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Bridging the gender divide: An experimental analysis of group formation in African villages*

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Abstract

Assortative matching occurs in many social contexts. We experimentally investigate gender assorting in sub-Saharan villages. In the experiment, co-villagers could form groups to share winnings in a gamble choice game. The extent to which grouping arrangements were or could be enforced and, hence, the distribution of interaction costs were exogenously varied. Thus, we can distinguish between the effects of homophily and interaction costs on the extent of observed gender assorting. We find that interaction costs matter − there is less gender assorting when grouping depends on trust. In part, this is due to trust based on co-memberships in gender-mixed religions.

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1 Introduction

Assortative matching – the tendency for people to interact with others who are similar to themselves more often than with others who are dissimilar – has been observed along many different dimensions and in many different networking and group formation contexts. The dimensions studied include race, religion, age, education, occupation and sex. The types of ties that have been investigated include acquaintanceship, friendship, co-membership in voluntary organizations, advice seeking, and support. And the contexts include whole nations, communities, workplaces, and school classes. See for instance the evidence cited in McPherson, Smith-Lovin and Cook, 2001, Hitsch et al. 2005, Belot and Francesconi 2006, Fisman et al. 2008, Goyal 2007, and Jackson 2009.

Wherever it is observed and irrespective of the characteristics upon which it is based, assortative matching is a cause for potential concern as it "limits people's social worlds in a way that has powerful implications for the information they receive, the attitudes they form, and the interactions they experience" (McPherson, Smith-Lovin and Cook, 2001: p. 415). Despite these concerns, we know very little about the causes of assortative matching. It could be driven by homophily, i.e., the desire to interact with similar people, or by opportunity and the corresponding variations in group formation and search costs. It could also result from equilibrium sorting on certain attributes, as in Becker's (1973) model of the marriage market.

Some evidence supports the idea that opportunity matters. The distribution of a population across salient types or with respect to salient characteristics is clearly important. However, in general, this explains only a proportion of observed assortative matching. Zipf (1949) and Gans (1968) among others present evidence that is consistent with geographical proximity causing assorting, especially with respect to race, ethnicity, class, education, and occupation. Fischer (1982) shows that patterns of assortative matching tend to get stronger as more types of relationship are shared, i.e., that assortative matching is in some sense cumulative.

Causality is difficult to establish, however. A few longitudinal survey studies (Hallinan & Smith 1985, Burt 2000) and a large number of experimental studies in social psychology (see Huston & Levinger 1978 for a review) have enabled researchers to distinguish between selection and socialization effects with the weight of evidence supporting selection when the assorting relates to attitudes, beliefs, and values. But there is a great deal more to be learnt.

The aim of this paper is to shed light on the factors associated with assortative matching in the context of group formation. We are interested in understanding the mechanisms that lead informal groups in African rural communities to self-segregate on the basis of gender. To our knowledge, assortative matching by gender has not been studied in sub-Saharan Africa. In the countries where it has been studied, it is not generally as prevalent as assorting based on race, education, and age (McPherson, Smith-Lovin and Cook, 2001). However, it has been found to vary markedly according to the context, being weaker within kin groups (Marsden, 1987) and stronger in more sex segregated environments such as in the workplace and, in the US, in voluntary organizations (Bielby & Baron 1986, Kalleberg et al 1996, McPherson & Smith-Lovin 1982, 1986, 1987, Popielarz 1999). Other variations relate to the type of individual being studied, with assortative matching on gender being stronger in young children (Smith-Lovin and McPherson 1993, Maccoby 1998), less educated individuals and among African Americans and Hispanics as compared to Anglo Saxons in the US (Marsden 1987, Blau et al 1991, Verbragge, 1977). Extrapolating from these regularities, one might expect grouping by gender to be common in rural Africa and other parts of the rural developing world.

This expectation is supported by causal empiricism. Anyone who has convened meetings in sub-Saharan villages has witnessed men and women dividing into their respective groups before taking their seats. A similar tendency is reflected in the way group-oriented development interventions are presented. Women and men are often separated during participatory research exercises on the grounds that they talk more freely (see, e.g., Chambers, 1994 and Welbourn, 1991). Micro finance similarly tends to be offered to single sexed – most often all female – groups on the grounds that it is empowering (Pitt et al, 2006) and that women are more responsive to social sanctions (Murdoch, 1999 and Rahman, 1998). Additionally, experimental work in a Nairobi slum shows men and women in single sex pairs make higher contributions in a one shot public good game, while sex heterogeneity reduces the contributions of women but not men (Greig and Bohnet, 2009). Yet, given that some research has shown that assortative matching has a cumulative effect and given the potential constraints that it places on information flows, attitudes, and aspirations, it is doubtful that reinforcing emergent assorting on gender is advisable.

To address this concern, we need to know more about what drives emergent assorting on gender in the formation of informal groups. To this end, we apply an economic perspective to an empirical analysis of assorting by gender within the context of a group formation experiment conducted in 14 Zimbabwean villages. Within the experiment, group formation is beneficial as it allows group members to share risk. This in turn enables them to take on more risk and, thereby, to secure higher expected rewards. By experimentally altering the institutional context, the costs associated with forming different groups are varied exogenously and randomly across the 14 villages.

In one treatment the grouping and risk sharing agreements are perfectly enforced by us, the experimenters. The only costs associated with co-grouping relate to the group formation process and to potential moral hazard, i.e., members of ego's group taking more risk than ego would prefer.[1] In a second treatment the grouping and risk sharing agreements are supported only by

[1] Theory predicts that under these circumstances individuals will choose to group with others who have similar levels of risk aversion to themselves (Barr and Genicot, 2008). (Barr, Dekker and Fafchamps, 2008) found no

trust. In this case the costs associated with co-grouping depend on the extent to which others can be trusted. In a third treatment the grouping and risk sharing agreements can be socially enforced, i.e., if someone wishes to renege on their group, they have to do so in public and therefore potentially expose themselves to the wrath of fellow villagers. In this third treatment the costs of grouping include as before the group formation and moral hazard costs. In addition, they also depend on the likely impact of the threat of social disapproval and on the value of pre-existing relationships that may be jeopardized if someone reneges.

Applying dyadic analysis to the experimental data combined with data from surveys and genealogical mapping exercises, we find that mixed gender dyads are significantly less likely to join the same group than same-sex dyads. This finding is consistent with the aforementioned studies based outside sub-Saharan Africa and focusing on other types of interaction. However, when groups are supported only by trust (second treatment) assortative matching by gender is less prevalent. No significant difference in assorting by gender is observed between the first and third treatments. This suggests that, in the context of our experiment, the extent to which individuals assort into groups based on gender depends on interaction costs that partly depend on the nature of the interaction being considered. When trust is essential, we observe less assortative matching by gender.

