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ABSTRACT:

Punishment has been shown to be an effective reinforcement mechanism. Intentional or not, punishment will likely generate spillover effects that extend beyond one's immediate decision environment, and these spillovers are not as well understood. We seek to understand these secondary spillover effects in a controlled lab setting using a standard social dilemma: the voluntary contributions mechanism game. We find that spillovers from punishment lead to either more or less cooperative behavior depending on the history of play. If subjects have direct experience with a punishment mechanism, they will contribute more to the public good after observing others' punishment. The reverse is true of those who observe others' punishment but have no exposure to direct experience with punishment.

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1. Introduction

Punishment is a tool commonly used to discourage bad behavior in favor of good behavior. Though it is unlikely that a punishment mechanism will possess such a narrow scope that it only alters the behavior of the one punished, very little research in economics has considered the broader impacts of observing punishment.[2] This implies that our current understanding of punishment effects may be incomplete. For instance, punishment could be more effective than currently thought if the observers of punishment subsequently reduce their own undesirable behavior. While less likely, the opposite is also possible where the gains in behavioral modification of the punished individual could be offset by an adverse reaction from observers of the punishment. It is the goal of this paper to strengthen our understanding of punishment mechanisms by using experimental methods to explore how the observation of others being punished might affect behavior.

Punishment is intended to discourage undesirable behavior or actions[3], with effectiveness shown to depend on its frequency, intensity and immediacy (Anderson and Stafford 2003, Johnston 1972). Since Becker's seminal work in "Crime and Punishment: An Economic Approach" (1968), economists have become increasingly interested in the behavioral effects of punishment. The importance of punishment spillovers in organizations is apparent (Bandura 1971). Punishment attached to low effort conveys the information that adherence to a behavioral standard is socially beneficial and supports a collective goal. Punishment therefore provides information on the acceptability of a behavioral standard, as well as providing an incentive to

[2] see Xiao and Houser 2010

[3] e.g. Andreoni, Harbaugh, Vesterlund 2003, Fehr and Fischbacher 2004, Ostrom, Walker and Gardner,1992, Xiao and Houser 2010

adhere to it.[4] However, when free riding incentives are present, as in many organizational environments with public goods features, the individual decision maker still faces a behavioral dilemma even with punishment potential.[5] Our paper is novel in that it experimentally investigates whether observing punishment of someone else in a social dilemma setting can increase one's own socially beneficial behaviors (even when there is no chance that the observer will be punished).

Because of the difficulties in isolating the variables of interest in a field setting, we use laboratory methods to conduct our analysis of punishment spillovers. Our design uses a simple social dilemma environment (i.e., a public goods game) that produces a well-defined outcome variable. If subjects respond to observing punishment by contributing more towards public goods, this would imply vicarious punishment is real and that we currently underestimate the beneficial behavioral effects of punishment mechanisms. On the other hand, if observed punishment reduces cooperative behavior—the direction of punishment spillovers are contrary to the usual expectation—then researchers have overestimated the ability of punishment to reduce undesired behaviors in a broader sense

2. Literature

Outside economics, researchers have invoked social learning research to hypothesize that observed punishment leads to a decrease in undesirable behavior (see Arvey and Jones 1985, Trevino 1992 and references therein). In the psychology literature, there has been significant interest in the topic of vicarious punishment (see Malouff, Thorsteinsson, Schutte, and Rooke

[4] Of course, the effectiveness of punishment depends greatly on the group members' regard for the established standard. If a boss institutes a particular rule that he sees as integral to the success of the firm, but that the employees find useless, punishment for breaking this rule will not improve behavior and in fact may have the opposite effect (Trevino 1992).

[5] Also, while severe punishment penalties may be effective at reducing undesirable behavior, such punishment may be used infrequently due to prohibitively high costs (Xiao and Houser 2010).

2009 and references therein). Specifically, Malouff et al (2009) is a meta-study of psychology experiments examining the existence of vicarious punishment, or “whether the observer is, in effect, punished.” They find that, in general, subjects react in the predicted direction which can be explained by learning in their setting. In addition to the learning story, these studies also use nonstandard subject pools (most used child subjects which may not be applicable to our setting) and outcome measures that are not clearly defined or tasks that are not incentivized, which introduce potential confounds or a lack of saliency into the environment. In what follows, we will refer to a more general “spillover effect”, noting that the term “vicarious punishment” implies the intended deterrent effect of punishment, while the general term is more flexible.

Experimental economics research has shown that punishment can be effective in sustaining cooperation in public goods settings (i.e., the decision setting we use in our experiments), and in some cases may be more effective than the use of positive reinforcement (See Dickinson 2001, Masclet, Noussair, Tucker and Villeval 2003, Noussair and Tucker 2005, Sutter, Haiger, and Kocher 2010). However, Anderson and Stafford (2003) find that past punishment has a negative effect on compliance. Nevertheless, these studies only examined the direct effect of punishment on the individual being punished. The question of punishment spillovers has not been adequately addressed, though some indirect effects have been identified.

Results from the laboratory have shown that third parties care about others’ behavior even when they are strictly observers with no financial stake in the outcome (Fehr and Fischbächer 2004, Croson and Konow 2007). A separate strand of experimental economics literature has documented the impact punishment can have on adherence to social norms (Bendor and Mookherjee 1990, Ostrom, Walker, and Gardner1992, Fehr and Gächter 2000, Anderson and Putterman 2006, Carpenter, Mathews and Ong’Ong’a 2004, Xiao and Houser 2010). Thus, there is reason to believe that those observing punishment may change their own behavior when involved in a social dilemma where social norms play an important role.

In addition to examining the spillover effects of punishment, we also explore the role that social distance between punished agent and observer plays in the strength of the spillover effect. Trevino (1992) shows that reduced social distance increases social learning and motivation, but the direction of the net behavioral change is not clear. Reduced social distance between team members may enhance social learning of observed punishment such that the observer is more likely to avoid the prohibited behavior. However, other findings suggest that the observer may be more prone to respond with negative emotions, attitudes and behaviors (Brockner, Grover, Reed, DeWitt, and O'Malley 1987, Brockner 1990, and Brockner & Greenberg 1990). Dickinson and Villeval (2008) find that reduced social distance crowds out the effectiveness of negative sanctions (i.e., increased monitoring), though they examine principal-agent relationships as opposed to peer relationships with exogenous punishment. Overall, existing research highlights the interest in and relevance of this topic, but we contribute to the literature with several design elements that allow a clean estimation of punishment spillover effects.

3. Model

Punishment is often implemented when self interested behavior negatively impacts others. Such is the case in a social dilemma, which led us to use a voluntary contributions mechanism (VCM) game in our experimental design. In a VCM game, a subject is given an endowment of tokens, E, which represents money or an endowment of ability. A subject's choice, x, is the amount contributed (money or effort) to the group and the subject keeps the remainder, E-x. The tokens are converted to money where the tokens kept return α and the tokens given to the group account return β to each of the N individuals in the group where $\alpha > \beta$ and $\alpha < N * \beta$. Given this standard structure, utility with no punishment, $U_{i,N}(x)$, can be specified as

$$(1) \qquad U_{i,N}(x) = \alpha(E - x_i) + \beta \sum_{j=1}^{N} x_j$$

where x_i is the amount contributed to the group account by subject i. The above assumes the standard typical money-maximizing agent. With these assumptions, complete free-riding is the predicted outcome since utility is maximized at the boundary where $x_i = 0$. Because of this outcome, punishment is seen as a way to increase contributions to the group. When punishment is added, the utility function, $U_{i,P}(x)$, is now

$$(2) \qquad U_{i,P}(x) = \alpha(E - x_i) + \beta \sum_{j=1}^{N} x_j - I(\bar{x}) * (P * d)$$

where P is the probability of punishment of level d and $I(\bar{x})$ is an indicator function equal to 1 if the individual's contributions to the group are below a threshold level of $\bar{x}$ and zero otherwise. With this addition, contributions to the group will depend on the threshold, the probability of punishment and its severity. Prior studies looking at observance of punishment have focused on manipulating these variables and/or the information available about them. As an example, the psychology study by Schnake (1986) studied college students' reaction to a confederate being punished for low output. The subjects in this study had no idea that punishment for low effort was possible. Once they observed punishment of someone else, they were able to update their prior (presumably incorrect) beliefs of $I(\bar{x})$, P and/or d. In other words, subjects were maximizing $U_{i,N}(x)$ before observance of punishment, and $U_{i,P}(x)$ after observing punishment. In a related economics study, Xiao and Hauser (2010) examine how the observance of endogenous punishment to fellow group members affects contributions. In this instance, subjects can be assumed to be maximizing $U_{i,P}(x)$ with uncertainty about $I(\bar{x})$, P and d. Since maximization of this utility depends on these variables, behavior can be expected to change as more information is gathered about the relevant variables. The effect can again be viewed in terms of updating prior beliefs of what a subject views the relevant punishment variables are. These are important studies in understanding how subjects update their prior beliefs.

