The Impact of Social Ties on Group Interactions: Evidence from Minimal Groups and Randomly Assigned Real Groups

By Lorenz Goette, David Huffman and Stephan Meier*

Economists are increasingly interested in how group membership affects individual behavior. The standard method assigns individuals to 'minimal' groups, i.e. arbitrary labels, in a lab. But real groups often involve social interactions leading to social ties between group members.
Our experiments compare randomly assigned minimal groups to randomly assigned groups involving real social interactions. While adding social ties leads to qualitatively similar, although stronger, in-group favoritism in cooperation, altruistic norm enforcement patterns are qualitatively different between treatments. Our findings contribute to the micro-foundation of theories of group preferences and caution against generalizations from 'minimal' groups to groups with social context.
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Teams, departments, communities etc. are very prevalent in organizations and society. Membership in those groups may affect the willingness of members to engage in prosocial behaviors like altruistic cooperation, or norm enforcement, which enhance efficiency but involve no personal material reward (see, e.g., Akerlof and Kranton, 2000, 2005). Group boundaries might impact prosocial behavior through two psychological channels: (1) by creating a 'label' that can cause someone to 'identify' with the group, and (2) by affecting the circle of people an individual interacts with, leading to formation of social ties with fellow group members.

A large body of evidence from "minimal group" experiments in psychology (e.g., Tajfel et al. (1971)) sheds light on the first of these effects, showing that "even the most minimal group assignment can affect behavior" (Akerlof and Kranton, 2000, p. 720). That is, even a "minimal group", which in its purest form is nothing more than an arbitrary label, can increase subjects' willingness to help individuals with the same label, as opposed to subjects with a different, arbitrarily assigned label.

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This finding inspired *social identity theory* (see, e.g. Tajfel and Turner, 1979), which assumes that individuals "identify" with even arbitrary group labels, and favor those with the same label.

Virtually all papers in economics focus on this 'labeling' effect of groups by using the standard methodology inherited from social psychology to induce group membership in the laboratory. Participants are randomly assigned to 'minimal' groups, orthogonal to any social relations outside the lab, e.g., to the 'blue' or the 'red' group. In recent years, the *American Economic Review* alone published a number of studies which rely on this procedure (e.g. Charness et al., 2007; Sutter, 2009; Heap and Zizzo, 2009; Chen and Li, 2009). Minimal group manipulations in economic experiments randomly assign participants to groups, orthogonal to any social relations outside the lab. This approach has a key advantage over using existing groups like ethnic groups or clans (see, e.g. Fershtman and Gneezy, 2001; Bernhard et al., 2006; Falk and Zehnder, 2006), which is that the random assignment to minimal groups allows making inferences about the *causal* effect of group membership.[1] For example, Chen and Li (2009) use "minimal groups" to provide a rich picture of how group identity can influence behavior when group interest and self-interest are in conflict. Group identity is shown to increase sharing altruistically with in-group members, and to cause individuals to be more lenient towards in-group members when punishing them for norm violations.

However, real groups are typically more than just a label. Close-knit teams and communities, in particular, are characterized by intense within-group interactions, leading to social ties or friendships within the group. A social tie is "an affective weight attached by an individual to the well-being of another individual" which develops over time through social interactions (van Winden et al., 2008). Importantly, social ties can lead to emotional bonds, distinct from any strategic or reputation based effects. Previous evidence shows that (endogenously formed) friendships and reduced social distance are positively correlated with cooperation in prisoner's dilemmas, and giving in dictator games (e.g. Leider et al., 2009a; Goeree et al., 2010; Bohnet and Frey, 1999a,b). In terms of mechanisms, social ties have been argued to generate empathy, which in turn is believed to enhance pro-social behavior (Singer et al., 2006; Singer and Fehr, 2005; van Winden et al., 2008). Social groups could thus affect pro-social behavior through the channel of creating social ties and empathy, a channel that is missing from minimal groups by construction. This raises an important question: Is the effect of minimal groups just a quantitatively different (weaker) version of what is observed in real groups, or do minimal groups behave *qualitatively* different from real groups?

We argue, and show experimentally, that "minimal groups" will miss important aspects of real groups and that they will be misleading when translating results from such laboratory experiments to the field. We implemented a design that allows the comparison of minimal groups (MG treatment), and groups with real

[1]Another reason for the widespread adoption of the technique may be convenience; groups can be formed instantly in the lab, under complete control of the experimenter.

social interactions leading to social ties (SG treatment). Importantly, both types of groups were randomly assigned, isolating the effect of group membership per se, unlike papers using groups, networks, or friendships that are formed endogenously (e.g. Leider et al., 2009a; Goeree et al., 2010) or differ in terms of member characteristics (see, e.g. Fershtman and Gneezy, 2001; Bernhard et al., 2006). Another key aspect of the design is anonymity: Individuals knew whether they interacted with someone from their own group, or another group, but not whom. Furthermore, they did not even learn the actions of the other player until after groups had been disbanded and individuals were geographically segregated. Thus, group membership could not have affected behavior for strategic, or reputation reasons.[2]

