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Abstract

We study centralized and decentralized enforcement in social dilemmas with income inequality and incomplete information. Subjects are randomly assigned different endowments before playing public goods games in which endowments can be observed or unobserved. After gaining experience with peer punishment and a simple central authority, groups voted on their preferred enforcement institution. Under complete information (endowments observed), most groups voted for peer punishment. Under incomplete information (endowments unobserved), most groups voted for central authority, and results suggest this preference was largely driven by subjects with lower incomes. Since free-riding could not be targeted when incomes were not observed, subjects with larger incomes tended to under-contribute, encouraging groups to self-impose central authority.

Keywords	Public goods; peer punishment; central authority; cooperation; experiment; institutions
Taxonomy	Public Goods, Income Inequality, Asymmetric Information
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Corresponding Author	Lawrence De Geest
Corresponding Author's Institution	Suffolk University
Order of Authors	Lawrence De Geest, David Kingsley

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Research Data Related to this Submission

Data set	https://github.com/lrdegeest/InstitutionalChoice

Data and analysis

Repository for working paper "Endowment Heterogeneity, Incomplete Information & Institutional Choice in Public Good Experiments". Includes data and analysis.

SUFFOLK UNIVERSITY

Lawrence R. De Geest
Department of Economics
Suffolk University
73 Tremont Street
Boston, MA 02108

January 21, 2019

Dear editors,

Myself and my co-author, David C. Kingsley of the University of Massachusetts-Lowell, are very excited to submit our manuscript, "Endowment Heterogeneity, Incomplete Information & Institutional Choice in Public Good Experiments", for your consideration.

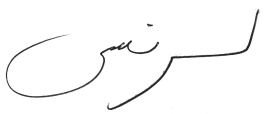
Our paper explores the choice between two enforcement institutions in a social dilemma with income inequality. Subjects are assigned endowments (Low, Middle, or High) and are then assigned to treatments that vary whether or not they can observed the endowments of other group members. In each treatment, subjects gain experience with two enforcement institutions, a central authority with a constant fixed cost and peer punishment with no fixed cost, before voting for which one to implement. To the best of our knowledge, ours is the first study of its kind to examine both income inequality, asymmetric information and institutional choice in public goods.

Our main finding is that groups prefer enforcement by central authority when endowments are unobserved – though this preference is driven by subjects with lower incomes, who bear most of the cost of incomplete information in a peer punishment regime. These subjects receive more punishment even when fully cooperative, while subjects with high endowments choose contributions that obscure their endowments, disabling the effective targeting of freeriders. As a result, we see that low-endowment cooperators are discouraged to further cooperate, while high-endowment freeriders are encouraged to go on freeriding. By contrast, when endowments are observed, targeting is restored, and groups by and large choose to continue with peer punishment.

Our findings compliment other recent studies and suggest that income inequality and incomplete information can explain preferences for central authorities. Given the relevance of information asymmetries and income inequality to real-life social dilemmas, we believe our paper will be of great interest to readers of your journal. Moreover, we believe there is a lot of work yet to be done on this topic, and as such discuss the potential future avenues for this research.

We hope you enjoy our manuscript and we look forward to your response.

Sincerely,



Lawrence R. De Geest

- We vary the observability of endowments in a heterogeneous-endowment public good experiment.
- Groups try out central authority and peer punishment before voting which one to implement.
- In *Unobserved* subjects with lower (higher) endowments prefer central authority (peer punishment).
- Cooperating low endowment members receive more punishment in *Unobserved*.
- Subjects with higher endowments in *Unobserved* undermine targeted sanctions by disguising contributions.

Endowment Heterogeneity, Incomplete Information & Institutional Choice in Public Good Experiments

Lawrence R. De Geest*, David C. Kingsley

Abstract

We study centralized and decentralized enforcement in social dilemmas with income inequality and incomplete information. Subjects are randomly assigned different endowments before playing public goods games in which endowments can be observed or unobserved. After gaining experience with peer punishment and a simple central authority, groups voted on their preferred enforcement institution. Under complete information (endowments observed), most groups voted for peer punishment. Under incomplete information (endowments unobserved), most groups voted for central authority, and results suggest this preference was largely driven by subjects with lower incomes. Since free-riding could not be targeted when incomes were not observed, subjects with larger incomes tended to undercontribute, encouraging groups to self-impose central authority.

Keywords: Public goods, peer punishment, central authority, cooperation, experiment, institutions *JEL:* C92, D02, H41

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**Department of Economics, University of Massachusetts Lowell.
Email address: ldegeest@suffolk.edu (Lawrence R. De Geest)

1. Introduction

Cooperation in social dilemmas requires well-designed institutions that spur individuals to act in the common interest. One of the most straightforward ways to promote cooperation is to punish selfishness with monetary sanctions. But who should levy these sanctions? Enforcement can be meted out by the individuals themselves through peer punishment or by a central authority. While both institutions provide similar benefits – they increase the private cost of acting selfishly – they impose different social costs. Peer punishment can be low cost when a credible threat is established and sanctions need not be used, or it can be high cost if anti-social punishment breaks out. On the other hand, enforcement by a central authority necessarily entails a fixed cost, even under full cooperation. For example, in the absence of speeding, the police and the court system which supports them remains costly. It is thus of interest to determine when, and under what conditions, peer punishment is abandoned in favor of central authority.

Nicklisch et al. (2016) suggest that *imperfect information* may explain why modern society is largely characterized by central authorities rather than peer punishment. When individuals do not perfectly observe each other's behavior because of some noise beyond their control, peer punishment becomes less effective. Specifically, imperfect information makes it difficult to target free-riders, limiting the costs of free-riding. At the same time, the mis-guided punishment of cooperators reduces the benefit of cooperation and risks sparking retaliatory punishment. Beyond the research of Nicklisch et al. (2016), which we return to below, this intuition is supported in two papers which investigate the effect of imperfect information within peer punishment. Both Grechenig et al. (2010) and Ambrus and Greiner (2012) report that introducing noise into peer punishment mechanisms significantly lowers net earnings relative to conditions without the opportunity to punish. Briefly, they observe that more, but less intense, punishment is deployed. The effect of this punishment, relative to the effect observed under perfect information, is that it attenuates the increase in contributions from free-riders who are punished and exacerbates the decrease in contributions from cooperators who are punished.[1]

Nicklisch et al. (2016) investigate a similar peer punishment regime with noise but allow group members to self-select into a centralized punishment or a no punishment regime. The

[1] In related work, Fellner et al. (2011) study a public goods game with heterogeneous productivity types, high and low, and vary whether a) subjects are aware of the heterogeneity and b) whether subjects can link contributions to types. The authors also find that subjects exploit information asymmetries. When subjects are aware of the heterogeneity, but cannot link contributions to types, high types contribute less.

centralized punishment regime effectively delegated the peer punishment mechanism to a single member of the group. Their results suggest that as the amount of noise increases, the preference for peer punishment decreases. However, this does not necessarily imply a preference for centralized punishment as an anti-dote to imperfect information. The preference for the centralized punishment regime in the noisy treatments is sensitive to the behavior of the delegated authority. Centralized punishment is preferred in the noisy treatments when the delegated authorities impose less mis-guided punishment.[2]

The literature thus, intuitively, suggests that preferences across institutions designed to solve social dilemmas are sensitive to the relative effectiveness of those institutions. Further, the relative effectiveness across institutions appears to be, at least partially, explained by imperfect information. It follows that institutional preferences might also be tied to *incomplete information*, the case when individuals do not perfectly observe certain characteristics about others (Harsanyi, 1967). One way incomplete information could come about in social dilemmas is if income (more broadly, one's available resources) is heterogeneous but unobserved, giving individuals the opportunity to choose contributions that do not reveal their relative income and thus their maximum contribution.

In this paper, we show there is a link between *incomplete information* and a preference for central authority when there is income inequality within groups. Income inequality is implemented by exogenously varying subject endowments.[3] We develop three, linear public good experiments: one in which each group member receives an equal endowment, and two in which group members receive unequal endowments: Low (10), Medium (20) and High (30). Across the unequal, heterogeneous endowment treatments, we alter the information about endowments available to group members. In our *Observed* treatment, group members always observe group member endowments alongside their contributions after each period. In our *Unobserved* treatment, group members only observe the contributions of group members, and receive no information concerning their endowment. In all treatments, a subject always knows their own endowment, as well as the distribution of endowments across group members.[4] After gaining experience with both peer punishment and central authority, groups vote for an enforcement institution for the remainder of their session.

[2]Corroborating evidence for this preference is provided by Ertan et al. (2009), who observed groups as they voted on how to implement peer punishment in a public goods game. By and large, groups elected to punish only low contributers, while no group ever allowed the punishment of high contributers.

[3]In this paper we use the terms "income" and "endowment" interchangeably.

[4]Note that complete versus incomplete information here refers to whether subjects observed each other's incomes. All our treatments have perfect information since subjects perfectly observe each other's contributions.

In contrast to Nicklisch et al. (2016), the alternative institution here is a deterrent central authority with perfect information which enforces the social optimum equilibrium. At the same time, it also imposes a relatively high fixed cost. As such, incomplete information primarily impacts the effectiveness of the peer punishment mechanism. In this sense, we are not investigating behavior across institutions; rather, we create an alternative which ought to be equally effective across all treatments and investigate how the relative effectiveness of peer punishment alters institutional preference across treatments. Therefore our design compliments Nicklisch et al. (2016) and motivates incomplete information as another plausible mechanism through which central authority institutions have come to dominate modern society.

Our main finding is that groups with heterogeneous endowments and incomplete information tended to choose central authority over peer punishment, largely due to the votes from subjects with lower incomes. At the same time, heterogeneous groups with complete information tended to prefer peer punishment at the same rates as homogeneous groups. Therefore, as suggested in Reuben and Riedl (2013), endowment heterogeneity alone does not drive the preference for central authority. Instead, this preference appears to stem directly from the ineffectiveness of peer punishment to improve overall group welfare, particularly for subjects with lower incomes. Consistent with other studies, our results suggest peer punishment was ineffective because it could not be targeted at free-riders. Subjects with high incomes earned similar payoffs across information treatments since they were able to evade sanctions in incomplete information by under-contributing and thus not revealing their endowments. By contrast, subjects with lower incomes earned significantly less under incomplete information, and were often met with sanctions, even when they contributed their full endowments. The costs of incomplete information thus fell mostly on subjects with lower incomes, who were discouraged to cooperate, while subjects with higher incomes were encouraged to free-ride. When endowments were observed, targeting improved, and both the disincentive to free-ride and incentive to cooperate were restored.

Our study compliments recent work on imperfect information and income inequality in social dilemmas, reinforcing the idea that income inequality combined with information asymmetries can undermine self-governance. While the direct contribution of this paper is to the experimental literature on social dilemmas, our results extend to real-world scenarios that involve consensus and enforcement. For instance, research on economic development suggests that societies with higher levels of income inequality under-invest in public goods like education (Easterly, 2007). Similarly, studies on micro-finance in developing countries

find that group lending is more successful when groups are homogeneous; since micro-finance loans rely on peer monitoring and enforcement to ensure repayment, homogeneous groups are better at creating consensus and enforcing it (Cassar et al., 2007). In general, evidence suggests that income inequality limits the ability of groups to reach consensus (e.g. about what constitutes fairness) and therefore limits their capacity for self-governance in settings with asymmetric information. Our results suggest that groups in these environments will seek out well-designed and unbiased central authorities to promote cooperation.

The rest of our paper proceeds as follows. Section 2 presents our experiment design and hypotheses. Section 3 presents and discusses our results. Finally, Section 4 concludes and proposes future directions for this research.

