Experimental Tests of Core Theory and the Coase Theorem: Inefficiency and Cycling

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Abstract

We examine experimentally the bargaining process and the final allocation of payoffs in games that differ in terms of whether the core exists and in the initial allocation of property rights among the players. This paper highlights the interaction among property rights, transaction costs, and the empty core. Our experimental results indicate that the existence of the core is an important determinant of bargaining generally and the Coase theorem in particular. They confirm our conjecture that when the core is empty and property rights are ill defined, Coasean efficiency breaks down. Among other results, our experiments show that the number of inefficient (non-Pareto-optimal) agreements and bargaining rounds with cycling are significantly greater when the core is empty than when the core exists, especially when property rights are ill defined. Our results suggest an economic role for specific property right arrangements to resolve the empty core.

1. Introduction

The Coase theorem asserts that, given well-defined property rights and absent transaction costs, Pareto-efficient allocations will emerge via bargaining among players to internalize any externality among them, regardless of the initial assignment of property rights.[1] Coase (1960) highlights the central importance of transaction costs and property rights for resource allocation, focusing on efficient property right structures when transaction costs are significant. This is in contrast to many subsequent interpretations of the paper that focus their analyses on a

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[1] We believe it is important to clarify the assumptions and to analyze the robustness of the Coase theorem because it continues to be a central paradigm for economists and public choice and legal scholars. As Dixit and Olson (2000, pp. 309–10) write, "Coase's article has been arguably the single largest influence on thinking about economic policy for the last three decades."

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world of zero transaction costs and on the efficiency of the final allocations and their invariance to initial property right assignments.

In terms of core theory, the Coase theorem can be interpreted as asserting that, regardless of the initial allocation of property rights among the players, and irrespective of whether or not the core of a superadditive characteristic function exists, the grand coalition will always emerge (for a review of core theory, see Telser 1994). However, Aivazian and Callen (1981) show that with more than two players, in a setting in which there are gains from cooperating and forming coalitions to internalize the externality, the Coasean efficiency result may fail. Specifically, using a three-participant example, they demonstrate that it is possible for the core to be empty under one set of property rights, although the core is nonempty under an alternative set of property rights. Aivazian and Callen (2003) further demonstrate that the breakdown of Coasean efficiency because of the empty core becomes more likely if transaction costs are imposed on the negotiations.[2]

The purpose of this paper is to test experimentally important implications of the theory of the core and to shed light on the robustness of the Coase theorem. The experiments involve alternative bargaining scenarios in which there are pecuniary gains to the players from cooperating and forming coalitions. The observed bargaining process and the final allocation of payoffs among the players in these experiments serve as tests of the implications of the Coase theorem and of core theory generally.[3]

Our experiments address not only the robustness of the Coasean efficiency result but also the issue of bargaining in the empty core generally.[4] Economic theory is ambiguous about bargaining in the empty core. On the one hand, theory predicts that bargaining when the core of the game is empty leads to cycling and either inefficient solutions or solutions that involve long periods of

[2] Aivazian and Callen (2003) make the reasonable assumption that the costs of forming a coalition are convex in the number of players in the potential coalition. It is wrong to conclude that once transaction costs are introduced, the problem of the empty core disappears and a Pareto-optimal solution obtains. Rather, in such circumstances, negotiations may break down more quickly, and which specific coalition structure (the grand coalition or a proper subcoalition) obtains cannot be specified a priori. Even if transaction costs were to force the equilibrium, nothing ensures that the equilibrium is Pareto optimal.

[3] Existing experimental results on the Coase theorem do not account for bargaining situations in which the core is empty (Hoffman and Spitzer 1982, 1986; Harrison and McKee 1985; Harrison et al. 1987). Indeed, in all previous experimental tests of the Coase theorem, the underlying game is assumed to have a core. In view of the Harrison and McKee (1985) result that the Coase theorem holds experimentally when the core exists, it is important to see experimentally what happens when the core of the bargaining game is empty, especially since the empty core highlights the importance of coalitional rationality.

