Group Identity and Cooperation in Infinitely Repeated Games*

Bo Chen† Sherry Xin Li‡ Tracy Xiao Liu§

December, 2025

Abstract

We develop a theoretical framework and conduct a laboratory experiment to study how group identity affects cooperative behavior and strategy selection in infinitely repeated prisoner's dilemma (IRPD) games. We find that participants are more likely to cooperate and less likely to adopt the Always Defect strategy with ingroup members than with outgroup members or participants in the control condition. Ingroup pairs are also more likely to sustain cooperation but less likely to persist in defection over the dynamic course across supergames, compared to both outgroup and control pairs. However, these effects are statistically significant only in the low strategic risk environment when the discount factor

*We are grateful to the editor and two anonymous referees for their constructive comments and suggestions, which have greatly improved the paper. We would also like to thank Andy Brownback, Bram Cadsby, Gabriele Camera, Roy Chen, Yan Chen, Pedro Dal Bó, Miguel Fonseca, Guillaume Fréchette, Brit Grosskopf, Peter McGee, David Rojo-Arjona, Rajiv Sarin, Fei Song, and seminar participants at Chapman University, University of Arkansas, University of Exeter, University of Guelph, the 5th Annual Xiamen University International Workshop on Experimental Economics, the Texas Experimental Symposium, the Tsinghua Conference on Theoretical and Behavioral Economics, the Economic Science Association North America and World meetings for helpful comments. Research assistance was provided by Cangjian Cao, Yuqing Feng, Malcolm Kass, Anthony Kong, Danielle Purcell, Raiyan Syed, and Shuo Yang. IRB approvals were obtained at the University of Texas-Dallas (#MR 11-079) on March 11, 2011, and at the University of Arkansas (Protocol #2405542458) on July 22, 2024. Experiments were conducted at the Center and Laboratory for Behavioral Operations and Economics (CLBOE) at the University of Texas-Dallas and at the Behavioral Business Research Lab at the University of Arkansas, respectively. Li gratefully acknowledges the financial support provided by the University of Arkansas and the University of Texas-Dallas. Liu gratefully acknowledges financial support from the National Natural Science Foundation of China (72222005, 72342032). Financial support from NSFC (Grant No. 72573051) for Chen is also gratefully acknowledged. Any remaining errors are our own.

†Department of Economics, Southern Methodist University, Dallas, TX 75275. Email: bochen@smu.edu.

‡Department of Economics, Sam M. Walton College of Business, University of Arkansas, Fayetteville, AR 72701. Phone: 479-575-6222. Email: sli@walton.uark.edu.

§Department of Economics, School of Economics and Management, Tsinghua University, Beijing, China. Phone: 86-10-62793429. Email: liuxiao@sem.tsinghua.edu.cn.

is high enough for cooperation to be both a subgame perfect Nash equilibrium and risk dominant. In the high strategic risk environment where cooperation is a subgame perfect Nash equilibrium but not risk dominant, the impact of group identity on cooperation is less robust and only holds qualitatively.

Keywords: *Group identity, Cooperation, Infinitely repeated prisoner's dilemma, Repeated game strategies, Laboratory experiment.*

JEL Classification: C7, C91.

1 Introduction

The role of group identity in fostering cooperation among individuals is well-documented in the extensive literature of economics and psychology. Most of this literature has predominantly focused on one-shot interactions (e.g., Hargreaves Heap and Varoufakis, 2002; Charness et al., 2007; Goette et al., 2006, 2012) alongside a few studies that feature finitely repeated interactions (e.g., Eckel and Grossman, 2005; Chuah et al., 2014). These settings, however, differ substantially from many important real-world interactions, which often unfold over longer time horizons without well-defined endpoints, such as interactions among coworkers in the workplace, between employees and employers, between businesses and customers, or among research collaborators in academia. In these long-term interactions, individual agents face the credible threat of future punishment for taking advantage of others and thus are likely to refrain from such opportunistic behavior.

In addition, social factors, such as group identity, may play crucial roles in these long-term real-world interactions. For example, organizations adopt various group-building practices to cultivate a shared group identity to motivate employees from diverse backgrounds to work together and ensure successful long-term cooperative relationships (Graves, 2014; O'Hara, 2014). Unfortunately, monetary incentives may overshadow the potential importance of social factors (e.g., group identity) in the one-shot or finitely repeated interactions with pre-specified endpoints that are mainly featured in the current literature. This gap may limit the extent to which we can apply the insights we have gleaned from group identity to real-world situations.

This study extends our understanding by investigating how group identity affects individual cooperative behavior and strategy selection in the infinitely repeated prisoner's dilemma (**IRPD**) both theoretically and experimentally. On the one hand, this game provides an excellent platform to study the potential effects of group identity in strategic settings beyond short-term interactions such as one-shot or limited, pre-specified rounds of interactions. On the other hand, game theory predicts that in the IRPD (in the absence of group identity), both cooperative and non-cooperative choices can be selected by agents in equilibrium. However, while cooperation can be sustained as an equilibrium under certain conditions (see the literature review in Section 2), it often fails to prevail. Despite its importance, the potential role of group identity in shaping individual choices of actions and strategies remains considerably understudied in infinitely repeated games.

In this paper, we develop a theoretical model and conduct a laboratory experiment to investigate whether and how group identity influences individuals' cooperation and strategy selection in infinitely repeated games. Our theoretical framework combines the existing models of group identity and the IRPD and generates predictions on the impact of group identity on the subgame perfect Nash equilibrium (SPNE) and risk dominance. It yields testable hypotheses on the role of group identity in affecting cooperation and repeated game strategies.

In our laboratory experiment, we introduce group identity by first randomly assigning participants into two groups. Their group identity is then further reinforced through an incentivized collective problem-solving task that involves ingroup communication. In our 3 (group affiliations) $\times$ 2 (discount factors) factorial between-subject design, participants interact exclusively with ingroup (outgroup, or other) counterparts in the ingroup (outgroup, or control) treatments and play an IRPD game with a discount factor $\delta = \frac{1}{2}$ (or $\frac{2}{3}$), i.e., a higher (or lower) strategic risk for cooperation. All participants play under a fixed matching protocol within a supergame.

Our experimental results largely support the hypotheses derived from our theoretical framework, i.e., shared group identity enhances cooperation in the IRPD game. However, the strength of this positive effect depends on the discount factor. When the discount factor is sufficiently large to create a low-strategic-risk environment for cooperation (e.g., $\delta = \frac{2}{3}$ in our experiment), group identity leads to more sustained, higher levels of cooperation among ingroup members. Compared to both outgroup pairs and those in the control treatment without groups, ingroup pairs are more likely to maintain cooperation and less likely to persist in defection across supergames. Their cooperation is also less reliant on prior experiences; instead, they are more likely to restart cooperation in subsequent supergames when paired with ingroup than with outgroup or in the control treatment. Overall, participants in the ingroup treatment are significantly less likely to adopt the Always Defect (AD) strategy compared to the control and outgroup treatments.

In contrast, when the discount factor is low (e.g., $\delta = \frac{1}{2}$ in our experiment) so that cooperation is an SPNE but not risk dominant, the effects of group identity on cooperation, its dynamics, and strategy selection remain positive but are statistically insignificant. Last but not least, we find that although individuals' initial beliefs about their co-player's cooperation in the first round of the first supergame, which are shaped by the group categorization and enhancement task, are significantly higher for ingroup than for outgroup members, we find no systematic evidence that initial beliefs interact with group identity to differentially affect cooperation.

Our study differs substantially from Camera and Hohl (2021), the only other study in this research area to our knowledge. Camera and Hohl (2021) address a different question, that is, whether group effects can emerge in strategic settings where it is difficult for individuals to observe characteristics on which to base their categorizations and decisions. In their experiment, participants are randomly assigned to three color-coded groups. Their group identity is reinforced through ingroup, fixed-pair interactions during the first set of two indefinitely repeated supergames. Participants then play the second set of two supergames (about 20 rounds each) as strangers with both ingroup and outgroup members, while their counterparts' group affiliations and ID numbers remain hidden. The researchers find no impact of group identity on decisions in the second set of supergames, suggesting that group effects are unlikely to emerge in the absence of observable characteristics that individuals can use for group categorization and biased decision-making. The researchers attribute this null finding to several factors: the relatively weak form of induced group identity, the presence of three groups, participants' anonymous encounters with a random mix of ingroup and outgroup members, and the lack of information about group affiliations and track records. Appendix A details the differences between their study and ours.

Our study builds on a substantial body of literature that documents the crucial roles of social motivations in economic decision-making (e.g., Bandiera et al., 2009, 2010; Ashraf and Bandiera, 2018). We focus on group identity, an important social motivation central to social psychology, sociology, anthropology, and political science, but one that has not received much attention in economics until the seminal work of Akerlof and Kranton (2000). We examine its impact in a foundational setting: cooperation in infinitely repeated prisoner's dilemma games. These games pose a persistent challenge for sustaining cooperation, making them a rigorous testbed for evaluating the influence of group identity. Our investigation provides a valuable addition to the literature by identifying strategic risk for cooperation as a primary factor in determining whether and when social motivations, such as group identity, can promote sustained cooperation. Our study is among the early efforts to investigate the impact of group identity in infinitely repeated games. We theoretically and experimentally identify key conditions under which group identity can promote cooperation in these strategically demanding settings. That is, group identity enhances cooperation, but reliably and significantly only when strategic risk is low, i.e., when the discount factor is high enough that cooperation is both an SPNE and risk dominant. Otherwise, its effects on cooperation are weak or qualitative.

The rest of the paper proceeds as follows. Section 1.1 reviews the literature on group identity and IRPD. Section 2 introduces a theoretical framework that motivates our experiment. Section 3 outlines the experimental design and hypotheses. Section 4 presents the results. Section 5 concludes and discusses practical implications for organizational policy design.

1.1 Literature Review

Recent years have witnessed two fast-growing strands of literature on group identity and infinitely repeated games. This section reviews the relevant literature in experimental economics, with a particular focus on studies that are closely related to this research.

1.1.1 Group Identity

Social identity is a person's sense of self, derived from the perceived membership in social groups. Since the introduction of social identity theory by Tajfel and Turner (1979, 1986), the power of identity in shaping individual behavior has been documented by extensive psychology research (reviewed in Brewer, 1991; Hewstone et al., 2002; Abdelal et al., 2009).

In economics, the concept of social identity was first integrated into economic modeling by the seminal work of Akerlof and Kranton (2000), with further developments in their subsequent studies (Akerlof and Kranton, 2005, 2008, 2010). Experimental economics literature has shown that shared group identity has an important impact on other-regarding preferences (e.g., Chen and Li, 2009; Fehr et al., 2013; Kranton et al., 2020) as well as trust and trustworthiness (e.g., Hargreaves Heap and Zizzo, 2009; Falk and Zehnder, 2013). Research also reveals that shared group identity fosters cooperation in public goods games (e.g., Eckel and Grossman, 2005; Charness et al., 2014; Charness and Holder, 2018), improves coordination in coordination games (e.g., Charness et al., 2007; Chen and Chen, 2011), influences altruistic punishment (e.g., Bernhard et al., 2006; Goette et al., 2006; Weng and Carlsson, 2015; Paetzel and Sausgruber, 2018), and increases often wasteful effort in intergroup conflicts (e.g., Cason et al., 2012, 2017; Hargreaves Heap et al., 2017). A number of survey papers provide reviews or meta-analyses on this literature, such as Akerlof and Kranton (2010), Balliet et al. (2014), Costa-Font and Cowell (2015), Lane (2016), Pechar and Kranton (2017), Charness and Chen (2019), Shayo (2020), and Li (2020).

Overall, a key finding in this literature highlights the positive effect of shared group identity on cooperation in the prisoner's dilemma game. It further shows that the strength of this effect often depends on the nature of the groups and the salience of group identity. One strand of this literature focuses on artificially created group identities in laboratory settings. For example, in a one-shot prisoner's dilemma game, Charness et al. (2007) find more cooperation toward ingroup members when artificially induced groups are reinforced by the presence of a (passive) ingroup audience or through payoff dependence with the ingroup. Similar findings on the influence of experimentally induced group identity on cooperation in the prisoner's dilemma are reported by Aksoy (2015) and Hargreaves Heap and Varoufakis (2002). Another strand of research examines naturally occurring social groups. For example, Goette et al. (2006, 2012) find ingroup favoritism and outgroup discrimination in cooperation, fully mediated through beliefs, in randomly assigned natural groups with pre-existing *social ties*; however, these intergroup differences disappear in the randomly assigned *minimal* groups with no prior connections. Further evidence of ingroup favoritism and outgroup discrimination in the prisoner's dilemma has been found in various naturally occurring groups, such as US college students with Asian and European surnames (Chen et al., 2014), primary school children from different language groups in Italy (Angerer et al., 2016), villagers of different religions in India (Chakravarty et al., 2016), and Israeli and Palestinian students in Israel (Goerg et al., 2016).

While the studies above use the one-shot prisoner's dilemma game, a smaller number of studies explore finitely repeated interactions using the prisoner's dilemma or other strategic games. For instance, Chuah et al. (2014) find that in a finitely repeated prisoner's dilemma experiment, participants cooperate more with others who share either their religious or ethnic identity, but they do *not* cooperate *less* with those who share neither identity, relative to the baseline where no identity information is revealed.[1] Other studies also find group identity effective in increasing cooperation or coordination in various finitely repeated strategic games. For example, Eckel and Grossman (2005) show that incentive mechanisms based on inter-team tournaments boost intra-team cooperation in a repeated linear public goods game framed as a team production. Similarly, Weng and Carlsson (2015) demonstrate strong, artificially induced group identity positively impacts cooperation in both homogenous- and heterogeneous-endowment groups in a repeated linear public goods game, although the strength of the effect depends on the intensity of the group identity. Chen and Chen (2011) and Chen et al. (2020) find that when a salient, artificially induced common group identity is present, learning leads to better ingroup coordination toward the efficient high-effort equilibrium in a minimum-effort coordination game, relative to the

[1]Several studies examine the effect of group identity on intergroup conflict by using the intergroup prisoner's dilemma game which is played between two groups with multiple players in each group (Goren and Bornstein, 2000; Halevy et al., 2012; Weisel, 2016; Cason et al., 2019).

baseline without a common group identity.

Unlike the studies mentioned above, our paper expands this important line of research to the context of *infinitely* repeated games. We examine how group identity influences individual choices of strategies in infinitely repeated games, a research topic that has received limited attention in prior literature. Our study offers deeper insight into human strategic reasoning in social interactions that lack a predetermined endpoint.

1.1.2 Infinitely Repeated Games

This subsection reviews the literature on infinitely (or indefinitely) repeated games with perfect monitoring in which participants can perfectly observe the opponents' actions. Dal Bó and Fréchette (2018) provide comprehensive discussions and meta-analyses on a wide variety of repeated games, including finitely repeated games and repeated games with imperfect public or private monitoring.

A vast body of theoretical research examines the determinants of cooperation in infinitely repeated games (see Mailath and Samuelson (2006) for a review). Despite significant progress, theoretical predictions about individual behavior remain inconclusive. Specifically, the "folk theorem" establishes the existence of a continuum of equilibria in IRPD (Friedman, 1971; Fudenberg and Maskin, 1986), but it is silent on which equilibrium will prevail.[2] Experimental studies have been instrumental in addressing this question. Early experiments on infinitely repeated games (e.g., Roth and Murnighan, 1978; Murnighan and Roth, 1983; Feinberg and Husted, 1993; Palfrey and Rosenthal, 1994) show that while cooperation rates tend to be higher when cooperation is sustainable in equilibrium, subjects do not choose it as often as expected. Comparing infinitely repeated with finitely repeated prisoner's dilemma games of the same expected length, Dal Bó (2005) finds that the level of cooperation is higher in the former. This important finding suggests that the credible threat of future retaliation—rather than the expected number of rounds—drives higher cooperation in infinitely repeated games. In contrast, Lugovskyy et al. (2017) find no consistent differences in overall cooperation rates between finitely and infinitely repeated linear public goods games. Dal Bó and Fréchette (2011) show that cooperation is high under discount factors for which cooperation can be supported in equilibrium, but drops sharply to negligible levels when it cannot be supported in equilibrium.

