Does the Allocation of Property Rights Matter in the Commons?

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Abstract:

A popular solution to the Tragedy of the Commons is to create private property rights to access the commons. If resource users care about equity, they may be unwilling to respect property rights regimes that lead to less equitable outcomes. We explore in a series of laboratory experiments whether it is possible to undermine the efficiency of property rights solutions through the allocation process. We find that both the extent to which property rights are enforced and how they are allocated significantly affect extraction and compliance. Our findings suggest that one of the most popular allocation methods is suboptimal: we observe that occasional enforcement and the grandfathering of property rights is dominated by no enforcement and the equal allocation or inverse grandfathering of property rights. Our results challenge the view that equity is irrelevant for property rights solutions to the commons problem.

Keywords: tragedy of the commons; privatization; grandfathering; equity; social norms

JEL Codes: A13, C92, D23, D62, D63, H23

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1. Introduction

“The allocation might be on the basis of wealth, by the use of an auction system. It might be on the basis of merit, as defined by some agreed-upon standards. It might be by lottery. Or it might be on a first-come, first-served basis, administered to long queues. These, I think, are all the reasonable possibilities. They are all objectionable.”

- Hardin (1968)

Economists typically advocate solving the Tragedy of the Commons through either government regulation of use or through the allocation of property rights (privatizing the commons). The property rights approach has gained increasing popularity with policymakers, particularly in the United States.¹ There are now a number of cap-and-trade markets for air pollution in the US: examples include the Regional Clean Air Incentives Market (RECLAIM), the Regional Greenhouse Gas Initiative (RGGI) and California’s carbon market under the Global Warming Solutions Act of 2006. Property rights solutions in fisheries have gone from being illegal in US waters from 1996-2002 to now accounting for 65% of fish caught in federal waters. In addition, privatizing access to public pastures has been used for some time to control overgrazing in the US (Gardner, 1963).

When property rights solutions are implemented in practice, governments typically try to set the total quantity of rights assigned at some socially efficient target. For example, individuals are allocated rights to harvest and the total sum of all the rights allocated equals a socially desirable total harvest. Further “economic” efficiency gains might be possible if rights are tradable: the rights to harvest should end up in the hands of those who value them the most.² Regardless of whether property rights are tradable or not, if the total amount of rights allocated equals the socially efficient outcome (in terms of internalizing externalities), then the actual allocation of rights should not matter for social efficiency as long as property rights are perfectly enforced.

In practice, however, property rights represent a form of social contract and are almost always, to some degree, incomplete. In particular, property rights are rarely perfectly enforced. In most societies, the actions of citizens and firms are not constantly monitored to ensure that all

¹ See Engel and Lueck (2008) for an overview and an introduction to a series of papers on property rights and the environment.

² Whether economic efficiency is independent of allocation has been the topic of both recent theoretical (Mackenzie et al., 2008; Anderson et al., 2011) and empirical (Fowlie and Perloff, 2013) debate.

laws and rights are being respected at all times. Thus, many property rights rely on a mix of both external penalties and social norms in order to function (Tyler, 1990; Ellickson, 1991). Social norms are unenforced rules of behavior that are based on beliefs of how individuals should behave in a given situation. Humans are presented with countless opportunities to violate the property rights of others with little to no chance of detection but strong social norms appear to prevent this from happening on a regular basis.

There is growing evidence that social norms can be eroded by the introduction of formal regulatory institutions. Gneezy and Rustichini (2000a), for example, demonstrate that parents were less deterred from arriving late in day-care centers after the introduction of a monetary fine. Cardenas et al. (2000) provide experimental evidence that the regulatory solution for an environmental dilemma failed because it appeared to crowd out cooperative behavior.³ Furthermore, there is widespread experimental evidence that humans have social preferences (Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000; Fehr and Gächter, 2000; Charness and Rabin, 2002; Camerer, 2003), which supports field observations of humans incurring personal costs to adhere to a social norm that they perceive as equitable (Libecap, 1989; Tyler, 1990; Winter and May, 2001).

Thus, property rights solutions to the commons problem may depend on how they interact with social norms, in ways that have not been apparent to many scholars. We demonstrate in a controlled experimental framework how subtle behavioral manipulations can undermine property rights solutions by discouraging compliance. Our laboratory experimental setup and our behavioral interventions are straightforward. Subjects play a negative externality game in groups of four (similar to the game in Walker et al. (2000)). Each subject makes an extraction decision; extraction confers private benefits but imposes a social cost on everyone in the group. The symmetric Nash equilibrium is to have too much extraction relative to the socially optimal amount. We introduce a property rights solution by capping the total amount of extraction at the socially optimal level and then allocate rights to extract this total.

Our first behavioral intervention varies the degree to which property rights are enforced. Interestingly, we find a clear non-monotonic relationship between compliance and enforcement. When individuals are never monitored, property rights are better respected and extraction is closer to the social optimum than when individuals are occasionally monitored. This suggests

³ However, see Abatayo and Lynham (2016) for a recent critique of this finding.

that external monitoring may crowd out intrinsic motivations to adhere to a social norm. Not surprisingly, when monitoring is very frequent, subjects respect property rights. Yet, in terms of social efficiency, frequent monitoring does not fare better than no monitoring when taking into account the costs associated with monitoring.⁴

The findings from the first intervention provide new insights for the experimental literature studying sanctions and compliance with laws (Schulze and Frank, 2003, Tyran and Feld, 2006; Kube and Traxler, 2011; Schildberg-Horisch and Strassmair, 2012). This literature suggests in contrast to our findings that non-deterrent (i.e. mild) sanctions, despite some drawbacks, are still useful to stimulate compliance. For example, Tyran and Feld (2006) observe that imposing mild sanctions increases contributions in a public goods experiment and, in particular, if they are endogenously imposed. Kube and Traxler (2011) show that formal sanctions partially crowd out informal sanctions (peer sanctions) in a public goods experiment but they still increase social welfare. Schulze and Frank (2003) study a corruption experiment and find that sanctions destroy the intrinsic motivation for honesty but that they also reduce corruption. More in line with our results are the recent findings in Schildberg-Horisch and Strassmair (2012) who simultaneously manipulate fine and detection probability and report evidence against the deterrence hypothesis from one-shot stealing experiments. In particular, they observe that small deterrence incentives backfire and that stealing is less pronounced in the absence of deterrence. We observe that a cap increases extraction behavior and reduces compliance if there is a low detection probability holding the fine constant, and that this effect is robust over time.

Our second behavioral intervention varies how property rights are allocated. We demonstrate that allocation matters by allocating property rights either equally or proportional or inverse to past extraction. Compliance is higher and extraction is lower when property rights are allocated equally or inversely. This suggests that social norms like respecting the property rights of others are stronger when they lead to more equitable outcomes (in our context, the equal allocation of rights equalizes payoffs in the second half of the experiment and the inverse allocation equalizes payoffs across the whole experiment). Thus, equity matters for property

⁴ As we will explain later, this result depends on the assumption that the fines collected from property rights violators are one-for-one efficiency losses and are not put to any socially beneficial use.

rights solutions in this setting. We also find interesting patterns in terms of treatment interactions. Preferences for equitable allocations appear to matter more in situations that have weak external enforcement and thus rely primarily on norms for compliance.⁵ Equity concerns appear to be irrelevant when external enforcement dominates behavior.

While there are experimental studies on property right allocation regimes and in particular emissions trading auctions (Benz and Ehrhart, 2007; Murphy and Stranlund, 2007; Goeree et al, 2010; Cason and Raymond, 2011; Raymond and Cason, 2011; Grimm and Ilieva, 2013), our set of property right allocation regimes has never been studied experimentally.⁶ Perhaps the study closest to ours is Cason and Raymond (2011), which reports results from a laboratory emissions trading experiment, which included a treatment where pollution permits are allocated either equally or unequally. In contrast to our set-up, allocations are not based on past behavior and are imposed at the start of the experiment before subjects make any decisions. In an environmentally framed version of their experiment, subjects are told that the unequal allocation is due to some firms having higher pollution control costs; in a neutrally framed version of the experiment, no explanation is given for the inequity. They find that the allocation method does not appear to affect compliance when the experiment is neutrally framed but dishonest reporting does decline in the equal allocation treatment when the experiment is framed within an environmental context (which supports our general findings). The focus of their study is on the truthful self-reporting of emissions, whereas we focus on the causal link between allocation, extraction, and efficiency. The focus on the interplay between allocations and equity concerns in the context of property allocation regimes is a novel contribution of our study and the no enforcement treatment and inverse grandfathering treatment are unique to our experimental design.

The rest of the paper is organized as follows. Section 2 presents a conceptual framework designed to generate testable hypotheses. Section 3 describes our experiment designed to test

⁵ See Johansson-Stenman and Konow (2010) for an excellent discussion of the “equality norm” in the environmental context, including evidence on the degree to which laboratory subjects prefer outcomes that equalize payoffs (Andreoni and Miller, 2002; Gächter and Riedl, 2006; Konow, 2010).

⁶ The experimental emissions trading literature tends to compare auctions to grandfathering, with a focus on market performance and price effects. Benz and Ehrhart (2007) focus on price discovery and not compliance: it is impossible to cheat in their experimental design. Murphy and Stranlund (2007) observe differences in compliance behavior when they compare emissions mandates to emissions trading. Goeree et al. (2010) compare auctions and grandfathering with a focus on the secondary spot market, they do not compare compliance behavior. Grimm and Ilieva (2013) compare auctions and grandfathering: they find that frequent auctions are less efficient than infrequent auctions. They do not find an effect from the initial allocation procedure.

these hypotheses and Section 4 summarizes the results. Section 5 interprets our results and discusses the relevance of our findings to other contexts.

2. A Conceptual Model of Allocation and Social Preferences

In the next section we explain our experimental design in detail but it can be summarized as follows. Subjects in groups of four play a standard negative externality game where each subject must decide how many tokens to “extract”. Typically there is too much extraction, relative to the socially optimal level. We then introduce a cap on total extraction at the socially optimal level and allow subjects to play for five more periods within the same groups. We experimentally vary how this cap is allocated: proportional to extraction in the first five periods, equally, or in inverse proportion to extraction in the first five periods. The goal is to vary the perceived equity of the cap allocation by either increasing or decreasing existing inequities in payoffs from the first five periods. Subjects typically earn more in the second stage of the game if all group members respect the new cap.

In this section we now present a conceptual model to show how equity preferences might influence preferences over the different cap allocation regimes. The model is intended to be a stylized representation of our experiment and not an exact replica. Consider a setting with 4 individuals indexed by $i \in \{1,2,3,4\}$ who each receive a monetary outcome x_i and prefer equal over unequal outcomes; i.e., they dislike inequity. Adopting the inequity aversion model and notation of Fehr and Schmidt (1999), the utility to agent i from outcomes $\{x_1, x_2, \dots, x_n\}$ is given by:

$$U_i(x) = x_i - \alpha_i \frac{1}{n-1} \sum_{j \neq i} \max\{x_j - x_i, 0\} - \beta_i \frac{1}{n-1} \sum_{j \neq i} \max\{x_i - x_j, 0\}$$

where α parameterizes the distaste of person i for disadvantageous inequality in the first nonstandard term (“other people receive more than me”), and β parameterizes the distaste of person i for advantageous inequality in the final term (“I receive more than others”). We follow Fehr and Schmidt and assume that $\beta_i \leq \alpha_i$ and $0 \leq \beta_i < 1$. To capture a key design feature of our experiment and to derive testable hypotheses, we consider three different allocation mechanisms and assume that each allocation consists of two stages. The first stage (Stage 1) is

identical across all three allocation mechanisms and consists of the following outcomes for the four agents: $\{x_1, x_2, x_3, x_4\}$ with $0 < x_1 < x_2 < x_3 < x_4$.

