Communication and Coordination in the Collective Resistance Game*

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Abstract

This paper presents a laboratory collective resistance (CR) game to study how different forms of non-binding communication among subordinates can help coordinate their collective resistance against a leader who transgresses against their rights. Contrary to the predictions of analysis based on purely self-regarding preferences, we find that non-binding communication about intended resistance increases the incidence of no transgression even in the one-shot laboratory CR game. In particular, we find that the incidence of no transgression increases from 7 percent with no communication to 16-37 percent depending on whether communication occurs before or after the leader’s transgression decision. Subordinates’ messages are different when the leaders can observe them, and the leaders also appear to use the observed messages to target specific subordinates for transgression.

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\textit{Key words}: Communication, Coordination, Collective Resistance, Laboratory Experiment, Social Preferences

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1. Communication, Collective Resistance, and Leader Transgression

This paper introduces the laboratory collective resistance (hereafter CR) game to investigate how communication between “subordinates” can facilitate their collective resistance against a leader who transgresses against their rights. Our main goal is to introduce and evaluate the hypothesis that when social preferences are important in motivating the behavior of some of the beneficiaries of transgression in the laboratory CR game, communication between subordinates can facilitate collective resistance and deter leader transgression. This occurs even when an analysis based on pure-self interest predicts that communication should not affect the incidence of transgression in this game.

A recent theme in both political economy (e.g., Weingast, 1995, 1997; Acemoglu et al., 2003) and organizational economics (e.g., Williamson, 1985; Miller, 1992) is that opportunistic transgression by the leader—for example, confiscation of subordinates’ assets, unilateral alteration of fiscal arrangements between the headquarter and divisions—can have significant negative effects on societies and organizations. Furthermore, several recent contributions on the political economy of leader transgression emphasize that the coordination problem faced by the subordinates is crucial in determining whether transgression will take place. In his influential work, Weingast (1995, 1997, 2005) demonstrates how the state can use a “divide-and-conquer” strategy to prevent coordinated subordinate resistance, by sharing some of the confiscated surplus with a subset of subordinates. Acemoglu et al. (2005) also emphasize the importance of the divide-and-conquer strategy, and they derive further predictions regarding the conditions under which the ruler can successfully use this strategy to extract surplus from subjects.

Most contributions to this literature on leader transgression assume, however, that all people are motivated only to maximize their own material payoffs. This literature, therefore,
pays little attention to the possibility that concerns beyond narrow self-interest by some subordinates may facilitate collective resistance against the abuse of power by leaders.

Specifically, consider Weingast’s analysis of divide-and-conquer (hereafter DAC) transgression. We first start with the Basic CR game illustrated in Figure 1 (Weingast, 1997). This game captures the following ideas. First, successful transgression allows the leader to extract surplus from the subordinates and increases the leader’s private payoff, even though it reduces total surplus in society because some of the surplus is destroyed in the process. In the Figure 1 payoffs, for example, successful transgression against a subordinate reduces the subordinate’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 subordinates regardless of whether or not it succeeds, and the subordinates face a coordination problem in deciding whether to challenge the leader. In particular, the leader will be deposed of power if and only if both subordinates incur the cost to challenge him. Third, multiple equilibria exist in the top subgame in which the leader transgresses against both subordinates. Both subordinates challenging the leader and both subordinates acquiescing are possible equilibria, so this subgame is an assurance (also known as a “stag hunt”) game under standard money-maximizing preferences.

It is straightforward to show that the overall CR game has two (pure-strategy) subgame perfect equilibria: \((T; (A, A); (A, A))\) and \((NT; (C, A); (C, A))\). In the “no transgression” \((NT)\) subgame, acquiesce is a dominant strategy for both subordinates. Therefore, if the leader expects that the subordinates will be playing the Nash equilibrium \((A, A)\) in the “transgression” \((T)\) subgame, he will transgress. On the other hand, if he expects that the subordinates can succeed in coordinating on the “challenge” Nash equilibrium in the transgression subgame, he will not
transgress. Importantly, in this case, the surplus-maximizing outcome without transgression can be supported as one of the possible subgame perfect equilibria.¹

As Weingast points out, besides transgressing against both subordinates, the leader can also use a divide-and-conquer strategy. Figure 2 illustrates the divide-and-conquer CR game. In this example, when the leader transgresses against only one subordinate he shares 1 of the 3 units of the confiscated surplus with the other subordinate as an attempt to gain her support. Importantly, when the leader can use a divide-and-conquer strategy, the outcome of “no transgression against any subordinate” cannot be supported as part of an equilibrium in this game. To see why, first note that if the leader expects that a transgression against both subordinates will be met with coordinated challenge, then he will refrain from such transgression. However, when the leader transgresses against only one subordinate, supporting the transgression increases his material payoff, so the beneficiary of the transgression will not challenge the transgression. Knowing this, the victim will not incur the cost to challenge the leader. Hence, successful resistance against divide-and-conquer transgression will never occur.

