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COORDINATING COLLECTIVE RESISTANCE THROUGH COMMUNICATION AND REPEATED INTERACTION

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Abstract: This paper presents a laboratory collective resistance (CR) game to study how different forms of repeated interactions, with and without communication, can help coordinate subordinates' collective resistance to a "divide-and-conquer" transgression against their personal interests. In the one-shot CR game, a first–mover (the "leader") decides whether to transgress against two responders. Successful transgression increases the payoff of the leader at the expense of the victim(s) of transgression. The two responders then simultaneously decide whether to challenge the leader. The subordinates face a coordination problem in that their challenge against the leader's transgression will only succeed if both of them incur the cost to do so. The outcome without transgression can occur in equilibrium with standard money-maximizing preferences with repeated interactions, but this outcome is not an equilibrium with standard preferences when adding non-binding subordinate "cheap talk" communication in the one-shot game. Nevertheless, we find that communication (in the one-shot game) is at least as effective as repetition (with no communication) in reducing the transgression rate. Moreover, communication is better than repetition in coordinating resistance, because it makes it easier for subordinates to identify others who have social preferences and are willing to incur the cost to punish a violation of social norms.

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Key words: Communication, Cheap Talk, Collective Resistance, Divide-and-Conquer, Laboratory Experiment, Repeated Games, Social Preferences

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Coordinating Collective Resistance through Communication and Repeated Interaction

1. Introduction

Organizational and political leaders often engage in strategic behavior to extract surplus from their subordinates, and such behavior can have significant welfare impacts on organizations (Miller, 1992; Gibbons, 2001) and societies (North and Weingast, 1989; Weingast, 1995, 1997). A strategy widely used by leaders is called "divide-and-conquer," in which a leader extracts surplus from a *victim* and shares it with a *beneficiary* to gain the latter's support. This paper introduces the laboratory repeated collective resistance (hereafter CR) game to investigate how repeated interactions and communication between "responders" can coordinate their resistance towards divide-and-conquer transgression that attacks their personal interests. In the one-shot laboratory CR game, a first–mover (the "leader") decides whether to transgress against two responders. Successful transgression increases the payoff of the leader at the expense of the victim(s) of transgression. The two responders then simultaneously decide whether to incur the costs to challenge the leader. In making this decision, the two responders face a coordination problem in that their challenge against the transgression will only succeed if both of them resist.

In this game, the leader can either transgress against both responders, or can practice divide-and-conquer transgression targeted at only *one* responder and share part of the surplus expropriated from the victim with the other responder. The use of divide-and-conquer strategy to ensure that the beneficiary of a transgression will not join forces with the victim to challenge the transgression is widely observed in many settings. For example, a political leader may expropriate wealth from one group of citizens, and share some of this expropriated wealth with another group of citizens to obtain their support (Weingast, 1995, 1997; Acemoglu et al., 2004). The management of a university may unilaterally alter fiscal arrangements between the central administration and various schools to confiscate surplus from these schools, and share some of the confiscated surplus with other schools to gain their support. A firm that is negotiating contracts with several unions may offer stringent terms to some unions and more favorable terms to others to create significant divergent interests among the unions.[3]

The importance of divide-and-conquer strategies has long been recognized. In chapter one of *The Art of War*, Sun Tze (476--221BC) observed that "(w)ar is primarily a game of deception … When the enemy are united, one should try to cause internal dissension." In chapter 3 of *The Prince*, Machiavelli (1513), when giving advice regarding how to rule a colony, advises that "(a)nyone who rules a foreign country should take the initiative in becoming a proctor of the neighboring minor powers and contrive to weaken those who are powerful within the country itself." Despite the widely recognized importance of divide-and conquer strategies, only recently has a small literature emerged that systematically studies their importance. Several recent contributions explicitly emphasize that the extent to which the responders can solve the coordination problem they face when attempting to organize collective resistance is crucial in determining whether divide-and-conquer transgression will occur in the first place (Weingast, 1995, 1997, 2005; Acemoglu et al., 2004).

In his pioneering work in this area, Weingast (1995, 1997) presents a game-theoretic model to demonstrate how a leader can use the divide-and-conquer (hereafter DAC) strategy to prevent coordinated resistance, by sharing some of the confiscated surplus with a subset of subordinates. Our laboratory CR game is based on this model of DAC transgression (which

[3] Kutalik and Biddle (2006) discuss how concessions imposed through bankruptcy court in recent disputes between managements and unions of several airlines have targeted specific unions. Some unions have joined forces to form the *Airline Workers United*—an across occupations and airlines organization—to counter this divide-and-conquer strategy. Richard Turk, Communication Officer for Airline Mechanics Fraternal Association (AMFA), argues that "(P)art of the reason for the effectiveness of the airlines' divide-and-conquer strategy is the serious lack of communication and planned strategies amongst many unions--even at the same company …The coming together of activists from throughout the industry can be a large step in the right direction."

Weingast refers to as the Sovereign-Constituency Transgression game). As we shall explain in detail later, in the one-shot version of the CR game, the beneficiary of a DAC transgression has no financial incentive to challenge as successful transgression increases her payoff. Since the transgression will only be thwarted if *both* the victim and the beneficiary challenge, the victim of a DAC transgression will also not challenge as she expects that the beneficiary will not do so. This implies that transgression by the leader will always take place in the one-shot CR game.

Weingast (1995, 1997) further points out that the folk theorem of repeated games (Fudenberg and Maskin, 1986; also see Abreu, 1988, and Wen, 2002) implies that if the CR game is repeated indefinitely, then by adopting trigger strategies the subordinates can support their preferred outcome of no transgression. The leader is deterred from practicing transgression by the threat of collective resistance. Of course, because there are multiple equilibria in the indefinitely repeated CR game, whether or not repetition will in fact reduce transgression is an empirical question. Furthermore, because the one-shot CR game has multiple equilibria the analysis by Benoit and Krishna (1987) suggests that even with finite repetition, the outcome without transgression can be supported as an equilibrium in the (early periods) of the finitely repeated CR game. This paper reports a laboratory test of whether indefinite and finite repetition in fact reduces DAC transgression.

Besides investigating whether repetition reduces transgression, we also study whether communication reduces transgression and compare its effect to that of repeated interactions. An implicit assumption in Weingast (1995, 1997, 2005) is that both the leaders and the responders are only concerned with their own material interests. This assumption ensures that the beneficiary of a DAC transgression never challenges in the one-shot CR game, which drives the result that all equilibria involve some type of transgression in the one-shot CR game. Furthermore, if all the players in the CR game are only concerned about their own material interests, then allowing for non-binding communication (“cheap talk”) (Crawford and Sobel,

1982) between the beneficiary and the victim will not change the prediction that transgression occurs in all one-shot CR games if the divide-and-conquer strategy is available.

Recent contributions on social preferences, however, suggest that while many individuals have standard self-regarding preferences, *some* individuals may be *altruistic punishers* or have other types of social preferences (see, for example, Camerer, 2003, Gintis et al., 2005; and the references cited there). Those individuals may be willing to incur the cost to punish a violation of social norms, even when they are not directly hurt by such violations, and even when there is no significant scope for repeated interactions. In the CR game, suppose the beneficiary of a DAC transgression regards transgression by the leader as socially unacceptable, and therefore is willing to incur personal costs to engage in altruistic punishment against the leader. If the victim knows this, then the victim will also incur the cost to resist transgression. These observations suggest that some successful collective resistance against DAC transgression can occur in equilibrium in the one-shot CR game when social preferences are present. Furthermore, with heterogeneous preferences, non-binding communication, by providing the opportunity for the responders to signal their "types" to others, can alter behavior and deter transgression.

This experiment employs a complete factorial design to investigate how communication and repeated interactions may affect DAC transgression. The two treatment variables are the matching protocol and the communication protocol. The matching protocol varies from random matching to various forms of repeated interactions, while the communication protocol allows for both the presence and the absence of communication—a simple binary, nonbinding message regarding "intended" resistance to a transgression. This design allows us to investigate how adding communication affects transgression when the matching protocol is held constant at the Random Matching treatment to control for repeated game effects. It also allows us to investigate how embedding communication in each form of repeated interactions affects behavior.

We find that both indefinite and finite repetition (without communication) reduce transgression. Contrary to a model based on standard preferences, we find that communication reduces transgression even with random matching; in fact, communication alone is at least as effective as repetition in reducing transgression. Consistent with the hypothesis that communication facilitates type identification in the presence of heterogeneous preferences, the "intended choice" communicated by the beneficiary is critical for increasing resistance to DAC transgression. We also identify an interesting mechanism that limits the effectiveness of repeated interactions in facilitating cooperation in this game. In particular, even if a beneficiary has social preferences and would like to punish a transgressing leader, she may not challenge the leader in the repeated game because she risks revealing her social preference type to the leader. This can be particularly costly to her if the other responder has standard preferences and will not challenge DAC transgression as a beneficiary, as the leader may then target his future transgressions against this responder who has social preferences. This problem does not arise when responders can communicate intentions privately, which may be one reason why even such nonbinding communications more effectively deter transgression.

2. Communication, Repeated Interaction, and the Laboratory Collective Resistance Game

Figure 1 illustrates the laboratory CR game implemented in this study, which is based on Weingast (1995, 1997) and captures the following ideas. First, successful transgression allows the leader to extract surplus from the responders and increases his *private* payoff, even though it reduces *total* surplus in society because some surplus is destroyed in the process. In the Figure 1 payoffs, for example, successful transgression against a responder reduces the responder's payoff by 6 and increases the leader's payoff by 3, since a transgression destroys half of the confiscated surplus. Second, challenging is costly to the responders regardless of whether or not it succeeds, and the responders face a coordination problem in deciding whether to challenge the leader. In particular, the transgression will fail (in which case the leader fails to extract any surplus from the responders and also incurs a loss compared to the no transgression benchmark) if and only if *both* responders incur the cost to challenge him. Third, multiple equilibria exist in the top subgame in which the leader transgresses against both responders. Both responders challenging the leader and both responders acquiescing are possible equilibrium responses by the responders, so this subgame is a “stag hunt” game. Fourth, besides transgressing against both responders, the leader can also use a divide-and-conquer strategy. For example, in the Figure 1 payoffs when the leader transgresses against only one responder he shares 1 of the 3 units of the expropriated surplus with the other responder as an attempt to gain her support.

