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Unintended consequences of enforcement in a cooperative institution: Experimental evidence from Tanzanian fishers

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ABSTRACT

Small-scale fisheries in developing countries employ the majority of the world's fishers and are a critical source of income and nutrition for billions of people, yet they frequently suffer from overfishing. To date, institutional reforms have largely consisted of those that have worked well in developed countries, but are poorly suited to the institutional contexts of most developing countries, which are characterized by weak state capacity and poor enforcement. We study the introduction of an enforcement institution among Tanzanian fishers using a novel artefactual field experiment. Results suggest that enforcement mechanisms can sometimes damage cooperative behavior as players shift from cooperative harvest strategies to more destructive ones, which causes the common-pool resource to be depleted faster. We explore the mechanisms by which this undesirable outcome arises and argue that institutional reform should consider that resource users make jointly determined decisions about gear choice, including illegal ones, and harvest rates.

1. Introduction

Small-scale fisheries off the coasts of developing countries are a prime example of the important role that institutions (formal or informal) play in creating wealth and shaping development outcomes in rural communities. Small-scale fisheries are known to employ the majority of world fishers and to provide food and livelihoods to a vast number of people living in coastal areas. Approximately 90 percent of the 38 million people recorded by the FAO globally as fishermen are classified as small-scale (FAO, 2008). However, because it is difficult to exclude others from entering a fishery and because use of a fishery is rivalrous, fisheries naturally suffer from the "tragedy of the commons," whereby excessive entry results in overexploitation of the resource (Gordon, 1954; Hardin, 1968).

Developed coastal nations have typically responded to the threat of overexploitation through a "top-down" approach by using legislature and enforcement infrastructure to enclose the commons—e.g., through regulated and restricted access programs (Reimer and Wilen 2013)—and by fostering incentives to maximize the economic value of the fishery—e.g., through catch share programs. Such institutional innovations have resulted in billions of dollars of new wealth (Wilen, 2006). Developing countries, unfortunately, often lack the infrastructure and institutions necessary to follow the top-down approach of their developed counterparts (Ostrom, 2005). As such, small-scale fisheries in developing nations are often unregulated, or regulations are poorly enforced (FAO, 2008), yet a growing body of empirical and theoretical work has identified conditions under which a "bottom-up" approach to common-pool resources has flourished (Ostrom, 1990; Ostrom et al., 1999; Basurto and Coleman, 2010).

We studied the conditions that foster improved common pool resource management in small-scale fisheries in rural Tanzania. In this setting, a form of village-based institution known as a beach management unit (BMU) plays a key role in certain management tasks, including monitoring fishing catches, endorsing fishing permits, and administering other national policies on illegal gear. BMUs are part of an important trend in small-scale fishery management known as co-management in which some roles and responsibilities for management are devolved from central governments to local communities. As of 2011, an estimated 130 fisheries in 44 countries were co-managed but evidence on the effectiveness of these systems is mixed (Cinner et al., 2012; Gutiérrez et al., 2012).

We designed and implemented an artefactual field experiment with

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fishermen in a randomly selected group of BMUs. Groups of five fishers played a dynamic common-pool resource game in which payoffs evolve during game play as a function of stock size and group harvest, a design consistent with the importance of dynamics, stock effects, and path dependence in social-ecosystem interactions in experiments and the real world (Cardenas et al., 2013). Players used spoons to scoop out beans from a bin which, it is explained to them, represents catching fish in a body of water. They played numerous rounds during which they harvested beans and dumped them in their own personal bucket, which represented their own boat. The participants were informed before they began that they will be financially compensated at the end of the game for the amount of beans they individually harvest. The game was carefully designed to mirror the real world characteristics of the fishers' lives, including on fish stock size, harvest strategies, and opportunities to engage in illegal behavior. In particular, a ban on illegal gear is a formal regulation, which is supposed to be locally enforced by BMUs, but is characterized by imperfect compliance despite fishers having common knowledge about the existence of this regulation, the types of gear that are prohibited, the rationale for the prohibition, and the authority vested in the BMU committee to enforce it (Etiegni et al., 2011; Luomba et al., 2016). In local parlance, illegal gear types are referred to as "haram", i.e., forbidden according to Islamic jurisprudence. It is therefore widely understood that certain types of gear are illegal because they are particularly destructive to fisheries.

By altering features of the game when played with different groups, we generated experimental variation in the possibility of punishment when engaging in "illegal" behavior and studied how this institutional feature affects behavior and fishery-level outcomes. Our results show that when faced with possible punishment from using illegal gear, participants harvest at significantly higher levels than those that do not face possible punishment. Fishing groups in the enforcement treatment were four times more likely to completely deplete and collapse the resource. Importantly, these outcomes are not driven either by higher use of illegal gear (which is never more than ten percent in either treatment) nor by actual punishment (which happens only once in all groups across all villages). Instead, the possibility of enforcement induced individuals to harvest during each round at significantly higher rates than in the comparison group.

We document that significantly more individuals pursue self-interested behaviors in this enforcement treatment. This pattern occurs irrespective of actual cheating/enforcement during the game, and increases with experience across multiple rounds of the game. Individuals in the enforcement game are more likely to adopt harvest strategy profiles that overexploit the resource. Taken together, our experimental results with actual resource users suggest that institutional reforms that target specific behaviors when agents are simultaneously making multiple self-interested or cooperative choices may result in unintended consequences for both the group of resource users and for the resource itself. We interpret these results in reference to the crowding-out literature (see, e.g., the review by Bowles and Polania-Reyes, 2012), which posits that external incentives such as economic incentives or formal enforcement regimes can sometimes displace the intrinsic motivations of individuals to behave in pro-social manners. In our study, we find evidence that the imposition of an enforcement institution in our common-pool resource game relaxes an internal constraint to harvest at a sustainable level, which tends to cause overharvesting and massive resource depletion contrary to the goals of the policy.

The rest of the paper is organized as follows. Section 2 provides additional background on the experimental literature related to common pool resources and small-scale fisheries. Section 3 explains the experimental design and provides a detailed description of the experimental game. Section 4 provides the main analyses, which document the higher resource use and degradation in the game that include an enforcement institution. Section 5 concludes by discussing possible behavioral motivations that drive our results and the policy implications of our results.

2. Experimental literature on small-scale fisheries

The drivers of cooperation or self-interested behavior among fishers are numerous and their relative importance in different settings remains only partially understood because individual variables and social/institutional settings interact in complicated ways (Vollan and Ostrom, 2010; Anderies et al., 2011; Aswani et al., 2013). This paper contributes to the growing literature that uses experimental methods to study behavior and institutions in small-scale fisheries, in particular the roles that cooperation and self-interested behavior play in the successful management of such fisheries.

Of particular relevance to our study is the experimental literature on the impacts of external regulations on individual behavior in small-scale fisheries. Typically, studies of the exogenous imposition of a fixed quota system of harvest combined with some mechanism of enforcement generally conclude that the effectiveness of regulations varies greatly across time, across social and environmental settings, by intensity of enforcement, and by experimental design features, such as whether communication was allowed among subjects. For example, Cardenas et al. (2000) document declining effectiveness of enforcement as subjects realized enforcement was weak and the consequences of noncompliance were tolerable. Others have identified a role for "crowding out" of intrinsic motivations by external regulations (Ostrom, 2000). Whether external factors displace internal ones, or whether regulation "crowds in" intrinsic motivation, varies across individuals as well as environmental characteristics, economic incentives, and other geographical factors (Rode et al., 2013).

