

University of Groningen

Effects of Network Segregation in Intergroup Conflict

Takács, Károly

Published in:
Connections

IMPORTANT NOTE: You are advised to consult the publisher's version (publisher's PDF) if you wish to cite from it. Please check the document version below.

Document Version
Publisher's PDF, also known as Version of record

Publication date:
2006

[Link to publication in University of Groningen/UMCG research database](#)

Citation for published version (APA):
Takács, K. (2006). Effects of Network Segregation in Intergroup Conflict: An Experimental Analysis. *Connections*, 27(2), 59-76.

Copyright

Other than for strictly personal use, it is not permitted to download or to forward/distribute the text or part of it without the consent of the author(s) and/or copyright holder(s), unless the work is under an open content license (like Creative Commons).

The publication may also be distributed here under the terms of Article 25fa of the Dutch Copyright Act, indicated by the "Taverne" license. More information can be found on the University of Groningen website: <https://www.rug.nl/library/open-access/self-archiving-pure/taverne-amendment>.

Take-down policy

If you believe that this document breaches copyright please contact us providing details, and we will remove access to the work immediately and investigate your claim.

Downloaded from the University of Groningen/UMCG research database (Pure): <http://www.rug.nl/research/portal>. For technical reasons the number of authors shown on this cover page is limited to 10 maximum.

Effects of Network Segregation in Intergroup Conflict: An Experimental Analysis¹

Károly Takács²
University of Groningen

Dense in-group and scarce out-group relations (network 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. Subjects were connected via computers and connection patterns were manipulated to detect forms of social control that are activated conditional on expectations and on the composition of the artificially created ego-network. Results confirm the influence of behavioral confirmation and the conditional impact of internalized selective incentives. As an aggregated consequence of these social control effects, harmful intergroup outcomes were least likely when members of the groups were arranged in a mixed network.

INTRODUCTION

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; Bornstein, Winter, and Goren, 1996). 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. The emphasis here will be on the role of incentives that stem from interpersonal relations and social networks. The importance of 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). Network effects are attributed to the fact that individuals are influenced by the presence, opinion, expectations, and behavior of friends, neighbors, colleagues, and relevant others, 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 a limited amount of studies 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, 2004). Structural considerations were disregarded by previous experiments on intergroup relations (an exception is Grö ß er and Schram, 2006). In general, the experimental literature that takes account of networks is limited but growing (for an overview, see Kosfeld, 2003). These avenues

¹ Address of correspondence: Interuniversity Center for Social Science Theory and Methodology / Department of Sociology, University of Groningen, Grote Rozenstraat 31, 9712 TG Groningen, the Netherlands. E-mail: k.takacs@rug.nl. The paper was revised while the author was a junior fellow at the Collegium Budapest / Institute for Advanced Study and a Magyar fellow at the Netherlands Institute for Advanced Study, Wassenaar.

² I am grateful to Roel Popping, Frans Stokman, Andreas Flache, and Wim Liebrand for their valuable and thorough advises, comments, and criticisms. I thank Anthony Oberschall, John Dovidio, Marcel Lubbers, Carljine 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 Smaniotta, Martin van der Gaag, Alinda van Bruggen, Jan Kratzer, and reviewers 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.

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; Takács, 2001) will be used that represents the dichotomy of interdependencies within the groups (provision of a public good) and between the groups (intergroup competition for a scarce resource). In this game, players are divided in two groups. Every player can decide to contribute or not to the provision of a public good. Contribution is costly. The number of contributors is compared between the groups. All members of the group with more contributors receive a public good reward v and all members of the other group receive a "public bad" d . In case the number of contributors are equal, all players receive a punishment reward c ($v > 0 > c > d$). 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 contributors, nothing happens, the status quo is preserved. It means that if both groups have less contributors than a minimal contributing set (MCS), no public good or bad is provided (cf. van de Kragt, Orbell, and Dawes, 1983). In this paper, an outcome will be called intergroup conflict, if one or both of the groups receive negative public rewards (c or d), or equivalently, the number of contributors at least in one group is above the threshold (MCS). Assuming no other incentives, the outcome of this game should not depend on the network connections group members might have between each other.

In order to capture relevant network effects, the IPG model has been extended by assuming dyadic mechanisms of social selective incentives and behavioral confirmation (Takács, 2001). These forms of social control have been shown to be possible underlying mechanisms why social networks might influence the likelihood of intergroup conflict. The extended model predicts that in particular, network segregation affects the likelihood of intergroup conflict and the relationship can be characterized by an S-shape function. This implies that segregation is likely to promote intergroup conflict, but in extreme ranges of segregation, an additional change does not result in an increase in the likelihood of conflict (Takács, 2001). These theoretical predictions directly lead to the main question and hypothesis of this study. In the context of a laboratory environment, is intergroup conflict indeed more likely when group members are arranged in a segregated network?

