The Impact of Segregation on Intergroup Conflict:

An Experimental Study

Károly Takács[1]

[1] I am grateful to Roel Popping, Frans Stokman, Andreas Flache, and Wim Liebrand for their valuable and thorough advises, comments, and criticisms. I thank John Dovidio, Marcel Lubbers, Carlijne Philips, Rafael Wittek, Marcel van Assen, Brent Simpson, Alexander Gattig, Christian Steglich, René Thorenvlied, Corine Hoeben, Uwe Matzat, Béla Janky, Marieke van der Wal, Rita Smaniotto, Martin van der Gaag, Alinda van Bruggen, and Jan Kratzer for their comments on the experimental design and/or on an earlier version of this paper. I also would like to thank Sicco Strampel, Jeanette Loos, and Rie Bosman for their practical help in conducting the experiments.

Address of correspondence: Collegium Budapest / Institute for Advanced Study, Szentháromság u. 2, 1014 Budapest, Hungary. E-mail: ktakacs@colbud.hu. The research was conducted while the author was a graduate student at the Interuniversity Center for Social Science Theory and Methodology, University of Groningen, the Netherlands.

Abstract

Dense in-group and scarce out-group relations (segregation) often support the emergence of conflicts between groups. A key underlying mechanism is social control that helps to overcome the collective action problem within groups, but contributes to harmful conflicts among them in segregated settings. In this study, a new experimental design is introduced to test whether internalized social control affects contribution decisions in intergroup related collective action. Subjects played single-shot Intergroup Public Good games in two groups of five without communication. Seating patterns were manipulated to detect forms of social control that are activated conditional on expectations and on neighborhood composition. Results confirm the influence of behavioral confirmation and the conditional impact of internalized traitor and selective incentives. As an aggregated consequence of these social control effects, harmful intergroup outcomes were least likely when members of the groups were seated in a mixed pattern.

JEL classification: C91; C92; D74; H41; Z13

Keywords: Intergroup conflict; Segregation; Team games; Social control; Neighborhood effects

Single-shot social dilemma experiments consistently find nonzero cooperation rates. A lot of people act against their egoistic interests and make sacrifices for the collectivity also in strictly impersonal settings in which no communication is allowed and subjects are completely strangers to each other. In a competition situation with another group, experiments find even higher contribution rates to the provision of a public good (Bornstein, Erev, and Rosen, 1990; Schopler and Insko, 1992; Bornstein and Ben-Yossef, 1994; Insko et al., 1994). When intense intergroup competition leads to negative consequences for members of both groups, public "bads" are provided instead of public goods. Why do people still act in favor of their groups under such circumstances?

This paper argues that the monetary payoff structure of experimental games does not fully describe the incentives of subjects in the laboratory. There are also other substantial utility concerns. The emphasis here will be on the role of incentives that stem from *structural embeddedness*. The importance of structural embeddedness and social networks in social dilemmas was highlighted by both theoretical (e.g., Marwell, Oliver, and Prahl, 1988; Gould, 1993; Flache and Macy, 1996; Chwe, 1999) and empirical studies (e.g., McAdam, 1986; Chong, 1991; Finkel and Opp, 1991; Gould, 1995; Sandell and Stern, 1998). Previous research showed that *dense* network relations help the establishment of collective action (Marwell, Oliver, and Prahl, 1988; Marwell and Oliver, 1993; Gould, 1993). Network effects are attributed to the fact that individuals are influenced by the presence, opinion, expectations, and behavior of friends, neighbors, colleagues, and the family, when they decide to participate in collective action. These mechanisms can be summarized as *social control* (cf. Kornhauser, 1978; Gibbs, 1981; Black, 1984; Heckathorn, 1990; 1993; Macy, 1993; Villareal, 2002).

Only few research have tried, however, to describe and measure these effects in a controlled environment (some indications are given for the presence of social control by Yamagishi, 1986; van de Kragt, Dawes, and Orbell, 1988; Rapoport, Bornstein, and Erev, 1989; McCusker and Carnevale, 1995; Gächter and Fehr, 1999; Rege and Telle, 2001). Structural considerations were disregarded by previous experiments on intergroup relations, but also by the whole experimental literature on social dilemmas. As research in this field is dominated by psychologists and economists, the neglect of sociological concerns is not that surprising. These avenues, however, should be pursued to gain

further insights into determinants of individual behavior in social dilemmas. This paper argues that social control in certain forms and also elementary structures might be present in the laboratory and can make a significant difference to contribution decisions, even when subjects do not know each other and are not allowed to communicate.

As a model of intergroup relations, an extension of the Intergroup Public Goods (IPG) game (Rapoport and Bornstein, 1987) will be used that nicely represents the dichotomy of interdependencies within the groups (provision of a public good) and between the groups (intergroup competition for a scarce resource). A group wins a public good from the competition, if more members contributed to the provision than in the other group. Among members of the defeated group, a "public bad" is distributed, which is the worst case scenario. Contribution of equal strength leads to mutually harmful outcomes (clash punishment). The IPG game in this form is intended to model group competitions such as civil war, conflicts between pupil groups, fights between football supporters or urban gangs. In case of only few initiators, nothing happens, the status quo is preserved. Under a certain threshold, no competitive action is established. In this paper, an outcome will be called *intergroup conflict*, if one or both of the groups receive negative public rewards (public bads), or equivalently, at least in one group a competitive collective action is established.

Structural embeddedness has been incorporated into this model by assuming the influence of dyadic social control mechanisms, namely *social selective incentives*, *behavioral confirmation*, and *traitor rewards* (Takács, 2001). These forms of social control have been shown to be possible underlying mechanisms of the effect of *segregation* on intergroup conflict. The model predicts that the effect of segregation on the likelihood of intergroup conflict can be characterized by an S-shape function. This implies that segregation is likely to promote intergroup conflict, but in certain ranges of segregation, an additional change does not result in an increase in the likelihood of conflict (Takács, 2001). These results directly lead to the main question of this study. Does segregation really increase the likelihood of intergroup conflict? In the context of a laboratory environment, is intergroup conflict more likely when group members are arranged in a segregated pattern?

Social control in experiments

This study will examine what types of external and internalized social control influence the decision of subjects to contribute or not to the provision of intergroup public goods in controlled experimental conditions. It will be explored what forms of social control back the segregation effect on intergroup conflict, if there is any.

Three fundamental forms of social control will be considered as possible mechanisms. The first is the distribution of *social selective incentives*, such as prestige or respect. Empirical studies show that social selective incentives or sanctions are disseminated mainly locally, through interpersonal relations (Sandell and Stern, 1998). Selective incentives often become internalized (Scott, 1971; Kornhauser, 1978; Coleman, 1990: 293) as *contribution norms* that create a cognitive reward for cooperation. This motive, that can be summarized as "I do the right thing for the group", is relevant even if individual decisions are completely confidential (Opp, 1989).

The second prominent form of social control is *behavioral confirmation* (Lindenberg, 1986) that expresses the subject's desire to conform the expected behavior of other individuals. It means that doing the same as the other one has a value by itself and increases the utility of both sides independently from future interactions. Finkel and Opp (1991) have found that participation in collective political action can be largely explained by willingness to conform to the behavioral expectations of important others. In empirical collective action situations (e.g., strikes, demonstrations, and revolutions) people are confirmed positively or negatively for participation by friends and from other network ties. Chong (1991) and Oberschall (1994) described this as an assurance process. There is indication for the relevance of an assurance process also in public good experiments (Yamagishi, 1986; McCusker and Carnevale, 1995; Rege and Telle, 2001). Behavioral confirmation can have a two-fold effect: confirmation by participating fellows provides an incentive for contribution and confirmation by free riders works against contribution. Even if others are not able to monitor individual choice, behavioral confirmation might effect decisions as an internalized mechanism or imitation strategy (Asch, 1956; Dawkins, 1976; Pingle, 1995).

The third form of social control is present in network relations between members of the opposite groups and is referred to as a *traitor reward*. It is a social selective incentive that punishes contribution and rewards defection. Members of the competing groups have contradictory interests in intergroup competition. Since their social tie is valuable for them, they reward each other's action that is against the own group's interest (e.g., Kuran, 1995, 9-10). This form of social control is also likely to be internalized as a fear from local conflict and benefit for local harmony. Betraying someone else elicits an unpleasant feeling of guilt; therefore people try to avoid this (e.g., Poundstone, 1992: 223). Traitor rewards can provide an explanation why contact can help to normalize intergroup relations (cf. Allport, 1954).

As an aggregated consequence of these forms of dyadic social control, the network structure of individual relations influences the likelihood of intergroup conflict. Dense in-group relations and scarce out-group relations are correlated with extensive distribution of social selective incentives between fellows and limited realization of traitor rewards. Hence, *segregation* supports the emergence of harmful intergroup clashes. The key elements of the explanatory mechanism are the fundamental forms of social control.

A major difference compared to real situations is that subjects are unknown to each other in the laboratory, consequently there are no social network relations between them. Can social control operate under such circumstances?

Experimental evidence shows that face-to-face contact facilitates cooperation in conflict situations (cf. Drolet and Morris, 2000). Previously, this finding was explained by the social psychological process of rapport that is conceptualized as a "state of mutual positivity and interest that arises through the convergence of nonverbal expressive behavior in an interaction" (Drolet and Morris, 2000: 27; Tickle-Degnen and Rosenthal, 1990). There is no doubt that when subjects are able to communicate with nonverbal signs or are able to send emotional signals, they influence the behavior of each other in the social dilemma task. The question is whether *minimal contact* has an additional effect that is due to the activation of internalized social control.

To test the presence of different forms of social control and the segregation effect on intergroup conflict in a controlled environment, in this research a *new experimental*

design is introduced. In the experiments, seating patterns were varied and visibility conditions were manipulated in order to detect forms of social control that are activated conditional on the neighborhood composition. Minimal contact was introduced between neighboring subjects in the form that subjects were able to see their neighbors and they were able to identify the group membership of each other. Extensive nonverbal communication was disallowed and decision times were restricted to avoid initiations of signaling. It was tested whether this minimal contact is sufficient to activate internalized forms of social control.

Additional to minimal contact, in later parts of the experiments, behavioral confirmation and selective incentives were introduced as monetary side-payments. Obviously, these effects are expected to be stronger than internalized effects. With their introduction a meaningful comparison can be made between the size of monetary and internalized social incentives.

