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Abstract

We test the effect of inequality on peer punishment in a common-pool resource (CPR) experiment with equal endowments (*Equal*) or unequal endowments (*Unequal*). Peer punishment reduces extractions in both treatments, but it is more effective in *Unequal*. Subjects with lower endowments coordinated around an Equal Earnings norm, subjects with higher endowments matched, and peer punishment tightened this coordinate-and-match dynamic. By contrast, there was less coordination in *Equal*, and as a result, more peer punishment and lower payoffs.

Keywords: Inequality; Common-Pool Resources; Cooperation; Peer Punishment
JEL Codes: C92, H41, D82

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1 Introduction

Institutions like communication, voting, and peer punishment can enable self-governance in common-pool resources (CPRs) when users have equal capacities (i.e., endowments) to extract the resource (Cason and Gangadharan, 2015; Ostrom, 2006, 1990).[1] But in many cases users have unequal endowments (e.g., some users have more nets to cast into a fishery). Andersson and Agrawal (2011) examine more than two-hundred CPR case studies across three continents to study the effects of variation in endowments (wealth) on conservation. They find that inequality and conservation are negatively associated where institutions are weak or non-existent.[2] This suggests that institutions moderate the effects of inequality.

What is unclear is whether inequality directly impacts the effectiveness of institutions. Controlled lab experiments can help clarify the relationship between inequality and institutional effectiveness by holding the institution constant and varying inequality (Ostrom, 2006). Experiments find mixed evidence on the effect of endowment heterogeneity on voting (Margreiter et al., 2005) and communication (Cardenas, 2003; Hackett et al., 1994). But little is known about the interaction between inequality and punishment in CPRs.[3] We fill this gap in the literature.

Our experiment is based on the homogeneous endowment CPR game by Kingsley (2015) and similar to the nonlinear public goods game with heterogeneous endowments and punishment by Kingsley (2016). Subjects were grouped into fours and played fifteen periods of a CPR game. We have 2×2 treatments: $\{$*Equal*, *Unequal*$\} \times \{$*No Punishment*, *Punishment*$\}$. In our *Equal* treatments, each subject was given an endowment of 50 experimental dollars (EDs). In our *Unequal* treatments, two subjects were given endowments of 40 EDs (*Low*) and two subjects were given 60 EDs (*High*). The distribution of endowments was random

[1]Similarly, introducing the opportunity to punish into public good (PG) games with equal endowments has been shown able to increase cooperation sufficiently to offset the costs associated with punishment, particularly in longer duration experiments (Gächter et al., 2008; Fehr and Gächter, 2002, 2000). However, it is not uncommon to observe no increase in cooperation when punishment is weak or expensive (Egas and Riedl, 2008; Nikiforakis and Normann, 2008; Sefton et al., 2007), or when punishment is perverse or poorly targeted at free riders (Ertan et al., 2009; Bochet et al., 2006; Cinyabugama et al., 2006).

[2]The case studies in Andersson and Agrawal (2011) are all forest commons. The authors test the effects of inequality with a reduced form regression where forest condition is the dependent variable and the right hand side variables include inequality, institutional strength (measured by a "collective action index"), an interaction and a set of controls. The main findings are (a) a negative effect of inequality and (b) a positive interaction effect of inequality and institutional strength. To the best of our knowledge, there is no empirical evidence on how endowment heterogeneity affects the use or effectiveness of peer enforcement. The closest study is by Bardhan et al. (2002), who show that peer enforcement is less effective under ethnic heterogeneity.

[3]Results from PG games with unequal endowments and punishment are mixed. Reuben and Riedl (2013) and Visser and Burns (2015) find that punishment increases cooperation while Kingsley (2016) finds no increase in cooperation. However, it is unclear how these results from the PG literature carry over to the CPR literature, as the literature that compares behavior across PG and CPR settings also finds mixed results (De Geest and Stranlund, 2019; Cartwright, 2016).

and fixed across periods. We then vary whether subjects could peer punish using the standard 1:3 punishment technology (in which subjects pay one ED to punish a target three EDs). Like Kingsley (2016), during the punishment stage subjects could observe extractions from the CPR but not individual endowments.

Groups that self-govern a commons must agree on (1) the total level of extraction and (2) how that total is divided among group members (Ostrom, 2006). Inequality in our design does not change the solution to the first problem: the aggregate social optimum is the same across treatments. Instead, inequality introduces three normatively appealing ways groups can achieve the social optimum (i.e., solve the second problem): Equal Extractions, Equal Proportions, and Equal Earnings (Cappelen et al., 2007; Nikiforakis et al., 2012; Reuben and Riedl, 2013; Kingsley, 2016). In *Equal* each of these extraction norms coincide with the symmetric social optimum. However, in *Unequal* each norm dictates a different pattern of extractions and, importantly, a different distribution of the socially optimal group earnings. This is important because peer punishment is most effective when groups agree on a norm (and thus agree on which extractions should be punished). So, if groups with inequality struggle to agree on a division of the social optimum, it is possible that peer punishment will be less effective.

We find that peer punishment is, in fact, more effective in *Unequal* than *Equal*. In the absence of punishment there is no difference in earnings across treatments. However, when punishment is introduced, and after accounting for the costs of punishment, groups in *Equal* earn less across all periods, while groups in *Unequal* earn a similar amount relative to their no punishment counterparts. In later periods, earnings are significantly higher in *Unequal* with punishment relative to *Equal*. This is particularly interesting because average extractions across the punishment treatments are indistinguishable.

However, treatment effects are not limited to central tendency. For example, De Geest and Stranlund (2019) show that coordination in social dilemmas like CPR games can be measured by testing differences in behavioral variance across treatments. We show that an important effect of punishment in *Unequal* was on the variation in extractions. Using the variation test introduced by De Geest and Stranlund (2019) we find that punishment reduced the variation in extractions in *Unequal* (for both *Low* and *High*) but not in *Equal*. In other words, punishment appeared to induce more coordinated behavior in *Unequal* than *Equal*.

Evidence of better coordination in *Unequal* also bears out in how subjects used punishment. To get a more detailed look at enforcement across treatments we calculate the expected cost of punishment – the probability of punishment times the magnitude of punishment – for each possible extraction. While unconditional average punishment is similar across treatments, the conditional expected costs of punishment were higher in *Equal* than *Unequal*, stemming from a significantly higher probability of punishment in *Equal*. Taken together, our results point to a greater degree of coordination and agreement on the appropriate level of extractions in *Unequal*, resulting in less punishment and higher earnings.

Our main contribution to the CPR literature is to show that inequality can make peer punishment more effective. One plausible explanation is that inequality created a focal point in the choice space, making it easier for groups to coordinate on and enforce an acceptable division of total extractions.[4] Our results show that *Low* types coordinated around the Equal Earnings norm, and in response, *High* types matched. This coordinate-and-match dynamic appeared when there was no punishment. Introducing punishment reinforced it: the variation of extractions by both types falls over time, and most telling, the distribution of extractions of *Low* pile up at the Equal Earnings norm. While this division of extractions was not optimal for *Low* types, the use of punishment indicates it was acceptable. Punishment was mainly targeted at extractions that revealed *High* types, while extractions below the *Low* endowment were rarely targeted, likely to avoid misguided punishment, which can hinder the institution's effectiveness (Nicklisch et al., 2016).

In *Equal* there was more punishment but less coordination. Cason and Gangadharan (2015) find similar results in a CPR with homogeneous endowments and argue it is because of the inherent complexity of nonlinear strategic settings. CPR games like ours are nonlinear, meaning the social optimum and Nash equilibrium lie on the interior of the choice set, rather than on the boundaries (as they would in a linear game). As a result, it is harder for subjects to distinguish cooperative behavior and enforce it with punishment. Therefore, the salience of inequality may improve coordination by emphasizing the Equal Earnings norm as a focal point.

Of course, whether this holds true in other scenarios (e.g., more extreme inequality, self-governance with multiple institutions, and so on) is an open question. We discuss ways to improve our study and topics for future research in our conclusion.

2 Experiment design and methods

We implement a 2×2 design in which we vary the distribution of endowments within groups (*Equal* or *Unequal*) and whether subjects have the opportunity to punish each other (*No Punishment* or *Punishment*). We use the same CPR and peer punishment design as Kingsley

[4]Focal points are salient details about a game that influence perceptions but not incentives (Sugden, 1995; Sugden and Zamarrón, 2006). In our design, heterogeneous endowments do not change incentives, since returns from the private account are fixed and returns from the CPR only depend on the total level of extractions by other group members. However, inequality is often a salient feature in strategic settings: Reuben and Riedl (2013) point out that "heterogeneity...can shift attention from one focal norm to another".

