European Economic Review 76 (2015) 239–252

Contents lists available at ScienceDirect

European Economic Review

journal homepage: www.elsevier.com/locate/eer

# On the effects of group identity in strategic environments

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ARTICLE INFO

*Article history:*
Received 13 September 2013
Accepted 12 March 2015
Available online 23 March 2015

*Keywords:*
Group identity
Centipede game
Stag hunt game
Experiment

*JEL classification:*
C72
C91
C92
D83

ABSTRACT

We examine differences in behavior between subjects interacting with a member of either the same or different identity group in both a centipede game and a series of stag hunt games. We find evidence that subjects interacting with outgroup members are more likely to behave as though best-responding to uniform randomization of the partner. We conclude that group identity not only affects player's social preferences, as identified in earlier research, but also affects the decision making process, independent of changes in the utility function.



## 1. Introduction

It is well established that group identity manipulations can increase altruism, positive reciprocity and the desire for maximising social welfare among ingroup partners, even when group assignment is based on arbitrary criteria (for an overview, see Chen and Li, 2009; Chen and Chen, 2011; Goette et al., 2012a,b). In this paper, we consider the possibility that group identity affects not only preferences, but also affects a player's decision making through the underlying belief-formation process or belief-action correspondence. This idea shares some relationship with the concept of social projection in psychology (Robbins and Krueger, 2005; Acevedo and Krueger, 2005; Ames et al., 2011), but has not received attention in the economics literature.

We report the results of two separate experiments which reveal that people interacting with outgroup partners in strategic interactions are, ceteris paribus, more likely to behave as though their opponent is behaving randomly. We show that this effect can augment, diminish, or even reverse the impact of group identity on social preferences, and in the latter case can result in counter-intuitive outcomes, for example, outcomes contrary to predictions of standard preference-driven models of group identity.

Our first experiment was originally designed to examine the impact of social preferences on behavior in the centipede game. Participants were assigned to almost "minimal groups" according to their preferences over paintings (following Chen and Li, 2009) and interacted with ingroup or outgroup members. Following the social-preference hypothesis, our prior was

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http://dx.doi.org/10.1016/j.euroecorev.2015.03.001
0014-2921/

that increased altruism, positive reciprocity and the desire for maximising social welfare would lead to pairs from the same identity group to continue longer and reach more efficient outcomes. However, we found that the opposite was the case, with pairs from different groups (outgroups) continuing longer. The explanation we found for this counter-intuitive result was that subjects were more likely to act as though the behavior of outgroup members was random, compared to behavior when facing ingroup members. This could occur either because subjects form different beliefs about the strategic sophistication of outgroup members or feel less able to predict their behavior. With the exponentially-increasing payoffs in the centipede game, the best response to uniform randomization of the opponent is to choose continue.

We implemented a follow-up experiment (Experiment 2), applying the same group identity manipulation in a series of extended stag hunt games, to further test the explanations of behavior suggested by the results in the centipede game (Experiment 1). Experiment 2 allowed us to test our explanation independently using new data, and importantly, without relying on stated beliefs of participants. Confirming our initial results, we again found that subjects interacting with outgroup members were more likely than those interacting with ingroup members to choose strategies which were best responses to uniform randomization (BRUR) by their partner.

Results across the two experiments point at two intuitive interpretations for why subjects behave as if outgroup members are treated as acting randomly. In the first, outgroups are seen as being genuinely strategically unsophisticated in comparison with ingroup members, thus raising an individuals' self-esteem. Alternatively, subjects may feel less able to predict outgroup behavior and, having no idea about what they will do, apply the principle of insufficient reason and assign equal probability to all possible actions.

As an additional test of our findings, we elicited participants' beliefs about the behavior of their opponents in both experiments and found differences in the treatment effects across experiments. In Experiment 1, we found no treatment effect between elicited beliefs but support for changes in the belief-action correspondence between group affiliation treatments. In Experiment 2, there was a significant treatment effect, suggesting that the group identity manipulation also affected the belief-formation process itself. These conflicting findings suggest that our belief data is not sufficiently reliable to distinguish between the interpretations described in the previous paragraph, but add to the emerging discussions on biases in belief elicitation (e.g. Schlag et al., 2014; Schotter and Trevino, 2014) and on the underlying relationship between (ex post) stated beliefs and behavior in strategic interactions (e.g. Costa-Gomes and Weizsäcker, 2008; Rubinstein and Salant, 2014).

Overall, our insights contribute to the understanding of the effects of group identity in strategic interactions, and more specifically, fully explain a number of puzzling empirical and experimental observations in bargaining and market environments. Graddy (1995), for example, showed that white fishmongers charge less to Asian customers (in take it or leave it offers); Ayres (1991) found that test buyers get worse deals from car salespeople of same gender or race. A recent experimental study closely related to our work is Li et al. (2011) who also use group identity manipulations to study seller–buyer relationships in oligopolistic markets. Their results show that sellers charge lower prices to buyers of the other group than of the same group and is consistent with our results of an uncertainty-driven discrimination if salespeople are less certain about the relevant outgroups' bargaining strategy than that of ingroups. Increased uncertainty regarding outgroup behavior also provides an explanation of why employers are often less willing to hire people of different gender or ethnic groups, even in the absence of any preference for discrimination.

## 2. Experiment 1: centipede game

The study was designed to investigate the role of social preferences in behavior in the centipede game.[1] To this end, we used a seven-legged centipede game with exponentially-increasing payoffs, as depicted in Fig. 1. In this game, two players (labelled neutrally as player types 1 and 2 respectively) alternately faced the decision to continue or stop, $a \in \{C, S\}$, until one of them chooses stop, which ends the game, or player 2 chooses $C$ at the final node. The unique subgame-perfect Nash equilibrium is such that players choose to stop at each of their decision nodes, the game thus ending at the first node.

If group identity increases reciprocity, a natural hypothesis is that subjects playing with an ingroup member are more likely to continue at any given decision node compared to subjects interacting with an outgroup member. Theoretically, increased altruism and concerns for social-welfare maximization would make players continue longer by making later nodes relatively more attractive; positive reciprocity would also lead to continue longer as players repay the favor of continuing by doing likewise.[2] This can be summarized in the following hypothesis.

[1] The centipede game has attracted much attention both in the theoretical and experimental game theory literature. It has been repeatedly demonstrated in experimental studies that the game is rarely terminated at the first node, the unique subgame-perfect Nash equilibrium in the game. Most of the literature has argued that the systematic deviations from the subgame-perfect equilibrium outcome result from some form of bounded rationality (Rosenthal, 1981; Aumann, 1995, 1998). Boundedly rational explanations of behavior in the experimental literature on the centipede game include quantal response equilibria (Fey et al., 1996; McKelvey and Palfrey, 1998), learning (Nagel and Tang, 1998; Rapoport et al., 2003), varying abilities to perform backward induction or limited depths of reasoning (Palacios-Huerta and Volij, 2009; Levitt et al., 2011; Gerber and Wichardt, 2010; Kawagoe and Takizawa, 2012; Ho and Su, 2013). With the exception of McKelvey and Palfrey (1992) and Fey et al. (1996), who allow for altruistic behavior, none of these papers has explicitly tested for the possible import of social preferences in the centipede game.

