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Monitoring and punishment networks in an experimental common pool resource dilemma

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Abstract

With the aid of a lab experiment, we explored how imperfect monitoring and punishment networks impacted appropriation, punishment and beliefs in a common pool resource appropriation dilemma. We studied the differences between the complete network (with perfect monitoring and punishment, such that everyone can observe and punish everyone else) and two "imperfect" networks (that systematically reduce the number of subjects who could monitor and punish others): the directed and undirected circle networks. We found that free riders were punished in all treatments, but the network topology impacted the type of punishment: the undirected circle induced more severe punishment and prosocial punishment compared to the other two networks. Both imperfect networks were more efficient because the larger punishment capacity available in the complete network elicited higher punishment amount.

1. Introduction

There is a growing consensus that the rapid depletion and collapse of many shared common pool resources (CPRs) has ushered in the sixth mass extinction event. Peer monitoring and punishment has been highlighted as an important means to mitigate a "tragedy of the commons", given the evidence that participants in both laboratory and field settings tend to punish free riders (Ostrom *et al*., 1994; Fehr and Gächter, 2000). This tendency is often called 'prosocial punishment' to reflect individuals' willingness to incur personal costs to discipline free riders even in one-shot interactions (Herrmann *et al*., 2008). This runs counter to predictions based on standard assumptions of payoff maximizing self-interested agents, which entail the emergence of a second-order dilemma arising from a failure to engage in such a form of self-governance. A closer look at the experimental evidence, however, shows that past experimental studies have focused primarily on perfect peer monitoring, where everyone can monitor and punish everyone else appropriating from the shared CPR. The extant data do not reflect the incredible diversity of self-governance arrangements documented so far, of which imperfect monitoring and punishment institutions are an important part. Appropriators' opportunities to monitor and punish are frequently restricted due to biophysical and/or socio-political barriers. Some examples include imperfect monitoring and punishment arrangements around downstream irrigation systems, recreational fishing grounds and hunting wildlife, state boundaries, dykes and other walled structures, and exclusionary social categories like gender (Leader-Williams and Milner-Gulland, 1993; Ostrom *et al*., 1994; Agarwal, 2001; Henrich *et al*., 2006; Ostrom, 2010).

Our main contribution is to examine experimentally how differences in structural features of imperfect peer monitoring and punishment institutions impact cooperation in a nonlinear CPR dilemma. To this end, we systematically varied the following two features: the number of peers that an individual can monitor and punish in a given group, and whether the

relationship between two individuals is symmetric (so that they can simultaneously monitor and punish one another). Specifically, we performed a lab experiment in which we assigned individuals to one of three types of networks which exogenously varied their ability to monitor and punish their neighbors in a one-shot nonlinear CPR dilemma: the complete network (CN), and the imperfect undirected and directed circle networks (UCN and DCN respectively). This allowed us to examine the causal role of two key structural properties: network density and directedness. Network density specifies the number of agents that an individual can monitor and punish in relation to the total number of individuals in the group (the number of existing ties divided by the number of possible ties). Directedness instead specifies whether the monitoring and punishment relationship between two agents is asymmetric (a unidirectional relationship between individuals such that each tie points from the monitor to the appropriator being monitored and, possibly, punished). The choice of UCN and DCN was also supported by the field observation that, while some agents may be temporarily or permanently isolated from others due to for instance geographical, political or socio-cultural constraints, they generally cannot be excluded from the benefits of the CPR.

The CN has the highest network density because all potential relationships between group members are actually realized: everyone can punish all other players since each individual is connected to everyone else. In such dense networks, it is plausible that agents will be able to deter over-appropriation more effectively provided that individuals in this treatment take advantage of the larger combined punishment capacity. However, this need not be the case; in fact, the opposite may happen if punishers struggle to coordinate. That is, denser networks may be less effective at deterring non-cooperative behavior if the threat of being punished diminishes as the number of potential targets increases and the coordination of prosocial punishment becomes more difficult. In addition, the increasing number of potential targets to sanction may reduce the severity of assigned sanctions. Conversely, incomplete and

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less dense networks like UCN may present a relatively clearer picture of the roles of individuals located at different nodes (and of the ensuing responsibility for deterring freeriding). Such monitoring and punishment roles are arguably clearer in the DCN, because the network structure specifies a single individual who can punish one target; by removing other potential punishers from the decision setting, this eliminates any ambiguity about the individual's responsibility.[1]

Importantly, network density and directedness delineate the "burden of responsibility" in different ways across appropriators in the CN, UCN and DCN. Research from both social psychology and experimental economics suggests that this element of responsibility is key, especially in the context of one-shot interactions. In social psychology, the "bystander effect" consistently documents how the increased availability of other potential helpers – who are strangers – decreases the likelihood of helping across different settings (Fischer *et al.*, 2011). To illustrate, Cryder and Loewenstein (2012) found that this burden of responsibility impacts prosocial choices in experimental social dilemmas: participants were three times more likely to donate money to others when they were solely responsible for a recipient's outcome than when this responsibility was shared with others.

Similarly, experimental economics research on "moral wiggle room" also points to the importance of clearly defined rules between the giver, recipient, and payouts in influencing cooperation, primarily in dictator games (Dana *et al.*, 2007). Dictators have no "wiggle room" to interpret selfish behavior favorably when selfish dictators are easily recognized by recipients. But generous behavior decreases when responsibility is clouded, for instance when

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[1] Evidence from natural resource governance, primarily from qualitative and correlational field studies, suggests that network properties impact cooperation in CPR dilemmas but remain similarly inconclusive as to exactly how (Bodin and Crona, 2009). For instance, King (2000) found that dense social ties amongst Kenyan fisherman positively impact cooperation as they were better able to manage over-appropriation of local fisheries. But Bodin and Crona (2008) found that fishers in another Kenyan village were reluctant to call out rule-breaking and had few institutions to overcome overexploitation of local fisheries, suggesting dense social ties have a null or negative effect.

recipients do not know the range of payout or dictators can choose to randomly determine selfish versus fair payouts (Dana *et al*., 2007). While this finding has been replicated several times in the dictator game context, mixed results from other strategic settings question how generalizable these findings are (Lind *et al*., 2019). But taken together, contributions within this strand of literature suggest that when the burden of responsibility of acting prosocially is specified clearly to individuals, then cooperative actions are more likely.

Another strand of the experimental economics literature speaks to this question by varying the responsibility for punishment through exogenous changes to the monitoring and punishment network in public goods (PG) games and shows that outcomes are contingent on the network structure (Carpenter *et al*., 2012; Leibbrandt *et al*., 2015; Boosey and Isaac, 2016). For example, Carpenter *et al*. (2012), the closest study to ours, varied the network structure by restricting both monitoring and punishment opportunities in a linear PG contribution game with a strangers-matching protocol. They found that contributions are similar across CN and UCN networks, but that the punishment amount is highest in DCN, followed by UCN and lastly by CN. They argued that the network property of directedness clearly specified the responsibility of assigning punishment from a single agent to another: "more punishment is used to maintain or increase contributions in directed networks as the asymmetry in the relations between any pair of subjects gives different monitoring roles to different subjects, which, in turn, increases punishment expenditures" (Carpenter *et al*., 2012: 95). Leibbrandt *et al*. (2015) held monitoring constant (by giving all subjects perfect feedback about other group member's choices) but varied punishment opportunities and found that the network topology significantly affected contributions and punishment decisions in PG games, but not overall efficiency levels. They argued that this is because the greater the number of people who can punish and be punished, the greater the contributions to the public good and the greater the amount of punishment used in the group. Extant studies, however, provide no direct evidence on how imperfect monitoring

and punishment networks and punishment capacity impact CPR appropriation, punishment behavior and efficiency.

Finally, our study adds to a third strand of literature; that is, the literature on CPR dilemmas, which has largely focused on the benefits of *perfect* peer monitoring and punishment institutions.[2] In order to capture the complex dynamics of CPRs that appropriators typically face in the field, we modelled the CPR dilemma as a nonlinear dilemma, thus following the literature on nonlinear CPR experiments pioneered by Ostrom *et al.* (1992). Table 1 summarizes experimental variables in related investigations.[3] Nearly all nonlinear CPR dilemma experiments without a punishment stage find that appropriation moves towards the Nash allocation following a pulsing pattern (see Ostrom, 2006; Saijo *et al.*, 2017). Ostrom *et al.* (1992) were the first to experimentally explore how adding perfect peer monitoring and sanctioning opportunities affected appropriation. They found that monitoring and sanctioning marginally mitigated over-appropriation relative to a baseline without any punishment, but that the punishment given was variable and the cost of sanctioning reduced payoffs to levels below what was the socially-optimal level when there was no opportunity to communicate with others in the group. Cason and Gangadharan (2015) revisited Ostrom *et al.*'s (1992) appropriation dilemma to confirm findings that peer sanctioning has a positive but limited impact on appropriation, highlighting that net earnings were not significantly different from the no punishment baseline. They noted that subjects may find it more difficult to distinguish

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[2] Individuals take resource units in CPR appropriation dilemmas, whereas PG games model a 'provision' dilemma, where others' contributions generate a positive supply externality because the good is non-excludable and non-rival (Ostrom, 2006) and there have been discussions about whether patterns of cooperative behavior between CPR and PG games are distinct (Apesteguia and Maier-Rigaud, 2006; Kingsley and Liu, 2014; Cason and Gangadharan, 2015). See Cardenas (2000), Ostrom (2006), Poteete *et al.* (2010) and Anderies *et al.* (2011) for a review of CPR experiments in the lab and field, and Sturm and Weimann (2006) and Noussair and van Soest (2014) for a review CPR experiments alongside other environmental dilemmas. Chaudhuri (2011) surveys recent PG experiments.

