Using Collaborative Bargaining to Develop Environmental Policy when Information is Private

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Abstract

In many cases governments invite interest groups to use collaborative bargaining to resolve environmental conflicts. If the parties fail to reach agreement, the government threatens to impose a backstop policy. Bargaining models have predicted that any agreements will be influenced, variously, by self-interest, equity, or entitlement (to the status quo). Although most such models assume that the parties are well informed about one another's utility functions, this assumption conflicts with the reality of negotiations over environmental policy. We develop a laboratory experiment to investigate the impact of private information. Subjects who bargain under this constraint are almost as likely to reach (approximately efficient) agreements as those bargaining under full information. We also find that equity plays a less important role, and entitlement a more important role, under private information than under full information. There is only limited evidence to suggest that parties are drawn to the Nash bargain.

Keywords: collaborative bargaining, consensus-building, private information, Nash bargain, egalitarian, entitlement, fairness, focal points, laboratory experiments

I. INTRODUCTION

Environmental conflicts can often be characterized as disputes among stakeholders over the allocation of public goods with multiple attributes. For example, forestry companies, ranchers, recreational users, and environmentalists may be in disagreement both about the allocation of public lands among alternative uses and about the level of constraints to be placed on those lands. Environmental economists have often assumed that in such cases policy will be set, or at least informed, by cost-benefit analysis; and they have devoted enormous creativity to the development of valuation techniques – such as contingent valuation, choice experiments, travel costs, and hedonic pricing – to provide the pecuniary estimates required for this analysis (see, for example, Champ et al. 2003 and Freeman 2003).

Yet in many cases, governments set environmental policy not by employing cost-benefit analysis, but by authorizing stakeholders to engage in collaborative bargaining.[1] For example, in the United Sates, Section 10 (a) of the Endangered Species Act requires that habitat conservation plans be drawn up for lands hosting endangered species. As the Act has been implemented, landowners submitting land use plans may have the process expedited if they form consensus-building councils with environmentalists who sign-off on the plans (e.g. Beatley 1992, Aengst, et. al, 1997). More broadly, the United States Negotiated Rule Making Act encourages government agencies to employ a process in which the groups that would be affected by a proposed regulation are given the opportunity to develop a consensus-based alternative (Pritzker and Dalton 1995). And typical of a myriad of local, informal processes, loggers and environmentalists have cooperated to develop a plan to reintroduce grizzly bears into Montana. (Fischer, 2008)

[1] Also known as negotiated rulemaking, deliberative democracy, and consensus-building.

Although the use of collaborative bargaining[2] is widespread, to date economists have paid limited attention to the efficiency or equity of this method of policy formation. We argue that collaborative bargaining places stakeholders in a situation similar to that in which they are bartering over private goods. Just as two individuals might make Pareto improving trades of their stocks of wheat and fish, two stakeholder groups might both be made better off if they could trade off changes in one aspect of environmental policy against changes in another. For example, if environmentalists and developers are in dispute over the fraction of public land that is to be set aside as environmental preserve, A, and the severity of use restrictions, R, that should be placed on that reserve, the opportunities available to them might be represented using an Edgeworth box such as Figure 1.

Figure 1 near here

Suppose a government agency must select a policy pair (A_g, R_g). Lacking credible information about the stakeholders' preference functions, the government invites them to employ unstructured bargaining to select a policy acceptable to both of them. The parties bargain knowing that if they fail to reach agreement, the government will impose a policy of its own choice. The government might announce in advance that, if agreement cannot be reached, it will maintain existing policy; or it may threaten to implement a replacement policy. Such a threat point – which becomes the parties' "backstop" position - is represented as *B* in Figure 1.

If it is assumed (i) that the only argument in the parties' utility functions is own consumption, and (ii) that all parties are well-informed about each others' utilities, the classic model of barter makes several predictions concerning the outcome that will be reached. These include that the parties will be able to reach an agreement that is Pareto superior to *B*, and that

[2] See, especially: Amy (1985), Coglianese (1997), Harter (1982), Pritzker and Dalton (1995), and Wondolleck and Yaffee (2000).

that agreement will be Pareto efficient. Further, Nash (1950) argued that the selected outcome would maximize the product of the parties' gains, relative to the backstop – the Nash bargain, represented by *N* in Figure 1.

More recently, theorists have recognized that bargainers' utility functions may contain additional arguments, such as *equity* or *entitlement*. With regard to equity, stakeholders might be willing to trade some of their own gains for outcomes that reduced inequality. With respect to entitlement, stakeholders who felt "entitled" to the status quo might balk at movements towards outcomes that were conditioned on a new backstop policy set by the government.

In this paper, we employ a laboratory experiment to test the hypotheses generated by the Nash, equity, and entitlement arguments as they relate to agreements reached under collaborative bargaining. As our interest is bargaining over environmental policy, we establish three criteria for our experimental design. First, in keeping with the multi-dimensional nature of environmental policy, our subjects negotiate over more than one characteristic, as in Figure 1. Second, to parallel real world collaborative bargaining, subjects are allowed to engage in face-to-face bargaining. Finally, in keeping with the assumption that stakeholders in environmental negotiations have only limited information about their opponents' utility functions, subjects are prevented from obtaining information about their opponents' payoffs.

To meet these criteria, we implement a private information extension of an experiment that we developed in Bruce and Clark (forthcoming), (henceforth B&C). In B&C, each subject was presented with his or her own payoff table generated from a Cobb-Douglas function over two goods, X and Y. One individual was assigned the initial allocation (X_1, Y_1) and a second the allocation (20-X_1, 20-Y_1). Subjects were then given a limited amount of time, face-to-face, to negotiate a reallocation of these goods. If they reached an agreement, each received the associated payoff from his or her payoff table; otherwise each received the payoff associated with the backstop.

In B&C, we used a full information design, providing subjects with copies of their opponent's payoff tables, and allowing them to share tables with each other. Here, we make three changes to ensure that subjects' payoff information remains private. First, the payoff tables received by subjects are fixed to immovable lecterns that are unobservable by opponents. Second, we introduce a personal exchange rate between the experimental currency subjects earn from negotiated outcomes, and their real dollar earnings. These personal exchange rates are shown privately to subjects but not given to them, and are changed after each round. Third, we switch from a between- to a within-subject design, necessitated by the change of exchange rates between rounds while replicating the four treatments of B&C.[3] As a result of these changes, we argue that any claims made by subjects to their opponents about the dollar benefits or costs they would receive at given allocations would not be verifiable or credible.

As in B&C, we present subjects with four treatments. In Treatments I and II, we set the payoff functions such that the efficient outcome at which the parties' payoffs are equalized, *E*, is the same as the Nash bargain, *N*. In Treatment I, as illustrated in Figure 1, the backstop outcome *B* is identical to the status quo *Q*, (representing the case in which the government will continue the current policy if the parties cannot reach agreement). In Treatment II, *Q* is separated from *B* and lies outside the bargaining lens associated with *B*. Treatments III and IV repeat I and II, respectively, except that exchange rates are altered so as to separate *E* from *N*, and place it outside the bargaining lens associated with *B*. Treatment IV is represented in Figure 2.

Figure 2 near here

[3] Thus, instead of subjects experiencing the same treatment with different opponents over five rounds, they experience a different treatment with a different opponent on each of four rounds.