Blonski et al. (2011) argue that traditional criteria for assessing cooperation in re-

[2]For a recent approach to equilibrium selection using Moore machines and implementation errors, see Ioannou (2025).

peated games overlook the sucker's payoff—the cooperator's payoff when the opponent defects. They provide experimental evidence to support that risk dominance better predicts cooperation than the equilibrium criterion (Blonski et al., 2011; Dal Bó and Fréchette, 2011). However, Dal Bó and Fréchette (2018) further note that neither Nash equilibrium nor risk dominance provides a sufficient condition for high levels of cooperation in repeated games.[3] A meta-analysis by Dal Bó and Fréchette (2018) concludes that high cooperation will not emerge unless the repeated game parameters make cooperation robust to strategic uncertainty.

Besides the literature on the key built-in parameters of repeated games discussed above, a growing number of studies investigate the impact of individual characteristics and social factors including risk, trust, and social preferences (e.g., Dreber et al., 2014; Kölle et al., 2023; Gill and Rosokha, 2024). Other research studies environment-related factors such as community enforcement (Kandori, 1992). Related experimental investigations suggest that the matching protocol and the number of human subjects may affect cooperation in these settings (Camera et al., 2013a; Duffy and Ochs, 2009; Duffy and Xie, 2016). In addition, communication may or may not promote cooperation in IRPD (Camera et al., 2013b; Arechar et al., 2017).

Our study connects the research on group identity with the literature on repeated games, particularly infinitely repeated games. By highlighting the important role of group identity and the conditions under which it operates, this study enhances our understanding of the potential power of social incentives in shaping cooperation and strategy choices in infinitely repeated games and thus extends previous findings, which suggest that cooperation in these games is mainly driven by game parameters (e.g., Reuben and Suetens, 2012; Cabral et al., 2014; Dreber et al., 2014; Dal Bó and Fréchette, 2018). Building on these studies, our theoretical framework develops explicit predictions for all treatments (ingroup, outgroup, and control), clarifying when identity promotes cooperation and when its influence is limited.

2 Theoretical Framework

This section outlines the theoretical framework that underpins our experimental design. We start with the baseline model, followed by the introduction of social preferences and group identity, as well as their effects on cooperation and risk dominance.

[3] Breitmoser (2015) shows that when the risk-dominance criterion is satisfied, a Markov perfect equilibrium in Semi-Grim Strategies exists.

2.1 Baseline Model

Consider an IRPD game with discount factor δ. The stage game with monetary/nominal payoffs is shown in Game A of Figure 1, where the reward (R), temptation (T), punishment (P) and sucker's (S) payoffs satisfy $T > R > P > S$. Without loss of generality, we normalize the payoffs using positive affine transformations, resulting in the prisoner's dilemma game in Game B where $g, l > 0$.[4]

[Figure 1 about here]

Here g represents the net gain from defection (D) when the opponent cooperates (C) while $-l$ is the loss from C when the opponent defects. As is standard (Stahl II, 1991), the minimal discount factor $\underline{\delta}$ sustaining mutual cooperation in a subgame perfect Nash equilibrium (SPNE) in Game B can be calculated as

$$\underline{\delta} = \frac{g}{1+g}. \tag{1}$$

As noted in the literature (Blonski and Spagnolo, 2015), $\underline{\delta}$, while widely used as an indicator of the likelihood of cooperation, does not account for l. This can be problematic, as cooperation becomes increasingly risky if l is sufficiently large.

Therefore, another strong predictor of cooperative behavior is risk dominance, first introduced by Harsanyi and Selten (1988). The criterion of risk dominance captures the idea that in games with multiple equilibria, players tend to favor the equilibrium that minimizes the risk of a poor payoff due to uncertainty about the opponents' choices.[5] Following Blonski and Spagnolo (2015), it suffices to focus on two key strategies in the repeated prisoner's dilemma, the "Grim Trigger" strategy (σ^G) and the "Always Defect" strategy (σ^D).[6] We define (σ^G, σ^G) to be risk dominant if σ^G is a best response to $\frac{1}{2} \circ \sigma^G + \frac{1}{2} \circ \sigma^D$, which, in our context, is equivalent to

$$\frac{1-(1-\delta)l}{2} \geq \frac{(1-\delta)(1+g)}{2} \quad \Leftrightarrow \quad \delta \geq \delta_{RD} := \frac{l+g}{1+l+g}. \tag{2}$$

[4] In theory, this normalization will not impact behavior under broad utility assumptions. For a discussion on the validity of this normalization in experimental studies on the repeated prisoner's dilemma, see footnote 10 in Dal Bó and Fréchette (2018), as well as the experimental findings in Kagel and Schley (2013).

[5] Theoretical support for risk dominance can be found in the evolutionary game theory literature (Kandori et al., 1993; Young, 1993) and the global games literature (Carlsson and Van Damme, 1993).

[6] Blonski and Spagnolo (2015) demonstrate that no cooperative equilibrium is less risky than the Grim Trigger equilibrium, which therefore defines the riskiness of cooperation in IRPD. Conversely, the Always Defect equilibrium is the only "safe" equilibrium where both players avoid the loss payoff $-l$.

Additionally, $\delta_{RD} > \underline{\delta}$.

2.2 IRPD Game with Group Identity

Building on Charness and Rabin (2002) and Chen and Li (2009), we integrate group identity into the IRPD game to assess whether and how such social factors affect cooperation incentives.

Specifically, given a pure action profile $\boldsymbol{a} \in \{C, D\}^2$, denote the nominal payoff of player $i \in \{1, 2\}$ to be $\pi_i(\boldsymbol{a}) \in \{1, 0, -l, 1+g\}$. We define player i's utility function from $\boldsymbol{a}$, for $j \neq i$, as

$$u_i(\boldsymbol{a}) = \omega_j \times \pi_j(\boldsymbol{a}) + (1 - \omega_j) \times \pi_i(\boldsymbol{a}), \quad (3)$$

where $\omega_j = \rho(1 + Ia)r + \theta(1 + Ib)s$ represents the weight player i assigns to player j's nominal payoff $\pi_j(\boldsymbol{a})$. By definition, $\omega_1 = \omega_2$, so both players have symmetric social preferences. Here, ρ reflects a player's charity concerns, θ measures envy, and a and b capture the effects of group identity. The indicators r, s, I are such that $r = 1$ if $\pi_i(\boldsymbol{a}) > \pi_j(\boldsymbol{a})$ (and 0 otherwise), $s = 1$ if $\pi_i(\boldsymbol{a}) < \pi_j(\boldsymbol{a})$ (and 0 otherwise), and $I = 1$ if i and j belong to the same group (and 0 otherwise). Throughout the analysis, we refer to $I = 1$ as the **ingroup** case and $I = 0$ as the **outgroup** (or **control**).[7]

Given the number of parameters involved, we introduce the following assumption to simplify and streamline our analysis:

Assumption 1 *The parameters ρ, θ, a, and b in the utility function (3) need to satisfy the following conditions:*

(A_1) $\theta < 0 < \rho < 1$,

(A_2) $-1 < b < 0 < a$,

(A_3) ρ, θ, a and b are all close to zero,

so that the stage game under the utility function $u_i(\boldsymbol{a})$ remains a prisoner's dilemma.

The intuition behind Assumption 1 is as follows. Condition (A_1) suggests that players prioritize their own nominal payoffs but dislike disparities in payoffs. Condition (A_2) indicates stronger charity concerns and weaker envy toward ingroup members. Condi-

[7] Prior studies in both psychology and economics have shown that outgroup and neutral interactions are behaviorally indistinguishable in the absence of explicit outgroup bias (Brewer, 1999; Balliet et al., 2014; Chuah et al., 2014). Therefore, we adopt this parsimonious specification used in Chen and Li (2009) where we model group identity as an increase in the weight a player places on an ingroup member's payoff without introducing hostility toward outgroup members.

tion (A_3) ensures that cooperation remains a relevant issue under social preferences.[8] Overall, Assumption 1 provides a disciplined and tractable way to study how moderate social preference concerns, especially group identity, affect cooperation incentives without changing the strategic structure of the game, e.g., turning a PD game into a coordination game.[9]

With the utility function defined in (3), each player's utility from action profile $\boldsymbol{a}$ in the original prisoner's dilemma Game B can now be expressed as Game C for an outgroup opponent ($I = 0$) and Game D for an ingroup opponent ($I = 1$) in Figure 2.

[Figure 2 about here]

In Game C, the payoffs $1 + G$ and $-L$ are given by

$$u_1(C, D) = \theta(1 + g) + (1 - \theta)(-l) = -L,$$

$$u_1(D, C) = \rho(-l) + (1 - \rho)(1 + g) = 1 + G,$$

while in Game D, the payoffs $1 + \widehat{G}$ and $-\widehat{L}$ are defined as

$$u_1(C,D) = \theta(1 + b)(1 + g) + [1 - \theta(1 + b)]\,(-l) = -\widehat{L},$$

$$u_1(D, C) = \rho(1 + a)(-l) + [1 - \rho(1 + a)]\,(1 + g) = 1 + \widehat{G}.$$

We can readily verify that under Assumption 1, we have $\widehat{G}, G, \widehat{L}, L > 0$ and

$$-L < -\widehat{L} < -l, 1 + \widehat{G} < 1 + G < 1 + g. \tag{4}$$

In other words, players are more attentive to ingroup members' welfare ($1 + \widehat{G} < 1+G$) and are less averse to payoff disparities within the group ($-L < -\widehat{L}$). As a result, when interacting with ingroup opponents, players are less inclined to deviate from cooperation and experience a better sucker's payoff, compared to outgroup opponents.

[8] Although Assumption 1 may appear somewhat specific, it is consistent with the experimental literature. Condition (A_1) is commonly known as inequality aversion in the literature. For experimental evidence supporting this concept across various games, see the discussions in Fehr and Schmidt (1999), Bolton and Ockenfels (2000), and Charness and Rabin (2002). Meanwhile, condition (A_2) is consistent with the subjects' social preferences estimated in Chen and Li (2009).

[9] This restriction ensures that cooperation remains a meaningful strategic problem and that the comparative statics analysis of discount factor thresholds remains valid. As shown in later analysis, even such small deviations can shift equilibrium thresholds in nontrivial ways, affecting the conditions under which cooperation can be sustained.

2.3 Impact of Group Identity

We proceed in two steps: (i) analyzing the minimal discount factor for sustaining cooperation as an SPNE, and (ii) examining risk dominance as a measure of robustness under strategic uncertainty.

As in equation (1), the minimal discount factor that sustains cooperation in an SPNE depends on the temptation and loss parameters. Incorporating social preferences yields the cutoffs $\delta^* = \frac{G}{1+G}$ for the outgroup/control case ($I = 0$) and $\widehat{\delta^*} = \frac{\widehat{G}}{1+\widehat{G}}$ for the ingroup case ($I = 1$). Proposition 1 compares δ^* and $\widehat{\delta^*}$ and presents the comparative statics with respect to the charity and envy parameters.

Proposition 1 (Group Identity and SPNE) *In the repeated prisoner's dilemma with utility function (3), the minimal discount factors sustaining cooperation in an SPNE are such that* $\widehat{\delta^*} < \delta^*$. *In addition, we have* $\frac{\partial \widehat{\delta^*}}{\partial \rho} < 0$, $\frac{\partial \delta^*}{\partial \rho} < 0$, *and* $\frac{\partial \widehat{\delta^*}}{\partial \theta} = 0$, $\frac{\partial \delta^*}{\partial \theta} = 0$.

Proof: See Appendix B.

Proposition 1 shows that group identity promotes cooperation by lowering the minimal discount factor required to sustain cooperation in an SPNE. Moreover, both δ^* and $\widehat{\delta^*}$ decrease with stronger charity concerns (ρ), while envy (θ) has no impact. The inequality $\widehat{\delta^*} < \delta^*$ reflects that group identity, by amplifying charity concerns, reduces the incentive to deviate from cooperation.

We next investigate the impact of group identity on the risk dominance of cooperation, adopting two complementary approaches. First, we assess the discount factor cutoffs for risk dominance, denoted as δ^*_{RD} for the setting without group identity ($I = 0$), and as $\widehat{\delta}^*_{RD}$ for that with group identity ($I = 1$). A calculation similar to (2) yields

$$\delta^*_{RD} = \frac{L+G}{1+L+G}, \quad \widehat{\delta}^*_{RD} = \frac{\widehat{L}+\widehat{G}}{1+\widehat{L}+\widehat{G}}. \tag{5}$$

As in Blonski and Spagnolo (2015) and Dal Bó and Fréchette (2011), the cutoffs δ^*_{RD} and $\widehat{\delta}^*_{RD}$ represent key thresholds for the risk dominance of cooperation.

A second measure of the riskiness of choosing the Grim Trigger equilibrium over the Always Defect equilibrium is the Nash product difference first introduced by Harsanyi and Selten (1988). Specifically, let $U_i(\sigma_i, \sigma_j)$ denote player i's repeated game payoff, and $U_j(\sigma_j, \sigma_i)$ denote player j's payoff, from the strategy profile (σ_i, σ_j). Define

$$\Delta U_D(\delta) := \left(U_i\left(\sigma^D, \sigma^D\right) - U_i\left(\sigma^G, \sigma^D\right)\right)\left(U_j\left(\sigma^D, \sigma^D\right) - U_j\left(\sigma^G, \sigma^D\right)\right),$$

$$\Delta U_G(\delta) \coloneqq \left(U_i\left(\sigma^G,\sigma^G\right) - U_i\left(\sigma^D,\sigma^G\right)\right)\left(U_j\left(\sigma^G,\sigma^G\right) - U_j\left(\sigma^D,\sigma^G\right)\right),$$

$$\Delta U(\delta) \coloneqq \Delta U_D(\delta) - \Delta U_G(\delta).$$

The Nash product $\Delta U_D(\delta)$ represents the players' payoff gains from selecting the Always Defect equilibrium, while $\Delta U_G(\delta)$ captures the gains from Grim Trigger. The difference $\Delta U(\delta)$ measures the relative riskiness of the Grim Trigger equilibrium compared to the "safe" Always Defect equilibrium: if $\Delta U(\delta) > 0$ or equivalently $\Delta U_D(\delta) > \Delta U_G(\delta)$, the Grim Trigger equilibrium is riskier, and risk dominance favors the Always Defect equilibrium, and vice versa. A larger $\Delta U(\delta)$ implies greater riskiness associated with Grim Trigger. We calculate $\Delta U(\delta; I = 0)$ and $\Delta U(\delta; I = 1)$ explicitly as:

$$\Delta U\left(\delta; I = 0\right) = \left(1-\delta\right)^2 L^2 - \left[1-\left(1-\delta\right)\left(1+G\right)\right]^2,$$

$$\Delta U\left(\delta; I = 1\right) = \left(1-\delta\right)^2 \widehat{L}^2 - \left[1-\left(1-\delta\right)\left(1+\widehat{G}\right)\right]^2.$$

Proposition 2 (Group Identity and Risk Dominance) *In the repeated prisoner's dilemma with utility function (3), group identity strictly improves the risk dominance of the Grim Trigger equilibrium. In particular, we have*

(a) $\widehat{\delta}^*_{RD} < \delta^*_{RD}$, $\frac{\partial \widehat{\delta}^*_{RD}}{\partial \rho} < 0$, $\frac{\partial \widehat{\delta}^*_{RD}}{\partial \theta} < 0$, *and* $\frac{\partial \delta^*_{RD}}{\partial \rho} = \frac{\partial \delta^*_{RD}}{\partial \theta} < 0$,

(b) $\Delta U(\delta; I = 1) < \Delta U(\delta; I = 0)$ *for all* $\delta \in (0, 1)$.

Proof: See Appendix B.

