Forcing the two groups to pool their winnings may reduce the utility

[5] For a discussion of how this also applies to contracts enforced via repeated interaction, see for instance Platteau (1995) and Fafchamps (1999).

of both: very risk averse agents dislike the increase in risk associated with the gamble choices of less risk averse agents, and less risk averse agents dislike the reduction in expected returns associated with the gamble choices of the very risk averse agents. In these circumstances, we expect to observe multiple groups sorted by risk preferences: less risk averse individuals join with similar individuals and select high risk, high return gambles, while more risk averse individuals join other, similarly risk averse individuals and select safe gambles.

Multiple risk pooling groups may also arise as a consequence of group formation costs. This is because the marginal insurance benefit from a larger group falls with group size, while the marginal cost of adding a new member is likely to either remain constant or increase with group size. The costs of group formation are likely to play a role in the experiment not least of all because participants were given less than a day to form groups. In practice, many groups were formed on the morning of day 2, while participants were registering for the second round. When group size is constrained by group formation costs, assortative grouping by risk preferences may still occur, but participants with similar risk preferences may be divided into several groups. Furthermore, in the presence of group formation costs the net benefit from risk pooling need not be positive for individuals with low risk aversion. Consequently, such individuals are less likely to group.

Group formation costs are likely to vary between pairs of agents depending on their individual characteristics and geographical and social proximity (Jackson 2008), where the latter might take the form of family ties or co-memberships in religious congregations or community-based organizations. If such variations in group formation costs are significant and large, assortative matching by risk preferences may not take place.

Equipped with a better understanding of the factors affecting group formation when the agreements are exogenously enforced (treatment 1), we now explore the likely effects of enforce-

ment based on intrinsic and endogenous extrinsic incentives (treatments 2 and 3). We first note that, in the absence of intrinsic or endogenous extrinsic incentives, a participant's best response is to cheat, i.e., as long as other group members respect their promise to share their winnings, it is always in a participant's selfish interest to join a group, take the highest risk gamble, share winnings if he realizes a low payoff, and defect if he realizes a high payoff. If all participants are aware of this, it is not in anyone's interest to form a group. Consequently, we expect groups to be formed only by individuals who believe they face sufficient incentives – intrinsic or extrinsic – not to cheat each other.

In treatment 2, only intrinsic incentives are at work. Consequently, we expect participants to form groups with those who they are more inclined to trust, who they believe feel more altruistic towards them or to whom they feel more altruistic. Participants may, in accordance with Hamilton (1964), expect more altruism from close kin. They may also be more inclined to trust those with whom they easily identify, such as participants of the same gender, age cohort, or ethnicity (Akerlof and Kranton 2000), or with whom they share religious beliefs and interact frequently via community-based organizations. So, we expect religion, kinship, co-membership in various groups, and demographic similarities to be more strongly associated with group formation under treatment 2 than under treatment 1. We also expect groups to be smaller under treatment 2.

In treatment 3, while intrinsic incentives remain, extrinsic incentives are also brought into play. In models of repeated interaction between self-interested agents *à la* Coate and Ravallion (1993), enforcement is facilitated by agents anticipating, valuing and so wishing not to jeopardize future interactions. In our experiment the risk sharing is not repeated. However, in treatment 3, because individuals can only defect publicly and because group formation is conducted non-anonymously, the decision to group with another individual may take on the characteristics of one

embedded within a series of repeated interactions. In this context, the extrinsic incentives that are salient to the issue of enforcement relate to threatened reductions in the anticipated value of future social or economic interactions. If an individual is excluded from future interactions the value of those interactions is lost completely. And even if they are not excluded, the value of the interactions may be reduced due to feelings of shame or a weakened bargaining position if they publicly defect within the experiment. To the extent that this happens, we would expect, first, more group formation under treatment 3 than under treatment 2 and, second, for group formation under treatment 3 to be more strongly associated with existing, repeated interactions such as co-memberships in religious and community-based organizations, especially where those interactions are voluntary.

However, it is important to bear in mind that, while the group formation under treatment 3 may be viewed as one in a series of repeated interactions, it still remains unique not least of all in terms of the explicit nature of the rules that apply. And it is this uniqueness that provides the most likely explanation as to why Barr and Genicot (2008) observed less group formation under treatment 3 than under treatment 2. In the light of this finding Barr and Genicot (2008) theoretically explored the implications of individuals effectively asking themselves 'Is it worth me jeopardizing my ongoing repeated interaction with j by joining the same group as j in this unusual and unique situation and then having it turn sour because one of us cheats?' They formally show that, if either (1) extrinsic incentives involve costs of the type described above not only for the cheater but also the cheated or (2) individuals have inconsistent time preferences and so fear that they would defect on learning that the winnings from their gamble were high even though they know that this would jeopardize or devalue anticipated future interactions, then less group formation will occur under treatment 3 than under treatment 2. Both theories also predict that pairs of individuals who are involved in highly valued repeated interactions,

especially where continued involvement is voluntary, will refrain from grouping under treatment 3 but, ceteris paribus, not under treatment 2. Here, by aligning the experimental data with data on existing repeated interactions through kinship, religious and community-based organization memberships, we can test this second prediction.