Note that when the leader transgresses against only one responder, the beneficiary of transgression will always prefer to acquiesce, which implies that the victim of transgression will also acquiesce. Therefore, even when he expects that a transgression against *both* responders will be met with coordinated challenges, by using the DAC strategy the leader can eliminate the threat of coordinated challenge by the responders. These observations imply that the one-shot game has three (pure strategy) equilibria, and “no transgression” is not one of them.[4] In one equilibrium, the leader transgresses against both responders, with the expectation that such full scale transgression will not be met by coordinated resistance. In each of the other two equilibria, the leader transgresses against one of the responders and shares some of the expropriated surplus with the other responder, with the expectation that no responder will challenge him. Furthermore, if all beneficiaries are self-regarding and this is common knowledge to all players, then non-binding communication between the beneficiary and the victim will not change the prediction that “no transgression” cannot be supported as an equilibrium.

As Weingast (1995, 1997) points out, if the CR game is repeated indefinitely then sufficiently patient players can adopt trigger strategies to support the outcome of (No

[4] Allowing for the possibility of mixed-strategy equilibrium does not change the key implications of the CR game, so we shall focus on pure-strategy equilibria in the text.

Transgression, Acquiesce, Acquiesce) as an equilibrium. For example, the players could adopt the following trigger strategies. For the leader, if either responder A or B acquiesced any transgression in an earlier period, transgress against both A & B thereafter. Otherwise, do not transgress. For responder A, if responder B challenges every previous transgression, then challenge any transgression in current period and acquiesce otherwise. If responder B acquiesces any previous transgression, then acquiesce forever. Responder B adopts the mirror image of A's strategy. It can be shown that given the payoffs in Figure 1, these strategies support (No Transgression, Acquiesce, Acquiesce) as an equilibrium outcome if the discount factor δ is larger than $1/4$.[5]

Nevertheless, the outcome in which the leader always practices DAC transgression is still an equilibrium in the indefinitely repeated game. For example, the outcome that the leader always practices DAC transgression against responder A can be supported by the following strategy profile. Regardless of the past history, the leader always chooses DAC transgression against A, A and B both always acquiesce except when the leader transgresses against both. It is therefore useful to find out whether DAC transgression is observed in the indefinitely repeated game, or whether coordinated resistance eliminates transgression most of the time.

Besides indefinite repetition, we also implement treatments in which subjects play the CR game with a fixed, known horizon in this study. We are motivated to conduct these treatments for two reasons. First, because there are multiple equilibria in the CR game, even finite repetition of the stage game can expand the set of equilibria (Benoit and Krishna, 1987). For example, while No Transgression cannot be supported as an equilibrium outcome in the one-shot CR

[5] This result is proved in an appendix available upon request from the authors. Intuitively, the key incentive problem here is to ensure that a beneficiary does not want to deviate. This requires that for a beneficiary, the short run gain from deviating to acquiescing a DAC transgression is smaller than the long run loss from foregone cooperation, which requires that $2 \le (8-2)\frac{\delta}{1-\delta} \Rightarrow \delta \ge \frac{1}{4}$. In the appendix, we show that all other no-deviation conditions are also satisfied.

game, it can be supported as equilibrium outcome up to the second-to-the-last period of the finitely repeated CR game by the trigger strategy considered above in the absence of discounting.

Second, as suggested by Kreps et al. (1982), if players are uncertain about the preferences of other players, then this incomplete information can also expand the set of equilibria even with finite repetition. Moreover, as we demonstrate below, if some players have social preferences, then the no transgression outcome can emerge as part of an equilibrium even in the one-shot game when players have incomplete information about preferences. This implies that uncertainty regarding whether other players have social preferences is another reason why that transgression can be substantially reduced in the finitely repeated CR game.

Our discussion to this point has focused on the case of standard preferences. We now investigate how collective resistance against DAC transgression might occur in equilibrium in the one-shot CR game with incomplete information if some agents have social preferences. A variety of social preference models would likely provide similar conclusions, and our goal is not to distinguish between them. Our objective is to identify plausible equilibrium mechanisms through which communication and incomplete information about social preferences can affect behavior in the CR game. For this purpose we use an extension that builds on Cox et al. (2007). They assume that in a (two-player) sequential move game, when a second-mover with social preferences makes her decision after observing the action chosen by the first-mover, the second-mover's marginal rate of substitution between her income and the income of the first-mover depends on her emotional state toward the first-mover. This emotional state consists of the unconditional benevolence (or malevolence) and the conditional benevolence (or malevolence) toward this other person. The unconditional emotional state is not affected by the action taken by the first-mover. The conditional emotional state, however, depends on whether the second-mover thinks positively or negatively about the action taken by the first-mover. If the action by the first-mover is perceived negatively (positively) by the second-mover, this triggers conditional malevolence (benevolence) toward the first-mover.

Following this approach, we assume that all agents are of two types. With probability *p* an agent has standard preferences, and with probability *(1-p)* the agent has social preferences. An agent's type is his/her private information. Our main concern is to examine whether the presence of beneficiaries who have social preferences will change behavior, so for simplicity we assume that the only conditional emotional reaction that can be triggered in the CR game is the negative reaction toward a leader by a responder when the leader practices DAC transgression. Therefore, we assume that if responder *i* is a Social Preferences type (hereafter the *SP*-type) she regards a DAC transgression by the leader as illegitimate, and has a utility function

$$U_i\left(y_L, y_i, y_j\right) = \left\{\frac{1}{\alpha}\left[y_i^{\alpha} + \theta y_L^{\alpha} + \rho(y_L^{\alpha} + y_j^{\alpha})\right], \quad \begin{matrix} \theta \in (-1,0) \text{ if } a_L \in \left\{TAB, TA, TB\right\} \\ \theta = 0 \text{ if } a_L = NT \end{matrix} \right. \tag{1}$$

Here, y_i is agent *i*'s income, y_L is the leader's income, y_j is the income of the other responder, θ is the (conditional) emotional state variable, $0 < \rho < -\theta$ measures the strength of unconditional altruism toward other agents, and $\alpha \leq 1$ (and $\alpha \neq 0$) is an elasticity of substitution parameter. TAB denotes transgression against both responders, and TA and TB denote divide-and-conquer transgression against A and B, respectively.

A leader who is an *SP*-type has a utility function

$$U_L\left(y_L, y_i, y_j\right) = \left\{\frac{1}{\alpha}\left[y_L^{\alpha} + \rho(y_i^{\alpha} + y_j^{\alpha})\right]\right. \tag{2}$$

where y_i and y_j are the income of responder *i* and *j*, respectively. (1) and (2) reflect the assumption that the only conditional emotional reaction that we consider is the negative reaction that an SP-type responder has toward a leader who practices DAC transgression.

If an agent is a standard type (hereafter the *S*-type), then regardless of whether he/she is a leader or a responder, he/she has a (possibly constant relative risk averse) utility function[6]

$$U_i\left(y_i\right)=\frac{1}{\alpha}y_i^{\alpha}. \tag{3}$$

One can show that under reasonable assumptions of parameter values for p, θ, ρ and α, the following strategies constitute a Perfect Bayesian equilibrium: (i) Both types of leader practice DAC transgression. (ii) When DAC transgression occurs, an *S*-type victim challenges with probability $\beta=\frac{\left(1-p\right)(9^{\alpha}+\theta 8^{\alpha}+\rho 8^{\alpha}+\rho-7^{\alpha}-\rho 7^{\alpha})+p(9^{\alpha}-8^{\alpha})}{p(7^{\alpha}+\rho 7^{\alpha}-8^{\alpha}-\theta 8^{\alpha}-\rho 8^{\alpha}-\rho)}\in\left(0,1\right)$, while an *SP*-type victim always challenges. An *S*-type beneficiary always acquiesces, and an *SP*-type beneficiary challenges with probability $\gamma=\frac{2^{\alpha}-1}{\left(1-p\right)7^{\alpha}-\left(1-p\right)}\in\left(0,1\right)$. (iii) When TAB transgression occurs, all types of responders challenge (iv) When there is no transgression, all types of responders acquiesce.[7]

In this equilibrium joint resistance against DAC transgression occurs with a positive probability, and victims challenge more often than beneficiaries. An S-type beneficiary has no incentive to challenge since successful DAC transgression increases her income. An *SP*-type beneficiary regards a DAC transgression as illegitimate and is willing to incur the cost to punish the leader provided that she believes that the victim will challenge with a sufficiently high probability. A victim prefers that a DAC transgression be thwarted, but the *SP*-type victim has a

[6] Our earlier discussion about the one-shot and the repeated CR game with standard preferences assumes that all players are risk neutral. It is straightforward to verify that whenever agents seek to maximize a utility function that only depends on and is increasing in their own monetary payoff (as in (3)), the conclusion still holds that all equilibria feature transgression in the one-shot CR game. The conclusion that the “no transgression” outcome can be supported as an equilibrium in both the indefinitely repeated and finitely repeated CR games also holds, although the critical discount factor that supports this can change when the utility function of a standard type changes.

