THE IMPACT OF GROUP MEMBERSHIP ON COOPERATION AND NORM ENFORCEMENT:

Evidence using Random Assignment to Real Social Groups

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I. Introduction

The success of organizations depends on individuals working efficiently with each other. Importantly, efficient actions often require non-selfish behavior, e.g., sharing of information even when it is impossible to tell that someone is withholding something, providing effort when it will not be rewarded, or sanctioning selfish behavior by others even when it is costly to do so.[1] Another important feature of organizations is that they are typically divided into salient groups. E.g., firms are divided into different divisions, and projects are often divided among different teams. These groups determine the circle of individuals with whom a worker interacts with on a daily basis.

This paper investigates whether mere membership in a group may increase pro-social behavior towards other group members, through the formation of social ties. If this is the case, then forming groups has an additional important benefit for organizations. Although the traditional unit of analysis for studying organizations in economics has been the individual, our hypothesis is in line with an alternative view, which holds that membership in a social group transforms individuals, leading to internalized roles, norms and values that affect behavior. This view has been advanced in social psychology (see Haslam, 2001, for an overview), in work on the economics of identity (Akerlof and Kranton, 2000) and in the literature on social capital (Putnam, 1993; Knack and Keefer, 1997; La Porta et al., 1997; Costa and Kahn, 2004). We also test a related, long-standing hypothesis in sociology and social psychology, that group assignment may have a dark side, in the form of reduced willingness to cooperate with individuals outside the group,

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[1] Reputation and repeated-game incentives are also important means of deterring selfish behavior (see, e.g., Milgrom and Roberts, 1992). There are many settings, however, where such purely selfish deterrents may prove ineffective. For example, discount rates may simply be too high to implement the efficient solution in repeated games (because of, e.g., a high exogenous separation hazard), or in instances where the principal cannot monitor agents effectively, but agents on the same level of hierarchy could. It is these cases that we are concerned with here.

or even outright hostility towards outsiders (Hewstone, Rubin, and Willis, 2002; see also Durlauf, 1999).

Our main methodological contribution is a new experimental design, which avoids some of the primary confounds encountered by previous studies on this topic. One type of previous evidence comes from choice experiments conducted in the field with members of existing social groups.[2] These studies find that individuals are more willing to cooperate with, and trust, members of their own social group as opposed to outsiders. Some of these studies also look for evidence of hostility between groups, and find that hostility does exist. Kollock (1998) finds that members of college fraternities are willing to sacrifice money to reduce the payoff of a subject that is not a member of their fraternity, and Bernhard, Fehr, and Fischbacher (2005) find that members of clans in Papua New Guinea punish members of other clans more harshly. Although these studies provide valuable facts about the behavior of existing social groups, there is a fundamental problem in identifying the effect of group formation *per se*, which is that these groups are not based on random assignment. Existing groups are typically formed endogenously, and group members differ systematically in terms of personal characteristics, experiences, and culture. By contrast, the ideal experiment would study cooperation and punishment behavior following the random assignment of otherwise identical individuals to different social groups.

Laboratory experiments in social psychology provide another type of evidence. In these experiments, individuals are randomly assigned to different groups by the experimenter, based on a patently irrelevant characteristic, such as a preference for paintings by Kandinsky or Klee (Tajfel and Turner, 1979; Rabbie, Schot, and Visser, 1989; Tajfel, Billig, Bundy, and Flament, 1971; Yamagishi, Jin, and Kiyonary, 1999).

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[2] Groups used in previous studies include college fraternities (Kollock, 1998), religious sects (Fershtman and Gneezy, 2000), different universities in Japan (Shinada, Yamagishi, and Ohmura, 2004), citizens of southern and northern European countries (Bornhorst, Ichino, and Schlag, 2004), clans in Papua New Guinea (Bernhard, Fehr, and Fischbacher, 2005), and residents of different city districts (Falk and Zehnder, 2005).