To summarize: pairs of individuals who are linked by existing relationships and are demographically similar may face lower group formation costs and so may be more likely to group under all three treatments; existing relationships and demographic similarities that are associated with altruism and mutual trust are likely to be even more important under treatment 2 than under treatment 1; the value of the future series of interactions associated with a relationship and how voluntary that relationship is, could enhance the extrinsic incentives that support group formation under treatment 3, however, pairs of individuals with such relationships may choose not to group during the experiment in order to protect these relationships from the unavoidable consequences of one or other party succumbing to the temptation to defect; and group formation may be assortative with respect to risk preferences, although this effect may not be observed if the other aforementioned factors vary greatly across potential co-grouping pairs.

And what if group formation is not assortative on risk preferences due to prevailing variations in other factors? Should we expect group joining individuals to adjust their risk taking behaviour to bring it into line with the preferences of their group? If neither enforcement nor self-commitment were an issue, we would expect grouping individuals to take more risk in the second round since group formation reduces individual exposure to risk. And, having controlled for this, we would also expect individuals' risk taking in the second round to be correlated with their risk preferences, as revealed by their first round gamble choices. However, if enforcement and self-commitment concerns remain after the group formation decisions have been made risk-taking in the second round may deviate from this prescribed pattern.

First, consider the issue of self-commitment under treatment 3: a grouping individual, who fears the temptation to defect in public should his gamble yield a high payoff, may choose a low-return-low-risk gamble in order to minimize that temptation. And second, consider the importance of signaling an intention not to defect in treatments 2 and 3: being able to state with conviction that one chose a relatively low-return-low-risk gamble in the second round is an effective way of signaling an intention not to defect.[6] Finally, under all three treatments, we might expect co-group members who are heterogeneous in terms of their risk preferences to negotiate an agreement to converge on a set of gamble choices that, at least to some extent, suites their collective preferences. The experimental design provided no mechanism whereby such agreements could be enforced, but to the extent that group members may be coerced into revealing their gamble choices and outcomes ex post, pressure could have been brought to bear on those choosing gambles deemed too risky or too safe.

Having summarized the salient predictions from the literature, we turn to the empirical testing strategy. This is organized around two sets of regressions, one focusing on group formation, the other on risk taking.

2.3 Empirical formulations: Group formation

To study group formation, we follow Fafchamps and Gubert (2007) and Arcand and Fafchamps (2008) and estimate a dyadic regression model of the following form. Let $m_{ij} = 1$ if i forms a risk pooling group with individual j, and 0 otherwise. The network matrix $M \equiv [m_{ij}]$ is symmetrical since $m_{ij} = m_{ji}$ by construction. As noted by Fafchamps and Gubert (2007), this implies that the regressors must enter the regression model in symmetric form. We therefore

[6] This point has been explored in depth by Platteau (1996) who argues that, in rural African societies, sharing norms induce people not to seek personal enrichment. Risk taking is seen as an effort to leave the group and rise above one's peers and the consequent social pressure leads to conformism.

estimate the following model:

$$\begin{aligned} m_{ij} &= \beta_1 d_{ij} + \beta_2 t_{ij} + \beta_3 (t_{ij} * d_{ij}) + \beta_4 |z_i - z_j| \\ &\quad + \beta_5 (z_i + z_j) + \beta_6 |g_i^1 - g_j^1| + \beta_7 (g_i^1 + g_j^1) + u_{ij} \end{aligned} \tag{1}$$

where d_{ij} is a vector of the characteristics of the relationship between individuals i and j, i.e., their social and geographic proximity and indicators of whether they are engaging in repeated interactions and how voluntary those interactions are, t_{ij} is a vector of dummy variables indicating which treatment individuals i and j played under, g_i^k denotes the gamble choice of individual i in round $k = \{1, 2\}$, z_i is a vector of the relevant characteristics of individual i, and $\beta_1, \beta_2, \beta_3, \beta_4, \beta_5, \beta_6$, and β_7 are the parameters to be estimated.

Regression model (1) is used to test the predictions presented in the previous subsection. Regressor $|g_i^1 - g_j^1|$ is the difference in gamble choices in Round 1, our proxy for differences in risk preferences. A significantly negative coefficient β_6 is evidence of assortative grouping by risk preferences. The prediction that less risk averse individuals are less likely to enter into risk sharing is tested by including $g_i^1 + g_j^1$ as a regressor. A significantly negative coefficient β_7 can be taken as evidence in support of this prediction.