Up to this point utility has been defined in terms of monetary payoffs. As decades of research have shown, utility is more general than that. Our study diverges from prior studies by examining the behavioral implications of observing punishment that has no direct impact on one's *monetary* payoff. Our interest lies in understanding how the observance of punishment in an independent social dilemma may affect one's own-group contributions (i.e., contributions in a separate and distinct social dilemma). More specifically, we examine a utility function of agent *i* where she *only* observes punishment and this observation will not affect her own monetary payoffs. Consider utility, $U_{i,O}(x)$, defined as

$$(3) \qquad U_{i,O}(x) = \alpha(E - x_i) + \beta \sum_{j=1}^{N} x_j + O(P) * \gamma(V)$$

where $O(P)$ is an indicator function equal to 1 if punishment of someone else is observed and zero otherwise, and $\gamma(V)$ is a function of some vector V of personal characteristics which maps the agent's reaction to observed punishment into her contributions. This function will include, among other things, a history of punishment, one's natural tendency to be a "cooperator" or other relevant features that may change individual behavior after observance of punishment. One of our objectives is to uncover some of the specific factors that influence that sign and magnitude of γ(V). Notice that in model (2) the personal experience of punishment affects contributions through money maximization while the observation of punishment in (3) does not affect monetary outcomes. As such, a strict money-maximizing agent will not change her behavior and will contribute zero to the group regardless of observing punishment or not. In other words, Model (3) reduced to Model (1).

> *Hypothesis 1: If agents are money-maximizers, behavior will not change when punishment is observed.*

Above we assume that the punishment variables are fixed and that agents have full information on these variables and that they know they will not be punished. For the more general case of utility maximization shown in (3), observed punishment may increase or decrease contributions, depending on agent perceptions of the fairness of the punishment mechanism. This may occur for a variety of reasons. In line with Trevino's proposition 4a, an anti-social reaction can occur if the punishment is viewed as unfair.[6] Nevertheless, we have noted in Section 2 that the majority of the existing research on punishment in public goods games finds that it increases cooperative behaviors among those directly punished. Thus, our initial hypothesis regarding behavioral effects of observed punishment follows:

> *Hypothesis 2: The observance of punishment will lead a subject to increase her own contributions.*

For observed punishment to affect behavior, whether in the pro-social or anti-social direction, the agent must internalize the observed punishment as if it were her own. Our experimental design includes two features that may increase the saliency of the punishment. The first feature is a treatment where personal and direct experience with a punishment mechanism is

[6] This is similar to Frey's (1993) hypothesis that monitoring workers may "crowd out" the intrinsic motivation to perhaps the job task. While Hypothesis 2 refers to the observation of punishment, one might argue that this could reduce the intrinsic motivation of being socially cooperative.

given before subjects are strictly observers of punishment. In order to internalize the punishment as happening to them, agents must first know how they would react to being punished. Thus, it may be important for an agent to have experience with a punishment mechanism before they can internalize observed. This leads to our third Hypothesis.

> *Hypothesis 3: Direct experience (history) with a punishment mechanism will help subjects internalize the observation of punishment, thus strengthening the behavioral effects.*

Notice that Hypothesis 3 is general and testable even if our data do not support Hypothesis 2. Should we find evidence of anti-social punishment effects in our environment, support for Hypothesis 3 would imply that observed punishment causes similar behavioral effects (thus strengthening the *total* behavioral effect of punishment).

The second design feature we include is a social distance manipulation between the one being punished and observer. Reduced social distance should lead to the observance of punishment being more personal and thus we expect that the magnitude of the reaction to be increased when social distance is decreased in line with Trevino's (1992) proposition 1c. This leads to our fourth Hypothesis.

> *Hypothesis 4: Decreasing social distance between two agents will help internalize the observation of punishment, thus strengthening the behavioral effects.*

4. Experimental Design

The experiments were conducted in the xs/fs lab at Florida State University. Subjects were assigned computer terminals at random and used software programmed in z-Tree (Fischbacher, 2007). In what follows we refer to super-groups and VCM groups to avoid confusion. For example, if a session involved 18 subjects, two super-groups of 9 subjects are randomly formed and remain fixed for the entire experimental session. We will refer to these groups as super-group 1 and super-group 2. Subjects will play 3-person VCM games with other subjects within their super-group, and we refer to these 3-person groups as the VCM groups. A super-group of 9 subjects allows us to randomly form new VCM groups every round from within one's super-group. Having two super-groups allowed us to randomly match each subject from super-group 1 with a subject from super-group 2 for the purposes of observed punishment treatments.

In each session, there were three games played by each VCM group. We will refer to the three as Game A, Game B, and Game C. Game A is a standard VCM game, which is used as a baseline and gave subjects experience playing the game and learning about the social norms of others. This is important since the theory dictates that we control for awareness of these norms. Subjects were asked to allocate 10 tokens into an "individual" or "group" account in whole token increments. Tokens allocated to the individual account yielded $0.025 to that person alone and tokens allocated to the group account returned $0.0125 to all three members of that subject's VCM group. Thus, a money-maximizing agent would prefer to put all 10 tokens in the individual account (earning $.25) while the social optimum is for all subjects to put all 10 tokens in the group account (earning $.375). The only real difference in our baseline compared with many other standard VCM games is that super-group 1 subjects always made decisions first, sequentially followed by the decisions of super-group 2. Since our main interest is examining how others react to seeing someone punished, a sequential move was needed. Game A does not allow for punishment, but the sequential move structure is needed to ensure that results in Games B and C are not simply a function of sequential decisions across super-groups 1 and 2.

Game B was similar to A except that a punishment mechanism was introduced. Punishment was only possible for VCM groups within super-group 1 in Game B. If a VCM group member contributed less than 5 tokens to the group account, there was a 50% chance of being punished by losing $0.25 (equal to an expected punishment cost of $0.125, or 5 tokens) of their period earnings. This still leaves the strategy structures weakly intact in that a risk-neutral money-maximizing agent would be indifferent between complete free riding and contributing 5 tokens to the group account where their expected payoff is $.075 in each case. The word "punished" was specifically used in the experiment for the exogenous punishment mechanism.

The size and probability of punishment were structured so that we would have a reasonable number of subjects being punished, which is necessary to test our predictions. In addition to the punishment mechanism for super-group 1, recall that each subject was matched with an "other-group counterpart" from the other super-group. These counterparts remained fixed throughout the experiment and never interacted in the VCM games. In each round of Game B the subjects in super-group 1 made a decision and outcomes (i.e., private payoffs, group payoffs and punishments) were determined. Thereafter, the partner from the unpunished super-group 2 is informed whether her other-group counterpart was punished or not and how often they had been punished previously and the costs of such punishment. Those in super-group 2 then made a decision in their respective Game B where they personally do not have the possibility of being punished. Because other-group counterparts never play each other, any behavioral response by the unpunished group can't be due to an attempt to increase the first person's contribution nor can it be reciprocal as a means to rewarding the first person.

Game C is similar to Game B except now subjects from both super-groups 1 and 2 are subject to the same punishment mechanism. Thus, subjects in super-group 2 can observe punishment *and* can be directly punished themselves in Game C. The sequence of decisions was the same. Subjects played all three games in a single session where Game A was always played first. To control for order effects across punishment treatments, half the experiments were ran in game order ABC and half in order ACB. Counterbalancing the treatment order for Games B and C is also necessary to generate the differential history needed to test hypothesis 3.