[3] Some studies have not been included in this review either because they have a linear (rather than a nonlinear) payoff structure or because the experimental design is too different from ours (Velez *et al.*, 2009; Cardenas, 2011; Isaksen *et al.*, 2019). Furthermore, we have omitted those which focus on other types of sanctioning via external authorities, sanctioning outsiders or using rewards instead (Vyrastekova and van Soest, 2008; De Geest *et al.*, 2017; De Geest and Stranlund, 2019).

prosocial from antisocial behavior since nonlinear payoffs push the social optimum and Nash equilibrium closer together. They also suggested that curtailing freeriding can be more challenging because the gains from defection are larger in the nonlinear CPR environment (relative to linear PG games). When replicating Cason and Gangadharan (2015), Kingsley (2015) confirmed that punishment alone does not improve welfare in the CPR game despite increasing cooperation.

{Table 1 about here}

Our experiment featured a nonlinear CPR dilemma similar to that of Ostrom *et al*. (1992), Cason and Gangadharan (2015) and Kingsley (2015). As discussed above, a consistent finding in this literature is that perfect institutions may improve cooperation but not necessarily efficiency. Based on this premise, and given that our main aim was to test the effect of "imperfect" institutions that systematically reduce the ability to observe and punish peers (and thereby the burden and cost of acting prosocially), we chose the case of perfect peer monitoring and punishment as the comparison group. This design choice is similar to Boosey and Isaac (2016) and Carpenter *et al*. (2012) who also keep perfect monitoring and punishment as their control. However, since experiments with no monitoring and punishment represent an important baseline for comparison, we will also discuss our findings in relation to earlier CPR experiments without punishment (in the results section 3), while recognizing the many design differences (as noted in table 1). For instance, compared to prior CPR experiments that largely use a partner-matching protocol, we used stranger-matching to address potential reputation effects (discussed in section 2).

Another important design feature is the type of punishment used in this experiment. Compressing monitoring and punishment opportunities into the network structure is similar in spirit to institutions studied in Casari and Plott (2003) and De Geest *et al*. (2017). In Casari and Plott's (2003) CPR experiment, an agent's choice to inspect her peers simultaneously revealed

both the target's appropriation choice and sanctioning exacted, with an important distinction being that sanctioning is automatically assigned under certain conditions. De Geest *et al.* (2017) studied the impact of imperfect monitoring and punishment between two groups of CPR users instead, where "insiders" could simultaneously observe and sanction none, all or a subset of the "outsiders" or "poachers". While they observed little variation in appropriation, they found that insiders were unable to coordinate their sanctions to fully deter the outsiders in the partial and perfect monitoring and punishment treatments as their sanctions were too low and they were unwilling to punish low levels of poaching. We focused on how imperfect opportunities to monitor and punish fellow appropriators affect the management of the CPR.

Lastly, we also elicited information on beliefs that players hold about others' expected appropriations. Eliciting beliefs may induce subjects to think through their decisions more carefully and is thereby one means to address the previously voiced concern about the considerable confusion that may arise from the complexity of nonlinear environments noted by Cason and Gangadharan (2015). Extant experiments that elicit beliefs over others' contributions in PG games (Croson, 2000; Neugebauer *et al.*, 2009; Fischbacher and Gächter, 2010; Gächter and Renner, 2010; Smith, 2013) or expected appropriation in CPR games (Velez *et al.*, 2009), consistently finds that people's stated beliefs are 'optimistic' as they underestimate others' appropriations (or overestimate contributions). We examine whether beliefs vary due to the monitoring and punishment network structure, a question which is previously unexplored in the literature to the best of our knowledge.[4]

We find that while appropriations are similar across types of networks, differences in punishment choices arise from the network structure, which in turn impact efficiency. More

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[4] Whether eliciting beliefs affects cooperation in this nonlinear CPR setting is an open question. Past work has found mixed evidence ranging from either having no effect on cooperation in linear voluntary contribution PG games (Wilcox and Feltovich, 2000), a negative effect through inducing more self-interested behavior (Croson, 2000) or a positive effect through inducing behavior closer to the socially-optimal level (Gächter and Renner, 2010).

precisely, we found that free riders were punished in all treatments, in line with past studies. However, we also found that the network impacted the type of punishment realized: UCN attracted more severe punishment and more frequent prosocial punishment compared to CN and DCN. Overall this suggests that the effect of making the "burden of responsibility" clearer by reducing the network density or number of potential punishers can depend on whether the network is directed or if pairs have asymmetric monitoring and punishment relations. In addition, imperfect monitoring and punishment networks are more efficient because the larger punishment capacity available in CN elicited more punishment overall. Finally, while individuals underestimated others' appropriation from the commons in all treatments, they were more optimistic in the imperfect networks.

The paper proceeds as follows. Section 2 details the experimental game, design, procedures and hypotheses. Section 3 presents the results, and section 4 discusses their implications.

2. Experimental design and hypotheses

2.1 Procedures and overview

The experiment was held at the London School of Economics and Political Science's Behavioural Research Lab from 20-26 November 2015 and was advertised as a study on economic decision-making. Participation was open to registered students; subjects had no previous experience in CPR or network experiments. The experimental game was executed using z-Tree, and subjects interacted anonymously with each other by taking decisions in private (Fischbacher, 2007).

Our design consisted of a between-subjects CPR game with three monitoring and punishment network treatment groups: CN, UCN and DCN. The network treatment and the subject's node type within the network (A, B, C, or D) were held constant throughout each

experimental session. An experimental session consisted of 15 decision-rounds, and this was common knowledge. A stranger-matching procedure was used, where each round started with the computer randomly forming four-person network groups by selecting one subject of each type. Thus, while the node type and the network treatment were fixed in each session, the composition of the network group in each round depended solely upon chance and was independent of group formation in any of the other rounds. We conducted seven sessions with either 16 or 20 subjects, for a total of 172 subjects, where each subject participated only in one session. Since group membership was randomized at the beginning of each round, the independent observations are at the session-level (the number of sessions was three for CN, and two for both UCN and DCN). Table 2 summarizes the experimental design and the number of observations in each network treatment.[5]

{Table 2 about here}

Each round in each session consisted of two stages. In stage one, subjects received an endowment of 25 tokens and decided how to divide it between a common account and a private account. As explained in more detail in the following section, a subject's decision of how much of her endowment to allocate to the common account can be interpreted as a decision about how much to appropriate from the CPR. In stage two, eligible subjects could monitor and assign punishment points to those in their network group. Alongside making an appropriation decision, subjects could also input their beliefs about the expected token placement of the other three group members in the common account in stage one in each round to estimate what their payoffs would be.

The network structure and payoff function, parameter values, punishment rules and

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[5] The original experiment included an additional treatment called the Directed Line Network where the network was similar to the Directed Circle Network except that one link was missing. For reasons related to manuscript length and limited comparability with the other treatments, it is not included in this paper. The interested reader can find the design and results in Shreedhar *et al*. (2018).

number of rounds were common knowledge. Experimental tokens were converted to British pounds (GBP) at the end of the experiment at the rate of 25 experimental tokens = GBP 1, and average earnings were around GBP 7.5.

2.2 The common pool game with networks

Let e be the total amount of tokens available to each subject (i.e., a subject's initial endowment). As previously mentioned, in the first stage subjects decide how many tokens from their initial endowment to allocate between a common account and a private account, where allocation to the private account is assumed to yield a constant marginal return w. We denote by $x_i \in [0, e]$ the amount that individual i allocates to the common account. Consistent with the CPR literature, we interpret x_i as the appropriation choice of subject i; the total appropriation of all subjects within the network group is given by $X = \sum_i x_i$.

The group return from appropriation is $F(X) = aX - bX^2$. This nonlinear payoff structure is commonly used in the literature and aims to approximate the complexity arising from socio-ecological dynamics that agents typically face in the field (Ostrom *et al.* 1992). The return from appropriation to an individual agent is proportional to the ratio between her appropriation effort (x_i) and the total effort of all appropriators (X). The ratio x_i/X is referred to as the 'individual distribution factor' (Apesteguia and Maier-Rigaud, 2006), and captures the negative externality accruing to the group from an individual's appropriation decision: the higher x_i is in relation to X, the higher is i's appropriation from the CPR, allowing an appropriator to capture a larger share of returns from the CPR ($aX - bX^2$). The first-stage payoff of each agent i is given by:

$$\pi_i^1 = w(e - x_i) + \left({x_i}/{X}\right)(aX - bX^2). \tag{1}$$

As per equation (1), subjects made their CPR appropriation decision by dividing tokens between the private and common accounts in stage one, and also stated their expectation about each of the other group members' appropriation decision (instructions and interface snapshots

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are provided in online appendix A and B respectively). At the end of stage one, each subject was informed only of his/her earnings from the common and private account, and of the total earnings from stage one.

In stage two, subjects simultaneously monitor and make punishment decisions regarding their neighbors, depending on the types of networks they are embedded in, as illustrated in figure 1. A tie between two subjects within a network group indicates that they are connected to each other in a monitoring and punishment relationship, and the arrowhead points to the agent whose appropriation can be monitored and punished. For each subject i, N_i denotes her monitoring and punishment neighborhood, i.e., the set of subjects $j \neq i$ who can be monitored by i. Subject i can thus observe how many tokens subjects in N_i placed into their private and group accounts in stage one, and can furthermore assign deduction points to target individuals in the neighborhood. An undirected tie between any two nodes implies that the pair of subjects can monitor and punish each other as in the CN and UCN (panels 1a and 1b). Conversely, a directed tie denotes an asymmetric relationship between two agents, so that only the appropriator towards whom the arrowhead points can be observed and punished but not vice versa, as in the DCN (panel 1c). Thus, if a tie runs from i to j, then i can monitor j ($j \in N_i$) but j cannot monitor i ($i \notin N_j$). To summarize, in the CN all subjects can monitor and punish one another; in the UCN subjects monitor and punish those to whom they are directly connected (e.g., A can monitor and punish B and D, and A is in turn monitored by B and D, but not C); and asymmetric pairwise relationships exist in the DCN because A can only monitor and punish B, B can only monitor and punish C, and so on. In terms of network properties, the only difference between CN and UCN is that the latter has a lower network density (66 per cent for UCN vs 100 per cent for CN) and is incomplete; while the only difference between UCN and DCN is that the latter has directed rather than undirected ties.

