Journal of Conflict Resolution
http://jcr.sagepub.com/

An Intercultural Examination of Cooperation in the Commons

Jeffrey Carpenter and Juan Camilo Cardenas

Journal of Conflict Resolution published online 7 March 2011
DOI: 10.1177/0022002710393922

Journal of Conflict Resolution
000(00) 1-20

DOI: 10.1177/0022002710393922
http://jcr.sagepub.com

An Intercultural Examination of Cooperation in the Commons

Jeffrey Carpenter[1] and Juan Camilo Cardenas[2]

Abstract

We design a real-time, intercultural common pool resource experiment using participants from cultures that derive different benefits from a global public good (extraction vs. conservation of biodiversity resources) to analyze the effect of group affiliation on cooperative behavior. We also collect survey attitudes toward conservation to augment our experimental results. We find that when participants interact interculturally, extraction choices change significantly and that these changes can be attributed to an amplification of the relationship between attitudes and choices cued by the intercultural treatment.

Keywords

cooperation, common pool resource, global public goods, group affiliation, social identity theory, cross-culture

Introduction

Cooperation is hard enough to achieve in local commons situations; however, building cooperation at the global level may be an even greater challenge. While there can be substantial agent heterogeneity in local situations, the dimensions on which decision makers differ increase when the problem is global. For example, conserving

[1]Department of Economics, Middlebury College, Middlebury, VT, USA & IZA
[2]Facultad de Economía–CEDE, Universidad de los Andes, Bloque C, Bogotá, Colombia

Corresponding Author:
Jeffrey Carpenter, Department of Economics, Middlebury College, Middlebury, VT 05753, USA & IZA
Email: jpc@middlebury.edu

tropical ecosystems is problematic locally because many individuals rely on the benefits of extraction for their livelihoods. Although the local agents may be heterogeneous with respect to wealth and education (among other things), they all consider extraction to be the main benefit. At the global level, however, there are a number of other agents who consider conservation to be their biggest concern. With this in mind, it becomes important to study the strategic behavior of globally linked agents who differ in their attitudes toward conservation.

Economists have recently become interested in cross-cultural comparisons of behavior in experiments.[1] However, the comparisons that economists have made, so far, have come from running the same experiment in a number of different locations and then comparing across locations. While this is a worthwhile endeavor, we propose that equally interesting results might occur when one allows participants from different cultures to make decisions in the same experiment. We call this an intercultural experiment. With this motivation in mind, we conducted an experiment in which students from Colombia and the United States interacted in real time within the same session.

Because we expected cultural differences to matter most in situations where there are preexisting differences in the opinions and attitudes of the decision makers, we framed our experiment as a commons situation in which players extract resources from a forest that also provided substantial nonuse (i.e., conservation) benefits.[2] We felt this frame would be salient because Colombia and the United States represent countries at odds over who has the right to benefit from extracting resources, who should pay for conservation, and who should bear the opportunity costs of either conserving or transforming forested land for agriculture. Colombia represents those countries that host much of the world's biodiversity, while being a primary beneficiary of its direct use and extraction. At the same time, the United States benefits mostly from conservation (nonextraction) of tropical forests because of potentials for pharmaceutical research, carbon sequestration, and nonuse values in general.

The behavioral model we have in mind stems from the social identity theory developed by psychologists over the last quarter century. Tajfel and Turner (1979) originally identified three factors that contribute to the formation of an in-group bias: the extent to which individuals identify with a group, the extent to which the prevailing context provides the basis for comparing across groups, and the perceived relevance of the comparison group. Individuals are likely to be motivated by group comparisons when an in-group is central to their self-definition and a given comparison is meaningful or the outcome is contentious. We believe that our experimental protocol activates all these factors. The salience of in-group formation is often determined not by the links among in-group members but by what members discover they have in common with respect to the out-group. In our case, none of our participants need to be particularly patriotic because the fact that they are playing in a group with a group from another country is likely to make the obvious in-group ties more relevant.

Likewise, our experiment is framed to draw attention to an obvious difference between the groups that is highly contentious: the right to extract from a biodiversity-rich resource.

We base our expectations about the relevance of our protocol partly on the environmental literature, which is rife with examples of conservation dilemmas exacerbated by constituencies that act based on very different attitudes toward preservation. For example, Zanetell and Knuth (2004) examine the willingness of Venezuelan fishermen to participate in community-based management programs. They find that their attitudes toward conservation of the fishery predict participation and that the rate of participation directly affects the successfulness of the program. Likewise, Odell (2005) documents that stakeholders base conservation policy choices on their attitudes which, in turn, depend on whether they benefit from extraction or not. Because the stakeholders are heterogeneous with respect to their source of benefits, conflict arises.[3] Similar work has been surveyed by Keohane and Ostrom (1995), who document the various heterogeneities that affect extraction choices and the amount of conflict over conservation. Much of the emphasis of their volume is on the source of benefits of the involved parties and their resulting attitudes and extractive behavior.

Naturally arising group affiliations are our only experimental manipulation.[4] This allows us to examine common pool resource behavior both cross-culturally (because we run control sessions in each country) and interculturally. However, because we also elicited conservation attitudes in a survey, we are able to test whether the conservation attitudes that participants bring to the lab (as part of their culture) affect behavior and whether any of these relationships intensify or weaken when groups interact interculturally. Being more specific, based on our reading of the environmental literature sampled above, we expected conservation attitudes to predict play in our common pool resource (CPR) experiment. However, we did not anticipate that the relationship between the attitudes and behavior would depend on whom one is playing with. Interestingly, the environmental literature sampled above does, however, suggest that conflict may result when there are differences in conservation attitudes because people anchor the intensity of their opposition on the magnitude of the perceived difference between groups. Situations in which the between-group differences in attitudes appear small are resolved much more frequently than when the differences are large. Given that we collected data both behavior and attitudes, we can test whether the intensity of the relationship between the two depends on whether the game is played interculturally or not.

