Regrouping to Reduce Overfishing: Evidence from a Series of Lab-in-the-Field Experiments in Mexico

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ABSTRACT

Overfishing has become a major global issue that endangers ecosystems and the livelihoods of millions of people. Weak enforcement and illicit fishing behaviors limit the effectiveness of institutional arrangements designed to curb overfishing. In this paper, we designed and tested a series of potential interventions to reduce overexploitation driven by illegal fishing. We use surveys, interviews, and common pool resource experiments to investigate the behavior of Mexican fishermen in the upper part of the Gulf of Baja California. We find that resource exploitation can be reduced using a mechanism that regroups fishermen according to their past fishing exploitation levels. More precisely, we observe that the announcement of this mechanism and the actual regrouping reduce common pool exploitation, regardless of group composition. Further experimental evidence shows that the regrouping intervention also achieves the desired outcomes in environments with informational asymmetries or regrouping imperfections.

Key words: Common pool resources, experiments, fisheries, group membership, social norms.
JEL codes: A13, C93, H41, O13, Q22.

INTRODUCTION

The world's stock of many sea-dwellers is dwindling, and overfishing has become a global issue that endangers ecosystems and the livelihoods of millions of people. The negative impacts of overfishing proportionally fall on developing countries and regions where a significant fraction of the population depends mainly on fishing because alternative sources of income and nutrition are scarce. At the same time, pressure over marine resources continues to rise, primarily to match the increasing global demand for food (Cai and Leung 2017). In response, scientists and policy makers have focused on institutional arrangements that recognize the management of fisheries as a commons problem, which implies tracking population stock and fishing capacity, examining the effect of subsidies and other exogenous incentives, and understanding fisher behavior to

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address dilemmas between individual and collective interests (Kvamsdal et al. 2016). However, even when formal or informal management systems are in place, illegal and unreported fishing remain common issues worldwide (Sumaila et al. 2020), which calls into question the effectiveness of these arrangements in the presence of imperfect enforcement and illicit fishing behaviors.

In this paper, we study a series of potential interventions to reduce overexploitation driven by illegal fishing. We use a data-driven methodological approach, based on surveys and lab-in-the-field experiments, in order to test alternative behavioral interventions tailored to the context of El Golfo de Santa Clara ("El Golfo"), a fishing community located north of the coastal zone of the Mexican state of Sonora. First, we conducted surveys to understand the socioeconomic context, drivers of illegal fishing, and fishermen's values and perceptions of the Gulf corvina fishery management system (called MCC hereafter for its name in Spanish). The information gathered suggests that the current management of fishing resources is perceived as neither fair nor legitimate, and that there is no significant social disapproval of illegal fishing behavior and a lack of incentives to reduce overfishing.

Based on the survey findings, we designed four different types of interventions, each targeting a potential overfishing driver: (1) *social disapproval* of fishing behaviors; (2) *venting* about the management system; (3) *priming* to condemn overfishing, and (4) *regrouping* fishermen with similar levels of resource exploitation. We piloted these interventions using a lab-in-the-field common pool resources experiment with 155 fishermen in San Felipe, Baja California—a nearby community similar to our target population that is far enough to avoid contamination and learning among fishermen in El Golfo, where we conducted our main lab-in-the-field experiment.

El Golfo and San Felipe are located in the upper part of the Gulf of Baja California, a bioecological hotspot recognized for being home to important endemic fish and rare marine mammals, some of which are critically endangered by overharvesting and illegal fishing. Over the last decade, many important conservation steps have been taken to fight the depletion of local marine resources in the Gulf.[1] Despite these efforts, illegal fishing practices are still common, and law enforcement is weak, so overfishing remains a threat to the sustainability of the fishing stocks and the survival of endemic species, undermining the ongoing conservation efforts.

Building upon our pilot results, we developed and tested different variants of the most promising intervention, *regrouping*, in El Golfo with 320 fishermen. The objective of the regrouping intervention is to increase the benefits of avoiding overfishing by signaling more sustainable behaviors. After a public ranking, we grouped *like-minded* fishermen together, and potentially removed free-riding behaviors from highly cooperative groups. The regrouping intervention attempts to introduce a social norm that prescribes sustainable fishing. This is done by recognizing and rewarding sustainable fishing and thus signaling social approval. Thus, the regrouping intervention targets social expectations, including empirical and normative expectations, in order to change the current socially accepted practice of overfishing (Bicchieri 2016).

To understand distinctive features of regrouping as a mechanism to reduce overexploitation, our main experimental design includes four new treatments. In the first treatment, *complete ranking*, which corresponds to the treatment in the pilot, participants were told that a public ranking would be formed using their extraction levels in previous rounds (i.e., *observation period*). Then,

1. These policies include the creation of a Natural Protected Area; permanent bans on vaquita (the world's smallest cetacean) and totoaba fishery; compensation schemes to avoid offseason fishing; and a management system of quotas and nontradable permits for the Gulf corvina fishery (called Manejo Compartido por Cuotas en el Golfo de California, or MCC for short).

fishermen with the lowest extraction totals were placed at the top of the ranking, while participants with the highest extraction sums were placed at the bottom. In the second treatment, *ranking with noise*, fishermen were told that there was some uncertainty in the public announcement and ranking because of the inaccuracy of the information used to form the new groups. In the third treatment, *only group 1 ranking*, only the first five participants with the lowest level of extraction (group 1) were announced publicly. The fourth treatment, *only group 1 ranking+*, corresponded to the third treatment but participants in group 1 were exposed to higher levels of monitoring than were the other groups.

Our lab-in-the-field experiments deliver several novel findings. We find consistent evidence from both fishermen subject pools that the regrouping intervention significantly reduces common pool exploitation. Importantly, in our main experiment we find that there is less overexploitation not only after the announcement of the regrouping mechanism but also after the regrouping has taken place, indicating that the intervention has a positive long-term impact. Furthermore, we observe less overexploitation in all groups, especially in the groups with the least exploitative fishermen but also in the groups with the most exploitative fishermen. Our results suggest that the individual extraction in the group formed by fishermen with the least extraction levels (called the SFG group) decreases in about 1 experimental unit on average, which represents a 26% reduction compared with the average extraction in the baseline groups. Similarly, there is also a significant but smaller reduction in the other groups; for instance, extraction in the group with the highest extraction level is 0.5 experimental units less in the regrouping period.

Lastly, although average individual extraction in all treatments and control groups is above the quota set in the experiment (which in the context of the MCC implies an illicit behavior), results suggest that compliance with the quota is more frequent after regrouping. Results suggest that membership to the least extractive group has a positive effect on compliance with a quota in the regrouping period. Also, we find that extraction levels equal to or less than the quota happen 49% of the time in the SFG groups, which doubles the compliance level compared with the control group (28%) in the same period and compared with all other treatment groups combined (23%), pooling all rounds before regrouping.