To help us interpret this finding, we examine whether the lower gender assorting in the trust treatment is associated with an increase in the importance of social network ties. Drawing from the literature on assorting and from our own understanding of the Zimbabwean villages in which the experiment was conducted, we investigate the possible roles of kinship, religious co-affiliation, and co-memberships in community-based organizations (CBOs). Kinship has been found to be associated with less assorting on gender in some contexts (Marsden, 1987), and it is likely to be

evidence of this in the dataset used here, but in a similar experiment conducted in Colombia and involving a much larger sample of individuals this tendency was observed (Attenasio, Barr, Cardenas, Genicot and Megir, 2009).

associated with higher levels of trust. Affiliation to the same religious community is associated with trust in the experimental literature (Fershtman and Gneezy 2001) and, while some religious congregations are gender specific, most are not. Further, while voluntary organizations in the US have been found to be highly segregated by gender, this is not always the case in our studied Zimbabwean villages. In these villages, choirs, dance groups, and football and netball clubs are highly sex-specific, but organizations serving an economic purpose are not. Using data from the same experiment, Barr, Dekker, Fafchamps (2008) found kinship, co-religion, and CBO membership to be associated with an increased likelihood of joining the same group. Here, we find that co-religion becomes more salient when grouping depends on trust and that this explains some but not all of the decline in gender assorting in the trust treatment. So, assorting by gender is not due to lower interaction costs associated with it being easier to trust members of the same sex. On the contrary, when trust is important gender assorting diminishes as non-assortative relations such as religious co-membership come to the fore.

The paper is arranged as follows. In Section 2 we present our conceptual framework and testing strategy. The data are discussed in Section 3. The empirical results are presented in Section 4. And, in Section 5, we conclude

2 Empirical strategy

We start by briefly summarizing the experimental design and conceptual framework before presenting our testing strategy.

2.1 Experimental design

The experiment involves a simple gamble choice game. One series of two rounds of this game was played in each of the 14 villages in the study. The rounds took between one and two hours each and were held on consecutive days. The day before the experiment started in each of the selected villages, each household was visited and invited to send an adult of a specific gender to be a subject in the experimental series in their village. Whether a man or a woman was requested was randomly determined, although if none of the specified gender was present, a member of the other was acceptable. They were told that, preferably, their representative would be either the household head or their spouse as these are the principle decision makers in the households.

In the first round, played the day after the recruitment, 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 with equally likely high and low earnings. Riskier gambles had higher expected returns. After selection of the gamble choice the game was played and realized gains were paid to the participants in private. 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]

Once the first round of gamble choices was complete the 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. They were told that, within 'sharing groups', second round winnings would be pooled and shared equally.[3]

[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. This is similar in magnitude to average experimental winnings of Z\$158 in round 1 and Z\$172 in round 2.

[3] 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 informal risk sharing, which has been extensively studied in village communities (e.g., Udry 1994, Ligon, Thomas and Worrall 2001, and Fafchamps and Lund 2003), and group lending with joint liability (e.g., Karlan 2007, Besley and Coate 1995, Ghatak 1999 and 2000). There is no lending in our experiment, but participants de facto invest a sure amount (gamble 1) into various risky investments (gambles 2

The economic literature has proposed three possible mechanisms to explain (positive) assortative matching, i.e., the observation that interacting individuals share many common characteristics (Belot and Francesconi 2006, Currarini et al, 2007). One possibility is homophily, namely, that people prefer to interact with people like themselves. A second possibility is opportunity: people economize on search and group formation costs by joining a group with individuals who are socially and geographically close. Because proximate individuals tend to share similar characteristics, this shows up as assortative matching. A third possibility arises when people share common preferences, i.e., they all prefer to interact with individuals with certain characteristics, and when group size is fixed. A canonical example of such a situation is the original marriage market model proposed by Becker (1973). In this model, all brides and grooms prefer a wealthy partner but because they can only marry one person and prefer to marry rather than remain single the stable marriage equilibrium exhibits assortative matching with the wealthiest bride marrying the wealthiest groom, the second wealthiest bride marrying the second wealthiest groom, and so on.

In empirical work it is easy to document assortative matching but notoriously difficult to disentangle the effects of the different causal mechanisms. However, due to its experimental nature, our study provides a rare opportunity to do just this. By design, the size of the groups that individuals can form is restricted only by village size, i.e., by the number of participants; they can even remain as singletons should they choose. This restricts the likelihood of assorting emerging due to common preferences. However, if common preferences for grouping with either women or men are present, *ceteris paribus*, it would be indicated by more grouping on the part of the preferred sex. So, if gender assorting in group formation is observed, but after controlling for other factors women and men are found to be equally engaged in group formation, common

to 6).

preferences can be ruled out as the causal mechanism.

This would then leave homophily and assorting due to variations in group formation costs to be distinguished between. To do this we perturb the distribution of the costs of interaction between pairs of individuals by randomly assigning each village to one of three different institutional environments. In the first treatment, equal sharing of winnings among group members is exogenously enforced by the experimenter: having joined a sharing group, the members cannot subsequently change their mind. So, regardless of gamble outcomes, winnings are pooled and shared equally.

In the second treatment, each member of a sharing group can separately and secretly leave their groups after knowing the outcome of their gamble. In this case, they keep their winnings but receive no share of the winnings of others in the group. In this treatment, the cost of joining a group depends on the level of trust.

The third treatment differs from the second in that individuals who leave their group have to publicly confirm this in front of all other village participants. In this treatment, the cost of co-grouping depends on the threat of social disapproval and on the fear of the damage that public defection would cause to existing valuable relationships.

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 the first day. The participants were then given approximately 24 hours to form a group. If they chose to do so they had to register together on the second day of the game. The second round gamble choices were made during private interviews and no rules were applied to or recommendations made concerning gamble choices within groups. Under treatment 2, decisions to leave groups and, under treatment 3, intentions to leave groups were also made during these meetings. Under treatment 3, decisions to leave groups had to be confirmed by the leavers revealing themselves to all present when invited to do so after all participants had made their decisions in private interviews. Finally, under all treatments, each participant received their winnings during a second, brief, private interview just prior to being dismissed.