Differences between the allocation mechanisms emerge in Stage 2. The total amount to be allocated in Stage 2 is 50% more than in Stage 1. This is designed to reflect the fact that, in our experiment, the cap in Stage 2 raises total expected payoffs by 50%. We are explicitly assuming that agents comply with the caps in Stage 2 of our experiment. We denote our first allocation mechanism as “grandfathering” and this is designed to exacerbate existing inequalities in outcomes from Stage 1. This allocation is fairly simple: in Stage 2, agents receive 1.5 times the same payoff they received in Stage 1. Therefore, the four overall outcomes are $\{2.5x_1, 2.5x_2, 2.5x_3, 2.5x_4\}$ with $2.5x_1 < 2.5x_2 < 2.5x_3 < 2.5x_4$ (*grandfathering*). The second allocation is denoted “equal sharing” where all individuals receive an equal share in the second stage equivalent to $1.5\bar{x} = 1.5 \frac{1}{n} \sum_{i=1}^n x_i$. The overall allocation is therefore $\{x_1 + 1.5\bar{x}, x_2 + 1.5\bar{x}, x_3 + 1.5\bar{x}, x_4 + 1.5\bar{x}\}$ such that $x_1 + 1.5\bar{x} < x_2 + 1.5\bar{x} < x_3 + 1.5\bar{x} < x_4 + 1.5\bar{x}$ (*equal sharing*). The third allocation is “inverse sharing”, where each individual receives an allocation that completely reverses all inequities from the first stage and the overall allocation is now $\{2.5\bar{x}, 2.5\bar{x}, 2.5\bar{x}, 2.5\bar{x}\}$ (*inverse sharing*). Thus, the total amount allocated across the three mechanisms is the same but the difference lies in the distribution. In the Appendix, we calculate the utility each agent receives under each allocation mechanism and derive their preferences across allocations depending on the extent to which they dislike inequity. This leads to the following two results:

Proposition 1. If agents are sufficiently inequity-averse ($\beta_i \geq \frac{3}{4}$) then all agents prefer the inverse allocation to the equal allocation, which is preferred to the grandfathering allocation. In other words: *inverse sharing* > *equal sharing* > *grandfathering*.

Proposition 2. If agents do not have social preferences for equity ($\alpha_i = \beta_i = 0$), then only the agent who receives the least in Stage 1 strictly prefers the inverse allocation to the equal allocation, which is preferred to the grandfathering allocation. Further, the agent who receives the most in Stage 1 strictly prefers the opposite: grandfathering is preferred to equal, which is preferred to inverse.

Proofs for both propositions are provided in the Appendix.⁷ The intuition for Proposition 1 is as follows. The agents who get small payoffs in Stage 1 are obviously better off with the equal and inverse allocations because they receive more in the second stage and there is less inequity. But why do agents who receive large payments in Stage 1 dislike the grandfathering outcome? When concerns about fairness are strong enough, the benefit of receiving more in Stage 2 under grandfathering is outweighed by the large loss of utility caused by the inequitable payoffs. In other words, the benefit of more fairness is greater than the cost of foregone income. If agents don't have preferences for equity, as is assumed in Proposition 2, then they simply prefer the allocation that gives them the most. This will be grandfathering for agents with large allocations in Stage 1 and inverse sharing for agents with small allocations in Stage 1.

In our experimental design, compliance with an individual cap (that is weakly or never enforced) is costly from a purely self-interested perspective since it deviates from the Nash equilibrium.⁸ Therefore, if rational agents comply with caps in non-binding settings, it must be because they derive some utility from complying with the caps. We posit that the utility derived from complying with a new rule is dependent upon an agent's preferences over the allocation of payoffs that results under the new rule. In more formal terms, we assume that preferences over allocations are complete, transitive, and continuous and that the set of possible allocations is finite. Then, by the Utility Representation Theorem (Debreu, 1954), there exists a utility of compliance function $C(x)$ such that $C(x) > C(y)$ if and only if $x \succ y$.

As a result Proposition 1 implies that an agent with strong preferences for equity will receive more utility from complying with the inverse sharing allocation than the equal allocation. And they will receive more utility from complying with the equal sharing allocation rather than the grandfathering allocation. If agents choose to not comply with a particular allocation in our

⁷ Note that the condition that $\beta \geq 0.75$ is not an extremely strong one. Beranek et al. (2015) report on the results from modified ultimatum and dictator games with three very different subject pools. The median estimated Beta is always greater than 0.525 and, in one subject pool, 73% of participants are classified as having a $\beta \geq 0.5$. Blanco et al. (2011) find that 11% of their subjects can be characterized as having a $\beta \geq 0.83$. Furthermore, in order to observe treatment differences in our experiment, we only require that a fraction of group members have $\beta \geq 0.75$.

⁸ As will be explained in the next section, we designed our experiment in such a way that under the treatment with no monitoring and no enforcement of caps, a subject can always make more money by exceeding their cap, regardless of the cap allocated to them. This can be seen clearly in the instructions provided in the Appendix. In the table labeled "PROFIT FROM TOKENS", along the row with 50 indicating the total number of tokens extracted by the group, it can be seen that an individual subject always makes more by extracting more tokens (which is a diagonal move down and to the right).

experiment then this will always lower overall efficiency since the cap is set at the peak of the social efficiency function (see figure in the instructions in the Appendix). Thus, more compliance means higher overall efficiency and lower compliance always means lower overall efficiency. Thus, we conjecture that if agents have preferences for equity, treatments that allocate rights more equitably will have higher rates of compliance and higher social efficiency. Note that in order to observe treatment differences across allocation regimes, we only need a subpopulation with $\beta \geq \frac{3}{4}$. For example, if only 1 out of the 4 group members is a strong altruist, this will lead to behavioral patterns in line with our predictions. This leads to our main testable hypotheses:

Hypothesis 1 (Compliance). In the absence of equity preferences, compliance under inverse sharing should be identical to compliance under grandfathering.

Hypothesis 2 (Efficiency). In the absence of equity preferences, overall efficiency under inverse sharing should be identical to overall efficiency under grandfathering.

Rejection of Hypotheses 1 and 2 would provide empirical support for our conceptual model of inequity aversion and compliance. In the next section, we explain the details of the experimental design we use to test these two hypotheses.

3. Experimental Design

A total of 21 experimental sessions were conducted on the campus of the University of Hawai‘i at Mānoa. There was a total of 280 participants, recruited using the ORSEE software (Greiner, 2015). Average earnings were about \$16 and \$11 net of the show-up fee paid. We conducted no pilot studies and report all data from experiments conducted for this project. The experiments were conducted using z-Tree (Fischbacher, 2007). All subjects participated in the same negative externality game before we introduced an intervention and different treatments. The design of the pre-intervention experiment, as well as the instructions and procedures followed those of Walker et al. (2000), although we simplified the game. Subjects were paid \$1.00 for every 100 “computer dollars” they earned.

At the start of each experimental session, subjects were randomly and anonymously placed in groups of four and remained in the same group for the duration of the experiment. The identity of the other group members was never revealed. In the pre-intervention experiment, subjects made decisions for five periods. Each period, they must decide how many tokens to extract: they can extract any number between 0 and 25. Extraction is capped at 25 tokens to prevent subjects from earning negative profits. Tokens extracted in one period do not carry over to the next. Tokens extracted earn subjects profits according to the following formula:

$$PROFIT = BENEFIT - COST = 25x - x(0.25X)$$

where x represents the number of tokens extracted by an individual subject and X represents the total number extracted by all four members of the group. Thus, there is a clear negative externality in this game. Each token extracted confers a private benefit but imposes both a private and a social cost. If subjects ignore the social cost, too many tokens will be extracted relative to the socially optimal amount. This is easily illustrated in the figure in the instructions provided in the Appendix. Total profit for the group is initially increasing as the group extracts more tokens. But once the total number of tokens extracted by the group exceeds 50, each additional token lowers the total profit for the group. If each member of the group extracts 25 tokens, then each individual and the group as a whole make no profit whatsoever. It is straightforward to show that total profits for the group are maximized if each individual extracts $50/4 = 12.5$ tokens (subjects could request fractions of tokens) but the symmetric Nash equilibrium of this game is where each subject extracts 20 tokens.

After these initial five periods, we introduced our intervention: a total cap on extraction (50 tokens) assigned at the individual level. Treatments varied how this cap was allocated and the degree to which it was enforced. We implemented nine treatments in a 3x3 factorial between-subjects design (Table 1). In the allocation dimension we had (i) equal, (ii) inverse or (iii) proportional allocation. In the enforcement dimension we had (i) no monitoring, (ii) a 10% chance of monitoring and (iii) a 50% chance of monitoring. The motivation for the allocation treatments was to both replicate real world practice and generate allocations that might be perceived as equitable or inequitable. Proportional allocation is very common in the field and essentially rewards those who have generated the most externalities and punishes those who have

shown restraint.⁹ The proportional allocation exacerbates inequities in payoffs from the first five periods. Equal allocation ignores past actions and should equalize post-intervention payoffs. Inverse allocation punishes those subjects who have previously imposed large externalities on others and should lead to more equitable overall payoffs as it rewards those who held back and received lower payoffs in the first five periods.

[Insert Table 1 about here]

After subjects were informed about the total cap, the mechanism to allocate individual caps was explained. In the equal allocation mechanism, each subject received an individual cap of 12.5 tokens. For proportional allocation, all of the extractions from the first five periods were summed and subjects received an individual share of the 50 token cap in direct proportion to how many tokens they had extracted. For example, if a subject's extraction in the first five periods accounted for 50% of the total number of tokens extracted, this subject would receive an individual cap of 25 tokens. If a subject's extraction in the first five periods accounted for 10% of the total number of tokens extracted, this subject would receive an individual cap of 5 tokens. In the inverse allocation treatment, subjects received an individual cap in inverse proportion to how many tokens they extracted in the first five periods. For example, suppose two people in the group each extracted 40% of the total tokens extracted in the first 5 periods and the other two group members each extracted 10% of the tokens. The group members who extracted 40% would each get an individual cap of 10% of 50 = 5 tokens. The group members who extracted 10% would each get an individual cap of 40% of 50 = 20 tokens. Once individual caps were established, they did not change from period to period as they were always based on the first five periods of play. Subjects did not know that their behavior in the first five periods would determine their cap in the next stage but they did know that they would be playing an additional game within the same group.

Monitoring of individual caps occurred as follows. In the no monitoring treatment, subjects were free to exceed their individual cap with no financial consequences. In the 10% and

⁹ There are obviously many practical reasons for why grandfathering is so common in the field, including potential efficiency benefits (Anderson et al., 2011). The intent here is not to critique grandfathering *per se*, but simply to have three different allocation methods that are easy to understand but also very likely to be perceived as either equitable or inequitable.

