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Evolving Entitlements: Intervening to Prevent a Collective Harm

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When market transactions generate negative externalities, the injured party may initiate court action to prevent harm or to obtain compensation. The political response, in some cases, has been to broaden the set of agents who can intervene through the court, often by admitting entirely new categories of potential intervenors. We employ an experimental market setting to investigate the effect of an increase in the number of potential intervenors (introduced as admitting an additional class of persons having the necessary standing in law). The results suggest that there will be a substantial increase in the number of actual interventions. The increase means that social resources expended on interventions will increase and there may be a consequent reduction in trading activity in the affected markets.

Key words: collective harm, court actions, experimental economics, public goods

Introduction

New Mexico's water law was recently amended. Under the 1982 and 1985 amendments to the existing water law, groups or individuals could approach the state engineer (the arbiter of water market transactions) under the "public welfare" clause to block water transfers (Dumars and Minnis). Before these amendments, those directly affected by a water transfer (for example, a water transaction which would lower the water table and expose a well pump) could approach the state engineer and ask for consideration. While the public welfare clause has not been clearly defined in New Mexico and is open to interpretation by the state engineer, it is currently interpreted as providing a potential avenue to intervene to protect environmental features such as riparian habitat or recreational use (Dumars and Minnis, p. 824). Under the public welfare clause, any group affected by a water transfer, other than those who were already entitled to approach the state engineer, can now intervene.

These changes to New Mexico's water law offer an example of more general cases in which market transactions generate negative externalities, with no immediate mechanism to facilitate the efficient exchanges between traders and injured parties. As in the New Mexico case, a number of social and political responses can emerge including regulations constraining behavior or a greater use of the courts to pursue compensation. In response to the claims of the injured parties, the legislature may define a set of expanded entitle-

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ments, beyond those conveyed through direct property interests, to a wider class of agents affected by the negative externality.

Expanded entitlements represent a new institutional arrangement that potentially alters the way existing markets operate. But who are the agents that participate in allocating society's resources under these new arrangements? In the example investigated in this article, a new class of agents that can intervene in a market transaction through legal process is added to the existing class of agents that have a direct property interest in the market. These new agents did not have an established legal claim before entitlements were expanded, whereas the existing, or original, agents have direct interest in the property rights involved in the transactions.

The use of court-ordered injunctions for externality problems reduces or eliminates a large part of the transaction costs associated with alternatives such as class actions (Cooter and Ulen). Under a regime of expanded entitlements, organizations such as the Sierra Club become empowered and can sue to preserve environmental amenities without having to identify other potential beneficiaries, in particular, beneficiaries with direct property interests. In addition, several intervenors may sue simultaneously, because agents are distinct (this is not a class action). Each intervention increases the probability of at least one successful intervention. However, any successful intervention confers a benefit to all who suffer the externality, so the outcome is a pure public good. Those who seek to restrict or prevent the original transaction benefit from successful intervention, but the outcome for social welfare is not necessarily positive since the original transactors lose potential gains from trading.

We investigate the boundary between market and collective decision making created by introducing a new group of intervenors to society's decision-making framework. Our investigation focuses on the impact of expanding the set of intervening agents on market transactions. We hypothesize that the result may be an increase in the level of interventions against market exchanges across a broad class of environmental goods. This article presents an empirical test of this hypothesis using experimentally generated data.

Since this institutional change is recent, especially in the environmental area, field data which would permit assessing the impacts of enlarging the set of potential intervenors on the volume of interventions undertaken are currently unavailable. To investigate the effects of increasing the pool of potential intervenors, an experimental market is constructed with the essential features of the institutions just described. The changes in individual and group behavior are observed as the institutional setting evolves toward a broader set of entitlements. In the experimental market a transaction between two traders imposes a negative externality on others, each of whom would benefit if this transaction is prevented. Secondary markets through which agents can absorb the external effects are unavailable, the only avenue available is legal. A single successful intervention is sufficient to block the transaction and is a public good. In the experimental market two distinct groups suffer the externality. Initially, only members of one group can intervene although the benefits of successful intervention are enjoyed by both groups. Members of the second group are subsequently permitted to intervene.

The experimental data show that increasing the number of potential intervenors, by admitting a new class of potential intervenors, increases the number of interventions. This increase comes from the newly empowered individuals who actively intervene while those originally able to intervene reduce their interventions. Interventions by the new group more than compensate for the reduced interventions of the original group. From a policy perspective, the empirical results suggest that expanding entitlements will lead to more interventions to prevent market transactions. Whether the interventions are successful or not, increasing their number raises the costs for traders in these markets and reduces the volume of transactions.