These observations imply that this game has three (pure strategy) equilibria, and no transgression is not one of them. In one equilibrium, the leader transgresses against both subordinates, expecting that this transgression will not be met by coordinated resistance by the subordinates. In each of the other two equilibria, the leader transgresses against one of the subordinates and shares some of the expropriated surplus with the other subordinate, expecting that no subordinate will challenge him.² Furthermore, extending the analysis of the one-shot CR

¹ Besides the two pure-strategy subgame perfect equilibria, there is also another subgame perfect equilibrium in which the two subordinates acquiesce with probability 5/6 in the transgression subgame and always acquiesce in the no transgression subgame, and the leader transgresses against the subordinates. Because allowing for the possibility of mixed-strategy equilibrium does not change the key implications of the CR game, we shall focus on pure-strategy equilibria in the text.
² The conclusion that subordinates will not challenge a divide-and-conquer transgression, so that no transgression cannot be supported as an equilibrium, still holds if agents seek to maximize a more general utility function as long
game to allow for communication generates another prediction. Because the beneficiary has no incentive to challenge the transgression, non-binding communication between the beneficiary and the victim will not change the conclusion that collective resistance against DAC transgression will not occur in equilibrium. Hence, such communication will not change the prediction that no transgression cannot be supported as an equilibrium.

This conclusion, however, can change significantly if a possibility exists that the beneficiary may be motivated by concerns other than narrow self-interest. Evidence provided by a fast-growing literature on “social preferences” (see, for example, Charness and Rabin, 2002; Camerer, 2003, chapter 2; Gintis, et al., 2005 and the references cited there) suggests that while some segment of the population exhibit purely self-regarding preferences, a significant portion of the population are motivated by strong reciprocity. They are willing to incur personal cost to help those who help them, as well as to incur personal cost to punish those who violate cooperative and other social norms (Fong et al., 2005). Strong reciprocity emphasizes how individuals are willing to engage in conditional cooperation and altruistic punishment even when such actions cannot be expected to entail net personal gains in the future. That is, strong reciprocity can affect behavior even when there is no significant scope for repeated interaction. For example, the literature on social preferences has documented how conditional cooperation and altruistic punishment are important in laboratory one-shot games (Gintis, et al., 2005). Strong reciprocity thus differs from “weak reciprocity” emphasized by models of reputational as their utility increases in their own monetary payoff. Moreover, introducing simple inequity aversion, such as that captured in Fehr and Schmidt’s (1999) well-known model, does not affect the beneficiaries’ dominant strategy to acquiesce in this divide-and-conquer subgame. A beneficiary challenging a transgression reduces the disutility from earning more than the transgression victim. But he also reduces his material payoff and increases his disutility from earning more than the leader when the resistance succeeds. Therefore, acquiescing remains a dominant strategy for the beneficiary. Furthermore, one can show that this implies that a leader with inequity aversion still prefers divide-and-conquer transgression to no transgression. Therefore, incorporating inequity aversion does not change the conclusion that “no transgression against any subordinate” cannot be supported as part of an equilibrium. Inequity aversion could change the set of equilibria for other payoff parameters, however, such as the lower leader payoffs used in Weingast’s (1997) original version of the game.
considerations in repeated game models in economics (Fudenberg and Maskin, 1986), and also differs from models of “reciprocal altruism” of repeated interactions in biology (Trivers, 1971).

In earlier studies, such as in the widely-studied ultimatum game, researchers have mainly focused on cases in which the person who incurs the cost to punish a violation of social norms were hurt by such violations. In an interesting recent study, Fehr and Fischbacher (2004) find evidence that third parties who are not directly hurt by a violation of social norms will nevertheless incur the cost to punish the violator.\(^3\) In the CR game, suppose the beneficiary is an altruistic punisher and regards transgression by the leader as socially unacceptable, and therefore is willing to incur personal costs to engage in altruistic punishment against the leader.\(^4\) If the victim knows that the beneficiary is willing to do so, then the victim will also incur the cost to resist transgression. These observations suggest that some successful collective resistance against divide-and-conquer transgression can actually occur in equilibrium. Furthermore, non-binding communication, by providing the opportunity for the subordinates to signal their “types” to others, can alter behavior and deter transgression.

Motivated by these observations, this study investigates the effects of various restrictive communication on the one-shot CR games.\(^5\) Subordinate subjects could send binary messages

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3 For example, they consider a dictator game in which A allocates a fixed amount between herself and B. A third party, whose material payoff is not affected by the proposed allocation by A, then can choose whether to incur the cost to punish A for making an unfair allocation. They find that most third parties punished dictators who took more than half of the surplus.