50% monitoring treatments, the experimental software would generate a random draw at the end of each period and use this to determine if there would be monitoring. If monitoring occurred, all subjects in the session would be monitored. If monitored and found to be exceeding his/her cap, a subject had all profits for that period confiscated. The cost of non-compliance equates to foregoing profits in the non-compliant period only, there are no additional penalties imposed on the subject. Importantly, the consequences of their extraction decision were still imposed on the other members of their group; the negative externality associated with extraction did not disappear even if a subject's profits were confiscated. Subjects played this modified version of the game for five periods and then the experiment ended. The full experimental instructions are provided in the Appendix.

The enforcement probabilities were chosen with a specific intent. Under an equal allocation cap of 12.5 tokens each, a rational risk-neutral agent should always exceed their cap when the monitoring probability is 10% but should always respect their cap when the monitoring probability is 50%. In other words, assuming risk neutrality, full compliance with the property rights regime can be supported as a symmetric Nash equilibrium in the 50% regime but not the other regimes.¹⁰ The 0% enforcement regime was designed to replicate settings where rights are completely norm based and there are no financial penalties for violating the property rights institution.

It is useful to consider what are the Nash equilibrium predictions when the caps are asymmetric and also when subjects have inequity aversion. The average set of caps under both proportional and inverse allocation is 10, 12, 13, and 15.¹¹ With no enforcement and no inequity aversion, the Nash equilibrium prediction is unchanged and all agents will want to exceed their

¹⁰ Under enforcement, individuals face a choice between complying with the cap and exceeding their cap. If we denote an individual's assigned cap as x^C , then the profit from complying is $PROFIT = 25x^C - 0.25x^C(X)$ and the expected profit from not complying is $E(PROFIT) = (1 - p)[25x - 0.25x(X)]$ where p is the probability of getting caught. As an illustrative example, suppose all group members have been assigned an equal cap of 12.5 tokens each and three group members are complying with the cap. With a 10% enforcement probability, the payoff to the fourth member for complying is 156.25 and the maximum expected payoff from not complying is 211. With a 50% enforcement probability, the payoff to complying is still 156.25 but the maximum expected payoff to not complying is 117.

¹¹ More precisely, in the proportional regime, the mean lowest cap is 10.2, the mean second lowest cap is 11.9, the mean second highest cap is 13.1 and the mean highest cap is 14.8. For the inverse regime, the mean lowest cap is 10.3, the mean second lowest cap is 11.7, the mean second highest cap is 13, and the mean highest cap is 15.1. A Kolmogorov-Smirnov test confirms that there is no statistically significant difference between the distribution of caps in the proportional regime and the distribution of caps in the inverse regime ($p=0.97$), suggesting that our randomization into treatments was successful.

caps. With 10% enforcement and risk-neutral agents, some asymmetric equilibria can now be supported as Nash equilibria that involve either the agent with the cap of 15 complying or both the agent with the cap of 13 and the agent with the cap of 15 complying. Thus, we expect some partial compliance with asymmetric caps and 10% enforcement. With 50% enforcement, all four agents will want to comply with their caps. So, with some small qualifications, our earlier statement regarding rational compliance with equal caps still holds with asymmetric caps: some agents will rationally choose to cheat with 10% enforcement but all agents should want to rationally comply with 50% enforcement.

If we consider Nash equilibrium responses with inequity averse utility functions similar to that presented in the conceptual model (with $\beta = 0.75$ and $\alpha = 1$) then, with no enforcement, full compliance with the equal caps allocation can be supported as a Nash equilibrium. The intuition is that the utility gained from exceeding 12.5 is less than the utility cost of the inequity that this causes. For the case with asymmetric caps, compliance by all agents with their caps cannot be supported as a Nash equilibrium with no enforcement. There are a large number of symmetric Nash equilibria ranging from 13 to 22 but all of these involve, at most, two agents complying with their caps. The majority of these Nash equilibria consist of all agents exceeding their caps. With 10% enforcement and inequity aversion, the equal cap allocation is still a Nash equilibrium. With asymmetric caps, the prediction is also unchanged: a range of symmetric equilibria from 13 to 22 can be supported as Nash equilibria but there is no Nash equilibrium where all agents comply with their caps. With 50% enforcement and inequity aversion, the equal allocation of 12.5 and the asymmetric allocation of 10, 12, 13, and 15 can now both be supported as Nash equilibrium outcomes.

4. Results

This section is divided into five main parts. First, we provide a descriptive overview across all treatments (4.1). Then, we focus on the role of the three different property right enforcement regimes (4.2) before we investigate the role of the three different property right allocation regimes (4.3). Then, we investigate the combined role of property right enforcement and allocation regimes (4.4), before we conclude this section with an analysis of how different types react to the different regimes (4.5).

4.1 Descriptive overview

In the five periods before the treatment interventions, we observe that subjects extract on average 18.3 out of 25 tokens leading to an efficiency of 70.8%, which is well below the social optimum (100%) but also above the symmetric Nash prediction of 64% efficiency (and extraction of 20 tokens). We also observe that mean extraction increases in the first five periods by on average 7.7% per period (period 1=15.4, period 2=17.8, period 3=18.8, period 4=19.4, period 5=20.2), reducing efficiencies from 88.4% in period 1 to 57.2% in period 5.¹² In addition, we observe that there is considerable variance across groups. For example, we observe that 16.9% of the groups extract 60 tokens or less (combined) whereas 24.7% of the groups extract at least 85 tokens. These findings are consistent with previous experimental research on common pools and public goods and suggest that social norms play an important role for extraction.

After the treatment interventions, we observe decreases in extractions allowing efficiencies to increase substantially. On average, extractions are 11.2% lower (mean extraction is now 16.3 tokens) and efficiencies are 11% to 16.2% higher in periods 6-10 than in periods 1-5 (depending on whether we take into account sanctioning costs). There is also a temporal pattern in the post-intervention periods, albeit less pronounced. While extraction drops 26.3% in period 6 directly after the treatment interventions, extractions increase in consecutive periods by an average of 3.4%. Yet, period 10 extractions are still 14.6% lower on average than in period 5.

4.2 Property right enforcement regimes

We start by investigating the impact of the enforcement regime on extractions. Table 2 reports mean group extractions for the different monitoring regimes (0% detection, 10% detection, 50% detection) in pre- and post-intervention periods. We have pooled the different allocation treatments by enforcement regime. The table shows that all three regimes reduce extractions. The strongest mean reduction occurs in the 50%-regime (-19.22%, Mann-Whitney test, $n=48$, $z=4.10$, $p<0.001$) and is followed by the 0%-regime (-10.99%, Mann-Whitney test, $n=42$, $z=2.20$, $p=0.028$). The 10%-regime, in contrast, reduces extractions only marginally (-4.45%, Mann-Whitney test, $n=50$, $z=1.73$, $p=0.084$).

¹² In Period 5, we cannot reject a null hypothesis of 20 tokens (the symmetric Nash equilibrium) of extraction (t-test, $n=70$, $t=0.558$, $p=0.579$). This suggests that choices have converged to the Nash prediction before the treatment interventions are introduced.

[Insert Table 2 about here]

The left panel of Figure 1 illustrates mean extraction for all five post-intervention periods and distinguishes between the three enforcement regimes. We observe robust differences between these regimes as the dashed red line for the 10%-regime is always above the solid blue line for the 0%-regime and the solid blue line is in all periods above the dotted green line for the 50%-regime. We also observe that the lines for the 10%- and 0%-regimes are increasing while the 50%-regime line is relatively stable. Indeed, the 50%-regime has the strongest lasting impact on reducing group extractions (-28.5%, comparing period 5 to period 10 within the 50%-regime, Mann-Whitney test, $n=48$, $z=4.995$, $p<0.001$). Yet, we also find that the 0%-regime still has a statistically significant lasting impact (-12.9%, comparing period 5 to period 10 within the 0%-regime, Mann-Whitney test, $n=42$, $z=2.617$, $p=0.0089$). In contrast, the 10%-regime has no statistically significant lasting impact when comparing period 5 to period 10 (-3.0%, Mann-Whitney test, $n=50$, $z=1.107$, $p=0.268$).

[Insert Figure 1 about here]

We test for statistically significant differences between treatment regimes using two different approaches. First, we use a non-parametric approach where we collapse all of our data into a single mean observation for each group in each experimental session. This is done because, first, individual-level observations are unlikely to be independent: group members influence each other. Second, period-level observations are unlikely to be independent: decisions in previous periods are likely to influence current and future periods. Thus, we collapse 1,400 observations into 70 truly independent observations. An observation is the average outcome for all members of a group in all post-intervention periods of the experiment. We then run Mann-Whitney two-tailed tests on this collapsed dataset. Our second approach is parametric and we run random effects generalized least squares regressions with standard errors clustered at the group level (this is the approach suggested by Harrison (2007) in his critique of non-parametric tests using experimental data). This approach uses all 2,800 observations (pre- and post-intervention) but specifically accounts for the panel nature and error structure of the data collected.

Starting with the non-parametric approach, the mean extraction in the 0%-regime for all post-intervention periods is 16.1 and the corresponding mean for the 10%-regime is 18.6. This difference is statistically significant for a Mann-Whitney test ($p=0.004$, $n=46$) and, for the sake of comparison, for a simple t-test ($p=0.001$, $t=-3.48$, $n=46$). The mean extraction in the 50%-regime is even lower (14.0) and this is statistically significantly different to the 0%-regime at the 5% level of significance ($p=0.03$, $n=45$, Mann-Whitney test). Our second approach is presented in Table 3 Columns (1), (3), and (4) where we regress outcomes in a given period on a dummy variable for the post-intervention periods (6-10) and this dummy variable interacted with the three different extraction regimes. The regression model in Column (1) corroborates that the three regimes produce significantly different extraction rates. As can be seen by the *Post-intervention* $\times$ *10%-regime* interaction, the 0%-regime is significantly more effective (by 1.64 tokens per period) in reducing extractions than the 10%-regime ($p<0.01$). While the 0%-regime significantly reduces extractions by 2.08 tokens ($p<0.001$), we can calculate from the *Post-intervention* $\times$ *10%-regime* interaction that the 10%-regime reduces extractions by merely 0.44 tokens on average ($2.08-1.64$). The 50%-regime is more effective in reducing extractions than the 0% and the 10%-regimes. As compared to the 10%-regime it reduces extractions by an additional 1.62 tokens per period ($p<0.05$). The non-parametric and parametric findings lead to our first result.

RESULT 1: *The property right enforcement regimes decrease extractions in a non-monotonic manner: extraction is lowered less with a 10% detection probability than with a 0% probability. The 50% detection probability lowers extractions the most.*

Next, we investigate the impact of the enforcement regime on individual compliance. We say that an individual complied if she did not exceed her individual cap. In total, we observe that individuals complied 53.5% of the time. There are significant differences between the three different enforcement regimes. Unsurprisingly, compliance is largest in the 50%-regime in which individual caps were respected 78.5% of the time. Interestingly, compliance appears to be lower in the 10%-regime (36.2%) than in the 0%-regime (45.5%).