Our data suggest that mixing groups of students (i.e., half from the United States and half from Colombia) does lead to a significant increase in the individual extraction choices of the Colombian participants, which are balanced by a significant reduction in the individual extraction choices by the students from the United States. The fact that Colombians extract more and Americans extract less is in line with what one would expect if preexisting attitudes are intensified by intercultural interaction. Moreover, our analysis indicates that a substantial amount of the treatment differences in individual extraction can be explained by the participants' attitudes toward conservation and the amplification of these attitudes in the intercultural treatment.

Experimental Design

Using current Internet technology, we were able to run real-time experiments in which half our participants were students from a private Colombian university in Bogotá, and half were students in the United States at a private college in Vermont. The fact that both subject populations are drawn from elite schools in the respective countries indicates that many of the characteristics of the participants are similar with respect to the underlying distribution of characteristics in the two countries. Compared to other students in their countries, our participants tend to be better off, from the dominant racial group and of higher relative ability. We also conducted sessions with homogeneous groups of Colombian and American students to control for and compare base levels of cooperativeness in the two settings. In total 120 students participated: 40 in 5 all-Colombian sessions, 40 in 5 all-American sessions, and 40 in the 5 mixed sessions. After each experimental session, we conducted a survey to gather our participants' attitudes toward conservation. Each author conducted the sessions in his home country in the local language (the instructions for the experiment appear in the online data replication file).

Our hypotheses are that both Colombian students, who live in a region that benefits mostly from extracting from a CPR, and American students, who benefit mostly from conservation, will behave differently, that these differences will be exacerbated in the mixed treatment, but that we can make sense of any differences using their surveyed attitudes toward conservation.

Our CPR experiment is similar to the one used by Cardenas, Stranlund, and Willis (2000), which was initially based on the experiments discussed in Ostrom, Gardner, and Walker (1994). As in Ostrom, Gardner, and Walker (1994), the design maintains the incentive structure of a nonlinear commons extraction problem with a symmetric Nash strategy that is not dominant, and where the social optimum extraction is lower than the extraction predicted by the symmetric Nash equilibrium.[5] However, we preferred the payoff function used by Cardenas et al. because it includes an element that motivates this research. Specifically, nonuse benefits of a common pool resource also accrue to players when there is no extraction from the CPR. Experimentally, this meant that with no extraction of the resource, players still received earnings, and these earnings would begin to fall as the aggregate extraction increased beyond some point, because of the reduction of the conservation benefits.

The payoff function is based on a simple model of a fixed number of homogenous agents that benefit from both the extraction of a forest for which there is joint access and from the externalities that flow from the conservation of the forest. In each round of the game, each player is given an endowment of effort, e, that can be allocated between extracting resources—which increases individual benefits from extraction but decreases group benefits from conservation—and providing labor to an unrelated activity that yields private benefits. In our experiment, the social optimum occurs when everyone chooses to extract for one month and the symmetric Nash equilibrium occurs where everyone extracts for six months (see the appendix for the derivations).

To communicate player decisions back and forth during mixed sessions, we used an Internet messaging program which allowed us to transfer data instantly between Bogotá and Middlebury. As the eight players per session entered the classroom in which the experiments were conducted, they saw the instant messaging software projected on a screen. Additionally, they were able to see the preexperiment conversation between the two authors as it happened (e.g., we discussed how many participants had shown up). We projected the screen to assure participants that there were four additional participants in the other country. When a session was ready to begin, we turned off the projector to assure that individual choices were anonymous.

The CPR stage lasted fifteen rounds (this was common knowledge), and each round proceeded as follows. Players were given small pieces of paper on which they were told to write their player numbers, the round number, location, and the number of months they wanted to spend extracting from the commons. The experimenter in each location collected the decision sheets after each round and sent the individual decisions to his counterpart. Once the subtotals from each location had been recorded, they became common knowledge, as each experimenter wrote the round number, the months spent in the forest by players in Bogotá, the months spent in the forest by Middlebury players, and the total months spent by the entire group, on the blackboard. No individual decisions were shown in public. Although subtotals were innocuous information in theory (i.e., players only needed the total to calculate payoffs), we recorded these figures to reinforce the fact that the commons was split into two subgroups. Each round was completed when the players calculated their earnings and recorded them on their earnings record sheets.

To make the protocol for the homogeneous sessions as close to the protocol for the mixed sessions as possible, the eight players in the homogeneous sessions were split into two subgroups of four and one subgroup was brought into an adjoining room. In this case, after the players made their decisions, one experimenter would go to the other room to exchange subgroup totals. When making aggregate extraction decisions public in the homogeneous sessions, each experimenter wrote the round, subgroup one's months, subgroup two's months, and the total number of months spent extracting, on the board.

At the end of fifteen rounds, players were asked to total their payoffs and hand in their earnings record sheets. All participants faced the same payoff table, but they were paid differently per point earned. Colombians were paid 2.5 Colombian pesos per point earned, while U.S. participants were paid 0.02 cents per point earned. At an exchange rate of 2,200 pesos per dollar when we ran the experiments, this represents a 9/5 ratio for payments. This ratio was chosen because we estimated that it would maintain differences in the purchasing power for these two particular populations of students.[6] Including the show-up fee, participants in Middlebury received an average of \$14.70, ranging from \$11.00 to \$19.00, and their Colombian counterparts received an average of \$7.88, ranging from \$5.45 to \$11.82.

Participants filled out a survey after the last round of the CPR experiment in which they were asked for basic demographic data (e.g., years of schooling, sex), their attitudes toward conservation or extraction of common pool resources, and whether players (in the mixed sessions only) believed there were people on the other end of the Internet connection. This last question was motivated by the dictator game results of Frohlich, Oppenheimer, and Moore (2001) in which some participants did not believe that they were really matched with someone else in the experiment. In our case, most participants appear to have believed our protocol. On an integer scale from 1 to 5, where 5 meant one completely believed there were players in the other country, the average response was 4.80 for Vermonters and 4.25 for Colombians.