2.2 Empirical formulation

Building on the work of Barr, Dekker and Fafchamps (2009), our empirical analysis starts with the estimation of a dyadic model as follows. Let $m_{ij} = 1$ if i joins a risk sharing 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 explanatory variables must enter the model symmetrically. So, the first model that we estimate is:

$$\begin{aligned} m_{ij} &= \beta_0 + \beta_1|f_i - f_j| + \beta_2(f_i + f_j) + \beta_3 d_{ij} + \beta_4(t_{ij} * d_{ij}) \\ &\quad +\beta_5|z_i - z_j| + \beta_6(z_i + z_j) + v_{ij} + u_{ij} \end{aligned} \tag{1}$$

where f_i indicates the sex of i, equaling 1 if i is female and zero if i is male, d_{ij} is a vector of the characteristics of the pre-existing relationship between individuals i and j, t_{ij} is a vector of dummy variables indicating which treatment individuals i and j played under, z_i is a vector of other relevant characteristics of individual i including their gamble choice in the first round, v_{ij} are village fixed effects, u_{ij} is the error term, and β_0 to β_6 are the coefficients to be estimated.

A negative and significant coefficient β_1 indicates assortative matching by gender in the group formation process. A positive (negative) β_2 indicates that women engage in more (less) grouping activity than men and would indicate the presence of a common preference. Coefficients β_3 and β_4 capture the effects of pre-existing network ties and variations in those effects across treatments.[4] Coefficients β_5 and β_6 capture the effects of a number of other individual

[4] The interaction effects between pre-existing network ties and the experimental treatments were the focus in

characteristics that serve as controls.

We then expand the model to include two additional sets of interaction terms:

$$\begin{aligned} m_{ij} &= \beta_0 + \beta_1|f_i - f_j| + \beta_2(f_i + f_j) + \beta_3 d_{ij} + \beta_4(t_{ij} * d_{ij}) \\ &\quad +\beta_5|z_i - z_j| + \beta_6(z_i + z_j) + \gamma_1(t_{ij} * |f_i - f_j|) \\ &\quad +\gamma_2(t_{ij} * (f_i + f_j)) + v_{ij} + u_{ij} \end{aligned} \tag{2}$$

A significant positive (negative) coefficient γ_1 indicates that assortative matching by gender is lower (higher) in the corresponding treatment, while coefficient γ_2 picks up the differential effects of the treatments on grouping by women and men.

Models (1) and (2) are estimated using a Logit. When estimating these models 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, by experimental session). The second approach corrects for possible non-independence not only within dyadic pairs sharing a common i or j 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.[5]

Barr, Dekker, and Fafchamps (2008). Note that t_{ij} does not appear on its own. This is because the treatments were applied at the village-level and the model includes village fixed effects. Because the treatments were randomly assigned to villages, the inclusion of these fixed effects is, in principle, unnecessary. However, the number of villages under each treatment is small, so, imbalances across villages are a potential concern.

[5] Nichols and Zeckhauser (2004) argue that when the number of clusters is less than 50, clustering may result in standard errors that are too large. Fafchamps and Gubert (2007) propose a standard error correction method that does not require clustering by village. We experimented with this method as well and in general obtain smaller standard errors. In view of Nichols and Zeckhauser's comment, the results presented here should be construed as conservative in the sense that the effects of interest may be biased towards insignificance.

3 The data

The experiment was conducted in 23 Zimbabwean villages in 2001. However, in this paper we use the data from only 14 of these villages. Of the remaining 9, 3 made up a control sample in which no group formation was allowed and 6 were not fully enumerated during the various surveys and mapping exercises 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. They have a strong agricultural focus and a stable composition. Most heads of households and their spouses have resided in the village for at least one decade. Due to the random selection of settlers, the adult inhabitants of these villages are less likely to be genetically related to each other compared to members of the non-resettled villages. However, they engage more in associational activity and have more marriage ties within the villages (see Barr (2004) and Dekker (2004) for details).

Data on the participants' individual characteristics, including their sex, age, education, and their position within the household, were collected at the time of the experiment. Data on household incomes and holdings of livestock wealth were obtained from the Zimbabwe Rural Household Dynamics Study (ZRHDS), collected by Bill Kinsey and his team of field researchers in 1999 and constructed by Trudy Owens and Hans Hoogeveen. Kinsey et al. (1998), Gunning et al. (2000) and Hoogeveen and Kinsey (2001) discuss this dataset in detail.

In the analysis we make use of information relating to three types of pre-existing ties. Data on kinship ties are drawn from specifically designed social mapping exercises. These were conducted in 1999 and 2001 by village focus groups involving one representative from each household residing in each village (Dekker 2004). The data on memberships in religious congregations and CBOs are drawn from a 2000 survey by Barr (see Barr 2004 for details). For the purpose of the analysis presented here, CBOs include only those that have an explicit economic purpose – e.g., micro-finance, mutual insurance, funeral societies, irrigation and livestock rearing cooperatives. Sports clubs, choirs, and dance groups are excluded from the analysis because they were found by Barr, Dekker, and Fafchamps (2008) not to affect group formation within the experiment, and also because they are highly sex-segregated.[6]

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.[7] These observations form the basis for our analysis. Just over half of the participants (52 percent) were women. The average participant is middle-aged and has slightly more than primary education. Two thirds of the sample are married and are either a household head or a spouse of a household head. Annual household monetary income and livestock wealth are approximately log-normally distributed and are incorporated into the analysis in log form.[8] The majority of the participants have a religious affiliation – most often with one of the many apostolic churches existing in Zimbabwe. On average, participants belong to between two and three CBOs with an economic purpose.

Also reported in Table 2 is the proportion of the sample playing under each of the treatments, the proportion who joined groups, the average gamble choices (where the gamble choice identifier is treated as being cardinal for brevity), and the average winnings per subject in each round of

[6] We have no information on geographical proximity. However, the villages are small and the majority of the experimental subjects have lived in the same village for between one and two decades. Gans (1968) and Michaelson (1976) both found that the importance of geographical proximity as a determinant of tie formation declines over time.

[7] 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 qualitatively unchanged.

[8] To avoid losing observations with no income or livestock wealth, we use log(crop income+1) and log(livestock wealth+1). Livestock wealth is measured in money terms using local market prices for trained oxen, household data on numbers of livestock of different types, and applying the following weights: 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).

the experiment.[9] Treatment 2 is under-represented in the sample. This is the result of having to drop a number of villages due to incomplete data. However, there remain sufficient observations under each treatment to make meaningful comparisons. Gamble choices in round 1 are included in the logit regressions to control for attitudes towards risk. Winnings in round 1 are included to control for income effects – and for the possibility that individuals take high winnings in the first round as indication that their luck is in and that, as a consequence, they have no need for insurance in the form of risk sharing. Grouping decisions are the focus of our analysis: just under half of the participants joined sharing groups in the second round of the experiment and the average group size is just over 3 members.