In addition to the above, the social distance of the other-group counterparts varied according to two social distance treatments: friends and strangers. In the friend treatment, social distance is reduced using the following methods. After subjects were seated, but before instructions for the game were distributed, subjects were given a list of general topics to discuss with the person next to them for a short get-to-know-you-session. This person was eventually the "other-group counterpart" in the experiment and they knew this. Subjects were told they had five minutes in which to talk to the person next to them. In the stranger treatment subjects were simply assigned an "other-group counterpart" (OGC) at random. Neither "friends" nor "strangers" were allowed to talk after the experiment began. Once the experiment began, the subjects kept the same "other-group counterpart" throughout the experiment. Recall that the person from super-group 2 observed the punishment outcome of their OGC in super-group 1. Table 1 gives a summary of the treatments and the number of subjects in each treatment.

Table 1: Number of subjects per treatment[7]

	Chatted with other group counterpart	
Order of Game play	Yes (Friends)	No (Strangers)
ABC	18	24
ACB	18	36

[7] The first session, represented in the upper right quadrant of Table 1, had 24 subjects and the return on the public and private accounts were doubled compared to the other sessions. When this treatment is compared to the treatment with chat and the same game order, there is no statistical difference in second-mover's average contributions (t-test, p-value = .432). We also ran two 18 subject sessions of the no chat, ACB treatments since subjects in the first were initially handed the instructions with the doubled payoffs. The error was quickly caught and they were handed the correct instructions. The appendix has regressions ran with and without this session showing that the basic results do not change.

Game A was the baseline. In Game B, only first movers could be punished while in Game C both first and second movers could be punished.

Before each treatment, subjects were given written instructions that were read out loud by the experimenter and were followed by a short quiz to ensure subjects understood the instructions. Once all subjects completed the quiz, the first 10-period treatment (Game A) started. In the instructions, subjects were given information only about the current treatment but knew others would follow. The same procedure was carried out for all three 10-period treatments of the experiment where instructions were read out loud followed by a quiz. A total of 96 subjects participated in the experiment (recruitment used ORSEE software (Greiner 2004)). On average, subjects made $20.46 for about an hour of their time.

5. Results

Game A was meant to make sure subjects understood the computerized game interface, the social norms in this game and generated baseline predictions on initial levels of cooperative behavior. In Game A, subjects contributed an average of 2.3 tokens to the public account over ten periods. The decline typical of VCM games is evident as first period contributions averaged 3.0 while last period contributions averaged 1.9. Since our interest lies in vicarious punishment, the behavior of the second movers is most relevant. On average, second movers contributed 2.4 tokens to the public account in Game A, with contributions declining over the 10 rounds.

Game B allows us to observe the behavior of the subjects when they saw their other-group-counterpart (OGC) was punished (or not), but they themselves could not be punished. In Game B, there were a total of 480 observations (48 second movers who played the game 10 times). The average amount contributed by second movers in Game B was 1.8. Of these 390 observations, second movers observed their OGC being punished 84 times. There was no statistical difference in contributions conditional on observing if a subject's OGC was punished or not (1.8 vs. 1.8). These summary statistics may mask underlying trends though.

In the left portion of Figure 1 we see that when second movers in game B already have experience playing game C, their average contributions of 2.6 are much higher if they see their OGC punished than average contributions of .9 without this experience. This difference is statistically significant (t-test, p=.01). In the right portion of Figure 1, it is shown that contributions when a subject's OGC was not punished are statistically similar for the two treatment orders (p>.10). Because the reaction to seeing one's OGC punished depends on the treatment order, this implies that experience with punishment is an important determinant of the vicarious punishment effect.

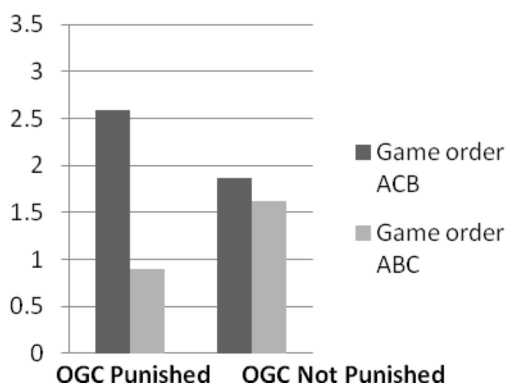

Figure 1: Average contributions, Game B, Punished, Game Order

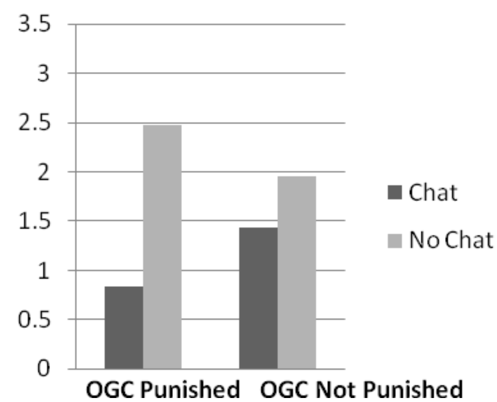

Figure 2: Average contributions, Game B, Punished, CHAT

Figure 2 looks at the same breakdown with regards to the chat treatment. On the left hand side , it is shown that those who saw their OGC punished contributed significantly less if they chatted than if they did not chat (t-test, p=.01). On the right side of Fig. 2, we see that there is a significant "chat" effect for those not observing OGC punishment as well (t-test, p=.05). Thus, chatting with someone seemed to lower overall contributions, even more so when subjects observed their OGC was punished.[8]

In addition to the above analysis, it is also useful to examine how observing punishment affects those subjects who are more inclined to be generous versus selfish (see Figures 3 and 4). Figure 3 looks at the contributions in Game B of those subjects who contributed more than the average person in the first period of Game A. While a significant difference is not observed in the first 5 periods, a statistically significant difference is found in the final rounds of Game B (t-test, p<.01). Thus, subjects who are initially more generous are contributing much less in the final 5 periods if they observe their OGC was punished. Figure 4 shows the opposite effect in the final rounds for *below* average contributors (t-test, p<.01).

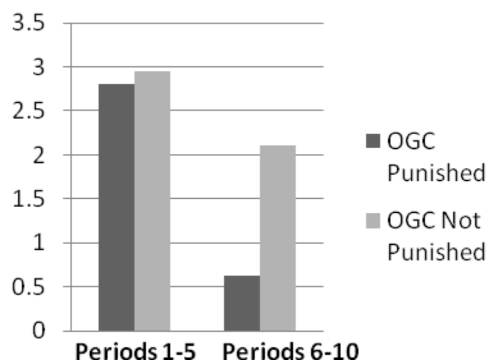

Figure 3: Average contributions in Game B, ***Above*** Average Contributors

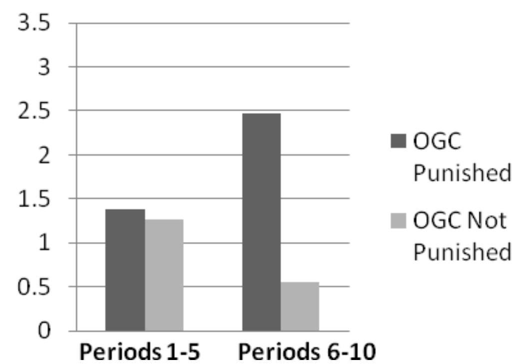

Figure 4: Average contributions in Game B, ***Below*** Average Contributors

Though the above analysis is suggestive, a more comprehensive econometric analysis controlling for multiple factors is needed to fully examine our data. We use a random effects model to account for unobserved individual effects and cluster on the individual level to control for correlation of error terms in our panel data. The dependant variable for cooperation is "tokens contributed" to the public account by second movers. The main explanatory variable is *OGC*

[8] Though not the focus of the paper, something worth further exploration is why chat seems to lower contributions. Recall also that this result does *not* refer to chatting with individuals within one's own social dilemma group.