Proposition 2 demonstrates that stronger charity concerns (ρ) or weaker envy (i.e., less negative θ) reduce the riskiness of cooperation by lowering the risk-dominance discount factor thresholds, δ^*_{RD} and $\widehat{\delta}^*_{RD}$. Group identity further enhances risk dominance of cooperation by lowering the discount factor cutoff ($\widehat{\delta}^*_{RD} < \delta^*_{RD}$) and reducing the riskiness of the Grim Trigger equilibrium for **all** δ (Proposition 2(b)). In particular, the inequality $\Delta U(\delta; I = 1) < \Delta U(\delta; I = 0)$ indicates a lower relative payoff risk under group identity, expanding the basin of attraction of the cooperative equilibrium in the sense of Blonski and Spagnolo (2015). The rationale is that group identity amplifies charity and dampens envy among ingroup members, making cooperation less risky and thus more likely to emerge.

To further illustrate Proposition 2(b), consider a simple setting where $b = \theta = -\varepsilon$, $a = \rho = \varepsilon$, with $\varepsilon \in (0, 1)$ being small, hence consistent with Assumption 1. We can then explicitly calculate that for $\delta \in \{\frac{1}{2}, \frac{2}{3}\}$,

$$\Delta U\left(\frac{1}{2}; I = 1\right) - \Delta U\left(\frac{1}{2}; I = 0\right) = -\frac{1}{2}\varepsilon^2(g+l+1)(1+l-g-\varepsilon+2g\varepsilon-\varepsilon l),$$

$$\Delta U\left(\frac{2}{3}; I=1\right) - \Delta U\left(\frac{2}{3}; I=0\right) = -\frac{2}{9}\varepsilon^2(g+l+1)(2+l-g-2\varepsilon+2g\varepsilon-2\varepsilon l),$$

which, together with $1+l>g$ (i.e., (C,C) being efficient) and ε small, implies that

$$\begin{aligned}\Delta U\left(\frac{1}{2}; I=1\right) - \Delta U\left(\frac{1}{2}; I=0\right) < 0,\\ \Delta U\left(\frac{2}{3}; I=1\right) - \Delta U\left(\frac{2}{3}; I=0\right) < 0.\end{aligned} \tag{6}$$

Inequality (6) aligns with Proposition 2(b). Note that Grim Trigger is not risk dominant when $\delta = \frac{1}{2}$, $I=0$, and ε is small. In this case, although group identity improves risk dominance of cooperation, the effect is fragile. The reason is that $\Delta U\left(\frac{1}{2}; I=1\right)$is close to 0, which is the cutoff for Grim Trigger to become risk dominant. This contrasts with the IRPD with $\delta = \frac{2}{3}$, where Grim Trigger is already risk dominant even without group identity. Thus, the impact of group identity on promoting cooperation is more salient when $\delta = \frac{2}{3}$, where cooperation becomes more robustly sustained in players' behavior.

Overall, the theoretical analysis predicts that group identity enhances cooperation primarily when the environment already permits self-enforcing cooperation, that is, when the discount factor is sufficiently high for cooperation to be both an SPNE and risk dominant. Otherwise, its effect is limited when cooperation is inherently risky.

3 Experimental Design and Hypotheses

3.1 Experimental Design

To investigate how group identity influences strategy choices in IRPD, we employ a 3×2 factorial experiment design. The design varies the group membership of paired co-players and the discount factor δ. Table 1 summarizes the design and the experimental instructions are included in Appendix C.

We adopted the group manipulation and enhancement protocol from Chen and Chen (2011), which was based on the design of the Random-Between-Treatment in Chen and Li (2009). In each session of the group treatments, 12 participants were *randomly* assigned to either a green group or an orange group, with 6 members in each. Participants first studied five pairs of paintings by Kandinsky and Klee for three minutes. They then completed an eight-minute task to identify the artist of two additional paintings. Anonymous communication via text chat on the computers was allowed *within* each group for ingroup members to share information on this task. Participants were instructed not to share any information that could identify them and not to use obscene or offensive language. The communication was not restricted to the paintings. Afterwards, participants submitted their answers individually, and each correct answer was worth 100 points. We focus on the laboratory-induced artificial group identities, rather than naturally occurring social identities, for two reasons: (a) to avoid potential confounds from stereotypes tied to natural identities, and (b) to gain insights that extend beyond specific identity categories (see Charness and Chen (2019) and Li (2020) for detailed discussions on this approach).

In the control treatment, there was no Kandinsky/Klee group assignment. Participants completed the painting task independently, with no communication allowed. As in the group treatments, they submitted individual answers for the additional paintings, again earning 100 points for each correct answer.

[Table 1 and Figure 3 about here]

We used a between-subject design during the repeated games stage. Participants were always paired with someone from the same group in the ingroup treatment, with someone from the other group in the outgroup treatment, and with another participant in the session in the control treatment.

Our design of IRPD games was based on the setup with $\delta = \frac{1}{2}$ and $R = 40$ in Dal Bó and Fréchette (2011). Participants played a symmetric prisoner's dilemma game (see Figure 3) repeatedly. We used neutral language, labeling actions as A (Cooperate) and B (Defect). The computer randomly determined the number of rounds in each supergame (referred to as a 'block' in the experiment) based on $\delta \in \{\frac{1}{2}, \frac{2}{3}\}$. After each round, the supergame continued to the next round with probability of δ, and participants remained paired with the same co-player. If the supergame ended (with probability $1 - \delta$), participants were paired with a new co-player. The entire session concluded once the first supergame after 60 minutes of gameplay finished.[10]

At the beginning of each round in every supergame, before making their choices, we elicited participants' beliefs about their co-player's choice using the quadratic scoring rule (Nyarko and Schotter, 2002). Participants were asked to estimate the likelihood that their co-player would choose Cooperate or Defect (see Appendix D.1 for a screenshot). Participants were paid based on the accuracy of their beliefs. At the end of each round, we provided feedback on the participant's own choice, the game payoff, and their co-player's choice for that round.

[10]The cutoff is 50 minutes in Dal Bó and Fréchette (2011). It is extended to 60 minutes in our study to account for the additional time required for belief elicitation in each round.

The lower half of the decision screen displayed a history window, showing each participant's entire history of choices, co-players' choices, and earnings from all previous rounds of all previous supergames (see Appendix D.2 for a screenshot).[11] This information was public and explained in the experimental instructions before the games started. In the group treatment, we elicited individual group sentiment–specifically, attachment to ingroup and outgroup, respectively. This elicitation occurred twice: once after the painting task and again after the prisoner's dilemma games. The self-reported group attachment scores showed that, before the prisoner's dilemma games, ingroup attachment was significantly higher than outgroup attachment in all group treatments ($p < 0.001$, Wilcoxon matched-pairs signed-rank test). This manipulation check confirms the success of our group intervention using the painting task.

We conducted thirty-six independent sessions, including eighteen at the Center and Laboratory for Behavioral Operations and Economics at the University of Texas - Dallas (UTD) in the summer of 2014 and eighteen at the Behavioral Business Research Lab at the University of Arkansas (UA) in the fall of 2024.[12] Therefore, each of the six treatments included six independent sessions, three from UTD and three from UA. Each session comprised twelve participants, except that two control sessions at UTD each consisted of 10 participants. This yielded 212 participants at UTD and 216 at UA, totaling 428 for the experiment. Participants were allowed to participate in only one session. The computerized experiment was programmed in z-Tree (Fischbacher, 2007). Each session lasted approximately 90 minutes. Participants were paid based on their cumulative earnings in all rounds plus their earnings in the painting task and the $5 participation fee. The exchange rate was 200 points for $1. The average earnings were $28 per participant.

3.2 Hypotheses

Recall that the control and outgroup treatments both correspond to the case without group identity ($I = 0$), while the ingroup treatment ($I = 1$) incorporates identity-weighted preferences. Building on this distinction, we now apply the theoretical predictions from Section 2 to derive testable hypotheses about how group identity affects individual action choices and repeated game strategies.

According to Proposition 1, the minimal discount factor sustaining cooperation in

[11] The co-players' ID numbers were not shown in the history window to avoid confounding reputation effects.

[12] Participants were recruited through email using the online recruiting software ORSEE (Greiner, 2015) at UTD and SONA at UA.

an SPNE is lower when a subject is paired with an ingroup member compared to with an outgroup member or in the control treatment, i.e., $\underline{\widehat{\delta^*}} < \delta^*$. In our experiment, the discount factor cutoff when social preferences and identity are absent is 0.4. Since the experiment uses discount factors of 0.5 and 0.67, both exceeding 0.4, our hypotheses will focus on comparisons based on risk dominance and riskiness of playing Grim Trigger.

Proposition 2 implies that the risk of choosing cooperation, supported by Grim Trigger strategies, is always lower in the ingroup than in either the control or the outgroup treatments, as measured by both the risk dominance discount factor cutoff and the riskiness of adopting Grim Trigger. However, whether the risk dominance discount factor cutoff exceeds or falls below the baseline cutoff ($\delta_{RD} = 0.61$ in (2)) depends on the specific values of social preference and social identity parameters and is therefore undetermined. Table 2 illustrates the key theoretical thresholds using numerical calculations based on moderate social preferences ($\rho = 0.15$ and $\theta = -0.05$) and group identity parameters ($a = 0.467$ and $b = -0.931$) estimated by Chen and Li (2009).[13] The results show that group identity improves risk dominance for both discount factor settings, although the behavioral impact may be limited when $\delta = \frac{1}{2}$, as the near zero value of $\Delta U \left(\delta = \frac{1}{2}; I = 1\right)$ indicates that cooperation is only weakly risk dominant.

[Table 2 about here]

Based on these theoretical predictions, we formulate the following hypotheses:

Hypothesis 1 ***(Cooperative Choice)*** *Subjects cooperate more with ingroup members in IRPD compared to with outgroup members or in the control treatment.*

Hypothesis 2 ***(Repeated Game Strategies)*** *Subjects are less likely to choose the Always Defect (AD) repeated game strategy with ingroup members compared to with outgroup members or in the control treatment.*

These hypotheses follow directly from the theoretical results that group identity lowers the discount factor threshold for cooperative SPNE and strengthens the risk dominance of Grim Trigger (the most risky cooperative equilibrium) relative to Always Defect. However, our theoretical analysis does not yield sharp predictions about how the magnitude of the group identity effect on cooperation varies with δ. Resolving this question would require knowledge of the underlying social preference parameters, charity (ρ) and envy (θ), which are unobservable. We therefore rely on our experiment evidence for further insight.

[13] A similar calculation using the parameter estimates from Kranton et al. (2020), i.e., $a' = 0.457$ and $b' = -0.625$, yields qualitatively identical results. Detailed calculations are available upon request.

4 Results

In this section, we investigate how group identity influences cooperation and efficiency, the role of beliefs, and dynamics in cooperative behavior as well as how group identity shapes individual strategic choices.

4.1 Treatment Effects on Cooperation

Table 3 presents the average cooperation rate, standard errors, and the two-sided ranksum exact tests conducted at the level of independent sessions/groups. The control (or outgroup) treatment for each δ has six independent sessions, while the ingroup treatment for each δ consists of six sessions, resulting in twelve independent groups, as the two groups within each session do not interact.

Table 3 shows that in the *first* round of the *first* supergame, cooperation rates range from 60.6% in control to 80.6% in ingroup under $\delta = \frac{1}{2}$ ($p = 0.014$, ranksum exact test), and from 66.7% in control to 79.2% in ingroup under $\delta = \frac{2}{3}$ ($p = 0.059$). The cooperation rate is marginally higher in ingroup than in outgroup for both δ's ($p > 0.10$). As expected in repeated interactions, cooperation rates are generally higher than those observed in the one-shot prisoner's dilemma games. Nevertheless, the impact of group identity on cooperation in the first round of the first supergame of our study aligns with findings from one-shot game settings. For instance, Simpson (2006) divides participants into two groups based on their art preferences and finds cooperation rates of 64% for the ingroup and 68% for the outgroup in a between-subject design, whereas the cooperation rate is 53.8% for the ingroup, significantly higher than the 36.5% observed for the outgroup, in a within-subject design. Similarly, Goette et al. (2006, 2012) find that the cooperation rate is 69% for the ingroup, significantly higher than 50% for the outgroup in randomly assigned groups with social ties. However, in randomly assigned *minimal* groups, the ingroup-versus-outgroup difference in cooperation is only 10% and statistically insignificant.

[Table 3 and Figure 4 about here]

Table 3 further shows that across all supergames, individual cooperation rates are higher in ingroup compared to the control and outgroup. Although the cross-treatment differences in cooperation are statistically insignificant under $\delta = \frac{1}{2}$ ($p > 0.10$) for the first round or all rounds of the supergames, they are statistically significant under $\delta = \frac{2}{3}$ (81.2% for ingroup vs. 42.6% for control or 49.2% for outgroup, $p \leq 0.001$ in the first rounds; 73.7% for ingroup vs. 37.2% for control or 42.2% for outgroup,

$p \leq 0.001$ in all rounds of all supergames). No significant differences in cooperation are found between the control and the outgroup treatments regardless of δ. These aggregate-level findings are consistent with Figure 4, which presents the time series of average first-round cooperation across supergames for the two universities separately. The top three panels of the two figures reveal considerable cross-section variations in the impact of group identity on cooperation under $\delta = \frac{1}{2}$. In half of the ingroup sessions, cooperation stabilizes between 40% and 50% after a moderate decline, while in the other half, it drops sharply and remains below 20% for most time. In no case does cooperation consistently remain above the basin of attraction, helping explain the insignificant impact of ingroup membership on cooperation across all supergames as seen in Table 3. In contrast, the bottom three panels of the two figures show a different pattern for $\delta = \frac{2}{3}$. In four sessions, ingroup cooperation stays between 80% and 90%, and in other two, mostly above 60%. In all sessions, cooperation stays above the basin of attraction. It indicates a substantial and stable improvement in cooperation compared to the control and outgroup treatments.

To further investigate how group identity influences individual cooperation, we conduct a linear probability regression analysis (Wooldridge, 2010), using a cluster bootstrap-t procedure to account for the small number of sessions (Cameron et al., 2008). The dependent variable is a dummy variable for cooperation. The independent variables include the ingroup and the outgroup treatment dummies, with the control treatment being omitted. As in all other regression analyses, this analysis includes a location dummy to control for any site-specific effects. The standard errors are clustered on both the individual subject and experiment session levels. As shown in Table 4, the results based on the first rounds of the supergames (Columns 1-4) or all rounds across all supergames (Columns 5-8) are similar to the nonparametric results reported in Table 3.

[Table 4 about here]

Specifically, Table 4 shows that under $\delta = \frac{1}{2}$, the average cooperation rate is qualitatively higher in the ingroup treatment than the control treatment (0.176 in Column 1, $p = 0.119$; 0.181 in Column 5, $p = 0.095$). For $\delta = \frac{2}{3}$, the coefficient for ingroup matching is 0.405 in Column 3 and 0.379 in Column 7 ($p < 0.001$ for both), indicating that cooperation is 40.5 and 37.9 percentage points higher, respectively, in the ingroup treatment relative to the control. These effects remain robust after accounting for time trends. In addition, we find no differences in cooperation between the outgroup and the control treatments ($p > 0.10$) except for in Column 8 (0.134, $p = 0.064$) when we control for time trend. These findings can be summarized in Result 1.

Result 1 ***(Choice of Cooperation)***

1a.) Ingroup matching significantly increases cooperation under $\delta = \frac{2}{3}$. Under $\delta = \frac{1}{2}$, it has a qualitative or marginally significant positive effect on cooperation due to substantial heterogeneities across sessions.

1b.) Outgroup matching does not affect cooperation relative to the control treatment under both δs.