Theory and Hypotheses

To the representative individual, the result of intervening, or "going to court," is uncertain.[1] In deciding to intervene, an individual must weigh the certain costs of intervention against the expected gains. The payoff to the status quo is normalized to equal zero. A necessary condition for an individual to undertake intervention to prevent or limit an exchange is that the utility of the expected return from intervention is positive, that is,

$$(1) \qquad E(\Delta\text{Utility from intervention}) = U[p(G-C) + (1-p)(-C)] > 0,$$

where p is the probability of success of the intervention, G is the gain if successful, and C is the cost of intervening. If there is only one potential intervenor, the above expression provides the information needed to predict whether an intervention will occur.

When there are multiple potential intervenors, the individual payoff is still defined by (1), but the probability p is no longer the individual's probability of success but the probability that any (at least one) intervention is successful. Any one successful intervention is a public good, as all sufferers of the externality benefit from the action. Even though the individual is more likely to intervene because the value of (1) is higher in this case, the problem of free riding makes an individual's decision to intervene something that can only be investigated empirically. In the example of the amendments to the New Mexico water law, the amendments give a new class of agents the right to approach the state engineer. Previously, agents who were directly affected by the water transfer could intervene, and any successful intervention would confer some benefit on each affected agent. Entry of a new class of agents affects every agent's decision to intervene since the probability of at least one successful intervention is given by a binomial density and is a function of the number of intervenors.

The public good provision game being played by our intervenors is functionally similar to Palfrey and Rosenthal's participation problem in which individuals must choose whether or not to contribute a fixed amount to provide a public good, and the good will only be supplied if at least a specified fraction of the group contribute. In the present setting no explicit threshold of contributions is necessary for the public good to be supplied, but there is a best outcome for the group: the equilibrium level of interventions is defined as equating the marginal payoff to having another participant (change in expected value) to the cost of participation. If the game generates an interior solution, in the Nash equilibrium some individuals will be intervenors while others will not. Those who choose to intervene will not wish to withdraw their intervention nor will those not intervening wish to attempt an intervention. With the parameters chosen in our experimental setting, the efficient outcome is an interior solution since the probability of at least one successful intervention is strictly increasing in the number of interventions attempted, but the marginal probability is declining.

[1] We are not the first to investigate the effects of uncertainty on individual willingness to contribute to public goods. Suleiman and Rapoport, for example, have also investigated such settings. Our work differs from previous work in this area by focussing on the role of intergroup interactions and the effects of institutional evolution on individual decisions.

The participation game yields efficient outcomes in that the number of individuals who voluntarily participate in the provision of the public good is efficient. However, this prediction is contingent on the individual agents being risk neutral. With risk-averse individuals, the efficient outcome depends on the existence of a money-back provision whereby those that offer to contribute have their contributions returned in the event the collective good is not provided. Since the court costs are incurred "up front," there is no possibility of a money-back provision in the intervention setting. That is, even if an inefficient level of intervention occurs, the individuals cannot have their court fees returned. Furthermore, the outcome is stochastic so the failure to obtain a successful intervention does not necessarily imply that too few persons undertook to intervene. In this setting, risk-averse agents will underprovide the public good. Applying the participation game results to the current problem yields the following testable hypothesis:

> *Hypothesis 1*: When the members of the original group of potential intervenors are the only ones permitted to intervene, the level of interventions will be less than the participation game equilibrium.

Individual rationality implies that the newly recognized individuals will intervene if their expected gain exceeds the cost; that is, they will behave in the same fashion as the original group of potential intervenors. But the new agents are not the same as the original class of potential intervenors. For the new individuals, the costs of intervening exceed expected gains, but their interventions increase the probability of success—they can contribute to the provision of the public good. Motives are difficult to observe, but regardless of the actual motivation, the testable hypothesis is as follows:

> *Hypothesis 2*: When a new class is permitted to intervene, the members do intervene and the effect is an increase in the total number of interventions undertaken.

In the Nash equilibrium, the interventions undertaken by those in the new class will lead the original class members to reduce their level of intervention. However, when the probability of success for the members of the new group is lower, more interventions must be undertaken to maintain the previous probability of at least one success.

