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Group Identity and Social Preferences

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We present a laboratory experiment that measures the effects of induced group identity on social preferences. We find that when participants are matched with an ingroup member, they show a 47 percent increase in charity concerns and a 93 percent decrease in envy. Likewise, participants are 19 percent more likely to reward an ingroup match for good behavior, but 13 percent 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 toward an ingroup match. (*JEL* C91, D03, Z13)

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 (Rose McDermott, forthcoming), and the formation of human capital (James Coleman 1961).

Social identity theory was developed by Henri Tajfel and John 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 is a way 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 we do not 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.

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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 and adopts behaviors that are consistent with the stereotypes associated with the group identity. For example, Margaret Shih, Todd L. Pittinsky, and Nalini 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 pretest 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, Pittinsky, and Ambady (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. More recently, Daniel J. Benjamin, James J. Choi, and A. Joshua Strickland (2007) find that making ethnic, racial, or gender identity salient also causes risk and time preferences to conform to common stereotypes.

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 George A. Akerlof and Rachel E. Kranton (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 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, Roland Bénabou and Jean 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 (SWM) 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 et al. 1971), we induce group identity using participant artwork preferences. Furthermore, we compare different group assignment methods and their effects on behavior. Unlike social psychology experiments, however, 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,

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

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 Gary Charness and Matthew 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 toward ingroup members? Are they more likely to forgive or to punish perceived bad intentions of ingroup members? Third, are they more likely to choose SWM actions when matched with an ingroup member compared to when matched with an outgroup member? Finally, by varying the experimental design, we can address the question of what creates group effects.

The rest of the paper is organized as follows. Section I reviews the social psychology and experimental economics literature on social identity. Section II presents the experimental design. Section III presents the analysis and results. Section IV concludes.

I. Literature Review

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

A. *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), Kay Deaux (1996), Michael A. Hogg (2002), 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: priming natural social identities, and 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 often used in social identity research. Research in social psychology has found that subtly making different natural social identities salient through priming can affect behavior and outcomes, such as test performance (Joshua Aronson, Dianne Quinn, and Steven Spencer 1998), walking speed (John Bargh, Mark Chen, and Lara Burrows 1996) or person perception (Bargh and Paula Pietromonaco 1982).

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 nonoverlapping 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 (Brian Mullen, Rupert Brown, and Colleen 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 trade-offs between self-interest and group interest.

Summarizing 15 years 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."[2] Several factors have been found to enhance or mitigate ingroup bias, for example, category salience, group status, and relevance of the comparison dimensions (Mullen, Brown, and Smith 1992). Furthermore, summarizing 40 years of social psychological research on intergroup relations, Marilynn B. Brewer (1999) concludes that ingroup formation and attachment is psychologically primary while attitudes toward outgroups is not.

More recently, however, a number of studies have not found 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, Toshio Yamagishi and Toko 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.

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 (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 Michael D. Silver 2000), maximizing intergroup differences (Tajfel and Turner 1986), and the metacontrast principle (Turner 1985), i.e., maximizing intergroup 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 uses 24 games incorporating a wide variety of incentives, which enables us to get a robust estimate of the functional forms and parameters.

B. *Social Identity Research in Experimental Economics*

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

[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.

In economic experiments that prime natural identities, gender and ethnicity in particular, the results are mixed. On the one hand, Jamie Brown-Kruse and David Hummels (1993) and C. Bram Cadsby and Elizabeth Maynes (1998) use a pregame 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. On the other hand, John L. Solow and Nicole Kirkwood (2002) and Rachel T. A. Croson, Melanie B. Marks, and Jessica Snyder (2003) find that the effect of gender on levels of contribution is significant. Interestingly, Croson, Marks, and Snyder (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.

Three recent studies using natural groups find significant effects of group identity on behavior. Helen Bernhard, Ernst Fehr, and Urs Fischbacher (2006) use a dictator game experiment with third-party punishment in two distinct, native social groups in Papua New Guinea. They find that third parties show stronger altruism toward 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. Lorenz Goette, David Huffman, and Stephan 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, Fehr, and Fischbacher (2006), they also find that third-party punishment is stronger when a violation affects an ingroup member as opposed to an outgroup member. Lastly, Tomomi Tanaka, Colin F. Camerer, and Quang Nguyen (2008) find that group behavior in Vietnamese village communities is affected by the status of the ethnic groups. While Khmer (poor minority) show strong ingroup favoritism, Vietnamese (majority) and Chinese (rich minority) do not show ingroup bias when they are matched with Khmer, but do so when they are matched with each other.

In comparison with natural group identities, using induced identities might give the experimenter more control over the identity formation process. The extent to which induced identity affects behavior depends on the strength of the social identity. Catherine C. Eckel and Philip J. 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. 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, Luca Rigotti, and Aldo Rustichini (2007) report a series of experiments on the effects of group membership on individual behavior in prisoner's 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. Kendra N. McLeish and Robert J. 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.

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 group identity on social preferences in a wide variety of games and by evaluating the effects of various components in creating group identity.