Our Results

Before we describe our results in detail, we first describe our analytical strategy. We begin by examining extraction choices at the group level to test for obvious differences in the treatments and for differences that might be driven by our adaptation of the standard CPR experiment. Because offsetting changes in individual behavior can by masked at the group level, we then turn our attention to the individual choice data. At the individual level, participants chose an integer level of extraction between zero and eight in each of fifteen rounds. Therefore, our experiment generates a panel of data. Twenty-seven percent of our extraction data are potentially censored because participants could not extract for more than eight months, nor could they extract for less than zero months. Because we felt that this fact was most likely to bias our point estimates, we chose a Tobit regressor that accounts for censoring at both ends of the strategy space.[7]

To account for individual heterogeneity in our panel, we included random effects in all our regressions. We chose random effects over fixed effects because our control variables from the survey do not change from one period to the next, and therefore, in a fixed effects framework, we would not have been able to distinguish the time-invariant effects of our controls from those of the fixed effects (Wooldridge 2002: 266).

Figure 1 compares the three treatments based on the average group total months spent extracting from the commons over the fifteen rounds of the experiment. The symmetric Nash equilibrium for any round of the game predicts that each player should allocate six units of effort (months) to extraction, and therefore, the group total should be forty-eight units (see Appendix A). Using this figure, we can graphically analyze the differences in behavior between our two homogeneous treatments and the mixed treatment. Overall, we see that players extract less than the Nash prediction in each treatment, but extraction levels approach the equilibrium prediction

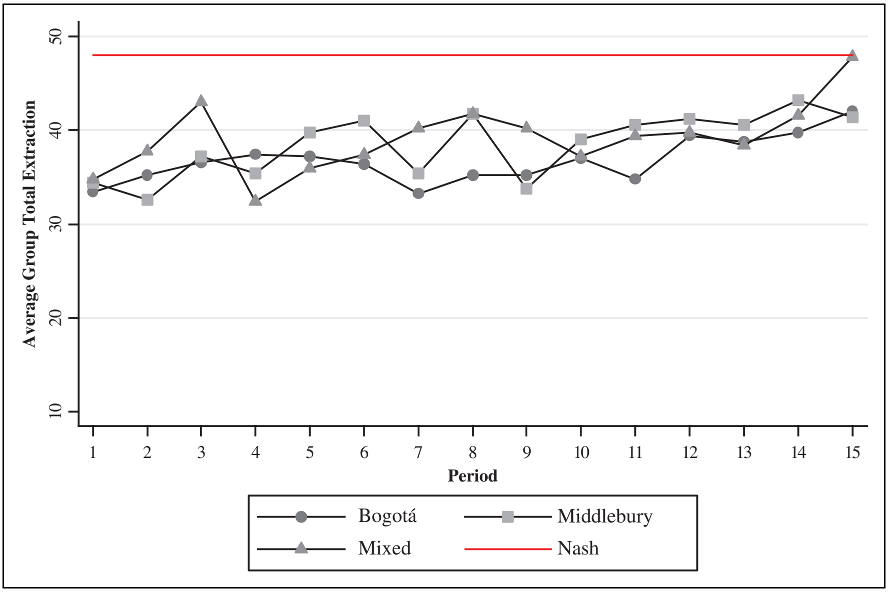

Figure 1. Subgroup extraction decisions by treatment

by the end of the game. Further, there do not appear to be differences at the group level either between subject populations or between the homogeneous treatments and the mixed treatment.

Result 1—Splitting Groups is Innocuous

From a theoretical perspective, splitting participants into two subgroups of four persons should be an innocuous change in the design, and it is for homogeneous groups.

Splitting homogeneous groups into two subgroups does not affect behavior. We compared levels of extraction between our homogeneous Middlebury treatment to three further control sessions we ran with Middlebury students in which the groups were not divided into subgroups, and found no difference in individual behavior. This claim is based on regressing individual extraction choices on a constant and an indicator variable that takes the value of one for those participants in the three sessions in which the groups were not split. The indicator variable in this regression is far from significant ($p = .95$), and therefore, we can be confident that our group-splitting procedures, on their own, are not driving our results.[8]

Result 2—Overall Group Extraction

Overall group extraction is lower than the symmetric Nash prediction but increases over time.

Table 1. Group Total Months Extracted (Pooled across Periods)

	Bogotá	Middlebury	Mixed
Mean	36.77	38.49	39.19
Standard deviation	7.04	7.36	7.34
t-test (pooled mean = 48)	$p < .01$	$p < .01$	$p < .01$
t-test (last period mean = 48)	$p = .09$	$p = .22$	$p = .95$

Table 1 presents the summary statistics for the group total months extracted by the participants in our three treatments. Tests on the data pooled across rounds (line 3) show that the central tendency of behavior in each treatment is significantly below the theoretic prediction of forty-eight months. However, this result must be tempered by tests limited to the last period only (last line of Table 1). In period 15, none of the treatments are significantly below the theoretic level if one uses the standard 5 percent cutoff. This indicates that group average behavior tends toward the symmetric Nash prediction in each treatment. In unreported regressions, we confirm our *t*-test results. We regressed group extraction levels (with group-level random effects because the group is the level of observation) on round indicators to be agnostic about functional form. In each case, the later rounds, specifically rounds twelve to fifteen, show evidence of a significant upward trend compared to first round choices.

Result 3—No Treatment Differences at the Group Level

There are no group-level differences in extraction among the three treatments. Table 1 indicates that the mean group extraction levels appear to be very similar. However, as a more rigorous test, we regressed group-level extraction on treatment indicators. Neither the Bogotá nor the mixed coefficients are significantly different from zero ($p = .50$ and $p = .78$, respectively), indicating that there are no group-level differences between either of these two treatments and the omitted treatment, the homogeneous Middlebury experiment. Further, comparing the coefficients on the Bogotá and mixed treatments, we also find no significant difference ($p = .30$).