{Figure 1 about here}

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Punishing others entails a personal cost which depends on the number of deduction points assigned. Each agent i can punish j in her monitoring and punishment neighborhood ($j \in N_i$) to reduce j's payoff from the first stage by p_j^i at a personal unit cost of $c_a = 1$ per deduction point assigned. Each punishment point received reduces j's payoff by $c_r = 3$. Following recent CPR experiments (table 1), the fee-to-fine ratio – which represents the cost to the punisher from levying one deduction point and the cost to the target – is 1:3, and it is held constant across all network treatments. The maximum amount of punishment points that could be assigned was bounded by the player's earnings in the first stage, thus allowing for a system of graduated sanctions for appropriators determined by the subjective choice of the agent assigning punishment (Fehr and Gächter, 2000). Before proceeding to the next stage, each individual could calculate the total cost of deduction points assigned to others. Equation (2) presents the earnings to each agent from stage two, which could range between 0 and the net earnings from stage one after the cost of assigning and receiving deduction points:

$$\pi_i^2 = \max\left\{0, \ \pi_i^1 - c_a \sum_{j \in N_i} p_j^i - c_r \sum_{i \in N_j} p_i^j\right\}. \tag{2}$$

At the end of stage two, each subject was informed only of his/her total earnings from stage one, the total cost of punishment points given to and received by others, and net earnings from stage two.

Let us consider the standard prediction for this CPR game. If we assume that subjects are rational self-interested agents who choose to maximize their earnings in each round and apply the logic of backward induction, each individual will not issue punishment points in stage two since it is costly to do so ($p_i^j = 0$, for all i and $j \in N_i$). Consequently, subjects will recognize that nobody will punish in this second stage and choose to appropriate at the socially-inefficient level in stage one in all types of network groups. Thus the standard prediction is that the availability of peer monitoring and punishment in stage two cannot deter free riding in stage one.

Unlike standard treatment comparisons that change only one characteristic at a time, the comparisons of this study necessitate changes in both information and punishment to reflect the underlying change in the type of network architectures studied here. However, as noted in table 1, all institutional features were kept fixed across all treatments, including the use of $n = 4$ individuals per network group, 15 periods of play, and a strategy space of $\{0, 1, \ldots, 24, 25\}$ tokens to be allocated to the common and/or the private account. Parameter values were chosen to ensure a good spread in values between the Nash and Pareto equilibrium allocations – 16 and 10 tokens which will yield a payoff of 189 and 225 tokens respectively – while keeping the ratio of Nash to Pareto equilibrium strategies (which determines the size of the negative externality from appropriation) comparable to recent CPR experiments in the literature (table C1 in the online appendix compares parameters to past studies).

The experimental data may show that repeated interactions can influence play across time, which can arise from subjects learning about the rules of the game, or about the preferences or 'types' of subjects in each session, as well as from strategic play during the early rounds in the game. Since the analysis is for one-shot games, we used a stranger-matching design to balance the need for repeated experience by subjects while attempting to reduce reputation effects (e.g., as in Kreps *et al*., 1982). We followed Andreoni and Croson's (2008) suggestion that it is more prudent to use a stranger-matching design when studying single-shot games.[6] While perfect stranger-matching could mitigate investments in reputations, only five

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[6] For instance, Andreoni and Croson (2008) review this literature to show that strangers-matching design may induce more variability in decision-making in PG games; for example Croson (1996) finds partners-matching induces more cooperation in a linear PG game, whereas Andreoni (1988) finds that it is strangers (also see Cox and Stoddard, 2015). Fehr and Gächter (2000) found relative to a no punishment control group, that stranger-matching induces no stable contribution standard whereas partner-matching does for groups with perfect peer punishment (punishment patterns were similar); but Casari (2005) notes that it is unclear how generalizable these findings are given the variable fee-to-fine ratio used. The strangers-matching protocol also allows us to check the evolution of cooperative behavior when appropriation from the commons takes the form of a series of one-shot or infrequent encounters between appropriators. However, a potential disadvantage of not using the alternative partners-matching protocol is that it may not capture the essence of more conventionally conceptualized commons dilemmas where individuals have stable positions within a fixed group over time (Ostrom *et al*., 1994; Ostrom, 2006).

rounds could be played with this protocol due to constraints like lab capacity, and this is arguably too short a period to allow for learning of the rules of the game. Moreover, past studies have not clearly established large differences in outcomes between stranger and perfect-stranger-matching (Fehr and Gächter, 2000, 2002). We tried to address these concerns, however, by first anonymizing an individual's identity while displaying their decisions in stage two (barring their node type) on the computer screen. Secondly, the probability of being in the same group in the following period was small (about 0.25 (0.5) per cent for 20 (16) subjects per session), also helping to mitigate strategic play in earlier periods of the session.

Finally, we paid participants for one randomly selected round of the 15 rounds played at the end of the game to mitigate strategic play in the early rounds and potential wealth effects. The round was selected at random by drawing one of 15 numbered ping pong balls at the end of the experiment. After this, subjects were paid their earnings from the CPR game, and the potential earnings from their stated expectation about the appropriation of others in the group for that round (if it matched the actual choices that the others made).[7] Since it was possible to receive a negative payoff in a given period, we instituted a rule that if players received negative payoffs in the round selected for payment, they would obtain no payment (we did not invoke this rule in any instance of payment).

Much care was taken to ensure that subjects understood the rules of the experiment and the incentive structure. Printed instruction booklets, with detailed explanations of the network

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[7] To encourage individuals to state their expectations of what each of the three other group members would place in the common account in the randomly selected payment round, each player could earn four tokens for each correct guess. Individuals were asked state their expectations of others' choices in exactly the same way in all network treatment groups. We choose to pay people if their expectations were correct because previous studies suggest that belief accuracy is higher, i.e. stated beliefs about other players' choices correspond more closely to their actual choices, when beliefs are incentivized (Gächter and Renner, 2010). There is mixed evidence as to whether or not hedging plays a role in economic experiments. We used low stakes as a precautionary measure against the possibility of hedging (Blanco *et al*., 2010; Schlag *et al*., 2015), but we cannot rule out that incentivizing beliefs impacts behavior differently from other designs. In addition to the payoffs table included in the instruction booklet, subjects could use a calculator feature on the stage one decision page to estimate their payoffs to different token allocations placed by other group members.

structure and the CPR game, were placed at the computer terminals. The framing of instructions was maintained consistent across all treatments, similar to that in the literature in trying to avoid value-laden language. For example, the common pool appropriation decision was framed in terms of allocating tokens to a common account. The punishment decision was framed as the assignment of deduction points to the other people in the network group.[8] Apart from numerical examples to illustrate payoffs within the instruction text, an earnings table (see appendix A) was also displayed showing potential payoffs in tokens corresponding to different token placements in the common account by the subject and total tokens placed by the three others in the group to aid comprehension, similar to the earnings table used in Apesteguia and Maier-Rigaud (2006). To further familiarize subjects with the incentive structure and rules, subjects first read the instructions by themselves at the start of the session, and the instructions were then read aloud by the experimenter. After this, subjects answered trial questions at the end of the instruction booklet, which was checked by the experimenter before proceeding with the experiment.[9] Experimental sessions typically lasted for an hour and 15 minutes, with 15 minutes of instructional time.

2.3 Hypotheses

Building upon insights from the existing literature, we formulate hypotheses about whether and how appropriation and punishment outcomes vary across network structures. Specifically, our first hypothesis is

 H1: There is no difference in appropriation across CN, UCN and DCN.

 This hypothesis builds upon results from recent works in the PG game context, which

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[8] Framing can impact cooperation even when the incentive structure is held constant, as seen from recent evidence of framing PG games as an appropriation (or 'take') or provision dilemmas ('give') (Fosgaard *et al*., 2014; Cox and Stoddard, 2015; Frackenpohl *et al*., 2016; Gächter *et al*., 2017). Framing effects are sensitive to design features of the games like the complexity of the game's payoff structure – for instance Isaksen *et al*. (2019) show that contributions to a linear PG are higher if the externality is framed positively as a 'public good' rather than negatively as a 'public bad', but find no framing effects in the nonlinear PG game or the CPR game.
[9] At the end of the instructional period, subjects were asked if they had any questions or any difficulties understanding the experiment. No subject reported any difficulty understanding the procedures.

suggest that average contributions tend to be similar across networks where all players are connected to one another, as is the case with the CN, UCN and DCN (Carpenter *et al.*, 2012; Boosey and Isaac, 2016).

To better understand how the network structure impacts punishment, we consider the "punishment amount", which refers to the total number of punishment points received by an individual in stage two of the CPR game in a given round. In addition, we estimate the "punishment severity" by weighting the average punishment received by the number of potential punishers targeting each subject (denoted by the in-degree). That is, we assign weights of 3, 2 and 1 in the CN, UCN and DCN, respectively. Lastly, we also consider the "punishment incidence", which is whether the subject received any punishment in a given round. In denser networks, like the CN, each individual faces a higher number of potential punishers than in less dense networks; this, in turn, may result in a higher punishment amount being received by each individual, as well as a higher incidence of punishment. On the other hand, less dense networks like UCN, which features fewer punishment opportunities, may increase the severity of punishment allocated per opportunity. How this potential increase in punishment severity affects the amount and the willingness to punish is an open empirical question. Turning to the available experimental evidence, Carpenter *et al.* (2012) found that punishment amounts are similar between CN and UCN networks, but that the punishment amount used in the DCN is higher, possibly because the attribute of directedness specifies clearer monitoring and punishment responsibilities for each subject. Thus, we hypothesize that:

H2a: The amount of punishment and incidence elicited in UCN is at least as large as in CN, but punishment severity is higher in UCN.