[7] This result is derived in an appendix available upon request. When the parameter values are such that this equilibrium exists, there are also other equilibria in the CR game with incomplete information about social preferences, including an equilibrium in which both types of beneficiaries, as well as both types of victims, always acquiesce when a DAC transgression takes place. We focus on the mixed strategy equilibrium shown in the text because among all equilibria, this best describes the observed behavior in the Random Matching/No Communication Treatment. For certain parameter values—for example, when the probability that an agent is an SP-type is low—the unique equilibrium in the CR game with incomplete information involves all subordinates acquiescing to a DAC transgression.

stronger incentive to challenge than the *S*-type because the former also regards a DAC transgression as illegitimate and wants to punish the leader. [8]

Now suppose that after they observe the choice made by the leader but before they make their actual choices, the responders have the opportunity to indicate simultaneously to each other their intended choices in non-binding communication. That is, each responder can send a message of either “I intend to challenge” or “I intend to acquiesce” to the other. One can show that under reasonable assumptions of parameter values, the following strategies constitute a Perfect Bayesian equilibrium: (i) Both types of leader choose No Transgression. (ii) If DAC transgression occurs, both types of victim will indicate an intention to challenge and will do so if the beneficiary also indicates an intention to challenge. The *S*-type beneficiary will indicate an intention to acquiesce and will do so, while the *SP*-type beneficiary will indicate an intention to challenge and will do so. (iii) If the leader transgresses against both responders, all types of responders will indicate an intention to challenge and will do so. (iv) When there is no transgression, all types of responders will indicate an intention to acquiesce and will do so.[9]

In this semi-separating equilibrium, cheap talk is partially informative and the beneficiary’s message helps coordinate resistance. Of course, just like in any cheap talk games, a babbling equilibrium in which all the messages are uninformative and ignored always exists. Our experimental design allows us to study the effects of communication and compare them to the effects of repeated interactions.

3. Experimental Design

[8] The existence of this equilibrium does not depend on the fact that the SP-type agents are also motivated by unconditional altruism. One can show that even if $\rho = 0$, under reasonable assumptions of parameter values for p, θ and α, the strategies described in the text constitute an equilibrium. We include unconditional altruism to make the model closer to Cox et al. (2007), and to investigate how allowing for unconditional altruism for the SP-type leader will affect the incidence of transgression. It turns out that in the equilibrium described in the text, unconditional altruism is not sufficient to prevent the SP-type leader from transgressing.

[9] In the appendix, we prove this result and describe more carefully the posterior beliefs of the responders, and how each responder’s choice of action depends on her own message as well as the message of the other responder.

This study consists of 52 independent sessions across eight different treatments, as summarized in Table 1, involving a large sample of 468 separate human subjects who participated at two universities. Subjects were recruited by e-mail, web page and classroom announcements from the general student population, and all were inexperienced in the sense that they participated in only one session of this study, although some had participated in other economics experiments that were completely unrelated to this research project. Sessions lasted for at least 48 decision periods. The two treatments featuring random matching (with and without communication) are based on data reported in Cason and Mui (2007), while the other six treatments involving repeated interactions are new treatments conducted for this study. To our knowledge, nearly all of the related laboratory research has focused on either varying the degree of repeated interaction or studying the effects of communication. Ours is one of the first to examine their interaction and complementarity using a full factorial design.[10]

The experiment instructions employed neutral terminology. For example, "Person 1" chose "earnings square" A, B, C or D—which was the transgression decision—and then "Persons 2 and 3" simultaneously selected either X or Y—which was the challenge decision. (Instructions for the Indefinite Repetition/Communication are in the appendix.) In the communication treatments the responders send a restrictive message to the other responder in their group: an "intended" choice (either X or Y), prior to committing to an actual challenge or

[10] The importance of infinitely repeated interaction in facilitating cooperation has been emphasized in the literature, and the laboratory offers a useful environment in which one can implement a probabilistic termination design to directly assess the effects of indefinite repetition. As Duffy and Ochs (2006) point out, however, surprisingly few laboratory studies have exploited this possibility to identify the empirical conditions under which indefinitely repeated play actually facilitates cooperation. Some exceptions include Palfrey and Rosenthal (1994) who consider an indefinitely repeated threshold public goods game, and Dal Bó (2005), Duffy and Ochs (2006) and Dal Bó and Frechette (2007) who consider the indefinitely repeated prisoner's dilemma. Interestingly, a similar "dichotomy" exists in the experimental literature on communication, which has focused on communication in one-shot games (see, for example, Cooper et al., 1989, 1992; Palfrey and Rosenthal, 1991; Crawford, 1998). Few studies have considered repeated play and communication simultaneously (see Hackett et al., 1994; Wilson and Sell, 1997, who study finitely repeated play with communication). In many field settings, however, both communication and repetition are likely to be present. A full factorial design allows us to study the joint impacts of communication and repetition, and to create "counter-factual" environments in which only either communication or repetition exists, so that we can better decompose the effect of each form of coordination as well as understand their interaction.

acquiesce decision.

Each session had nine participants, but two sessions were always conducted simultaneously so 18 subjects were present in the lab for each data collection period. In the random matching treatments, the instructions emphasized that subjects were randomly re-grouped each period. The regrouping occurred separately within the two groups of nine subjects in the lab, although this was not mentioned in the instructions.

The Random Matching/No Communication treatment serves as the baseline. We introduce repeated games both with pre-determined horizons and with probabilistic termination. All matching treatments include conditions where the communication opportunity is present or absent in the stage game. In all treatments, subject roles remained fixed: throughout each session leaders always remained leaders, and responders always remained responders.

In the Long Horizon (hereafter LH) Finite Repetition treatment, participants were randomly grouped to form a three-person group, with one person of each type in each group, and these groupings remained fixed for all 50 periods of the experiment. In the Indefinite Repetition treatment, groupings lasted for a random number of periods. At the end of each decision period in this treatment, the experimenter threw an eight-sided die, and for die rolls of 1, 2, 3, 4, 5, 6 or 7 the groupings remained unchanged for another period. Whenever the die roll was 8 on any throw, then the current grouping was immediately terminated. If the total number of periods conducted in the session at that point exceeded 49, or if less than 30 minutes remained in the two and a half hour time period reserved for the session, then the session was also terminated at that time. Otherwise, each participant was randomly re-grouped with two other participants to form a new three-person group, with one person of each type in each group, and the same procedure was used to determine whether groupings would continue at the end of each period.

In the Equivalent Horizon (hereafter EH) Finite Repetition treatment, subjects were randomly regrouped at the end of every 8th period. The experimenter also made a verbal announcement that regrouping was taking place at these periods. These sessions lasted for 48 total periods. In the Indefinite Repetition treatment, since groupings end with a probability of 1/8 in each period, this implies that the repeated game has an expected horizon of 8 periods. Dal Bó (2005) argues that if one wants to compare the difference between finite and indefinite repetition of a particular game in the laboratory where the latter is implemented using the method of probabilistic termination, one should consider a finitely repeated game where its horizon is the same as the *expected* horizon of the indefinitely repeated game. This motivates the specific choice of our Equivalent Horizon Finite Repetition treatment.

Subjects' earnings were designated in "experimental francs." They were paid for all periods, and their cumulative francs balance was converted to either Australian or U.S. dollars at exchange rates that resulted in earnings that considerably exceeded their opportunity costs. The per-person earnings typically ranged between US$25 and US$40 for the Purdue sessions and between A$30 and A$60 for the Monash sessions.[11] Exchange rates were chosen before beginning data collection based on the time required to complete pilot sessions. Sessions without communication ran more quickly—some as short at 75 minutes including instructions—while those with communication typically required 1.5 to 2.5 hours. We employed more generous conversion rates for the longer sessions to compensate subjects for the longer time in the lab.

4. Results

4.1 Transgression

Figure 2 presents the time series of the rate the leaders transgress, separately for all eight treatments. These transgressions are overwhelmingly the divide-and-conquer type, and

[11] The exchange rate between U.S. and Australian dollars was approximately 1 AUD = 0.75 USD when the experiment was conducted.

transgression against both responders is uncommon. Nearly half of the attempts to transgress against both responders occur in periods 1 through 10, but these transgressions are met with successful coordinated resistance 69 percent of the time. This discourages leaders from pursuing this most aggressive type of transgression, and after these initial 10 periods 95.5 percent of the transgressions are the DAC type.

The figure shows that transgression rates vary between about 50 and 85 percent in the early periods, and the dispersion across treatments increases over time. Transgression rates exhibit a downward trend in all four communication treatments (shown with dashed lines), but several of the no communication treatments have an upward trend. In the baseline treatment with no communication and random matching, the transgression rate rises above 95 percent in the last ten periods. Because our main interest is in stable behavior following the learning phase, in some of the data analysis we will exclude the initial 20 periods and focus on the later periods.

Result 1: *Repeated play, especially over a long horizon, reduces the rate that leaders transgress.*

Support: Table 2 presents the transgression rates for each individual session for the later periods. Considerable variation exists across sessions within all treatment conditions. Averaged across sessions, transgression occurs in these later periods 92.5 percent of the time in the Random Matching/No Communication baseline shown on the upper left. Adding repetition (but not communication) decreases transgression, with a highly significant decrease for LH Finite Repetition (Mann-Whitney U=7.5; for sample sizes n=8, m=6, one-tailed p-value<0.05) and a marginally significant decrease for EH Finite Repetition (U=13; one-tailed p-value<0.10), but no significant difference for Indefinite Repetition (U=25; *ns*).

Result 2: *Communication is at least as effective as repeated play in reducing the transgression rate.*

Support: Adding communication, even retaining the Random Matching environment, results in a highly significant decrease in transgression (U=10; n=m=8, one-tailed p-value<0.05).

The interaction of communication and repetition decreases transgression even further, and in these late periods the transgression rate falls below 45 percent in the LH Finite Repetition/Communication treatment. This is significantly less than the Random Matching/ Communication level (*U*=10; one-tailed *p*-value<0.05). The other two repetition and communication treatments result in marginally significant decreases in transgression relative to the Random Matching/Communication level (*U*=12 and *U*=12.5; both one-tailed *p*-value<0.10), but all communication/repetition treatments have significantly less transgression than the Random Matching/No Communication baseline. Importantly, the transgression rate in the Random Matching/Communication treatment is not significantly different from the best repeated play no communication treatment (LH Finite Repetition), indicating that communication alone is at least as effective as repetition in reducing transgression (*U*=20; *ns*).