Strikingly, treatment reliably produces in-group favoritism, i.e., higher cooperation between in-group members than in a setting where no groups are assigned at all. In contrast to studies using real social groups, these minimal-group experiments tend not to find hostility effects. It would be premature, however, to conclude based on this evidence that group membership does not lead to hostility. The groups in these studies are artificial, in the sense that they are created by the experimenter, and lack the social ties of real social groups, which may play an important role in how individuals behave towards members of other groups.

We conduct experiments using the random assignment of individuals to real social groups, exploiting the fact that individuals are randomly assigned to platoons during a four-week phase of officer training in the Swiss Army. In this sense our design is similar to the classic Robber's Cave Experiment (Sherif et al., 1961), in which 11-year old boys were randomly assigned to one of two groups in a summer camp. In this study, the equivalent of war among 11-year olds erupted, e.g., hostile actions such as name-calling, pranks, and stereotyping, when the two groups were brought together to compete in various games. Our design differs from the Robbers Cave study in that there is no institutionalized competition between the groups, and thus we study the impact of group membership on cooperation and hostility in the absence of competitive pressures between groups. We also study behavior in an anonymous, one-shot interaction, which eliminates repeated game effects and isolates the impact of group assignment on non-selfish motives.

Our first experiment is a simple simultaneous-move prisoners' dilemma, played as a one-shot interaction between individuals who are anonymous except for platoon affiliation. We conduct the experiment three weeks into a four-week Joint Officer Training Program. We find significantly more cooperation when subjects interact with a member from their own platoon than when they interact with a member from another platoon. This finding is striking given that the groups have only existed for three weeks, and will be dissolved in one week at the end of training. Importantly, the finding is consistent with in-group favoritism but also with out-group hostility. Subjects may defect more against the out-group simply because they are selfish in this case, or because they actively dislike of the out-group.

In the second experiment, we add "third-party punishment" (Fehr and Fischbacher, 2004) to the prisoners' dilemma. This provides us with clear and simple measures of norm enforcement (punishment of defection). We do not find evidence of hostility between groups in punishment: punishment is the same, whether the person being punished is a member of the punisher's own platoon or another platoon. Thus, our results lend no support to the conjecture that strong social ties within a group necessarily create hostility towards others. This suggests that earlier findings may confound the effect of group ties with non-random differences between groups or with institutionalized competition between groups.

We do find that group membership has an impact on norm enforcement, in the form of harsher punishment when the victim of defection is from the same platoon as the punisher. This effect is distinct from hostility, however, as it is related to the victim of defection, not the identity of the defector. These findings, and the finding from Experiment 1, are both compatible with in-group favoritism. Thus, taken together, in-group favoritism explains all the differences between treatments in our experiments.

The remainder of this paper is structured as follows: Section II explains the random assignment to platoons that we use as the manipulation of group identity, section III introduces the experimental design and the predictions. Section IV reports the results and section V concludes.

II. Random Group Assignment

In this study, we exploit a particular feature of the officer training-program in the Swiss Army that generates random assignment to social groups.

Institutional Background: The Swiss Army requires all Swiss males to perform at least 300 days of military service.[3] After medical and psychological examinations, most young

[3] About half of that time is served in basic training, while the rest of the 300 days is served in three-week episodes every year.

men are admitted to basic training for twenty-one weeks. In week 7 of basic training, there is an opportunity to be selected to aspire to become an officer – the only way to obtain this rank. Those selected then go through ten weeks of officer candidate training. About one fourth of them are then selected to become officers, while the others are sent to training programs to become non-commissioned officers.

The Joint Officer Training Program: Whereas officer candidate training is specific for each branch of service, and occurs in separate locations, the next phase brings new officers from all braches of service together, to the same location, for four weeks of the Joint Officer Training Program (JOTP). Officers are randomly assigned to a platoon at the beginning of training, and go through all instruction with their platoon. Thus officers spend virtually all time during the day with their platoon. Subjects of study include principles of security, combat in large military units, logistics, and leadership. At the end of the four weeks, the platoons are dissolved and officers are once again sent to separate locations, for further, advanced training specific to each branch of service.