We find that our subjects are able to reach agreements, at outcomes that are at or near Pareto efficiency, in an encouragingly high percentage of cases; although, as in B&C, we find that subjects are only weakly attracted to the Nash bargain. In keeping with the predictions made by a number of authors, we find that considerations of equity appear weaker in our experiment than in those that provide subjects with full information about their opponents' utility functions. Surprisingly, however, we find robust evidence that the status quo has a significant effect on negotiated outcomes when it differs from the backstop. These results suggest that collaborative bargaining under private information is capable of yielding efficient policies; and that these policies will be influenced by considerations of both entitlement and equity.

The remainder of the paper is organized as follows. In Section II we provide a review of the relevant literature on unstructured bargaining under private information and establish the hypotheses we will test. In Section III we describe our experimental design; while in Section IV we present our results. Section V concludes the paper with a brief discussion of the effects of private information on the use of collaborative bargaining to set public policy.

2. COLLABORATIVE BARGAINING UNDER PRIVATE INFORMATION

The literature on unstructured bargaining ascribes at least three motivations to negotiators: own consumption, equity, and entitlement. We describe these motives briefly and identify the predicted bargaining outcomes associated with each of them under private information.

2.1 Consumption (Nash)

In early formulations, most cooperative bargaining theory assumed that negotiators wished to maximize the benefits from their own consumption of the goods under consideration. In such cases, Nash (1950) predicted that parties with full information about each other's utility functions would select the outcome that maximized the product of their gains relative to the backstop, *B*.[4] The resulting Nash bargain, *N*, must be both Pareto superior to *B* and Pareto efficient, and so lie on the contract curve within the bargaining lens associated with *B*, as in Figure 1.

Nash's model required that the parties have full information about one another's preferences, an unrealistic assumption in collaborative bargaining over environmental disputes. However, Harsanyi (1977), building on earlier work by Zeuthen (1930), showed that under reasonable assumptions negotiators could reach the Nash bargain even if neither was well-informed about the other's preferences. In Harsany (1977), negotiators play a kind of "brinkmanship" game in which both attempt to commit themselves to their current positions. The impasse that would result is resolved when the party that is less willing to accept the risk of a collapse of negotiations makes a concession. Harsanyi argues that if this risk is defined as the ratio of the cost of accepting the other party's offer to the cost of accepting the backstop position, the parties will alternate making concessions until they reach the Nash bargain.

Harsanyi's model has two implications for collaborative bargaining. On the one hand, it predicts that, even in the face of private information, any negotiated outcome will approach the Nash bargain. On the other hand, because it requires that parties engage in repeated attempts to commit themselves to their respective bargaining positions, it opens the strong possibility that at some point they will misperceive their relative bargaining strengths, thereby causing negotiations

[4] Other axiomatic models have been proposed by Raiffa (1953), Kalai and Smorodinsky (1975), and Gupta and Livne (1988). We restrict our discussion to the Nash bargain, which has been the focus of most of the experimental bargaining literature.

to break down. [5] In terms of our experiment, this suggests that fewer agreements will be reached when information is private than when it is full.

2. 2 Equity

A number of experimenters - notably, Nydegger and Owen (1975), Roth, Malouf, and Murnighan (1981), Hoffman and Spitzer (1985), Shogren (1997), and Bruce and Clark (2010 and forthcoming) - find that their subjects are drawn towards Pareto efficient outcomes that equalize payoffs – illustrated as *E* in Figures 1 and 2. Fehr and Schmidt (1999) and Bolton and Ockenfels (2000) have argued that these results imply that negotiators' utility functions include an aversion to inequality.

Many authors have suggested, however, that subjects will be less likely to reach "equitable" outcomes under private than full information. One argument, presented by Roth and Malouf (1979), Roth, Malouf, and Murnighan (1981), Hoffman and Spitzer (1985), and Rhoads and Shogren (2003), is that subjects in a private information game can disguise the motive for their offers, forestalling retaliation from opponents who might object to offers based strictly on self interest. A second argument is that, even if the parties would like to obtain egalitarian outcomes, they may be unable to reach such outcomes if they lack information about their opponents' utility functions. Parties can only identify egalitarian outcomes if they are able to compare payoffs. For both reasons, we anticipate that subjects will be less likely to reach egalitarian outcomes when they have private information, as in our experiment, than when they have complete, or full, information.

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[5] Pratt and Zeckhauser (1992) have also questioned whether negotiators in multi-dimensional bargaining will be able to reach efficient agreements.

2.3 Entitlement

In B&C we argued that both the focal point (Schelling, 1960; Bazerman, 1985; and Binmore, et. al., 1989) and entitlement (Nozick, 1974; Zajac, 1995; and Gachter and Riedl, 2005) literatures suggest that if the status quo (*Q* in Figure 2) differs from the backstop, *B*, negotiated outcomes will be drawn towards efficient allocations contained in the (Nash-irrelevant) bargaining lens defined by *Q* rather than that by *B*. That is, "entitlement" might become a third argument in bargainers' utility functions.

Our expectation is that if negotiators play as the game described in Harsanyi (1977), a feeling of entitlement will have a similar effect on outcomes when parties have only private information as when they have full information. This is because in Harsanyi's model, if a party believes it is entitled to a particular position, the presence of such a position will raise the cost to that party of conceding to an opponent's offer of any other outcome; and when the party's cost of conceding increases, so will its bargaining power. As this unwillingness to concede is independent of the party's information concerning its opponent's utility function, the presence or absence of such information should not affect the qualitative predictions concerning entitlement.

2.4 Summary

Summarizing, we posit three hypotheses concerning the outcomes of bargaining:

- *Consumption*: The parties will negotiate to the Nash bargain, *N*, conditioned on *B*.
- *Entitlement*: The parties will negotiate to a Pareto efficient allocation within the bargaining lens conditioned on *Q*, not on *B*.
- *Equity*: The parties will encounter difficulty negotiating to the Pareto efficient allocation at which payoffs are equalized, *E*.

3. EXPERIMENTAL DESIGN

3.1 Structure of the Experiment

As in B&C, we recruit subjects in groups of ten, and give each an induced value payoff function over two abstract goods, *X* and *Y*. Five subjects are assigned one payoff function (denoted here for exposition as “environmentalists”), and five another (“developers”), based on their prior choice of seat in the room. To generate convex indifference curves, we use Cobb Douglas payoff functions to map from X,Y allocations to experimental currency:

$$\text{Experimental Currency}_{Env} = a_{Env} X_{Env}{}^{\alpha} Y_{Env}{}^{1-\alpha} + b_{Env} \qquad (1)$$

$$\text{Experimental Currency}_{Dev} = a_{Dev} X_{Dev}{}^{\alpha} Y_{Dev}{}^{1-\alpha} + b_{Dev} \quad . \qquad (2)$$

In the Edgeworth Box created by this specification the contract curve is a diagonal line with total constant payoffs. Each individual of type *i* is endowed with an integer allocation of ($X_{i,Q}$, $Y_{i,Q}$), with the total quantity of *X* and *Y* set at 20 units each. Across all treatments, we set the backstop *B* at ($X_{Env,B}$, $Y_{Env,B}$) = (18, 7) and ($X_{Dev,B}$, $Y_{Dev,B}$) = (2, 13), or for brevity, (18,7)/(2,13). As a result, the portion of the contract curve within the bargaining lens defined by *B* is located between (X_{Env}, Y_{Env}) = (12, 12) and (X_{Env}, Y_{Env}) = (14, 14). Because risk preference is thought to influence bargaining outcomes (Murnighan et al. 1988), we elicit subjects’ risk attitudes using the method of Holt and Laury (2002) before giving them bargaining instructions.