Result 3: *Holding the matching protocol constant, adding communication always reduces the transgression rate.*

Support: The decrease in transgression from adding communication is highly significant for Random Matching (*U*=10; *n*=*m*=8, one-tailed *p*-value<0.05), Indefinite Repetition (*U*=3; *n*=*m*=6, one-tailed *p*-value<0.01) and LH Finite Repetition (Mann-Whitney approximation *z*=1.78; *n*=*m*=18, one-tailed *p*-value<0.05).[12] The decrease is marginally significant for EH Finite Repetition (*U*=8; *n*=*m*=6, one-tailed *p*-value<0.10).

4.2 Resistance in the Divide-and-Conquer Subgames

As noted above, well over 90 percent of the late-period transgressions are the DAC type. Recall that a beneficiary with standard, money-maximizing preferences has a dominant strategy to acquiesce when facing DAC transgression, which is why the outcome of no transgression cannot be supported as an equilibrium in the one-shot game with standard preferences. As

[12] Each grouping of three subjects is statistically independent in the LH Finite Repetition treatment because subjects are never regrouped. This leads to 18 independent observations per treatment for this matching protocol.

pointed out in section 2, beneficiary resistance could be interpreted as an indication of social preferences, particularly in the random matching condition, or as part of a repeated game strategy with standard preferences in the other treatments.

The overall resistance rate is considerably higher for victims than for beneficiaries. This is consistent with the mixed strategy equilibrium identified in the equilibrium with social preferences summarized in section 2. Across all 8 treatments, victims start in early periods resisting about 50 to 70 percent of the time on average, but this rate usually declines over time to the 20 to 40 percent range in late periods. By comparison, average beneficiary resistance rates usually start in a range between 20 and 40 percent in early periods, and decline to range between 10 and 20 percent in later periods.

The leader, of course, cares most about the rate that the responders both resist simultaneously, since joint resistance is needed to stop a transgression. Therefore, our main conclusions here focus on joint resistance.

Result 4: *Without communication, repeated play does not increase the rate of successful joint resistance to DAC transgression.*

Support: Table 3 displays the joint resistance rates for each session for the later periods. Although the overall treatment average joint resistance rate nearly doubles (to 14 percent) in the LH Finite Repetition relative to the Random Matching baseline, too much variation exists across sessions for this increase to be statistically significant (U=16; *ns*). Likewise, neither of the modest increases in joint resistance in the EH Finite Repetition (U=18; *ns*) or the Indefinite Repetition (U=18; *ns*) treatments are statistically significant. We also estimated random effects probit regressions that are more powerful because they include all observations of DAC transgression, and not just the session averages, but they provide identical conclusions.

Result 5: *Adding communication, especially in conjunction with repeated play, significantly increases the rate of successful joint resistance to DAC transgression.*

Support: The right side of Table 3 shows that joint resistance rates are roughly 2 to 4 times higher with communication compared to the Random Matching/No Communication baseline. The increase in the Random Matching/ Communication treatment is only marginally significant (U=19; n=m=8, one-tailed p-value<0.10), but the increases in the EH Finite Repetition/Communication (U=9; n=8, m=6, one-tailed p-value<0.05) and Indefinite Repetition/Communication (U=4.5; n=8, m=6, one-tailed p-value<0.05) are highly significant. The increase in resistance for the LH Finite Repetition/ Communication treatment is driven largely by one session, so this increase is not statistically significant (U=15.5; n=8, m=6, *ns*).[13] Again, random effects probit regressions provide identical conclusions, although the significance levels are even higher because these tests are more powerful.

We close this subsection by noting that the session heterogeneity in transgression and resistance (e.g., in Tables 2 and 3) is highly correlated. Leaders apparently reduce their practice of DAC transgression in sessions where they face more early coordinated resistance. The average transgression rate exceeds 85 percent after period 20 for the 16 sessions in which the successful DAC joint resistance rate in periods 1-20 is less than 10 percent. By comparison, the average transgression rate is only 42 percent after period 20 for the 7 sessions in which the successful DAC joint resistance rate in periods 1-20 is over 40 percent. Over all 52 sessions, the Spearman rank correlation coefficient between early (periods 1-20) successful DAC joint resistance and late (after period 20) transgression is -0.58 (p-value<0.0001).

4.3 Coordinating Resistance: A "Type Identification" Interpretation

The equilibrium presented at the end of section 2 illustrates how even restrictive cheap talk could change outcomes if some individuals have social preferences, because this communication provides opportunities for responders to signal their types. In the repeated games

[13] The successful joint resistance rates also increase significantly when communication is allowed, holding the matching condition constant, in all but the LH Finite repetition condition.

the responders have additional incentives to indicate their type. Repeated play can lead even money-maximizing beneficiaries to resist DAC transgression in equilibrium, but because multiple equilibria exist the responders face a coordination problem in identifying which type of equilibrium they will play. Moreover, responders who have social preferences or wish to be perceived has having social preferences in the repeated game may have an incentive to signal their type through early period resistance to DAC transgression, particularly when they do not have communication opportunities. In this subsection we investigate the content of their communication and the patterns of resistance over time in the repeated game to understand better how responders might coordinate resistance.

Result 6: *The "intended choice" communicated by the beneficiary is critical for increasing resistance to DAC transgression.*

Support: Table 4 displays the different combinations of intended resistance to DAC transgression communicated in the four treatments that featured communication, as well as the resulting frequencies of actual resistance. Victims of DAC transgressions indicate an intention to challenge about 70 to 75 percent of the time, and beneficiaries indicate an intention to challenge about 30 to 35 percent of the time. As noted in the previous subsection, however, in the later periods responders actually challenge at about half these indicated rates. Nevertheless, Table 4 indicates that communication helps coordinate successful resistance. Rows 1 and 4 show that successful joint resistance never occurs more than 4 percent of the time when the beneficiary does not signal intended resistance. By contrast, row 3 shows that successful joint resistance occurs 48 to 70 percent of the time when both responders indicate intended resistance.

Table 5 presents statistical support for the conclusion that both victims and beneficiaries choose to resist a DAC transgression when the beneficiary or (especially) when both responders indicate that they intend to resist. These fixed effects logit models indicate that the likelihood of actual resistance for both victims and beneficiaries is always significantly higher when only the beneficiary or when both responders indicate an intention to resist, compared to the omitted case of no intended resistance. As discussed in section 2, a semi-separating equilibrium exists in which the beneficiary's message is used by the victim to update her belief about the beneficiary's type, thereby coordinating resistance. In this equilibrium the victim's intention is not used by the beneficiary to update his beliefs. The regression results indicate that the impact of the victims' intention is always smaller and sometimes insignificant, consistent with this interpretation that it is often considered uninformative "babbling." In additional regressions (not reported here) we find that the influence of the beneficiary's intention is stronger in the later periods than in the earlier periods of the sessions.

Type signaling is also possible with repeated play when communication is not possible, but it may require several periods of interaction within the same group. For a beneficiary to indicate an intention to resist a DAC transgression, for example, it would be useful for that responder to have been a beneficiary of DAC transgression earlier within the same grouping. Earlier periods provide an opportunity for the beneficiary to send a (costly) signal to the victim that she intends to resist this type of transgression in later periods.

Result 7: *In the repeated games, earlier period resistance to DAC transgression by the beneficiary significantly increases victims' resistance to later DAC transgression; however, adding communication increases both responders' resistance more substantially.*

Support: Table 6 presents a series of fixed effects logit models of DAC resistance for the three repeated game treatments without communication. These models are similar to those in Table 5, except that the intended resistance communications are replaced by actual previous resistance to the same kind of DAC transgression in that same grouping. For example, if the leader transgresses against Responder A in period 4 of a particular grouping of the repeated game, and the last time that the leader transgressed against Responder A during that grouping was in period 2, then the actual previous resistance choices by the victim (A) and beneficiary

(Responder B) are used as explanatory variables for the current actual resistance decisions. The table shows that these previous choices strongly influence later resistance by the victim, in a pattern similar to that discussed for intentions in Table 5.

Table 7 reports that these same patterns are present, but are weaker, in the Communication treatments. The logit models in this table include both the previous play as well as the communication content, essentially combining the explanatory variables used in Tables 5 and 6. Previous resistance has a weak and sometimes inconsistent influence on current resistance, especially for the beneficiary. By contrast, the variables representing different intention messages shown in the middle of the table indicate that both responders strongly increase actual resistance when the beneficiary alone, or both responders, indicate an intention to resist. Likelihood ratio tests shown toward the bottom of this table indicate that the communications are always jointly highly significant determinants of both victim and beneficiary resistance. Past resistance for this same pair of responders, by contrast, has a much weaker and often insignificant influence on current resistance. Apparently communication is more effective than the history of past resistance to coordinate responders' current resistance.

Note that even beneficiaries who have strong social preferences and would like to punish transgressing leaders may not resist in these repeated games because resistance can be quite costly. They must pay the small (1 experimental franc) actual current period cost of resistance, but beneficiaries also risk revealing their social preference type to the leader. A leader who faces a pair of responders composed of one money-maximizer and one social preference type, for example, would like to prey on the social preference type because the money-maximizing beneficiary has a dominant strategy to acquiesce. Leaders have access to the same history of actions as the responders, and successful signaling to a fellow responder can also result in signaling to the leader. The leader does not observe the beneficiary's intentions in the communication treatments, which at least for the random matching condition avoids this risk.

Cason and Mui (2007) provide corroborating evidence by systematically manipulating whether the leader can observe different types of communication between responders. This influenced what intentions were expressed, and private communications more effectively deterred transgression.

5. Concluding Remarks

Despite the fact that the use of divide-and-conquer strategy by organizational and political leaders is pervasive, only recently have scholars begun to study systematically the implications of divide-and-conquer transgression (Weingast, 1995, 1997, 2005; Acemoglu et al., 2004). In particular, Weingast (1995, 1997) develops a collective resistance game which shows that with standard preferences, joint resistance against DAC transgression will never occur in the one shot CR game, and argues that indefinite repetition can reduce transgression. This paper extends his analysis to consider the implications of finite repetition of the CR game, as well as the implications of heterogeneous social preferences and non-binding communication. It also reports a laboratory experiment to evaluate the effects of communication and repeated interactions in this strategic environment.