Punished, which equals one if they saw their partner was punished and zero otherwise. Recall that a money-maximizing agent has a weakly dominant strategy to contribute nothing to the public account and, specifically, second movers have identical monetary incentives to contribute in Game B compared to the baseline Game A because second movers do not face a punishment threat in Game B. Thus, Game B allows a more clean examination of the vicarious punishment effect compared to Game C data. Model 1 results in Table 2 are from a basic regression on Game B data that includes only *OGC Punished* and a variable for the decision round, *Period,* as explanatory variables. The significant negative coefficient on *Period* implies declining contributions across rounds as is typical in traditional VCM games, but observance of one's OGC being punished (i.e., vicarious punishment) is insignificant. Figures 1-4, however, imply that additional controls are needed in the analysis (i.e., models 2-5).

Model 2 (Table 2) includes additional dummy variables for *Chat* (=1 for reduced social distance treatments) *Game ACB* (=1 when subjects in Game B have already played Game C where they faced their own punishment threat) and a continuous variable for *First Period Contributions*. From Model 2, notice that there is no main (pure) effect of *Chat* on contribution levels. What is shown in Model 2 is that having already played game C significantly increases the contributions of second mover subjects in game B. Additionally, a positive correlation is seen between first period contributions and contributions in Game B.

Model 2, however, cannot discriminate between whether the previous history of Game C play increases Game B contributions because of past OGC punishment or because of past direct punishment (as both are possible in Game C). To explore whether *Game ACB* matters differentially when one observes counterpart punishment or not, or whether *Chat* and *First Period Contributions* interacts with OGC punishment, we add interaction terms to produce our

Table 2: Regression Analysis: Contributions in Game B.

Variable	**Model 1**	**Model 2**	**Model 3 (all periods)**	**Model 4 (periods 1-5)**	**Model 5 (periods 6-10)**
OGC Punished	-.400 (.311)	-.359 (.306)	-.993* (.602)	-1.657*** (.556)	.484 (.781)
Period	-.124*** (.034)	-.172*** (.040)	-.175*** (.040)	-.336*** (.115)	-.154* (.083)
Game ACB		2.284*** (.598)	2.065*** (.610)	3.223*** (1.219)	2.319*** (.748)
Chat		-.445 (.488)	-.377 (.485)	-.301 (.538)	-.440 (.525)
First Period Contributions		.190** (.091)	.191** (.087)	.277*** (.096)	.116 (.098)
Game ACB * OGC Punished			1.464*** (.543)	2.490*** (.735)	.421 (.730)
Chat * OGC Punished			-.366 (.497)	-.595 (.595)	-.572 (.765)
First Period Contr. * OGC Punished			.008 (.129)	.224* (.128)	-.441** (.198)

Notes: Standard errors are corrected for clustering at the subject level. Three (***), two (**), and one (*) stars indicate statistical significance at the 1%, 5%, and 10% respectively.

final Models 3, 4, and 5. Model 3 is estimated for data on all 10 periods of game B contributions, whereas Models 4 and 5 are estimates based on data from periods 1-5 and 6-10, respectively.[9]

From the estimates in Models 3-5, the general decline in contributions over time is robust (*Period* estimate), as is the pure effect of punishment treatment history reflected in the *Game*

[9] The fifth period was arbitrarily chosen because it is the halfway point in the Game. Our results are robust to changing this cutoff point between early and later rounds. We also control for gender effects in Models 2-5, though gender is statistically insignificant in all instances.

ACB variable. We now turn to Hypotheses 1-3. From these more complete models 3-5, it is clear that the vicarious punishment effect depends on the history of play. Observation of punishment leads to a decrease in contributions when second movers have no history of Game C play (coefficient estimates on *OGC Punished* in models 3 and 4), but it leads to *increased* contributions when second movers have a history of Game C play (coefficient estimates of *Game ACB*OGC Punished* in models 3 and 4). One can also see from Models 3, 4, and 5 that some of the key behavioral effects may be limited to the first half of the game (rounds 1-5), and also differ based on the propensity of a subject to contribute initially (e.g., see *First Period Contr* variable interaction term). Overall, we find:

Result 1: Agents are not simple money-maximizers. Observed punishment affects behavior.

We find support for Hypothesis 2. Independent of punishment history, we find that observed punishment decreases contributions to one's own VCM game, which is consistent with the idea that the punishment mechanism is viewed as unfair. Interestingly, in the last half of the game (rounds 1-5) this effect appears to be stronger for those subjects who initially were the most cooperative.[10] The impact of punishment history will be explored further below. Thus, we have:

Result 2: Observed punishment (controlling for punishment history) has the adverse (anti-social) effect of decreasing contributions.

[10] We find evidence that the adverse punishment spillover effect in game B (treatment order ABC) is less severe in the first 5 rounds for those subjects with higher first period (game A) cooperation. That is, in Model 4, we estimate a statistically significant positive coefficient on the interaction term *First Period Contr.*OGC Punished*. The pure effect of being initially cooperative is to be more cooperative following OGC punishment, though the magnitude of this effect is overwhelmed by the main effect of observed OGC punishment. Thus, absent the history of game C play, the decrease in contributions following OGC punishment is *less severe* in the initial rounds for the most initially cooperative subjects, though this interaction effect reverses in rounds 6-10 (Model 5).

As for Hypothesis 3, while history with punishment matters, it *reverses* the sign of the behavioral effect on observed punishment, which is not consistent with Hypothesis 3 but merits further analysis (see below).[11] The lack of significance on the key interaction term in model 5 indicates that this punishment-history result is concentrated in the first 5 periods of play in Game B. We therefore have:

Result 3: History or experience with a punishment mechanism significantly impacts the observed-punishment effect. However, it reverses rather than strengthens the effect.

In other words, Result 3 highlights that subjects with direct punishment history become more cooperative when observing punishment, whereas the no-history tendency is for observed punishment to decrease cooperative behavior in one's own social dilemma. This effect is estimated to occur only in the first half of the Game B data (rounds 1-5), so it appears that the history effect may be more short-term.

Finally, we examine Hypothesis 4 by referring to the *Chat*OGC punished* interaction term. It is clear from all model estimates that *Chat* does not have a significant main effect or interaction effect with observed punishment. Thus, the data do not support Hypothesis 4.

Result 4: Social distance does not affect contributions nor does it impact the behavioral effect of observed punishment.

5.1 Further Analysis using Game C data

Result 3 suggests that experience with punishment matters in how subjects react to seeing their OGC punished. Game C data can help us test which part of experience matters by comparing subjects who only observed to the punishment of others to subjects who had only

[11] Of course, here we refer to immediate experience with punishment institutions in the same experimental session, as opposed to one's experience with VCM game punishment from a previous experiment, although it would be of interest to explore the limits of any experience effect in future research.

direct punishment. Specifically, we examine second-movers who played game B before game C and compare their initial two rounds of contributions in Game C with the contributions of first-movers in the initial two rounds of Game C. Thus, we are comparing first mover and second movers who have played the same number of total periods, but the second movers' history in the 10 rounds of the prior game was *only* observed punishment history. For first movers, the previous treatment history involved a direct punishment institution.[12] From this comparison, we can discern if direct or observed punishment history has more impact on cooperative behavior. We arbitrarily focus on the first two rounds of the final game to avoid the buildup of new history effects on first- and second-mover contributions. Those with only direct punishment experience contributed an average of 4.3 tokens, while those who only observed punishment contributed a significantly higher 5.3 tokens (t = -1.89, p=.06).

Result 5: Experience with observed punishment has a stronger effect than experience with direct punishment in promoting cooperative behavior in Game C (everyone can potentially be punished).

Results highlighted previously in Figure 1 indicate that history with the direct punishment institution (game C) is a significant predictor of how second movers respond to observed punishment. That result did not distinguish second movers who actually experienced direct punishment from those who did not. We now consider the subset of second movers who played *and* were punished in game C before they played game B (n=16) to those who played game C but were never punished (n=11). We again focus on the initial two rounds of behavior in the final game before confounds build up. For the subset having played *and* been punished in game C,

[12] Strictly speaking, the analysis would compare *only* those subjects experiencing direct punishment to those *only* having experienced observed punishment. This greatly reduces our sample size such that we would have low power to detect a significant effect, and such a comparison might also be criticized on the basis of sample selection regarding first movers who experienced direct punishment.

those who observed OGC punishment contributed 3.3 tokens on average compared to 1.2 tokens for those who did not observe OGC punishment (t-test, p=.06). For the subset of subjects having played Game C but *never* were punished, those who observed OGC punishment contributed 4.5 tokens on average, compared to 3.2 tokens for those who did not observe OGC punishment, which is a statistically insignificant difference (t-test, p = .53). Thus, while consistent with Figure 1 results, this shows more explicitly that direct punishment history plays a significant role in shaping cooperative behavior in the presence of observed punishment.