H2b: The punishment amount, severity and incidence elicited by DCN is at least as high as in CN and UCN.

Understanding individual-level punishment behavior allows us to examine the types of

punishment given in relation to the appropriation behavior of the target agent. Past studies have highlighted three broad non-standard motivations for punishing peers, namely: (i) prosocial inclinations to punish free riders, (ii) antisocial punishment to penalize cooperators, and (iii) punishment due to errors (Ostrom *et al*., 1992; Herrmann *et al*., 2008; Casari and Luini, 2012). When considering prosocial punishment, past studies indicate that punishing free riding is generally triggered by negative emotions that arise from a violation of either reciprocity or fairness norms, as well as from feeling exploited (being the 'sucker') (Ostrom *et al*., 1994; Fehr and Schmidt, 2001; Frey and Meier, 2004; Fehr *et al*., 2005). Ostrom *et al*. (1992) further differentiated between one-round prosocial punishment received because the person fined was the highest (or one of the highest) appropriator in the current round, and lagged punishment where the punished person was one of the highest appropriators in the prior round. Regarding antisocial punishment, they noted the occurrence of blind revenge, which is targeted at low appropriators by those who received punishment in a previous round. Other studies also remark on the possibility of targeted revenge aimed at an identifiable punisher when counter-punishment opportunities are available, as well as spiteful or expressive punishment by those who punish because they have a 'hard-wired' taste for it (Nikiforakis, 2008; Jensen, 2010; Casari and Luini, 2012). While our experiment was not designed to differentiate between these motivations, it can nonetheless shed some light on this issue.

To measure whether different network treatments affect prosocial punishment and antisocial punishment, we define them as follows. "Prosocial punishment" is the punishment given to a target agent who allocated more tokens to the common account than the theoretical Pareto optimal benchmark of 10 tokens. "Antisocial punishment", on the other hand, is measured as the punishment given to a target agent who appropriates at or below this Pareto optimal level. Building on Ostrom *et al*. (1992), antisocial punishment is likely to stem from three sources: blind revenge by someone who was a low CPR appropriator in the previous

round; free rider revenge if the punisher was a free rider who appropriated above the Pareto optimal level and was punished in the last round; or other types of antisocial punishment arising from error or a hard-wired taste for punishment. However, unlike in Ostrom *et al.* (1992), we can eliminate the possibility of lagged prosocial punishment (targeting an agent who over-appropriated in the previous round) and targeted antisocial counter-punishment (as a response to being punished by the target in the previous round), since we use a stranger-matching design without a counter-punishment stage where individual players cannot be identified in the subsequent round as their identities are anonymized. Assuming that those types with a hard-wired and vengeful predisposition to punish were evenly distributed across network treatments, ex-ante, we reason as follows.

The past experimental literature on the bystander effect shows that a reduction in the number of possible "helpers" increases prosocial behavior (Cryder and Loewenstein, 2012). This implies that the burden of responsibility is more diffused in the CN due to a larger number of potential punishers who can act prosocially by sanctioning free riders in stage two. The UCN, thus, will offer less ambiguity about an individual's responsibility to punish free riders since they are less densely connected and, arguably, the DCN can offer even more clarity because it is directed. Free riders may be more likely to be punished – especially in the DCN – as it offers unambiguous responsibility to monitor and punish a single agent. Indeed, Carpenter *et al.* (2012) found that free-riders – defined as those who contribute nothing to the public good – are more likely to be punished in DCN relative to the UCN (but that the difference was not statistically significant).

By this logic, it is also possible that the clearer delineation of responsibility afforded by UCN and DCN in this CPR dilemma may also attenuate the extent of antisocial punishment. We remain agnostic about whether the pattern of types of antisocial punishment – namely blind revenge, free rider revenge and residual cases such as punishment by error – differs across

network groups.

H3: There is more prosocial punishment and less antisocial punishment in UCN and DCN than in CN.

3. Results

We report the results in four subsections, corresponding to each outcome, i.e., appropriation, punishment, efficiency (as measured by net payoffs) and beliefs. In each, we consider if outcomes vary across networks by using random-effects panel regression models with robust standard errors, instead of standard statistical tests, due to our experimental design: network treatments assigned at the session level, stranger-matching for each of the 15 rounds to form network groups of four and a payout from one randomly selected round. This model allows us to control for potential session-fixed effects using session dummies and learning using round dummies. We also include individual-level controls for gender, experience in lab sessions, and whether student's main discipline of study was economics.[10] Table 3 reports the summary statistics for the key outcomes by each network treatment for each session pooled across all observations and rounds.

{Table 3 about here}

3.1 Appropriation

When we look at appropriation levels in the CN (in table 2), the average amount of tokens allocated to the common account pooled across all rounds was 61.5 per cent of the endowment or 15.37 tokens. This figure is close to recent findings from previous studies with perfect monitoring and punishment: for example, 59 per cent and 66.4 per cent respectively in Kingsley (2015) and 56.6 per cent and 62 per cent respectively in Cason and Gangadharan (2015). It is

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[10] Of the total sample, 59.35 per cent was female, 16.15 per cent studied economics, and the majority (46.35 per cent) took part in 1-5 experiments and 26 per cent in no experiment previously. Table C2 (in online appendix C) presents attributes by treatment group and table C3 presents summary statistics for the key outcomes by each network treatment for each session pooled across groups.

also comparable to (albeit marginally lower than) recent CPR experiments without any punishment, e.g., 74.8 per cent in Apesteguia and Maier-Rigaud (2006) and 67 per cent in Kingsley and Liu (2014). Turning to the UCN and DCN, we found that appropriation was similar to CN at a little under 15 tokens.

Figure 2 illustrates that the average appropriation for each round begins close to the mid-point of the choice interval in all treatments near the Pareto optimal level of 10 tokens and trends towards the Nash equilibrium across all networks. In line with Ostrom *et al.* (1992) and other recent works, appropriations follow a mild pulsing pattern since allocations to the common account increased and then decreased, with a tendency for the variance to decrease over time. The increase in average appropriation in rounds 8–15 compared to rounds 1–7 is statistically significant only in the imperfect UCN and DCN networks (at 5 per cent and 1 per cent respectively in a random-effects regression with session dummies; results in online appendix table C4).

{Figure 2 about here}

Table 3 reports the results from the random-effects regression models (with session and round fixed effects and controls). Individual-level appropriation is regressed on network treatment dummies (with CN as the omitted category) in model (1), and while the coefficients UCN and DCN are positive, they are not statistically significant. This result confirms that appropriation from the CPR is similar across the perfect (CN) and imperfect (UCN and DCN) networks. In addition, the differences between UCN and DCN are also not statistically significant (Wald test p-value = 0.524 in model 1). Thus, we cannot reject H1.[11]

Result 1: On average, appropriation is not different across CN, UCN and DCN.

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[11] We replicated the results using random-effects Tobit regressions and random-effects regressions with bootstrap standard errors (they are omitted for brevity, but available on request), and random-effects regressions with lagged variables and multilevel regression models with clustering at the session and subject levels (online appendix tables C9 and C10 respectively).

3.2 Punishment

Table 2 shows that the average punishment amount received was the highest in CN (11.7 points), compared to UCN and DCN (6.7 and 2.6 points respectively). This suggests that the higher punishment capacity due to the greater number of opportunities offered by denser networks elicited higher levels of punishment points received on average. When we turn to punishment severity to control for the different punishment capacity offered by each network, the figure shows that CN and UCN seem to elicit marginally higher punishment severity at 3.9 and 3.4 respectively, compared to the DCN, which elicited 2.6. From figure 3(a), the average share of individuals receiving any punishment in a given round is again similar in the CN and UCN at 46 per cent and 44 per cent respectively, and lower in the DCN at 14 per cent. Figures 3(b) and 3(c) display the average punishment amount received and severity by subjects across the three network groups in each round. We see that punishment patterns are erratic in the CN, consistent with previous results from Cason and Gangadharan (2015). Average punishment amount and severity also show uneven patterns in both imperfect networks. The differences in average punishment amount and in severity between rounds 8–15 and rounds 1–7 are not statistically significant in any of the networks, but the likelihood of an individual receiving punishment is marginally lower in all networks in this second period (online appendix table C5).

{Figure 3 about here}

Table 4 checks whether the average amount of punishment points received by individuals, and their severity and incidence, differs across networks. The results show that CN elicits higher punishment amounts than DCN (the difference is significant at 1 per cent) but that the difference with the UCN is not statistically significant. On the other hand, the UCN elicited more severe punishment than the CN (the difference is significant at 5 per cent) and that punishment severity is not different between CN and DCN. The likelihood of receiving

punishment is also higher in the UCN relative to the CN (the odds are around 2.3 times higher with the difference significant at 1 per cent) and lower in the DCN (probability is 0.3 times lower, with the difference significant at 1 per cent). Differences between UCN and DCN coefficients show that punishment amount and incidence is higher in UCN (Wald test p-value < 0.01 from models 2 and 4), but that the difference in punishment severity is not statistically significant (Wald test p-value = 0.12 in model 3).

{Table 4 about here}

When we restricted attention to only those who appropriated over the Pareto optimal level of 10 tokens, we found that the results were qualitatively similar, the main difference being that punishment amount, severity and incidence were all higher (online appendix table C6). These results can be summarized as follows:

Result 2a: The punishment amount elicited by UCN is similar to that in CN, but punishment severity and incidence is higher in UCN than in CN.

Result 2b: The punishment amount and incidence elicited by DCN is lower than in both CN and UCN, but punishment severity is similar to both CN and UCN.

The above results imply that we cannot reject H2a but cannot fully support H2b.