While we find no treatment differences at the group level, there may still be individual differences that balance each other when aggregated. For example, if our framing of the CPR problem is salient and our participants bring preconceived attitudes toward conservation that may be heightened in one treatment of the experiment or another, we expected that American players would reduce their extraction because the nonextraction benefits would be more salient while the Colombian players would feel more entitled to extract more of the resource, given their direct benefits from consumption. If this is the case, the two effects may cancel each other at the group level.

Table 2. Survey Response Summary Statistics

	Overall	Bogota	Middlebury
Female	0.52 (0.50)	0.52 (0.50)	0.52 (0.50)
Year of college	2.70 (1.90)	3.72 (1.90)	1.66 (1.21)
Q1: Locals maintain extraction	2.08 (1.05)	2.13 (1.18)	2.03 (0.92)
Q2: Locals reduce extraction	4.20 (0.86)	4.27 (0.94)	4.13 (0.79)
Q3: Only locals should benefit	2.49 (1.29)	2.55 (1.39)	2.42 (1.19)
Q4: Every one should benefit	3.58 (1.23)	3.85 (1.20)	3.32 (1.21)
Q5: Other countries have rights	3.28 (1.13)	3.17 (1.19)	3.39 (1.05)
Q6: International nonprofit is best	0.33 (0.47)	0.26 (0.45)	0.38 (0.49)
Q7: Community organization is best	0.21 (0.41)	0.26 (0.45)	0.17 (0.38)

Note: Mean (standard deviation).

Result 4—Reactions to the Mixed Treatment are Significant

Colombian participants increase extraction in mixed groups, American participants reduce extraction, and the extraction differences within the mixed treatment are significant.

Table 2 summarizes, at the overall and country level, the survey data that we collected and used in our regression analysis. The seven conservation attitude questions are discussed below in detail in the support of result 5, but the two demographic variables, *Female* and *Years of College* are used as controls in all the regressions. In Table 3, regression ($R1$) we present the results of regressing individual extraction choices on treatment indicators and two demographic controls: years of college and sex.[9] The omitted/reference category is the homogeneous Middlebury treatment. To begin, we see that there are no significant differences between extraction choices in the two homogeneous control treatments (i.e., the Bogotá coefficient is not significant), confirming that there is no role of "culture" in our experiment as it is typically measured in cross-cultural studies.

However, the Middlebury mixed coefficient is significant and indicates that Middlebury players tend to extract 0.81 months less in the mixed treatment than in the homogeneous treatment. This result is interesting because it is consistent with the idea that people from the United States have stronger preferences for conservation in reference to a global commons like the rainforest but that these preferences only significantly affect behavior when interacting with an obvious out-group, the Colombians.

To test whether Colombian students behave differently in the mixed treatment, we can compare the coefficients on the Bogotá and Bogotá mixed indicator variables. This comparison suggests that there are significant differences in behavior. Specifically, Colombians in the mixed treatment extract for 1.18 months more (the point estimate arising from this comparison) than Colombians in the homogeneous treatment ($p = .001$). Therefore, interacting with American students leads Colombians to extract more than they would otherwise.

Table 3. Are there Treatment Differences in Individual Extraction?[a]

	R1	R2
Bogotá (indicator)	−0.27 (0.36)	−0.22 (0.35)
Bogotá mixed (indicator)	0.92 (0.35)***	0.22 (0.37)
Middlebury mixed (indicator)	−0.81 (0.33)***	−0.28 (0.33)
Female (indicator)	0.66 (0.27)***	0.11 (0.28)
Years of college	−0.13 (0.07)*	−0.08 (0.06)
Q1: Locals maintain extraction		0.02 (0.13)
Q2: Locals reduce extraction		−0.38 (0.15)**
Q3: Only locals should benefit		−0.16 (0.14)
Q4: Every one should benefit		0.42 (0.15)***
Q5: Other countries have rights		−0.55 (0.16)***
Q6: International nonprofit is best		−0.13 (0.30)
Q7: Community organization is best		−1.31 (0.56)**
Constant	5.05 (0.24)***	7.27 (1.11)***
Observations	1785	1755

[a] Dependent variable is the individual level of extraction in period *t*.
Note: * indicates significant at the 10%, ** 5%, and *** 1% level.

Lastly, we can compare the coefficients on the two mixed treatment regressors to see whether Colombians and Americans behave differently in the mixed treatment. Remember that behavior in the two control treatments was not different but, because American students reduce extraction and Colombian students increase extraction in the mixed treatment, a significant gap in behavior emerges. In the mixed treatment, Colombians extract 1.73 more months than Americans ($p < .001$).[10]

Finding treatment differences in an intercultural experiment is a major contribution of this experiment. However, to complete the story, and to be consistent with our hypothesis about why the pools of students that we study are interesting, we also need to provide an explanation of the differences that we have found. Recall that our basic hypothesis is that the attitudes toward conservation of our mixed treatment participants are cued by the slight frame of our experiment (including the fact that they are playing with students who may have different attitudes toward conservation) and that these attitudes influence play in our intercultural setting.

Result 5—Conservation Attitudes Explain our Treatment Differences

When we add specific aspects of culture such as surveyed attitudes toward conservation, our treatment differences disappear. In other words, our specific measures of culture perform better than indicators for location.