[4] It is worth noting that technological nonconvexities can induce inefficiency and instability even when the core exists. On double-auction experiments, see Van Boening and Wilcox (1996) and the literature cited therein.

bargaining and many bargaining rounds.[5] As a consequence, the nonexistence of the core may engender not only transaction costs associated with direct bargaining and the opportunity costs of (negotiation) time but also costs associated with a decrease in the value of the exchange opportunity as it is delayed by prolonged bargaining and the prospect of settling on a non-Pareto-optimal coalition. On the other hand, there exists a large game theory literature arguing that when the core is empty, bargaining participants will utilize alternative strategies or solution concepts that result in Pareto-optimal outcomes or that when the core is empty, constraints on coalitional actions may emerge to induce efficiency.[6]

The paper is organized as follows: Section 2 develops the hypotheses to be tested. Section 3 describes the experimental setup, and Section 4 reports the results. Section 5 briefly concludes.

2. Development of Hypotheses

The Coase theorem is silent about the bargaining process, including the length of bargaining time, the number of bargaining proposals needed to reach agreement, and the possibility of cycling instead of converging to a Pareto-optimal outcome. The presumption is that when transaction costs are zero, none of these factors are significant or have a bearing on the achievement of Pareto-optimal outcomes. Yet when the core is empty, cycling and bargaining time may come into play as emphasized by Shubik (1983, p. 150) and Aivazian and Callen (1981, 2003). Indeed, Bernholz (1997, p. 422) argues that the empty core is isomorphic to cyclical preferences.

Core theory suggests that, when the core is empty, bargaining is bound to be more protracted because of cycling, and Pareto-optimal agreements are less likely to obtain than when the core exists. The time it takes to reach a resolution and the number of bargaining proposals proxy for the erosion of exchange opportunities. We hypothesize that the cost of exchange opportunities lost to bargaining

[5] Shubik (1983, p. 150) writes, "A game that has a core has less potential for social conflict than one without a core, since every cooperative or collusive effective demand can be granted. A coreless game, on the other hand, must leave at least one coalition unsatisfied, that is, short of its potential. At least one group of players can always do better by dropping out and going it alone. If they try it, though, another group will be able to make a better offer to some of the dropouts in a new alignment, and the bargaining may continue at great length." Of course, it is possible that participants will recognize that the core is empty, which causes them to settle on a suboptimal (proper subcoalition) outcome quickly, recognizing that allocations in the grand coalition (composed of all bargaining group members) cannot satisfy all of the individual and coalitional rationality constraints. However, it is more likely that participants will recognize that the core is empty only after trying to bargain to a grand coalition and failing to reach consensus as cycling ensues. Telser (1994) provides examples of trading situations with empty cores.

[6] For example, Shubik (1983, p. 151) writes, "In a coreless game then, some constraints on coalitional activity must be operative in the society or else they will be engendered during the play of the game." Similarly, Telser (1994) discusses the role of various mechanisms or constraints on the bargaining process to resolve an empty core and yield efficient outcomes, and Bernholz (1997) posits a similar role for binding contracts.

is greater when the core is empty than when the core exists. We expect that reaching a bargaining solution requires more cycles, longer time, and more bargaining proposals when the core is empty, especially if the initial allocation of property rights is also ill defined.

Although both transaction costs and the empty core may attenuate the achievement of an efficient outcome, and although it may be hard to distinguish between these two effects in practice, the transaction technology is fixed in our experimental design. Any differences that occur should be due to the empty core and not the transaction costs structure. Our experiments in effect control for transaction costs.[7]

These considerations lead to the following hypotheses expressed in the alternative:

Hypothesis 1A. The number of inefficient agreements (including no agreement) reached is greater when the core is empty than when the core is nonempty.

Hypothesis 2A. The amount of cycling is greater when the core is empty than when the core is nonempty.

Hypothesis 3A. Bargaining time is greater when the core is empty than when the core is nonempty.

Hypothesis 4A. The number of bargaining proposals is greater when the core is empty than when the core is nonempty.