SOCIAL CONTROL AND NETWORK EFFECTS IN EXPERIMENTS

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

The following fundamental forms of social control will be considered as possible mechanisms. In-group social selective incentives, such as prestige, respect, and status either reward those who contributed to the group welfare (e.g., Lovaglia, Willer, and Troyer, 2003) or punish those who did not make contributions. Empirical studies show that social selective incentives are disseminated mainly locally, through interpersonal relations (Sandell and Stern, 1998) and are often internalized as contribution norms that create a cognitive reward for cooperation (Scott, 1971; Kornhauser, 1978; Coleman, 1990: 293). Individuals feel rewarded when they "did the right thing for the group" (Opp, 1989).

A similar form of social control is present in network relations with out-group members. Members of the competing groups, however, have contradictory interests in intergroup competition and therefore they reward each other's action that is against the in-group interest (e.g., Kuran, 1995, 9-10). These relations therefore transmit social selective incentives that punish contribution and reward defection. Out-group selective incentives are also likely to be internalized as a fear from dyadic conflict and benefit for local harmony. Their relevance can provide an explanation why contact can help to normalize intergroup relations (cf. Allport, 1954).

Another prominent form of social control is behavioral confirmation that expresses the desire to conform to the expected behavior of relevant individuals. It means that doing the same as relevant others has a positive value by itself and increases the utility of both sides independently from future interactions. In empirical collective action situations (e.g., strikes, demonstrations, and revolutions) participation in collective political action can be largely explained by willingness to conform to the behavioral expectations of relevant others (e.g., Finkel and Opp, 1991; Chong, 1991; Oberschall, 1994). There is indication for the relevance of such a mechanism also in public good experiments (Yamagishi, 1986; McCusker and Carnevale, 1995; Rege and Telle, 2004). Behavioral confirmation has a two-fold effect: confirmation by participating in-group alters 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 affect decisions as an internalized mechanism or imitation strategy (Asch, 1956; Dawkins, 1976; Pingle, 1995).

As an aggregated consequence 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 in-group members and limited realization of out-group selective incentives. Hence, network segregation supports contributions to harmful intergroup competitions and consequently to the emergence of harmful conflicts. The underlying mechanisms responsible for this are the fundamental forms of social control.

A major difference compared to field 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 and a “minimum network” have an additional effect that is due to the activation of internalized social control.

HYPOTHESES AND EXPERIMENTAL DESIGN

Minimal contact and social control

To test the presence of different forms of social control and the segregation effect on intergroup conflict in a controlled environment, a new experimental design is introduced. In the experiments, the seating arrangement of subjects and visibility conditions were manipulated in order to detect forms of social control that are activated conditional on the composition of the ego-network that is created experimentally. Minimal contact was introduced between connected subjects in the form that subjects were able to see to whom they are connected and they were able to identify the group membership of each other. Verbal and nonverbal communication was disallowed to avoid application of other forms of social control and signaling. It was tested whether this minimal contact is sufficient to activate internalized forms of social control.

In later parts of the experiments, additional to minimal contact, monetary side-payments were introduced as representations of external behavioral confirmation and in-group selective incentives. These effects are expected to be stron-

ger than internalized effects. With their introduction a meaningful comparison can be made between the size of monetary and internalized social control. With regard to forms of social control, the following hypotheses are explicated.

Selective incentives: In-group selective incentives have a positive effect on contribution propensities. More connections to members of the in-group mean the distribution of selective incentives from multiple sources. Hence, the higher the number of in-group members in the ego-network, the higher the contribution rate is.

The presence of contacts to members of the opposite group triggers a similar, but opposite effect. Internalized out-group selective incentives have a negative effect on contribution propensities. The higher the number of members of the opposite group in the ego-network, the lower the contribution rate is. Because of its similarity with in-group selective incentives, this form of social control was not introduced in a monetary form in the experiments.

The effect of behavioral confirmation is not only dependent on the composition of the ego-network, but also on expected decisions of alters. It is presumed that subjects do not make qualitative differences between alters who are members of the same group.

Behavioral confirmation of in-group members: Behavioral confirmation is predicted to 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 in the ego-network. If the former is higher, the effect is positive. If the latter is higher, the effect is negative. It is assumed that the size of the effect is a linear function of the difference between the two.

For the operationalization of behavioral confirmation, the expectations of subjects were measured by asking them to forecast the decision of their left and right neighbors before every decision round.