Result 1 shows that Hypothesis 1 is statistically supported under $\delta = \frac{2}{3}$ but only qualitatively or marginally supported under $\delta = \frac{1}{2}$ primarily due to large heterogeneities across the sessions (Figure 4). Recall our theoretical analysis suggests that group identity strengthens the risk dominance of cooperation, particularly for the case of $\delta = \frac{2}{3}$. In contrast, when $\delta = \frac{1}{2}$, our numerical results show that cooperation lies near the boundary of risk dominance ($\Delta U(\delta = \frac{1}{2}; I = 1) \approx 0$), creating greater strategic uncertainty about the opponent's choice. Result 1, thus, is consistent with the substantial cross-session heterogeneities observed in our experimental data.

Moreover, our findings of ingroup favoritism without outgroup discrimination align with our model assumption and prior research in psychology and economics. Brewer (1999) and Balliet et al. (2014) both conclude based on their reviews of several decades of economic and psychological research that intergroup bias is largely driven by ingroup favoritism rather than outgroup hostility. Similarly, Chuah et al. (2014) report ingroup favoritism but no outgroup discrimination in a repeated prisoner's dilemma experiment among Malaysian participants of different religious and ethnic backgrounds.

The impact of group identity on efficiency closely mirrors cooperation. For each pair of players, efficiency is defined as their actual joint payoff as a proportion of the maximum possible joint payoff (80 tokens).[14] Appendix E shows that, relative to the control treatment, ingroup matching has a marginally significant impact on efficiency under $\delta = \frac{1}{2}$, but leads to a significant increase in efficiency with a varying degree from 12.304 to 15.648 percentage points (Columns 3-4 and 7-8, $p < 0.001$) under $\delta = \frac{2}{3}$. No significant differences in efficiency are found between the outgroup and control treatments.

[14] In the first rounds of all supergames, average efficiency under $\delta = \frac{1}{2}$ is 69.4% (control), 75.9% (ingroup), and 70.7% (outgroup) while under $\delta = \frac{2}{3}$ it is 76.7%, 92.4%, and 78.9%, respectively. Looking at all rounds of all supergames, average efficiency under $\delta = \frac{1}{2}$ is 68.9% (control), 75.5% (ingroup), and 70.0% (outgroup), while under $\delta = \frac{2}{3}$ it is 75.5%, 90.0%, and 77.3%, respectively.

4.2 Beliefs

In our experiment, at the beginning of each round before one's choice was made, we elicited individual's incentivized belief of how likely their co-player would cooperate.[15] Note that beliefs elicited, except for the *initial* ones in the first round of the first supergame, are influenced by prior interactions among participants and are therefore endogenous. Accordingly, we focus on *initial* beliefs which are exogenous to all decisions, since they are elicited *after* the group manipulation but *before* any decisions being made in the repeated games. Regressions in Table 5 show that initial beliefs of the co-player's cooperation are higher in ingroup than in outgroup ($p = 0.021$ for $\delta = \frac{1}{2}$, 0.071 for $\delta = \frac{2}{3}$). The differences between the ingroup (or outgroup) and control treatments are not statistically significant ($p > 0.10$).[16]

[Tables 5 and 6 about here]

How do initial beliefs in the first stage game of the first supergame affect cooperation? Since initial beliefs were elicited before any cooperation decisions were made, they were exogenous to all later cooperation behavior. Results are reported in Table 6. We find that in the first round of the first supergame (Columns 1-4), participants start off to be more cooperative with the ingroup or outgroup than in the control treatments ($\alpha_1, \alpha_2, p \leq 0.10$). For both discount factors, the higher the initial beliefs of co-players' cooperation is, the more likely that one cooperates ($\alpha_4, p < 0.01$). However, there is no statistical difference in the impact of beliefs on cooperation in the initial round across the ingroup, outgroup or the control treatments ($\alpha_5, \alpha_6, \alpha_5 - \alpha_6$, $p > 0.10$).

We also extend this analysis to examine dynamics effects, i.e., how well initial beliefs predict cooperation in the subsequent supergames (Columns 5–8). To capture these dynamics and potential treatment differences, we include interaction terms such as Initial Belief $\times$ Block and three-way interactions among Ingroup (or Outgroup), Initial Belief, and Block (Columns 6 and 8). The results show that in the first round across all supergames, cooperation shows no significant difference under $\delta = \frac{1}{2}$ (α_1, α_2, $p > 0.10$, Column 6), while it is significantly higher for ingroup compared to the outgroup or control treatment under $\delta = \frac{2}{3}$ (α_1, $\alpha_1 - \alpha_2$, $p < 0.05$, Column 8), consistent with the findings in Columns 1-4 of Table 3. Regarding beliefs, we find that initial beliefs

[15] The treatment effects of group identity on beliefs reported in Appendix F are also highly consistent with the effects on cooperation in Table 3. Recent studies have shown that belief is a good predictor for subjects' choices in repeated games (Aoyagi et al., 2024; Gill and Rosokha, 2024).

[16] Summary statistics show that the average initial beliefs of the co-player's cooperation are 63.6% (control), 71.2% (ingroup), and 60.2% (outgroup) under $\delta = \frac{1}{2}$ and are 65.9%, 72.0%, and 63.8% under $\delta = \frac{2}{3}$, respectively.

predict the dynamics of cooperation significantly for $\delta = \frac{2}{3}$ (α_4, $p < 0.05$), but only qualitatively for $\delta = \frac{1}{2}$ (α_4, $p > 0.10$). In addition, for both discount factors, the impact of initial beliefs declines over time (α_7, $p \leq 0.10$). Although this downward trend does not significantly differ for ingroup (or outgroup) compared to the control treatment (α_8 and α_9, $p > 0.10$), the decline appears less steep for ingroup than for outgroup, with a marginally significant difference under $\delta = \frac{2}{3}$ ($\alpha_8 - \alpha_9$, $p = 0.076$) and an insignificant difference under $\delta = \frac{1}{2}$ ($\alpha_8 - \alpha_9$, $p = 0.193$). In other words, the positive effect of initial beliefs on cooperation diminishes more slowly in the ingroup than in the outgroup. This observation may partly explain the sustained gap in cooperation. Due to large standard errors, this effect is only marginally significant for $\delta = \frac{2}{3}$ and not significant for $\delta = \frac{1}{2}$.

Result 2 ***(Initial Beliefs and Their Impact on Dynamics of Cooperation)***

2a.) Initial beliefs about the co-player's cooperation, shaped by the group identity treatment, are significantly higher in the ingroup than in the outgroup.

2b.) Initial beliefs predict the dynamics of cooperation significantly under $\delta = \frac{2}{3}$ but only qualitatively under $\delta = \frac{1}{2}$. Its effect exhibits a downward trend over time, and some qualitative evidence suggests that this decline appears less steep for ingroup than for outgroup.

The economics literature on group identity suggests that intergroup differences in behavior may stem from preferences (e.g., Chen and Li, 2009; Kranton and Sanders, 2017; Kranton et al., 2020), beliefs (e.g., Goette et al., 2006; Falk and Zehnder, 2013; Ockenfels and Werner, 2014; Cubel et al., 2024), or both (e.g., Everett et al., 2015; Angerer et al., 2016; Goerg et al., 2016; Li, 2020). In our experiment, we find limited evidence that beliefs drive intergroup differences in cooperation, pointing instead to a more important role for preferences, consistent with our theoretical framework.

4.3 Group Identity, Decision Dynamics, and Other Determinants of Cooperation

In this section, we examine the impact of group identity on the dynamics of cooperation. We approach this question from several angles, including stability in behavior across supergames, patterns within supergames, and other determinants of cooperation influenced by intergroup differences.

Stability To assess behavioral stability across supergames, we analyze the average likelihood of cooperating (or defecting) in the first round of supergame t, conditional on the choice C (or D) made in the first round of supergame $t-1$, i.e., playing $C_t|C_{t-1}$ (or $D_t|D_{t-1}$). Results are presented in Tables 7 and 8 with each *independent* group (or session) as an independent observation and reported by treatment.[17] For cooperation stability ($C_t|C_{t-1}$), we find no difference across treatments for $\delta = \frac{1}{2}$ (62.0%, 69.8%, and 69.5% for control, ingroup, and outgroup, $p > 0.10$ for all pairwise comparisons, ranksum exact test), but under $\delta = \frac{2}{3}$, it is significantly higher with ingroup (93.7%) than with outgroup (80.3%, $p = 0.001$) or in the control treatment (74.1%, $p = 0.002$). For stability in defection ($D_t|D_{t-1}$), we find that under $\delta = \frac{2}{3}$, the ingroup stability (62.1%) is lower than in the outgroup (85.1%, $p = 0.015$) or control (83.3%, $p = 0.053$) treatments. A similar pattern emerges under $\delta = \frac{1}{2}$, although with weaker statistical significance (90.3%, 82.8%, and 91.3% for control, ingroup, and outgroup, $p = 0.067$ between control and ingroup; $p > 0.10$ for other pairwise comparisons).

In the extended analysis, we incorporate longer lags by examining the likelihood of continued cooperation after 2 to 5 consecutive cooperations, i.e., $C_t|C_{t-1}|C_{t-2}$, ..., $C_t|C_{t-1}|C_{t-2}|C_{t-3}|C_{t-4}|C_{t-5}$. For $\delta = \frac{2}{3}$, consistent with the $C_t|C_{t-1}$ pattern, we find that ingroup matches exhibit significantly greater stability than both in the outgroup and control treatments ($p < 0.01$), with no significant difference between the latter two. However, this pattern does not hold for $\delta = \frac{1}{2}$, where no significant differences in cooperation stability are observed across treatments. In the similar analysis on defection (i.e., $D_t|D_{t-1}|D_{t-2}$, ..., $D_t|D_{t-1}|D_{t-2}|D_{t-3}|D_{t-4}|D_{t-5}$), we find that the likelihood of continued defection after 2 to 5 consecutive defections is qualitatively lower with ingroup compared to the outgroup and the control treatments for both values of δ ($p > 0.10$).

Overall, these across-treatment comparisons on stability suggest that ingroup matches are more likely to persist in cooperation but less likely to persist in defection, compared to both outgroup and control treatments. This pattern is statistically significant at the conventional levels for $\delta = \frac{2}{3}$ but holds only qualitatively for $\delta = \frac{1}{2}$. Notably, the stability of cooperation, not defection, lasts over longer lags in supergames.

[Tables 7 and 8 about here]

Lenience and Forgiveness We next turn to the dynamics within supergames. Table 9 reports the average likelihood of cooperating at round t after playing with the co-player $(C_{t-1}, D_{t-1})|(C_{t-2}, C_{t-2})$, $(D_{t-1}, C_{t-1})|(D_{t-2}, D_{t-2})$, or $(D_{t-1}, D_{t-1})|(D_{t-2}, C_{t-2})$

[17] As in Table 3, the observation unit is an independent session/group. There are six independent sessions in the control or outgroup treatments for each δ. In the ingroup treatment, since the two groups within each session never interact, there are twelve independent groups, two for each session, for each δ.

in the previous two rounds within a supergame.[18] These dynamic patterns correspond to lenience, forgiveness, or positive response to punishment, respectively. Similar to Tables 7 and 8, results are presented with each independent group (or session) as an independent observation. We find no significant differences across treatments in the likelihood of being lenient (i.e., $C_t|(C_{t-1}, D_{t-1})|(C_{t-2}, C_{t-2})$) or forgiving (i.e., $C_t|(D_{t-1}, C_{t-1})|(D_{t-2}, D_{t-2})$) ($p > 0.10$ for both δ's, ranksum exact test), possibly due to the low frequencies of the play patterns $(C_{t-1}, D_{t-1})|(C_{t-2}, C_{t-2})$ and $(D_{t-1}, C_{t-1})|(D_{t-2}, D_{t-2})$. However, ingroup pairs are significantly more likely to respond positively by cooperating following the co-player's punishment (i.e., $C_t|(D_{t-1}, D_{t-1})$ $|(D_{t-2}, C_{t-2})$), compared to outgroup pairs or those in the control treatment. This effect is evident only under $\delta = \frac{2}{3}$ (29.6% for ingroup, compared to 13.5% for control and 10.4% for outgroup, $p = 0.017$ or 0.009). Under $\delta = \frac{1}{2}$, the differences hold only qualitatively (11.8% for control, 13.7% for ingroup, and 9.4% for outgroup, $p > 0.10$).

The findings above on decision dynamics are summarized in Result 3.

[Table 9 about here]

Result 3 ***(Impact of Group Identity on Decision Dynamics)***

3a.) Across supergames, ingroup pairs are more likely to sustain cooperation but less likely to persist in defection compared to both outgroup and control treatments.

3b.) Within supergames, ingroup pairs also show greater tendency to cooperate in response to the co-player's punishment, compared to outgroup or control pairs. These patterns hold with statistical significance for $\delta = \frac{2}{3}$ but not for $\delta = \frac{1}{2}$.

Other Determinants of Cooperation We next investigate the impact of group identity on other determinants of cooperation using a linear probability model in Table 10. The dependent variable is the dummy variable for cooperation. Following Table 6 of Dal Bó and Fréchette (2011), our analysis focuses on the first round of all supergames and includes three key explanatory variables: whether the co-player cooperated in the first round of the previous supergame, the number of rounds in the previous supergame, and whether the subject cooperated in the first round of the first supergame. The first two variables capture the subjects' prior experiences; the third reflects their initial cooperative tendency. We also interact these variables with the ingroup and outgroup

[18] We also examine the likelihood of cooperation conditional on other patterns of play in rounds $t-1$ and $t-2$ within each supergame, focusing on those that occur with a frequency greater than 5% across all observations. Specially, we analyze the average rates for $C_t|(C_{t-1}, C_{t-1})|(C_{t-2}, C_{t-2})$, $C_t|(D_{t-1}, D_{t-1})|(D_{t-2}, D_{t-2})$, and $C_t|(C_{t-1}, D_{t-1})|(D_{t-2}, D_{t-2})$. However, we find no statistically significant differences across treatments in any of these dynamic patterns.

dummies to test for potential treatment differential effects. The constant term (β_0) captures baseline cooperation in the control treatment, regardless of prior experience or initial tendency to cooperate. Based on Dal Bó and Fréchette (2011), we expect higher cooperation when the co-player cooperated in the previous supergame, the previous supergame lasted longer, or the subject was initially cooperative. Whether these effects differ by group identity is an empirical question. The overall effects of the explanatory variables for ingroup or outgroup and their comparisons are reported in the lower panels of Table 10.[19] Consistent with Dal Bó and Fréchette (2011), all three explanatory variables show important impact on cooperation. Our discussions will focus on cross-treatment comparisons.

[Table 10 about here]

Results show that under low strategic uncertainty ($\delta = \frac{1}{2}$) , co-player's previous cooperation, the duration of the previous supergame, and participants' initial cooperative tendency almost all significantly affect cooperation across the three treatments (β_1 and β_3 for control, $p < 0.05$; $\beta_1 + \beta_6, \beta_2 + \beta_8, \beta_3 + \beta_{10}$ for ingroup, $p < 0.05$; $\beta_1 + \beta_7, \beta_2 + \beta_9, \beta_3 + \beta_{11}$ for outgroup, $p < 0.01$). In addition, co-player's prior cooperation has a *differentially greater* impact on subsequent cooperation for ingroup than in the control ($\beta_6 = 0.276, p = 0.006$). The duration of the previous supergame also differentially impacts cooperation more in the ingroup and outgroup treatments than in the control ($\beta_8 = 0.021, p = 0.066$; $\beta_9 = 0.021, p < 0.016$). In other words, cooperative behavior in the ingroup or outgroup treatment is more strongly shaped by past experiences than in the control. However, past experiences and initial cooperative tendencies do not differentially affect ingroup and outgroup ($p > 0.10$ for all pairwise comparisons in the bottom panel of Column 1). Moreover, the ingroup treatment dummy variable has a negative effect on cooperation ($\beta_4 = -0.115, p = 0.039$), suggesting that players who defected initially in the IRPD games and observed the co-player's defection from the previous supergame are less likely to begin subsequent supergames cooperatively in the ingroup treatment compared to the control. This observation indicates that under high strategic uncertainty ($\delta = \frac{1}{2}$), individuals show stronger negative reciprocity toward ingroup members' defection, consistent with findings in previous studies (McLeish and Oxoby, 2011).