The results of Hoffman and Spitzer (1982, 1986) suggest that when the core is nonempty, so bargaining is more likely to reach an efficient resolution, participants are likely to split payoffs equally on the basis of fairness considerations. We conjecture that when the core is empty, efficient solutions, including even splits, are less likely. This yields the following hypothesis:

Hypothesis 5A. The number of even-split solutions is lower when the core is empty than when the core is nonempty.

The Coase theorem asserts that efficiency obtains independent of initial property rights. However, it is precisely when property rights are ill defined that bargaining ensues and an empty core is more likely to arise. To test whether

[7] An alternative solution concept to the core is the bargaining set (Aumann and Maschler 1964). While the core represents allocations to which there are no objections, the bargaining set is based on the notions of objections and counterobjections. For example, the bargaining set $M_1^{(i)}$ is the set of all individually rational allocations that are coalitionally stable in the sense that every objection by an individual has a counterobjection. As Aivazian, Callen, and Lipnowski (1987) show, from the perspective of the Coase theorem, the bargaining set $M_1^{(i)}$ definition of coalitional stability is problematic. Every coalition structure, including no coalition at all, is potentially stable. This means that the attainment of the grand coalition is not assured. Thus, perhaps a different interpretation of our results is that instead of cycling, players reach stable subcoalitional agreements outside the (Pareto-optimal) grand coalition. Such an interpretation would be consistent with the bargaining set notion of stability, but it is important to note that these Pareto-suboptimal outcomes are inconsistent with the Coase theorem.

initial property rights matter, we investigate scenarios with and without a controller. The controller has the initial property rights, including the right to unilaterally appropriate for herself all bargaining gains. In contradistinction, no player has specific property rights in the no-controller situation. These considerations lead to the following parallel hypotheses about the controller:

Hypothesis 1B. The number of inefficient agreements reached (including no agreement) is greater when there is no controller than when there is a controller, especially when the core is empty.

Hypothesis 2B. The amount of cycling is greater when there is no controller than when there is a controller, especially when the core is empty.

Hypothesis 3B. Bargaining time is greater when there is no controller than when there is a controller, especially when the core is empty.

Hypothesis 4B. The number of bargaining proposals is greater when there is no controller than when there is a controller, especially when the core is empty.

Hypothesis 5B. The number of even-split solutions is less when there is no controller than when there is a controller, especially when the core is empty.

3. Research Design and Method

3.1. Participants and Administration

Three hundred six senior business and economics students from a large Canadian university participated in the experiment.[8] Students were recruited via classroom announcements, flyers, and e-mail newsletters. They were guaranteed a flat sum of $10 for participating as well as a chance to make additional income based on their performance in the experiment.[9] Students signed up and agreed to appear at a specific time to participate.

The sessions were run via networked computers in a research trading laboratory. One of the researchers was present at all times. A maximum of 12 students could participate at one time, and sessions ran consecutively throughout the day. As students arrived for their assigned sessions, they were randomly assigned to one of the four treatment groups. Each group was composed of three players, and the participants did not know the identity of the other two people in their group. The experiment took about 1 hour to complete.

3.2. Experimental Design and Materials

A 2×2 factorial design experiment was used to test the hypotheses. The two independent variables, manipulated between subjects, were the existence of a

[8] Two groups were dropped from the analysis (one group did not understand the materials, and for one group there was a computer malfunction), which left 300 students, or 100 groups.

[9] Ethics approval required the participants to be informed that they could earn up to $25 more than the show-up fee. This information functioned as an incentive for participation but, on the basis of the results, did not appear to influence the negotiation behavior.

core and the existence of a controller, yielding four cases: (1) an empty core and a controller ($N = 26$), (2) a nonempty core and a controller ($N = 26$), (3) an empty core and no controller ($N = 25$), and a nonempty core and no controller ($N = 23$).[10]

3.2.1. The Core

The only difference between the non-empty-core condition and the empty-core condition was the maximum payoff amount if all three members of a group were party to an agreement. When the core existed, the maximum amount the three players could split was 600 lire.[11] Alternatively, when the core was empty, the maximum amount the three players could split was 500 lire. All other instructions were the same.

