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Local Environmental Control and Institutional Crowding-Out

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Summary. — Regulations that are designed to improve social welfare typically begin with the premise that individuals are purely self-interested. Experimental evidence shows, however, that individuals do not typically behave this way; instead, they tend to strike a balance between self and group interests. From experiments performed in rural Colombia, we found that a regulatory solution for an environmental dilemma that standard theory predicts would improve social welfare clearly did not. This occurred because individuals confronted with the regulation began to exhibit less other-regarding behavior and made choices that were more self-interested; that is, the regulation appeared to crowd out other-regarding behavior.

Key words — institutional crowding-out, external regulation, local environmental quality, experiments, South America, Colombia

1. INTRODUCTION

Economic institutions are designed to alter behavior, to stimulate actions intended to produce outcomes that are socially superior to those expected to flow from self-regarding individual choices. A small empirical literature suggests, however, that institutions designed to induce Pareto-superior outcomes may affect individual choices in surprising and contrary ways. In this paper we present results from a series of experiments designed to study the effects of external regulatory control of local environmental quality. We find that subjects made themselves worse-off when they faced a modestly enforced government-imposed regulation that standard theory would predict to be welfare-improving. The reason for this

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mystifying result appears to be that when subjects were confronted with a regulatory constraint on their behavior, they tended, on average, toward purely self-interested behavior (that is, toward pure Nash strategies), while in the absence of regulatory control their choices were significantly more group-oriented.

We are by no means the first to suggest that institutions designed and expected to do good might actually engender contrary behavior. A number of authors have suggested that paying a monetary reward to motivate socially desirable behavior may actually do the opposite because it may crowd out an individual's sense of public-spiritedness. Titmus (1971) suggested that individuals donate blood more willingly when they do so purely voluntarily than when they are offered money for their donations. [1] In the environmental arena, Frey and Oberholzer-Gee (1997) found that Swiss residents were willing to accept nuclear waste disposal in their community purely out of a sense of public spiritedness about twice as frequently as when they were offered compensation for accepting the negative externality. Kunreuther and Easterling (1990) found a similar phenomenon in Nevada; raising tax rebates failed to engender support for siting a nuclear waste facility at Yucca Mountain near Las Vegas.

Evidence that regulatory institutions may crowd out public motivations in favor of greater self-interest is not limited to the effects of monetary incentives. Ostman (1998) suggests that external control of common pool resources may have a negative effect by shifting responsibility to the regulatory agency and essentially absolving individuals from other-regarding moral obligations. Frolich and Oppenheimer (1998) designed a series of experiments to operationalize Rawls' (1971) "veil of ignorance" in the context of private contributions to a pure public good. In addition to a standard contribution game, subjects played a game in which their payoffs resulted from a random reassignment of individual payoffs. This veil over the link between individual choices and payoffs forced subjects to consider the consequences of their choices on the payoffs of the rest of their group. Indeed, the game was designed so that randomizing the assignment of payoffs generated a dominant strategy to contribute the efficient amount to the public good. As predicted, subjects did contribute significantly more to the public good than under the standard treatment. More important, however, they also found strong evidence that the institution of random payoffs shifted individual motivations toward greater self-interest despite the fact that the institution was designed to force a stronger group-orientation. Put simply, the institution served its intended purpose, but it seemed to crowd out other-regarding preferences. [2]

We are interested in examining the effects of external institutions (rules and regulations imposed from outside a community) on behavior in an experimental setting, in particular the effects of external control of environmental quality in rural settings of the developing world. Our design has a number of features, which combine to make it rather unique. (The details of our experimental design are provided in Section 2.) First, rather than conducting experiments in a laboratory setting, our experiments are conducted in three rural villages of Colombia, South America. Second, we consciously designed our experiments to approximate an environmental quality problem that rural villagers in developing countries are likely to face. Specifically, subjects were asked to decide how much time they would spend collecting firewood from a surrounding forest, while realizing that this activity has an adverse effect on local water quality because of soil erosion. Third, we confront a subset of subjects with a government-imposed quota on the amount of time that can be spent collecting firewood. The quota is only modestly enforced, which is typical of command-and-control environmental policies that rural villagers in the developing world actually face. Despite the weak enforcement of the quota, standard economic theory predicts that the external control will produce more efficient choices.

We consider two treatments to examine whether external control of local environmental quality may crowd out group-oriented behavior. Each group of subjects plays a number of initial rounds of the game without regulation and without being able to communicate with each other. A subset of groups go on to play additional rounds in which they are confronted with the government-imposed regulation. The other groups also play additional rounds, but instead of facing an external regulation, individuals are allowed to communicate with others in their group between rounds.

Allowing some groups to communicate was motivated by the fact that local cooperative efforts are frequently the alternative to external regulation in developing countries. Moreover, by the fact that relatively more is known about the role of face-to-face communication in enhancing levels of cooperation in experiments of this general type. Much of the literature on this subject is summarized by Ostrom, Gardner and Walker (1994), and Ledyard (1995). In brief, the findings show that communication enhances the likelihood of individuals shifting from relatively self-interested decisions to more group-oriented ones.

In Section 3 we report our results. Consistent with findings of the experimental literature on contributions to public goods and exploitation of common properties (Ledyard, 1995; Ostrom, 1998), we find that when subjects do not face external restrictions and cannot communicate with each other, their decisions tend to be neither pure Nash strategies nor efficient choices, but somewhere between these extremes. Absent regulation, the simple ability to communicate allows individuals to make more efficient choices. But, our results about the effects of external regulation are new—surprisingly, regulatory control caused subjects to tend, on average, to make choices that were closer to their pure Nash strategies. Consequently, average individual payoffs were lower than in the absence of regulation, and much lower than the payoffs of those subjects who were simply allowed to communicate with each other, in spite of the fact that the regulatory institution was designed to induce more efficient choices.

Institutional crowding-out suggests that well-intentioned but modestly enforced government controls of local environmental quality and natural resource use may perform rather poorly, especially as compared to informal local management. In Section 4 we discuss this and other implications of the crowding-out effect, as well as suggest ways in which this line of research should be extended.