4 Different considerations may be responsible for why some beneficiaries may consider the divide-and-conquer transgression to be undesirable even though it increases their material payoffs. A beneficiary may identify with the welfare of the victim, or he may have concerns about social welfare (Charness and Rabin, 2002) and disapproves the transgression because it reduces total surplus. Our experiment is not designed to differentiate between these different social motivations in affecting behavior in the CR game. As a first attempt to study the role of social preferences in collective resistance and transgression, we aim at finding out empirically whether and how communication can affect behavior in the CR game when the pure self-interest model predicts that it should not. If communication does affect behavior, then this result and our findings will provide justification for further work aiming at understanding the relative importance of different motivations in affecting behavior in the CR game.

5 We focus on one-shot game to abstract from the possibility that the prospect of repeated interactions may motivate the beneficiary to resist DAC transgression. Of course, reputational considerations associated with repeated interactions can be important in deterring leader transgression. This idea has been emphasized by many scholars
that indicate their intended actions among available options in the CR game.

In our experiment, we first investigate whether *Ex Post Communication* may facilitate collective resistance. In this treatment, after they observe the choice made by the leader but before they make their actual choices, the subordinates indicate to each other their intended choices in non-binding communication. The leader does not observe these messages.

We also investigate the effect of *Public Ex Ante Communication* on resistance and transgression. In this treatment, the subordinates indicated to each other an intended play for every subgame, and this was communicated to the leader prior to the leader’s decision. While citizens only communicate their intended choices in the subgame actually chosen by the leader in the Ex Post Communication, Public Ex Ante Communication allows the subordinates to observe the intentions communicated in all subgames before the leader’s choice, so that intentions communicated for subgames that are not chosen by the leader can affect subordinates’ behavior. Furthermore, Public Ex Ante Communication also allows for the possibility that observing the communicated intentions may affect the leader’s behavior. We also consider the case of *Private Ex Ante Communication*, which differs from Public Ex Ante Communication in only one way: the intentions expressed by the subordinates are not observed by the leader. Although Private Ex Ante Communication also allows the subordinates to observe the intentions communicated in all subgames, it does not allow the leader to base his decision on the subordinates’ intentions.

While an analysis that is based on purely self-regarding preferences predicts that non-binding communication should have no effect on the incidence of no transgression, we find that even in this one-shot laboratory CR game, the incidence of no transgression increases from 7

(see, for example, Gibbons, 2001; and Weingast, 1997). Van Huyck et al. (2001) report a laboratory study of confiscation with repeated interactions between a dictator and a single citizen. (This is compared to the case where the dictator can commit to a particular allocation in Van Huyck et al., 1995.) In Cason and Mui (in preparation) we investigate how different kinds of repeated interactions affect the incidence of transgression and resistance.
percent with no communication to 16-37 percent in these three communication treatments. The incidence of no transgression is higher on average in Ex Post Communication than Public Ex Ante Communication, but due to substantial variation across sessions this difference is not statistically significant. One plausible interpretation of this session variation is that it reflects random allocation of subject preference types (e.g., some strong reciprocators and some purely self-regarding) to sessions. Finally, a comparison of the Public and Private Ex Ante Communication indicates that subordinates’ messages are different when the leaders can observe them, and that the leaders also appear to use the observed messages to target specific subordinates for transgression.

2. **Experimental Design**

This study consists of 36 independent sessions across five different treatments, as summarized in Table 1, involving the participation of 324 separate human subjects who participated in the sessions conducted at two universities. All subjects 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.

The No Communication treatments serve as baselines to evaluate the impact of the alternative cheap talk messages. The results were relatively uninteresting in the Basic CR game, since transgressions in this game were almost uniformly met with resistance and so transgressions did not occur after the first few periods. Therefore we chose to concentrate the data collection on the divide-and-conquer CR game.

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. (Ex Post Communication instructions are in the appendix.) In the cheap talk treatments the subordinates send a restrictive message to the other subordinate in their group: an “intended” choice (either X or Y), prior to committing to an actual challenge or acquiesce decision.

As explained in the previous section, the communication treatments differ in both the timing and in who observes the messages that are exchanged. In the Ex Post and the Private Ex Ante cheap talk treatments, only the two subordinates who are exchanging the messages observe the message content. In the Public Ex Ante treatment, the leader also observes the messages. In the Ex Post treatment, the subordinates first learn the transgression choice of the leader and then exchange messages for only the subgame determined by the leader. In the Ex Ante treatments, the subordinates indicate an intended choice for all four possible transgression subgames. These binary messages were exchanged simultaneously; that is, a subordinate did not learn the other subordinate’s message(s) until completely specifying all of her message(s).

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. 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. Subject roles remained fixed: leaders always remained leaders, and subordinates always remained subordinates. All sessions were planned for 50 periods, but some of the Ex Ante cheap talk sessions did not complete all 50 periods because they ran slowly and the time period allocated for the session expired.