Tests of the predictions regarding the effects of group formation costs, intrinsic motivations, and endogenous extrinsic incentives relate to the significance and the signs of the coefficients in β_1, β_3, and β_4. Significantly, positive elements in β_1 can be taken as evidence that group formation costs are important and decline with social proximity. Significantly negative elements in β_4 can be taken as evidence that i's and j's who are more similar in terms of characteristics z are more likely to join the same group, most probably because they face lower group formation costs. We shall look in more detail at predictions relating β_3 after we have presented our data.

Significant elements in β_2, identify the impact of the treatments on the likelihood of any individual joining a group. A negative coefficient on a dummy variable indicating treatment 2 and a larger negative coefficient on a dummy variable indicating treatment 3 would be consistent with the findings of Barr and Genicot (2008).

Finally, significant elements in β_5 identify individual characteristics that are associated with the formation of groups. To see why, suppose that individuals with a large value of z form larger groups. This implies that $E[m_{ij}]$ is an increasing function of $z_i + z_j$ – and hence that β_5 is positive.

Model (1) is estimated using a logit. When estimating model (1) it is essential to correct standard errors for non-independence across observations. Non-independence arises in part because residuals from dyadic observations involving the same individual i are correlated – negatively or positively – with each other. Standard errors can be corrected for this type of non-independence by clustering either by dyad as proposed by Fafchamps and Gubert (2007), or by village (and, hence, experimental session). The second approach corrects for possible non-independence not only within dyadic pairs sharing a common element but also across all the dyads participating in the same experimental session. Because we have data from 14 village sessions we are able to apply the second, more rigorous approach. In addition, in some specifications we include village fixed effects to control for all village-level unobservables. Their inclusion renders pure treatment effects unidentifiable, but enhances the robustness of all the other estimated coefficients.

2.4 Empirical formulations: Risk taking after group formation

In their original analysis of the experimental data, Barr and Genicot (2008) found that group members engaged in more risk taking only under treatment 1, when the risk sharing arrangements were exogenously and perfectly enforced. This is consistent with the hypothesis presented

above that issues of self-commitment and signaling my cause relatively risk neutral group members to choose less risky gambles in the second round under treatments 2 and 3. Here, to investigate this issue further, we look for evidence of convergence in gamble choices within groups and variations in the tendency to converge across treatments.

To test this hypothesis we estimate a model of the form:

$$\begin{aligned} |g_i^2 - g_j^2| &= \gamma_0 m_{ij} + \gamma_1 d_{ij} + \gamma_2 t_{ij} + \gamma_3 (t_{ij} * m_{ij}) + \gamma_4 |z_i - z_j| \\ &\quad + \gamma_5 (z_i + z_j) + \gamma_6 |g_i^1 - g_j^1| + \gamma_7 (g_i^1 + g_j^1) + u_{ij} \end{aligned} \tag{2}$$

Where, as before, g_i^k represents i's gamble choice in round k and the dependent variable $|g_i^2 - g_j^2|$ is the absolute difference between $i's$ and $j's$ gamble choices in Round 2. If participants played in Round 2 as they played in Round 1, $\gamma_6 = 1$. If $0 < \gamma_6 < 1$, it indicates that there is convergence in behavior irrespective of group membership: participants choose more similar gambles in Round 2 than in Round 1, perhaps because of learning. Conditional on this, if co-group members converge in terms of their gamble choices, then we should observe $\gamma_0 < 0$ and if the extent of this convergence is greater under treatments 2 and 3 due to issues of self-control and signaling and we use treatment 1 as the basis for comparison, we should observe $\gamma_3 < 0$.

If there is assortative grouping by risk preferences, we would expect gamble choices in Round 2 to be strongly correlated with gamble choices in Round 1. Under this scenario, having controlled for Round 1 gamble choices and found $0 < \gamma_6 < 1$, we would expect $\gamma_0 = 0$. If, on the other hand, the effects of variable group formation costs, intrinsic motivations, and endogenous extrinsic incentives prevail and, hence, bring together individuals with different risk preferences, one may expect little or no correlation between Round 1 and 2 gamble choices – $\gamma_6 = 0$ or small – but convergence in Round 2 gamble choices – $\gamma_0 < 0$ – if co-group members moderate

their gamble choices so as to bring them into line with the risk preference profile of the group. And, once again, if the extent of this convergence is greater under treatments 2 and 3 we should observe $\gamma_3 < 0$.

3 The data

The experiments were conducted in 23 Zimbabwean villages in 2001. In this paper we use the data from 14 of these villages. Of the remaining 9, 3 made up the control sample in which no group formation was allowed and 6 were not fully enumerated during the various surveys upon which we draw. Of the 14 villages in our sample, 10 were established in the early 1980s as result of land redistribution. These resettled villages are relatively small and geographically concentrated. Due to the random selection of settlers, the inhabitants of these villages are less likely to have kinship ties to each other. However, they engage more in associational activity and have more marriage ties within the village (. Barr 2004, Dekker 2004)

The participants' individual characteristics, including their age, gender, education level and their position within the household, were collected at the time of the experiment. Data on household incomes and wealth (in livestock) are obtained from the Zimbabwe Rural Household Dynamics Study (ZRHDS), collected by Bill Kinsey and composed by Owens and Hans Hoogeveen. Kinsey, Burger and Gunning (1998), Gunning, Hoddinott, Kinsey and Owens (2000), and Hoogeveen and Kinsey (2001) discuss this dataset in detail.