3.2.2. The Controller

In the experimental conditions with a controller, one member of each three-person group was assigned the role of controller at the beginning of each trial on the basis of a random draw. The controller had the right to decide unilaterally how to split the payoff amount. For example, the controller could decide to take all of the money for herself. In the no-controller conditions, each member of the group had equal rights.[12] The remaining materials were identical irrespective of the controller condition.

3.2.3. Procedures

The experiment required that the participants first read the instructions individually. The instructions informed the participants of their experimental label or name—a single letter A through L—and that they would be asked to attempt to obtain an agreement with two other parties in each of five trials. In each trial, the groups were asked to split specific amounts of money among themselves. The materials were very careful to avoid any mention of negotiation, bargaining, or opponent. Instead, participants were asked to come to an agreement regarding

[10] Each group participated in five negotiation trials, and the rules and members of the group remained the same in each trial. The trials differed only with respect to the player who was randomly assigned to be the controller. Panel data techniques are used in the regression analyses to control for the number of trials.

[11] An exchange rate of 100 lire = Can$1 was employed for payment to participants.

[12] We operationalized the controller slightly differently from previous experimental studies (Hoffman and Spitzer 1982, 1986; Harrison and McKee 1985) but in a manner more consistent with the analysis of Aivazian and Callen (1981, 2003). In prior experimental papers, the controller unilaterally selects from a discrete set of options that includes a specific allocation to the controller and the other party. Once an option is chosen, the other party can bargain in order to influence the controller and reach a mutually acceptable joint decision. In our study, the controller had the absolute right to choose (1) the maximum payoff, (2) which players she would like to include in the agreement, and (3) the allocation of the payoff among the players, including the option of keeping it all for herself. Here too the other participants could try to influence the controller to reach a mutually acceptable joint decision. The essential difference is that in our case, given the maximum payoff chosen by the controller, the allocation among the parties was continuous rather than discrete.

Table 1
Maximum Amounts to Split (in Lire)

Players	Empty Core	Nonempty Core
3	500	600
2	400	400
1	0	0

how to split the money.[13] The pool of money to be split depended on the number of parties in the agreement and whether or not the core exists (see Table 1).

The instructions also informed the participants assigned to controller conditions that the controller would be randomly selected at the beginning of each of the five negotiation trials. No mention of the controller was made in the materials for the no-controller conditions. Once all participants read the instructions, the researcher reviewed the instructions and answered any questions.[14] We believe, on the basis of the questions asked by participants, observations of the sessions, verification checks between the online bargaining and the hard copies handed in by the participants, and our analysis of the online bargaining, that the participants understood the materials and the experimental procedures.

At the start of each of the five negotiation trials, the researcher announced the identity of the randomly selected controller in the experimental conditions with a controller. The researcher then started the trial, and the participants communicated via the computer to determine how to split the money. All communication was done via an e-mail-type program in which the participants typed in messages and sent them to their counterparts. The messages appeared immediately in the recipients' mailboxes and were stored there, with the most recent ones appearing at the top of the screen. Participants were not required to respond to any message or offer, while side agreements were possible. Information was symmetric, as all messages could be observed by all members of the three-person group. However, members could choose to address a message to only one group member.

For each trial, the participants were given 10 minutes to reach an agreement, which was ample time based on pretesting results and observations of the sessions. If an agreement was reached, the players logged out of that particular negotiation trial and completed the hard-copy questions for the trial. If an agreement was not reached within the 10-minute allotment, the computer program shut off, and all communications between the participants stopped. The

[13] The materials are based on those used in the earlier experimental literature (Hoffman and Spitzer 1982; Harrison and McKee 1985).