[2] McKelvey and Palfrey (1992) and Fey et al. (1996) provide formal theoretical models for the case of altruism. In both the imperfect information model in the former paper, and the AQRE model in the latter, a higher proportion of altruists increases the probability of the game ending at later nodes.

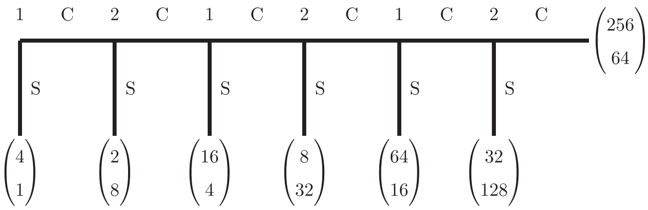


**Fig. 1.** Centipede game with exponentially-increasing payoffs (Experiment 1).

**Hypothesis 1.** *Participants in the ingroup treatment are more likely to choose continue at any given node than those in the outgroup treatment.*

### *2.1. Design*

The experiment was divided into four parts: a group identity task, participation in a centipede game, elicitation of beliefs regarding the partner's behavior, and a post-experiment questionnaire.

*Part* 1: Following the procedure in Chen and Li (2009), we used a modified version of the "minimal group paradigm" of Tajfel and Turner (1979) to induce group identity among participants. In this paradigm, group membership is constructed from artificial contexts to prevent any reasonable association of particular group membership with ability, social preferences, or the like. Participants stated their preferences over five pairs of paintings in this task, with each pair consisting of one painting by Paul Klee and one by Wassily Kandinsky. The identities of the painters were not revealed to participants at this stage. Based on their relative preferences, half of the participants (12 out of 24 per session) were assigned to the "Klee group" and the other half to the "Kandinsky group". The group assignment remained fixed for the course of the experiment. After the group assignment, participants had to guess who of the two painters created two additional paintings. To enhance the effect of group identity, participants were given the possibility of communicating within their own group via a chat program. Participants were incentivized with 10 points for each correct guess. Participants received no feedback on performance until all decision-making parts of the experiment were completed.

*Part* 2: Before the start of the centipede game, depicted in Fig. 1, participants were informed about their player type (I or II) which was drawn randomly. Treatment allocation for each session was random, with half of the participants matched with a member of the same group (*ingroup* treatment) and the other half with a member of the other group (*outgroup* treatment). We used the strategy method (see, e.g. Brandts and Charness, 2011) to elicit participants' strategies as we were interested in the full strategy vector and not only the outcome.[3] A participant chose continue/stop at each of her three nodes sequentially, from left to right. The current decision node was highlighted on screen, see screenshot in Appendix B.2. Participants were informed that they would not learn the decisions of their respective matching partner until all decisions were made in all parts.[4]

*Part* 3: We elicited participants' beliefs about the population behavior of their matched partner types. More specifically, participants guessed how many out of 12 players (all of whom are playing in the role of their respective matching partner in the game) chose "stop" at each of their three decision nodes. Similar to the presentation of decision nodes in part 2, the elicitation method was implemented sequentially for each node (see Appendix B.2). A prize of 100 points was paid for a correct guess.[5] Participants learned about the task only after making their own decisions so as not to influence behavior in the actual games. After all decisions in part 3 were made, a matching partner for the game was randomly drawn to determine the game's outcome and participants were informed about their performance in a all parts of the experiment.

*Part* 4: Participants completed a short post-experiment questionnaire.

*Procedures*: The experiment was programmed and conducted using z-Tree (Fischbacher, 2007). Sessions took place in Lakelab, the experimental economics laboratory at the University of Konstanz. Participants were student volunteers recruited from the subject pool of the University; economics and psychology students were excluded from participation. Each subject participated in only one session. We conducted 4 sessions, each comprised of 24 participants (96 participants in total). After the experimenter read out the rules for participation, subjects received a set of written instructions about the general procedure of the experiment (see Appendix B.1 for the instructions). At the end of a session, points earned across all

[3] Kawagoe and Takizawa (2012) find no difference in behavior between the direct-response and strategy method implementation of the game. Consult Brandts and Charness (2011) for a comparison of these two methods over many studies.

[4] Note that participants played a second identical centipede game, but with a subject drawn from the opposite group to the first game. We decided against using the observations of the second game in the analysis because of order effects. A two-sided Wilcoxon rank-sum test for the outgroup data rejects the hypothesis that strategies chosen by subjects playing an outgroup member in the first game are drawn from the same distribution as strategies chosen by subjects playing an outgroup member in the second game ($p$ value = 0.058). The same test for ingroup data was insignificant ($p$ value = 0.160). We speculate that the order effect is due to subjects "anchoring" on their initial choice (54 out of 96 subjects chose an identical strategy in both game).

[5] It is well known that this method elicits beliefs about the modal action. Hurley and Shogren (2005) and Schlag and Tremewan (2014) show that it also elicits an interval for the mean probability, in our case of width 1/13. As a test of the robustness of our results we use a variety of probabilities from the elicited intervals. We chose this method because it is easily understood by subjects. It also has the advantage over scoring rules of being robust to risk aversion.

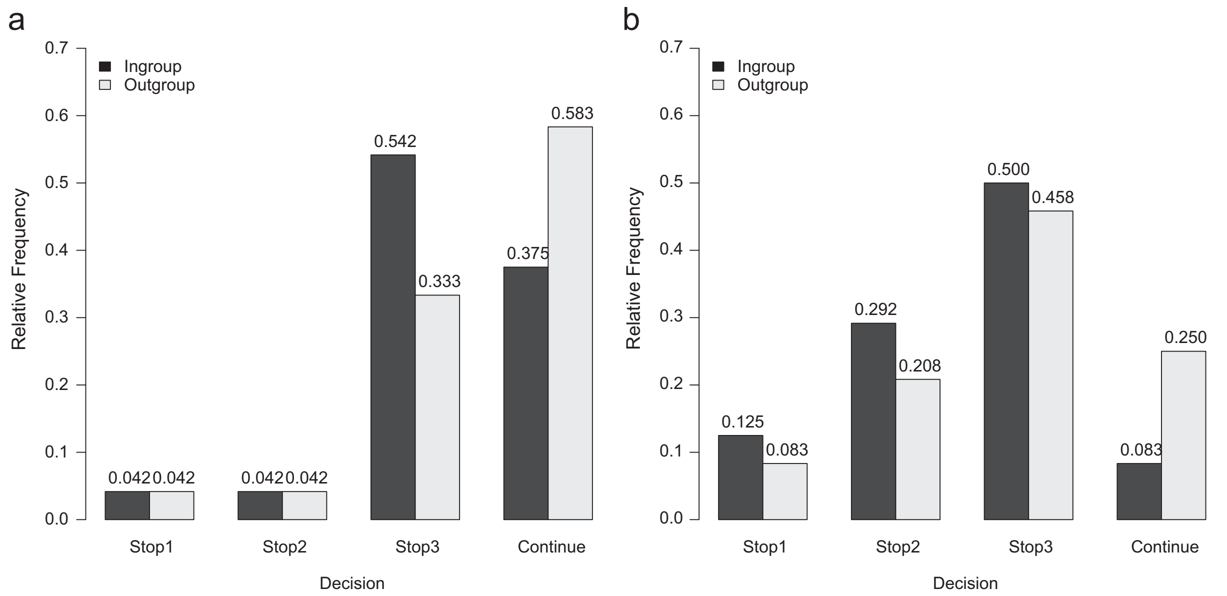


**Fig. 2.** Strategy distributions by player roles and treatment (centipede game). (a) Strategy distribution player 1 and (b) strategy distribution player 2.