(2015), which is based on the canonical CPR model introduced by (Ostrom et al., 1992).[5] Payoffs to agent i in the absence of punishment are

$$\pi_i = w(e_i - g_i) + \frac{g_i}{G}V(G), \tag{1}$$

where e_i is the agent's endowment, g_i is the agent's extraction from the CPR, w is the fixed return from the private account, n is the group size, $G = \sum_{i=1}^{n} g_i$, and $V(G) = aG - b(G)^2$ is the production function of the CPR. The game is a social dilemma when $a > w > b$ and $0 < b < 1$. Setting $w = 1$, $a = 6$, $b = 0.025$, and $n = 4$ the socially optimal aggregate extraction G^S and the Nash equilibrium aggregate extraction G^N are

$$\begin{aligned} G^S &= \frac{a-w}{2b} &&= 100 \\ G^N &= \frac{n(a-w)}{b(n+1)} &&= 160 \end{aligned} \tag{2}$$

Our design uses a simple implementation of inequality. In each session, subjects were randomly assigned into groups of four and stayed in their groups for the duration of the experiment. Subjects within each group were then randomly assigned an endowment. In *Equal*, all subjects received the same endowment of 50 Experimental Dollars (EDs) at the start of each period. In *Unequal*, two subjects in each group received a low endowment (*Low* = 40), and the other two group members received a high endowment (*High* = 60). Note that the sum of endowments was identical across treatments ($\sum_{i=1}^{n} e_i = 200$). Subjects retained these endowments for the duration of the experiment. Subjects knew the distribution of endowments in their group, but they were never informed which group member had which endowment.

In our model (just like in Ostrom et al., 1992) the agent's own endowment does not enter her best-response, and neither do the endowments of her group members. As a result, endowment heterogeneity does not affect the aggregate Nash equilibrium or social optimum, and thus does not affect group earnings at these points. However, groups can vary the distribution of these group earnings depending on the pattern of extractions across group members.

Table 1 shows extractions and earnings for the benchmarks in our design. First, we assume a symmetric Nash equilibrium where each group member extracts 40 EDs from the CPR. In this case each group member would earn 90 EDs in *Equal* and *Low* (*High*) members

[5]This design was introduced by Kingsley and Liu (2014) and is also used in De Geest and Stranlund (2019).

would earn 80 (100) EDs in *Unequal* each period.[6]

At the social optimum we consider three normatively appealing extraction norms: Equal Extractions, Equal Proportions, and Equal Earnings. As shown in Table 1, this distinction is irrelevant in *Equal* as each plausible norm dictates identical extractions and earnings across group members. However, in *Unequal*, each plausible extraction norm dictates a different distribution of group earnings. Under an Equal Extractions norm each group member extracts 25 EDs and *Low* members earn 102.5 EDs and *High* members earn 122.5 EDs. Under an Equal Proportions norm *Low* (*High*) members extract 20 (30) EDs and earn 90 (135) EDs each period. Finally, under an Equal Earnings norm *Low* (*High*) members extract 29 (21) EDs and earn 112.5 (112.5) EDs each period.

Table 1: Theoretical benchmarks. Period earnings, in EDs, are listed in parenthesis next to extractions. Inequality splits the social optimum into three norms: Equal Extractions, Equal Proportions, and Equal Earnings. Note that the symmetric social optimum for *Equal* defines each norm.

	Homogeneous Endowments	**Heterogeneous Endowments**	
	Equal: 50	*Low*: 40	*High*: 60
Nash	40 (90)	40 (80)	40 (100)
Social (Equal Extractions)	25 (112.5)	25 (102.5)	25 (122.5)
Social (Equal Proportions)	25 (112.5)	20 (90)	30 (135)
Social (Equal Earnings)	25 (112.5)	29 (112.5)	21 (112.5)

The experiment instructions displayed both individual and group payoff tables so that subjects could clearly discern the relationship between individual and group earnings (Kingsley and Liu, 2014). Further, subjects were shown that extracting 100 EDs would maximize group earnings to ensure that, across treatments, subjects had the same understanding of the individual and group incentives.

Our experiment proceeded as follows. Subjects first decided how much of their endowment to allocate between two accounts: Account 1 (the group account) or Account 2 (the private account). After choosing extractions, subjects were shown their individual extraction, the group's aggregate extraction, and their individual period earnings. In *No Punishment*, subjects would then continue to the next period.

[6] Note that at the conversion rate of 100 EDs = \$1, a 20 ED per period difference in earnings, across 50 periods, implies a difference of 1000 EDs or \$10 between *High* and *Low* earnings.

In *Punishment* subjects would then proceed to the punishment stage. In the punishment stage, subjects chose how many deduction points to assign to each group member. Subjects in *Equal* and *Unequal* could observe each group member's extraction by random ID, but they could not observe each group member's endowment. Subjects in *Unequal* knew the distribution of endowments, but at no point in the experiment did they learn which group members had which endowments. We use the standard 1:3 punishment technology in which one punishment point assigned to an individual cost the sender 1 ED and the receiver 3 EDs. Subjects were constrained only by their initial payoffs when assigning punishment (and so payoffs in a period could be negative). Therefore payoffs in *Punishment* were

$$\pi_i = w(e_i - g_i) + \frac{g_i}{G}V(G) - \sum p_{ij} - c\sum p_{ji}, \tag{3}$$

where $\sum p_{ij}$ is the sum of punishment sent by i to all other group members j and $\sum p_{ji}$ is the sum of punishment received by i from j at cost $c = 3$.

2.1 Implementation

Subjects were recruited from the undergraduate population at the University of Massachusetts Amherst. Data was collected in Spring 2012 at the Cleve E. Willis Experimental Economics Laboratory. A total of eight sessions were conducted with a total of 120 subjects including 8 groups in each of our 4 treatments. The average session lasted approximately one hour. Subjects earned an average of about \$15.00, with a standard deviation of about \$3.00.[7]

3 Results

Taken together, our results show that punishment was more effective in *Unequal* than *Equal*.

We begin with earnings in *Equal* and *Unequal*. There are no differences in earnings across treatments without punishment. However, earnings with punishment are higher in *Unequal* than *Equal*. Within treatments, punishment reduced earnings in *Equal*, but had no overall effect on earnings in *Unequal*. Breaking down the effect by endowment we find that punishment increased earnings for *Low* types and decreased earnings for *High* types.

We then look at extractions and punishment. To see whether subjects in *Equal* and *Unequal* coalesced around one of the plausible extraction norms in Table 1, we consider ranksum and signrank tests of average group extractions, and changes in the variation of extractions over time.[8] While average extractions with punishment are similar in *Equal*

[7] Our experiment instructions are in the appendix.

[8] To make our results easier to read we move some test statistics to footnotes. In addition, test statistics

and *Unequal*, the variation in extractions was significantly lower in *Unequal*. Subjects in *Unequal* appear to coordinate-and-match around the Equal Earnings norm for *Low* types: the distribution of extractions by *Low* types centers at the norm, *High* types appear to match it, and introducing punishment tightened this coordinate-and-match dynamic. As a result, there was significantly less punishment in *Unequal*, and it was more effective at changing behavior.[9]

3.1 Earnings

Table 2 shows average earnings across treatments for all 15 periods, early periods (Early, periods 1-7), and late periods (Late, periods 8-15). Comparing *Equal* and *Unequal*, there is no significant difference in earnings without punishment.[10] But when punishment is introduced, earnings are significantly higher in *Unequal* in late periods.[11]

The introduction of punishment significantly reduced earnings in *Equal* across all periods, although the difference is not significant in late periods.[12] By contrast, punishment had no effect on average earnings in *Unequal*.[13] However, there was variation in earnings within endowment types. For *Low* types punishment significantly increases their earnings during the late periods.[14] *High* types earn significantly less overall, and the difference is driven by earnings in late periods.[15]

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and p-values are the same for several of the signrank tests. This is due to the test itself. The signrank test calculates for each group k and endowment e and some benchmark b the difference $d_{ke} = \bar{x_{ke}} - b$ and then calculates the signed-ranking $r_{ke} = \text{sign}(d_{ke})\text{rank}(|d_{ke}|)$ to create the test statistic $z = \sum_i^{n_{ke}} r_{ke}$. So in a case where all groups across two treatments (or endowments) with or without punishment have average extractions above or below the tested level (e.g. $b = 25$), the signrank tests will return the same test statistics and p-values.