Our results show that both the labeling and social ties aspects of groups have important and distinctive effects on prosocial behavior. While minimal group experiments can capture the particular types of in-group favoritism triggered by be labeling aspect of groups, our results show that they are misleading in important ways when trying to think about behavior of real groups in the field. In particular, the way that in-group defectors are treated, and the response to defection against and in-group member, are sharply different from in groups with real social ties. As such, the paper contributes to the microfoundation of theories of social group preferences, by showing that social ties have to be taken into account. We discuss a potential mechanism for the difference between SG and MG based on heightened *empathy* towards the in-group in SG, on top of the group label effects present in MG.

Methodologically, the paper points to a trade-off when choosing between randomly assigned groups with real social ties, and minimal groups, as the group manipulation. While group manipulations with social ties have more external validity for the many types of groups with this feature, it is likely to be more costly to arrange such a manipulation than simply assigning minimal groups in the lab. In the case that researchers need to use lower-cost minimal group manipulations, but nevertheless want to understand groups with social ties, our results can provide some guidance on the particular ways in which behavior in minimal groups diverges from behavior in groups with social ties.

The results can help reconcile different findings of studies using real (but non-randomly assigned) social groups, and studies using minimal groups. For example, Bernhard et al. (2006) find that tribes in Papua New Guinea punish harder if someone is ungenerous to an in-group member, and punish in-group members equally hard for defection as out-group members, just as we observe in the social group treatment. By contrast, Chen and Li (2009) use a minimal group manipulation, and find that in-group members are punished less hard for defection than outsiders, as we observe in our minimal groups treatment. Our results sug-

[2]The seminal study by Sherif et al. (1961), which randomly assigned 11-year-old boys to groups at a summer camp, studied behavior in non-anonymous group activities. Many other experiments vary social distance or social ties in the lab by lifting anonymity, which makes it difficult to isolate anonymity effects from effect of social ties (e.g. Dufwenberg and Muren, 2006).

gest that these differences can potentially be reconciled by incorporating separate mechanisms for labeling, and social ties, aspects of groups into theories of group behavior.[3]

The paper proceeds as follows: Section I describes the set-up of the experiment, the different manipulations of group identity, and the experimental design. Section II presents the results. Section III concludes.

I. Experimental Design

A. *Set-Up*

All Swiss males are required to perform military service, beginning with twenty-one weeks of basic training. In week seven, about one fourth are selected to go through ten weeks of officer-candidate training. Of these, one fourth are promoted to officers and continue on to the Joint Officer Training Program (JOTP). Whereas officer-candidate training is specific for each branch of service, and occurs in separate locations, JOTP brings new officers from all branches of service together, to the same location, for four weeks. Officers are randomly assigned to a platoon at the beginning of JOTP, and spend virtually all time during the day with their platoon. Training involves mainly coursework, on principles of security, combat in large military units, logistics, and leadership. At the end of JOTP, the platoons are dissolved and officers are once again sent to separate locations, for further, advanced training specific to each branch of service.

In week three of the four-week training, we conducted two experiments with these officer candidates[4] using two approaches to manipulating group membership (for more details and preliminary results, see Goette et al., 2006).

B. *Group Assignment Treatments*

We used two methods of manipulating group membership:

Randomly-Assigned Social Groups (SG): We used the random assignment of candidates to platoons as our manipulation of groups involving social ties. Each platoon was identified by a different number. On average, a platoon contained 21

[3]The results contribute as well to the related literature on social networks, which finds stronger pro-social behavior between individuals with strong social ties (Dufwenberg and Muren, 2006; Leider et al., 2009a,b; Goeree et al., 2010). A key open question in that literature is whether strong ties affect pro-social behavior, or whether people become friends with individuals towards whom they are inclined to be pro-social. Such sorting is highlighted by the finding that individuals tend to have strong social ties with people who are similar on various dimensions (Leider et al., 2009a; Goeree et al., 2010). Another, related problem is disentangling the effects of friendship from the effects of a shared social group label; friends typically share membership in one or more social groups. In our study the random assignment to a social group with intense social interactions serves as an exogenous manipulation of social ties, which sheds light on the causal effect of social networks. We demonstrate that social ties do affect behavior, distinct from group labeling (identity) effects.

[4]All participants were males. An interesting question that we do not address here is whether there would be differential effects by gender.

soldiers in our sample. Assignment to platoons was random, and stratified according to the different branches of service. The army assigns platoons orthogonally to any previous social ties among officers with the aim of promoting exchanges of perspectives among different individuals and branches of service.