experimental parts were added up and converted into Euros at an exchange rate of 20 Points = 1 Euro. In addition, each participant was given a 3 Euro show-up fee. Sessions lasted 45 minutes (including time for payment) and participants earned between 4.25 and 20 Euros (8.60 on average), paid out privately at the end of the experiment.

### 2.2. Results

We start by summarising the players' strategies, separated by player roles, in Fig. 2. The distribution of strategies does appear to be different between treatments, with subjects stopping earlier in ingroup than in outgroup interactions. Indeed, the modal decision for players, pooling data across player roles, is to stop at the third decision node in the ingroup treatment and to always continue in the outgroup treatment. This evidence is also reflected by the fact that the distribution of stopping nodes differs weakly between treatments when pooling over player roles (two-sample Wilcoxon rank-sum, $p$ value = 0.087). The distribution of outcomes shows that the treatment differences, that do exist in behavior, lead to substantial differences in realized payoffs. In fact, subjects playing outgroup members earn 58 points on average compared to 35 points for those playing ingroup members. The hypothesis that strengthening of positive social preferences would push the distribution of stopping nodes in the ingroup treatment to the right of the outgroup treatment is clearly rejected by the data. On the contrary, it appears that any treatment effect is in the opposite direction of Hypothesis 1.

Even though the experimental data fail to find evidence for the social preference hypothesis by considering only strategies, it is still possible that social preferences play a role. If subjects believe for some reason that ingroup players are more likely to stop earlier than outgroup players, this could counteract any effect of strengthened social preferences. This possibility is however also not supported by the elicited beliefs summarized in Table 1. Stated beliefs about behavior of the partner's population are very similar across treatments, with the distributions being significantly different only in the case of the player 1s' stated beliefs at the last decision node (two-sided Wilcoxon rank-sum test, $p$ value = 0.084; after adjusting for multiple comparisons using Bonferroni correction even this difference is not significant at any conventional level). Furthermore, we also observe similar variance in elicited beliefs between treatments, implying that subjects do not estimate or report their belief with more noise in the outgroup treatment. Overall, the induced group identity does not seem to modify stated beliefs towards behavior of the matched partner.

To further test whether changes in beliefs are obscuring a social preference effect, we report the results of probit regressions on the probability of a player continuing in Table 2. All three decisions of a subject are included, with standard errors clustered at the individual level. Model 1 includes only a dummy for playing an ingroup member and shows that subjects playing outgroups are almost 10% more likely to continue. Model 2 also controls for the beliefs about the number of players that will continue at the *subsequent* decision node. Note that, including this variable as a regressor, we lose the observations of player 2s' final decision node, as there are no subsequent decisions. The results show that the subjects' beliefs are consistent with their decisions whether or not to continue, and this relationship is significant at the 1% level. The ingroup effect remains significant for all levels of beliefs.

Model 3 adds an interaction term between the elicited belief and the ingroup treatment dummy to allow for the possibility that the relationship between reported beliefs and actions differs between treatments. The estimated coefficient of the interaction is significant at the 5% level and the estimated marginal effects show that the impact of a change in belief

**Table 1**
Elicited beliefs (centipede game).

| Player type | Node | Elicited belief | |
|---|---|---|---|
| | | Ingroup | Outgroup |
| 1 | 2 | 1.33 | 1.54 |
| | | (2.76) | (3.59) |
| | 4 | 3.54 | 3.42 |
| | | (3.68) | (3.74) |
| | 6 | 7.29 | 9.13 |
| | | (3.94) | (3.50) |
| 2 | 1 | 1.96 | 1.33 |
| | | (4.18) | (3.07) |
| | 3 | 3.42 | 3.13 |
| | | (4.09) | (3.30) |
| | 5 | 9.00 | 8.33 |
| | | (3.15) | (3.25) |

*Notes*: Average number of subjects, out of 12, guessed to stop at each node (by player type and treatment). Standard deviation in parentheses.

**Table 2**
Probability of continuing estimated by probit model (Experiment 1).

| | Model 1 | Model 2 | Model 3 |
|---|---|---|---|
| *Coefficients* | | | |
| Ingroup (=1) | −0.276* | −0.388* | −0.845*** |
| | (0.157) | (0.215) | (0.323) |
| Belief | | 0.174*** | 0.126*** |
| | | (0.024) | (0.031) |
| Ingroup × belief | | | 0.097** |
| | | | (0.047) |
| Constant | 0.631*** | 0.043 | 0.261 |
| | (0.116) | (0.197) | (0.230) |
| No. of observations | 288 | 240 | 240 |
| Log likelihood | −177.284 | −93.74 | −92.052 |
| Wald stats | 3.09 | 52.97 | 60.23 |
| ($\chi^2$, $p$ value) | 0.079 | $<0.001$ | $<0.001$ |
| Pseudo $R^2$ | 0.009 | 0.237 | 0.251 |
| *Marginal effects*[a] | | | |
| Ingroup effect | −0.097* | | |
| Ingroup effect (for belief=0) | | −0.152* | −0.323** |
| Ingroup effect (for belief=1) | | −0.154* | −0.292** |
| Ingroup effect (for belief=2) | | −0.151* | −0.251** |
| Ingroup effect (for belief=3) | | −0.144* | −0.205** |
| Ingroup effect (for belief=4) | | −0.133* | −0.157** |
| Ingroup effect (for belief=5) | | −0.119* | −0.111* |
| Ingroup effect (for belief=6) | | −0.104* | −0.071 |
| Ingroup effect (for belief=7) | | −0.088* | −0.037 |
| Ingroup effect (for belief=8) | | −0.072** | −0.012 |
| Ingroup effect (for belief=9) | | −0.057** | 0.004 |
| Ingroup effect (for belief=10) | | −0.044** | 0.015 |
| Ingroup effect (for belief=11) | | −0.033* | 0.019 |
| Ingroup effect (for belief=12) | | −0.024* | 0.020 |
| Belief effect (for ingroup=0, belief at mean) | | 0.034*** | 0.027*** |
| Belief effect (for ingroup=1, belief at mean) | | 0.042*** | 0.047*** |

*Notes*: This table presents coefficients and marginal effects of probit regressions. The dependent variable in all regressions is the probability of continuing. Standard errors in parentheses are clustered by subject. Belief is the number of subjects out of 12 expected to continue at the subsequent node (mean=6.99).