After reading general instructions about the bargaining to take place, subjects are then seated across from each other in pairs, one environmentalist with one developer. Each is then given specific instructions and a payoff table (denominated in experimental currency) for the first bargaining round. These payoff tables are fixed to immoveable lecterns, and thus cannot be revealed to opponents. While studying the instructions and payoff table, subjects are each privately shown a personalized slip of paper with their exchange rate from experimental currency to real (New Zealand) dollars for that round. To keep the experimental currency functions constant across all treatments, yet make the 'real' payments resulting from a given (X,Y) allocation identical with those in B&C, our individual exchange rates needed to contain both a multiplicative and additive term, or

$$\text{Real Payoff}_{Env} = c_{Env}\text{Experimental Currency} + d_{Env} \quad (3)$$

$$\text{Real Payoff}_{Dev} = c_{Dev}\text{Experimental Currency} + d_{Dev} \quad (4)$$

After studyingt their own instructions and payoff tables (denominated in experimental currency), and knowing their individual exchange rates, each pair is then allowed a four minute period of unstructured bargaining in which they can discuss mutually acceptable integer allocations of *X* and *Y*. Agreements have to be technically feasible (not exceeding a total of 20 units of X or Y), and described by one party on a form, and counter-ticked by the other with a different colored ink.

After the first bargaining round, decision slips are collected and recorded, half of subjects change seats, and then each is given instructions, a payoff table, and a new currency exchange rate for the next round. This is repeated to create four rounds in total, with each round corresponding to one of our four treatments. Across sessions, the sequence of treatments experienced is rotated systematically among eight possible orders in which the exchange rate alternates between each round.[6]

To control for the effects of accumulating income on risk preference, only one of the four rounds is implemented at the end of a session, chosen by the throw of a die. We prevent credible offers of cash side payments after the experiment by (i) ensuring that total earnings are constant

[6] Sessions were run in the order (I, III,II, IV),(I, IV, II, III), (II, III, I, IV), (II, IV, I, III), (III, I, IV, II), (III, II, IV, I), (IV, I, III, II) and (IV, II, III, I), then repeated.

along the contract curve and (ii) using a different privately held random draw for each person when being paid to determine which round to count.

Logistically, during the risk elicitation phase, the ten subjects per session are seated at widely spaced individual tables in two rows, with an empty row in between adjacent to the back row. During the bargaining phase, the front row of subjects (unbeknown to them all of one type) is turned around and seated at empty tables across from their first set of opponents. There are thus two tables separating each member of the bargaining pair. In subsequent rounds the two types alternate in having to switch one table to the right. Our design is unusual in that subjects are allowed full, unrestricted communication with their opponents during each four minute round. They are warned that threatening or abusive language will not be tolerated, and each pair's conversation is recorded with a micro-cassette player located midway between them to one side of the tables. While this unstructured, face to face communication introduces "uncontrolled aspects of social interaction" (Roth 1995) and minimizes "social distance" (Hoffman, McCabe and Smith 1996), it also parallels the in-person, unstructured negotiation used in most forms of government-sanctioned collaborative bargaining.

We argue that this game offers a stronger test of efficiency and the Nash bargain than has been available in most previous papers. Unlike the one dimensional "divide the pie" game of Roth and Malouf (1979) and others,[7] where every outcome other than disagreement is Pareto efficient, in our game fewer than three percent of potential outcomes are efficient. Also, whereas the Nash bargain in Roth and Malouf's game is "focal," in the sense that it divides the number of lottery tickets evenly, in our game N has no clear focal value relative to the backstop. Finally, unlike all other "private information" games of which we are aware, we allow subjects to bargain

[7] This game was also employed by Roth, Malouf, and Murnighan (1981) and Roth and Murnighan (1982).

face-to-face, a condition that Harsanyi's model suggests is important for the prediction that negotiators will reach a Nash bargain.

3.2 Design Features of the Treatments

As mentioned, all four treatments are implemented in every session, one in each round. These treatments each present subjects with similar payoff tables, but vary the location of the status quo allocation *Q* and the inequality of payoffs at the Nash bargain in a 2x2 design. Returning to our experimental payoff functions (1) and (2) and exchange rates (3) and (4), in all treatments we choose the *a*'s, *b*'s, α, *c*'s and *d*'s to keep constant the following:

1. the size of the Edgeworth Box: $X_{Env} + X_{Dev} = 20$ and $Y_{Env} + Y_{Dev} = 20$
2. the size of the bargaining lens (55 cells)
3. the *B* allocation: $(X_{EnvB}, Y_{EnvB}) = (18, 7)$ and $(X_{DevB}, Y_{DevB}) = (2,13)$.
4. the *N* allocation: $(X_{EnvN}, Y_{EnvN}) = (13, 13)$ and $(X_{DevN}, Y_{DevN}) = (7,7)$
5. the sum of real payoffs at *B*: $c_{Env}[a_{Env}18^{\alpha}7^{1-\alpha} + b_{Env}] + d_{Env} +$

$$c_{Dev}[a_{Dev}2^{\alpha}13^{1-\alpha} + b_{Dev}] + d_{Dev} = \$28.77$$

6. the sum of all contract curve payoffs, including at *N* or *E*:

$$c_{Env}[a_{Env}13^{\alpha}13^{1-\alpha} + b_{Env}] + d_{Env} + c_{Dev}[a_{Dev}7^{\alpha}7^{1-\alpha} + b_{Dev}] + d_{Dev} = \$45.50.$$

To ensure adequate bargaining incentives, we set the parameters to ensure that the total payoffs are substantially higher along the contract curve (including *N* or *E*) than at *Q* or *B*.

To simplify the presentation of experimental currency payoffs, subjects are provided a coloured payoff table showing the specific earnings they would receive for all feasible combinations of *X* and *Y*. The "nominal" payoff that subjects would receive for a given allocation is identical across treatments, making the tables they receive on each round similar but not identical.[8] Experiment parameters are reported in Table 1. In treatments where *Q* and *B* are identical, they are identified on a payoff table as a single yellow cell. In treatments where they differ, *Q* and *B* are identified by green and red cells, respectively. A sample payoff table for an environmentalist in Treatment II is provided in Figure 3.

Table 1 near here

Figure 3 near here

Treatment I. Treatment I is our control, with no divergence between *Q* and *B*, at (18,7)/(2,13), nor between *N* and *E*, at (13,13)/(7,7). The real payoffs for the environmentalist and developer are approximately equal at *Q*=*B*, at \$14.67 and \$14.10, respectively. Real payoffs are exactly equal at *N*=*E*, at \$22.75. Treatment I is thus a discrete implementation of Figure 1. Both the Nash and egalitarian hypotheses predict that the parties will agree to *N*; the entitlement hypothesis predicts more broadly that the parties will settle on the contract curve within the lens. This would include *N* or the adjacent Pareto efficient allocations (12,12)/(8,8) and (14,14)/(6,6).