The middle panel of Figure 1 illustrates mean group compliance in periods 6-10 and distinguishes between the three enforcement regimes. Group compliance equals one if all group

members in all groups in a given enforcement regime complied and zero if all group members in all groups of a given enforcement regime did not comply. We observe that the dotted green line for the 50%-regime is above the two other lines. The solid blue line for the 0%-regime is above the dashed red line for the 10%-regime in all but one period.

To investigate whether the differences across enforcement regimes are statistically significant we use in Table 3 Column (2) a Probit model for the post intervention periods with individual random effects regressing compliance on treatments (0%-regime is the omitted category), size of the individual cap, and period dummies. Standard errors are clustered at the group level. We observe that compliance is lower in the 10%-regime than in the 0%-regime and this difference is marginally statistically significant at $p=0.074$. We also observe that compliance is higher in the 50%-regime than in the 0%-regime ($p<0.001$) and that the higher the individual cap, the higher is the compliance.

RESULT 2: *Property right enforcement regimes increase compliance in a non-monotonic manner: compliance is marginally lower with a 10% detection probability than with a 0% probability. Compliance is highest with a 50% detection probability.*

Turning to efficiency, we find that the mean efficiency in periods 6-10 is 83.7% in the 0%-regimes, which is substantially higher than in the 10%-regimes where the corresponding mean efficiency is below 70%, regardless of whether penalties are taken into account (Mann-Whitney test, $n=46$, $p<0.003$).¹³ The 50%-regime has the highest mean efficiency (94.14%; Mann-Whitney test, 50% against 10%-regime, $n=45$, $z=2.14$, $p=0.033$) but only if penalties are not taken into account. If penalties are taken into account, the efficiency rate is only 81.8%, which is not statistically significantly different to the efficiency rate in the 0%-regime (Mann-Whitney test, $n=45$, $z=0.569$, $p=0.57$). The different efficiency rates in the three enforcement regimes after taking into account penalties can also be seen in the right panel of Figure 1. This

¹³ Taking penalties into account means subtracting them from the group's total profits when calculating overall efficiency, i.e. assuming that penalties represent a social loss. Not taking penalties into account means calculating group profits before penalties are deducted, i.e. assuming that penalties are redistributed to a regulator or other entity but are not lost. We recognize that assuming penalties represent a social loss is a very strict assumption (i.e. the shadow cost of confiscation is equal to one). We do not mean to imply that the revenues from fines will always be destroyed or lost in bureaucratic coffers. The reason for the distinction is simply to examine social efficiency: (i) from the perspective of solely the resource extractors, and (ii) from the perspective of all potential stakeholders.

figure indicates that there are no obvious visual differences between the 0%- and 50%-regimes. In contrast, the 10%-regime has a lower efficiency than the 50% regime in periods 7-10.

In Table 3 Column (3) we observe that the 0%-regime significantly increases efficiency by 11.38 percentage points in the post intervention periods before accounting for penalties ($p < 0.01$). The regression also shows that outcomes in the 10%-regime are significantly less efficient than in the 0% regime before penalties are taken into account and that outcomes in the 50%-regime are more efficient than in the 0%-regime ($p < 0.05$ for both). However, after taking penalties into account (Column (4)), efficiencies in the 50%-regime are not statistically different to the 0%-regime and efficiencies are significantly lower in the 10%-regime as compared to the 0%-regime (8.72 percentage points, $p < 0.05$).

[Insert Table 3 about here]

Our interpretation of the previous findings is the following. Stricter enforcement leads to greater compliance but the relationship is not monotonic. Weak enforcement of the rules is worse than no enforcement whatsoever. This result is reminiscent of field examples in Ostrom (1990) and Gneezy and Rustichini (2000a) and laboratory results in Cardenas et al. (2000). Weak enforcement of the rules appears to displace a social norm to do the right thing that is present when there is no enforcement. This finding is also consistent with Charness' (2000) responsibility alleviation effect or Falk and Kosfeld's (2006) controlling effect: pro-social preferences are lowered when an external authority influences outcomes.¹⁴

4.3 Property right allocation regimes

Table 4 reports mean group extractions for the different allocation regimes (proportional, equal, inverse) in pre- and post-intervention periods. We have pooled the different enforcement

¹⁴ An alternative explanation is that many participants in the 0%-regimes took the cap at face value and were unaware of the possibility of exceeding the cap. However, a closer look at the data questions this interpretation. First, we observe large differences in compliance in the three different 0% regimes and a low compliance level in the 0%-proportional regime. If many participants took the 0% cap at face value, there is little reason to believe that this misperception depends on the treatment. Second, as there is substantial non-compliance in all 0% regimes and participants can observe this in their groups (in 17 out of 21 groups at least one participant exceeded the cap in Period 6), one should expect that participants who may have taken the caps at face value quickly learn that they can exceed the cap.

treatments by allocation regime. All three regimes significantly reduce extractions. We observe the largest mean reductions in the inverse-regimes (-14.76%, Mann-Whitney test, $n=50$, $z=3.14$, $p=0.002$). The reductions in the equal and proportional regimes are similar (-9.65% and -8.92%, Mann-Whitney test, $p<0.048$ for both).

[Insert Table 4 about here]

The left panel of Figure 2 illustrates mean extraction over all post-intervention periods and distinguishes between the three allocation regimes. We observe that the solid blue line for the proportional regimes is always above the dotted green line for the inverse regimes. The dashed red line for the equal regimes lies in between these two and is more stable over time, reminiscent of the temporal stability of the 50% enforcement regime. The allocation of property rights clearly has an impact on the level and trend of extraction.

[Insert Figure 2 about here]

We start testing for treatment differences using a non-parametric approach. The mean extraction in the proportional regime for all post-intervention periods is 17.33 and the corresponding mean for the inverse regime is 15.31. This difference is statistically significant for a Mann-Whitney test ($p=0.02$, $n=48$) and, for the sake of comparison, for a simple t-test ($p=0.02$, $t=2.39$, $n=48$). The mean extraction for the equal regime is 16.33 and this is not statistically different to the extraction in the proportional or the inverse regime, using non-parametric tests. Turning to our parametric regression-based testing approach, Table 5 is the equivalent of Table 3 but for the property right allocation regimes. In Column (1), we observe that extractions decrease after the intervention in the proportional regimes (-1.47 tokens, $p<0.01$). The regression model also shows that extractions are lower in the inverse than in the proportional regimes ($p=0.066$) and that there are no significant differences in extractions between the proportional and equal regimes ($p=0.624$).

There are large differences in compliance between the three different allocation regimes. Compliance is significantly lower in the proportional (38.7%) than in the equal (61.6%) and inverse regimes (60%). The difference between proportional and equal is significant at $p<0.01$

using a Mann-Whitney test and the same is true for the difference between proportional and inverse. In the middle panel of Figure 2 we can see that the solid blue line for the proportional regime is clearly below the other two lines. We also observe that compliance seems to fall over time, in particular in the proportional and inverse regimes. Again, behavior is more stable in the equal allocation regime. Column (2) in Table 5 corresponds to Column (2) in Table 3 but uses the different property right allocation regimes as dependent variables. We observe that individual compliance is significantly stronger in the equal and inverse regimes as compared to the proportional regime ($p < 0.01$ for both). It is important to remember that our experimental design ensured that the caps in the proportional and inverse regimes are essentially identical. If we interact property right allocation regimes with the cap size we find that the interaction is highly significant for the equal and inverse regimes but not statistically significant for the proportional regime. Together, this suggests that caps fail to constrain low and high extractors in the proportional regime. There is no difference in individual compliance between the equal and inverse regimes. Thus, we reject Hypothesis 1 (Compliance), although we do not observe statistically significant evidence that compliance is higher under inverse compared to equal which would provide even stronger evidence that allocation matters for compliance.

RESULT 3: *The allocation of the cap significantly affects compliance: compliance is significantly lower in the proportional than in the equal and inverse regimes.*

The right panel of Figure 2 illustrates efficiency rates after taking into account penalties. This figure suggests that efficiencies are lower in the proportional than in the inverse regimes. The solid blue line for the proportional regimes drops below 70% in periods 8-10, a value that is similar to pre-intervention efficiencies. Indeed, the increase in efficiencies from pre- to post-intervention periods (66.6% to 70.5%) is insignificant for the proportional regimes (Mann-Whitney test, $n=46$, $z=0.80$, $p=0.423$) whereas it is significant for the inverse regimes (Mann-Whitney test, $n=50$, $z=2.28$, $p=0.023$).

We start comparing efficiency differences across allocation regimes using non-parametric tests. The mean efficiency before taking penalties into account in the proportional regime is 77.2% and the mean efficiency in the inverse regime is 86.5%. This difference is only weakly significant using a Mann-Whitney test ($p=0.089$, $n=48$). Mean efficiency before penalties are

taken into account in the equal allocation regime is 82.7% and this is not statistically different to either the proportional or inverse regimes. If we calculate efficiencies after taking penalties into account, the differences become more pronounced. Mean efficiency in the proportional regime is only 70.5% but 83.5% in the inverse regime and this difference is now statistically significant ($p=0.004$, $n=48$). Efficiency in the equal allocation regime is 78.8%, resulting in a larger absolute gap between proportional and equal, but this difference is not statistically significant ($p=0.11$, $n=45$) using a non-parametric Mann-Whitney test although the difference is significant using a simple one-tailed t-test ($p=0.04$, $n=45$).

Turning to our parametric regression-based testing approach, Column (3) in Table 5 reveals that group efficiencies significantly increase in the post-intervention periods ($p<0.01$) and that there are no significant differences in post intervention increases in group efficiencies between the allocation regimes before taking penalties into account. However, as can be seen in Column (4), as soon as penalties are considered – which are frequent in the proportional treatments as indicated by the low rate of compliance –we find that the inverse regime significantly increases efficiency as compared to the proportional regime ($p<0.05$). The combination of our non-parametric and parametric findings leads to the following result and our rejection of Hypothesis 2 (Efficiency).

RESULT 4: *The allocation of the cap affects efficiency: the inverse regime is more efficient than the proportional and equal regimes, after penalties are taken into account.*

[Insert Table 5 about here]

4.4 Property right enforcement-allocation regimes

So far we have separately analyzed the effect of enforcement and allocation regimes on extractions, compliance, and efficiency. We now analyze whether there are *enforcement* $\times$ *allocation* regime interactions. Table 6 shows the pre- and post-intervention mean group extractions. Figure 3 shows the change of extractions, compliance, and efficiencies in the post-

intervention periods for the nine different enforcement-allocation regimes. In addition, Table 7 corresponds to our previous regression tables but distinguishes between all nine different enforcement-allocation regimes.

[Insert Table 6 about here]

[Insert Figure 3 about here]

The results in Table 6 further confirm the unique impact of the inverse regime. The inverse allocation regime leads to a statistically significant reduction in extraction, regardless of the level of enforcement. This is not true for any of the other allocation regimes. Furthermore, the magnitude of the reduction, conditional on enforcement level, is always larger for the inverse regime. Not surprisingly, the 50% enforcement regime also always leads to a statistically significant reduction in extraction.