Experimental Design

The experiments reported here are designed to investigate the responses in individual behavior when the institutional setting changes. The experimental market captures the essential features of the institutional evolution and the affected markets.[2] In the instruc-

[2] The critical precepts of the experimental method as set out in (Smith) are nonsatiation, salience, dominance, privacy, and parallelism. Nonsatiation in the reward medium used in the experimental market is necessary for the subjects to base their decisions on the relative size of the payoffs. Salience of the rewards is necessary to motivate the subjects to incur the decision costs required to make good decisions in the experimental market. Dominance of the monetary payoffs is necessary for the experimenter to maintain control over the decision-making environment. That is, the experimental payoffs must dominate "any subjective costs or values associated with participation in the activities of an experiment" (p. 934). To strengthen dominance it may be necessary to establish privacy such that the subjects are given information only on their own payoff alternatives. Thus, the issue of fairness may be avoided when these are not germane to the experimental setting. Parallelism refers to the condition that the decision environment in the experimental market mimic, as much as possible, the essential features of the naturally occurring setting.

Table 1. Experimental Design: Number of Agents

Sessions	No. of Class A Agents	Pr(*Success*)	No. of Class B Agents	Pr(*Success*)
S1	5	0.5	—	—
S2	5	0.5	—	—
S2.1	5	0.5	—	—
S3	5	0.5	5	0.1
S3.1	5	0.5	4	0.1
S3.2	5	0.5	5	0.1
S4	5	0.5	9	0.3
S4.1	5	0.5	10	0.3
S4.2	5	0.5	10	0.3

Note: An experiment session indicates an independent running of the design.

tions given to subjects, they are told that a trade of some good (tokens) is about to take place between two agents (these agents are not actually participating in the experiment and are not present in the laboratory) and that this exchange will impose a loss on themselves and others.[3] They know what this individual loss will be and they are offered the right to intervene to obtain an injunctive remedy. Subjects may choose to intervene without knowing the choices taken or outcomes realized by the other subjects. Success from intervening is uncertain. The probability that each individual intervention will be successful is announced in advance, but the group's success probability is not announced, nor is it known by the subjects or the experimenter in advance.

There are two distinct classes of potential intervenors in the experimental setting. The first class is referred to as "Class A," these are the persons deemed to have a direct property interest in the marketplace transaction. For example, these agents may draw water from the same system in which some transaction is about to take place. The second class of potential intervenors is referred to as "Class B," these are deemed to have no direct property interest. For example, these agents may derive employment from recreation on water courses in the system or may want to protect an endangered riparian habitat on a river affected by the proposed transaction. Members of Class A historically suffer more of the negative impacts of the proposed exchange. They are presumed to have a higher standing in court which is reflected in a higher probability of a successful intervention. In New Mexico, a state where diversion defines the beneficial use of water, an agent diverting water downstream from a proposed transaction will have higher standing than an agent claiming losses due to insufficient instream flows. Members of Class B do not have a direct property interest in the transfer. They are presumed to have a lower standing in court than Class A agents which translates as a lower probability of success in intervening. As the experiment begins, the potential intervenors consist of only Class A agents. Later, the sequence of institutional settings evolves by expanding the group of potential intervenors to include agents of Class B.

The experimental design is presented in tables 1 and 2. The subjects participate in

[3] The traders are simulated in the experimental markets since the behavior being investigated is that of the potential intervenors. Complete experimental instructions are available from the authors.

Table 2. Experimental Design: Structure of Sessions

Rounds	S1	S2	S2.1	S3	S3.1	S3.2	S4	S4.1	S4.2
	(number of rounds each treatment)								
A-Private	6	6	4	5	4	3	4	3	3
A-Public	5	5	5	—	—	—	—	—	—
A-Public (B agents present)	—	—	—	5	5	4	5	4	4
AB-Public	—	—	—	4	4	5	5	5	5

Note: Rounds are described by the type of agent able to intervene (A or B) and the nature of the intervention (a private or a public good).

only one session. Each session consists of rounds, over which institutional settings may change. The sequence of institutional settings corresponds to the stages of the institutional evolution as new groups are given intervention rights. Subjects are informed that each new institution will be in place for an unknown number of rounds. All sessions begin with rounds in which the intervention is a private good (A-Private rounds) in order to demonstrate to subjects the role of market trades, the nature of an intervention, and mechanism of intervening. Following this are rounds in which intervention is a public good (A-Public and AB-Public rounds in the tables). Of the nine sessions that constitute this experiment, three (S1, S2, and S2.1) have only Class A agents. The other six sessions have both classes of agents in the laboratory. As the public good rounds progress, the institution evolves to allow the Class B agents to intervene (the AB-Public rounds). Thus, each session is made up of a series of rounds that evolve through three institutional settings. The number of rounds in each session and how many rounds are allocated to each institutional setting are given in table 2. This design enables us to examine how subject behavior changes as the number and type of potential intervenors is increased.