II. 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. Furthermore, 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.

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.

A. *Stage 1: Group Assignment*

All five experimental treatments contain 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, with one painting within each pair by Klee, and the other by Kandinsky.[4] 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.

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 RandomWithin 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 enables 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 enables us to evaluate any experimenter demand effects in the original within-subject design. The latter also enables 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 to the artists.[5] In the RandomWithin and RandomBetween

[4] Wassily Kandinsky (1866–1944) was one of the first creators of pure abstraction in modern painting. His friend, Paul Klee (1879–1940), was also among the significant modern artists of the twentieth century. Their paintings were used by classic studies of social identity in social psychology (Tajfel et al. 1971).

[5] 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.

treatments, participants were given the five pairs of paintings along with the answer keys. They had five minutes to study these paintings to prepare them for the second stage.

B. *Stage 2: Online Chat*

After being assigned into groups, subjects in the Original, RandomWithin, and RandomBetween 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.[6] 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. The subjects were allowed to discuss any information during chatting, but 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.[7] This part of the design is used to enhance group identity.

Since the online chat might have created a feeling of generalized reciprocity toward 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 enables us to identify the effects of the online chat component on behavior.

C. *Stage 3: Other-Other Allocation*

In the third stage of the Original, RandomWithin, RandomBetween, and NoChat treatments, 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 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.[8]

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.[9] 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

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

[7] Seventy-seven percent of the participants provided correct answers to both paintings, 19 percent provided one correct answer, and only 4 percent provided zero correct answers.

[8] 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.

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

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 enables us to identify the effects of other-other allocation.

D. *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), as well as an extension of some of the games.[10] The Appendix presents a description of the set of games and the summary statistics for each game. Specifically, we selected 5 two-person dictator games and 16 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 the game 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.[11] In all other treatments, subjects made decisions in 12 games in each session.[12] 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.

E. *Summary*

In sum, we conducted one control and five treatments. We have a total of 36 independent sessions. At the end of each experimental session, we conducted a postexperiment 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

[10] We thank Gary Charness for helping us select the games and sharing data and programs.

[11] 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.

[12] 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 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.

TABLE 1—FEATURES OF EXPERIMENTAL SESSIONS

Treatments	Group assignment	Chat	Other-Other	Within/Between	No. sessions	No. 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)

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

and response statistics, experimental instructions, and data are included in the supplemental material.

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, and the number of subjects used in the analysis (the last column in brackets). Overall, 36 independent computerized sessions were conducted in the Research Center for Group Dynamics (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.[13] We used z-Tree (Fischbacher 2007) to program our experiments. Most of our subjects were students from the University of Michigan.[14] Participants were allowed to participate in only one session. Each treatment session lasted approximately one hour, whereas each control session lasted about 30 to 35 minutes.[15] 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.

III. Results

In this section, we first investigate how group identity affects participant social preferences. We then address the question of what creates the group effects.

Several common features apply throughout our analysis and presentation. First, our general null hypothesis is that behavior does not differ between the treatments. Second, we use a 5 percent statistical significance level as our threshold (unless stated otherwise) to establish the significance of an effect.

A. *Effects of Group Identity on Social Preferences*

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 B.

[13] Despite our announcement that each subject can participate in only one session and our pre-experiment screening, four subjects participated twice. In all analyses, we exclude the second-session data for these subjects.

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

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

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 made decisions under three scenarios: if the two other individuals came from her own group; if they came from the other group; and if one came 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 presents the average allocation 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-left panel presents the average allocation between two ingroup members. The top-right 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 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 significantly more than that allocated to an outgroup member (a square). The difference between ingroup and outgroup allocations (normalized by endowment) is between 32.2 percent and 38.4 percent, statistically significant at the 1 percent level for each of the five rounds (*t*-statistics for one-tailed tests for paired samples). Therefore, 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.[16] 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.[17] To illustrate, let π_A and π_B be players A's 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

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

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

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

[16] In addition to Charness and Rabin (2002), key social preference models include Rabin (1993), David K. Levine (1998), Fehr and Klaus M. Schmidt (1999), Gary E. Bolton and Axel Ockenfels (2000), Amin Falk and Fischbacher (2006), and James C. Cox, Daniel Friedman, and Steven Gjerstad (2007).

[17] A CES model of social preference incorporating both distribution and reciprocal preferences, which extends the model of Cox, Friedman, and Gjerstad (2007), is estimated and discussed in Yan Chen and Sherry Xin Li (2008).