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. Sessions without cheap talk ran more quickly—some as short at 75 minutes including instructions—while those with cheap talk 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.

3. **Results**

3.1 **Basic Collective Resistance Game**

In the Basic CR game (cf Figure 1) the leader can only transgress against both subordinates or transgress against neither. Both of these choices are parts of equilibrium strategy profiles. While leaders attempt to transgress about 30 percent of the time during the first 10 periods, they quickly learn that this is an unprofitable strategy. The subordinates play an assurance coordination (stag hunt) game in the transgression against both subgame, but they clearly coordinate to mutually challenge. Subordinates challenge 126 out of 128 times they face transgression, successfully resisting 62 of the 64 transgressions. Only 14 out of the 360 leader choices in the final 30 periods were for transgression.  

3.2 **Divide- and-Conquer Collective Resistance Game without Communication**

Adding the divide-and-conquer (DAC) option to the CR game (cf Figure 2) changes the set of equilibria, and under purely self-regarding preferences the outcome of no transgression observed in the Basic CR game is not among them. Furthermore, even if some beneficiaries are

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

7 This result is also not surprising, since challenge is a best response for this subgame whenever a subordinate believes that the other subordinate will challenge with a probability that exceeds 1/6. See Battalio et al. (2001) for a thorough experimental analysis of stag hunt games with differing optimization incentives.
altruistic punishers, collective resistance may be unlikely to occur without mechanisms that allow subordinates to signal their types to one another. This leads to the following hypothesis:

**H1:** The no transgression outcome occurs less frequently in the DAC than the Basic CR game.

The data clearly provide strong support for this hypothesis. The no transgression rate is high and rises across periods in the Basic CR game, and is much lower and declines across periods in the DAC CR game. During the final half of the sessions, the no transgression rate is above 90 percent in the Basic CR game and is less than 10 percent in the DAC CR game. The differences in these rates are highly statistically significant for any time period, even based on the most conservative test that employs only one observation for each session (Mann-Whitney $U=0$ for sample sizes $n=8$, $m=4$; $p$-value $<0.01$).

Leaders most frequently choose the divide-and-conquer option in this game without communication, and they select this strategy at a rate that rises smoothly from 73 percent in periods 1-10 to 94 percent in periods 41-50. Transgressions against both subordinates are frequently resisted, as in the Basic CR game.Figure 3 shows that successful resistance to DAC transgressions is much less common. Recall that the beneficiary of a DAC transgression who is self-regarding has a dominant strategy to acquiesce. The beneficiary’s challenge rate begins below 25 percent and falls over time. Victims challenge at a higher rate, but their challenges are usually unsuccessful and so this rate falls over time. In the final 10 periods the rate of successful joint resistance falls below 4 percent, and after period 20 the expected payoff for the leader of a DAC transgression exceeds the payoff of 6 for no transgression.

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8 In the DAC game without communication, the challenge rate to transgressions against both is 109 out of 134 (81 percent), and successful joint resistance occurs in 43 out of 67 of these transgressions (64 percent). The expected payoff for the leader from a transgression against both is therefore 4.3.
3.3 Cheap Talk Communication in the Divide- and-Conquer Collective Resistance Game

As discussed in the introduction, if some beneficiaries are altruistic punishers, even restrictive cheap talk communication could change outcomes in the CR game because it provides opportunities for the subordinates to signal their types to others. Our treatments add this cheap talk communication in three different ways in order to thoroughly explore the following principal research hypotheses:

**H$_2$: Communication increases the frequency of the no transgression outcome in the DAC CR game.**

**H$_3$: Communication increases subordinate resistance in the DAC subgames.**

These hypotheses are obviously related, and we expect that either both or neither would be supported. Support for H$_3$ would likely cause support for H$_2$, as increased resistance discourages leaders from transgression.

Figure 4 presents the time series of rates that the leader chooses no transgression for all four treatments with the DAC transgression option available. As noted above, this rate is low and declines over time in the treatment without communication. By contrast, the rates rise over time or remain relatively stable for the communication treatments, and leaders choose no transgression at the highest rate in the Private Ex Ante Communication treatment. The figure therefore provides some initial qualitative support for hypothesis H$_2$.

This figure obscures substantial variation in transgression rates across sessions, however, so the statistical evidence in support of H$_2$ is weaker. Table 2 displays the rates that leaders chose no transgression for the 36 individual sessions. The No Communication treatment has many of the lowest rates of no transgression, while the Private Ex Ante and Ex Post Communication treatments have some of the highest rates of no transgression. Nevertheless, these latter two
treatments also have sessions with no (or virtually no) leader choices of no transgression.