[9] Our data and code can be found online at `https://github.com/lrdegeest/InequalityCPR`.

[10] Overall: $z = 0.84$, $p = 0.40$; Early: $z = 0.53$, $p = 0.60$; and Late: $z = 1.47$, $p = 0.14$.

[11] Overall: $z = 1.47$, $p = 0.14$; Early: $z = 1.26$, $p = 0.21$; and Late: $z = 2.31$, $p = 0.02$.

[12] Overall: $z = 2.52$, $p = 0.01$; Early: $z = 3.26$, $p < 0.01$; and Late: $z = 1.26$, $p = 0.21$. It is not uncommon for punishment to decrease payoffs in the short-run, as the welfare gains from punishment are typically realized in the long run (Gächter et al., 2008).

[13] Overall: $z = 0.42$, $p = 0.67$; Early: $z = 1.16$, $p = 0.25$; or Late: $z = 1.16$, $p = 0.25$.

[14] Overall: $z = 0.00$, $p = 1.00$, Early: $z = 1.58$, $p = 0.12$, and Late: $z = 2.31$, $p = 0.02$.

[15] Overall: $z = 2.84$, $p < 0.01$; Early: $z = 1.37$, $p = 0.17$; and Late: $z = 2.84$, $p < 0.01$.

Table 2: Average period earnings, in EDs, across treatments. Averages are calculated at the group level and over time: aggregate (all periods), the first seven periods (Early) and the remaining eight periods (Late). Standard deviations are shown in parentheses. There are $N = 8$ independent groups per treatment.

	Aggregate (All periods)		**Early (Periods 1-7)**		**Late (Periods 8-15)**	
	No Punishment	*Punishment*	*No Punishment*	*Punishment*	*No Punishment*	*Punishment*
Equal	103.75	80.92	105.72	69.75	101.77	92.08
	(4.85)	(34.40)	(4.27)	(44.71)	(4.81)	(15.86)
Unequal	105.91	100.77	106.42	97.51	105.40	104.03
	(15.24)	(13.90)	(11.28)	(15.97)	(18.77)	(11.03)
Low	92.50	93.30	96.82	87.24	28.12	88.18
	(7.63)	(12.06)	(5.81)	(13.06)	(6.95)	(7.54)
High	119.32	108.24	116.02	107.77	122.62	108.71
	(6.19)	(11.62)	(5.30)	(11.63)	(5.39)	(12.40)

3.2 Extractions

Table 3 shows average extractions in each treatment. Punishment significantly lowers extractions overall in *Equal* ($z = 2.31$, $p = 0.02$) and in *Unequal* ($z = 3.36$, $p < 0.01$), and in both treatments the effect starts early and gets more pronounced over time.[16] When we break up the effect by endowment in *Unequal*, we find punishment significantly reduced extractions by *High* types ($z = 2.94$, $p < 0.01$) but not *Low* types ($z = 0.26$, $p = 0.79$).[17]

[16] *Equal* and Early: $z = 1.68$, $p = 0.09$. *Equal* and Late $z = 2.16$, $p = 0.03$. *Unequal* and Early: $z = 2.10$ $p = 0.04$. *Unequal* and Late $z = 3.36$, $p < 0.01$.

[17] For *Low* types we observe slightly lower extractions early ($z = 1.99$, $p = 0.05$) and slightly higher extractions late ($z = 1.68$, $p = 0.09$). For *High* types we observe no difference early ($z = 0.999$ $p = 0.317$) and a significant decrease late ($z = 3.26$, $p < 0.01$).

Table 3: Average extractions across treatments. Averages are calculated at the group-level and over time: all periods, the first seven periods (Early) and the remaining eight periods (Late). Standard deviations are shown in parentheses. There are $N = 8$ independent groups per treatment.

	Aggregate (All periods)		**Early (Periods 1-7)**		**Late (Periods 8-15)**	
	No Punishment	*Punishment*	*No Punishment*	*Punishment*	*No Punishment*	*Punishment*
Equal	33.12	29.46	32.03	28.95	34.22	29.98
	(2.70)	(3.20)	(2.54)	(2.86)	(2.53)	(3.63)
Unequal	31.75	28.81	31.01	28.67	32.49	28.95
	(4.54)	(2.73)	(2.98)	(3.00)	(5.71)	(2.51)
Low	29.43	29.32	30.75	27.85	28.12	30.80
	(3.01)	(2.67)	(2.45)	(2.83)	(3.08)	(1.52)
High	34.06	28.30	31.27	29.50	36.86	27.09
	(4.70)	(2.77)	(3.59)	(3.12)	(4.08)	(1.84)

Figure 1 shows the distributions of extractions across treatments, endowments and time (Early and Late). The first observation that stands out in the distributions is evidence of coordination-and-matching around the Equal Earnings norm in *Unequal*: *Low* types coordinated around it, and *High* types matched it. Extractions by *Low* (Panels B and E) with or without punishment are consistent with the Equal Earnings norm: signrank tests fail to reject the hypothesis that average group extractions by *Low* are different from 29 (*No Punishment*: $z = 0.14, p = 0.89$; *Punishment*: $z = 0.07, p = 0.94$).[18] The effect is most striking in late periods with punishment. Panel E in Figure 1 shows extractions by *Low* piling up right on top of the Equal Earnings norm.

[18]Extractions by *Low* are significantly different from the Equal Extractions and the Equal Proportions norm with and without punishment ($z = 2.52, p = 0.01$).

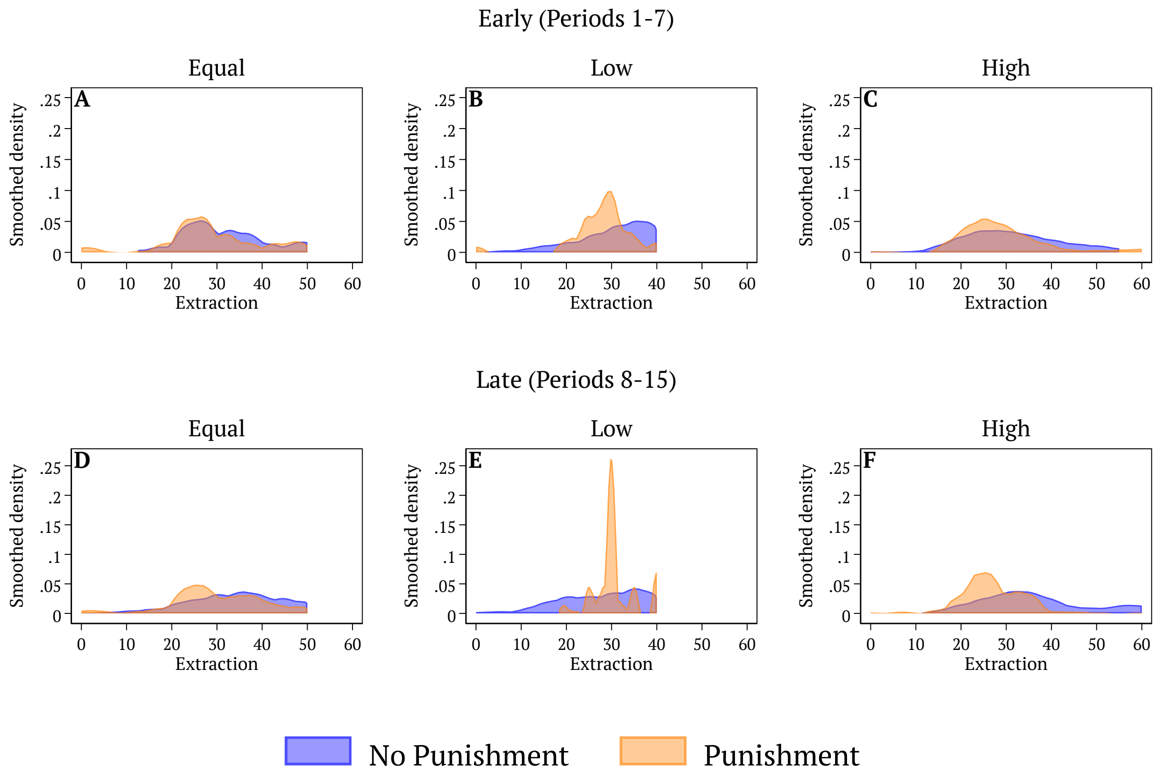

Figure 1: Distributions of extractions over time. Distributions are broken up for each endowment by *No Punishment* and *Punishment* and by Early (periods 1-7) and Late (periods 8-15).