Turning to the characteristics of the relationships between participants, we use the kinship data to construct a variable indicating whether a dyad is related either by blood or marriage.[10] When interpreting results relating to this variable, it is important to recall that each household was invited to send only one representative to the experimental session in their village. So, husbands and wives are never present together, and people are in the same experiment as their children or siblings only if they live in separate households. Furthermore, most villages in the study were made up of stranger households at the time of their resettlement in the early 1980s. As a consequence the majority of the kinship ties in the dataset are between in-laws. Religious co-membership is captured by a dummy variable indicating that members i and j of a dyad belong to the same religious congregation and a count variable is used to capture the number of CBOs (serving an economic purpose) in which both i and j are members.

Table 3 summarizes the dyadic sample containing each possible pair of participants within

[9] Descriptive statistics on winning rates support the randomness of the lottery in the experiment: (1) the overall winning rate was 48% in round 1 and 46% in round 2; (2) winning is independent of gamble choice in both rounds; and (3) winning is independent between rounds 1 and 2.

[10] Barr et al. (2008) separated out genetic relatedness and ties due to marriage, using an estimated Hamilton's ratio to capture the former and a dyadic proximity measure to capture the latter. Here, because we wish to interact elements of d_{ij} with other variables and because genetic relatedness is rare in the dataset, we collapse the two types of family ties into one.

each of the 14 villages. Because the average group size is small, only 7 percent of all within-village dyads are in the same group. Given that the sample is nearly equally divided between male and female participants, just under half of all dyads are made up of one female and one male. The average dyad contains just over one female.

Although almost nine out of every ten participants belong to a religious congregation, only 19 percent of the dyads belong to the same church. This reflects the diversity of faiths present in each of the studied villages. The average dyad share membership in just under one CBO. Table 3 also summarizes the dyadic control variables used in the analysis.

5 Empirical results

5.1 Group formation and gender

Given that the data we are working with comes from a randomized experiment, it is appropriate to begin with a simple cross-tabulation of the data. As well as revealing the overall level of gender assorting in the data, this approach has the added advantage of revealing small cell sizes which can lead to spurious findings in multivariate analysis.

Cross tabulations are reported in Table 4 in the form of a 4×4 matrix. The top left-hand cell relates to the full dyadic sample described in Table 3. All the other cells relate to sub-samples variably defined. In the top row of the matrix, the full sample, pooled across treatments, is divided into sub-samples with respect to dyad type: in the second column all female dyads are considered; in the third all male dyads are considered; and in the fourth mixed gender dyads are considered. The number of dyadic observations in each cell is listed at the top of the cell. Mixed gender dyads represent roughly half in each treatment.

The top right-hand cell shows that, across all treatments only 2.1 percent of mixed gender dyads co-group whereas 14 percent of female dyads and 10 percent of male dyads co-group. The differences in co-grouping between mixed gender dyads and both types of same-gender dyad are statistically significant and indicate assortative matching by gender: participants in the experiment are much more likely to form a group with individuals of the same sex. This is also illustrated by the gender composition of the groups. Of the 47 groups formed during the experiment, 17 (36 percent) groups are male only, 21 (45 percent) are female only and only 9 (19 percent) are mixed groups. The difference in co-grouping between female dyads and male dyads, however, is not statistically significant suggesting that the assorting is not due to common preferences.

The second, third, and fourth rows of the table split the sample by treatment. In the first column of Table 4 we see that, while between 11 and 12 percent of dyads co-grouped under Treatment 1 (externally enforced contracts), only 9 percent co-grouped under Treatment 2 (enforcement based on trust), and just two percent co-grouped under Treatment 3 (enforcement based on social sanctioning). These findings are discussed in detail in Barr and Genicot (2008) who also explore their theoretical implications.

The fourth column of Table 4 is the most interesting for our purpose. While only two percent of mixed-gender dyads co-group under Treatment 1, over five percent co-group under Treatment 2. The opposite pattern is observed for male and female dyads: in both cases, the proportion of dyads that co-group is smaller in Treatment 2 than Treatment 1. This suggests that Treatment 2 affects mixed gender and same gender dyads differently. In Treatment 3 not even one percent of mixed gender dyads co-group.[11] These differences in treatment effects on same- and mixed-gender dyads are presented graphically in Figure 1. The full height of the columns in the histogram indicate the proportion of dyads co-grouping under each treatment. Each column is divided into same-gender (teal green) and mixed-gender (orange) segments.

[11] Care should be taken when considering further sub-divisions of this cell.

The Figure shows clearly the overall decline in co-grouping and the simultaneous increase in mixed-gender grouping as we move from Treatment 1 to Treatment 2.

Table 4 also shows that female dyads are more likely than male dyads to share a family tie and/or a religious co-membership. They are also more likely to belong to the same CBO. In contrast, mixed gender dyads resemble the full sample in terms of family ties and religious and CBO co-memberships. These differences justify moving on to multivariate analysis to safeguard against confounding effects.

Coefficient estimates for models (1) and (2) are presented columns 1 and 2 of Table 5 respectively. Controls include family ties, religious co-membership, co-memberships in CBOs, and interaction terms between these network variables and treatments 2 and 3. We see in model (1) (first column of the table) that, even with the controls, the mixed gender dyad dummy has a highly significant negative coefficient. This confirms the result reported in Table 4 and indicates that the gender assorting in the experiment is not due to the structure of the networks of family ties and religious and CBO co-memberships or to imbalances in any other dimension for which we have data or to any village-level factors. The marginal effect that can be derived from the estimated coefficient indicates that mixed gender dyads are five percentage points less likely to co-group than same-gender dyads.

The second column of Table 5 reports results from model (2) which contains interaction terms between the 'mixed gender dyad' dummy and indicator variables for Treatments 2 and 3.[12] We see that the coefficient on 'T2 x mixed gender dyad' is positive and significant, while the coefficient on 'T3 x mixed gender dyad' is not significantly different from zero. These findings are broadly consistent with those reported in Table 4 and Figure 1. The coefficient on 'mixed gender dyad' dummy is much larger in model (2) than in model (1); according to model (2),

[12] Treatment 1 is the basis for comparison.

under Treatment 1 mixed gender dyads are eight percentage points less likely to co-group than same-sex dyads, whereas under Treatment 2 they are less than one half of a percentage point less likely to co-group. This seems to suggest that, under Treatment 2, there is no gender assorting. However, a linear restriction test indicates that gender assorting remains statistically significant under treatment 2.

Table 5 also confirms that the observed gender assorting is unlikely to be due to common preferences. The coefficient on 'number of females' and its interaction with each of the treatment identifiers are insignificantly different from zero. Unlike the comparison of all female and all male dyads in Table 4, these insignificant coefficients tell us that irrespective of whether we look at same or mixed sex dyads, women and men are equally active in group formation under each of the treatments.

To summarize, gender assorting was observed under all treatments but varied systematically across treatments and no evidence of a cross-gender difference in grouping activity was observed. The latter indicates that the assorting is not due to a common preference and, since the main difference between treatments relates to the mechanisms available to enforce group formation and, correspondingly, the costs associated with group-formation, these findings indicate that gender assorting is not exclusively due to homophily – interaction costs matter.