2. EXPERIMENTAL DESIGN

As noted in the introduction, we designed our experiments to confront our subjects with a social dilemma concerning environmental quality, the structure of which would closely mimic their actual experiences. Toward that end, our field experiments were undertaken during the summer of 1998 in three areas in Colombia. The specific locations were chosen because they each have predominantly rural populations with significant interests in local natural resources and environmental quality. In addition, payoffs for the game were generated from a model of individual efforts to collect firewood from local forests. Private and social interests diverge in the model because we assumed that higher levels of firewood extraction would heighten soil erosion and ultimately damage local water quality. We consciously framed our experiment so that the subjects were fully aware that they were playing a game with this specific relationship between firewood extraction and water quality in place.

(a) *The subjects*

In the Colombian village of Encino, located in the eastern Andean region, residents enter local tropical cloud forests to extract firewood, log timber on a small scale, and to hunt. Like all of the sites we visited, water for consumption and irrigation comes nearly untreated from local rivers. Of the three areas that we visited, the relationship between forest cover and water quality is most critical in Encino, and the residents of this village are acutely aware of the problem. Water quality degradation caused by forest cover losses is less severe in the villages of Circasia and Filandia in the Quindio coffee region in the mid-Andes; it is nevertheless a significant problem. In Quindio, subjects for our experiments were drawn specifically from a group of families whose livelihood is related to the extraction and processing of natural fibers from local forests. As in Encino, water is drawn from local rivers and residents are aware that extracting forest products can lead to lower water quality. In Nuqui, located on the Pacific coast, villagers harvest coastal mangroves for firewood and other wood products, but their water comes from further inland; hence, they do not experience a direct link between their exploitation of local sources of wood and water quality. They face a similar dilemma, however, because their exploitation of the mangroves for wood adversely affects coastal fish populations upon which they also depend.

To sum up, the population from which the subjects for these experiments were drawn consists of rural households that live in areas that depend heavily on local forests for wood products. In each location, exploitation of local forests affects another aspect of their livelihoods adversely: water quality in Encino and Quindio, and fish populations in Nuqui. Hence, the subjects face social dilemmas in their daily lives that are similar to the one we confront them with in the experiments. In each of the three settings, the participants generally knew each other well, having lived in the same village for most of their lives. Schooling, age and income levels varied significantly for the participants within each group. Most participants had fewer than six years of schooling, roughly half were between 30 and 50 years old, and all were 16 or older.

(b) *Payoffs*

The payoffs for our experiments were generated by a simple model of a fixed number of homogeneous individuals that exploit a local forest for firewood. In each round of the games, each individual is given an endowment of time e that can be allocated to collecting firewood or to providing labor to an unrelated market. Let x_i denote the amount of time individual i spends collecting firewood from the common, and let w denote the prevailing wage for labor. Then, i's decision to provide $(e - x_i)$ units of labor to the formal sector yields a payoff of $w \times (e - x_i)$. Time spent collecting firewood from the forest yields a private benefit, which we assume takes the quadratic form $g(x_i) = \gamma x_i - \phi(x_i)^2/2$, where γ and ϕ are strictly positive and are chosen in part to guarantee $g(x_i) > 0$, for $x_i \in [1, e]$. The strict concavity of $g(x_i)$ indicates diminishing marginal private returns to time spent collecting firewood.

Subjects were told explicitly that their decision to spend time extracting firewood would affect water quality in the area adversely. We assumed that water quality q is a quadratic function of the aggregate amount of time individuals in the community spend collecting firewood; specifically, $q(\sum x_j) = q^0 - \beta(\sum x_j)^2/2$, where $\beta > 0$, and q^0 is interpreted to be water quality in the absence of firewood extraction. Again these parameters are chosen in part to guarantee $q(\sum x_j) > 0$ for all feasible $(\sum x_j)$. An individual's valuation of water quality is $f(\sum x_j) = \alpha q(\sum x_j)$, where α is another positive constant.

Define $u(x_i, \sum x_j)$ to be the sum of the sources of utility for an individual exploiter of the local forest. Parameters were chosen, in part, to guarantee that $u(x_i, \sum x_j) > 0$ for all possible x_i and $\sum x_j$. To facilitate scaling individual payoffs, we take an individual's payoff function to be a positive, monotonic transformation F of u. In particular, $F(u) = (\mu^\eta/\delta)(u)^\eta$, where μ, δ, and η are all positive constants. An individual's payoff function is then

$$\begin{aligned} U\left(x_i, \sum x_i\right) &= (\mu^\eta/\delta)\left[f\left(\sum x_j\right) + g(x_i) + w(e - x_i)\right]^\eta \\ &= (\mu^\eta/\delta)\left[\alpha\left(q^0 - \beta\left(\sum x_j\right)^2 \Big/ 2\right)\right. \\ &\quad \left. + (\gamma x_i - \phi(x_i)^2/2) + w(e - x_i)\right]^\eta. \end{aligned} \tag{1}$$

Each group consisted of $n = 8$ subjects, and each subject was allocated $e = 8$ units of time in each round. Pre-testing of the experimental designs at the Humboldt Institute for Biodiversity in Villa de Leyva, Colombia, led us to denominate units of time as months per year. Scale concerns led us to choose the following remaining parameter values: $w = 30$; $\gamma = 97.2$; $\phi = 3.2$; $q^0 = 1372.8$; $\beta = 1$; $\alpha = 1$; $\mu = 2$; $\delta = 16{,}810$ and $\eta = 2$. Individual payoffs were therefore calculated from the payoff function

$$\begin{aligned} U\left(x_i, \sum x_i\right) &= (4/16{,}810)\left[1372.8 - \left(\sum x_j\right)^2 \Big/ 2 + 97.2x_i\right. \\ &\quad \left. - 3.2(x_i)^2/2 + 30(8 - x_i)\right]^2. \end{aligned} \tag{2}$$

Subjects were given a table of payoffs (Figure 1, excluding the highlighting of some of the cells) as a function of individual choices and the choices of all other participants.