The different efficiency rates in the three allocation regimes after taking into account penalties can also be seen in the right column of Figure 3. The line for the inverse allocation regime is always above the proportional regime, regardless of enforcement probability (except for the final period in the 50% regime). The differences between the inverse regime and the equal regime are starkest in the 0% enforcement cases and disappear in the 10% and 50% enforcement regimes. There are still differences between the allocation regimes when enforcement occurs 50% of the time. In terms of extraction, the proportional regime line is always higher (except for one period) than the other two regime lines. In terms of compliance, compliance is lower in every single period for the proportional allocation line compared to the other allocation lines.

Table 7 uses four regression models where we regress extraction, compliance, and efficiency (without and with penalties taken into account) in a given period on the nine different allocation-enforcement regimes. As the omitted category we use the 10%-proportional regime. In Column (1), we observe that the 10%-proportional regime does not reduce extractions in periods 6-10. However, seven of the eight other regimes reduce extractions compared to the 10%-proportional regime ($p < 0.1$). In Column (2), we observe that individual compliance is larger in all eight other regimes, and highly significant in seven of these eight regimes ($p < 0.005$). Columns (3) and (4) show that 6 (if penalties are not taken into account) to 7 (if penalties are

taken into account) regimes have significantly higher efficiencies than the 10%-proportional regime ($p < 0.1$).

Table 7 also corroborates the relatively poor performance of the 0%-proportional regime. For example, we also observe in Column (2) that compliance in the 0%-proportional regime is lower than in the 0%-equal and 0%-inverse regimes ($p < 0.01$). While the 0% and 10% proportional regimes are clearly poor performers, the 50%-proportional regime is not different from the 50%-equal and 50%-inverse regimes when it comes to the reduction in extractions. However, interestingly, individual compliance is still significantly lower in the 50%-proportional regime than in the 50%-equal and 50%-inverse regime ($p < 0.05$). Lastly, Columns (3) and (4) show that the 50%-regimes perform relatively well as long as penalties are not taken into account. This is because they significantly reduce extractions and increase compliance. If penalties are taken into account, the 0%-inverse regime performs better than all other regimes.

[Insert Table 7 about here]

RESULT 5: *The proportional regime with a 10% enforcement level is the worst performing institution in terms of extractions, compliance and efficiencies.*

Our interpretation of Result 5 is that occasional enforcement and the unfair allocation of property rights leads to the “worst of both worlds”. Weak enforcement crowds out social norms to comply with the rules and the proportional allocation triggers inequity aversion, which reduces willingness to comply.

4.5 Types and their reactions to property right enforcement and allocation regimes

There is considerable experimental evidence that not all individuals are purely selfish but that some individuals are willing to voluntarily restrict extraction in the commons (Ostrom, 2000; Fehr and Leibbrandt, 2011), and that many individuals are conditional cooperators (Fischbacher et al, 2001; Keser and van Winden, 2002; Frey and Meier, 2004; Shang and Croson, 2008). However, we still know little about how these different types react to behavioral interventions such as those described in this paper. Knowing how different types react to

different property right enforcement and allocation regimes will allow us to look beneath the surface of average intervention effects and identify how they affect extraction and compliance.

We decided to characterize subjects into four common types: unconditional low extractors, conditional low extractors, conditional high extractors, and unconditional high extractors. We characterized exactly half of our subjects as conditional. To be classified as a conditional extractor, individual extractions needed to correlate with the mean extractions of the other three group members in the five pre-intervention periods with $r > .3$ (Pearson correlation). All other subjects were classified as ‘unconditional’. We also characterized exactly half of our subjects as low extractors. To be classified as low extractors, total individual extractions needed to be below 94 tokens in the first five pre-intervention periods (18.8 tokens per period, on average). All other subjects were classified as high extractors.

Table 8 illustrates the compliance behavior of these four types in the 0% and 10% enforcement regimes and in the proportional, equal, and inverse allocation regimes (for 0% and 10% enforcement). Recall that the penalty in the 10% enforcement regime was too low to deter self-interested individuals from exceeding their individual cap when caps are allocated equally. But that with asymmetric caps, self-interested individuals with high caps (e.g. 15) will want to rationally comply with their caps.

[Insert Table 8 about here]

Interestingly, we observe that high extractors, both conditional and unconditional, adjust their extraction behavior more between the different enforcement regimes than low extractors: compliance rates for the low extractors are almost identical between 0% and 10% enforcement but conditional high extractors are almost twice as likely to comply with 0% enforcement compared to 10% enforcement. This suggests that weakly enforced rules have different impacts on different behavioral types.

There are also interesting patterns in terms of the allocation regimes. For unconditional high extractors, compliance in the inverse (36.7%) and equal regimes (38.6%) is more than double the compliance rate in the proportional regime (15.6%), even though they receive the most generous caps under proportional allocation. This violates the predictions of a standard game-theoretic model, especially for 10% enforcement. These findings provide speculative

evidence that high extractors are voluntarily willing to restrict themselves under certain property right regimes and support the model of inequity-averse compliance in Section 2.

Low extractors also adjust their extraction and compliance behavior to the different regimes, but in a quite different manner. We can see that low extractors, although in general more willing to comply than high extractors, have much lower compliance in the proportional regimes (14.7-20%) as compared to the equal and inverse regimes (53.3-76.5%). This may be evidence of inequity aversion or it could simply be rational self-interested behavior: low extractors receive the lowest caps under the proportional regime. This behavior could also be explained by reversion to the mean. Thus, the introduction of property right enforcement and allocation regimes can move low extractors to more self-interested behavior whereas the opposite seems to be the case for high extractors who deviate more from self-interested behavior.

5. Conclusion

Our results demonstrate that the allocation and enforcement of property rights matter for compliance and extraction. In terms of enforcement, we obtain an “Enforce Enough or Don’t Enforce at All” result akin to Gneezy and Rustichini (2000b). Compliance with the newly created property rights is higher and extraction is lower when there is no enforcement whatsoever compared to a 10% probability of getting caught. In terms of allocation, the most equitable allocation in terms of overall payoffs – the inverse regime –outperforms the other allocation mechanisms. Equitable allocation with no enforcement performed better than inequitable allocation with high enforcement. In conclusion, our results challenge the view that equity is irrelevant for the functioning of property right regimes. Our results appear to be driven by a combination of high extractors respecting property right regimes that punish them but are allocated in an equitable manner and low extractors disregarding property right regimes that punish them and could be perceived as inequitable.

Our findings are naturally most informative to field settings where property rights are poorly enforced, as is the case in many developing countries. In field settings where the enforcement of property rights is guaranteed, allocation is probably less important. However, as the scope of property rights solutions moves beyond industrial applications (such as cap-and-trade pollution programs) to more micro level contexts (such as individual harvesting or household emissions rights), our findings will have increasing relevance for industrialized

countries with strong property rights. The monitoring of compliance at the level of individuals is much more costly than monitoring a small number of large firms, implying that detection probabilities will be lower. Furthermore, behavioral motivations and social norms are likely to be much more prevalent among households than among large firms. This is particularly relevant to any future legislative attempts, nationally or internationally, to regulate the greenhouse gases produced by farmers. Our findings may also be germane to other public policy issues that rely on both norms and punishment for compliance, such as taxes (Andreoni et al., 1998), crime (Elster, 1989), worker compensation (Fehr et al., 1998), and voting (Barry, 1988).

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Figures

Figure 1: Post-intervention Extraction, Compliance, and Efficiency in Property Right Enforcement Regimes

Figure 2: Post-intervention Extraction, Compliance, and Efficiency in Property Right Allocation Regimes

Figure 3: Post-intervention Extraction, Compliance, and Efficiency in Property Right Enforcement-Allocation Regimes

Tables

Table 1: Treatment Matrix

Allocation Regime: (number of subjects) \ Detection probability: (number of subjects)	0% (n=84)	10% (n=100)	50% (n=96)
Proportional (n=92)	0%-proportional (n=28)	10%-proportional (n=32)	50%-proportional (n=32)
Equal (n=88)	0%-equal (n=24)	10%-equal (n=36)	50%-equal (n=28)
Inverse (n=100)	0%-inverse (n=32)	10%-inverse (n=32)	50%-inverse (n=36)

Table 2: Pre- and post-intervention group extractions in property right enforcement regimes

Enforcement Regime: Detection probability (Number of groups)	Mean extraction in pre-intervention periods 1-5 (standard deviation)	Mean extraction in post-intervention periods 6-10 (standard deviation)	Difference in percentage & significance (Mann-Whitney test)
0% (N=21)	18.09 (2.02)	16.10 (2.81)	-10.99% **
10% (N=25)	19.49 (2.15)	18.62 (2.10)	-4.45% *
50% (N=24)	17.38 (2.369)	14.04 (2.08)	-19.22% ***

Notes: * p<0.1, ** p<0.05, *** p<0.01.

Table 3: Extraction, compliance, and efficiency depending on property right enforcement regime

model	(1)	(2)	(3)	(4)
Dependent Variable	extraction	individual compliance	group efficiency w/o penalties	group efficiency accounting for penalties
Post-intervention	-2.084*** (0.534)		11.377*** (3.008)	11.379*** (3.007)
10% regime		-0.406* (0.228)		
50%-regime		1.352*** (0.259)		
Post-intervention x 10%-regime	1.642*** (0.636)		-8.024** (3.890)	-8.722** (3.908)
Post-intervention x 50%-regime	-1.616** (0.684)		8.596** (3.403)	-3.731 (3.868)
Individual cap		0.201*** (0.053)		
Constant	18.345*** (0.281)		70.793*** (1.989)	70.770*** (1.988)
N (groups)	2800 (70)	1400 (70)	2800 (70)	2800 (70)
R-sqr	0.083		0.140	0.024

Notes: *p<0.1, **p<0.05, ***p<0.01. Robust standard errors clustered on group level in parentheses. Omitted category is the 0%- regime. Post-intervention equals one in periods 6-10 and zero in periods 1-5. Models 1,3,4 are random-effects GLS regressions. Model 2 is a random-effects Probit regression and controls for period effects.

Table 4: Pre- and post-intervention group extractions in property right allocation regimes

Allocation Regime (Number of groups)	Mean extraction in pre-intervention periods 1-5 (standard deviation)	Mean extraction in post-intervention periods 6-10 (standard deviation)	Difference in percentage & significance (Mann- Whitney test)
proportional (N=23)	19.03 (2.00)	17.33 (2.88)	-8.92% **
equal (N=22)	18.07 (2.15)	16.33 (2.10)	-9.65% **
inverse (N=25)	17.96 (2.44)	15.31 (2.97)	-14.76% ***

Notes: * p<0.1, ** p<0.05, *** p<0.01.

Table 5: Extraction, compliance, and efficiency depending on property right allocation regime

model	(1)	(2)	(3)	(4)
Dependent Variable	extraction	individual compliance	group efficiency w/o penalties	group efficiency accounting for penalties
Post-intervention	-1.470*** (0.540)		9.180** (3.690)	2.085 (3.336)
Equal-regime		0.982*** (0.286)		
Inverse-regime		1.010*** (0.296)		
Post-intervention x Equal-regime	-0.366 (0.746)		1.585 (4.418)	4.959 (4.192)
Post-intervention x Inverse-regime	-1.306* (0.710)		4.988 (4.320)	9.355** (3.846)
Individual cap		0.222*** (0.059)		
Constant	18.345*** (0.281)		70.793*** (1.989)	70.770*** (1.988)
N (groups)	2800 (70)	1400 (70)	2800 (70)	2800 (70)
R-sqr	0.040		0.065	0.021

Notes: *p<0.1, **p<0.05, ***p<0.01. Robust standard errors clustered on group level in parentheses. Omitted category is the proportional regime. Post-intervention equals one if periods are 6-10, and zero if periods are 1-5. Models 1,3,4 are random-effects GLS regressions. Model 2 is a random-effects Probit regression and controls for period effects.