2. Experimental design and procedures

The experiment consisted of three treatments: a control in which endowments were equal and thus observed (*Equal*), a treatment in which endowments were unequal and unobserved (*Unobserved*), and finally a treatment in which endowments were unequal and observed (*Observed*). Each treatment contained eighteen periods broken into six phases of three periods each and was coded in z-Tree (Fischbacher, 2007). Groups of five subjects were randomly formed at the beginning and maintained for all eighteen periods. Instructions for each phase were distributed at the beginning of each phase and required each participant to correctly answer a set of comprehension questions before the experiment would continue.[5] The sections below describe each phase of the experiment. A summary illustration of procedures within a treatment is provided here in Figure 1.

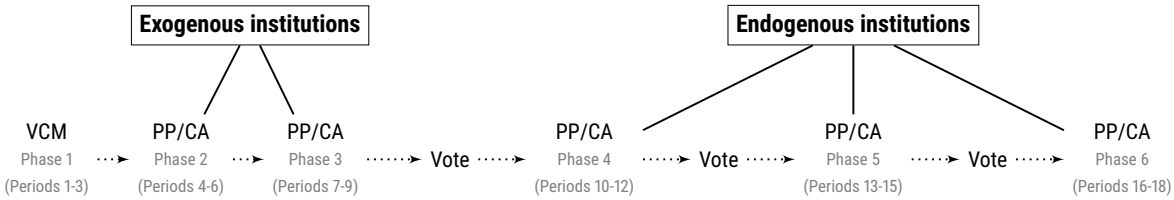

Figure 1: Summary of our treatment phases. Each group participated in six phases. The first phase (periods 1-3) was a VCM (i.e. no punishment). In phases 2 (periods 4-6) and 3 (periods 7-9) either peer punishment or central authority was exogenously imposed, with the order of imposition reversed across sessions. Groups could then vote on three occasions for an institution to be implemented in phases 4 (periods 10-12), 5 (periods 13-15), and 6 (periods 16-18).

[5]Complete experimental instructions are available in the appendix in Section AppendixC.

2.1. Voluntary contribution baseline

Phase 1 (periods 1 - 3) in all treatments introduced the standard voluntary contribution mechanism (VCM), wherein subjects decided how much to contribute to a group account. Each subject was given a fixed endowment of experimental currency (herein referred to as experimental dollars, EDs) which could be allocated between a private account and a group account. Payoffs to subject i were

$$\pi_i = (e_i - x_i) + \alpha \sum_{j=1}^{n} x_j \tag{1}$$

where x_i is the subject's contribution to the group account, e_i is the subject's endowment, $\alpha = 0.4$ is the marginal per capita return (MPCR) from the public good, and $\sum_{j=1}^{n} x_j$ represents the sum of contributions to the group account from all group members. With n players, $\frac{1}{n} < \alpha < 1$, and a known last period, there is a unique, symmetric Nash equilibrium where everybody free-rides and contributes nothing to the public good. Similarly, there is a symmetric social optimum where subjects contribute their entire endowment to the public good.

In the baseline treatment each participant received the same, fixed, endowment of 20 EDs. We will refer to this treatment as *Equal*. In the other two treatments the endowments were heterogeneous. In both heterogeneous treatments the distribution of group endowments was identical and known. Each group was composed of two High endowment members who received 30 EDs, one Middle endowment member who received 20 EDs, and two Low endowment members who received 10 EDs. Endowments in the heterogeneous treatments were randomly assigned and maintained for the entire experiment (i.e. once a High type always a High type). Note that the aggregate level of endowments, or the resources available for contributing towards the public good, was the same across all three treatments ($\sum_{i=1}^{n} e_i = 100$ EDs).

In all treatments subjects were shown the following information after each group member had made their contribution decision: aggregate contribution to the group account; the individual contributions of their group members by random ID; their individual period earnings; their total earnings (equal to the sum of their individual period earnings); and a history of outcomes in previous periods.[6]

Additionally in the heterogeneous endowment treatments, group members either ob-

[6]The random ID and the order of presentation of the contributions of one's group members was randomized each period to avoid reputation effects.

served or did not observe the endowment of each group member after each period. For example, in the *Observed* treatment, group members not only observed the contribution decisions but they also observed that group member's endowment. In contrast, in the *Unobserved* treatment, group members would only observe the contribution decisions and would not observe the group member's endowment.

The experimental literature on public goods has established that contributions tend to lie between the Nash and social outcomes, and decline with repetition (Davis and Holt, 1993; Ledyard, 1995; Chaudhuri, 2011). In the absence of any institutional constraints on behavior results from linear public good experiments suggests that endowment heterogeneity has no effect on average contributions or earnings (Buckley and Croson, 2006; Reuben and Riedl, 2013). Given the lack of a behavioral response to endowment heterogeneity, the availability of endowment information is not expected to effect behavior. This leads to our first hypothesis:

Hypothesis 1. *In the VCM periods, average earnings will not be statistically different across the Equal, Observed, or Unobserved treatments.*

2.2. Exogenously imposed institutions

During phase 2 (periods 4 - 6) and phase 3 (periods 7 - 9), each group was introduced to two types of institutions, peer punishment and central authority, explained in detail below. To account for order effects the order of imposition was reversed across the six experimental sessions conducted for each treatment. Therefore, approximately half of all groups in each treatment had the peer punishment mechanism imposed in phase 2 followed by the central authority mechanism in phase 3, while the order was reversed for the other half.[7]

2.2.1. Peer punishment mechanism

Peer punishment alters i's payoffs as follows:

$$\pi_i = (e_i - x_i) + \alpha \sum_{j=1}^{n} x_j - c \sum_{j \neq i}^{n-1} P_{ij} - r \sum_{j \neq i}^{n-1} P_{ji} \tag{2}$$

where P_{ij} represents the number of reduction points that i imposes on other group members j at a cost of $c = 1$, and P_{ji} represents the number of reduction points that other group

[7]Specifically, 11, 12, and 11 groups in *Equal*, *Observed*, and *Unobserved* participated in peer punishment followed by central authority while 12 groups in all treatments participated in the central authority followed by peer punishment. As described in the appendix in Section AppendixA, no order effects were observed.

members j impose upon i at a cost of $r = 4$. In order to avoid excessive losses subjects were allowed to impose up to 10 reduction points per period and, similar to Reuben and Riedl (2013), losses on any given period were bounded at zero unless the subject imposed punishment.[8] The costs associated with imposing "Reduction Points" were referred to as "Administrative Costs" and costs associated with receiving "Reduction Points" were referred to as "Reduction Costs".

Since punishment is costly to deploy, group welfare only improves when contributions rise sufficiently to offset these costs.[9] Research suggests that peer punishment tends to be effective when contribution norms are unambiguous and thus deviations are easily observed and can be punished (Gurerk et al., 2006; Nikiforakis et al., 2012; Reuben and Riedl, 2013; Neitzel and Saaksvuori, 2013; Kingsley, 2016). In public good experiments several plausible norms may emerge. Reuben and Riedl (2013) suggest that efficiency, equal payoffs, equal contributions, and contributing an equal proportions of one's endowment are all plausibly appealing.[10]

Reuben and Riedl (2013) investigate the effect of endowment heterogeneity on peer punishment with complete information, similar to our *Observed* treatment. Results suggests that groups successfully deploy punishment to increase contributions and induce cooperation. This suggest that endowment heterogeneity alone will not substantially hinder the effectiveness of peer punishment:

Hypothesis 2. *Average earnings within the peer punishment mechanism will not be statistically different across the Equal and Observed treatments.*

The importance of the emergent contribution norm is that punishment can be effectively targeted at free-riders. As mentioned above, Grechenig et al. (2010) and Ambrus and Greiner (2012), investigate the impact of imperfect information on peer punishment. When contributions are observed with error peer punishment is found to lower group welfare below their

[8] Similar to Reuben and Riedl (2013) payoffs under the peer punishment mechanism are in fact $\pi_i = \max[0, (e_i - x_i) + \alpha \sum_{j=1}^{n} x_j - r \sum_{j \neq i}^{n-1} P_{ji}] - c \sum_{j \neq i}^{n-1} P_{ij}$.

[9] Peer punishment has been shown able to improve welfare, but it is not uncommon to observe no increase or even a decrease in group welfare when peer punishment is relatively weak, expensive to deploy, or when retaliatory/mis-guided punishment breaks out (Fehr and Gächter, 2002; Bochet et al., 2006; Sefton et al., 2007; Egas and Riedl, 2008; Nikiforakis, 2008; Chaudhuri, 2011).

[10] In linear, boundary solution, public good experiments with homogeneous endowments all of these plausible contribution norms coincide with each member contributing their entire endowment. With endowment heterogeneity norms of efficiency, equal proportions, and equal payoffs continue to coincide. Nonlinear public good experiments can be used so that norms of equal payoffs, equal contributions, and contributing an equal proportions of one's endowment can each be efficient to differentiate between these competing norms without foregoing efficiency (Kingsley, 2016).

no-punishment baseline levels. Results suggest that when free-riders were punished they did not increase their subsequent contributions. This suggests that the received punishment was either too mild to alter incentives or that free-riders expected imperfect information to shield them from future punishment. Further, when cooperators were punished they tended to decrease their subsequent contributions. In this sense, imperfect information mitigates the costs of free-riding as well as the benefits of cooperation under peer punishment.

Following this logic, incomplete information is expected to have a similar effect on peer punishment. In *Unobserved* there is an opportunity for group members with higher endowments to conceal their status by lowering their contribution toward levels observed by group members with lower endowments. Thus, there is an opportunity for free-riding high members to avoid punishment as well as cooperating low members to be punished. If behavior under incomplete information is similar to that observed under imperfect information we expect peer punishment to be less effective:

Hypothesis 3. *Average earnings within the peer punishment mechanism will be significantly higher in the Equal and Observed treatments relative to the Unobserved treatment.*

2.3. Central authority

Finally, central authority alters i's payoffs as follows:

$$\pi_i = (e_i - x_i) + \alpha \sum_{j=1}^{n} x_j - c - ps\,(e_i - x_i) \tag{3}$$

where $c = 6$ is the fixed cost of implementing the central authority for each group member, $s = 1.2$ is the level of the sanction, and $p = 1$ is the probability of being monitored. The sanction is proportional to the deviation from the social optimum contribution $(e - x_i)$. Since $ps > 1 - \alpha$, the expected cost of free-riding is strictly greater than the benefit and the institution is deterrent.[11] The intuition is that a deterrent institution will alter incentives such that it is in an individual's self-interest to contribute to the public good (Becker, 1968; Stigler, 1970; Polinsky and Shavell, 1979; Ehrlich, 1996). Within the public goods literature, deterrent central authority mechanisms have been shown to increase contributions

[11]There are various ways researchers have designed the deterrence of central authority mechanisms. Galbiati and Vertova (2008) consider minimum contribution obligations, Tyran and Feld (2006) considered fixed sanctions that were not proportional to one's contribution, while Kamijo et al. (2014) and Andreoni and Gee (2012) consider mechanisms that only punish the worst offender (lowest contributor). Regardless of the the methodology, results suggest that deterrent central authority mechanisms effectively increase contributions.

and, depending on the fixed costs, to increase group welfare (Kamei et al., 2015; Markussen et al., 2013). For consistency across institutions the fixed costs associated with the central authority was referred to as "Administrative Costs" and any sanction imposed by the central authority was referred to as "Reduction Costs".