[14] Initial pretests involved paper-and-pencil tasks and face-to-face bargaining. However, the face-to-face bargaining resulted in participants almost always evenly splitting the payoffs. The even-split solutions appeared to be a function of the fact that many of the subjects were classmates and knew each other and is consistent with the well-known fairness bias in bargaining games (Hoffman et al. 1994). To mitigate this bias, an anonymous electronic format was developed instead. The materials were pretested several times for understandability as well as to ensure that the software was functioning appropriately.

participants then completed the questions for that particular trial. When no agreement was reached, the parties in that group earned no income for that trial.

Once all five negotiation trials were completed, the participants answered questions about the experiment as well as demographic questions. The participants were then paid the show-up fee of $10 and any income earned during the experimental sessions.

4. Experimental Results

4.1. Univariate Statistics

Table 2 summarizes the data and variable definitions.[15] The data were individually coded by two graduate students unaware of the purpose of the study. The two coders agreed over 90 percent of the time on the rate of cycling, the number of bargaining proposals made, time spent negotiating, and negotiation outcomes. The remaining coding differences were resolved by one of the researchers. This researcher also performed test checks of the coding and found no discrepancies.

Table 2 indicates that, on average, 14 percent of the bargaining trials ended in an inefficient solution, that is, other than a grand coalition. The same percentage of trials exhibited cycling behavior. A trial took about 5 minutes on average and resulted in approximately three proposals. About 59 percent of the rounds ended in an even split among the three subjects. Even splits were defined as 200 lire each for the nonempty core participants and 166–170 lire each for the empty-core participants.

Table 3 shows univariate statistics comparing the nonempty core and the empty core. The *t*-statistics and the Wilcoxon test yield similar results. Consistent with the theory of the core and in contradiction to the Coase theorem, the empty-core case is characterized by significantly (at conventional levels) more inefficient outcomes, more trials with cycling, more time spent bargaining, and more proposals than the case of a nonempty core.[16] The empty-core case is also characterized by splits that are significantly less even. This latter result holds even though the experimental stakes were relatively low and, hence, so are the costs of being fair.[17]

Table 4 shows univariate statistics comparing the controller and no-controller cases. Again, the *t*-statistics and the Wilcoxon test yield similar results. Consistent

[15] Note that the cycling definition excludes three-player coalitions who have decided to collude and who are bargaining only about the appropriate allocation.

[16] It bears emphasizing that the statistical significance of the extra time and proposals induced by the empty core does not tell us about their economic significance, that is, the magnitude of the extra economic costs.

[17] Therefore, our results are consistent with the idea that fairness is more prevalent for high stakes than low stakes because of the relative cost of fairness, as found by Telser (1995) in the ultimatum game. However, other studies provide evidence that the size of the stake has little effect on fairness in the ultimatum game. See Hessel, Sloof, and Van De Kuilen (2004).

Table 2
Summary Data

	Mean	Median	SD	Min	Max
INEFF	.14	.00	.35	.00	1.00
CYCLE	.14	.00	.36	.00	1.00
TIME	5.04	4.65	2.57	.63	10.0
PROPOSAL	2.82	2.00	2.77	.00	20.0
EVEN	.59	1.00	.49	.00	1.00

Note. INEFF = number of inefficient (non-Pareto-optimal) agreements in a bargaining round (trial); CYCLE equals one if the bargaining round exhibits cycling and zero otherwise, where cycling occurs if (1) two players propose to collude and a third player interjects with an alternative two- or three-player coalition proposal, or (2) two players propose to collude and one of these players breaks away and proposes to collude with the third player, or (3) three players propose to collude and two of these propose to break away and form a two-party coalition; TIME = time expended to reach an agreement in a bargaining round (in minutes); PROPOSAL = number of proposals made in a bargaining round, where a proposal is defined as any suggestion put forward by one of the players as to how to split the payoff; EVEN = number of even splits in a bargaining round.

with the Coase theorem, there is no statistical difference between the number of inefficient cases with a controller and without a controller, nor is there a significant difference in cycling behavior.[18] Nevertheless, cases with a controller are characterized by significantly more time spent bargaining and more proposals than the no-controller cases. Not surprisingly, there are significantly fewer even splits when there is a controller (who can unilaterally appropriate the payoffs for herself) relative to the no-controller scenario.

