Group Identity and Social Preferences

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Abstract

Identity is a central concept in the social sciences. In this study, we present a laboratory experiment that measures the effects of induced group identity on participant social preferences. We find that when participants are matched with an ingroup member (as opposed to an outgroup member) they show a 47% increase in charity concerns when they have a higher payoff and a 93% decrease in envy when they have a lower payoff. Likewise, participants are 22% more likely to reward an ingroup match for good behavior, but 13% less likely to punish an ingroup match for misbehavior. Furthermore, participants are significantly more likely to choose social-welfare-maximizing actions when matched with an ingroup member. All results are consistent with the hypothesis that participants are more altruistic towards an ingroup match. As a result, ingroup matching generates significantly higher expected earnings than outgroup matching.

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JEL Classification: C7, C91

1 Introduction

Social identity is commonly defined as a person's sense of self derived from perceived membership in social groups. When we belong to a group, we are likely to derive our sense of identity, at least in part, from that group. While standard economic analysis focuses on individual-level incentives in decision-making, group identity has been shown to be a central concept in understanding phenomena in social psychology, sociology, anthropology and political science. It is used to explain such phenomena as ethnic and racial conflicts, discrimination, political campaigns (McDermott forthcoming), and education (Coleman 1961).

Social identity theory was developed by Tajfel and Turner (1979) to understand the psychological basis for intergroup discrimination. According to this theory, social identity has three major components: categorization, identification and comparison. The first component, categorization, is the process of putting people, including ourselves, into categories. Labelling someone as a Muslim, a female, or a soldier are ways of defining these people. Similarly, our self-image is associated with what categories we belong to. Social psychology experiments show that people quickly and easily put themselves and others into basic categories. The second component, identification, is the process by which we associate ourselves with certain groups. Ingroups are groups we identify with, and outgroups are ones that we don't identify with. The third component, comparison, is the process by which we compare our groups with other groups, creating a favorable bias toward the group to which we belong.

One insight from social identity theory is that the groups to which people belong mean something to them. Once a person sees herself as part of a group, she derives self-esteem from that group membership (McDermott forthcoming). To explore this concept, Shih, Pittinsky and Ambady (1999) study social identity and stereotype susceptibility with a group of Asian-American female undergraduates given a math test under three conditions. A third of the students completed a questionnaire focused on their female identity before taking the test. Another third completed a pre-test questionnaire that focused on their Asian identity. The control group filled out a gender- and ethnicity-neutral questionnaire. Results show that, relative to controls, participants earned the highest test scores when the questionnaire emphasized their Asian identity and the lowest when it emphasized their female identity. Shih et al. (1999) conclude that the questionnaire, which made one of their multidimensional social identities salient, changed the women's performance according to powerful stereotypes associated with each identity, i.e., Asians possess excellent quantitative skills and women do not.

As group identity affects individual behavior, many experiments in social psychology assess whether and to what extent people interact with ingroup and outgroup members differently. Most of these experiments confirm Tajfel's finding that group membership creates ingroup enhancement in ways that favor the ingroup at the expense of the outgroup.[1] Many of these experiments use the minimal group paradigm. In a typical minimal group experiment, subjects are randomly assigned to groups, which are intended to be as meaningless as possible. The subjects then assign points to anonymous members of both their own group and the other group. In these studies, subjects tend to award more points to people who are identified as ingroup members. Experiments involving ratings of ingroup and outgroup members have found that participants tend to rate ingroup members higher than outgroup members.

The systematic introduction of identity into economic analysis starts with Akerlof and Kranton

[1] We review this literature in more detail in Section 2.

(2000). In their study, they propose a neoclassical utility function, where identity is associated with different social categories and expected respective behaviors, i.e., a prescription or norm for behavior. Deviations from the prescription cause disutility. They apply this model to different analyses of gender discrimination, the economics of poverty and social exclusion, the household division of labor (Akerlof and Kranton 2000), the economics of education (Akerlof and Kranton 2002) and contract theory (Akerlof and Kranton 2005). More recently, Benabou and Tirole (2006) present a complementary theoretical framework, which emphasizes the management of beliefs and the cognitive mechanisms leading to identity investments.

To endogenize the social norm in the Akerlof and Kranton (2000) model, and to understand the role of social identity in determining behaviors such as reciprocity, distribution and social-welfare-maximizing actions, it is crucial to systematically measure the effect of identity on social preferences.

In this paper, we use laboratory experiments to measure the effects of group identity on participant social preferences. Like classical social psychology experiments (Tajfel, Billig, Bundy and Flament 1971), we induce group identity using participant painting preferences. Furthermore, we compare different group assignment methods and their effects on behavior. However, unlike social psychology experiments, which focus on allocation between other participants, we use a much wider class of games to systematically measure the effects of identity on various aspects of social preferences, such as distribution and reciprocity preferences. As group behavior is predominantly investigated in other-other allocation tasks in the social psychology literature, little is known about whether and when it is sustained when there is a conflict with self-interest. We choose a sample of simple games from Charness and Rabin (2002), incorporate social identity into the social preference model, and estimate its effects on social preferences.

Specifically, we are interested in several questions. First, are participants more difference averse toward ingroup members than outgroup members? If so, to what extent? Second, are participants more likely to reciprocate positively towards ingroup members? Are they more likely to forgive or to punish perceived bad intentions of ingroup members? Third, are they more likely to choose social-welfare-maximizing actions when matched with an ingroup member compared to when matched with an outgroup member? Lastly, by varying the experimental design, we can address the question of what creates group effects.

In this study, we find that participants matched with an ingroup member show more charity when their payoffs are higher, and less envy when they are behind in earnings. Furthermore, other things equal, participants are more likely to reciprocate positively to ingroup than to outgroup members. They are more forgiving towards bad behaviors from ingroup matches compared to outgroup matches. Furthermore, participants are significantly more likely to choose social-welfare-maximizing (hereafter shortened as SWM) actions when matched with an ingroup member. As a result, expected earnings are significantly higher when participants are matched with an ingroup rather than an outgroup member.

This paper is organized as follows. Section 2 reviews the social psychology and experimental economics literature on social identity. Section 3 presents the experimental design. Section 4 presents the model and hypotheses. Section 5 presents the analysis and main results. Section 6 discusses methodological issues in inducing group identity. Section 7 concludes.

2 Literature Review

In this section, we review the social identity literature in social psychology and experimental economics respectively.

2.1 Social Identity Research in Social Psychology

The social psychology literature on social identity is enormous. In summarizing its main methodologies and findings, we rely primarily on several major survey articles, Tajfel and Turner (1986), Deaux (1996), Hogg (2003), McDermott (forthcoming), and a number of recent studies not yet incorporated into a major survey.

Two major experimental methods in social identity research have been used extensively in social psychology, (1) priming natural social identities, and (2) inducing (artificial) group identities. We will briefly summarize the former, and focus our discussion on the latter, as it is more closely related to our research.

Priming is an experimental technique by which a stimulus, such as a list words, a questionnaire, posters, or an article, is used to sensitize the subject to a later presentation of the similar stimulus. Research in social psychology has found that subtly making different natural social identities salient through priming can impact behavior and outcomes, such as test performance (Aronson, Quinn and Spencer 1998), walking speed (Bargh, Chen and Burrows 1996) or person perception (Bargh and Pietromonaco 1982). In addition, how an identity is made salient also impacts the directions of the outcomes (Shih, Ambady, Richeson, Fujita and Gray 2002). For instance, activating a social category tends to elicit outcomes consistent with the stereotypes (i.e. assimilation effects) while activating an exemplar of the social category tends to elicit stereotype inconsistent outcomes (i.e. contrast effects) (Dijksterhuis, Spears, Postmes, Stapel, Koomen, van Knippenberg and Scheepers 1998).

A second experimental method in social identity research relies on induced group identities in the laboratory. In particular, the experimental method designed to test social identity theory (Tajfel and Turner 1979) is called the minimal group paradigm, whereby groups are created using trivial and sometimes almost meaningless tasks. The criteria for a group to be minimal (Tajfel and Turner 1986) include:

1. Group assignment rule: subjects are randomly assigned to non-overlapping groups on the basis of some trivial tasks.

2. No social interaction takes place between the subjects, where social interactions include both face-to-face and technology-mediated interactions, such as online chat.

3. Group membership is anonymous.

4. The decision task requires no link between a chooser's self interest and her choices. Two tasks are common in measuring ingroup bias. In the first task, each subject awards amounts of money to pairs of other subjects who are anonymous except for their group membership. Another frequently used task is evaluative ratings of other subjects (Mullen, Brown and Smith 1992).

Of the four criteria for groups to be minimal, the fourth one is the least likely to be satisfied in economics environments, where many decisions involve tradeoffs between self-interest and group interest.

Summarizing fifteen years[2] of social identity research using the minimal group paradigm, Tajfel and Turner (1986) conclude that "the trivial, ad hoc intergroup categorization leads to ingroup favoritism and discrimination against the outgroup." Several factors have been found to enhance or mitigate ingroup bias , for example, category salience, group status, and relevance of the comparison dimensions (Mullen et al. 1992). Furthermore, summarizing forty years of social psychological research on intergroup relations, Brewer (1999) concludes that ingroup formation and attachment is psychologically primary while attitudes toward outgroups is not.

More recently, however, a number of studies do not find ingroup favoritism with minimal groups. A common feature of these studies is that they violate the fourth criterion for groups to be minimal. For example, Yamagishi and Kiyonari (2000) find that, in a modified Prisoner's Dilemma game with a large number of strategies, while players cooperate more with an ingroup member than with an outgroup member in the simultaneous move game, the group effect disappears in the sequential game (where all players were first movers).[3] They argue that expectations from generalized reciprocity from ingroup members (in the simultaneous move game) is the source of ingroup favoritism in a minimal group. By contrast, in a sequential game when direct reciprocity is possible, group effects are eliminated. In a follow-up study using the Prisoner's Dilemma with binary choices, Simpson (2006) replicate the Yamagishi and Kiyonari (2000) result that first movers in the sequential treatment are equally likely to cooperate with ingroup and outgroup members. Furthermore, he finds that, given cooperation by a first mover,[4] second movers are more likely to cooperate with ingroup than outgroup members. However, he fails to replicate the group effects for the simultaneous treatments.

To our best knowledge, there is no definitive answer to the question of what generates group effects in the social psychology literature. Two competing hypotheses are pure categorization alone (Tajfel and Turner 1986) and expectations of generalized reciprocity among ingroup members (e.g. Yamagishi and Kiyonari (2000)). Some other fundamental questions remain open. For example, does social identity change behavior by influencing agent's expectations about fellow ingroup members' behavior or by changing the agent's preferences? If the latter, what functional forms or basic axioms best explain the data? Candidate behavioral principles include maximizing the average payoffs of ingroup members (Brewer and Silver 2000), maximizing inter-group differences (Tajfel and Turner 1986), and the metacontrast principle (Turner 1985), i.e., maximizing inter-group differences and minimizing intragroup differences. By using one game in any given study as has been typical in social psychology studies of social identity, it is unlikely for the researchers to obtain a robust estimate of functional forms across a wide variety of situations and games.

By contrast, our study use 24 games incorporating a wide variety of incentives, which enables us to get a robust estimate of the functional forms and parameters.

[2]Tajfel et al. (1971) is considered the first experiment using the minimal group paradigm.

[3]There were no second movers in the sequential treatment, although the participants were led to believe that they existed. Every first mover was paid as if the second mover had given the same amount as the first mover.

[4]In this treatment, all players were in fact second movers, and were led to believe that first movers existed and cooperated.

2.2 Social Identity Research in Experimental Economics

There have been a number of economic experiments on group identity, using either natural or induced identities.

In economic experiments that primes natural identities, gender and ethnicity in particular, the results are mixed. On the one hand, Brown-Kruse and Hummels (1993) and Cadsby and Maynes (1998) use a pre-game questionnaire to prime gender identity and find that gender does not have a significant effect on participant contributions in a voluntary contribution public goods experiment. However, on the other hand, Solow and Kirkwood (2002) and Croson, Marks and Snyder (2003) find that the effect of gender on levels of contribution is significant. Interestingly, Croson et al. (2003) find that, in a threshold public goods game with multiple equilibria, coordination and group efficiency increase among women who interact with members of a naturally occurring group, while the effects are opposite for men.

In experiments using race or ethnicity as the natural identity, results are mixed as well. For example, Glaeser, Laibson, Scheinkman and Soutter (2000) combine two experiments and a survey to measure levels of trust and trustworthiness. They find that in trust games, subjects who are paired with a partner of a different race or nationality send back less money to their partner. This finding supports the idea that trustworthiness declines across the line of races or nationalities. In another study, Fershtman and Gneezy (2001) use various games to study different aspects of ethnic discrimination. They find a systematic mistrust of men of Eastern origin in Israeli Jewish participants, and identify mistaken ethnic stereotypes (as opposed to a "taste of discrimination") as the source of mistrust. Bouckaert and Dhaene (2004) adopt a similar approach as in Fershtman and Gneezy (2001) but use male small businessmen of distinct ethnic origins to investigate inter-ethnic trust and reciprocity in Belgium. They find that trust and reciprocity on average are independent of the ethnic origin of either participant or the opposite party. They argue that ethnic biases vanish when the two parties of different ethnic origins share enough other characteristics such as gender, socio-professional status and place of residence.

Two recent studies using natural groups find significant effects of group identity on behavior. Bernhard, Fehr and Fischbacher (2006) use a dictator games experiment with third-party punishment in two distinct, native social groups in Papua New Guinea. They find that third parties show stronger altruism towards ingroup victims and give ingroup norm violators more lenient judgments. Relevant to our study, dictators in their study are seen as upholding social norms when they transfer money to ingroup members. Therefore, ingroup favoritism is a strong force in altruistic norm enforcement and sharing decisions. Goette, Huffman and Meier (2006) examine the effects of group membership in a Prisoner's Dilemma game using natural groups (platoons) in the Swiss Army. They find more cooperation when subjects interact with ingroup members. In a second experiment similar to Bernhard et al. (2006), they also find that third-party punishment is stronger when a violation affects an ingroup member as opposed to an outgroup member.