The decision parameters differ between the classes of agents in the experiment. Class B agents have a lower cost of intervening as well as a lower probability of success. The lower cost of intervening represents a lower technical requirement to intervene (for instance, in the water example this would arise because this class of intervenor would not have to undertake hydrological surveys). The lower probability of success represents the agents' lower standing in law. All information is common knowledge in these experiments. That is, the losses, the cost of an intervention attempt, and the individual probability of success are known to all agents. Each class of agent is informed of the other's parameters and told that this is common knowledge. Since the subjects decide whether to intervene without knowing the decisions of the others, they are playing a game of complete but imperfect information (as specified in Palfrey and Rosenthal).

In all sessions there are five Class A agents. The parameters in sessions S3, S3.1, and S3.2 are constructed to simulate a "strict" version of the court's behavior in granting potential intervenors standing in law. Only four or five Class B agents are admitted and their probability of success on intervention is set at a fairly low 0.1. Sessions S4, S4.1, and S4.2 represent a more "lenient" version of the court's behavior. Here nine or ten Class B agents are admitted and their probability of success is set at 0.3.[4]

[4] These experiments are designed to investigate a specific policy change rather than to test a theoretical proposition. For this reason, the experimental design deliberately does not address a situation in which there are a small number of Class B agents with a low probability of success (and vice versa). A liberal stance toward the new class (B) on the part of the court would imply high probability of success *and* a large set of agents.

A round of the experiment begins as the subject is told, through an information slip, of the number of tokens (experimental currency) the traders intend to exchange and the loss the subject will suffer if the exchange is not prevented. If intervention is allowed, the subject indicates whether he or she wishes to intervene, without knowing whether any other agent has purchased an intervention attempt. Having chosen to intervene, the subject is handed a ten-sided die to roll.[5] One or more successful interventions prevents the exchange. The subject's earnings after each round are calculated as follows:

$$Earnings = Endowment - I(Cost\ of\ Intervention) - (1-S)(Loss),$$

where $I = 1$ if agent intervened, and $S = 1$ if any intervention was successful. A record sheet allows each subject to keep track of each round's actions and earnings throughout the session.

These are hand-run experiments and all subjects are recruited from principles of economics classes.[6] Participation is voluntary and subjects earn money, not extra course credits. The subjects have no prior experience in the laboratory setting being investigated. The sessions last between 1.5 and 2 hours and the subjects earn average payoffs of $25.

Empirical Evaluation of Intervention Behavior

The efficient level in the participation game is defined as the number of interventions for which the marginal gain to the group just exceeds the cost of intervening. Where the outcome of a successful intervention is a public good, each intervention increases the probability of at least one successful intervention. Because of this, interventions will be undertaken up to the point where the expected gain (to the group) just covers the cost of intervention. The game's equilibrium is economically efficient. The equilibrium number of interventions in a round is found as the solution to:

$$\max(n)\ \{\mathrm{Pr}_n(success)\cdot loss\cdot N > Cost\},$$

where the objective is maximized over integer values of n, the Pr_n function is the probability distribution function, *loss* is measured at the individual level, as are *Costs* and N is the size of the group affected by the public good. Interventions, counted by the index n, will continue as long as the difference between the change in the expected group gain (loss avoided) and the cost of another intervention is positive. For each individual in each round, the *Cost* is fixed, as is N; the *loss* is known and the probability of success can be (theoretically) calculated from available information. Group losses, ($loss\cdot N$), are determined by the total number in both classes of agents since benefits from intervention accrue to all agents, regardless of whether they can intervene or not.

In the rounds in which only Class A agents can intervene, but intervention is a public good (labeled rounds 6–10), each A agent has a probability of success of 0.5 and the

[5] The intervention attempt is generated by rolling a die and the outcome is "success" or "failure" depending on the roll of the die. This is to simulate the randomness associated with "going to court" or intervening to prevent a transaction. The decision to purchase a roll of the die and the outcome of an individual roll are private—there is no means for the subjects to identify which persons (if any) purchase the roll of the die.

[6] The student body at UNM is more heterogeneous than the average student body, with a mean age of 27 years, a high proportion of part-time, nontraditional, returning, and older-age students, as well as more minorities. This tends to reflect more the heterogeneity observed in the "real world." The estimated behavioral models suggest that student subjects are responding systematically to the experimental parameters, which is evidence against the idea that students are "just playing a game."