*** Indicates significance at the 1% level.

** Significance at the 5% level.

* Significance at the 10% level.

[a] Marginal effects reported for Model 3 take into account the interaction term.

at the mean is almost twice as strong in the ingroup treatment.[6] The ingroup effect remains large and significant for low belief levels but disappears at higher levels, i.e. when the partner is believed to be more likely to continue.[7]

### 2.3. Discussion

Group identity manipulations have previously been shown to strengthen social preferences (i.e. increase altruism, propensity to positively reciprocate, and desire for social-welfare maximization), but we find evidence to the contrary in our exponentially-increasing centipede game: subjects continue longer with outgroups and are more likely to choose to continue with outgroups, even if their stated beliefs indicate a high probability that their partner will stop at the next node.

Our explanation for this counter-intuitive result is that subjects facing outgroup members are more inclined to behave as though their partner acts randomly (alternative explanations will be discussed in Section 4). Given the payoffs in our game, a risk-neutral and self-interested player should only stop it she believes the probability of her partner stopping at the subsequent node is greater than 6/7, a rather high degree of certainty. There are two possible explanations for the observed behavior. Subjects may either regard outgroups as less strategically sophisticated, or find it hard to make predictions about their behavior and thus place less weight on beliefs derived from their own reasoning process. The former explanation is not consistent with our belief data (which does not differ between treatments) whereas the latter concords with the significant interaction effect in the probit regressions (see Table 2).

## 3. Experiment 2: extended stag hunt games

We designed a follow-up experiment, in which we implement a series of extended stag hunt games (Fig. 3), to further test our explanation of the results of Experiment 1. In each of the five $3 \times 3$ extended stag hunt games we employ in Experiment 2, the action set of a row player consists of $a_i = \{U, M, D\}$, with the corresponding action set of a column player of $a_j = \{L, C, R\}$. Because of the symmetric payoff structure we do not describe the column player's actions separately. Selecting action '*U*' guarantees a certain but low payoff; choosing '*M*' earns a medium payoff in the case of coordination and some insurance in the case of miscoordination, but less than the payoff from '*U*'; choosing '*D*' earns a high payoff in the case of coordination, and nothing otherwise. There are three pure-strategy Nash equilibria, characterized by each player choosing the same strategy (*U*, *M*, or *D*).

To control for the possibility that an alternative unconsidered explanation makes one of the actions more attractive in one of the treatments, the payoffs in the games have been chosen such that the best response to uniform randomization is '*U*' in games 1 and 4, '*D*' in games 2 and 3, and '*M*' in game 5. This also gives us a strong test our behavioral hypothesis without relying on elicited beliefs. We state now our first behavioral hypothesis for Experiment 2.

**Hypothesis 2.** *Subjects in the outgroup treatment are more likely than those in the ingroup treatment to select the best response to uniform randomization* (*BRUR*). *This corresponds to* '*U*' *in games* 1 *and* 4, '*D*' *in games* 2 *and* 3, *and* '*M*' *in game* 5.

In each Nash equilibrium, players receive the same payoff as each other, which eliminates fairness concerns, and as a one-shot simultaneous game, there is also no room for reciprocity. In A.2, we show that introducing altruism does not affect the equilibria of the game. However, more altruistic players face a higher (lower) cost of miscoordination after playing '*U*' ('*D*'). So if social preferences do play a role, we would expect subjects partnered with ingroup members to be more likely to choose the latter strategy.

The fact that the best response to uniform randomization (BRUR) of the other player differs between games allows us to control for this possible effect of altruism and other, unconsidered, effects by comparing outcomes in the different games *within* treatments, as follows.

**Hypothesis 3.** *The proportion of subjects who best respond to uniform randomization will be greater than the proportion of subjects who choose the same action* (*U*, *M*, *D*) *in a game where it is not the best response to uniform randomization. This effect should be greater in the outgroup treatment than the ingroup treatment.*

The intuition behind this hypothesis is straightforward and predicts, for instance in Game 1, that the proportion of participants who choose '*U*' (the BRUR action in this game) will be greater than the proportion who chose '*U*' in Games 2, 3, and 5 (where it is not the BRUR action). Note that we expect some subjects in the ingroup treatment will also BRUR, just fewer than in the outgroup treatment, which is why we specify that the magnitude of changes in proportions between games will differ between treatments. Finally, Hypothesis 4 looks for additional support of our explanation by considering elicited beliefs.

**Hypothesis 4.** *Subjects in the outgroup treatment report beliefs that are closer to uniform randomization than those in the ingroup treatment.*

[6] Marginal effects have been calculated taking into account the issues raised in Ai and Norton (2003) regarding interaction terms in non-linear models.

[7] Controlling for whether the decision is a subject's first, second, or third, shows that subjects are less likely to continue at later nodes, but does not affect the conclusions of Model 3 in Table 2.

| | L | C | R |
|---|---|---|---|
| U | 4, 4 | 4, 2 | 4, 0 |
| M | 2, 4 | 5, 5 | 2, 0 |
| D | 0, 4 | 0, 2 | 6, 6 |

Game 1

| | L | C | R |
|---|---|---|---|
| U | 2, 2 | 2, 1 | 2, 0 |
| M | 1, 2 | 6, 6 | 1, 0 |
| D | 0, 2 | 0, 1 | 10, 10 |

Game 2

| | L | C | R |
|---|---|---|---|
| U | 3, 3 | 3, 2 | 3, 0 |
| M | 2, 3 | 6, 6 | 2, 0 |
| D | 0, 3 | 0, 2 | 11, 11 |

Game 3

| | L | C | R |
|---|---|---|---|
| U | 3, 3 | 3, 2 | 3, 0 |
| M | 2, 3 | 4, 4 | 2, 0 |
| D | 0, 3 | 0, 2 | 7, 7 |

Game 4

| | L | C | R |
|---|---|---|---|
| U | 3, 3 | 3, 2 | 3, 0 |
| M | 2, 3 | 6, 6 | 2, 0 |
| D | 0, 3 | 0, 2 | 8, 8 |

Game 5

**Fig. 3.** Extended stag hunt games (Experiment 2).

### *3.1. Design*

For the design of Experiment 2, which consisted of 4 parts, we followed the same general procedure as in Experiment 1: a group identity task, decisions in a series of games, elicitation of beliefs regarding partner's behavior, and a post-experiment questionnaire.

*Part* 1: The group identity procedure was identical to the one used in part 1 of Experiment 1.