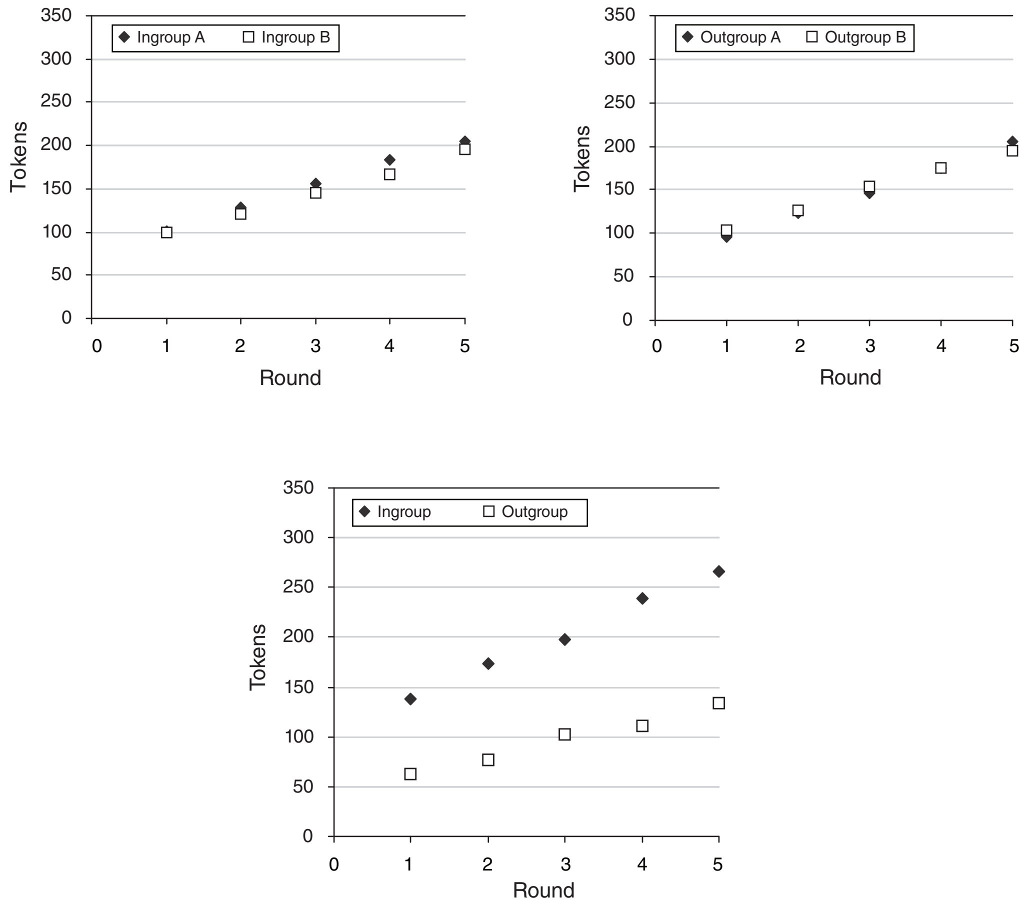

Figure 1. Other-Other Allocations in the Original Treatment

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

$$u_B(\pi_A, \pi_B) = w_A^I \, \pi_A + (1 - w_A^I)\, \pi_B. \tag{1}$$

We use player B's data from the sequential games to estimate the parameters of equation (1). Our maximum-likelihood estimation on our binary-response data uses a logit specification:

$$\Pr(action1) = \frac{e^{\gamma u(action1)}}{e^{\gamma u(action1)} + e^{\gamma u(action2)}},$$

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

TABLE 2—DISTRIBUTION PREFERENCES: MAXIMUM LIKELIHOOD ESTIMATES FOR PLAYER B BEHAVIOR

Panel A:	Charity	Envy				
	ρ	σ				
Control	0.427	−0.049				
(N = 536)	(0.022)***	(0.025)**				
Panel B:	Outgroup charity	Outgroup envy	Ingroup charity	Ingroup 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 = 1,896)	(0.021)***	(0.019)***	(0.018)***	(0.021)	(0.112)***	(0.192)***

Notes: Panel A reports estimates for the control sessions without identity, while panel B reports estimates for treatment sessions with identity.
*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

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

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 1 (Distribution Preferences): *Participants exhibit charity (envy) when their match receives a lower (higher) payoff than they do. Their charity (envy) toward an ingroup match is significantly greater (less) than that toward an outgroup match.*

Support. In Table 2, the charity parameter ρ is 0.427 for the control sessions, while in the Original treatment, $\rho_o = 0.323$ for outgroup matches, and $\rho_i = \rho_o(1 + a) = 0.474$ for ingroup matches. All estimates are statistically significant at the 1 percent level. Likewise, the envy parameter σ is -0.049 for the control sessions and $\sigma_o = -0.112$ for outgroup matches, whereas it is $\sigma_i = \sigma_o\,(1 + b) = -0.008$ for ingroup matches. The parameter estimates for envy are statistically significant at the 5 percent level for the control and 1 percent level for the outgroup matching. We cannot reject that it is zero for the ingroup matching. The effect of group identity on charity is measured by the parameter $a = 0.467$ ($p < 0.01$), while the effect of group identity on envy is measured by the parameter $b = -0.931$ ($p < 0.01$).