The assignment mechanism is ideal, in several ways, for investigating the impact of group membership on behavior. First, random assignment allows causal inferences. Trainees knew that platoon composition was designed to be identical and that nobody could choose which platoon to join. Indeed, statistical tests reveal no significant differences in platoon composition, by branch of service, education, or age. Second, there was no competition between the groups (or trainees) for evaluations or other resources. Relative performance evaluations were completed previously, in candidate training. Thus, there was no function of the group assignment, other than to effect the circle of individuals with whom an officer interacts most frequently. Third, social interactions within a platoon were intense. Platoon members spent the whole workday with their group, for the three weeks leading up to our experiments. This tends to create strong social ties. An indication that the manipulation lead to social ties is provided by decisions of how to spend free time when off-duty: In a questionnaire, officers in our study indicated that they spent significantly more time off-duty with members of their own platoon. Most of the trainees (79.8 percent) knew people in other platoons, mostly from earlier stages of their training. Yet, platoon assignment caused them to endogenously choose to seek out each other's company during what little off-duty time they had, even though the schedules and off-duty times were exactly the same for all platoons.

Randomly-Assigned Minimal Groups (MG): In the minimal group assignment individuals were also randomly assigned to groups, each identified by a different number, as in *SG*. In the case of *MG*, however, the assignment did not have any significance in term of social interactions and social ties; an individual was assigned to the group with the same number as the last digit of the individual's government-assigned social security number. This number is heavily used in the Swiss Army, and thus individuals know their own number. Importantly, the last digit of the Swiss social security number is randomly determined. We made sure to explain this feature of the social security number to the individual and explained that they were in group 1 if their last digit was 1, group 2 if it was 2, etc.. Given that 230 individuals participated in the experiment, on average 23 trainees shared the same number.

Thus, unlike in *SG*, in *MG* groups did not have any content in terms of a shared history of social interactions and resulting strong social ties. This is the key feature of minimal group type designs, where groups are simply arbitrary labels, assigned orthogonal to any real social relations (Tajfel and Turner, 1979).[5]

[5]Strictly speaking *MG* does not meet all the the criteria of the original minimal group design used by psychologists, nor do other minimal group experiments by economists. For example, psychologists typically make it so that the decision maker's choices have no consequences for his or her own material

Importantly, the design provides an especially strong assurance of anonymity, such that differences in behavior in *SG* and *MG* cannot be due to strategic motives, associated with anticipated future interactions in the *SG* groups (see Leider et al., 2009a). First, decisions in the experiment were made anonymously: Subjects knew the platoon affiliation of the other player, but no other identifying information. Thus, individuals' actions were known only to themselves. Second, participants learned about the outcome of the experiment (and their payoffs) after JOTP was over, the groups were dissolved, and individuals were back at home in their civil life. This geographic separation creates substantial costs to social interaction and communication between former group members. Subjects were fully informed about anonymity, and the timing of when they would learn about the game outcomes. Thus, due to the absence of identifying information, and due to lack of information about game outcomes, differences in behavior between *SG* and *MG* are not easily explained by any type of *strategic* concerns. Nevertheless, there may be important psychological consequences of the lack of social ties in *MG* compared to *SG*, which influenced behavior.

C. *Experiments*

We conducted two experiments to examine the impact of group assignment on behavior.

Experiment 1: Cooperation. The game was a simultaneous prisoners' dilemma. The players, labeled A1 and A2, were each endowed with 20 points. They simultaneously decided whether to keep the points or pass all of them to the other player. Passed points were doubled. Thus, keeping the points equaled defection and passing the points equaled cooperation.

Experiment 1 involved two conditions in a between-subject design. In the in-group condition, subjects interacted anonymously, except for being informed that the other player was a member of their group - either their platoon or the minimal group. The out-group condition was the same, except subjects were informed that the other player was a member of another group. Group affiliation was clearly marked on the decision sheets. These conditions allow us to examine how group assignment affected cooperation.

Experiment 2: Norm Enforcement. In Experiment 2, we added players B1 and B2, each endowed with 70 points. B1 could assign up to 10 deduction points from his endowment to A1, and B2 could do the same with respect to A2. Each deduction point subtracted three points from the A-player, and cost the B-player one point of his endowment. Punishment could therefore cost A-players up to 30 and B-players up to 10.[6] In the analysis, we will show punishment points from the perspective of B-players which can be between 0 and 10. The B-players could condition their choices on the actions of A1 and A2. Thus Experiment

payoffs. Like other minimal groups experiments in economics, *MG* does incentivize the choices of decision makers, and contrasts these material incentives against non-material, group-based motives.

[6]Payoffs of A-players can, however, not be negative.

2 incorporated the possibility of third-party punishment (Fehr and Fischbacher, 2004). Giving a third party the possibility to punish other players removes any motives that are related to the material payoff consequences of these players. The design is suited for examining whether individuals engage in altruistic enforcement of norm (i.e., differential punishment of defection) and how the motives related to punishment vary with the group composition and treatment.

To examine the impact of group membership on norm enforcement, we varied the composition of players in each game in a between-subject design. For the remainder of the paper, we refer to the group composition in Experiment 2 from B1's perspective. Thus, A1 always refers to the player that the B-player can punish, while we refer to the other A-player as A2. The four different group compositions we implemented are shown in Figure 1.