In contrast, when facing low strategic uncertainty ($\delta = \frac{2}{3}$), the treatment effects follow different patterns. On the one hand, the coefficient for the ingroup treatment dummy β_4 ($0.517, p < 0.001$) is significantly larger than that for the outgroup treatment

[19] P values for these tests are corrected for multiple hypothesis testing using the Bonferroni method.

dummy β_5 (β_4 vs. β_5, $p < 0.001$). It indicates that irrespective of participants' initial cooperative tendency or past experiences, the likelihood of restarting cooperation in subsequent supergames is 51.7 percentage points higher in the ingroup treatment compared to the control, or 54.8 percentage points higher compared to the outgroup treatment. On the other hand, while participants' cooperation is generally influenced by co-player's prior cooperation, the previous supergame duration, and their initial cooperative tendency in both the control ($\beta_1 \sim \beta_3$, $p < 0.01$) and outgroup treatments ($\beta_1 + \beta_7$, $\beta_2 + \beta_9$, and $\beta_3 + \beta_{11}$, $p < 0.01$)—as is the case under $\delta = \frac{1}{2}$—these effects largely disappear in the ingroup treatment under $\delta = \frac{2}{3}$. Specifically, these effects for ingroup are either marginally significant ($\beta_2 + \beta_8, p = 0.067$) or insignificant ($\beta_1 + \beta_6$ and $\beta_3 + \beta_{10}$, $p > 0.10$), showing a notable departure from what we observe under $\delta = \frac{1}{2}$. Moreover, we find that participants' cooperative decisions are significantly less affected by their co-player's previous cooperation in the ingroup compared to both the control ($\beta_6 = -0.151, p = 0.035$) and the outgroup treatments ($\beta_6 < \beta_7, p = 0.021$). These findings suggest that cooperation in the ingroup treatment is less dependent on the co-players' previous cooperation than in the other two treatments.

Result 4 summarizes these findings above.

Result 4 ***(Impact of Group Identity on the Determinants of Cooperation)***

4a.) Under $\delta = \frac{1}{2}$, subjects' cooperation shows greater dependence on their previous experiences and initial cooperative tendency in the ingroup and outgroup treatments than in the control treatment, although no differences are found between ingroup and outgroup.

4b.) Under $\delta = \frac{2}{3}$, subjects' cooperation shows less dependence on their previous experiences in the ingroup treatment than in the other two treatments. Participants are also more likely to restart with cooperation in subsequent supergames in the ingroup treatment compared to the other two treatments.

Result 4 suggests that under $\delta = \frac{1}{2}$, cooperation is strongly affected by past experiences and initial cooperative tendencies across all treatments and more so for the ingroup treatment than the control, while under $\delta = \frac{2}{3}$, cooperative choices in the ingroup treatment are more resilient to past experiences than in the control and outgroup treatments. These findings align with Result 1. That is, under $\delta = \frac{2}{3}$, this strong resilience, irrespective of past experiences, helps explain the sustained high levels of cooperation observed among ingroup members. Conversely, the absence of such resilience under $\delta = \frac{1}{2}$ may account for the weaker, *albeit* still positive, effect of ingroup identity on cooperation.

4.4 Impact of Group Identity on Repeated Game Strategies

While the analyses above focus on cooperative choices, this subsection examines how group identity influences the selection of repeated game strategies. Following the literature on strategy estimation (e.g., Dal Bó and Fréchette, 2011; Fudenberg et al., 2012; Ioannou, 2025), we consider a set of six candidate repeated game strategies: Always Defect (AD), Always Cooperate (AC), Grim Trigger (Grim), Tit for Tat (TFT), Win Stay Lose Shift (WSLS), and a trigger strategy with a two-period punishment (T2). To minimize potential confounds from learning effects, we focus on the second half of all repeated games in each session.[20] Specifically, we examine the importance of each strategy s^k, where $k \in \{1, ..., 6\}$. We define an indicator function $y_{imr}\left(s^k\right) = 1\left\{s_{imr}\left(s^k\right) + \gamma\epsilon_{imr} \geq 0\right\}$, where y_{imr}, being 1 when cooperation is observed and 0 otherwise, represents the observed action for individual $i \in \{1, ..., I\}$ with co-player $m \in \{1, ..., M\}$ in round $r \in \{1, ..., R\}$. The term $s_{imr}(s^k)$ takes the value of 1 if strategy s^k implies cooperation conditional on the action history, and -1 otherwise. Errors are allowed in decision making and are captured by ϵ_{imr} and its variance γ. Therefore, the likelihood for player i using strategy s^k is specified as

$$p_i(s^k) = \prod_{m=1}^{M}\prod_{r=1}^{R}\left(\frac{1}{1+\exp\left(-\frac{s_{imr}(s^k)}{\gamma}\right)}\right)^{y_{imr}}\left(\frac{1}{1+\exp\left(\frac{s_{imr}(s^k)}{\gamma}\right)}\right)^{1-y_{imr}}.$$

The log-likelihood function for strategy s^k is $\sum_{i=1}^{I}\ln\left(\sum_{k=1}^{6} p(s^k)\, p_i(s^k)\right)$, where $p(s^k)$ represents the explanatory power of strategy s^k for our experiment data.

Table 11 presents the estimated proportion of the data attributed to each strategy across treatments. Since our focus is on the impact of group identity on strategy selection, we emphasize comparisons across treatments (columns). Bootstrapped standard errors are shown in parentheses. Consistent with previous studies (Dal Bó and Fréchette, 2018, 2019; Romero and Rosokha, 2018), AD, Grim, and TFT account for most of the data.[21]

Table 11 shows that under $\delta = \frac{1}{2}$, the defect strategy (AD) accounts for 94.2% in the control, 64.1% in the ingroup, and 82.0% in the outgroup treatments. Under $\delta = \frac{2}{3}$, the proportion of AD is 55.8% in the control, 13.3% in the ingroup, and

[20]These strategies are detailed in Table G.1 of Appendix G. We also expand the strategy set to include the eleven strategies in Table 3 of Fudenberg et al. (2012) and present the results in Table G.2 of Appendix G.

[21]Camera et al. (2012) find that Grim is not frequently used by subjects from more diverse backgrounds (e.g., MBA students and white-collar workers) in repeated games with longer duration and random rematching within each supergame.

49.9% in the outgroup. These results indicate that, for a given δ, participants are less likely to choose AD in the ingroup treatment compared to the control and outgroup treatments. But these differences are insignificant under $\delta = \frac{1}{2}$ ($p > 0.10$, z-test for equality of coefficients) and significant under $\delta = \frac{2}{3}$ (55.8% for control vs. 13.3% for ingroup, $p = 0.029$; 49.9% for outgroup vs. 13.3% for ingroup, $p = 0.014$).

[Table 11 about here]

Under $\delta = \frac{1}{2}$, TFT accounts for 4.5% in the control, 17.9% in the ingroup, and 10.5% in the outgroup treatments. Under $\delta = \frac{2}{3}$, these proportions are 33.9%, 39.4%, and 22.3%, respectively. While TFT is qualitatively more common in the ingroup treatment than the other treatments, the differences across treatments are not statistically significant. Comparing the two other non-defect strategies, Grim or AC, between the ingroup treatment with the other two treatments, we find that participants are *qualitatively* more likely to choose AC ($p > 0.10$ under both δ values) and Grim ($p = 0.057$ between ingroup and control, $p > 0.10$ for other pairwise comparisons, under $\delta = \frac{2}{3}$). The remaining two strategies, WSLS and T2, rarely appear in the experiment. The estimated size for the parameter γ in all treatments is relatively large, which suggests that bounded rationality plays an important role in the decision process. Result 5 summarizes these findings.

Result 5 ***(Repeated Game Strategies)*** *Under $\delta = \frac{2}{3}$, participants in the ingroup treatment are significantly less likely to choose the Always Defect (AD) strategy compared to in the control and outgroup treatments. In addition, under $\delta = \frac{2}{3}$, participants are marginally more likely to choose Grim with ingroup members than in the control treatment. These patterns hold only qualitatively for $\delta = \frac{1}{2}$.*

Similar to Result 1's support for Hypothesis 1, Result 5 provides statistical support for Hypothesis 2 under $\delta = \frac{2}{3}$, but only qualitative support under $\delta = \frac{1}{2}$. It reinforces our early discussion on the influence of group identity on cooperative choices. It also broadens our understanding of the positive effects of group identity in strategy selection especially under $\delta = \frac{2}{3}$: participants are less likely to adopt the defect strategy (AD) with ingroup members, but it remains unclear which specific cooperative strategies are favored instead, with only marginal support for Grim. This lack of conclusive finding aligns with our early analysis in Table 9 which shows no statistically significant impact of group identity on lenience and forgiveness within the supergames dynamics. Therefore, the analyses at both the strategy level and the level of cooperation dynamics suggest that while group identity discourages non-cooperative behavior (AD) under

$\delta = \frac{2}{3}$, its influence on the selection of any particular cooperative strategies remains inconclusive.

5 Conclusion

We develop a theoretical framework and conduct a laboratory experiment to investigate how group identity affects individuals' cooperation and strategies in the IRPD. The framework embeds group-contingent social preferences into the IRPD to examine the impact of group identity on SPNE and risk dominance, yielding testable hypotheses about the effects of group identity on cooperation and strategy choice. Our experimental results provide supportive evidence for the framework and demonstrate that the impact of group identity hinges on the discount factor, which determines the strategic risk environment for cooperation. When the discount factor is high enough for cooperation to satisfy both SPNE and risk-dominance criteria, ingroup pairs cooperate more, are less likely to adopt the Always Defect strategy, and sustain cooperation more while persisting less in defection across the supergames, compared to outgroup or control pairs. In contrast, when the discount factor is lower so that cooperation remains an SPNE but is no longer risk dominant, these patterns become weaker and more heterogeneous across sessions. Moreover, although the group identity treatment initially raises expectations of cooperation from ingroup relative to outgroup partners, those beliefs do not differentially shape cooperation over time across treatments, suggesting that group identity operates through preferences rather than beliefs.

Long-term cooperation is crucial for organizations navigating an increasingly diverse workforce in modern life. Our findings provide evidence supporting the effectiveness of identity-building practices as a means of fostering sustained cooperation among employees. It is worth noting that existing literature suggests a stronger impact of group identity in groups with real social ties compared to artificial minimal groups (Goette et al., 2006, 2012). Accordingly, the magnitudes we observe in our minimal group laboratory setting likely represent a lower bound for the impact of group identity on long-term cooperation and strategic behavior in real-world organizations with real social ties.

This paper contributes to the economics literature on game theory by introducing a critical and effective non-pecuniary behavioral factor, group identity, into the analysis of infinitely repeated games. Unlike previous studies focusing on monetary factors (e.g., payoffs) and game environments (e.g., discount factors), this study shows that co-players' group identity significantly influences participants' actions and strategies.

These findings enhance our understanding of how behavioral factors can help address cooperation challenges in infinitely repeated interactions, a complex and important research domain in game theory.

Furthermore, incorporating group identity into repeated games allows us to examine its impact at the strategy level. We find that participants are less likely to adopt the defect strategy with ingroup members compared to the baseline without group distinctions. While prior studies have documented the impact of group identity on preferences and beliefs (e.g., Chen and Li, 2009; Ockenfels and Werner, 2014), our evidence on repeated game strategies offers new insight into how identity shapes strategic adaptation and long-run cooperation.

This paper represents a valuable step toward understanding the role of group identity in infinitely repeated games. Future research could extend this line of inquiry, both theoretically and experimentally, by examining how group identity and strategic risks interact in other game environments, such as coordination games (e.g., Chen and Chen, 2011), principal-agent problems, gift-exchange games, repeated games with imperfect monitoring (Aoyagi and Fréchette, 2009), and those involving costly punishment (Dreber et al., 2008). Another promising direction is to investigate group identity in more natural settings, such as evaluating how identity-based social groups (e.g., teams in organizations) influence cooperation in real-world repeated interactions, as well as studying the implication of group identity in prisoner's dilemma with group decisions (Kagel and McGee, 2016; Cason et al., 2019; Cooper and Kagel, 2023).

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List of Figures

Figure 1: Games A and B

	C	D
C	R, R	S, T
D	T, S	P, P

Game A

	C	D
C	$1, 1$	$-\ell, 1+g$
D	$1+g, -\ell$	$0, 0$

Game B

Figure 2: Games C and D

	C	D
C	$1, 1$	$-L, 1+G$
D	$1+G, -L$	$0, 0$

Game C ($I = 0$)

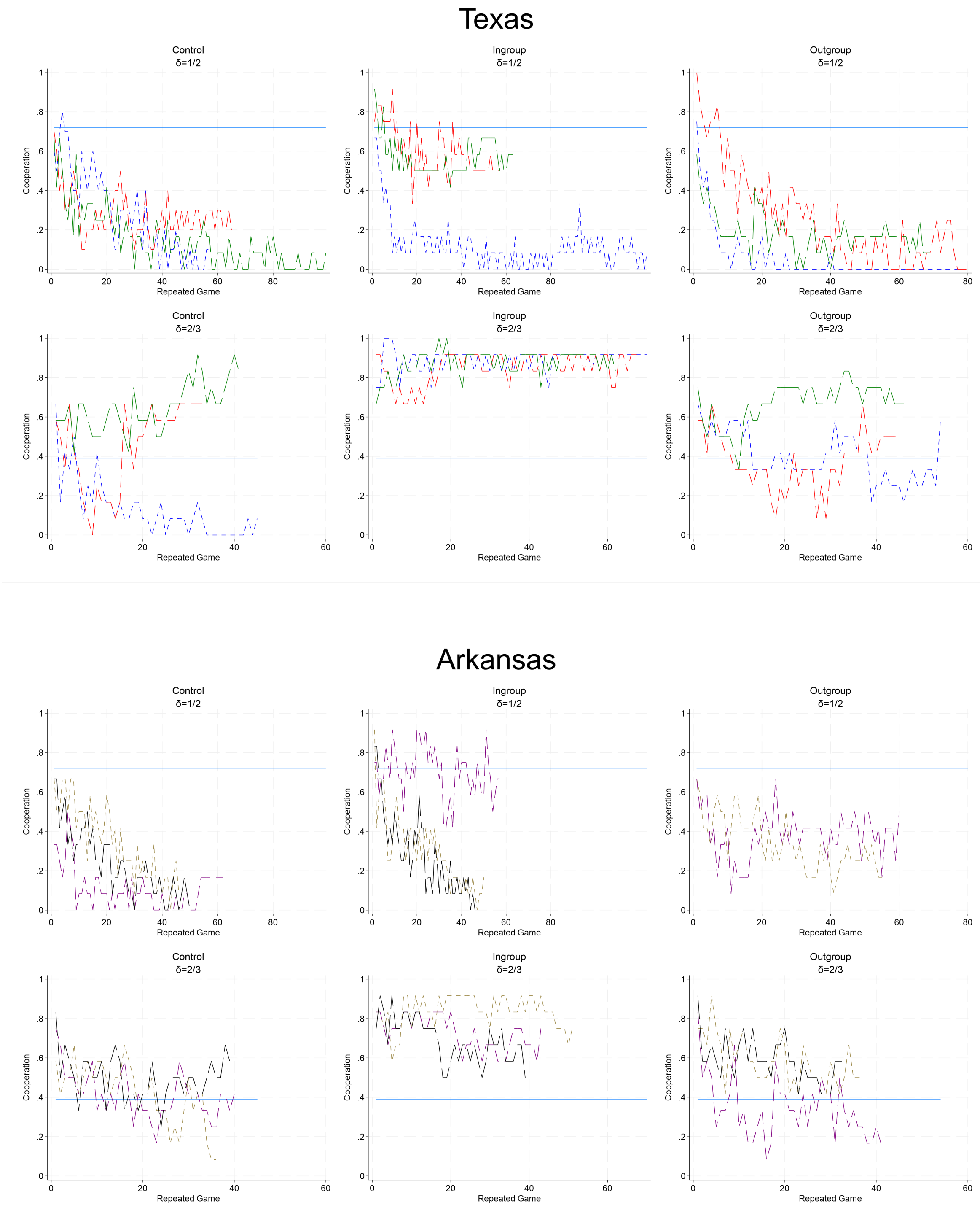
	C	D
C	$1, 1$	$-\widehat{L}, 1+\widehat{G}$
D	$1+\widehat{G}, -\widehat{L}$	$0, 0$

Game D ($I = 1$)

Figure 3: Stage Game Payoffs

		Player 2	
		C	D
Player 1	C	$40, 40$	$12, 50$
	D	$50, 12$	$25, 25$

Figure 4: Time Series of Cooperation Rate by Treatment and Session (First Round)



Note: The horizontal line in each diagram represents the basin of attraction of Always Defect.