*Part* 2: Participants were asked to make decisions in a series of five extended stag hunt games, shown in Fig. 3. As in Experiment 1, they were reminded of which group they were part of ("You are in the Klee/Kandinsky group"), then informed that their decisions would be matched with the decisions of a member of a particular group ("Your decision will be matched with the decision of a randomly selected participant from the Klee/Kandinsky group") depending on the treatment. Participants were allocated to treatments randomly, with roughly half of the participants matched with a member of the same group (*ingroup* treatment) and the rest with a member of the other group (*outgroup* treatment).[8] As all five games are symmetric, every subject assumed the role of a row player. Moreover, the sequence of presentation of the five games was randomized.[9] Finally, participants were informed of the decisions of their partner, only after all decisions in the experiment had been made, and paid for one randomly chosen game.

*Part* 3: We elicited participants' beliefs about the behavior of their partner using a quadratic scoring rule (QSR). Subjects could manipulate two sliders to indicate the probability with which they believed their partner would choose '*L*' or '*C*' and a third slider indicating the probability assigned to '*R*' moved automatically so that the three numbers added to one. To avoid the difficulty of explaining the QSR, the payoffs a subject would earn, given their guesses and the three possible decisions of their parter, were displayed and updated as the sliders were moved. The actual belief elicitation was preceded by a tutorial which explained the mechanism and allowed subjects to familiarize themselves with the payoffs associated with different positions of the sliders. Beliefs were elicited about each of the five games, with one randomly chosen for payment. Again, the games were presented in a randomized order for each subject. As in Experiment 1, participants learned about the task only after making their own decisions so as not to influence behavior in the actual games. They were also informed that the game for which they were paid in part 2 would be different from that paid in part 3 to reduce the possibility of hedging.

*Part* 4: In the questionnaire, subject's were presented a series of 15 binary choices between a certain payoff (ranging from 0.5 Points to 7.5 Points in 0.5 steps) and a lottery that paid 10 Points with probability 0.5 and nothing otherwise. One of these choices was randomly chosen and paid accordingly. Then, participants completed a short post-experiment questionnaire regarding a number personality attitudes which also included questions about the group assignment.

*Procedures*: The experiment was conducted with z-Tree (Fischbacher, 2007) in the Vienna Center for Experimental Economics. Participants were student volunteers recruited from the universities in Vienna using ORSEE. Each subject participated in only one session. We conducted 5 sessions with between 22 and 28 participants per session (120 participants in total, 64 in the outgroup treatment and 56 in the ingroup treatment). Instructions for each part were presented on screen. At the end of a session, points earned across all experimental parts were added up and converted into Euros at an exchange rate of 1 Point = 0.5 Euro. In addition, each participant received a 3 Euro show-up fee. Sessions lasted about 45 minutes (including time for payment) and participants earned between 4 and 19.30 Euros (12.34 on average). Participants received their payments privately at the end of the experiment.

### 3.2. Results

We begin by reporting the distributions of actions, separated by treatment, for the first game played by each subject. In each of these games, shown in Fig. 4, the label of an action (Up, Middle, Down) is capitalized whenever it is a best response to uniform randomization (BRUR) of the opponent. The distributions of actions are also summarized in Table A4. The most commonly chosen action in the ingroup treatment in all games is Down. In contrast, the most commonly chosen action in the outgroup treatment is the BRUR action in four out of five games (the exception being Game 4). These two results are in line with both our main hypothesis, that subjects facing an outgroup partner are more likely to choose a BRUR action, and also previous results in coordination games (e.g. Chen and Chen, 2011) in the sense that increased altruism helps subjects in the ingroup coordinate on the most efficient equilibria.

Comparing the proportion of subjects choosing the BRUR actions across treatments and all five games, we find that participants in the outgroup treatment choose BRUR actions 56% of the time, which is significantly larger than the 41% of BRUR actions chosen by participants in the ingroup treatment (one-sided $z$ test, $z = 1.659$, $p$ value $= 0.048$). The results are clearly in support of Hypothesis 2.

Concerning Hypothesis 3, Tables A6 and A7 report the difference between the proportion of players choosing BRUR in a *given* game and the proportion of subjects who choose the same action in a *different* game where it is not the best response to uniform randomization of the opponent. Out of the 16 possible comparisons, 14 in the outgroup and 13 in the ingroup are in the hypothesized direction as can be seen by the (positive) signs of the differences in these tables. We find that in 14 out of these 16 possible comparisons, the magnitude of this change is greater in the outgroup than the ingroup treatment. Taking the average effect over all five games, a BRUR action is 36 percentage points more likely to be chosen than an equivalently labelled non-BRUR action in the outgroup treatment, compared to only a 6 percentage points change in the ingroup treatment.

To test this formally, we use a Fisher's exact test looking for a non-random relationship between actions and the games. This gives a $p$ value of 0.933 in the ingroup treatment, but a $p$ value of 0.001 in the outgroup treatment. Thus, we find strong

[8] The precise number depended on how many subjects were in each identity group. This design was necessary for matching purposes.

[9] We are concerned that subjects may have seriously considered only the first game they played, and seeing the similarity with the following games simply chosen the same option, resulting in order effects. In fact, 36% of subjects chose the same action for all five games. For comparability with the centipede game in Experiment 1, we only present in the result section the first game played (the order of games was randomized so the sample sizes are uneven).