Varying the group membership of A1 allows us to investigate how the group identity of the person being punished matters. We also study how punishment varies with the group affiliation of A2, the person affected by A1s actions. The Web Appendix provides a translation of the instructions for the SG manipulation. Instructions for MG are identical except for substituting the minimal group definition of groups for platoons.

D. *Procedure*

The experiment was conducted with paper-and-pencil in a large auditorium. The experiment lasted 45 minutes. The study was conducted in groups of 3 platoons per session, which were seated far apart from each other. The different treatments SG and MG were conducted in separate sessions.

Special care was taken to ensure anonymity. Subjects knew that payoffs would be mailed to home addresses ten days after the experiment, so that all participants would only learn the outcome of the experiment after JOTP was over and they were no longer with their group members. Points earned were converted into Swiss Francs (one point = 0.25 CHF) and the subjects earned on average CHF 14.4 (approximately \$14). There was no show-up fee.

In total, 458 subjects participated in the experiments: 228 in the 'Social Groups' treatment and 230 in the 'Minimal Groups' treatment. Overall, 222 were assigned the role of A-players and participated in Experiment 1. Half were assigned to the in-group treatment, and half to the out-group treatment. Having made their choices in experiment 1, we elicited the subjects' beliefs about in-group and out-group cooperation, independent of the condition, they were in, i.e. beliefs about the proportion of participants who cooperated in each condition. To incentivize these stated beliefs, we gave participants one point for each prediction within 10 percent of the actual proportion.

After participating in Experiment 1, these same subjects participated as A-players in Experiment 2 (A-players did not know about Experiment 2 until after Experiment 1). The fact that A-Players always did Experiment 2 after Experiment 1 introduces a possible order effect for the A-players, which make interpretation of A-player's behavior in Experiment 2 less clear. But choices of the A-players in Experiment 2 are not of interest for our purposes; we are only interested in norm enforcement behavior of B-players, where order effects are not an issue.

236 subjects were assigned the role of Bs. They participated only in Experiment 2, and were assigned to one of four treatments (as illustrated in Figure 1). We elicited B-players deduction points using the strategy method, i.e., they specified how many points to deduct from their associated A-player for each possible combination of actions by A1 and A2 (for a discussion of the strategy method, see, e.g., Brandts and Charness, 2000). After the subjects had made their choices in experiment 2, we also elicited their beliefs about the cooperation rates of the A-players in the group composition to which they were assigned.

II. Results

We present the effects of the group manipulations in two steps. We first analyze Experiment 1, showing the impact on cooperation, and second, analyze Experiment 2, which shows the impact on norm enforcement.

> **Result 1:** *SG generates larger in-group effects on cooperation, and beliefs about cooperativeness, than MG.*

Panel (a) in Figure 2 shows the fraction of participants cooperating with an in- or an out-group member for both SG and MG. Overall, Panel (a) shows that there is in-group favoritism. Averaging across treatments, when matched with an in-group member 65 percent of participants cooperate while only 50 percent do so when matched with an out-group member. The difference of 15 percentage points is statistically significant in a Fisher's exact test ($p < 0.05$). Comparing the in-group effect for the two group manipulations shows that the effect is about twice as large, and statistically significant, for SG while it is smaller and not significant for MG: In-group cooperation rates are 20 percentage points higher than out-group in SG (Fisher's exact test; $p < 0.05$) versus 10 percentage points in MG (Fisher's exact test; $p = 0.32$). The 10 percentage point difference in the size of the in-group effects is not, however, statistically different across treatments (Fisher's exact test; $p = 0.20$). Notably, there is no quantitative difference at all in cooperation rates with out-group members, comparing MG and SG. So the higher rate of in-group favoritism in SG solely reflects increased cooperation in in-group interactions.

Panel (b) in Figure 2 shows that the same in-group effect is also evident in individuals' beliefs. Overall, participants expect in-group cooperation to be more pronounced than out-group cooperation (t-test; $p < 0.01$). The difference in beliefs about cooperation frequency is about twice as large for SG compared to MG (t-test; $p < 0.05$), exactly the same pattern as was observed for actual cooperation behavior.

[Figure 2 and Table 1 about here]

Table 1 shows the effect of different group manipulations on cooperation and beliefs in a regression format. Columns (1) and (2) present results from logit regressions in which the dependent variable is 1 if the participant cooperates and 0 otherwise. It shows that the probability of a participant cooperating significantly increases if they are matched with an in-group member, and that in MG this in-group effect is not significantly lower.[7] Columns (3) and (4) show results from two-limit tobit regressions in which the dependent variable is the participants' beliefs about the percentage of participants cooperating in an in- or out-group match. Because we have two measures of beliefs for each individual, standard errors are adjusted to allow for arbitrary correlation of the error term between observations for the same individual. Thus, unlike in Panel (b) of Figure 2, the standard errors in the regression provide the correct formal basis for assessing statistical significance. Participants expected 15.3 percentage points more in-group cooperation than out-group cooperation across the two treatments (column (3), $p < 0.01$). Also, for beliefs we have enough statistical power to find that the difference in the in-group effect between SG and MG is statistically significant (column (4), $p = 0.06$).