Overall, we find that regrouping treatments reduce individual extraction, even when controlling for fishermen characteristics. This finding provides evidence for the robustness and internal validity of this intervention, as all different variants of the regrouping mechanism (i.e., informational asymmetries or regrouping imperfections) significantly reduce overexploitation. Regrouping among the least exploitative fishing boats can be promoted by providing information on extraction, and symbolically awarding sustainable behaviors.

Our study complements the experimental literature investigating common pool resource exploitation (Baland and Platteau 1996; Agrawal 2003; Ostrom 2015). This literature stream provides empirical and laboratory experimental evidence that users sometimes refrain from overexploiting common pool resources even if there is open access (Walker, Gardner, and Ostrom 1990; Ostrom, Gardner, and Walker 1994; Cárdenas 2000). The empirical evidence often emphasizes the importance of taking into account the specifics of the setting and interactions between different factors (Ostrom and Nagendra 2006; Vélez, Murphy, and Stranlund 2010). Laboratory experimental evidence mainly focuses on isolating a number of factors reducing exploitation, such as communication (Hackett, Schlager, and Walker 1994) and decentralized enforcement mechanisms (Ostrom, Walker, and Gardner 1992) in abstract common pool resource settings. Rather, the main feature of our approach is that it is customized for the context. Our approach not only takes into consideration the local possible choice set while being informed by economic theory, but it also *learns* from contextual findings. We use survey methods and pilot trials to inform the design of the treatments to be tested using lab-in-the-field experiments. We are unaware of any other studies following our approach in selecting and developing potential interventions to reduce common pool resource exploitation.

The closest related studies use similar experimental methodologies and apply them to field settings with common pool resource users (Cárdenas and Ostrom 2004; Cárdenas and Carpenter 2008; Vélez, Murphy, and Stranlund 2010; Carpenter and Seki 2011; Fehr and Leibbrandt 2011; Anderies et al. 2011; López et al. 2012; Cavalcanti, Engel, and Leibbrandt 2013; Castillo and Saysel 2005; Brick, Visser, and Burns 2012; Kittinger et al. 2013). These studies expand the findings from standard laboratory experiments and shed light on the factors driving resource exploitation. For example, Chávez, Murphy, and Stranlund (2018) use framed field experiments to study the effects of poaching on the ability of common pool resource users to coordinate their harvests when the government enforcement is weak. Other literature explores the unintended consequences of weak enforcement in common pool resources (Cárdenas, Stranlund, and Willis 2000; Vélez, Stranlund, and Murphy 2009; MacColl et al. 2018).

Regarding the institutional arrangements explored in this study, there are a few experiments investigating the use of regrouping to reduce noncooperative behavior (Burlando and Guala 2005; Gächter and Thöni 2005; Page, Putterman, and Unel 2005; Gunnthorsdottir et al. 2007; Brekke et al. 2011; De Oliveira, Croson, and Eckel 2015; Cabrera et al. 2013). These studies investigate cooperation in the context of public goods games and generally find that regrouping increases individuals' contributions. Our regrouping intervention is closest to Gächter and Thöni (2005) and Gunnthorsdottir, Houser, and McCabe (2007), who also use endogenous group formation in a "meritocracy mechanism" in which participants are regrouped depending on their cooperativeness. A key difference is that we implement an observation period after the announcement of the regrouping mechanism where participants remain in their old group before they are regrouped. As a result, we give participants the chance to adjust their behavior in light of an upcoming intervention, and furthermore, we can investigate strategic considerations during the observation period. We also choose to include an observation period because we believe that this period is necessary to lower potential resistance among the fishermen, in case regrouping in the field is actually implemented.

Additionally, although to a lesser extent, this study contributes to the experimental literature on social disapproval to increase cooperativeness in standard public goods games (Masclet et al. 2003; Rege and Telle 2004; Noussair and Tucker 2005), venting (Dickinson and Masclet 2015), and moral priming (Benjamin, Choi, and Strickland 2010; Drouvelis, Metcalfe, and Powdthavee 2010). We decide to use context-specific venting and moral priming interventions, as we conjecture that they are most likely to have an impact. Interestingly, in these common pool resource settings, we do not observe that these interventions affect resource exploitation of fishermen, which suggests that more research is needed to understand their scope and the interactions with specific field contexts.

The rest of the paper is organized as follows. First, we provide contextual information about the field setting. Then, we describe the preexperimental insights from surveys and pilots that informed the design of our main intervention. Later, we present the experimental design for the regrouping treatment. Finally, we report and discuss the findings of the main experimental intervention at the individual and group levels.

BACKGROUND: EL GOLFO CONTEXT

Our study is conducted in El Golfo de Santa Clara, one of the four communities that comprise the Gulf corvina (*Cynoscion othonopterus*) fishery in the upper part of the Gulf of Baja California, Mexico, north of the coastal zone of the Mexican state of Sonora. This gulf is a bioecological hotspot recognized for being home to several endemic fish and marine mammals. In this context, three relevant species are the vaquita, a rare species of mammal and the smallest cetacean known, the totoaba, and the Gulf corvina. These species are endemic, and the first two in particular are classified on the Red List of the International Union for Conservation of Nature and Natural Resources as highly endangered species (Rojas-Bracho and Taylor 2017).

Many conservation efforts led by government and nongovernment organizations[2] have been implemented to stop the depletion of marine resources (EIA 2016). In 1993, a marine reserve[3] was created with a general conservation program for the upper part of the Gulf. This program includes, for instance, the first temporary ban of gill nets to protect the vaquita in 2002. However, since the beginning, monitoring and rule enforcement have been important challenges for the institutional capacity of local and federal authorities in Mexico (Comisión Nacional de Áreas Naturales Protegidas 2007). The threats to marine species led to a general ban in 2011 of all fishing activities in the core zone of the reserve, while in other zones exceptions were granted to the corvina fishery.

In addition, the MCC was established in 2011 as a management system that includes a fishing quota and a nontradable permit scheme for corvina fishery. This system has two main objectives: (1) to sustain the natural stock of corvina by reducing the catch levels and protecting spawning aggregations, and consequently, (2) to preserve the livelihoods of the four main fishing communities in the upper part of the Gulf of Baja California. To do so, the program relies on a total catch limit defined by the government fishing technical institution called INAPESCA. Fishing permits to comply with the catch limit are distributed to vessels, individuals, or cooperatives in four communities: El Golfo, San Felipe, Bajo Rio, and Río-Cucapá. Given that El Golfo mainly relies on corvina, this community has received 65% of the permits. Finally, the MCC established that the fishing policy federal institution, CONAPESCA, local police, and federal agents are in charge of the law enforcement of the system.[4]

Despite all these efforts, there are irregular and illegal fishing practices in this zone. For instance, vaquita has no commercial demand but is caught by accident when people illegally fish totoaba, which is captured for its swim bladder, a delicacy in Chinese markets sold for thousands of dollars per pound. In addition to that, the protection of these endemic species is undermined by fishermen not using the proper gear, fishing in forbidden zones of the reserve or without permits for corvina, harvesting above the quota, and illegally trading and renting these permits, among other practices.