In addition to experiments designed to study the effects of natural identities on decision making, there is a large related literature on gender and economic decision making. We refer the readers to the surveys of Croson and Gneezy (2004) and Eckel and Grossman (Forthcoming) for a detailed description of the main results. Another related body of literature examines the economic consequences of diversity. This literature is based on the idea that an ethnically-mixed community faces the trade-off between the benefits of diversity and the costs of preference conflicts. Alesina and Ferrara (2005) review the main contributions in this area and document the effect of ethnic heterogeneity on economic growth, public goods provision, community formation and social capital.

One potential problem with using natural identities in experiments is that an individual's natural identity is associated with multiple social categories. Therefore, a participant in various situations might identify with different groups depending on which categories are most salient. For example, a participant might be Asian, female, an engineering student, and a lesbian. Because of this potential ambiguity, behavior might be sensitive to the particular priming method used. In comparison, using induced identities could give the experimenter more control over the identity formation process as well as the strength of participant identities.

The extent to which induced identity affects behavior depends on the strength of the social identity. Eckel and Grossman (2005) use induced team identity to study the effects of varying identity strength on cooperative behavior in a repeated-play public goods game in the laboratory. In one of the weak identity treatments satisfying criteria 1 - 3 of the minimal group paradigm, subjects are randomly assigned to one of the teams, identified only by the group color. They find that "just being identified with a team is, alone, insufficient to overcome self-interest." However, actions designed to enhance team identity, such as group problem solving, contribute to higher levels of team cooperation. Their finding suggests that high degrees of team identification may limit individual shirking and free-riding in environments with a public good. Charness, Rigotti and Rustichini (2006) report a series of experiments on the effects of group membership on individual behavior in Prisoners' Dilemma and Battle of the Sexes games. In the Tajfel-style minimal group treatment satisfying criteria 1-3, they find no statistical difference in the rate of cooperation with ingroup and outgroup members. In other treatments where groups are more salient, the authors find that group membership significantly affects individual behavior. McLeish and Oxoby (2007) study the effects of group identity in simple bargaining games using induced identity. They find that negative outgroup opinion can reinforce ingroup identity, making ingroup members more cooperative with each other. Furthermore, in response to unfair offers, responders engage in greater punishment towards ingroup members.

While previous experiments have demonstrated when and to what extent social identity affects individual behavior in various types of games, none of them systematically estimates its effects on social preferences. This study contributes to the literature by investigating the role of identity in diverse situations. We do this by using a wide variety of games and by systematically investigating the effects of various components in induced group identities.

3 Experimental Design

Our experimental design addresses the following objectives: to determine the effects of group identity on various aspects of participant social preferences and to evaluate the effect of group identity on social welfare. On the methodological front, we evaluate different ways of creating group identity in the laboratory, to explore the formation of groups and to investigate the foundation of what group identity is.

A key design choice for the experiment is whether to use participants' natural identities, such as race and gender, or to induce their identities in the laboratory. As explained in the previous section, both approaches have been used in the lab. However, because of the multi-dimensionality of natural identities and the resulting potential ambiguous effects in the laboratory, we use induced identity, which gives the experimenter greater control over the participant's guiding identity.

The experiments have five treatments and one control. In the treatment sessions, there are four stages. The first stage is a group assignment stage. The second stage is a collective problem solving stage using an online chat program. The third stage is an other-other allocation stage, where each participant allocates tokens to two other participants. The fourth stage is a set of two-person sequential games. While subjects in different treatments participated in different stages, subjects in the control sessions participated only in the fourth stage.

3.1 Stage 1: Group Assignment

All five experimental treatments contains the Group Assignment stage, where we explore two different group assignment methods. In our **Original** treatment, subjects reviewed five pairs of paintings by two modern artists, Klee and Kandinsky,[5] with one painting within each pair by Klee, and the other by Kandinsky. Without being told the artist of each painting, participants reported independently which painting in each pair they preferred. Based on their reported painting preferences, subjects were divided into two groups, the Klee group and the Kandinsky group. Subjects were privately informed about their group membership and the number of people in their group. Groups remained the same throughout the experiment.

Note that this group assignment process differs from that of the minimal group paradigm experiments in social psychology, where group assignment is purely random even though participants are led to believe it is based on their true painting preferences.[6] The use of true preferences might cause two potential problems, unbalanced group size[7] and correlation between painting and social preferences. To investigate whether they affect decision making in some systematic manner, we perform two types of analysis. First, we examine the correlations between group size and individual choices (and subsequent earnings), as well as the correlation between individual painting preferences and individual choice. We find no significant correlation in either case. Furthermore, we find that individual painting preferences are not correlated with demographics. Second, we include the group size and/or individual painting preferences as additional covariates in all relevant regression analyses in Section 5 and find that neither coefficient is different from zero statistically or economically. Inclusion of these variables causes very little change in coefficient estimates of all other variables. Therefore, we conclude that unequal group size does not add confounding effects to the impact of a sole differentiation between ingroup and outgroup. Our design of grouping participants based on true painting preferences does not affect our estimates of group identity on social preferences.

To experimentally evaluate the difference between our group assignment based on true painting preferences and random assignment, we used two treatments with random assignment, i.e., **RandomWithin** and **RandomBetween**. In both treatments, at the beginning of the experiment, each participant randomly drew one from a stack of envelopes, each of which contained either a Maize or a Blue slip, which determined whether they were assigned to the Maize group or the Blue group. The only difference between the two random treatments is in Stage 4. In the Ran-

[5]Wassily Kandinsky, (1866-1944), a Russian-born artist, was one of the first creators of pure abstraction in modern painting. Kandinsky, an accomplished musician, is known for his attempts to capture music in his paintings. Paul Klee (1879-1940) was born in Switzerland and lived in Germany and Switzerland. Klee refused to draw hard distinctions between art and writing and often incorporated letters and numerals into his paintings, but he also produced series of works that explore mosaic and other effects. His late works, characterized by heavy black lines, are often reflections on death and war (see, e.g., Selz (1957)). Their paintings were used by classic studies of social identity in social psychology (Tajfel et al. 1971).

[6]Exceptions include Yamagishi and Kiyonari (2000) and Simpson (2006).

[7]In our sample, 40% of the participants prefer Klee while 60% prefer Kandinsky.

domWithin treatment, participants made decisions for both ingroup and outgroup matches. In the RandomBetween treatment, however, in two of the four sessions, participants were matched with only ingroup members and made one (ingroup) decision in each game. Similarly, in the remaining two sessions, they were matched only with outgroup members and made one (outgroup) decision in each game. A comparison of the RandomWithin and the Original treatments allows us to explore any difference between random assignment versus group assignment based on true painting preferences, while a comparison of the RandomWithin and RandomBetween treatments allows us to evaluate any experimenter demand effects in the Original within-subject design. The latter also allows us to evaluate the importance of the presence of an outgroup in creating the group effects.

At the end of the first stage, after being categorized into two groups, subjects in the Original treatment were given the answer key[8] to the artists. In the RandomWithin and RandomBetween treatments, participants were given the 5 pairs of paintings along with the answer keys. They had 5 minutes to study these paintings to prepare them for the second stage.

3.2 Stage 2: Online Chat

After being assigned into groups, subjects in the original and the two random treatments subsequently participated in a second task that involved group communication via a chat program on computers. The task was to answer two questions on which artist made each of two additional paintings.[9] Given ten minutes, subjects voluntarily exchanged information with own-group members via a chat program to help one another obtain correct answers. Separate chat channels were used so information could be shared only within a group. Though any information was allowed during chatting, conversations focused mainly on the paintings. Experimenters monitored the chat process from the server and log files were saved subsequently. Everyone was free to submit answers individually after the chat. One hundred tokens were rewarded to each participant for each correct answer.[10] This part of the design is used to enhance group identity.

Since the online chat might have created a feeling of generalized reciprocity towards ingroup members, to disentangle the effects of reciprocity and categorization, we added a **NoChat** treatment, where the online chat stage was taken out while every other aspect of the design remained the same as the Original treatment. A comparison of the NoChat and Original treatments allows us to identify the effects of the online chat component on behavior.

3.3 Stage 3: Other-Other Allocation

In the third stage of the Original, RandomWithin, RandomBetween and NoChat treatment sessions, every subject was asked to allocate a given number of tokens between two other anonymous participants. No one was allowed to allocate tokens to herself. This feature of the experimental design is used widely in the minimal group paradigm in social psychology. Psychologists consistently find ingroup favoritism and outgroup discrimination in other-other allocations, i.e., individuals allocate

[8]The five pairs of paintings are: 1A *Gebirgsbildung*, 1924, by Klee; 1B *Subdued Glow*, 1928, by Kandinsky; 2A *Dreamy Improvisation*, 1913, by Kandinsky; 2B *Warning of the Ships*, 1917, by Klee; 3A *Dry-Cool Garden*, 1921, by Klee; 3B *Landscape with Red Splashes I*, 1913, by Kandinsky; 4A *Gentle Ascent*, 1934, by Kandinsky; 4B *A Hoffmannesque Tale*, 1921, by Klee; 5A *Development in Brown*, 1933, by Kandinsky; 5B *The Vase*, 1938, by Klee.

[9]Painting #6 is *Monument in Fertile Country*, 1929, by Klee, and Painting #7 is *Start*, 1928, by Kandinsky.

[10]77% of the participants provided correct answers to both paintings, 19% provided one correct answer. Only 4% provided zero correct answer.

significantly more rewards to those from their own group and less to those from a different group. We adopt this design feature for two purposes: to replicate the findings in the social psychology literature and to enhance group identity further. Turner (1978) finds that this other-other allocation procedure, if followed by self-other allocation, can help enhance the sense of group identity.[11]

In our study, the stage of other-other allocations had five rounds. From round 1 to round 5, the total number of tokens to be allocated increased from 200 to 400 with an increment of 50 tokens in each round. We used the strategy method to elicit participant strategy profiles.[12] During each round, everyone decided how to allocate tokens between another two people under three scenarios, if both of them came from her own group, if both came from the other group, and if one came from her own group and the other from a different group. It was public information that only one round of their decisions would be randomly selected by the computer to compute payoffs. At the end of the second stage, a random sequence of ID numbers was generated by the computer to decide who allocated tokens to whom. Everyone allocated tokens between the two participants whose IDs directly followed hers in the sequence. Therefore, one's payoff in this stage was the sum of the tokens allocated to her by the two people whose IDs preceded hers in the random sequence.

Again, to understand the effects of other-other allocation on the strength of group identity, we added a **NoHelp** treatment, where both the online chat and the other-other allocation stages were taken out. Therefore, a comparison of the NoHelp and the NoChat treatments allows us to identify the effects of other-other allocation.

3.4 Stage 4: Two-Person Sequential Games

While the first three stages are designed to induce and enhance group identity, we use the fourth stage to investigate the impact of group identity on social preferences and economic outcomes. In this stage, subjects made decisions in a series of two-person sequential move games selected from Charness and Rabin (2002)[13] as well as an extension of some of the games. Appendix A presents a description of the set of games as well as the summary statistics for each game. Specifically, we selected five two-person dictator games and sixteen two-person response games. Furthermore, to investigate the sensitivity of Player B's response to the cost in self-benefit, we added three games that were based on Berk31 (Charness and Rabin 2002) with a varied amount for Player B payoff. Altogether, we have a total of 24 games.

The two-person response games fall into three categories. For games in the first category, B incurs no cost to help or punish A. For games in the second category, B needs to sacrifice her own self-interest to help A. For games in the third category, B incurs a cost if she penalizes A.

In the control and the Original treatment, subjects made decisions in seven to ten games in each session.[14] In all other treatments, subjects made decisions in twelve games in each session.[15]

[11] In Turner (1978), participants were asked to allocate tokens in two conditions. In one condition, everyone was asked to allocate awards to two other individuals (other-other) before dividing awards between herself and the other person with whom she was matched (self-other). In the other condition, the order was reversed. Turner finds an order effect on whether one was willing to trade self-interest for other's welfare. Specifically, ingroup favoritism was significant in the self-other choices if they were preceded by other-other allocations. However, it was not significant when the order was reversed.

[12] See Charness and Rabin (2005) for a discussion of the use of strategy methods in experimental games.

[13] We thank Gary Charness for helping us select the games.

[14] Game Set 1 includes Dict 1, Dict 3, Resp 1a, Resp 2b, Resp 5a, Resp 5b, Resp 10; Game Set 2 includes Dict 2, Dict 4, Dict 5, Resp 2a, Resp 3, Resp 4, and Resp 11. Game Set 3 includes Resp 1b, 6-9, 12, and 13a-d.

[15] Game Set 1 includes Dict 1, Dict 3, Resp 1a, Resp 2b, Resp 5a, Resp 5b, Resp 6, Resp 10, and Resp 13a-d, while

For each game, each participant was randomly matched with another participant and they were randomly assigned roles A or B. No feedback was given until the end of the experiment. This procedure is similar to that in Charness and Rabin (2002). For the RandomBetween treatment, each participant decides which strategy to use in each game, while in the remaining four treatments, we use the strategy method to solicit participant decisions under two scenarios: if the participant's match is from the same group, and if her match is from the other group. At the end of the experiment, two of the games were randomly selected by the computer to compute the payoffs, as announced in the instructions.

3.5 Summary

In sum, we conducted one control and five treatments, of which the Original treatment were designed to address our central questions while the remaining four treatments were designed to address methodological issues. We have a total of 9 independent control sessions, 15 Original, 4 NoChat, 2 NoHelp, 2 RandomWithin and 4 RandomBetween treatment sessions. At the end of each experimental session, we conducted a post-experiment survey, which contains questions about demographics, past giving behavior, strategies used during the experiment, group affiliation, and prior knowledge about the artists and paintings. The survey and response statistics are included in Appendix C.