Since aggregate group endowments were equivalent across treatments and deterrence alters each group members incentives to contribute their entire endowment we have the following hypothesis:

Hypothesis 4. *Average earnings within the central authority will not be statistically different across the Equal, Observed, or Unobserved treatments.*

2.4. Endogenous Selection

After these institutions were exogenously imposed each group had three opportunities to choose their institution before phases 4, 5, and 6. Voting was by majority and each subject had to vote to operate within either the peer punishment or central authority mechanism. Research suggests that groups will self-impose deterrent central authority institutions when it benefits them, but that they are sensitive to the fixed costs associated with the central authority (Kamei et al., 2015; Markussen et al., 2013).

In Kamei et al. (2015) subjects chose between operating within a peer punishment or a central authority mechanism. The central authority monitored all subjects with certainty but the level of the sanction (proportional to the deviation from the socially optimum contribution) was determined through a voting procedure. The level of the sanction could be non-deterrent ($s \in \{0, 0.4\}$) or deterrent ($s \in \{0.8, 1.2\}$). Across treatments, the fixed cost of the institution could be low ($c = 0$) or high ($c = 5$). When the central authority was costless (costly) a large majority of groups favored central authority (peer punishment). In Markussen et al. (2013), groups could chose to operate within a central authority, a peer punishment mechanism, or a VCM. Across four treatments they manipulated the parameters of the central authority that sanctioned, with certainty, the deviation from contributing one's entire endowment. The sanction level was either deterrent ($s = 0.8$) or non-deterrent ($s = 0.4$) and the cost was either low ($c = 2$) or high ($c = 8$). Results suggest that only the deterrent and low cost central authority was preferred to the VCM and to the peer punishment mechanism. In both papers, groups tended to choose the institution that increased their earnings.

Our central authority was designed to mirror this literature. That is, the central authority is deterrent, enforces the social optimum, but does so at a relatively high fixed cost.

The central authority presented here solves the problems associated with mis-guided or ineffectively targeted peer punishment: free-riders will be punished such that their self-interest is to contribute fully towards the public good and cooperators will have no possibility of punishment. Therefore, while behavior within the central authority is expected to be similar across treatments it's purpose is to offer a costly alternative to peer punishment. The benefit of simplifying the central authority is that variation in institutional preference should be driven entirely by the relative effectiveness of peer punishment across treatments. Following the hypotheses stated above, if the peer punishment mechanism is proven to be less effective in *Unobserved*, relative to *Observed*, while the central authority mechanism is equally effective, the expectation is that more groups will choose to self-impose the central authority in *Unobserved* relative to *Observed*:

Hypothesis 5. *The proportion of groups choosing to operate within the central authority mechanism will be greater in Unobserved relative to Equal and Observed treatments.*

Finally, it is important to highlight the trade-off in our design with regards to the number of periods in each phase and the number of phases. Shorter phases allowed us to give subjects experience with both institutions and multiple voting opportunities. However, this limits our analysis to the short-run dynamics we can observe in three periods. In the discussion we point out how future work can build on this design to examine long-run patterns.

3. Results

Data was collected in April, May, November, and December of 2016 at the University of Massachusetts Amherst Cleve E. Willis Experimental Economics Laboratory. A total of eighteen sessions were conducted (350 participants in 70 groups). The average session lasted approximately 75 minutes.

We organize our results into two sections. First, we take a look at payoffs and voting behavior across treatments; this section will serve to test our main hypotheses. We will show that a clear picture comes into focus: when endowments cannot be observed, groups eschew peer punishment for central authority. The second section of our results will try to explain how this shift in preferences for enforcement institutions may have come about.[12]

[12]The data and code for our analysis can be found at https://github.com/lrdegeest/InstitutionalChoice.

3.1. Overall results

In Table 1 we report the average group payoffs across our three treatments in each exogenously imposed institution. To compare average payoffs we use Kruskill-Wallis tests.[13] Average group payoffs are statistically equivalent across all treatments in the VCM ($\chi^2_2 = 3.31$, $p = 0.191$), consistent with our first hypothesis. In the absence of any institutional constraints on behavior, endowment heterogeneity has no significant effect on behavior. Turning to the central authority, column 3 in Table 1, we see a weakly significant difference across group payoffs ($\chi^2_2 = 5.01$, $p = 0.082$). However, when only the last period of each exogenous phase is compared this difference falls to insignificance ($\chi^2_2 = 3.46$, $p = 0.177$). This is expected given the deterrence of the central authority, and consistent with our fourth hypothesis.

Table 1: Average Payoffs.

	N	VCM	Peer Punishment	Central Authority
Equal	23	32.92	27.52	31.32
		(2.98)	(6.01)	(2.66)
Observed	23	31.14	29.52	31.56
		(4.03)	(6.25)	(2.03)
Unobserved	24	30.39	24.10	29.93
		(4.92)	(6.20)	(2.72)

Average group earnings in each exogenously imposed institution.

Standard deviations are presented in parentheses.

We now turn to our second and third hypotheses. Comparing the payoffs in the peer punishment mechanism, we see a significant difference across the three treatments for all periods ($\chi^2_2 = 7.25$, $p = 0.027$), and for just the last period ($\chi^2_2 = 17.22$, $p = 0.01$). As expected, there is no significant difference in payoffs under peer punishment when endowments are equal or when endowments are heterogeneous and observed (*Equal* vs. *Observed* $z = 1.02$, $p = 0.31$). However, payoffs are significantly lower in *Unobserved* relative to the other two treatments (*Equal* vs. *Unobserved* $z = 1.74$, $p = 0.08$; *Observed* vs. *Unobserved* $z = 2.60$, $p = 0.01$). Our results suggest that endowment heterogeneity, in linear public good

[13] Each Kruskill-Wallis and Wilcoxon Rank-Sum test presented are conducted using the group average as the unit of observation. We have 23, 23, and 24 group level observations in *Equal*, *Observed*, and *Unobserved* respectively.

experiments with complete information, does not significantly alter the successful use of peer punishment. However, under incomplete information, peer punishment is less successful.

The relative success of peer punishment has a direct bearing on group preferences for institutions. Each group, in each treatment, voted prior to phase 4, phase 5, and phase 6. Across *Equal*, *Observed*, and *Unobserved*, we have 69, 72, and 69 group level voting observations, respectively. To quantify the relationship between treatments and preference for central authority, χ^2 tests are used to determine whether a group's treatment is independent of their voting preference. As shown in Panel A of Figure 2, there is a clear preference for the central authority when endowments are heterogeneous and unobserved (*Equal* vs. *Unobserved*: $\chi^2 = 19.63$, $p = 0.01$; *Observed* vs. *Unobserved*, $\chi^2 = 24.68$, $p = 0.01$). Overall, groups in *Unobserved* self-imposed the central authority on 46 out of 69 (67%) voting opportunities. By contrast, the proportion of groups imposing central authority in *Equal* and *Observed* was 29% (20 out of 69) and 25% (18 out of 72), respectively. Indeed, there is no difference in voting behavior across *Equal* and *Observed* ($\chi^2 = 0.28$, $p = 0.59$).[14]

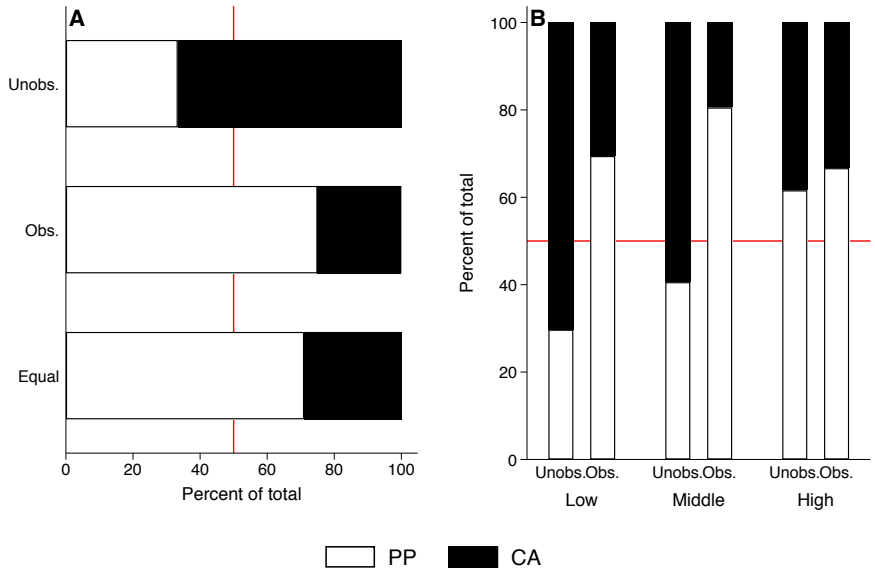

Figure 2: Proportion of votes for the central authority (CA) and peer punishment (PP). Panel A displays aggregate voting across treatments. Panel B displays voting by endowments in the heterogeneous endowment treatments.

Importantly, the preference for central authority is not symmetric across endowments.

[14]We do not observe large changes in how groups vote across phases. The proportion of groups choosing central authority in phase 4, phase 5, and phase 6 was 0.30, 0.26, and 0.30 in *Equal*; 0.29, 0.25, and 0.21 in *Observed*; and 0.70, 0.70, and 0.61 in *Unobserved*.

As shown in Panel B of Figure 2, the difference in voting between *Observed* and *Unobserved* is driven largely by the Low and Middle endowments. Significantly more Low and Middle endowments voted for the central authority in *Unobserved* vs. *Observed* (Low: $\chi^2 = 44.5$, $p = 0.01$; Middle: $\chi^2 = 23.67$, $p = 0.01$). However, there is no significant shift in voting among High endowments ($\chi^2 = 0.79$, $p = 0.37$).

The institutional choice literature suggests that subjects vote for the institution under which they earn more (Markussen et al., 2013; Kamei et al., 2015). To compliment the above analysis we present a probit model to investigate the impact of the earnings difference across institutions on one's vote for the central authority in Table 2. For simplicity we analyze only the individuals first vote in period 10, immediately after each institution was exogenously imposed. The difference in earnings is calculated as the difference between the average that the subject earned during the 3 periods of the central authority and the average earned during the 3 periods of peer punishment. There is also an *Observed* indicator to account for a treatment effect. The first column includes observations for all endowments while columns (2)-(4) consider Low, Middle, and High members independently. In each specification, the earnings differential has a significant effect on voting choice: subjects who earned more in the central authority institution tended to vote for it. After controlling for the earnings differential, being in *Observed* reduced the probability of self-imposing the central authority. However, this overall effect does not appear robust. While it is negative for each endowment, it only remains significant among Middle subjects.

Table 2: Earnings differential and voting for central authority.

	All	Low	Middle	High
	(1)	(2)	(3)	(4)
Earning Difference	0.075***	0.062***	0.061**	0.90***
	(0.014)	(0.021)	(0.030)	(0.018)
Observed	-0.364**	-0.443	-0.860**	-0.247
	(0.185)	(0.328)	(0.429)	(0.289)
Constant	-0.279**	-0.084	-0.311	-0.287
	(0.132)	(0.290)	(0.328)	(0.18)
Observations	235	94	47	94
Wald χ^2	36.61	16.09	12.87	24.18
Prob $> \chi^2$	0.000	0.000	0.002	0.000
Pseudo R^2	0.186	0.183	0.180	0.200

Standard errors clustered at the group level.