Table 5 also presents the coefficients and standard errors for family ties and religious and CBO co-memberships and their interactions with treatment dummies. Family ties provide a basis for co-grouping only under Treatment 3. Religious co-membership provides a basis for co-grouping in Treatments 2 and 3, but not in Treatment 1. Co-memberships in economic CBO's provide a basis for co-grouping in Treatments 1 and 2, but according to a linear restriction test become a significant reason for not co-grouping in Treatment 3. Barr and Genicot (2008) and Barr, Dekker, Fafchamps (2008) investigate this finding in detail and attribute it to a fear of jeopardizing valuable relationships through spur-of-the-moment defaults.

5.2 Gender assorting and trust

We now investigate why assortative grouping by gender is less pronounced under treatment 2 where grouping depends on trust. The results presented in Table 5 indicate that gender assorting is not driven by higher trust between individuals of the same gender. Indeed, if this were the case, we would observe *more* gender assorting under treatment 2 – not less. So, why is there less gender assorting under treatment 2?

One possible explanation is suggested by observing, in Table 5, that co-religion is more important in treatment 2, indicating that it provides a basis for trust. If co-religion is associated with trust and there is no gender assorting into religions, the increased importance of trust under treatment 2 could cause individuals to seek members of their own religious congregation to group with and select into more gender-diverse groups as a result.

To establish whether this is the case, we need to do two things. First we need to show that there is no or very little gender assorting into religious congregations. Second, we need to establish whether and how much of the reduction in gender assorting between Treatments 1 and 2 is due to the increased importance of co-religion.

Table 4 tackles the first of these two tasks. There we see that mixed gender dyads are only slightly (under four percentage points) less likely than same-sex dyads to be members of the same religious congregation.[13]

The second task can be performed by augmenting model (2) to include an additional interaction term 'T2 x mixed gender dyad x Religious co-membership'. If the coefficient on the former is positive and significant, this means that the increased importance of religious co-membership

[13] Although small in magnitude, the difference is significant according to a Chi-squared test corrected for village clustering, (Chi-square=5.26, p=0.022)

under Treatment 2 accounts for at least some of the reduction in gender assorting between treatments 1 and 2. If, in addition, the coefficient on 'T2 x mixed gender dyad' is no longer statistically significant once the interaction term is included, we can conclude that the increased importance of religion in treatment 2 fully explains the decrease in gender assorting.

The results of this exercise are shown in the second column of Table 6. (The first column in Table 6 is identical to column 2 in Table 5 and is shown to provide a basis for comparison.) To avoid spurious inference regarding 'T2 x mixed gender dyad x Religious co-membership', we include an interaction term 'T2 x Number of females in dyad x Religious co-membership' to control for any imbalance across cells. The coefficient on 'T2 x mixed gender dyad x religious co-membership' is positive and highly significant and, according to a linear restriction test, the sum of the the coefficients on 'mixed gender dyad', 'T2 x mixed gender dyad' and 'T2 x mixed gender dyad x religious co-membership' is statistically insignificant indicating that there is no gender assorting in group formation among dyads sharing a religious co-membership. Finally, the coefficient on 'T2 x mixed gender dyad' is still significant but smaller in magnitude. These findings indicate that religious co-membership precludes assortative grouping on gender and that this explains part of the decline in gender assorting between treatments 1 and 2. However, while assortative grouping on gender continues to be observed under treatment 2 among dyads that do not share a religious co-membership this too is diminished in comparison to treatment 1.

To test the robustness of these findings, in the third column of Table 6 we include four additional interaction terms. The first is an interaction between the family tie dummy and 'T2 x mixed gender dyad'. The second is an interaction between co-membership in CBO's and 'T2 x mixed gender dyad'. These interaction terms allow us to explore the possibility that other types of network tie are performing a similar role to co-religion under treatment 2 at the same time as checking that our findings regarding co-religion are not spurious. We also include the interaction terms ’T2 x Number of females x Family’ and ’T2 x Number of females x Co-memberships in CBOs’ to control for any imbalance across cells. Neither of the new interaction terms with the mixed gender dyad dummy bear significant coefficients and the estimated coefficients on ‘T2 x mixed gender dyad x Religious co-membership’ and ‘T2 x mixed gender dyad’ change very little. This indicates that our results regarding religion is not driven by family ties or co-membership in CBOs.

Note the significant negative coefficient on ’T2 x Number of females x Family.’ This suggests that within family networks women engage in less grouping activity than men under treatment 2. This is interesting as it suggests that, compared to men, women are either less trusting of or less trustworthy towards their kin, principally their in-laws.

In Appendix 1 we present several additional models that include a number of other interaction terms of potential interest. These indicate that family ties may be associated with less gender assorting under Treatment 3. However, given that less than 1 percent of mixed gender dyads co-group under treatment 3 and given that we find no difference in gender assorting between Treatments 1 and 3 in Table 5, we suspect that this finding may be driven by small cell sizes.

6 Conclusion

The study of networks has brought to light the empirical importance of assortative matching in many social contexts. Three types of causal mechanisms have been highlighted in the growing literature on the factors that may underlie assortative matching: common preferences combined with constraints on the number of network ties that can be formed; homophily, i.e., a preference for interacting with similar others; and opportunity or the costs associated with interacting with similar others. Here, using an experimentally derived dataset combined with very rich data on preexisting network ties and individual characteristics, we take some important steps towards distinguishing between these three possible causal mechanisms.

Our focus is assortative grouping by gender, a form of assorting that, while only occasionally observed in OECD-country-based studies, has been casually observed by many in developing countries and especially sub-Saharan Africa, the region in which our experiment was conducted. Our data supported these casual observations, indicating that women and men do tend to segregate when forming interactive groups.

Common preferences were unlikely to be the underlying mechanism leading to the assorting on gender observed within the experiment because no constraints were placed on group size. However, they could have generated an observable difference in grouping activity between women and men. No such difference was observed leading to the tentative conclusion that common preferences relating to gender are not present, i.e., it is not the case that everyone wishes to interact with men or that everyone wishes to interact with women.

To identify the possible effects of interaction costs, we made use of the treatment variations applied during the experiment. Three treatments were applied: in the first, grouping agreements were externally enforced by the experimenter; in the second, grouping agreements could be secretly broken, so grouping depended on trust; and in the third, grouping agreements could be publicly broken, so grouping could be supported by social sanctions. Each of these treatments would have generated a different distribution of interaction costs across dyads. So, if the extent of gender assorting is found to vary across the treatments it can be taken as evidence that interaction costs matter.