Next, we proceed to analyze individual-level punishment behavior. We first examine if the punishment amount received was influenced by whether the target of the punishment was a free rider, i.e., an agent who allocated a token amount greater than the Pareto optimal level of 10 tokens to the common account. We estimated the difference between the appropriation of a subject in a given round and the Pareto equilibrium allocation of 10 tokens, such that a positive deviation denotes appropriation above the socially-optimal level or free riding, and zero and negative values denote allocations at or below the socially-optimal level. From figure 4, it is clear that individuals engage in prosocial punishment in all networks since those who appropriated more than the socially-optimal appropriation level are punished more severely

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than lower appropriators. In addition to confirming this established finding from the literature (Ostrom *et al*., 1992; Fehr and Gächter, 2000; Cason and Gangadharan, 2015), we also see some antisocial punishment, i.e., punishment targeting those who extract at or less than the Pareto equilibrium, albeit at lower levels (Herrmann *et al*., 2008).

{Figure 4 about here}

In order to better understand individual-level punishment behavior, we examined individual punishment flows between pairs of players to factor in whether the target of punishment was a free rider and the motivations of the punisher. We mapped the frequency of different types of pairwise punishment flows, i.e., the number of times a type of punishment was given between two subjects. Building on Ostrom *et al*. (1992),[12] we first categorized pairwise punishment flows according to no punishment (standard prediction) and prosocial punishment (aimed at those who appropriated above the Pareto optimal level). The remaining instances of punishment were categorized as antisocial punishment since they are aimed at those appropriating at or below the Pareto optimal level. Of these antisocial punishment pairwise flows, we further subdivided punishment flows into three categories: "blind revenge" if the person giving punishment was a low CPR investor who was fined by a person fined in a previous round; "free rider revenge" if the person giving punishment was a free rider who appropriated above the Pareto equilibrium level round in the last round and was punished; and lastly "other" types of antisocial punishment that do not fit these two categories and offer no obvious explanation as they may stem from either error or a hard-wired taste for punishment or some combination of both. To account for the differences in monitoring and punishment

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[12] While Ostrom *et al*. (1992) did not report the share of punishment choices where people chose to assign no punishment, they found that 77 per cent of all sanctioning choices was aimed at those who in the previous round were high appropriators in the CPR, an additional 5 per cent was classified as blind revenge, and 11 per cent as errors. An additional 7 per cent was aimed at subjects who had been heavy investors in the CPR in earlier (but not the most recent) rounds, but as previously discussed, our stranger-matching design rules out this last type of punishment from our game. We are very grateful to our anonymous referees for encouraging us to unpack the types of punishment.

capacity across networks, we considered the shares of each type of punishment given on the basis of the opportunities to give punishment in each network (i.e., three, two and one in CN, UCN and DCN respectively).

When we look to the descriptive statistics presented in figure 5, it is clear that those in the DCN and CN chose to levy no punishment 85.8 per cent and 80 per cent of times respectively compared to those in the UCN at 72.8 per cent. This is consistent with the previous result that the average punishment incidence was higher in UCN and lower in DCN relative to CN, which was based on the aggregated outcomes in table 2. Interestingly, it appeared that prosocial punishment was used more often in the UCN – it constituted 22.7 per cent of all pairwise punishment flows compared to 16.8 per cent and 11 per cent of the time in CN and DCN respectively. There is also a marginally higher share of free rider revenge in this treatment – 2.6 per cent versus 1.9 per cent and 1.5 per cent in CN and DCN respectively – possibly reflecting the likelihood that free riders were more likely to be punished more frequently in UCN. Parallel to this, given the higher incidence of punishment overall, blind revenge is also a bit higher in UCN at 0.93 per cent compared to 0.41 per cent in CN and 0.33 per cent in DCN. Overall, assuming that the total frequency of antisocial punishment is the sum of how often blind revenge, free rider revenge and other types of punishment were levied between pairs, UCN seems to elicit a marginally higher incidence of antisocial punishment (at around 4.9 per cent, mostly driven by higher free rider revenge) relative to CN and DCN (both at around 3.2 per cent).

{Figure 5 about here}

To assess whether prosocial and antisocial punishment flows are statistically different across networks, we regressed the share of the instances of each type of pairwise punishment on the network dummies in random-effects regressions. The omitted category was CN and we controlled for stage one earnings (since the maximum amount of punishment that could be

assigned was bound by stage one earnings), subject attributes, and added round and session dummies. Comparing UCN to CN gives us the effect of lower network density on punishment, and comparing UCN and DCN gives us the effect of directedness or asymmetric monitoring relationships between pairs of agents. The results presented in table 5 show that there is more prosocial punishment in UCN relative to CN (statistically significant at 1 per cent in model 1). The coefficients on the imperfect networks are also different from each other (Wald test p-value < 0.01 in model 1), confirming that UCN elicits more frequent prosocial punishment than DCN. On the other hand, as seen in model 2, the levels of antisocial punishment are not statistically different between CN and UCN. Differences in antisocial punishment between UCN and DCN are also not statistically significant (Wald test p-value = 0.91 in model 2).[13] Overall, this suggests that lower network density increases prosocial punishment in undirected networks but not necessarily in directed networks.

Result 3: Prosocial punishment was used more frequently in UCN than in CN and DCN. There are no differences in the frequency of antisocial punishment across networks.

{Table 5 about here}

The above results lend partial support for H3.

3.3 Efficiency

The average payoffs accruing to individuals pooled across all 15 rounds are lower than the socially-optimal level in all networks, but are marginally higher in UCN and DCN compared to CN. Table 3 also formally supports this result, as the coefficients of DCN and UCN are both positive and statistically significant at 1 per cent, but the coefficients on the imperfect networks are not different from each other (Wald test p-value = 0.918 in model 5). Figure 6 illustrates

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[13] Results were replicated when separate regressions are run by restricting observations to these two networks and with UCN as the omitted category. Poisson regression models show that those in the UCN chose to levy prosocial punishment more frequently compared to those in the CN and DCN (online appendix table C11). Random-effects regression models also show that prosocial punishment was also more severe in the UCN when the subject was a free-rider (their appropriation of more 10 tokens) (online appendix table C12).

that payoffs start above the Nash equilibrium level, but trend downwards in all networks. The erratic punishment patterns in all treatments – but especially in the CN – are the major reason for the variation in efficiency, a result similar to Cason and Gangadharan (2015). Average payoffs decline in all treatments and the decline is larger in the imperfect networks (online appendix table C7).

{Figure 6 about here}

3.4 Beliefs about the appropriation of others in the group

The average beliefs are higher in CN compared to UCN and DCN. Table 3 provides formal support for this as stated beliefs are around 2-3 tokens lower in all imperfect networks relative to the CN (significant at 1 per cent), but the UCN and DCN coefficients are not different from each other (Wald test p-value = 0.247 in model 6). Notably, economics students state higher beliefs which were closer to appropriation levels (by a magnitude of 1-0.66, significant at 1 per cent) relative to non-economics students in table 3. This is in line with findings that economics training leads people to expect others to defect more in social dilemmas (Frank *et al*., 1993). Figure 7 shows that stated beliefs over others' appropriation start at the Pareto appropriation and trend towards the Nash equilibrium in all networks. Expectations that others appropriate less than the Nash equilibrium tend to decline in all treatments (online appendix table C8). Overall, this suggests that individuals retain more optimistic beliefs in imperfect networks.

{Figure 7 about here}

4. Discussion

This paper has explored the effectiveness of imperfect peer monitoring and punishment networks in promoting cooperation in an experimental CPR dilemma. We considered three types of networks – CN, UCN and DCN – which systematically reduced the number of potential punishers in a group by varying the structural attributes of network density and

directedness. By doing so, it changed the perceived "burden of responsibility" to personally undertake costly peer monitoring and punishment aimed at mitigating free riding, an important feature according to studies that suggest that the availability of other agents who can engage in peer sanctioning reduces one's tendency to act prosocially (by shouldering the costly punishment). By focusing on a nonlinear CPR appropriation dilemma – a neglected class of social dilemma games that are said to be quite relevant to understanding more complex socio-ecological dynamics in the field – our experiment extended the emerging literature that studies whether imperfect monitoring and punishment networks affect cooperation levels relative to perfect institutions, which has so far focused on linear public good games. In this way, our paper confirms some key conclusions drawn from these different strands of literature, but also contributes to the external validity of prior experimental results in a new context with different design choices. For example, average appropriation levels in our experiment were comparable to previous nonlinear CPR experiments with perfect peer monitoring and punishment (Cason and Gangadharan, 2015; Kingsley, 2015) and lower than those without sanctioning institutions (Apesteguia and Maier-Rigaud, 2006; Kingsley and Liu, 2014; Cason and Gangadharan, 2015; Kingsley, 2015).

We found that relative to the complete network, the imperfect UCN and DCN networks do not affect appropriation in systematically different ways. This confirms recent findings from comparable public goods games employing these network structures that contributions are similar across these three networks (Carpenter *et al*., 2012; Boosey and Isaac, 2016). It also falls in line with findings from De Geest *et al*. (2017) that there is not much systematic variation in the average appropriation of group members across perfect and imperfect monitoring and punishment treatments (keeping in mind that their experiment varies the ability of insiders to monitor and punish outsiders). Before discussing why this may be the case, we first turn to our results on how imperfect monitoring and punishment network impact punishment choices.

Our result that there is a greater amount of punishment points received in networks with higher punishment capacity (e.g., the CN and UCN) corresponds to findings in Leibbrandt *et al*. (2015) that sanctioning is higher when more structural opportunities to sanction are available. We also found that free riders are less likely to be sanctioned in UCN (which also sees the lowest frequency of any type of punishment) and the prosocial punishment flows are highest in the UCN. This result suggests that lowering the number of potential punishers (or "helpers") increases prosocial behavior and is in line with the studies on the bystander effect (Cryder and Loewenstein, 2012). However, when monitoring and sanctioning roles are made even clearer – as in the DCN – we do not see even greater prosocial punishment relative to the DCN. These results differ from those predicted by the bystander effect studies and Carpenter *et al*. (2012), who find that punishment levels are amongst the lowest in the CN and that more prosocial punishment is used in DCN (to maintain or raise contributions), compared to CN and UCN. A possible reason is that people may prefer coordinated punishment and may thus be willing to punish someone only when another individual is willing to do so as well (Ramalingam *et al*., 2016; Molleman *et al*., 2019). Another related reason may be that individuals may act as "rational bystanders" if they believe that helping is only effective when more than one help-giver pitches in (Greitemeyer and Mügge, 2013) or that their personal contribution will be superfluous (Krueger and Massey, 2009); this is not unlikely in more complex environments like the nonlinear CPR appropriation dilemma. To unpack this further, a replication using a larger number of treatments that vary the potential number of punishers across both directed and undirected networks of varying sizes and types would be most useful.