Table 3. Marginal Group Success Probabilities for Public Good Rounds

	A-Public Rounds	AB-Public Rounds	
Intervenor	Only Class A Can Intervene	Class A Intervenes	Class B Intervenes
1	0.500	0.500	0.500
2	0.250	0.250	0.250
3	0.125	0.125	0.125
4	0.062	0.062	0.062
5	0.031	0.031	0.031
6		0.003	0.009
7		0.003	0.007
8		0.002	0.004
9		0.002	0.003
10		0.002	0.003
11			0.001
12			0.001
13			0.001
14			0.001
15			
Group size (N)	5	5, 5	5, 10
Pr(*success*)	0.5	0.5, 0.1	0.5, 0.3

marginal probabilities of at least one successful intervention, $\Delta Pr_n(success)$, are found from the binomial probability distribution. These marginal probabilities are shown in table 3.

When Class B agents can also intervene (rounds 11–15), the equilibrium number of interventions includes both classes of agents. The individual intervention strategy when the outcome is a public good and both agent classes can intervene is described in the following example. Consider the situation in which either type of agent can be the first intervenor. The marginal success probability for the A agent is 0.5, while it is 0.3 for the B agent. From the group's perspective, an intervention by the A agent will maximize the increase in the probability of a successful intervention and maximize the social surplus.[7] If the second intervenor is of Class A, the probability of a successful intervention increases by 0.25 compared to only 0.15 if the second intervenor is of Class B. In order to maximize the increase in success probability for each successive intervenor, all five Class A agents should precede any of the Class B intervenors. Beyond five, there are no more Class A agents, so further interventions must be taken up by others. As the last two columns in table 3 show, the marginal probability of success drops dramatically after all the Class A agents have intervened.[8]

[7] The surplus is defined as the difference: $\{\Delta Pr_n(success) \cdot loss \cdot N - Cost\}$. The surplus generated by a Class A intervenor is \$6.25 − \$1.00 = \$5.25. The surplus generated by a Class B intervenor (for parameters in the 5 A, 5 B rounds) is \$1.88 − \$0.50 = \$1.38.

[8] The marginal success probabilities are calculated as the change in the multinomial probability of at least one success, given that the previous interventions were undertaken by Class A agents up to five interventions and by Class B agents thereafter.

Table 4. Critical Marginal Probability Values Used to Determine Efficient Number of Interventions

Group Size	Class A Agent Loss	Class B Agent Loss	Critical ΔPr_n
5	$2.50	$1.25	0.080
	3.00	1.50	0.067
	3.50	1.75	0.057
9	2.50	1.25	0.057
	3.00	1.50	0.044
	3.50	1.75	0.038
10	2.50	1.25	0.053
	3.00	1.50	0.044
	3.50	1.75	0.038
14	2.50	1.25	0.042
	3.00	1.50	0.035
	3.50	1.75	0.030
15	2.50	1.25	0.040
	3.00	1.50	0.033
	3.50	1.75	0.029

Note: Each critical value is calculated by solving $\Delta Pr_n = Cost/(loss \cdot N)$.

The participation game equilibrium number of interventions is determined by the number of potential intervenors, the loss to be avoided (the gain from a successful intervention), the cost of intervention, and the marginal probability of successful intervention. Values of the marginal probabilities that equate the marginal expected gain from intervention with the cost of intervention are given for each experimental scenario in table 4. These critical values, along with the data reported in table 3, can be used to determine the Nash-equilibrium number of interventions for each round.

Table 5 presents the equilibrium and observed level of interventions in each round and the interventions by both classes of agents, as well as an indication of when the level of interventions is the participation game equilibrium. To evaluate Hypothesis 1 that Class A (those with a direct property interest) participation will be less than expected when intervention is a public good, we focus on the intervention behavior in the A-Public rounds (six through 10). There are 27 such rounds over the sessions 3 through 4.2. The level of interventions is the predicted equilibrium in eight of these rounds, above the predicted equilibrium in eight rounds, and below the predicted equilibrium in the other 11 rounds. The frequency of below equilibrium outcomes is 0.41. Although the result is not strong, the data cannot reject Hypothesis 1: when the original group of potential intervenors are the only ones permitted to intervene, the level of interventions will be less than the participation game equilibrium. By way of comparison with other findings, Simmons et al. and von de Kragt, Orbell, and Dawes find that the level of undercontributing is 0.45 in the treatments most similar to those reported here. There is evidence, however, that the subjects are playing the participation game. In the earlier sessions S1 through S2.1, there are 29 relevant rounds and in 18 of these the level of intervention is below the equilibrium (frequency, *f*, is 0.62). The overall payoff to intervention is higher in sessions S3 through S4.2, since there are more agents present (Class B agents