Network segregation and experimental implementation

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 theoretical prediction, 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 than in a mixed structure or in a control condition with no networks.*

Furthermore, Takács (2001) also specified the impact of the relative size of social control mechanisms on intergroup conflict. As in-group selective incentives always drive towards contribution and behavioral confirmation might drive towards contribution as well as towards defection, the segregation effect on intergroup conflict is stronger where in-group selective incentives are relatively important when compared to behavioral confirmation. In order to test this theoretical prediction, in one experimental condition external in-group selective incentives and in another experimental condition external behavioral confirmation were introduced as additional monetary side-payments. On the basis of the theoretical prediction, the hypothesis about these effects is as follows:

The segregation effect on the likelihood of intergroup conflict will be stronger in the monetary selective incentives condition than in the monetary behavioral confirmation condition.

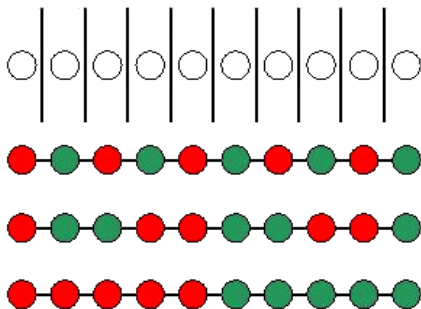


Figure 1 Structural Conditions in the Experiments: Control Condition, Low, Medium, and High Segregation. Note: red and green nodes indicate members of red and green group. In the Control Group no color labels were introduced and panel walls separated the subjects.

To test the above hypotheses, three types of network arrangements were implemented between sessions; with low, medium, and high segregation (see Figure 1). In addition, every experiment started with a control condition, in which subjects made their decisions in isolation without the knowledge of their group membership. After the control condition, color labels indicating group membership were introduced and subjects were arranged in one of the network conditions (low, medium, or high segregation) that are shown in Figure 1. For instance, in the case of low segregation, all subjects in the red group were seated next to members of the green group.

Subjects could see the composition of their ego-network on their computer screen. This intervention is targeted to assess internalized social control effects in the presence of minimal contact.

The IPG game and experimental implementation

The experiment used a series of single-shot IPG games as a model of competitive intergroup relations. The payoffs of the game used in the experiment 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 approximately 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 2. Each member of the group with more contributors received 15 points and each member of the group with less contributors lost 15 points as long as there were at least three contributors in the winning group ($v=15$; $d=-15$; $MCS=3$). A minimal contributing set with three persons was chosen in order to avoid that few coincidental contributions would have affected the result and in order to decrease individual efficacy in the experiment. Less than three contributions were insufficient to produce a public good and these contributions were lost to these individuals. When the number of contributors was equal in the groups and was over the minimal contributing set, all subjects lost 11 points ($c=-11$).

Figure 2. The IPG Game Used in the Experiments

payoffs in points	number of contributors in the green group						
	0	1	2	3	4	5	
number of contributors in the red group	0	0	0	0	15	15	15
	1	0	0	0	15	15	15
	2	0	0	0	15	15	15
	3	-15	-15	-15	-11	15	15
	4	-15	-15	-15	-15	-11	15
	5	-15	-15	-15	-15	-15	-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. In addition to these payoffs, defectors could keep the endowment of 11 points, and every subjects received 15 points to ensure positive payments.

Everyone received these public good and “bad” rewards, regardless of the decision to keep or give away the bonus of 11 points. Figure 2 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.

In order to obtain more reliable 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 and what others were doing in the same round. In this way, every decision round could be handled in an equivalent way. This method was applied in earlier team game experiments by Bornstein and Ben-Yossef (1994).

Every experiment consisted of four parts (see Table 1). In Part I, subjects made their decisions in isolation. In Part II, subjects played the IPG game with minimal contact in different network configurations that are represented in Figure 1.

Table 2. The Number of Sessions by Experimental Conditions

	level of segregation		
	low	medium	high
<i>monetary behavioral confirmation is introduced in Part III</i>	3	4	3
<i>monetary in-group selective incentives introduced in Part III</i>	3	4	3