Table 6: Pre- and post-intervention group extractions in property right enforcement and allocation regimes

Regime: (Number of groups)	Mean extraction in pre-intervention periods 1-5 (standard deviations)	Mean extraction in post-intervention periods 6-10 (standard deviations)	Difference in percentage & significance (Mann- Whitney test)
0% & inverse (N=8)	17.31 (2.68)	14.68 (2.47)	-15.2% *
10% & inverse (N=8)	19.78 (1.46)	18.02 (1.99)	-8.9% *
50% & inverse (N=9)	16.92 (2.17)	13.46 (2.47)	-20.4% **
0% & equal (N=6)	18.42 (1.55)	16.57 (3.49)	-10.0%
10% & equal (N=9)	18.63 (3.17)	18.00 (2.19)	-3.4%
50% & equal (N=7)	17.06 (2.27)	13.96 (1.51)	-18.2% **
0% & proportional (N=7)	18.70 (1.38)	17.33 (2.09)	-7.3%
10% & proportional (N=8)	20.17 (0.82)	19.92 (1.67)	-1.2%
50% & proportional (N=8)	18.18 (2.79)	14.75 (2.06)	-18.9% **

Notes: * p<0.1, ** p<0.05, *** p<0.01.

Table 7: Extraction, compliance, and efficiency depending on property right enforcement & allocation regime

model	(1)	(2)	(3)	(4)
Dependent Variable	extraction	individual compliance	group efficiency w/o penalties	group efficiency accounting for penalties
Post-intervention	0.443 (0.697)		-4.596 (6.097)	-4.364 (6.122)
Post-intervention in 0% Proportional-regime	-1.679* (0.982)	0.460 (0.308)	12.688* (7.251)	12.454* (7.274)
Post-intervention in 0% Equal-regime	-2.267* (1.317)	1.364*** (0.325)	13.741* (7.877)	13.493* (7.880)
Post-intervention in 0% Inverse-regime	-3.470*** (1.034)	1.694*** (0.402)	20.780*** (7.420)	20.431*** (7.488)
Post-intervention in 10% Equal-regime	-0.959 (0.864)	1.313*** (0.306)	10.297 (6.750)	8.188 (6.929)
Post-intervention in 10% Inverse-regime	-1.650* (0.847)	0.934*** (0.254)	12.353* (6.781)	12.297* (6.800)
Post-intervention in 50% Equal-regime	-4.027*** (0.994)	2.831*** (0.416)	24.073*** (6.867)	13.994* (8.284)
Post-intervention in 50% Inverse-regime	-4.444*** (1.044)	2.783*** (0.390)	23.617*** (6.685)	15.079** (6.730)
Post-intervention in 50% Proportional-regime	-3.933*** (0.979)	1.980*** (0.344)	26.726*** (6.543)	7.136 (7.444)
Individual cap		0.212*** (0.057)		
Constant	18.345*** (0.281)		70.793*** (1.991)	70.770*** (1.990)
N (groups)	2800 (70)	1400 (70)	2800 (70)	2800 (70)
R-sqr	0.093		0.156	0.038

Notes: *p<0.1, **p<0.05, ***p<0.01. Robust standard errors clustered on group level in parentheses. Omitted category is the 10%-proportional regime. Post-intervention equals one if periods are 6-10, and zero if periods are 1-5. The variables "Post-intervention in ..." represent interactions between post-intervention and the corresponding treatment. Models 1,3,4 are random-effects GLS regressions. Model 2 is a random-effects Probit regression and controls for period effects.

Table 8: Types and their reactions to property right enforcement & allocation regimes

Type		Enforcement regimes		Allocation regimes (0% and 10%)		
		0%	10%	proportional	equal	inverse
Unconditional low extractor	Compliance <i>N (individuals)</i>	51.8% 22	47.6% 21	14.7% 15	72.9% 17	61.8% 11
Conditional low extractor	Compliance <i>N (individuals)</i>	57.4% 23	53.3% 15	20.0% 9	53.3% 12	76.5% 17
Conditional high extractor	Compliance <i>N (individuals)</i>	45.0% 20	24.2% 33	37.8% 18	35.3% 17	23.3% 18
Unconditional high extractor	Compliance <i>N (individuals)</i>	24.2% 19	32.9% 31	15.6% 18	38.6% 14	36.7% 18

Notes: Types were defined as follows: All subjects whose own extraction correlates with the extraction of their group members for $>.3$ in periods 1-5 (50% of the sample), are characterized as conditional types; the others as unconditional types. All subjects who extract less than 94 tokens in periods 1-5 (50.4% of the sample) are characterized as low extractor; the others as high extractor. Compliance is the percentage with which subjects complied with the cap in periods 6-10.

Appendix – Proofs

Proof of Proposition 1

In order to determine preferences, we calculate the utility that each of the four agents will receive under the three different allocation mechanisms. We start with Agent 1, who receives the least under grandfathering and the most under inverse sharing. The utility of Agent 1 under grandfathering is given by the following:

$$U_1^G(x) = 2.5x_1 - \alpha_1 \frac{1}{3} [(2.5(x_4 - x_1) + 2.5(x_3 - x_1) + 2.5(x_2 - x_1))] - 0.$$

And their utility under equal sharing is given by:

$$U_1^E(x) = x_1 + 1.5\bar{x} - \alpha_1 \frac{1}{3} [(x_4 - x_1) + (x_3 - x_1) + (x_2 - x_1)] - 0.$$

It is clear that agent 1 always receives more utility from equal sharing since their personal payoff is strictly higher (by assumption $x_1 < \bar{x}$ since $\bar{x} = \frac{1}{n} \sum_{i=1}^n x_i$ and $x_1 < x_2 < x_3 < x_4$) and they experience strictly less inequity aversion under equal sharing (the negative utility is two and a half times as large under grandfathering). Under inverse sharing, the utility of Agent 1 is given by:

$$U_1^I(x) = 2.5\bar{x}.$$

This allocation provides more utility than equal sharing since the personal payoff is strictly higher ($2.5\bar{x} > x_1 + 1.5\bar{x}$, by definition) and negative utility from inequity aversion has disappeared. Agent 1 strictly prefers inverse to equal, which is preferred to grandfathering.

We now turn our attention to Agent 2. Under grandfathering, their utility will be the following:

$$U_2^G(x) = 2.5x_2 - \alpha_2 \frac{1}{3} [(2.5(x_4 - x_2) + 2.5(x_3 - x_2))] - \beta_2 \frac{1}{3} [2.5(x_2 - x_1)].$$

And their utility under equal sharing is given by:

$$U_2^E(x) = x_2 + 1.5\bar{x} - \alpha_2 \frac{1}{3} [(x_4 - x_2) + (x_3 - x_2)] - \beta_2 \frac{1}{3} [(x_2 - x_1)].$$

Under inverse sharing, the utility of Agent 2 is given by:

$$U_2^I(x) = 2.5\bar{x}.$$

We calculate the difference in utility for Agent 2 between equal sharing and grandfathering:

$$\Delta U_2^{E-G}(x) = -1.5x_2 + 1.5\bar{x} - \alpha_2 \frac{1}{3} [(x_4 - x_2) + (x_3 - x_2)]$$

$$\begin{aligned}
& +\alpha_2 \frac{5}{6} [((x_4 - x_2) + (x_3 - x_2))] - \beta_2 \frac{1}{3} [2(x_2 - x_1)] + \beta_2 \frac{5}{6} [(x_2 - x_1)] \\
& = -1.5x_2 + 1.5\bar{x} + \alpha_2 \frac{1}{2} [((x_4 - x_2) + (x_3 - x_2))] + \beta_2 \frac{1}{2} [(x_2 - x_1)].
\end{aligned}$$

Under what conditions will the term on the right-hand-side be positive? This expression is essentially $1.5(\bar{x} - x_2)$ plus two positive numbers (since $x_1 < x_2 < x_3 < x_4$), so its sign hinges on whether or not the following condition holds:

$$\alpha_2 \frac{1}{2} [((x_4 - x_2) + (x_3 - x_2))] + \beta_2 \frac{1}{2} [(x_2 - x_1)] \geq 1.5(x_2 - \bar{x})$$

Expanding the term for $\bar{x}$, multiplying by $\frac{2}{3}$, and rearranging gives:

$$\begin{aligned}
& \alpha_2 \frac{1}{3} [((x_4 - x_2) + (x_3 - x_2))] + \beta_2 \frac{1}{3} [(x_2 - x_1)] \geq x_2 - \frac{x_1 + x_2 + x_3 + x_4}{4} \\
& \frac{\alpha_2}{3} x_4 + (\beta_2 \frac{1}{3} - 2 \frac{\alpha_2}{3}) x_2 + \frac{\alpha_2}{3} x_3 - \frac{\beta_2}{3} x_1 \geq \frac{3}{4} x_2 - \frac{1}{4} x_1 - \frac{1}{4} x_3 - \frac{1}{4} x_4 \\
& \left[\frac{\alpha_2}{3} x_4 + \frac{1}{4} x_4 \right] + \left[\frac{\alpha_2}{3} x_3 + \frac{1}{4} x_3 \right] + (\beta_2 \frac{1}{3} - 2 \frac{\alpha_2}{3} - \frac{3}{4}) x_2 + \left(\frac{1}{4} - \frac{\beta_2}{3} \right) x_1 \geq 0 \\
& \left[\frac{\alpha_2}{3} + \frac{1}{4} \right] x_4 + \left[\frac{\alpha_2}{3} + \frac{1}{4} \right] x_3 + (\beta_2 \frac{1}{3} - 2 \frac{\alpha_2}{3} - \frac{3}{4}) x_2 + \left(\frac{1}{4} - \frac{\beta_2}{3} \right) x_1 \geq 0.
\end{aligned}$$

The first two terms on the LHS are clearly positive. The sum of the third and fourth terms is potentially negative. Since x_4 and x_3 are both greater than x_2 and x_1 , if the sum of their coefficients (which are positive) are greater or equal than the negative of the sum of the coefficients on the other variables, then the total sum will always be positive:

$$\left[\frac{\alpha_2}{3} + \frac{1}{4} \right] + \left[\frac{\alpha_2}{3} + \frac{1}{4} \right] \geq - \left[(\beta_2 \frac{1}{3} - 2 \frac{\alpha_2}{3} - \frac{3}{4}) + \left(\frac{1}{4} - \frac{\beta_2}{3} \right) \right].$$

The LHS and RHS above actually equal each other, therefore the condition does hold and,

$$\left[\frac{\alpha_2}{3} + \frac{1}{4} \right] x_4 + \left[\frac{\alpha_2}{3} + \frac{1}{4} \right] x_3 + (\beta_2 \frac{1}{3} - 2 \frac{\alpha_2}{3} - \frac{3}{4}) x_2 + \left(\frac{1}{4} - \frac{\beta_2}{3} \right) x_1 > 0,$$

which implies that $\Delta U_2^{E-G}(x) > 0$ and Agent 2 always receives more utility under equal sharing.