Additional sources of variation in the effectiveness of regulation in experiments comes from the intensity of the regulation. Beckenkamp and Ostmann (1999) found non-linear effects of regulatory punishment on overharvest of a common resource: sanctions that were either too lax or too strict failed to achieve regulatory goals, most likely because subjects felt little incentive to change (in the former case) or insufficient respect for what was perceived to be an unfair policy (in the latter). Finally, drawing on a larger experimental literature in other fields (Cardenas et al., 2003), research has also shown that communication serves a critical function in shaping the effectiveness of an experimental regulatory regime (Velez et al., 2010).

Our study contributes to this literature by exploring fishermen behavior under a regulatory structure that is important and widespread in real-world, small-scale fisheries. In particular, we consider the enhanced enforcement of a ban on especially detrimental gear types; this contrasts with the more common focus on harvest-level restrictions such as quotas (e.g., Velez et al., 2010). This focus is valuable because it mimics the rules facing many artisanal fishers, including those in the hundreds of BMUs in Tanzania and neighboring countries around Lake Victoria. Namely, fishermen with common knowledge of allowed versus prohibited gear types/methods make choices over gear type/method and harvest levels. Individual fishing income is a joint function of both of these choices, and both decisions influence outcomes of other local fishermen; thus, collective fishing income is a joint function of these two choices in aggregate. While a regulatory regime to improve fisheries in Tanzania could conceivably impose quotas on fishermen rather than enforce a ban on illegal gear, this is far from the reality of Tanzanian fishermen and well beyond the current capacity of the Tanzanian state.

Fishermen in our study grapple with institutional designs closely related to their actual fishing lives.[1] For example, in one treatment group fishermen are able to choose illegal gear without fear of formal punishment. But because cheating is partially observable (at the group level but not at the individual level), it is possible that other individuals

[1] In our baseline Harvest-only treatment, there is only the strategic issue of common-pool resource management. This is similar to the fisheries context but differs from reality by excluding the key role played by illegal gear use in undermining resource health.

Fig. 1. Players harvesting from the communal bin of beans in the field.

in the group cast suspicion on certain fishers, criticize them publicly, ignore them, or otherwise use tools of informal institutional pressure. In this way, this control group (which we later refer to as the Illegal Gear group) reflects existing informal norms, understandings of acceptable social behavior, and the emotions generated by adherence to or deviation from those norms, which previous studies have shown are important drivers of cooperative behavior (Lopéz et al., 2012). On the other hand, under our main treatment of interest, where external enforcement is bolstered, enforcement of an existing regulation alters the incentives regarding only one of the two choices that fishers face. In particular, harvest levels are unrestricted by formal institutions but illegal gear usage becomes potentially discoverable and punishable by the group. This institutional design mimics the role that local BMUs are supposed to be playing in their fishing communities.

3. Experimental design

For this study, we designed a dynamic common-pool resource game and implemented it in randomly selected villages in two districts of Tanzania.[2] The districts were selected because they represent coastal marine fisheries (Mafia island) and Lake Victoria fisheries (Ukerewe island), which are the two most important fishing areas in the country. We gathered the universe of BMUs within each of these districts from local district fisheries officers and randomly selected ten for inclusion. Within each village, we randomly selected fishermen using a lottery system for all fishermen at the local landing site at the time of our arrival. We then randomly assigned fishermen to one of three versions of the game; more details on these game variants are presented below. Thus, treatment effects are identified by comparing game-based behavior across experimental groups under random assignment.

This common-pool resource game and setting is unique in a number of ways. First, it is truly dynamic in that the payoff functions and strategy spaces evolve each round of the game. This design contrasts with the more common reliance in the literature on repeated one-shot games, and incorporates recent insights from the literature on the importance of stock effects and path dependence in shaping choices (Cardenas et al., 2013; Herr et al., 1997). This dynamism makes the link between the institutions in the game and real-life institutions more direct and realistic.

Second, we implemented the game with real fishermen in rural, relatively poor areas of Tanzania. The fishery management challenges and institutional failures they encounter on a daily basis means we are studying the interaction of behavior and institutions among precisely the stakeholders most affected by those individual and collective choices.

Third, developing countries often do not have the legislative, legal, and enforcement infrastructure that is necessary to support "top-down", centralized fisheries-regulation processes akin to that of developed nations. In an effort to improve fishery resources in developing countries, many governments worldwide are engaging small-scale fishery users in co-management systems, which involve partnerships between government agencies and local fishery users to share responsibility and authority for management of fishery resources. For example, a recent study identified 130 fisheries in 44 countries operating under co-management schemes (Cinner et al., 2012; Gutiérrez et al., 2012). Our experiment generates knowledge about how institutional structure affects fisher behavior in a way potentially applicable to these numerous external contexts.

3.1. Basic game details

The dynamic common-pool resource game was played by groups of five fishers. The participants were presented with a large plastic bin (approximately two-thirds of one meter in diameter) of a quantity of dried beans, where the bin represents the fishery as a common pool resource and the beans in the bin represent fish. The circumference of the bowl could accommodate six to seven people around it, so groups of five had ample room to reach across the edge of the bin (Fig. 1). Each fisher was given a personal plastic cup and a metal spoon, which represents their boat and fishing gear, respectively. The participants were given 30 s to simultaneously scoop the beans out of the big bin and into their own personal buckets. After each round, the amount of beans in each fisher's cup was weighed via digital scale and the amount left in the bin (i.e., the remaining stock) was calculated. In-between each 30-s round, the measured stock of remaining beans was used to calculate the amount the fish stock had grown between those rounds, and this growth was added to the remaining stock of beans before fishing begins in the next round. We assume that growth follows a logistic growth function, which is a common way to measure the evolution of fish stocks over time (e.g., Schaefer, 1957). Moreover, the continually changing stock size creates a rich, complex strategy space, which closely mirrors the real-world fishery dynamics and are a key methodological characteristic of our experimental game.

The participants were allowed to communicate throughout the entire game and played the game simultaneously so there was some general observation of how much the other players were harvesting. The participants were told that there would be between 7 and 15 rounds with the game randomly ending sometime within that interval; this information avoids terminal round effects and incentivizes players to consider stocks and harvests in future rounds. In reality, the game randomly ended sometime between 7 and 10 rounds. For clarity, we refer to one of these 7–10 round sequences of play as a cycle. Each group plays three cycles with the composition of the group staying the same for all cycles. Further, fish stocks were reset to 2 kg at the beginning of each cycle. This feature allows participants to learn from prior mistakes and experiment with new management strategies.

Our experimental game is inspired in part by Knapp and Murphy (2010), who conduct a similar game to examine rent dissipation under alternative management institutions, such as individual quotas or an Olympic-style derby. For their experiment, Knapp and Murphy also use beans and a common bowl on a table where participants emulate fishing by scooping beans out of the bowl with metal kitchen measuring cups; however, in contrast to our experiment, their game is not truly

[2] Note that, in these areas, each village was associated with a single BMU.

dynamic since the stock of beans is reset at the beginning of every round, thereby not considering how stock dynamics influence game play. Further, while Knapp and Murphy consider the effect of gear choice on rent dissipation, they do not consider the option of secretly using illegal fishing gear, as we do.

The basic game instructions and payoffs were explained to the participants in Swahili by Tanzanian enumerators. Subsequently, players participated in two 30-s practice rounds to allow players to get accustomed to harvesting beans from the communal bin. Following Knapp and Murphy (2010), we also use the practice round harvests as a measure of a player's harvesting "skill" in our analysis.[3] Importantly, practice harvests take place before any additional game-variant instructions are provided (discussed below), and thus, our measure of a player's skill is independent of a player's treatment assignment.