With regard to forms of social control, the following hypotheses are explicated. Social control is expected to enter subjects' considerations in an internalized form when eye contact is established and in an external form when monetary side-payments are included.

Selective incentives: Selective incentives both in an internalized and in a monetary form have a positive effect on contribution propensities. This form of social control operates between individuals from the same group. *More connections to group fellows means the distribution of selective incentives from multiple sources.* Hence, the more group fellows are in the neighborhood in the laboratory, the higher the contribution rate is.

On the other hand, the effect of behavioral confirmation is not only dependent on the composition of the network neighborhood, but also on expected decisions of neighbors. It is presumed that subjects do not make qualitative differences between fellow neighbors.

Behavioral confirmation: Behavioral confirmation both in an internalized and in a monetary form have an effect on contribution propensities. The direction and the size of the effect depend on the number of expected contributors and on the number of expected defectors among neighboring group fellows. If the former is higher, the effect is positive.

If the latter is higher, the effect is negative. The size of the effect is a linear function of the difference between the two.

The presence of neighbors from the opposite group triggers the effect of internalized traitor rewards. For the sake of simplicity, this form of social control was not introduced in a monetary form in the experiments.

Traitor rewards: Internalized traitor rewards have a negative effect on contribution propensities. The higher the number of members of the opposite group in the neighborhood, the lower the contribution rate is.

Network connections are conceptualized as adjacency in the seating configuration in the experiment. As neighbors are expected to be the direct source of social control, different neighborhood compositions would lead to different contribution propensities. At the aggregated level, different outcomes can be predicted for different neighborhood structures. From the nature of the specified social control mechanisms it follows that segregation is likely to promote intergroup conflict (cf. Takács, 2001). On the basis of this result, the following hypothesis can be formulated for the IPG experiments:

SEGREGATION HYPOTHESIS: *In a segregated structure, contribution rates will be higher and intergroup conflict will be more likely.*

Furthermore, Takács (2001) also specified the impact of the relative size of social control mechanisms on intergroup conflict. Results demonstrated that the segregation effect on intergroup conflict is stronger where selective incentives are relatively important when compared to behavioral confirmation. In order to test this theoretical prediction, a *normative pressure* condition and a *confirmation pressure* condition was implemented in the experiments. In the normative pressure condition, selective incentives were introduced as monetary side-payments. In the confirmation pressure condition, monetary behavioral confirmation rewards were included. On the basis of the theoretical prediction, the hypothesis for the experiments is as follows:

The segregation effect on the likelihood of intergroup conflict will be stronger in the normative pressure condition than in the confirmation pressure condition.

Experimental design

In this section, a new experimental design is introduced that is invented to test the hypotheses formulated in the previous section. A key element of the design is the use of a series of single-shot IPG games as a model of competitive intergroup relations.

The payoffs of the game used in the experiments are outlined here. There were two groups: the red group and the green group consisting of five members each. Every player had to decide individually whether to keep a bonus of 11 points completely (1 point was equivalent to 1 NLG = 0.42 USD) or to give all of it to help their group in the competition. Depending on the number of contributors in the groups, public good and "bad" rewards were distributed equally among all group members. The sizes of these rewards in the experiments are shown in Figure 1.

Everyone received these rewards, regardless of the decision to keep or give away the bonus of 11 points. Figure 1 does not include the bonus reward that is added to the payoff of those subjects who decided to keep the bonus. Moreover, to ensure positive payoffs, every subject was entitled to an additional payment of 15 points at the end of the experiment.

FIGURE 1 SOMEWHERE HERE

In order to obtain more data in the experiments, the game was played many times in each session, but subjects received payments in a randomly selected single round only. No information was provided during the experiment about what has happened in earlier rounds. This method was applied in earlier team game experiments by Bornstein and Ben-Yossef (1994).

Another key element of the design is the treatment of structural embeddedness. Three types of seating arrangements are implemented between the experiments with full segregation, a complete mixture, and a medium segregation condition. Additionally, in the control condition, subjects made decisions in isolation. Every experiment started with a control condition, in which isolated subjects had to make their decisions without the knowledge of their group membership. After the control

condition, color labels were introduced and subjects were seated due to the structural patterns that are shown in Figure 2. This intervention is targeted to assess internalized social control effects in the presence of minimal contact. For the operationalization of internalized behavioral confirmation, the expectations of subjects was measured by asking them to forecast the decision of their left and right neighbors before every decision round.

After subjects made decisions in this condition, external incentives were introduced (cf. Table 1). This intervention aimed at mapping neighborhood effects and providing a meaningful comparison for the relative size of the effect of internalized social incentives. As the importance of internalized social incentives varies across individuals the analysis intends to demonstrate the relative importance of internalized social control on average and the extent of variation between subjects. The natural order of experimental parts shown in Table 1 cannot be altered, because once identities are assigned to subjects there is no logical way back to a no-identity treatment. The design is therefore not perfectly counterbalanced, and results have to be interpreted with the reservation that control for ordering effects was not possible.

FIGURE 2 SOMEWHERE HERE

TABLE 1 SOMEWHERE HERE

With regard to monetary side-payments of behavioral confirmation and selective incentives two conditions were implemented between experimental sessions. Next to the payoffs that were present in the beginning of the experiments (see Table 2), in the confirmation pressure condition external behavioral confirmation incentives (b=5 NLG), in the normative pressure condition external selective incentives (s=5 NLG) were introduced in Part III of the experiment (cf. Table 1). In Part IV, in both conditions the other type of incentives was also introduced. Subjects received 5 NLG behavioral confirmation reward if one of their fellow neighbors chose the same action as they did and received 10 NLG if two of their fellow neighbors acted on the same way. Selective incentives were distributed regardless of the decision of neighbors. Contributing subjects received 5 NLG for each fellow neighbor they had. In the low clustering condition there

was no change due to the absence of fellow neighbors. To summarize, the experiment has followed a 2×3 block-design that is represented in Table 2.

Experiments were combined with repeated IPG games. Repeated games followed single-shot games in all four experimental parts. Experiments were designed so as to exclude possible influences of previous decisions. Subjects were explicitly told before every part that previous parts and repeated games are completely independent from the next part. New parts always started after a short break and with introductory instructions that attempted to create the impression as if nothing has happened before in the experiment. This manipulation, however, cannot perfectly exclude the possibility of history effects that will be discussed later among control variables.

TABLE 2 SOMEWHERE HERE

The model for explaining contribution propensities

Main effects: social control

This section describes the model that is used to test the hypotheses in the IPG game experiments. Besides the main effects of social control that are believed to be the underlying mechanisms of the segregation effect on intergroup conflict, the influence of personal characteristics are discussed that are handled as control variables.

For the analysis of experimental data multilevel logistic regression is used (Bryk and Raudenbush, 1992; Goldstein, 1995). There are two levels in this case. Single decisions are the lower level observations and characteristics of the subjects, who took these decisions, are the group level observations. The two-level model corrects for the methodological problem that observations within the subjects are not independent. For the binary dependent variable of individual contribution, the logit transformation is used. Formally, let the function P_{ri} denote the propensity of actor i to cooperate in the rth single-shot game. The propensity of cooperation is specified by the logit link function (Goldstein 1995: Chapter 7), which is the natural logarithm of the quotient of the probability of contribution $P_{ri}(C)$ and the probability of defection $P_{ri}(D)$:

$$P_{ri}^{I} = \ln\left(\frac{P_{ri}(C)}{P_{ri}(D)}\right) = \alpha_0 + \varepsilon_i + \xi_{ri}, \quad (1)$$

where α_0 is the baseline contribution propensity. Previous experiments found that α_0 depends primarily on the payoff parameters, on individual efficacy (which is a function of group size and the threshold of collective action) and on experimental conditions of confidentiality and anonymity (whether subjects know each other, whether they are in the same room, etc.). Notation ε_i stands for a subject level error term and ξ_{ri} is intra-individual variation. The latter term represents the residual variance that is not estimated in models that include the random intercept α_0. It is assumed that the subject level error has a zero expected value and has a normal distribution, that is

$$\varepsilon_i \sim N(0, \sigma^2),$$

where the variance σ^2 is going to be estimated. This baseline model does not contain any explanatory variables and allows to model behavior in the anonymous control condition (Part I).

Intra-individual variation results from experimental manipulations. These main factors are relevant after the introduction of minimal contact in Part II. Additional reasons for intra-individual variation that can already be present in the control condition are stochastic individual decisions, consideration of mixed strategies, or simply inconsistency. In the simplest model, it is assumed that intra-individual variation is not correlated with round number *r* and has a zero expected value. However, this assumption will be relaxed and a trend element will be added, if there are indications of learning the structure of the game through the experiment.

With the introduction of minimal contact (Part II), internalized social incentives expected to affect individual decisions. Positive selective incentives (*s*) are received from each fellow neighbor in case of a contributing decision. Behavioral confirmation (*b*) is a reward for adjusting the actual decision to the expected decision of a fellow neighbor. Traitor rewards (*t*) provide an incentive against contribution in presence of a neighbor from the other group. When internalized, these forms of social control create

non-monetary incentives for the subjects, which can be expressed as part of their utility functions. Since both monetary and these non-monetary incentives enter the game, the "real" payoff matrix is not equivalent to the "monetary" payoff matrix. In the "real" game, contribution could even be a dominant strategy. Formally, in the experimental game contribution is a dominant strategy of individual *i*, if assuming a linear utility function on rewards and linear effects of neighborhood size,

$$f_i(s_i - b_i) > g_i t_i + 11 \tag{2}$$

holds, f_i denotes the number of fellow and g_i the number of opposite neighbors of *i*. The relative weight of the utility of monetary rewards and of the utilities attached to different forms of non-monetary incentives can change from person to person.[2] Therefore, no specific form of utility function is assumed that could be applied to everyone. In the analysis, only mean sizes of social incentives and their variances will be estimated.

In Part II, the propensity of cooperation will also be dependent on internalized selective incentives, behavioral confirmation, and traitor rewards, as expressed by equation (3):

$$P_{ri}^{II} = \alpha + s_0 f_i + b_0 \left(\hat{f}_{cti} - \hat{f}_{dti} \right) - \left(t^+ + t^- \right) g_i + \varepsilon_i + \xi_{ri}. \tag{3}$$

Parameter s_0 denotes internalized selective incentives (prestige, respect, etc.) and b_0 stands for internalized behavioral confirmation rewards. These parameters are estimated from the experimental results. The expression after b_0 within the brackets denotes the difference between the expected number of contributing and defecting fellow neighbors in decision round *r*. If the latter is higher, behavioral confirmation decreases the likelihood of contribution.