We use this assignment as our manipulation of social groups. Assignment to platoons is random, and stratified according to the different branches of service. The army intentionally does this to promote contacts and exchanges of perspectives among the different branches of service, particularly because the material taught in JOTP applies equally to all branches of service.

The assignment mechanism is ideal, in several ways, for investigating the impact of group membership on behavior. First, the trainees know that the composition of the platoons is identical and that nobody could choose which platoon to join. Table 1 provides a check of this randomization. Statistical tests reveal no significant differences in platoon composition, by branch of service, education, or age. Second, there is no competition between the groups (or trainees) for evaluations or other resources. Evaluations were completed previously, in candidate training. Third, despite the stratified random assignment to the platoons, social ties form very quickly. As we show in Table 2, the officer candidates spend significantly more off-duty time with their fellow platoon members. This is remarkable in itself, given that 79.8 percent of the trainees know people

in other platoons, mostly from earlier stages of their training. Yet, they choose to spend most of what little off-duty time they have with members of their platoon.

III. The Experimental Design

We conduct two experiments, which study the impact of random assignment to real social groups on cooperation and norm enforcement.

Experiment 1: Cooperation. The game is a simultaneous prisoners' dilemma. The players, labeled A1 and A2, are each endowed with 20 points. They must simultaneously decide whether to keep the points or to pass all of them on to the other player. Points that are passed are doubled. Thus, keeping the points is equivalent to defection, while passing on the points equals cooperation.

There were two treatments in Experiment 1. In the in-group treatment, the subjects were informed that the other player in this experiment was a member of their platoon. In the out-group treatment, the subject was informed that the other player was a member of another (specified) platoon. The group composition was clearly marked on the decision sheets. These two treatments allow us to examine how the group membership of the second player affects cooperation. In-group favoritism and inter-group hostility both predict less cooperation in the out-group treatment than in the in-group treatment.

Experiment 2: Norm Enforcement. In Experiment 2, we add two players, labeled B1 and B2, each endowed with 70 points. B1 can assign up to 10 deduction points to A1, and B2 to A2. Each deduction point reduces the points of the A-player by three points. The B-players can condition their choices on the actions of A1 and A2. Thus Experiment 2 incorporates the possibility of third-party punishment, and is suited for examining how norm violations (i.e., defection) are punished and what the determinants of punishment are.

To examine the impact of group membership on norm enforcement, we vary the composition of players in each game. For the remainder of the paper, we will always 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.

Varying the group membership of A1 allows us to examine the direct effect of group membership on punishment. If there is inter-group hostility, then we should see more punishment if B1 can punish an A1-player from a different platoon. Varying the group membership of A2 allows us to look at a different potential motive for punishment. If B1 and A2 are from the same platoon, then B1 might punish A1 more harshly for defection against A2. Notice that this retaliation in service of the in-group is different from hostility, as A2's identity is varied while A1 is held constant.

Procedures: The experiment was conducted with paper-and-pencil in a large auditorium, three platoons at a time. The subjects were ordered into the auditorium and did not know of the experiment in advance. Different platoons were seated in separate areas. In each session, if a subject was in an out-group treatment, the member of the other platoon was from a platoon not present in the same session. The experiment lasted 45 minutes, and the subjects earned on average CHF 17.10 (approximately $13.00). All earnings came from choices in the experiment; there was no show-up fee.

Overall, 228 subjects participated in the experiments. 116 subjects 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. After participating in Experiment 1, these same subjects participated as A-players in Experiment 2. This procedure introduces a possible order effect for the A-players, but this is not a problem because our research questions do not involve comparing the behavior of A-players across Experiments 1 and 2. 112 subjects were assigned the role of Bs. They participated only in Experiment 2, and were assigned to one of four treatments. We used the strategy method for the B players to indicate their deduction points, i.e., they specified how many points to deduct from their associated A-player for each possible combination of actions by A1 and A2.