Figure 3 shows the effect of SG and MG on punishment behavior, varying the identity of the person who can be punished (player A1).

Result 2: *In-group members are punished less than out-group members in MG, but are punished just as hard as out-group members in SG.*

When A1 defects, B-players in MG show a tendency to punish less hard when A1 is from their group. By contrast, in SG, in-group defectors are punished virtually the same as out-group defectors. The difference in MG is sizable: in-group defectors receive around 1.5 deduction points less on average than out-group defectors. In the cases in which A1 cooperates, there is no difference in punishment between in- and out-group members - in both treatments. Notice also that the punishment of out-group members is virtually identical in MG and SG.

[Figure 3 about here]

Figure 4 shows punishment behavior varying the identity of A2, the counterpart of the person who can be punished.

[7]At the same time, we are also unable to reject the null of no effect in MG at conventional significance levels.

Result 3: *Defection against in-group members is punished more heavily in SG, whereas in MG the group affiliation of the victim of defection has no influence on punishment.*

In the SG treatment, shown in Panel (a), the B-player punishes harder when the victim of defection is an in-group member. Defection gets punished by 1.4 points more on average when the victim is an in-group member than an out-group member. By contrast, no such effect is present in the MG treatment. In this treatment, B-players punish A1 equally hard regardless of the membership of A2. In Result 2 and Result 3, the differences in punishment between the treatments are confined to the case in which A1 defects. Thus, we interpret them as differences in norm enforcement. Again, the treatments only differ in the ways that individuals react to the presence of in-group members in the A-player interaction. Punishment is harder in SG compared to MG if the victim is from the in-group. Punishment is essentially identical in SG and MG if the victim is from the out-group.[8]

[Figure 4 and Table 2 about here]

Table 2 summarizes the results in a regression format. The table shows coefficients from two-limit tobit regressions in which the dependent variable is the number of deduction points assigned to A1. We prefer the tobit because, overall, 53 percent of all punishment choices are zero, and 9 percent of all choices are 10 points. Thus, there is significant censoring that needs to be taken into account in the estimates.[9] Because we use four observations per individual, one for each possible combination of actions by the A-players, we adjust the standard errors for clustering on individuals.

Columns (1) - (3) show that group identity has no influence on punishment behavior when A1 cooperates. Columns (4) - (6) test for the differential effect of group membership on punishment in the SG and MG treatment that we saw in the figures. The Tobit estimates confirm the results seen in the graph: There is significantly less punishment of A1 in MG if he is a member of the B-player's group, but not in SG. Conversely, there is significantly more punishment of A1, if A2 is in the B-player's group in SG, but not in MG

Column (6) combines the two treatments and estimates interaction effects for the impact of group membership on punishment to test whether the reaction of punishment to group compositions also differ in a statistically significant way. The table shows that responses to the group membership of A1, and the group membership of A2, both differ significantly between the two treatments ($p = 0.03$

[8] In specifications not reported here, we also tested the model in columns (4), (5) and (6) where interacted a dummy variable indicating whether A2 defected with his group membership. We don't find any significant interaction in the two treatments overall, nor in any one of them.

[9] Using OLS does not change any of the conclusions about statistical significance, and delivers quantitatively similar estimates.

for A1, and $p = 0.08$ for A2). An F-test on both coefficients also confirms that group membership acts differently on punishment in the two treatments ($p = 0.03$). An interaction effect between the group membership of A1 and the group membership of A2 was not statistically significant and thus is not included in the regressions.