El Golfo de Santa Clara's community relies almost entirely on the fisheries sector. Among the 4,000 people in El Golfo, a quarter are fishermen and the corvina production and its commercialization constitute the primary (legal) income of the community. Since the ban limited the corvina

2. Such organizations include the Environmental Defense Fund, Greenpeace, and Sea Shepherd Conservation Society.

3. The reserve is called (in Spanish) Reserva de la Biosfera Alto Golfo de California y Delta del Río Colorado.

4. Original permits were handed out to those who were members of fishing cooperatives or enrolled in the program as individual fishers/vessels based on fishing tradition. The catch limit (the quota) is granted to each permit, regardless how it is used. It could be the case of an association using a single permit for multiple vessels, or a fisherman having more than one permit (say, one at this name and another for a vessel he bought). Periodically, the environmental authorities define the current population stock, such that the total number of fish that is sustainable to be caught is distributed equally among all permits.

fishing season to occur only from the end of February through the end of April, Mexican authorities established compensation to cover the loss of profit derived from the prohibited fisheries. This compensation plan is based on money transfers aimed at covering the opportunity costs of fishing and includes permit holders, captains, helpers, and fishermen and their wives, who gut the fish at home (Secretaria de Desarrollo Social de México 2017). Given the current illegal and irregular fishing practices and the high probability of the vaquita becoming extinct, El Golfo de Santa Clara and other fishing communities struggle to prevent the closure of the corvina fishery by the Mexican government.

METHODOLOGICAL APPROACH: PREEXPERIMENTAL INSIGHTS

Our methodological approach is based on (1) a preexperimental survey; (2) a pilot for different types of interventions; and (3) testing of different variants of the most promising intervention in our target community with a lab-in-the-field experiment. In this section, we briefly present survey insights, elaborate on the experimental designs, and present our pilot findings. A more detailed discussion of our approach to intervention design can be found in Batista et al. (2018).

SURVEY

The preexperimental fieldwork was developed and conducted together with the Environmental Defense Fund. This fieldwork includes the implementation of surveys to understand the socioeconomic context, fishermen beliefs on the drivers of illegal fishing, and fishermen perceptions of the management system of corvina fishery (the MCC) and the authorities in charge of enforcing it. The generated insights informed the design of the experimental interventions. For the survey, we used a convenience sampling in a cross-sectional study between March and May 2016. In total, we conducted surveys with 168 fishermen and permit owners in El Golfo, which constituted about 5% of the town's population at the time.

Survey results indicate that the MCC is not perceived as fair, legitimate, or beneficial. Also, fishermen complain that their opinions have not been taken into account in the design of the management system. More precisely, 39% completely disagree with the application of the MCC to manage the corvina fishery, and 69% feel that their concerns and needs have not been taken into account in the decisions to implement the management system with permits and quotas. Furthermore, 78% report that the MCC has reduced their income and a third said that the system has worsened their quality of life. Also, about half of the fishermen stated they were not satisfied with the degree of participation in the decisions regarding the management of fishing resources in the community.

The survey and interviews also show that a large portion of fishermen perceived not complying with the MCC to be acceptable or inconsequential, and that more serious illegal fishing practices are caused by other nonlocal fisher groups. A third of the non–permit owners admit to having either exceeded their quota, fished without a permit, or fished with a fake permit. In addition, 49% think that slightly exceeding the quota is acceptable and 41% think that exceeding the quota is "not a big deal." Only 9.9% think that fishing more than the quota allows is not acceptable. At the same time, survey results indicate that almost half of the respondents felt that the state government was not doing a good job at regulating the fishing sector in the region. Moreover, interviews with fishermen and local leaders revealed that the behavior of others was often the reason behind other illicit behaviors. Locals referred to foreign fisher groups, or criminals disguised as fishers, as those to blame for poaching and fishing totoaba. Regardless of the veracity of these claims, the references to the behavior of others and the efficacy of government authorities hint at a status-seeking form of social comparison. By blaming others, fishermen aim to differentiate what types of behaviors are considered illicit and, ultimately, to protect the social status of the local fishers.

PILOT TREATMENTS

Our pilot treatments were designed based on the survey findings with the goal to identify the most promising intervention. The survey suggested that (1) many fishermen felt that their voices were not heard and that they did not agree with the MCC system; (2) illegal fishing was not considered wrong; (3) fishermen cared about social status but (4) did not perceive that fishing above the quota endangered their social status.

We implemented the pilot experiment with 155 fishermen in San Felipe in September 2016. As in the main experiment explained below, we conducted a common pool resource game of 10 rounds, divided in two sets. The first five rounds are used to establish a within-subjects baseline and rounds 6 to 10 are used to implement a treatment for each experimental session. At the beginning of the experiment, fishermen are randomly assigned to groups of five participants, and in each period, fishermen make a single decision about how many fish units to extract from a common pool at the group level. A more detailed explanation of this procedure is presented in the experimental design section. In total, we run four experimental sessions to test four treatments:

(1) *Venting:* This intervention was aimed at giving fishermen a voice and the possibility to express their thoughts and emotions to local authorities—something that they felt was lacking. To achieve this, participants were told that they could send an anonymous message to CONAPESCA (the fishing authority that regulates the corvina fishery) before the start of the sixth period. The instructions stated that we would deliver the messages to CONAPESCA. Using a piece of paper, fishermen decided whether they wanted to write a message. Fishermen had the opportunity to send messages to the fishing authority in every period until the end of the game. Sending a message did not have any monetary costs to participants. The conjecture was that this particular venting possibility increases cooperation among fishers, similar to what has been observed when individuals have the opportunity to vent to their peers (Xiao and Houser 2005; Dickinson and Masclet 2015).

(2) *Priming to condemn overfishing (pope framing):* This intervention was intended to activate a moral norm, sense of duty, and signal that resource overexploitation is wrong. We read specific messages on the ethics of protecting the environment. These messages were portions of the pope's encyclical letter of 2015, *Laudato Si'*. Each message was delivered to each participant accompanied by a photo of the Catholic Pope Francis. After reading the messages, participants made their extraction decisions and the experiment continued as in the baseline rounds. The conjecture was that such moral priming reduces extraction in the common pool resource game (Benjamin, Choi, and Strickland 2010; Drouvelis, Metcalfe, and Powdthavee 2010).[5]

5. The list of all messages sent in rounds 6 to 10 are available upon request.

(3) *Social disapproval:* This intervention was also aimed at providing fishers with a signal that resource overexploitation is wrong, by activating a self-sanctioning mechanism to punish illegal fishing, following the procedure in López et al. (2008). Starting from period 6, each participant decided how many sad faces they wanted to send to other participants knowing their extractions (anonymity was still preserved; the fishermen did not know the identities of the individuals they could send the sad faces to). Sending a sad face cost one peso. Once the participants sent the sad faces, an assistant posted the number of sad faces each participant received in the round. Receiving sad faces had no monetary cost to participants (see also López et al. 2008). The conjecture was that such social disapproval reduces extraction (Masclet et al. 2003; Rege and Telle 2004; Noussair and Tucker 2005).