Experimental instructions for the original treatment are included in Appendix B. Those for all other treatments are posted on the first author's web page.[16]

[Table 1 about here.]

Table 1 summarizes the features of experimental sessions, including Treatments, Group Assignment methods, whether a treatment includes the online chat stage, whether it includes the other-other allocation stage, whether it uses a within- or a between-subject design, the number of sessions in each treatment, the total number of subjects in each treatment, as well as the number of subjects used in the analysis (the last column in brackets). Overall, 36 independent computerized sessions were conducted in the RCGD lab at the University of Michigan from January to July 2005, and in May and June 2007, yielding a total of 566 subjects, of which 562 were used in the analysis.[17] We used z-Tree (Fischbacher 1999) to program our experiments. Most of our subjects were students from the University of Michigan.[18] Participants were allowed to participate in only one session. Each treatment session lasted approximately one hour, whereas each control session lasted about thirty to thirty-five minutes.[19] The exchange rate was set to 100 tokens for $1. In addition, each participant was paid a $5 show-up fee. Average earnings per participant were $18.85 for those in the treatment sessions and $14.40 for those in the control sessions. Data are available from the authors upon request.

Game Set 2 includes Dict 2, Dict 4, Dict 5, Resp 1b, Resp 2a, Resp 3, Resp 4, Resp 7-9, and Resp 11-12.

[16] The URL is not shown in the current version to preserve anonymity.

[17] Despite our announcement that each subject can only participate in one session and our pre-experiment screening, four subjects participated twice. In all analyses, we exclude the second time data for these subjects. Subjects #16 in Session 1 (Original), and #3 in Session 11 (Control) are excluded from the analysis, as they participated in the pilot. Subjects # 1 and # 10 in Session 4 (Original) are excluded from the analysis, as they participated in a previous session.

[18] A few subjects were staff members at the University of Michigan.

[19] Recall that participants in control sessions participated only in the fourth stage of the experiment.

4 Hypotheses

In this section, we introduce our main hypotheses. For each hypothesis, we first state the null hypothesis formally and then discuss the alternative hypothesis.

The first hypothesis relates to the second stage of the experiment in which participants were asked to allocate a fixed amount of money to two other participants.

HYPOTHESIS 1 (Other-other Allocation)**.** *In other-other allocations, participants will allocate the same amount of money to ingroup and outgroup members.*

Based on findings from the social psychology literature on identity, we expect that participants will allocate more money to ingroup members than to outgroup members. That is, they will exhibit ingroup favoritism and outgroup discrimination.

We now consider the effects of group identity on participant distribution preferences.

HYPOTHESIS 2 (Charity)**.** *With induced group identity, participants who receive a higher payoff than their matches show the same level of charity concern towards both ingroup and outgroup members.*

HYPOTHESIS 3 (Envy)**.** *With induced group identity, participants who receive a lower payoff than their matches show the same level of envy towards both ingroup and outgroup members.*

An extension of the ingroup favoritism result from other-other allocations implies that a participant with a higher payoff than her match will show more charity concern towards an ingroup rather than an outgroup match. Likewise, we motivate our alternative hypothesis on the effects of group identity on envy based on related social psychology literature on reaction to a close other's success. The self-evaluation maintenance theory (Tesser 1998) proposes that when a close other performs well on a relevant task it threatens self-esteem. As the result, people are inclined to react negatively to successes by close others on relevant tasks. Gardner, Gabriel and Hochschild (2002) show, however, that if interdependent self-construals are primed, successes of a friend become causes for celebration rather than costs to self-esteem. Since the empirical results in related social psychology research is mixed, we expect that a participant with a lower payoff than her match will treat an ingroup and an outgroup match differently, but we do not specify the direction of the alternative hypothesis *a priori*.

In addition to distribution implications, in Section 5, we develop an empirical model of reciprocity, where the respective likelihoods of reward and punishment depend on the cost and benefit of reciprocity. The next two hypotheses look at the effects of group identity on the likelihood of positive and negative reciprocity.

HYPOTHESIS 4 (Positive Reciprocity)**.** *With induced group identity, participants are equally likely to reward good behavior from an ingroup and an outgroup match.*

HYPOTHESIS 5 (Negative Reciprocity)**.** *With induced group identity, participants are equally likely to punish bad behavior from an ingroup and an outgroup match.*

With regard to reciprocity, it seems plausible that participants may be more likely to reward good intentions from an ingroup match. However, one may also take good behavior from an ingroup match for granted, and thus be less likely to reward it. Similarly, when an ingroup match behaves badly, one could either be more forgiving and thus less likely to retaliate, or more hurt and thus more likely to retaliate. It is therefore especially important to examine the empirical evidence regarding group identity and reciprocity behavior.

Hypothesis 6 states the effect of group identity on the likelihood of choosing social welfare maximizing (SWM) actions. Throughout the paper, we refer to actions that maximize joint payoffs as SWM actions.

HYPOTHESIS 6 (Social Welfare)**.** *With induced group identity, participants are equally likely to choose SWM actions when matched with an ingroup and outgroup person.*

Alternatively, if people care more about an ingroup match, participants would be more likely to choose SWM actions when matched with an ingroup member.

5 Main Results

In this section, we first examine the effects of group identity on other-other allocations. We then investigate how group identity affects participant social preferences, including distribution preferences, reciprocity and SWM behavior. All results in this section use data from the Original treatment and the control. Results from the other four treatments are presented in Section 6.

Several common features apply throughout our analysis in this section. First, standard errors in the regressions are clustered at the individual level to control for the potential dependency of individual decisions across games.[20] Second, we use a 5% statistical significance level as our threshold (unless stated otherwise) to establish existence of causal effects.

We first investigate whether participants show ingroup favoritism when allocating tokens between two other individuals. Recall that, during each of the five rounds of other-other allocations, a participant makes decisions under three scenarios: if the two other individuals come from her own group; if they come from the other group; and if one comes from her own group and one from the other group. Social psychology experiments demonstrate that participants allocate tokens equally between two other persons in the first two scenarios, while in the last scenario, they persistently give more tokens to the ingroup match. The main difference between the other-other allocation stage of our experiment and the social psychology experiments is that, in our experiment, allocations translate into real monetary payoffs at a pre-announced exchange rate.

[Figure 1 about here.]

Figure 1 presents the average allocation amount per participant across all sessions by round under each of the three scenarios. In all graphs, the horizontal axis is the number of rounds, while the vertical axis is the number of tokens allocated. The top panel presents the average allocation between two ingroup members. The middle panel exhibits the average allocation between two outgroup members. The bottom panel presents the average allocation between an ingroup and an outgroup member. The top and middle panels show that, on average, participants allocate an almost equal amount to two other individuals, if they are both from an ingroup or an outgroup. In the bottom panel, however, the average number of tokens allocated to an ingroup member (a diamond) is substantially more than that allocated to an outgroup member (a square). The difference is economically sizable and statistically significant.

[20] We do not cluster the standard errors at the session level, as participants made all their decisions independently, and they did not get any feedback on their decisions until the end of the experiment.

Result 1 (Other-Other Allocation)**.** *When allocating a fixed number of tokens between two other individuals, participants allocate significantly more to an ingroup than to an outgroup member. The difference between ingroup and outgroup allocations (normalized by endowment) is between 32.2% and 38.4%.*

Support. *The average allocation to an ingroup match is substantially greater than that to an outgroup match. The difference is statistically significant at the 1% level for each of the five rounds (t-statistics for one-tailed tests for paired samples).* ∎

By Result 1, we reject Hypothesis 1. In other words, with real incentives and groups based on true painting preferences, we replicate the ingroup favoritism result of minimal group paradigm experiments.

Next, we analyze the effect of group identity on distribution preference, i.e., charity and envy, without reciprocity. We first extend Charness and Rabin's social preference model to incorporate group identity. In the two-person model of social preference developed by Charness and Rabin (2002), an individual's utility function is a weighted average of her own and her match's monetary payoffs.[21] To illustrate, let π_A and π_B be Player A and B's monetary payoffs, respectively. Let w_A denote the weight that Player B puts on A's payoff. Player B's preference is represented by:

$$\begin{aligned} u_B(\pi_A, \pi_B) &= w_A\pi_A + (1 - w_A)\pi_B & (1) \\ &= (\rho r + \sigma s)\pi_A + [1 - (\rho r + \sigma s)]\pi_B, & (2) \end{aligned}$$

where $r = 1$ if $\pi_B > \pi_A$, and $r = 0$ otherwise. Similarly, $s = 1$ if $\pi_B < \pi_A$, and $s = 0$ otherwise. Therefore, the weight B places on A's payoff, $w_A = \rho r + \sigma s$, may depend on the comparison between A's and B's payoffs. The parameter ρ measures B's charity concern when her payoff is higher than her match's, while σ measures B's envy when her payoff is lower than her match's. We incorporate group identity into the model by redefining the weight that Player B puts on A's payoff as

$$w_A^I = \rho(1 + Ia)r + \sigma(1 + Ib)s, \qquad (3)$$

where $I = 1$ if Players B and A belong to the same group, and $I = 0$ otherwise. The parameters, a and b, capture the additional ingroup effect for charity and envy, respectively. For example, when B receives a higher payoff than A, the parameter ρ measures the charity effect for an outgroup match, while $\rho(1 + a)$ measures the charity effect for an ingroup match. The difference, a, measures the additional effect of ingroup identity on an individual's charity concerns. Therefore, the new utility function for Player B is

$$u_B(\pi_A, \pi_B) = w_A^I\pi_A + (1 - w_A^I)\pi_B. \qquad (4)$$

We use Player B's data from the sequential games to estimate the parameters of Equation (4). Our maximum-likelihood estimation on our binary-response data uses a logit specification:

$$\text{Prob(action 1)} = \frac{e^{\gamma \cdot u(action1)}}{e^{\gamma \cdot u(action1)} + e^{\gamma \cdot u(action2)}}, \qquad (5)$$

where the parameter γ reflects the sensitivity of the choices to utility differences. When $\gamma = 0$, this model is reduced to a random choice model with equal probability. When γ is arbitrarily large,

[21] A CES model of social preference incorporating both distribution and reciprocal preferences is estimated and discussed in Appendix D.

the probability of choosing the action with higher utility approaches one. In general, the higher the value of γ, the sharper the model predictions (McFadden 1981).

[Table 2 about here.]

Table 2 reports the results of our parameter estimation. As a benchmark, we estimate the charity and envy parameters for the control sessions. For the treatment sessions, we report the parameter estimates for both ingroup and outgroup matches as well as their differences, as represented by parameters a and b. We now summarize our main results based on the estimates.

Result 2 (Charity)**.** *Participants show charity concerns when their match receives a lower payoff than themselves. Their charity towards an ingroup match is significantly greater than that towards an outgroup match.*

Support. *In Table 2, the charity parameter ρ is 0.427 for the control sessions. In the Original treatment sessions, $\rho_o = 0.323$ for outgroup matches, and $\rho_i = \rho_o(1 + a) = 0.474$ for ingroup matches. All estimates are statistically significant at the 1% level. The effect of group identity on charity is measured by the parameter a. It is 0.467 and is statistically significant at the 1% level.* ∎

By Result 2, we reject Hypothesis 2 at the 1% level in favor of the alternative hypothesis that participants show more charity concern towards an ingroup match. More precisely, participants show a 47% increase in charity concerns towards an ingroup match compared with an outgroup match. This is the first main result of the paper. Rewriting Equation (4) with the estimated parameters yields:

$$u_B(\pi_A, \pi_B) = 0.474\pi_A + 0.526\pi_B \tag{6}$$

when A is an ingroup match. In comparison, B's utility function becomes:

$$u_B(\pi_A, \pi_B) = 0.323\pi_A + 0.677\pi_B \tag{7}$$

when A is an outgroup match, and

$$u_B(\pi_A, \pi_B) = 0.427\pi_A + 0.573\pi_B \tag{8}$$

when A is a match in the control sessions.

Result 3 (Envy)**.** *Participants exhibit envy when they receive a lower payoff than their match. However, with induced group identity, participants show significantly less envy towards an ingroup match than an outgroup match.*

Support. *In Table 2, the estimate of the envy parameter σ is -0.049 in the control sessions. In Original treatment sessions, $\sigma_o = -0.112$ for outgroup matches, whereas $\sigma_i = \sigma_o(1+b) = -0.008$ for ingroup matches. The parameter estimates are statistically significant at the 5% level for the control and 1% level for the outgroup matching. We can not reject that it is zero for the ingroup matching. The identity parameter, $b = -0.931$ ($p < 0.01$), indicates that ingroup matching significantly reduces envy.* ∎

By Result 3, we reject Hypothesis 3 at the 1% significance level. The result indicates that, when participants have a lower payoff than their ingroup match, they show a 93% decrease in envy. Again, it is informative to rewrite Equation (4) using the estimated parameters. For an ingroup match, this yields:

$$u_B(\pi_A, \pi_B) = -0.008\pi_A + 1.008\pi_B, \tag{9}$$

which is statistically equivalent to maximizing one's own payoff. In comparison, B's utility function becomes:

$$u_B(\pi_A, \pi_B) = -0.112\pi_A + 1.112\pi_B \tag{10}$$

when A is an outgroup match. When A is a match in the control sessions, we obtain:

$$u_B(\pi_A, \pi_B) = -0.049\pi_A + 1.049\pi_B. \tag{11}$$

Together, Results 2 and 3 suggest that group identity affect people's distribution preferences differently depending on the relative positions. Participants show more charity, but less envy when matched with an ingroup member. Both effects, however, are consistent with putting more weight on an ingroup match's payoff, compared to the control and outgroup matching. Furthermore, Equations (6) - (11) highlight the difference between our identity model and altruism models such as that of Basu (2006), where the weight on the other person's payoff is independent of payoff distribution.