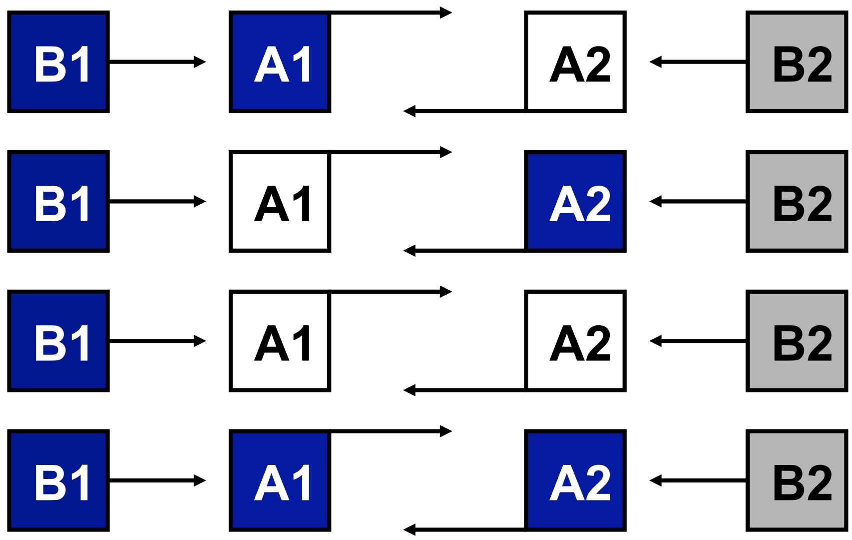
III. Conclusion

We find that minimal groups, in the form of arbitrary labels, cause individuals to be more likely to cooperate altruistically with in-group members than out-group members, and believe that in-group members are relatively more cooperative. Adding social ties makes both of these patterns more extreme, but only through increasing cooperation towards, and beliefs about, in-group members; behavior and beliefs about out-group members are unchanged. In terms of punishment, minimal groups lead to weaker punishment of in-group defectors than out-group defectors. Adding social ties eliminates this tendency, and generates another effect, which is stronger punishment when the victim of defection is from the in-group. Similar to the cooperation results, adding social ties only affects punishment behavior in interactions involving the in-group.

Our results imply that, both conceptually and empirically, economists should take into account that social ties are an important factor in group interactions, within organizations and societies. For example, organizations such as firms are often divided into sub-groups like departments, or teams, that may have more or less social interactions. While members in these groups share a group label, our findings suggest that the intensity of within-group social interactions may have important effects on how group members behave towards each other and towards outsiders. With relatively superficial groups, individuals might use group membership as a cue for being more lenient in punishing a defector. If groups involve social ties, however, in-group defectors may not get off easy, and there may be strong retaliation for defection on fellow group members.

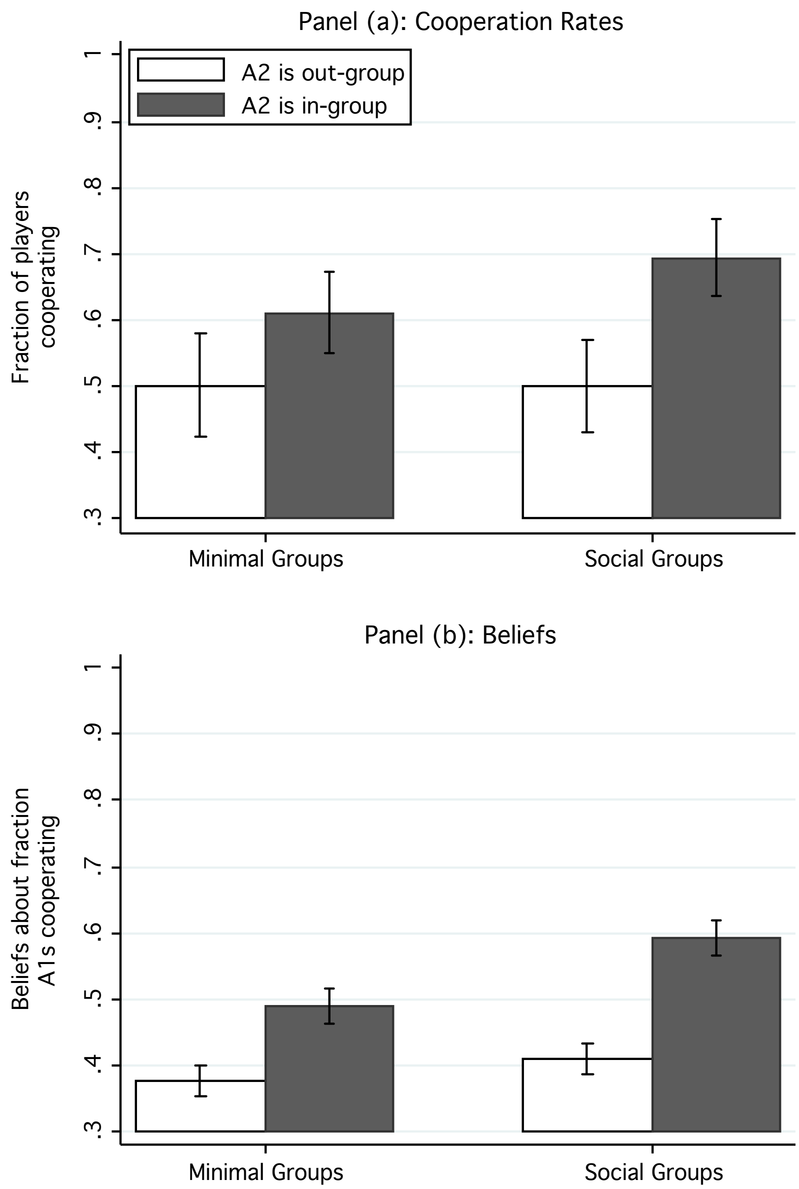
Based on the findings in this paper, investigating the mechanisms that underly the effects of social ties within groups seems to be a fruitful direction for future research. Based on existing evidence from psychology, one candidate mechanism is an impact of social ties on *empathy* between group members. Individuals in social relationships have been shown to exhibit stronger empathy for each other than for strangers, even at the neural level, when brain activity indicates that people "feel" the other's pain in response to adverse stimulus in the form of a mild electric shock (e.g., Stinson and Ickes, 1992; Cialdini et al., 1997; Singer and Fehr, 2005). On the other hand, if the person being subjected to the adverse stimulus first defected in a prisoner's dilemma game, this has been shown to eliminate empathy towards the defector, and even lead to enjoyment of observing them suffer same type of adverse stimulus (Singer and Fehr, 2005; Singer et al., 2006). Thus, heightened empathy triggered by social interactions might explain the tendency to punish especially hard when the in-group is harmed, because the punisher feels the victim's pain more acutely. At the same time, defection by in-group members may tend to eliminate empathy, helping explain why in-group members are not punished more leniently for defection.

