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The Effect of Entitlements and Equality on Cooperative Bargaining with Private, Unverifiable Information

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Abstract

In many bargaining situations a third party is authorized to impose a backstop position on the bargainers. Prominent examples include governments who use collaborative policy-making between stakeholders to set public policy, but also compulsory arbitration in labour negotiations. Axiomatic models of cooperative bargaining, such as the Nash bargain, presume that the status quo allocation will have no effect on the outcome parties reach if it differs from the backstop set by the third party. In contrast, experimental findings have suggested that both equality of outcomes and entitlement (where the status quo establishes a focal point) may affect the agreements bargainers reach, at least under full information. This paper extends the investigation of the effect of equality and entitlement on cooperative bargaining to the case where parties have private, unverifiable information concerning the value of outcomes. We use a two-party, two-attribute experimental design in which subjects take part in unstructured, face-to-face bargaining to jointly select from among approximately 200 potential outcomes. We find that, relative to full information, parties who bargain under private information are almost as likely to reach agreements as those under full information, and that these agreements are still approximately Pareto efficient. Further, the effect of the status quo (rather than backstop) allocation seems amplified under private information, while the effect of equality is dampened, but not eliminated.

Keywords: cooperative bargaining, private information, Nash bargain, egalitarian, entitlement, fairness, focal points

JEL Classifications: C92, D74, H44, Q58

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I. Introduction

Many public policy debates can be characterized as disputes among stakeholder groups over the selection of a public good that has multiple attributes. For example, in the environmental sphere, developers, recreational users, and environmentalists might be in conflict over both the number of acres of public land that are to be set aside to protect endangered species and the degree to which hotels, ski hill operators, and hikers will be given access to that land. Similarly, disputes about education policy may be concerned not only with the determination of the number of schools to be built, but also with the development of curriculum, the selection of the student-teacher ratio, and the provision of computers.

The Edgeworth box model provides a natural tool for analyzing many of the issues involved in these debates. Assume, for example, that environmentalists and developers are in dispute over both the area of public land that is to be set aside as environmental preserve, A, and the level of restrictions, R, that are to be place on use of that area. (R, for example, might range from as little as a restriction on off-road vehicles and motorized boats to a complete ban on hikers and canoeists.) Assuming convexity of preferences, we obtain the standard diagram, with the set of Pareto efficient points represented by the diagonal line, in Figure 1.

A government agency that is charged with the responsibility of selecting a policy defined by some pair (A_g, R_g) faces the well-known problem that it lacks credible information about the stakeholders' utility functions. Although numerous techniques have been suggested for obtaining this information – including, for example, contingent valuation, choice experiments, and plebiscites – we focus on a technique that has been used widely in the environmental arena but which has received little attention from economists: namely, collaborative decision-making.[1]

In this process, the government invites stakeholders to negotiate in unstructured bargaining to select a policy that is acceptable to all of them. If the parties fail to reach agreement, the government threatens, either explicitly or implicitly, to impose a policy of its own choice. For example, the government might announce that it would maintain the existing policy or that it would introduce some new policy that it has proposed.

If the government's threat point – which is also the parties' backstop position - is represented as *B* in Figure 1, a number of questions immediately present themselves. Will the parties be drawn towards one of the outcomes predicted by axiomatic bargaining theorists (such as Nash, 1950; or Kalai and Smorodinsky, 1975)? If the Nash bargain differs from the efficient outcome that equalizes the parties' payoffs, will the parties move from the former to the latter? And if the threat point announced by the government differs from the status quo policy, will the latter influence the outcome chosen by the parties?

Each of these questions was investigated by Bruce and Clark (2010a and 2010b: henceforth, B&C) in papers that employed laboratory experiments involving two-person, two-good bargaining games. Briefly, these experiments found that subjects were able to reach agreements that were Pareto efficient, or "near" to efficient; that when the parties

[1] Also known, variously, as negotiated rulemaking, deliberative democracy, and consensus-building. Leading sources include: Aengst et. al. (1997), Amy (1985), Coglianese (1997), Crowfoot and Wondolleck (1990), Harter (1982), Pritzker and Dalton (1995), and Wondolleck and Yaffee (2000).

received unequal payoffs at the Nash bargain, there was a tendency for them to move towards an (efficient) outcome that equalized payoffs; and that the latter tendency was magnified if the payoffs at the status quo were equal whereas the payoffs at the government-selected backstop were not. All of these results were found despite the fact that subjects were presented with roughly two hundred options from which to choose.