We now subtract the utility from equal sharing from the utility from inverse sharing:

$$\begin{aligned}
\Delta U_2^{I-E}(x) &= 2.5\bar{x} - x_2 - 1.5\bar{x} + \alpha_2 \frac{1}{3} [((x_4 - x_2) + (x_3 - x_2))] + \beta_2 \frac{1}{3} [(x_2 - x_1)] \\
&= \bar{x} - x_2 + \alpha_2 \frac{1}{3} [((x_4 - x_2) + (x_3 - x_2))] + \beta_2 \frac{1}{3} [(x_2 - x_1)].
\end{aligned}$$

This is equivalent to $\frac{2}{3}\Delta U_2^{E-G}(x)$, i.e. two-thirds times the expression for the difference between equal sharing and grandfathering. We know from the above that this expression is strictly positive; therefore, Agent 2 receives higher utility from the inverse sharing allocation compared to the equal allocation.

We now turn our attention to Agent 3. Under grandfathering, utility for Agent 3 is given by:

$$U_3^G(x) = 2.5x_3 - \alpha_3 \frac{1}{3} [(2.5(x_4 - x_3))] - \beta_3 \frac{1}{3} [2.5(x_3 - x_2) + 2.5(x_3 - x_1)].$$

Under equal sharing, their utility is given by:

$$U_3^E(x) = x_3 + 1.5\bar{x} - \alpha_3 \frac{1}{3} [(x_4 - x_3)] - \beta_3 \frac{1}{3} [(x_3 - x_2) + (x_3 - x_1)].$$

And under inverse sharing, it is:

$$U_3^I(x) = 2.5\bar{x}.$$

We start by calculating the difference in utility between equal sharing and grandfathering:

$$\begin{aligned} \Delta U_3^{E-G}(x) &= -1.5x_3 + 1.5\bar{x} - \alpha_3 \frac{1}{3} [(x_4 - x_3)] - \beta_3 \frac{1}{3} [(x_3 - x_2) + (x_3 - x_1)] + \alpha_3 \frac{1}{3} [(2.5(x_4 - x_3))] \\ &\quad + \beta_3 \frac{1}{3} [2.5(x_3 - x_2) + 2.5(x_3 - x_1)] \\ &= -1.5x_3 + 1.5\bar{x} + \alpha_3 \frac{1}{3} [1.5(x_4 - x_3)] + \beta_3 \frac{1}{3} [1.5(x_3 - x_2) + 1.5(x_3 - x_1)] \\ &= 1.5 \left[-x_3 + \bar{x} + \alpha_3 \frac{1}{3} [(x_4 - x_3)] + \beta_3 \frac{1}{3} [(x_3 - x_2) + (x_3 - x_1)] \right] \\ &= 1.5 \left[-x_3 + \frac{1}{4}x_1 + \frac{1}{4}x_2 + \frac{1}{4}x_3 + \frac{1}{4}x_4 + \alpha_3 \frac{1}{3} [(x_4 - x_3)] + \beta_3 \frac{1}{3} [(x_3 - x_2) + (x_3 - x_1)] \right] \\ &= 1.5 \left[\left[\frac{1}{4} - \beta_3 \frac{1}{3} \right] x_1 + \left[\frac{1}{4} - \beta_3 \frac{1}{3} \right] x_2 + \left[-\frac{3}{4} + \beta_3 \frac{2}{3} - \alpha_3 \frac{1}{3} \right] x_3 + \left[\frac{1}{4} + \alpha_3 \frac{1}{3} \right] x_4 \right] \\ &= \left[\frac{3}{8} - \beta_3 \frac{1}{2} \right] x_1 + \left[\frac{3}{8} - \beta_3 \frac{1}{2} \right] x_2 + \left[-\frac{9}{8} + \beta_3 - \alpha_3 \frac{1}{2} \right] x_3 + \left[\frac{3}{8} + \alpha_3 \frac{1}{2} \right] x_4. \end{aligned}$$

The first two coefficients on the RHS are possibly negative, the third coefficient is negative (since $\beta_3 < 1$) and the fourth coefficient is positive. We first define the conditions under which

the sum of the third and fourth terms will be positive. Since x_4 is greater than x_3 , the following condition guarantees that the sum will be positive:

$$\left[\frac{3}{8} + \alpha_3 \frac{1}{2}\right] + \left[-\frac{9}{8} + \beta_3 - \alpha_3 \frac{1}{2}\right] \geq 0$$

$$\beta_3 - \frac{6}{8} \geq 0$$

$$\beta_3 \geq \frac{3}{4}.$$

$\beta_3 \geq \frac{3}{4}$ guarantees that $\left[-\frac{9}{8} + \beta_3 - \alpha_3 \frac{1}{2}\right] x_3 + \left[\frac{3}{8} + \alpha_3 \frac{1}{2}\right] x_4 > 0$. We now wish to establish the conditions under which the whole expression is positive. Since x_4 and x_3 are both strictly greater than x_2 and x_1 , the sum will always be positive if:

$$\begin{aligned} -1.5 \left[\left[\frac{1}{4} - \beta_3 \frac{1}{3} \right] + \left[\frac{1}{4} - \beta_3 \frac{1}{3} \right] \right] &\leq 1.5 \left[-\frac{3}{4} + \beta_3 \frac{2}{3} - \alpha_3 \frac{1}{3} \right] + \left[\frac{1}{4} + \alpha_3 \frac{1}{3} \right] \\ &\quad - \left[\left[\frac{1}{2} - \beta_3 \frac{2}{3} \right] \right] \leq \left[-\frac{1}{2} + \beta_3 \frac{2}{3} \right]. \end{aligned}$$

This condition clearly holds so we can conclude that the whole expression is positive as long as $\beta_3 \geq \frac{3}{4}$, implying that Agent 3 prefers the equal allocation to grandfathering if they have strong inequity aversion. We now compare inverse sharing to equal sharing for Agent 3:

$$\begin{aligned} \Delta U_3^{I-E}(x) &= 2.5\bar{x} - \left[x_3 + 1.5\bar{x} - \alpha_3 \frac{1}{3} [((x_4 - x_3))] - \beta_3 \frac{1}{3} [(x_3 - x_2) + (x_3 - x_1)] \right] \\ &= \bar{x} - x_3 + \alpha_3 \frac{1}{3} [((x_4 - x_3))] + \beta_3 \frac{1}{3} [(x_3 - x_2) + (x_3 - x_1)]. \end{aligned}$$

which is the same expression as $\frac{2}{3} \Delta U_3^{E-G}(x)$, which we now know to be positive if $\beta_3 \geq \frac{3}{4}$. Thus, Agent 3 prefers inverse sharing to equal sharing if they have sufficiently strong preferences for equity.

Turning our attention to Agent 4, their utility under grandfathering will be given by the following:

$$U_4^G(x) = 2.5x_4 - 0 - \beta_4 \frac{1}{3} [2.5(x_4 - x_1) + 2.5(x_4 - x_2) + 2.5(x_4 - x_3)].$$

And their utility under equal sharing will be:

$$U_4^E(x) = x_4 + 1.5\bar{x} - 0 - \beta_4 \frac{1}{3} [(x_4 - x_1) + (x_4 - x_2) + (x_4 - x_3)].$$

Agent 4's utility under inverse sharing will be given by:

$$U_4^I(x) = 2.5\bar{x}.$$

We start by subtracting the utility from grandfathering from the utility from equal sharing:

$$\begin{aligned}\Delta U_4^{E-G}(x) &= 1.5\bar{x} - 1.5x_4 + \beta_4 \frac{1}{3} [-x_4 + x_1 - x_4 + x_2 - x_4 + x_3] + \\ &\quad \beta_4 \frac{1}{3} [2.5(x_4 - x_1) + 2.5(x_4 - x_2) + 2.5(x_4 - x_3)].\end{aligned}$$

What must be the value of β_4 in order for this difference to be greater than or equal to zero?

$$1.5\bar{x} - 1.5x_4 + \beta_4 \frac{1}{3} [-3x_4 + x_1 + x_2 + x_3] + \beta_4 \frac{1}{3} [7.5x_4 - 2.5x_1 - 2.5x_2 - 2.5x_3] \geq 0$$

$$1.5\bar{x} - 1.5x_4 + \beta_4 \frac{1}{3} [-3x_4 + x_1 + x_2 + x_3 + 7.5x_4 - 2.5x_1 - 2.5x_2 - 2.5x_3] \geq 0$$

$$1.5\bar{x} - 1.5x_4 + \beta_4 \frac{1}{3} [4.5x_4 - 1.5x_1 - 1.5x_2 - 1.5x_3] \geq 0$$

$$\bar{x} - x_4 + \beta_4 \frac{1}{3} [3x_4 - x_1 - x_2 - x_3] \geq 0$$

$$\beta_4 \left[x_4 - \frac{1}{3}x_1 - \frac{1}{3}x_2 - \frac{1}{3}x_3 \right] \geq x_4 - \frac{x_1 + x_2 + x_3 + x_4}{4}$$

$$\beta_4 \left[x_4 - \frac{1}{3}x_1 - \frac{1}{3}x_2 - \frac{1}{3}x_3 \right] \geq \frac{3}{4} \left[x_4 - \frac{1}{3}x_1 - \frac{1}{3}x_2 - \frac{1}{3}x_3 \right]$$

$$\beta_4 \geq \frac{3}{4}.$$

$\beta_4 \geq \frac{3}{4}$ guarantees that Agent 4 prefers the equal sharing allocation to the grandfathering allocation. We now compare inverse sharing to equal sharing:

$$\Delta U_4^{I-E}(x) = \bar{x} - x_4 + \beta_4 \frac{1}{3} [3x_4 - x_1 - x_2 - x_3].$$

The expression for ΔU_4^{I-E} is identical to that for ΔU_4^{E-G} , thus the same condition guarantees that Agent 4 will prefer inverse sharing to equal sharing.

Q.E.D.