Result 1 indicates that, when participants have a higher payoff, they show a 47 percent increase in charity concerns toward an ingroup match compared with an outgroup match. When participants have a lower payoff than their ingroup match, they show a 93 percent decrease in envy. This is the first main result of the paper, which rejects the null hypothesis that group identity has no influence on participant distribution preferences. Rewriting equation (1) with the estimated charity parameters yields

$$u_B(\pi_A^i, \pi_B) = 0.474\pi_A^i + 0.526\pi_B, \text{ when } \pi_B > \pi_A^i, \tag{2}$$

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

$$u_B\,(\pi_A^o, \pi_B) = 0.323\pi_A^o + 0.677\pi_B, \text{ when } \pi_B > \pi_A^o, \tag{3}$$

when A is an outgroup match, and

$$u_B(\pi_A^c, \pi_B) = 0.427\pi_A^c + 0.573\pi_B, \text{ when } \pi_B > \pi_A^c, \tag{4}$$

when A is a match in the control sessions.

Similarly, it is informative to rewrite equation (1) using the estimated envy parameters. For an ingroup match, this yields

$$u_B(\pi_A^i, \pi_B) = -0.008\pi_A^i + 1.008\pi_B, \text{ when } \pi_{\mathrm{B}} < \pi_A^i, \tag{5}$$

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

$$u_B(\pi_A^o, \pi_B) = -0.112\pi_A^o + 1.112\pi_B, \text{ when } \pi_B < \pi_A^o, \tag{6}$$

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

$$u_B(\pi_A^c, \pi_B) = -0.049\pi_A^c + 1.049\pi_B, \text{ when } \pi_B < \pi_A^c. \tag{7}$$

Result 1 suggests that group identity affects 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 (2)–(7) highlight the difference between our identity model and altruism models such as that of Kaushik Basu (2006), where the weight on the other person's payoff is independent of payoff distributions.

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 three other explanatory variables, including B's cost to reward A, A's benefit from B's reciprocation, and B's payoff lag when B rewards A. Player B's cost of reciprocation is measured as her payoff difference when choosing the reciprocating action over the alternative. Player A's benefit from B's reciprocation is computed as the gain in A's payoff if B chooses to reciprocate. Player B's payoff lag is measured by how much B's payoff falls behind A's when B chooses to reward A. Since, by rewarding A, B gets a payoff that never exceeds her match's in all the positive-reciprocity games, the difference in their payoffs enables us to examine how B's envy affects positive reciprocity.

Table 3 presents the results of logit specifications for factors that determine B's likelihood to reciprocate. The coefficients are probability derivatives. The unit of these variables is 100 tokens in the regressions.

Panel A presents the results of logit specifications for the control (column 2) and the treatment (column 3). In column 4, we further interact each of the covariates with the ingroup dummy to examine group-contingent effects. As the estimates in the treatment are largely consistent with those in the control, we will focus on the former for our discussions.

Results in column 3 indicate that on average participants are significantly more likely to reward ingroup members. An ingroup matching increases B's likelihood to reward A by 18.6 percent ($p < 0.01$). Player B also cares about the outcome of her choice to reward A. Specifically, a 100-token increase in the benefit to A increases B's likelihood to reward by 8.9 percent ($p < 0.01$). Furthermore, we find an effect of distributional concerns on positive reciprocity, i.e., player B is

Table 3—Logit Regression: Determinants of Reciprocity

Panel A	Probability (B rewards A)		
	Control	Treatment	Treatment
Ingroup		0.186	0.117
		(0.028) ***	(0.083)
Cost to reward	−0.101	−0.041	−0.019
	(0.055)*	(0.034)	(0.037)
Benefit to A if B rewards	0.206	0.089	0.055
	(0.055)***	(0.033)***	(0.035)
B's payoff behind A if B rewards	−0.120	−0.078	−0.056
	(0.047)***	(0.025)***	(0.027)**
(Cost to reward) × Ingroup			−0.055
			(0.033)*
(Benefit to A) × Ingroup			0.076
			(0.025)***
(B behind A) × Ingroup			−0.160
			(0.051)***
Constant	−0.420	−0.282	−0.235
	(0.199)**	(0.117)**	(0.122)*
Observations	156	550	550
Log-likelihood function	−94.58	−348.98	−347.66
Pseudo R^2	0.113	0.063	0.067

Panel B	Probability (B punishes A)		
	Control	Treatment	Treatment
Ingroup		−0.128	−0.117
		(0.027)***	(0.035)***
Cost to punish	−0.206	−0.320	−0.262
	(0.043)***	(0.038)***	(0.039)***
Damage to A if B punishes	0.028	0.043	0.035
	(0.012)**	(0.008)***	(0.008)***
B's payoff ahead of A if B punishes	−0.133	−0.105	−0.080
	(0.042)***	(0.025)***	(0.025)***
(Cost to punish) × Ingroup			0.084
			(0.041)**
(Damage to A) × Ingroup			−0.011
			(0.008)
(B ahead of A) × Ingroup			−0.131
			(0.030)***
Constant	−0.121	−0.056	−0.064
	(0.042)***	(0.028)**	(0.027)**
Observations	250	874	874
Log-likelihood function	−110.65	−398.06	−395.72
Pseudo R^2	0.125	0.191	0.196

Notes: Panel A includes Resp 5a, Resp 1a, Resp 2a, Resp 3, Resp 4, Resp 8, and Resp 9. Panel B includes Resp 2b, Resp 10, Resp 11, Resp 1b, Resp 6, Resp 7, Resp 12, and Resp 13a–d. Standard errors in parentheses are clustered at the individual level.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

less likely to return A's favor if doing so causes her to get a lower payoff than A. Specifically, a 100-token increase in the payoff gap decreases B's likelihood to reward A by 7.8 percent ($p < 0.01$). This suggests that envy restrains positive reciprocity.