Result 6: Experience with direct punishment increases the effectiveness of the punishment mechanism through observance of punishment.

The two preceding results relied on between-subjects analysis, either comparing first movers and second movers (Result 5) or different subsets of second movers (Result 6). A within subjects comparison offers the general advantage of control over individual differences present in between subject comparisons.[13] In order to more carefully examine if subjects respond to observed punishment similarly to if they had experienced punishment directly, we will first examine responses to direct punishment and then compare how these same subjects respond to observed OGC punishment. The extant literature defines "vicarious" punishment to mean that observed punishment deters undesirable (in this case, non-cooperative) behavior. In other words, direct punishment is typically assumed to have the intended deterrent effect, and so vicarious punishment has similar behavioral effects.

To pursue this within subjects analysis, we must identify the second movers who were directly punished at some point during Game C and who also observed OGC punishment in

[13] For a given number of subjects, there is increased statistical power in a within-subjects test. The counterbalanced order of our games in the ABC and ACB session helps minimize the concern of ordering effects being the source of our test results.

Game B. Out of the 48 second movers, 15 meet this criteria. Keep in mind that token contributions during the same period of direct punishment occur *before* the punishment is administered, and so we use this as a proxy for the subject's baseline contribution. We find that average token contributions for the period one receives direct punishment was .41, average contributions for the period after one is directly punished was 2.4, and average contributions of these same subjects after observing OGC punishment in Game B was 1.26 tokens. Thus, the average subject response to direct punishment is in the beneficial direction of increased contributions in the next round (t-test, $p < .01$), the response to observed OGC punishment is significant and in the same direction as the direct punishment effect (t-test, $p < .01$), but this "vicarious" punishment effect is not as large in magnitude as the direct punishment effect (t-test, $p = .04$). Figure 5 shows these results by subject. This final result, though based on simple comparison of means is in some sense more general than Table 2 results in that it includes game B and game C data. Table 2 used the more "clean" data from game B (where second movers could not also be directly punished) to highlight the importance of history on punishment spillovers, whereas this last result simply shows that the average punishment spillover effect is in the same beneficial direction as the direct punishment effect (i.e., vicarious punishment exists).

Result 7: Vicarious punishment exists, though the effect is not as strong as direct punishment.

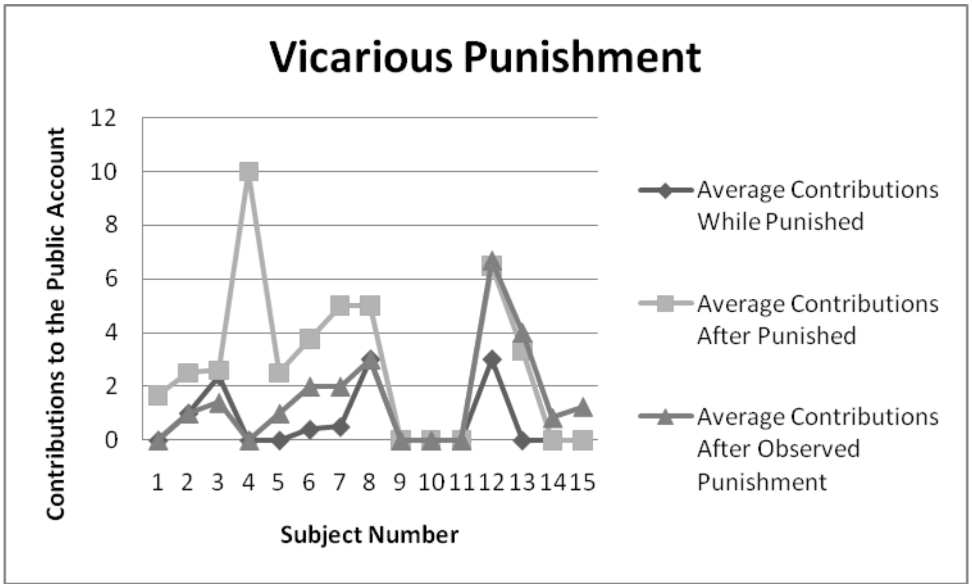

Figure 5: Contributions pre-punishment, post-punishment, and post-vicarious punishment (i.e., following observed OGC punishment)

6. Discussion

Punishment is typically modeled in a reinforcing manner where the effect of punishment reinforces desired behavior in a simple, predictable direction. We show that how punishment affects others is not so simple. The purpose of this study is to provide insight into the broader effectiveness of punishment mechanisms via spillover effects. We add to the prior research by identifying a pure behavioral punishment spillover effect. To do so, we have employed experimental methods to systematically examine what may influence the direction and magnitude of punishment spillover effects. Our design differences include a main treatment where subjects know with certainty that they will not be punished after observance of the same and they also know the exact contributions which lead to external punishment. Understanding how direct punishment effects differ from punishment spillover effects is important for punishment mechanism design, as is a better understanding of when punishment has its intended deterrent effect or not. A main result that emerges from our data is that the immediate response (i.e., periods 1-5 of Game B) of subjects who observe punishment is an adverse response of contributing less to the provision of a public good, but this response is surprisingly reversed when the subject has previous experience with direct punishment mechanisms (i.e., Game C experience). Thus, one's previous history with punishment mechanisms appears to play a key role in determining the direction of the punishment spillover effect.

History of behavior with a direct punishment in place is not the same as actually being punished, so we further examined the data to clarify which component of experience seems to drive this result. The first aspect of experience that seems to matter is if a subject had actually been previously punished. The old adage, "walk a mile in my shoes," seems to ring true in this setting as those who had been punished previously themselves contributed more to the public good when they saw someone else was punished than those that did not observe punishment. In addition to experiencing punishment themselves, we found that a history of seeing someone else being punished, but no history of self punishment, led to a more cooperation behavior when faced with the possibility of one's own punishment. This history of observing others punished seemed to make current punishment more salient and resulted in more cooperative behavior than those subjects who only had a history of self punishment. We also found that the *less* cooperative a subject is initially, the more likely the subject will display vicarious punishment effects later in the game.

A main theme of this paper is that spillover effects of punishment seem more complicated than the effects of direct punishment. These effects should be taken into consideration going forward in both the design of punishment mechanisms and when organizations are considering implementing punishment as a reinforcing tool. Previous experience is of primary concern in understanding these vicarious punishment effects.

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Appendix A: Robustness checks

Table 2a: Main regression re-ran to check for robustness of results

Variable	**Model 3 (Original)**	**Model 3a (exclude session 4)**	**Model 3b (include a dummy for session 4)**
OGC Punished	-.993* (.602)	-1.275** (.619)	-1.003* (.604)
Period	-.175*** (.040)	-.167*** (.045)	-.175*** (.040)
Game ACB	2.065*** (.610)	1.914*** (.760)	1.958*** (.749)
Chat	-.377 (.485)	-.346 (.645)	-.271 (.630)
First Period Contributions	.191** (.087)	.186* (.112)	.185** (.088)
Game ACB * OGC Punished	1.464*** (.543)	1.396*** (.528)	1.477*** (.547)
Chat * OGC Punished	-.366 (.497)	-.155 (.532)	-.371 (.497)
First Period Contr. * OGC Punished	.008 (.129)	.073 (.119)	.011 (.130)

Notes: Standard errors are corrected for clustering at the subject level. Three (***), two (**), and one (*) stars indicate statistical significance at the 1%, 5%, and 10% respectively. In model 3b, the dummy variable for session 4 has a p-value of .726.