We find that both indefinite and finite repetition reduces transgression. Contrary to the predictions of a model based on standard preferences, communication reduces transgression even with random matching. Furthermore, communication alone is at least as effective as repetition in reducing transgression. In the repeated games, earlier period resistance to DAC transgression by the beneficiary significantly increases victims' resistance to later DAC transgression; however, adding "intended choice" communication increases both responders' resistance more.

An important feature of the laboratory CR game is that it allows for interesting (and endogenous) role asymmetries among the players. While the importance of coordination and cooperation has been widely studied by economists, the CR game embeds an interesting coordination problem in a larger novel context. In this game, the responders' ability to solve the coordination problem they face determines the leader's transgression incentives. Moreover, when the leader engages in divide-and-conquer transgression, this endogenously leads to asymmetric beneficiary and victim roles for the responders. This paper shows that this asymmetry leads to an interesting mechanism that limits the effectiveness of repeated interaction in facilitating cooperation in this game. Namely, even a beneficiary who has social preferences and would prefer to punish transgressing leaders may not resist in a repeated CR game, because she may risk revealing her type to the leader and consequently be a victim in future DAC transgressions. This reflects how fundamental and endogenous asymmetries in the role of players in a game may influence repeated play, an insight that is missing and not obviously obtainable in existing laboratory studies of indefinitely repeated games, which tend to focus on the prisoner's dilemma (Dal Bó, 2005; Duffy and Ochs, 2006; Dal Bó and Frechette, 2007) and related public good games (Palfrey and Rosenthal, 1994).

Like most other contributions in organizational economics and political economy, the emerging literature on divide-and conquer transgression adopts the assumption that all agents have standard preferences and are only motivated to pursue their own material interests. However, in light of the increasing evidence of the importance of heterogeneous social preferences (Camerer, 2003, Gintis, et al., 2005; and the references cited there), a systematic investigation of the implications of social preferences for collective action should be a top priority (Ostrom, 2003). This study is an exploration in this direction, focusing on the importance of social preferences in affecting the incidence of collective resistance and divide-and-conquer transgression. We believe it is important to understand how some mechanisms that may be deemed ineffective in facilitating collective resistance when all agents have standard preferences might be effective in facilitating collective action in the presence of social preferences.

While a model based on standard preferences suggests that communication should not affect the incidence of leader transgression in the Random Matching/No Communication treatment, the model in section 2 illustrates how even very restrictive and nonbinding communication can reduce the incidence of leader transgression in the CR game when agents have incomplete information about social preferences. Our empirical findings provide initial evidence that social preferences can be important in affecting collective resistance and divide-and-conquer transgression. Since communication facilitates type identification in the presence of heterogeneous social preferences, it can help coordinate collective resistance and deter transgression. These results suggest the natural conjecture that societies and organizations that have invested resources in fostering social interactions that facilitate type identification among different groups of individuals who are potential targets of divide-and-conquer transgression—even when these interactions may not be political in nature per se—may be less likely to suffer from divide-and conquer transgression by their leaders. Future theoretical, field and laboratory studies that investigate the implications of this conjecture may provide a more complete understanding of the role of collective resistance in deterring transgression.

Table 1: Experimental Design (468 Total Subjects)

	No Communication	Communication
Random Matching	8 Sessions (72 Subjects) 6 at Monash Univ., 2 at Purdue Univ.	8 Sessions (72 Subjects) 6 at Monash Univ., 2 at Purdue Univ.
Long Horizon Finite Repetition (50 Periods)	6 Sessions (54 Subjects) 2 at Monash Univ., 4 at Purdue Univ.	6 Sessions (54 Subjects) 2 at Monash Univ., 4 at Purdue Univ.
Equivalent Horizon Finite Repetition (8 Periods)	6 Sessions (54 Subjects) 2 at Monash Univ., 4 at Purdue Univ.	6 Sessions (54 Subjects) 2 at Monash Univ., 4 at Purdue Univ.
Indefinite Repetition (7/8 probability of continuation)	6 Sessions (54 Subjects) 2 at Monash Univ., 4 at Purdue Univ.	6 Sessions (54 Subjects) 2 at Monash Univ., 4 at Purdue Univ.

Table 2: Rates for Independent Sessions that Leaders Transgressed (Sessions ordered lowest to highest)

	No Communication	Communication
	66.7%	37.8%
Random Matching	90.0%	56.7%
	91.1%	65.6%
	94.4%	77.8%
	97.8%	86.7%
	100.0%	87.8%
	100.0%	93.3%
	100.0%	96.7%
Treatment Average	**92.5%**	**75.3%**
	41.1%	10.0%
Long Horizon Finite Repetition	43.3%	22.2%
	66.7%	38.9%
	68.9%	47.8%
	85.6%	52.2%
	98.9%	94.4%
Treatment Average	**67.4%**	**44.3%**
	34.5%	44.0%
Equivalent Horizon Finite Repetition	77.4%	51.2%
	78.6%	58.3%
	92.9%	60.7%
	97.6%	61.9%
	97.6%	84.5%
Treatment Average	**79.8%**	**60.1%**
	40.4%	15.1%
Indefinite Repetition	93.3%	28.0%
	95.7%	38.9%
	98.2%	77.8%
	100.0%	78.4%
	100.0%	81.4%
Treatment Average	**87.9%**	**53.2%**

Note: The early periods 1-20 are excluded from these calculations.

Table 3: Rates for Independent Sessions that Responders Successfully Jointly Resist a Divide-and-Conquer Transgression (Sessions ordered highest to lowest)

	No Communication	Communication
	23.7%	43.1%
Random	15.0%	24.3%
Matching	8.9%	21.2%
	4.4%	12.7%
	3.4%	8.5%
	1.2%	4.8%
	0.0%	2.6%
	0.0%	1.2%
Treatment Average	**7.1%**	**14.8%**
	34.8%	88.9%
Long Horizon	25.8%	15.0%
Finite Repetition	11.7%	14.0%
	8.1%	10.8%
	3.6%	5.7%
	0.0%	0.0%
Treatment Average	**14.0%**	**22.4%**
	18.5%	26.5%
Equivalent Horizon	11.5%	21.6%
Finite Repetition	9.5%	17.3%
	6.2%	12.5%
	3.2%	9.9%
	1.3%	8.1%
Treatment Average	**8.4%**	**16.0%**
	28.9%	57.1%
Indefinite	12.6%	36.0%
Repetition	6.3%	25.0%
	5.3%	17.5%
	1.9%	17.1%
	1.7%	8.9%
Treatment Average	**9.5%**	**26.9%**

Note: The early periods 1-20 are excluded from these calculations.

Table 4: Challenge and Successful Resistance for Different Combinations of Communicated Messages

	Message Combination:	Random Matching: Victim Challenge	Random Matching: Beneficiary Challenge	Random Matching: Success Joint Resist	Indefinite Repetition: Victim Challenge	Indefinite Repetition: Beneficiary Challenge	Indefinite Repetition: Success Joint Resist	Equiv. Horiz. Finite: Victim Challenge	Equiv. Horiz. Finite: Beneficiary Challenge	Equiv. Horiz. Finite: Success Joint Resist	Long Horizon Finite: Victim Challenge	Long Horizon Finite: Beneficiary Challenge	Long Horizon Finite: Success Joint Resist
(1)	Only Victim	114/443	19/443	5/443	72/263	12/263	7/263	86/246	19/246	9/246	53/155	10/155	4/155
	Indicates Resistance	25.7%	4.3%	1.1%	27.4%	4.6%	2.7%	35.0%	7.7%	3.7%	34.2%	6.5%	2.6%
(2)	Only Beneficiary	31/78	18/78	9/78	17/33	7/33	5/33	21/38	16/38	11/38	15/24	10/24	8/24
	Indicates Resistance	39.7%	23.1%	11.5%	51.5%	21.2%	15.2%	55.3%	42.1%	28.9%	62.5%	41.7%	33.3%
(3)	Both Responders	190/228	117/228	110/228	132/140	93/140	91/140	110/115	81/115	80/115	87/88	57/88	56/88
	Indicate Resistance	83.3%	51.3%	48.2%	94.3%	66.4%	65.0%	95.6%	70.4%	69.6%	98.9%	64.8%	63.6%
(4)	Neither Responder	20/141	5/141	0/141	6/73	2/73	0/73	7/105	1/105	0/105	8/142	0/142	0/142
	Indicates Resistance	14.2%	3.5%	0.0%	8.2%	2.7%	0.0%	6.7%	1.0%	0.0%	5.6%	0.0%	0.0%

Table 5: Fixed Effects Logit Models of DAC Challenge Decision Based on Communicated Messages

Dependent Variable = 1 if Responder Challenges a DAC Transgression

Message Combinations:	Random Matching: Victim Challenge	Random Matching: Beneficiary Challenge	Indefinite Repetition: Victim Challenge	Indefinite Repetition: Beneficiary Challenge	Equiv. Horizon Finite: Victim Challenge	Equiv. Horizon Finite: Beneficiary Challenge	Long Horizon Finite: Victim Challenge	Long Horizon Finite: Beneficiary Challenge
Only Victim	0.12	0.07	1.92**	0.45	2.38**	1.31	1.90**	0.63
Indicates Resistance	(0.35)	(0.59)	(0.56)	(0.82)	(0.51)	(1.06)	(0.46)	(1.14)
Only Beneficiary	1.45**	1.74**	3.18**	2.71**	2.61**	4.80**	2.30**	4.21**
Indicates Resistance	(0.39)	(0.61)	(0.68)	(0.92)	(0.62)	(1.18)	(0.62)	(1.21)
Both Responders	4.01**	3.23**	5.84**	4.66**	6.40**	4.89**	6.79**	4.70**
Indicate Resistance	(0.42)	(0.59)	(0.68)	(0.85)	(0.74)	(1.10)	(1.13)	(1.11)
1/period	4.19**	2.21*	0.87	-0.06	0.58	0.85	3.35**	4.59**
	(0.83)	(0.98)	(0.84)	(0.94)	(0.90)	(0.89)	(1.18)	(1.62)
Log likelihood	-250.8	-127.7	-144.0	-86.9	-127.9	-69.8	-98.2	-47.9
Observations	855	464	500	458	479	331	376	282

Notes: All models are estimated with subject fixed effects. Some subjects were dropped due to zero variation in challenge decision. Standard errors are shown in parentheses. ** denotes significance at the one-percent level; * denotes significance at the five-percent level; † denotes significance at the ten-percent level (all two-tailed tests).