Results in column 4 suggest that the explanatory variables exhibit group-contingent effects. First, cost interacted with the ingroup dummy enters with a marginal effect -0.055 ($p < 0.10$), which suggests a marginally stronger response to the cost of positive reciprocity toward an ingroup than toward an outgroup match. Furthermore, given how much B's choice would benefit A, B is more likely to reward an ingroup A, as indicated by the marginal effect of the interaction between the variables Benefit to A and the ingroup dummy (0.076, $p < 0.01$). Lastly, the marginal effect of the interaction term of the ingroup dummy and how much B is behind A is -0.160 ($p < 0.01$), which suggests that the negative effect of envy on positive reciprocity is stronger toward an ingroup match.

Panel B of Table 3 presents a similar analysis for the negative reciprocity games. The independent variables include the ingroup dummy, B's cost to punish, the damage to A if B punishes, and the payoff gap if B punishes. These variables are constructed in a similar way as their counterparts in the analysis for positive reciprocity.[18] 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 concerns and negative reciprocity decisions. For the treatment, we present results without the interaction terms in column 3 and those with the interaction terms in column 4.

Results in column 3 indicate that ingroup matching significantly reduces the likelihood of punishment by 12.8 percent ($p < 0.01$). This implies that B is more lenient toward misbehavior by an ingroup player A. The likelihood to punish is sensitive to cost and the potential consequence of punishment. A 100-token increase in B's cost lowers the likelihood of punishment by 32.0 percent ($p < 0.01$), while a 100-token increase in the potential damage to A (who has misbehaved) increases the likelihood of punishment by 4.3 percent ($p < 0.01$). In addition, B's decision on whether to punish is affected by distribution preferences. A 100-token increase in the payoff gap when B is ahead decreases the likelihood to punish by 10.5 percent ($p < 0.01$), indicating that charity concerns suppress the likelihood to punish. Results in column 4 indicate that group membership affects the likelihood to punish not only directly (-0.117, $p < 0.01$), but also through its interactions with the cost of punishment and participant charity concerns. The marginal effect of the interaction term of the ingroup dummy and B's cost to punish is 0.084 ($p < 0.05$), indicating that punishment is less price-sensitive toward an ingroup match. In other words, a reduction in cost generates a smaller increase in B's likelihood to punish an ingroup match compared to an outgroup match. Finally, the marginal effect of the payoff gap interacting with the ingroup dummy is -0.131 ($p < 0.01$), which suggests that charity concerns suppress the likelihood of punishment more toward an ingroup than toward an outgroup match.

RESULT 2 (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 toward misbehavior from an ingroup match compared to an outgroup match.*

Support. In Table 3, the marginal effects of the ingroup match variable are 0.186 ($p < 0.01$) in column 3 of panel A and -0.128 ($p < 0.01$) in column 3 of panel B.

Result 2 indicates that 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 toward an ingroup match. Furthermore, the analyses 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

[18] B's cost to punish is her opportunity cost when she chooses to punish A. The damage to A if B punishes is computed as the loss of A's payoff if B chooses to punish.

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

	Matching conditions			Alternative hypotheses and p-values		
	Ingroup	Outgroup	Control	Ingroup > Outgroup	Ingroup > Control	Control > Outgroup
Player A	0.629 [676]	0.509 [676]	0.57 [381]	0.000	0.047	0.048
Player B	0.68 [790]	0.529 [790]	0.638 [447]	0.000	0.095	0.001
Overall	0.656 [1,466]	0.520 [1,466]	0.606 [828]	0.000	0.022	0.000

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

is consistent with Brent Simpson (2006), where, in sequential prisoner's dilemma games, given cooperation by a first mover, second movers are more likely to cooperate with an ingroup than with an outgroup match.[19] However, our negative reciprocity result is just the opposite of that in McLeish and Oxoby (2007), where in simple bargaining games with induced identity, in response to unfair offers, responders engage in greater punishment toward ingroup members. This indicates that the effect of group identity on negative reciprocity might be sensitive to the environment and the specific games.

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.