The data on memberships in various religious and civil groups and organizations are drawn from a survey by Barr in 2000 (see Barr (2004) for details). For the purpose of the analysis presented here, civil groups and organizations are divided into those that have an explicit economic purpose – e.g., micro-finance, mutual insurance, funeral societies, irrigation and livestock rearing cooperatives – and those that are primarily social – e.g., sports and dance clubs and

choirs. Co-memberships in the former are assumed to be both voluntary and associated with valuable series of repeated interactions. Co-memberships in the latter, while voluntary are assumed to be less valuable. Co-memberships in religious organizations are assumed to be less voluntary in the sense that an individual who wishes to express their religious convictions must tolerate their co-congregationists and less valuable in the sense that it is not the relationships with co-congregationists that are the principal concern in this context, but rather the act of worship and the expression of religious convictions. This notwithstanding, co-memberships in religions may support mutual trust.

We also have detailed information on the family and marriage ties between the participants. These kinship data are derived from specifically designed social mapping exercises conducted in 1991 and 2001 by focus village groups involving at least one representative from each household residing in each village (Dekker 2004). Blood ties are assumed to be associated with altruism and to be involuntary. Marriage ties between in-laws — recall that our design precluded married couples turning up to sessions – are associated with many reciprocal obligations in Zimbabwe and may be highly valued as a result. However, they are not voluntary in the sense that, no matter how displeased a Zimbabwean is with his in-laws the reciprocal obligations remain and the repeated interaction is highly likely to continue.[7]

Combining the assumptions stated here about the characteristics of the different ties that might exist within dyads and are captured by our data with our earlier predictions relating to the impact of the set of available enforcement mechanisms on group formation we can make the following statements about coefficients β_3 in model (1). Significantly positive coefficients on interactions between the treatment 2 dummy and the tie characteristics described in the

[7] We have no information on geographical proximity. However, given that average proximity varies village by village, with resettled villages tending to be more nuclear and traditional villages more disbursed, the inclusion of village fixed effects in the analysis should control for a substantial part of any geographical proximity effect.

preceding two paragraphs can be taken as evidence that intrinsic motivations are important and will identify the types of relationships that are associated with altruism and trust. A significantly positive coefficient on the interaction between the treatment 3 dummy and co-memberships in organizations with an economic purpose can be taken as evidence that voluntarily repeated interaction is facilitating enforcement in the experiment. A significantly negative coefficient on the interaction between the treatment 3 dummy and co-memberships in organizations with an economic purpose can be taken as evidence that individuals are choosing to protect these relationships from the unavoidable consequences of a party to such a relationship being tempted to defect within the experiment. And significantly positive coefficients on interactions between the treatment 3 dummy and other tie characteristics can be taken as evidence that these tie characteristics provide a conducive environment of enforcement through endogenous extrinsic incentives. Here, the less vulnerable marriage ties and co-memberships in religious congregations might be of importance.

4 Summary statistics

Table 2 presents the characteristics of the 382 participants who took part in both rounds of the experiment in the 14 villages.[8] These observations form the basis for our analysis. Across the sample just under half of the participants joined sharing groups in the second round. The sample is evenly split between men and women. The average participant is middle-aged and has slightly more than primary education. Two thirds of the sample is married and is either a household head or the spouse of a household head. Both annual household monetary income

[8] Of the participants in the first round in these villages, 19 did not turn up on the second day, sending a replacement from the same household in their stead. Because we do not have first round gamble choice data for the replacements, they are excluded from the analysis that follows. However, if we do not control for gamble choice in the group formation regressions and include the replacements, the other findings remain qualitively unchanged.

and livestock wealth are approximately log-normally distributed and are incorporated into the analysis in log form.[9]

With respect to membership in various groups and organizations, we see that the majority of participants belong to a religious group – most often one of the many apostolic religions indigenous to Zimbabwe. On average, participants belong to between three and four economic groups and between no and one social group.

Turning now to the characteristics of the relationships between the participants, we first use the kinship data to construct a relatedness variable indicating the proportion of genes two individuals are likely to share. A mother and her child, for instance, share half of their genes – the other half of the child's genes being provided by the father. Two siblings are also expected to share half of their genes. Among non-human species there is now a considerable body of evidence supporting Hamilton's hypothesis that altruism is positively associated with relatedness (Brembs 2001).