Table 5 shows summary data for each of the four core/controller possibilities. The no-controller/empty-core case is the most interesting since it is the most inimical to the Coase theorem, involving both ill-defined initial property rights and an empty core. Indeed, in comparison with the other three cases, this case shows the greatest number of inefficiencies and trials in which cycling occurs. As indicated by the superscript letters, these latter differences are also significant at the 5 percent level (on the basis of a t-test). However, other cases have significantly longer bargaining times and a greater number of proposals.

Table 5 also allows comparisons between the empty core with a controller and without a controller for the various outcomes, and such comparisons are quite important. Since the empty-core Pareto-optimal solution (500 lire) yields less overall profit than the non-empty-core Pareto-optimal solution (600 lire), it could be argued that incentives alone, and not the emptiness of the core, drive the observed inefficiencies.[19] By holding the empty core constant, our comparison removes the incentive issue. Indeed, untabulated results show that having an empty core without a controller yields significantly more inefficiencies (.26 versus

[18] These efficiency results for the nonempty core and controller/no-controller scenarios are in line with the findings of Hoffman and Spitzer (1982, 1986), who had inefficiency rates of 10.5 percent and 7 percent, respectively.

[19] We do not find this argument compelling since we believe that it is precisely the issue of incentives that distinguishes core existence from the empty core for all but rather uninteresting cases.

Table 3
Impact of Core Existence on Performance Outcomes

	Empty Core	Nonempty Core	t-Statistic	Wilcoxon Test
INEFF	.21 (.00)	.07 (.00)	4.86 [.00]	4.71 [.00]
CYCLE	.20 (.00)	.10 (.00)	3.11 [.00]	3.09 [.00]
TIME	5.36 (5.19)	4.70 (4.20)	2.88 [.00]	2.72 [.01]
PROPOSAL	3.52 (3.00)	2.10 (1.00)	5.98 [.00]	6.87 [.00]
EVEN	.42 (.00)	.77 (1.00)	−8.36 [.00]	−7.81 [.00]

Note. Values are means (medians) and univariate statistics. Two-tailed p-values are in square brackets.

Table 4
Impact of the Controller on Performance Outcomes

	Controller	No Controller	t-Statistic	Wilcoxon Test
INEFF	.13 (.00)	.15 (.00)	−.87 [.38]	−.88 [.38]
CYCLE	.13 (.00)	.16 (.00)	−.94 [.35]	−.94 [.35]
TIME	5.41 (5.33)	4.63 (4.11)	3.43 [.00]	3.37 [.00]
PROPOSAL	3.24 (2.00)	2.37 (1.00)	3.61 [.00]	3.37 [.00]
EVEN	.50 (1.00)	.69 (1.00)	−4.25 [.00]	−4.17 [.00]

Note. Values are means (medians) and univariate statistics. Two-tailed p-values are in square brackets.

.16; p = .05) and more trials with cycling (.26 versus .14; p = .00) than having an empty core with a controller.

4.2. Panel Data Regression Analyses

Table 6 shows fixed-effects panel regressions for each of a number of dependent variables on a core existence dummy variable and a controller dummy variable. The panel data techniques control for learning by the experimental subjects over multiple trials.

Consistent with hypothesis 1A and core theory, results for INEFF show that there are significantly more inefficient bargaining resolutions when the core is empty than when the core exists, which contradicts Coasean efficiency. In contrast, initial property rights do not appear to matter, which is consistent with Coase and inconsistent with hypothesis 1B.

The results for CYCLE indicate that there is significantly more cycling when the core is empty than when the core exists. Contrary to hypothesis 2B, the results suggest that cycling is unrelated to initial property rights.