With our calibrated model, we can predict behavior in other games. We use the Battle of Sexes game (BoS hereafter) as an example. Cooper, DeJong, Forsythe and Ross (1989) find that, in BoS, subjects converge to a frequency of choices that is close to the mixed strategy equilibrium. Taking a version of BoS such as the one on the left (Charness et al. 2006), we can use Equations (6) - (11) to incorporate the effects of group identity on distribution preferences. The original BoS game has a mixed strategy equilibrium of $((0.25, 0.75), (0.75, 0.25))$, with a probability of coordination equal to 0.188. With ingroup matching, we use Equations (6) and (9) to generate the transformed game in the center, which has a mixed strategy equilibrium $((0.33, 0.67), (0.67, 0.33))$, with a probability of coordination equal to 0.22. Similarly, with outgroup matching, we use Equations (7) and (10) to generate the payoff matrix on the right, which has a mixed strategy equilibrium of $((0.247, 0.752), (0.752, 0.247))$, with a probability of coordination equal to 0.186. This leads to a directional prediction that the probability of coordination is the highest for ingroup matching.

Original BoS

	A	B
A	3, 1	0, 0
B	0, 0	1, 3

Transformed: Ingroup

	A	B
A	2.052, 1	0, 0
B	0, 0	1, 2.052

Transformed: Outgroup

	A	B
A	2.354 , 0.776	0, 0
B	0, 0	0.776, 2.354

This prediction is consistent with previous experimental findings that participants with salient group identities are more likely to coordinate towards efficient outcomes in games with multiple Pareto efficient equilibria (Charness et al. 2006, Croson et al. 2003).[22]

[22] With the simple calibrated model, we cannot predict more subtle effects of natural groups on equilibrium selection, such as the gender difference in Croson et al. (2003).

To formally investigate the effects of group identity on reciprocity, we use a logit model to examine separate games of positive and negative reciprocity. In games of positive reciprocity, Player A's entry into the game is associated with good intentions, whereas in games of negative reciprocity, A's entry reflects bad intentions.

In games in which A's entry shows good intentions, B's choice on whether to reward A can be affected not only by A's group identity, but also by other factors. Specifically, we consider four other explanatory variables including benefit to B due to A's entry, B's cost to reward A, A's benefit from B's reciprocation, and B's payoff lag when B rewards A. In the positive-reciprocity games, benefit to B due to A's entry is measured by the difference between B's expected payoff when A enters[23] and B's payoff when A opts out. B's cost of reciprocation is measured as her payoff difference when choosing between the reciprocating action and the alternative. Player A's benefit from B's reciprocation is computed as the gain in A's payoff if B chooses to reciprocate. Since, by rewarding A, Player B gets a payoff that never exceeds her match's in all the positive-reciprocity games, the distance in their payoffs, i.e., how much B falls behind A, allows us to examine how B's envy relates to positive reciprocity.

[Table 3 about here.]

Table 3 presents the results of the logit model. The coefficients are probability derivatives. The unit of these variables is 100 tokens in the regressions. Specifications (1) and (2) present the models of positive reciprocity for the control and treatment sessions, respectively. Consistent with Hypothesis 4, participants are more likely to reward ingroup members. The ingroup matching, compared to outgroup matching, increases the likelihood of Player B's positive reciprocation by 22%. In addition, we find that benefit to B due to A's entry is positive but not significant, while B's cost reduces the probability of rewarding A but the effect is only significant at the 10% level. Furthermore, the benefit to A if B rewards significantly increases B's likelihood to reward by 8%. An interesting finding is the interaction between reciprocity and distribution preferences, i.e., B is less likely to return A's favor if doing so causes her to get a lower payoff than her match. In other words, an outcome with more equal payoffs will increase the probability of positive reciprocation, which is consistent with envy.

We now present a similar analysis for the negative reciprocity games. Specifically, we examine the effects of five independent variables on the likelihood of B's punishment, including the match's group, B's loss in payoff due to A's entry, B's cost of punishment, damage to A if B punishes, and B's relative payoff in comparison to A's. These variables are constructed in a similar way as their counterparts in the analysis for positive reciprocity.[24] In all the Pareto-damaging games, B, by punishing, gets a payoff that either equals or exceeds A's. Hence, the design enables a comparison between participant charity concern and negative reciprocity decisions.

The logit results on negative reciprocity for the control and treatment sessions are presented in columns (3) and (4) of Table 3. These results indicate that the ingroup matching significantly reduces the likelihood of punishment by 12.8% ($p < 0.01$). This implies that Player B is more lenient towards misbehavior by an ingroup Player A. Compared to other independent variables, the cost variable has the largest effect. The results suggest that increasing B's cost by 100 tokens

[23] B's expected payoff if A enters is computed by using the empirical distribution of B's actual behavior.

[24] Player B's loss due to A's entry is the difference in B's payoff if A chooses Out and her expected payoff if A enters. B's cost to punish is her opportunity cost when choosing to punish A. The damage to A caused by B's choice to punish is computed as the loss of A's payoff if B chooses to punish.

will lower the likelihood of punishment by 26.5% ($p < 0.01$) in the control and 31.6% ($p < 0.01$) in the treatment. We also find that the Damage to A variable has a positive sign. This coefficient indicates that the likelihood of B choosing to punish A is 4.2% ($p < 0.05$) higher if the punishment imposes an additional 100 tokens of potential damage to A who has misbehaved. Again, participant distribution preferences play a crucial role. Specifically, if B is in the lead, the likelihood of B choosing to punish A decreases by 17.1% ($p < 0.05$) in the control or 10.3% ($p < 0.01$) in the treatment if the payoff difference rises by 100 tokens.

Result 4 (Reciprocity)**.** *The participant reciprocity preference is significantly different between ingroup and outgroup matches. Participants are more likely to reward an ingroup than an outgroup match for good behavior. They are significantly more forgiving towards misbehaviors from an ingroup match compared to an outgroup match.*

Support. *In Table 3, the marginal effects of the ingroup match variable are* 0.218 *(*$p < 0.01$*) for specification (2) and* -0.128 *(*$p < 0.01$*) for specification (4).* ∎

By Result 4, we reject hypotheses 4 and 5, as participants are more likely to reward an ingroup member's good behavior but less likely to punish an ingroup member's misbehavior. Both of these findings are again consistent with putting more weight on an ingroup match's payoff, or being more altruistic towards an ingroup match. Furthermore, the analyses also provide insights into the connection between distribution and reciprocity preferences. Specifically, an outcome with more equal payoffs will increase the probability of both positive and negative reciprocity. Our positive reciprocity result is consistent with Simpson (2006).

Next, we investigate the effect of group identity on the tendency to choose SWM actions, a third important element in social preference. We compute the proportion of participants who make SWM decisions for both the control and treatment sessions. In doing so, we exclude three games, Dict 5, Resp 5a and 5b, and role B in game Resp 9, as the outcomes in these games have the same aggregate payoffs. The results are presented in Table 4. Game-by-game results are not presented due to space limitations, but are available from the authors upon request.

[Table 4 about here.]

Table 4 reports the proportion of SWM decisions for Players A and B, as well as for all players, for three conditions: ingroup matching (column 2), outgroup matching (column 3), and control sessions (column 4). Column 5 presents the alternative hypothesis that participants are more likely to choose SWM decisions when matched with an ingroup member than when matched with an outgroup member, as well as the p-values for paired-sample t-tests for Players A and B, as well as for all players. Column 6 presents the p-values for t-tests of proportions for the alternative hypothesis that participants in treatment sessions are more likely to choose SWM decisions when matched with an ingroup member than are participants in control sessions. The last column presents similar test results for the alternative hypothesis that outgroup matches are less likely to lead to SWM outcomes than matches in control sessions.

Result 5 (Social Welfare Maximization)**.** *Both Players A and B are significantly more likely to choose SWM decisions when matched with an ingroup member than when matched with an outgroup member. Compared with the control session, participants are more likely to choose SWM decisions if matched with an ingroup member, but less likely to do so if matched with an outgroup member.*

Support. *Column 5 in Table 4 presents the p-values for paired-sample t-tests,* $p < 0.01$*, for Players A and B as well as for all participants, obtained by comparing the proportion of SWM decisions for ingroup vs. outgroup matchings. Column 6 presents the p-values for t-tests of proportions,* $p < 0.05$ *for Player A and for all players, whereas* $p < 0.10$ *for Player B, comparing the ingroup vs. control sessions. Column 7 presents the p-values for t-tests of proportions,* $p < 0.05$ *for Players A, B and for all players, comparing outgroup vs. control sessions.* ∎

By Result 5, we reject Hypothesis 6 in favor of the alternative hypothesis that group identity has a significant effect on the likelihood of SWM choices. Comparing the treatment results with those from the control sessions, we find that participants are significantly more likely to choose SWM actions for the ingroup matches, and are more likely to withdraw SWM actions for the outgroup match. Result 5 predicts that, in games with a unique Pareto efficient outcome, people with salient group identities are more likely to choose SWM actions when they are matched with an ingroup member. This prediction is consistent with findings of previous experiments. For example, in a Prisoner's Dilemma game, participants are more likely to choose cooperation when matched with an ingroup member (Goette et al. 2006). Similarly, in a voluntary contribution public goods game, participants are more likely to contribute when they are matched with ingroup members (Eckel and Grossman 2005).

Given the above results regarding the effect of group identity on social preferences, we expect that group identity will also have an effect on the final payoff. Next, we report the actual average earnings by role and experiment conditions. To extract the maximum information out of the data, we also use simulations to compute each participant's expected payoff when she is matched with every member of the opposite role in her session. For example, in the actual experiment, a Player A is randomly matched with one Player B in her session and the payoffs for both players are determined by their stated strategies. In the simulation, however, a Player A is hypothetically matched with every Player B in her session. Her expected payoff is the average payoff she gets from each match. We compute the expected earnings for all players and present the results across all games in Table 5.[25]

[Table 5 about here.]

Table 5 reports the actual average earnings and the expected earnings for Players A and B and for all players, for three matching conditions: ingroup (column 2), outgroup (column 3), and control sessions (column 4). Columns 5 to 7 present the alternative hypotheses, as well as the p-values for paired-sample t-tests for Players A, B and over all players.

Result 6 (Earnings)**.** *Participants' actual average earnings and expected earnings are significantly higher when they are matched with an ingroup member than with an outgroup member. Compared to the control sessions, ingroup matching yields marginally higher earnings whereas outgroup matching yields significantly lower earnings. Comparisons of expected earnings are all associated with higher confidence levels than those of actual average earnings.*

Support. *Column 5 in Table 5 presents the p-values for paired-sample t-tests,* $p < 0.01$ *for Player A, B and over all players, comparing expected earnings from matching with an ingroup vs. an outgroup member. A comparison between columns 2 and 4 shows that ingroup matching is associated with slightly higher expected earnings than matching in control sessions. It is statistically*

[25]Game-by-game comparisons are not presented due to space limitations, but are available from the authors upon request.

significant at the 10% level only for Player B. For Player A and for over all participants, we fail to reject the null hypothesis that expected earnings are equal. Column 7 presents the p-values for the equality of mean tests, $p < 0.05$ *for Player B and for over all participants, comparing expected earnings from outgroup matching vs. control sessions. We cannot reject that the expected earnings are equal for Player A. Cross-condition comparisons of actual average earnings show the same pattern, but with lower significance levels. The reason is that, in the simulation, each participant is hypothetically matched twice, once with ingroup members and once with outgroup members, whereas she is matched only once with either an ingroup member or an outgroup one under the actual treatment conditions.*[26] ∎

As shown above, the induced group identity introduces a gap in earnings (actual average earnings and expected earnings alike) between the ingroup and outgroup matches. This gap arises more from the loss in outgroup matching than from the gain in ingroup matching, in comparison to the control sessions. In other words, the economic outcome resulting from ingroup matching is made only marginally better than the outcome in the control group. However, outgroup matching does make agents significantly worse off compared to the scenario where there is no group.

In this section, we have examined the effects of group identity on three aspects of social preference: distribution preferences, reciprocity preferences and social welfare maximization. With induced identity, when matched with an ingroup member, participants show more charity when they have a higher payoff than their match, and less envy when they have a lower payoff. Other things equal, participants are more likely to reciprocate positively to an ingroup than to an outgroup match. They are more forgiving towards bad behaviors from ingroup matches compared to outgroup matches. Furthermore, participants are significantly more likely to choose SWM actions when matched with an ingroup member. As a result, expected earnings are significantly higher when participants are matched with an ingroup as opposed to an outgroup member. While ingroup matching yields expected earnings comparable to those in the control sessions, outgroup matching yields significantly lower expected earnings than does control sessions.

6 Methodological Issues

In this section, we address the question of what creates the group effects. An open question from social identity research is whether pure categorization alone or generalized reciprocity among ingroup members creates group effects. Results from prior research which largely focuses on one or two games suggest that the answer might be game and process specific. By using a large number of games, and various combinations of components to induce group identity, our design is uniquely suited to answer this question.

Our Original treatment is not minimal, as criteria 1, 2 and 4 are not satisfied. However, it provides a rich environment to evaluate the effects of various components through comparison with other treatments. In this section, we investigate the effects of group assignment methods, chat, and other-other allocations, and a within- vs. between-subject design. In assessing the effect of each component, we use three types of analysis:

1. At the treatment level, we examine the proportion of subjects who differentiate between

[26] Appropriate recombination of individual strategies in simulations can improve the efficiency of the estimation. See Mullin and Reiley (2006) for the use of related techniques.

ingroup and outgroup matches for within-subject treatments by using the t-test of proportions.[27] This information is summarized in Table 6.

[Table 6 about here.]

2. At the game level, we use the Fisher's exact probability test to investigate if different treatments lead to different behavioral choices by players when facing an ingroup or outgroup match, respectively. We report the p-values of 2-side test for games where significant difference is found. The test statistics for all other games are available from the authors upon request.