Extrapolation of B&C's results to "real world" situations is hampered, however, by the problem that in their experiments, subjects were either given their opponents' payoff tables (the "full information" treatments) or were allowed to reveal their tables to their opponents (a "limited information" treatment). Neither of these treatments corresponds with the situation faced by real world negotiators, who cannot credibly reveal their preference functions to their opponents (and cannot learn their opponents' preference functions in any other way). Yet, as we argued above, it is this inability to obtain information about stakeholders' preferences that has induced government agencies to consider using collaborative decision-making in the first place.

In order to better reflect real world negotiations, we sought a way to prevent subjects from revealing or obtaining credible information about each others' cardinal payoff functions, while preserving bilateral, face-to-face unstructured bargaining. In the experiment described in this paper, we took two steps to achieve this goal. First, we gave each subject only his/her own payoff table, showing the experimental currency he/she would earn from approximately 200 or more potential allocations of two goods. This payoff table was fixed to an unmovable lecturn that could not be observed by the opponent. Second, we introduced a personal exchange rate for each subject between the experimental currency he or she earned from negotiated outcomes, and real dollar earnings. These were privately shown to subjects but not given to them, and were changed for all subjects after each round. In this way, any claims made by subjects to the other party about the cardinal benefits or costs they would receive at any given allocation would not be verifiable or credible.

In most other respects, our experiment followed B&C (2010b). The payoff table presented to each subject was generated from a Cobb-Douglas function over two goods, X and Y. The first individual was assigned an initial allocation (X_1,Y_1) and the second the allocation (20-X_1, 20-Y_1). After studying their instructions and tables, pairs of subjects were then given four minutes for unstructured face-to-face bargaining to reallocate these goods. If they reached an agreement, each party received the associated value from his or her payoff table, (transformed by an exchange rate containing a scalar and additive term); otherwise each received the payoff associated with the backstop (again transformed).

In real payoff terms, we presented our subjects with the same four treatments as in B&C (2010b). In Treatments I and II, we chose payoff functions such that the Pareto efficient outcome at which the parties' payoffs were equalized, *E*, was the same as the Nash bargain, *N*. (Treatment I is depicted by Figure 1.) In Treatments III and IV, exchange rates were altered such that *E* was separated from *N*, and lay outside the bargaining lens associated with *B*. Treatment II repeated Treatment I, and Treatment IV repeated Treatment III, except that the status quo allocation, *Q*, now differed from the backstop *B*. In Treatment II the status quo offered unequal payoffs to the two parties, while in Treatment IV the status quo offered equal payoffs. Treatment IV is represented in Figure 2.

Employing these treatments, we tested the same three hypotheses that had been examined under full information in B&C (2010b). The first is that individuals prefer outcomes that increase the dollar value of their consumption of X and Y, in which case they are predicted to negotiate to the Nash bargain. The second is that individuals will have a sense of "entitlement" to their status quo allocation, in which case they are predicted to negotiate to Pareto efficient allocations contained in the (Nash irrelevant) bargaining lens defined by *Q* rather than *B*. The third is that, in face-to-face bargaining at least, individuals may be willing to sacrifice some of their private utility in exchange for an increase in the equality of final payoffs, in which case, they may be drawn towards *E*. Under full information, B&C (2010b) found some evidence that all three of these motivations were in operation. In this paper, we wished to examine whether these results would hold when subjects had access only to their own payoff tables and exchange rates, and could not credibly share this information with opposing stakeholders.

We anticipated that private information would reduce support for (efficient) egalitarian outcomes relative to Nash or entitlement-based outcomes, for two distinct reasons. First, when subjects are not able to receive credible information about their opponents' cardinal benefits, they in effect have less information with which to satisfy their *own* preferences regarding distribution – because they cannot compare their own payoffs with those of their opponent. But they still have sufficient information to determine whether a given offer increases their own utility via own consumption, either relative to *B* or relative to *Q*. In short, with equal outcomes not credibly identifiable, they may not be pursued. Second, if opponents lack credible information about subjects' cardinal benefits, then subjects may be less likely to make or accept disadvantageous equal offers based on the fear that more unequal offers would be punished with disagreement.