(c) *Nash strategies and the balance between self-interested and other-regarding behavior*

Because extracting firewood generates a pure public bad in the form of lower water quality, standard theory predicts that purely self-interested individuals will spend more time harvesting firewood than is socially optimal. Indeed, one common reference point for experiments of this type is the one-shot, complete-information Nash equilibrium (the standard model of purely self-interested strategic behavior) and another is the outcome at which group welfare is maximized. Although we do not ignore these benchmarks, we believe that for an investigation of whether external controls on individual behavior crowd out group-oriented behavior, a more appropriate benchmark are the individuals' pure Nash strategies—that is, individual payoff-maximizing choices taking the choices of the rest of the group as fixed. In fact, we take the difference between an individual's

Nash best-response to the choices of the other players in the group and his or her actual choice to be an indicator of how that individual balances self-interests and those of the entire group.

To illustrate the point, suppose there are eight players and each of seven players chooses to spend two months collecting firewood from the surrounding forest. Since the sum of the seven players' choices is 14 months, Figure 1 indicates that the eighth player's payoff-maximizing response—the individual's Nash best-response—is to spend eight months collecting firewood. (We have highlighted the cells in Figure 1 that indicate an individual's pure Nash strategy, although these cells were not highlighted for the participants.) This choice is made purely out of self-interest, without regard for the welfare of the others in the group. Note that player eight's payoff in this outcome is 776 points, while each of the other seven receive 535 points (for each of them, the sum of the others' choices is 20 months, while they choose two months).

Now imagine that the eighth player chooses three months instead of eight, while the other seven players continue to choose two months. We consider this to be a significantly more group-oriented choice—it is costly because that player's payoff is now 652 points instead of 776: however, each of the other players' payoffs increase from 535 points to 606 (for each of them, the sum of the others' choices is now 15 months, while they choose two months). Much of our analysis in Section 4 is based upon the differences between the players' actual choices and their Nash best-responses: choices that are close to Nash responses indicate relatively self-interested behavior, while those that are further away indicate stronger other-regarding behavior.

As for the standard benchmarks, it is straightforward to show that in our design the optimal amount of time each individual should spend collecting firewood is one month. [3] On the other hand, since a pure strategy Nash equilibrium requires that every player's choice be a best-response to every other player's best-response, in this context the Nash equilibrium is reached if every individual decides to spend six months collecting firewood from the nearby forest. It is worth noting that at the Nash equilibrium, subjects earn only about 24% of the payoffs attainable in the efficient outcome.

As noted in the introduction, we confront a subset of groups of subjects with a quota on the amount of time that can legally be spent collecting firewood and an inspection/penalty protocol to enforce compliance to the quota. The effect of this regulation on the relative balance of self-interested and other-regarding behavior is the primary focus of this work. Suppose that the quota is s. An audit of an individual's activities occurs with probability π. An individual found to have spent $x_i > s$ time collecting firewood in a particular period faces a penalty p on every unit of time in excess of s. Thus, a participant faces an expected penalty of $\pi p(x_i - s)$. Assuming risk-neutrality, the Nash strategy for an individual in this treatment maximizes $U(x_i, \sum x_i) - \pi p(x_i - s)$. Since the expected penalty is an additional cost of collecting firewood, individuals should choose to spend less time collecting firewood than they would in the absence of any regulation.

Under the regulation-with-enforcement treatment, we chose the individual quota s to be the efficient choice; that is, $s = 1$. We chose the probability of an audit to be $\pi = 1/16$, and the unit penalty for exceeding $s = 1$ to be $p = 100$; therefore, the subjects faced an expected marginal penalty for violating the standard of $\pi p = 100/16 = 6.25$. This enforcement regime is rather weak in the sense that the expected marginal penalty is not sufficient to induce risk-neutral players to comply with the quota. We chose a relatively weak enforcement protocol because we believe that weak enforcement best characterizes the state-imposed regulations our subjects actually encounter. In rural communities of developing countries like those where our experiments were conducted, monitoring and enforcement of state and federal regulations is likely to be quite lax because of high monitoring costs and limited budgets. [4]

Assuming risk neutrality, we have calculated the expected individual payoffs under our regulation-with-enforcement treatment, in which expected penalties are subtracted from gains, and show them in Figure 2. [5] (Subjects were not given this table.) Again, the highlighted cells indicate Nash responses to the choices of all the other players: these responses form the benchmark that indicates the balance between self-interested and other-regarding behavior when the subjects face the regulatory control. As expected, for most aggregate choices of time spent in the forest by others, the Nash best-response is lower than in the absence of regulation. In fact, the Nash equilibrium is reached when each player chooses to spend five units of time harvesting firewood as opposed to six