We also construct a network measure of social proximity due to marriage. To this effect, we begin by constructing, for each village, a social network based on available information on blood and marriage ties across all adult members of the village. Then we calculate the distance within this network between all villagers i and j, where distance is defined as the length of the shortest path between i and j. When i and j are not in the same component of this network, the distance between them is not defined – i.e., it is infinite. To overcome this practical difficulty, we follow Fafchamps, Goyal and van der Leij (2005) and use proximity defined as 1/distance, instead. Note that in our analysis this variable identifies in-laws as spouses are in the same household. Finally, to isolate the effect of proximity due to marriage, we set the resulting proximity variable

[9] To avoid losing observations with no income or livestock wealth, we use log(crop income+1) and log(livestock wealth+1), where livestock wealth is measured in money terms using local market prices for trained oxen, data on numbers of livestock of different types and applying the following weights during calculations: trained oxen 1.00; cow 0.71; bull 0.83; young oxen 0.59; calf 0.18; sheep 0.08; goat 0.06; pig 0.06 (Hoogeveen and Kinsey 2001)

equal to zero if i and j are genetically related.

Table 3 summarizes the data on the characteristics of the link between the experimental participants. Only pairs from the same village are considered. We see that only 7% of all possible within-village pairs of participants joined the same risk sharing group. Even under treatment 1 where there was perfect exogenous enforcement, only 12% of the pairs joined the same group – well below the 100% required for efficient risk sharing (without transactions costs). This suggests either that the equal sharing rule prevented the formation of groups by participants with dissimilar risk preferences, or that group formation costs and enforcement issues prevailed in their affect on grouping decisions. We revisit this point in the next section.

Only 21% of the pairs are co-members of the same religion. This reflects the diversity of faiths present in each of the studied villages. The average pair has just under one economic group memberships in common, but few share a social group membership. There is, however, significant variation in both co-group memberships variables, with the maxima for common economic and common social group memberships being 9 and 2 respectively. The relatedness variable takes values between zero and 0.5 (the theoretically possible maximum in the absence of in-breeding). However, for all but 2 percent of the dyads in our sample relatedness is zero and, as a consequence, average relatedness is very low. This is due to the prevalence in our sample of resettled villages which at their inception were made up of stranger households. Furthermore each household could only send one participant to the experimental sessions. Social proximity due to marriage is more commonplace: this variable takes a value greater than zero for almost 20 percent of the dyadic sample, with the maximum value being 0.5 (indicating parents-in-law or siblings-in-law). The relative prevalence of marriage ties has been emphasized by Dekker (2004) who found that, in resettled villages, marriage has been used to create social ties between households that initially shared nothing in common.

The dyadic sample is unbalanced across treatments, with treatment 2 being under-represented. This is the result of having to drop a number of villages due to incomplete data. The dyadic data on gamble choices is presented in the last panel of Table 3. Since the gamble choice variables take values from 1 to 6, the maximum difference in gamble choices is 5 and the maximum sum is 12. The average absolute difference is 1.23 in round 1 and 1.13 in round 2, suggesting some convergence in gamble choices between the two rounds. We also see that the average sum of gambles $g_i + g_j$ rises between the two rounds, supporting Barr and Genicot (2008)'s finding that some participants chose riskier gambles in the second round.

5 Empirical results: Group formation

The coefficient estimates for model (1) are presented in Table 4. The logit regression in the first column includes treatment dummies but no village fixed effects and no interaction terms. The value of this regression is that it identifies the raw treatment effects. In the second column the treatment dummies are replace with a full set of village fixed effects. This regression is reported merely as a stepping-stone. The regression in the third column includes the all important interaction terms between treatments and the variables capturing the characteristics of the dyadic relationships.

We begin by noting that neither the difference nor the sum of first round gamble choices is ever significant. That β_6 is not significantly different from zero implies that group formation is not assortative with respect to risk preferences. And that β_7 is not significantly different from zero implies that more risk averse individuals are no more likely to join sharing groups – or to join larger groups. These results suggest that the sharing groups were not formed in a way that would have maximized the mutual welfare gains from risk sharing in the absence of group formation costs. They are consistent with earlier findings based on survey data. For instance,

Fafchamps and Gubert (2007) and De Weerdt and Fafchamps (2007) both find that risk sharing relationships are no more likely between individuals with different risk profiles or abilities to bear risk.

This notwithstanding, there is evidence of assortative grouping with respect to gender and age, suggesting that participants find group formation less costly with others who are like themselves. Participants also find group formation less costly with co-members of economic groups and with blood relatives, although being related by marriage does not appear to be associated with lower group formation costs and neither does belonging to the same religion or choir, sports, or dance club.

Turning to the coefficients on the treatment dummies and interaction terms, we see that, in accordance with Barr and Genicot (2008), there is less group formation in treatment 3 – with extrinsic incentives. Treatment 2 is intermediate in terms of group formation, but statistically indistinguishable from treatment 1. Surprisingly, blood relatives are significantly less likely to group under treatment 2 suggesting that there is distrust or possibly rivalry between family members who live in different households. However, it is important to bear in mind that only 2% of the dyads in our sample are genetically related and that this very small proportion is then subdivided across the three treatments. Under treatment 3, co-members in economic organizations are significantly less likely to group, suggesting that valuable, voluntary relationships may be protected. In contrast, those belonging to the same religious congregation and those who are related by marriage are significantly more likely to group under this treatment, suggesting that these relatively involuntary, ongoing relationships that are associated with either an established moral code or well defined reciprocal obligations provide a good support for enforcement through endogenous, extrinsic incentives.