The comparison of contribution rates in Parts I and II will provide the opportunity to test the main hypotheses about the presence of internalized social control mechanisms and segregation effects. In Part III, monetary side-payments were introduced between connected subjects. This intervention aimed to provide a meaningful comparison of the relative size of the effect of internalized social incentives and monetary side-payments. With regard to monetary side-payments, two conditions were implemented between experimental sessions. Next to the payoffs that were present in the beginning of the experiments (see Table 2), in Part III, in the monetary behavioral confirmation condition external behavioral confirmation incentives (5 points), in the monetary selective incentives condition external in-group selective incentives (5 points) were introduced (cf. Table 1). In Part IV, in both conditions the other external incentives were also introduced. Subjects received 5 points of behavioral confirmation reward if one of their in-group neighbors chose the same action as they did and received 10 points if two of their in-group neighbors acted the same way. In-group selective incentives were distributed regardless of the decision of neighbors. Contributors received 5 points for each in-group neighbor they had. Out-group selective incentives were not introduced in a monetary form. In the low segregation condition (six sessions) there was no change due to the absence of in-group neighbors and this condition was used as a control condition. To summarize, the experiment has followed a 2 × 3 block-design that is represented in Table 2.

Table 1. Overview of Experimental Parts

Part I	<i>anonymous control condition</i>	<i>panel walls isolate subjects</i>
Part II	<i>minimal contact is established</i>	<i>networks (low / medium / high segregation)</i>
Part III	<i>one form (b/s) of social control is introduced in a monetary form</i>	<i>networks (low / medium / high segregation)</i>
Part IV	<i>the other form (s/b) of social control is introduced in a monetary form</i>	<i>networks (low / medium / high segregation)</i>

The order of experimental parts shown in Table 1 was not altered, since 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.

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.

METHOD

Subjects

203 subjects took part in the experiments at the University of Groningen, in the Netherlands. 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 show up. Four sessions failed to be completely filled. In these cases, computer players were included.³ 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 partici-

³ This meant 1, 2, 2, and 2 cases in these four sessions.

pants did not have a significant influence on the behavior of subjects in the IPG games.⁴

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 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). Total payoffs varied between 14 and 32 points with an average of 21.1 points that was equivalent to 8.9 USD. 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. Upon arrival, subjects were randomly seated at a computer.⁵ Panel walls separated the subjects to ensure their privacy. Subjects received instructions on paper and on their screen.⁶ 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.

⁴ A group-level control variable indicating the presence of a computer player was not significant when added to any of the multivariate models discussed in the Results section.

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

⁶ Full instructions are available in Takács (2002: 101-104).

In each of the four experimental parts, subjects played five rounds of single-shot IPG games, and a randomly chosen number of repeated games afterwards. In every decision round, subjects had to decide whether they would keep the 11 points 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 the amount of 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, panel 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 arranged 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 each part.⁷

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 departures.

ANALYSIS OF CONTRIBUTION PROPENSITIES

This section describes the logic of analysis that is used to test the main hypotheses. 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 subjects, who took these

⁷ In the repeated games, subjects were informed about the result of the previous round.

decisions, and their characteristics are the higher level observations. The two-level model corrects for the methodological problem that observations within the subjects are not independent. Multilevel models take care of this dependency and separates within subject and between subject variance. 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 r th single-shot game. Note that while the probability of contribution is between 0 and 1, the propensity can take any value. 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. 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, formally

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

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. Since nothing distinguishes between single-shot game rounds, only a low intra-individual variation is expected within an experimental part that might be due to individual uncertainty or 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.

Main effects: social control

With the introduction of minimal contact and network structures (Part II), the effect of segregation on intergroup relations and the presence of underlying internalized mechanisms can be tested. The number of in-group ties is predicted to have a positive effect on contribution rates as minimal contact allows for the activation of internalized in-

group selective incentives rewarding contribution and punishing defection (s_0). The number of out-group ties is predicted to provide an incentive against contribution because of internalized out-group selective incentives (t_0). An auxiliary assumption here is that internalized selective incentives affect contribution propensities as a linear function of the number of ties. This number varies between subjects; it is zero in Part I for all subjects and might be 0, 1, or 2 in later parts of the experiment depending on the network condition.

The expected behavior of in-group alters is relevant for internalized behavioral confirmation as subjects are predicted to adjust their actual decisions to the expected decision of alters from their group. The difference between the expected number of contributing in-group alters and the expected number of defecting in-group alters is predicted to have a positive effect on contribution rates (captured by the parameter b_0). As it was expressed earlier, if the expected number of contributors is higher, behavioral confirmation increases the likelihood of contribution. On the other hand, in case there are more defectors among alters, behavioral confirmation decreases the likelihood of contribution. Because of the simple network patterns used in the experiment, the difference can only take integer values between -2 and 2.

The parameter values of s_0 , t_0 , b_0 are estimated from the experimental results. 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. Therefore, no specific form of utility function is assumed that could be applied to everyone. In the simplest model, only the average individual importance of internalized social control is estimated, but some presented models will allow for a random variance in the size of these effects. Models with random effects 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, variances of the effects of different forms of internalized social control will be estimated, as well as their covariances.