(4) *Regrouping—sustainable group:* The goal of this intervention was to use the fishers' status concerns through recognizing and rewarding sustainable behavior, which may also activate a new social norm (see the regrouping treatment section for more details). To test the effect of regrouping, we introduced a publicly announced ranking based on individual extraction levels in rounds 6 and 7. In the final rounds (8–10), we regrouped the participants according to their ranking: the fishermen in the first five places formed the first group: the sustainable fisher group (SFG); the fishermen ranked in places 6 to 10 formed the second group; and those in places 11 to 15 formed the third group. In these new groups, participants played the three last rounds.

PILOT FINDINGS

In this section, we briefly summarize the main patterns in each treatment. Figure 1 provides a first overview that visualizes the impact of each treatment. More precisely, it illustrates the average extraction levels across rounds for each of the four treatments in the pilot experiment. We can see that mean exploitation levels are always well above the social optimum during the baseline rounds (3.54 to 4.05 out of 6; the social optimum being 2). After the intervention implementations in the fifth round, average extractions in rounds 6–10 rather increase than decrease in three of the four treatments (from 3.43 to 3.45 in pope framing, $p = 0.99$; 3.88 to 4.20 in social approval, $p = 0.23$; 3.54 to 3.85 in venting, $p = 0.14$).[6]

Interestingly, in the social disapproval intervention fishermen increased the number of sad faces they sent over the course of the experiment (see online appendix table A1), but sad faces were also sent to those complying with the quota (14.6% of the sad faces were sent to fishermen who did not exceed the quota), which undermines the functioning of this intervention and reinforces the suspicion that illegal fishing is not socially disapproved. Similarly, venting to the fishery authority did not reduce extraction and may have had some unintended effects. On average, participants sent messages in 54% of the rounds, and 50 out of 60 participants vented at least once during the experiment. Interestingly, we observe a considerable fraction of negative and demanding messages to the fishing authority, suggesting that this type of venting may have triggered negative emotions.[7] Priming to condemn overfishing (pope framing) did not have any significant

6. See also online appendix table A2, where we compare the average individual extraction in the baseline (rounds 1–5) to average individual extraction in the treatments (rounds 6–10) using a Wilcoxon rank sum test.

7. We classified participants' messages according to the following categories: positive or neutral messages (13%); negative messages (16%); messages with explicit demands or request (44%); messages with specific questions (8%); all other types of messages (19%).

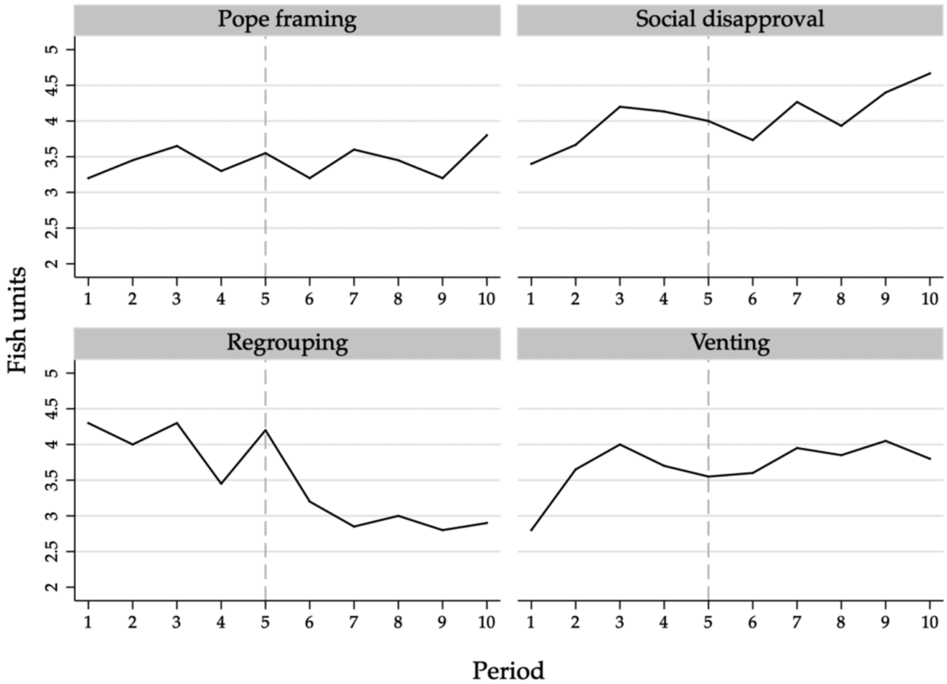

Figure 1. Average Extraction Levels in Pilot Experiment in San Felipe. This figure illustrates average individual extraction per intervention in the common pool resource game. The lowest extraction level is 1 and the highest is 6. The quota established in the game is 2 experimental units. The interventions were introduced after the fifth period.

effect. We chose messages related to fisheries and natural resource management from the pope's encyclical letter of 2015, *Laudato Si'*, because previous fieldwork suggested that the Catholic pope was a popular figure in the area. We were expecting that reading this message aloud before each round would cause extraction to decrease. The lack of an impact may have to do with the fact that a considerable fraction of non-Catholic participants (41%) were present.

In contrast, the regrouping intervention was successful in reducing mean exploitation levels (from 4.05 to 2.95, $p < 0.01$). Further, we observe during this intervention that exploitation levels were reduced across all groups in the session and not just in the SFG (we provide a detailed analysis in the results section). Consequently, we decided to further investigate this intervention in our main experiment at El Golfo and test its robustness, as explained in the next section.

EXPERIMENTAL DESIGN

PROCEDURE

We designed a framed lab-in-the-field experiment to reflect the harvesting activities in the corvina fishery, in a manner that is relevant and familiar to the fishers in the region. More precisely, participants took part in a static common pool resource game following the design of Vélez, Murphy, and Stranlund (2010), for the pilot in San Felipe and the main intervention in our target community in El Golfo. In both communities, local community members were hired to recruit participants (18+ years) who had been active corvina fishermen during the previous season. We did not recruit fishermen who focused on catching other species and corvina permit holders who did not fish. In all sessions, fishermen gathered in a hotel conference room where a group of research assistants implemented the experiment. The same person read the instructions aloud at each site for all fishermen.[8]

In the experiment, participants made extraction decisions framed as fishing. Local fishermen were placed in groups of five, with each deciding individually how many fish (1–6 units) to harvest in each of the 10 fishing rounds. The first five rounds served as a baseline of participant extraction levels. This period captures a regulated fishery with a management system that includes a fishing quota, like the one operating in the region for the corvina fishery. All participants stayed in the same five-person group for the first five rounds in all treatments. Interventions took place after the fifth period, and fishing behavior was tracked for five additional rounds post-treatment. Participants were told that fishing more than 2 units (which constitutes the social optimum) was not allowed and that a penalty of one Mexican peso would be imposed for each extracted unit detected above the quota. It was common knowledge that the probability of monitoring in a given period was 2%.[9] The participants were told that fishing authorities established the quota to promote the sustainability of the fishery.