The number of neighbors from the opposite group affects contribution rates through traitor rewards. These are positive (t^+) and negative (t^-) selective incentives rewarding

[2] For this reason, the subscript *i* is used for non-monetary incentives in equation (1). Later on, this subscript will be omitted.

defection and punishing contribution.[3] For the sake of simplicity, their sum is denoted by t_0. In the simplest model, only the average individual importance of internalized social control is estimated, therefore the subscript i is omitted for estimates s_0, b_0, and t_0. However, some presented models will allow for a random variance in the size of these effects. These models will assume that the effects of internalized social control for the subjects are normally distributed around their means. This is consistent with the statement that individuals do not assign the same relative utility for social control, but the utilities are scattered normally around a certain mean evaluation. In this part of the analysis, the variances of the influences of different forms of internalized social control will be estimated, as well as their covariances.

Finally, when explicit monetary payoffs of social control (s and b) are introduced (Part III and IV), equation (3) has to be extended with additional terms. The simple model in its general form can be written as

$$P_{ri} = \alpha_0 + \left(s_0 f_i + b_0 \left(\hat{f}_{cri} - \hat{f}_{dri}\right) - t_0 g_i\right) p^{II} + s_1 f_i p^s + b_1 \left(\hat{f}_{cri} - \hat{f}_{dri}\right) p^b + \varepsilon_i + \xi_{ri}. \quad (4)$$

The p^j dummies denote experimental parts: p^{II} indicates whether or not minimal contact is present, p^s denotes whether or not external selective incentives are introduced, and p^b shows the presence of external confirmation rewards. Parameters s_1 and b_1 in equation (4) need to be estimated and therefore they are distinguished from the monetary values s and b. The utility of these monetary rewards might differ subject by subject, therefore, part of the multilevel analysis will allow for a random variation in their sizes over the subjects.

Control variables and interaction effects

Previous experiments revealed several important factors that influence cooperation rates in social dilemmas (e.g., Ledyard, 1995). The inter-individual variation of

[3] Takács (2001) considered only positive traitor incentives. The consideration of both positive and negative traitor rewards does not add complications to the model.

contribution propensities in intergroup related collective action might also depend on personal characteristics, like gender, college major, experience in similar experiments, attitudes towards risk, or social orientations. These factors will be included in the analysis as control variables; therefore no hypotheses are explicated about their effects. They are included as controls because they enrich research with interesting insight and comparisons can be made with previous findings.

For instance, there are contradictory findings in previous social dilemma experiments about whether women or men are more cooperative (e.g., Isaac, McCue, and Plott, 1985; Stockard, van de Kragt, and Dodge, 1988; Mason, Phillips, and Redington, 1991; Frank, Gilovich, and Regan, 1993; Brown-Kruse and Hummels, 1993; Nowell and Tinkler, 1994; Cadsby and Maynes, 1998; Eckel and Grossman, 1998; Ortmann and Tichy, 1999). Most subjects participating in experiments are students at different faculties of the university. Direction of study might cause individual differences in willingness of contribution. Previous research found that economists have lower contribution rates (Marwell and Ames, 1981; Carter and Irons, 1991; Frank, Gilovich, and Regan, 1993), although there are also experiments that do not find this effect (Isaac, McCue, and Plott, 1985; for an overview, see Ledyard, 1995: 161, 179).

Besides these background variables, relevant factors include attitude measures that indicate special forms of individual utility functions. Previous findings show that attitudes towards risk correlate with contribution propensities (Suleiman and Or-Chen, 1999). Since the contribution decision involves the possibility of a higher reward, but also involves the risk of losing the bonus completely, subjects with a risk-seeking attitude might have higher contribution rates (Budescu, Rapoport, and Suleiman, 1990). On the other hand, there are arguments that in repeated social dilemmas risk aversion increases cooperation (Raub and Snijders, 1997; van Assen and Snijders, 2002). In the experiments of this study, attitudes towards risk were included only as control variables. For the measurement of risk preferences, questions with preference comparisons (see Farquhar, 1984) were used.

Utility functions can also include altruistic elements, which certainly influences rational decision-making in social dilemma experiments (e.g., Liebrand, 1984; Doi, 1994). Subjects, who order positive utilities for the gains of others, behave differently from individualistic ones. For the approximation of such utilities, standard questions

regarding social orientations were used. They consisted of a series of decomposed games with an unknown person.[4] The measurement presumed that individuals are only *prosocial* (cooperative), *individualistic*, or *competitive*. Previous research found only these types relevant in describing human behavior (van Lange et al., 1997; van Lange, 1999; Suleiman and Or-Chen, 1999). Among each type an egalitarian tendency was distinguished (cf. van Lange, 1999). Although in a two-person PD game or in a public good experiment higher contribution rates are expected from prosocial subjects, it is not so evident in the IPG game. One could argue that subjects who order utility weights for rewards of unknown others, would do this equally for everyone, including out-group members. Consequently, their contribution rates would not be different from individualistic subjects. A counter-argument is that prosocial (and also egalitarian) orientation is associated with high utility for social identity, which is obtainable in a relational comparison with the out-group. Hence prosocial orientation is primarily directed towards in-group members. Results will show whether prosocial individuals are more concerned about harmful outcomes and thus abstain from contribution or whether they have higher contribution propensities and are even the initiators of harmful intergroup conflict.

Some of the participants knew each other. As acquaintances might influence actual decisions in the experiment, we included the number of acquaintances in the experiment as a control variable. Not only the number, but also the exact seating location of acquaintances can be a relevant factor. However, inclusion of such variables would add substantial complexity to the explanatory model without much theoretically grounded gains.

In part of the analysis, interaction effects of background variables and social control are also included, because the relative size of internalized social control in the utility function might depend on certain personal characteristics. There are contradictory findings in previous experiments about whether people are more likely to think of others of the same sex to be contributors and in general, whether men or women are more likely to be thought of as better contributors (Ortmann and Tichy, 1999; Solnick and Schweitzer, 1999). For explorative reasons, interactions between gender and social

[4] The exact questions can be found in Takács (2002).

control and interactions between social orientations and social control are also included as control variables.

Since experiments were designed to separate motives in single-shot situations from incentives that are present in repeated play, no history effects are expected on single-shot decisions, but as a test of this hypothesis, previous outcomes of iterated games were included as control variables in part of the analysis.[5]

Method

Subjects

203 subjects took part in the experiments at the University of Groningen. Subjects were recruited via e-mail and board advertisements promising monetary rewards for participation. All 203 subjects completed the decision tasks and only two have failed to complete the post-decision questionnaire. Altogether, 21 sessions took place and subjects made 4060 single-shot game decisions (20 each). The intended number of participants was ten in all the 21 experimental sessions. On average, thirteen subjects were invited to the sessions as it was anticipated that some would not come. Four sessions failed to be completely filled. In these cases, computer players were included. Subjects were told that they are programmed in a way to resemble human behavior. In fact, they were simple programs playing mixed strategies with condition-dependent probabilities of contribution. Human decisions in the incomplete experiments are also included in the analysis, but computer decisions are excluded. The inclusion of simulated participants did not have a significant influence on the behavior of subjects in the IPG games.

114 (56.2%) subjects were female. 187 (92.1%) subjects were university students at the time of the experiments and 16 had already graduated. Students came from all faculties of the university: 55 studied behavioral or social sciences, 47 subscribed for literary studies or art, 26 studied natural sciences, 17 studied law, 13 studied economics, 10 were students at the business faculty, there were 8 students of medical science, 8 subjects studied spatial sciences, and one subject read philosophy. Because of

[5] In the repeated games, subjects were informed about the result of the previous round.

similarities and for the sake of simplicity, economic, business, and spatial sciences were merged in the analysis (furthermore, these faculties have the same physical location) and the student of philosophy was allocated to the category of literary studies and art. The college major of two subjects was unknown.

Single-shot games (only the decision rounds) took approximately three minutes in each experimental part. During this time subjects had to make five decisions. The entire experiment was on average 80 minutes long.

The payoff for subjects was contingent on their decisions, as well as on the decisions of other participants of the session. Individual payoffs were calculated on the basis of outcomes in the single-shot and in the repeated games. From the single-shot games, only one was selected randomly in each experimental part to be included in the calculation. This payoff had a weight of five rounds (the number of single-shot games in one experimental part). Payoffs varied between 14 and 32 Dutch guilders with an average of 21.1 NLG. Reserve subjects received 10 NLG for their appearance. If subjects ran out of decision time, a random decision was implemented with 50% chance of contribution. For all such cases, final payment was decreased by 1%. This happened only 26 times out of 4060 decisions (0.64%). Random decisions are not included in the analysis.

Procedure

Experiments were conducted in the same computer laboratory.[6] Upon arrival, subjects were randomly seated at a computer. Removable walls separated the subjects to ensure their privacy. Subjects received instructions on paper and on their screen.[7] After reading the instructions they were allowed to ask the experimenter questions. After the questions had been answered, subjects were not allowed to talk. All participants strictly adhered to the rules. After the questions, an examination of understanding followed.

Subjects understood the task quite well, on average they answered 16.5 questions in five minutes, from which 13.7 (83%) were right. The mean proportion of correct answers was 80% with a standard deviation of 18%. Only nine subjects gave more wrong than

[6] The computer program for the experiment was written by Sicco Strampel in Delphi 5.

[7] Instructions are avialable in Takács (2002: 101-104).

right answers, one of the nine gave answers that could be considered random. One subject did not answer any of the quiz questions.

Every experiment consisted of four parts. In each part, subjects played five rounds of single-shot IPG games, followed by a number of repeated games. Experiments began with an anonymous start (Part I). Group membership was not yet announced. In every decision round, subjects had to decide whether they would keep the 11 NLG bonus or give it to help their group to achieve success in the competition. These two options appeared in a randomized order on their screen. The bonus was represented also graphically as a bag of money. Subjects were assured of the anonymity of their decisions and that they would receive any money they earned during the experiment in sealed envelopes, after the experiments had ended. In the single-shot games, it was announced that every decision counts towards the final payment, but that only one game of each part would be chosen randomly for payment.