Table 5. Actual and Efficient Interventions of Experimental Sessions by Round

	Round														
Interventions	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Session 1															
Efficient	5,5[a]	5,5	5,5	5,5	5,5	3,3	3,3	3,3	3,3	3,3					
Class A	0,2	1,0	1,1	4,2	1,1	1,2	1,**3**	1,1	0,1	2,0					
Session 2															
Efficient	5,5	5,5	5,5	5,5	5,5	3,3	4,4	3,3	3,3	3,3					
Class A	4,2	4,**5**	**5**,4	3,3	4,4	2,**3**	2,1	**3,3**	2,4	2,4					
Session 2.1															
Efficient	5,5	5,5	5,5	5,5		3,3	4,4	3,3	3,3	3,3					
Class *A*	4,**5**	4,4	4,4	4,4		4,**3**	**4**,3	2,1	2,5	2,**3**					
Session 3															
Efficient	5	5	5	5	5	3	4	3	3	3	4	4	4	4	
Class A	3	5	4	5	4	4	3	2	1	3	1	1	1	3	
Class B						0	0	0	0	0	1	2	2	4	
Total	3	**5**	4	**5**	4	4	3	2	1	**3**	2	3	3	7	
Session 3.1															
Efficient	5	5	5	5		3	4	3	3	3	4	4	4	4	
Class A	4	5	2	0		4	4	3	2	1	3	3	2	3	
Class B						0	0	0	0	0	1	1	1	0	
Total	4	**5**	2	0		4	**4**	**3**	2	1	**4**	**4**	3	3	
Session 3.2															
Efficient	5	5	5			3	4	3	3		4	4	4	4	4
Class A	5	3	4			4	2	4	2		3	3	2	3	3
Class B						0	0	0	0		2	1	1	2	0
Total	**5**	3	4			4	2	4	2		5	**4**	3	5	3
Session 4															
Efficient	5	5	5	5		3	4	3	3	3	4	4	4	5	4
Class A	5	5	3	3		2	3	4	3	3	1	2	1	0	2
Class B						0	0	0	0	0	4	5	4	1	4
Total	**5**	**5**	3	3		2	3	4	**3**	**3**	5	7	5	1	6
Session 4.1															
Efficient	5	5	5			3	4	3	3		4	4	4	5	4
Class A	4	4	3			5	4	4	4		4	4	4	4	4
Class B						0	0	0	0		5	4	2	4	3
Total	4	4	3			5	**4**	4	4		9	8	6	8	7
Session 4.2															
Efficient	5	5	5			3	4	4	3		4	4	4	5	4
Class A	5	4	5			3	3	1	3		2	3	4	3	2
Class B						0	0	0	0		2	1	2	2	2
Total	**5**	4	**5**			**3**	3	1	**3**		**4**	**4**	6	**5**	**4**

Note: Numbers in bold indicate that the number of interventions was the participation game equilibrium.
[a] Double entries for Sessions 1, 2, and 2.1 indicate that there were two A groups in each round, acting separately in contributing to the public good. Class Bs are not present in any of these sessions.

are included in the calculation of total losses). The subjects appear to respond by intervening more.

To evaluate Hypothesis 2, that a new group of intervenors will increase the total number of interventions, consider the participation behavior in the AB-Public rounds (11 through 15) when both classes of agents are allowed to intervene. In only eight of the 28 rounds is the total number of interventions less than the equilibrium level (f is 0.29). Sessions 4 through 4.2 are particularly striking since in only one of 15 rounds is the number of interventions below the predicted equilibrium.

As discussed above, given the parameters in the experiments, it is not efficient for Class B agents to intervene in any round. Yet in 26 of the 28 rounds they intervene when they can. During rounds six through 10, in which only the original class (A) can contribute to the public good, the intervention rate is efficient in 15 of 57 rounds (f is 0.26). During rounds 11 through 15, when both classes can intervene, Class A agents only intervene efficiently in five of the 28 rounds (f is 0.18), and in none of these rounds is the total level of intervention efficient.[9] When Class B agents intervene, the Class A agents respond by reducing their interventions. Whether Class B agents are motivated by altruism or decreasing absolute risk aversion cannot be determined within the experiment.