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

3.2. The role of information in peer punishment

Our results so far speak to a straightforward outcome: under incomplete information, central authority is preferred to peer punishment. However, it is not immediately evident why this preference would be driven by subjects with smaller endowments, Low in particular. Here we explore the role of information in peer punishment and how it alters institutional choice. Since the central authority imposes a constant, fixed cost, subjects could theoretically improve their lot in peer punishment by establishing and enforcing a contribution norm. Our focus here is comparing *Observed* with *Unobserved* in peer punishment, as noted above, the role of information in the central authority regime here is minimal.[15]

Figure 3 summarizes the contributions and payoffs across *Observed* and *Unobserved* under peer punishment. Aggregating across all endowments we can see that, both, average contributions and average payoffs are significantly greater with complete information. Comparing *Observed* vs. *Unobserved*, we observe average contributions of 11.9 vs. 16.1 ($z = 3.62$; $p < 0.01$) and average payoffs of 24.1 vs. 29.5 ($z = 2.60$; $p < .01$). However, the impact of information is not symmetric across endowments. Figure 3 shows the average contributions,

[15]Results on our central authority phases, in which the central authority simply enforced the social optimum and subjects complied, can be found in AppendixB.

the average contributions as a percentage of endowment, and the average payoffs for each endowment in *Observed* and *Unobserved*. The impact of observing endowments within the peer punishment mechanism is immediately clear: it increases the contributions of High members and substantially reduces the level of payoff inequality across endowments. Comparing *Observed* vs. *Unobserved* we observe average contributions of 7.3 vs. 8.7 among Low members ($z = 1.63$; $p = 0.103$), 13.5 vs. 15.8 among Middle members ($z = 1.14$; $p = 0.256$), and 15.6 vs. 23.7 among High members ($z = 3.66$; $p < 0.01$).

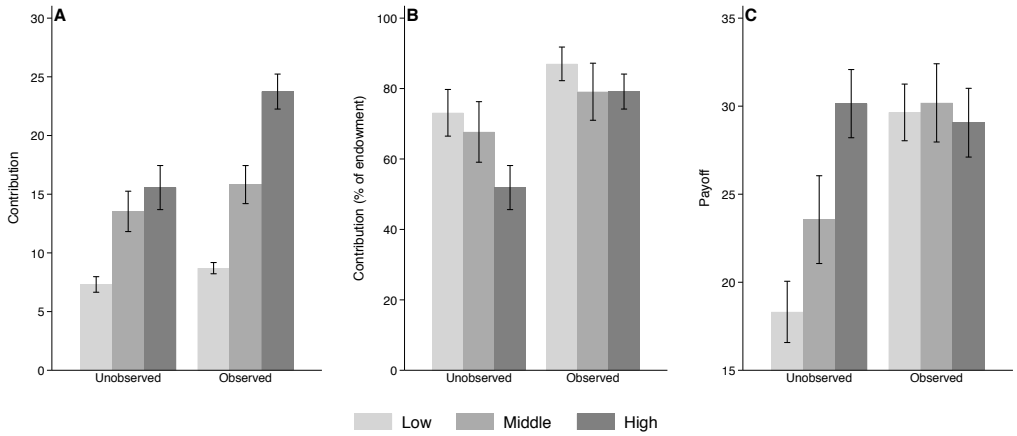

Figure 3: Average contributions and payoffs across endowments and *Observed/Unobserved*, peer punishment and exogenous groups. Error bars show the standard error of the mean.

Importantly, both the Low and Middle endowments are significantly better off within peer punishment when endowments are observed while High members are not significantly affected. Specifically, comparing *Observed* vs. *Unobserved* average payoffs are 18.3 vs. 29.6 among Low members ($z = 4.01$; $p < 0.01$), 23.6 vs. 30.2 among Middle members ($z = 2.83$; $p < 0.01$), and 30.1 vs. 29.1 among High members ($z = 0.543$; $p = 0.587$). To probe further, we looked at how much punishment was deployed and how it was allocated. Overall, a similar amount of punishment was deployed across treatments (*Observed* vs. *Unobserved*: 1.34 vs. 1.59; $z = 0.405$; $p = 0.686$). However, interesting patterns emerge when we look at sanctions supplied and received by each endowment endowment. Figure 4 breaks down the supply and receipt of sanctions across endowments and information settings. Panel A displays the average amount of punishment sent while Panel B displays the average amount of punishment received. Both figures show the cost of sanctions (recall the cost of each sanction is 4 $\times$ the sanction).

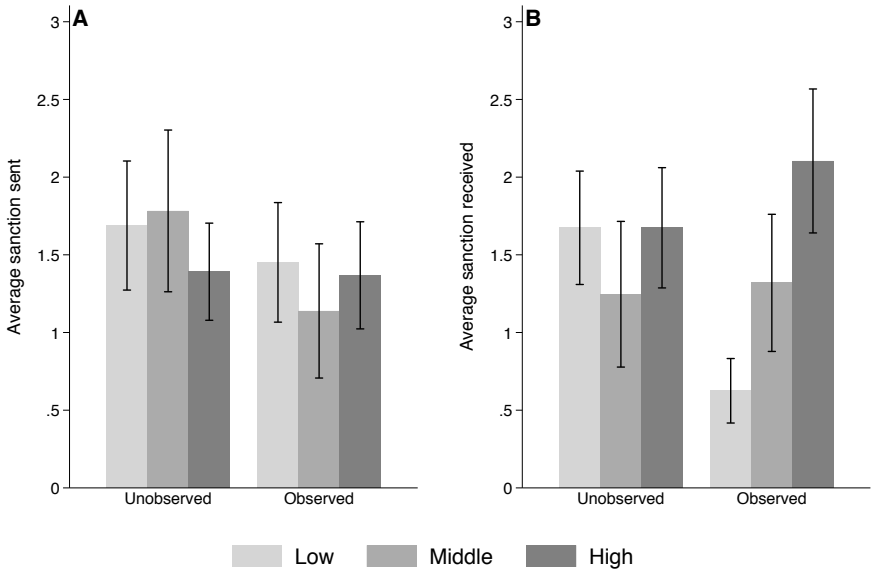

Figure 4: Sanctions supplied and received by endowment and *Observed/Unobserved*, exogenous groups. Error bars show the standard error of the mean.

Information seemed to have little impact on how sanctions were supplied. Panel A suggests that across *Observed* and *Unobserved* there is no statistical difference in the average amount of punishment sent from Low members (1.45 vs. 1.69; $z = 0.011$; $p = 0.99$), from Middle members (1.14 vs. 1.78; $z = 1.364$; $p = 0.17$), or from High members (1.37 vs. 1.39; $z = 0.053$; $p = 0.96$). On the other hand, as suggested in Panel B, information about endowments seemed to impact how sanctions were allocated, specifically reducing the amount of punishment received by Low members (*Observed* vs. *Unobserved*: 2.50 vs. 6.70; $z = 2.713$; $p < 0.01$). With no significant differences observed across Middle members (5.28 vs. 4.99; $z = 0.194$; $p = 0.85$) or High members (8.42 vs. 6.70; $z = 1.066$; $p = 0.29$).

While it appears that subjects were better able to target sanctions at free-riders under complete information, a regression analysis suggests that the impact of information is more nuanced. Table 3 estimates subject payoffs controlling for a subject's contribution, the contributions of others in one's group, a time trend (Period), an information (*Observed*) indicator, and an interaction with contribution.

Table 3: Estimating the effect of information on payoffs in peer punishment.

	Low		Middle		High	
	(1)	(2)	(3)	(4)	(5)	(6)
Contribute	0.864***	0.692**	-0.051	-0.370	-0.006	-0.277***
	(0.25)	(0.34)	(0.26)	(0.31)	(0.10)	(0.09)
Contribution of Others	0.298***	0.296***	0.321***	0.330***	0.239***	0.225***
	(0.04)	(0.04)	(0.08)	(0.07)	(0.05)	(0.04)
Observed	4.157**	0.169	0.608	-8.887	-4.191**	-18.341***
	(1.56)	(4.38)	(2.12)	(7.48)	(1.88)	(5.60)
Observed X Contribute		0.490		0.635		0.697***
		(0.43)		(0.40)		(0.21)
Period	-0.518	-0.596	-0.110	-0.319	1.786**	1.166
	(0.48)	(0.46)	(0.71)	(0.72)	(0.85)	(0.83)
Constant	-2.462	-0.952	9.789***	14.090***	16.235***	22.285***
	(2.67)	(3.15)	(2.77)	(3.46)	(3.10)	(3.05)
Observations	282	282	141	141	282	282
F	47.02	50.43	16.50	18.42	9.93	10.66
Prob > F	0.000	0.000	0.000	0.000	0.000	0.000
R^2	0.658	0.663	0.382	0.427	0.142	0.227

Standard errors clustered at the group level.

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

First, we consider the results presented in columns (1), (3), and (5). As expected, an increase in the contributions from the other members of one's group increases one's payoff across all endowments. This specification holds the marginal effect of one's own contribution constant across each treatment for each endowment and allows us to interpret the coefficient on *Observed* as the average marginal effect of the treatment. We will relax this assumption next but it is worth noting two effects across endowments. First, all else equal, a Low member can expect to earn 4.16 more, a Middle member can expect to earn a statistically equivalent amount, and a High member can expect to earn 4.19 less in *Observed*. Second, the marginal effect of increasing one's own contribution is only positive among Low members, while it is negative and insignificant among Middle and High members.

In columns (2), (4), and (6) the positive effect of the contributions of others remains. The coefficient on Contribute now represents the marginal effect of increasing one's contribution

within *Unobserved*. We see that increasing one's contribution has a positive, insignificant, and negative effect on one's payoffs among Low, Middle, and High members respectively. The coefficient on the interaction term represents the change in the marginal effect of increasing one's contribution in *Observed* and while it is positive for all endowments it is only significant among High members. Combining terms and estimating the average marginal effect of one's contribution in *Observed*, we observe that it is positive and significant for Low and High members: Low (1.18 (0.28), $p < 0.01$), Middle (0.26 (0.33), $p = 0.43$), and High (0.42 (0.19), $p = 0.04$).[16]

To provide greater context for the effect of information within the peer punishment regime we predict the average marginal effects at focal contribution levels. Among Low members the predicted difference in payoffs across treatments is insignificant at a contribution of 0 (0.17 (4.38), $p = 0.97$) and positive at a contribution of 10 (5.07 (1.51), $p < 0.01$). Among Middle members the predicted difference in payoffs across treatments is insignificant at a contribution of 0 (-8.89 (7.48), $p = 0.24$) and 10 (-2.54 (3.75), $p = 0.50$) but it is positive at a contribution of 20 (3.81 (2.07), $p = 0.07$). And, finally, among High members the average marginal effect is negative at a contribution of 0 (-18.34 (5.60), $p < 0.01$), 10 (-11.37 (3.67), $p < 0.01$), and 20 (-4.40 (2.00), $p = 0.03$) while it is insignificant at a contribution of 30 (2.27 (1.74), $p = 0.15$).

To summarize, these results suggest that the impact of information depends largely on one's income. Recall that peer punishment is ineffective, even harmful, when it cannot be targeted. There are two sources of this harm. First, when punishment cannot be targeted at free-riders, it reduces the costs of free-riding and limits the incentives for those punished to increase their contributions. Second, when punishment is aimed at cooperators, it reduces the benefits of cooperation and limits their incentives to continue contributing.

With incomplete information, this intuition takes a very specific form: cooperative Low members (those contributing 10) are significantly better off in *Observed*. Put another way, after controlling for the contributions of others, a cooperative Low member earns significantly less when information is incomplete, indicating that they are targeted despite cooperating. The result is similar for a cooperative Middle member who contributes 20. By contrast, this relationship is reversed for High members. Non-cooperative, free-riding, High members earn significantly less in *Observed*, or alternatively, significantly more in *Unobserved*. This implies that High members are able to avoid punishment while free-riding when information is incomplete.