In Table 4, we list the seven pieces of attitudinal data that we elicited in our survey disaggregated by treatment. In the first five cases, our participants responded with the extent to which they agreed with each of the statements. Responses were recorded on a 5-point Likert scale where 1 indicated *complete*

Table 4. Mean Surveyed Attitude toward Conservation

	Bogotá	Bogotá Mixed	Middlebury Mixed	Middlebury
Q1: People who live nearby and extract resources from biodiversity-rich ecosystem should keep extracting the same amount they extract today	2.02 [1.10] (0.03)	2.35 [1.28] (0.17)	2.20 [0.75] (0.25)	1.95 [0.97] (−0.06)
Q2: People who live nearby and extract resources from biodiversity-rich ecosystem should reduce their current extraction	4.20 [0.95] (−0.03)	4.40 [0.86] (−0.15)	4.15 [0.73] (−0.12)	4.12 [0.82] (−0.04)
Q3: Those who live in the countries where the ecosystems are located should be the only people who have rights to benefits from conservation	2.55 [1.14] (0.06)	2.55 [1.32] (−0.11)	2.65 [1.20] (−0.05)	2.31 [1.16] (0.15)
Q4: People from all countries should have the same rights to benefits from conservation of these ecosystems and resources	3.63 [1.24] (0.06)	4.32 [0.92] (0.20)	3.20 [1.12] (−0.04)	3.38 [1.23] (−0.17)
Q5: People from other countries where there is less biodiversity should have rights to benefits from conservation in biodiversity-rich countries	3.20 [1.17] (0.01)	3.10 [1.22] (0.05)	3.15 [0.96] (−0.29)	3.51 [1.06] (−0.12)
Q6: An international nonprofit conservation organization is the best way to administer conservation	0.30 [0.46] (0.12)	0.18 [0.39] (0.17)	0.62 [0.49] (−0.18)	0.29 [0.46] (−0.09)
Q7: A community organization of the local users is the best way to administer conservation	0.22 [0.42] (0.08)	0.36 [0.48] (−0.37)	0.08 [0.26] (−0.03)	0.20 [0.40] (0.02)

Note: (i) responses for the first five questions were recorded on a 5-point Likert scale where 1 indicated complete disagreement and 5 indicated complete agreement, (ii) [standard deviation], and (iii) (raw correlation between response and extraction).

disagreement and 5 indicated *complete agreement*. The first two statements examined how people felt about the current extraction strategies of the people who live nearby biodiversity-rich ecosystems, the second three statements were constructed to elicit perceptions of the allocation of property rights in these ecosystems, and the last two pieces of data come from a question in which we asked the students to pick (from a list) the organization that they thought would be best at managing rich ecosystems. Q6 is an indicator variable for those people who stated that an international nonprofit would be best, and Q7 indicates a preference for a community of local users.

What is particularly interesting about Table 4 is that there are few significant differences in the responses either across the mixed treatment or between the mixed and homogeneous treatments. Based on *t*-tests, only five of the twenty-eight comparisons are different at the 5 percent level. Our first reaction is that this suggests that we have achieved randomization into treatment with respect to the attitudes and, more importantly, that our survey responses were not endogenous with respect to play in the CPR experiment.

The few significant differences that we see in Table 4 make sense. For example, the fact that more American students believe that an international nonprofit organization would be the best way to manage a CPR indicates that people who benefit mostly from nonextraction see such rights being best protected by an international organization. Likewise, more Colombian students thinking that local user groups are the best solution is consistent with the fact that they benefit disproportionately from extraction.

The lack of response differences in Table 4 also suggests that to explain our treatment effects in terms of these attitudes, we must consider that the mixed treatment amplifies the importance of these attitudes. We offer suggestive evidence of this in Table 4, which also reports the raw correlations between each surveyed response and extraction. Notice that the correlations tend to be stronger in the mixed treatments.[11] We return to this below.

Our strategy to show that specific aspects of culture, such as conservation attitudes, account for the treatment differences in play is a simple invocation of omitted variable bias. If the treatment differences that we see in Table 3 ($R1$) are due to the differential impact of conservation attitudes on extraction, then the coefficients on the treatment indicators are biased because the error term will be correlated with the omitted conservation attitudes. However, when we add conservation attitudes to the regression, some of the variation absorbed by the treatment indicators should be soaked up by the attitudes, and their inclusion should push the treatment indicator coefficients to zero.

In regression ($R2$) of Table 3, we add the surveyed conservation attitudes and see that they do, indeed, wash away the treatment differences reported in ($R1$). Of particular interest is the fact that we see participants who think that local people should reduce their extraction from actual ecosystems do themselves extract less in the experiment. Likewise, those people who feel strongly that all countries should have rights in the management of biodiversity-rich ecosystems and those people who

Table 5. Predicted Extraction Levels for Conservation Attitudes (by Treatment)

	Bogotá	Bogotá Mixed	Middlebury Mixed	Middlebury
Locals maintain extraction	4.79	6.10	4.78	4.55
Locals reduce extraction	5.00	5.95	4.72	4.83
Only locals should benefit	4.81	5.20	5.36	5.12
Every one should benefit	4.60	4.98	5.26	5.09
Other countries have rights	4.86	4.96	4.67	4.85
International nonprofit is best	5.06	5.63	3.98	4.07
Community organization is not best	4.79	5.59	4.49	4.72

Notes: These predictions come from interacting the treatments with all the attitudinal measures in equation [R2], Table 3; The predictions account for censoring and are determined at intermediate values (i.e., 2, 3, 4) of each question.

think that local communities are the best managers of CPRs extract less in the experiment. At the same time, however, those people who think that everyone should benefit from rich ecosystems tend to extract more in the experiment. Given that the treatment indicators are no longer significantly different than zero in ($R2$) and many of the attitudes do predict behavior, we conclude that the treatment differences we see in our experiment do reflect culture to the extent that attitudes partially form the basis for culture.

We can take our analysis one step further by being more specific about how our experiment translates conservation attitudes into real extraction choices. Based on Table 4, we know that it is not simply mean attitude differences that drive differences in experimental extraction choices. Instead, it must be that the mixed treatment intensifies attitudes and exacerbates the differences in responses to these attitudes. Returning to Table 4, one not only notices that the correlations among attitudes and extraction are stronger in the mixed treatment, which is the evidence of the amplification of the effect of attitudes, we also see that attitudes are translated into extraction choices differently by participants in the two countries. Colombian participants who strongly agree with the statements in Table 4 tend to extract more, while American participants that strongly agree extract less. This difference in the signs of the raw correlations is consistent with the fact that Americans benefit mostly from conservation, while Columbians benefit mostly from extraction.