Following the practice rounds, fishermen begin playing the three cycles of the game with cash incentives. Fishermen earn money throughout the game via bean harvesting, which is paid at the rate of 2000 Tanzanian Shillings ($1.17 USD) per 1000 g harvested in the games; these average payouts were calculated to be equivalent to average daily earnings of fishermen in that context. The players are otherwise not primed in any way and are not told any goal of the game.

3.2. Game variants and treatments

In addition to the basic game described above, which we will refer to as the "Harvest Only" treatment, we designed two different versions of the game to capture real-world aspects of the local fishery institutional environment (Fig. 2). The additional instructions for these game variants were given to the players immediately after the two practice rounds. In the "Illegal Gear" treatment, players had the opportunity to make a choice to "use illegal gear" by indicating secretly to the enumerator with a small card that shows legal versus illegal types of fishing nets. If they chose to use illegal gear, their payoffs are secretly doubled in the software program used by the enumerator to track individual earnings. This choice matches closely to the daily decision fishermen face: to use illegal fishing gear that boosts individual harvests at the expense of others or adhere to the commonly understood law to only use certain types of fishing gear. Note that given this protocol, the moderator knows the choice made by the fisherman, and the fishermen knows the moderator will know his choice prior to making it, but it is unobserved by the other players in the game.[4]

After each round of the Illegal Gear game is played, the game moderator removes from the common pool resource an amount of beans equal to the additional payoff from the private choice(s) to use illegal gear. At this point the group observes whether someone in the group has chosen to unilaterally deviate from the cooperative choice, although the identity and number of the deviators remains unknown.

The third treatment game that we implement, called the "Enforcement" treatment, adds one key institutional feature to the Illegal Gear version of the game—the introduction of random "patrols" that can catch fishermen using illegal gear and the opportunity for the group to punish anyone either caught, or who they believe, has used illegal gear. At the end of each round, a random number generator determines whether a patrol will occur that round; the probability of a patrol each round is calibrated to be 0.50. If a patrol occurs, it reveals the fishing gear choice of one of the five fishermen, chosen at random. Thus, at the beginning of the round there is a 10% probability that any fisherman will have their choice revealed to the group. If a player is caught using illegal gear, the group decides whether to forgive the

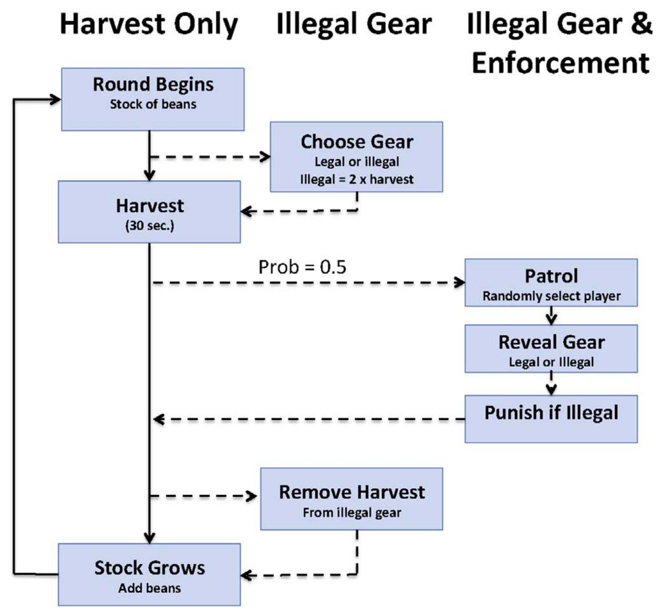

Fig. 2. Depiction of the different components associated with each treatment. Note that boxes under the Illegal Gear and Enforcement treatments are additions to the baseline Harvest Only treatment.

player and set no penalty, to fine the player 500 g of their harvest, or to make them sit out for the next round. The choice of whether and how enforcement occurs is left to deliberation and consensus, as is the norm in these communities.

To summarize, the three treatments can be characterized by the following individual expected payoff function for a given round:

$$Payoff = \begin{cases} x & \text{if treatment } = \text{ Harvest Only} \\ [1+z]\times x & \text{if treatment } = \text{ Illegal Gear} \\ [1+z]\times x - 0.1\times P\times z & \text{if treatment } = \text{ Enforcement} \end{cases}$$

where x represents the value of harvest and z is an indicator variable that takes the value of one if illegal gear is used and zero otherwise, and P is an expected penalty if caught cheating. Thus, players can double their round-specific payoffs in the Illegal Gear and Enforcement treatments by using illegal gear; however, this must be weighed against the expected cost of getting caught in the Enforcement treatment.

3.3. Expected outcomes and hypotheses

Maximizing group returns to the game requires players to solve the dynamic commons problem associated with the stock of beans. Given the resource dynamics and initial stock at the beginning of the game, group earnings are maximized by foregoing harvests in the first few rounds so that the stock of beans can grow to a greater size, thereby permitting larger and sustained harvests in the later rounds.[5] However, an individual player has an incentive to deviate from the optimal group strategy by harvesting in the earlier rounds. Specifically, a player receives the full benefit of unilaterally increasing his own harvest, but only incurs a portion of the associated cost of foregone future harvests because this cost is borne by all players in the game (Gordon, 1954; Hardin, 1968). Thus, solving the commons problem requires the group to self-organize and form rules to internalize this externality (Ostrom et al., 1999).

Players are allowed to communicate to one another and can observe

[3] Participants are paid for their practice round harvests to ensure that participants take the practice rounds seriously.

[4] This characteristic of the game protocol might conceivably affect fishers' choices regarding illegal gear use despite our clear instructions that the game and associated survey were for research purposes alone. The low rates of observed illegal gear use in the game should be interpreted in light of this feature.

[5] Given the uncertainty regarding the end of the game, a group's payoff-maximizing strategy for the first seven rounds is approximately equal to the payoff-maximizing strategy for the infinite-horizon game, which is simply to forgo harvests and let the stock grow until maximum sustainable yield can be harvested in perpetuity.

Table 1
Number of unique players and survey takers, by BMU and treatment.

BMU	District	Played Game			Took Survey		
		Harvest Only	Illegal Gear	Enfor-cement	Harvest Only	Illegal Gear	Enfor-cement
1	Ukewere	5	5	5	1	2	1
2	Ukewere	5	5	5	5	5	5
3	Ukewere	5	5	5	5	5	5
4	Ukewere	5	5	5	5	5	5
5	Ukewere	4	4	4	4	4	4
6	Mafia	5	5	5	5	5	5
7	Mafia	5	4	5	5	4	5
8	Mafia	5	5	5	5	5	5
9	Mafia	5	5	5	5	5	5
10	Mafia	0	5	4	0	5	4
Subtotal		44	48	48	40	45	44
Total		140			129		

the actions of the other players in all treatment groups. The Harvest-only treatment is a relatively conducive environment for the group to self-organize and solve the commons problem. In contrast, the Illegal Gear treatment provides players with an additional action whereby a confidential gear choice is made before each round, with illegal gear doubling a player's harvest during the round. With this additional option to unilaterally and confidentially deviate from the group cooperative strategy, we would expect cooperation to be less likely to occur in the Illegal Gear treatment. In turn, group outcomes in the Harvest Only treatment should be relatively closer to those emerging from an optimal cooperative group strategy. In particular, we would expect group earnings and the final stock of beans to be higher, on average, under the Harvest Only treatment, and expect the number of times the stock is exhausted to be higher under the Illegal Gear treatment.