As expected, we found that under private information, agreement rates and support for Pareto efficiency were only slightly lower than under full information; and revealed preference for egalitarian outcomes was lessened, but not eliminated. Unexpectedly, however, we found that the effect of status quo entitlements (that differed from the backstop) was greater under private information than under full, affecting agreements both when payoffs were more and were less equal at *Q* than at *B* (and its associated Nash bargain).

The rest of the paper is organized as follows. In Section 2 we provide a review of the relevant literature in (unstructured) bargaining with private information. In Section 3 we describe our experimental design, while in Section 4 we present our results. Section 5 concludes the paper with a brief discussion of the effects of private information on the use of collaborative bargaining to set public policy.

II. Unstructured Bargaining and Private Information

Since our paper explores the Nash, entitlement, and egalitarian arguments in bargainers' utility functions, as in B&C (2010b), but under private information with "cheap talk," we briefly review the literatures relevant to this intersection.

1. Components of bargainers' utility functions

B&C (2010b) argue that the utility functions of the bargainers described in Figures 1 and 2 may contain three different arguments. First, subjects may wish to maximize their personal benefits from *consumption* of X and Y. If that was their sole motivation, Nash (1950) predicted that they would select the outcome that maximized the product of their gains relative to the backstop.[2] (See for example, Nydegger and Owen, 1975; and Roth and Malouf, 1979.) As the Nash bargain, *N*, must be both Pareto superior to *B* and Pareto efficient[3], it will lie on the contract curve within the bargaining lens associated with *B*, as in Figure 1.

Note that our two-dimensional game provides a much stronger test of these hypotheses than does the one-dimensional "divide the pie" game of Roth and Malouf (1979)[4] for two reasons: whereas in their game every outcome except "disagreement" was Pareto efficient, in our game, fewer than three percent of the possible outcomes are efficient; and whereas the Nash bargain in their game was "focal," in the sense that it divided the number of lottery tickets evenly, in our game *N* has no clear focal value relative to the backstop.

Second, a number of experimenters - notably, Nydegger and Owen (1975), Roth, Malouf, and Murnighan (1981), Hoffman and Spitzer (1985), Shogren (1997), and Bruce and Clark (2010a and b) - found that their subjects were drawn towards Pareto efficient outcomes that equalized payoffs – illustrated as *E* in Figures 1 and 2. Fehr and Schmidt

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[2] Other axiomatic models have also been proposed in 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.

[3] Some authors have questioned whether, in multi-dimensional bargaining, negotiators will be able to reach agreement or, if so, reach an efficient outcome. See, for example, Pratt and Zeckhauser (1992) and Binmore, et. al. (1998).

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

(1999) and Bolton and Ockenfels (2000) have argued that these results implied that negotiators are *equalitarian*: their utility functions include an aversion to inequality.

Finally, B&C (2010b) argued that both the focal point (Schelling, 1960; Bazerman, 1985; and Binmore, et. al., 1989) and entitlement (Nozick, 1974; and Zajac, 1995) literatures suggest that if the status quo (*Q* in Figure 2) differs from the backstop, *B*, the negotiated outcome might be drawn towards efficient allocations contained in the (Nash – irrelevant) bargaining lens defined by *Q* rather than by *B*. That is, "entitlement" might be a third argument in bargainers' utility functions.

Summarizing, we test 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 negotiate to the Pareto efficient allocation at which payoffs are equalized, *E*, even if it is not Pareto superior to *B*.

2. Private information

Many studies in unstructured bargaining have suggested that subjects will be less likely to reach "equitable" outcomes under private information than full information, either because they are able to "hide" self-interested motivation and the retaliation it might bring, (Roth and Malouf (1979), Roth, Malouf, and Murnighan (1981), Hoffman and Spitzer (1986), and Rhoads and Shogren (2003)), or because they will have difficulty identifying equitable outcomes.