Finally, differences in punishment flows across network treatments can result in a variation in efficiency between perfect and imperfect networks – the heavier punishment amount used in the CN reduces average payoffs to levels below the Nash equilibrium strategy payoff. The significant loss of efficiency from punishment expenditures echoes findings

originally highlighted in Ostrom *et al.* (1992) and replicated in Cason and Gangadharan (2015), that the costs of perfect peer monitoring and punishment can significantly erode payoffs in the CPR dilemma. In the UCN networks, however, lower aggregate levels of more severe punishment – which tends to be more prosocial – elicits marginally higher payoffs. Lower punishment expenditures also elicit marginally higher payoffs in the DCN. Overall, these findings suggest that neither removing ties nor changing the network property of directedness necessarily increases efficiency due to the same underlying dynamics. They also confirm findings from previous studies that perfect peer monitoring and punishment institutions need not be socially efficient. In addition, we show that even the imperfect peer monitoring and punishment institutions considered here need not be socially efficient.

Why do peer punishment and monitoring not have a more positive impact on cooperation? One reason may be that since free riding takes longer to be identified in these nonlinear environments, a longer period of interactions is necessary for individuals to realize the long-run benefits of punishment (Gächter *et al.*, 2008). There may well be differences across networks if learning is faster in the CN because networks are denser; we found that beliefs were closer to the actual appropriation levels in the CN compared to UCN and DCN networks. De Geest *et al.* (2017) also have some evidence that full observability of the outsiders allowed the insiders to better coordinate their harvests, despite their inability to fully deter the outsiders. However, another possibility may be that peer monitoring and sanctioning are effective only when they are combined with other institutional features that allow individuals to make the burden of responsibility of acting prosocial clearer with each other by communicating to coordinate their harvesting and punishment strategies (Ostrom *et al.*, 1992; Boyd *et al.*, 2010; Cason and Gangadharan, 2016; Molleman *et al.*, 2019).

Our study is not without some limitations. Firstly, our results emerge from the interaction of imperfect monitoring and punishment opportunities, i.e., we cannot unpack if

such patterns are due to different monitoring networks that determine the information feedback available to players, or punishment opportunities that determine the possibility of sanctioning peers. Given this, it could well be that the effect of the different networks on appropriation could be washed away when introducing punishment. Past studies, however, offer mixed evidence on the separate impact of feedback alone. For instance, Carpenter *et al*. (2012) and Boosey and Isaac (2016) consider the CN and UCN. The former vary both monitoring and punishment opportunities (similar to our paper) and the latter manipulate only punishment networks and hold perfect feedback fixed across both treatments, but both papers find qualitatively similar results in terms of appropriation, aggregate and individual-level punishment flows and efficiency. Cox and Stoddard (2015) also found that varying the individual and aggregate-level feedback does not impact cooperation in a linear PG dilemma with a stranger-matching design (also see Apesteguia and Maier-Rigaud (2006) and Cason and Khan (1999)). Nevertheless, we cannot rule out that perfect monitoring will have distinct effects when we vary only punishment networks, and we believe that this is an important empirical question for future investigation. Similarly, whether eliciting beliefs affects cooperation in this nonlinear CPR setting is also an open question. Past work has found mixed evidence ranging from either having no effect (Wilcox and Feltovich, 2000), a negative effect through inducing more self-interested behavior (Croson, 2000), or a positive effect through inducing behavior closer to the socially-optimal level (Gächter and Renner, 2010).

To take this work forward, some natural extensions are to first separate the effects of perfect and imperfect monitoring (and the observability of others' choices) from the sanctioning opportunities available to each agent, by manipulating the punishment network structures relative to cases without any monitoring and sanctioning. To ensure that the differences are not driven simply by network density or directedness, a larger set of networks

could be considered as in Carpenter *et al*. (2012).[14] In addition, to check how sensitive our results are to the design choices used in this experiment, future studies can examine how behavior evolves over a different set of features, including using a benchmark without punishment and a larger number of rounds, relying on partner as opposed to stranger-matching, and using different types of punishment technologies.

Supplementary materials

The supplementary material for this article can be found online.

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Table 1. Nonlinear CPR experiments with and without monitoring and punishment

Studies	Ostrom, Walker & Gardner (1992)	Casari & Plott (2003)	Apesteguia & Maier-Rigaud (2006)	Apesteguia (2006)	Kingsley & Liu (2014)	Cason & Gangadharan (2015)	Kingsley (2015)	This study
Rounds & group size	25, 8	27-33, 8	20, 4	50, 6	15, 4	10, 4	15, 4	15, 4
Nash/Pareto tokens	1.8	1.8	1.6	1.3	1.6	1.6	1.6	1.6
Matching	Partners	Partners	Partners	Partners	Partners	Partners	Partners	Strangers
Expected appropriation of group members (Beliefs)	No	No	No	No	No	No	No	Yes
Monitoring & Punishment stage 2	Yes; after 10 rounds	Yes	No	No	No	Yes	Yes	Yes
Group members appropriation feedback	Perfect & free to group members	No; at cost if requested	No; group & own earnings	No; group & own earnings	No; only own earnings	Perfect & free to group members	Perfect & free to group members	Imperfect & free; based on network
Fee: Fine	1:2 - 1:4	Inspector gets fine	-	-	-	1:3	1:3	1:3
Punishment amount & condition	Subjective; fixed upper bound; one fine limit	If CPR overused; fixed proportion of overuse	-	-	-	Subjective; fixed upper bound	Subjective; up to 100% of stage 1 earnings	Subjective; up to 100% of stage 1 earnings

Notes: All studies reviewed here use lab experiments with students. The ratio of Nash/Pareto tokens determines the size of the externality (Apesteguia and Maier-Rigaud, 2006; Cason and Gangadharan, 2015).

Table 2. Experimental design

Variables	CN	UCN	DCN	All
# Sessions	3	2	2	7
# Subjects	60	36	40	136
# Groups	225	135	150	510
# Observations	900	540	600	2040
Network characteristics				
Network type	Perfect	Imperfect	Imperfect	-
# In-degree, Out-degree	3,3	2,2	1,1	-
Directedness	No	No	Yes	-
Experimental parameters				
Endowment		25		
a		25		
b		0.25		
w		5		
Group size (n)		4		
Fee-to-Fine Ratio		1: 3		
Nash tokens (payoff)		16 (189)		
Pareto tokens (payoff)		10 (225)		
Nash/Pareto tokens		1.6		
Nash/Pareto payoff (% share)		84		

Table 3. Descriptive statistics by network

Outcomes	Appropriation	Punishment			Net payoffs	Beliefs	# Obs, Subjects
		Amount	Severity	Incidence			
Complete Network (CN)							900, 60
Session 1	15.84	7.74	2.58	0.32	171.84	14.15	
	(2.97)	(10.37)	(3.46)	(0.32)	(41.37)	(2.27)	
Session 2	14.99	6.52	2.17	0.34	183.33	12.74	
	(2.87)	(15.14)	(5.05)	(0.36)	(40.09)	(2.62)	
Session 3	15.28	20.81	6.94	0.71	161.91	12.86	
	(2.75)	(23.77)	(7.92)	(0.31)	(48.07)	(2.73)	
CN-All	15.37	11.69	3.9	0.46	172.36	13.25	
	(2.87)	(18.44)	(6.15)	(0.37)	(44.01)	(2.62)	
Undirected Circle Network (UCN)							540, 36
Session 1	14.9	4.24	2.12	0.4	188.56	12.84	
	(2.63)	(4.70)	(2.35)	(0.28)	(28.19)	(2.08)	
Session 2	14.02	9.93	4.96	0.49	189.03	12.49	
	(2.58)	(8.86)	(4.43)	(0.24)	(26.78)	(1.62)	
UCN-All	14.51	6.77	3.38	0.44	188.77	12.69	
	(2.64)	(7.40)	(3.70)	(0.27)	(27.47)	(1.89)	
Directed Circle Network (DCN)							600, 40
Session 1	15.1	2.17	2.17	0.14	189.91	13.07	
	(2.51)	(4.39)	(4.39)	(0.20)	(25.79)	(1.82)	
Session 2	14.51	3.04	3.04	0.14	188.61	11.45	
	(3.53)	(6.19)	(6.19)	(0.17)	(31.69)	(2.38)	
DCN-All	14.8	2.61	2.61	0.14	189.26	12.26	
	(3.07)	(5.36)	(5.36)	(0.19)	(28.80)	(2.26)	
All							2040, 136
All networks	14.98	7.71	3.38	0.36	181.67	12.81	
& sessions	(2.89)	(13.69)	(5.38)	(0.33)	(36.91)	(2.37)	

Notes: Appropriation is the average number of tokens allocated to the common account by an individual in each session. Punishment amount is the average of the total punishment received by an individual from other group members in stage two of the CPR game. Punishment severity is this punishment amount received divided by the number of potential punishers targeting each subject, which is determined by the in-degree in each network, i.e., by 3, 2 and 1 in the CN, UCN and DCN respectively. Punishment incidence is whether the subject received any punishment in a given round. Net payoffs are the returns to the subject after the cost of giving and receiving punishment have been deducted from their stage one earnings. Beliefs are the average of the number of tokens that subjects expect other group members to place in the common account in a given round.