The evidence of undercontribution (and, hence, failure to reject Hypothesis 1) when intervention is a public good is strong, since, in over half the rounds, the number of interventions is less than the efficient number. During the A-Public rounds (six through 10), some Class A agents fail to participate in 30 of the 57 rounds ($f = 0.53$), and during the AB-Public rounds (11 through 15) this proportion rises to 0.82. It must be noted however, that in only 12 of the 52 rounds ($f = 0.23$) when intervention is a private good and in the best interests of the individual, do all individuals intervene—in all other rounds some individuals chose not to intervene. Intervention rates, relative to the efficient level, are lower in the A-Private good rounds than the A-Public good rounds, but free riding is strongest in the AB-Public rounds.

A much richer investigation is possible using econometric methods. The simplest specification of the model of actual interventions is that the actual number of interventions is equal to the efficient number, that is, $I = \alpha I^* + \epsilon$, where ϵ is a random error term. The null hypothesis is H_0: $\alpha = 1$. The estimate of α is 0.699 with a standard error of 0.030, hence the null hypothesis is rejected in favor of an alternative of undercontribution. This simple specification provides a direct test of Hypothesis 1, but it only explains 11.6% of the variation in the dependent variable. A more detailed "reduced-form" specification that accounts for the institutional features that change between rounds as well as the heterogeneity of the potential intervenors (Class A and B) should perform better in a statistical sense.

During the private rounds, the variables that determine the efficient number of interventions are the probability of a success, the loss incurred, and the cost of intervention. During the public rounds the number of potential intervenors becomes important, as does the class composition of the group. Table 6 reports the estimated regression models that include these variables. Dummy variables are included to indicate the public rounds (A-Public and AB-Public), as well as an interaction term for Class B agents in the AB-Public rounds.

Included in the specification of the model is a variable indicating no successful interventions in the previous round. This variable is included as a control for an aspect of the experiments that may be influential. Even though in practice agents would not un-

[9] In only two of these rounds is the number of Class A interventions zero (session 4, round 14 and session 3.1, round 4). The total number of interventions falls short of the equilibrium number of interventions in both cases.

Table 6. Regression Models of Group Intervention Behavior

Variable	Model I (OLS)	Model 2 (FGLS)	Model 3 (DVLS)	Mean
Constant	−1.201	−0.225		
	(−2.86)	(−0.51)		
Loss	1.108	0.760	0.220	2.456
	(5.47)	(3.46)	(0.86)	
Pr(*Success*)	3.120	3.774	5.500	0.378
	(4.07)	(4.60)	(6.19)	
Group size (N)	0.085	0.064		7.691
	(2.85)	(2.25)		
Rounds 6–10	−1.501	−1.704	−0.862	0.433
	(−4.43)	(−5.46)	(−4.41)	
Rounds 11–15	−2.158	−2.194	−1.397	0.289
	(−4.73)	(−5.03)	(−5.16)	
Class B in rounds 11–15	2.523	2.125	2.051	0.144
	(6.85)	(5.93)	(5.90)	
No success last round	−0.325	−0.193	−0.195	0.129
	(−1.29)	(−0.81)	(−0.84)	
Adj. R^2	0.540	0.515	0.610	
Log likelihood	−288.7	−278.9	−267.2	−367.6

Note: Dependent variable is the number of interventions per round. The mean is 2.402. Figures in parentheses are t-statistics for the test of H_o: $\beta_i = 0$. Sample size is 194 for all regressions. OLS is ordinary least squares; FGLS is feasible generalized least squares; DVLS is dummy variable least squares.

dertake repeated rounds of intervening, in the experiments subjects intervene over a sequence of repeated rounds. Intervention behavior may, therefore, be influenced by the consequences of previous behavior. The simplest form of intertemporal response would be that a failure of the group to successfully intervene may cause more than the efficient number of people to contribute to interventions in the following round. The estimated coefficients in all models for this variable are not statistically different from zero, providing evidence that this is not a serious problem in the experiments, and that subjects are able to treat each round as an independent optimization problem.