3. At the affective level, we analyze self-reported group attachments in the post-experiment survey by using OLS and ordered logit regressions.

   In the post-experiment survey, subjects report the strength of their ingroup attachment on a scale between 1 and 10, where a higher score represents stronger ingroup attachment. We pool all treatments and use OLS and ordered logit models to investigate the determinants of self-reported group attachment. The independent variables include four dummy variables, *Paintings*, *Chat*, *Other-Other Allocation*, and *Within-subject*, where *Paintings* equals one for treatments where subjects are grouped based on their painting preferences and zero for random assignment, *Chat* equals one for treatments with chat and zero otherwise, *Other-Other allocation* equals one for treatments containing the other-other allocation stage and zero otherwise, *Within-subject* equals one for all within-subject treatments and zero otherwise.

   [Table 7 about here.]

   Table 7 indicates that both the OLS and ordered logit models yield consistent results on how various design components affect group attachment. We include both specifications, as the OLS coefficients have more straightforward interpretations than those of the ordered logit.

We first examine group assignment methods. In our Original treatment, participants are assigned groups based on their true painting preferences, while in the RandomWithin and RandomBetween treatments, they are randomly assigned to groups. The latter conforms to the first criterion in the minimal group paradigm in psychology. To evaluate any systematic difference between random assignment and assignment based on true painting preferences, we compare the Original and the RandomWithin treatments, which only differ in the group assignment methods.

Result 7 (Group Assignment)**.** *Comparing the Original and the RandomWithin treatments, we find no significant difference in the proportion of participants who differentiates between ingroup and outgroup matches. At the game level, we find no significant behavioral difference between the two treatments except in two games. Finally, group assignment methods do not affect self-reported group attachments.*

[27] We also examine the proportion of *decisions* which are different between ingroup and outgroup matches, and find that the results are similar except in one case, NoChat and NoHelp are significantly different in role B decisions ($p = 0.07$ for role A and $p = 0.01$ for role B). However, the decision-level analysis treats each decision as an independent observation, so the difference might be overestimated. Therefore, we choose to report the individual level results.

Support. *Statistical support has three parts: (1) Table 6 indicates that the proportions of participants who differentiate between ingroup and outgroup matches are 0.39 for both role A and B in the Original treatment, and 0.34 and 0.44 for role A and B, respectively, in the RandomWithin treatment. P-values of the two-sided t-tests for the equality of proportions are 0.65 and 0.60 for role A and B, respectively. (2) At the game level, the Fisher exact probability test suggests no significant behavioral difference for role A (*$p > 0.05$*) in 23 games. The only exception is A's outgroup decision in game Resp 7, where the fraction of As who choose Enter to reduce B's payoff is 0.50 in the RandomWithin treatment and 0.12 in the Original treatment (*$p = 0.031$*), indicating stronger outgroup discrimination under the random assignment. For role B, the Fisher exact test reveals no significant difference (*$p > 0.10$*) except for B's ingroup decision in Resp 2a. Specifically, the fraction of Bs who choose to reward an ingroup A is 0.75 in the RandomWithin treatment and 0.28 in the Original treatment (*$p = 0.019$*), indicating stronger ingroup favoritism under random assignment. (3) In Table 7, the coefficient estimates of the Paintings dummy are 0.26 in OLS and 0.21 in ordered logit, respectively. Neither is significantly different from zero (*$p > 0.10$*).* ∎

Result 7 indicates that the two group assignment methods, random assignment and assignment using true painting preferences, creates no significant difference in participant behavior or attitude in 23 out of 24 games, which is largely consistent with our earlier analysis within the Original treatment that there is no correlation between individual choice and painting preferences. However, even though we find no significant difference in participant behavior in most of our games, generically, random assignment has many advantages over group assignment based on participant preferences. For example, it can reduce experimenter demand effects. We do not preclude that, group assignment based on participant preferences might induce behavioral differences in other games. In both laboratory and field experiments, random assignment is an important method in obtaining probabilistically equivalent groups. To our best knowledge, the only field experiment using randomized real groups is Goette et al. (2006). We, therefore, recommend using random assignment to groups whenever possible.

Next, we separately investigate the effects of the two components, the online chat and the other-other allocations. Both components could induce generalized reciprocity within one's own group. We evaluate the effects of these components by taking them off, one at a time. Fundamentally, we want to address the questions of what creates the group effects. Is categorization sufficient or is it necessary for group members to interact and help each other?

To investigate the effects of the online chat stage, we compare the NoChat and the Original treatments. Compared to the Original treatment, the NoChat treatment takes away the online chat stage, while everything else remains the same.

Result 8 (Online Chat)**.** *Comparing the NoChat and the Original treatments, we find no significant difference in the proportion of participants who differentiates between ingroup and outgroup matches. At the game level, we find no significant behavioral difference between the two treatments except in two games. Self-reported attachments are significantly higher in the treatments with Chat.*

Support. *Statistical support has three parts: (1) Proportion of t-tests based on Table 6 indicate that the proportions of individuals who differentiate between ingroup and outgroup matches are 0.39 for both roles and 0.45 for both roles in the Original and NoChat treatments. P-value of two-sided proportion t-tests are 0.33 and 0.36 for role A and B, respectively. (2) At the game level, the Fisher exact probability test shows that the NoChat and Original treatments yield no significant* *behavioral difference except in the following two cases. In the first case, A's ingroup decisions in game Resp 2a are significantly different in the two treatments. The fraction of As who choose Enter to help an ingroup B is 0.72 in the Original treatment and 0.31 in the NoChat treatment, indicating that the chat stage leads to significantly stronger ingroup favoritism ($p = 0.007$). In the second case, B's ingroup decisions in game Resp 1a are significantly different in the two treatments. The fraction of Bs who choose Right to reward an ingroup A is 0.65 in the Original and 0.94 in the NoChat treatments, respectively, indicating stronger ingroup favoritism in the absence of the chat stage ($p = 0.043$). (3) In Table 7, the coefficient estimates of the Chat dummy are 1.135 in OLS and 0.792 in ordered logit, respectively. Both are significantly greater than zero ($p < 0.01$).* ∎

Result 8 indicates that, while online chat has a significant effect on behavior in only two out of 24 games, it does significantly increase self-reported group attachments, i.e., the affective aspect of group identification process.

To investigate the effects of the other-other allocation stage, we compare the NoChat and NoHelp treatments. While the NoChat treatment takes away the online chat stage, the NoHelp treatment takes away both the online chat and the other-other allocation stage.

Result 9 (Other-Other Allocation)**.** *Comparing the NoChat and the NoHelp treatments, we find no significant difference in the proportion of participants who differentiate between ingroup and outgroup matches. At the game level, the two treatments yield no significant behavioral difference except in one game. Lastly, other-other allocation has no significant effect on self-reported attachments to groups.*

Support. *Statistical support has three parts: (1) Proportion of t-tests based on Table 6 indicate that the proportions of individuals who differentiate are 0.45 and 0.56 in NoChat and NoHelp treatments, respectively. The p-values are 0.32 for both roles. (2) At the game level, the Fisher exact probability test shows that choices are not significantly different ($p > 0.05$) in the two treatment except in one game. In game Resp 11, the fraction of As who choose Enter to reduce outgroup B's payoff is 0.37 in NoChat and 0.87 in NoHelp ($p = 0.033$), indicating less outgroup discrimination in the NoChat treatment. (3) In Table 7, the coefficient estimates of the other-other allocation dummy are -0.313 in OLS and 0.010 in ordered logit, respectively. Neither is significantly different from zero ($p > 0.10$).* ∎

Result 9 indicates that, unlike in Turner (1978), the other-other allocation stage has no significant effect on participant behavior (in 23 out of 24 games) nor their self-reported attachment to groups in our experiments.

Lastly, we evaluate the effects of within- vs. between-subject design. In our Original treatment, for each game, participants make two decisions, one under each of two scenarios when they have an ingroup and an outgroup match. This, again, might be subject to an experimenter demand effect. It also makes the presence of an outgroup more salient. To investigate the extent to which the presence of an outgroup affects behavior, we compare the RandomWithin and the RandomBetween treatments. Note that the t-test of proportions of group-differentiating participants at the treatment level can not be applied here, as in the RandomBetween treatment each subject makes only one decision in each game, with either an ingroup or an outgroup match, while in the RandomWithin treatment they make two decisions under both scenarios.

Result 10 (Within- vs. Between-Subject Design)**.** *At the game level, we find no significant behavioral difference for both player As and Bs, except in two games where player Bs' choices reveal* *stronger ingroup favoritism and outgroup discrimination in the within-subject design than in the between-subject design. Self-reported attachments are not significantly different.*

Support. *Statistical support has two parts: (1) At the game level, the Fisher exact probability tests suggest no significant behavioral difference for player As in any game ($p > 0.10$). The tests reveal no significant difference for player Bs in 22 games ($p > 0.10$), and significant difference in two games. When facing an outgroup match in game Dict 3, all 8 player Bs choose (300,600) over (700,500) in the RandomWithin treatment, significantly greater than 3 out of 8 player Bs who make the same choice in the RandomBetween treatment ($p = 0.026$), indicating stronger outgroup discrimination. When facing an ingroup match in game Resp 2a, 6 out of 8 player Bs choose (750,375) over (400,400) in the RandomWithin treatment, significantly greater than 1 out of 8 player Bs who chooses so in the RandomBetween treatment ($p = 0.041$), indicating stronger positive reciprocity towards an ingroup match in the within-subject treatment. (2) In Table 7, the coefficient estimates of the Within-subject dummy are -1.187 in OLS and -0.817 in ordered logit, respectively, and not significantly different from zero ($p > 0.10$).* ■

Result 10 has several implications. At the game level, even though we do not find significant behavioral difference between the within- and between-subject designs for the majority of the games, in the two games where we do find a significant difference, a within-subject design induces stronger ingroup favoritism and outgroup discrimination. The latter indicates stronger group effects when the presence of an outgroup is more salient, which is consistent with Charness et al. (2006), and Brewer (1999).

On the methodological level, analysis from this section, together with findings from other laboratory experiments on group identity, teaches us several lessons. To induce group identities in the laboratory, random assignment is as effective as group assignment based on participant true painting preferences. Furthermore, a between-subject design induces the same level of group effects as a within-subject design in most games. Whenever there is any difference, the latter induces stronger group effect. To enhance and strengthen group identity, a problem-solving stage, such as an online chat or puzzle-solving, can be effective, while the other-other allocation does not have significant effects.

On a psychological level, our analyses shed light on what creates group effects. In our set of games, dividing people into different groups, by random assignment or weak preferences, in itself, can generate group effects in a large class of games, although that effect can be weak in some games. Generalized reciprocity, while not necessary to induce group effect, can enhance and strengthen attachment to groups. Furthermore, based on prior research and our own study, we find that group effects differ across games. By using the transformed utility functions, Equations (6) to (11), we can make out-of-sample predictions on whether one is likely to observe the translation of group effects to behaviorial differences in new games.

7 Conclusion

Social identity theory has been applied to a broad array of issues across the social sciences, including prejudice, stereotyping, social competition, negotiation, language use, motivation and commitment, collective action, and industrial protest (Haslam 2004). Although it was only recently introduced into economics (Akerlof and Kranton 2000), it has the potential to shed light on many interesting economic issues and provide a novel and refreshing alternative to established theories.

Empirical work on social identity theory in social psychology focuses largely on other-other allocation games, where participants' benefits are not affected by their allocation decisions, and more recently, on variants of the Prisoners' Dilemma game. To formalize social identity theory mathematically and use it to analyze economic problems, it is important to systematically measure the effects of identity in a large class of games in the economic domain. This study does so by investigating the effects of identity on social preferences through twenty-four two-person sequential games in the laboratory.

In our experiments, we induce group identity by using different group assignment methods (classical Klee and Kandinsky paintings or random assignment), enhance group attachment by combinations of a problem-solving task and an other-other allocation game (in some treatments), and estimate group effects using twenty-four self-other sequential allocation games. We use the latter to measure the effects of identity on various aspects of social preference, including distribution, reciprocity and SWM actions.

We find that group identity has a significant effect on distribution preferences. When participants are matched with an ingroup member (as opposed to an outgroup member), they show more charity when they have a higher payoff; however, they show less envy when they have a lower payoff. Both results are consistent with participants putting more weight on the ingroup match's payoff in their own utility function.

We also present the empirical evidence for the effects of identity on participant reciprocity preferences. Rather than taking an ingroup match's good intentions for granted, participants are significantly more likely to reward an ingroup match for good behaviors, compared to an outgroup match. Furthermore, they are less likely to punish an ingroup match for misbehaviors.

Finally, we find that participants are significantly more likely to choose SWM actions when matched with an ingroup member. As a result, ingroup matching generates significantly higher expected earnings compared to outgroup matching.

Compared to the social psychology research on social identity, this study differs in several important aspects. First, the set of games is much larger and varied, including the other-other allocation games from social psychology and a variety of sequential games involving different degrees of conflict of interests between self and other. This feature leads to a more robust estimate of our empirical model. Second, our empirical model of social identity and social preference, which is calibrated over a large set of games, can make out-of-sample forecasts on a new set of games, and thus can explain disparate findings from prior research. In comparison, the (parameter-free) behavioral principles from social psychology research are not sensitive to the game parameters or the economic environment, which leads to less precise predictions. Third, we systematically investigate the effects of different components in the experimental design. By using pair-wise comparisons of our five treatments, we are able to provide a comprehensive answer to the question of what creates group effects. We find that pure categorization itself is sufficient to create group effects, which might manifest itself differently in different games. By comparison, generalized reciprocity through group problem-solving, such as online chat, significantly increases attachment to groups, but does not change behavior significantly in most of our games. Last but not least, we use real monetary incentives and no deception in our protocol.