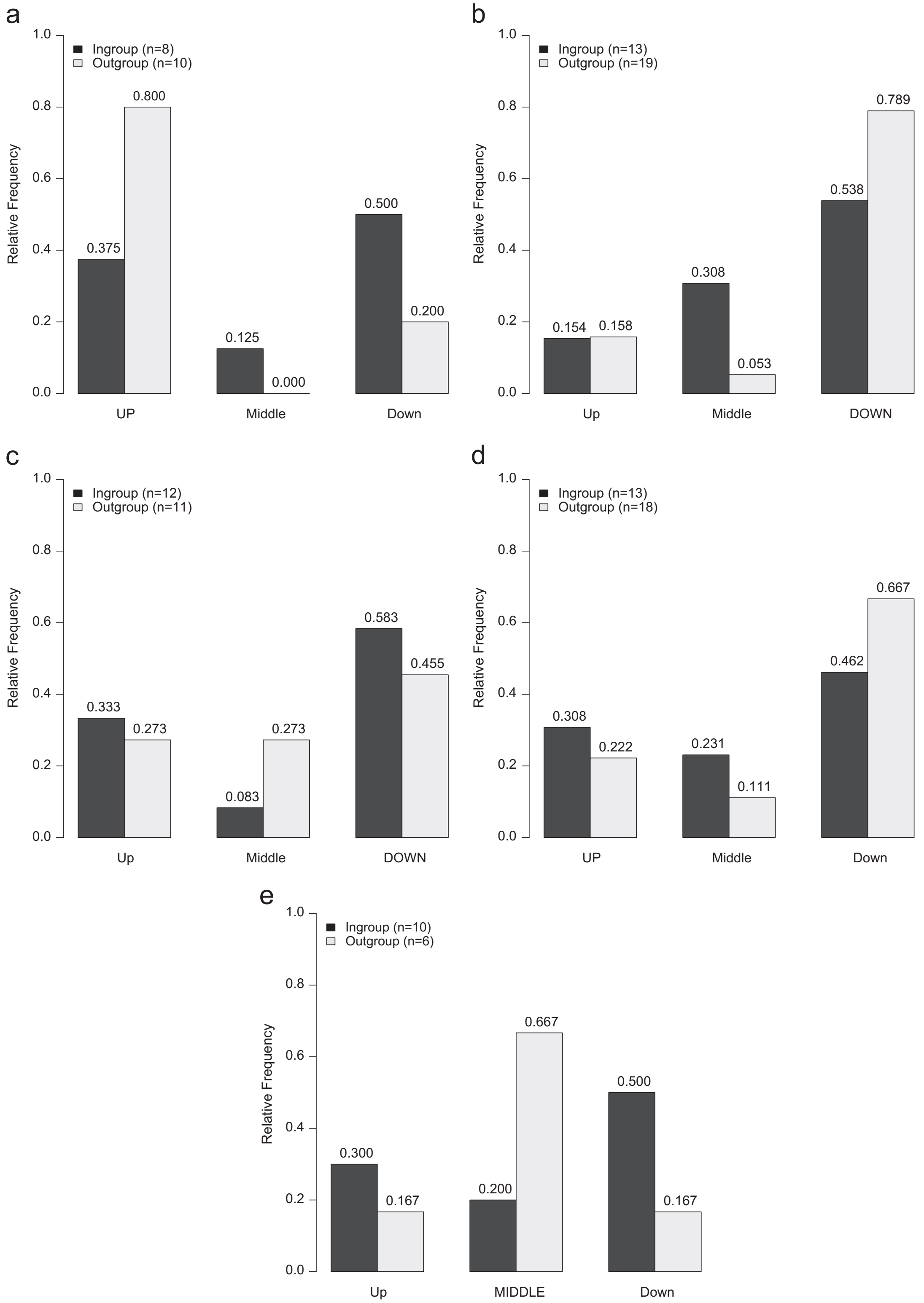


**Fig. 4.** Decisions in the extended stag hunt games (capitalized action in a game denotes BRUR). (a) Game 1, (b) Game 2, (c) Game 3, (d) Game 4, (e) Game 5.

evidence that the distribution of actions between games differs significantly in the outgroup treatment, but no such evidence for the ingroup treatment.

Turning to elicited beliefs, Table A5 shows the average probability placed on each of the three possible actions in each game for the ingroup and outgroup treatments respectively. To test whether subjects in the outgroup treatment have beliefs closer to uniform randomization we consider the Euclidian distance between the elicited beliefs and placing a probability of a third on each action. Two-sided Wilcoxon rank-sum tests, using pooled data across games, reveal no difference in the distributions of these distances between the ingroup and outgroup treatment ($z=0.156$, $p$ value$=0.88$).[10] Consistent with the results on actions, subjects in the ingroup treatment place on average the highest probability on the event that their partner chooses Down. Also, it appears that subjects in the outgroup treatment tend to place higher weight on BRUR actions. Indeed, the probability placed on the partner playing the BRUR action does differ significantly between treatments (two-sided Wilcoxon rank-sum, $p$ value$=0.01$). No treatment differences are found in the distributions of probabilities placed on '*D*' ($p$ value$=0.84$) or '*U*' ($p$ value$=0.40$).

To study the belief action relationship in a similar manner to our analysis of Experiment 1, we report probit regressions on the probability of choosing the BRUR action in Table 3. Model 1 supports the findings of the non-parametric tests, showing that outgroups are 15% more likely to chose the BRUR action. However, in stark contrast to Experiment 1, Model 2 shows that this difference is entirely accounted for by elicited beliefs: the coefficient on the belief a subject states about the probability their partner will play BRUR is highly predictive of the probability that they themselves will choose that action, while the ingroup dummy becomes close to zero. Introducing an interaction effect does not improve the fit (Model 3).

### 3.3. Discussion

The experimental data provide strong evidence that BRUR is more common in the outgroup treatment than in the ingroup treatment, confirming our main hypothesis regarding behavior. We do not find evidence that subjects in the outgroup treatment are more likely to report beliefs that their partner is playing a uniformly random strategy in the game (Hypothesis 3). Beliefs do differ by treatment, but such that subjects believe that outgroups are more likely to be playing the BRUR actions. In the next section we discuss the implications of these results in relation to our results from the first experiment.

## 4. General discussion

Taken together, the behavioral data (from Experiment 1 and 2) provides strong evidence that people interacting with outgroup members are more likely to behave as though their partner is uniformly randomising. Our findings are consistent with evidence in psychology that social projection, in the sense of perceived similarity and hence predictability of others, is stronger in ingroup than in outgroup interactions (Robbins and Krueger, 2005; Ames et al., 2011). Moreover, our results are also consistent with a recent neuroscientific study of Baumgartner et al. (2012), which identifies differences in the activation of the mentalising system (a region associated with social reasoning or projection in the brain) of subjects when facing ingroup and outgroup behavior.

The fact that subjects interacting with outgroup members are more likely to BRUR can have counter-intuitive effects if one is only considering social preferences. In our centipede game, increased BRUR in the outgroup treatment has the opposing effect on behavior to the one predicted by increased social preferences amongst ingroup members. In Experiment 1, the former effect outweighed the latter. This particular outcome, however, rests crucially on the specific payoff structure of the game. For example, Chen and Li (2009) report results on a two-person sequential game with the same structure as a two-legged centipede game Resp 8, Table A2, p.455 and find that ingroups continue longer than outgroups. However, given the payoff structure in their game, the BRUR action is to stop (because the potential payoff gains from continuing are small compared to our exponentially increasing payoffs), so both effects are working in the same direction.

Our stag hunt games are similar to the minimum effort game in Chen and Chen (2011). They find that ingroup pairs are able to coordinate on equilibria that are pareto superior to those chosen by outgroup pairs, a result they attribute to social preferences. We find evidence supporting this argument in that ingroup pairs in our experiment have, as their modal choice, the strategy that leads to the pareto-optimal equilibria in all five games. However, in one of our games (Game 2) where this strategy is also the BRUR action, the BRUR effect outweighs the social-preference effect and is selected by a higher proportion of players in the outgroup treatment than in the ingroup treatment. Thus, the effect we have identified is not disputing the well-established impact of group identity on social preferences. These two effects are rather complementary or countervailing, depending on the strategic environment and precise payoffs.

In the remainder, we consider alternative explanations for our treatment effects, both in actions and beliefs, in the two experiments considered in this paper. In particular, we consider the effects of social preferences, risk preferences, level-k reasoning, and biases of the belief elicitation mechanisms.

[10] As we used the quadratic scoring rule, risk aversion would bias responses towards placing one third on each action which could cause problems if subjects in one treatment happened to be more risk-averse than the other. However the number of safe choices in the certainty equivalent elicitation is almost identical between treatments (ingroup=8.25; outgroup=8.02; Wilcoxon rank-sum test: $p$ value=0.67).