A system of imperfect monitoring was implemented as follows. At the end of each round a participant from each group picked a ball from a bag that contained 50 balls. Five of these were marked with the participant's identification number (1–5). If the participant's number was selected from the bag, a research assistant monitored him privately. If the participant was caught over extracting ($>$ 2 units), the penalty was subtracted from the individual's earnings.

We used a payoff function where the earnings of participant i in each round of the game were represented in equation 1, as follows:[10]

$$\pi = e_i + p(y_i - 1) - c(y_i + y_{-i}) - d(y_i + y_{-i}) - E[m|y_i], \tag{1}$$

where y_i represents the individual extraction level (1–6); y_{-i} refers to $\Sigma_{j \neq i} y_i$, the aggregate extraction level of the other group members; e_i is the initial endowment; p is the constant price in Mexican pesos of the units harvested; and c and d are positive constants. Following Vélez, Stranlund, and Murphy (2009), these components reflect the social dilemma in which $d(y_i + y_{-i})$ captures the cost externality that is typical of common pool resource problems, while $c(y_i + y_{-i})$ captures negative externalities that reduce individual existence or nonuse values. Thus, individual earnings depend on both individual extraction and the extraction by others in the group. Note however that our design does not capture an intertemporal dimension of the common pool resources and the extraction of one round does not affect the availability of the resources in the next round, as is the case in real-life systems. The last term is the expected reduction in payoff by being monitored, conditional on individual extraction (i.e., one Mexican peso, MXN, per each unit above the quota of 2 units). Parameters p, c, d, and e_i were defined following the design of Vélez, Stranlund, and Murphy (2009), and final payoffs were converted using an exchange rate of MXN 18.27 per experimental unit. According to this payoff structure, the Nash equilibrium was that participants

8. An English version of the instructions can be found in the online appendix.

9. We chose a very low probability to capture the expected low probability of monitoring in the field.

10. We did not present this payoff function to participants. We use it to construct the payoff table included in online appendix table A3, which was given to the participants during the experiment.

extract 6 fish units in each round. As in the field, the expected penalty from the monitoring system is too low to deter the participants from exceeding the quota.

We followed the same protocol in San Felipe (for the pilot) and El Golfo. After the instructions, participants played a practice round that allowed them to become familiar with the forms that they would fill out and with the game dynamics. After the practice round, fishermen were randomly placed in one of the three or four groups included in each session. Once in groups, participants made their extraction decisions and communicated this information to the research assistants on paper. The research assistants summed the total level of extraction for each group and provided this information to each group member individually. With this information, participants calculated their earnings using a payoff table (see table A3 in the online appendix).

At the end of the 10 rounds, we conducted a survey gathering socioeconomic information from the fishermen. Subsequently, the earnings from the game were paid in private. We paid every round to take each decision equally seriously. The average earnings in the experiment were MXN 529.83 (USD 29), including a show-up fee of approximately MXN 100 (USD 5.50). These earnings double the estimated daily MCC compensation of MXN 267 (USD 14.7), covering the opportunity cost of fishermen in this region.

REGROUPING TREATMENT DESIGN

Based on the pilot results, we replicated the most effective intervention (regrouping—sustainable group) with 320 fishermen in El Golfo between November and December 2016. We carried out a between-subjects experiment with a control condition. In the control, subjects played the baseline game not just for the first five rounds but for all 10 rounds ($N = 50$ fishermen). To study the robustness of the regrouping intervention, we tested four new treatments (see table 1 for a summary of the experimental treatments). For the treatment groups, the intervention was introduced after the fifth round. In each treatment, we held three to five experimental sessions with 15 participants each. The distribution of treatments and control conditions was randomized across 21 experimental sessions.[11]

In the first treatment (*complete ranking*), participants were told that a public ranking would be formed using their extraction levels in rounds 6 and 7, similarly as in the pilot. In round 8, the fishermen with the lowest extraction sums were placed in the top ranking, while participants with the highest extraction sums were placed at the bottom of the ranking. Using this ranking, the group composition of the session changed, and this information was displayed in public with identifiers of the subjects (see table A4 in the online appendix). That is, each participant knew where he was placed but did not know the identity of the other players. Before round 8, the participants in the first five places formed the first group: the sustainable fisher group; the participants ranked in places 6 to 10 formed the second group; and those in places 11 to 15 formed the third group. With the new groups' compositions, participants played the last three rounds of the game.

Additionally, we study three additional treatments subject to informational asymmetries or regrouping imperfections. In treatment *noise,* fishermen were told that there was some uncertainty in the public announcement and ranking because of the inaccuracy of the information

11. We conducted one or two sessions every day during the span of four weeks. At the beginning of the session, we include the following message as is stated in the experimental instructions: "The activity you are going to participate in today is different from activities other people from your community have participated in. We therefore ask you to make decisions based on the instructions we are going to explain to you and not based on comments you have heard."

Table 1. Experimental Treatments

Treatment	Monitoring Probability	Groups Shown in Ranking	Errors in Ranking	Session
Control	1/50	None	–	3
Complete ranking	1/50	All	No	5
Noise	1/50	All	Yes	5
Only the SFG	1/50	Only the first	No	4
Only the SFG+	1/50, 1/25	Only the first	No	4

Note: In all treatments there was a quota of 2 units per participant in each period.

used to form the new groups. This "noise" was included in the treatment to simulate the actual fishery, where it is not always possible to gather accurate data about the amount of fish harvested by each person. Although the ranking was displayed to participants, the researchers' description of what was happening included a few paragraphs mentioning this uncertainty. In two of the experimental sessions with noise, the ranking revealed to the participants was actually the right placement according to the extraction levels (subtreatment *framed noise*). In the other two sessions, participants in places 4 and 5 (or the last two members of the SFG) were exchanged with participants in places 14 and 15 of the ranking (subtreatment *real noise*). This information was not disclosed to the participants.

As the name indicates, in the *only the SFG* treatment, only the ranking of the players who would constitute group 1 (the SFG) was announced publicly. The composition of the other two groups (T and Q) was exposed but without mentioning the place of each participant. Their positions in the poster were displayed without following any particular order (table A5 in the online appendix). Finally, to test the effect of more monitoring, in the last treatment, *only the SFG+*, we implemented a design similar to *only the SFG* in which just the ranking of the SFG was announced publicly but the probability of being monitored also changed from 1/50 to 1/25 for the participants who were part of the SFG. The rest of the participants faced the same probability as in the other treatments (1/50) and the details of their ranking were not announced.