Treatment II. In Treatment II, *Q* is separated from *B*, but all other parameters are unchanged from Treatment I. *Q* shifts "south-west" from (18,7)/(2,13) to (16,4)/(4,16), yielding unequal real initial values for the environmentalist and developer of \$0.00 and \$27.30, respectively.[9] Note that *Q* lies outside the bargaining lens created by *B*, so that an environmentalist is better off at every point within the bargaining lens associated with *B* than he or she is at *Q*, whereas the

[8] The experimental currency payoff tables have different boundaries in rounds implementing Treatments I/II vs. rounds implementing Treatments III/IV. While given allocations of X and Y always yield the same experimental currency, peripheral allocations that yield at least one party infeasible negative *real* earnings under one set of exchange rates would yield both parties positive real earnings under another. Thus Treatments I and II have 199 eligible allocations. Treatments III and IV lose 63, but gain 79, yielding 215 eligible allocations. Calculators were provided for each person.

[9] If this allocation had been the backstop, the Nash bargain would have occurred at (10,10)/(10,10), with payoffs of \$9.10 and \$36.40 respectively.

developer is worse off. In Treatment II the Nash and egalitarian hypotheses still predict agreement at *N=E*, but the entitlement hypothesis predicts that agreements will move south-west along the contract curve to lie within the "historical bargaining lens" formed by *Q*, reflecting the developer's initial advantage. These would include any of (8,8)/(12,12), (9,9)/(11,11), (10,10)/(10,10), (11,11)/(9,9) or (12,12)/(8,8).

Treatments III and IV. Treatments III and IV replicate the Treatment I/II comparison, but with *E* separated from *N*. The physical locations of *Q*, *B* and *N*, and the experimental currency they generate, remain as in Treatments I and II, but the exchange rates change so as to shift the location of *E* south-west to (10,10)/(10,10). At this allocation real earnings are equalized at $22.75 each, whereas at *N* the environmentalist and developer would earn $36.40 and $9.10, respectively. Unfortunately, the introduction of unequal real payoffs at *N* also requires the introduction of unequal real payoffs at *B*, with $28.32 and $0.45 for the environmentalist and developer, respectively. Faced with this confound, in Treatment IV we elect to equalize real payoffs at *Q* at $13.65 each. In this way, by comparing Treatments I and II we test whether *unequal* payoffs at *Q* derail agreements to equal payoffs at an *N* conditioned on an equal *B*; whereas in Treatments III and IV we test whether an *equal Q* derails agreements to an unequal *N* from an unequal *B*.

The Nash bargaining hypothesis for both Treatments III and IV is that the parties will agree to *N*. The egalitarian hypothesis is that they will agree to *E*. The entitlement hypothesis is that the parties will agree to a Pareto efficient allocation within the bargaining lens defined by *Q* (=*B)* in Treatment III as in Treatment I, but by *Q* in Treatment IV as in Treatment II.

4. THE RESULTS

Sixteen experiment sessions with ten subjects each were run at the University of Canterbury in August and September of 2010. Our within-subject design resulted in 80 decision pair outcomes for each of Treatments I, II, and III and 78 for Treatment IV (as two pairs were given faulty payoff tables in one round). Each outcome consisted of a physical allocation of X and Y between the Environmentalist and Developer, $(X_{Env}, Y_{Env})/(X_{Dev}, Y_{Dev})$, and their resulting real earnings. Each session took roughly 90 minutes, and subjects earned on average NZ $22.27 (1.00NZ$ = 0.75US$).

To provide some intuition, our results are summarized graphically in Figure 4 by treatment. We divide our discussion of results as follows. We begin by comparing agreement rates and proximity to Pareto efficiency across treatments. We then characterize the location of agreements in each treatment; and we test whether the Nash, egalitarian, or entitlement hypotheses can explain changes observed in these agreements across treatments. Finally, we provide some results comparing bargaining under full information in B&C with private information here.

4.1 Agreement Rates and Proximity to Pareto Efficient Outcomes

Our results suggest that even under conditions of private information, with as many as two hundred options facing them, subjects were able in most cases to reach agreements that were approximately Pareto efficient. Table 2 indicates, for example, that agreement rates ranged between 72 and 85 percent by treatment overall, rising with experience to the range of 80 to 95 percent by Round 4. These rates were lower when *Q* differed from *B*, but the differences were small – falling from85 percent in Treatments I and III, to 72 to 73 percent in Treatments II and IV. More formally, using the sixteen session averages for each treatment in two-tailed signed rank tests for paired samples, these differences in agreement rates were significant at the 5 percent level when payoffs at the Nash were unequal (III vs. IV, p = .04), but not when they were equal (I vs. II, p = .23). Alternatively, comparing the coefficients on treatment dummies from random effects logit regression produced similar results, although the difference in agreement rates was now close to being significant even between Treatments I and II (p = .054).[10]

Table 2 near here

With respect to Pareto efficiency, Table 2 reports that by the third round of bargaining only about a third of agreements were precisely on the contract curve for most treatments. We think, however, that a better indicator comes from measuring the physical or pecuniary deviation of agreements from the contract curve. This is because allocations immediately adjacent to the contract curve offered additional options for distributing payoffs with little sacrifice in joint earnings. Beginning with physical deviations, we measure the Euclidean distance of agreements to the nearest Pareto efficient allocation.[11] To illustrate magnitudes, an agreement one diagonal unit from the contract curve is 1.41 units away; an agreement two units from the curve is 2.83 units away, and *B* is 7.78 units away. As reported in Table 3, we find that agreements in all treatments are close to the contract curve. Overall, average distance ranged from 1.00 in Treatment I, to 1.65 in Treatment II, with no pair-wise difference between treatments significant at the 5 percent level in either sign rank or regression-based tests.

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[10] We regress pair agreements on treatment and round dummies, and the composition of the pair in terms of risk preference, age, sex, ethnicity, economics course completion, math course completion, self-reported grade average (A, B, or C range), and English as a first language. Regressions are run with risk, age and grade entered as pair averages, or alternatively as pair differences. The p value from a test comparing the coefficients on Treatment I vs. II using pair differences is reported above; the p value based on the specification using pair differences is .07.

[11] If the closest allocation on the contract curve to an agreement is $(X_{env,cc}, Y_{env,cc})$, then the Euclidean distance between them is $((X_{env} - X_{env,cc})^2 + (Y_{env} - Y_{env,cc})^2)^{1/2}$. If an agreement is equidistant to two cells on the contract curve, distance is measured to the averaged coordinates.

Similar support for Pareto efficiency comes from measuring the shortfall in joint earnings of pairs from what they could have made on the contract curve (NZ$45.50). Again to illustrate magnitudes, an agreement one diagonal unit from the contract curve would reduce joint earnings by $0.46 - $0.51 depending on where it occurred. We report in Table 3 that the average joint shortfall in earnings ranged from $0.50 in Treatment I, to $1.69 in Treatment II. As with geometric distance, we did not find any pair-wise difference between treatments to be significant.

Table 3 near here

4.2 Treatment Results

In this section, we discuss and compare the results of each of the four treatments. In Section 4.3, we summarize the relative success of own-consumption, entitlement and equity factors in predicting the agreements reached. For these purposes, we define three key measures:

- The Euclidean distance between each agreement and the Nash bargain, *N*, (at which payoffs are also equalized in Treatments I and II).
- The Euclidean distance between each agreement and the outcome (10,10)/(10,10), which is the outcome at which payoffs are equalized, *E*, in Treatments III and IV.
- An index of the relative earnings shares of the two parties at each agreement versus what the shares would have been at the two key allocations.[12] This index takes the absolute difference between the environmentalist's share of earnings at the actual agreement and at *N*, and subtracts from it the absolute difference between the environmentalist's share at the agreement and at (10,10)/(10,10). The index can range from -0.3, where a pair's division of earnings corresponds exactly to that at (10,10)/(10,10), to +0.3, where it

[12] We cannot simply compare how joint earnings differ because they are identical at *N* and *E*.

corresponds exactly to that at *N*. A value of 0 indicates that the pair's division of earnings is half way between what it would be at the two key allocations.[13]

Table 4 reports the values of these three measures for each treatment. Table 5 reports the *p* values from tests that compare whether these values differ by treatment, as predicted by the own-consumption, entitlement, or equity hypotheses.