Proof of Proposition 2

We adopt the same approach as before and calculate the utility that each of the four agents will experience under the three different allocation mechanisms. We now assume that agents have no fairness preferences, i.e. no inequity aversion ($\alpha_i = \beta_i = 0$). We start with Agent 1, who receives the least under grandfathering and the most under inverse sharing. The utility of Agent 1 under grandfathering is given by the following: $U_1^G(x) = 2.5x_1$. And their utility under equal sharing is given by $U_1^E(x) = x_1 + 1.5\bar{x}$. Under inverse sharing, the utility of agent 1 is given by $U_1^I(x) = 2.5\bar{x}$. By assumption $x_1 < \bar{x}$ since $\bar{x} = \frac{1}{n} \sum_{i=1}^n x_i$ and $x_1 < x_2 < x_3 < x_4$. Therefore, $2.5x_1 < x_1 + 1.5\bar{x} < 2.5\bar{x}$ and Agent 1 strictly prefers inverse to equal to grandfathering. We now turn to Agent 2. The utility of Agent 2 under grandfathering is given by the following: $U_2^G(x) = 2.5x_2$. And their utility under equal sharing is given by $U_2^E(x) = x_2 + 1.5\bar{x}$. Under inverse sharing, the utility of Agent 2 is given by $U_2^I(x) = 2.5\bar{x}$. Since x_2 could be greater or less than $\bar{x}$, there will be some cases where Agent 2 prefers inverse to equal to grandfathering and some cases where the agent prefers grandfathering to equal to inverse. The same logic applies to Agent 3. The utility of Agent 3 under grandfathering is given by the following: $U_3^G(x) = 2.5x_3$. And their utility under equal sharing is given by $U_3^E(x) = x_3 + 1.5\bar{x}$. Under inverse sharing, the utility of Agent 3 is given by $U_3^I(x) = 2.5\bar{x}$. Since x_3 could be greater or less than $\bar{x}$, there will be some cases where Agent 3 prefers inverse to equal to grandfathering and some cases where the agent prefers grandfathering to equal to inverse. Preferences will depend on how skewed the distribution of initial payoffs is. Agent 4's preferences are very straightforward. The utility of Agent 4 under grandfathering is given by the following: $U_4^G(x) = 2.5x_4$. And their utility under equal sharing is given by $U_4^E(x) = x_4 + 1.5\bar{x}$. Under inverse sharing, the utility of Agent 4 is given by $U_4^I(x) = 2.5\bar{x}$. Since this agent receives the largest allocation in Stage 1, it is always true that $x_4 > \bar{x}$. Therefore, $2.5x_4 > x_4 + 1.5\bar{x} > 2.5\bar{x}$ and Agent 4 strictly prefers grandfathering to equal sharing to inverse sharing.

Q.E.D.

Appendix - Instructions

EXPERIMENTAL INSTRUCTIONS

Introduction

Welcome to my experiment. For showing up on time, I will pay you a \$5 show-up fee. In addition, you may receive additional earnings as the result of the outcomes in the experimental session. Today's session will take about an hour. Please do not communicate with other participants during the experiment.

Details

At the beginning of this decision making experiment you will be matched with three other people, randomly selected from the people in this room, to form a group of four. You will remain in this group of four people for the duration of the whole decision making experiment. The identity of the other members of your group will never be revealed. You are going to make decisions with the other members of your group for five periods. Each period, you must decide how many tokens to extract: you can extract any number between 0 and 25. You can extract fractions of tokens, up to one decimal place. Tokens extracted in one period do not carry over to the next.

Tokens you extract each round will earn you a cash BENEFIT, which I describe below. But, any tokens you extract will also COST you money, which I also describe below.

IMPORTANT NOTICE: You should consider all monetary values (including your profits) as "computer dollars". At the end of the experiment we will pay you in cash an amount equal to \$1.00 for every 100 computer dollars you earn.

BENEFITS FROM TOKENS YOU EXTRACT

Let's denote the number of tokens you extract as x . The benefit to you from extracting tokens is given by the following formula:

$$BENEFIT = 25x.$$

At the end of these instructions is a table of the benefits of extracting tokens. You can use this table during the experiment to inform your decisions. For example, let's say you order 10 tokens in a given decision period. That token order will earn you BENEFITS of 250 computer dollars. Study this table carefully. If you have any questions raise your hand and I will help you.

Extracting "tokens" earns you a cash BENEFIT. But, there is a cost for all the tokens that you extract. This cost depends both on the amount you extract and the amount everyone else in your group extracts.

TOKEN COSTS

In each period, what you pay for tokens that you order equals the number of tokens you order times the Token Cost for that period. The Token Cost depends on how many tokens you order, as well as how many tokens the other members of your group order. Let's denote the total number of tokens withdrawn by your group (including yourself) as X . Then the Token Cost is as follows:

$$\text{Token Cost} = \frac{1}{4} X = 0.25 X.$$

If your group extracted a total of 40 tokens, the Token Cost would be 10. Your individual cost for the period equals the number of tokens you order times the Token Cost:

$$COST = x(0.25 X).$$

Now let's assume you had ordered 10 of the 40 tokens ordered by the group. Your individual cost would thus be $10 \times 10 = 100$ computer dollars. Please look at the tables at the end of these instructions and find the table labeled "COST FROM TOKENS". This table shows how your individual cost changes as you extract more and more tokens AND your group extracts more and more tokens.

Your profit for each period is equal to the benefit minus the cost:

$$PROFIT = BENEFIT - COST = 25x - x(0.25 X)$$

To continue the example, if you extracted 10 tokens and your group extracted a total of 40, then your profit for the period would be $250 - 100 = 150$ computer dollars. Please look at the tables at the end of these instructions and find the table labeled "PROFIT FROM TOKENS". This table shows how your profit changes as you extract more and more tokens AND your group extracts more and more tokens.

The total profit earned by your group thus depends on your decisions and the decisions of your group members (see the figure below). If no one in your group extracts any tokens, no one will earn any profit. If your group extracts 40 tokens, the group will earn a total profit of 600. If your group extracts 80 tokens, the group will earn a total profit of 400. If the group extracts 100 tokens, no one will earn any profit.

Individually, you earn the highest profit when your group members only extract a few tokens and you extract as many tokens as possible. For example, if your group members each extract 10 tokens and you extract 25 tokens, you will earn a profit of 281.

Each period, you choose how much to extract. At the end of each period, the extractions and profits of all the members of your group are revealed.

After the five periods are over, you will take part in a different decision game with the same group. Once both of these experiments are over, the computer will sum your profit from every period and I will pay you in cash in private. Remember that each computer dollar is worth one cent of real money.

I highly encourage clarifying questions. Thank you for your participation.

TABLES

BENEFIT FROM TOKENS

This Table Displays the Benefits for Various Token Extractions

TOKENS	BENEFITS
0	0
1	25
2	50
3	75
4	100
5	125
6	150
7	175
8	200
9	225
10	250
11	275
12	300
13	325
14	350
15	375
16	400
17	425
18	450
19	475
20	500
21	525
22	550
23	575
24	600
25	625

COST FROM TOKENS

This Table Displays Costs for Various Individual and Group Token Extractions

		The Number of Tokens You Extract					
		0	5	10	15	20	25
The Total Number of Tokens Extracted by Your Group	0	0	-	-	-	-	-
	5	0	6.25	-	-	-	-
	10	0	12.5	25	-	-	-
	15	0	18.75	37.5	56.25	-	-
	20	0	25	50	75	100	-
	25	0	31.25	62.5	93.75	125	156.25
	30	0	37.5	75	112.5	150	187.5
	35	0	43.75	87.5	131.25	175	218.75
	40	0	50	100	150	200	250
	45	0	56.25	112.5	168.75	225	281.25
	50	0	62.5	125	187.5	250	312.5
	55	0	68.75	137.5	206.25	275	343.75
	60	0	75	150	225	300	375
	65	0	81.25	162.5	243.75	325	406.25
	70	0	87.5	175	262.5	350	437.5
	75	0	93.75	187.5	281.25	375	468.75
	80	0	100	200	300	400	500
	85	0	106.25	212.5	318.75	425	531.25
	90	0	112.5	225	337.5	450	562.5
	95	0	118.75	237.5	356.25	475	593.75
	100	0	125	250	375	500	625

PROFIT FROM TOKENS

This Table Displays Profits for Various Individual and Group Token Extractions

	The Number of Tokens You Extract					
	0	5	10	15	20	25
0	0	-	-	-	-	-
5	0	118.75	-	-	-	-
10	0	112.5	225	-	-	-
15	0	106.25	212.5	318.75	-	-
20	0	100	200	300	400	-
25	0	93.75	187.5	281.25	375	468.75
30	0	87.5	175	262.5	350	437.5
35	0	81.25	162.5	243.75	325	406.25
40	0	75	150	225	300	375
45	0	68.75	137.5	206.25	275	343.75
50	0	62.5	125	187.5	250	312.5
55	0	56.25	112.5	168.75	225	281.25
60	0	50	100	150	200	250
65	0	43.75	87.5	131.25	175	218.75
70	0	37.5	75	112.5	150	187.5
75	0	31.25	62.5	93.75	125	156.25
80	0	25	50	75	100	125
85	0	18.75	37.5	56.25	75	93.75
90	0	12.5	25	37.5	50	62.5
95	0	6.25	12.5	18.75	25	31.25
100	0	0	0	0	0	0

The Total Number of Tokens Extracted by Your Group

EXPERIMENTAL INSTRUCTIONS – PART 2

We are now going to run a very similar experiment to before. As you can see from the figure below, the group makes the most total profit when only 50 tokens are extracted. Your group probably extracted more than 50 tokens. We're going to cap the total number of tokens that your group can extract at 50. We're then going to divide this total in direct proportion to how many tokens each group member extracted in the first 5 periods. For example, if someone in your group extracted 100% of the tokens in the first 5 periods, they will get an individual cap of 50 tokens and everyone else in the group will have a cap of 0 tokens. If someone extracted 50% of the tokens, they will get an individual cap of 25 tokens. If someone extracted 10% of the tokens, they will get an individual cap of 5 tokens.

The experiment will be the same as before except that sometimes there will be monitoring to see if you are under or above your individual cap. If you are caught extracting more than your individual cap, your profits for that period will be zero.

When will there be monitoring?

At the end of every period, the computer will randomly choose a number between 1 and 10. If it chooses any number from 1 to 5, then there will be monitoring and the computer will check if you are above your cap. If it chooses any number from 6 to 10, then there will be no monitoring and if you have extracted more than your cap, nothing will happen.

This means that, each period, there is a 50% chance that there will be NO monitoring.

Everything else is the same as before. Please raise your hand if you have any questions

EXPERIMENTAL INSTRUCTIONS – PART 2

We are now going to run a very similar experiment to before. As you can see from the figure below, the group makes the most total profit when only 50 tokens are extracted. Your group probably extracted more than 50 tokens. We're going to cap the total number of tokens that your group can extract at 50. We're then going to divide this EQUALLY among the members of your group. This means that each group member will receive an individual cap of 12.5 tokens.

The experiment will be the same as before and there will be no monitoring to see if you are under or above your individual cap.

Please raise your hand if you have any questions.

EXPERIMENTAL INSTRUCTIONS – PART 2

We are now going to run a very similar experiment to before. As you can see from the figure below, the group makes the most total profit when only 50 tokens are extracted. Your group probably extracted more than 50 tokens. We're going to cap the total number of tokens that your group can extract at 50. We're then going to divide this total in inverse proportion to how many tokens each group member extracted in the first 5 periods. For example, suppose two people in your group each extracted 40% of the tokens in the first 5 periods and the other two group members each extracted 10% of the tokens. The group members who extracted 40% will each get an individual cap of 10% of $50 = 5$ tokens. The group members who extracted 10% will each get an individual cap of 40% of $50 = 20$ tokens.

The experiment will be the same as before except that sometimes there will be monitoring to see if you are under or above your individual cap. If you are caught extracting more than your individual cap, your profits for that period will be zero.

When will there be monitoring?

At the end of every period, the computer will randomly choose a number between 1 and 10. If it chooses number 10, then there will be monitoring and the computer will check if you are above your cap. If it chooses any number from 1 to 9, then there will be no monitoring and if you have extracted more than your cap, nothing will happen.

This means that, each period, there is a 90% chance that there will be NO monitoring.

Everything else is the same as before. Please raise your hand if you have any questions