3. Cheap talk?

It is often argued that when subjects play a *distributive* game, in which they divide a fixed sum – for example, in ultimatum, dictator, and "divide the pie" games – claims made by subjects in private information treatments will not be considered credible.[5] A claim by stakeholder J that he/she would "lose" $10 if he/she was to concede to K's demands, for example, will not be taken seriously by K. Empirical evidence tends to confirm this argument.[6]

As our game provides subjects with two goods to trade, however, Pareto improving trades are possible, making negotiations *integrative*. Following from Murnighan et. al. (1999), we hypothesize that the differences between outcomes under private and full information will be smaller in such games than they are in distributive games. That is, talk in our design may be "informative," rather than "cheap." First, it will not be advantageous for either party to make an offer that would leave it worse off than it would have been had it accepted its opponent's last offer. Such an offer might result in acceptance by the opponent, or cause him/her to make subsequent offers that are inferior to those that would arise from truthful revelation. This incentive to be truthful will lead stakeholder K to treat as credible any claim by J that his/her current offer is preferred to the last offer made by K. Second, although the parties have an incentive to

[5] (See Crawford, (1998), Murnighan, et. al. (1999), Croson, Boles, and Murnighan (2003), and the literature cited therein.)

[6] Croson, Boles, and Murnighan (2003) is an exception.

exaggerate their willingness to hold out for outcomes that provide them with relatively high payoffs, we predict that that incentive will be moderated by their concern that if they hold out for excessive gains, negotiations will collapse and they will earn only the backstop payoffs. As in Murninghan et. al. (1999), we anticipate that these constraints on cheap talk should allow subjects to approach Pareto efficient outcomes, even when they have only private information. That is, while cardinal claims about the amount by which one allocation is preferred to another may not be credible, ordinal claims about preference rankings will be.

III. Experimental Design

As in B&C (2010b), we recruited subjects in groups of ten, and gave each an induced value payoff function over two abstract goods, *X* and *Y*. Five subjects were assigned one payoff function (for exposition here denoted "environmentalists"), and five another ("developers"), based on their prior choice of seat in the room. To generate convex indifference curves, we used 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* was 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 *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 was 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 elicited subjects' risk attitudes using the method of Holt and Laury (2002) before given them bargaining instructions.[7]

After reading general instructions about the bargaining to take place, subjects were then seated across from each other in pairs, one environmentalist with one developer. Each was then given specific instructions and a payoff table (denominated in experimental currency) for the first bargaining round. Each subject's payoff table was visible only to that subject, attached by metal binding rings to a wooden lecturn fixed to the desk at which he/she was sitting. While studying their materials, subjects were 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 (2010b), our individual exchange rates had to contain both a multiplicative and additive term, or

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

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

Having studies their own instructions, payoff tables denominated in experimental currency, and knowing their individual exchange rates, each pair was then allowed a four minute period of unstructured bargaining in which they could discuss mutually acceptable integer allocations of X and Y. Agreements had to be technically feasible (not

[7] The outcome of the Holt Laury lottery choices was not determined until the completion of a session. As of this first draft, we have not yet conducted regression analysis that exploits subject risk preferences and demographics.

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 coloured ink.

After the first bargaining round, decision slips were collected and recorded, half of subjects changed seats, and then each were given instructions, a payoff table, and a new individual exchange rate for the next round. This was repeated to create four rounds in total, with each round corresponding to one of our four treatments. Across sessions, the sequence of treatments experienced was rotated systematically between one of 8 possible orders in which the exchange rate varied between each round.[8]

To control for the effects of accumulating income on risk preference, only one of the four rounds was implemented at the end of a session, chosen by the throw of a die. We prevented credible offers of cash side payments after the experiment by (i) ensuring that total earnings were constant 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 were 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) was turned around and seated at empty tables across from their first set of opponents. There were thus two tables separating each member of the bargaining pair. In subsequent rounds the two types alternated in having to switch one table to the right. Our design is unusual in that subjects were allowed full, unrestricted communication with their opponents during each four minute round. They

[8] 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.

were warned that threatening or abusive language would not be tolerated, and each pair's conversation was 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.