Table 4. Average outcomes across networks

Random-effects models: Dependent variables	(1)	(2)	(3)	(4)	(5)	(6)
			Punishment			
	Appropriation	Amount	Severity	Incidence	Beliefs	Net payoffs
Treat = 1, UCN	-1.756	3.283*	2.950***	0.831***	-1.654**	18.425***
	(1.200)	(1.939)	(0.906)	(0.213)	(0.700)	(6.075)
Treat = 2, DCN	-1.050	-4.455***	0.570	-1.133***	-2.590***	17.925***
	(1.162)	(1.696)	(1.162)	(0.254)	(0.833)	(5.848)
Appropriation		0.786***	0.347***	0.088***		
		(0.102)	(0.045)	(0.012)		
Sex = 1, Female	-0.651	1.580*	0.899**	0.084	-0.427	-0.374
	(0.566)	(0.831)	(0.404)	(0.127)	(0.464)	(2.715)
Economics = 1, Yes	-0.099	-1.596	-0.490	-0.173	1.343***	3.865
	(0.750)	(1.000)	(0.542)	(0.192)	(0.447)	(3.186)
Experience	0.190	-0.357	-0.115	-0.107	-0.189	0.965
	(0.315)	(0.505)	(0.292)	(0.075)	(0.302)	(1.530)
Constant	12.359***	-4.322**	-2.833**	-1.565***	11.421***	195.527***
	(1.325)	(2.014)	(1.113)	(0.314)	(0.837)	(7.831)
# Observations	2,040	2,040	2,040	2,040	2,040	2,040
# Subjects	136	136	136	136	136	136
Session & round dummies	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. Omitted category = CN. Random-effects logistic regression models are used for punishment incidence since the outcome is binary (Punishment incidence = 1 if yes, 0 if no). The UCN and DCN coefficients imply that the odds of being punished are 2.3 (higher than CN) and 0.3 (lower than CN) respectively.

Table 5. Types of punishment given across networks

Random-effects regression models Variables: Types of punishment given	(1) Prosocial punishment	(2) Antisocial punishment
Treat = 1, UCN	19.097***	1.392
	(7.161)	(1.814)
Treat = 2, DCN	1.665	1.109
	(5.716)	(2.140)
Stage 1 payoff	-0.124***	0.085***
	(0.021)	(0.015)
Sex = 1, Female	-1.041	0.552
	(3.386)	(1.012)
Economics = 1, Yes	-0.204	-0.464
	(3.209)	(1.134)
Experience	1.127	0.108
	(1.809)	(0.683)
Constant	29.060***	-5.726
	(7.288)	(3.848)
# Observations	2,040	2,040
# Subjects	136	136
Round + Session fixed effects	Yes	Yes

Notes: Robust standard errors in parentheses; *** $p<0.01$. Omitted category = CN.
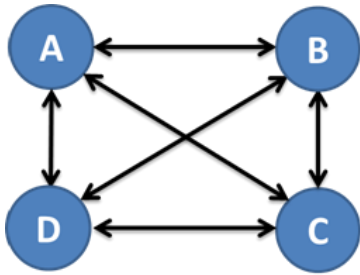
1a. Complete Network [0] (CN)
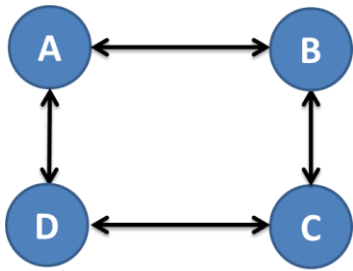
1b. Undirected Circle Network [1] (UCN)
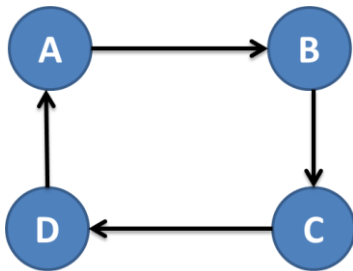
1c. Directed Circle Network [2] (DCN)

Figure 1. Network treatment groups.

Notes: Panels 1a, 1b and 1c illustrate three monitoring and punishment network groups with four types of nodes indexed by $i = A, B, C, D$. In the CN each pair of nodes is connected by an undirected tie to represent perfect monitoring, i.e., everyone can observe everyone's appropriations and can choose to punish any extractor in the network. In the UCN, the undirected tie implies connected nodes can simultaneously monitor and punish each other. In the DCN the directed tie signifies that each subject can only monitor and punish the subject in the direction it is connected to.
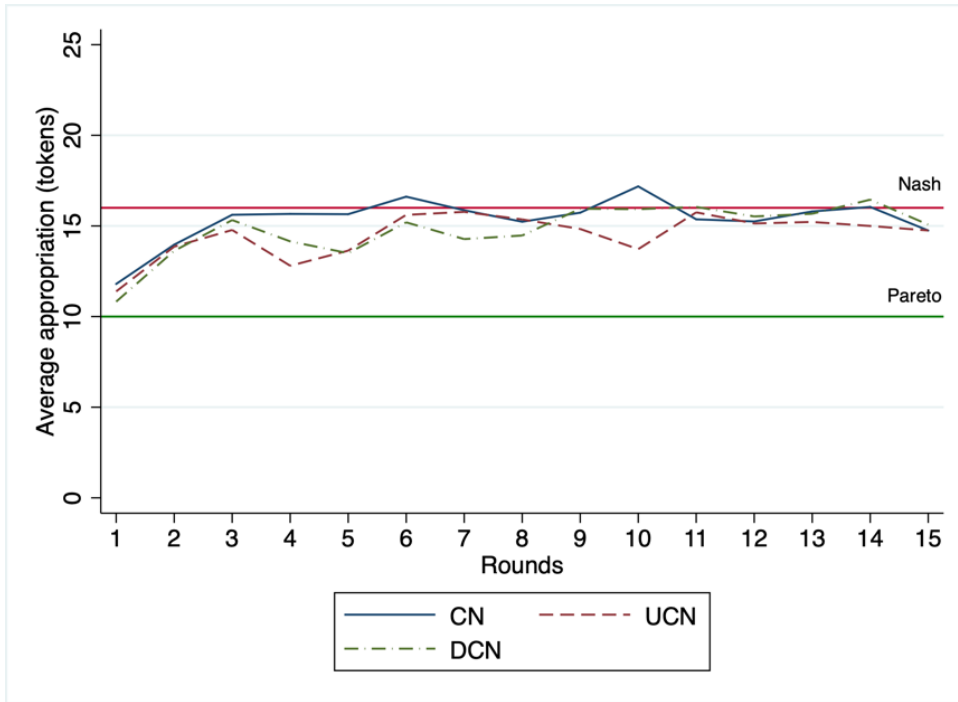
Figure 2. Appropriation and beliefs about others' appropriation.
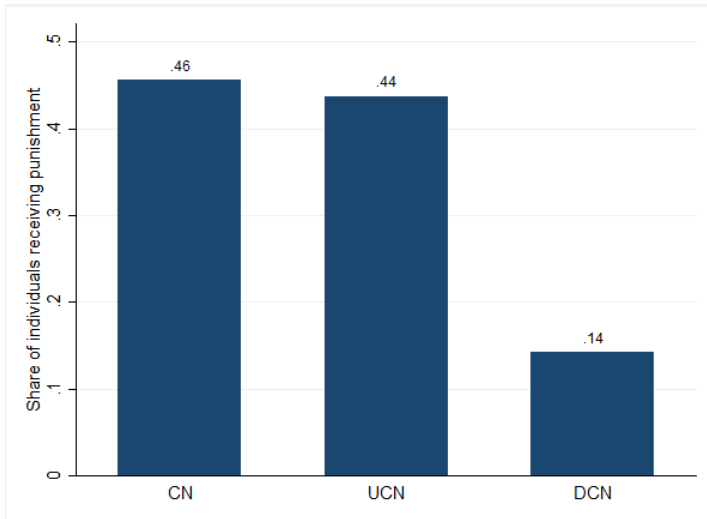
(a)
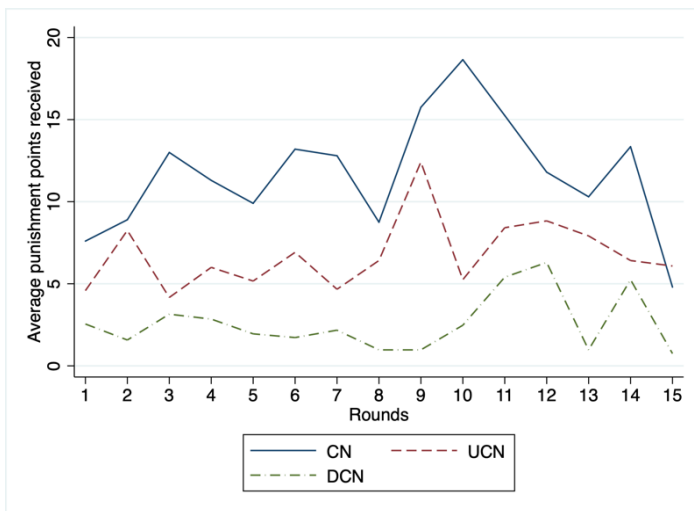
(b)
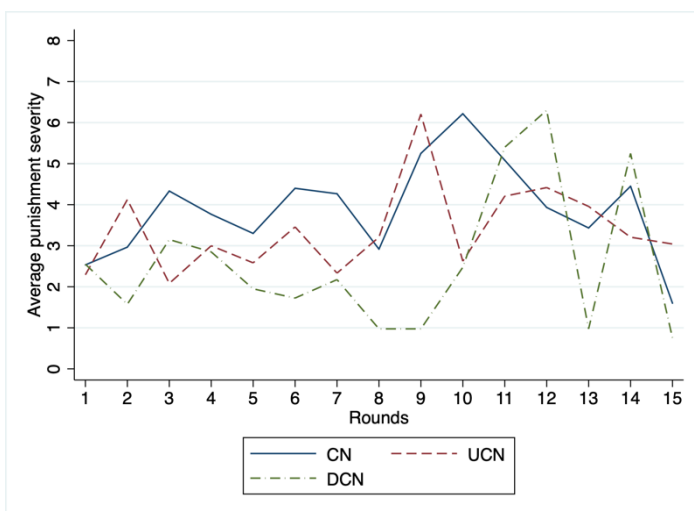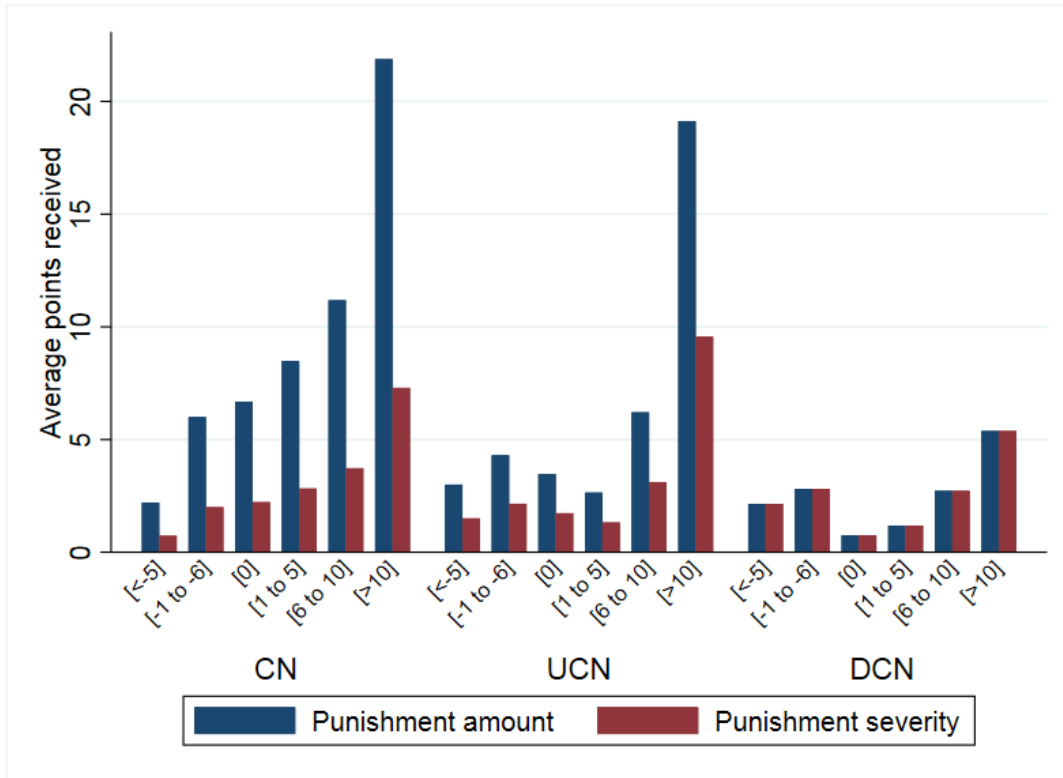