In the beginning of Part II, separator walls were removed and group membership was made public by the experimenter. Red and green flags were attached to the monitors and subjects also received an A-4 colored paper with the color of their group. In each condition, subjects were seated behind computers due to the neighborhood configuration of the given session. Participants could clearly see the indication signs of group membership of their neighbors, and with some effort they could also check membership of more distant subjects. Subjects played five rounds of the same IPG game again. Before every decision in Part II, III, and IV, subjects had to give their expectations about the subsequent decision of their neighbors. The five single-shot games were followed by repeated games.

In Part III, monetary payoffs for social control were introduced explicitly. In ten sessions (see Table 2) monetary confirmation rewards and in eleven sessions monetary selective incentives were incorporated. In Part IV, the other type of social control was also introduced in an explicit monetary form (see Table 1). As in the low clustering condition (six sessions) there were no fellow neighbors, this condition was used as a control condition (there was no change between Parts II, III, and IV).

Calculation and announcement of the individual results followed the experiment. Meanwhile subjects were asked to fill in a questionnaire on their computer. Monetary payments were supplied in sealed envelopes. The first subject, who had completed the

questionnaire, could go immediately to the experimenter to receive payment. Other subjects had to wait until they got a signal from the server. Hence, subjects left the laboratory individually, with a short time difference between their departure. They were informed about the aim of the study after the experiments.

Results

Contribution rates and conflict under different experimental conditions

As the consequence of dyadic social control, different outcomes were expected by clustering conditions. The segregation hypothesis predicted that conflict is least likely in the low clustering condition and is most likely in the high clustering condition. Table 3 summarizes the experimental outcomes by clustering conditions. The hypothesis that the outcomes of the IPG game are independent of clustering conditions can be rejected ($\chi^2(3)=46.370$, $p<0.001$).

TABLE 3 SOMEWHERE HERE

Table 3 shows that conflict was already quite likely the outcome in the control condition. It indicates that many subjects have contributed even when they were isolated, which cannot be explained by social control effects. Conflict was much less likely in the low clustering condition, and occurred most often in the high clustering condition, which supports the segregation hypothesis. On the other hand, conflict was almost as likely in the medium clustering condition as in high clustering. Conflict occurred in 85.83% of the cases in the medium and 88.57% of the cases in the high clustering condition (from unweighted outcomes; $t=0.613$, two-tailed $p=0.541$), which is counter to the segregation hypothesis.

Contribution rates by clustering conditions are summarized in Table 4. The differences between clustering conditions are the result of internalized *and* external social control. In order to test whether internalized social control can alone cause such differences between clustering conditions, results from Parts I and II are compared. The

comparison reveals that eye contact made an increase in contribution rates. The difference is significant at the 5% level, but not at the 1% level (t=1.722, one-tailed p=0.043). However, in Part II, the contribution rate was highest in the medium clustering condition, which contradicts the segregation hypothesis.

TABLE 4 SOMEWHERE HERE

Table 4 also shows average contribution rates in Parts III and IV of the experiment. The hypothesis that contribution rates are the same in the different conditions can be rejected both in Part III (ANOVA $F(2, 1010)=30.800$, $p<0.001$) and in Part IV (ANOVA $F(2, 1011)=108.721$, $p<0.001$). It was predicted that the introduction of monetary selective incentives would result in higher contribution rates than when behavioral confirmation is introduced in Part III. *Results confirm this hypothesis* (t=4.487, one-tailed $p<0.001$). Furthermore, earlier introduction of normative pressure made a difference also in Part IV (t=3.285, two-tailed p=0.001). This result indicates that history effects still play a role in determining individual decision, despite the lack of feedback regarding the results of single-shot games. Furthermore, figures in Table 4 also *support the hypothesis that under normative pressure the effect of segregation is stronger than under confirmation pressure*. In Part III, under normative pressure average contribution rates are higher in the high clustering condition (75.66%) than in medium clustering (63.82%). On the other hand, under confirmation pressure average contribution rates are higher in the medium clustering condition (58.42% vs. 47.33%).

Questionnaire data

Questionnaire data can only provide partial support for the results. Most subjects reported that they felt important differences between the experimental parts, but it is impossible to establish the mechanisms behind these differences from the answers. The analysis of actual behavior more convincingly shows the important factors that affect individual decisions.

Table 5 reports some descriptive statistics of relevant questions. The question on the importance of minimal contact (first column) was formulated as: "In the beginning of the experiment walls were separating you and your neighbor(s). Later the walls were removed and flags were attached to the monitors to indicate the group membership of participants. When you had to make your decisions, how important was this *difference* for you?"

Other questions reported in Table 5 were formulated similarly, but they were related to the difference between Part II and Part III and to the difference between Part III and Part IV, respectively. Descriptive statistics are displayed separately for sessions, in which external selective incentives were introduced first (second and fourth columns) and for those, in which external behavioral confirmation was introduced first (third and fifth columns). Only answers of those subjects who had a fellow neighbor are reported as only they received these new incentives. The hypothesis that subjects felt one form of social control more important at the first introduction can be rejected (Wilcoxon rank test W=3721.5, two-tailed p=0.135). Furthermore, the difference between these sessions is not significant with regard to changes made before Part IV (Wilcoxon W=3918.5, p=0.598). This is a somewhat surprising result, since contribution rates differed significantly between these sessions both in Part III and in Part IV (cf. Table 4), which implies that subjects' opinions do not match their behavior.

TABLE 5 SOMEWHERE HERE

Analysis of contribution propensities: a simple model

To understand the underlying mechanisms of the segregation effect on intergroup conflict, individual decisions have to be analyzed. The first model in Table 6 reports results for the two-level model expressed in equation (4). The second model assumes that estimates of social control over subjects are normally distributed around their mean. In this model the variances and covariances are estimated as random effects. All human decisions except 23 cases (0.006%) are included. In these 23 cases subjects did not present any expectations about the behavior of their neighbors. In total, 4011 decisions are included in the analysis for 203 subjects.

TABLE 6 SOMEWHERE HERE

The two models provide similar estimates. All effects are in the predicted direction. Hypotheses about the existence of internalized behavioral confirmation and internalized traitor rewards are supported. The effect of the third type of internalized social control (selective incentives) is not significant. As predicted, both forms of external social control have a significant effect.

Internalized behavioral confirmation has a very strong effect, approximately as strong as 5 NLG monetary incentives for behavioral confirmation. The parameter estimate of internalized behavioral confirmation 0.617 means that an additional fellow neighbor, who is expected to contribute, increases the odds by 85.34%. For instance, in the first model, for an average subject, the predicted likelihood of contribution is 49.05% in the control condition. In the minimal contact condition, the expected likelihood of her contribution, if she were to have one contributing fellow neighbor and no opposite neighbor is 66.55%. For this increase, internalized behavioral confirmation takes the most responsibility.

For testing hypotheses about random effects it is more appropriate to use deviance tests than the *t*-test (cf. van Duijn, van Busschbach, and Snijders, 1999: 192-193). Contribution rates between subjects have a high unexplained variance. The influence of behavioral confirmation and monetary selective incentives varies significantly between subjects. The hypothesis that the sizes of traitor rewards and internalized selective incentives are the same for the subjects cannot be rejected. High positive deviations from the average baseline contribution rate are correlated with negative deviations from the average importance of monetary rewards for confirmation. This is not surprising because subjects, who evaluate monetary gains less, contribute more to the success of their group.

The effect of personal characteristics and other control variables

To see which personal characteristics are responsible for high inter-individual variation, in the next part of the analysis the model is extended by background variables

and certain attitude measures. Furthermore, in the previous analysis it was assumed that intra-individual variation (ξ_{ir}) has a zero expected value and it is independent from the decision round *r*. This assumption was based on the randomized design of single-shot games (cf. Bornstein and Ben-Yossef, 1994). Every decision round could be handled in an equivalent way, since only one of the rounds would be randomly chosen for payment and subjects did not receive information about the outcomes of previous rounds. If contribution propensities are not stable in the single-shot games within experimental parts, then an independent trend element has to be included in the analysis and the assumption that intra-individual variation (ξ_{ir}) has a zero expected value has to be relaxed. As parts were separated by breaks, instead of checking for a single learning trend, it is better to distinguish between a within part and a between part learning trend in the analysis.

Again, two analyses are conducted: one assuming fixed social control effects without random variation and another assuming a random variation and covariation of these estimates (see Table 7). As the analysis controls for some disturbing procedural effects, results show the net effect of main variables.

There are remarkable changes in the parameter estimates of social control. The effect of internalized selective incentives became significant and the significant effect of internalized traitor rewards has disappeared. The large increase in the estimate of baseline contribution propensity (constant) also indicates that the omission of independent trends resulted in a systematic bias in previous estimates in Table 6. Because of the negative between parts tendency, the baseline contribution rate was underestimated and the decrease between Part I and Part II was attributed to the effect of internalized traitor rewards. On the basis of the analysis reported in Table 7, after controlling for a negative learning tendency, it turns out that on average, traitor incentives in an internalized form do not influence the decision of subjects. On the other hand, this interpretation and also the confirmation of the existence of internalized selective incentives has to be handled with reservations. The inclusion of a between parts trend in a linear functional form in the analysis does not stand on a firm theoretical basis. Furthermore, since the high correlation with experimental manipulations (the introduction of minimal contact and monetary forms of social control), the learning effect might include part of influence that should be attributed to other variables.

There is another complication in relation to the difference in contribution propensities between Parts I and II. Silent identification (Bohnet and Frey, 1999) enters social dilemma experiments, when subjects are able to see each other. The visibility of others decreases social distance, allows for empathy and helps to conceptualize the experimental situation. Similarly to prosocial attitudes, this might be expected to correlate with utility weights for social identity, consequently with higher contribution propensities. However, this effect cannot be separated from the influence of internalized social incentives that are not contingent on predictions (selective incentives and traitor rewards). If silent identification is a valid mechanism in the IPG game, the analysis overestimates the effect of internalized selective incentives. The unexpected positive sign of the t_0 estimate can also partly be explained by silent identification.