Design Features of Each Treatment

As mentioned, our four treatments are implemented in every session, one on each round. These treatments 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 chose the *a*'s, *b*'s, α, *c*'s and *d*'s in such a way as 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*:

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

In addition, we set the parameters to ensure that the total payoffs were substantially higher along the contract curve (including at *N* or *E*) than at *Q* or *B*.

To simplify the presentation of experimental currency payoffs, subjects were provided a colored payoff table showing the specific earnings they would receive for all feasible combinations of *X* and *Y*.[9] The experimental currency payoffs for given allocations were identical across treatments, making the table a subject received on each round similar though not identical.[10] The parameters for all four treatments are reported in Table 1. In treatments where *Q* and *B* were identical, they were identified on a payoff table as a single yellow cell. In treatments where they differed, *Q* and *B* were 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 treatment, with no divergence between *Q* and *B* ((18,7)/(2,13)). The real payoffs for the environmentalist and developer at *B* are approximately equal, at $14.67 and $14.10, respectively. In this treatment *N* coincides with *E* at (13,13)/(7,7), with payoffs of $22.75 for each party. Treatment I is thus a discrete implementation of Figure 1. Here both the Nash and egalitarian hypotheses predict that the parties will agree to *N*, while the entitlement hypothesis predicts only that

[9] Allocations that yield negative earnings for either party were excluded from consideration, yielding 199 possible allocations in Treatments I and II, and 215 allocations in Treatments III and IV. Calculators were provided for each person.

[10] Precisely speaking, the experimental currency payoff tables had different boundaries in rounds implementing Treatments I/II vs. rounds implementing Treatments III/IV. While given allocations of X and Y always yielded the same experimental currency, peripheral allocations that would yield at least one party negative *real* earnings under one set of exchange rates, and so be made ineligible, became non-negative and eligible in the other pair of treatments. In comparison to Treatment I/II's 199 eligible allocations, Treatment III/IV lost 63, but gained 79, yielding 215 eligible allocations.

the parties will settle on the contract curve within the lens. This would include *N*, or the adjacent Pareto efficient allocations on either side, (12,12)/(8,8) or (14,14)/(6,6).

Treatment II. In Treatment II, *Q* is separated from *B*, but all other parameters are left unchanged from Treatment I. *Q* is shifted "south-west" from (18,7)/(2,13) to (16,4)/(4,16), yielding unequal initial values for the environmentalist and developer of $0.00 and $27.30, respectively.[11] *Q* also 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 developer is worse off (except for allocations where the two lenses overlap). In Treatment II the Nash and egalitarian hypotheses still predict that the parties will agree to *N*=*E*. The entitlement hypothesis, however, predicts that agreements will move south-west along the contract curve to be within the "historical bargaining lens" formed by *Q*, reflecting the developer's initial advantage.

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

[11] 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.

I to II we test whether an *unequal* Q derails agreements to an equal N conditioned on an equal B; whereas in Treatments III to 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 B (=Q) in Treatment III, but by Q in Treatment IV.

IV. 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 pairs for each of our four treatments. 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 earnings. Each session took roughly 90 minutes, and subjects earned on average NZ $22.27 (1.00NZ$ = 0.75US$).

We divide our discussion of the 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 test whether the Nash, egalitarian, or entitlement hypotheses can explain how these agreements change across treatments in this private information setting.

<u>Agreement Rates and Proximity to the Contract Curve</u>

Table 2 provides descriptive statistics regarding agreement rates. We present these for each treatment overall, as well as disaggregated by whether subjects experienced the treatment in their first, second, third or fourth rounds. This disaggregation can show the effect of generalized learning about the bargaining environment and incentives, though not specific learning about a given treatment. As shown in Table 2, agreement rates ranged between 72% and 85% overall, but rose with experience over rounds to the range 80% to 95% by Round 4. The introduction of a divergence between initial and backstop allocations appears to lower overall agreement rates, from 85% in Treatments I or III, to 72-73% in Treatments II or IV. We test for this formally using non-parametric tests and panel regression analysis. Using the 16 session averages for each treatment in two-tailed signed rank tests for paired samples, the difference in agreement rates is significant at the 5% level when the Nash was unequal (III vs. IV, $p = .04$), but not when the Nash was equal (I vs. II, $p = .23$). Comparing the coefficients on treatments from random effects logit regression produces similar results, though the difference in agreement rates is now close to being significant even between Treatments I vs. II ($p = .054$).[12]

Were these agreements Pareto efficient? Table 2 reports the proportion of agreements that were precisely on the contract curve. We think, however, that a better indicator comes from measuring the physical or financial 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.