After the two experiments, we elicited beliefs about other players' behaviors in the treatments in which the subjects had participated. Finally, the subjects filled in a short demographic questionnaire.

Special care was taken to ensure anonymity: Payoffs were determined after the experiment, and hence, in "the heat of the moment", subjects did not know the outcome of the experiments. The payoffs were mailed to the subjects' home address ten days after the experiment, i.e., after the JOTP was over.

IV. Results

We first present results from Experiment 1, showing the impact of randomly assigned groups on cooperation, and then present results of Experiment 2, on the effect of group membership on norm enforcement.

RESULT 1: Cooperation and expected cooperation of others is significantly higher in the in-group compared to the out-group treatment.

Table 3 presents the results of Experiment 1, which show that people are more likely to cooperate with a member of their own platoon than with a member of another platoon. While 69 percent cooperate if they are paired with an in-group member, only 50 percent do so when playing with an out-group member (the differences is statistically significant at the 95 percent level in a Fisher exact test). Thus, random assignment to real social groups leads to higher cooperation in a completely anonymous interaction with another member from your own group than in an anonymous interaction with a member from another group.

[Table 3 about here]

The difference between in- and out-group treatment is also reflected in subjects' beliefs (see Table 3). On average, subjects in the in-group treatment expect 57 percent of their own platoon members to cooperate, whereas subjects in the out-group treatment expect only a 41 percent cooperation rate from members of another platoon (t-test; $p<0.001$). In fact, the differential in cooperation is fully reflected in subjects' beliefs. Regressing the decision to cooperate on a dummy variable for the in-group treatment, and also the expected cooperation rate for other players, the in-group dummy becomes essentially zero, and is insignificant, whereas the beliefs are still strongly associated with the decision to cooperate.

The design of Experiment 1 does not allow us to address two important questions. First, does daily interaction with a member of one's own platoon change beliefs about this person's behavior, or does it also change preferences towards him? Second, does the higher cooperation rate among in-group members reflect only in-group favoritism, or does it also indicate the presence of out-group hostility, i.e., a desire to harm the out-group? By analyzing norm enforcement behavior, in Experiment 2, we are able to shed some light on these questions.

RESULT 2: No difference in punishment of in- and out-group members.

Figure 1 shows punishment behavior of Bs in Experiment 2 as a function of A1's behavior and A1's identity. It is noteworthy that defection is punished more harshly than cooperation. B1 punish cooperation with approximately 2 points while defection is punished with approximately 4 points. The main result is that B1s do not condition their punishment on whether they are punishing an in-group member and out-group member. The point estimates show that B1s assign the same deduction point to A1, independent of whether he is from their platoon or not. This holds for deduction points assigned for defecting as well as cooperating. Thus, our results lend no support to the conjecture that strong group ties, resulting from random assignment to groups, lead to hostility towards outsiders.

[Figure 1 about here]

RESULT 3: Punishment is stronger when A1's defection affects an in-group member as opposed to an out-group member.

Figure 2 shows punishment behavior of B1s in Experiment 2 as a function of A1's behavior and A2's identity. The figure shows that, in the case that A1 defects, punishment behavior is affected by varying the group membership of A2. If B1 and A2 are from the same platoon, A1 is assigned approximately 1.4 deduction points more for defection, compared to a case where A2 is from a different platoon. Thus, social ties lead to individuals to retaliate more strongly against defection when the victim is an in-group member. A further interesting question is whether this retaliation in service of the in-group is more pronounced when the defecting A1 is from the out-group.