MY MONTHS IN THE FOREST

THEIR MONTHS IN THE FOREST

	0	1	2	3	4	5	6	7	8	
0	619	670	719	767	813	856	896	933	967	**0**
1	619	669	717	764	809	851	890	926	959	**1**
2	617	667	714	760	804	845	883	918	950	**2**
3	615	664	711	756	798	838	875	909	940	**3**
4	613	660	706	750	792	831	867	900	929	**4**
5	609	656	701	744	784	822	857	889	917	**5**
6	605	651	695	737	776	813	847	877	905	**6**
7	600	645	688	729	767	803	836	865	891	**7**
8	595	638	680	720	757	792	824	852	877	**8**
9	588	631	672	711	747	780	811	838	862	**9**
10	581	623	663	700	735	768	797	823	846	**10**
11	573	614	653	689	723	755	783	808	830	**11**
12	565	605	642	678	711	741	768	792	813	**12**
13	556	594	631	665	697	726	752	775	795	**13**
14	546	583	619	652	683	711	736	758	776	**14**
15	536	572	606	638	668	695	719	739	757	**15**
16	525	560	593	624	653	678	701	721	737	**16**
17	513	547	579	609	636	661	683	701	717	**17**
18	501	534	565	594	620	643	664	681	696	**18**
19	488	520	550	578	603	625	645	661	674	**19**
20	475	506	535	561	585	606	625	640	653	**20**
21	461	491	519	544	567	587	605	619	630	**21**
22	447	476	502	527	548	567	584	597	608	**22**
23	433	460	485	509	529	547	563	575	585	**23**
24	418	444	468	490	510	527	541	553	561	**24**
25	402	428	451	472	490	506	520	530	538	**25**
26	387	411	433	453	470	485	498	507	514	**26**
27	371	394	415	434	450	464	476	484	490	**27**
28	355	377	396	414	430	443	453	461	466	**28**
29	338	359	378	395	409	421	431	438	442	**29**
30	322	341	359	375	389	400	409	415	418	**30**
31	305	324	341	355	368	378	386	392	394	**31**
32	288	306	322	336	347	357	364	368	371	**32**
33	272	288	303	316	327	335	341	345	347	**33**
34	255	270	284	296	306	314	319	323	324	**34**
35	238	253	266	277	286	293	297	300	300	**35**
36	221	235	247	257	265	272	276	278	278	**36**
37	205	218	229	238	245	251	254	256	255	**37**
38	189	200	211	219	226	231	233	234	233	**38**
39	173	184	193	201	206	211	213	213	212	**39**
40	157	167	175	182	188	191	193	193	191	**40**
41	142	151	159	165	169	172	174	173	171	**41**
42	127	135	142	148	152	154	155	154	152	**42**
43	113	120	126	131	134	136	137	136	133	**43**
44	99	106	111	115	118	119	119	118	115	**44**
45	86	92	96	100	102	103	103	101	99	**45**
46	73	78	82	86	87	88	88	86	83	**46**
47	61	66	69	72	73	74	73	71	68	**47**
48	51	54	57	59	60	61	60	58	55	**48**
49	40	44	46	48	49	48	47	45	43	**49**
50	31	34	36	37	38	37	36	34	32	**50**
51	23	25	27	28	28	28	27	25	23	**51**
52	16	18	19	20	20	19	18	17	15	**52**
53	10	12	12	13	13	12	11	10	8	**53**
54	6	7	7	7	7	7	6	5	4	**54**
55	2	3	3	3	3	3	2	2	1	**55**

Figure 1. *Individual payoffs and Nash responses.*

MY MONTHS IN THE FOREST

THEIR MONTHS IN THE FOREST

	0	1	2	3	4	5	6	7	8	
0	619	670	713	755	794	831	865	896	923	**0**
1	619	669	711	752	790	826	859	889	915	**1**
2	617	667	708	748	785	820	852	881	906	**2**
3	615	664	705	743	779	813	844	872	896	**3**
4	613	660	700	738	773	806	835	862	885	**4**
5	609	656	695	731	766	797	826	851	874	**5**
6	605	651	689	724	757	788	816	840	861	**6**
7	600	645	682	716	748	778	804	828	848	**7**
8	595	638	674	708	739	767	792	815	833	**8**
9	588	631	666	698	728	755	780	801	818	**9**
10	581	623	656	688	717	743	766	786	803	**10**
11	573	614	647	677	705	730	752	771	786	**11**
12	565	605	636	665	692	716	737	754	769	**12**
13	556	594	625	653	678	701	721	738	751	**13**
14	546	583	613	640	664	686	704	720	732	**14**
15	536	572	600	626	649	670	687	702	713	**15**
16	525	560	587	612	634	653	670	683	693	**16**
17	513	547	573	597	618	636	652	664	673	**17**
18	501	534	559	581	601	618	633	644	652	**18**
19	488	520	544	565	584	600	613	624	631	**19**
20	475	506	528	549	566	581	594	603	609	**20**
21	461	491	512	531	548	562	573	581	587	**21**
22	447	476	496	514	530	542	553	560	564	**22**
23	433	460	479	496	511	522	532	538	541	**23**
24	418	444	462	478	491	502	510	515	518	**24**
25	402	428	444	459	472	481	488	493	494	**25**
26	387	411	427	440	452	460	467	470	470	**26**
27	371	394	409	421	431	439	444	447	447	**27**
28	355	377	390	402	411	418	422	424	423	**28**
29	338	359	372	382	391	396	400	401	399	**29**
30	322	341	353	363	370	375	377	377	375	**30**
31	305	324	334	343	349	353	355	354	351	**31**
32	288	306	315	323	328	332	333	331	327	**32**
33	272	288	297	303	308	310	310	308	303	**33**
34	255	270	278	284	287	289	288	285	280	**34**
35	238	253	259	264	267	268	266	262	257	**35**
36	221	235	241	245	247	247	244	240	234	**36**
37	205	218	222	225	227	226	223	218	211	**37**
38	189	200	204	207	207	206	202	197	189	**38**
39	173	184	187	188	188	186	182	176	168	**39**
40	157	167	169	170	169	166	162	155	147	**40**
41	142	151	152	152	151	147	142	136	127	**41**
42	127	135	136	135	133	129	123	116	108	**42**
43	113	120	120	119	116	111	105	98	89	**43**
44	99	106	105	103	99	94	88	81	72	**44**
45	86	92	90	87	83	78	72	64	55	**45**
46	73	78	76	73	69	63	56	48	39	**46**
47	61	66	63	59	55	49	42	34	24	**47**
48	51	54	51	47	42	36	28	20	11	**48**
49	40	44	40	35	30	23	16	8	-1	**49**
50	31	34	30	25	19	12	5	-3	-12	**50**
51	23	25	21	15	9	3	-5	-13	-21	**51**
52	16	18	13	7	1	-6	-13	-21	-29	**52**
53	10	12	6	0	-6	-13	-20	-28	-35	**53**

Figure 2. *Expected payoffs and Nash responses under regulation.*

units in the Nash equilibrium absent the regulatory control. It is worth noting that at the regulatory Nash equilibrium with risk-neutral subjects, an individual's expected payoff is 268 points. Because the enforcement protocol is too weak to induce perfect compliance to the quota, this value is about 42% of individual payoffs obtainable in the efficient outcome. But, since the regulation is intended to induce more efficient choices, each individual's expected payoff is about 73% higher than the Nash equilibrium payoffs in the absence of regulation.