The results for TIME show that it takes longer to reach an agreement when the core is empty than when the core exists, consistent with cycling. Since a longer time to reach an agreement implies a greater erosion of exchange opportunities, this result appears to contradict the Coase theorem. Furthermore, the existence of a controller also increases the time it takes to reach an agreement, contrary to the claim that the Coase theorem holds independent of initial property rights.

The results for PROPOSAL show that more bargaining proposals are required

Table 5

Impact of Core Existence and the Controller on Performance Outcomes

	Nonempty Core		Empty Core	
	Controller	No Controller	Controller	No Controller
INEFF	.09[a,b] (.00)	.03[b] (.00)	.16[a] (.00)	.26[c] (.00)
CYCLE	.13[a] (.00)	.07[a] (.00)	.14[a] (.00)	.26[c] (.00)
TIME	5.07[a] (4.93)	4.29[b] (3.95)	5.76[c] (5.83)	4.95[a] (4.45)
PROPOSAL	2.54[a] (2.00)	1.60[b] (1.00)	3.95[c] (3.00)	3.08[a] (3.00)
EVEN	.68[a] (1.00)	.86[c] (1.00)	.32[d] (.00)	.53[b] (1.00)

Note. Values are means (medians) and univariate statistics. Cells with the same superscript letters are not significantly different from one another at the 5% two-tailed significance level.

to reach an agreement when the core is empty than when the core exists, consistent with cycling. Again, more proposals are offered when there is a controller than when there is no controller. Unlike expenditure of time, these results do not overtly contradict the Coase theorem since more proposals do not automatically translate into a waste of resources. Nevertheless, they indicate that bargaining when the core is empty differs from bargaining when the core exists.

The results for EVEN show that when the core is empty, there are fewer even splits than when the core exists, consistent with hypothesis 5A, and, not surprisingly, there are fewer even splits when there is a controller who can unilaterally appropriate the payoffs, consistent with hypothesis 5B.[20]

Table 7 is similar to Table 6 but includes an interaction term that takes on the value of one when the core is empty and there is no controller and zero otherwise. As mentioned above, the case of an empty core and ill-defined property rights (no controller) is precisely where Coasean efficiency is expected to fail. Indeed, Table 7 shows that the number of inefficiencies is significantly greater when the core is empty and there is no controller than in all other possible combinations. Similarly, the number of trials in which cycling occurs is significantly greater when the core is empty and there is no controller than in all other possible combinations. In comparison, the interaction term is insignificant for all other outcomes.

4.3. *Sensitivity Analyses*

We performed sensitivity analyses by replicating Tables 6 and 7 using random-effects rather than fixed-effects panel data techniques. The results are almost

[20] We replicated Table 6 after controlling for the proportion of women in the group, the average age of the group, and the average number of economics and business courses taken by the group. The signs, statistical significances, and values of the estimated coefficients on CORE and CONTR are very similar to those of Table 6. We also find that the greater the proportion of women in the group, the fewer the number of inefficient solutions, and the less time and the fewer proposals needed to reach an agreement. On average, women appear to be better at bargaining than men. The greater the average age of the group, the greater the number of inefficient solutions, the greater the extent of cycling, and the more time spent bargaining. The average number of economics and business courses taken by the group has no significant effect in any of the regressions.

Table 6

Fixed-Effects Panel Data Regressions of Five Performance Measures on the Existence of the Core and Controller

	Estimation Method	CORE	CONTR	Fit Statistic	Log Likelihood
INEFF	Logit	1.34** [4.46] (.00)	−.21 [−.79] (.43)	24.04** (.00)	−190.46
CYCLE	Logit	.84** [3.05] (.00)	−.26 [−.99] (.33)	10.70** (.00)	−177.13
TIME	Ordinary least squares	.68** [3.07] (.00)	.80** [3.60] (.00)	11.00** (.00)	−3,198.51[a]
PROPOSAL	Negative binomial	.53** [9.50] (.00)	.32** [5.98] (.00)	123.61** (.00)	−1,066.89
EVEN	Logit	−1.61** [−7.80] (.00)	−.94** [−4.59] (.00)	71.24** (.00)	−295.06

Note. CORE equals one if empty and zero if not empty; CONTR equals one if there is a controller and zero if there is no controller. The panels (suppressed) are the trial numbers (1, . . . , 5). The fit statistic is an F-value for TIME and χ^2-values for all other dependent variables. The t-statistics are in square brackets, and two-tailed p-values are in parentheses.