This paper makes two contributions to the economics literature. The first contribution is a framework for the empirical foundation for incorporating identity into economic models. One area in economics in which social identity theory might prove especially valuable is the economics of organizations. Our results suggest that instead of modeling identity as a substitute for monetary rewards and thus a cost-saving device, a more prominent effect of identity is the increased likelihood of SWM actions and positive reciprocity.

A second contribution of this paper is its practical implications for organizational design. In neoclassical economics, the traditional approach to mechanism design relies heavily on incentives derived from Taylorism. However, this theory is silent about whether a deep sense of identity among employees within the firm is a worthwhile investment. Despite this lack, examples of identity creation abound. Nike founder Phil Knight and many of his employees have tattoos of the Nike "swoosh" logo on their left calves as a sign of group membership and camaraderie (Camerer and Malmendier 2005). Standard economic theory does not have an explanation for such phenomena. Our results suggest that creating a group identity would induce people to be more helpful to each other, and to increase the likelihood of SWM actions, which would improve payoffs for all relevant parties, the principal (firm) as well as the agents (workers). The use of social identity as a design tool is a promising direction of research, especially in environments where monetary incentives are limited, such as online communities (Ren, Kraut and Kiesler 2006).

There are several directions for fruitful future research. On the theory front, a formalization of group identity and its applications to various domains of organization design would help us better understand the effect of social identity on optimal contract and organizational hierarchies. On the empirical front, it would be interesting to explore the impact of social identity in practical mechanism design in the laboratory and the field.

APPENDIX A. Sequential Games with Self-Other Allocations

			Ingroup				Outgroup				% A Diff	% B Diff	Control			
Two-person dictator games																
		B chooses			Left	Right			Left	Right					Left	Right
Dict 1		(400,400) vs. (750,400)			0.30	0.70			0.45	0.55		0.26			0.33	0.67
Dict 2		(400,400) vs. (750,375)			0.67	0.33			0.73	0.27		0.26			0.82	0.18
Dict 3		(300,600) vs. (700,500)			0.68	0.32			0.86	0.14		0.24			0.76	0.24
Dict 4		(200,700) vs. (600,600)			0.34	0.66			0.63	0.38		0.29			0.5	0.5
Dict 5		(0,800) vs. (400,400)			0.56	0.44			0.77	0.23		0.24			0.64	0.36
Two-person response games: Bs payoffs identical																
	A stays out	If A enters, B chooses	Out	Enter	Left	Right	Out	Enter	Left	Right			Out	Enter	Left	Right
Resp 1a	(750,0)	(400,400) vs. (750,400)	0.46	0.54	0.26	0.74	0.63	0.37	0.48	0.53	0.21	0.22	0.29	0.71	0.32	0.68
Resp 1b	(550,550)	(400,400) vs. (750,400)	0.66	0.34	0.39	0.61	0.80	0.20	0.55	0.45	0.32	0.24	0.7	0.3	0.39	0.61
Resp 6	(100,1000)	(75,125) vs. (125,125)	0.46	0.54	0.18	0.83	0.36	0.64	0.33	0.68	0.21	0.24	0.3	0.7	0.35	0.65
Resp 7	(450,900)	(200,400) vs. (400,400)	0.95	0.05	0.10	0.90	0.83	0.18	0.29	0.71	0.14	0.17	0.83	0.17	0.13	0.87
Two-person response games: Bs sacrifice helps A																
	A stays out	If A enters, B chooses	Out	Enter	Left	Right	Out	Enter	Left	Right			Out	Enter	Left	Right
Resp 2a	(750,0)	(400,400) vs. (750,375)	0.46	0.54	0.67	0.33	0.73	0.27	0.80	0.20	0.30	0.15	0.59	0.41	0.73	0.27
Resp 2b	(550,550)	(400,400) vs. (750,375)	0.88	0.13	0.68	0.32	0.84	0.16	0.84	0.16	0.14	0.20	0.95	0.05	0.64	0.36
Resp 3	(750,100)	(300,600) vs. (700,500)	0.75	0.25	0.56	0.44	0.88	0.13	0.73	0.27	0.19	0.24	0.82	0.18	0.55	0.45
Resp 4	(700,200)	(200,700) vs. (600,600)	0.57	0.43	0.35	0.65	0.84	0.16	0.58	0.42	0.30	0.27	0.55	0.45	0.23	0.77
Resp 5a	(800,0)	(0,800) vs. (400,400)	0.75	0.25	0.46	0.54	0.89	0.11	0.59	0.41	0.24	0.18	0.81	0.19	0.45	0.55
Resp 5b	(0,800)	(0,800) vs. (400,400)	0.04	0.96	0.54	0.46	0.03	0.97	0.76	0.24	0.04	0.29	0	1	0.64	0.36
Resp 8	(725,0)	(400,400) vs. (750,375)	0.63	0.38	0.66	0.34	0.81	0.19	0.76	0.24	0.24	0.15	0.74	0.26	0.83	0.17
Resp 9	(450,0)	(350,450) vs. (450,350)	0.60	0.40	0.69	0.31	0.79	0.21	0.78	0.23	0.22	0.08	0.74	0.26	0.87	0.13
Two-person response games Bs sacrifice hurts A																
	A stays out	If A enters, B chooses	Out	Enter	Left	Right	Out	Enter	Left	Right			Out	Enter	Left	Right
Resp 10	(375,1000)	(400,400) vs. (350,350)	0.46	0.54	0.99	0.01	0.28	0.73	0.96	0.04	0.26	0.06	0.38	0.62	0.95	0.05
Resp 11	(400,1200)	(400,200) vs. (0,0)	0.76	0.24	0.95	0.05	0.57	0.43	0.89	0.11	0.21	0.07	0.82	0.18	0.91	0.09
Resp 12	(375,1000)	(400,400) vs. (250,350)	0.44	0.56	0.93	0.08	0.34	0.66	0.80	0.20	0.11	0.15	0.22	0.78	0.96	0.04
Resp 13a	(750,750)	(800,200) vs. (0,0)	0.89	0.11	0.95	0.05	0.81	0.19	0.86	0.14	0.08	0.10	0.83	0.17	0.91	0.09
Resp 13b	(750,750)	(800,200) vs. (0,50)	0.79	0.21	0.90	0.10	0.78	0.23	0.84	0.16	0.13	0.14	0.74	0.26	0.83	0.17
Resp 13c	(750,750)	(800,200) vs. (0,100)	0.85	0.15	0.91	0.09	0.81	0.19	0.73	0.28	0.10	0.19	0.78	0.22	0.78	0.22
Resp 13d	(750,750)	(800,200) vs. (0,150)	0.85	0.15	0.81	0.19	0.84	0.16	0.68	0.33	0.08	0.17	0.87	0.13	0.91	0.09

Notes:
1. Column “% A Diff” refers to the percentage of player A decisions which differentiate between ingroup and outgroup matches.
2. Column “% B Diff” refers to the percentage of player B decisions which differentiate between ingroup and outgroup matches.

APPENDIX B. Experimental Instructions

This is the experimenter's copy of instructions. Materials inside the square brackets are not displayed on the subject instructions. Beginning at Part 2, the participant has a banner displaying the group she belongs to at the top of every screen, which is not displayed here. In Part 3, we present the instructions on the sequential games using two games as examples. Instructions for other treatment sessions are identical except that the set of games are different as presented in Appendix A. Instructions for the control sessions are identical to Part 3 of the experiment except that the choices are not conditional on the group composition.

[New Screen]

This is an experiment in decision-making. The amount of money you earn will depend upon the decisions you make and on the decisions other people make. Your earnings are given in tokens. This experiment has 3 parts and 16 participants. Your total earnings will be the sum of your payoffs in each part. At the end of the experiment you will be paid IN CASH based on the exchange rate

$1 = <u>100</u> tokens.

In addition, you will be paid $5 for participation. Everyone will be paid in private and you are under no obligation to tell others how much you earn.

Please do not communicate with each other during the experiment. If you have a question, feel free to raise your hand, and an experimenter will come to help you.

[New Screen]

In Part 1 everyone will be shown 5 pairs of paintings by two artists. You will be asked to choose which painting in each pair you prefer. You will then be classified into one of two groups, based on which artist you prefer. Then you will be asked to answer questions on two other paintings. Each correct answer will bring you additional tokens. You may get help from or help other members in your own group while answering the questions.

The participants you are grouped with will be the same for the rest of the experiment.

After Part 1 has finished, we will give you instructions for the next part of the experiment.

[Waiting Screen]

[New Screen]

Now please choose which painting you prefer by clicking on either A or B from each pair.

After everyone submits answers, you will be privately informed of which group you are in.

Pair #1	1A (radio button)	1B (radio button)
Pair #2	2A (radio button)	2B (radio button)
Pair #3	3A (radio button)	3B (radio button)
Pair #4	4A (radio button)	4B (radio button)
Pair #5	5A (radio button)	5B (radio button)

[Waiting Screen]

[New Screen]

Based on your choices, you prefer the paintings by __ (Kandinsky or Klee).
You are assigned to the __ (Kandinsky or Klee) group.
The number of people in your own group is __.

[Waiting Screen]

[New Screen]
You will now receive two more paintings, painting #6 and #7. Please select the artist who you think made the paintings, respectively. For each correct answer, you will be rewarded with an additional 100 tokens. You may find the answer key to the 5 pairs of paintings useful.

Meanwhile, **you can use a group chat program to get help from or offer help to other members in your own group.** Except for the following restrictions, you can type whatever you want in the lower box of the chat program. **Messages will be shared** *only* **among all the members from your own group.** You will not be able to see the messages exchanged among the other group. People in the other group will not see the messages from your own group either.

Restrictions on messages

1. Please do not identify yourself or send any information that could be used to identify you (e.g. age, race, professional background, etc.).
2. Please refrain from using obscene or offensive language.

How to use the chat program

- Press Alt+Tab to switch to the chat program.
- Please wait while one of the experimenters comes to enter your ID number for you in the chat program.
- You can press Alt+Tab at any time to switch back and forth between the chat program and the decision screen.
- You will be given 10 minutes to communicate with your group members.

Please raise your hand if you have any questions.
My answers are:

Painting #6 is made by	Klee (radio button)	Kandinsky (radio button)
Painting #7 is made by	Klee (radio button)	Kandinsky (radio button)

[New Screen]
Please switch to the chat program by pressing Alt+Tab and close it.
You will find out your payoff from Part 1 at the end of the experiment.

[Waiting Screen]

[New Screen]
Now we start Part 2 of the experiment. You will be asked to make decisions in 5 rounds. In each round, you will have a certain number of tokens. The number varies from round to round. You will be asked to allocate these tokens between two other participants under three scenarios

1. if both are from your own group,
2. if both are from the other group, or
3. if one is from your group, and one is from the other group.

For each scenario, you must allocate *all* tokens between the two participants. Allocations have to be integers. *Do not allocate any tokens to yourself.* Your answers will be used to determine other participants' payoffs. Similarly, your payoff will be determined by others' allocations.

After everyone finishes recording their decisions, the computer will randomly select a round among the five rounds that is used to calculate the payoffs. Each round of decisions will have an equal chance of being chosen.

Next, the computer will generate a random sequence of the ID numbers. The first number in the sequence will be the ID number of the person who allocates to the second and third IDs. The second ID drawn will allocate to the third and fourth IDs, , and so on. The last ID will allocate to the first and second IDs. Therefore, your payoff will be the sum of tokens allocated to you by the two participants preceding you.

For example, the computer generates the following sequence of the ID numbers, 9, 4, 1, 5, 12, $\cdots$, 2, and 3. Then subject 9 will allocate tokens to subject 4 and 1. Subject 4 will allocate tokens to subject 1 and 5, $\cdots$, and so on. Subject 3 will allocate to subject 9 and 4. Therefore, subject 1's payoff will be the sum of the tokens allocated to her by subject 9 and subject 4.

[New Screen]

Please record your decisions under the three scenarios below.

Note: For each scenario, you must allocate *all* tokens between the two participants. Allocations have to be integers. *Do not allocate any tokens to yourself.*

Round 1

	A		B	
	A from your own group		B from your own group	
i)	( )	+	( )	= 200 tokens
	A from the other group		B from the other group	
ii)	( )	+	( )	= 200 tokens
	A from your own group		B from the other group	
iii)	( )	+	( )	= 200 tokens

[New Screen]

Please record your decisions under the three scenarios below.

Note: For each scenario, you must allocate *all* tokens between the two participants. Allocations have to be integers. *Do not allocate any tokens to yourself.*

Round 2

	A from your own group		B from your own group	
i)	( )	+	( )	= 250 tokens
	A from the other group		B from the other group	
ii)	( )	+	( )	= 250 tokens
	A from your own group		B from the other group	
iii)	( )	+	( )	= 250 tokens

[New Screen]

Please record your decisions under the three scenarios below.

Note: For each scenario, you must allocate *all* tokens between the two participants. Allocations have to be integers. *Do not allocate any tokens to yourself.*

Round 3

	A from your own group		B from your own group	
i)	( )	+	( )	= 300 tokens
	A from the other group		B from the other group	
ii)	( )	+	( )	= 300 tokens
	A from your own group		B from the other group	
iii)	( )	+	( )	= 300 tokens

[New Screen]

Please record your decisions under the three scenarios below.

Note: For each scenario, you must allocate *all* tokens between the two participants. Allocations have to be integers. *Do not allocate any tokens to yourself.*

Round 4

	A from your own group		B from your own group	
i)	( )	+	( )	= 350 tokens
	A from the other group		B from the other group	
ii)	( )	+	( )	= 350 tokens
	A from your own group		B from the other group	
iii)	( )	+	( )	= 350 tokens

[New Screen]

Please record your decisions under the three scenarios below.

Note: For each scenario, you must allocate *all* tokens between the two participants. Allocations have to be integers. *Do not allocate any tokens to yourself.*

Round 5

	A from your own group		B from your own group	
i)	( )	+	( )	= 400 tokens
	A from the other group		B from the other group	
ii)	( )	+	( )	= 400 tokens
	A from your own group		B from the other group	
iii)	( )	+	( )	= 400 tokens

[New Screen]

You will find out your payoff from Part 2 at the end of the experiment.