The Mann-Whitney test indicates that the Public Ex Ante Communication treatment does not significantly increase the frequency of the no transgression outcome ($U=23$, $n=m=8$; $ns$), but Ex Post Communication does increase the frequency relative to the No Communication baseline ($U=10$, $n=m=8$; $p$-value=0.01). Although no transgression is most frequent overall in the Private Ex Ante Communication treatment, this treatment also has the greatest variation across sessions. Therefore, the increase in no transgression (relative to the no communication baseline) is marginally insignificant ($U=20$, $n=m=8$; $p$-value=0.102). The no transgression rates are not significantly different across any of the three communication treatments; for example, the Mann-Whitney $U$ statistic is 21 for the Ex Post versus Public Ex Ante comparison ($n=m=8$ two-tailed $p$-value=0.247). Pooling over all 24 sessions that featured any communication, this test does reject the null hypothesis that communication does not increase the rate of no transgression ($Z=1.835$, $n=8$, $m=24$; $p$-value=0.033). The bottom of Table 2 indicates that the lower transgression rate in the communications treatments led to modestly higher efficiency.

Although the data provide support for hypothesis $H_2$ in the Ex Post Communication treatment and marginally in the Private Ex Ante Communication treatment, surprisingly this support is not accompanied by corresponding support for hypothesis $H_3$. The subordinate resistance in the divide-and-conquer subgames generally declines over time, and does not appear

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A drawback of these nonparametric tests is that they discard an enormous amount of data when collapsing all the information for each session into a single summary statistic. Therefore, we also conducted random effects probit models of the leaders’ transgression decisions that use all the data. These models provide similar conclusions to the Mann-Whitney tests, and so to conserve space we do not report them in detail. The individual leader subjects are the random effects, and the models include a time trend and site dummies to account for any possible systematic variation across subject pools. Dummy variables for the communication treatments indicate that the Public Ex Ante Communication treatment is not significantly different from the no communication baseline ($t=0.51$; $ns$), but that the Ex Post Communication significantly increases the likelihood of the no transgression ($t=1.93$; one-tailed $p$-value=0.027). Again, results for the Private Ex Ante Communication treatment are at the margin of traditional significance levels ($t=1.56$; one-tailed $p$-value=0.059), and the effects of communication are significant when pooling across the communication treatments ($t=1.68$; one-tailed $p$-value=0.047).
to be systematically different across treatments after the initial 10 or 20 periods. This is confirmed by a series of Mann-Whitney tests based on the later periods 21-50 (similar to those reported for hypothesis H2), with insignificant $U$ statistics that range from 22 to 43. The only test that approaches statistical significance is the greater successful joint resistance in the Ex Post Communication treatment ($U=19$, $n=m=8$; $p$-value=0.086). Nevertheless, the data do indicate a strong relationship between the early (periods 1-20) rates of successful joint resistance and late (periods 21-50) leader choices of no transgression. Across the 24 communication sessions, the simple correlation between these two measures is 0.90, with a highly significant Spearman rank correlation coefficient of 0.82. The four sessions with the highest early resistance had an average late no transgression rate of 0.75, while the ten sessions with the lowest early resistance had an average late no transgression rate of 0.05. This again highlights the substantial variation across sessions, possibly arising from different preference types being randomly allocated to sessions.

Our results indicate that cheap talk does matter, but it only leads to a modest reduction in leader transgression overall. This is perhaps because subjects often fail to follow through on the “intended” choices indicated in their cheap talk. For example, in the Ex Post Communication treatment, victims of DAC transgressions indicate an intention to challenge about 70 to 75 percent of the time during later periods, but they only actually challenge in about 30 to 35 percent of the periods. Beneficiaries of DAC transgressions indicate an intention to challenge about 30 to 35 percent of the time, but they only actually challenge in about 17 percent of the periods. The difference between intended and actual challenge rates is even more pronounced in the Ex Ante Communication treatments.

Successful resistance requires coordination, and Table 3 indicates that the cheap talk appears to play a role here. Victims and beneficiaries are both much more likely to challenge a
DAC transgression when the beneficiary’s cheap talk message was an intention to challenge. The highest challenge and successful resistance rates occur when the cheap talk messages indicate that both subordinates indicate resistance. The lowest rates occur when neither subordinate indicates resistance in the cheap talk, as shown at the bottom of the table. These observations hold for all three communication treatments.

Table 4 presents statistical support for the conclusion that both victims and beneficiaries choose to challenge a DAC transgression when the beneficiary of the transgression or (especially) when both subordinates indicate that they intend to challenge. The likelihood of actual resistance for both victims and beneficiaries, estimated using these random effects probit models, is always significantly higher when both subordinates indicate an intention to resist. Intended resistance by the beneficiary alone also usually increases the actual resistance probability. Model specifications labeled (2) in this table indicate that intentions indicated for the DAC subgame not actually chosen by the leader also have a (more modest but statistically significant) impact on resistance. In particular, note that the beneficiary is significantly less likely to resist a transgression when the victim failed to indicate an intention to support this beneficiary for the subgame (not chosen) in which the beneficiary is a victim of transgression. This suggests that in both the Private Ex Ante Communication and Public Ex Ante Communication Treatments, even cheap talk for subgames that are not chosen by the leader can affect the incidence of collective resistance.