[a] $R^2 = .04$.

** $P < .01$.

Table 7

Fixed-Effects Panel Data Regressions of Five Performance Measures on the Existence of the Core, Controller and an Interaction Term

	Estimation Method	CORE	CONTR	CORE × (1 − CONTR)	Fit Statistic	Log Likelihood
INEFF	Logit	.64+ [1.65] (.10)	1.04+ [1.75] (.08)	1.65** [2.47] (.01)	30.81** (.00)	−174.75
CYCLE	Logit	.14 [.38] (.71)	.65 [1.41] (.16)	1.42** [2.49] (.01)	17.15** (.00)	−173.90
TIME	Ordinary least squares	.70* [2.26] (.02)	.78** [2.46] (.01)	−.03 [−.08] (.94)	7.29** (.00)	−3,198.51[a]
PROPOSAL	Negative binomial	.37** [4.04] (.00)	.35** [3.16] (.00)	.17 [1.17] (.24)	50.98** (.00)	−997.33
EVEN	Logit	−1.53** [−5.71] (.00)	−1.05** [−3.19] (.00)	−.19 [−.45] (.65)	85.48** (.00)	−278.62

Note. CORE equals one if empty and zero if not empty; CONTR equals one if there is a controller and zero if there is no controller. The interaction term equals one when the core is empty and there is no controller and zero otherwise. The panels (suppressed) are the trial numbers (1, . . . , 5). The fit statistic is an *F*-value for TIME and χ^2-values for all other dependent variables. The *t*-statistics are in square brackets, and two-tailed *p*-values are in parentheses.

[a] $R^2 = .04$.

+ $p < .10$.

* $p < .05$.

** $p < .01$.

identical to those of Tables 6 and 7. We also employ a panel data Poisson regression for PROPOSAL and obtain results similar to the negative binomial. We further use panel data probit regressions in place of the logit regressions for INEFF, CYCLE, and EVEN and again obtain similar results. We also employ panel data ordinary least squares regressions for all dependent variables and obtain qualitatively similar results.

In additional sensitivity analyses, we redefine cycling as the number of cycles (rather than if cycling occurs in the trial). This new definition of cycling yielded very similar results.[21] For all regressions, the conventional standard errors were reestimated to obtain jackknife standard errors and bootstrap standard errors. The bootstrap standard errors are based on 50 data points per run. Overall, the jackknife and bootstrap standard errors were the same or smaller than the conventional standard errors.

5. Conclusion

Our experimental results highlight the interaction among property rights, transaction costs, and the empty core. They show that efficiency may not obtain under some property rights structures when the core is empty. Specifically, they indicate that the number of inefficient (non-Pareto-optimal) solutions and the number of bargaining rounds (trials) with cycling are significantly greater when the core is empty than when the core exists, irrespective of the initial allocation of property rights. Furthermore, we find that when the core is empty, both inefficiencies and cycling during the bargaining process are significantly more likely to occur, especially when property rights are ill defined. We also find that bargaining time and the number of bargaining proposals are significantly greater when the core is empty than when the core exists. However, we fail to find that time to resolution and the number of bargaining proposals are greater when initial property rights are ill defined even when the core is empty. We further find that the number of even splits of the payoffs among the players is affected significantly by both the existence of the core and the initial allocation of property rights.

Our results have potential implications for economic policy. They imply that voluntary bargaining mechanisms cannot always be relied on to internalize externalities and to generate mutually advantageous outcomes. As a consequence, there may be an economic role for constraints on the bargaining process—for example, specific property rights arrangements or contractual provisions—to resolve the empty core.

[21] Negative binomial regressions were estimated instead of logistic regressions since cycling is a count variable under the new definition.

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