Turning to *High* types (Panels C and F), we can reject the hypothesis that their extractions adhere to any of the identified extraction norms.[19] Instead, punishment leads *High* types to match extractions by *Low* types. Average extractions by *High* are significantly different from 29 in *No Punishment* ($z = 2.38, p = 0.02$), but they are not significantly different in *Punishment* ($z = 0.70, p = 0.48$).

Coordination in *Unequal* is also seen in the reduced variation in extractions. The standard deviations in Table 3 show that with punishment the variation in extractions decreased over time in *Unequal* but not *Equal*. Moreover, punishment clearly narrows the distributions of extractions in Figure 1 by both *High* and *Low*, particularly in late periods. To check for statistical significance we use χ^2 tests from a version of the modified Levene's test of equal variances for clustered panel data introduced by De Geest and Stranlund (2019).[20] Since

[19] Equal Earnings and Equal-Extractions with and without punishment: $z = 2.52, p = 0.01$. Equal-Proportions: *No Punishment*: $z = 2.24, p = 0.03$; *Punishment*: $z = 2.10, p = 0.04$.

[20] The test accounts for the correlation of observations within groups and over time. There are three steps. First, regress extractions on a punishment treatment indicator while controlling for group and subject random effects and clustering standard errors at the group level. Then calculate the residuals. Finally, regress the residuals on the punishment indicator.

punishment needs time to take hold, we focus our tests on the later periods (i.e., we test for differences in variation in Panels D, E and F in Figure 1).

Results from our tests confirm that punishment led to a significant reduction in variance in *Unequal*. The variation in extractions is significantly less among *Low* types ($\chi^2 = 31.74, p < 0.01$) and *High* types ($\chi^2 = 4.58, p = 0.03$) in late periods with punishment relative to no punishment. This suggest that introducing punishment tightened the coordinate-and-match dynamic we observe in *Unequal*.

By contrast, we find less evidence that subjects in *Equal* (Panels A and D in Figure 1) coalesced around the social optimum. While punishment significantly decreased extractions in *Equal*, there is still a large density of extractions above the social optimum. As a result, average group extractions with punishment were significantly greater then the symmetric social optimum ($z = 2.38, p = 0.02$), and there is no difference in the variation of extractions ($\chi^2 = 0.02, p = 0.89$).

3.3 Punishment

Now we look at how the use of punishment may have influenced coordination in *Unequal* and the lack thereof in *Equal*. Figure 2 shows counts of punishment in Panels A and B and average punishment in Panels C and D. Two points stand out.

First, subjects in *Equal* punished nearly twice as often as in *Unequal* (Panels A and B). There were 526 counts of punishment in *Equal*, or about a 40% unconditional probability of punishment; there were just 271 counts of punishment in *Unequal* (about a 20% unconditional probability of punishment). The difference between *Equal* and *Unequal* is significant ($\chi^2 = 8.37, p < 0.01$).[21] In addition, the unconditional probability of punishment was evenly spread between *Low* (125 cases) and *High* (146 cases), with no significant difference between them ($\chi^2 = 0.25, p = 0.62$).

[21]We report χ^2-tests from a random effects probit model with a treatment indicator.

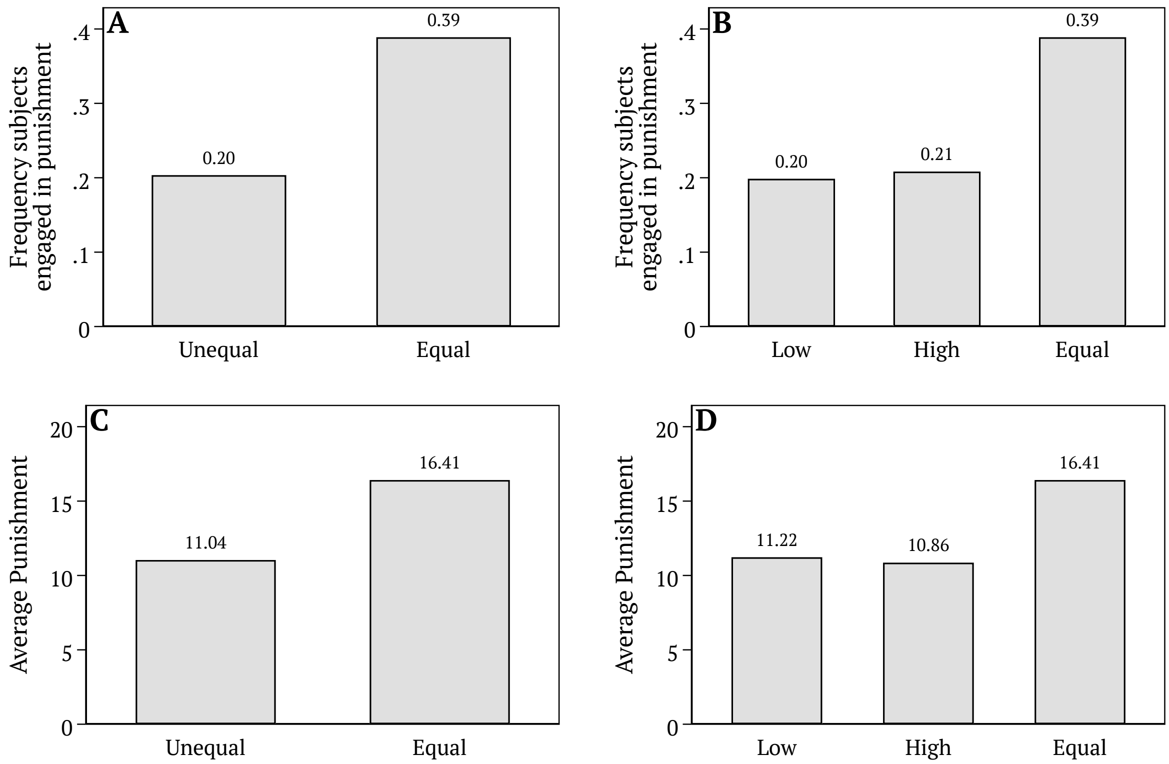

Figure 2: Frequency of punishment and average punishment.

The second point that stands out from Figure 2 is that there was no difference in the magnitude of punishment across treatments. Panels C and D show average punishment for all positive instances (when punishment was greater than zero). While average punishment was larger in *Equal* than *Unequal*, the difference is not significant ($z = 0.53, p = 0.60$). Once again the difference between *Low* and *High* is not significant ($z = 0.84, p = 0.40$).

Next we turn to targeting. To understand how subjects targeted punishment at each other, we need to look at average punishment at different extractions. Moreover, punishment is clearly probabilistic, so we also need to look at the likelihood of punishment at different extractions. In other words, we need to look at *expected* punishment.

To estimate expected punishment we calculated the conditional probability of punishment times the conditional punishment size. We estimated the probability of punishment $P(s > 0)_{ijkt}$ (s for sanction) from a probit regression and the punishment magnitude $\mathbb{E}[s|s > 0]_{ijkt}$

from a Poisson regression (since punishment data are count data). Our full specification is

$$P(s>0)_{ijkt} = \Phi(\beta_0 + \beta_1 g_{ikt} + \beta_2 g_{jkt} + \beta_3 \bar{g}_{kt} + \beta_4 \sum_{j}^{n_k} s_{ijk,t-1} + \beta_5 \text{period} + \mu_i + \epsilon_{ikt}) \quad (4a)$$

$$\mathbb{E}[s|s>0]_{ijkt} = \exp(\alpha_0 + \alpha_1 g_{ikt} + \alpha_2 g_{jkt} + \alpha_3 \bar{g}_{kt} + \alpha_4 \sum_{j}^{n_k} s_{ijk,t-1} + \alpha_5 \text{period} + \nu_i + \varepsilon_{ikt}) \quad (4b)$$

where g_{ikt} is the extraction of subject i in group k and period t, g_{jkt} is the extraction of a target j, $\bar{g}_{kt}$ is the average extraction in group k in period t, $\sum_j^{n_k} s_{ijk,t-1}$ is the total amount of punishment received by i in the previous period, period is the period t, $\mu_i(\nu_i)$ are individual random effects, and $\epsilon_{ikt}(\varepsilon_{ikt})$ is the idiosyncratic error. Standard errors are clustered at the group level.