For a better calibration of social control effects, in some sessions from Part III on, external social control is introduced in the form of monetary side-payments. External selective incentives (s) and behavioral confirmation (b) are predicted to have a positive effect on contribution rates and these parameters are also need to be estimated. Effects of external social control can clearly be separated from internalized social control, as in Part II of the experiments only internalized social control could have an effect. The size of the effect of external control, however, might interact with the size of the effect of internalized social control. In general,

the utility of 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 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 influence rational decision-making in social dilemma experiments (e.g., Liebrand, 1984; Doi, 1994). Sub-

jects, 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.⁸ 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 at all 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, the number of acquaintances in the experiment is included as a control variable. 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 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.

⁸ The exact questions can be found in Takács (2002).

Table 3. Outcomes by Segregation Conditions in the Experiments

segregation condition in the experiment	outcome of the decision round		Total
	no competitive action	conflict	
control condition (unknown group membership)	26.97% (271)	73.03% (734)	100% (1005)
low segregation	50.23% (428)	49.77% (424)	100% (852)
medium segregation	13.75% (160)	86.25% (1004)	100% (1164)
high segregation	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

RESULTS

Contribution rates and conflict under different experimental conditions

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

the result of internalized *and* external social control. In order to test whether internalized social control can alone cause such differences between segregation conditions, results from Parts I and II are compared. The comparison reveals that minimal 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 $p=0.043$). In Part II, the contribution rate was highest in the medium segregation condition, which *contradicts the segregation hypothesis*. 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

Table 3 shows that conflict was already quite likely 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 segregation condition, and occurred most often in the high segregation condition, which supports the segregation hypothesis. On the other hand, conflict was almost as likely in the medium segregation condition as in high segregation. Conflict occurred in 85.83% of the cases in the medium and 88.57% of the cases in the high segregation condition (from unweighted outcomes; $t=0.613$, two-tailed $p=0.541$).

Contribution rates by segregation conditions are summarized in Table 4. The differences between segregation conditions are

Table 4. Average Contribution Rates in Different Segregation Conditions and Parts of the Experiment

incentives introduced first	segregation level			Total
	low	medium	high	
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				
<i>b</i> (confirmation)	-	58.42% (190)	47.33% (150)	53.53% (340)
<i>s</i> (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				
<i>b</i> (confirmation)	-	62.63% (190)	68.00% (150)	65.00% (340)
<i>s</i> (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 segregation conditions only illustrates coincidental baseline contribution rates in the different experimental sessions.

(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 monetary in-group selective incentives 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 in the presence of monetary in-group selective incentives, the effect of segregation is stronger than in the presence of monetary behavioral confirmation. In Part III, in the monetary in-group selective incentives condition average contribution rates are higher in the high segregation condition (75.66%) than in medium segregation (63.82%). On the other hand, in the monetary behavioral confirmation condition average contribution rates are higher in the medium segregation condition (58.42% vs. 47.33%).

Table 5. 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)
$\sigma_{ui}^2(s_0)$.000 (.000)
$\sigma_{ui}^2(s)$.300** (.139)
$\sigma_{ui}^2(b_0)$.196*** (.093)
$\sigma_{ui}^2(b)$.326*** (.226)
$\sigma_{ui}^2(t_0)$.009 (.050)
Covariances are reported below			
-2 Log Likelihood model		4480	4430
Improvement χ^2 (df in parentheses)		939*** (5) [#]	50*** (20)

Table 5b. 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); $\alpha: 0.174^{**}$ (0.066); $\sigma^2: 0.674^{+++}$ (0.087).

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 5 reports results for the two-level model on contribution propensities without control variables.⁹ 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.

The two models provide similar estimates. All effects are in the predicted direction. Hypotheses about the existence of internalized behavioral confirmation and internalized out-group selective incentives are supported. This means that contribution rates have increased with the difference between the number of expected in-group contributors and defectors and they have decreased with the number of out-group contacts. The effect of internalized in-group selective incentives is not significant. According to this result, the number of in-group contacts does not enforce contributions, if one controls for internalized behavioral confirmation. As predicted, both forms of external social control have a significant effect. It is important to note, however, that this simple model did not include any control variables.

Contribution rates between subjects have a high unexplained variance.¹⁰ The influence of behavioral confirmation and monetary in-group selective incentives varies significantly between subjects. The hypothesis that the sizes of 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, 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 . 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.