Table 4 near here

Table 5 near here

Treatment I: Table 4 illustrates that in Treatment I, in which payoffs were equalized at the Nash bargain (*N*=*E*), and the backstop equaled the status quo (*B*=*Q*), agreements had an average Euclidean distance of roughly two units from *N*. This closeness to *N* is also reflected in the finding that agreements were further away from the allocation (10,10)/(10,10),), (3.5 units), and that the mean index of environmentalist's share of earnings was closer to the Nash bargain than to (10,10)/(10,10), at +.14. Indeed, agreements in Treatment I turn out to be closer to *N* than in any other treatment. Given that *N* here is consistent with all three bargaining hypotheses, it may be that the discrepancies that remain between it and the observed agreements may reflect the complexity of the bargaining task that subjects faced in our within-subject, private information design. Consistent with this view, we note that agreements in Treatment I appeared to move closer on average to *N*, the later in a session they were experienced (from 2.16 in Round 1 to 1.85 in Round 4).

[13] Note that this index does not capture the absolute distance of agreements to either key allocation, but only the relative success of either allocation in predicting earnings shares. Agreements north-east or south-west of the key allocations would yield values capped at -0.3 or +0.3, but this occurred in only 6 percent of agreements.

Treatment II: In Treatment II, the status quo *Q* diverged "south-west" from *B*, in favour of the developer. This created a (Nash- irrelevant) bargaining lens south-west of that defined by *B*. As Table 4 illustrates, this change caused agreements to move south-west on average, consistent with the entitlement hypothesis, but not with Nash or egalitarian bargaining. As confirmed by the signed rank and regression based test results in Table 5, Treatment II agreements were significantly further from N (13,13)/(7,7) than those in Treatment I and closer to (10,10)/(10,10); they also resulted in the environmentalists' earnings shares moving closer to what they would have been at (10,10)/(10,10). In short, the allocation that the parties started with influenced the agreements they reached, even though that allocation would not have been the backstop imposed if negotiations failed.

Treatment III: Relative to Treatment I, this treatment changed the rates at which subjects exchanged experimental for real currency, causing the payoffs at *B* and its associated *N* to become unequal in favour of the environmentalist. Relative to Treatment I, parties seeking to equalize their payoffs would need to move "south-west" from *N*, at (13,13)/(7,7), to *E*, at (10,10)/(10.10), (which lay outside the bargaining lens defined by *B*). Our findings from this treatment provide moderate support for the hypothesis that subjects either wish, or are able, to seek equitable outcomes. From Table 4 and the associated tests in Table 5, it is seen that agreements in Treatment III were significantly further away from *N* than in Treatment I, and that the environmentalists' shares of earnings grew closer to what they would have been at *E*.

Treatment IV: Comparing Treatment IV to Treatment III provides a second chance to test for the entitlement effect that arises when the status quo, *Q*, differs from the backstop, *B*. Here, however, we examine the effect of a *Q* that offers equal payoffs on support for a Nash bargain that offers unequal payoffs. In this case, entitlement effects would again pull agreements "south-west" of those in Treatment III; and this is what we find. As indicated by the distance measures in Table 4, agreements in Treatment IV are closer on average to the allocation (10,10)/(10,10) than in any other treatment, as egalitarian and entitlement effects reinforce one other at the expense of the Nash bargain. Indeed, as Table 2 indicates, a full 32 percent of pair agreements in Treatment IV were exactly at (10,10)/(10,10), in contrast to 19 percent in Treatment II, 12 percent in Treatment III, and 7 percent in Treatment I.

Similarly, comparing Treatment IV to Treatment II gives us a second chance to test for inequality aversion, as payoffs at *N* (and its associated *B*) become unequal. Unlike the Treatment I versus III comparison, however, the status quo *Q* now differs from *B*, rather than coinciding. Perhaps because of the additional degree of complexity brought by the divergence of *Q* from *B*, the moderate support we found previously for inequality aversion is now only suggestive. Although the mean distance of agreements from *N* rises from 3.26 in Treatment II to 3.72 in Treatment IV, the change is not significant in any of our three statistical tests in Table 5. Similarly, although the mean distance from (10,10)/(10,10) falls from 2.88 to 2.41, and the earnings share index falls from -.03 to -.08, these changes too are not significant.

4.3 Comparing Motivating Factors

Nash bargain: Our results provide only weak support for the Nash bargain. On the one hand, as Harsanyi's model predicts, we found that in most cases our subjects were able to reach agreements and that those agreements were both Pareto improving (relative to *B*) and "close" to Pareto efficient (making them also close to the Nash bargain). On the other hand, we found that when the status quo differed from the backstop (and, to a lesser extent, when the equal payoff outcome differed from the Nash bargain), agreements were drawn away from the Nash bargain, often becoming Pareto inferior to the backstop. .

Equity: Our hypothesis was that subjects would experience difficulty selecting the outcome that equalized payoffs, even if that was their goal, as they lacked credible information about one another's payoff functions. If we focus on comparison of Treatments II and IV, this hypothesis appears to be confirmed, as there were no statistically significant movements either away from *N* or towards *E*. However, there is some evidence that, between Treatments I and III, there was some movement away from *N* and that the environmentalists' share of earnings increased.

Entitlement: We hypothesized that bargainers would act as if one, or both, of them was "entitled" to the status quo and that, therefore, agreements would be drawn away from the bargaining lens conditioned on the backstop. Comparisons of Treatment II with I and of Treatment IV with III support this hypothesis as, in both, the status quo allocation had a significant effect on the agreements reached by the parties. Indeed, this effect was sufficiently strong that subjects were often induced to accept outcomes that were Pareto inferior to the backstop.

4.4 Full versus Private Information

As described in Section 1, in a previous paper (Bruce and Clark, forthcoming), we conducted experiments similar to those described here, but in which subjects were able to reveal their payoff functions to one another. Although there were several differences between that paper and this one, in Round 1 of Treatments I and II the two papers differed only with respect to the amount of information the parties were provided concerning their opponents' payoff functions and with respect to the length of time they were given to negotiate (three minutes in the former experiment, four here). Comparing the results from Round 1 in these two treatments, we find that private information had the following effects: First, it lowered agreement rates significantly in Treatment I (Mann Whitney two tailed p value = .013, session equals unit of observation), though not sufficiently to be significant in Treatment II (p value = .180). Second, with respect to

Pareto efficiency, private information increased the distance between agreements and the contract curve by an insignificant amount in Treatment I (*p* value = .108), and by a significant amount in Treatment II (*p* value = .043). However, in neither treatment did joint earnings drop significantly (I: *p* value = .561; II: *p* value = .083). Third, in both Treatments I and II, (where the Nash yields equal payoffs), private information increased the distance between agreements and the Nash bargain (I *p* value = .042, II *p* value = .021). Finally, agreements moved closer to the "entitlement outcome" (10,10)/(10,10) in Treatment II (*p* value = .021); and environmentalists' share of earnings decreased (*p* value = .021). In short, among inexperienced Round 1 subjects, private information significantly lowered agreement rates, but did not lower the gains parties achieved from bargaining if they did reach agreement. Private information did, however, reduce support for the Nash (= Egalitarian) allocation, and greatly amplified the effect of the status quo.