The Enforcement treatment introduces a stochastic enforcement mechanism for punishing the use of illegal gear. In theory, we would expect this enforcement mechanism to create an environment that is more conducive to self-organization relative to the Illegal Gear treatment since the threat of punishment increases the individual cost of deviating from the group cooperative strategy. We would therefore expect group outcomes in the Enforcement treatment to lie in-between the Harvest-only and Illegal Gear treatments in terms of its deviation from the cooperative group strategy. In particular, we would expect, on average, group earnings and the final stock of beans to be higher under the Enforcement treatment, and the number of times the stock is exhausted and the extent of illegal gear use to be higher under the Illegal Gear treatment.

We use several outcome variables from the game to test the above hypotheses. Our outcomes of interest can be categorized into three groups: harvest rates, resource sustainability, and illegal gear choice. Specifically, we test for differences across treatments for the following average outcomes. To study harvest rates, we primarily use total group and individual earnings, which measure group and individual performance, although first-round harvests and remaining third-round stock, which measure a group's success in delaying early-round harvests, also provide valuable information on harvest rates. But success in delaying early-round harvests is also closely related to resource sustainability, as are the outcomes of number of rounds per game and the probability of exhausting the resource, which measure the extent to which harvesting practices are sustainable. Finally, we study difference across treatments in the proportion of illegal gear usage across gear choices, rounds, and players, which measure the degree of illegal harvesting practices. These illegal gear outcomes provide useful information on the mechanisms by which fishermen in the different treatment arms sustain or degrade the resource.

Table 2
Average baseline characteristics and their balance across treatments.

Individual Characteristics	Treatment			p-value*
	Harvest Only	Illegal Gear	Enforcement	
Age (yrs)	37.7	38.8	42.1	0.27
Education (yrs)	8.23	7.19	7.55	0.25
Married (1 = Y,0 = N)	0.92	0.90	0.88	0.82
Captain (1 = Y,0 = N)	0.21	0.33	0.31	0.48
Weekly Income (1000 TZS)	32.7	63.9	60.4	0.36
Practice Harvests (grams)	221	196	215	0.15

*p-value for the F-statistic testing the joint hypotheses of no difference between means.

4. Analysis

This section presents our empirical analysis of the hypotheses above. We begin by providing evidence on the internal validity of the experiment, namely that our randomization procedure ensured statistical balance across treatment groups. We then turn to descriptive evidence on our main hypotheses in Section 4.2. The key result of this paper—higher rates of resource degradation and collapse in the enforcement treatment—can be seen even in this limited setting. Our analysis then turns to two econometric frameworks in an effort to identify the effect of different institutional regimes on behavior while controlling for potentially important confounding factors. These are our preferred results and they are discussed and presented in Section 4.3 below. In particular, columns (3)–(6) of Table 4 contain the central results of our paper. Finally, Section 4.4 analyzes the available evidence on what behavioral mechanisms might explain the main counter-intuitive result from Section 4.3.

4.1. Sample size and balance across treatments

Table 1 presents the number of unique participants in each BMU and treatment. In total, 140 unique individuals from ten BMUs (five from Ukewere and five from Mafia) participated in the experimental game.[6] In addition to playing the experimental game, individuals participated in a survey that collected demographic information, such as age, education, marital status, income, and whether they captained

[6] Note that there were not always enough fishers at a landing site to populate each treatment arm with five players. Thus, in some instances, the game was only played with four players (e.g., BMU 5), whereas in other BMUs, not all treatment arms were played (e.g., the Harvest Only game was not played at all in BMU 10). Further, due to various reasons, not all individuals were able to participate in all three cycles. For example, Cycle 3 for the Harvest Only game was not played in BMU 5. We address this issue by controlling for the number of players in a group in our econometric analysis below.

Table 3
Descriptive statistics of game outcomes, by treatment and cycle.

Outcome Variables	Harvest-only			Illegal Gear			Enforcement		
	1	2	3	1	2	3	1	2	3
Average group earnings (grams)	2253.8	2421.6	2757.1	2026.7	2356.3	2693.0	2519.2	2800.0	2998.5
Average individual earnings (grams)	487.3	496.7	567.6	424.2	493.2	563.7	527.3**	586.1	627.6
Average first-round group harvest (grams)	1007.9	635.9	404.9	1062.6	683.2	552.1	1055.1	987.9	1048.0**
Average remaining stock after 3rd round (grams)	605.3	1612.6	1977.9	775.5	1468.6	1642.0	176.8	472.2*	554.2**
Average length of game (rounds)	6.125	6.250	8.571	5.444	7.000	8.222	4.667	4.333**	4.889**
Proportion of groups depleted beans before 7th round	0.500	0.375	0.000	0.556	0.222	0.111	0.889	0.778**	0.667**
Proportion of gear choices with illegal gear				0.098	0.077	0.029	0.087	0.027**	0.036
Proportion of gear choices with illegal gear in first round				0.116	0.093	0.023	0.098	0.047	0.140**
Proportion of players that used illegal gear at least once				0.140	0.116	0.070	0.233	0.093	0.140
Proportion of gear choices with illegal gear for those who cheat				0.632	0.778	0.375	0.395*	0.369	0.492

Note: reported statistical tests exclude the Harvest Only treatment to focus on the relationship between the Enforcement and Illegal Gear groups.
*p-value < 0.1; ** p-value < 0.05; *** p-value < 0.01 for testing the hypothesis that Illegal gear treatment = Enforcement treatment.

their own boat (Table 2). Note that there were some instances in which a fisher participated in the experimental game but did not take or complete a full survey. In particular, only a couple of players from BMU 1 were able to take the survey, which prohibits us from using BMU 1 to estimate the relationship between demographic variables and game outcomes in our econometric analysis below. Accordingly, we drop BMU 1 from our analysis and use only the remaining players that played the game and completed a survey, leaving us with 125 participants from 26 groups.[7]

The p-values presented in Table 2 indicate that we cannot reject the joint hypotheses (at 95% confidence) that there are no differences in means across treatments. However, two-sample Wilcoxon rank-sum tests reject the following null hypotheses (at 95% confidence) that the distributions are the same: practice harvests (Harvest Only vs. Illegal Gear, p-value = 0.04), and weekly income (Illegal Gear vs. Enforcement, p-value = 0.03). We address these differences between groups by adding baseline characteristics as controls in our analysis below.

4.2. Descriptive analysis

Table 3 presents averages for the outcome variables from the game. As discussed above, our outcomes relate to harvest rates (both group and individual harvests), resource sustainability (e.g. stock depletion in a given round and overall resource exhaustion), and illegal gear usage (to study the mechanisms by which other outcomes are achieved). In general, the average group and individual earnings indicate that a considerable amount of learning took place over cycles in each of the treatments. For example, earnings increased by approximately 35 percent between cycles 1 and 3 in the Illegal Gear treatment. Much of the learning that took place was with respect to controlling harvests in the opening rounds. In the first cycle, for instance, average first-round harvests were approximately half of the initial stock of beans in all treatments, resulting in negligible growth and lower stocks in subsequent rounds. Accordingly, the average stock of beans was well below its initial value after three rounds and most groups exhausted the resource before the seventh round, resulting in games ending well below they were expected to.[8] In all treatments, the average remaining stock after three rounds and the proportion of groups that exhausted the resource improved over cycles. Despite these improvements, however, average earnings were well below those that could have been earned by following the optimal group strategy in which the resource was left to grow in the opening rounds, thereby permitting larger harvests in later rounds.[9]

Overall, there is little discernible difference in earnings across treatments at the group or individual level for a given cycle (Table 3). While the average earnings were slightly larger under the Enforcement treatment, the differences in average earnings between experimental groups are statistically indistinguishable in cycles 1 and 2, though they are significantly higher in cycle 3 in the enforcement game. The similar earnings across treatments, however, does not imply that the groups followed similar harvesting strategies. For cycles 2 and 3, groups in the Harvest Only and Illegal Gear treatments appear to follow a relatively more sustainable harvesting strategy, with lower harvests in the initial rounds, higher stock levels in the later rounds, and lower incidences of stock exhaustion (Fig. 3 and Table 3). In contrast, groups in the Enforcement treatment tended to follow a relatively more exploitative harvesting strategy, with higher harvests in the initial rounds, lower stock levels in later rounds, and higher incidences of resource exhaustion. Although groups in the Harvest Only and Illegal Gear treatments followed more sustainable harvesting strategies that are closer to the optimal cooperative group strategy, groups did not take advantage of the higher stocks in the later rounds, resulting in group earnings that are similar to those generated by the less sustainable strategies under the Enforcement treatment.