[16] When marginal effects are reported within the text, standard errors will be reported in parentheses.

The above regression analysis on payoffs across endowments suggests that the cost of incomplete information appears to fall largely on lower income group members. Despite contributing similar, and largely cooperative, amounts in both treatments, Low and Middle endowment group members earn significantly less in *Unobserved* relative to their *Observed* counterparts. We have also observed that while the overall deployment of punishment is similar across treatments the allocation of punishments is significantly altered across endowments. This suggests that punishment is ineffectively deployed when information is incomplete. To investigate this directly we present a similar regression to the one above to understand the determinants of punishment received. Like the previous regression, Table 4 estimates punishment received controlling for a subject's contribution, the contributions of others in one's group, a time trend (Period), an information (*Observed*) indicator, and an interaction with contribution.

Table 4: Estimating the effect of information on punishment received in peer punishment.

	Low		Middle		High	
	(1)	(2)	(3)	(4)	(5)	(6)
Contribute	-1.712***	-1.604***	-0.623**	-0.280	-0.712***	-0.419***
	(0.27)	(0.37)	(0.26)	(0.26)	(0.12)	(0.09)
Others contribution	0.127***	0.128***	0.114*	0.103*	0.228***	0.242***
	(0.04)	(0.03)	(0.07)	(0.06)	(0.06)	(0.05)
Observed	-4.356***	-1.873	-0.462	9.745	4.530**	19.824***
	(1.36)	(4.61)	(1.81)	(7.04)	(1.70)	(6.14)
Observed X Contribute		-0.305		-0.682*		-0.753***
		(0.46)		(0.39)		(0.23)
Period	0.750	0.798*	0.552	0.776	-1.930**	-1.261
	(0.46)	(0.43)	(0.69)	(0.70)	(0.82)	(0.78)
Constant	11.120***	10.179***	7.112***	2.489	11.686***	5.145*
	(2.59)	(3.24)	(2.35)	(2.96)	(2.83)	(2.91)
Observations	282	282	141	141	282	282
F	10.63	13.04	1.78	2.01	9.51	9.25
Prob > F	0.000	0.000	0.149	0.100	0.000	0.000
R^2	0.470	0.473	0.205	0.279	0.305	0.384

Standard errors clustered at the group level.

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

First, we consider the results presented in columns (1), (3), and (5). As above, this specification holds the marginal effect of one's own contribution constant for each endowment across treatments. For each endowment we observe that increasing one's contribution significantly lowers the amount of sanctions received and an increase in the contributions of one's group members increases the amount of sanctions received. This suggests that group members were sanctioned less as they increased their contributions and that more cooperative groups tended to impose more sanctions. The average marginal effect of *Observed* varies across endowments. All else equal, a Low member can expect significantly less, a Middle member can expect insignificantly less, and a High member can expect significantly more sanctions when endowments are observed relative to when they are unobserved.

To investigate how sanctions were distributed, we interact *Observed* with one's contribution in columns (2), (4), and (6). First note that an increase in the contributions of one's group remains positive and significant. The more interesting implication of this specification is to consider the marginal effect of increasing one's contribution on the amount of sanctions received across treatments. For Low members in *Unobserved* increasing one's contribution significantly reduces sanctions received, and while the change is insignificant(-0.305), the marginal effect is larger and significant in *Observed* (-1.91 (0.30), $p < 0.01$). For Middle members in *Unobserved* increasing one's contribution has no significant effect on the amount of sanctions received. However, there is a significant reduction in *Observed* and the marginal effect is negative and significant (-0.96 (0.35), $p < 0.01$). For High members in *Unobserved* increasing one's contribution significantly lowers the amount of sanctions received. Further, there is a significant reduction in *Observed* and the marginal effect is negative and significant (-1.17 (0.23), $p < 0.01$). Overall, the impact of increasing one's contribution in *Observed* has a larger, negative, effect on the amount of sanctions received for each endowment.

The above analysis implies that the benefit, measured as the reduction in sanctions received, of increasing one's contribution is greater when endowments are observed. To further demonstrate this we will again estimate the average marginal effect of information at focal contribution levels. Among Low members the reduction in sanctions received across treatments is insignificant at a contribution of 0 (-1.87 (4.61), $p = 0.69$) and negative and significant at a contribution of 10 (-4.92 (1.20), $p < 0.01$). Similarly, among Middle members the reduction in sanctions received across treatments is insignificant at a contribution of 0 (9.74 (7.04), $p = 0.17$) and 10 (2.92 (3.40), $p = 0.39$). However, at a contribution of 20 Middle members can expect to receive significantly less sanctions in *Observed* (-3.90 (1.93), $p = 0.05$). Turning this analysis around, both Low and Middle members who con-

tribute their entire endowment receive significantly more sanctions in *Unobserved* relative to *Observed*.

Among High members the results are more stark. At contributions of 0, 10, and 20 high members receive significantly more sanctions in *Observed* (19.82 (6.14), $p < 0.01$); (12.29 (3.91), $p < 0.01$); and (4.76 (1.89), $p = 0.02$ respectively). Similar to the other endowments, High members receive significantly less sanctions when they contribute their entire endowment of 30 (-2.77 (1.65), $p = 0.10$). Put another way, low contributing High members were significantly less sanctioned in *Unobserved*, suggesting that their free-riding was not be effectively targeted.

4. Discussion

We studied the role of incomplete information in the choice between peer punishment and central authority enforcement institutions designed to induce cooperation and enhance group welfare in social dilemmas. We presented three, linear public good experiments: one in which each group member receives an equal endowment (*Equal*), and two in which group members receive unequal endowments and either observe endowments of other group members (*Observed*) or not (*Unobserved*). Groups were exposed to peer punishment and central authority before voting for one enforcement institution. When incomes across subjects are unequal, we find that individuals in *Observed* tended to prefer central authority, while individuals in *Unobserved*, particularly those with lower incomes, tended to prefer central authority. This is largely because lower income subjects earned less under incomplete information, while higher income subjects earned about the same in *Observed* as *Unobserved*, since they could not be targeted with sanctions.

Since our main research question focused on the cost of incomplete information on decentralized enforcement, we designed our experiments with a simplistic central authority institution to better delineate the choice between the two enforcement institutions. In contrast to Nicklisch et al. (2016) the alternative institution here is a deterrent central authority with perfect information which enforces the social optimum, but at a relatively high fixed cost. As such, we created an alternative which is equally effective across all treatments and investigated how the relative effectiveness of peer punishment alters institutional preference across treatments. The design motivates incomplete information as another plausible mechanism, as a compliment to the work of Nicklisch et al. (2016), through which central authority institutions have come to dominate modern society.

However, it is important to recognize that a realistic central authority will not resemble

the all-knowing adjudicator in our design. If it is possible for subjects to somehow fool or even corrupt third-party enforcement, then there may be conditions under which peer punishment is actually preferred to central authority – or, as in Nicklisch et al. (2016), no enforcement institution at all. On the problem of incomplete information, lessons from the mechanism design literature remind us there exist no mechanisms that induce efficient provision of public goods when agents have private information (Hurwicz, 1972). By realistic we mean mechanisms that are incentive compatible, satisfy an agent's participation constraint, and work for any distribution of preferences. In fact, the only mechanism that can maximize efficiency under these three conditions is a dictator-style enforcer (Satterthwaite, 1975) similar to our current central authority. Exactly what is the second-best (and arguably more appealing) central authority is a promising topic for future research.

Examining long-run dynamics of income inequality and incomplete information is another important topic for future work. Since our main interest was choice of enforcement institutions by groups, our design used short phases, the upside being that subjects gained more experience with each institution and had more opportunities to vote. The downside is that we are unable to tease out the emergent contribution norms that motivated punishment like Carpenter and Matthews (2009) and Reuben and Riedl (2013). In particular, Reuben and Riedl (2013) show that under complete information, subjects enforce multiple contribution norms that account for differences in wealth. However, our results suggest that targeting is ineffective under incomplete information, which may stunt the emergence of optimal contribution norms. Moreover, subjects may further exploit incomplete information by establishing contribution norms that do not require them to reveal their wealth.

More broadly, our study points to the importance of understanding how income inequality affects social capital in groups.[17] Our findings suggest that societies faced with less heterogeneity may be less dependent on formal institutions because they more effectively employ informal, peer to peer, institutions. In our introduction we pointed to evidence from the microfinance literature that suggests homogeneous groups are better at attracting and repaying loans (Cassar et al., 2007). Group lending is a commonly used tool to deliver credit to the poor in developing countries and is, by definition, informal. Such arrangements depend on informal, peer to peer, mechanisms to encourage repayment of loans that are given without collateral. Other research suggests that trust and social norms are highest in socially homogeneous and low income-inequality countries, while investment in public

[17]The term social capital captures the stock of social norms, networks of civic engagement, and the trustworthiness that define a society or group (Coleman, 2000).

goods like education is lowest in countries with high income inequality (Knack and Keefer, 1997; Zak and Knack, 2001; Easterly, 2007). Knack and Keefer (1997) show that trust and cooperative social norms are conducive for growth and that these characteristics are higher in socially homogeneous and low income inequality societies. Zak and Knack (2001) model the relationship between trust and economic performance and argue that low trust reduces investment and subsequent growth. This low trust poverty trap is more likely when existing institutions are weak and society is heterogeneous. Alesina and La Ferrara (2000) investigate the source of social capital with respect to engagement in civic associations and find that civic engagement declines as communities become more heterogeneous and unequal.

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AppendixA. Order effects

All of the analysis presented combines experimental sessions which alternated the order of imposition of the peer punishment and the central authority mechanisms. Specifically, in *Observed* (*Unobserved*) there were 12 (11) groups which participated in peer punishment followed by the central authority and 12 (12) groups which participated in the central authority followed by peer punishment. The order of imposition may effect the effectiveness of either institution if subjects gain experience with the incentives beyond that provided by the 3 VCM periods. For example, the central authority implicitly imposes the rule that each group member contribute their entire endowment, any deviation from this rule is punished with certainty. If this effectively establishes cooperation within groups they may more effectively deploy peer punishment when given the opportunity. To investigate we compare average earnings in each institution across sessions within the same treatment. For brevity, we present aggregate group level earnings in both imposed institutions across *Observed* and *Unobserved*. Results were similar in *Equal*, across the VCM periods, and by endowment. No significant order effects are observed.

Table A.5: Average earnings by order of imposition

	Peer Punishment	Central Authority
	Observed	
PP then CA	30.61	32.04
($N = 12$)	(7.13)	(0.98)
CA then PP	28.43	31.08
($N = 12$)	(5.61)	(2.74)
z	0.664	0.376
p	0.51	0.71
	Unobserved	
PP then CA	25.22	30.91
($N = 11$)	(7.75)	(2.35)
CA then PP	23.06	29.04
($N = 12$)	(4.81)	(2.93)
z	0.862	1.541
p	0.39	0.12

Ranksum tests are conducted at group level.

Standard deviations are reported in parentheses.