[Figure 2 about here]

Figure 3 compares treatments where B1 and A2 are from the same platoon, but where the platoon affiliation of A1 varies. The results are suggestive of an interaction effect. If B1 and A2 are from the same platoon, A1's defection is punished particularly harshly if he is an out-group member. However, while the point estimates are suggestive of this effect, each comes with a wide margin of error. We are therefore unable to detect a significant impact of varying the group identity of A1 on the retaliation motive identified in Figure 2.

[Figure 3 about here]

Table 4 summarizes our results regarding norm enforcement. It reports regressions of the number of deduction points assigned to A1 on two dummy variables: the first variable equals 1 if A1 belongs to B1's platoon and 0 otherwise; the second equals 1 if A2 belongs to B1's platoon and 0 otherwise. The regressions address a potential shortcoming of the results discussed in Figures 1 to 3, for which standard errors were calculated assuming independent observations. This assumption could be problematic given that two observations in each cell come from the same individual. We address this issue in Table 4 by clustering on individual, i.e., correcting the standard errors to allow for arbitrary correlation of the error term across observations for the same individual.

[Table 4 about here]

Column 1 of Table 4 reports the results for the case where A1 defects. As can be seen, the identity of A1 has no impact on punishing behavior. However, A2's identity has a significant impact on deduction points, even with the conservative method used to correct the standard errors. Column 2 displays the results for the case where A1 cooperates. The estimates confirm the impression from figure 1 and 2, that in this case neither A1's identity nor A2's identity significantly affect B1's deduction points.

In Columns 3 and 4 we report results for the cases where we allow punishment to depend on the interaction between A1 and A2's identity. As before, we are unable to detect the exact origin of the impact of A2's identity on norm enforcement. The coefficient on A2's identity now reflects the case where A1 is from another platoon. Punishment is significantly higher in this case. However, the point estimate for the interaction effect between A1's and A2's identity is negative, indicating that when B1

assigns deduction points to somebody from his own platoon, A2's identity matters less. However, the latter is statistically significant.

V. Conclusions

Within organizations, individuals work in groups, e.g. divisions of a company, or project teams. An important factor determining the success of organizations is the willingness of individuals to take non-selfish, efficiency-enhancing actions. In this paper we test whether mere membership in a group has an impact on individuals' willingness to behave non-selfishly, thus affecting performance of organizations.

In contrast to previous studies, our experimental design uses random assignment of individuals to real social groups. This avoids problems of selection on individual characteristics when groups are formed endogenously. It also avoids the artificiality and lack of social content of groups created in laboratory experiments.

We find that after only three weeks, random assignment to a group leads to the formation of social ties, and increases willingness of individuals to engage in non-selfish, cooperative behavior with members of the same group. We find no evidence that group membership leads to hostility, but we do find that group membership has an impact on norm enforcement: individuals punish norm violation more harshly if the victim is a member of their own group.

Our results imply that groups have an important, additional benefit for organizations. Interesting questions for further research include determining the optimal group size for sub-groups within an organization. Also, it is interesting to investigate whether the hostility observed in previous studies, between endogenously-formed groups, is explained purely by demographic differences, or whether it is driven by an interaction between demographics and social ties.

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TABLE 1 – TEST OF RANDOMIZATION

	Branches of Service				Education			Age
	Infantry	Mechanised Infantry	Rescue Units	Other	Appren-ticeship	High School	Other	
Mean	23.3%	12.8%	18.0%	45.8%	31.4%	55.6%	12.8%	21.58
s.d. of means across platoons	5.5%	6.2%	7.6%	10.6%	11.8%	9.7%	11.1%	0.53
Difference between platoons (*p*-value)	0.93	0.6	0.52	0.41	0.36	0.41	0.073	0.41

TABLE 2 – OFF-DUTY TIME SPENT WITH MEMBERS OF DIFFERENT PLATOONS (# OF TIMES PER WEEK)

	Own platoon	Other platoon
Mean	2.24	0.6
Fraction with zero times per week	7%	53%
Fraction with more than one time per week	47%	7%

Notes: Number of observations= 216. The two distributions are significantly differet according to a Wilcoxon Signed-Rank test ($p < 0.001$).