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 p-values for McNemar's test for matched samples for 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. Column 6 presents the p-values for the test 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 3 (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 sessions, 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 McNemar's test for binomial proportion for matched samples, $p < 0.01$, for players A and B as well as for all participants, obtained by comparing the proportion of SWM decisions for ingroup versus outgroup matchings. Column 6 presents the p-values for the test of proportions, $p < 0.05$ for player A and for all players,

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

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

	Matching conditions			Alternative hypotheses and p-values		
	Ingroup	Outgroup	Control	Ingroup > Outgroup	Ingroup > Control	Control > Outgroup
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]			
Overall	513.1	483.5	502.7	0.000	0.114	0.016
	[1,883]	[1,891]	[1,069]			
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]			
Overall	514.1	485.0	504.3	0.011	0.198	0.057
	[927]	[972]	[1,069]			

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

whereas $p < 0.10$ for player B, comparing the ingroup versus control sessions. Column 7 presents the p-values for the tests of proportions, $p < 0.05$ for players A, B, and for all players, comparing outgroup versus control sessions.

Result 3 indicates 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 3 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, Huffman, and Meier 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 results above 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.

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 (column 5) and unpaired-sample (columns 6 and 7) t-tests. We find that participants' actual average earnings and expected earnings are significantly higher when they are matched with an ingroup member than with an outgroup member ($p < 0.01$ for expected earnings and $p = 0.011$ for actual earnings over all players). Compared to the control sessions, ingroup matching yields higher earnings, although the difference is not statistically significant ($p = 0.114$ for expected and $p = 0.198$ for actual earnings), whereas outgroup matching yields significantly lower earnings ($p = 0.016$ for expected and $p = 0.057$ for actual earnings). Comparisons of expected earnings are all associated with higher confidence levels than those of actual average earnings.[20]

Therefore, 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 subsection, 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 toward bad behavior from ingroup matches than 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.

B. *What Creates Group Effects?*

In this subsection, 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 (random assignment to groups), 2 (no social interactions), and 4 (no link between choice and self interest) 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, other-other allocations, and a within- versus 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 ingroup and outgroup matches for within-subject treatments by using the test of proportions.[21] This information is summarized in Table 6.

2. At the game level, to investigate if treatments with different design components change individual choices when facing an ingroup or an outgroup match, we first use the Fisher exact probability test. We then use the Holm-Bonferroni procedure to correct for possible

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

[21] We also examine the proportion of *decisions* that 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.

TABLE 6—PROPORTION OF PARTICIPANTS WHO DIFFERENTIATE BETWEEN INGROUP AND OUTGROUP MATCHES

	Original	NoChat	NoHelp	RandomWithin
Proportion of participants who differentiate				
Role A	0.39	0.45	0.56	0.34
Role B	0.39	0.45	0.56	0.44
	Original vs. RandomWithin	Original vs. NoChat	NoChat vs. NoHelp	Original vs. NoHelp
p-values of two-sided test for the equality of proportions				
Role A	0.65	0.33	0.32	0.06
Role B	0.60	0.36	0.32	0.06

multiple comparisons.[22] All p-values are two-tailed. The differences are considered statistically significant if the Holm-Bonferroni adjusted p-value is less than or equal to 0.05.

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; and *Within-Subject* equals one for all within-subject treatments and zero otherwise.

   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 to 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 differ only in group assignment methods. Comparing these two treatments, we find no significant difference in the proportion of participants who differentiate between ingroup and outgroup matches ($p = 0.65$ and 0.60 for roles A and B, respectively, two-sided test of proportions). At the game level, we find no significant behavioral difference between the two treatments ($p > 0.05$ for role A, $p > 0.10$ for role

[22] Holm-Bonferroni correction is applied since we conduct four pairs of treatment-wise comparisons on the game level, i.e., Original versus RandomWithin, Original versus NoChat, NoChat versus NoHelp, and Random-Within versus Random-Between, which are subject to potential multiple-comparison problems. See Yoav Benjamini and Yosef Hochberg (1995) for detailed discussions on the Bonferroni-type procedure. We thank an anonymous referee for suggesting this test.

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

	Self-reported group attachment	
Dependent variable	OLS	Ordered logit
Paintings	0.260	0.210
	(0.653)	(0.486)
Chat	1.135	0.792
	(0.216)***	(0.121)***
Other-other 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^2	0.04	0.014

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

B, Fisher exact probability tests with Holm-Bonferroni adjustments). Finally, group assignment methods do not affect self-reported group attachments ($p > 0.10$).

This result indicates that the two group assignment methods create no significant difference in participant behavior or attitude in any of the 24 games. However, even though we find no significant difference in participant behavior in any of our games, 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, Huffman, and Meier (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 question 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. Comparing these two treatments, we find no significant difference in the proportion of participants who differentiate between ingroup and outgroup matches ($p = 0.33$ and 0.36 for roles A and B, respectively). At the game level, we find no significant behavioral difference between the two treatments except A's ingroup decisions in game Resp 2a. In this game, 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, which suggests that the chat stage leads to significantly stronger ingroup favoritism ($p = 0.007$). Self-reported attachment is significantly higher in the treatments with Chat ($p < 0.01$). In Table 7, the coefficient estimates of the *Chat* dummy are 1.135 ($p < 0.01$) in OLS and 0.792 ($p < 0.01$) in ordered logit, respectively. This result indicates that, while online chat has a significant effect on behavior in only one out of 24 games, it does significantly increase self-reported group attachment, 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 former takes away the online chat stage, the latter takes away both the online chat and the other-other allocation stage. Comparing these two treatments, we find no significant difference in the proportion of participants who differentiate between ingroup and outgroup matches ($p = 0.32$ for both roles, test of proportions). At the game level, the Fisher exact probability tests corrected by the Holm-Bonferroni procedure show no significant behavioral difference in any of the games ($p > 0.05$). Lastly, the other-other allocation has no significant effect on self-reported attachment to groups. In Table 7, the coefficient estimates of the *Other-other allocation* dummy are -0.313 ($p > 0.10$) in OLS and 0.010 ($p > 0.10$) in ordered logit, respectively. This indicates that, unlike in Turner (1978), the other-other allocation stage has no significant effect on participant behavior, nor the self-reported attachment to groups in our experiments.