In total, there was very little illegal gear used in the Illegal Gear and Enforcement treatments. For instance, although over 20% of players in the Enforcement treatment used illegal gear at least once, this amounted to only 9% of all gear choices in the first cycle. There is some evidence that illegal gear usage was slightly higher in the Illegal Gear treatment; however, this is only the case in the first two cycles. Indeed, the proportion of gear choices with illegal gear decreases more over cycles in the Illegal Gear treatment relative to the Enforcement treatment, such that there is no discernible difference between the two treatments by the third cycle. The differential trend between cycles is primarily driven by the reduction in illegal gear use by just a few players that cheat in the Illegal Gear treatment. As shown in Table 3, the reduction in illegal gear usage in the Illegal Gear treatment is driven by both a reduction in the proportion of players that cheat and the frequency in which illegal gear is used by the cheaters. In contrast, the frequency in which illegal gear is used by the cheaters in the Enforcement treatment stays relatively constant, and even increases in cycle 3.

[7] Removing BMU 1 from the analysis has little effect on our analysis below.

[8] Note that there are only a few instances in which the stock of beans were completely exhausted. In the majority of cases that we consider to be "exhaustion events," a small amount of beans still remained in the communal bin, but the group decided that too few beans remained to justify harvest in subsequent rounds. In such cases, the stock of beans were treated "as if" they were exhausted.

[9] For example, under the optimal group strategy, a group could have harvested approximately 5000 g after 8 rounds (average number of rounds is 8.5).

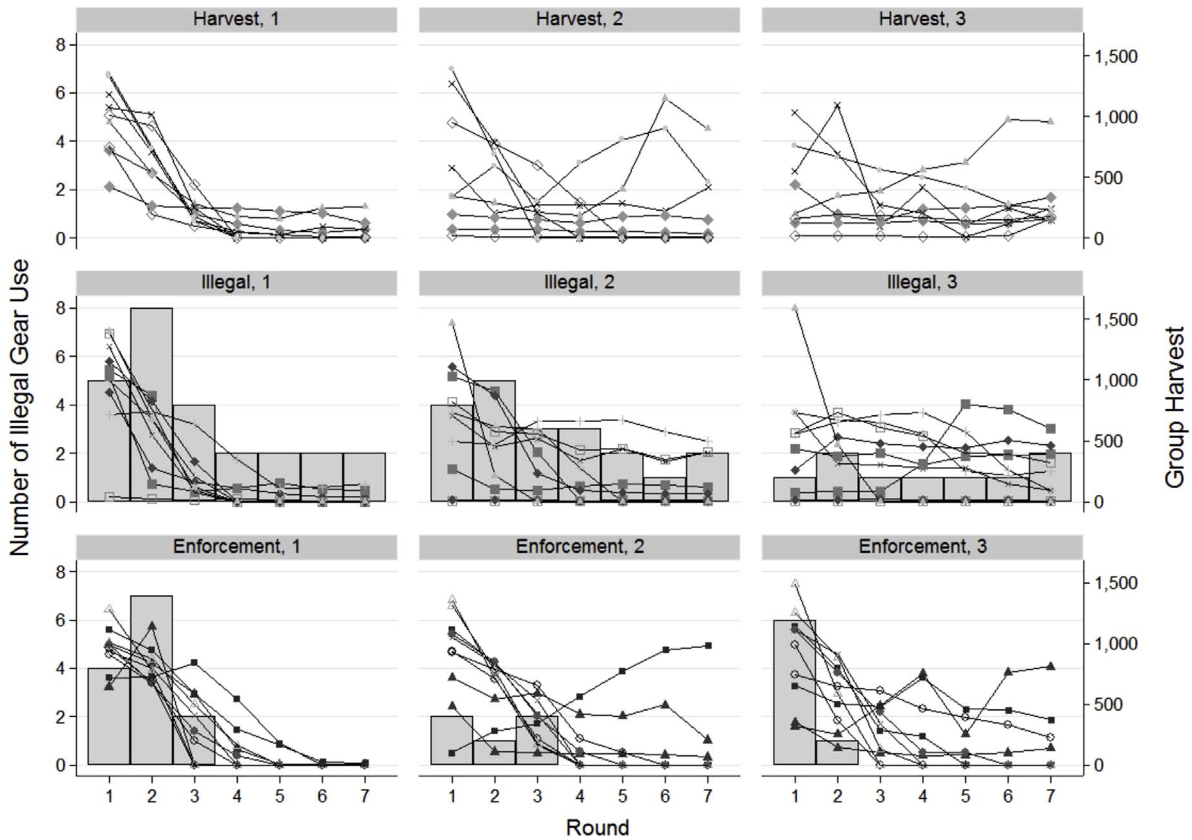

Fig. 3. Group harvests (grams) and illegal gear uses by round, cycle, and treatment arm.

Of the 25 instances in which a player used illegal gear in the Enforcement treatment, only two players were caught, and in each instance, the remaining group members did not punish the cheating player. Looking at Fig. 3, it is not surprising that only two players were caught using illegal gear in the Enforcement treatment—cheaters often used illegal gear at the same time or in the same round. Fig. 3 also shows that illegal gear use is concentrated in the early rounds for the Enforcement treatment, but not so much the Illegal Gear treatment; however, this is largely because the stock was exhausted after a few rounds for many of the groups in the Enforcement treatment.

Overall, comparing the descriptive statistics across treatments provides little evidence to support our initial hypotheses. In fact, Fig. 3 and Table 3 suggest that groups in the Enforcement treatment followed a more exploitative harvesting strategy relative to the Illegal Gear treatment, which is opposite to what we expect. Further, this exploitative behavior takes place without any discernible difference in illegal gear use. This evidence suggests a key mechanism through which the Enforcement institution results in fishing behavior that is destructive to the common-pool resource, which we explore further below.

4.3. Econometric analysis

The descriptive statistics presented in Table 3 provide little evidence to support our initial predictions in Section 0, and suggest that the Enforcement treatment may have had the opposite effect of inducing more exploitative behavior relative to the Illegal Gear treatment. However, the descriptive statistics do not control for any differences between groups that might influence our outcomes. Despite individuals being randomly assigned to treatments in our experiment, Table 2 demonstrates that differences in average baseline characteristics still exist between experimental groups—particularly in practice round harvests, which are a proxy for harvesting skill. In this section, we further explore the game outcomes presented in Table 3 using econometric methods that control for observed and unobserved factors that may influence our earlier results.