After estimating the parameters in Equation 4 we plugged them back in and calculated the derivatives for each possible extraction.[22] For *Equal* the range was set to $g_{ikt} \in [0, 50]$. We split the estimation for *Unequal* in two parts: first when extractions pooled *Low* and *High* ($g_{ikt} \in [0, 40]$) and second when extractions revealed *High* ($g_{ikt} \in [41, 60]$). For each extraction in both treatments we calculated the predicted probability of punishment and the predicted magnitude of punishment. Multiplying each probability with the corresponding magnitude gave us the expected punishment from an average group member. Multiplying this number by three gave us the total expected punishment to a subject from their three group members.

Expected punishment is shown in Figure 3. In both treatments, higher extractions from the CPR were targeted with more punishment. However, expected punishments for extractions between $[0, 40]$ were higher in *Equal* than *Unequal*. Reinforcing the results from Figure 2, the difference in expected punishment is driven by a higher conditional probability of punishment in *Equal*. In *Equal*, 35% of all extractions equal to or below 40 were punished. That number falls to 19% in *Unequal*.

[22] The estimated average marginal effects are in Table A1.

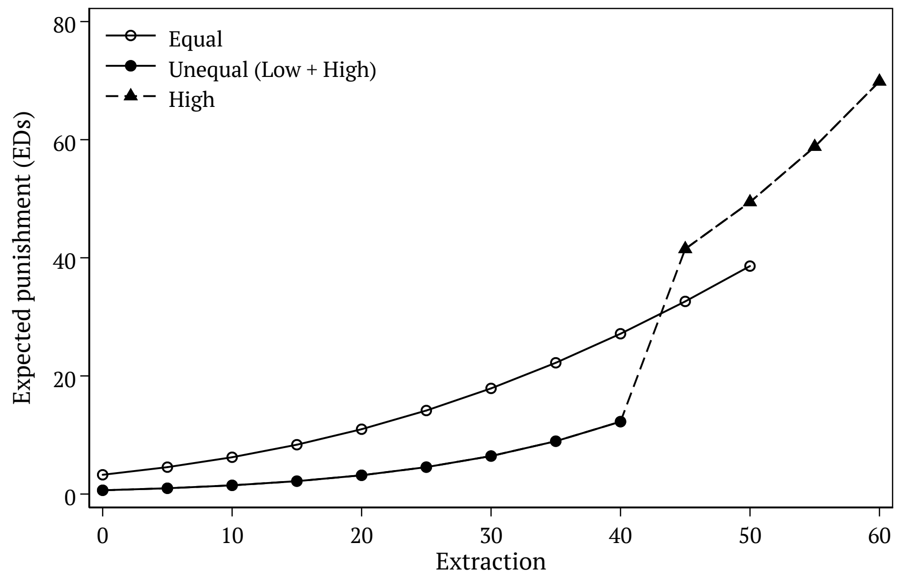

Figure 3: Expected punishment across treatments and endowments.

In addition, Figure 3 shows both targeting and restraint in *Unequal*. Expected punishments in *Unequal* only ramp up at extractions of 40 and above. By definition, such extractions could only have come from *High* types. So when *High* revealed themselves, they were targeted with stiffer punishments. This created an incentive for *High* types to reduce their extractions to 40 or below.

At the same time, subjects in *Unequal* did not try to root out *High* types pooling among *Low* types. That is, we do not observe punishment targeted at extractions below 40 in the hopes of hitting a *High* type. In fact there was very little targeting of extractions around 21 (extractions by *High* if they complied with the Equal Earnings norm). This can be plausibly explained by subjects wanting to avoid mistakenly punishing *Low* types, since misguided punishment can lead to an unravelling of cooperation (Nicklisch et al., 2016).

Finally, we find that subjects were more responsive to punishment in *Unequal*. Like Cason and Gangadharan (2015) and Masclet et al. (2003) we estimate a linear model of changes in extractions in period $t+1$ in response to punishment in t while controlling for the tendency for subjects to adjust their extractions to the group mean (the variable Deviation). We estimate separate models for extractions above and below the average group extraction in a given period while interacting treatment and endowment indicators with punishment received in t.

Our results are shown in Table 4. The coefficient to Deviation is negative and confirms the "regression to the mean" effect. Models (1) and (2) show that subjects who extracted below the group average reduced their extractions in the next round. The effect is consistent across endowments and treatments, and we find no significant difference between *Equal* and *Unequal*.

However, Model (3) provides further evidence that punishment was more effective in *Unequal*. Only in *Unequal* was punishment effective at reducing extractions when subjects extracted above the average, and we find a significant treatment effect. When we break down the effect of punishment by endowments in Model (4) we see that the effect in *Unequal* is driven by *High* types. This is likely due to the fact that *High* types were heavily targeted with punishment when they revealed their endowments, as shown in Figure 3.

Table 4: Changes in extractions in response to punishment. We estimate the same models as Cason and Gangadharan (2015) and Masclet et al. (2003): one for extractions above the group average, another for extractions below the group average. "X" indicates an interaction. We control for subject random effects and we cluster standard errors at the group level. We test for treatment differences between *Equal* and *Unequal* using χ^2 tests.

	Extractions below average		Extractions above average	
	(1)	(2)	(3)	(4)
Deviation in t	-0.839***	-0.852***	-1.128***	-1.175***
	(0.12)	(0.12)	(0.12)	(0.13)
Punishment in t X *Equal*	-0.065***	-0.064***	-0.003	0.001
	(0.01)	(0.01)	(0.04)	(0.04)
Punishment in t X *Unequal*	-0.123***		-0.305***	
	(0.04)		(0.11)	
Punishment in t X *Low*		-0.045***		0.438
		(0.01)		(0.28)
Punishment in t X *High*		-0.160***		-0.482***
		(0.04)		(0.08)
Constant	3.117***	3.127***	-1.932**	-2.593***
	(1.07)	(1.05)	(0.96)	(0.92)
N	451	451	414	414
χ^2 test for treatment differences	1.77		7.31***	

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

4 Discussion

Institutions moderate the effects of inequality in common-pool resource (CPR) management (Andersson and Agrawal, 2011). At the same time, experiments have shown that inequality can alter the effectiveness of institutions like communication (Cardenas, 2003; Hackett et al., 1994) and voting (Margreiter et al., 2005). In this paper we test how inequality in the form of heterogeneous endowments affects peer punishment in a CPR experiment.

Inequality splits the socially optimal level of extractions into three plausible norms around which groups could coordinate: Equal Extractions (subjects have the same extractions), Equal Proportions (subjects extract the same proportion of their endowments) and Equal Earnings (subjects have different extractions but the same earnings). We find that punishment reduced extractions in both our *Equal* and *Unequal* endowment treatments. However, punishment was more effective in *Unequal*. *Low* types appeared to coordinate around the Equal Earnings norm, *High* types matched the extractions of *Low* types, and punishment tightened this coordinate-and-match dynamic. The variation in extractions fell, punishment was sparse, and it was mostly targeted at *High* types who revealed their endowments. By contrast, there was more punishment in *Equal*, but it did not reduce the variation in extractions, and it did not change the behavior of subjects who tended to extract above the group average, leading to significantly lower payoffs. Taken together, our results suggest that inequality made punishment more effective and improved coordination within groups.

One plausible explanation for our results is that the Equal Earnings norm was a focal point around which *Low* types coordinated.[23] Indeed, there is a remarkable pileup of extractions by *Low* types right on top of the Equal Earnings level of extraction in the latter stages of the punishment treatment. While the salience of the Equal Earnings norm could be explained by inequity aversion (Fehr and Schmidt, 1999), it may also simply be because it was the social optimum norm at which *Low* payoffs were highest.[24] Either way, with subjects coordinating around this point, it was easier for subjects to distinguish and then target non-cooperation, thus making punishment more effective. This also explains why we see less punishment in *Unequal*, particularly on the extensive margin (the probability of punishment).

Despite the equivalence of the Equal Earnings norm across treatments, we observe less coordination, more punishment, and lower earnings in *Equal*. Cason and Gangadharan (2015) also document high social costs of punishment in a CPR game with homogeneous endowments. The authors attribute this to the fact that coordination is difficult in nonlinear strategic settings like CPR games because the social optimum and Nash equilibrium are on the interior rather than on the boundary of the choice set. Therefore, our study suggests that inequality may improve coordination by creating focal points which allow groups to

[23]Some studies on public goods games also see coordination by low endowment types around an equal earnings norm. See for example Kingsley (2016).

[24]There is mixed evidence of whether subjects display inequity aversion in social dilemmas; see for example Dreber et al. (2014); Filippin and Raimondi (2016) and Ahn et al. (2003).

better discern cooperative behavior.