Among personal background variables, gender has no significant effect, although simple descriptive statistics showed that women had higher contribution rates (55.94%) than men (52.14%). Based also on descriptive statistics, subjects who already graduated were more contributive (61.54%) than students (53.58%). This effect is not significant in the model, as it is ruled out by other variables, mainly by social orientations. The analysis of college major does not reveal an effect of economics training. In the experiments, students of natural sciences and law had the lowest contribution rates (48.17% and 48.66%, respectively). They were followed by students of economics (51.54%) and medical sciences (53.55%; this was used as a reference category in models of Table 7). Subjects who studied literary studies or art and students of social sciences had the highest contribution rates (56.91% and 55.85%, respectively). However, the effect of college major is also ruled out by other variables. The argument that experience matters at all is questioned by the insignificant effect of participating in a similar experiment before. Again, the difference in descriptive statistics (56.14% vs. 51.44%) could be explained by selection on attitude measures.

Subjects were characterized as strongly risk-averse, if they chose for risk-averse alternatives both in simple and complex gambles. 91 subjects (45.3%) were strongly risk-averse towards gains, 92 (45.8%) were strongly risk-averse towards mixed gambles, and 83 (39.5%) were strongly risk-seeking towards losses. Although no attempt was made to estimate utilities more precisely, descriptive statistics support that most

individuals have an S-shape utility function (e.g., Tversky and Kahneman, 1992). Effects of risk-aversion and loss-aversion are not significant in the models.

The only personal characteristics that are found significant in explaining contribution propensities are social orientations. For questions about social orientations, 77 (37.9%) subjects gave inconsistent answers. Inconsistency was a significant predictor of contribution rates, which is probably related to the relevance of calculation abilities. Among subjects, who gave consistent answers, 76 (61.3%) were prosocial, which is higher than in previous experiments (for an overview see Schulz and May, 1989). As an exception, Liebrand (1984) found a similar high rate in his experiments conducted in Groningen. Results clearly support the argument that prosocial (and also egalitarian) orientation is primarily directed towards in-group members and therefore increases contribution rates in the IPG game. The strong effects also indicate that social orientations are important predictors of behavior in intergroup situations. Individuals with prosocial and egalitarian attitudes seem to be responsible for the emergence of mutually harmful outcomes.

There was no significant effect of delay time at the start of the experiment and of how many others were acquainted to subjects in the laboratory. These factors that are related to the experimental environment did not disturb the behavior of subjects.

TABLE 7 SOMEWHERE HERE

Although Bayesian learning effects cannot enter the series of single-shot games, as experimental time passes, subjects might understand the structure of the game better and can become more experienced with the decision task. Previous experiments of iterated PD, public good, and IPG games found that subjects approach the all-defection equilibrium over time (Isaac, McCue, and Plott, 1985; Andreoni, 1988; Andreoni and Miller, 1993; Bornstein, Winter, and Goren, 1996; Goren and Bornstein, 2000; Goren, 2001), which results in decreasing cooperation rates.

FIGURE 3 SOMEWHERE HERE

In this study, a decay of contribution is found for the series of single-shot games (see Figure 3). Contribution rates decreased for those, who had some misunderstanding of the task before the game, but also for those, who answered quiz questions correctly. Besides the decreasing within part trend, in the last round of every part contribution rates increased significantly. This is a surprising result, since subjects knew that the outcome of the last round would neither be announced. This is exactly the opposite of what would be predicted on the basis of arguments of traditional game theory even if subjects had the incorrect perception that they are playing repeated games. This endgame effect was there independent of gender, social orientations, previous decisions, and other personal characteristics. By analyzing last rounds only, model parameters were similar to those values that were reported in Table 7, including the insignificant effect of internalized selective incentives. It means that higher contribution propensities in the last rounds cannot be explained by the reduction of cognitive dissonance ("in the last round I have to be nice, otherwise I cannot look at my fellow neighbors"). The resulting U-shape trend, however, has some correspondence to experimental findings in the iterated two-person PD and in collective action games (Rapoport and Chammah, 1965; Guttman, 1986).

Besides a within part trend, a between parts trend is also included in the models in Table 7 as a control variable. Both trends are highly significant, as well as the puzzling endgame effect. Trends and endgame effects are not the only unexpected procedure effects. After controlling for the results of repeated games, it emerged that a mutually harmful draw (clash punishment) "burns in" the memory of subjects and increases contribution propensities also in the single-shot games. Unfortunately, this points to a weakness of the present design. This also indicates that subjects use their long-term memory to estimate whether or not their decision could make a difference for the outcome in the forthcoming single-shot game. If they believe that a draw will occur, a single individual contribution can turn the outcome to victory.

Interaction effects

As Table 7 demonstrated, the significant effect of internalized traitor rewards disappeared after the inclusion of learning trends. It might be possible that this form of

social control is mistakenly conceptualized and traitor rewards have a different nature. They might stem from the presence of the other group as a whole or they exist only in certain dyadic relations.

The extension of the model by interaction effects helps with some clarification (see Table 8). It seems that internalized traitor rewards are activated in the dyadic context, but not in every neighborhood relation. Only neighbors of the opposite sex provide a significant control in the form of traitor rewards. This indicates that *internalized pressure against contribution* in the presence of opposite group members is *activated only, when a substantive distinction can be made apart from minimal group membership*. Gender is possibly the most apparent characteristic that can be the source of this distinction between strangers. With respect to the interaction between gender and internalized behavioral confirmation, no significant effect is found on contribution propensities. However, descriptive statistics showed that subjects expected contribution more from fellow neighbors of the same sex and additionally, women were expected to contribute more.

Acquainted neighbors did not experience stronger social control than unknown ones did. Similar to the insignificant effect of the number of acquainted subjects in the experiment, this result can probably be attributed to the fact that they were not close acquaintances or to subjects' tendency to view laboratory conditions as impersonal. Prosocial and egalitarian attitudes were not correlated with higher relative weight of internalized social control. Only the interaction between traitor rewards and prosocial orientation proved to be significant. This effect indicates that prosocial subjects like to be "local heroes", who contribute even when they are surrounded by members of the other group. This is another indication of how prosocial attitudes can be harmful in the intergroup context.

Additionally, an interaction variable was included to test whether or not subjects, who did not fully understand the experimental task, have different learning tendencies. The insignificant effect indicates that learning the structure of the game during the experiment is a general tendency and does not depend on the initial stage of understanding.

TABLE 8 SOMEWHERE HERE

Discussion

The focus of this study differed from the mainstream experimental tradition of social dilemmas and attempted to incorporate sociological insights into the explanation of individual contribution rates. The main objective was to show how internalized and external social control enter into simple experimental situations and can affect individual decisions in an intergroup competition situation that we modeled by an Intergroup Public Goods game (Rapoport and Bornstein, 1987). As an aggregated result of different forms of social control, it was demonstrated *why segregation might induce the emergence of conflict between groups*. To discover the underlying mechanisms, the study investigated what is the exact nature of *social control* and what are the forms that are already present in a condition with only eye contact between subjects. For the test of hypotheses, a *unique experimental design* was introduced based on special seating arrangements in the laboratory. With this setup, social control of the close environment, that is believed to be influential also in real life, was targeted in an experimental environment.

Comparison of clustering conditions showed that intergroup conflict was least likely in a completely mixed setting and was most likely when members of the groups were seated according to a segregated pattern, which confirms the segregation hypothesis. Furthermore, as predicted, *the segregation effect was stronger under normative pressure than in the confirmation pressure condition*.

By analyzing individual decisions, mechanisms of social control were uncovered that cause the segregation effect on the aggregated level. *Behavioral confirmation* is found to be the form of social control, which strongly affects individual contribution propensities, also in an internalized form. Subjects adjusted their decisions towards the expected decision of their fellow neighbors even when only eye contact has been established among them. Estimates of model parameters indicate that under the chosen reward structure, internalized confirmation pressure affected contribution propensities as much as monetary confirmation incentives did. Concerning behavioral confirmation, however, one should be aware of that part of the significant effect could be due to the bi-directional relationship between own behavior and expectations about the behavior of

others. Subjects could have formulated their expectations about choices of their neighbors in order to avoid cognitive dissonance, although this sounds less convincing than the opposite mechanism.

On the other hand, no strong support was found for the presence of other forms of internalized social control in the minimal contact condition. Internalized selective incentives had a significant effect after controlling for a between parts trend. However, the net effect of these incentives might diminish, if silent identification (Bohnet and Frey, 1999) is considered as a disturbing factor. Internalized traitor rewards might be activated in a dyad with minimal contact, but it is not a general mechanism. Its clear presence was found only between neighbors of the opposite sex. External social control that was introduced in a form of additional monetary incentives had a significant effect.

Contribution rates in the minimal contact condition were highest in the medium clustering condition, which is a somewhat puzzling result. A possible explanation is that there is a *ceiling effect*, which means that a presence of a single fellow neighbor activates sufficient internalized social control to enhance contribution to almost full certainty. This explanation is supported by evidence of high likelihood of conflict in the medium clustering condition (cf. Table 3). Another reason might be that the strength of internalized social control is a nonlinear function of the number of fellow neighbors. As a consequence, there is a marginal decrease in the segregation effect on the likelihood of intergroup conflict and already medium levels of clustering are often associated with harmful outcomes.

Among personal characteristics, strong effects of social orientations were found. Subjects with prosocial and egalitarian attitudes were more contributive and consequently were also more responsible for the emergence of mutually harmful outcomes between the groups than others. Another indication of that prosocial orientations are correlated with more generous behavior for the in-group, but more hostile behavior towards the out-group, is the positive interaction effect of traitor rewards and prosocial orientation. This implies that subjects with prosocial orientation behave more likely as local heroes. If members of the other group surround them, they do not surrender at all. As a macro consequence, mutually harmful outcomes can occur even in the case of complete mixing, if there are enough prosocial individuals.

To summarize, the present study demonstrated that laboratory experiments with minimal contact between subjects provide an important insight for understanding structural effects and the influence of internalized social control in intergroup situations. Results support policy arguments to promote interethnic relations and decrease segregation in order to help conflict resolution.

References

Allport, G. W., 1954. The Nature of Prejudice. Reading, MA: Addison-Wesley.

Andreoni, J., 1988. Why free ride? Strategies and learning in public goods experiments. Journal of Public Economics 37, 291-304.

Andreoni, J., Miller, J. H., 1993. Rational cooperation in the finitely repeated prisoner's dilemma: experimental evidence. Economic Journal 103, 570-585.

Asch, S., 1956. Studies of independence and conformity. Washington, DC: American Psychological Association.

Black, D. J., 1984. Social Control as a Dependent Variable. In: Black, D. J. (Ed.): Towards a General Theory of Social Control. vol.1. New York, Academic.