Two analyses are conducted again: one assuming fixed social control effects without random variation and another assuming a random variation and covariation of these estimates (see Table 6). 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 in-group selective incentives became significant and the significant effect of internalized out-group selective incentives 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 5. 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 out-group selective incentives. On the basis of the analysis reported in Table 6, after controlling for a negative learning tendency, it turns out that on average, out-group selective 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 in-group 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. However, this effect cannot be separated from the influence of internalized selective incentives that are not contingent on predictions. 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 orientat-

⁹ For goodness-of-fit, $-2 \log$ likelihood statistics and χ^2 tests of improvement are indicated at the bottom of tables.

¹⁰ 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).

Table 6a. 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 random slopes of main effects	
FIXED EFFECTS					
α (constant) baseline contr. propensity	?	1.378**	(.423)	1.516***	(.409)
s_0 internalized in-group selective incentives	+	.186*	(.082)	.188*	(.081)
t_0 internalized out-group selective incentives	-	.165	(.086)	.142	(.086)
b_0 internalized behavioral confirmation	+	.586***	(.067)	.591***	(.080)
s monetary in-group selective incentives	+	.769***	(.109)	.699***	(.127)
b monetary behavioral confirmation	+	.718***	(.108)	.705***	(.126)
<i>Personal characteristics and other subject-level variables</i>					
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)
<i>Procedure effects</i>					
within part trend		-.215***	(.036)	-.213***	(.036)
endgame effect		.373**	(.125)	.370**	(.126)
between parts trend		-.397***	(.060)	-.379***	(.061)
last iterated game was a draw		.538***	(.149)	.515***	(.152)
last iterated game was lost		.185	(.122)	.199	(.125)
last iterated game was won		.214	(.123)	.275*	(.125)
RANDOM EFFECTS					
inter-individual variance σ^2		.574+++	(.083)	.559+++	(.116)
$\sigma^2 ui (s_0)$.000	(.000)
$\sigma^2 ui (t_0)$.002	(.050)
$\sigma^2 ui (b_0)$.202+++	(.096)
$\sigma^2 ui (s)$.322+++	(.152)
$\sigma^2 ui (b)$.421+++	(.246)
<i>Covariances are reported below</i>					
-2 Log Likelihood model			4480		4430
Improvement χ^2 (df in parentheses)			939*** (5)#		50*** (20)

Table 6b. Random Effects: Estimated Covariances

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

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)

tions. The analysis of college major does not reveal an effect of economics training. 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. Effects of risk-aversion and loss-aversion, however, 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.

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. In this study, a decay of contribution is found for the series of single-shot games. 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 not 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. By analyzing last rounds only, model parameters were similar to those values that were reported in Table 6, including an insignificant effect of internalized in-group 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 6 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 (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 winning the public good.

Interaction effects

As Table 6 demonstrated, the significant effect of internalized out-group selective incentives disappeared after the inclusion of learning trends. It might be possible that this form of social control is mistakenly conceptualized and out-group selective incentives 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 7). It seems that internalized out-group selective incentives 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 out-group selective incentives. 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 in-group 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,

Table 7a. 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					
<i>Main variables</i>					
α (constant) baseline contr. propensity	?	1.346***	(.402)	1.491**	(.477)
s_0 internalized in-group selective incentives	+	.176*	(.082)	.165*	(.084)
t_0 internalized out-group selective incentives	-	.223	(.132)	.238	(.134)
b_0 internalized behavioral confirmation	+	.589***	(.119)	.618***	(.141)
s monetary in-group selective incentives	+	.769***	(.110)	.745***	(.135)
b monetary behavioral confirmation	+	.703***	(.109)	.681***	(.125)
<i>Personal characteristics and other subject-level variables</i>					
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 sci.		-.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 qs		-.397*	(.180)	-.404*	(.176)
prosocial orientation		.330	(.206)	.353	(.202)
egalitarian orientation		.419*	(.203)	.394*	(.200)
number of acquainted subjects in the exp.		-.066	(.089)	-.066	(.087)
delay (minutes) at the start of the exp.		.006	(.007)	.006	(.007)
quiz questions answered correctly %		-.004	(.005)	-.005	(.005)
<i>Procedure effects</i>					
within part trend		-.178	(.121)	-.188	(.122)
endgame effect		.379**	(.126)	.381**	(.127)
between parts trend		-.397***	(.061)	-.386***	(.062)
last iterated game was a draw		.527***	(.150)	.495**	(.157)
last iterated game was lost		.180	(.123)	.186	(.128)
last iterated game was won		.214	(.124)	.266*	(.128)
<i>Cross-level interactions</i>					
$t_0 \times$ number of acquainted opposite neighbors		-.153	(.196)	-.164	(.194)
$b_0 \times$ number of acquainted in-group 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 in-group neighbors of the same sex		-.038	(.084)	-.128	(.102)
$b_0 \times$ number of female in-group 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 \wedge quiz questions correct %		.000	(.001)	.000	(.001)
RANDOM EFFECTS					
inter-individual variance σ^2		.563+++	(.082)	.512+++	(.084)
$\sigma^2 ui (s_0)$.000	(.000)
$\sigma^2 ui (t_0)$.000	(.000)
$\sigma^2 ui (b_0)$.143+++	(.089)
$\sigma^2 ui (s)$.549+++	(.187)
$\sigma^2 ui (b)$.379+++	(.240)
<i>Covariances are reported below</i>					
-2 Log Likelihood model		4211		4169	
Improvement χ^2 (df in parentheses)				42** (20)	
vs. previous model		36*** (11)		29** (11)	