[New Screen]

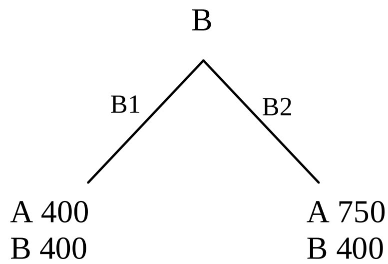
Now we start Part 3 of the experiment. You will make decisions in 7 different games. Each decision and outcome is independent of each of your other decisions, so that your decisions and outcomes in one game will not affect your outcomes in any other game.
In every game, you will be anonymously matched with one other participant. You will then be asked to make a decision under two scenarios

1. if your match comes from *your own group*;
2. if your match comes from *the other group*.

For every decision task, you will be randomly matched with a different participant than in the previous decision. Your decision may affect the payoffs of others, just as the decisions of your match may affect your payoffs.
There are roles in each game, A or B. Some games only have decisions for one role whereas other games have multiple decisions. In games with multiple decisions, these decisions will be made sequentially, in alphabetical order: person A will make a decision first and, next, person B will make a decision.
You will not be informed of the results of any previous period or game prior to making your decision.
Only two out of the seven games played will be randomly selected by the computer for computing payoffs. Each game is equally likely to be drawn.
We will proceed to the decisions once the instructions are clear. Are there any questions?

[New Screen, Game 1, Player A]

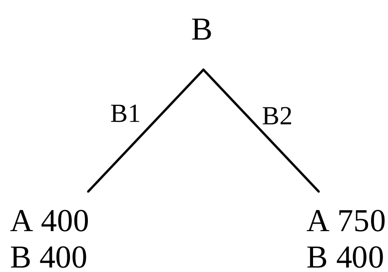
In this period, you are person A.
You have no choice in this game.
Person B's choice determines the outcome.
If person B chooses B1, you will each receive 400.
If person B chooses B2, you will receive 750, and person B will receive 400.



I have no choice in this game. Okay

[New Screen, Game 1, Player B]

In this period, you are person B.
You may choose B1 or B2.
Person A has no choice in this game.
If you choose B1, you will each receive 400.
If you choose B2,person A will receive 750 and you will receive 400.

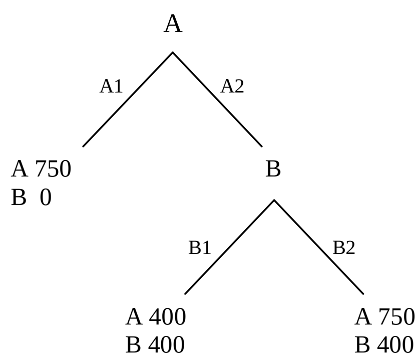


Decision
If person A is from my own group, I choose B1 (radio button) or B2 (radio button).
If person A is from the other group, I choose B1 (radio button) or B2 (radio button).

Submit

[New Screen, Game 3, Player A]

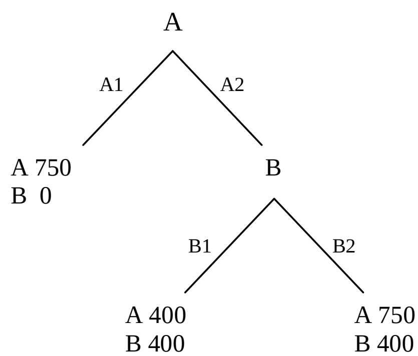
In this period, you are person A. You may choose A1 or A2.
If you choose A1, you will receive 750, and person B will receive 0.
If you choose A2, then person B's choice of B1 or B2 will determine the outcome. If you choose A2 and person B chooses B1, you will each receive 400. If you choose A2 and person B chooses B2, you will receive 750, and s/he will receive 400.
Person B will make a choice *without* being informed of your decision. Person B knows that his or her choice only affects the outcome if you choose A2, so s/he will choose B1 or B2 on the assumption that you have chosen A2 over A1.



Decision
If person B is from my own group, I choose A1 (radio button) or A2 (radio button).
If person B is from the other group, I choose A1 (radio button) or A2 (radio button).

Submit

[New Screen, Game 3, Player B]
In this period, you are person B. You may choose B1 or B2.
Person A has already made a choice. If s/he has chosen A1, s/he will receive 750, and you will receive 0. Your decision only affects the outcome if person A has chosen A2. Thus, you should choose B1 or B2 on the assumption that person A has chosen A2 over A1.
If person A has chosen A2 and you choose B1, you will each receive 400. If person A has chosen A2 and you choose B2, then person A will receive 750, and you will receive 400.



Decision
If person A is from my own group, I choose B1 (radio button) or B2 (radio button).
If person A is from the other group, I choose B1 (radio button) or B2 (radio button).

Submit

[New Screen]

You will find out your payoff from Part 3 at the end of the experiment. OK

[New Screen]

In Part 1, the correct answers to the two painting questions are

#6 by Klee

#7 by Kandinsky.

Your payoff from Part 1 is __ tokens.

In Part 2, round __ is selected to compute the payoffs.
The sequence of the ID numbers is __.
Your payoff from Part 2 is __ tokens

In Part 3, round __ and __ are selected to compute the payoffs.
Your payoff from Part 3 is __ tokens.

Your total payoff is __ tokens.

The exchange rate is $1 = 100 tokens.

The show up fee is $5.

So your earning from this experiment is $__.

Please remain seated and you will be asked to complete a survey.

Appendix C. Post-Experiment Survey

(*summary statistics in italics in parentheses*)

Please answer the following survey questions. Your answers will be used for this study only. Individual data will not be exposed.

1. What is your age? _______________ (*Mean 21.1, Std Dev 3.2, Median 21, Min 17, Max 37*)
2. What is your gender?
   (a) Female (*56.9%*)
   (b) Male (*43.1%*)
3. How many siblings do you have? _______________ (*0 siblings 10.0%, 1-2 73.4%, 3 or more 16.6%*)
4. What is your major at the University of Michigan? _______________
5. Are you an undergraduate or graduate student?
   (a) Undergraduate student (*82.3%*)
   (b) Graduate student (*17.7%*)
6. Which year are you in your program? (*Mean 2.4, Std Dev 1.3, Median 3, Min 1, Max 9*)
7. Have you ever participated in any economics or psychology experimental studies before?
   (a) Yes. (*74.1%*) Please specify the number of times _______________
   (*Mean: 5.0, Std Dev 5.2, Median 4, Min 1, Max 50*)
   (b) No. (*25.9%*)
8. What do you consider your racial or ethnic background to be?
   (a) White (*48.2%*)
   (b) Black (*11.0%*)
   (c) Hispanic (*4.0%*)
   (d) Asian (*30.3%*)
   (e) Other, please specify _______________ (*6.5%*)
9. In the past twelve months, have you donated money to or done volunteer work for charities or other nonprofit organizations?
   (a) Yes. (*72.0%*) Please specify the amount $_____ (*Mean 262, Std Dev 1,072, Median 50, Min 0, Max 13,000.*) or the number of hours _____ (*Mean 64.5 hours, Std Dev 115, Median 30, Min 0, Max 1,200.*)
   (b) No. (*28.0%*)
10. You were assigned to the _______________ group during the experiment.

(a) Klee (*39.9%*)
(b) Kandinsky (*60.1%*)

11. On a scale from 1 to 10, please rate how much you think communicating with your group members helped solve the two extra painting questions. (*Mean 6.27, Std Dev 2.97, Median 7, Min 1, Max 10*)

12. On a scale from 1 to 10, please rate how closely attached you felt to your own group throughout the experiment. (*Mean 4.0, Std Dev 2.7, Median 3, Min 1, Max 10*)

13. In Part 2 when you were asked to allocate money between two other participants, how would you describe the strategies you used?

    (a) Try to allocate money equally between them. (*38.0%*)
    (b) Try to allocate more money to the one who was from your own group. (*46.6%*)
    (c) Try to allocate more money to the one who was from the other group. (*1.3%*)
    (d) Randomly (*8.6%*)
    (e) Other. (*5.5%*) Please specify ______________

14. In Part 3 when you were asked to decide on payoffs received by your match and yourself, how would you describe the strategies you used? Please select all that apply.

    (a) Try to earn as much money as possible for myself. (*52.2%*)
    (b) Try to earn as much money as possible for me and my match. (*49.4%*)
    (c) Try to earn more money than my match. (*6.3%*)
    (d) Reward those who were nice to me and punish those who were nasty to me. (*8.9%*)
    (e) Other. (*9.3%*) Please specify ______________

15. In Part 3 when you were asked to decide on payoffs received by your match and yourself, did it affect your decision in any way which group your match came from?

    (a) Yes (Go to Question 16) (*34.5%*)
    (b) No (Go to Question 17) (*65.5%*)

16. Please tell us how your match's group membership affected your decision. Compared with having a match from the other group, if I was matched with someone from my own group:

    (a) I was more likely to choose equal payoff. (*34.2%*)
    (b) I was more likely to be nice to my match when she was nice to me. (*12.6%*)
    (c) I was more likely to punish my match if she was not nice to me. (*2.0%*)
    (d) I was more likely to choose actions that increase our total payoff. (*36.7%*)
    (e) I was more likely to help him/her at my own expense. (*3.5%*)
    (f) Other. (*11.1%*) Please specify ______________.

17. On a scale from 1 to 10, please rate how familiar you were with the paintings made by Klee and Kandinsky, respectively, before this experiment. (*Klee: Mean 1.5, Std Dev 1.5, Median 1, Min 1, Max 10. Kandinsky: Mean 1.9, Std Dev 2.0, Median 1, Min 1, Max 10*)

Appendix D. A CES Model of Group Identity and Social Preference

In this appendix, we provide an alternative model of group identity and social preference by applying the model of Cox, Friedman and Gjerstad (2007) (CFG hereafter) which is based on a modified CES utility function. This model relaxes the assumption of linearity in our model. Specifically, player B's utility function is defined as

$$u_B(\pi_A, \pi_B) = \begin{cases} \frac{1}{\alpha}(\theta\pi_A^\alpha + \pi_B^\alpha), & \text{if } \alpha \in (-\infty, 0) \cup (0, 1]; \\ \pi_B\pi_A^\theta, & \text{if } \alpha = 0. \end{cases}$$

The elasticity of substitution parameter, α, captures the convexity of indifference curves. The emotional state, θ can be decomposed into three parts: (1) the residual benevolence or malevolence parameter, θ_0, that indicates unconditional altruism, (2) response to reciprocity, $\tilde{\theta}(r, s)$, which is a function of the reciprocity motive, r, and the status motive, s, and (3) a mean zero error term ε. In sum, $\theta = \theta_0 + \tilde{\theta}(r, s) + \varepsilon$, where $\tilde{\theta}(r, s) = 0$ in dictator games.

Applying the first-order Taylor series expansion to $\tilde{\theta}$ yields $\tilde{\theta} = a \cdot \frac{m - \tau_I \cdot m_0 \cdot s - \tau_O \cdot m_0 \cdot (1-s)}{m_g - m_b}$, where a captures the responsiveness to reciprocity. Let m denote the maximum payoff player B can guarantee herself given player A's choice is *Enter*. The variable m_0 represents the property right (entitlement) in a neutral setting, e.g., in the control sessions with no groups. Unlike the original CFG model in which the status-based property right variables are endogenous for each individual game, we define ingroup (or outgroup) property rights as the product of an ingroup multiplier τ_I (or an outgroup multiplier τ_O) and an exogenous neutral property right m_0.[28] For robustness check, we define m_0 in two ways, either as the average player B's payoff assuming player A's choice is *Enter*, or as the average of player B's payoff weighted by the distribution of actual choices in the *control* sessions assuming player A's choice is *Enter*. The intergroup difference is captured by the indicator variable s which takes a value of 1 if player B's match comes from the ingroup, or a value of 0 if her match comes from the outgroup. We further normalize the reciprocity variable, i.e., $(m - \tau_I \cdot m_0)$ for ingroup or $(m - \tau_O \cdot m_0)$ for outgroup, by $(m_g - m_b)$, where m_g(or m_b) represents the maximum (or minimum) of player B's payoff unconditional on player A's choices.

The idiosyncratic error term ε is assumed to have the exponential power distribution in CFG. However, since their estimates of the parameters in the dictator and ultimatum games reduce the distribution to a uniform distribution, we assume that the distribution has a uniform density on $[-b, b]$.

Player B's decision is to choose action, *Left* or *Right*, to maximize $u_B(\pi_A, \pi_B)$. In the data analysis, we code the choice of *Left* as $y_i = 1$ and *Right* as $y_i = 0$. Since $\alpha > 0$, it implies that $y_i = 1$ if and only if $\theta\pi_{AL}^\alpha + \pi_{BL}^\alpha > \theta\pi_{AR}^\alpha + \pi_{BR}^\alpha$.[29] That is, $y_i = 1$ if and only if

$$\begin{aligned} &\varepsilon < \frac{\pi_{BL}^\alpha - \pi_{BR}^\alpha}{\pi_{AR}^\alpha - \pi_{AL}^\alpha} - \theta_0 - \tilde{\theta}(r, s) && \text{when } \pi_{AR} > \pi_{AL}, \text{ or} \\ &\varepsilon < -\frac{\pi_{BL}^\alpha - \pi_{BR}^\alpha}{\pi_{AR}^\alpha - \pi_{AL}^\alpha} + \theta_0 + \tilde{\theta}(r, s) && \text{when } \pi_{AR} < \pi_{AL}. \end{aligned}$$

Let p_{ig} denote the probability that participant i of role B chooses action *Left* in game g. Then, p_{ig} is the cumulative error function distribution $F(z; b) = \frac{z+b}{2b}$ evaluated at

[28] Since there are 19 response games in our study, making the property right variables endogenous for each individual game would dramatically increase the number of parameters that need to be estimated and make the maximum likelihood estimation infeasible.