(d) *The experiments*

Each session of the experiment involved eight subjects and two monitors. The subjects sat at individual desks that were distributed in a circle with enough separation between the desks so they could not look at another's work. Except in periods when communication was allowed, the desks faced away from the center of the circle. In each round, each subject would choose how many units of time, $x_i \in [0, 8]$, to spend collecting firewood from a local forest. Subjects were given the payoffs table (Figure 1 without the shading) and they knew that the other participants consulted the same table. Thus, although individuals could not know in advance what the others would choose, they knew that their decisions were based on the same payoffs. Once a subject made a decision for a particular round, this decision was written on a slip of paper. When all subjects had made their decisions, a monitor collected each slip of paper and gave them to another monitor who recorded the individual decisions and calculated the total for the group. This total was announced to the subjects, who then determined their own payoffs from the payoffs table. Subjects kept a record of their own payoffs as a check on the monitor's record.

Each session began with some welcoming remarks within which the subjects were told that the session would last approximately two hours. A monitor would then read the instructions to the participants. (The instructions are available from the authors.) Results from pre-tests of the experiment led us to decide not to give the subjects written instructions because of the wide variation in levels of literacy among the subjects. The instructions explained the basic setting of the game, how points were earned, how these points were converted to cash at the end of the session, and the procedures of the game. The instructions included three different examples to familiarize the subjects with the payoffs and the procedures. Two practice rounds were conducted. The monitor asked for questions at several points, and when there were no further questions the game began with round one. Large, readable posters of the payoff table, the forms the subjects used during the game, and the examples from the instructions were placed on one wall of the "field lab."

In total, 14 groups from three villages played two treatments of the game. Each of the 14 groups played 8–11 initial rounds of the game. During these initial rounds individuals made their choices without communicating with the others in their session or with the monitors. The subjects did not know how many rounds would be played. After the initial rounds the monitors would stop the game and announce a new set of rules for the forthcoming rounds. At the beginning of the session, the subjects were not told that the rules would change at some point in the session.

(i) *Communication treatment*

After the initial rounds, nine groups were told that they could now communicate with each other between rounds for three minutes. Between rounds the subjects turned their desks toward each other. They could talk to each other about anything, but they could not threaten others or agree to transfers of cash at the end of the game. Once three minutes had passed, the subjects were required to turn their desks back around and make their individual choices in private. These groups played additional 9–12 rounds in this way.

(ii) *Regulation treatment*

The other five groups were not allowed to communicate after the initial rounds. Instead they faced a regulation that stipulated they should spend no more that one unit of time collecting firewood from the forest in each round. They were told that after all had made their choices in a round, there was a one chance in two that one of them would be selected for an audit to verify compliance with the rule. After all had submitted their choices for the round and the aggregate amount of time spent in the common was announced, a die was rolled to determine whether an inspection would occur that period. An inspection would take place only if an even number came up. To determine which individual would be inspected if an even number was rolled, a number between one and eight would be drawn from a hat and that person's choice would be audited. Thus the probability that any one player would be audited for a particular round was 1/16. Once a player was chosen to be audited, a monitor would walk to that person's desk and check for compliance. If the audited player was found to be in violation for that round, a penalty of 100 points per unit of time above the time quota would be subtracted from that player's payoff for that round. Although the other players knew that an audit had been conducted and who had been audited, they did not know whether a penalty was assessed, nor the extent of the penalty. These groups played an additional 9–12 rounds under these rules.

At the end of each session, total points for each individual were calculated. Subjects were paid that number in pesos for their participation. For the villages in which the experiments were conducted, a daily minimum wage centered around 7,000 pesos (about US $5.40 at the time). Including practice rounds, most participants engaged in 20 rounds of decisions. If all subjects made the efficient choice in each round, they would have each earned about 12,900 pesos (approximately $11.73 US) in the experiment. Average earning for the experiments was 7,884 pesos.

3. RESULTS

We begin the analysis of the experimental data by considering average choices. Figure 3 and the third column of Table 1 summarize the average decisions made by 112 participants formed into 14 groups of eight villagers each. Nine of these groups (denoted COM) would ultimately be allowed to communicate between rounds after 8–11 rounds of not being able to communicate. The other five groups (denoted REG) would be subject to the imperfectly enforced time quota, $x = 1$, after 8–11 rounds of no communication. As indicated earlier, we ended the first and second stages at different points to be sure that terminal rounds could not be anticipated. For the first stage, we therefore consider only the first eight rounds of first-stage decisions for each group. All groups played nine rounds in the second stage, and some a few more; therefore, we consider only the first nine rounds of second-stage decisions for each group.

On the left portion of Figure 3 are depicted the average decisions of the participants in the two collections of groups for the eight rounds before they either were allowed to communicate or were faced with the regulation. Clearly, the Nash equilibrium ($x = 6$ for each subject) was not reached, nor did the efficient solution ($x = 1$) obtain. The average over the eight rounds for the nine groups who would later communicate was 4.39, and for the other groups, 4.32. Furthermore, Figure 3 suggests that average choices were relatively stable throughout the first stage for both sets of groups. As one would expect with randomly formed groups and identical experiments, there was no statistical difference between the two sets of groups (Table 1, row 1, column 3).

The second-stage results for the groups that faced the time quota are quite interesting. Figure 3 shows that in the first round after the rule was introduced, average time spent collecting firewood plunged to below two. But as the players became comfortable with the quota and the weak consequences of exceeding the quota, and as they understood that others were also violating the regulation, average choices rose over the rounds to exceed four units of time collecting firewood. The erosion of the influence of the regulation is unmistakable—if one compares the final three rounds of the first and second stages, one finds no statistical difference in average choices (Table 1, row 3a, column 3), indicating that by the end of the second stage the regulation had no effect on average choices. Furthermore, the average choice of those facing the regulation for the first three rounds of the second stage was 2.60, while for the final three rounds it was 4.13, a statistical and sizable difference (Table 1, row 4a, column 3).