Coordinated resistance was much more successful when the beneficiary indicated an intention to challenge—rising from less than 5 percent to up to 30 percent. This led us to conjecture that leaders were using the public Ex Ante messages to identify which subordinate they could successfully transgress against. Figure 5 provides evidence that supports this
conjecture. It displays the five most common cheap talk messages observed by the leader for DAC subgames for this treatment. The Type B and Type D cases are situations in which both subordinates indicate an intention to challenge a transgression against a particular subordinate (denoted X), while transgressions against the other subordinate (here denoted Y) do not have coordinated resistance indicated in the cheap talk. In both of these cases, transgressions against Y are more than seven times more frequent than transgressions against X.\(^{10}\) Also note that leaders are much more likely to transgress against neither subordinate when both subordinates indicate an intention to challenge both DAC transgressions (Type E). One can conclude from this figure that indicating an intention to challenge a DAC transgression as a beneficiary was quite risky; if the other subordinate did not also indicate a challenge as a beneficiary, this subordinate was likely to be the victim of a DAC transgression.

A comparison of the public and private ex ante messages reveals that subordinates apparently understood how the public cheap talk influenced the leader’s transgression decision, and their messages differ when the leaders can observe them. For the DAC subgames, subordinates were twice as likely to indicate intentions to only challenge transgressions as a victim in the public cheap talk treatment (60 percent) compared to private cheap talk treatment (31 percent). By comparison, they were much more likely to indicate intentions to challenge DAC transgressions both as a victim and a beneficiary in the private cheap talk treatment (46 percent) than the public cheap talk treatment (17 percent). Overall this led to a much lower intended challenge rate as the beneficiary of a DAC transgression in the Public Ex Ante treatment—80 out of 807 cases (10 percent). By comparison, the intended challenge rate as the

\(^{10}\) A probit model (not shown) of the leaders’ decision regarding which subordinate to transgress against, conditional on choosing the DAC transgression (with leaders as a random effect in the error term), indicates a significantly greater propensity to transgress against Y in the Type B (\(t\)-statistic=4.33) and Type D (\(t\)-statistic=6.64) cases in which joint resistance to a transgression against X is indicated in the cheap talk.
beneficiary was 314 out of 714 cases in the Private Ex Ante treatment (44 percent) and 306 out of 890 cases in the Ex Post treatment (34 percent).

4. Concluding Remarks

Centralized authority can be exercised by political and organizational leaders to promote the efficient functioning of societies and organizations. However, since leaders can also exercise their authorities opportunistically, societies and organizations need to have mechanisms to deter opportunistic behavior by the leaders. Recently, authors such as Weingast (1995, 1997, 2005) and Acemoglu et al. (2003) have emphasized that the coordination problem faced by the subordinates when deciding whether to resist leader transgression is crucial in determining whether transgression will take place.

This paper presents a laboratory collective resistance game to investigate the relationship between leader transgression and collective resistance, as well as how different, limited forms of communication between subordinates may affect the incidence of leader transgression and subordinates’ collective resistance. We find that when the leader transgresses against both subordinates, the subordinates succeed in coordinating resistance, and this deter leaders from engaging in this most aggressive form of transgression. We also find that consistent with Weingast’s (1995, 1997) prediction, in the baseline CR game without communication, transgression almost always occurs, and it mainly takes the form of DAC transgression.

However, while an analysis based on purely self-regarding preferences implies that non-binding communication will not affect the incidence of collective resistance against DAC transgression and should not increase the incidence of no transgression in one-shot CR games, we find that non-binding restrictive communication increases the incidence of no transgression in
the CR games. Subordinate communication had a larger impact on leader transgression decisions than on actual collective resistance, but sessions with greater early resistance clearly had lower transgression rates in later periods. Our results suggest the need for further research that investigates the under-explored question of how heterogeneities in preferences among subordinates may change the nature of the coordination problems faced by subordinates when deciding whether to resist leader transgression, and how mechanisms that take into account the importance of social preferences can facilitate collective resistance against the abuse of power by political and organizational leaders.