We found significant differences in the extent of gender assorting across treatments. In particular, we observed significantly less assorting in the second treatment. This leads to the conclusion that interaction costs are important and, more specifically, that gender assorting in sub-Saharan villages is not driven by relative distrust between men and women.

In contrast, co-membership in religion was associated with an increased likelihood of co-grouping under treatment 2. So, in an additional series of analyses we set out to investigate whether the two findings were linked. Thus, we established that, (1) there was significant but quantitatively minimal gender assorting into religious congregations, (2) among members of the same religious congregation, there was no gender assorting under treatment 2 in the experiment, and this explained some but not all of the decline in gender assorting as we moved to that treatment. In general, men and women not belonging to the same religious congregation still assorted but also assorted less under treatment 2.

While these findings shed some light on the relative importance of the possible causal mechanisms underlying gender assorting in sub-Saharan villages, they also raise a new set of unanswered questions. Specifically, what type of network is supporting trust and, thereby, leading to the decline in gender assorting in treatment 2 even among members of different religious congregations? And why does this network not come to the fore when trust does not matter?

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Figure 1: Co-grouping by different dyad types

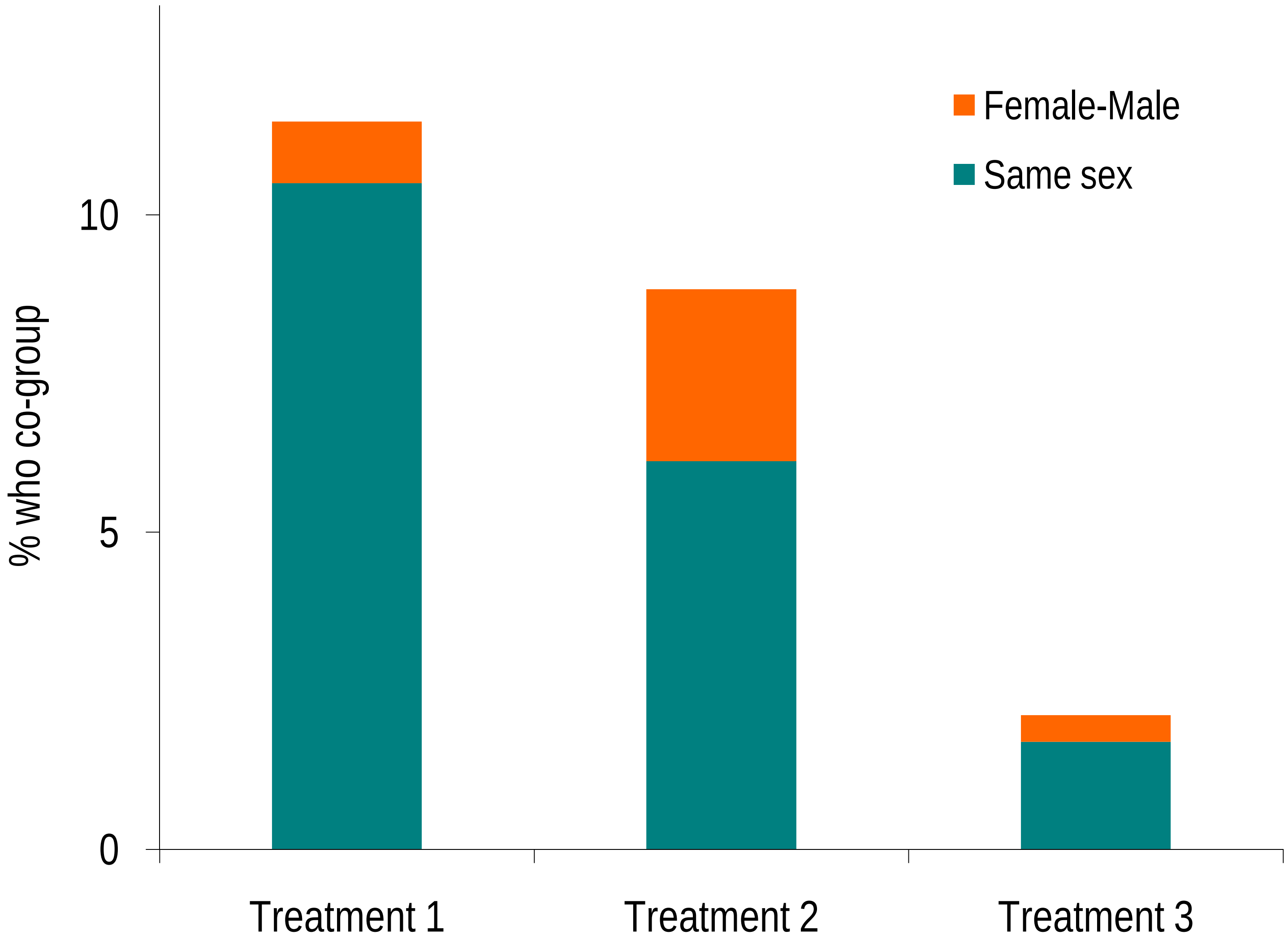

Table 1. Gamble choices in Z$ and implied relative risk aversion coefficients

Choice	High payoff	Prob.	Low payoff	Prob.	EV	RA class	RA coeff.
1	100	50%	100	50%	100	Extreme	infinity to 7.51
2	190	50%	90	50%	140	Severe	7.51 to 1.74
3	240	50%	80	50%	160	Intermediate	1.74 to 0.81
4	300	50%	60	50%	180	Moderate	0.81 to 0.32
5	380	50%	20	50%	200	Slight-neutral	0.32 to 0.00
6	400	50%	0	50%	200	Neutral-negative	0 to -ve infinity

Table 2. Characteristics of participants

	Percentage or mean	Std. Dev.
Subject Characteristics		
Female	52.1%	
Age	41.971	17.750
Years of schooling	6.762	3.207
Household head	41.9%	
Spouse of household head	21.5%	
Married	66.5%	
Annual household income (1,000sZim$)	2.562	3.374
Ln(Annual household income + 1)	7.185	1.418
Houeshold livestock wealth (1,000sZim$)	11.656	10.124
Ln(Household livestock wealth + 1)	8.195	2.902
Belongs to a religious community	87.7%	
Memberships in CBOs	2.30	2.29
Resettled household	75.9%	
Experimental variables		
Played under treatment 1	41.6%	
Played under treatment 2	23.3%	
Played under treatment 3	35.1%	
Joined a group in round 2	48.7%	
Size of group joined (=1 for singletons)	3.168	3.011
Gamble choice in round 1	3.231	1.170
Gamble choice in round 2	3.589	1.130
Winnings in round 1 (Zim$, 2001)	157.13	106.60
Winnings in round 2 (Zim$, 2001)	169.65	121.71
Observations	382	