[29] For $\alpha \leq 0$, player B's utility is 0 or $-\infty$ by choosing *Left* in Dict 5, and Resp 5a-5b, and *Right* in Resp 11, and Resp 13a-13d. Since these choices occur in the observed data we must have $\alpha > 0$.

$$z = \begin{cases} \frac{\pi^{\alpha}_{BL} - \pi^{\alpha}_{BR}}{\pi^{\alpha}_{AR} - \pi^{\alpha}_{AL}} - \theta_0 - \tilde{\theta}(r,s), & \text{if } \pi_{AR} > \pi_{AL}; \\ -\frac{\pi^{\alpha}_{BL} - \pi^{\alpha}_{BR}}{\pi^{\alpha}_{AR} - \pi^{\alpha}_{AL}} + \theta_0 + \tilde{\theta}(r,s), & \text{if } \pi_{AR} < \pi_{AL}. \end{cases}$$

Thus, the log likelihood function is $\ln L = \sum_{g} \sum_{i} (y_{ig} \ln p_{ig} + (1 - y_{ig}) \ln(1 - p_{ig}))$. Table 8 reports the parameter estimates and standard errors. In panel A, $m_0$is coded as the average player B's payoff assuming player A chooses *Enter*. In panel B, it is coded as the average of player B's payoff weighted based on the sample's actual choices assuming player A chooses *Enter* in the *control* sessions.

[Table 8 about here.]

We conduct the likelihood ratio tests for three hypotheses, respectively, 1) the residual benevolence or malevolence term $\theta_0 > 0$; 2) player B responds appropriately to reciprocity and status concerns, i.e., $a > 0$; and 3) the ingroup multiplier of entitlement is smaller than the outgroup one, i.e., $\tau_I < \tau_O$. The likelihood ratio tests reject all the three null hypotheses in favor of the alternatives at the 1% level.

Rejection of $\theta_0 = 0$ in favor of $\theta_0 > 0$ indicates that on average, player B's utility is strictly increasing in own payoff *and* her match's payoff, which suggests the unconditional altruism by player B.

Rejection of $a = 0$ in favor of $a > 0$, i.e., the emotional state function θ is increasing in the reciprocity motive r, is consistent with the findings in CFG and our findings using the Charness and Rabin model.

Furthermore, rejection of $\tau_I = \tau_O$ in favor of $\tau_I < \tau_O$ shows that the emotional state function θ is increasing in status variable s. Recall that status variable takes the value of 1 if the match comes from the ingroup and 0 otherwise. Hence, $\tau_I < \tau_O$ suggests that the weight put on player A's payoff in player B's utility function is higher for an ingroup player A than for an outgroup one. This result is consistent with our findings on reciprocal preferences using the empirical models — participants are more likely to reward an ingroup match for good behavior, but less likely to punish an ingroup match for misbehavior.

Since the convexity parameter of indifference curves, α, captures the distribution/relative payoffs, we extend the CFG model to incorporate the group-dependent distributional concern by separately estimating ingroup and outgroup α. The maximum likelihood estimates and standard errors are presented in Table 9. Likelihood ratio tests show that ingroup α is significantly smaller than outgroup α ($p < 0.01$), suggesting that the indifference curves for ingroup match are more convex than those for outgroup match. In other words, own payoffs and the match's payoffs relate to each other more like complements for ingroup match, and substitutes for outgroup match, respectively. Other parameter estimates are similar to those in the model with homogeneous α.

[Table 9 about here.]

Although the CFG model based on the non-linear utility function is more general than the linear model in Charness and Rabin (2002, CR hereafter), the latter has several advantages. First, CR allows us to parse out the charitable concern from envy explicitly, while the distributional preferences are integrated in α (curvature of the indifference curves) and θ (unconditional altruism) in the CFG model. Second, CR allows the positive and negative reciprocal preferences to have different sizes, while CFG assumes the sensitivity to reciprocity (captured by the parameter a) has the same magnitude regardless of the directions of the reciprocal concerns. Third, due to the linearity feature of the utility, the parameters in the CR model yield more intuitive interpretations on the ingroup and outgroup effects. Since the results from the CFG model don't contradict those from the CR model, we will focus on the latter in the paper.

Treatments	Group Assignment	Chat	Other-Other	Within/Between	# Sessions	# Subjects (A)
Control	n/a	no	no	n/a	9	134 (133)
Original	painting	yes	yes	within	15	240 (237)
NoChat	painting	no	yes	within	4	64 (64)
NoHelp	painting	no	no	within	2	32 (32)
RandomWithin	random	yes	yes	within	2	32 (32)
RandomBetween	random	yes	yes	between	4	64 (64)
Total					36	566 (562)

Notes:
The last column (in brackets) indicates the number of subjects used in data analysis.

Table 1: Features of Experimental Sessions

	Charity	Envy				
	ρ	σ				
Control	0.427	-0.049				
($N = 536$)	(0.022)***	(0.025)**				
	Outgrp Charity	Outgrp Envy	Ingrp Charity	Ingrp Envy	Identity Parameters	
	ρ_o	σ_o	$\rho_o(1+a)$	$\sigma_o(1+b)$	a	b
Treatment	0.323	-0.112	0.474	-0.008	0.467	-0.931
($N = 1896$)	(0.021)***	(0.019)***	(0.018)***	(0.021)	(0.112)***	(0.192)***

Notes:
1. The top panel reports estimates for the control sessions without identity, while the bottom panel reports estimates for treatment sessions with identity.
2. Standard errors in parentheses are clustered at the individual level.
3. Significant at: * 10% level; ** 5% level; *** 1% level.

Table 2: Distribution Preferences: Maximum Likelihood Estimates for Player B Behavior

Dependent Variables	Prob(B rewards A)		Prob(B punishes A)	
	Control	Treatment	Control	Treatment
	(1)	(2)	(3)	(4)
Ingroup match		0.218		-0.128
		(0.035)***		(0.027)***
Benefit to B due to A's entry	0.453	0.151		
	(0.436)	(0.105)		
B's cost to reward A	-0.328	-0.114		
	(0.232)	(0.063)*		
Benefit to A if B rewards	0.204	0.076		
	(0.053)***	(0.032)**		
How much B's payoff is behind A's	-0.130	-0.077		
if B rewards	(0.047)***	(0.024)***		
Damage to B due to A's entry			0.018	-0.001
			(0.018)	(0.009)
B's cost to punish A			-0.265	-0.316
			(0.071)***	(0.047)***
Damage to A if B punishes			0.040	0.042
			(0.019)**	(0.009)***
How much B's payoff is ahead of A's			-0.171	-0.103
if B punishes			(0.070)**	(0.029)***
Constant	-2.148	-0.849	-0.211	-0.049
	(1.681)	(0.434)*	(0.100)**	(0.053)
Observations	156	550	250	874
Pseudo R-square	0.12	0.06	0.13	0.19

Notes:

1. (1) and (2) include Resp 5a, Resp 1a, Resp 2a, Resp 3, Resp 4, Resp 8 and Resp 9.
2. (3) and (4) include Resp 2b, Resp 10, Resp 11, Resp 1b, Resp 6, Resp 7, Resp 12, Resp 13a-d.
3. Coefficients are probability derivatives.
4. Non-index variables are measured in unit of 100 tokens.
5. Standard errors in parentheses are clustered at the individual level.
6. Significant at: * 10% level; ** 5% level; *** 1% level.

Table 3: Logit Regression: Determinants of Reciprocity

	Matching Conditions			Alternative Hypotheses and P-values		
	Ingroup	Outgroup	Control	Ingr > Outgr	Ingr > Contr	Contr > Outgr
Player A	0.629	0.509	0.57	0.000	0.047	0.048
	[676]	[676]	[381]			
Player B	0.68	0.529	0.638	0.000	0.095	0.001
	[790]	[790]	[447]			
Over all	0.656	0.520	0.606	0.000	0.022	0.000
	[1466]	[1466]	[828]			

Notes:

1. Games Dict 5, Resp 5a and 5b and role B in game Resp 9 are excluded, as all outcomes yield the same aggregate payoff.
2. Number of observations is in square brackets.
3. P-values are computed based on standard errors clustered at the individual level.

Table 4: Proportion of SWM Decisions and the Effects of Social Identity

	Matching Conditions			Alternative Hypotheses and P-values		
	Ingroup	Outgroup	Control	Ingr > Outgr	Ingr > Contr	Contr > Outgr
			Expected Earnings			
Player A	521.5	507.6	519.7	0.001	0.424	0.107
	[945]	[949]	[533]			
Player B	504.6	459.2	485.8	0.000	0.054	0.013
	[938]	[942]	[536]			
Over all	513.1	483.5	502.7	0.000	0.114	0.016
	[1883]	[1891]	[1069]			
			Actual Earnings			
Player A	526.7	506.5	522.2	0.095	0.362	0.127
	[464]	[487]	[533]			
Player B	501.5	463.5	486.4	0.023	0.201	0.100
	[463]	[485]	[536]			
Over all	514.1	485.0	504.3	0.011	0.198	0.057
	[927]	[972]	[1069]			

Notes:
1. Earnings are in tokens.
2. Number of observations is in square brackets.
3. P-values are computed based on standard errors clustered at the individual level.

Table 5: The Effects of Social Identity on Expected and Actual Earnings

Proportion of Participants who Differentiate between Ingroup and Outgroup Matches				
	Original	NoChat	NoHelp	RandomWithin
Role A	0.39	0.45	0.56	0.34
Role B	0.39	0.45	0.56	0.44
P-Values of Two-sided t-tests for the Equality of Proportions				
	Original vs. RandomWithin	Original vs. No Chat	No Chat vs. No Help	Original vs. No Help
Role A	0.65	0.33	0.32	0.06
Role B	0.60	0.36	0.32	0.06

Table 6: Proportion of Participants who Differentiate between Ingroup and Outgroup Matches

Dependent Variable:	Self-reported group attachment	
	OLS	Ordered Logit
Paintings	0.260	0.210
	(0.653)	(0.486)
Chat	1.135	0.792
	(0.216)***	(0.121)***
O-O Allocation	-0.313	0.010
	(0.308)	(0.114)
Within-subject	-1.187	-0.817
	(0.778)	(0.544)
Constant	4.178	
	(0.551)***	
Observations	426	426
R-squared	0.04	0.014

Notes:
1. Standard errors are clustered at the session level.
2. R-squared is the adjusted R-squared for OLS and pseudo R-squared for ordered logit.
3. Significant at: *** 1% level.

Table 7: Effects of Design Components on Self-Reported Group Attachment

	Panel A		Panel B	
Parameter	Estimate	Robust SE	Estimate	Robust SE
α	0.6	0.11	0.48	0.12
a	1.35	0.52	0.99	0.62
b	1.27	0.07	1.27	0.07
θ_0	0.28	0.09	0.31	0.09
τ_I	1.1	0.06	1.11	0.09
τ_O	1.26	0.1	1.33	0.2
Log likelihood	-1127.07		-1131.83	

Table 8: Maximum Likelihood Results from CFG Model with Homogeneous α

	Panel A		Panel B	
Parameter	Estimate	Robust SE	Estimate	Robust SE
Ingroup α	0.39	0.08	0.32	0.07
Outgroup α	0.83	0.11	0.76	0.11
a	1.4	0.46	1.2	0.54
b	1.27	0.07	1.26	0.07
θ_0	0.28	0.09	0.32	0.1
τ_I	1.11	0.05	1.12	0.07
τ_O	1.24	0.07	1.27	0.11
Log likelihood	-1122.26		-1127.55	

Table 9: Maximum Likelihood Results from CFG Model with Heterogeneous α

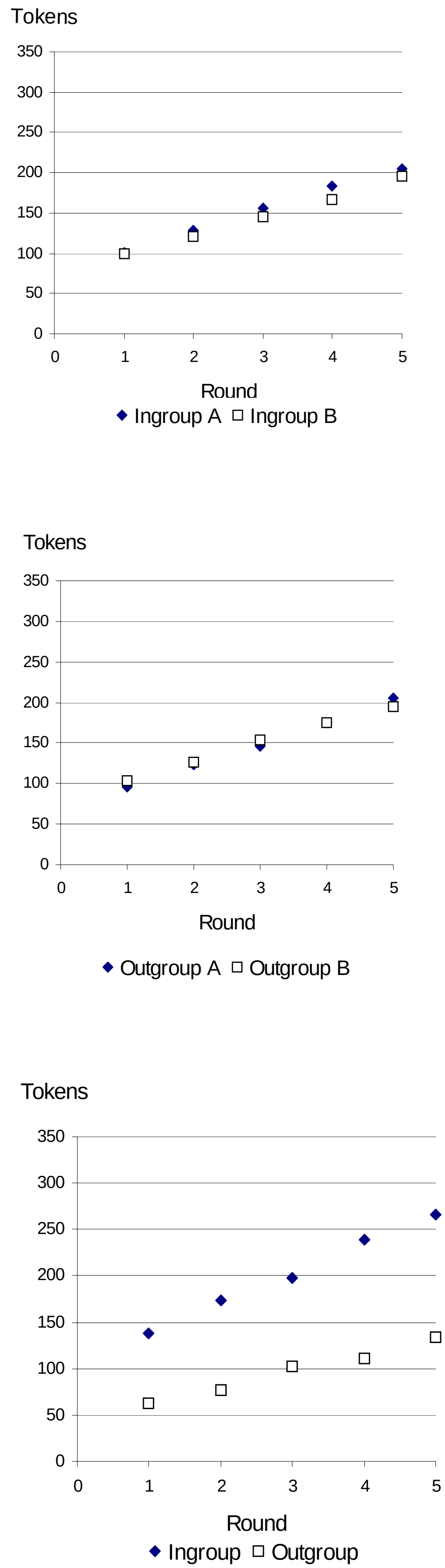


Figure 1: Other-Other Allocations in the Original Treatment

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