TABLE 3 – COOPERATION AND BELIEFS IN IN-GROUP AND OUT-GROUP TREATMENT		
	Fraction deciding to cooperate	Average Expected cooperation rate
In-group treatment	69.4 %	56.8 %
Out-group treatment	50.0 %	40.5 %
Test of difference	Fisher's exact test: $p < 0.05$	*t* - test: $p < 0.001$

TABLE 4 – DEDUCTION POINTS VARYING THE IDENTITY OF A1 AND A2

Variable	Column 1	Column 2	Column 3	Column 4
A1 from platoon of B1 (=1)	0.077	0.055	0.622	0.113
	(0.624)	(0.487)	(0.830)	(0.604)
A2 from platoon of B1 (=1)	1.44**	0.372	1.97**	0.427
	(0.610)	(0.505)	(0.782)	(0.678)
A1 *and* A2 from platoon of B1 (=1)			-1.33	-0.14
			(1.440)	(1.050)
Constant	3.28***	1.77***	3.56***	1.8***
	(0.566)	(0.371)	(0.626)	(0.394)
R squared	0.136	0.0261	0.142	0.0262
N	221	222	221	222

Notes: Dependent variable: deduction points. Coefficients of OLS-regression. Robust standard errors adjusted for clustering on individuals in parentheses. Treatment effects are absorbed. Columns 1 and 3 show the cases where A1 defects, and columns 2 and 4 show the cases where A1 cooperates.
Level of significance: ** 0.01<p<0.05, *** p<0.01