Table 3. Summary statistics for the dyadic sample

	Percentage or mean	Std. Dev.
Dyadic variables of specific interest		
Join same group in experiment	7.3%	
Female-male dyad	49.3%	
Number of females in dyad	1.073	0.708
Family (genetically related or related by marriage)	21.4%	
Religious co-membershipgroup	19.3%	
Co-memberships in CBOs	0.940	1.134
Control variables: Dyadic differences		
Difference in Round 1 gamble choice	1.230	1.066
Difference in Round 1 winnings	116.457	95.794
Difference in age	19.400	14.105
One a household head, one not	44.8%	
Difference in years of schooling	3.545	2.720
Difference in ln(annual household income + 1)	1.171	1.301
Difference in household livestock wealth	2.288	3.143
Control variables: Dyadic sums		
Sum of Round 1 gamble choices	6.466	1.621
Sum of Round 1 winnings	323.597	150.778
Sum of ages	84.029	24.842
Number of household heads in dyad	1.289	0.685
Sum of years of schooling	13.819	4.527
Sum of ln(annual houehold incomes + 1)	14.477	2.147
Sum of household livestock wealth	16.601	4.085
Observations	10470	

Table 4. Dyadic Cross-tabulations

	All dyads		Female dyads		Male dyads		Female-male dyads	
All treatments	N= 10470		N= 3032		N= 2272		N= 5166	
	mean	*non zero*	*mean*	*non zero*	*mean*	*non zero*	*mean*	*non zero*
Female-male dyads	0.493	49.3%						
Join same group in exp.	0.073	7.3%	0.139	13.9%	0.102	10.2%	0.021	2.1%
Family	0.214	21.4%	0.270	27.0%	0.165	16.5%	0.203	20.3%
Religious co-membership	0.193	19.3%	0.229	22.9%	0.176	17.6%	0.180	18.0%
Co-mememberships in CBOs	0.940	57.8%	1.431	73.4%	0.653	43.3%	0.779	55.0%
Treatment 1	N= 4532		N= 1486		N= 840		N= 2206	
	mean	*non zero*	*mean*	*non zero*	*mean*	*non zero*	*mean*	*non zero*
Female-male dyads	0.487	48.7%						
Join same group in exp.	0.115	11.5%	0.214	21.4%	0.188	18.8%	0.020	2.0%
Family	0.196	19.6%	0.234	23.4%	0.181	18.1%	0.177	17.7%
Religious co-membership	0.186	18.6%	0.209	20.9%	0.171	17.1%	0.176	17.6%
Co-mememberships in CBOs	0.980	58.6%	1.584	76.6%	0.564	41.7%	0.732	52.9%
Treatment 2	N= 1698		N= 354		N= 488		N= 856	
	mean	*non zero*	*mean*	*non zero*	*mean*	*non zero*	*mean*	*non zero*
Female-male dyads	0.504	50.4%						
Join same group in exp.	0.088	8.8%	0.164	16.4%	0.094	9.4%	0.054	5.4%
Family	0.221	22.1%	0.316	31.6%	0.148	14.8%	0.224	22.4%
Religious co-membership	0.199	19.9%	0.333	33.3%	0.131	13.1%	0.182	18.2%
Co-mememberships in CBOs	0.718	51.9%	1.085	74.6%	0.479	34.0%	0.703	52.8%
Treatment 3	N= 4240		N= 1192		N= 944		N= 2104	
	mean	*non zero*	*mean*	*non zero*	*mean*	*non zero*	*mean*	*non zero*
Female-male dyads	0.496	49.6%						
Join same group in exp.	0.021	2.1%	0.037	3.7%	0.030	3.0%	0.009	0.9%
Family	0.230	23.0%	0.302	30.2%	0.159	15.9%	0.221	22.1%
Religious co-membership	0.199	19.9%	0.223	22.3%	0.203	20.3%	0.183	18.3%
Co-mememberships in CBOs	0.986	59.2%	1.342	69.1%	0.822	49.6%	0.858	58.0%

Notes: 'Female-male dyads', 'Join same group', Same religious group', and 'Family' are all dichotomous (0,1) variables, so their means and percentages of non-zeros are equivalent; 'Comemberships. in CBOs' is a count variable, so the means and percentages of non-zeros are not identical.

Table 5. Dyadic analysis of treatment responses by different dyad types

	(1)	(2)
Female-male dyad	-1.954 ***	-2.530 ***
	(0.424)	(0.503)
T2 x Female-male dyad		1.525 **
		(0.639)
T3 x Female-male dyad		-0.033
		(0.380)
Family (blood or marriage)	-0.096	-0.120
	(0.328)	(0.335)
Religious co-membership	0.061	0.072
	(0.074)	(0.093)
Co-mememberships in CBOs	0.134 **	0.112 *
	(0.060)	(0.060)
T2 x Family	-0.396	-0.348
	(0.337)	(0.330)
T2 x Religious co-membership	0.332 **	0.408 **
	(0.167)	(0.204)
T2 x Co-mememberships in CBOs	0.162	0.151
	(0.177)	(0.159)
T3 x Family	1.104	1.111 *
	(0.668)	(0.638)
T3 x Religious co-membership	1.038 ***	1.015 ***
	(0.226)	(0.243)
T3 x Co-mememberships in CBOs	-0.345 ***	-0.301 **
	(0.133)	(0.120)
Number of females in dyad	0.019	0.027
	(0.145)	(0.172)
T2 x Number of females in dyad		1.013
		(0.715)
T3 x Number of females in dyad		-0.066
		(0.438)
Other controls (*): included but not shown		
Pseudo R-squared	0.226	0.233

(*) absolute differences in and sums of age, household headship dummies, years of schooling, log household income, log livestock wealth, 1st round gamble choices, and 1st round winnings;
Notes: Estimated Logit coefficients presented; corresponding standard errors (in parentheses) adjusted to account for non-independence within villages/sessions by clustering; n=10470 throughout; *** - significant at 1%; ** - significant at 5%; * significant at 10%.