In contrast, the communication groups were able to make more efficient choices. The average choice of these subjects shows a statistically significant decline in months of effort to extract firewood from 4.39 for all rounds in the first stage (in which they could not communicate) to 3.53 after communication was allowed (Table 1, row 2b, column 3), an indication of greater cooperation. Comparing the final three rounds before communication with the final three communication rounds shows a similar reduction (Table 1, row 3b, column 3). Moreover, unlike the regulation groups, that social improvement was relatively stable from the early second stage rounds to the last; there is no statistically significant difference between the mean choice for the first three rounds

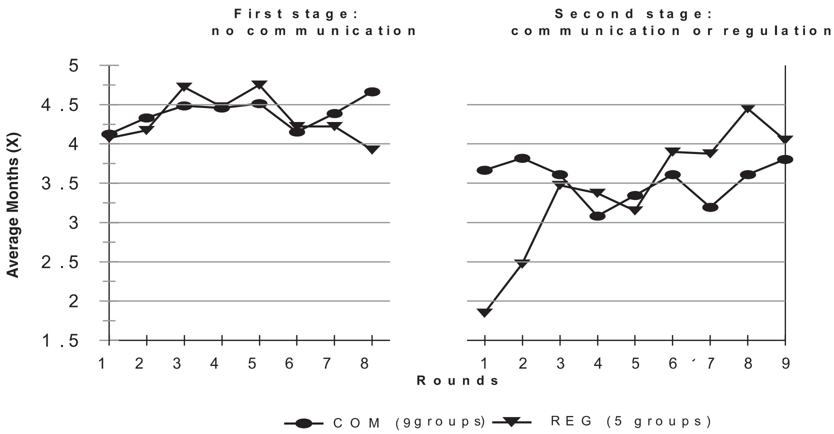

Figure 3. *Average individual decisions.*

after communication and for the mean choice for the final three rounds (Table 1, row 4b, column 3).

Although the snapshot provided by analyzing average choices is illuminating, it does not tell us much about how institutions affect the

Table 1. *Summary of statistical tests*[a]

Situation	Cases	Average months	*p*-value	Average deviations	*p*-value	Average earnings ($)	*p*-value
1. COM vs REG, first stage	COM (all rounds)	4.39		3.19		370	
	REG (all rounds)	4.32	0.76	3.20	0.85	377	0.54
2a. REG	First stage (all rounds)	4.32		3.20		377	
	Second stage (all rounds)	3.40	0.00	3.32	0.42	449	0.00
2b. COM	First stage (all rounds)	4.39		3.19		370	
	Second stage (all rounds)	3.53	0.00	4.34	0.00	471	0.00
3a. REG last 3 rounds	First stage (last 3 rounds)	4.13		3.53		403	
	Second stage (last 3 rounds)	4.13	0.96	1.66	0.00	366	0.07
3b. COM last 3 rounds	First stage (last 3 rounds)	4.40		3.19		369	
	Second stage (last 3 rounds)	3.54	0.00	4.37	0.00	470	0.00
4a. Sustainability REG	Second stage (first 3 rounds)	2.60		4.96		537	
	Second stage (last 3 rounds)	4.13	0.00	1.66	0.00	366	0.00
4b. Sustainability COM	Second stage (first 3 rounds)	3.70		4.07		455	
	Second stage (last 3 rounds)	3.54	0.40	4.37	0.26	470	0.88

[a] The *p*-values are from the Wilcoxon rank sum test.

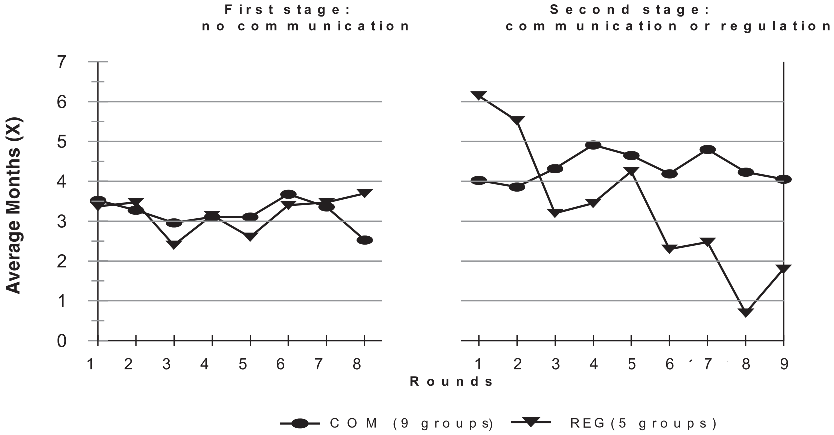

Figure 4. *Average deviations from individual Nash best-responses.*

balance between self-interested and other-regarding behavior. For this we need to analyze the average deviation of the decisions of the participants from their individual Nash strategies in each round. [6] These comparisons are in Figure 4 and column 4 of Table 1.

The left-hand side of Figure 4 shows that both sets of groups, those that would later be subjected to a rule and those that would later be allowed to communicate, made choices in the first stage that were more than three units lower on average than their Nash best-responses. Thus, without the ability to communicate and without outside intervention, the participants made choices that were, on average, significantly more group-oriented than their Nash strategies. As expected, there was no statistical difference between the communication and regulation groups, who averaged 3.19 and 3.20, respectively (Table 1, row 1, column 4).

When the regulation was introduced, several outcomes are of note. First, the average deviation from best-responses in the second stage remained statistically unchanged from the first stage rounds (Table 1, row 2a, column 4). But in comparing the last three rounds of the first stage with the last three rounds of the second stage, allowing participants to adjust to the experiment in the first stage and to the regulation in the second, we observe a sizable and statistical change in average deviations from Nash responses. In the first stage the average for the final three rounds was 3.53, while during the final three rounds of the second stage, participants were, on average, within 1.66 of their Nash responses (Table 1, row 3a, column 4). Clearly, as participants gained experience with the time quota, the imperfect monitoring, and the financial sanctions, they moved rather rapidly toward their self-interested responses; the average deviation from Nash responses in the first three rounds of the second stage of 4.96 plummeted to 1.66 by the final three rounds (Table 1, row 4a, column 4).

One of the groups facing the regulation behaved very differently from the others. Its members actually made choices that were much further from their best responses than they did prior to the imposition of the time quota. The average deviation from their best-responses rose from 4.83 in the final three rounds of the first stage to 6.75 in the final three rounds under the regulation. Excluding this unusual group, the average deviation of the other groups facing the regulation was 0.39 in the final rounds of the second stage. This value is not statistically different from zero, implying that these subjects were, on average, essentially playing their Nash best-responses after they gained some experience under the regulation.