Bohnet, I., Frey, B. S., 1999. The sound of silence in prisoner's dilemma and dictator games. Journal of Economic Behavior and Organization 38, 43-57.

Bornstein, G., Ben-Yossef, M., 1994. Cooperation in intergroup and single-group social dilemmas. Journal of Experimental Social Psychology 30, 52-67.

Bornstein, G., Erev, I., Rosen, O., 1990. Intergroup competition as a structural solution to social dilemmas. Social Behavior 5, 247-260.

Bornstein, G., Winter, E., Goren, H., 1996. Experimental study of repeated team-games. European Journal of Political Economy 12, 629-639.

Bryk, A.S., Raudenbush, S. W., 1992. Hierarchical linear models: Applications and data analysis methods. London, Sage.

Budescu, D. V., Rapoport, Am., Suleiman, R., 1990. Resource dilemmas with environmental uncertainty and asymmetric players. European Journal of Social Psychology 20, 475-487.

Burt, R. S., 1998. The gender of social capital. Rationality and Society 10, 5-46.

Carter, J., Irons, M., 1991. Are economists different, and if so, why? Journal of Economic Perspectives 5, 171-177.

Chong, D., 1991. Collective action and the civil rights movement. Chicago, Chicago University Press.

Chwe, M. S-Y., 1999. Structure and strategy in collective action. American Journal of Sociology 105, 128-156.

Dawkins, R., 1976. The selfish gene. Oxford, Oxford University Press.

Doi, T., 1994. Social orientation analysis of the common and individual interest problem. In U. Schulz et al. (Eds.), Social dilemmas and cooperation. Berlin, Springer Verlag.

Drolet, A. L., Morris, M. W., 2000. Rapport in conflict resolution: accounting for how face-to-face contact fosters mutual cooperation in mixed-motive conflicts. Journal of Experimental Social Psychology 36(1), 26-50.

Eckel, C. C., Grossman, P. J., 1998. Are women less selfish than men? Evidence from dictator experiments. Economic Journal 108, 726-735.

Farquhar, P. H., 1984. Utility assessment methods. Management Science 30, 1283-1300.

Finkel, S. E., Opp, K-D., 1991. Party identification and participation in collective political action. Journal of Politics 53, 339-371.

Flache, A., Macy, M. W., 1996. The weakness of strong ties: Collective action failure in a highly cohesive group. Journal of Mathematical Sociology 21, 3-28.

Frank, R. H., Gilovich, T., Regan, D., 1993. Does studying economics inhibit cooperation? Journal of Economic Perspectives 7, 159-171.

Gächter, S., Fehr, E., 1999. Collective action as a social exchange. Journal of Economic Behavior and Organization 39, 341-369.

Gibbs, J. P., 1981. Norms, Deviance, and Social Control: Conceptual Matters. New York, Elsevier.

Goldstein, H., 1995. Multilevel statistical models. London, Edward Arnold Publishers.

Goren, H., 2001. The effect of out-group competition on individual behavior and out-group perception in the Intergroup Prisoner's Dilemma (IPD) game. Group Processes and Intergroup Relations 4(2), 160-182.

Goren, H., Bornstein, G., 2000. The effects of intragroup communication on intergroup cooperation in the repeated Intergroup Prisoner's Dilemma (IPD) game. Journal of Conflict Resolution 44, 700-719.

Gould, R. V., 1993. Collective action and network structure. American Sociological Review, 58, 182-196.

Gould, R. V., 1995. Insurgent identities: Class, community, and protest in Paris from 1848 to the Commune. Chicago, University of Chicago Press.

Guttman, J. M., 1986. Matching behavior and collective action: Some experimental evidence. Journal of Economic Behavior and Organization 7, 171-198.

Heckathorn, D. D., 1990. Collective sanctions and compliance norms: A formal theory of group-mediated social control. American Sociological Review 55, 366-384.

Heckathorn, D. D., 1993. Collective action and group heterogeneity: Voluntary provision versus selective incentives. American Sociological Review 58, 329-350.

Insko, C. A., Schopler, J., Graetz, K. A., Drigotas, S. M., Currey, D. P., Smith, S. L., Brazil, D., Bornstein, G., 1994. Interindividual-intergroup discontinuity in the prisoner's dilemma game. Journal of Conflict Resolution 38, 87-116.

Isaac, R. M., McCue, K. F., Plott, C., 1985. Public goods provision in an experimental environment. Journal of Public Economics 26, 51-74.

Kornhauser, R. R., 1978. Social Sources of Delinquency: An Appraisal of Analytic Models. Chicago, University of Chicago Press.

Kuran, T., 1995. Private truths, public lies: The social consequences of preference falsification. Cambridge, MA: Harvard University Press.

Ledyard, J. O., 1995. Public goods: A survey of experimental research. In J. H. Kagel and A. E. Roth (Eds.), The handbook of experimental economics. Princeton: Princeton University Press.

Liebrand, W. B. G., 1984. The effect of social motives, communication and group size on behavior in an N-person multi-stage mixed-motive game. European Journal of Social Psychology 14, 239-264.

Lindenberg, S., 1986. The paradox of privatization in consumption. In A. Diekmann and P. Mitter (Eds.), Paradoxical effects of social behavior: Essays in honor of Anatol Rapoport. Heidelberg: Physica.

Macy, M. W., 1993. Backward looking social control. American Sociological Review 58, 819-836.

Marwell, G., Ames, R., 1981. Economists free ride, does anyone else? Experiments on the provision of public goods, IV. Journal of Public Economics 15, 295-310.

Marwell, G., Oliver, P. E., 1993. Critical mass in collective action. Cambridge: Cambridge University Press.

Marwell, G., Oliver, P. E., Prahl, R., 1988. Social networks and collective action: A theory of the critical mass. III. American Journal of Sociology 94, 502-534.

McAdam, D., 1986. Recruitment to high-risk activism: The case of freedom summer. American Journal of Sociology 92, 64-90.

McCusker, C., Carnevale, P. J., 1995. Framing in Resource Dilemmas: Loss Aversion and the Moderating Effects of Sanctions. Organizational Behavior and Human Decision Processes 61(2), 190-201.

Nowell, C., Tinkler, S., 1994. The influence of gender on the provision of a public good. Journal of Economic Behavior and Organization 25, 25-36.

Oberschall, A. R., 1994. Rational choice in collective protests. Rationality and Society 6, 79-100.

Opp, K.-D., 1989. The rationality of political protest: A comparative analysis of rational choice theory. Boulder, CO: Westwiew.

Ortmann, A., Tichy, L. K., 1999. Gender differences in the laboratory: Evidence from prisoner's dilemma games. Journal of Economic Behavior and Organization 39, 327-339.

Pingle, M., 1995. Imitation versus rationality: An experimental perspective on decision making. Journal of Socio-Economics 24, 281-316.

Poundstone, W., 1992. Prisoner's Dilemma. New York, Doubleday.

Rapoport, Am., Bornstein, G., 1987. Intergroup competition for the provision of binary public goods. Psychological Review 94, 291-299.

Rapoport, Am., Bornstein, G., Erev, I., 1989. Intergroup competition for public goods: Effects of unequal resources and relative group size. Journal of Personality and Social Psychology 56, 748-756.

Rapoport, An., Chammah, A. M., 1965. Prisoner's dilemma: A study in conflict and cooperation. Ann Arbor, MI: University of Michigan Press.

Raub, W., Snijders, C., 1997. Gains, losses, and cooperation in social dilemmas and collective action: The effects of risk preferences. Journal of Mathematical Sociology 22, 263-302.

Rege, M., Telle, K., 2001. An experimental investigation of social norms. Discussion Papers No. 310, Oslo, Statistisk Sentralbyra.

Sandell, R., Stern, C., 1998. Group size and the logic of collective action: A network analysis of a Swedish temperance movement 1896-1937. Rationality and Society 10, 327-345.

Schelling, T. C., 1968. The life you save may be your own. In S. B. Chase (Ed.), Problems in public expenditure analysis. Washington: Brookings Institution.

Schopler, J., Insko, C. A., 1992. The discontinuity effect in interpersonal and intergroup relations: Generality and mediation. European Review of Social Psychology 3, 121-151.

Schulz, U., May, T., 1989. The recording of social orientations with ranking and pair comparison procedures. European Journal of Social Psychology 19, 41-59.

Scott, J. F., 1971. Internalization of norms. Englewood Cliffs, NJ: Prentice-Hall.

Selten, R., 1975. Re-examination of the perfectness concept for equilibrium points in extensive games. International Journal of Game Theory 4, 25-55.

Solnick, S. J., Schweitzer, M. E., 1999. The influence of physical attractiveness and gender on ultimatum game decisions. Organizational Behavior and Human Decision Processes 79, 199-215.

Stockard, J., van de Kragt, A. J., Dodge, P. J., 1988. Gender roles and behavior in social dilemmas: Are there sex differences in cooperation and in its justification? Social Psychology Quarterly 51, 154-163.

Suleiman, R., Or-Chen, K., 1999. Providing step-level public goods under uncertainty: The case of probable external supply. In M. Foddy, M. Smithson, S. Schneider, and M. Hogg (Eds.), Resolving social dilemmas: Dynamic, structural, and intergroup aspects. Philadelphia: Psychology Press / Taylor and Francis.

Takács, K., 2001. Structural embeddedness and intergroup conflict. Journal of Conflict Resolution 45, 743-769.

Takács, K., 2002. Social networks and intergroup conflict. Groningen, ICS Dissertation Series.

Tickle-Degnen, L., Rosenthal, R., 1990. The nature of rapport and its nonverbal correlates. Psychological Inquiry 1, 285-293.

Tversky, A., Kahneman, D., 1992. Advances in prospect theory: Cumulative representation of uncertainty. Journal of Risk and Uncertainty 5, 297-323.

van Assen, M. A. L. M. and Snijders, C. (2002). Effects of risk preferences in social dilemmas: A game-theoretical analysis and evidence from two experiments. In: Suleiman, R.; Budescu, D., Messick, D. M., and Fischer, I. (Eds.): Contemporary Approaches to Social Dilemma Research. Cambridge, MA, Cambridge University Press.

van Duijn, M. A. J., van Busschbach, J. T., Snijders, T. A. B., 1999. Multilevel analysis of personal networks as dependent variables. Social Networks 21, 187-209.

van Lange, P. A. M., 1999. The pursuit of joint outcomes and equality in outcomes: an integrative model of social value orientation. Journal of Personality and Social Psychology 77, 337-349.

van Lange, P. A. M., Otten, W., De Bruin, E. M. N., Joireman, J. A., 1997. Development of prosocial, individualistic, and competitive orientations: Theory and preliminary evidence. Journal of Personality and Social Psychology 73, 733-746.

van de Kragt, A. J. C., Dawes, R. M., Orbell, J. M., 1988. Are people who cooperate 'rational altruists'? Public Choice 56, 233-247.