**Table 3**
Probability of playing BRUR estimated by probit model (Experiment 2).

| | Model 1 | Model 2 | Model 3 |
|---|---|---|---|
| *Coefficients* | | | |
| Ingroup (=1) | −0.383* | −0.056 | −0.548 |
| | (0.231) | (0.292) | (0.574) |
| Belief | | 0.035*** | 0.030*** |
| | | (0.005) | (0.006) |
| Ingroup × belief | | | 0.015 |
| | | | (0.012) |
| Constant | 0.157 | −1.588*** | −1.353*** |
| | (0.157) | (0.326) | (0.364) |
| No. of observations | 120 | 120 | 120 |
| Log likelihood | −81.779 | −50.667 | −49.867 |
| Likelihood-ratio | 2.76 | 64.99 | 66.59 |
| ($\chi^2$, $p$ value) | 0.096 | $<0.001$ | $<0.001$ |
| Pseudo $R^2$ | 0.017 | 0.391 | 0.400 |
| *Marginal effects*[a] | | | |
| Ingroup effect | −0.152* | | |
| Ingroup effect (for belief=0) | | 0.007 | −0.059 |
| Ingroup effect (for belief=0.1) | | 0.011 | −0.073 |
| Ingroup effect (for belief=0.2) | | 0.016 | −0.068 |
| Ingroup effect (for belief=0.3) | | 0.020 | −0.035 |
| Ingroup effect (for belief=0.4) | | 0.022 | 0.020 |
| Ingroup effect (for belief=0.5) | | 0.022 | 0.077 |
| Ingroup effect (for belief=0.6) | | 0.019 | 0.113 |
| Ingroup effect (for belief=0.7) | | 0.015 | 0.119 |
| Ingroup effect (for belief=0.8) | | 0.010 | 0.100 |
| Ingroup effect (for belief=0.9) | | 0.006 | 0.071 |
| Ingroup effect (for belief=1) | | 0.003 | 0.043 |
| Belief effect (for ingroup=0, belief at mean) | | 0.008*** | 0.008*** |
| Belief effect (for ingroup=1, belief at mean) | | 0.008*** | 0.009*** |

*Notes*: This table presents coefficients and marginal effects of probit regressions. The dependent variable in all regressions is the probability of playing BRUR. Belief is the probability with which the subject believes their partner plays BRUR (mean=0.445).
***indicates significance at the 1% level, **significance at the 5% level, *significance at the 10% level
[a] Marginal effects reported for Model 3 take into account the interaction term.

The literature on social preferences suggests that subjects in the centipede game should continue longer with ingroup members, which is the opposite of what we find. It is highly unlikely that our group identity manipulation decreased altruism, positive reciprocity or social welfare concerns, as this would contradict a large body of literature. As shown in A.2, a simple model of social preference does not change the equilibria in the extended stag hunt games. Increased altruism does decrease the risk associated with choosing '*D*', which may explain why it is the modal choice in all five games in the ingroup treatment, i.e. social preferences may play a role in our data. However, social preference arguments cannot explain why the behavior of outgroups is so strongly related to the best response to uniform randomization, which changes between games.

Higher sensitivity to risk when interacting with ingroups in the centipede game could also explain stopping earlier. However, in the coordination games the modal choice of those in the ingroup treatment was the riskiest action ('*D*'). Similarly, a level-k explanation would involve subjects in the ingroup treatment behaving at a higher level in the centipede game but a lower level in three out of five of the coordination games.[11] Thus, neither risk nor level-k reasoning can simultaneously explain both sets of results.

We now turn to beliefs were we find inconsistent results across the two experiments. While in Experiment 1 there is no treatment effect, in Experiment 2 there is. Neither result shows subjects in the outgroup treatment being more likely to believe that their partner will uniformly randomize. However, there are a number of reasons to questioning the accuracy and relevance of our elicited beliefs. The review of Schotter and Trevino (2014) cites a number of papers which find that beliefs, elicited ex post, are good predictors of actions in games. We also find that beliefs are informative in Experiment 1, as shown by the significant coefficients in Table 2, and in Experiment 2 where they explain the whole treatment effect on actions in Table 3.[12] However, taking average values across papers reviewed by Schotter and Trevino (2014), only around 2/3 of subjects best-respond to their stated beliefs. It is largely unknown whether the remaining 1/3 of subjects' deviation from

[11] See Kawagoe and Takizawa (2012) for a proof that higher levels stop earlier in the centipede game. In the stag hunt games, with the typical assumption of level-0 players randomising uniformly, all levels greater than zero choose the best response to uniform randomization. In Games 1, 2, and 5, more players in the outgroup treatment choose this higher level action.

[12] Actions chosen first may influence beliefs elicited ex post. The evidence on this is ambiguous (see Schlag et al., 2014), so here we remain agnostic as to whether the actions were influenced by beliefs or vice-versa and simply note the strong relationship.

best-responding is due to noise, systematic biases in the elicited beliefs, or whether those participants are following a different decision theory altogether.

Rubinstein and Salant (2014) identify two biases, associated with ex post belief elicitation: self-similarity (subjects are being more likely to believe that partners choose the same action as oneself) and strategic justification (subjects try to ex post rationalize their actions in their belief statement). They find that asking for population frequencies promotes self-similarity bias, whereas asking for the probability that a particular subject will take a given action increases strategic-justification bias. We used population frequencies in Experiment 1 but asked for the probabilistic choice of an individual in Experiment 2. In the light of Rubinstein and Salant (2014), it is possible that the biases they identify, particularly self-similarity bias, which would be strengthened for ingroup members (because of social projection), may have contributed to the contradictory results on stated beliefs across experiments. This supposition is supported by Bernold et al. (2014) who find a weaker correlation between actions and beliefs in a Prisoners' dilemma game when framed as a Wall Street game as opposed to a Community game, where participants presumably identify more with each other in the latter.

Besides the different belief elicitation mechanisms between the two experiments, our experimental games differed in that the first experiment involved asymmetric games, whereas in the second they were symmetric. Self-similarity bias, which is likely to be affected by group identity, will probably play a stronger role in symmetric games, where one can simply assume that the other person is reasoning in an identical manner to oneself.

A more straightforward explanation for the lack of coherence between our findings on actions and elicited beliefs is that beliefs elicited ex post are not fundamental to the original decision making process. This is the conclusion of Costa-Gomes and Weizsäcker (2008) who find that the beliefs that determine actions were drawn from a different distribution than elicited beliefs.[13]

Initially, we found interesting differences in the strength of relationship between beliefs and actions across treatments in the centipede game. We speculated that rather than operating through beliefs, the differences in actions we observed were due to a difference in the way beliefs derived from introspection were translated into actions when interacting with outgroups or ingroups. However, given the contrasting results in the follow-up experiment, it is conceivable that the differences we observed are more likely to be caused by systematic bias in the elicited beliefs.