Methodologically, our results point to potential limitations of using the minimal-group paradigm for understanding how group membership — in the presence of social ties — affects economic interactions. "Minimal" groups will only capture the labeling effect of groups and not the effect of social ties. Thus, the results indicate that other random assignments, to groups with social ties, may be more desirable, if the goal is to understand groups in the real world that have such content. If the convenience aspect of minimal groups nevertheless makes them more attractive, our results may be useful for understanding the particular ways in which the results will be misleading with respect to behavior of groups with social ties. At a more fundamental level, our results also raise the question of what constitutes a 'social' group and whether it can be created in the lab. This appears to be a fruitful avenue for future research.



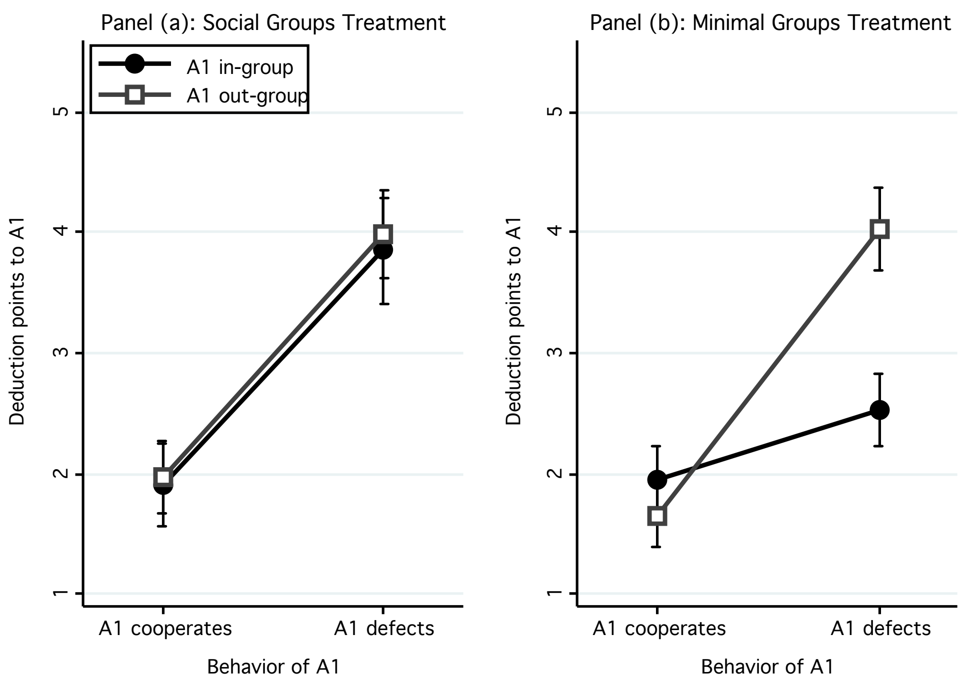
Note: The figure shows the group compositions in the four treatments in Experiment 2. The game allowed B1 to punish A1, and B2 to punish A2, conditional on the actions and A1 and A2 in a simultaneous prisoners' dilemma game. The dark shading indicates the four possible group combinations for B1, A1, and A2, which were implemented as different treatments (players with the same shading are from the same group). The design deliberately did not vary all possible combinations of B1 and B2 group roles, because of number of observations, so the effect of B2 group identity on B1 behavior is not studied. The pattern of B-player (and A-player) group compositions was identical across the *SG* and *MG* treatments.