(c)

Figure 3. Punishment received.

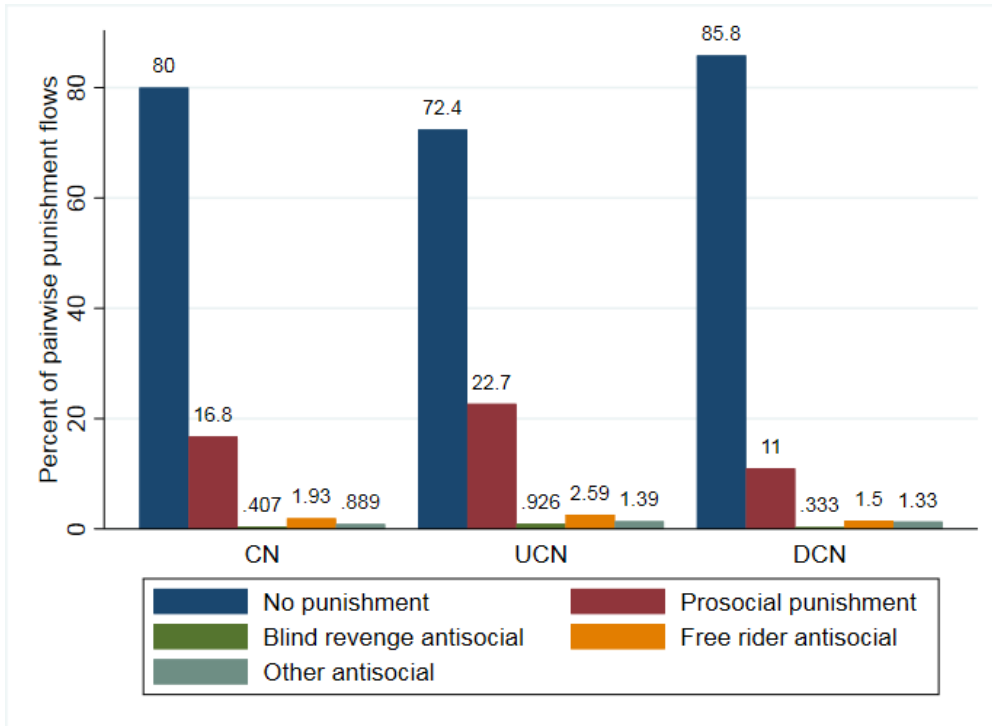**Figure 4.** Punishment by deviation from Pareto optimal appropriation.
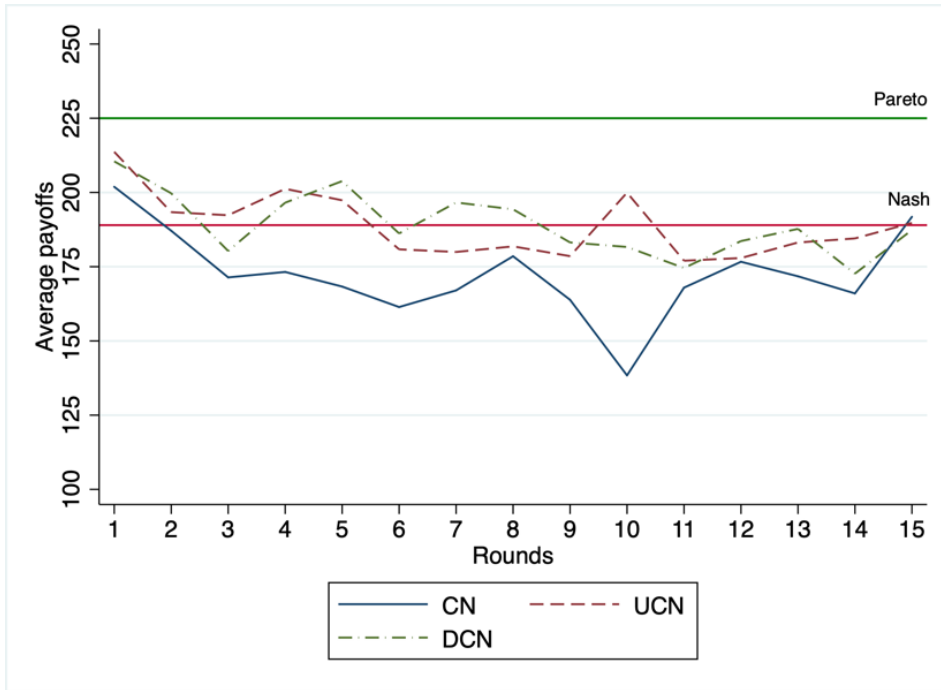
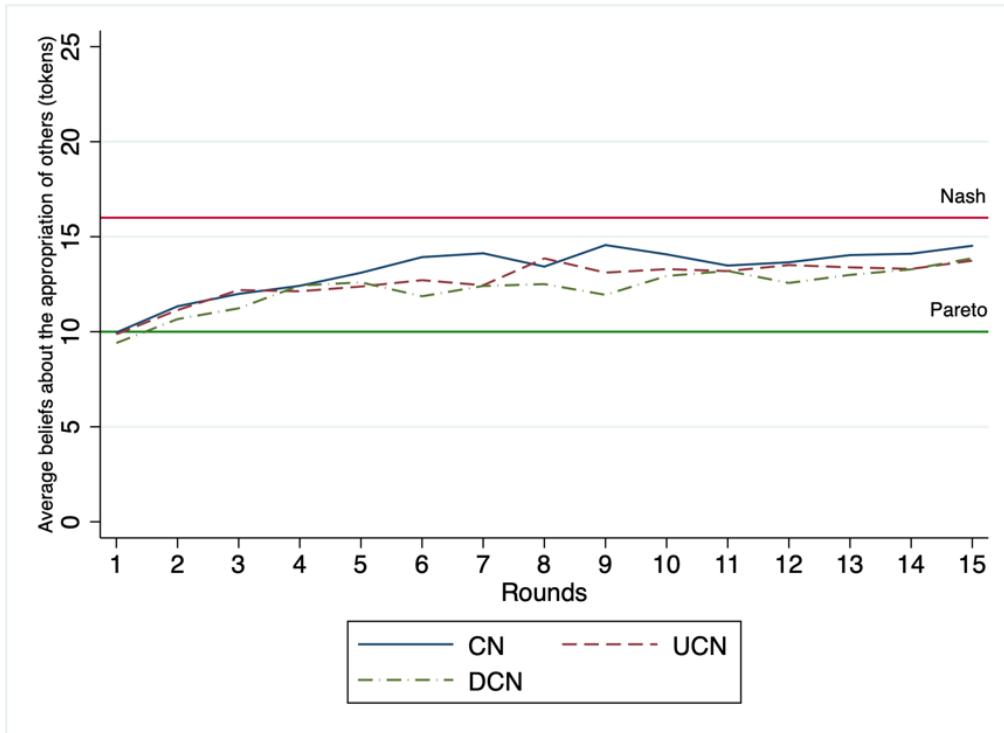**Figure 5.** Types of pairwise punishment flows.

Figure 6. Average payoffs.

Figure 7. Average beliefs about the appropriation of others.