The narrow takeaway from our study is that policy makers need to take into account inequality when designing and implementing incentive-based institutions (like peer enforcement) to manage CPRs. The general takeaway from our study – and many other studies, going back to Baland and Platteau (1999) – is that the effect of inequality on conservation is ambiguous. Inequality can take many forms and impact cooperation to conserve CPRs in different ways (Baland et al., 2018). With regards to endowment heterogeneity there are many promising topics for future research.

For starters, future research is required to investigate the relationship between inequality and the coordination of behavior around focal points. Starting from the premise that focal points represent plausible norms of behavior, research could measure the agreement among subjects (as a measure of salience) on the appropriateness of each plausible norm across equal and unequal groups using the coordination game created by Krupka and Weber (2013). Following the results presented in this study, the hypothesis would be that norms rated as more appropriate would enable better coordination. To alter the salience of these plausible norms, endowment heterogeneity could be generated exogenously (as in this study) or endogenously (e.g. using a real effort task), since earned wealth can shift notions about fairness.[25] In our current study with exogenous endowment heterogeneity, we observe coordination around the Equal Earnings norm. If endowment heterogeneity were instead generated through a real effort task, the Equal Earnings norm may fall out of favor, and we may observe coordination around the Equal Contribution norm or the Equal Proportion norm.

Beyond the underlying mechanism determining how inequality creates focal points, it is unclear whether our results hold for different levels of inequality within groups. Extreme inequality in particular may alter behavior and coordination in several ways.

For one, extreme inequality may simplify coordination. This is because endowments determine externalities and outside options. The very poor have few outside options but impose small externalities when they extract the CPR, while the very rich impose large externalities but have more outside options (Dayton-Johnson and Bardhan, 2002).[26] The upshot is that coordinating on the socially optimal level of extractions may be easier if the poor can simply extract at full capacity while the rich substitute away from the CPR. This

[25] Earned endowments lead to significantly different behavior in simple bargaining games (e.g., Korenok et al., 2017; Oxoby and Spraggon, 2008). There is mixed evidence of the effects of earned versus assigned endowments on cooperation in more complex games like social dilemmas (e.g., Antinyan et al., 2015; Spraggon and Oxoby, 2009; Kroll et al., 2007a).

[26] Cardenas et al. (2002) provide supporting evidence of this idea from a CPR field experiment with inequality, in which the returns to the CPR and to the private good vary across subjects. However, the authors do not look at inequality in the form of endowment heterogeneity, nor do they explore differences between mild and extreme inequality.

in stark contrast to our design with mild inequality where *Low* types can still impose non-trivial external costs on *High* types (the symmetric Nash equilibrium in our design is 40, the *Low* endowment), putting pressure on *High* types to coordinate.

Extreme inequality may also influence peer enforcement. Baland and Platteau (1999) suggest that rich agents could use their largess to police the commons, an example of the "Olson effect" in which rich agents privately provide public goods (Olson, 1965). Results from our experiment with relatively mild inequality do not support this idea. When choosing punishment, subjects were only restricted by their initial payoffs, meaning *High* types typically had more power than *Low* types, but results shows that *High* were not significantly more likely to punish, nor impose significantly larger punishment. However, the picture may change if inequality is extreme. On the one hand, high-income agents may take up the role of private enforcer for the common good. On the other hand, the power asymmetry could see them crowd-out lower-income agents from the CPR.

Finally, future research can explore how inequality interacts with multiple institutions. For example, punishment in some (but not all) CPR games is more effective when combined with communication (Cason and Gangadharan, 2016; Janssen et al., 2010; Ostrom et al., 1992), while Kroll et al. (2007b) show that voting is more effective when combined with punishment in public goods games and (Bernard et al., 2013).[27] In a CPR with endowment heterogeneity, voting and punishment may speed up the process of coordinating around focal points and sanctioning deviations.

[27]Bernard et al. (2013) show that how voting is carried also matters.

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A Expected punishment average marginal effects

Table A1: Estimated average marginal effects from Equation 4.

	Extensive margin			Intensive margin		
	(1)	(2)	(3)	(4)	(5)	(6)
	Equal	Unequal	High	Equal	Unequal	High
Target Extraction	0.012***	0.008**	-0.003	0.013***	0.029	0.039
	(0.00)	(0.00)	(0.00)	(0.00)	(0.02)	(0.06)
Own Extraction	0.002	-0.000	-0.002	0.011*	0.013	0.068
	(0.00)	(0.00)	(0.01)	(0.01)	(0.06)	(0.16)
Average Extraction	0.001	-0.001	0.008	-0.045***	0.017	-0.014
	(0.00)	(0.00)	(0.01)	(0.02)	(0.04)	(0.22)
Lagged Sanctions Received	0.000	0.000	-0.000	0.001	0.002	0.014
	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.02)
Period	-0.028***	-0.010**	0.003	-0.076***	0.004	0.051
	(0.01)	(0.00)	(0.01)	(0.02)	(0.08)	(0.13)
N	1344	1302	42	526	243	28

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

B Experiment instructions

Attached are the instructions for *Unequal* × *Punishment*. The instructions are the same in the *Equal* treatment, except all subjects receive the same endowment.

Welcome to the experiment.

Please note that communication between participants is not permitted. If you have a question please raise your hand. This information packet will explain the decision you will make and how your decision affects your individual earnings. The experiment consists of 2 practice questions, 1 practice round and 15 paid rounds. You will be randomly grouped together with 3 other people into groups of 4. Your group will remain the same throughout the experiment. At no point during this experiment will the other members of your group be known to you. All decisions you make will remain anonymous to other participants and to the experiment moderator. You will be compensated, privately and in cash, at the end of the experiment.

You will find your unique identification number on an index card with this packet. Keep your ID confidential, it is used to facilitate all transactions and to maintain your anonymity. This ID will not be shared with any other member of your group.

Please enter this ID into the webpage which is loaded onto the computer and press *Submit*. Your screen should now confirm the ID you entered. If correct press *Continue*. Otherwise raise your hand. You should now see a screen which requires a password in order to continue. The moderator will announce this password once everyone has read these instructions and has successfully answered all practice questions.

When you are ready begin reading through the instructions. If at any time you have a question please raise your hand. You will find 2 examples and 2 practice questions toward the end of the packet. When these have been answered and everyone is comfortable with the instructions we will begin.

Thank You.

Instructions

Decision:

At the beginning of each round you will each receive an endowment of Experimental Dollars (EDs). Your group will consist of **two high endowment** members who will receive **60 ED** each round and **two low endowment** members who will receive **40 ED** each round. This initial allocation of EDs is random and will remain the same throughout the experiment. The decision you are asked to make consists of allocating your EDs between two accounts. Specifically, on each round you will be asked how many of your EDs you would like to invest in Account 1.

Account 1:

You can choose to invest any whole number of your EDs (less than or equal to your endowment) into Account 1. The payoffs you earn from Account 1 depend **not only** on the amount you invest but also on the investment decisions of the other 3 members of your group. The formula for Account 1 payoffs accompanies Table 1 below.

Account 2:

After choosing how many of your EDs to invest in Account 1 your remaining EDs will automatically be invested in Account 2. The payoffs you receive from Account 2 depend **only** on your investment. Each ED you invest in Account 2 gives you a payoff of 1 ED. For example, if a high endowment member invested 20 ED into Account 1 they would earn 40 ED from Account 2 (i.e. their initial endowment of 60 ED minus their Account 1 investment of 20 ED). If a low endowment member invested 20 ED into Account 1 they would earn 20 ED from Account 2 (i.e. 40 ED – 20 ED).

Total Individual Payoff:

Your total earnings *per round* are the sum of your payoffs in Account 1 and your payoffs in Account 2. You can accumulate additional earnings each round. At the conclusion of the experiment your accumulated ED will be converted into cash such that 100 ED is worth $1.00.

Table 1 describes your **total individual payoffs** where the row labeled **X** shows the different investment levels in Account 1 that **you** can choose (in steps of 5 for presentation). The column labeled **Y** shows the different **sums of investment** in Account 1 that the **other 3** members of your group may choose (in steps of 5 for presentation). Tables 1A and 1B show the total payoffs you earn if you choose to invest X and the sum of the investment of the others is Y depending on whether your initial endowment is 60 ED or 40 ED.

In other words, the entry corresponding to column **Y** and row **X** indicates your payoffs in case your investment into Account 1 is X and the sum of the investment of the others is Y.