5. DISCUSSION

Although there is now a substantial literature on the use of collaborative negotiation to develop environmental policy, very little is known about the nature of the agreements that parties can be expected to reach. The purpose of this paper has been to provide some information about this question by exposing subjects to a laboratory game that incorporates many of the characteristics of real world bargaining. The most important of these are: that environmental policies are composed of multiple characteristics (thereby creating the opportunity for parties to make "trade offs"); that bargaining is unstructured and takes place face-to-face; and that parties are not well informed about one another's payoff functions.

We find that collaborative bargaining is promising: even though we provided our subjects with roughly two hundred allocations from which to choose, and gave them only limited time to negotiate, they were able to reach agreements in a high percentage of cases (between 72 and 85 percent) and they chose agreements that secured most of the potential gains from trade compared to the backstop (90 to 97 percent). At the same time, we also found that subjects were drawn away from the Nash bargain when their payoffs were unequal at the latter and when the status quo allocation differed from the backstop. We conclude that collaborative negotiation may allow stakeholders to devise mutually-acceptable, Pareto improving policies even if they are unable to reveal their preferences to one another. Furthermore, these policies will be influenced by considerations of entitlement and equity, in addition to material self-interest.

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FIGURE 1 An Edgeworth box representation of cooperative bargaining

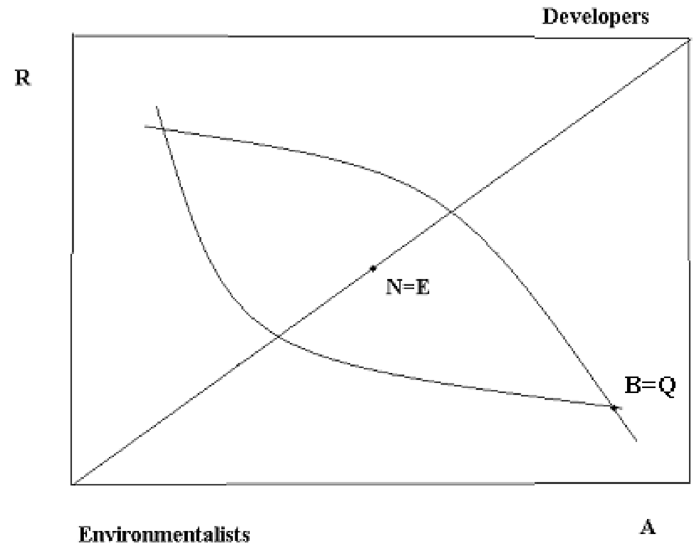


FIGURE 2 Collaborative bargaining with *Q≠B* and *N≠E*

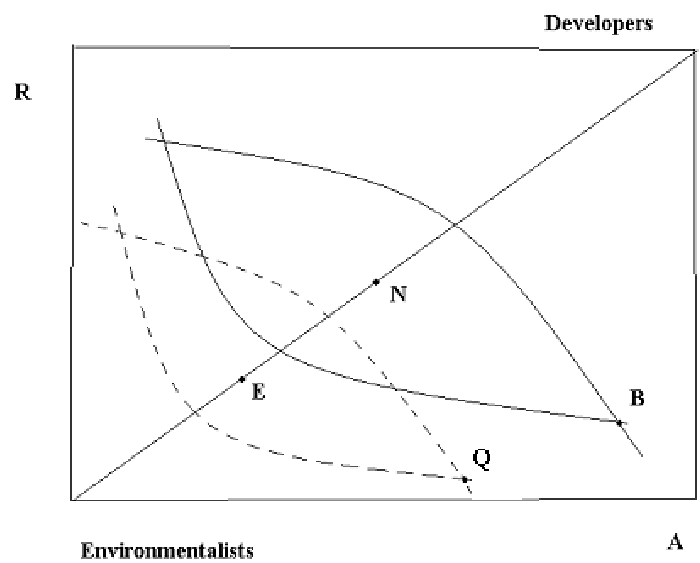


FIGURE 3 A Payoff Table Denominated in Experimental Currency (Environmentalist, Treatment II)