Figure 1. Group Composition in the Third-Party Punishment Game



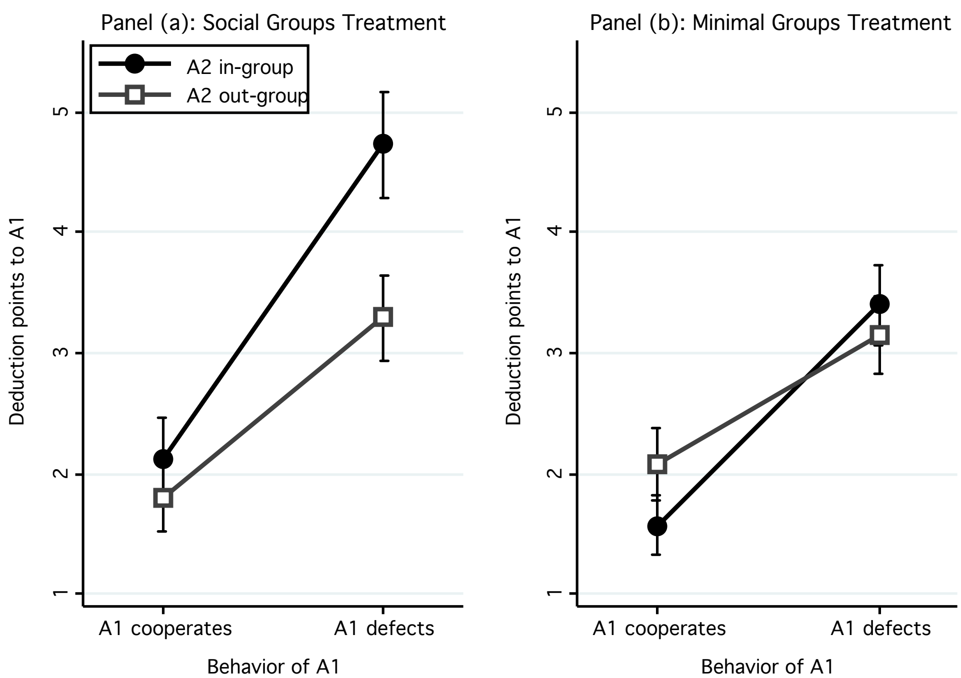
Notes:
Cooperative behavior and beliefs in Experiment1. Bars show standard errors of the mean.

Figure 2. Effect of Group Manipulation on Cooperation and Beliefs



Notes: Punishment behavior in Experiment 2. Bars show standard errors of the mean.

FIGURE 3. EFFECT OF GROUP MEMBERSHIP ON PUNISHMENT, VARYING GROUP IDENTITY OF A1



Notes: Punishment behavior in Experiment 2. Bars show standard errors of the mean.

Figure 4. Effect of Group Membership on Punishment, Varying Group Identity of A2

TABLE 1—EFFECT OF GROUP MANIPULATION ON COOPERATION AND BELIEFS

Dependent Variable:	**Cooperation (=1)**		**Beliefs**	
	(1)	(2)	(3)	(4)
A2 in-group	.638**	.817**	.153***	.185***
	(.279)	(.388)	(.0174)	(.023)
Minimal Groups (=1)	-.203	-1.09e-17	-.0742**	-.0397
	(.278)	(.412)	(.0327)	(.0346)
(A2 in-group)×Minimal		-.372		-.0679*
		(.559)		(.0347)
Constant	.0889	-1.50e-16	.429***	.413***
	(.238)	(.273)	(.0244)	(.0251)
N	222	222	428	428
Log Likelihood	-147.789	-147.566	-65.750	-64.913

Note: (Robust) standard errors in parentheses. Column (1)-(2) report coefficients of logit regressions in which the dependent variable is 1 if A1 cooperates and 0 otherwise. Column (3)-(4) report coefficients of tobit regressions where the dependent variable are the beliefs about cooperation rates. Standard errors in columns (3) and (4) are clustered on the individual level.

Level of significance: $*p < 0.1$, $**p < 0.05$, $***p < 0.01$.

Table 2—Effect of Group Manipulation on Punishment

	A1 Cooperates			A1 Defects		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatments	Social	Minimal	Both	Social	Minimal	Both
A1 out-group (=1)	.0962	-.968	.0752	-.816	3.53***	-.62
	(2.41)	(1.4)	(1.94)	(2.09)	(1.15)	(1.72)
A2 in-group (=1)	1.1	-1.99	.916	4.62**	.198	3.8**
	(2.42)	(1.39)	(1.96)	(2.09)	(1.1)	(1.68)
Minimal Groups (=1)			2.28			-1.9
			(2.09)			(1.85)
(A1 out-group)×Minimal			-1.21			4.73**
			(2.55)			(2.17)
(A2 in-group)×Minimal			-3.29			-3.6*
			(2.55)			(2.1)
A2 defects (=1)	-4.84***	-3.28***	-3.86***	-8.11***	-4.36***	-5.79***
	(1.77)	(.884)	(.861)	(1.48)	(.841)	(.761)
Constant	-3.8	.565	-2.31	4.37**	2.07*	3.98***
	(2.34)	(1.35)	(1.65)	(1.85)	(1.13)	(1.48)
N	223	248	471	223	248	471

Note: Dependent variable: # of deduction points. Coefficients of tobit regression. Robust standard errors corrected for clustering on individual level in parentheses.

Level of significance: $*p < 0.1$, $**p < 0.05$, $***p < 0.01$.

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