EXPERIMENTAL RESULTS

DATA

The average individual extraction by round and treatment is reported in figure 2. The figure shows that the intervention reduced average individual extraction in all four treatments. Average individual extraction is always above the quota of 2 units.[12] Extraction dropped in rounds 6 and 7 in the observation period, when the announcement and ranking procedures took place. In rounds 8–10 when individuals were regrouped, the SFG reduced extraction in all treatments and in particular in the *only the SFG+* treatment. Groups 2 and 3 extracted more than the SFG group, and group 3 was systematically the group with the highest level of extraction. However, as we show in our regression models, group 3 also significantly reduced extractions compared with the first five rounds.

Table 2 reports summary statistics of the socioeconomic characteristics of the sample that we use in our models as controls. We captured this information using a short post-experiment

12. Considering all rounds, a large fraction of subjects chose to extract 1 unit (7.5%) and 2 units (18.3%). However, more subjects chose to extract 3 units (18.97%), 4 units (19.78%), 5 units (18.88%), and 6 units (16.59%).

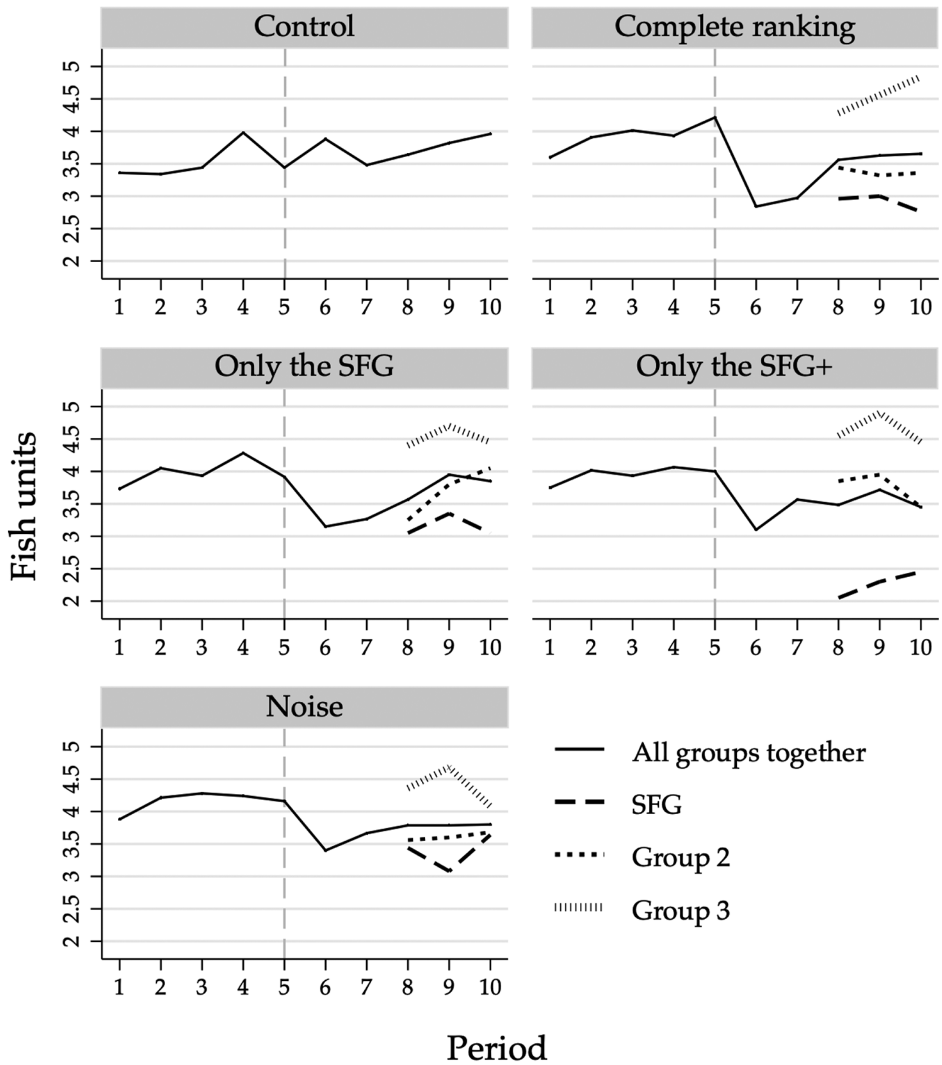

Figure 2. Average Individual Extraction per Regrouping Treatment. This figure illustrates the average individual extraction per treatment in the common pool resource game. The choice set in the experiment ranges from 1 to 6 experimental units. The interventions were introduced after the fifth period.

survey. The variables include level of education, captain status (i.e., whether the participant works as the captain of the boat), redline (i.e., the percentage above the quota established by the MCC that participants declare they would catch), age, marital status, whether fishermen have a permit, and whether they are Catholic.

INDIVIDUAL-LEVEL EFFECT

We used random effect models to evaluate the effect of the intervention on extraction levels at the individual level, considering the three stages of the game (the baseline, observation, and regrouping periods). Our data include 10 rounds of decisions by 320 male fishers from El Golfo.

Table 2. Descriptive Statistics of Survey Variables

Variable	Obs.	Mean	Std. Dev.	Min.	Max.
Education (years)	320	7.97	5.53	0.00	16.00
Captain (yes = 1)	320	0.54	0.49	0.00	1.00
Redline (%)	320	0.19	0.39	0.00	1.00
Age (years)	320	41.90	13.27	17.00	77.00
Married (yes = 1)	320	0.65	0.47	0.00	1.00
Catholic (yes = 1)	320	0.59	0.49	0.00	1.00
Permit (yes = 1)	320	0.19	0.39	0.00	1.00

Our dependent variable is individual extraction in each round. For each treatment, we include two dummy variables, one for rounds 6 and 7 (i.e., observation period), another variable for rounds 8 to 10 (i.e., regrouping period), and a variable for the game period (from 1 to 10). Also, we controlled by the set of individual characteristics gathered in the post-experimental survey presented in table 2.[13] We also included an indicator variable *monitored* that is equal to 1 if the participant was monitored in the previous round, and 0 otherwise. In total, monitoring occurred in only 62 out of 3,200 rounds (approximately 1/50), and in 80% of them, fishermen extraction was above the quota.

For the *noise* treatment, we merged both sessions from *framed noise* and *real noise* for the analysis. However, to address the effect of real noise, we included a dummy (*real noise*) in the regression analysis of this condition.[14]

We are interested in the coefficient for the periods' dummy in the regression, which captures the effect of each period compared to the baseline rounds. Table 3 reports the overall effects of the intervention on individual extraction for each treatment using linear random effects by individual and clustered robust standard errors at the session level. Results show a significant reduction in individual extraction in both the observation and the regrouping periods compared with the first five rounds. This effect was not significant in the control condition in which participants played the 10 rounds without any intervention. We do not observe an effect of being monitored in the previous round, as the estimated coefficient changes signs across treatments and it is mostly insignificant. In the case of the *noise* treatment, the coefficient of *real noise* was not statistically significant.[15]

To study what effect is stronger in reducing extraction, we compared the coefficients of the observation and regrouping periods from the models reported in table 3. Using a Wald test, we found that the reduction of extraction was significantly higher for rounds 6–7 than for rounds 8–10 (see table A8 in the online appendix). Moreover, we estimated an additional model following a difference-in-difference design to check the robustness of our results when we use all data from the baseline rounds in each treatment and control group in the regression analysis. These results are reported in table A9 in the online appendix and they corroborate the findings presented in

13. See table A6 in the online appendix for estimated results for all covariates.

14. The results of the separate noise treatments are presented in table A10 in the online appendix. Results indicate no differences in estimated coefficients between these the two types of noise, which is similar to the results in table A6 showing that the coefficient for noise is statistically not different than 0.