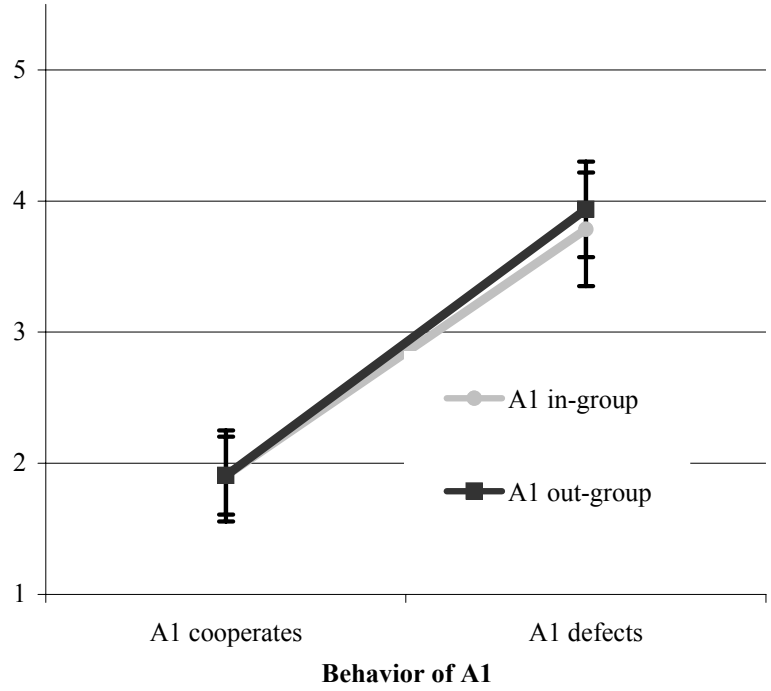

FIGURE 1: NORM ENFORCEMENT AND THE IDENTITY OF A1

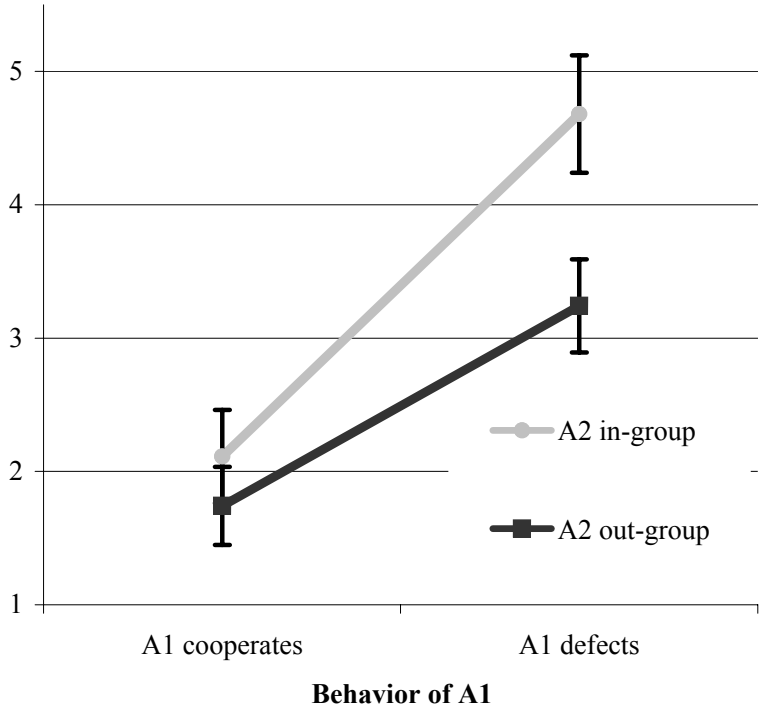

FIGURE 2: NORM ENFORCEMENT AND THE IDENTITY OF A2

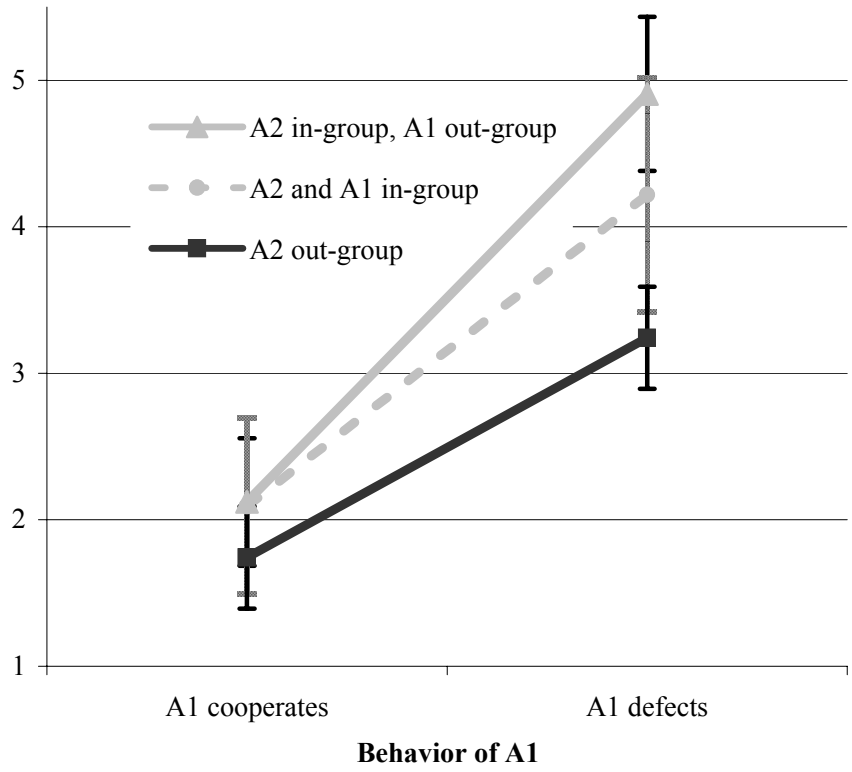

FIGURE 3: NORM ENFORCEMENT AND THE IDENTITY OF A2 AND A1