Result 6—The Mixed Treatment Amplifies the Effect of Conservation Attitude Differences

Participants from a culture that benefits mostly by extraction from real CPRs tend to extract more when their attitudes are strong. Participants from a culture that benefits mostly from the conservation of real CPRs tend to extract less when their attitudes are strong. On top of this, the mixed treatment amplifies these effects.

The obvious way to substantiate result 6 is by adding the interactions of attitudes and treatments to (*R*2 in Table 3). The full set of interaction results is long and cumbersome; therefore, we summarize it more intuitively in Table 5, where we report the predicted extraction levels for players in each treatment who have intermediate attitudes (i.e., values of 2, 3, or 4). For example, a participant in Bogotá with an intermediate view of our survey question about locals maintaining their extraction levels is predicted to extract 4.79 months, after accounting for possible censoring. What is important about Table 5 is the presence of two dynamics picked up by our regression model. First, intermediate attitudes tend to lead to lower extraction levels in the homogeneous Bogotá experiment compared to the mixed treatment. The same player with intermediate views about local extraction who chose to extract for 4.79 months in the homogeneous treatment is predicted to extract for 6.10 months when playing with someone from Middlebury. At the same time, Middlebury players with intermediate attitudes are more likely to reduce extraction when they play in the mixed treatment. In other words, the mixed treatment tends to amplify the importance of the attitudes for both sets of participants. While some of the effects look modest, the underlying regression indicates most of the effects are statistically significant.[12]

Discussion

Economists have begun to use experiments to examine the cultural components of behavior in economically relevant situations. Along the way, we have discovered interesting differences between nations and cultures in bargaining behavior (Roth et al. 1991), the evolution of fairness norms (Henrich et al. 2001), and trust (Croson and Buchan 1999). Our experiment contributes to this literature by comparing the behavior of Colombian and American students in the common pool resource experiment. In this respect, we find little difference between the extraction choices in our two homogeneous CPR experiments (recall Tables 2 and 3 and Figure 1).

While our cross-cultural comparisons are interesting, we feel our major contribution is to explore the behavioral implications of attitudes when participants from different cultures interact. Our experiment is a first step in this direction, and we have uncovered interesting results because we have focused on two groups who are likely to feel differently about resource management, given that they benefit differentially from extraction versus conservation. This is also the reason that we chose to examine behavior in the CPR game. Although local users do a surprisingly good job at creating decentralized institutions to regulate the use of common pool resources, global commons problems can be more complicated. Global commons involve additional sources of heterogeneity among agents, which make cooperation harder to implement. Keohane and Ostrom (1995) documented cases such as the management of groundwater and irrigation systems and the oil industry where different heterogeneities affect the possibilities of achieving collective action.

The fact that the coefficients on the mixed treatment indicators in Table 3 (*R*1) are significantly different from their homogeneous baselines suggests that there is some effect of playing with people from another group that could be linked to the extensive literature mentioned previously on in-group biases. As this model of behavior suggests, we could be witnessing one group trying to make a point to the other group. In our case, it appears that because the Colombians extract much more in the mixed treatment that they could be signaling that they will not be told what to do with their resources by the rich folks in the north. At the same time, because the Americans extract significantly less in the mixed treatment, they could be trying to set a "good example." After controlling for attitudes in *R*2, the treatment differences disappear. This suggests that if out-group biases are at play, they are working through the attitudes of the participants. In fact, as Table 5 indicates, the mixed treatment tends to intensify the effect of one's attitudes. This could easily be due to an in-group bias (Reid 2006).

Obviously, our preferred explanation for the patterns we see in the data is that interactions with salient out-groups lead to the intensification of relevant attitudes and this affects behavior. Not only is this explanation consistent with our data; it is consistent with the lengthy literatures on social identity theory and attitudes toward conservation reviewed in the introduction. However, can we reasonably rule out other possible explanations? One alternative that has been suggested is related to inequality aversion: in the mixed treatment the relatively poorer Colombian participants might expect transfers from the wealthier American participants. To implement the transfer, Colombians extract more and Americans extract less. Although this is consistent with the overall pattern of extraction choices that we see in our data, there are three reasons why we think this is not the correct explanation. To begin with, both sets of participants occupy similar class positions in society (i.e., both schools attract mostly affluent students), and the groups knew this about each other. This implies that, although there might be absolute differences in wealth, there is little reason to feel sympathy for a participant from the other county. In addition, controlling for the purchasing power differences in the payoff tables discussed above, Colombians in the mixed treatment did not earn significantly more than their American counterparts, so if some transfer convention did arise, it was unsuccessful. Most importantly, however, this hypothesis cannot explain why the conservation attitudes of the participants are so strongly correlated with extraction choices.[13]

There are various implications of our results for the regulation of a global commons. Preference differences with respect to a key issue may inhibit cooperation rather than opening new dimensions for negotiation. What could be regarded as the same good, for example, trees, may in fact be valued differently by parties with different perspectives. Our experiment suggests that perspective differences may be exacerbated when even small differences and attitudes foster the creation of out-groups in the minds of decision makers. Forests have direct use value from their extractive wood and nonwood products and provide nondirect use values that emerge mostly from ecological functions highly dependant on the level of extraction and pressure. The design of institutions, local and global, that pursue the sustainability of forests should first recognize such heterogeneities (and the possible polarizing affect of differences) and design incentives and regulatory mechanisms that push people closer to the optimal extraction level of the resulting asymmetric game.