Although the increase in the proportion of individuals who make group-contingent choices in the NoChat (0.45) and the NoHelp (0.56) treatments is not significant ($p = 0.32$), it does indicate that the other-other allocation stage might actually decrease group-contingent behavior, possibly by a crowding-out effect. Specifically, with the other-other allocation stage, individual could make costless group-contingent choices, and might not feel obliged to make as many (costly) group-contingent choices in the subsequent allocation games, as in the treatment without other-other allocations.

When we compare the NoHelp with the Original treatments in Table 6, 56 percent of the participants in the NoHelp treatment (as opposed to 39 percent in the Original treatment) make group-contingent decisions. The difference is weakly significant ($p = 0.06$). In addition to the possible crowding out effect, we conjecture that group effect induced by categorization deteriorates over time. The larger proportion of group-contingent choices in the NoHelp treatment might be due to the fact that the sequential games took place immediately after the group categorization, rather than one or two stages later. Even though the online chat has a moderate boost in group attachment, it may not be sufficient to offset the deterioration of group effect. Our data offer some supporting evidence.[23] An improved design should control for the time between group assignment and the two-person sequential games, by giving subjects an individually completed task, which takes the same amount of time as that of the chat and other-other allocation stages.

Lastly, we evaluate the effects of *within- versus 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 test of proportions of group-differentiating participants at the treatment level cannot 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. At the game level, we find no significant behavioral difference for both players A and B ($p > 0.10$). Self-reported attachment is not significantly different. In Table 7, the coefficient estimates of the *Within-subject* dummy are -1.187 ($p > 0.10$) in OLS and -0.817 ($p > 0.10$) in ordered logit, respectively, and not significantly different from zero.

On the methodological level, the analysis from this section, together with findings from other laboratory experiments on group identity, teaches us several lessons. To induce group identity in the laboratory, random assignment is as effective as group assignment based on participant true

[23] Looking at the allocation games stage in all within-subject treatments, we find a 1 percentage point decrease of group-contingent choices per game ($p < 0.02$).

painting preferences. Furthermore, a between-subject design induces the same level of group effects as a within-subject design. To enhance and strengthen group identity, a problem-solving stage, such as an online chat or puzzle-solving, can increase group attachment and might have a moderate effect on behavior, while the other-other allocation does not have significant group effect.

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. Helping tasks, which presumably creates generalized reciprocity, can enhance and strengthen attachment to groups. Furthermore, based on prior research and our own study, we find that group effects differ across games. Therefore, it would be useful to be able to make out-of-sample predictions on whether one is likely to observe the translation of group effects to behaviorial differences in new games. One starting point is to use the transformed utility functions, equations (2)–(7), for out-of-sample forecasting.

IV. 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 (Alexander 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 prisoner's 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 24 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 24 self-other sequential allocation games. We use the latter to measure the effects of group identity on various aspects of social preferences, 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 group 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 behavior, compared to an outgroup match. Furthermore, they are less likely to punish an ingroup match for misbehavior.

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 interest 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 preferences, 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 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 pairwise 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, group problem-solving, such as online chat, significantly increases attachment to groups, but does not change behavior significantly in most of our games. Finally, 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 of 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 organization 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 Ulrike Malmendier 2007). 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 (Yuqing Ren, Robert E. Kraut, and Sara Kiesler 2007).

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: Sequential Games with Self-Other Allocations