To investigate whether established moral codes associated with religious affiliations are im-

portant, we divide religious affiliation into two groups, apostolic and non-apostolic. In the study area religious affiliation is diverse but there are many differences in ethos between non-apostolic religions – mostly Catholics and Protestants – and apostolic faiths – which combine an evangelical outlook with an emphasis on local religious visionaries.[10] For our purpose, the difference is of interest because of the emphasis that apostolic faiths put on public contrition. Treatment 3 mimics the public contrition process – and the peer pressure that goes with it. Hence, members of apostolic faiths may better understand and value the enforcement mechanism available to them under treatment 3 and may, as a consequence, be more likely to group. In addition, this dissection of the religious co-membership variable may identify the effects of different established moral codes on intrinsic incentives.

The first column of Table 5 contains the salient coefficient from the regression presented in the third column of Table 4. Then, in the second column of Table 5 we present the salient coefficients from a regression that differs only in as much as we separate out co-memberships in non-apostolic and apostolic congregations. Thus, we see that both types of faith play a role in the significance of religious co-memberships in treatment 3 (coefficients statistically indistinguishable), while the apostolic faiths may provide better support for enforcement based on intrinsic incentives alone.

One other significant result is worthy of note. The significant positive coefficient on the sum of years of schooling indicates that more educated participants were more likely to join sharing groups and joined larger groups, possibly because they understood the insurance implications more fully.

[10] For a description of apostolic groups in Zimbabwe and how they differ from traditional Christian faiths, the reader is referred to Bourdillon, Mashita and Glickmann (1977) and Daneel (1977).

6 Empirical results: Second round gamble choices

The estimated coefficients for model (2), in which the dependent variable is the absolute difference in round 2 gamble choices, are presented in Table 6. As in Table 4, the regression in the first column contains treatment dummies but no village fixed effects, the regression in the second column contains village fixed effects, but no treatment dummies, and the regression in the third column contains village fixed effects and interaction terms between the co-group members (in the experiment) indicator and the treatment dummies. Note, first, that the coefficient on the difference in round 1 gambles $|g_i^1 - g_j^1|$ is positive and significant, but well below 1, in all three regressions. This indicates that participants tended to converge on a sub-set of gamble choices regardless of whether and how they chose to group. This may be due to learning. The regressions also show that there was a further, separate convergence effect associated with co-membership in a sharing group and the regression in the third column indicates that this convergence effect was no stronger in treatments 2 and 3. Thus, while we find no evidence of risk-taking in the second round being affected by issues of self-control or a desire to signal an intent not to defect from a group, we do find evidence of co-group members endeavouring to curb their risk-taking behaviour to suit the risk preference portfolios of their groups.

Two other significant results are worthy of note. First, older and better educated dyads choose less similar gambles, possibly an indication that they 'know their own mind'. And second, co-memberships in both economic and purely social organizations are associated with less similar gamble choices in round 2, while religious co-membership is associated with more similar gamble choices. Possibly, while religions encourage conformity, other types of organization encourage individual expression.

7 Conclusion

It is common for households to pool risk, for instance through mutual insurance arrangements or through group loans with joint liability. The mechanisms by which risk pooling arrangements are enforced varies with the institutional and legal environment. Our aim was to investigate the extent to which the enforcement mechanisms available affect who shares risk with whom and how this in turn affects subsequent risk taking behavior. Our approach was to apply dyadic regression analysis to data from a specifically designed behavioral experiment, two surveys and a genealogical mapping exercise.

We consider three types of contract enforcement mechanism – perfect enforcement by an outside party; enforcement through intrinsic incentives such as altruism and mutual trust; and enforcement through endogenous, extrinsic incentives relating to the fear of partial or full exclusion from future interactions.

We find evidence of variations in group composition across the three treatments. Contrary to expectations, genetic relatedness does not support enforcement based on intrinsic incentives, even though it is associated with lower costs of group formation in general. Co-memberships in community-based organizations that serve an economic purpose also appear to be associated with lower group formation costs and may, due to their value and voluntary nature, also support enforcement based on endogenous, extrinsic incentives. However, these qualities appear to lead experimental participants to keep such relationships out of play, thereby protecting them from the unavoidable consequences of someone being tempted to defect from their group within the experiment. In contrast, religious co-memberships and marriage relations unequivocally support enforcement based on endogenous, extrinsic incentives, possibly due to their less voluntary nature and links to defined moral codes and obligations. Further, apostolic faiths appear to engender trust or reciprocal altruism and, thus, support enforcement based on intrinsic motivations alone.