15. In online appendix table A11, we explore the behavior of those participants misplaced in the ranking of the real noise conditions. Overall, we found that cooperators placed in the last two positions of the ranking continued to show a relative low extraction level, while free riders placed in the SFG continued a high extraction level after the sorting procedure.

Table 3. Intervention Effect on Individual Extraction

Variable	Control Extraction	All Treatments Extraction	Complete Extraction	Only SFG Extraction	Only SFG+ Extraction	Noise Extraction
Observation period	–0.09	–1.04***	–1.42***	–1.05***	–0.84***	–0.83***
	(0.22)	(0.11)	(0.23)	(0.04)	(0.23)	(0.15)
Regrouping period	–0.15	–0.79***	–0.99***	–0.67**	–0.77***	–0.72***
	(0.34)	(0.12)	(0.30)	(0.30)	(0.20)	(0.17)
Monitored (yes = 1)	–0.19	–0.08	0.23*	–0.54***	–0.35	0.52*
	(0.93)	(0.17)	(0.12)	(0.14)	(0.40)	(0.27)
Real noise (yes = 1)						0.13
						(0.14)
Constant	4.24***	4.12***	4.89***	2.96**	4.76***	3.63***
	(0.40)	(0.54)	(1.12)	(1.24)	(0.70)	(0.48)
Individual-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	500	2,700	750	600	600	750

Note: Random-effects model estimation using the GLS estimator. Clustered robust standard errors at the session level are in parentheses. Control variables include age, education level, redline, round, and dummy variables for captains, married, Catholic, and permit owners. Significance levels: *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.10$.

table 3, namely negative effects on individuals' extraction in the observation and ranking periods for all treatments, and a negative and significant average treatment effect of the intervention as a whole.

GROUP EFFECTS

We conducted a Wilcoxon rank sum test per treatment to compare the distribution of extraction at the group level between the baseline and intervention periods. Results are reported in table 4. Comparing the baseline rounds to the intervention rounds, we find that there is no significant

Table 4. Average Group Extraction by Treatment

Treatment	Obs.	Rounds 1–5 Mean	Rounds 6–10 Mean	Wilcoxon Test *P*-Value
Control	20	3.51	3.75	0.449
All treatments pooled	108	3.93	3.54	0.000
Complete ranking	30	3.93	3.33	0.020
Noise	30	4.15	3.68	0.016
Only the SFG	24	3.98	3.55	0.093
Only the SFG+	24	3.95	3.46	0.125
Treatment	**Obs.**	**Rounds 6–7 Mean**	**Rounds 8–10 Mean**	**Wilcoxon Test *P*-Value**
Control	10	3.68	3.80	0.545
All treatments pooled	54	3.24	3.68	0.000
Complete ranking	15	2.90	3.61	0.029
Noise	15	3.53	3.79	0.164
Only the SFG	12	3.20	3.78	0.056
Only the SFG+	12	3.33	3.55	0.623

Note: Wilcoxon two-tailed test at group level. Individual observations are pooled accordingly with player's group in the game, so that the number of observations shows the number of groups for each treatment.

Table 5. Group-Specific Effect

Variable	Control Extraction	All Treatments Extraction	Complete Extraction	Only SFG Extraction	Only SFG+ Extraction	Noise Extraction
Observation period	–0.09	–1.04***	–1.41***	–1.05***	–0.84***	–0.83***
	(0.22)	(0.11)	(0.23)	(0.04)	(0.23)	(0.15)
Regrouping period	–0.15					
	(0.34)					
SFG		–1.10***	–1.38***	–0.83**	–1.28***	–0.85**
		(0.16)	(0.26)	(0.35)	(0.36)	(0.33)
Group 2		–0.79***	–1.05***	–0.71*	–0.50***	–0.83***
		(0.12)	(0.28)	(0.38)	(0.15)	(0.15)
Group 3		–0.49***	–0.53	–0.47**	–0.52**	–0.47**
		(0.14)	(0.40)	(0.23)	(0.25)	(0.23)
Monitored (yes = 1)	–0.19	–0.07	0.27**	–0.53***	–0.31	0.48**
	(0.93)	(0.16)	(0.12)	(0.14)	(0.36)	(0.25)
Real noise (yes = 1)						0.14
						(0.14)
Constant	4.24***	4.14***	4.87***	2.97**	4.79***	3.67***
	(0.40)	(0.53)	(1.03)	(1.23)	(0.70)	(0.47)
Individual-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	500	2.700	750	600	600	750

Note: Random-effects model estimation using GLS estimator. Clustered robust standard errors at the session level are in parentheses. Control variables include age, education level, redline, round, and dummy variables for captains, married, Catholic, and permit owners. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

difference in the control group in which extraction tends to increase. In contrast, considering all treatments together, we find that average group extraction decreases from approximately 4 fish units to 3.5 and that this difference is statistically significant at $p < 0.01$. On the other hand, comparing the observation period (rounds 6 and 7) and the regrouping period (rounds 8 to 10), the extractions increased in all conditions in the last three rounds of the game and, despite this difference being significant for only two treatments, this could indicate a last-round effect. Nevertheless, average group extraction in all treatments is still below their respective baseline average extraction. Considering that the control group keeps increasing in these periods, we find these results as evidence for a positive effect of the regrouping intervention.

Furthermore, in table 5 we report group-specific effects on the individual level of extraction for each treatment. We analyzed the intervention effect considering the behavior of the new groups formed after the ranking procedure: the SFG, group 2, and group 3. In this model, the new groups (the SFG, group 2, and group 3) are identified with a dummy variable. Thus, coefficients for these indicator variables capture the effect of regrouping mediated by group-specific membership. We replicated the same specification for each treatment including all socioeconomic and game covariates as before.[16] We find that extraction levels were reduced across all newly formed groups and not just in the SFG. However, as expected, we found that the treatment effect was stronger in the SFG, followed by the second and the third group in all treatments.[17] Using a Wald test, we corroborated that the SFG effect was significantly higher than the other two groups in all treatments (see online appendix table A8).

16. See table A7 in the online appendix for estimated results for all covariates.

17. The decrease in group 3 for the *complete ranking* regression is not statistically significant at least at the 90% confidence level.