Appendix

The Underlying Model of CPR Behavior

Let x_i denote the amount of time individual i spends collecting resources from the forest, and let w denote the marginal return on effort not allocated to extraction. Then, i's decision to provide $(e - x_i)$ units of labor to the private alternative yields a payoff of $w(e - x_i)$. Effort spent extracting from the forest yields a private benefit, which we assume takes the nonlinear form $g(x_i) = \gamma x_i - \tau(x_i)^2/2$, where γ and τ are strictly positive and are chosen in part to guarantee $g(x_\mathrm{i}) > 0$, for $x_i \in [1, e]$. The strict concavity of $g(x_i)$ indicates diminishing marginal private returns to extraction.

In the case of the group externality from aggregate extraction, individual payoffs decrease with $\sum x_i$ because, for instance, biodiversity or water regulation benefits diminish for all group members. We can assume then that q is a quadratic function of the aggregate amount of time individuals in the group spend collecting resources; specifically, $q\left(\sum x_j\right) = q^0 - \left(\sum x_i\right)^2/2$, where q^0 is interpreted to be biodiversity or water quality in the absence of extraction. Again these parameters are chosen so that $q(\sum x_i) > 0$ for all feasible $\sum x_i$.

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

$$U_i\left(x_i, \sum x_i\right) = k\left[\left(q^0 - \left(\sum x_i\right)^2/2\right) + \left(\gamma x_i - \tau(x_i)^2/2\right) + w_i(e - x_i)\right]^\eta. \quad \text{(A1)}$$

Each group consisted of $n = 8$ subjects, and each subject was allocated $e = 8$ units of time in each round. As in Cardenas (2003), we choose parameter values: $k = 0.0024$, $\eta = 2$, $q^0 = 1372.8$, $\gamma = 97.2$, $\tau = 3.2$, $w_i = 30$, and $e = 8$. Individual payoffs were therefore calculated from the payoff function:

$$U_i\left(x_i, \sum x_i\right) = 0.00024\left[\left(1372.8 - \left(\sum x_i\right)^2/2\right) + \left(97.2x_i - 3.2(x_i)^2/2\right) + 30(8 - x_i)\right]^2. \quad \text{(A2)}$$

All subjects were given the same table of payoffs (the instructions, including the payoff table, are reproduced in the online data replication file), which listed how much they would earn as a function of their choices and the choices of the other group members. Because extracting resources generates a public bad (here, lower biodiversity or water quality), standard theory predicts that purely self-interested individuals will spend more time harvesting resources than is socially optimal. Indeed, one common reference point for experiments of this type is the one-shot, complete-information Nash equilibrium, and another is the outcome at which group welfare is maximized. Since players' payoffs are identical and the game is played a finite number of times so that Selten's (1973) theorem applies, we only discuss the symmetric Nash equilibrium as a benchmark. Let x denote the common amount of time each individual spends extracting in any symmetric outcome. Using equation (A1), the joint welfare function is $W(x) = n(k)\left[\left(q^0 - (nx)^2/2\right) + \left(\gamma x - \tau(x)^2/2\right) + w(e-x)\right]^{\eta}$. The first-order condition for the maximization of $W(x)$ requires $-xn^2 + \gamma - \tau x - w = 0$. Solving for x and substituting the actual parameter values yield optimal individual amounts of time spent extracting, $x^* = (\gamma - w)/(\tau + n^2) = 1$. That is, if all eight players choose one month in the forest, the Pareto optimal solution is achieved. The equivalent conditions for the symmetric Nash equilibrium require that $x^{nash} = (\gamma - w)/(\tau + n) = 6$.

Authors' Note

We thank the Workshop in Political Theory and Policy Analysis at Indiana University for funding this project, and Ken Bickers, Dan Brayton, Jessica Holmes, Peter Matthews, Mike McGinnis, Elinor Ostrom and James Walker for comments on earlier drafts. Additionally, Cardenas acknowledges the support of a Research and Writing Grant from the John D. and Catherine T. MacArthur Foundation; Carpenter acknowledges the National Science Foundation (SES-CAREER 0092953). We are indebted to Maria-Claudia Lopez, Landreth Freeman, Tom Kelly, Bob Prasch, and Will Pyle for their help in running the experiment. Replication materials are available at the journal's website: http://jcr.sagepub.com.

Declaration of Conflicting Interests

The author(s) declared no conflicts of interest with respect to the authorship and/or publication of this article.

Funding

The authors thank the MacArthur and National Science Foundations for financial support.

Notes

1. Recent examples include Carpenter et al. (2004); Croson and Buchan (1999); Henrich (2000); Kachelmeier and Shehata (1992); Ockenfels and Weimann (1999); and Roth et al. (1991).
2. For additional support of our choice to frame our instructions, see Harrison (2005) who outlines the pitfalls of implementing context-free experiments.
3. Other examples include Lindsey, du Toit, and Mills (2005); Rockloff and Lockie (2004); and Sant (1996).

4. Instead of taking advantage of naturally arising difference in affiliation, Blackwell and McKee (2003) pursue a parallel approach in which group affiliation in a global public good is induced in the lab. Their results dovetail with in the sense that they identify important structural factors that influence contributions to a global commons.
5. Although asymmetric equilibria exist, given the symmetric positions of our players with respect to the instructions, endowments, payoff tables, and other factors, it is natural to focus (like most of the rest of the related literature) on the symmetric equilibrium.
6. We estimated the 9/5 ratio based on (1) a typical bundle of recreation expenditures (pizza and beer) college students purchase and (2) typical wages for students on both campuses.
7. There are alternative empirical strategies. One might use the ordered logit (or probit) estimator because the dependent variable is discrete. Another option might have been to account for the discrete nature of the data using an interval regressor in which we might have assumed, for example, that choices of 7.56 were recorded as 7 (or 8). Neither of these alternatives substantially change our results.
8. The details of this regression can be found in the data replication file (available online). We would have included demographic controls in the analysis, but we did not collect survey data in the nonsplit sessions.
9. One might also be concerned that the demographics had different effects in the different treatments. As part of our preliminary analysis, we interacted years of college and gender with the treatments but found that the interactions were not jointly significant.
10. It is also interesting to note that women extract more than did men ($p = .013$) and schooling appears to reduce extraction ($p = .08$), but discussions about these findings are beyond the scope of this article.
11. In fact, pooling results for brevity sake, ten of the fourteen correlations are significant at the 5 percent level or better for the mixed treatments, while only five of fourteen are for the homogeneous treatments.
12. The interaction terms clearly matter as one would expect from Table 4: thirteen of the twenty-one coefficients are significant at the 10 percent level or better, and overall, the chi-square test of joint significance has a p value well below .01.
13. As a more explicit test of whether inequality aversion might be playing a role, we tried adding a few other survey questions that might proxy distributional preferences. Specifically, we asked respondents to answer the following two-part question about payoff satisfaction: I would be most satisfied if I had played (one to eight months) and the other seven had played an average of (one to eight) months. An imperfect, but possibly useful, measure of inequality aversion would be the extent to which people answered the same number for both questions. As it turns out, while there is some evidence for an overall effect, this measure of inequality aversion cannot explain the treatment differences very well.