Table 6: Fixed Effects Logit Models of DAC Challenge Decision Based on Previous DAC Challenges within Current Grouping:

No Communication Treatments

Dependent Variable = 1 if Responder Challenges a DAC Transgression

	Indefinite Repetition		Equiv. Horizon Finite		Long Horizon Finite	
Resistance in Previous DAC Transgression of this Match:	Victim Challenge	Beneficiary Challenge	Victim Challenge	Beneficiary Challenge	Victim Challenge	Beneficiary Challenge
Only Victim Resisted	1.40**	0.49	1.21**	0.50	0.77**	0.47
	(0.24)	(0.48)	(0.27)	(0.52)	(0.28)	(0.49)
Only Beneficiary Resisted	1.57**	0.88	2.13**	1.06	1.48*	2.09**
	(0.54)	(0.58)	(0.56)	(0.67)	(0.59)	(0.67)
Both Responders Resisted	3.52**	0.76	2.96**	2.01**	2.22**	1.05†
	(0.80)	(0.57)	(0.68)	(0.64)	(0.68)	(0.56)
1/period	8.92**	-1.32	8.94**	4.42†	6.55**	-5.87†
	(2.90)	(4.57)	(2.63)	(2.62)	(2.36)	(3.45)
Log likelihood	-204.8	-88.6	-159.7	-63.1	-166.3	-86.8
Observations	569	271	410	235	468	281

Notes: All models are estimated with subject fixed effects. Some subjects were dropped due to zero variation in challenge decision. Standard errors are shown in parentheses. ** denotes significance at the one-percent level; * denotes significance at the five-percent level; † denotes significance at the ten-percent level (all two-tailed tests).

Table 7: Fixed Effects Logit Models of DAC Challenge Decision Based on Previous DAC Challenges within Current Grouping and Current Period Messages Communicated: Communication Treatments

Dependent Variable = 1 if Responder Challenges a DAC Transgression

	Indefinite Repetition				Equiv. Horizon Finite				Long Horizon Finite			
Resistance to Previous DAC Transgression:	Victim Challenge	Beneficiary Challenge	Victim Challenge	Beneficiary Challenge	Victim Challenge	Beneficiary Challenge	Victim Challenge	Beneficiary Challenge	Victim Challenge	Beneficiary Challenge	Victim Challenge	Beneficiary Challenge
Only Victim Resisted	0.06 (0.29)	0.16 (0.41)	-0.59 (0.40)	0.39 (0.55)	0.51 (0.34)	0.08 (0.48)	0.46 (0.40)	0.11 (0.69)	0.57† (0.33)	-0.19 (0.48)	0.68† (0.39)	-0.47 (0.71)
Only Beneficiary Resisted	0.74 (0.76)	-0.08 (0.81)	-0.27 (1.01)	-0.93 (1.15)	1.78* (0.76)	-1.23 (1.00)	1.13 (1.33)	-2.19 (1.35)	2.34* (1.13)	0.50 (0.85)	3.54† (2.05)	-0.54 (1.58)
Both Responders Resisted	1.90** (0.43)	1.30** (0.43)	1.58* (0.62)	0.84 (0.74)	2.29** (0.51)	-0.69 (0.70)	1.64* (0.74)	-2.84* (1.18)	1.23** (0.43)	0.74† (0.44)	-0.09 (0.66)	-1.29 (0.97)
Messages:												
Only Victim Indicates Resistance			2.05** (0.68)	-0.51 (1.03)			2.11** (0.58)	1.26 (1.11)			1.76** (0.48)	0.28 (1.18)
Only Beneficiary Indicates Resistance			3.68** (0.86)	3.83** (1.36)			2.27** (0.70)	5.04** (1.47)			2.17** (0.65)	4.59** (1.38)
Both Responders Indicate Resistance			5.94** (0.84)	5.04** (1.27)			5.84** (0.89)	5.40** (1.33)			7.01** (1.20)	5.65** (1.30)
1/period	5.16* (2.34)	1.28 (2.06)	8.95* (3.56)	-0.04 (3.23)	4.40† (2.51)	-3.53 (3.81)	4.51 (3.20)	-0.31 (5.33)	4.85† (2.65)	5.63* (2.78)	7.39* (3.53)	7.02 (5.71)
LR test: Previous DAC resist terms jointly insignificant			11.4*	2.8			6.4	9.4†			7.5	2.1
LR test: Current cheap talk terms jointly insignificant			130.8**	98.8**			79.0**	62.0**			101.0**	91.4**
Log likelihood	-154.1	-93.2	-88.7	-43.8	-115.0	-65.9	-75.5	-34.9	-134.7	-79.6	-84.2	-33.9
Observations	355	302	355	302	325	197	325	197	352	264	352	264

Notes: All models are estimated with subject fixed effects. Some subjects were dropped due to zero variation in challenge decision. Standard errors are shown in parentheses. ** denotes significance at the one-percent level; * denotes significance at the five-percent level; † denotes significance at the ten-percent level (all two-tailed tests). The likelihood ratio (LR) test statistics are distributed χ^2(3 d.f.).

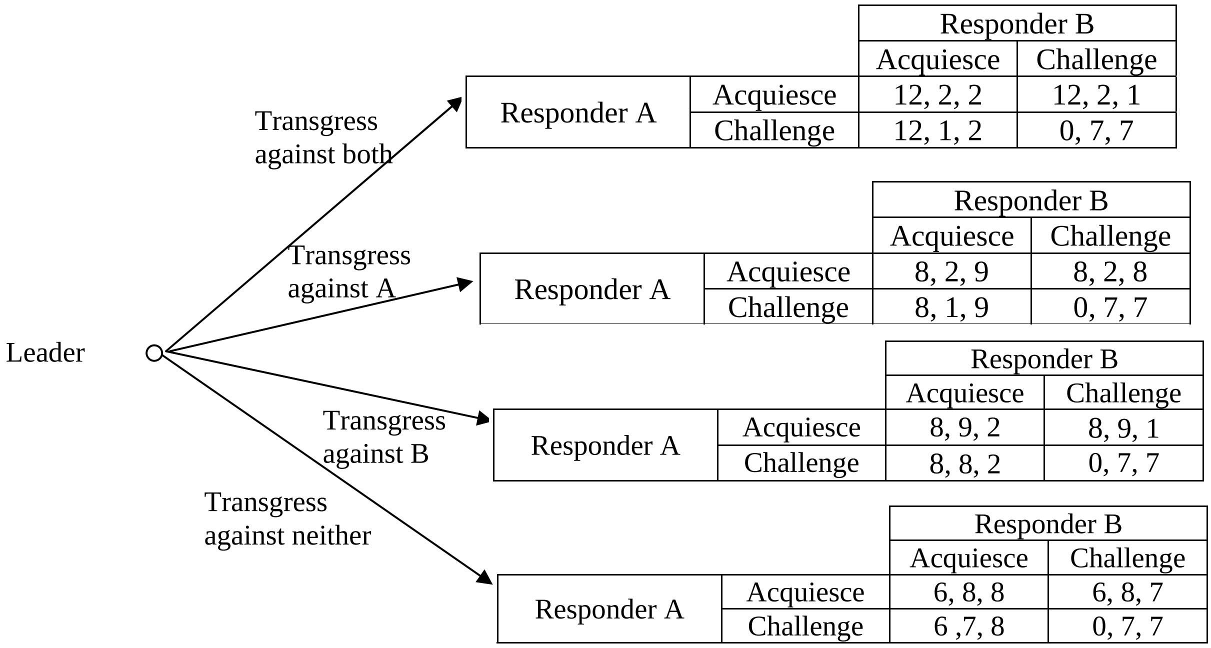

Figure 1: The Divide-and-Conquer Collective Resistance Game (payoffs are for (Leader, Responder A, Responder B))