Appendix B: Instructions

Experiment Instructions-No Chat Game Order ABC

This is an experiment on the economics of decision making. In addition to your participation fee, you will have the chance to earn money based on your decisions in this experiment. It is extremely important that you put away all materials including external reading material and turn off your cell phones and any other electronic devices. If you have a question, please raise your hand and I or one of my assistants will come by and answer your question privately.

You will be randomly and anonymously assigned to be in either group A or group B. You will remain in this group for the entire experiment. Each person in group A will be matched with another person from group B as his/her "other-group counterpart". So, if you are in group A, you will have a counterpart in group B. Today's experiment will last for 30 periods which are divided into 3 parts of 10 periods. The following instructions are for periods 1 – 10. Prior to the start of period 11, additional instructions will be given.

At the start of each decision period, you will face a decision and will be matched with two people **from your group** (so, if you are in group A, you are matched with two other group A individuals from the decision task, even though you will still have an assigned counterpart in group B). You will be randomly re-matched with a different pair of people for each decision. You will never be told who you are matched with in your group.

Today's experiment will last for 30 periods which are divided into 3 parts of 10 periods. The following instructions are for periods 1 – 10. Prior to the start of period 11, additional instructions will be given.

At the start of each decision period, you will face a decision and will be matched with two people **from your group** (so, if you are in group A, you are matched with two other group A individuals from the decision task, even though you will still have an assigned counterpart in group B). You will be randomly re-matched with a different pair of people for each decision. You will never be told who you are matched with in your group.

In each period, you will be given 10 tokens. Your task is to decide how many tokens to allocate to a group account and how many to allocate to an individual account. You can allocate anywhere from 0 to 10 tokens to each account, but the total allocated to both must sum to 10. (Negative allocations or fractional allocations are not allowed). Each token allocated to the individual account will generate a $0.025 payoff to you and you alone. Each token allocated to the group account, however, will generate $.0125 to you **and** $.0125 to each of the other two members of your group. Similarly, when other members of your group allocate their tokens into their individual accounts, it generates a payoff to that person and no one else. But, for each token another member of your group allocates to the group account, this generates $.0125 for each member of the group, including you. So, your total earnings in each period are equal to $.025 times the number of tokens you allocate to your individual account **plus** $.0125 times the *total* number of tokens you and the other members of your group place in the group account.

Let's go through some examples. Suppose you allocated 5 tokens to the individual account, 5 tokens to the group account and the total in the group account was 12 tokens (implying that the other two members of your group allocated a total of 7 tokens to the group account). In this example, your payoff would be $.025 times the 5 tokens you allocated to your individual account **plus** $.0125 times the 12 total tokens in the group account, for a total payoff of $.025*5 + $.0125*12 = $.28

If, on the other hand, you allocated 10 tokens to the individual account, 0 tokens to the group account and the total in the group account was 12 tokens, your payoff would be $.025 times the 10 tokens you allocated to your individual account **plus** $.0125 cents times the 12 tokens in the group account, for a total payoff of $.025*10 + $.0125*12 = $.40

As a final example, suppose you allocated 0 tokens to your individual account and the total tokens in the group account is 25. In this case, your earnings would be just the $.0125 times the 25 tokens in the group account (since you allocated no tokens to your individual account), for a payoff of $.0125*25 = $.32.

The members of group A will make their decisions first and then the members of group B will make their decisions. After all of the members of group B have made their decisions (i.e., allocations), a screen will be displayed showing the results of the period and your payoff for that period. No one else will see this results screen or how much your earnings are for the period.

<u>Summary</u>: You will need to decide how many of your 10 tokens to allocate to your group and individual accounts in each decision period. Your total payoffs in each period are equal to $.025 times the number of tokens you allocate to your individual account **plus** $.0125 times the total number of tokens you and the other group members allocate to the group account. The members of this group will be re-randomized after every decision period.

These instructions are for periods 11-20. Additional instructions will be given prior to the start of period 21.

The task is similar in this part except ***both members of group A and group B have a 50% chance of being punished $.125*** if they do not allocate at least 5 tokens to the group account. For instance, if you allocated 6 tokens to the individual account and 4 tokens to the group account, there is a 50% chance you will lose $0.125. This chance can be thought of like flipping a coin. If you allocated less than 5 tokens and the computer flips a coin and it is heads, you lose $.125, if it is tails, then nothing is subtracted from your payoff. On the other hand, if you allocate 5 or more tokens to the group account, there is no possibility of being punished. (note: whether punished or not, you would still earn $.025 for each token in you individual account plus $.0125 times the total number of tokens in the group account. Punishment, if it occurs, would simply subtract $.125 from your payoff for that decision period).

Again, at the start of each period, you will be matched with two people from your group for the task. In addition to this, each person in group A will be matched with the same person from group B as his/her "other-group counterpart". So, if you are in group A, you will have a counterpart in group B. You keep the same counterpart from the other group for decision periods 11-20. Each decision period will be the same in that group A subjects will make their allocations first. Following group A allocations, group B will be shown if their counterpart in group A was punished or not (but *not* what their counterpart's exact allocation choice was) and how many times they have been punished. After all of the members of group B have made their decisions, a results screen will be displayed showing the results of the decision period. This is repeated for 10 periods. The only thing that is changed for this part is that members of **both** group A and group B face the possibility of getting punished if placing less than 5 tokens in the group account and subjects in group B will observe if their group A counterpart was punished or not and how many times they have been punished.

Experiment Instructions-Chat Game Order ACB

This is an experiment on the economics of decision making. In addition to your participation fee, you will have the chance to earn money based on your decisions in this experiment. It is extremely important that you put away all materials including external reading material and turn off your cell phones and any other electronic devices. If you have a question, please raise your hand and I or one of my assistants will come by and answer your question privately.

You will be randomly and anonymously assigned to be in either group A or group B. You will remain in this group for the entire experiment. Each person in group A will be matched with another person from group B as his/her "other-group counterpart". So, if you are in group A, you will have a counterpart in group B.

You will have 5 minutes to chat with your counterpart from Group A or B prior to the beginning of the decision experiment. Below we list some suggested questions to give you ideas of things to ask your counterpart. The number on the note card in the upper left corner of your desk will be the same for you and your counterpart from the other group whom you are matched with. Remember, use this time to get to know the other person. At this time you may locate and chat with your counterpart from the opposite group.

1. If you could be any superhero and have super powers, which one would you like to have and why?
2. If you could transport yourself anywhere instantly, where would you go and why?
3. What is one item that you really should throw away, but probably never will?
4. What is your major?
5. What year are you in school?
6. What is your favorite musical act?
7. If they made a movie of your life, what would it be about and which actor would you want to play you?
8. What's your favorite cartoon character, and why?
9. What's the ideal dream job for you?
10. What thought or message would you want to put in a fortune cookie?
11. If you had to give up a favorite food, which would be the most difficult to give up?
12. What is one food you'd never want to taste again?
13. If you won a lottery ticket and had a million dollars, what would you do with it?
14. You've been given access to a time machine. Where and when would you travel to?
15. Mount Rushmore honors four U.S. presidents: Washington, Jefferson, Lincoln, and Roosevelt. If you could add any person to Mount Rushmore, who would you add and why?
16. In your opinion, which animal is the best (or most beautiful) and why?

17. Growing up, what were your favorite toys to play with as a child?

Today's experiment will last for 30 periods which are divided into 3 parts of 10 periods. The following instructions are for periods 1 – 10. Prior to the start of period 11, additional instructions will be given.

At the start of each decision period, you will face a decision and will be matched with two people **from your group** (so, if you are in group A, you are matched with two other group A individuals from the decision task, even though you will still have an assigned counterpart in group B). You will be randomly re-matched with a different pair of people for each decision. You will never be told who you are matched with in your group.

In each period, you will be given 10 tokens. Your task is to decide how many tokens to allocate to a group account and how many to allocate to an individual account. You can allocate anywhere from 0 to 10 tokens to each account, but the total allocated to both must sum to 10. (Negative allocations or fractional allocations are not allowed). Each token allocated to the individual account will generate a $0.025 payoff to you and you alone. Each token allocated to the group account, however, will generate $.0125 to you **and** $.0125 to each of the other two members of your group. Similarly, when other members of your group allocate their tokens into their individual accounts, it generates a payoff to that person and no one else. But, for each token another member of your group allocates to the group account, this generates $.0125 for each member of the group, including you. So, your total earnings in each period are equal to $.025 times the number of tokens you allocate to your individual account **plus** $.0125 times the *total* number of tokens you and the other members of your group place in the group account.