ID: ________

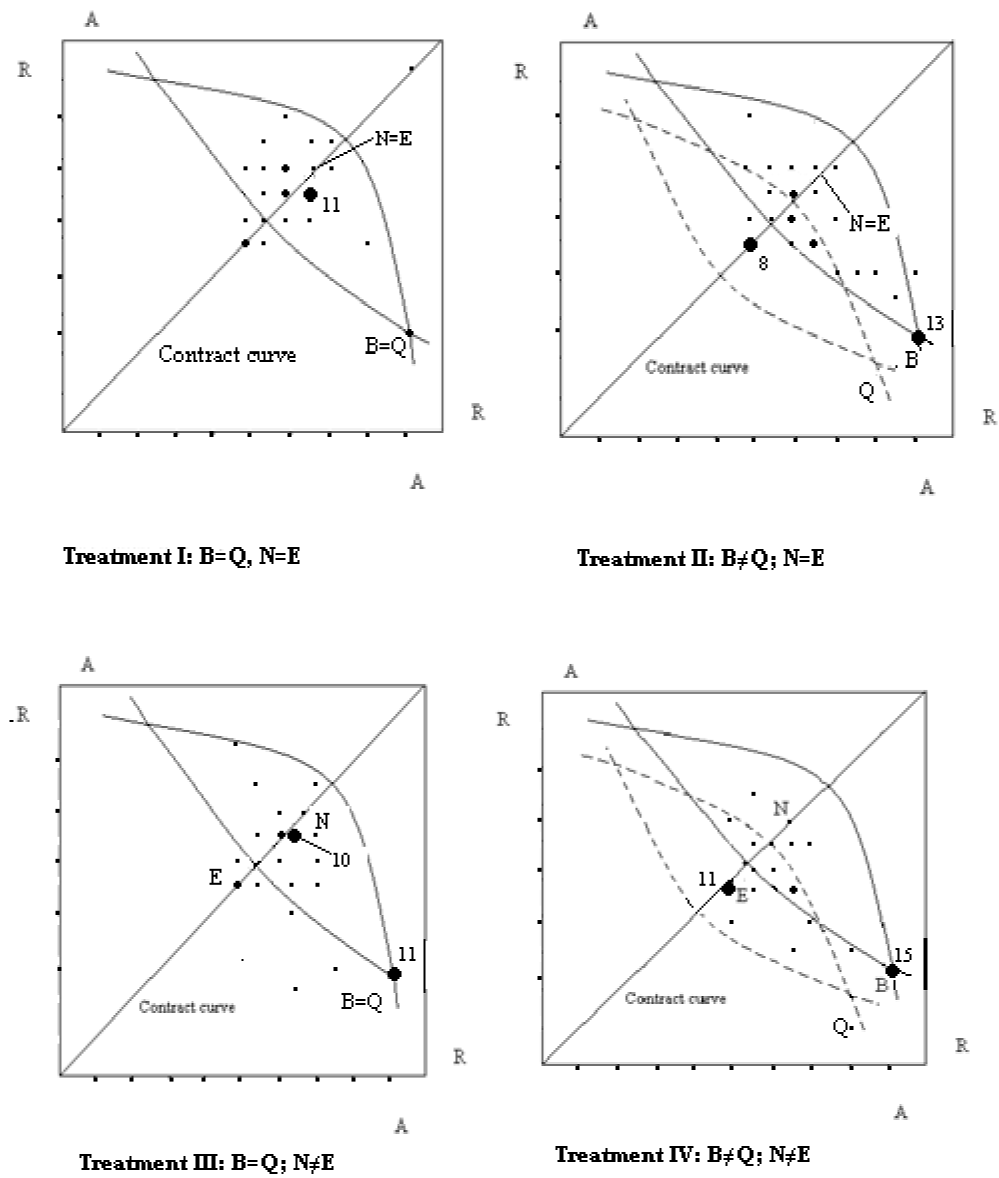
YOUR ROUND ___ EXPERIMENTAL CURRENCY EARNINGS

FROM YOUR FINAL HOLDINGS OF X AND Y

	20																					
	19					3.26	7.94	12.18	16.07	19.70	23.10	26.32	29.38	32.31	35.11	37.81	40.42	42.94				
	18					2.20	6.76	10.88	14.67	18.20	21.51	24.65	27.63	30.47	33.20	35.83	38.37	40.82	43.20	45.50		
	17					1.11	5.54	9.55	13.23	16.66	19.88	22.92	25.82	28.59	31.24	33.80	36.26	38.64	40.95	43.20		
	16					0.00	4.29	8.18	11.75	15.08	18.20	21.15	23.96	26.65	29.22	31.70	34.09	36.40	38.64	40.82	42.94	
	15						3.00	6.76	10.22	13.44	16.46	19.33	22.05	24.65	27.14	29.54	31.85	34.09	36.26	38.37	40.42	
YOUR	14						1.66	5.30	8.64	11.75	14.67	17.44	20.06	22.57	24.98	27.30	29.54	31.70	33.80	35.83	37.81	
	13						0.28	3.78	7.00	10.00	12.81	15.48	18.01	20.43	22.75	24.98	27.14	29.22	31.24	33.20	35.11	
FINAL	12							2.20	5.30	8.18	10.88	13.44	15.87	18.20	20.43	22.57	24.65	26.65	28.59	30.47	32.31	
	11							0.56	3.52	6.28	8.87	11.32	13.65	15.87	18.01	20.06	22.05	23.96	25.82	27.63	29.38	
HOLDINGS	10								1.66	4.29	6.76	9.10	11.32	13.44	15.48	17.44	19.33	21.15	22.92	24.65	26.32	
	9									2.20	4.55	6.76	8.87	10.88	12.81	14.67	16.46	18.20	19.88	21.51	23.10	
OF Y	8									0.00	2.20	4.29	6.28	8.18	10.00	11.75	13.44	15.08	16.66	18.20	19.70	
	7											1.66	3.52	5.30	7.00	8.64	10.22	11.75	13.23	14.67	16.07	
	6												0.56	2.20	3.78	5.30	6.76	8.18	9.55	10.88	12.18	
	5														0.28	1.66	3.00	4.29	5.54	6.76	7.94	
	4																	0.00	1.11	2.20	3.26	
	3																					
	2																					
	1																					
	0																					
		0	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**	**11**	**12**	**13**	**14**	**15**	**16**	**17**	**18**	**19**	**20**

YOUR FINAL HOLDINGS OF X

FIGURE 4 Observations from Rounds 2 through 4, Treatments I through IV



Note: n=60 in each of Treatments I, II, and III; n=58 in Treatment IV.

Legend:
- · 1-3 observations
- • 4-6 observations
- ● 7+ observations

TABLE 1: Parameters Used Across Treatments

All Treatments:	**Environmentalist**	**Developer**
Exp. Currency Fcn:	$U_{Env}(X,Y) = 4.55X^{1/2}Y^{1/2} - 36.41$	$U_{Dev}(X,Y) = 4.55X^{1/2}Y^{1/2} - 9.11$

Treatment I: (Q=B, N=E)	**Environmentalist**	**Developer**
Exchange Rate:	NZ$ = 1*ExpCurr + 0	NZ$ = 1*ExpCurr + 0
At *Q(=B)*:	Gets $14.67 from (18,7)	Gets $14.10 from (2,13)
At *N(=E)*:	Gets $22.75 from (13,13)	Gets $22.75 from (7,7)

Treatment II: (Q≠B, N=E)	**Environmentalist**	**Developer**
Exchange Rate:	See Treatment I.	See Treatment I.
At *Q*:	Gets $ 0.00 from (16,4)	Gets $27.30 from (4,16)
At *B*:	Gets $14.67 from (18,7)	Gets $14.10 from (2,13)
At *N(=E)*:	Gets $22.75 from (13,13)	Gets $22.75 from (7,7)

Treatment III: (Q=B, N≠E)	**Environmentalist**	**Developer**
Exchange Rate:	NZ$ = 1*ExpCurr + $13.65	NZ$ = 1*ExpCurr - $13.65
At *Q(=B)*:	Gets $28.32 from (18,7)	Gets $ 0.45 from (2,13)
At *N*:	Gets $36.40 from (13,13)	Gets $ 9.10 from (7,7)
At *E*:	Gets $22.75 from (10,10)	Gets $22.75 from (10,10)

Treatment IV: (Q≠*B*, N≠E)	**Environmentalist**	**Developer**
Exchange Rate:	See Treatment III.	See Tretament III.
At *Q*:	Gets $13.65 from (16,4)	Gets $13.65 from (4,16)
At *B*:	Gets $28.32 from (18,7)	Gets $ 0.45 from (2,13)
At *N*:	Gets $36.40 from (13,13)	Gets $ 9.10 from (7,7)
At *E*:	Gets $22.75 from (10,10)	Gets $22.75 from (10,10)

TABLE 2: Descriptive Statistics of Pair Bargaining Outcomes

	Overall	By Round When Exposed To Treatment			
		1	**2**	**3**	**4**
	N=80	N=20	N=20	N=20[1]	N=20
Agreement Rates					
T I: $Q = B, E = N$	.85	.60	.85	1.00	.95
T II: $Q \neq B, E = N$	.73	.55	.55	.85	.95
T III: $Q = B, E \neq N$	.85	.90	.75	.80	.95
T IV: $Q \neq B, E \neq N$	.72	.60	.65	.83[1]	.80
Proportion in Bargaining Lens:					
T I: $Q = B, E = N$	.89	.95	.85	.85	.90
T II: $Q \neq B, E = N$	.74	.70	.75	.70	.80
T III: $Q = B, E \neq N$	.78	.75	.80	.75	.80
T IV: $Q \neq B, E \neq N$	.65	.55	.65	.67[1]	.75
Contingent on Reaching Agreement:					
Proportion exactly on the Contract Curve:					
T I: $Q = B, E = N$	.26[2]	.08	.29	.25	.37
T II: $Q \neq B, E = N$	.31	.27	.36	.29	.32
T III: $Q = B, E \neq N$	.24	.17	.20	.38	.21
T IV: $Q \neq B, E \neq N$	.45	.67	.38	.53	.25
Proportion exactly at the Nash Bargain (13,13)/(7,7):					
T I: $Q = B, E = N$	.04[2]	.00	.06	.05	.05
T II: $Q \neq B, E = N$	.02	.00	.00	.00	.05
T III: $Q = B, E \neq N$	.03	.06	.00	.06	.00
T IV: $Q \neq B, E \neq N$	.02	.00	.00	.07	.00
Proportion exactly at (10,10)/(10,10) (Equalizes Earnings in III, IV):					
T I: $Q = B, E = N$	.07[2]	.08	.06	.10	.05
T II: $Q \neq B, E = N$	.19	.27	.27	.18	.11
T III: $Q = B, E \neq N$	.12	.11	.07	.13	.16
T IV: $Q \neq B, E \neq N$	.32	.58	.31	.27	.19

[1] N = 18 pairs, because in one session two pairs were given faulty payoff tables for Treatment IV when it was implemented as Round 3.