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Table 1: Experimental Design

Treatment		Group Assignment	Problem Solving	Texas		Arkansas	
				Number of Sessions	Number of Subjects	Number of Sessions	Number of Subjects
$\delta = \frac{1}{2}$	Control	None	Self	3	32	3	36
	Ingroup	Random	Chat	3	36	3	36
	Outgroup	Random	Chat	3	36	3	36
$\delta = \frac{2}{3}$	Control	None	Self	3	36	3	36
	Ingroup	Random	Chat	3	36	3	36
	Outgroup	Random	Chat	3	36	3	36
Total				18	212	18	216

Notes: We conducted eighteen sessions at UTD in 2014 and eighteen sessions at UA in 2024. Each session comprised twelve participants, except for two control sessions for $\delta = \frac{1}{2}$ at UTD each consisting of 10 participants. This yielded 212 participants at UTD and 216 at UA, totaling 428 for the experiment.

Table 2: Numerical Calculations of Key Thresholds in the Theoretical Framework

Parameter	I = 0 (Control)	I = 1 (Ingroup)
SPNE Cutoff	$\delta^* \approx 0.223$	$\widehat{\delta^*} \approx 0.1$
Risk Dominance	$\delta^*_{RD} \approx 0.56$	$\widehat{\delta}^*_{RD} \approx 0.496$
$\delta = \frac{1}{2}$	$\Delta U \left(\delta = \frac{1}{2}; I = 0\right) \approx 0.12$	$\Delta U \left(\delta = \frac{1}{2}; I = 1\right) \approx -0.007$
$\delta = \frac{2}{3}$	$\Delta U \left(\delta = \frac{2}{3}; I = 0\right) \approx -0.22$	$\Delta U \left(\delta = \frac{2}{3}; I = 1\right) \approx -0.31$

Table 3: Average Cooperation Rate (%) by Treatment

		Control	Ingroup	Outgroup	Control vs. Ingroup	Control vs. Outgroup	Ingroup vs. Outgroup
$\delta = \frac{1}{2}$		N=6	N=12	N=6			
First supergame	First round	60.6	80.6	73.6	0.014	0.186	0.184
		(5.61)	(4.02)	(5.86)			
	All rounds	56.6	75.2	65.7	0.010	0.221	0.239
		(2.85)	(4.15)	(6.32)			
All supergames	First round	23.1	42.8	25.9	0.250	0.699	0.553
		(3.60)	(8.97)	(5.07)			
	All rounds	20.7	39.7	22.7	0.250	0.589	0.437
		(3.11)	(8.80)	(4.33)			
$\delta = \frac{2}{3}$		N=6	N=12	N=6			
First supergame	First round	66.7	79.2	75.0	0.059	0.273	0.489
		(4.30)	(4.17)	(4.81)			
	All rounds	58.4	71.9	66.6	0.138	0.420	0.468
		(5.64)	(5.07)	(4.61)			
All supergames	First round	42.6	81.2	49.2	0.000	0.937	0.001
		(6.92)	(3.87)	(5.60)			
	All rounds	37.2	73.7	42.2	0.000	1.000	0.001
		(6.57)	(3.95)	(5.58)			

Note: The average cooperation rate is reported on the *independent* session/group level. Standard errors are included in the paratheses. Note that the control (or outgroup) treatment for each δ consists of six independent sessions; the ingroup treatment for each δ consists of six sessions and hence twelve independent *groups* since the two groups within each session do not interact. The last three columns report the two-sided p-values of the Wilcoxon ranksum exact test.

Table 4: Treatment Effect on Cooperation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
δ	1/2	1/2	2/3	2/3	1/2	1/2	2/3	2/3
Round	1st	1st	1st	1st	All	All	All	All
Time trend	No	Yes	No	Yes	No	Yes	No	Yes
Ingroup	0.176	0.186	0.405***	0.312***	0.181*	0.201*	0.379***	0.332***
	(0.107)	(0.112)	(0.082)	(0.048)	(0.102)	(0.103)	(0.080)	(0.052)
Outgroup	0.034	0.074	0.063	0.081	0.028	0.064	0.047	0.134*
	(0.051)	(0.085)	(0.092)	(0.064)	(0.044)	(0.079)	(0.091)	(0.068)
Block		-0.005***		-0.003		-0.004***		-0.000
		(0.001)		(0.004)		(0.001)		(0.004)
block × Ingroup		0.000		0.004		-0.000		0.002
		(0.001)		(0.004)		(0.001)		(0.004)
block × Outgroup		-0.001		-0.001		-0.001		-0.004
		(0.002)		(0.004)		(0.002)		(0.005)
UA dummy	0.103	0.039	-0.025	-0.027	0.076	0.021	-0.029	-0.030
	(0.071)	(0.066)	(0.058)	(0.057)	(0.068)	(0.062)	(0.057)	(0.054)
Constant	0.169***	0.360***	0.432***	0.491***	0.160***	0.329***	0.388***	0.390***
	(0.045)	(0.064)	(0.095)	(0.052)	(0.042)	(0.057)	(0.090)	(0.054)
Observations	14,000	14,000	9,888	9,888	27,380	27,380	28,392	28,392
No. subjects	212	212	216	216	212	212	216	216
Adjusted R^2	0.039	0.104	0.144	0.148	0.037	0.096	0.124	0.129
Ingroup vs. outgroup								
p value	0.195	0.350	0.000	0.001	0.148	0.229	0.000	0.003

Notes: The analysis is based on a linear probability model (Wooldridge, 2010) with cluster bootstrap-t procedure to correct for the small number of sessions (Cameron et al., 2008). Standard errors are clustered on the individual subject and experimental session levels. * $p < 10\%$, ** $p < 5\%$, and *** $p < 1\%$.

Table 5: Impact of Group Identity on Initial Beliefs

	(1)	(2)
δ	1/2	2/3
Round	Initial round of the first supergame	Initial round of the first supergame
Ingroup	0.075	0.061
	(0.057)	(0.037)
Outgroup	-0.035	-0.021
	(0.064)	(0.029)
UA dummy	-0.017	0.014
	(0.045)	(0.030)
Constant	0.645***	0.652***
	(0.056)	(0.024)
Observations	212	216
No. subjects	212	216
Adjusted R^2	0.016	0.002
Ingroup vs. outgroup		
p value	0.021	0.071

Notes: The dependent variable is a participant's initial belief of the likelihood of the co-player's cooperation (0∼1 inclusive). Linear regression models are used with cluster bootstrap-t procedure to correct for the small number of sessions (Cameron et al., 2008). Standard errors are clustered on the individual subject and experimental session levels. * $p < 10\%$, ** $p < 5\%$, and *** $p < 1\%$.

Table 6: Impact of Group Identity and Initial Beliefs on Cooperation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
δ	1/2	1/2	2/3	2/3	1/2	1/2	2/3	2/3
Round	1st	1st	1st	1st	1st	1st	1st	1st
Supergame	1st	1st	1st	1st	All	All	All	All
Time trend in the impact of belief	No	No	No	No	No	Yes	No	Yes
Ingroup (α_1)	0.149**	0.047	0.066*	0.155	0.033	0.024	0.427***	0.381***
	(0.063)	(0.201)	(0.032)	(0.152)	(0.147)	(0.155)	(0.101)	(0.103)
Outgroup (α_2)	0.156**	0.176	0.104**	0.073	0.062	0.060	0.003	-0.010
	(0.061)	(0.213)	(0.043)	(0.127)	(0.108)	(0.105)	(0.106)	(0.109)
Block (α_3)					-0.005***	-0.003***	-0.001	0.004**
					(0.001)	(0.001)	(0.001)	(0.002)
Initial belief (α_4)	0.702***	0.670***	0.963***	0.985***	0.027	0.131	0.256**	0.489***
	(0.102)	(0.163)	(0.081)	(0.130)	(0.080)	(0.092)	(0.107)	(0.088)
Ingroup × Initial belief (α_5)		0.146		-0.125	0.221	0.218	-0.045	-0.146
		(0.223)		(0.212)	(0.137)	(0.138)	(0.162)	(0.152)
Outgroup × Initial belief (α_6)		-0.034		0.049	-0.038	0.027	0.108	0.103
		(0.287)		(0.177)	(0.126)	(0.135)	(0.159)	(0.167)
Initial belief × Block (α_7)						-0.003*		-0.011**
						(0.001)		(0.004)
Ingroup × Initial belief × Block (α_8)						0.000		0.007
						(0.002)		(0.004)
Outgroup × Initial belief × Block (α_9)						-0.002		0.001
						(0.002)		(0.005)
UA dummy	-0.030	-0.029	0.089***	0.088***	0.041	0.042	-0.033	-0.031
	(0.051)	(0.053)	(0.028)	(0.028)	(0.068)	(0.068)	(0.057)	(0.058)
Constant	0.172*	0.192	-0.012	-0.027	0.348***	0.273***	0.280***	0.183***
	(0.094)	(0.137)	(0.056)	(0.081)	(0.077)	(0.076)	(0.048)	(0.051)
Observations	212	212	216	216	14,000	14,000	9,888	9,888
Adjusted R^2	0.197	0.191	0.390	0.386	0.110	0.112	0.170	0.178
Difference between ingroup and outgroup (p value)								
$\alpha_1 - \alpha_2$	0.894	0.521	0.367	0.621	0.838	0.815	0.01	0.019
$\alpha_5 - \alpha_6$		0.503		0.407	0.057	0.143	0.427	0.275
$\alpha_8 - \alpha_9$						0.193		0.076

Notes: The dependent variable is whether a participant cooperates. The analysis is based on a linear probability model (Wooldridge, 2010) with cluster bootstrap-t procedure to correct for the small number of sessions (Cameron et al., 2008). The first four columns focus on the initial round (i.e., the first round of the first supergame) while Columns 5-8 focus on the first round of all supergames. The initial belief is defined as one's belief of the co-player's cooperation in the first round of the first supergame, which is exogenous to their later cooperation decisions. Standard errors are clustered on the individual subject and experimental session levels. * $p < 10\%$, ** $p < 5\%$, and *** $p < 1\%$.

Table 7: Stability in Cooperation across Supergames (%) by Treatment

	Control	Ingroup	Outgroup	Control vs. Ingroup	Control vs. Outgroup	Ingroup vs. Outgroup
$\delta = \frac{1}{2}$	N = 6	N = 12	N = 6			
$C_t \| C_{t-1}$	62.0	69.8	69.5	0.385	0.255	0.892
	(4.8)	(5.0)	(4.5)			
$C_t \| C_{t-1} \| C_{t-2}$	72.0	77.2	77.4	0.553	0.485	0.840
	(4.6)	(4.6)	(3.8)			
$C_t \| C_{t-1} \| C_{t-2} \| C_{t-3}$	80.2	78.2	80.5	0.892	0.699	0.892
	(3.4)	(5.4)	(4.3)			
$C_t \| C_{t-1} \| \ldots \| C_{t-4}$	83.3	76.0	82.0	0.964	0.589	0.964
	(3.5)	(8.0)	(5.4)			
$C_t \| C_{t-1} \| \ldots \| C_{t-5}$	84.2	86.5	82.3	0.525	0.485	0.961
	(3.7)	(3.6)	(8.6)			
$\delta = \frac{2}{3}$	N = 6	N = 12	N = 6			
$C_t \| C_{t-1}$	74.1	93.7	80.3	0.001	0.818	0.002
	(8.0)	(1.2)	(4.5)			
$C_t \| C_{t-1} \| C_{t-2}$	82.1	95.5	84.4	0.001	1.000	0.001
	(6.1)	(0.8)	(4.0)			
$C_t \| C_{t-1} \| C_{t-2} \| C_{t-3}$	86.8	96.3	88.2	0.002	0.699	0.003
	(5.1)	(0.7)	(3.1)			
$C_t \| C_{t-1} \| \ldots \| C_{t-4}$	85.4	97.0	90.4	0.001	0.818	0.007
	(7.1)	(0.5)	(2.5)			
$C_t \| C_{t-1} \| \ldots \| C_{t-5}$	86.2	96.9	91.8	0.007	1.000	0.013
	(7.3)	(0.5)	(1.9)			

Notes: This table reports the average likelihood of cooperating in the first round of supergame t conditional on cooperation in the first round of lagged supergames in $t-1, ...,$ and $t-5$. The average rate is reported on the *independent* session/group level. Standard errors are included in the paratheses. Note that the control (or outgroup) treatment for each δ consists of six independent sessions; the ingroup treatment for each δ consists of six sessions and hence twelve independent *groups* since the two groups within each session do not interact. The last three columns report the two-sided p-values of the Wilcoxon ranksum exact test.

Table 8: Stability in Defection across Supergames (%) by Treatment

	Control	Ingroup	Outgroup	Control vs. Ingroup	Control vs. Outgroup	Ingroup vs. Outgroup
$\delta = \frac{1}{2}$	N = 6	N = 12	N = 6			
$D_t \vert D_{t-1}$	90.3	82.8	91.3	0.067	0.818	0.180
	(1.3)	(3.3)	(1.8)			
$D_t \vert D_{t-1} \vert D_{t-2}$	93.0	88.8	94.5	0.385	0.485	0.250
	(0.9)	(2.7)	(1.2)			
$D_t \vert D_{t-1} \vert D_{t-2} \vert D_{t-3}$	94.5	92.4	96.2	0.682	0.240	0.250
	(0.8)	(2.3)	(0.8)			
$D_t \vert D_{t-1} \vert \ldots \vert D_{t-4}$	96.0	93.6	97.2	0.682	0.132	0.180
	(0.6)	(1.9)	(0.5)			
$D_t \vert D_{t-1} \vert \ldots \vert D_{t-5}$	96.6	94.5	97.5	0.616	0.180	0.291
	(0.5)	(1.6)	(0.5)			
$\delta = \frac{2}{3}$	N = 6	N = 12	N = 6			
$D_t \vert D_{t-1}$	85.1	62.1	83.3	0.015	0.699	0.053
	(2.1)	(7.6)	(2.6)			
$D_t \vert D_{t-1} \vert D_{t-2}$	89.2	79.5	90.4	0.462	0.589	0.256
	(1.7)	(5.7)	(1.6)			
$D_t \vert D_{t-1} \vert D_{t-2} \vert D_{t-3}$	92.0	79.6	92.9	1.000	0.589	0.942
	(1.4)	(9.0)	(1.0)			
$D_t \vert D_{t-1} \vert \ldots \vert D_{t-4}$	93.1	83.8	94.3	0.562	0.310	0.792
	(1.1)	(9.6)	(0.7)			
$D_t \vert D_{t-1} \vert \ldots \vert D_{t-5}$	94.3	92.4	94.9	0.263	0.589	0.511
	(0.9)	(3.6)	(0.5)			

Notes: This table reports the average likelihood of defecting in the first round of supergame t conditional on defecting in the first round of lagged supergames in $t-1, ...,$ and $t-5$. The average rate is reported on the *independent* session/group level. Standard errors are included in the paratheses. Note that the control (or outgroup) treatment for each δ consists of six independent sessions; the ingroup treatment for each δ consists of six sessions and hence twelve independent *groups* since the two groups within each session do not interact. The last three columns report the two-sided p-values of the Wilcoxon ranksum exact test.