All in all, there are two possible explanations for the behavior we observed in our experiments. One possibility is that outgroup players are believed to uniformly randomize because they are perceived as less sophisticated than ingroup members. This argument is in line with the idea that group affiliation is ultimately aimed at increasing one's self-esteem. In line with this interpretation, Agranov et al. (2012) for instance show that the perception of the other person's strategic sophistication (through framing) affects own behavior in the beauty contest game. The other possibility is that participants find it more difficult to make predictions about behavior of outgroups, leading them to apply the principle of insufficient reason, and attribute equal probability to each choice of their outgroup partner. Both of these mechanisms result in the same observed choices of players. Therefore, without more reliable data on elicited beliefs, we cannot identify the mechanism behind our behavioral results.

## 5. Conclusion

In this paper we have identified a channel through which group identity can affect behavior in strategic environments in addition to the well-established influence through social preferences. Subjects interacting with outgroup members are more likely than those interacting with ingroup members to choose a best response to the belief that their partner is uniformly randomising amongst their available strategies. This result could have one of two equally reasonable and intuitive explanations: people may regard those from other groups as less strategically sophisticated, a phenomena not uncommon in the real world; alternatively, if one perceives another to be fundamentally different from oneself, it should be harder to make predictions about their behavior because of the uncertainty underlying their strategic reasoning.

We also find some interesting results regarding elicited beliefs, which we hope will spur further research into improving belief elicitation methods, and understanding the complex relationship between beliefs and actions in strategic interactions.

## Acknowledgments

We are grateful to the associate editor, three anonymous referees, Carlos Alós-Ferrer, Tore Ellingsen, Guido Friebel, Lorenz Götte, Charles Holt, Magnus Johannesson, Toshiji Kawagoe, Michael Kosfeld, Rachel E. Kranton, Hirokazu Takizawa, Marie Claire Villeval, and Irenaeus Wolff for helpful comments and discussions. In particular we thank Yan Chen and Sherry Li for providing us with the original z-tree code used in Chen and Li (2009). We also thank seminar participants at the University of Canterbury, University of Cologne, University of Frankfurt, University of Geneva, University of Heidelberg, University of Munich, Stockholm School of Economics, as well as conference participants at the IMEBE in Castellón, the THEEM in Kreuzlingen, the UECE Meeting in Lisbon, the 4th World Congress of the Game Theory Society in Istanbul, and the BABEE Workshop in San Francisco for useful comments. James Tremewan gratefully acknowledges financial support from the

[13] It is noteworthy that the strongest relationship between actions and beliefs tend to involve experiments where the game and belief elicitation are repeated many times, with the belief elicitation occurring immediately after each stage game. Our designs are closer to Costa-Gomes and Weizsäcker (2008) in that subjects first play a number of different games, then all beliefs are elicited subsequently.

Heinrich Graf Hardegg'sche Stiftung. An earlier version of this paper was titled "Social preferences and bounded rationality in the centipede game".

## Appendix A. Experiment 2 (stag hunt games): additional results

### A.1. Distribution of actions and beliefs in stag hunt games

**Table A4**
Distribution of actions (BRUR actions in bold face).

| Game | Ingroup | | | Outgroup | | |
|---|---|---|---|---|---|---|
| | *U* | *M* | *D* | *U* | *M* | *D* |
| 1 | **3** | 1 | 4 | **8** | 0 | 2 |
| 2 | 2 | 4 | **7** | 3 | 1 | **15** |
| 3 | 4 | 1 | **7** | 3 | 3 | **5** |
| 4 | **4** | 3 | 6 | **4** | 2 | 12 |
| 5 | 3 | **2** | 5 | 1 | **4** | 1 |

**Table A5**
Distribution of beliefs given in probabilities (BRUR actions in bold face).

| Game | Ingroup | | | Outgroup | | |
|---|---|---|---|---|---|---|
| | *U* | *M* | *D* | *U* | *M* | *D* |
| 1 | **30** | 24 | 47 | **60** | 18 | 22 |
| 2 | 34 | 24 | **42** | 14 | 16 | **70** |
| 3 | 26 | 21 | **53** | 20 | 32 | **48** |
| 4 | **22** | 18 | 61 | **38** | 15 | 47 |
| 5 | 17 | **33** | 49 | 30 | **29** | 41 |

**Table A6**
Changes in BRUR to non-BRUR behavior across games in ingroup treatment.

| Game | 1 | 2 | 3 | 4 | 5 |
|---|---|---|---|---|---|
| 1 | – | 0.22 | 0.04 | n.a. | 0.08 |
| 2 | 0.04 | – | n.a. | 0.08 | 0.04 |
| 3 | 0.08 | n.a. | – | 0.12 | 0.08 |
| 4 | n.a. | 0.15 | −0.03 | – | 0.01 |
| 5 | 0.08 | −0.11 | 0.12 | −0.03 | – |

**Table A7**
Changes in BRUR to non-BRUR behavior across games in outgroup treatment.

| Game | 1 | 2 | 3 | 4 | 5 |
|---|---|---|---|---|---|
| 1 | – | 0.64 | 0.53 | n.a. | 0.63 |
| 2 | 0.59 | – | n.a. | 0.12 | 0.62 |
| 3 | 0.26 | n.a. | – | −0.21 | 0.29 |
| 4 | n.a. | 0.06 | −0.05 | – | 0.06 |
| 5 | 0.67 | 0.61 | 0.39 | 0.56 | – |

| | $L$ | $C$ | $R$ |
|---|---|---|---|
| U | $a,a$ | $a,b$ | $a,0$ |
| M | $b,a$ | $c,c$ | $b,0$ |
| D | $0,a$ | $0,b$ | $d,d$ |

**Fig. A5.** General game.

| | $L$ | $C$ | $R$ |
|---|---|---|---|
| $U$ | $a, a$ | $a-(a-b)\alpha, b+(a-b)\alpha$ | $a-a\alpha, a\alpha$ |
| $M$ | $b+(a-b)\alpha, a-(a-b)\alpha$ | $c, c$ | $b-b\alpha, b\alpha$ |
| $D$ | $a\alpha, a-a\alpha$ | $b\alpha, b-b\alpha$ | $d, d$ |

**Fig. A6.** Game with altruism.

### A.2. *Effect of altruism in the extended stag hunt game*

Fig. A5 shows the payoff structure of the extended stag hunt games, with restrictions on the parameters $b<a<c<d$. Fig. A6 shows the same game assuming the utility function $U_i(x_i, x_j) = (1-\alpha)x_i + \alpha x_j$ where $\alpha \in [0, 0.5]$ represents the degree of altruism player $i$ feels towards player $j$. Given the parametric restrictions, it is easy to see that the Nash equilibria are unchanged for any degree of altruism. It can also be seen, however, that the cost of miscoordination after choosing $U$ is increasing in $\alpha$, whereas the cost of miscoordination after choosing $D$ is decreasing in $\alpha$ (the cost after $M$ depends on parameters). So although there is no change in equilibria, it is possible that participants, who feel more altruistic towards their partner, are less likely to choose $U$ and more likely to choose $D$.

## Appendix B. Supplementary data

Supplementary data associated with this paper can be found in the online version at http://dx.doi.org/10.1016/j.euroecorev.2015.03.001.

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