Finally, while we found no evidence of assortative group formation based on risk preferences, possibly because variations in other factors prevailed, we did find evidence of convergence in risk taking behaviour within groups ex post. And this convergence does not appear to be due to individuals either aiming to reduce the temptation to defect or signaling their intention not to defect. What remains to be investigated is whether this convergence is due to pure conformism or the result of an unobserved negotiation process.

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Tables

Table 1. Gamble choices in Z$ and implied relative risk aversion coefficients

Choice	High payoff	Low payoff	EV	RA class	RA coeff.
1	100	100	100	Extreme	infinity to 7.51
2	190	90	140	Severe	7.51 to 1.74
3	240	80	160	Intermediate	1.74 to 0.81
4	300	60	180	Moderate	0.81 to 0.32
5	380	20	200	Slight-neutral	0.32 to 0.00
6	400	0	200	Neutral-negative	0 to -ve infinity

Table 2. Characteristics of participants

	Mean	Std. Dev.
Subject Characteristics		
Female	52%	
Age	42.0	17.7
Years of schooling	6.8	3.2
Household head	42%	
Spouse of household head	21%	
Married	66%	
Annual income (Zim$)	2562	3374
Value of livestock wealth (Zim$)	11656	10124
Belongs to a religious community	88%	
Memberships in economic groups	3.30	2.85
Memberships in social group	0.38	0.61
Resettled household	76%	
Experimental variables		
Played under treatment 1	42%	
Played under treatment 2	23%	
Played under treatment 3	35%	
Joined a group in round 2	49%	
Average gamble choice in round 1	3.23	1.17
Average gamble choice in round 2	3.59	1.13
Observations	382	

Table 3. Descriptive statistics on dyadic data

	Mean	Std. Dev.
Group membership and social proximity:		
Chose to join same group in experiment	7%	
Chose to join same group under treatment 1	12%	
Chose to join same group under treatment 2	8%	
Chose to join same group under treatment 3	2%	
Belong to same religious group	20%	
Co-memberships in economic groups	0.940	1.134
Co-memberships in social group	0.083	0.290
Hamilton's relatedness index	0.005	0.038
Proximity due to marriage	0.031	0.072
Treatments:		
Treatment 1 (perfect, exogenous enforcement)	44%	
Treatment 2 (intrinsic enforcement only)	16%	
Treatment 3 (intrinsic and endogenous extrinsic)	40%	
Gambles:		
Difference in Round 1 gamble choice	1.230	1.066
Difference in Round 2 gamble choice	6.466	1.621
Sum of Round 1 gamble choices	1.144	1.005
Sum of Round 2 gamble choices	7.209	1.633
Observations	10470	

Table 4. Dyadic regressions on membership in the same risk pooling group

	(1)			(2)			(3)		
	coeff.	s.e.		coeff.	s.e.		coeff.	s.e.	
Risk aversion proxy									
Absolute difference in 1st round gambles	0.052	0.066		0.004	0.093		0.003	0.093	
Sum of first round gambles	0.037	0.054		0.025	0.052		0.030	0.052	
Social proximity									
Belong to same religious group	0.141	0.132		0.269	0.140	*	0.031	0.093	
Co-memberships in economic groups	0.108	0.057	*	0.130	0.051	**	0.141	0.057	**
Co-memberships in social group	-0.151	0.185		-0.390	0.175	**	-0.323	0.336	
Hamilton's relatedness	0.740	0.874		0.830	0.753		0.993	0.572	*
Related by marriage not blood (1/distance)	0.662	1.106		0.131	1.224		-0.807	1.902	
Treatment effects									
T2 (intrinsic motivations only)	-0.149	0.541							
T3 (intrinsic and endogenous extrinsic)	-1.769	0.330	***						
Treatment effects interacted with social proximity									
T2 x belong to same religious group							0.258	0.196	
T2 x comemberships in economic group							0.200	0.204	
T2 x comemberships in social group							-0.289	0.467	
T2 x related by marriage not blood (1/distance)							-1.315	1.987	
T2 x Hamilton's relatedness							-2.388	1.172	**
T3 x belong to same religious group							1.053	0.215	***
T3 x comemberships in economic group							-0.302	0.146	**
T3 x comemberships in social group							0.236	0.592	
T3 x related by marriage not blood (1/distance)							4.560	2.616	*
T3 x related by blood (Hamilton's relatedness)							0.865	3.300	
Absolute difference in individual characteristics									
Difference in gender	-1.903	0.425	***	-1.979	0.405	***	-1.997	0.391	***
Difference in age	-0.013	0.009		-0.016	0.008	**	-0.017	0.008	**
Difference in head or spouse dummy	-0.235	0.187		-0.174	0.155		-0.171	0.157	
Difference in years of schooling	0.022	0.041		0.028	0.043		0.026	0.043	
Difference in log of household income	0.090	0.090		0.063	0.054		0.059	0.053	
Difference in log of livestock wealth	0.036	0.051		0.014	0.038		0.010	0.037	
Sum of individual characteristics									
Sum of female dummies	0.101	0.116		-0.005	0.139		-0.006	0.144	
Sum of ages	0.006	0.005		0.001	0.004		0.002	0.004	
Sum of head and spouse dummies	-0.142	0.167		0.121	0.141		0.105	0.136	
Sum of years of schooling	0.039	0.036		0.065	0.032	**	0.067	0.034	**
Sum of log household income	-0.079	0.088		-0.034	0.075		-0.033	0.079	
Sum of log of livestock wealth	0.060	0.045		0.006	0.038		0.004	0.040	
Intercept	-2.760	1.728		-2.441	1.373	*	-2.320	1.400	*
Village dummies included	no			yes			yes		
Pseudo R-squared	0.171			0.215			0.224		
Observations	10470			10470			10470		