We estimate multilevel mixed linear models of the form:

$$y_{gc} = \lambda_c + \sum_{t=1}^{3} \pi_t treat_{tg} + \beta' \mathbf{x}_g + \alpha_{bmu} + \theta_g + \varepsilon_{gc} \quad (1)$$

where y_{gc} is the outcome of interest for group (or individual) g in cycle c; $treat_{tg}$ is an indicator variable that takes the value of one if group g received treatment t, and zero otherwise; $\mathbf{x}_g$ is a cycle-invariant vector of average group characteristics;[10] α_{bmu} and θ_g are mean-zero normally distributed BMU and group random effects, respectively; and ε_{gc} is a mean-zero normally distributed residual. As specified, the outcome variable is a linear function of a "cycle effect" (λ_c), which captures unobserved factors that are specific to a cycle that are group-invariant, and a "treatment effect" (π_t), which captures the average difference in the outcome variable of interest across treatments, all else equal. The estimated treatment effects are the primary parameters of interest for our analysis.

Our main results can be seen in Table 4, which presents the estimated parameters from equation (1) and generally supports the results from the descriptive analysis presented in Table 3.[11] Columns (1) and (2) reinforce our earlier conclusion that there is no discernible difference in total earnings between treatments. It is important to note the positive influence that practice harvests—i.e., harvesting skill—has on individual earnings, which is consistent with the findings of Knapp and Murphy (2010); however, there is no such relationship between average practice harvests and group earnings. As discussed in more

[10] Note that when groups are the unit of observation, $\mathbf{x}_g$ is a vector of mean characteristics for the group; in contrast, when individuals are the unit of observation, $\mathbf{x}_g$ is a vector of individual characteristics.

[11] Note that columns (1) through (6) in Table 4 use the Harvest-only treatment as the baseline, whereas columns (7) through (9) use the Illegal Gear treatment as the baseline. In addition, all columns use Cycle 2 as baseline.

Table 4
Estimated parameters for game outcomes in equation (1).

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Group earnings[a]	Individual earnings[a]	First-round group harvest[a]	Remaining stock after 3rd round[a]	Length of game (rounds)	Depleted beans before 7th round	Gear choice had illegal gear	Gear choice had illegal gear (1st Round)	Players used illegal gear at least once
Illegal Gear Treatment	−0.268 (0.384)	−0.013 (0.240)	0.520 (0.360)	−0.217†† (0.380)	−0.128 (1.209)	0.109 (0.205)			
Enforcement Treatment	0.226 (0.364)	0.103 (0.239)	0.597* (0.335)	−0.842**†† (0.358)	−1.478 (1.147)	0.426**† (0.196)	0.015 (0.036)	0.051 (0.040)	0.078* (0.042)
Cycle 2	0.227 (0.175)	0.151 (0.094)	−0.579*** (0.167)	0.588*** (0.142)	0.462 (0.500)	−0.154* (0.093)	−0.032** (0.015)	−0.040 (0.042)	−0.085* (0.049)
Cycle 3	0.497*** (0.177)	0.343*** (0.095)	−0.767*** (0.170)	0.752*** (0.144)	1.756*** (0.506)	−0.337*** (0.094)	−0.056*** (0.015)	−0.027 (0.042)	−0.085* (0.049)
Age[a]	−0.205 (0.236)	−0.074 (0.054)	0.322 (0.220)	−0.033 (0.232)	−1.072 (0.741)	0.087 (0.126)	−0.021** (0.009)	−0.044* (0.023)	−0.022 (0.027)
Education[a]	−0.354* (0.193)	−0.061 (0.049)	0.259 (0.189)	0.027 (0.194)	−0.233 (0.605)	0.071 (0.101)	−0.055*** (0.007)	−0.060*** (0.019)	−0.047** (0.022)
Married[a]	0.102 (0.230)	−0.070 (0.051)	−0.451** (0.211)	0.216 (0.226)	1.046 (0.724)	−0.087 (0.124)	−0.048*** (0.007)	−0.051** (0.021)	−0.039 (0.025)
Captain[a]	0.337* (0.183)	−0.029 (0.046)	−0.134 (0.178)	−0.041 (0.183)	0.281 (0.572)	−0.054 (0.096)	0.028*** (0.007)	0.036* (0.019)	0.053** (0.023)
Weekly Income[a]	−0.397** (0.175)	−0.040 (0.044)	0.161 (0.167)	0.073 (0.174)	−0.547 (0.548)	0.058 (0.093)	−0.003 (0.007)	0.012 (0.018)	0.012 (0.022)
Number of Players[a]	−0.259 (0.168)	−0.235** (0.093)	0.182 (0.169)	−0.012 (0.170)	−0.187 (0.526)	0.061 (0.088)	−0.001 (0.017)	−0.018 (0.020)	−0.033 (0.029)
Practice Harvests[a]	0.068 (0.206)	0.312*** (0.055)	0.571*** (0.195)	−0.341* (0.204)	−0.686 (0.648)	0.152 (0.110)	−0.024*** (0.009)	−0.032 (0.019)	−0.033 (0.024)
Ln(Var(θ))	−2.001	−3.308	−1.297	−1.649	−0.991	−15.86	−15.025**	−25.788	−2.364***
Ln(Var(α))	−0.794***	−0.817***	−0.956***	−0.732***	0.410	−1.392***	−2.651***	−3.311***	−14.848***
Average Rounds	–	–	–	–	–	–	5.5	–	–
Number of Cycles	3	3	3	3	3	3	3	3	3
Number of Groups	26	26	26	26	26	26	18	18	18
Number of Individuals	–	125	–	–	–	–	86	86	86
Number of Observations	77	342	77	77	77	77	1220	246	246

Notes: Cluster-robust standard errors in parentheses (clustered by group).
* p-value < 0.1; ** p-value < 0.05; *** p-value < 0.01 for testing hypothesis that the coefficient is equal to zero. † p-value < 0.1.
†† p-value < 0.05.
††† p-value < 0.01 for testing the hypothesis that Illegal gear treatment = Enforcement treatment.
[a] Indicates variable is normalized to mean zero and standard deviation of one.

detail below, large practice harvests is a strong predictor for more aggressive harvesting behavior, particularly in the opening rounds. Groups that have higher average practice harvests therefore tend to have lower stocks in future rounds, and in turn, do not perform better than groups with lower average practice rounds. In contrast, players that have higher "skill" relative to the rest of the group provides an opportunity to free ride off the conservation benefits from other players' actions, resulting in higher individual earnings for such players. Also worth noting is the strong negative relationship that the number of players in a group has on group and individual earnings, as would be expected with increased competition for common-pool resource.

Consistent with our earlier findings, columns (3) to (6) suggest that groups in the Enforcement treatment followed more exploitative harvesting strategies relative to the Harvest Only treatment, in terms of higher first-round harvest, lower third-round stocks, and likelihood of exhausting the resource. Once we control for group-specific factors related to harvesting practices, the Enforcement and Illegal Gear treatments are statistically different from one another in the remaining stock after three rounds (column 4) and the likelihood of depleting the resource (column 6). Of particular importance is the approximately 32% increased likelihood of exhausting the resource for groups in the Enforcement group (relative to Harvest Only) as compared to those in the Illegal Gear group (relative to Harvest Only).

While these results are broadly consistent with our descriptive analysis, there no longer appears to be a difference between Enforcement and Illegal Gear with respect to first-round harvests. The primary reason for this slight departure from our descriptive analysis is that we are conditioning on practice harvests, which is both a strong predictor of unsustainable harvesting practices and under represented in the Illegal Gear treatment (Table 3). Thus, our econometric results suggest that if groups in the Illegal Gear and Enforcement treatments had the same harvesting skill, as measured by practice harvests, they would both have similar first-round harvests, on average. It is also worth noting the strong influence that cycle fixed effects have on the outcome variables in columns (3) to (6), particularly cycle 3, indicating the general gravitation of all treatments towards more sustainable harvesting practices.