Table 7b. Random Effects: Estimated Covariances

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

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)

this result can probably be attributed to the fact that they were not close acquaintances or simply, subjects considered laboratory conditions impersonal. Prosocial and egalitarian attitudes were not correlated with higher relative weight of internalized social control. Only the interaction between out-group selective incentives and prosocial orientation proved to be significant. This effect indicates that prosocial subjects liked to be “local heroes”, who contributed even when they were surrounded by members of the other group. This is another indication of how prosocial attitudes can be harmful in the intergroup context.

DISCUSSION

The main objective of this study was to show how social control mechanisms enter into simple experimental situations and can affect individual decisions in social dilemmas. As an aggregated result of different forms of social control, it was demonstrated *how network 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 minimal contact between subjects. For the test of hypotheses, a unique experimental design was introduced based on special arrangements in the laboratory. With this setup, network based social control, which is believed to be influential also in real life, was the target of analysis in an experimental environment.

In the experiments, intergroup competitions were modeled by an Intergroup Public Goods game (Rapoport and Bornstein, 1987; Takács, 2001). Comparison of segregation conditions showed that intergroup conflict was least likely in a completely mixed setting and was most likely when members of the groups were arranged according to a segregated pattern, which confirms the segregation hypothesis. Furthermore, as predicted, the segregation effect was stronger in the presence of monetary in-group selective incentives than in the presence of monetary behavioral confirmation.

By analyzing individual decisions, social control mechanisms 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 in-group contacts even when only a minimal contact and “minimum network relations” have been established between them. Estimates of model parameters indicate that under the chosen reward structure, internalized behavioral confirmation affected contribution propensities as much as monetary behavioral confirmation. Concerning behavioral confirmation, however, a part of the significant effect might be due to the bi-directional relationship between own behavior and expectations about the behavior of others. Subjects formulated their expectations at the same time of their decisions; therefore the guess what others do is not obviously an exogenous variable. Subjects, for instance, could have formulated their expectations in order to avoid cognitive dissonance or to project their decision on others. This might have played a role for some subjects,¹¹ but it sounds unlikely that many subjects fitted their expectations to their behavior, which does not pay off, and not the behavior to expectations, which does.

Besides, no strong support was found for the presence of other forms of internalized social control. Internalized in-group selective incentives had a significant effect after controlling for a between parts trend. Internalized out-group selective incentives 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 segregation 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 in-group 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 segregation condition (cf. Table 3). Another reason might be that the strength of internalized social control is a nonlinear function of the number of in-group contacts. As a consequence, there is a marginal decrease in the segregation

¹¹ Only one subject revealed such motivations in the post-experiment questionnaire.

effect on the likelihood of intergroup conflict and medium levels of segregation are already associated with harmful outcomes.

Among personal characteristics, only social orientations had significant effects. 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 out-group selective incentives and prosocial orientation. This implies that sub-