Notes: All standard errors adjusted to account for non-independence within villages; *** significant at 1%; ** significant at 5%; * significant at 10%.

Table 5. Investigating the source of the religion effect - apostolic or non-apostolic

	(1)		(2)	
	coeff.	s.e.	coeff.	s.e.
Social proximity				
Belong to same religious group	0.031	0.093		
Belong to same apostolic group			-0.209	0.242
Belong to same non-apostolic religious group			0.202	0.170
Treatment effects interacted with social proximity				
T2 x belong to same religious group	0.258	0.196		
T2 x belong to same apostolic group			0.581	0.273 **
T2 x belong to same nonapostolic religious group			0.010	0.294
T3 x belong to same religious group	1.053	0.215 ***		
T3 x belong to same apostolic group			0.991	0.358 ***
T3 x belong to same non-apostolic religious group			1.464	0.259 ***

Notes: Coefficients and standard errors reported in column 1 are copied from regression (3), Table 4. Both regressions also contain all the other variables contained regression (3), Table 4. All standard errors adjusted to account for non-independence within villages; *** significant at 1%; ** significant at 5%; * significant at 10%.

Table 6. Difference in Round 2 gamble choices

	(1)			(2)			(3)		
	coeff.	s.e.		coeff.	s.e.		coeff.	s.e.	
Co-group members									
Belong to same risk pooling group	-0.262	0.102	**	-0.277	0.112	**	-0.327	0.169	*
Treatment effects									
T2 (intrinsic incentives)	-0.377	0.060	***						
T3 (intrinsic and extrinsic incentives)	-0.139	0.079							
Treatment effects interacted with cogroup members									
T2 x Belong to same risk pooling group							0.216	0.196	
T3 x Belong to same risk pooling group							0.063	0.241	
Risk aversion proxy									
Absolute difference in 1st round gambles	0.092	0.036	**	0.085	0.033	**	0.085	0.033	**
Sum of first round gambles	-0.049	0.022	**	-0.047	0.026	*	-0.046	0.026	*
Social proximity									
Belong to same religious group	-0.071	0.036	*	-0.074	0.037	*	-0.076	0.039	*
Co-memberships in economic groups	0.068	0.034	*	0.068	0.034	*	0.068	0.035	*
Co-memberships in social group	0.159	0.067	**	0.093	0.042	**	0.092	0.043	**
Hamilton's relatedness	-0.322	0.329		-0.343	0.316		-0.340	0.315	
Related by marriage not blood, 1/distance	-0.061	0.388		-0.103	0.436		-0.101	0.435	
Absolute difference in individual characteristics									
Difference female dummy	0.040	0.035		0.046	0.034		0.044	0.033	
Difference in age	0.003	0.002		0.003	0.001	*	0.003	0.001	*
Difference head or spouse dummy	-0.003	0.033		0.005	0.031		0.005	0.031	
Difference in years of schooling	-0.002	0.007		-0.005	0.007		-0.004	0.007	
Difference in log income	-0.031	0.023		-0.033	0.023		-0.033	0.023	
Difference in log livestock equivalent	-0.007	0.015		-0.005	0.014		-0.006	0.014	
Sum of individual characteristics									
Sum female dummy	-0.094	0.052	*	-0.065	0.057		-0.064	0.056	
Sum of age	0.005	0.001	***	0.004	0.002	**	0.004	0.002	**
Sum head or spouse dummy	0.032	0.047		0.039	0.046		0.038	0.046	
Sum in years of schooling	0.024	0.006	***	0.027	0.007	***	0.027	0.007	***
Sum of log income	0.009	0.011		0.009	0.019		0.009	0.019	
Sum of log livestock equivalent	-0.006	0.011		-0.002	0.010		-0.002	0.010	
Intercept	0.730	0.328	**	0.378	0.336		0.378	0.334	
Village dummies included	no			yes			yes		
Pseudo R-squared	0.070			0.084			0.084		
Observations	10470			10470			10470		

Notes: All standard errors adjusted to account for non-independence within villages; *** significant at 1%; ** significant at 5%; * significant at 10%.