Table 9: Dynamics within Supergames (%) by Treatment

	Control	Ingroup	Outgroup	Control vs. Ingroup	Control vs. Outgroup	Ingroup vs. Outgroup
$\delta = \frac{1}{2}$	N = 6	N = 12	N = 6			
$C_t\|(C_{t-1}, D_{t-1})\|(C_{t-2}, C_{t-2})$	36.7	51.7	47.0	0.602	0.667	0.939
Lenience	(18.6)	(18.3)	(16.9)			
$C_t\|(D_{t-1}, C_{t-1})\|(D_{t-2}, D_{t-2})$	31.8	24.9	34.6	0.297	0.506	0.123
Forgiveness	(7.1)	(9.2)	(6.9)			
$C_t\|(D_{t-1}, D_{t-1})\|(D_{t-2}, C_{t-2})$	11.8	13.7	9.4	0.982	0.483	0.786
Response to punishment	(3.1)	(4.8)	(4.2)			
$\delta = \frac{2}{3}$	N = 6	N = 12	N = 6			
$C_t\|(C_{t-1}, D_{t-1})\|(C_{t-2}, C_{t-2})$	43.3	32.7	35.2	0.497	0.446	0.946
Lenience	(11.5)	(8.6)	(8.3)			
$C_t\|(D_{t-1}, C_{t-1})\|(D_{t-2}, D_{t-2})$	30.7	32.0	34.1	0.871	0.903	0.790
Forgiveness	(9.5)	(8.0)	(2.9)			
$C_t\|(D_{t-1}, D_{t-1})\|(D_{t-2}, C_{t-2})$	13.5	29.6	10.4	0.017	0.240	0.009
Response to punishment	(1.4)	(4.5)	(2.2)			

Notes: This table reports the average likelihood of cooperating at round t after playing with the co-player $(C_{t-1}, D_{t-1})|(C_{t-2}, C_{t-2})$, $(D_{t-1}, C_{t-1})|(D_{t-2}, D_{t-2})$, or $(D_{t-1}, D_{t-1})|(D_{t-2}, C_{t-2})$ in the previous two rounds within a supergame. The average rate is reported on the *independent* session/group level. Standard errors are included in the paratheses. Note that the control (or outgroup) treatment for each δ consists of six independent sessions; the ingroup treatment for each δ consists of six sessions and hence twelve independent *groups* since the two groups within each session do not interact. The last three columns report the two-sided p-values of the Wilcoxon ranksum exact test.

Table 10: Impact of Group Identity on Other Determinants of Cooperation

	$\delta = \frac{1}{2}$ (1)	$\delta = \frac{2}{3}$ (2)
Co-player cooperated in round 1	0.141***	0.202***
of previous supergame (β_1)	(0.025)	(0.062)
Number of rounds in previous	0.003	0.013***
supergame (β_2)	(0.008)	(0.004)
Subject cooperated in round 1	0.130**	0.250***
of the first supergame (β_3)	(0.059)	(0.072)
Ingroup (β_4)	-0.115**	0.517***
	(0.052)	(0.090)
Outgroup (β_5)	-0.067	-0.031
	(0.049)	(0.074)
Ingroup $\times$ Co-player cooperated in round 1	0.276***	-0.151**
of previous supergame (β_6)	(0.087)	(0.066)
Outgroup $\times$ Co-player cooperated in round 1	0.051	-0.078
of previous supergame (β_7)	(0.052)	(0.062)
Ingroup $\times$ Number of rounds in previous	0.021*	-0.004
supergame (β_8)	(0.011)	(0.005)
Outgroup $\times$ Number of rounds in previous	0.021**	0.002
supergame (β_9)	(0.008)	(0.005)
Ingroup $\times$ Subject cooperated in round 1	0.117	-0.105
of the first supergame (β_{10})	(0.073)	(0.111)
Outgroup $\times$ Subject cooperated in round 1	0.033	0.131
of the first supergame (β_{11})	(0.067)	(0.087)
UA dummy (β_0)	0.080*	-0.055
	(0.045)	(0.049)
Constant	0.058	0.151**
	(0.045)	(0.067)
Observations	13,788	9,672
Adjusted R^2	0.169	0.229
Ingroup: overall effects (corrected *p* values in brackets)		
Co-player cooperated in round 1	0.418	0.051
of previous supergame ($\beta_1 + \beta_6$)	[0.001]	[0.447]
Number of rounds in previous	0.025	0.009
supergame ($\beta_2 + \beta_8$)	[0.023]	[0.067]
Subject cooperated in round 1	0.247	0.145
of the first supergame ($\beta_3 + \beta_{10}$)	[0.000]	[1.000]
Outgroup: overall effects (corrected *p* values in brackets)		
Co-player cooperated in round 1	0.192	0.124
of previous supergame ($\beta_1 + \beta_7$)	[0.007]	[0.000]
Number of rounds in previous	0.025	0.014
supergame ($\beta_2 + \beta_9$)	[0.000]	[0.002]
Subject cooperated in round 1	0.163	0.381
of the first supergame ($\beta_3 + \beta_{11}$)	[0.005]	[0.000]
Differences between ingroup and outgroup (corrected *p* values)		
$\beta_4 - \beta_5$	1.000	0.000
$\beta_6 - \beta_7$	0.261	0.021
$\beta_8 - \beta_9$	1.000	1.000
$\beta_{10} - \beta_{11}$	1.000	0.480

Notes: The analysis is based on a linear probability model (Wooldridge, 2010) with cluster bootstrap-t procedure to correct for the small number of sessions (Cameron et al., 2008). Standard errors in parentheses are clustered on the individual subject and experimental session levels. P values in the three lower panels are corrected for multiple hypothesis testing using the Bonferroni method. * $p < 10\%$, ** $p < 5\%$, and *** $p < 1\%$.

Table 11: Repeated Game Strategies Estimation (MLE)

	$\delta = \frac{1}{2}$			$\delta = \frac{2}{3}$		
	Control	Ingroup	Outgroup	Control	Ingroup	Outgroup
	(1)	(2)	(3)	(4)	(5)	(6)
AD	0.942***	0.641***	0.820***	0.558***	0.133	0.499***
	(0.057)	(0.203)	(0.106)	(0.175)	(0.086)	(0.122)
AC	0.000	0.101	0.014	0.049	0.129	0.103
	(0.000)	(0.100)	(0.030)	(0.040)	(0.116)	(0.093)
GRIM	0.000	0.015	0.039	0.049	0.298**	0.176**
	(0.012)	(0.080)	(0.053)	(0.057)	(0.118)	(0.087)
TFT	0.045	0.179	0.105	0.339**	0.394***	0.223*
	(0.056)	(0.120)	(0.090)	(0.143)	(0.145)	(0.125)
WSLS	0.000	0.064	0.000	0.000	0.047	0.000
	(0.000)	(0.089)	(0.000)	(0.000)	(0.034)	(0.000)
T2	0.013	0.000	0.022	0.004	0.000	0.000
γ	0.435***	0.453***	0.400***	0.495***	0.432***	0.508***
	(0.054)	(0.075)	(0.073)	(0.061)	(0.089)	(0.076)

Notes: The analysis is based on the second half of all supergames in each session. Bootstrapped standard errors are in parentheses. * $p < 10\%$, ** $p < 5\%$, and *** $p < 1\%$.

Online Appendices

A Comparisons to Camera and Hohl (2021)

<table>
<tr><td colspan="2"></td><td>Our paper</td><td>Camera and Hohl (2021)</td></tr>
<tr><td colspan="2">Main research question</td><td>How does group identity affect cooperative action and strategies in infinitely repeated games with different degrees of strategic risk?</td><td>Do group effects emerge in strategic settings where it is not easy to observe and compare characteristics on which to base categorizations and behaviors?</td></tr>
<tr><td rowspan="6">Experimental design</td><td>Creating group identity</td><td>Random assignment plus an incentivized game-irrelevant task that involves within-group communication</td><td>Random assignment plus two supergames within group in fixed pairs to enhance group identity</td></tr>
<tr><td>Groups</td><td>Ingroup vs. an outgroup</td><td>Ingroup vs. two outgroups</td></tr>
<tr><td>Within- or between-subject design</td><td>Between-subject design. Each participant is always paired with ingroup or outgroup members in the Ingroup or Outgroup treatments. Fixed matching within each supergame.</td><td>Within-subject design. Subjects interact as strangers from ingroup and outgroups across rounds within a supergame.</td></tr>
<tr><td>Repeated games</td><td>The rounds in a supergame continue with a probability of $\frac{1}{2}$ (or $\frac{2}{3}$) in the treatments with the high (or low) strategic risk for cooperation.</td><td>Two supergames with 18 rounds each plus an indefinite number of additional rounds with probability 0.75.</td></tr>
<tr><td>Information</td><td>Co-players' group-affiliations are salient given the separate ingroup and outgroup between-subject treatments. A history window shows everyone the history of their own choices and their co-players' choices in all previous supergames. The co-players' ID numbers were hidden to avoid confounding reputation effects.</td><td>Opaque information: "Consumers" are blind to "Producers"; "Producers" can see "Consumers" group affiliations but are blind to their and their group's track records.</td></tr>
<tr><td>Inequality</td><td>Symmetric payoffs. No inequality.</td><td>Group-dependent inequality in the Unequal Treatment</td></tr>
<tr><td colspan="2">Main findings</td><td>Group identity leads to higher cooperation with ingroup in games with low strategic risk for cooperation. Its impact is less robust in games with high strategic risk for cooperation. Subjects are less likely to adopt the Always Defect strategy with ingroup in both games regardless of the strategic risk for cooperation.</td><td>Finds no evidence of group biases. Suggests that group effects are less likely to emerge when players cannot easily observe and compare characteristics on which to base categorizations and behaviors.</td></tr>
</table>

B Proofs of Propositions

Proof of Proposition 1. Given inequality (4), we can immediately verify:

$$\widehat{\delta}^* = \frac{\widehat{G}}{1+\widehat{G}} < \delta^* = \frac{G}{1+G} < \underline{\delta} = \frac{g}{1+g},$$

which implies that group identity **further** facilitates cooperation compared to the case where $I = 0$ by lowering the minimal required discount factor for cooperation.

Next, it is straightforward to see that δ^* and $\widehat{\delta}^*$ are independent of θ, or $\frac{d\delta^*}{d\theta} = \frac{d\widehat{\delta}^*}{d\theta}$. For comparative statics in ρ, notice that δ^* strictly increases in G and $\widehat{\delta}^*$ strictly increases in $\widehat{G}$. In addition, by the definitions of $1+G$ and $1+\widehat{G}$, we have

$$\frac{dG}{d\rho} = -\ell - (1+g) < 0,$$
$$\frac{d\widehat{G}}{d\rho} = -(1+a)\ell - (1+a)(1+g) < 0.$$

We hence have $\frac{d\delta^*}{d\rho} < 0$ and $\frac{d\widehat{\delta}^*}{d\rho} < 0$. □

Proof of Proposition 2.

Part (a). By definition, in Game C $(I=0)$, the cooperative equilibrium (σ^G, σ^G) is risk-dominant if

$$\frac{1-(1-\delta)L}{2} \geq \frac{(1-\delta)(1+G)}{2}$$
$$\Leftrightarrow \delta \geq \delta^*_{RD} \equiv \frac{G+L}{1+G+L}$$

Similarly, in Game D $(I=1)$, (σ^G, σ^G) is risk-dominant if

$$\frac{1-(1-\delta)\widehat{L}}{2} \geq \frac{(1-\delta)(1+\widehat{G})}{2}$$
$$\Leftrightarrow \delta \geq \widehat{\delta}^*_{RD} \equiv \frac{\widehat{G}+\widehat{L}}{1+\widehat{G}+\widehat{L}}$$

The inequalities in (4) immediately imply that

$$\frac{1-(1-\delta)L}{2} \geq \frac{(1-\delta)(1+G)}{2} \Rightarrow \frac{1-(1-\delta)\widehat{L}}{2} \geq \frac{(1-\delta)(1+\widehat{G})}{2},$$

i.e., if (σ^G, σ^G) is risk-dominant in the repeated prisoners' dilemma of Game C where $I=0$, it is also risk-dominant in the repeated prisoners' dilemma of Game D where $I=1$. We hence have

$$\widehat{\delta}^*_{RD} = \frac{\widehat{G}+\widehat{L}}{1+\widehat{G}+\widehat{L}} < \delta^*_{RD} = \frac{G+L}{1+G+L}.$$

Furthermore, it is tedious but straightforward to verify that:

$$\frac{\partial \delta^*_{RD}}{\partial \rho} = \frac{\partial \delta^*_{RD}}{\partial \theta} = -\frac{1}{(1-\rho-\theta)^2(1+g+\ell)} < 0,$$
$$\frac{\partial \widehat{\delta}^*_{RD}}{\partial \rho} = -\frac{1+a}{(1+g+\ell)(1-\rho-\theta-a\rho-b\theta)^2} < 0,$$
$$\frac{\partial \widehat{\delta}^*_{RD}}{\partial \theta} = -\frac{1+b}{(1+g+\ell)(1-\rho-\theta-a\rho-b\theta)^2} < 0.$$

Part (b). Recall that $\Delta U(\delta; I = 0)$ can be written explicitly as:

$$\begin{aligned}\Delta U(\delta; I = 0) &= \Delta U_D(\delta) - \Delta U_G(\delta) \\ &= (1-\delta)^2 L^2 - [1 - (1-\delta)(1+G)]^2.\end{aligned}$$

Similarly, we have

$$\Delta U(\delta; I = 1) = (1-\delta)^2 \widehat{L}^2 - [1 - (1-\delta)(1+\widehat{G})]^2.$$

Again inequalities (4), reproduced below:

$$-L < -\widehat{L} < -\ell, \text{ and } 1 + \widehat{G} < 1 + G < 1 + g,$$

directly imply that for all $\delta \in (0, 1)$

$$\begin{aligned}&(1-\delta)^2 L^2 > (1-\delta)^2 \widehat{L}^2, \text{ and} \\ &[1 - (1-\delta)(1+G)]^2 < [1 - (1-\delta)(1+\widehat{G})]^2.\end{aligned}$$

Therefore, we have that for all $\delta \in (0, 1)$,

$$\Delta U(\delta; I = 1) < \Delta U(\delta; I = 0),$$

or group identity strictly reduces the riskiness of the Grim Trigger equilibrium for each discount factor δ. □

C Sample Experimental Instructions

We present the experimental instructions for the control treatment. Instructions for the ingroup and outgroup treatments also included the words/sentences in italics and square brackets below.

You are about to participate in a decision-making process in which you will play games with other participants in this room. What you earn depends on your decisions, partly on the decisions of others, and partly on chance.

As you came in you drew an index card [*a white envelope*] with a number on it. This number, randomly assigned, is your ID number used in this experiment to ensure anonymity of your decisions. Please do not show your ID number to anyone else.

Please turn off cellular phones now. We ask that you do not talk to each other during the experiment. If you have a question, please raise your hand and an experimenter will assist you.

This experiment consists of two parts and 12 participants. Your earnings in each part are given in points. At the end of the experiment you will be paid in private and in cash based on the following exchange rate \$1 = 200 points.

Your total earnings will be the sum of your earnings in each part plus a \$5 participation fee.

We will now start Part 1. The instructions for Part 2 will be given after Part 1 ends.

Part 1

[*Please open the white envelope and discreetly pull out the contents. It contains either a **Green** or an **Orange** card. The color represents the group that you are assigned to. The 12 participants in this experiment are randomly assigned to one of two groups of 6 people. If you drew a Green card, you will be in the Green group. If you drew an Orange card, you will be in the Orange group. The group assignment will remain the same throughout the experiment.*]

Please return the index card to the envelope now. Do not show them to others. Please raise your hand if you have any questions about this step.