jects 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 network 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 GW. 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 JH. 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 Ass'n.
- Black DJ. 1984. Social Control as a Dependent Variable. In: Black, DJ. (Ed.): *Towards a General Theory of Social Control*. vol.1. New York, Academic.
- Bohnet I & Frey BS. 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 AS & Raudenbush SW. 1992. *Hierarchical linear models: Applications and data analysis methods*. London, Sage.
- Budescu DV, Rapoport Am, & Suleiman R. 1990. Resource dilemmas with environmental uncertainty and asymmetric players. *European Journal of Social Psychology* 20: 475-487.
- Burt RS. 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 MS-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 Schulz U, et al. (Eds.), *Social dilemmas and cooperation*. Berlin, Springer Verlag.
- Drolet AL & Morris MW. 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 CC & Grossman PJ. 1998. Are women less selfish than men? Evidence from dictator experiments. *Economic Journal* 108: 726-735.
- Farquhar PH. 1984. Utility assessment methods. *Management Science* 30: 1283-1300.
- Finkel SE & Opp K-D. 1991. Party identification and participation in collective political action. *Journal of Politics* 53: 339-371.
- Flache A & Macy MW. 1996. The weakness of strong ties: Collective action failure in a highly cohesive group. *Journal of Mathematical Sociology* 21: 3-28.
- Frank RH, 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 JP. 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: 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 RV. 1993. Collective action and network structure. *American Sociological Review*, 58: 182-196.
- Gould RV. 1995. *Insurgent identities: Class, community, and protest in Paris from 1848 to the Commune*. Chicago, University of Chicago Press.
- Größer J. & Schram, A. 2006. Neighborhood Information Exchange and Voter Participation: An Experimental Study. *American Political Science Review*, forthcoming.
- Guttman JM. 1986. Matching behavior and collective action: Some experimental evidence. *Journal of Economic Behavior and Organization* 7: 171-198.
- Heckathorn DD. 1990. Collective sanctions and compliance norms: A formal theory of group-mediated social control. *American Sociological Review* 55: 366-384.
- Heckathorn DD. 1993. Collective action and group heterogeneity: Voluntary provision versus selective incentives. *American Sociological Review* 58: 329-350.
- Insko CA, Schopler J, Graetz KA, Drigotas SM, Currey DP, Smith SL, Brazil D, & Bornstein G. 1994. Interindividual-intergroup discontinuity in the prisoner's dilemma game. *Journal of Conflict Resolution* 38: 87-116.
- Isaac RM, McCue KF, & Plott C. 1985. Public goods provision in an experimental environment. *Journal of Public Economics* 26: 51-74.
- Kornhauser RR. 1978. *Social Sources of Delinquency: An Appraisal of Analytic Models*. Chicago, University of Chicago Press.
- Kosfeld M. 2003. *Network Experiments*. Institute for Empirical Research in Economics, University of Zürich Working Paper Series, No. 152.
- Kuran T. 1995. *Private truths, public lies: The social consequences of preference falsification*. Cambridge, MA: Harvard University Press.
- Ledyard JO. 1995. Public goods: A survey of experimental research. In Kagel JH & Roth A E. (Eds.), *The handbook of experimental economics*. Princeton: Princeton University Press.
- Liebrand WBG. 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.
- Lovaglia MJ, Willer R, & Troyer L. 2003. Power, Status, and Collective Action: Developing Fundamental Theories to Address a Substantive Problem. In Thye SR, Skvoretz J, & Lawler EJ. (Eds.), *Advances in Group Processes*, 20. Greenwich, CT: JAI Press.
- Macy MW. 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 PE. 1993. *Critical mass in collective action*. Cambridge: Cambridge University Press.
- Marwell G, Oliver PE, & Pahl 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 PJ. 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 AR. 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: Westview.
- Ortmann A & Tichy LK. 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.
- 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 AM. 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. 2004. The impact of social approval and framing on cooperation in public good situations. *Journal of Public Economics* 88: 1625-1644.
- 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.
- Schopler J & Insko CA. 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 JF. 1971. *Internalization of norms*. Englewood Cliffs, NJ: Prentice-Hall.
- Solnick SJ & Schweitzer ME. 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 AJ, & Dodge PJ. 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 Foddy M, Smithson M, Schneider S, & Hogg M. (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.
- van Assen MALM & Snijders C. 2004. Effects of risk preferences in social dilemmas: A game-theoretical analysis and evidence from two experiments. In: Suleiman R, Budescu D, Messick DM, & Fischer I. (Eds.): *Contemporary Approaches to Social Dilemma Research*. Cambridge, MA, Cambridge University Press.
- van Duijn MAJ, van Busschbach JT, & Snijders TAB. 1999. Multilevel analysis of personal networks as dependent variables. *Social Networks* 21: 187-209.
- van Lange PAM. 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 PAM, Otten W, De Bruin EMN, & Joireman JA. 1997. Development of prosocial, individualistic, and competitive orientations: Theory and preliminary evidence. *Journal of Personality and Social Psychology* 73: 733-746.
- van de Kragt AJC, Dawes RM, & Orbell JM. 1988. Are people who cooperate 'rational altruists'? *Public Choice* 56: 233-247.
- van de Kragt AJC, Orbell JM, & Dawes RM. 1983. The Minimal Contributing Set as a solution to public goods problems. *American Political Science Review* 77: 112-122.
- Villareal A. 2002. Political competition and violence in Mexico: Hierarchical social control in local patronage structures. *American Sociological Review* 67: 477-498.
- Walters AE, Stuhlmacher AF, & Meyer LL. 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.