Let's go through some examples. Suppose you allocated 5 tokens to the individual account, 5 tokens to the group account and the total in the group account was 12 tokens (implying that the other two members of your group allocated a total of 7 tokens to the group account). In this example, your payoff would be $.025 times the 5 tokens you allocated to your individual account **plus** $.0125 times the 12 total tokens in the group account, for a total payoff of $.025*5 + $.0125*12 = $.28

If, on the other hand, you allocated 10 tokens to the individual account, 0 tokens to the group account and the total in the group account was 12 tokens, your payoff would be $.025 times the 10 tokens you allocated to your individual account **plus** $.0125 cents times the 12 tokens in the group account, for a total payoff of $.025*10 + $.0125*12 = $.40

As a final example, suppose you allocated 0 tokens to your individual account and the total tokens in the group account is 25. In this case, your earnings would be just the $.0125 times the 25 tokens in the group account (since you allocated no tokens to your individual account), for a payoff of $.0125*25 = $.32.

The members of group A will make their decisions first and then the members of group B will make their decisions. After all of the members of group B have made their decisions (i.e., allocations), a screen will be displayed showing the results of the period and your payoff for that period. No one else will see this results screen or how much your earnings are for the period.

Summary: You will need to decide how many of your 10 tokens to allocate to your group and individual accounts in each decision period. Your total payoffs in each period are equal to $.025 times the number of tokens you allocate to your individual account **plus** $.0125 times the total number of tokens you and the other group members allocate to the group account. The members of this group will be re-randomized after every decision period.

These instructions are for periods 11-20. Additional instructions will be given prior to the start of period 21.

The task is similar in this part except ***both members of group A and group B have a 50% chance of being punished $.125*** if they do not allocate at least 5 tokens to the group account. For instance, if you allocated 6 tokens to the individual account and 4 tokens to the group account, there is a 50% chance you will lose $0.125. This chance can be thought of like flipping a coin. If you allocated less than 5 tokens and the computer flips a coin and it is heads, you lose $.125, if it is tails, then nothing is subtracted from your payoff. On the other hand, if you allocate 5 or more tokens to the group account, there is no possibility of being punished. (note: whether punished or not, you would still earn $.025 for each token in you individual account plus $.0125 times the total number of tokens in the group account. Punishment, if it occurs, would simply subtract $.125 from your payoff for that decision period).

Again, at the start of each period, you will be matched with two people from your group for the task. In addition to this, each person in group A will be matched with the same person from group B as his/her "other-group counterpart". So, if you are in group A, you will have a counterpart in group B. You keep the same counterpart from the other group for decision periods 11-20. Each decision period will be the same in that group A subjects will make their allocations first. Following group A allocations, group B will be shown if their counterpart in group A was punished or not (but *not* what their counterpart's exact allocation choice was) and how many times they have been punished. After all of the members of group B have made their decisions, a results screen will be displayed showing the results of the decision period. This is repeated for 10 periods. The only thing that is changed for this part is that members of **both** group A and group B face the possibility of getting punished if placing less than 5 tokens in the group account and subjects in group B will observe if their group A counterpart was punished or not and how many times they have been punished.

These instructions are for periods 21-30.

The task and payoffs in this set of decision periods is similar to before except that ***if you are in group A there is a 50% chance you will be punished $.125*** if you do not allocate at least 5 tokens to the group account. As a reminder, if you are in group A and you allocated 6 tokens to the individual account and 4 tokens to the group account, there is a 50% chance you will lose $0.125. This chance can be thought of like flipping a coin. If you allocated less than 5 tokens and the computer flips a coin and it is heads, you lose $.125, if it is tails, then nothing is subtracted from your payoff. On the other hand, if you allocate 5 or more tokens to the group account, there is no possibility of being punished. (note: whether punished or not, you would still earn $.025 for each token in you individual account plus $.0125 times the total number of tokens in the group account. Punishment, if it occurs, would simply subtract $.125 from your payoff for that decision period). ***If you are in group B***, ***you will not be punished, no matter what your allocation of tokens is.***

Again, at the start of each period, you will be randomly matched with two people from your group for the decision task. In addition to this, each person in group A will be matched with someone from group B as his/her "other-group counterpart". So, if you are in group A, you will have a counterpart in group B. You keep the same counterpart from the other group that you had previously for decision periods 21-30. Each decision period will be the same in that group A subjects will make their allocations first. Following group A allocations, group B will be shown if their counterpart in group A was punished or not (but *not* what their counterpart's exact allocation choice was) and how many times they have been punished**.** After seeing this information, group B subjects will make their allocation decisions. After all of the members of group B have made their decisions, a results screen will be displayed showing the results of the decision period. This will be repeated for 10 periods. The only thing that is different in these periods compared to periods 11-20 is that members of group A face the possibility of getting punished if placing less than 5 tokens in the group account and their counterparts from group B will observe if their group A counterpart was punished or not and how many times they have been punished before they make their own allocation decisions (and group B subjects will not face any possibility of being punished, regardless of their allocation decision).

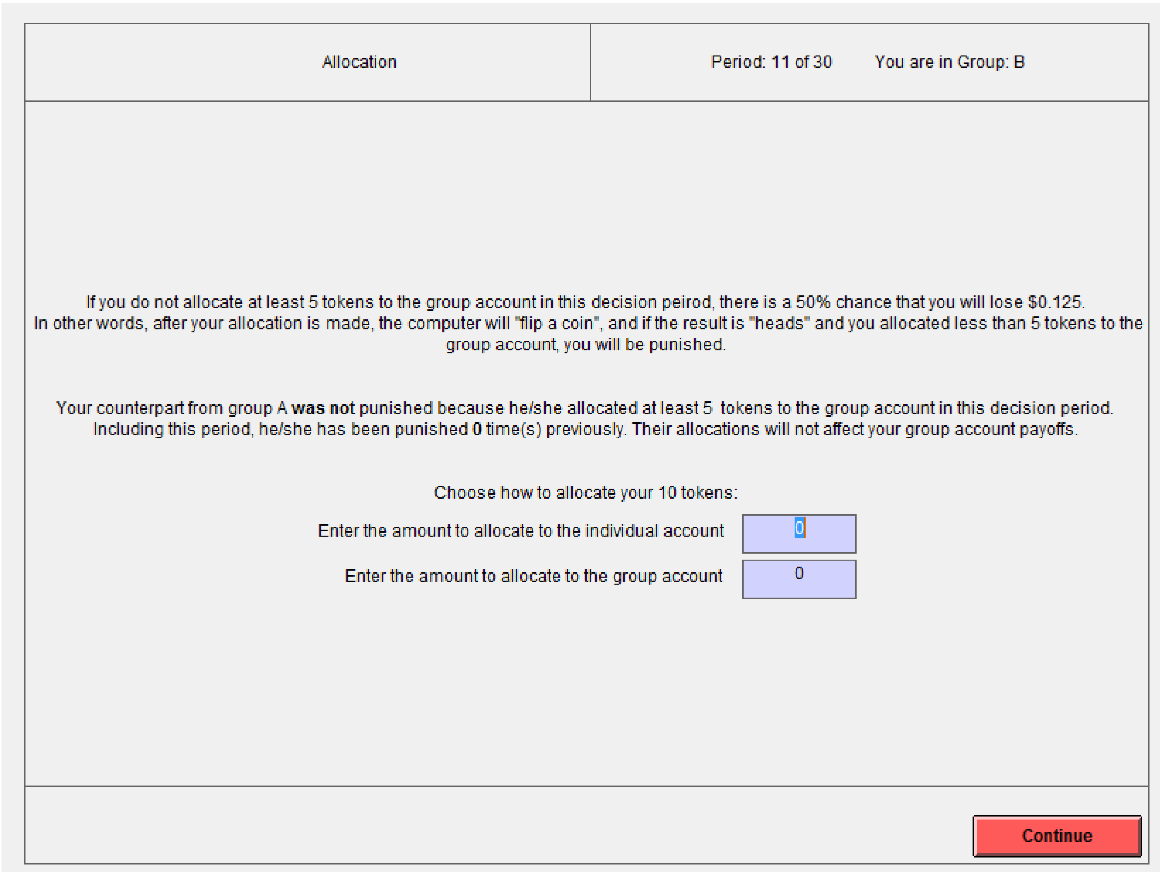
These instructions are for periods 21-30.