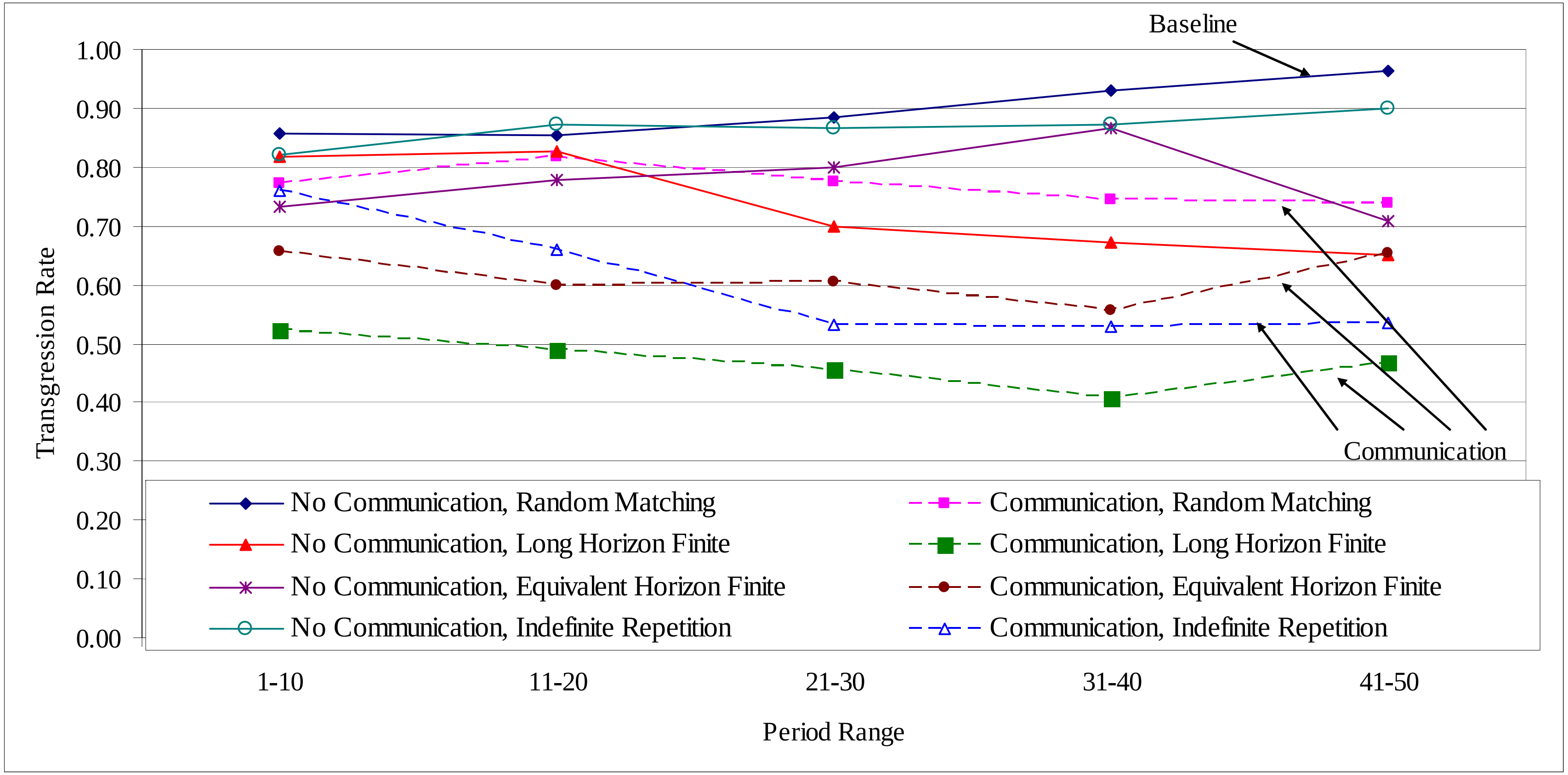

Figure 2: Transgression Rates for All Treatments

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Appendix: *Instructions for Indefinite Repetition/Communication* Treatment

This is an experiment in the economics of multi-person strategic decision making. The National Science Foundation has provided funds for this research. If you follow the instructions and make appropriate decisions, you can earn an appreciable amount of money. The currency used in the experiment is francs. Your francs will be converted to U.S. Dollars at a rate of 10 francs to one dollar. At the end of today's session, you will be paid in private and in cash.

It is important that you remain silent and do not look at other people's work. If you have any questions, or need assistance of any kind, please raise your hand and an experimenter will come to you. If you talk, laugh, exclaim out loud, etc., you will be asked to leave and you will not be paid. We expect and appreciate your cooperation.

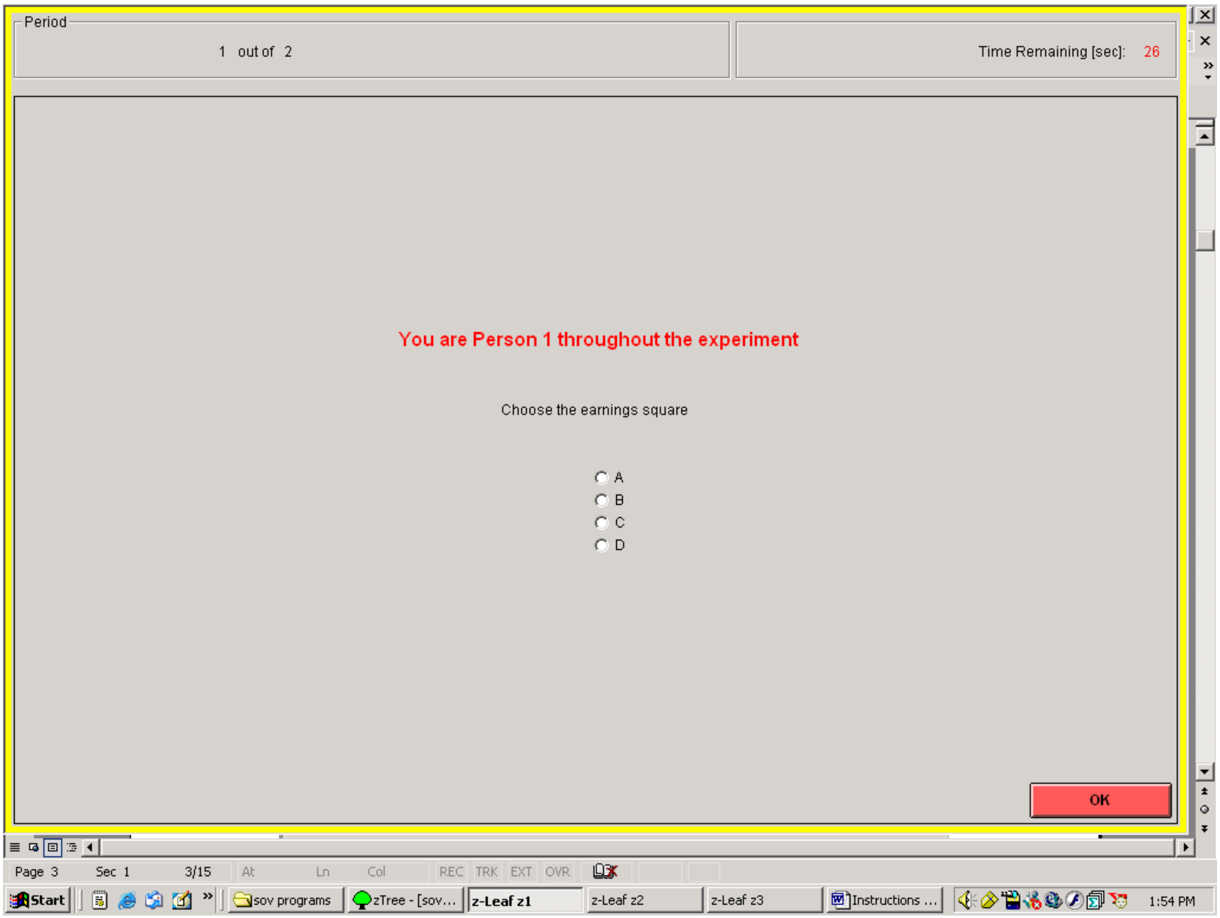
The experiment consists of many separate decision making periods. The 18 participants in today's experiment will be randomly split each period between three equal-sized groups, designated as **Person 1**, **Person 2** and **Person 3** groups. If you are designated as a Person 1, then you remain in this same role throughout the experiment. Participants who are not designated as a Person 1 switch randomly between the Person 2 and Person 3 roles at specific points in the experiment when individuals may be re-grouped, as explained later.

At the beginning of the experiment you will be randomly re-grouped with two other participants to form a three-person group, with one person of each type in each group. You will be grouped with these same two participants for a random number of periods, as explained later.

<u>Your Choice</u>

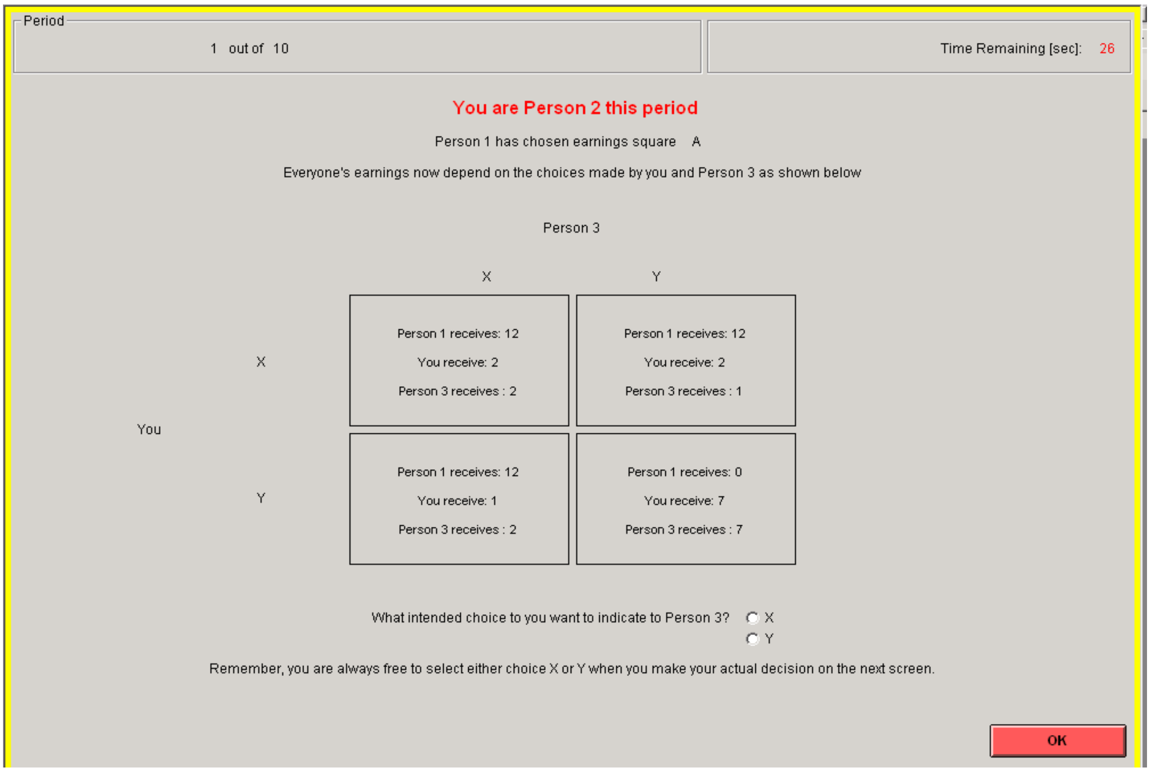
During each period, you and all other participants will make one choice. Earnings tables are provided on separate papers, which tell you the earnings you receive given the choices that you and others in your group make. If you are **Person 1** then you choose the earnings square, either **A**, **B**, **C** or **D**. You make this choice before the other two people in your group make their choice, on a decision screen as shown in Figure 1 on the next page.