As a first step to investigate whether social preferences and communication matter in collective resistance, we chose to study restrictive communication because it is easier to implement and control, and the simplicity of the messages makes it easier to quantify the messages and their impacts on behavior. In future studies, we plan to investigate how rich communication, in which subordinates can send free-form “chat” messages to others may affect behavior in the CR game (e.g., Cooper and Kagel, 2005). With rich communication, subordinates can go beyond merely indicating their intended actions, but can also offer justifications for their intentions, and to engage in debate regarding what constitutes appropriate behavior in collective resistance. Such studies will also enable us to examine the content of the messages sent by the subordinates to gain more direct insights about their motivations. We also plan to study how other forms of prior social interactions, such as the possibility of joint production by the subordinates prior to playing the CR game, may foster solidarity between the subordinates and deter transgression by the leader.
Table 1: Experimental Design

<table>
<thead>
<tr>
<th>Treatment</th>
<th>No Communication</th>
<th>Cheap Talk</th>
<th>Public Cheap Talk</th>
<th>Private Cheap Talk</th>
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<tbody>
<tr>
<td>Basic (Simultaneous Transgression)</td>
<td>4 Sessions (36 Subjects)</td>
<td>8 Sessions</td>
<td>8 Sessions</td>
<td>8 Sessions</td>
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<tr>
<td>Divide and Conquer (DAC)</td>
<td>8 Sessions (72 Subjects)</td>
<td>8 Sessions</td>
<td>8 Sessions</td>
<td>8 Sessions</td>
</tr>
<tr>
<td>Possible</td>
<td>2 at Monash Univ., 2 at Purdue Univ.</td>
<td>2 at Monash Univ., 2 at Purdue Univ.</td>
<td>2 at Monash Univ., 2 at Purdue Univ.</td>
<td>4 at Monash Univ., 2 at Purdue Univ.</td>
</tr>
</tbody>
</table>

Note: Fifty periods were conducted in all sessions except in the Public Ex Ante treatment. Four of those sessions completed only 44 periods, and two sessions completed only 29 periods.

Table 2: Rates for Independent Sessions that Leaders Transgressed Against Neither Subordinate

<table>
<thead>
<tr>
<th>Treatment</th>
<th>Basic CR Game (No DAC)</th>
<th>No Communication (with DAC)</th>
<th>Public Ex Ante Communication</th>
<th>Ex Post Communication</th>
<th>Private Ex Ante Communication</th>
</tr>
</thead>
<tbody>
<tr>
<td>(sessions ordered)</td>
<td>0.99</td>
<td>0.33</td>
<td>0.71</td>
<td>0.62</td>
<td>1.00</td>
</tr>
<tr>
<td>highest to lowest)</td>
<td>0.98</td>
<td>0.10</td>
<td>0.18</td>
<td>0.43</td>
<td>0.71</td>
</tr>
<tr>
<td>0.97</td>
<td>0.09</td>
<td>0.13</td>
<td>0.34</td>
<td>0.67</td>
<td></td>
</tr>
<tr>
<td>0.91</td>
<td>0.06</td>
<td>0.08</td>
<td>0.22</td>
<td>0.48</td>
<td></td>
</tr>
<tr>
<td>0.02</td>
<td>0.08</td>
<td>0.13</td>
<td>0.06</td>
<td></td>
<td></td>
</tr>
<tr>
<td>0.00</td>
<td>0.07</td>
<td>0.12</td>
<td>0.01</td>
<td></td>
<td></td>
</tr>
<tr>
<td>0.00</td>
<td>0.00</td>
<td>0.07</td>
<td>0.01</td>
<td></td>
<td></td>
</tr>
<tr>
<td>0.00</td>
<td>0.00</td>
<td>0.03</td>
<td>0.00</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Average Efficiencya | 98.6% | 84.0% | 86.3% | 86.4% | 89.0% |

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

aEfficiency is defined as the percentage of the maximum aggregate surplus (which corresponds to no transgression) realized by subjects.
Table 3: Challenge and Successful Resistance for Different Cheap Talk Messages

<table>
<thead>
<tr>
<th>Cheap Talk:</th>
<th>Ex Post Communication</th>
<th>Public Ex Ante Communication</th>
<th>Private Ex Ante Communication</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Victim Challenge</td>
<td>Beneficiary Challenge</td>
<td>Successful Joint Resistance</td>
</tr>
<tr>
<td>Only Victim</td>
<td>114/443</td>
<td>19/443</td>
<td>5/443</td>
</tr>
<tr>
<td>Indicates Resistance</td>
<td>25.7%</td>
<td>4.3%</td>
<td>1.1%</td>
</tr>
<tr>
<td>Only Beneficiary</td>
<td>31/78</td>
<td>18/78</td>
<td>9/78</td>
</tr>
<tr>
<td>Indicates Resistance</td>
<td>39.7%</td>
<td>23.1%</td>
<td>11.5%</td>
</tr>
<tr>
<td>Both Subordinates</td>
<td>190/228</td>
<td>117/228</td>
<td>110/228</td>
</tr>
<tr>
<td>Indicate Resistance</td>
<td>83.3%</td>
<td>51.3%</td>
<td>48.2%</td>
</tr>
<tr>
<td>Neither Subordinate</td>
<td>20/141</td>
<td>5/141</td>
<td>0/141</td>
</tr>
<tr>
<td>Indicates Resistance</td>
<td>14.2%</td>
<td>3.5%</td>
<td>0.0%</td>
</tr>
</tbody>
</table>
Table 4: Random Effects Probit Models of DAC Challenge Decision Based on Cheap Talk Messages