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[12] 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.

Beginning with physical deviations, we measure the Euclidean distance of agreements to the nearest Pareto efficient allocation.[13] To illustrate magnitudes, the distance of an agreement one diagonal unit from the contract curve would be 1.41 units; the distance of an agreement two units from the curve would be 2.83 units, and from *B* would be 7.78 units. As reported in Table 3, we find that agreements in all treatments tended to be moderately close to, though often not on, 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% level in either sign rank or regression-based tests.

Table 2 near here

Similar support for Pareto efficiency comes from measuring the shortfall in joint earnings of pairs from what was available (NZ$45.50) on the contract curve. 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. An agreement two units away would cost $1.84 - $2.03, while having *B* imposed would cost the pair $16.73. We find 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. We interpret these results to

[13] 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 was equidistant to two cells on the contract curve, distance was measured to the averaged coordinates.

confirm that, even in our cognitively demanding private information environment, support for Pareto efficiency is strong across all four treatments.[14]

Table 3 near here

Nash vs. Entitlement vs. Egalitarian Agreements

If most bargainers chose agreements that were "close" to efficient, were these agreements best explained by Nash, entitlement, or egalitarian theories? Table 4 reports three measures of the closeness of agreements to two key allocations: *N* ((13,13)/(7,7)), which equalizes payoffs in Treatments I and II, and the outcome (10,10)/(10,10), which equalizes payoffs (*E*) in Treatments III and IV. By comparing the movements of agreements across treatments relative to these two key allocations, we can identify which bargaining theories find support.

Our first two measures of closeness are the Euclidean distances between agreements and the two key allocations, respectively. As before, a one diagonal unit of deviation from a key allocation results in a distance of 1.41 units, and two results in 2.83 units. Our third measure of closeness is an index of the relative earnings shares of the two parties at agreements vs. what the shares would have been at the two key allocations.[15] The index takes the absolute value of the difference between the environmentalist's share of earnings at the actual agreement and at (13,13)/(7,7), and subtracts from it the absolute value of the difference between the environmentalist's share at the agreement and at (10,10)/(10,10). It can range from -0.3, where a pair's

[14] Our finding that parties were able to reach efficient agreements when the payoffs at the backstop were unequal (Treatments III and IV), appears inconsistent with the findings of Binmore, Shaked, and Sutton (1989), Binmore, et. al. (1991), and Binmore, et. al. (1998).

[15] We cannot simply compare joint earnings at agreements vs. at the two key allocations because joint earnings at the latter two are identical.

division of earnings corresponds to that at (10,10)/(10,10), to +0.3, where it corresponds exactly to that at *N* ((13,13)/(7,7)). A value of 0 indicates that the pair's division of earnings is half way between what it would have been at the two allocations.[16]

Table 4 near here

Table 5 near here

As Table 4 illustrates, the agreements in our control Treatment I appear closer to the allocation (13,13)/(7,7) than in any other treatment. Recall that in Treatment I this allocation was simultaneously *N*, *E*, and was consistent with the entitlement hypothesis (along with two adjacent Pareto efficient allocations). On average, agreements were roughly 2 units from *N*, or between one and two diagonal units away. This is corroborated by the fact that agreements in Treatment I were the furthest away from the allocation (10,10)/(10,10) of all treatments, and that the environmentalists' share of earnings were closer to what they would be at (13,13)/(7,7) than in any other treatment. In fact, one could interpret the discrepancies that do exist between agreements and *N/E* in Treatment I as a measure of the complexity of the bargaining task that subjects faced in our private information design. Consistent with this view, we note that agreements in Treatment I appeared to move closer to *N/E*, the later in a session subjects experienced it (from 2.16 as Round 1 to 1.85 as Round 4).