After learning which earnings square the Person 1 chose, then **Persons 2 and 3** make their choices. However, after learning Person 1's earnings square choice but before making their actual choice, Persons 2 and 3 have an opportunity to privately communicate to each other an "intended" choice. As noted on the example Intention Screen for Person 2 (see page 3), Persons 2 and 3 are not required to make the same actual choice as the intended choice they share with the other person, and they are always free to select either choice X or Y when they make their actual decision. Persons 2 and 3 indicate their intended choices simultaneously; for example, if you are Person 3 then you do not learn the intended choice of Person 2 until after you indicate your intended choice.

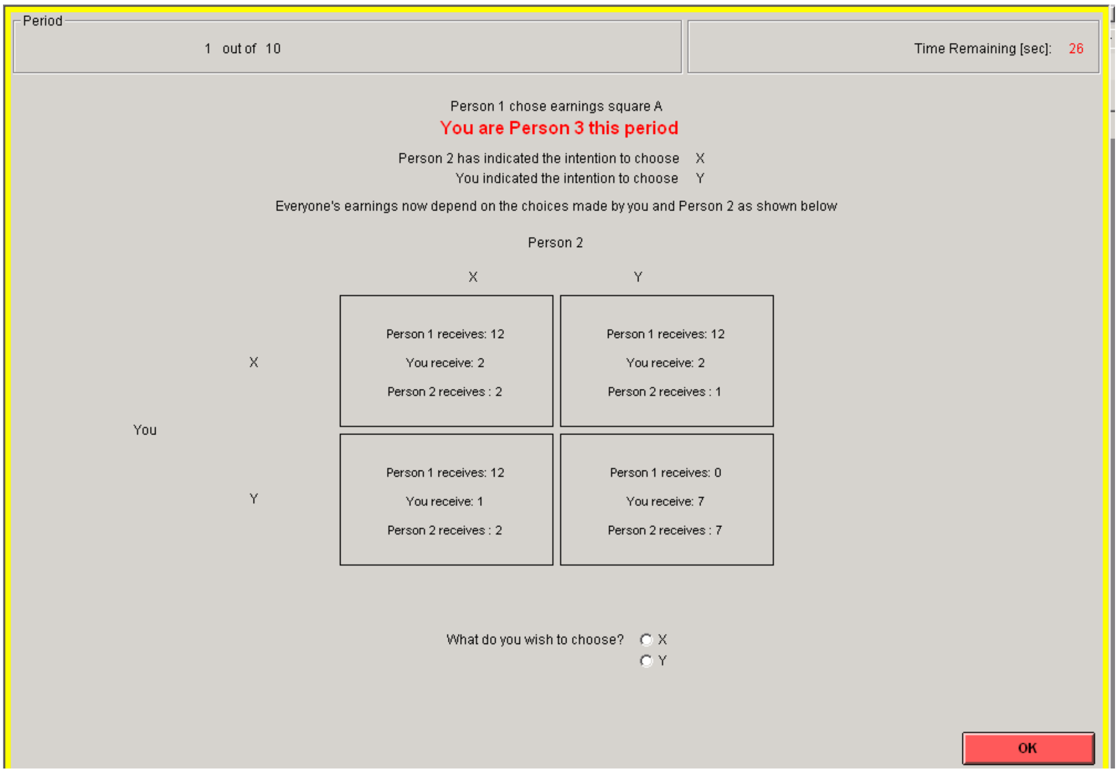


Decision Screen for Person 1

The computer program displays Person 2’s intended choice to Person 3, and it displays Person 3’s intended choice to Person 2. Only these two people observe these intended choices, and they are displayed on the top of the Decision Screen as shown on page 4. These intended choices should be recorded on the Personal Record Sheet. Persons 2 and 3 then make their actual choice simultaneously; for example, if you are Person 2 then you do not learn the actual choice of Person 3 until after you make your choice. Both Persons 2 and 3 may choose either **X** or **Y.**



Intention Screen for Person 2 (Person 3's is very similar)

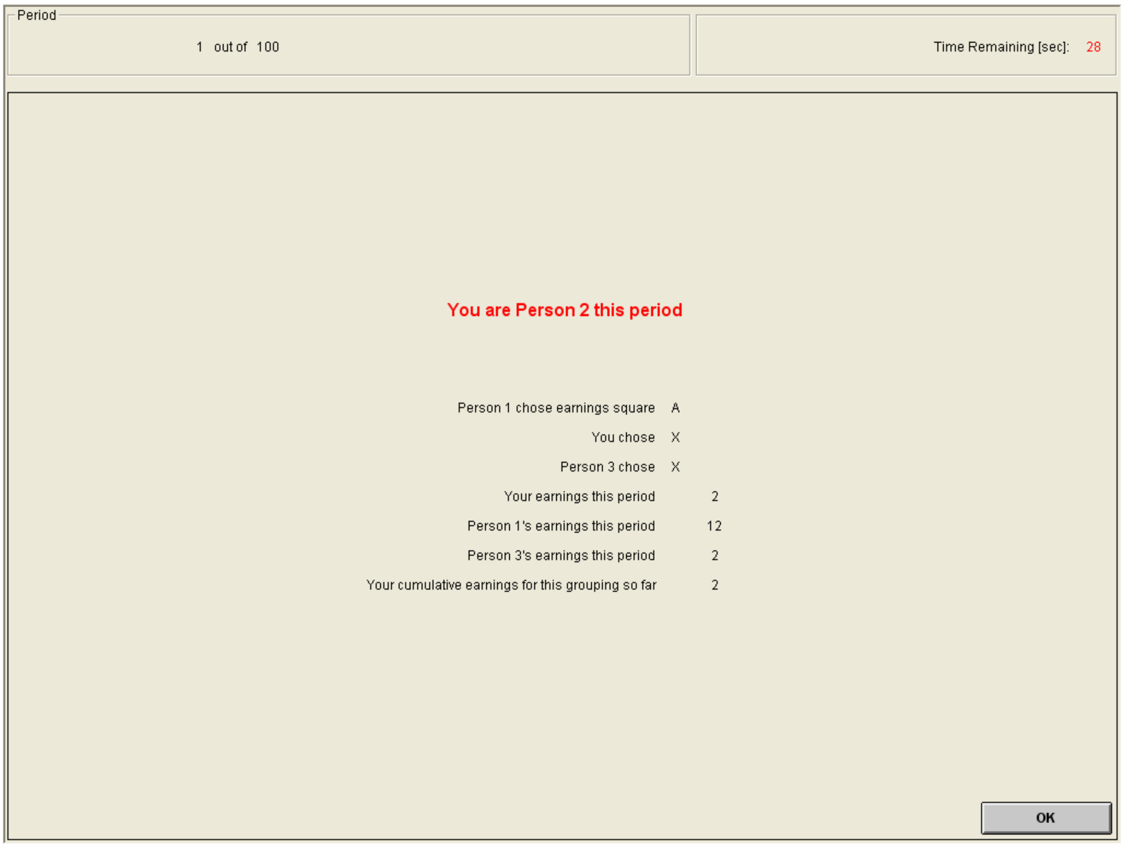


Decision Screen for Person 3 (Person 2's is very similar)

Your earnings from the choices each period are found in the box determined by you and the other two people in your group. If both Persons 2 and 3 choose **X**, then earnings are paid as shown in the box in the upper left on the screen. If both Persons 2 and 3 choose **Y**, then earnings are paid as shown in the box in the lower right on the screen. The other two boxes indicate earnings when one chooses **X** and the other chooses **Y**. To illustrate with a random example: if Person 1 chooses earnings square **A**, Person 2 chooses **X** and Person 3 chooses **Y**, then Person 1 earns 12, Person 2 earns 2, and Person 3 earns 1. You can find these amounts by looking at the appropriate square and box in your page of earnings tables.

The End of the Period

After everyone has made choices for the current period you will be automatically switched to the outcome screen, as shown below. This screen displays your choice as well as the choices of the people in your group. It also shows your earnings for this period and your earnings for the experiment so far.



Example Outcome Screen (Shown for Person 2)

Once the outcome screen is displayed you should record your choice and the choice of the others in your group on your Personal Record Sheet. Also record your current and cumulative earnings for this grouping. Click on the *OK* button on the lower right of your screen when the experimenter instructs you.

<u>The Random Ending to Each Grouping</u>

You will remain grouped with the same two other people in your group for some random number of periods. At the end of each decision period, we will throw an eight-sided die on the floor in front of some of the participants. The outcome of the roll will be announced verbally to everyone. If the die comes up 1, 2, 3, 4, 5, 6 or 7, then you will remain grouped with these same two people for another period; at the end of the next period, the die will be thrown again, and again the grouping will continue for at least another round if a 1, 2, 3, 4, 5, 6 or 7 is thrown.

If the die comes up an 8 on any throw, then the current grouping is immediately terminated. If the total number of periods conducted in the experiment at that point exceeds 49, or if less than 30 minutes remain in the two-hour block of time reserved for this lab session, then the experiment will also be terminated at that time. Otherwise, you will be randomly re-grouped with two other participants to form a new three-person group, with one person of each type in each group. You will remain grouped with these same two people for some random number of periods, with the same die-throwing rule to determine the termination of each random re-grouping of participants. At these re-grouping points, participants who are not designated as a Person 1 switch randomly between the Person 2 and Person 3 roles. The participants who may switch roles is determined randomly, and some may switch while others may not switch.

We will now pass out a questionnaire to make sure that all participants understand how to read the earnings tables and understand other important features of these instructions. Please fill it out now. Raise your hand when you are finished and we will collect it. If there are any mistakes on any questionnaire, I will summarize the relevant part of the instructions again. Do not put your name on the questionnaire.