The estimated treatment effects presented in columns (7) through (9) suggest that groups in the Enforcement treatment were slightly more inclined to use illegal gear relative to the Illegal Gear treatment, in terms of the number of players using illegal gear. However, there is no discernible difference between the two treatments in the proportion of gear choice devoted to illegal gear. Perhaps surprisingly, there is a noticeably strong negative influence of education and positive influence of being a captain on the use of illegal gear, suggesting that perceptions of and experience using illegal gear in the real-world influence that way that participants use illegal gear within the game.[12] That being said, these results should be interpreted with caution given the relatively few occurrences of illegal gear usage within the experiment.

4.4. Exploring mechanisms

Our analysis thus far has focused on aggregate outcomes at either the group or individual level. While such an analysis is useful for identifying whether differences exist between treatments in overall game performance, it does not explain how such differences came to be. For example, is the more exploitative harvesting by Enforcement groups due to a differential response to cheating by other players in previous rounds, or is it simply persistent behavior? To explore these mechanisms, we now analyze the drivers of illegal gear use and harvests in response to game-specific state variables, such as beginning stock levels and previous round (or game) cheating and harvest rates. Specifically, we ask: if individuals face the same stock levels and witness the same harvests (legal and illegal) in the previous round (or game), are they (a) more likely to cheat or harvest more aggressively in the prevailing round, and (b) does this differ across treatment arms?

We estimate multilevel mixed generalized linear models of the form:

$$g\{E(y_{igcr})\} = \lambda_c + \delta_r + (\pi + \gamma'\mathbf{z}_g) \times treat_i + \phi'\mathbf{z}_g + \beta'\mathbf{x}_i \quad y_{igcr} \sim F \\ + \alpha_{BMU} + \theta_g + \mu_i, \tag{2}$$

where $E(y_{igcr})$ is the expected value of a game outcome, such as harvests or illegal gear use, for individual i in group g, cycle c, and round r; $treat_i$ is an indicator variable that takes the value of one if individual i is in the Enforcement treatment, and zero otherwise; $\mathbf{x}_i$ is a cycle-invariant vector of individual characteristics; $\mathbf{z}_g$ is a vector of lagged group outcomes from the game, i.e., state variables, either from the previous round or the previous cycle; and α_{bmu}, θ_g, and μ_i are mean-zero normally distributed BMU, group, and individual random effects, respectively. Of particular interest to our analysis is the parameter π, which is the effect on y of the Enforcement treatment relative to the Illegal gear treatment when all state variables are evaluated at their means. In addition, the parameter vector ϕ is the effect of the state variables on y in the Illegal Gear treatment, while the parameter vector γ is the differential response to the state variables in the Enforcement treatment. These two parameter vectors measure whether individual players respond to the evolving game-specific state variables and whether this response differs between treatments. We use the logit function for g and the binomial distribution for F when using the binary variable illegal gear choice as our outcome variable, and the identity function and the normal distribution, respectively, when using normalized harvests as our outcome variable.

We begin by looking specifically at harvests and illegal gear use in the first round since all groups are starting with the same stock level and are free of any cycle-specific state dependence.[13] In addition, Fig. 3 shows that the majority of illegal gear use occurs within the first round of the cycle. Accordingly, we model harvests and illegal gear use to be a function of group outcomes from the previous cycle, such as total harvests (legal and illegal) and whether the resource was exhausted.[14] We also include an indicator variable for whether an individual is using illegal gear in our model for first-round harvests to investigate whether individuals harvest differently when they are concurrently using illegal gear. Column (1) of Table 5 displays the estimated parameters from equation (2) for illegal gear use, indicating that there is no detectable relationship between illegal gear use and previous-cycle outcomes. Further, when the state variables are evaluated at their means, there is no discernible difference in illegal gear use between the Enforcement and Illegal Gear treatments.

Individual harvests, in contrast, tend to be more responsive to outcomes from previous cycles. For instance, individuals under both treatments tend to harvest more in the first round when group harvests are larger in the previous cycle. Further, individuals in the Enforcement treatment tend to harvest relatively more when illegal harvest from the previous cycle is high and relatively less when the resource has been previously exhausted. Interestingly, there is no indication that individuals harvest differently when they are concurrently using illegal gear. Overall, the results in column (2) suggest that first-round harvests are generally larger in the Enforcement treatment when the state variables are at their means.

We now investigate harvests and illegal gear use in subsequent rounds, which we model to be a function of group outcomes from the previous round, such as the stock level, total harvests, and whether any

[12] Note that there is very little correlation between captaining a boat and education levels.

[13] Note that we do not include round fixed effects in equation when estimating illegal gear use and harvests for the first round only.

[14] Note that by considering first-round harvests and illegal gear use as a function of previous cycle outcomes, we are unable to consider first-round harvests in Cycle 1 as part of our estimation.

Table 5
Estimated parameters for first-round illegal gear use and harvests in equation (2).

Variables	(1)	(2)
	First-Round Illegal Gear Use	First-Round Harvest[a]
Enforcement Treatment	−0.311 (1.757)	0.963***[a] (0.356)
Cycle 3	0.189[a] (0.743)	0.227*** (0.081)
Age[a]	−0.072 (0.449)	0.039 (0.073)
Education[a]	−0.206 (0.345)	0.021 (0.059)
Married[a]	−2.447** (1.139)	−0.143 (0.205)
Captain	1.966** (0.906)	−0.269** (0.128)
Weekly Income[a]	−0.187 (0.424)	−0.033 (0.056)
Number of Players[a]	0.963 (1.371)	−0.403 (0.372)
Practice Harvests[a]	−0.021 (0.457)	0.002*** (0.001)
Last cycle group Illegal Harvests[a]	1.306 (0.846)	−0.085 (0.135)
Last cycle group Harvests[a]	−0.565 (0.797)	0.350*** (0.115)
Last cycle Exhausted Resource	−1.240 (1.313)	1.366*** (0.201)
Using Illegal Gear		0.120 (0.271)
Last cycle group Illegal Harvests[a] × Enforcement treatment	−1.532 (1.077)	0.431*** (0.166)
Last cycle group Harvests[a] × Enforcement treatment	−2.041 (2.711)	−0.055 (0.229)
Last cycle Exhausted Resource × Enforcement treatment	2.339 (1.960)	−1.288*** (0.295)
Using Illegal Gear × Enforcement treatment		−0.373 (0.348)
Var(α)	0.000	0.000
Var(θ)	0.000	0.356**
Var(μ)	0.000	0.108***
Number of Cycles	2	2
Number of Groups	18	18
Number of Individuals	82	82
Total Number of Observations	164	164

Notes: Cluster-robust standard errors in parentheses (clustered by group).
* p-value < 0.1; ** p-value < 0.05; *** p-value < 0.01.
[a] Indicates variable is normalized to mean zero and standard deviation of one.

Table 6
Estimated parameters for non-first-round illegal gear use and harvests in equation (2).