If one accepts the notion that the difference between the Nash response and the actual choice of an individual is an indication of how the person balances own interests against those of the rest of the group, the message is clear. After the subjects that faced the external regulation quickly adjusted to the relatively modest consequences of noncompliance, they made choices that were significantly closer to their purely self-interested Nash responses. Thus, it appears that the presence of an external control crowded out other-regarding behavior in favor of greater self-interest.

The participants that were allowed to communicate, in contrast, moved further from their Nash best-responses, making more efficient choices on average; their mean deviations rose from 3.19 before communication was allowed to 4.34 afterward (Table 1, row 2b, column 4). Comparing, as with the regulation groups, mean deviations for the final three rounds of the first and second stages, we find a similar move toward greater group-regarding decisions. The average deviation from best responses for the final three periods before communication was allowed was 3.19, compared with 4.37 for the final three rounds after communication was allowed (Table 1, row 3b, column 4). Finally, unlike the groups facing the outside regulation, once communication was allowed, the average deviation from the Nash responses was relatively stable: the mean deviation in the first three rounds of communication was 4.07, and for the final three rounds, 4.37 (Table 1, row 4b, column 4).

The difference between the effects of communication and regulation on the balance between self and other-regarding behavior could not be more stark. While external regulation quickly crowded out group-oriented behavior in favor of greater self-interest, the simple ability to communicate induced a shift from choices that were relatively group-oriented in the absence of communication to an even stronger group-orientation with communication.

And it should come as no surprise that these effects are reflected in the subjects' earnings. Figure 5 and column 5 of Table 1 analyze changes in average earnings. [7] Consistent with the average effort levels and the average deviations from individual Nash responses, the per round earnings of participants of the two sets of groups in the first stage were statistically identical, $370 and $377 (Table 1, row 1, column 5).

The average per round earnings of those that faced the regulation in the second stage rose statistically, from $377 to $449 (Table 1, row 2a, column 5). Clearly much of this increase is due to behavior in the first several second-stage rounds in which a high proportion of the participants complied with the regulation. But, of course, these initial gains quickly dissipated; comparing the last three rounds of the first and second stages, we have a statistical loss in average earnings from $403 per round in the first stage to $366 per round in the second (Table 1, row 3a, column 5). Even more dramatically, average earnings fell from $537 in the first three rounds of the second stage to $366 in the final three rounds (Table 1, row 4a, column 5).

Recall that one of the regulation groups made choices that were significantly more group-oriented during the second stage than the others. Looking at the earnings of this group apart from the rest provides a dramatic illustration of the welfare consequences of the crowding out effect of regulation. Average earnings over the last three rounds of the second stage for this group were $641 per round.

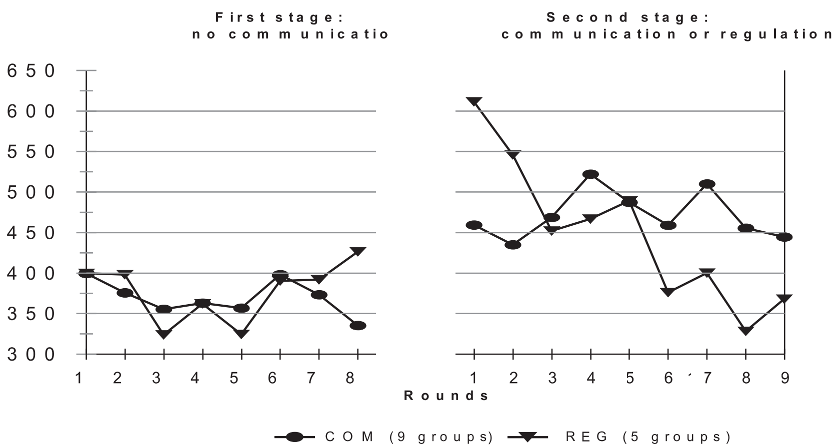

Figure 5. *Average individual earnings.*

The other regulation groups, who essentially played Nash strategies over those same rounds, earned $296. These same subjects earned an average of $373 per round over the last three rounds before the regulation was introduced.

In contrast, the communication groups earned consistently more when allowed to communicate. Considering all rounds, average earnings rose from $370 in the first stage to $471 in the second (Table 1, row 2b, column 5). Nearly the same gains are observed when considering only the last three rounds of the first and second stages (Table 1, row 3b, column 5). The gains afforded the participants that were allowed to communicate were stable in the second stage; there was no statistical difference in earnings at the beginning of the communications rounds and the final communications rounds (Table 1, row 4b, column 5). Finally, it is worth noting that at the end of the second stage, the communication groups were earning about 26% more, on average, than the regulation groups combined.

4. CONCLUDING REMARKS: IMPLICATIONS FOR POLICY AND FUTURE RESEARCH

We have presented evidence that indicates that local environmental policies that are modestly enforced, but nevertheless are predicted by standard theory to be welfare-improving, may be ineffective. In fact, such a policy can do more harm than good, especially in comparison to allowing individuals collectively to confront local environmental dilemmas without intervention. We have also argued, and presented evidence, that the fundamental reason for the poor performance of external control is that it crowded out group-regarding behavior in favor of greater self-interest.

If true in a wide range of environmental and other social contexts, the implications of our results are rather substantial. Economic theory will be a poor guide for designing environmental policies if it does not allow for other-regarding motivations, or if it fails to recognize that these motivations are not fixed with respect to institutional arrangements. Recognizing institutional endogeneity of the balance between self-interested and group-regarding behavior when it occurs will have profound implications for nearly every aspect of environmental policy design and evaluation, including:

—cost/benefit analyses to identify efficient environmental goals;
—the choice of environmental problems that can be addressed efficiently by government regulation;
—the federalism question of which level of government (local, state, or federal) should confront a particular environmental problem;
—the choice of control instrument (quotas, taxes, transferable property rights); and
—the design of enforcement strategies.

A complete accounting of the possible ramifications of the crowding-out hypothesis for environmental policy is well beyond the scope of this paper, but let us discuss a few.