Total Individual Payoff High Endowment (60 ED)

Notice that for **many** levels of group investment (**Y**) an increase in your individual investment (**X**) increases your individual payoff. To demonstrate, choose a couple values for **Y** and consider your payoffs as **X** increases. However, for **any** level of individual investment (**X > 0**) an increase in group investment (**Y**) decreases your individual payoff. To demonstrate, choose a couple values for **X** and consider your payoffs as **Y** increases. Spend a minute or two looking at Table 1A and ask any questions you have. **Bolded** values are referenced in the example problems.

Table 1A: High Endowment (60 ED)

	X												
Y	**0**	**5**	**10**	**15**	**20**	**25**	**30**	**35**	**40**	**45**	**50**	**55**	**60**
0	60.0	84.4	107.5	129.4	150.0	169.4	187.5	204.4	220.0	234.4	247.5	259.4	270.0
5	60.0	83.8	106.3	127.5	147.5	166.3	183.8	200.0	215.0	228.8	241.3	252.5	262.5
10	60.0	83.1	105.0	125.6	145.0	163.1	180.0	195.6	210.0	223.1	235.0	245.6	255.0
15	60.0	82.5	103.8	123.8	142.5	160.0	176.3	191.3	205.0	217.5	228.8	238.8	247.5
20	60.0	81.9	102.5	121.9	140.0	156.9	172.5	186.9	200.0	211.9	222.5	231.9	240.0
25	60.0	81.3	101.3	120.0	137.5	153.8	168.8	182.5	195.0	206.3	216.3	225.0	232.5
30	60.0	80.6	100.0	118.1	135.0	150.6	165.0	178.1	190.0	200.6	210.0	218.1	225.0
35	60.0	80.0	98.8	116.3	132.5	147.5	161.3	173.8	185.0	195.0	203.8	211.3	217.5
40	60.0	79.4	97.5	114.4	130.0	144.4	157.5	169.4	180.0	189.4	197.5	204.4	210.0
45	60.0	78.8	96.3	112.5	127.5	141.3	153.8	165.0	175.0	183.8	191.3	197.5	202.5
50	60.0	78.1	95.0	110.6	125.0	138.1	150.0	160.6	170.0	178.1	185.0	190.6	195.0
55	60.0	77.5	93.8	108.8	122.5	135.0	146.3	156.3	165.0	172.5	178.8	183.8	187.5
60	60.0	76.9	92.5	106.9	120.0	131.9	142.5	151.9	160.0	166.9	172.5	176.9	180.0
65	60.0	76.3	91.3	105.0	117.5	128.8	138.8	147.5	155.0	161.3	166.3	170.0	172.5
70	60.0	75.6	90.0	103.1	115.0	125.6	135.0	143.1	150.0	155.6	160.0	163.1	165.0
75	60.0	75.0	88.8	101.3	112.5	**122.5**	131.3	138.8	145.0	150.0	153.8	156.3	157.5
80	60.0	74.4	87.5	99.4	110.0	119.4	127.5	134.4	140.0	144.4	147.5	149.4	150.0
85	60.0	73.8	86.3	97.5	107.5	116.3	123.8	130.0	135.0	138.8	141.3	142.5	142.5
90	60.0	73.1	85.0	95.6	105.0	113.1	120.0	125.6	130.0	133.1	135.0	135.6	135.0
95	60.0	72.5	83.8	93.8	102.5	**110.0**	116.3	121.3	125.0	127.5	128.8	128.8	127.5
100	60.0	71.9	82.5	91.9	100.0	106.9	112.5	116.9	120.0	121.9	122.5	121.9	120.0
105	60.0	71.3	81.3	90.0	97.5	103.8	108.8	112.5	115.0	116.3	116.3	115.0	112.5
110	60.0	70.6	80.0	88.1	95.0	100.6	105.0	108.1	110.0	110.6	110.0	108.1	105.0
115	60.0	70.0	78.8	86.3	92.5	97.5	101.3	103.8	105.0	105.0	103.8	101.3	97.5
120	60.0	69.4	77.5	84.4	90.0	94.4	97.5	99.4	100.0	99.4	97.5	94.4	90.0
125	60.0	68.8	76.3	82.5	87.5	91.3	93.8	95.0	95.0	93.8	91.3	87.5	82.5
130	60.0	68.1	75.0	80.6	85.0	88.1	90.0	90.6	90.0	88.1	85.0	80.6	75.0
135	60.0	67.5	73.8	78.8	82.5	85.0	86.3	86.3	85.0	82.5	78.8	73.8	67.5
140	60.0	66.9	72.5	76.9	80.0	81.9	82.5	81.9	80.0	76.9	72.5	66.9	60.0

Total Individual Payoff: $\frac{X}{X+Y}\left[6 * (X+Y) - .025 * (X+Y)^2\right] + (60 - X)$

(Account 1 Payoff) + (Account 2 Payoff)

Total Individual Payoff Low Endowment (40 ED)

Notice that for **many** levels of group investment (**Y**) an increase in your individual investment (**X**) increases your individual payoff. To demonstrate, choose a couple values for **Y** and consider your payoffs as **X** increases. However, for **any** level of individual investment (**X > 0**) an increase in group investment (**Y**) decreases your individual payoff. To demonstrate, choose a couple values for **X** and consider your payoffs as **Y** increases. Spend a minute or two looking at Table 1B and ask any questions you have. **Bolded** values are referenced in the example problems.

Table 1B: Low Endowment (40 ED)

	X								
Y	**0**	**5**	**10**	**15**	**20**	**25**	**30**	**35**	**40**
0	40.0	64.4	87.5	109.4	130.0	149.4	167.5	184.4	200.0
5	40.0	63.8	86.3	107.5	127.5	146.3	163.8	180.0	195.0
10	40.0	63.1	85.0	105.6	125.0	143.1	160.0	175.6	190.0
15	40.0	62.5	83.8	103.8	122.5	140.0	156.3	171.3	185.0
20	40.0	61.9	82.5	101.9	120.0	136.9	152.5	166.9	180.0
25	40.0	61.3	81.3	100.0	117.5	133.8	148.8	162.5	175.0
30	40.0	60.6	80.0	98.1	115.0	130.6	145.0	158.1	170.0
35	40.0	60.0	78.8	96.3	112.5	127.5	141.3	153.8	165.0
40	40.0	59.4	77.5	94.4	110.0	124.4	137.5	149.4	160.0
45	40.0	58.8	76.3	92.5	107.5	121.3	133.8	145.0	155.0
50	40.0	58.1	75.0	90.6	105.0	118.1	130.0	140.6	150.0
55	40.0	57.5	73.8	88.8	102.5	115.0	126.3	136.3	145.0
60	40.0	56.9	72.5	86.9	100.0	111.9	122.5	131.9	140.0
65	40.0	56.3	71.3	85.0	97.5	108.8	118.8	127.5	135.0
70	40.0	55.6	70.0	83.1	95.0	105.6	115.0	123.1	130.0
75	40.0	55.0	68.8	81.3	92.5	**102.5**	111.3	118.8	125.0
80	40.0	54.4	67.5	79.4	90.0	99.4	107.5	114.4	120.0
85	40.0	53.8	66.3	77.5	87.5	96.3	103.8	**110.0**	115.0
90	40.0	53.1	65.0	75.6	85.0	93.1	100.0	105.6	110.0
95	40.0	52.5	63.8	73.8	82.5	90.0	96.3	101.3	105.0
100	40.0	51.9	62.5	71.9	80.0	86.9	92.5	96.9	100.0
105	40.0	51.3	61.3	70.0	77.5	83.8	88.8	92.5	95.0
110	40.0	50.6	60.0	68.1	75.0	80.6	85.0	88.1	90.0
115	40.0	50.0	58.8	66.3	72.5	77.5	81.3	83.8	85.0
120	40.0	49.4	57.5	64.4	70.0	74.4	77.5	79.4	80.0
125	40.0	48.8	56.3	62.5	67.5	71.3	73.8	75.0	75.0
130	40.0	48.1	55.0	60.6	65.0	68.1	70.0	70.6	70.0
135	40.0	47.5	53.8	58.8	62.5	65.0	66.3	66.3	65.0
140	40.0	46.9	52.5	56.9	60.0	61.9	62.5	61.9	60.0
145	40.0	46.3	51.3	55.0	57.5	58.8	58.8	57.5	55.0
150	40.0	45.6	50.0	53.1	55.0	55.6	55.0	53.1	50.0
155	40.0	45.0	48.8	51.3	52.5	52.5	51.3	48.8	45.0
160	40.0	44.4	47.5	49.4	50.0	49.4	47.5	44.4	40.0

Total Individual Payoff (40 ED): $\frac{X}{X+Y}\left[6 * (X+Y) - .025 * (X+Y)^2\right] + (40 - X)$

(Acct. 1 Payoff) + (Acct. 2 Payoff)

Total Group Payoff

Table 2 describes the **total group payoff**. That is, the sum of the total individual earnings for each member of the group. Where **X** + **Y** represents the sum of Account 1 investment by the group. Notice that total group payoff increases until a total of **100 ED** are invested into Account 1 and decreases thereafter. **Bolded** values are referenced in the example problems.