[*We will now start Part 1. The instructions for Part 2 will be given after Part 1 ends.*]

In Part 1 everyone will be shown five pairs of paintings by two artists, Kandinsky and Klee. Please refer to the folder insert for the name of artist who made each painting. You will have three minutes to study these paintings. Then you will be asked to answer questions about two other paintings. Each correct answer is worth 100 points. [*You may communicate with others in your group through a chat program while answering the questions.*]

You will now receive paintings 6 and 7. Please select the artist who you think made each painting. For each correct answer, you will be rewarded with 100 points. [*Meanwhile, you can use the group chat program below to communicate with others in your group. Please do not identify yourself or send any information that could be used to identify you (e.g. gender, race, and major). Please refrain from using obscene or offensive language. Except for these restrictions, you may type whatever you want in the lower box of the chat program. Messages will be shared only with your group members.*] You will be given up to 8 minutes to answer both questions. Submit your answers below when you are ready.

[*Note you are not required to give the same answers as your group members.*] You will find out about your earnings in Part 1 at the end of the experiment.

[*Please tell us how attached you feel to your group and the other group at this moment. Enter a number from 1 ("not attached at all") to 10 ("very closely attached") that most accurately reflects your feelings. (These answers will not affect your earnings.)*

Your attachment to your group ______

Your attachment to the other group ______]

General Instructions

In part 2 you will make decisions in rounds. You will be randomly paired with another participant in this room [***in your group/in the other group***] (called your **co-player**) for sequences of rounds. You will not know who your co-player is and vice-versa. Each sequence of rounds is referred to as a **block**. Once a block ends, you will be randomly paired with another participant [***in your group/in the other group***] (i.e., another co-player) for a new block. [*A participant in the **Green** group will only be paired with another participant in the **Green/Orange** group. A participant from the **Orange** group will only be paired with another participant in the **Orange/Green** group.*]

The number of rounds in a block is randomly determined. After each round, there is a 50% chance that the block will continue for another round and a 50% chance that the block will end. If the block continues, you will play with the same co-player. If the block ends, a new block will start where you will be paired with a different co-player. For example, if you finish round 5 of block 3, round 6 of block 3 will occur with a 50% chance, and round 1 of block 4 (a new block) will also occur with a 50% chance.

There will be no new blocks after 60 minutes from the start of Part 2. After 60 minutes, there will be a 50% chance that the block will continue for another round and a 50% chance that the current block (and the experiment) will end.

Choices and Payoffs

The choices and payoffs in each round are shown below:

		Co-player's choice	
		A	**B**
Your choice	**A**	**40**, 40	**12**, 50
	B	**50**, 12	**25**, 25

You and your co-player each can choose between two actions, A and B. The first entry in each cell represents your payoff, and the second entry your co-player's payoff. Your payoffs are bolded for your convenience.

In each round, before knowing each other's decision, you and your co-player will simultaneously choose an action by clicking on the radio button of the choice.

If you and your co-player both choose A, each of you get 40 points in this round.

If you choose A and your co-player chooses B, you get 12 points and your co-player gets 50 points in this round.

If you choose B and your co-player chooses A, you get 50 points and your co-player gets 12 points in this round.

If you and your co-player both choose B, each of you get 25 points in this round.

Therefore, your earnings depend on your decision and your co-player's decision in each round.

Your Guess on the Co-Player's Action

In each round before you choose an action, you will be asked to guess your co-player's choice in this round. You will need to enter your guess in a form as follows.

Your guessed probability that your co-player chooses A: _______

Your guessed probability that your co-player chooses B: _______

For example, if your guess is that your co-player chooses A with a 67% probability and B with a 33% probability, enter 67 in the top entry and 33 in the bottom entry. The two numbers you enter should be both integers between 0 and 100 (including 0 and 100). They need to add up to 100. Click "OK" when you are ready submit your guess.

You will be paid based on the accuracy of your guess.

If your guess is 100% accurate, you will get 2 points. For example, if your guess is that your co-player chooses A with a 100% probability and your co-player actually chooses A, you will get 2 points. If your guess is that your co-player chooses B with a 100% probability and your co-player actually chooses B, you will get 2 points.

If your guess is not 100% accurate, your earnings will be computed such that **the more accurate your guess is, the more points you will get.** Suppose your guess is that your co-player chooses A with a 90% probability and chooses B with a 10% probability. Then you will get 1.98 points if your co-player actually chooses A, or 0.38 points if your co-player actually chooses B. See below if you are interested in how your points are calculated in this example.

Your points if your co-player actually chooses A are $2-(1-90\%)^2-(0-10\%)^2 = 1.98$ points

Your points if your co-player actually chooses B are $2-(0-90\%)^2-(1-10\%)^2 = 0.38$ points

If your guess is 0% accurate, you will get 0 points. For example, if your guess is that your co-player chooses A with a 100% probability and your co-player actually chooses B, you will get 0 points. If your guess is that your co-player chooses B with a 100% probability and your co-player actually chooses A, you will get 0 points.

Note: Since your guess is made before knowing what your co-player actually chooses, the best thing you could do to get as many points as possible is to report what your actual guess is.

Earnings in Part 2

Your earnings in each round are the sum of your earnings in the decision-making task and the guessing task. Your total earnings in Part 2 are your cumulative earnings in all rounds. Recall that 200 points equal to $1.

In each round before you provide your guess and choose an action, your decisions, your co-player's decisions and your earnings in each of the previous rounds will appear in a history window.

Before we start, let's review some important points.

1. You will be randomly paired with a co-player [***in your group/in the other group***], and play with the same person in a sequence of rounds called a block. The number of rounds in a block is randomly determined.

2. At the end of each round, there is a 50% chance that the block will continue to the next round, and there is a 50% chance that a new block will start in which case you will be randomly paired with a different co-player [***in your group/in the other group***].

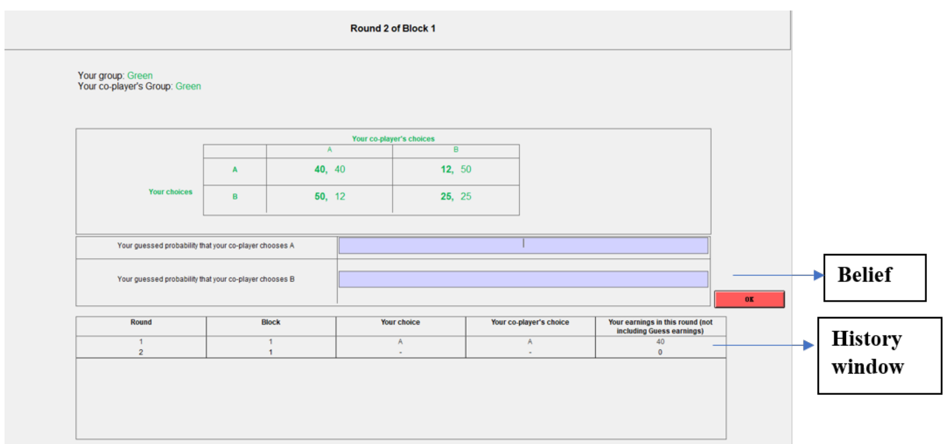
3. In each round you and your co-player each choose an action simultaneously before knowing each other's choice.

4. Before making a choice, you will need to guess on your co-player's choice. The more accurate your guess is, the more points you will earn.

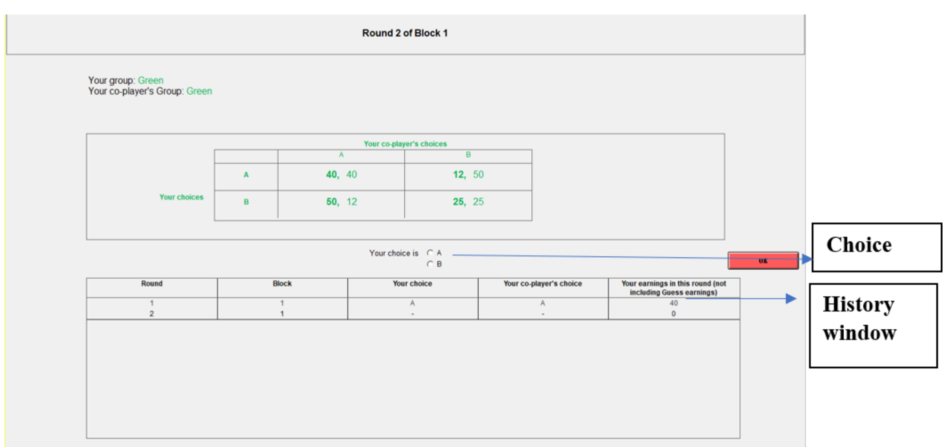
   If you have a question, please raise your hand.

D Sample Screenshots

D.1 Screenshot for Belief Elicitation



D.2 Screenshot for Decision



Note: The co-players' ID numbers were *not* shown in the history window to avoid confounding reputation effects.

E Efficiency

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
δ	1/2	1/2	2/3	2/3	1/2	1/2	2/3	2/3
Round	1st	1st	1st	1st	All	All	All	All
Time trend	No	Yes	No	Yes	No	Yes	No	Yes
Ingroup	6.655	6.869*	15.648***	14.459***	7.343*	7.880*	12.304***	12.779***
	(3.916)	(3.766)	(2.970)	(2.983)	(4.136)	(3.829)	(1.778)	(1.983)
Outgroup	1.268	1.086	2.176	1.711	2.728	2.417	3.160	5.142*
	(1.710)	(1.546)	(3.371)	(3.404)	(3.066)	(2.857)	(2.586)	(2.683)
Block					-0.160***	-0.144***	-0.089	0.013
					(0.040)	(0.038)	(0.128)	(0.145)
Block × Ingroup					-0.004	-0.018	0.141	0.052
					(0.044)	(0.042)	(0.139)	(0.156)
Block × Outgroup					-0.037	-0.036	-0.036	-0.157
					(0.057)	(0.054)	(0.159)	(0.171)
UA dummy	3.297	2.375	-1.331	-1.564	1.056	0.441	-1.346	-1.554
	(2.571)	(2.507)	(2.149)	(2.177)	(2.388)	(2.271)	(2.108)	(2.040)
Constant	67.955***	67.778***	77.327***	76.189***	74.486***	73.582***	79.140***	75.924***
	(1.564)	(1.501)	(3.392)	(3.313)	(2.286)	(2.093)	(1.937)	(2.058)
Observations	14,000	14,000	9,888	9,888	27,380	27,380	28,392	28,392
No. subjects	212	212	216	216	212	212	216	216
Adjusted R^2	0.062	0.056	0.241	0.171	0.160	0.135	0.248	0.178
Ingroup v. outgroup p value								
	0.179	0.138	0.000	0.000	0.300	0.197	0.001	0.004

Notes: The analysis is based on a linear regression model with a cluster bootstrap-t procedure to correct the small number of sessions (Cameron et al., 2008). The dependent variable is efficiency (0∼100) defined as each pair's actual joint payoff as the proportion of their maximum joint payoff possible (80 tokens). Standard errors are clustered on the individual subject and experimental session levels. * $p < 10\%$, ** $p < 5\%$, and *** $p < 1\%$.

F Treatment Effects on Beliefs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
δ	1/2	1/2	2/3	2/3	1/2	1/2	2/3	2/3
Round	1st	1st	1st	1st	All	All	All	All
Time trend	No	Yes	No	Yes	No	Yes	No	Yes
Ingroup	0.164	0.140	0.378***	0.209***	0.173	0.171	0.372***	0.293***
	(0.114)	(0.121)	(0.080)	(0.057)	(0.107)	(0.110)	(0.078)	(0.059)
Outgroup	0.042	0.038	0.037	0.068	0.036	0.036	0.042	0.126*
	(0.063)	(0.089)	(0.086)	(0.059)	(0.055)	(0.083)	(0.086)	(0.064)
Block		-0.006***		-0.004		-0.005***		-0.001
		(0.001)		(0.003)		(0.001)		(0.004)
Block × Ingroup		0.001		0.007*		0.000		0.003
		(0.001)		(0.003)		(0.001)		(0.004)
Block × Outgroup		0.000		-0.001		0.000		-0.004
		(0.001)		(0.004)		(0.001)		(0.004)
UA dummy	0.147*	0.073	0.037	0.040	0.118	0.055	0.016	0.015
	(0.076)	(0.072)	(0.057)	(0.055)	(0.072)	(0.065)	(0.056)	(0.053)
Constant	0.192***	0.441***	0.434***	0.517***	0.180***	0.397***	0.397***	0.416***
	(0.051)	(0.072)	(0.088)	(0.056)	(0.044)	(0.063)	(0.086)	(0.060)
Observations	14,000	14,000	9,888	9,888	27,380	27,380	28,392	28,392
No. subjects	212	212	216	216	212	212	216	216
Adjusted R^2	0.069	0.196	0.229	0.252	0.057	0.161	0.170	0.180
Ingroup v. outgroup								
p value	0.285	0.384	0.000	0.011	0.210	0.235	0.000	0.001

Notes: The analysis is based on a linear regression model with a cluster bootstrap-t procedure to correct the small number of sessions (Cameron et al., 2008). The dependent variable is subject's belief on the co-player's cooperation (0∼1). Standard errors are clustered on the individual subject and experimental session levels. * $p < 10\%$, ** $p < 5\%$, and *** $p < 1\%$.

G Analysis on Repeated Game Strategies

G.1 Repeated Game Strategies in Table 11

Strategy Category	Name of Strategy	Strategy Number	Initial Action	Continued Action
Defective	Always Defect (AD)	1	D	Always D
Cooperative	Always Cooperate (AC)	2	C	Always C
	Grim	3	C	C, but D forever as soon as the co-player chooses D
	Tit-for-Tat (TFT)	4	C	Copy the co-player's previous action
	Win Stay Lose Shift (WSLS)	5	C	C until choosing differently with the co-player
	Two Tits for Tat (T2)	6	C	D twice if the co-player chooses D

G.2 Repeated Game Strategies Estimation (MLE) Based on the Eleven strategies in Table 3 of Fudenberg et al. (2012)

	$\delta = \frac{1}{2}$			$\delta = \frac{2}{3}$		
	Control (1)	Ingroup (2)	Outgroup (3)	Control (4)	Ingroup (5)	Outgroup (6)
AD	0.669***	0.461***	0.660***	0.376***	0.097	0.378***
	(0.129)	(0.168)	(0.107)	(0.144)	(0.087)	(0.122)
AC	0.000	0.094	0.016	0.033	0.044	0.069
	(0.000)	(0.106)	(0.023)	(0.035)	(0.070)	(0.081)
GRIM	0.000	0.000	0.064	0.028	0.251	0.170
	(0.009)	(0.060)	(0.068)	(0.033)	(0.154)	(0.108)
TFT	0.044	0.118	0.056	0.193**	0.312*	0.191
	(0.045)	(0.089)	(0.060)	(0.097)	(0.180)	(0.122)
TF2T	0.000	0.009	0.000	0.062	0.050	0.016
	(0.000)	(0.114)	(0.000)	(0.064)	(0.060)	(0.027)
TF3T	0.000	0.024	0.000	0.016	0.031	0.029
	(0.000)	(0.028)	(0.000)	(0.025)	(0.056)	(0.019)
2TFT	0.000	0.090	0.000	0.044	0.068	0.000
	(0.009)	(0.112)	(0.035)	(0.060)	(0.114)	(0.037)
2TF2T	0.000	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
GRIM2	0.000	0.000	0.000	0.015	0.079	0.000
	(0.000)	(0.023)	(0.000)	(0.038)	(0.099)	(0.022)
D-TFT	0.286**	0.181**	0.204**	0.235***	0.069	0.149
	(0.117)	(0.091)	(0.096)	(0.082)	(0.089)	(0.095)
GRIM3	0.000	0.024	0.000	0.000	0.000	0.000
Γ	0.394***	0.437***	0.384***	0.454***	0.403***	0.479***
	(0.047)	(0.106)	(0.074)	(0.067)	(0.076)	(0.074)

Notes: The analysis is based on the second half of all supergames in each session. Bootstrapped standard errors are in parentheses. * $p < 10\%$, ** $p < 5\%$, and *** $p < 1\%$.