Reference

Blackwell, Calvin, and Michael McKee. 2003. "Only for My Own Neighborhood? Preferences and Voluntary Provision of Local and Global Public Goods." *Journal of Economic Behavior & Organization* 52 (1): 115-31.

Cardenas, Juan Camilo. 2003. "Real Wealth and Experimental Cooperation: Evidence from Field Experiments." *Journal of Development Economics* 70 (2): 263-89.

Cardenas, Juan Camilo, John Stranlund, and Cleve Willis. 2000. "Local Environmental Control and Institutional Crowding-out." *World Development* 28 (10): 1719-33.

Carpenter, Jeffrey, Amrita Daniere, and Lois Takahashi. 2004. "Cooperation, Trust, and Social Capital in Southeast Asian Urban Slums." *Journal of Economic Behavior & Organization* 55 (4): 533-51.

Croson, Rachel, and Nancy Buchan. 1999. "Gender and culture: International Experimental Evidence from Ttrust Games." *American Economic Review (Papers and Proceedings)* 89 (2): 386-91.

Frohlich, Norman, Joe Oppenheimer, and J. Bernard Moore. 2001. "Some Doubts About Measuring Self-interest Using Dictator Experiments: The Costs of Anonymity." *Journal of Economic Behavior & Organization* 46 (3): 271-90.

Harrison, Glenn. 2005. "Field Experiments and Control." In *Field Experiments in Economics*, ed. Jeffrey Carpenter, Glenn Harrison, and John List, 17-50. Greenwich and London: JAI Press.

Henrich, Joe. 2000. "Does Culture Matter in Economic Behavior? Ultimatum Game Bargaining among the Machiguenga Indians of the Peruvian Amazon." *American Economic Review* 90 (4): 973-79.

Henrich, Joseph, Robert Boyd, Samuel Bowles, Colin Camerer, Ernst Fehr, Herbert Gintis, and Richard McElreath. 2001. "In Search of Homo Economics: Behavioral Experiments in 15 Small-scale Societies." *American Economic Review* 91 (2): 73-8.

Kachelmeier, Steven, and Mohamed Shehata. 1992. "Culture and Competition: A Laboratory Market Comparison between China and the West." *Journal of Economic Behavior & Organization* 19 (2): 145-68.

Keohane, Robert, and Elinor Ostrom, eds. 1995. *Local Commons and Global Interdependence: Heterogeneity and Cooperation in Two Domains*. London: SAGE.

Lindsey, Peter, Johan du Toit, and M.G.L. Mills. 2005. "Attitudes of Ranchers towards African Wild Dogs *Lycaon pictus*: Conservation Implications on Private Land." *Biological Conservation* 125 (1): 113-21.

Ockenfels, Axel, and Joachim Weimann. 1999. "Types and Patterns: An Experimental East-West-German Comparison of Cooperation and Solidarity." *Journal of Public Economics* 71 (2): 275-87.

Odell, Jay. 2005. "A Biosocial Approach for Analyzing Environmental Conflicts: A case Study of Horseshoe Crab Allocation." *Bioscience* 55 (9): 735-48.

Ostrom, Elinor, Roy Gardner, and James Walker. 1994. *Rules, Games and Common-pool Resources*. Ann Arbor: University of Michigan Press.

Reid, Fraser. 2006. "Polarizing Effects of Intergroup Comparison." *European Journal of Social Psychology* 13 (1): 105.

Rockloff, Susan, and Stewart Lockie. 2004. "Participatory Tools for Coastal Zone Management: Use of Stakeholder Analysis and Social Mapping in Australia." *Journal of Coastal Conservation* 10: 81-92.

Roth, Alvin, Vesna Prasnikar, Masahiro Okuno-Fujiwara, and Shmuel Zamir. 1991. "Bargaining and Market Behavior in Jerusalem, Ljubljana, Pittsburgh and Tokyo: An Experimental Study." *American Economic Review* 81 (5): 1068-95.

Sant, Morgan. 1996. "Environmental Sustainability and the Public: Responses to a Proposed Marine Reserve at Jervis Bay, New South Wales, Australia." *Ocean and Coastal Management* 32 (1): 1-16.

Selten, Reinhard. 1973. "A Simple Model of Imperfect Competition, where 4 Are Few and 6 Are Many." *International Journal of Game Theory* 2 (1): 141-201.

Tajfel, Henri, and John Turner. 1979. "An Intergrative Theory of Intergroup Conflict." In *The Social Psychology of Intergroup Relations*, ed. G. Austin and S. Worchel, 33-47. Monterey, CA: Brooks/Cole.

Wooldridge, Jeffrey. 2002. *Econometric Analysis of Cross Section and Panel Data*. Cambridge, MA: MIT Press.

Zanetell, Brooke Ann, and Barbara Knuth. 2004. "Participation Rhetoric or Community-based Management Reality? Influences on Willingness to Participate in a Venezuelan Freshwater Fishery." *World Development* 32 (5): 793-807.