Table 2

X + Y	Group Earnings
0	200.0
5	224.4
10	247.5
15	269.4
20	290.0
25	309.4
30	327.5
35	344.4
40	360.0
45	374.4
50	387.5
55	399.4
60	410.0
65	419.4
70	427.5
75	434.4
80	440.0
85	444.4
90	447.5
95	449.4
100	**450.0**
105	449.4
110	447.5
115	444.4
120	**440.0**
125	434.4
130	427.5
135	419.4
140	410.0
145	399.4
150	387.5
155	374.4
160	360.0
165	344.4
170	327.5
175	309.4
180	290.0
185	269.4
190	247.5
195	224.4
200	200.0

Example Questions

Answers to all example questions are **bolded** in the corresponding table for your convenience.

1. Realizing that total group payoff is maximized when 100 ED are invested into Account 1 suppose that **each** member of your group invests **25 ED** in Account 1.

   a. What is the total individual payoff for each **high** and **low** endowment member of the group?

   Use Table 1A and 1B. Because each member of the group invested 25 ED into Account 1 we can find your total individual payoff using X = 25 and Y = 25 + 25 + 25 = 75. Recall that Y is simply the summation of the ED invested into Account 1 by the other 3 members of the group. So we simply need to determine the number at the intersection of X = 25 and Y = 75 in the Table.

   Each high endowment group member would earn 122.5 ED on this round (Table 1A)
   Each low endowment group member would earn 102.5 ED on this round (Table 1B)

   b. What is the total group payoff?

   Use Table 2. To determine total group payoff we need to determine the total number of EDs invested into Account 1. That is we need to find X + Y which in this case is 25 + 75 = 100.

   The group would earn 450 ED on this round

2. Suppose that in order to increase their individual earnings the **two low endowment** members of your group increase their Account 1 investment to **35 ED each**. Recall that for many levels of group investment (Y) an increase in individual investment (X) increases individual payoff. Assume that the **two high endowment** members maintain **25 ED** in Account 1.

   a. What is the total individual payoff for each of the **two low endowmen**t group members who invested **35 ED** in Account 1?

   *Use Table 1B. In this case we want to set X = 35 and **Y = 25 + 25 + 35 = 85**. Note that Y reflects the investment choices of two members at 25 ED and 1 member at 35 ED.*

   Each low endowment group member would earn 110 ED on this round

   b. What is the total individual payoff for each of the **two high endowment** group members who invested **25 ED** into Account 1?

   *Use Table 1A. In this case we want to set X = 25 and **Y = 25 + 35 + 35 = 95**. Again Note that Y reflects the investment choices of two members at 35 and 1 member at 25*

   Each high endowment group member would earn 110 ED on this round

   c. What is the total group payoff?

   Use Table 2 and simply determine X + Y. In each case, either using 35 + 85 or 25 + 95 the total group investment is 120 ED into Account 1.

   The group would earn 440 ED on this round

Practice Questions

1. In response to the additional Account 1 investment suppose that both high endowment group members choose to invest 35 ED into Account 1. Therefore, now all members of the group are investing 35 ED into Account 1.

   a. What is the total individual payoff for each **high endowment** member?

      For high endowment earnings use Table 1A with X = 35 and Y = 35 + 35 + 35 = 105.

   b. What is your total individual payoff for each **low endowment** member?

      For low endowment earnings use Table 1B with X = 35 and Y = 35 + 35 + 35 = 105.

   b. What is the total group payoff?

      Use Table 2 with X + Y = 140.

2. In order to reduce group investment into Account 1 suppose both **high endowment** members choose to invest **20 ED** into Account 1 and that both **low endowment** members choose to invest **30 ED** into Account 1.

   a. What is the total individual payoff for each **high endowment** member who invested **20 ED** in Account 1?

      Use Table 1A with X = 20 and ***Y = 20 + 30 + 30 = 80****. Note that Y reflects the investment choices of two members at 30 ED and 1 member at 20 ED.*

   b. What is the total individual payoff for each **low endowment** member who invested 3**0 ED** into Account 1?

      Use Table 1B with X = 30 and ***Y = 20 + 20 + 30 = 70****. Again Note that Y reflects the investment choices of two members at 20 ED and 1 member at 30 ED.*

   c. What is the total group payoff?

      Use Table 2 with X + Y = 100 [30 + 70 or 20 + 80].

Reductions

There is another decision that affects your earnings. After each round you will be shown the individual Account 1 investment decisions of each member of your group by random ID. These random IDs will change each round.

With this information you will have the opportunity to pay a Fee of 1 ED in order to Fine another player 3 ED. Each Fine of 3 ED you impose will cost you 1 ED. You can choose to impose any number of Fines on any number of other players but you must be able to pay the total Fee from the **current** rounds' earnings. The Fees and Fines are the same for each of you and will remain the same throughout the experiment. All **Fees paid** and **Fines received** will be subtracted from your earnings

For example, suppose that after a particular round you decide to impose 3 Fines on ID 10, 2 Fines on ID 20 and 0 Fines on ID 30. For simplicity assume that no other player decides to impose any Fines. You have therefore decided to impose 5 Fines (3+2) each of which will cost you 1 ED. Your earnings will be reduced by 5 ED in **Fees paid**. Further, ID 10, who received 3 Fines will have their earnings reduced by 9 ED (3*3) in **Fines received** and ID 20 will have their earnings reduced by 6 ED (2*3) in **Fines received**.

Now, suppose that ID 20 decided to impose 4 Fines on you. Having paid to impose 4 Fines ID 20's earnings will be reduced by an additional 4 ED in **Fees paid** for a total reduction of 10 ED and your earnings will be reduced by an additional 12 ED (4*3) in **Fines received** for a total reduction of 17 ED.

Each of you will learn that your earnings have been reduced by **Fees paid** and **Fines received** but you will not know who has reduced your earnings or how many members of the group have chosen to reduce your earnings.

3. Suppose that after a particular round you decide to place a total of 6 Fines on other players and that the members of your group place a total of 5 Fines on you. What are your total Fees paid and Fines received?

Password

Once all participants are comfortable with the instructions and have successfully completed the practice questions the password will be announced and we can continue with the experiment. Please remember that communication between participants is not permitted. Thank you for your patience, we will begin shortly.

Demonstration Rounds

Before we begin we will play 1 practice round to demonstrate the game. The result of this round is not included in your accumulated earnings.

Round by round information: After each round and after each participant has made their decision you will be provided with the following information:

1. Your individual Account 1 investment **X**
2. The sum of all Account 1 investment (including yours) by the group **X+Y**
3. Your Account 1 payoffs
4. Your Account 2 payoffs
5. Your total individual payoffs for the current round and
6. Your accumulated earnings up to this point in the game.

All information from previous rounds is always available by clicking the link labeled **History**.

Round 1:
To demonstrate that it is possible to equalize earnings across all players while maximizing group payoffs let's have **each high endowment member invest 21 ED** into Account 1 and **each low endowment member invest 29 ED** into Account 1. Press *submit*.

You should see your Account 1 investment and the group investment into Account 1 (Which should be 100 ED). You should also see that your total individual earnings are 112.5 on this round.

Please be patient, your screen will update only when each member of your group has submitted their choice. ***Do not hit the back button****. Please raise your hand if you think something is not working properly.*

Now, press *continue* and you should see a page depicting the individual investment choices of each member of your group by random ID (which should be either 21 or 29). To demonstrate how the reductions appear during the game let's have each of you impose 3 fines on **each** member of your group and press *submit*. You should now see that your earnings have been reduced by **9 ED in Fees Paid** and **27 in Fines Received** for a total earnings of 76.5 (i.e. 112.5 – 36).

Note that while you do not need to impose any fines after a particular round you will need to input an integer into this field to proceed (i.e. enter a 0 (zero) to impose no fines).

If there are no further questions we will begin the experiment. When you are ready click continue.