AppendixB. Central Authority

Results from our central authority phases were as expected. Figure B.5 shows the average contributions, the average contributions as a percentage of endowment, and the average payoffs for each endowment in *Observed* and *Unobserved*. (Recall that subject i was punished $1.2(e_i - x_i)$ with certainty when her contribution (x_i) was below her endowment (e_i)). Under a central authority, contributions, for the most part, were close to endowments, and whether endowments were or were not observed had little effect. Low and Middle endowments did not significantly change their contributions while High members showed a marginally significant increase in contributions when endowments were observed. Comparing *Observed* vs. *Unobserved* we observe average contributions of 9.1 vs. 9.4 among Low members ($z = 0.248$, $p = 0.80$), 19.3 vs. 18.0 among Middle members ($z = 0.448$, $p = 0.65$), and 28.5 vs. 27.0 among High members ($z = 1.775$, $p = 0.08$). With regards to payoffs, comparing *Observed* vs. *Unobserved* we observe average payoffs of 31.6 vs. 30.2 among Low members

($z = 2.07$, $p = 0.039$), 31.6 vs. 29.9 among Middle members ($z = 2.11$, $p = 0.035$), and 31.5 vs. 29.7 among High members ($z = 2.024$, $p = 0.042$), with differences explained in part by more instances of punishment by the central authority in *Unobserved*. However, these sanctions appear to somewhat be explained by learning effects and subject error, as in both treatments most sanctions happened in the first period of the central authority phase.

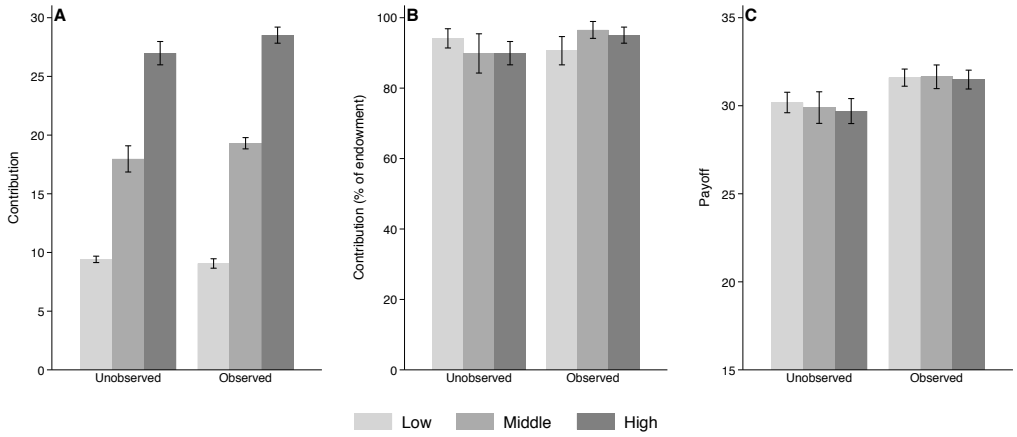


Figure B.5: Average contributions and payoffs across endowments and information settings, central authority and exogenous groups. Error bars show the standard error of the mean.

AppendixC. Experiment instructions

Welcome to the Experiment

Thank you for participating in our decision making experiment. The experiment is divided into 6 phases. There are a total of 18 periods. In each period you will have an opportunity to earn money, which is in addition to the $5 guaranteed for your participation in the experiment. Your earnings each period will depend on your decisions and the decisions of other participants.

Please read the following instructions carefully. Everyone must correctly answer the comprehension questions at the end before we can begin.

During the experiment you are not allowed to communicate with other participants. If you have a question please raise your hand.

During the experiment your earnings will be calculated in *Experimental Dollars* (*EDs* for short). You can earn *EDs* every period. At the end of the experiment, your total earnings in *EDs* will be converted to U.S. dollars at the following rate:

$$50 \text{ EDs} = \$1$$

At the end of the experiment your total earnings (including the $5 participation payment) will be paid to you, privately and anonymously, in cash.

In the experiment, each participant is randomly assigned to a group of 5. This means that you are in a group with four other participants. You will be part of the **same** group throughout the entire experiment. However, at no point will the members of your group be revealed. All of the decisions you make within the experiment are anonymous and will be kept confidential.

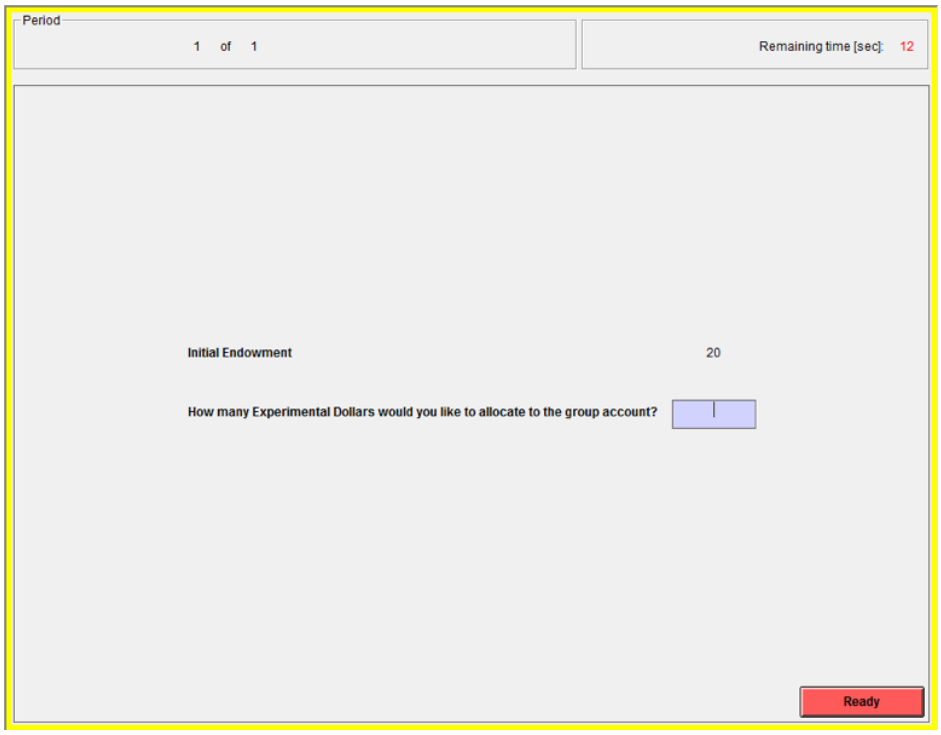
Instructions

In every period, each group member, yourself included, will be given an endowment of EDs. Two (2) members of the group will receive 30 EDs, two (2) members of the group will receive 10 EDs, and one (1) member of the group will receive 20 EDs. This initial allocation of EDs is random and will be maintained throughout the experiment. Whatever your endowment is in Period 1 will remain your endowment for the entire experiment. You will have to make one decision each period.

Your decision

Each of you will independently and anonymously decide how many of your EDs to allocate to the group account. You can allocate any integer between 0 and your endowment to the group account. Your remaining EDs will automatically be allocated to your private account. Your earnings depend on the number of EDs in your private account and the *total* number of EDs in the group account.

Example of allocation decision screen (assumes a 20 ED endowment)



How are period earnings calculated?

The earnings from your private account equal the number of EDs in your private account. Your private account earnings do not depend on the decisions of other group members. You simply keep all EDs that you choose *not* to allocate to the group account.

Your Private Account Earnings = (Your Endowment) – (EDs you allocate to the group account)

Your earnings from the group account equal 0.4 times the *total* number of EDs allocated to the group account. Thus, your group account earnings depend, in part, on the decisions of other group members.

Your Group Account Earnings = 0.4*(the total number of EDs allocated to the group account)

Your period earnings are the sum of your private account earnings and your group account earnings.

Your Period Earnings = Your Private Account Earnings + Your Group Account Earnings

Example 1

The example assumes the following:

	Endowment	Allocation to Group Account
You	20 EDs	10 EDs
Member A	30 EDs	0 EDs
Member B	30 EDs	30 EDs
Member C	10 EDs	0 EDs
Member D	10 EDs	10 EDs

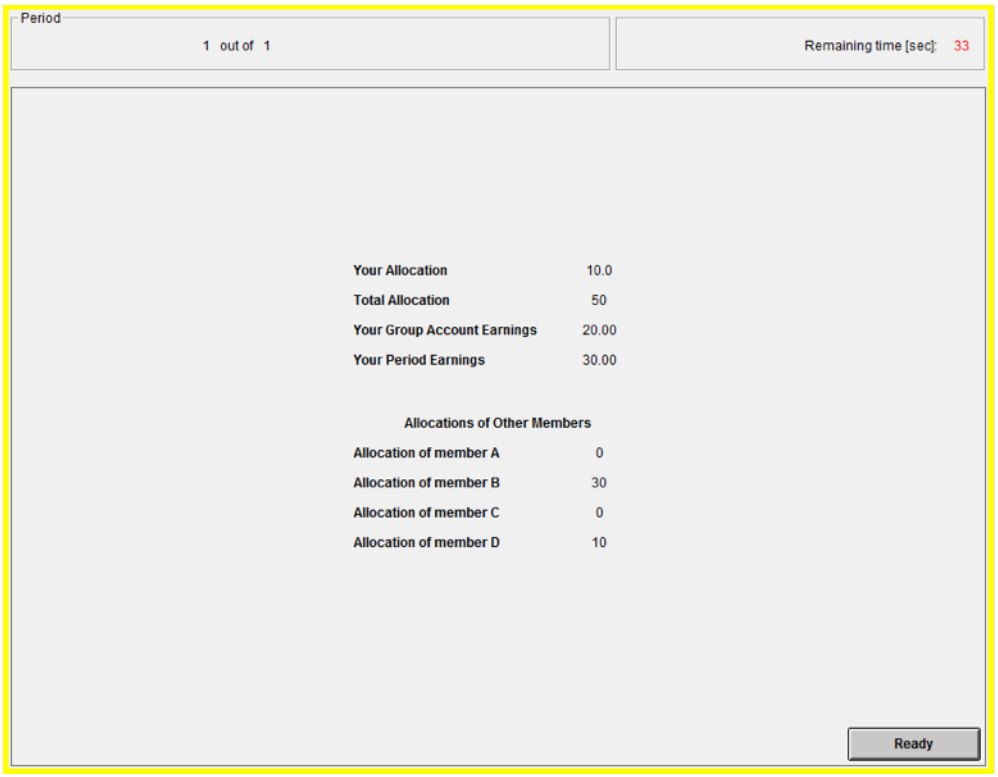
The total number of EDs in the group account = 10+0+30+0+10 = 50 EDs
So each group member earns = 0.4*50 = 20 EDs from the group account

What are your period earnings in this example?

You have a 20 ED endowment and you allocated 10 EDs:
Your period earnings = private account earnings + group account earnings

= (your endowment – your allocation) + (0.4*total group allocation)
= (20 – 10) + 0.4*50
= 10 + 20 = 30 EDs

Example of your payoff screen given the example above:



What are the period earnings of group member A in this example?

Group member A has a 30 ED endowment and allocated 0 EDs:
Their period earnings = (their endowment – their allocation) + (0.4*total group allocation)
= (30 – 0) + 0.4*50
= 30 + 20 = 50 EDs

What are the period earnings of group member B in this example?
Group member B has a 30 ED endowment and allocated 30 EDs:
Their period earnings = (their endowment – their allocation) + (0.4*total group allocation)
= (30 – 30) + 0.4*50

= 0 + 20 = 20 EDs

What are the period earnings of group member C in this example?
Group member C has a 10 ED endowment and allocated 0 EDs:
Their period earnings = (their endowment – their allocation) + (0.4*total group allocation)
= (10 – 0) + 0.4*50
= 10 + 20 = 30 EDs

What are the period earnings of group member D in this example?
Group member D has a 10 ED endowment and allocated 10 EDs:
Their period earnings = (their endowment – their allocation) + (0.4*total group allocation)
= (10 – 10) + 0.4*50
= 0 + 20 = 20 EDs

Note that, regardless of your endowment, for each ED you allocate to the group account, your earnings from the group account ***increase*** by 0.4*1 = 0.4 EDs and your earnings from your private account ***decrease*** by 1 ED.