Table A1—Games and Summary Statistics

			Control				Percent A Diff	Percent B Diff
	A stays out	If A enters, B chooses	Out	Enter	Left	Right		
Two-person dictator games								
Dict 1		(400, 400) vs. (750, 400)			0.33	0.67		0.26
Dict 2		(400, 400) vs. (750, 375)			0.82	0.18		0.26
Dict 3		(300, 600) vs. (700, 500)			0.76	0.24		0.24
Dict 4		(200, 700) vs. (600, 600)			0.50	0.50		0.29
Dict 5		(0, 800) vs. (400, 400)			0.64	0.36		0.24
Two-person response games: B's payoffs identical								
Resp 1a	(750, 0)	(400, 400) vs. (750, 400)	0.29	0.71	0.32	0.68	0.21	0.22
Resp 1b	(550, 550)	(400, 400) vs. (750, 400)	0.70	0.30	0.39	0.61	0.32	0.24
Resp 6	(100, 1000)	(75, 125) vs. (125, 125)	0.30	0.70	0.35	0.65	0.21	0.24
Resp 7	(450, 900)	(200, 400) vs. (400, 400)	0.83	0.17	0.13	0.87	0.14	0.17
Two-person response games: B's sacrifice helps A								
Resp 2a	(750, 0)	(400, 400) vs. (750, 375)	0.59	0.41	0.73	0.27	0.30	0.15
Resp 2b	(550, 550)	(400, 400) vs. (750, 375)	0.95	0.05	0.64	0.36	0.14	0.20
Resp 3	(750, 100)	(300, 600) vs. (700, 500)	0.82	0.18	0.55	0.45	0.19	0.24
Resp 4	(700, 200)	(200, 700) vs. (600, 600)	0.55	0.45	0.23	0.77	0.30	0.27
Resp 5a	(800, 0)	(0, 800) vs. (400, 400)	0.81	0.19	0.45	0.55	0.24	0.18
Resp 5b	(0, 800)	(0, 800) vs. (400, 400)	0.00	1.00	0.64	0.36	0.04	0.29
Resp 8	(725, 0)	(400, 400) vs. (750, 375)	0.74	0.26	0.83	0.17	0.24	0.15
Resp 9	(450, 0)	(350, 450) vs. (450, 350)	0.74	0.26	0.87	0.13	0.22	0.08
Two-person response games: B's sacrifice hurts A								
Resp 10	(375, 1000)	(400, 400) vs. (350, 350)	0.38	0.62	0.95	0.05	0.26	0.06
Resp 11	(400, 1200)	(400, 200) vs. (0, 0)	0.82	0.18	0.91	0.09	0.21	0.07
Resp 12	(375, 1000)	(400, 400) vs. (250, 350)	0.22	0.78	0.96	0.04	0.11	0.15
Resp 13a	(750, 750)	(800, 200) vs. (0, 0)	0.83	0.17	0.91	0.09	0.08	0.10
Resp 13b	(750, 750)	(800, 200) vs. (0, 50)	0.74	0.26	0.83	0.17	0.13	0.14
Resp 13c	(750, 750)	(800, 200) vs. (0, 100)	0.78	0.22	0.78	0.22	0.10	0.19
Resp 13d	(750, 750)	(800, 200) vs. (0, 150)	0.87	0.13	0.91	0.09	0.08	0.17

Note: Column "Percent A Diff" (Percent B Diff) refers to the percentage of player A (B) decisions that differentiate between ingroup and outgroup matches in treatments.

Table A2—Summary Statistics in Treatment Sessions

	Ingroup				Outgroup			
Two-person dictator games								
	Out	Enter	Left	Right	Out	Enter	Left	Right
Dict 1			0.30	0.70			0.45	0.55
Dict 2			0.67	0.33			0.73	0.27
Dict 3			0.68	0.32			0.86	0.14
Dict 4			0.34	0.66			0.63	0.38
Dict 5			0.56	0.44			0.77	0.23
Two-person response games: B's payoffs identical								
Resp 1a	0.46	0.54	0.26	0.74	0.63	0.37	0.48	0.53
Resp 1b	0.66	0.34	0.39	0.61	0.80	0.20	0.55	0.45
Resp 6	0.46	0.54	0.18	0.83	0.36	0.64	0.33	0.68
Resp 7	0.95	0.05	0.10	0.90	0.83	0.18	0.29	0.71
Two-person response games: B's sacrifice helps A								
Resp 2a	0.46	0.54	0.67	0.33	0.73	0.27	0.80	0.20
Resp 2b	0.88	0.13	0.68	0.32	0.84	0.16	0.84	0.16
Resp 3	0.75	0.25	0.56	0.44	0.88	0.13	0.73	0.27
Resp 4	0.57	0.43	0.35	0.65	0.84	0.16	0.58	0.42
Resp 5a	0.75	0.25	0.46	0.54	0.89	0.11	0.59	0.41
Resp 5b	0.04	0.96	0.54	0.46	0.03	0.97	0.76	0.24
Resp 8	0.63	0.38	0.66	0.34	0.81	0.19	0.76	0.24
Resp 9	0.60	0.40	0.69	0.31	0.79	0.21	0.78	0.23
Two-person response games: B's sacrifice hurts A								
Resp 10	0.46	0.54	0.99	0.01	0.28	0.73	0.96	0.04
Resp 11	0.76	0.24	0.95	0.05	0.57	0.43	0.89	0.11
Resp 12	0.44	0.56	0.93	0.08	0.34	0.66	0.80	0.20
Resp 13a	0.89	0.11	0.95	0.05	0.81	0.19	0.86	0.14
Resp 13b	0.79	0.21	0.90	0.10	0.78	0.23	0.84	0.16
Resp 13c	0.85	0.15	0.91	0.09	0.81	0.19	0.73	0.28
Resp 13d	0.85	0.15	0.81	0.19	0.84	0.16	0.68	0.33

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