Villareal, A., 2002. Political competition and violence in Mexico: Hierarchical social control in local patronage structures. American Sociological Review 67, 477-498.

Walters, A. E., Stuhlmacher, A. F., Meyer, L. L., 1998. Gender and negotiator competitiveness: A meta-analysis. Organizational Behavior and Human Decision Processes 76, 1-29.

Yamagishi, T., 1986. The provision of a sanctioning system as a public good. Journal of Personality and Social Psychology 51(1), 110-116.

Figures

Figure 1

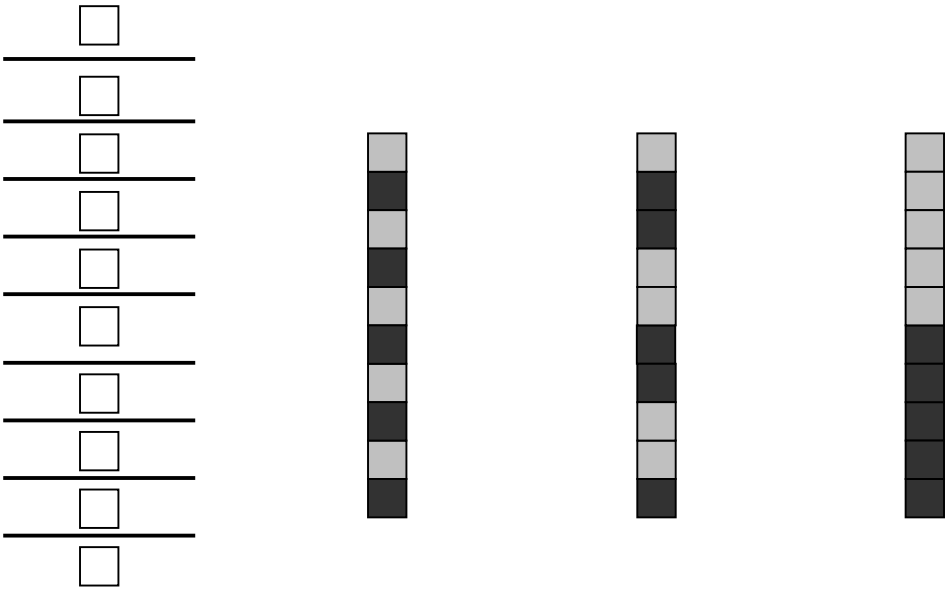
The IPG Game (with Clash Punishment) Used in the Experiments

payoffs 1 point = 1 NLG		*number of contributors in the green group*					
		0	1	2	3	4	5
number of contributors in the red group	0	0 0	0 0	0 0	15 -15	15 -15	15 -15
	1	0 0	0 0	0 0	15 -15	15 -15	15 -15
	2	0 0	0 0	0 0	15 -15	15 -15	15 -15
	3	-15 15	-15 15	-15 15	-11 -11	15 -15	15 -15
	4	-15 15	-15 15	-15 15	-15 15	-11 -11	15 -15
	5	-15 15	-15 15	-15 15	-15 15	-15 15	-11 -11

Note: The payoffs are public good rewards distributed to everyone in the red (bottom left corner of each cell) and in the green (top right corner) group.

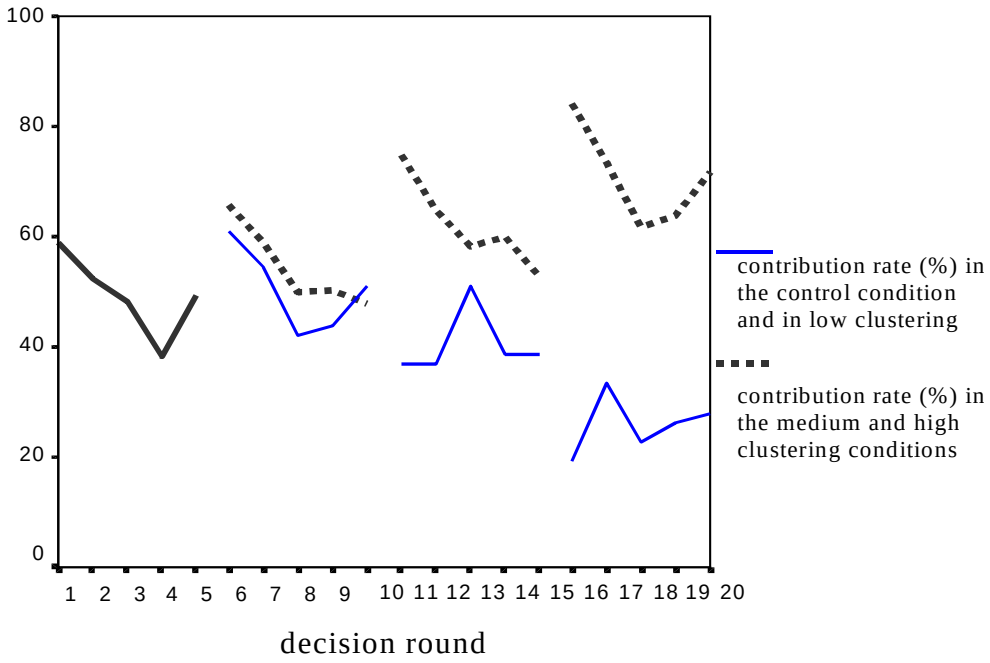
Figure 2

Structural Conditions in the Experiments: Control Condition, Low, Medium, and High Clustering



Notes: Light and dark colors indicate seats of members of the red and green group. In the control condition, no color labels were introduced.

Figure 3
Contribution Rates by Decision Rounds



Note. Part I: rounds 1-5; Part II: rounds 6-10; Part III: rounds 11-15; Part IV: rounds 16-20.

Tables

Table 1
Overview of Experimental Parts

Part I	*anonymous control condition*
Part II	*eye contact is established*
Part III	*one form (b/s) of social control is introduced in a monetary form*
Part IV	*the other form (s/b) of social control is introduced in a monetary form*

Table 2

The Number of Sessions by Experimental Conditions

level of clustering	low	medium	high
confirmation pressure (b first)	*3*	*4*	*4*
normative pressure (s first)	*3*	*4*	*4*

Table 3

Outcomes by Clustering Conditions in the Experiments

clustering condition in the experiment	*outcome of the decision round*		Total
	no competitive action	conflict (victory of one side or draw)	
control condition (unknown group membership)	26.97% (271)	73.03% (734)	100% (1005)
low clustering	50.23% (428)	49.77% (424)	100% (852)
medium clustering	13.75% (160)	86.25% (1004)	100% (1164)
high clustering	11.85% (120)	88.15% (893)	100% (1013)
Total N	24.27% (979)	75.73% (3055)	100% (4034)

Note. Cases in parentheses are weighted (multiplied) by the number of human decisions in the given game. For the χ^2 –test unweighted outcomes are used, N=420.

Table 4

Average Contribution Rates in Different Clustering Conditions and Parts of the Experiment

incentives introduced first	low clustering	medium clustering	high clustering	Total
*Part I**	49.64% (280)	51.81% (386)	46.61% (339)	49.45% (1005)
Part II	50.35% (282)	55.84% (385)	52.84% (335)	53.29% (1002)
Part III				
b (confirmation)	-	58.42% (190)	47.33% (150)	53.53% (340)
s (sel. incentives)	-	63.82% (199)	75.66% (189)	69.59% (388)
Part III total	40.35% (285)	61.18% (389)	63.13% (339)	55.97% (1013)
Part IV				
b (confirmation)	-	62.63% (190)	68.00% (150)	65.00% (340)
s (sel. incentives)	-	71.00% (200)	81.48% (189)	76.09% (389)
Part IV total	25.96% (285)	66.92% (390)	75.52% (339)	58.28% (1014)
Total (without Part I)	38.85% (852)	61.34% (1164)	63.87% (1013)	55.86% (3029)
Total	41.52% (1132)	58.97% (1550)	59.54% (1352)	54.26% (4034)

Notes. The number of cell-relevant cases is in parentheses. All human decisions are included.

* In Part I, subjects did not know their group membership and they did not see each other. Therefore their partition into the different clustering conditions only illustrates coincidental baseline contribution rates in the different experimental sessions.

Table 5

Frequencies of Answer Categories of Questionnaire Data

questions	*importance of the introduction of minimal contact*	*importance of the introduction of selective incentives in part III*	*importance of the introduction of behavioral confirmation in part III*	*importance of the introduction of behavioral confirmation in part IV*	*importance of the introduction of selective incentives in part IV*
very important (1)	28	13	15	22	22
important (2)	67	30	30	30	25
neutral (3)	42	18	12	12	9
not important (4)	40	8	2	5	3
not imp. at all (5)	24	1	2	1	2
total answers	201	70	61	70	61
mean	2.83	2.34	2.11	2.04	1.98
standard deviation	1.24	0.96	0.93	0.95	1.01
median	3	2	2	2	2
mode	2	2	2	2	2

Table 6

Results of Multilevel Logistic Regression on Contribution Propensities

independent variable	hypothesis about the direction of effect	multilevel model with fixed slopes of main effects	multilevel model assuming random slopes of social control effects
FIXED EFFECTS			
α baseline contribution propensity	?	-.038 (.082)	-.037 (.082)
s_0 internalized selective incentives	+	.109 (.072)	.117 (.072)
s external selective incentives	+	.407*** (.088)	.363*** (.104)
b_0 internalized behavioral confirmation	+	.617*** (.065)	.640*** (.077)
b external behavioral confirmation	+	.619*** (.104)	.615*** (.118)
t_0 internalized traitor rewards	-	-.175** (.055)	-.173** (.057)
RANDOM EFFECTS			
inter-individual variance σ^2		$.616^{+++}$ (.085)	$.628^{+++}$ (.121)
σ^2_{ui} (s_0)			.000 (.000)
σ^2_{ui} (s)			$.300^{++}$ (.139)
σ^2_{ui} (b_0)			$.196^{+++}$ (.093)
σ^2_{ui} (b)			$.326^{+++}$ (.226)
σ^2_{ui} (t_0)			.009 (.050)
Covariances are reported below			+
-2 Log Likelihood model		4480	4430
Improvement χ^2 (df in parentheses)		939*** (5)[#]	50*** (20)

Table 6b

Random Effects: Estimated Covariances

σ_{uxy}	ε_i	s_0	s	b_0	b
s_0	.000 (.000)				
s	-.252 (.108)	.000 (.000)			
b_0	.147 (.083)	.000 (.000)	-.194 (.085)		
b	$-.359^{++}$ (.131)	.000 (.000)	.128 (.132)	-.079 (.116)	
t_0	-.005 (.072)	.000 (.000)	.425 (.153)	-.169 (.109)	.176 (.165)

Notes. N=4011 decisions for 203 subjects. Iterative Generalized Least Squares estimates. Numbers in parentheses are standard errors. ** significant at the 1% level, *** significant at the 0.1% level (two-tailed). For testing random effects deviance tests are used: $^{++}$ significant at the 1% level, $^{+++}$ significant at the 0.1% level (significance of difference in deviance compared to model without random slopes, for random covariates deviance is compared to model without random covariates).