In Treatment II, the only design change from I was that the status quo allocation *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* and containing the allocation

[16] 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% of agreements.

(10,10)/(10,10) as its "Nash bargain". Consistent with the entitlement hypothesis, but not with Nash or egalitarian bargaining, this change caused agreements to move south-west on average. As illustrated in Table 4, (with signed rank and regression-based test *p* values given in Table 5), Treatment II agreements were significantly further from N/E (13,13)/(7,7) than those in Treatment I, closer to (10,10)/(10,10), and resulted in the environmentalists' earnings share moving closer to what it would be at (10,10)/(10,10). That is, under private information the allocation that the parties started with influenced the agreements they reached, even though it would not be the backstop imposed by the experimenter if negotiations failed.

Returning to Treatment I as our baseline, Treatment III changed subjects' exchange rates from experimental to real currency, which in turn changed the real earnings the parties would receive from any allocation. *B* and its associated *N* became very unequal in favour of the environmentalist. Relative to Treatment I, parties seeking equal value outcomes would again need to move "south-west" from N (13,13)/(7,7), this time to reduce inequality rather than to respect a status quo relative distribution. Environmentalists submitting to complete equality would even need to leave the bargaining lens defined by *B*. In practice, we find moderate support for reducing inequality. From Table 4 and the associated tests in Table 5, we see that Treatment III agreements were significantly further away from *N* (13,13)/(7,7) than in Treatment I, and that the environmentalist's chare of earnings grew closer to what it would be at (10,10)/(10,10). This is consistent with egalitarian, but not Nash bargaining. Perhaps because equal allocations were harder to identify under private information, the support we find for egalitarian bargaining (I vs. III) in Table 4 appears smaller in magnitude than the support we find for entitlement bargaining (I vs. II).

Additional insight into the relative support for entitlement vs. egalitarian bargaining can be gleaned by considering the agreements in Treatment IV. Comparing Treatment III to Treatment IV gives us a second chance to test for the effect of status quo entitlements *Q* that differ from the backstop *B*. Here, however, we examine the effect of an *equal* status quo entitlement on support for an *unequal* Nash bargain, whereas our comparison of Treatments I to II examined the converse. Support for entitlement bargaining would again pull Treatment III agreements "south-west" in Treatment IV. Once again, we find in Tables 4 and 5 that agreements in Treatment IV have indeed moved south-west, being both significantly further from *N* ((13,13)/(7,7)), closer to (10,10)/(10,10), and yielding a significantly lower share of earnings to environmentalists. As indicated in Table 4, agreements in Treatment IV are closer on average to the allocation that equalizes earnings than in any other treatment, as support for egalitarian and entitlement bargaining reinforce each other at the expense of Nash bargaining. Indeed, as the bottom of Table 2 indicates, a full 32% of pair agreements in Treatment IV were exactly at (10,10)/(10,10), in contrast to 19% in Treatment II, 12% in Treatment III, and 7% in Treatment I.

Finally, comparing Treatment II to Treatment IV gives us a second chance to test for the effect of egalitarian bargaining, now when *B* and *Q* differ rather than coinciding. Perhaps because the divergence of *Q* from *B* adds an additional degree of complexity, the moderate support we previously found for egalitarian bargaining is now only suggestive. The mean distance of agreements from N rises from 3.26 in Treatment II to 3.72 in

Treatment IV, but the difference is not significant in any of our three tests in Table 5. Similarly, 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, but these changes too are not significant.

V. DISCUSSION: PRIVATE INFORMATION AND PUBLIC POLICY

Bruce and Clark (2010b) found that under full information, parties to unstructured, face-to-face collaborative bargaining reached Pareto efficient agreements with surprising rapidity. They also found that parties chose the Nash bargain only when it also equalized earnings. Instead, B&C (2010b) found strong support for egalitarian efficient outcomes. In addition, entitlement effects were found when the status quo was equal and the backstop and Nash unequal, but not in the converse case.