Our results have implications for the performance of well-intentioned but only modestly enforced environmental polices, which currently exist in rather great numbers. One may be tempted to believe that inadequate enforcement of these policies, at worst, renders them ineffective, or that they only fall short of their intended goals. Our results suggest a more pessimistic possibility: policies intended to reach more desirable social states, but that are weakly enforced, may actually do more harm than good because their existence triggers the crowding-out of socially desirable behavior.

The remedy is not necessarily more vigorous enforcement. More stringent enforcement generally is more costly. Furthermore, the crowding-out phenomenon may very well imply that the potential welfare gains from regulation are less than standard theory would predict. Thus, in some cases, the welfare gains may not justify the costs of achieving acceptable levels of compliance.

Others have suggested that crowding-out of other-regarding behavior triggered by a particular regulation may spill over into other social dilemmas that a community faces (Weck-Hannemann & Frey, 1995). For example, the crowding-out of public spiritedness as a consequence of the government restriction on collecting firewood from a nearby forest may, in actual settings, make it more difficult for a community to deal with other environmental issues like the disposal of household waste. Thus, the effects and costs of crowding-out triggered by a particular regulation may extend well beyond the problem the regulation is intended to address.

On a more optimistic note, our results suggest that more research should be aimed at learning how institutions may be designed which promote cooperation and avoid crowding-out group-regarding motives and behavior. For example, government regulations are occasionally developed *in concert with* the efforts of local grass roots organizations. Active communication and effort at the local level might eliminate or reduce the tendency of government regulations to induce more self-interested behavior, perhaps because local participation reminds community members that they have the power to influence the well-being of their community and that "cheating" is more than simply a game against the government. If the affiliation of a regulation with local groups helps to avoid a crowding-out of cooperative spirit, then our results suggest that such a rule will be more successful than the regulation alone.

Relatedly, an implication is that the *framing* of a regulation may be even more important than currently appreciated. It now seems clear that institutions frame choices, and the way they are framed can have important effects on choice behavior (Bowles, 1998, p. 87). The manner in which a regulation is *marketed* may well make a difference in the degree of other-regarding behaviors exhibited by participants in a game as well as by members of society in the real world. Additional care in framing rules in ways more friendly to the objective of stimulating more socially efficient choices may produce substantial payoffs if doing so eliminates the tendency of external rules to crowd out other-regarding behavior.

These policy implications, as well as others not mentioned, also suggest potentially fruitful directions for additional research. For example, additional experiments should hold constant the type of rule, but vary the way it is framed to learn more about the effect of framing *per se* on crowding out. Further, to investigate whether local community efforts may or may not ameliorate crowding out associated with externally-imposed regulations, additional experiments should be undertaken in which one set of groups is subject to a rule and not allowed to communicate, while another set is subjected to the same rule, but where participants are allowed to communicate with each other.

It would be useful, as well, to conduct a number of experiments in the reverse order. Since external regulations are rather commonplace in many environmental dilemma settings, it would be useful to know whether the behavioral responses to an institutional change are symmetric. If more self-interested behavior follows a change to government regulation, would a move away from external regulation induce more other-regarding behavior? Sunstein's (1993) observation that preferences for a good or a right depend on whether it was initially an entitlement conferred by an external body suggests that the behavioral responses to an institutional change might not be symmetric; that is, behavioral responses may depend on the order in which institutional change occurs.

Previous research, and the research reported here, suggests that typically individuals in a local environmental dilemma do not make efficient choices; neither do they act in a purely self-regarding way. Regulations developed in the hopes of resolving environmental and social dilemmas typically aim at nudging individuals toward more socially efficient actions. Our results, and the related work of others, suggest that these good intentions can be thwarted if external rules trigger a loss of public spiritedness. Policies that do not take account of this phenomenon may very well do more harm than good.

NOTES

1. Although Titmus did not offer convincing evidence of this conjecture, Upton (1973) made a more compelling case. Among a group of previous donors in Denver and Kansas City, some were offered payments to give again while others were not. The rate of donating was substantially higher among those not offered the monetary reward.

2. In this sense, preferences may in some settings be endogenous, notwithstanding the generations of economists who have assumed preference exogeneity. Arguing the plausibility of endogenous preferences, Bowles (1998, p. 75) asserts, "Markets and other economic institutions do more than allocate goods and services; they also influence the evolution of values, tastes, and personalities." Similarly, in the arena of the environment, Sunstein (1993, pp. 223–224) argues that it may not be possible for government to assume preferences to be exogenous, because, " ... whether people have a preference for a good, a right, or anything else is often in

part a function of whether the government, or law, has allocated it to them in the first instance."

3. Since the player's payoffs are identical, optimality requires symmetric individual choices. Let x denote the common amount of time each individual spends collecting firewood in any symmetric outcome. Using [1], the joint welfare function is $W(x) = n(\mu^{\eta}/\delta)[\alpha(q^0 - \beta(nx)^2/2) + (\gamma x - \phi(x)^2/2) + w \times (e - x)]^{\eta}$. The first-order condition for the maximization of $W(x)$ requires $-\alpha\beta x n^2 + \gamma - \phi x - w = 0$. Solving for x and substituting the actual parameter values yields optimal individual amounts of time spent harvesting firewood, $x^* = (\gamma - w)/(\phi + \alpha\beta n^2) = 1$.

4. Even in industrialized countries, enforcement strategies for environmental and natural resource regulations appear to be rather weak in the sense that expected marginal penalties do not appear to be sufficient to ensure acceptable levels of compliance (Cohen, 1998). This also appears to be true of other forms of regulation; for example, income tax compliance in the United States (Andreoni, Erard, & Feinstein, 1998).

5. Clearly, we could not control the risk-attitudes of the subjects. Predominant risk-aversion would imply lower Nash equilibrium individual choices, while risk-loving would have the opposite effect.

6. For each individual and each round, the highlighted cells in Figures 1 and 2 give individual Nash best-responses. The choices of the rest of the group in a particular round were taken to be their actual choices. We calculated the difference between each individual's best response and their actual choice and averaged these differences for each round for the communication and the regulation groups.

7. Earnings data for those groups that faced the external regulation are net of the penalties that were assessed.

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