[2] Average calculated over 16 equally weighted session rates, though sessions contained different numbers of pairs reaching agreement for a given round.

TABLE 3: Geometric Distance and Loss in Earnings between Agreements and the Nearest Point on the Contract Curve

Treatment			Overall	By Round When Exposed to the Treatment 1	2	3	4
I	(Q=B; N=E)	Mean Distance to Contract Curve	.998 (1.002)[1]	1.473 (1.223)	.957 (1.116)	.849 (.781)	.893 (.938)
		Mean Loss (NZ$) in Joint Earnings	.50 (.88)	.90 (1.23)	.53 (1.14)	.32 (.52)	.40 (.59)
II	(Q≠B; N=E)	Mean Distance to Contract Curve	1.646 (2.010)	2.443 (3.104)	1.479 (1.687)	1.373 (1.648)	1.526 (1.720)
		Mean Loss (NZ$) in Joint Earnings	1.69 (3.87)	3.69 (7.18)	1.18 (1.95)	1.14 (2.57)	1.33 (2.77)
III	(Q=B; N≠E)	Mean Distance to Contract Curve	1.457 (1.739)	1.852 (2.193)	1.320 (1.411)	1.458 (1.834)	1.191 (1.454)
		Mean Loss (NZ$) in Joint Earnings	1.28 (3.28)	2.04 (5.31)	1.86 (1.55)	1.33 (2.45)	.87 (2.40)
IV	(Q≠B; N≠E)	Mean Distance to Contract Curve	1.414 (1.883)	.707 (1.206)	1.577 (1.852)	1.179 (1.725)	2.033 (2.351)
		Mean Loss (NZ$) in Joint Earnings	1.35 (3.08)	.47 (.93)	1.37 (2.74)	1.02 (2.12)	2.30 (4.71)

Associated Test P Values:	**Sign Rank Test** (N = 16 session averages for each treatment)	**Comparing Treatment Coefficients** Pair Average Specification[2] N = 250	Pair Difference Specification[2] N = 250
Mean Distance to CC:			
I = II?	0.234	0.085	0.106
III = IV?	0.796	0.572	0.594
I = III?	0.278	0.366	0.439
II = IV?	0.605	0.182	0.185
Mean Loss in Joint Earnings:			
I = II?	0.234	0.073	0.095
III = IV?	0.959	0.640	0.634
I = III?	0.134	0.347	0.422
II = IV?	0.642	0.203	0.196

[1] Standard deviations in parentheses. [2] See footnote 1 in Table 5 for an explanation.

TABLE 4: Mean Distance and Relative Deviation in Environmentalist's Share of Earnings between Agreements and Two Key Allocations

Treatment		Overall	By Round When Exposed to the Treatment 1	2	3	4
I (Q=B; N=E)	Distance to the Nash Bargain (13,13)/(7,7)	1.98 (1.29)[1]	2.16 (1.24)	2.18 (1.71)	1.82 (1.16)	1.85 (1.07)
	Distance to (10,10)/(10,10) (Not Equal)	3.54 (1.72)	3.66 (1.55)	3.78 (2.35)	3.08 (1.34)	3.73 (1.55)
	Index of Deviation in Env's Share of Earnings[2]	.14 (.17)	.15 (.17)	.14 (.16)	.11 (.18)	.15 (.18)
II (Q≠B; N=E)	Distance to the Nash Bargain (13,13)/(7,7)	3.26 (1.82)	4.32 (2.74)	3.20 (1.55)	3.17 (1.32)	2.75 (1.58)
	Distance to (10,10)/(10,10) (Not Equal)	2.88 (2.08)	3.19 (3.00)	2.86 (2.17)	2.43 (1.77)	3.11 (1.72)
	Index of Deviation in Env's Share of Earnings[2]	-.03 (.20)	-.12 (.21)	-.01 (.25)	-.05 (.17)	.04 (.17)
III (Q=B; N≠E)	Distance to the Nash Bargain (13,13)/(7,7)	2.59 (1.76)	2.81 (2.15)	2.56 (1.68)	2.69 (1.70)	2.31 (1.54)
	Distance to (10,10)/(10,10) (Equal)	3.31 (1.80)	3.55 (2.07)	3.17 (1.45)	3.44 (1.99)	3.08 (1.73)
	Index of Deviation in Env's Share of Earnings[2]	.06 (.20)	.10 (.21)	.05 (.19)	.05 (.22)	.06 (.21)
IV (Q≠B; N≠E)	Distance to the Nash Bargain (13,13)/(7,7)	3.72 (1.83)	4.57 (2.24)	3.77 (1.29)	3.04 (1.57)	3.67 (1.97)
	Distance to (10,10)/(10,10) (Equal)	2.41 (2.18)	1.68 (2.49)	2.29 (2.10)	2.53 (1.94)	2.94 (2.25)
	Index of Deviation in Env's Share of Earnings[2]	-.08 (.21)	-.18 (.22)	-.08 (.20)	-.01 (.22)	-.05 (.18)

[1] Standard deviations in parentheses.

[2] Ranges from -0.3, where the environmentalist's share of earnings corresponds to that at (10,10)/(10,10), to +0.3, corresponding to his share at (13,13)/(7,7).

TABLE 5: P Values from Sign Rank and Regression-Based Tests Comparing Agreements with Two Key Allocations: (Two Sided)

	Sign Rank Test	**Comparing Treatment Coefficients**	
	(N = 16 session averages for each treatment)	Pair Average Specification N = 250	Pair Difference Specification N = 250
Mean Distance to the Nash Bargain (13,13)/(7,7):			
I = II?	**0.003**	**0.000**[1]	**0.000**[1]
III = IV?	**0.006**	**0.000**	**0.000**
I = III?	**0.020**	**0.040**	**0.050**
II = IV?	0.148	0.276	0.241
Mean Distance to the Allocation (10,10)/(10,10):			
I = II?	**0.049**	**0.047**[1]	**0.037**[1]
III = IV?	**0.011**	**0.003**	**0.004**
I = III?	0.196	0.382	0.301
II = IV?	0.179	0.077	0.085
Index of Environmentalists' Share of Earnings:			
I = II?	**0.001**	**0.000**[2]	**0.000**[2]
III = IV?	**0.001**	**0.000**	**0.000**
I = III?	**0.007**	**0.036**	**0.031**
II = IV?	0.215	0.170	0.192

[1] Treatment coefficients estimated from random effects tobit regression of distance of pairs' agreement from specified allocation on treatment, round, risk aversion, age, sex, ethnicity, economics course completion, math course completion, English language status, and self-reported grade average. For risk, age and grades, pair averages or differences are tried alternatively.

[2] Treatment coefficients estimated from random effects linear regression of index of environmentalists' share of earnings on the same variables as above.