Table 6. Dyadic analysis of orgins of different treatment responses by different dyad types

	(1)	(2)	(3)
Female-male dyad	-2.530 ***	-2.530 ***	-2.531 ***
	(0.503)	(0.504)	(0.504)
T2 x Female-male dyad	1.525 **	1.399 **	1.391 **
	(0.639)	(0.662)	(0.685)
T3 x Female-male dyad	-0.033	-0.056	-0.020
	(0.380)	(0.399)	(0.322)
T2 x Female-male dyad x Belong to same religious grp		0.741 ***	0.767 ***
		(0.146)	(0.113)
T2 x Female-male dyad x Family			0.059
			(0.486)
T2 x Female-male dyad x Co-mememberships in CBOs			-0.036
			(0.140)
Family (blood or marriage)	-0.120	-0.120	-0.120
	(0.335)	(0.335)	(0.334)
Religious co-membership	0.072	0.072	0.075
	(0.093)	(0.093)	(0.094)
Co-mememberships in CBOs	0.112 *	0.112 *	0.113 *
	(0.060)	(0.060)	(0.060)
T2 x Family	-0.348	-0.366	0.650
	(0.330)	(0.334)	(0.432)
T2 x Religious co-membership	0.408 **	-0.264	-0.239
	(0.204)	(0.480)	(0.376)
T2 x Co-mememberships in CBOs	0.151	0.166	0.015
	(0.159)	(0.150)	(0.185)
T3 x Family	1.111 *	1.110 *	1.113 *
	(0.638)	(0.638)	(0.638)
T3 x Religious co-membership	1.015 ***	1.015 ***	1.013 ***
	(0.243)	(0.243)	(0.243)
T3 x Co-mememberships in CBOs	-0.301 **	-0.301 **	-0.304 **
	(0.120)	(0.120)	(0.122)
Number of females in dyad	0.027	0.027	0.026
	(0.172)	(0.172)	(0.173)
T2 x Number of females in dyad	1.013	1.013	1.015
	(0.715)	(0.715)	(0.716)
T3 x Number of females in dyad	-0.066	-0.066	-0.064
	(0.438)	(0.438)	(0.440)
T2 x Number of females x Religious co-membership		0.301	0.242
		(0.270)	(0.174)
T2 x Number of females x Family			-1.014 ***
			(0.221)
T2 x Number of females x Comememberships in CBOs			0.216
			(0.158)
Other controls (*): included but not shown			
Pseudo R-squared	0.233	0.234	0.236

(*) absolute differences in and sums of age, household headship dummies, years of schooling, log household income, log livestock wealth, 1st round gamble choices, and 1st round winnings;

Notes: Estimated Logit coefficients presented; corresponding standard errors (in parentheses) adjusted to account for non-independence within villages/sessions by clustering; n=10470 throughout; *** - significant at 1%; ** - significant at 5%; * significant at 10%.

Appendix Table 1. Adding other interaction terms to the dyadic analysis

	(1)	(2)	(3)	(4)	(5)	(6)
Female-male dyad	-2.624 ***	-2.510 ***	-2.317 ***	-2.530 ***	-2.530 ***	-2.530 ***
	(0.505)	(0.517)	(0.514)	(0.504)	(0.504)	(0.503)
T2 x Female-male dyad	1.491 **	1.538 **	1.593 **	1.524 **	1.525 **	1.524 **
	(0.686)	(0.639)	(0.636)	(0.639)	(0.639)	(0.639)
T3 x Female-male dyad	-0.019	-0.039	-0.034	-0.032	-0.035	-0.033
	(0.368)	(0.387)	(0.376)	(0.380)	(0.380)	(0.381)
Female-male dyad x Family	0.459					
	(0.424)					
Female-male dyad x Religious co-membership		-0.089				
		(0.332)				
Female-male dyad x Co-mememberships in CBOs			-0.248			
			(0.306)			
T3 x Female-male dyad x Family				1.219 ***		
				(0.411)		
T3 x Female-male dyad x Religious co-membership					0.017	
					(0.198)	
T3 x Female-male dyad x Co-mememberships in CBOs						-0.890
						(0.731)
Family (blood or marriage)	-0.074	-0.113	-0.103	-0.120	-0.121	-0.119
	(0.312)	(0.336)	(0.322)	(0.335)	(0.335)	(0.335)
Belong to same religious group	0.072	-0.075	0.078	0.071	0.077	0.072
	(0.092)	(0.211)	(0.089)	(0.094)	(0.094)	(0.093)
Co-mememberships in CBOs	0.111 *	0.110 *	0.006	0.112 *	0.112 *	0.112 *
	(0.060)	(0.060)	(0.160)	(0.060)	(0.060)	(0.060)
T2 x Family	-0.474	-0.357	-0.389	-0.347	-0.348	-0.348
	(0.371)	(0.332)	(0.317)	(0.331)	(0.330)	(0.330)
T2 x Religious co-membership	0.413 *	0.424 *	0.350 *	0.408 **	0.402 *	0.408 **
	(0.222)	(0.229)	(0.189)	(0.204)	(0.206)	(0.205)
T2 x Co-mememberships in CBOs	0.158	0.148	0.279	0.150	0.151	0.150
	(0.154)	(0.160)	(0.238)	(0.159)	(0.159)	(0.159)
T3 x Family	1.052	1.097 *	1.088 *	0.205	1.130 *	1.128 *
	(0.694)	(0.643)	(0.628)	(0.709)	(0.637)	(0.610)
T3 x Religious co-membership	1.013 ***	1.043 ***	1.011 ***	1.000 ***	1.239 ***	1.023 ***
	(0.241)	(0.239)	(0.244)	(0.226)	(0.382)	(0.253)
T3 x Co-mememberships in CBOs	-0.296 **	-0.299 **	-0.261	-0.308 **	-0.302 **	-0.082
	(0.120)	(0.120)	(0.137)	(0.121)	(0.122)	(0.444)
Number of females	0.041	0.008	-0.034	0.028	0.027	0.027
	(0.174)	(0.173)	(0.197)	(0.172)	(0.172)	(0.172)
T2 x Number of females in dyad	0.881	1.029	0.964	0.455	1.011	1.478 **
	(0.723)	(0.707)	(0.687)	(0.683)	(0.744)	(0.631)
T3 x Number of females in dyad	-0.045	-0.090	-0.060	-0.215	0.020	0.001
	(0.438)	(0.445)	(0.437)	(0.374)	(0.463)	(0.392)
Number of females x Family	-0.074					
	(0.215)					
Number of females x Religious co-membership		0.118				
		(0.160)				
Number of females x Comememberships in CBOs			0.065			
			(0.080)			
T3 x Number of females x Family				0.507 *		
				(0.288)		
T3 x Number of females x Religious co-membership					-0.218	
					(0.183)	
T3 x Number of females x Comememberships in CBOs						-0.100
						(0.305)
Other controls (*): included but not shown						
Pseudo R-squared	0.234	0.234	0.235	0.235	0.234	0.234

(*) absolute differences in and sums of age, household headship dummies, years of schooling, log household income, log livestock wealth, 1st round gamble choices, and 1st round winnings;

Notes: Estimated Logit coefficients presented; corresponding standard errors (in parentheses) adjusted to account for non-independence within villages/sessions by clustering; n=10470 throughout; *** - significant at 1%; ** - significant at 5%; * significant at 10%.

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