Three distinct econometric models are estimated and reported in table 6. Because the data are generated by different groups of subjects it is possible that heteroskedasticity is introduced by group effects and that the group effects may not be random, as is assumed in the ordinary least squares (OLS) regression specification. Two alternative methods for modeling groupwise heteroskedasticity are presented here. The first is a feasible generalized least squares (GLS) estimator, which is a maximum-likelihood estimator obtained using an iterative procedure detailed in Greene. A test of the hypothesis of homoskedasticity, based on the estimates from this model, indicates the presence of groupwise heteroskedasticity.[10] The second method of testing for and correcting heteroskedasticity is to use methods developed to analyze panel data. Using this strategy, the possible group effects can be modeled as fixed effects or random effects. Treating each group effect as an independent shift factor realizes the dummy variable least squares (DVLS) model. In

[10] The test statistic, described in Greene (p. 396), is distributed χ^2 with 11 degrees of freedom. The calculated value of 29.9 has a probability value of 0.002, hence the null hypothesis of homoskedasticity is rejected in favor of groupwise heteroskedasticity.

a random effects specification, the group effects are treated as if they are drawn randomly from a distribution of effects. A Hausman test for the difference between the fixed- and random-effects models finds no difference, and as a consequence, only the fixed-effects model is reported in table 6 (Greene, p. 479). The fixed-effects model has a higher value for the log-likelihood function than both the OLS and the GLS estimators. The fixed-effects (DVLS) model can be used to test the hypothesis that the group effects are the same, as well as testing for the presence of group effects over a model specified with the exogenous variables only. The hypothesis test for no group effects is rejected as is the hypothesis that the group effects are the same.[11]

Nearly all coefficients have the same sign and magnitude across the alternative specifications reported in table 6. In addition, all variables are statistically significant except for the past round, lack-of-success variable in all models and the *loss* variable in the fixed-effects model. Coefficient estimates indicate that an increase in the expected gain from intervention (a function of the loss and the probability of success) increases interventions. Increased group size increases interventions even though the effect is relatively small. Variables controlling for private or public rounds and for subject heterogeneity are statistically significant and of special interest as these are the "unknown" parameters of the model, about which we have no expectations.

Class A agents lower their intervention rate significantly in the public rounds compared with the private rounds. But the efficient number of interventions is lower in the public rounds. The average efficient number of private round interventions is five, during the A-Public rounds it is 3.2 and during the AB-Public rounds it is 4.1. Efficient interventions in the A-Public rounds should be 1.8 lower, on average, than for the private rounds. The estimated reduction in interventions during these rounds is 1.7 from the FGLS model, 1.5 from the OLS model, and 0.86 from the fixed-effects model. It appears that this variable is capturing much of the public good effect of a successful intervention. These estimates, especially from the fixed-effects model, support the result identified in the previous section that there is a slight overintervention in the public good rounds compared with the other rounds.

In the AB-Public rounds, Class A agents reduce their interventions by an even greater amount. This represents substantial underparticipation because the average efficient number of interventions during these rounds is 4.1, higher than the A-Public rounds. At the same time, Class B agents have a statistically significant participation rate, controlling for the other factors, of over two interventions per round. The efficient number of Class B interventions given the parameters of the experiments is zero. The hypothesis that the coefficient on the Class B dummy variable is zero is rejected at all conventional significance levels. Class B agents respond to the opportunity of intervening by doing so at an average rate of 2.25 interventions per round when they should not intervene at all.[12] Interventions per round average 4.8, higher than the average efficient number of interventions. The high level of Class B interventions is unexpected and cannot be explained within the design of these experiments. More research may reveal the motives of Class B agents, perhaps through the use of postsession debriefing questions or focus group

[11] The F-test of the hypothesis that the group effects are zero obtains a value of 3.94 and the test that all group effects are the same obtains a value of 2.75. Both test are distributed $F_{11,174}$ with a critical 95% value of approximately 1.90 (Greene, p. 468).

[12] The average number of Class B agent interventions from table 4 is 2.25. The coefficient on the Class B variable in the regression model is adjusted for the other factors that determine the efficient number of interventions.

discussions. Regardless of the motives, the entire group's surplus would be maximized by allowing only Class A to intervene, rather than relying on Class B to intervene.

Conclusions

Recent regulatory changes have expanded the potential number and variety of groups and individuals who have some claim to standing in disputes over the use of resources in our economy. An example is the recent change to New Mexico's water law that allows third parties to intervene with the state engineer under the public welfare clause. Since field data are unavailable, the experimental results reported here suggest that extending the right to intervene to prevent a market transaction to an additional group will increase the number of interventions.

The evolution of regulation described here has implications for emerging markets in environmental amenities. Increases in the number of interventions will (however marginally) decrease the number of trades in the market, since traders will have to preempt intervenors, or incur costs to defend the trade against the arguments of the intervenors, regardless of whether the interventions are successful. The welfare implications depend on the relative weights given to producing interests and preservation interests.

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