However, for each ED you allocate to the group account, the earnings of each of the other 4 members of your group ***increase*** by 0.4 EDs. Therefore, for each ED you allocate to the group account the total group earnings ***increase*** by 0.4*5 = 2 EDs.

You also obtain earnings from each ED allocated to the group account by others. You earn 0.4*1 = 0.4 EDs for each ED allocated to the group account by another member.

Example 2

Relative to Example 1 assume that you decrease your allocation to 0 EDS but nothing else changes:

	Endowment	Allocation to Group Account
You	20 EDs	0 EDs
Member A	30 EDs	0 EDs
Member B	30 EDs	30 EDs
Member C	10 EDs	0 EDs
Member D	10 EDs	10 EDs

The total number of EDs in the group account = 0+0+30+0+10 = 40 EDs
So each group member earns = 0.4*40 = 16 EDs from the group account
What are your period earnings in this example?
You have a 20 ED endowment and allocated 0 EDs:
Your period earnings = (your endowment – your allocation) + (0.4*total group allocation)
= (20 – 0) + 0.4*40
= 10 + 16 = 36 EDs (An increase of 6 EDs relative to Example 1

What are the period earnings of group member A in this example?
Group member A has a 30 ED endowment and allocated 0 EDs:
Their period earnings = (their endowment – their allocation) + (0.4*total group allocation)
= (30 – 0) + 0.4*40
= 30 + 16 = 46 EDs (A decrease of 4 EDs relative to Example 1)

What are the period earnings of group member B in this example?
Group member B has a 30 ED endowment and allocated 30 EDs:
Their period earnings = (their endowment – their allocation) + (0.4*total group allocation)
= (30 – 30) + 0.4*40
= 0 + 16 = 16 EDs (A decrease of 4 EDs relative to Example 1)

What are the period earnings of group member C in this example?
Group member C has a 10 ED endowment and allocated 0 EDs:
Their period earnings = (their endowment – their allocation) + (0.4*total group allocation)
= (10 – 0) + 0.4*40
= 10 + 16 = 26 EDs (A decrease of 4 EDs relative to Example 1)

What are the period earnings of group member D in this example?
Group member D has a 10 ED endowment and allocated 10 EDs:
Their period earnings = (their endowment – their allocation) + (0.4*total group allocation)
= (10 – 10) + 0.4*40
= 0 + 16 = 16 EDs (A decrease of 4 EDs relative to Example 1)

Compared with the earnings of Example 1, your earnings have increased (by 6 EDs) and the earnings of **each** of the other four members have decreased (by 4 EDs).

What information is provided after each period?

At the end of each period you will be shown the following:

- Your group account allocation
- The sum of the group account allocations by all members of your group
- Your group account earnings
- Your period earnings

You are also shown the individual group account allocations by each other member of your group by random ID. The random ID (labeled as A, B, C or D) represents a different group member **each period**.

You will also be presented with the history of choices from previous periods. This information includes the information above and your total earnings up to this point in the experiment. Your total earnings are the sum of your earnings from each period of the experiment.

Your Total Earnings = Sum of your Private Earnings each Period

Comprehension questions

Please answer the following questions. Raise your hand if you need any help. A member of the experiment team will check your answers when you are done. We will begin when everyone has finished. Thank you for your patience.

1) Suppose that each group member, **including** you, allocates 0 EDs to the group account.

Assume you have a 10 ED endowment and you allocate 0 EDs:

a. What are your private account earnings? _____

b. What is the total number of EDs in the group account? _____

c. What are your group account earnings? _____

d. What are your period earnings? _____

Assume you have a 20 ED endowment and you allocate 0 EDs:

a. What are your private account earnings? _____

b. What is the total number of EDs in the group account? _____

c. What are your group account earnings? _____

d. What are your period earnings? _____

Assume you have a 30 ED endowment and you allocate 0 EDs:

a. What are your private account earnings? _____

b. What is the total number of EDs in the group account? _____

c. What are your group account earnings? _____

d. What are your period earnings? _____

2) Suppose that each group member, **including** you, allocates their *entire* endowment to the group account.

Assume you have a 10 ED endowment and you allocate 10 EDs:

a. What are your private account earnings? _____

b. What is the total number of EDs in the group account? _____

c. What are your group account earnings? _____

d. What are your period earnings? _____

Assume you have a 20 ED endowment and you allocate 20 EDs:

a. What are your private account earnings? _____

b. What is the total number of EDs in the group account? _____

c. What are your group account earnings? _____

d. What are your period earnings? _____

Assume you have a 30 ED endowment and you allocate 30 EDs:

a. What are your private account earnings? _____

b. What is the total number of EDs in the group account? _____

c. What are your group account earnings? _____

d. What are your period earnings? _____

3) Suppose that each group member, ***excluding*** you, allocates 10 ED to the group account

Assume you have a 20 ED endowment and you allocate 0 EDs:

a. What are your private account earnings? _____

b. What is the total number of EDs in the group account? _____

c. What are your group account earnings? _____

d. What are your period earnings? _____

Assume you have a 20 ED endowment and you allocate 10 EDs:

a. What are your private account earnings? _____

b. What is the total number of EDs in the group account? _____

c. What are your group account earnings? _____

d. What are your period earnings? _____

Instructions for the next phase of 3 periods

In each of the next 3 periods you continue to interact with the same four group members and you make a decision about how much of your endowment to allocate to the group account. In each period your earnings are initially computed exactly as they were in phase 1 and will be referred to as your ***Initial Period Earnings***.

However, there is now an **Informal Institution** in effect which will affect your period earnings.

How does the Informal Institution affect period earnings?

In each period, after each group member has made their allocation decision, each of you will continue to be shown the individual allocations of each group member by random ID.

Each group member will now have the opportunity to assign Reduction Points to other group members. The number of Reduction Points assigned can be any integer between 0 and 10 and can be distributed in any way among group members. Note that you don't need to assign any Reduction Points and you can only assign up to 10 Reduction Points. For each Reduction Point you assign to another group member you will pay 1 ED. This cost is referred to as:

Your Administrative Costs = The number of Reduction Points *you assign to others*

For each reduction point that is assigned to you your initial period earnings will be reduced by 4 EDs. This cost is referred to as:

Your Reduction Costs = 4 * The number of Reduction Points *assigned to you from others*

Recall that your *Initial Period Earnings* are calculated and reported exactly as they were in phase 1. Nothing has changed except that there are additional costs. To calculate your period earnings when the Informal Institution is in effect you subtract your administrative costs and your reduction costs from your initial period earnings.

Note that your period earnings cannot be negative unless you assign Reduction Points. That is, you pay Administrative Costs.

Period Earnings =

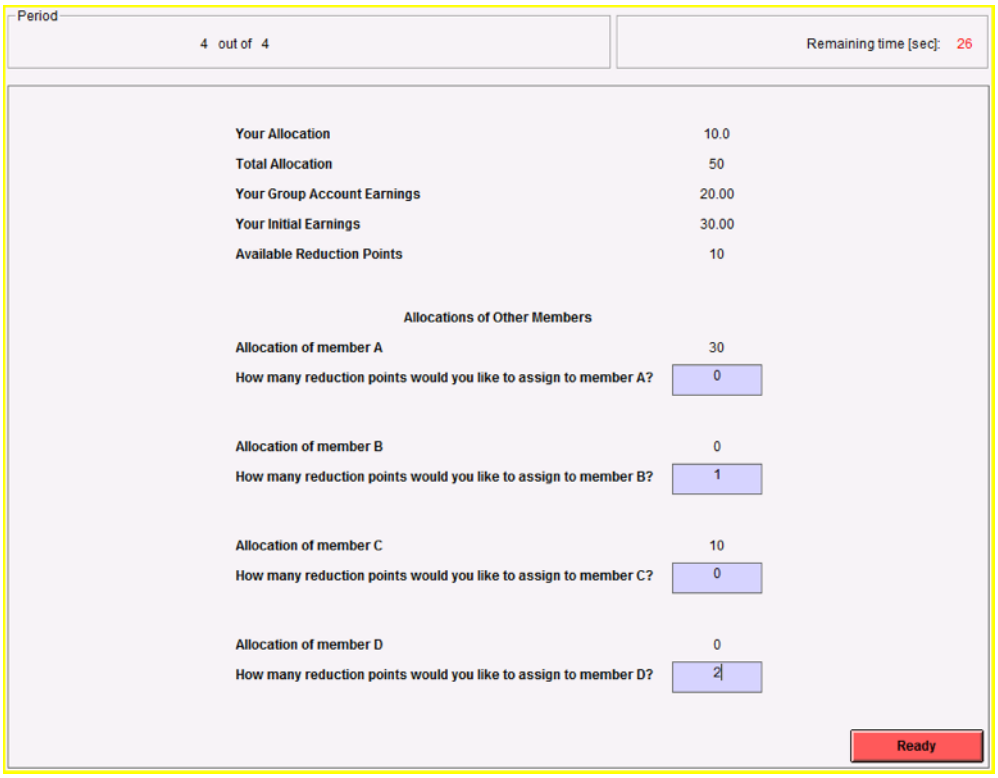
Max[Initial Period Earnings – Your Reduction Costs, 0] - Your Administrative Costs

What information is provided after each period?

Once each member has made their decisions concerning Reduction Points you will be shown:

- Your Administrative Costs
- Your Reduction Costs
- Your Period Earnings

Example of your Reduction Point input screen given the example below



An Example

The example assumes the following:

	Endowment	Allocation	Reduction Points Assigned	Reduction Points Received
You	20 EDs	10 EDs	1 to member B	1 from member A
			2 to member D	1 from member D
Member A	30 EDs	30 EDs	1 to you	None
			1 to member B	
			1 to member D	
Member B	30 EDs	0 EDs	None	1 from you
				1 from member A
				1 from member C
Member C	10 EDs	10 EDs	1 to member B	None
			1 to member D	
Member D	10 EDs	0 EDs	1 to you	2 from you
				1 from member A
				1 from member C

The total number of EDs in the group account is = 10+30+0+10+0 = 50 EDs
So each group member earns = 0.4*50 = 20 EDs from the group account
What are your period earnings in this example?
You have a 20 ED endowment, allocated 10 EDs, assigned 3 Reduction Points, and received 2 Reduction Points:
Your initial period earnings = private account earnings + group account earnings
= (your endowment – your allocation) + (0.4*total allocation)
= (20 – 10) + 0.4*50
= 10 + 20 = 30 EDs