<table>
<thead>
<tr>
<th>Cheap Talk:</th>
<th>Victim Challenge</th>
<th>Beneficiary Challenge</th>
<th>Victim Challenge</th>
<th>Victim Challenge</th>
<th>Beneficiary Challenge</th>
<th>Beneficiary Challenge</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(1)</td>
<td>(2)</td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>Only Victim</td>
<td>0.14</td>
<td>0.11</td>
<td>0.63**</td>
<td>0.59**</td>
<td>0.84**</td>
<td>0.82**</td>
</tr>
<tr>
<td>Indicates Resistance</td>
<td>(0.19)</td>
<td>(0.34)</td>
<td>(0.15)</td>
<td>(0.15)</td>
<td>(0.29)</td>
<td>(0.29)</td>
</tr>
<tr>
<td>Only Beneficiary</td>
<td>0.92**</td>
<td>1.25**</td>
<td>0.95**</td>
<td>0.97**</td>
<td>1.76**</td>
<td>1.78**</td>
</tr>
<tr>
<td>Indicates Resistance</td>
<td>(0.22)</td>
<td>(0.34)</td>
<td>(0.26)</td>
<td>(0.27)</td>
<td>(0.38)</td>
<td>(0.38)</td>
</tr>
<tr>
<td>Both Subordinates</td>
<td>2.45**</td>
<td>2.04**</td>
<td>1.75**</td>
<td>1.67**</td>
<td>2.17**</td>
<td>2.13**</td>
</tr>
<tr>
<td>Indicate Resistance</td>
<td>(0.22)</td>
<td>(0.31)</td>
<td>(0.28)</td>
<td>(0.28)</td>
<td>(0.41)</td>
<td>(0.41)</td>
</tr>
</tbody>
</table>

Victim Indicates Resist to other DAC$^a$

|                      | 0.43**           | 0.47*                  | 0.25             | 0.40†             |
|                      | (0.14)           | (0.20)                 | (0.15)           | (0.22)            |

Beneficiary Indicates Resist to other DAC$^a$

|                      | -0.16            | 0.06                   | 0.09             | 0.49†             |
|                      | (0.15)           | (0.25)                 | (0.14)           | (0.27)            |

1/period

|                      | 2.16**           | 1.09*                  | 2.87**           | 2.37**           |
|                      | (0.43)           | (0.51)                 | (0.48)           | (0.48)           |

Dummy = 1 if session was at Purdue

|                      | -0.30            | -0.11                  | -0.06            | -0.07            |
|                      | (0.34)           | (0.51)                 | (0.29)           | (0.28)           |

Constant

|                      | -1.16**          | -2.46**                | -1.30**          | -1.24**          |
|                      | (0.23)           | (0.38)                 | (0.20)           | (0.23)           |

Log likelihood

|                      | -377.3           | -221.1                 | -415.5           | -410.3           |

Observations

|                      | 890             | 890                    | 807             | 807              |

Notes: All models are estimated with subject random effects. 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).

$^a$ The “other DAC” in these variable definitions refers to the other divide-and-conquer transgression that is not chosen by the leader in this particular period.
References


Figure 1: The Basic Collective Resistance Game (payoffs are for (Leader, Subordinate A, Subordinate B))

Figure 2: The Divide-and-Conquer Collective Resistance Game (payoffs are for (Leader, Subordinate A, Subordinate B))
Figure 3: Challenge and Successful Resistance Rates for DAC Transgressions (No Communication)

Figure 4: No Transgression rates for all DAC game treatments
Type A: Only victim to challenge X, nobody to challenge Y
Type B: Both to challenge X, nobody to challenge Y
Type C: Only victim to challenge X, only victim to challenge Y
Type D: Both to challenge X, only victim to challenge Y
Type E: Both to challenge X, both to challenge Y

Note the much higher transgression against Y when joint challenge against X is indicated

Figure 5: Leader Transgression Decisions Depending on Subordinate Public Ex Ante Intentions

Note: These are the main combinations of DAC intentions (889 out of 1002 cases) observed by
Appendix: Instructions (Ex Post 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 50 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 in different decision making periods.

At the beginning of each decision making period 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. The groupings change every period, since you will be randomly re-grouped in each and every period.

Your Choice

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 that you are grouped with for the current decision making period. 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 you are grouped with for the current decision making period. 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. Then click on the *continue* button on the lower right of your screen. Remember, at the start of the next period all participants are randomly re-grouped, and you are randomly re-grouped each and every period of the experiment.

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.