The task and payoffs in this set of decision periods is similar to before except that ***if you are in group A there is a 50% chance you will be punished $.125*** if you do not allocate at least 5 tokens to the group account. As a reminder, if you are in group A and you allocated 6 tokens to the individual account and 4 tokens to the group account, there is a 50% chance you will lose $0.125. This chance can be thought of like flipping a coin. If you allocated less than 5 tokens and the computer flips a coin and it is heads, you lose $.125, if it is tails, then nothing is subtracted from your payoff. On the other hand, if you allocate 5 or more tokens to the group account, there is no possibility of being punished. (note: whether punished or not, you would still earn $.025 for each token in you individual account plus $.0125 times the total number of tokens in the group account. Punishment, if it occurs, would simply subtract $.125 from your payoff for that decision period). ***If you are in group B***, ***you will not be punished, no matter what your allocation of tokens is.***

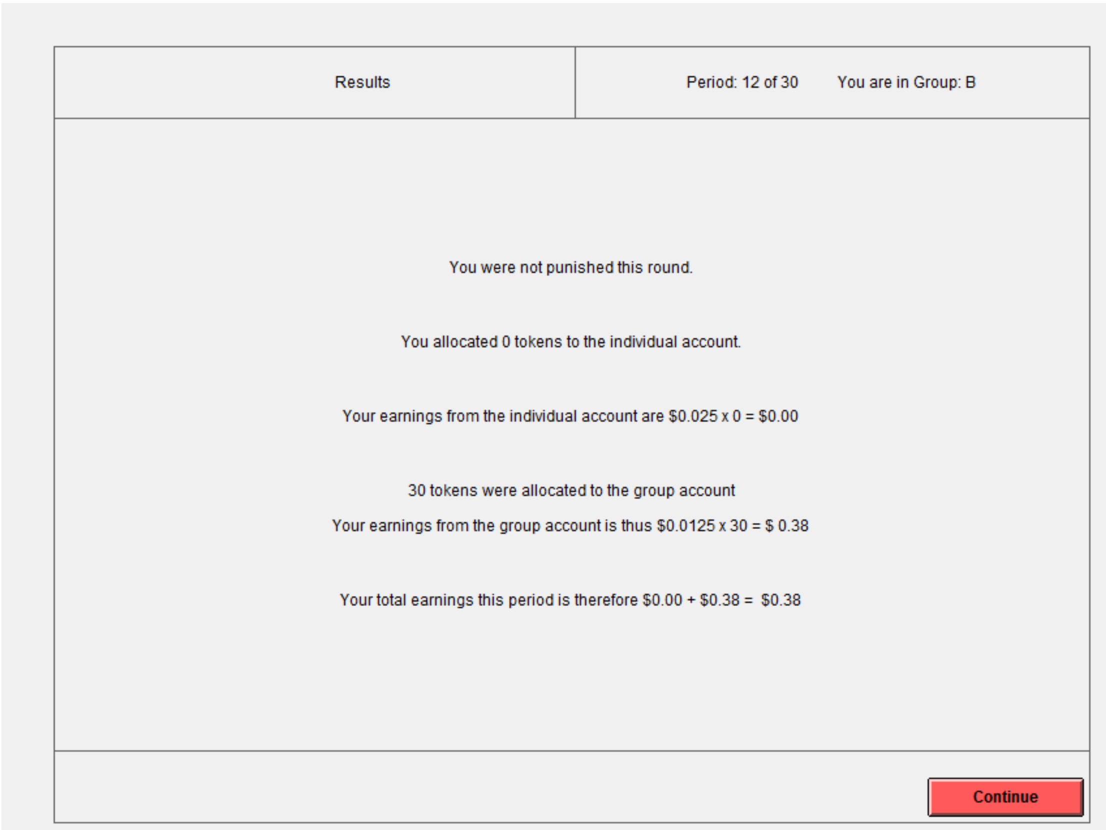
Again, at the start of each period, you will be randomly matched with two people from your group for the decision task. In addition to this, each person in group A will be matched with someone from group B as his/her "other-group counterpart". So, if you are in group A, you will have a counterpart in group B. You keep the same counterpart from the other group that you had previously for decision periods 21-30. Each decision period will be the same in that group A subjects will make their allocations first. Following group A allocations, group B will be shown if their counterpart in group A was punished or not (but *not* what their counterpart's exact allocation choice was) and how many times they have been punished**.** After seeing this information, group B subjects will make their allocation decisions. After all of the members of group B have made their decisions, a results screen will be displayed showing the results of the decision period. This will be repeated for 10 periods. The only thing that is different in these periods compared to periods 11-20 is that members of group A face the possibility of getting punished if placing less than 5 tokens in the group account and their counterparts from group B will observe if their group A counterpart was punished or not and how many times they have been punished before they make their own allocation decisions (and group B subjects will not face any possibility of being punished, regardless of their allocation decision).

Appendix C: Screenshots

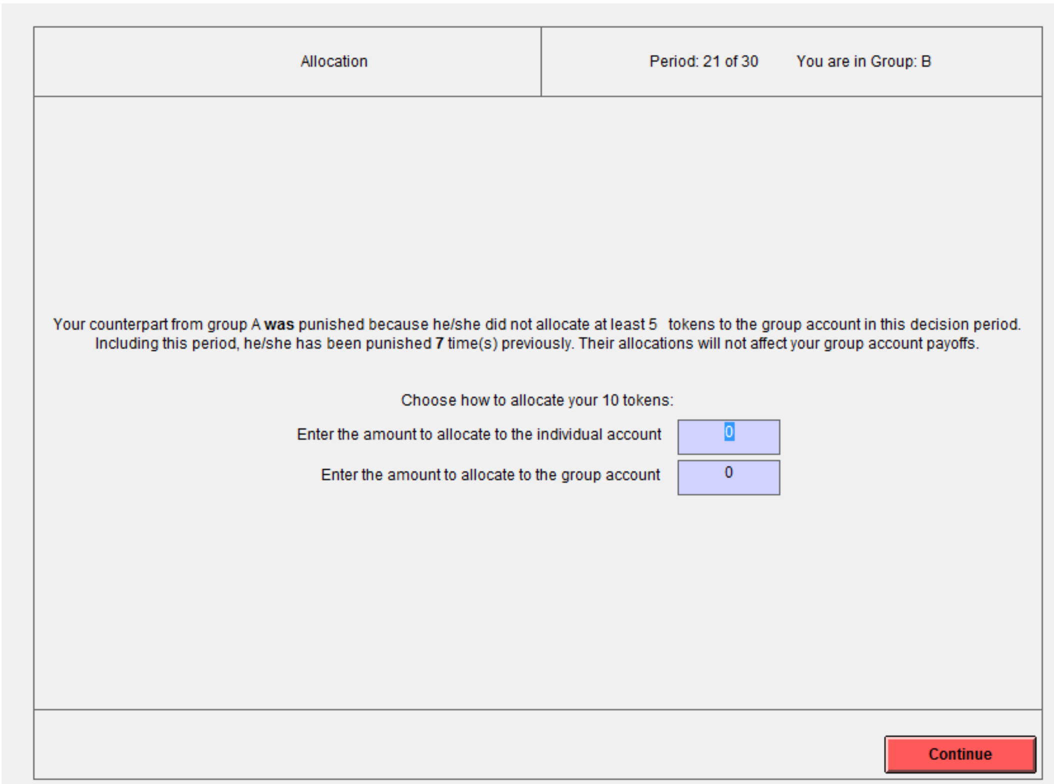
A decision screen of a subject in group 2 in Game C



The results screen of a subject in group 2 for Game C



A decision screen of a subject in group 2 in Game B



The results screen of a subject in group 2 for Game B