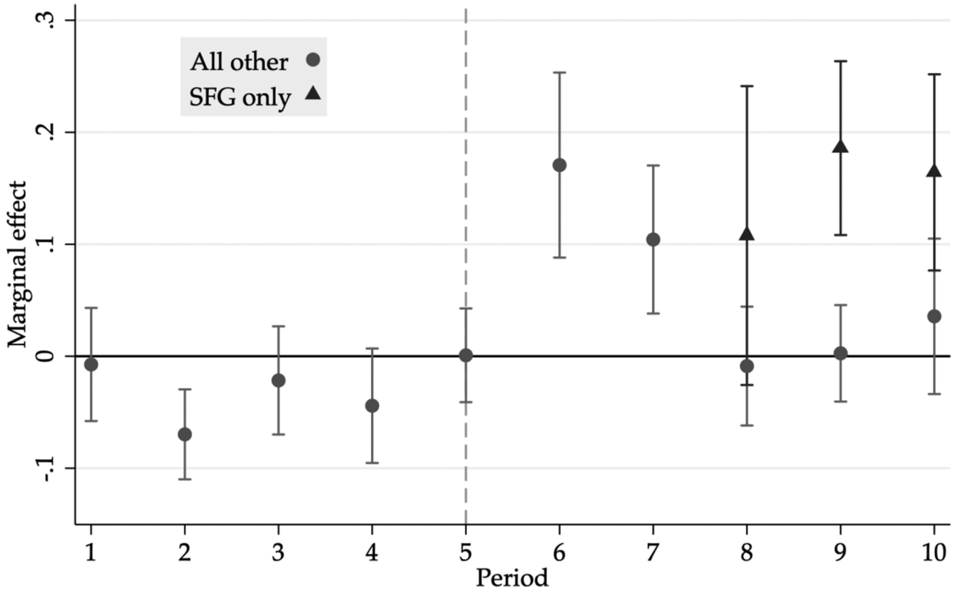

Figure 3. Estimated Effects on Compliance. Coefficient estimates reported from a linear probability model for all regrouping treatments (2,700 observations). Round 5 is used as the reference group (dashed vertical line). Dots indicate the point estimate for the marginal effect of each round, and vertical lines represent the confidence interval at the 95% confidence level.

COMPLIANCE

We also analyze the impact of the regrouping intervention as a whole on the compliance with the in-game quota of 2 fish units. We estimated a linear probability regression model using as dependent variable a binary variable for compliance equal to 1 if the extraction in a given round is less than or equal to the quota, and 0 otherwise. As shown earlier, group average and individual-level extraction were always greater than 2 for all treatments and control groups, but we find a statistically significant effect of the treatment on the probability of compliance.

In figure 3, we plot the estimated coefficients for each round of the game using round 5 as the reference group. We observe that compliance is increasing post-intervention, since after round 5 the estimated coefficients are positive and mostly significant. This holds in particular for rounds 6 and 7, which may suggest strategic behavior during the observation period when extractions are used to compute the ranking. In the last three rounds, the effect is driven by the SFG group. Furthermore, we observe that compliance is on average 49% for SFG groups in the regrouping period, compared with 28% for the control group in the same period, and with 23% for all other treated groups combined when pooling all rounds before regrouping. These results indicate that compliance with the quota increases with the intervention.

DISCUSSION

The fishermen in our field setting, like in many other settings, are stuck in a difficult economic situation. There is abundant evidence from field research and laboratory experimentation that it is difficult to establish sustainable fishing practices. This evidence is paralleled in our investigations among Mexican fishermen. All but one of our experimental interventions failed in generating change.

We provide evidence that the regrouping intervention has the potential to curb resource exploitation. In this intervention, fishermen are regrouped according to their exploitation levels, and the least exploitative fishermen are rewarded symbolically by making them part of a group of sustainable fishers. Interestingly, we observe that such regrouping caused a reduction in extraction also in the other less cooperative groups of fishers, albeit less so. In the sustainable fisher group, this reduction may be particularly due to group identity or conditional cooperation. In the other groups, it seems particularly likely that regrouping raised the awareness of fishers and increased feelings of guilt and wrongdoing, which could be the result of changed social expectations regarding the new desired behavior.[18]

With our experimental design and results we hope to provide insights for similar fishing communities in the "global south," where overfishing is common practice and where there is an expected belief about the behavior of others. We show in our experiments that it is possible to reduce extraction, increase compliance, and thus shift to more sustainable fishing.

The premise of our experiments was to study interventions that could, in principle, be implemented and hence we did everything possible to replicate the natural context of the fishermen in our laboratory experiment. This premise both constrained and liberated our thought process. On the one hand, it ruled out a number of potential interventions that functioned in simple laboratory environments but are difficult to implement in the field, such as the costly punishment of resource exploiters.[19] However, on the other hand, it also helped us focus on designing context-specific interventions rather than extending the existing body of laboratory research on common pool exploitation. Relatedly, we focused on testing the robustness of regrouping interventions instead of disentangling the different underlying mechanisms. This is why we tested common information constraints in the field instead of investigating the impact of regrouping per se.[20]

The regrouping of fishermen may occur in several ways in the upper part of the Gulf of Baja California. For example, in El Golfo, and in other fishing communities in Mexico (Méndez-Medina et al. 2020), a basic community enforcement system has been implemented where community members wait at the beach for the different fishing boats, to scan their hauls. These community members could be given authority to reward fishermen who work on the least exploitative fishing boats. As fishermen often work on different boats, this could provide some of them with an incentive to stay on boats where exploitative fishing is avoided and thus lead to regrouping. More generally, we observed positive impacts when symbolically rewarding fishermen in our experiments.

Our lab-in-the-field experiment incorporated the key fishing context with the goal that the findings can be generalized to the field. First, the experimental participant pool is the same as the target population (fishermen in this region). Second, we chose a framed common pool resource experiment, and it has been shown that fishermen's behavior in such experiments significantly correlates with actual fishing behavior (Fehr and Leibbrandt 2011). Third, there is related evidence of the function of symbolic awards for important field behavior (Ashraf, Bandiera, and

18. An alternative explanation is that there is some reduction in extractions in all groups because of an experimental Hawthorne effect.

19. Fear of retaliation constrains such forms of punishment, and informal discussions with fishermen in our sample clearly indicated their reluctance to use such punishment.

20. In our regrouping treatments, two changes were simultaneously introduced: regrouping and ranking. Thus, our experiment does not allow us to infer the impact of regrouping alone.

Jack 2014), including in a sample of fishermen (Cavalcanti and Leibbrandt 2015). This leads us to hope that a simple symbolic award (like special clothing with logos signaling sustainable fishing) for fishermen who decide to primarily work on the least exploitative fishing boats could mitigate overfishing. Such awards could also be eventually translated into more sophisticated certification schemes that could generate a premium price for sustainable fishing for those complying with the rules of sustainable fishing (Blomquist, Bartolino, and Waldo 2015; Wilen, Cancino, and Uchida 2012). We encourage future research using randomized interventions to test the extent to which such awards have the desired impact in similar field settings in which there is overexploitation and thus investigate the generalizability of our and related research on regrouping and awards.

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