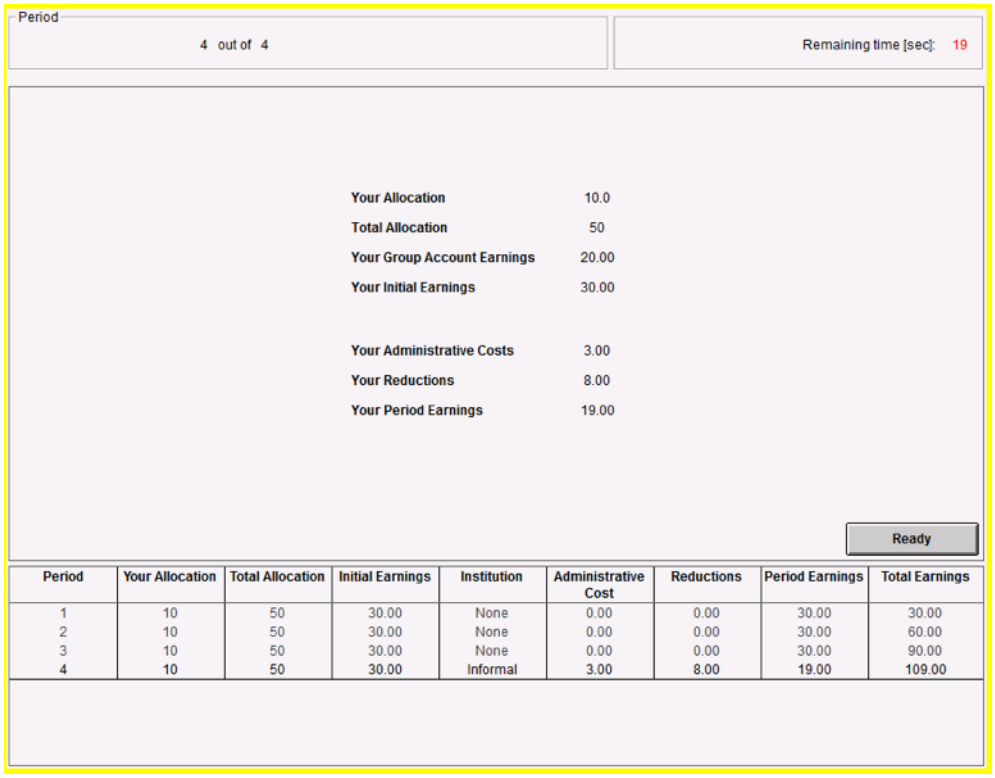
Your administrative costs = 1 ED per Reduction Point you assigned (you assigned 3)
= 1*3 = 3 EDs

Your reduction costs = 4 EDs per Reduction Point assigned to you (you received 2)
= 2*4 = 8 EDs

Your period earnings = your initial period earnings – your administrative costs – reduction costs

= 30 – 3 – 8 = 19 EDs

Example of your payoff screen given the example above



Period	Your Allocation	Total Allocation	Initial Earnings	Institution	Administrative Cost	Reductions	Period Earnings	Total Earnings
1	10	50	30.00	None	0.00	0.00	30.00	30.00
2	10	50	30.00	None	0.00	0.00	30.00	60.00
3	10	50	30.00	None	0.00	0.00	30.00	90.00
4	10	50	30.00	Informal	3.00	8.00	19.00	109.00

What are the period earnings of group member A in this example?

Member A has a 30 ED endowment, allocated 30 EDs, assigned 3 Reduction Points, and received 0 Reduction Points:

Member A's initial period earnings = (30 – 30) + 0.4*50

= 0 + 20 = 20 EDs

Member A's administrative costs = 1 ED per Reduction Point assigned (they assigned 3)

= 1*3 = 3 EDs

Member A's reduction costs = 4 EDs per Reduction Point received (they received 0)

= 0*4 = 0 EDs

Member A's period earnings = initial period earnings – administrative costs – reduction

costs
= 20 – 3 – 0 = 17 EDs

What are the period earnings of group member D in this example?
Member D has a 10 ED endowment, allocated 0 EDs, assigned 1 Reduction Point, and received 4 Reduction Points:
Member D's initial period earnings = (10 – 0) + 0.4*50
= 10 + 20 = 30 EDs

Member D's administrative costs = 1 ED per Reduction Point assigned (they assigned 1)
= 1*1 = 1 ED

Member D's reduction costs = 4 EDs per Reduction Point received (they received 4)
= 4*4 = 16 EDs

Member D's period earnings = initial period earnings – administrative costs – reduction costs
= 30 – 1 – 16 = 13 EDs

Comprehension questions

Using the example above please answer the following questions. Raise your hand if you need any help. A member of the experiment team will check your answers when you are done. We will begin when everyone has finished. Thank you for your patience.

1. Determine the period earnings for Member B in the example above. Member B has a 30 ED endowment, allocated 0 EDs, assigned 0 Reduction Points, and received 3 Reduction Points.

a. What are Member B's initial period earning? _____

b. What are Member B's administrative costs? _____

c. What are Member B's reduction costs? _____

d. What are Member B's period earnings? _____

2. Determine the period earnings for Member C in the example above. Member C has a 10 ED endowment, allocated 10 EDs, assigned 1 Reduction Point, and received 4 Reduction Points.

a. What are Member C's initial period earning? _____

b. What are Member C's administrative costs? _____

c. What are Member C's reduction costs? _____

d. What are Member C's period earnings? _____

Instructions for the next phase of 3 periods

In each of the next 3 periods you continue to interact with the same four group members and you make a decision about how much of your endowment to allocate to the group account. In each period your earnings are initially computed exactly as they were in phase 1 and will be referred to as your ***Initial Period Earnings***.

However, there is now a **Formal Institution** in effect which will affect your period earnings.

How does the Formal Institution affect period earnings?

In each period, after each group member has made their allocation decision, the formal institution will monitor the allocations of each group member. To monitor the allocations of each group member there is a fixed cost of 6 EDs that is paid by *each* group member. This cost is referred to as:

Your Administrative Costs = 6 Experimental Dollars

For each ED that one's allocation is below their endowment the formal institution reduces their period earnings by 1.2 EDs. This cost is referred to as:

Your Reduction Costs = 1.2 * (Endowment − Group Account Allocation)

Recall that your *Initial Period Earnings* are calculated and reported exactly as they were in phase 1. Nothing has changed except that there are additional costs. To calculate your period earnings when the Formal Institution is in effect you subtract your administrative costs and your reduction costs from your initial period earnings.

Period Earnings = Initial Period Earnings − Your Administrative Cost − Your Reduction Costs

Note that if you allocate your entire endowment your Reduction Costs = 0.

What information is provided after each period?

Once each member has made their allocation decision you will be shown:

- Your Administrative Costs
- Your Reduction Costs
- Your Period Earnings

An Example

The example assumes the following:

	Endowment	Allocation to Group Account
You	20 EDs	10 EDs
Member A	30 EDs	0 EDs
Member B	10 EDs	10 EDs
Member C	30 EDs	30 EDs
Member D	10 EDs	0 EDs

The total number of EDs in the group account = 10+0+10+30+0 = 50 EDs
So each group member earns 0.4*50 = 20 EDs from the group account

What are your period earnings in this example?

You have a 20 ED endowment and you allocate 10 EDs.
Your initial period earnings = (your endowment – your allocation) + (0.4*total allocation)
= (20 – 10) + 0.4*50
= 10 + 20 = 30 EDs

Your administrative costs = 6 EDs (this is the same for each group member)
Your reduction costs = 1.2*(your endowment – your allocation)
= 1.2*(20 – 10)
= 1.2*10 = 12 EDs

Your period earnings = your initial period earnings – your administrative costs – reduction costs
= 30 – 6 – 12 = 12 EDs

What are the period earnings of group member A in this example?

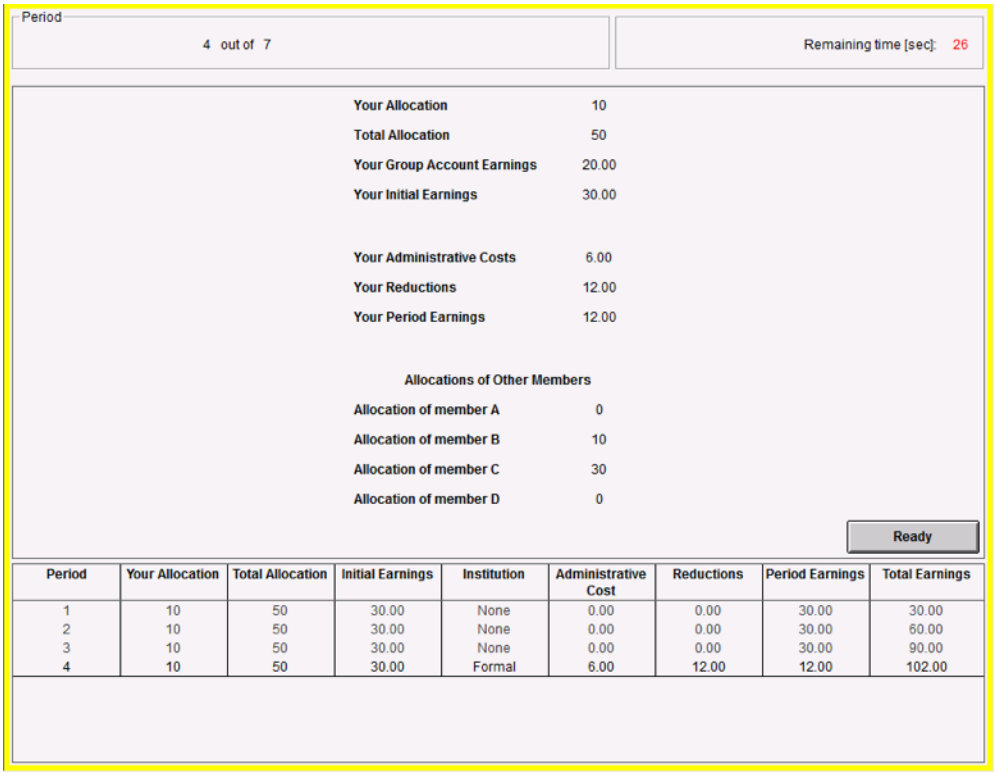
Member A has a 30 ED endowment and allocated 0 EDs:

Member A's initial period earnings = (30 – 0) + 0.4*50
= 30 + 20 = 50 EDs

Member A's administrative costs = 6 EDs

Member A's reduction costs = 1.2*(30 – 0)
= 1.2*30 = 36 EDs
Member A's period earnings = initial period earnings – administrative costs – reduction costs
= 50 – 6 – 36 = 8 EDs

Example of your payoff screen given the example above:



Period	Your Allocation	Total Allocation	Initial Earnings	Institution	Administrative Cost	Reductions	Period Earnings	Total Earnings
1	10	50	30.00	None	0.00	0.00	30.00	30.00
2	10	50	30.00	None	0.00	0.00	30.00	60.00
3	10	50	30.00	None	0.00	0.00	30.00	90.00
4	10	50	30.00	Formal	6.00	12.00	12.00	102.00

What are the period earnings of group member B in this example?
Member B has a 10 ED endowment and allocated 10 EDs:
Member B's initial period earnings = (10 – 10) + 0.4*50
= 0 + 20 = 20 EDs

Member B's administrative costs = 6 EDs

Member B's reduction costs = 1.2*(10 – 10)
= 1.2*0 = 0 EDs

Member B's period earnings = initial period earnings – administrative costs – reduction costs
= 20 – 6 – 0 = 14 EDs

Comprehension questions

Using the example above please answer the following questions. Raise your hand if you need any help. A member of the experiment team will check your answers when you are done. We will begin when everyone has finished. Thank you for your patience.

1. Determine the period earnings for Member C in the example above. Member C has a 30 ED endowment and allocated 30 EDs.

   a. What are Member C's initial period earning? _____

   b. What are Member C's administrative costs? _____

   c. What are Member C's reduction costs? _____

   d. What are Member C's period earnings? _____

2. Determine the period earnings for Member D in the example above. Member D has a 10 ED endowment and allocated 0 EDs.

   a. What are Member D's initial period earning? _____

   b. What are Member D's administrative costs? _____

   c. What are Member D's reduction costs? _____

   d. What are Member D's period earnings? _____

Instructions for Phase 4, 5, and 6

Each of the next 3 phases includes 3 periods. Before each phase each group member will be able to vote for which institution they wish to operate within.

That is, each group member can select to operate under the Informal Institution **or** under the Formal Institution.

Simply chose the institution that you would prefer to operate within. The institution that wins the majority will be implemented. You will be informed which institution is in effect and all rules remain exactly as they were described previously.

Do you have any questions? Please raise your hand.