Variables	(1)	(2)
	Used Illegal Gear Non-First Round	Legal Harvest Non-First Round[a]
Enforcement Treatment	−2.106 (2.004)	0.636*** (0.136)
Cycle 2	−1.333** (0.601)	0.008 (0.040)
Cycle 3	−3.804*** (0.874)	0.026 (0.039)
Age[a]	−0.688 (0.609)	−0.027 (0.048)
Education[a]	−1.219** (0.546)	0.003 (0.041)
Married[a]	−2.068 (1.636)	−0.152 (0.142)
Captain	2.450* (1.350)	−0.090 (0.088)
Weekly Income[a]	−0.534 (0.909)	−0.021 (0.041)
Number of Players[a]	0.512 (1.666)	−0.310** (0.157)
Practice Harvests[a]	0.003 (0.006)	0.001*** (0.000)
Stock Level[a]	0.196 (0.568)	0.126*** (0.025)
Last round group Harvests[a]	−0.423 (0.498)	0.238*** (0.031)
Last round Cheaters	−0.789 (1.378)	−0.168** (0.082)
Using Illegal Gear		−0.253** (0.117)
Stock Level[a] × Enforcement treatment	−1.816 (2.201)	0.710*** (0.067)
Last round group Harvests[a] × Enforcement treatment	1.662** (0.802)	−0.000 (0.047)
Last round Cheaters × Enforcement treatment	−0.150 (1.674)	−0.039 (0.130)
Using Illegal Gear × Enforcement treatment		0.895*** (0.189)
Var(α)	6.058	0.000
Var(θ)	0.000	0.045*
Var(μ)	5.314**	0.077***
Average Number of Rounds	4.03	4.03
Number of Cycles	3	3
Number of Groups	18	18
Number of Individuals	82	82
Total Number of Observations	1150	1150

Notes: Cluster-robust standard errors in parentheses (clustered by group).
* p-value < 0.1; ** p-value < 0.05; *** p-value < 0.01.
[a] Indicates variable is standardized to zero mean with standard deviation of one.

of the other players used illegal gear in the previous round. As before, we include an indicator variable for whether an individual is using illegal gear in our model for harvests. Column (1) of Table 6 supports our earlier findings that captains are more likely to use illegal gear on average, while the opposite is true for those individuals with higher education. In addition, there is some evidence that individuals in the Enforcement treatment are more likely to use illegal gear after large harvests in the previous round. This result is likely driven by the fact that the majority of non-first round illegal gear use in the Enforcement arm took place in the second round of cycle 1 after large first-round harvests (Fig. 3), and indeed, this result disappears if cycle 1 observations are removed from the estimation. In general, while the estimation results suggest that there is little difference in the patterns of illegal gear use between the two treatments, the relatively small amount of illegal gear use—particularly after the first round—provides little substantive evidence upon which to base any definitive conclusions.

Individual harvests, in contrast, are highly responsive to game-specific state variables, increasing with larger stock levels and with the last round's harvest, while decreasing with the existence of illegal gear use in the previous round. Of particular interest is the positive and significant differential response to stock levels by individuals in the Enforcement treatment, indicating that individual harvests are relatively higher in the Enforcement treatment for any given stock level, not just in the first round when stock levels are the same across all treatments. Also of interest is the fact that illegal gear has a negative influence on concurrent harvests in the Illegal Gear treatment and a positive influence in the Enforcement treatment, suggesting two considerably different harvesting strategies across treatments. However, this should be interpreted with care as the result is mainly driven by the large second-round harvests and illegal gear use by individuals in the Enforcement treatment in cycle 1, and disappears when cycle 1's observations are removed from the estimation.

Overall, the results in Tables 5 and 6 do not identify any treatment as being more prone to illegal gear use or any particular drivers of illegal gear use across treatments, which is perhaps expected given the relatively small number of observed occurrences of illegal gear use. In addition, it is not clear whether estimated differential responses to game-specific state variables are actually indicative of differences in state-dependent behavior or whether they result from persistent harvesting behavior that is correlated across cycles and/or rounds (Heckman, 1981). For example, the positive and significant coefficient on last cycle group harvest in Table 5 could arise simply because individuals always harvest large amounts regardless of the outcomes from previous cycles. Nonetheless, Tables 5 and 6 show that when we compare individuals across treatments within the same round and at the same stock level, individuals in the Enforcement treatment harvest at higher levels than those in the Illegal Gear treatment on average.

5. Discussion and conclusion

The results of our common-pool resource game suggest that when an enforcement mechanism is added, players on average display more exploitative harvesting behavior. This is an unintended consequence of the enforcement mechanism that was not predicted. The level of illegal gear usage was low across both treatments and the treatment effect of Enforcement on illegal gear was generally not significant. The most interesting result is that the enforcement game causes players to exploit the resource at a considerably higher rate. Our experiment cannot answer definitely what behavioral mechanisms underlie these outcomes, but the results, while surprising, are consistent with certain strands of the existing experimental literature on institutions. For example, prior studies have found that individuals often do not make decisions based off of purely self-interested motives (Fehr and Schmidt, 2000; Sobel, 2000). Rather, a variety of social factors including social preferences (Fehr and Fischbacher, 2002), reciprocity (Falk and Fischbacher, 2006), inequity aversion (Fehr and Schmidt, 1999), fairness (Frey and Pommerehne, 1993; Seidl and Traub, 2001), and general social norms determine an individual's behavior. Of direct relevance to our results, a subset of this literature has more recently focused on the "crowding out" hypothesis—one going back to the argument by Titmuss (1970) that blood donors are typically motivated by moral concerns rather than money and if monetary compensation is introduced it could decrease the supply of blood donations. As Rode et al. (2013) write,

> *"motivational crowding theory is based on the psychological notions of intrinsic vs. extrinsic motivation. Intrinsic motivation refers to doing an activity for its inherent satisfaction … Counter to a common assumption in economics, motivation crowding suggests that the effects of extrinsic motivators such as monetary incentives do not necessarily complement intrinsic motivations. Instead they may undermine ("crowd out") … intrinsic motivation."*

Cast against our results, the evidence suggests that the enforcement game may have crowded out cooperative strategies by focusing players' attention on avoiding illegal gear use. By focusing the game on penalizing those who use illegal gear, it distracted players from sustaining the resource and they responded by increasing their harvest rates.

In a closely related study, Cardenas et al. (2000) ran an experiment based on a static common-pool resource game, with and without enforcement mechanisms, with participants from rural Columbia. They found:

> *"After the subjects faced the external regulation … they made choices that were significantly closer to their purely self-interested Nash responses. Thus it appears that the pressure of an external control crowded out the other- regarding behavior in favor of greater self-interest."*

The crowding-out hypothesis has been demonstrated in 51 experimental studies (Bowles and Polania-Reyes, 2012). One important pattern to these findings is the nuanced role that context places. Individuals respond to subtle cues that help them determine what behavior is appropriate in a given setting (Nisbett and Ross, 1991).

The results from our experiment are consistent with this literature. The enforcement mechanism crowded out cooperative harvesting behavior, i.e. potential harvest foregone, in favor for more aggressive individual harvesting behavior.

A second but related hypothesis on the underlying motivations for observed game behavior is that players viewed early rounds of the Enforcement treatment as the time to take advantage of the common-pool resource for individual gain before the group's experience, or the stock's collapse, began to exert greater influence over their choices. Such outcomes could be explained, for example, by selfishness or by present-biased time preferences.

Does the introduction of a new institution to enforce regulations on illegal fishing gear have unintended consequences? The results from this unique artefactual experiment show that there is a significant increase in self-interested behavior and a significant reduction in cooperative behavior when an enforcement mechanism is present. While our experiment was not designed to identify the specific rationale driving players' choices, the quick destruction of the common-pool resource in situations when a new institutional reform is introduced has an important policy implication with potential applicability anywhere globally where co-management systems play a role in local fishery management. In particular, our results suggest that policymakers should exhibit caution when resource users have multiple dimensions along which they must behave cooperatively (Smith, 2012; Reimer et al., 2014). For example, in our setting, players could choose both harvest rates and illegal gear use, and appear to have both taken advantage of fairly lax regulation early on to use illegal gear and increased overall harvest rates, even as the stock was being rapidly degraded. To address one dimension of fisher choice while leaving the other unregulated by formal or informal institutions could perhaps result in unintended damage to fisheries.

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