[#]Basis of comparison: baseline multilevel logistic regression expressed in equation (2); α: 0.174** (0.066); σ^2: 0.674^{+++} (0.087).

Table 7

Results of Multilevel Logistic Regression on Contribution Propensities with Personal Characteristics and Procedure Effects

independent variable	hypothesis about the direction of effect	multilevel model with fixed slopes of main effects	multilevel model with random slopes of main effects
FIXED EFFECTS			
Main variables			
α (constant) baseline contr. propensity	?	1.378** (.423)	1.516*** (.409)
s_0 internalized selective incentives	+	.186* (.082)	.188* (.081)
s external selective incentives	+	.769*** (.109)	.699*** (.127)
b_0 internalized behavioral confirmation	+	.586*** (.067)	.591*** (.080)
b external behavioral confirmation	+	.718*** (.108)	.705*** (.126)
t_0 internalized traitor rewards	-	.165 (.086)	.142 (.086)
Personal characteristics and other subject-level variables			
gender (1=male)	-	-.176 (.143)	-.196 (.137)
student at the university (1=yes)	-	-.219 (.370)	-.352 (.357)
studies at the law faculty	?	-.109 (.366)	-.015 (.351)
studies natural sciences	?	-.057 (.344)	-.065 (.330)
studies economic, business, or spatial sci.	-	-.030 (.335)	.095 (.322)
studies social sciences	?	.068 (.309)	.136 (.296)
student of literary studies or arts	?	.056 (.316)	.133 (.303)
did a similar experiment before	-	-.154 (.136)	-.188 (.131)
strong risk aversion towards gains	-	-.163 (.135)	-.180 (.129)
strong loss aversion	+	.115 (.134)	.132 (.128)
consistent answers on social orientation qs	-	-.374* (.181)	-.400* (.173)
prosocial orientation	+	.511** (.183)	.487** (.175)
egalitarian orientation	+	.388* (.176)	.392* (.169)
number of acquainted subjects in the exp.	+	-.079 (.088)	-.093 (.085)
delay (minutes) at the start of the exp.	+	.008 (.007)	.006 (.007)
quiz questions answered correctly %	-	-.005 (.004)	-.005 (.004)
Procedure effects			
within part trend	0	-.215*** (.036)	-.213*** (.036)
endgame effect	0	.373** (.125)	.370** (.126)
between parts trend	0	-.397*** (.060)	-.379*** (.061)
last iterated game was a draw	0	.538*** (.149)	.515*** (.152)
last iterated game was lost	0	.185 (.122)	.199 (.125)
last iterated game was won	0	.214 (.123)	.275* (.125)
RANDOM EFFECTS			
inter-individual variance σ^2		$.574^{+++}$ (.083)	$.559^{+++}$ (.116)
σ^2_{ui} (s_0)			.000 (.000)
σ^2_{ui} (*s*)			$.322^{+++}$ (.152)
σ^2_{ui} (b_0)			$.202^{+++}$ (.096)
σ^2_{ui} (*b*)			$.421^{+++}$ (.246)
σ^2_{ui} (t_0)			.002 (.050)
Covariances are reported below			+
-2 Log Likelihood model		4247	4198
Improvement χ^2 (df) for model in right column			49*** (20)
vs. previous model		183*** (6)	184*** (6)

Table 7b

Random Effects: Estimated Covariances

σ_{uxy}	ε_i	s_0	s	b_0	b
s_0	.000 (.000)				
s	-.163 (.109)	.000 (.000)			
b_0	.037 (.083)	.000 (.000)	-.192^{+} (.090)		
b	-.287^{+} (.133)	.000 (.000)	.063 (.143)	-.084 (.123)	
t_0	-.018 (.071)	.000 (.000)	.476 (.169)	-.054 (.117)	.152 (.180)

Notes. N=4011 decisions for 203 subjects. Iterative Generalized Least Squares estimates. Numbers in parentheses are standard errors. * significant at the 5% level, ** significant at the 1% level, *** significant at the 0.1% level (two-tailed).

For testing random effects deviance tests are used: $^{+}$ significant at the 5% level, $^{+++}$ significant at the 0.1% level (significance of difference in deviance compared to model without random slopes, for random covariates deviance is compared to model without random covariates).

Table 8 *Results of Multilevel Logistic Regression on Contribution Propensities with Personal Characteristics, Procedure Effects, and Cross-level Interactions*

independent variable	hypothesis about the direction of effect	multilevel model with fixed slopes	multilevel model with random slopes
FIXED EFFECTS			
Main variables			
α (constant) baseline contr. propensity	?	1.346*** (.402)	1.491** (.477)
s_0 internalized selective incentives	+	.176* (.082)	.165* (.084)
s external selective incentives	+	.769*** (.110)	.745*** (.135)
b_0 internalized behavioral confirmation	+	.589*** (.119)	.618*** (.141)
b external behavioral confirmation	+	.703*** (.109)	.681*** (.125)
t_0 internalized traitor rewards	-	.223 (.132)	.238 (.134)
Personal characteristics and other subject-level variables			
gender (1=male)	-	-.089 (.146)	-.135 (.143)
student at the university (1=yes)	-	-.177 (.372)	-.201 (.364)
studies at the law faculty	?	-.162 (.368)	-.136 (.360)
studies natural sciences	?	-.101 (.349)	-.161 (.341)
studies economic, business, or spatial sciences	-	-.080 (.339)	-.002 (.330)
studies social sciences	?	-.001 (.312)	.000 (.305)
student of literary studies or arts	?	.045 (.317)	.066 (.309)
did a similar experiment before	-	-.179 (.136)	-.221 (.133)
strong risk aversion towards gains	-	-.172 (.134)	-.157 (.132)
strong loss aversion	+	.131 (.133)	.164 (.131)
consistent answers on social orientation questions	-	-.397* (.180)	-.404* (.176)
prosocial orientation	+	.330 (.206)	.353 (.202)
egalitarian orientation	+	.419* (.203)	.394* (.200)
number of acquainted subjects in the experiment	+	-.066 (.089)	-.066 (.087)
delay (minutes) at the start of the experiment	+	.006 (.007)	.006 (.007)
quiz questions answered correctly %	-	-.004 (.005)	-.005 (.005)
Procedure effects			
within part trend	0	-.178 (.121)	-.188 (.122)
endgame effect	0	.379** (.126)	.381** (.127)
between parts trend	0	-.397*** (.061)	-.386*** (.062)
last iterated game was a draw	0	.527*** (.150)	.495** (.157)
last iterated game was lost	0	.180 (.123)	.186 (.128)
last iterated game was won	0	.214 (.124)	.266* (.128)
Cross-level interactions			
$t_0\times$ number of acquainted opposite neighbors	-	-.153 (.196)	-.164 (.194)
$b_0\times$ number of acquainted fellow neighbors	+	.302 (.261)	.338 (.312)
$t_0\times$ number of opposite neighbors of the other sex	?	-.351** (.134)	-.373** (.137)
$t_0\times$ number of male opposite neighbors	?	.191 (.134)	.156 (.136)
$b_0\times$ number of fellow neighbors of the same sex	?	-.038 (.084)	-.128 (.102)
$b_0\times$ number of female fellow neighbors	?	.302 (.261)	.017 (.108)
$t_0\times$ prosocial orientation	-	.275* (.131)	.256* (.132)
$b_0\times$ prosocial orientation	+	.052 (.134)	.098 (.161)
$t_0\times$ egalitarian orientation	-	-.057 (.149)	-.025 (.149)
$b_0\times$ egalitarian orientation	+	.039 (.143)	.004 (.172)
within part trend × quiz questions correct %	-	.000 (.001)	.000 (.001)
RANDOM EFFECTS			
inter-individual variance σ^2		$.563^{+++}$ (.082)	$.512^{+++}$ (.084)
σ^2_{ui} (s_0)			.000 (.000)
σ^2_{ui} (s)			$.549^{+++}$ (.187)
σ^2_{ui} (b_0)			$.143^{+++}$ (.089)
σ^2_{ui} (b)			$.379^{+++}$ (.240)
σ^2_{ui} (t_0)			.000 (.000)
Covariances are reported below			+
-2 Log Likelihood model		4211	4169
Improvement χ^2 (df) for model in right column			42** (20)
vs. previous model		36*** (11)	29** (11)

Table 8b

Random Effects: Estimated Covariances

σ_{uxy}	ε_i	s_0	s	b_0	b
s_0	.000 (.000)				
s	.037 (.107)	.000 (.000)			
b_0	.004 (.072)	.000 (.000)	-.145 (.093)		
b	-.200^{++} (.118)	.000 (.000)	.201 (.152)	-.031 (.116)	
t_0	.000 (.000)	.000 (.000)	.000 (.000)	.000 (.000)	.000 (.000)

Notes. N=4011 decisions for 203 subjects. Iterative Generalized Least Squares estimates. Numbers in parentheses are standard errors. * significant at the 5% level, ** significant at the 1% level, *** significant at the 0.1% level (two-tailed).

For testing random effects deviance tests are used: $^{++}$ significant at the 1% level, $^{+++}$ significant at the 0.1% level (significance of difference in deviance compared to model without random slopes, for random covariates deviance is compared to model without random covariates).