Here, under private non-verifiable information conditions, we find that parties to unstructured, face-to-face collaborative bargaining seem almost as likely to reach agreement, and that those agreements are still approximately efficient. We find stronger support for entitlement effects, as agreements were affected both when the status quo was more equal than the backstop and Nash, but also when less equal. On the other hand, we find weaker support for egalitarian bargaining. We find that agreements shift significantly to reduce inequality when there is no divergence between the status quo and backstop, but not sufficiently to be significant when there is divergence. These findings lead us to two questions. First, how sure can we be that private information is driving any changes in results from B&C (2010b)? Second, what are the implications of our private information findings for public policy making based on collaborative bargaining?

First, it is important to recognize that we introduced two auxiliary changes in design between B&C's (2010b) full information experiment, and the current one. To keep private information unverifiable in unstructured bargaining, we added the complexity of an individual exchange rate between the experimental currency subjects saw on their payoff tables, and their actual earnings. Second, again to keep private information unverifiable, we switched from a between-subject design to a within-subject design, where subjects would experience a different treatment and alternating exchange rate on each round (with a different person), rather than the same treatment over multiple rounds (with a different person). Both of these changes would have increased the cognitive burden of the experiment for subjects beyond the increase inherent in a move to private information.

Fortunately, there are limited cases where a clean comparison remains possible between B&C's (2010b) findings and the current paper. Our current Treatments I and II used 1:1 exchange rates between experimental currency and real money, as effectively used by B&C (2010b) in all treatments. Second, those subjects who experienced Treatments I or II as their first round in our within-subject design would be indistinguishable from those in round one of B&C's (2010b) between-subject design. If we limit our comparisons then to the 20 round 1 pairs in B&C (2010b) Treatment I, 20 round 1 pairs in B&C (2010b) Treatment II, and the analogous 20 round 1 pairs in our Treatments I and II, we find that private information has the following effects. First, private information lowered agreement rates significantly in Treatment I (Mann Whitney two tailed *p* value = .013, session equals unit of observation), but not sufficiently to be significant in Treatment II (*p* value = .180). Regarding Pareto efficiency, private information increased the distance of agreements from the contract curve by an insignificant amount for Treatment I (*p* value = .108), but 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). Finally, in both Treatments I and II, (where the Nash bargain yields equal payoffs), private information increased the distance between agreements and the Nash bargain (I *p* value = .042, II *p* value = .021). Agreements moved closer to (10,10)/(10,10) in Treatment II (*p* value = .021), but not in Treatment I (*p* value = .245). In both treatments, however, private information reduced the environmentalists' share of earnings (I *p* value = .020, II *p* value = .021). In short, private information looks to have lowered agreement rates, but not significantly lowered the gains that 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 initial allocations that differed from the government/experimenter imposed backstop.

Our findings have several potential implications for governments who use collaborative bargaining by stakeholders to set public policy, whether in the environmental arena, or elsewhere. First, there is encouraging evidence that even with very limited time, roughly 200 allocations from which to choose, and only private, non-verifiable information as to preferences, subjects overall were still able to reach agreements most of the time (72% - 85%), particularly with experience (80-95%). These agreements secured most of the potential gains from trade compared to the backstop (90% - 97%). Second, governments wishing to move public policy away from historical allocations, particularly in ways that affect the relative distribution of benefits between stakeholders, may be frustrated to find that collaborative bargaining results in agreements heavily influenced by the status quo, rather than a new backstop policy. We found that a full 36%-48% of pair agreements lay outside the bargaining lens set by the backstop when the latter diverged from the parties' initial allocations, compared to only 13% in the control treatment. Thirdly, partially in line with previous bargaining experiments, we find that bargainers reveal less concern with equalizing gains from bargaining under private, non-verifiable information about payoffs than full information. But evidence of egalitarian preferences persists in our face-to-face bargaining design, albeit in weakened form, as agreements moved away from the Nash bargain towards the equal outcome when the former ceased to equalize payoffs. In one sense, this movement is all the more persuasive given the difficulty pairs would have in credibly determining the allocations at which earnings would be equalized. Finally, for scholars of axiomatic bargaining theories, we find continuing evidence that the Nash bargain is a poor predictor of the efficient outcome parties will reach under unstructured bargaining when it is not also 'focal' in other ways.

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FIGURE 1 An Edgeworth box representation of collaborative bargaining

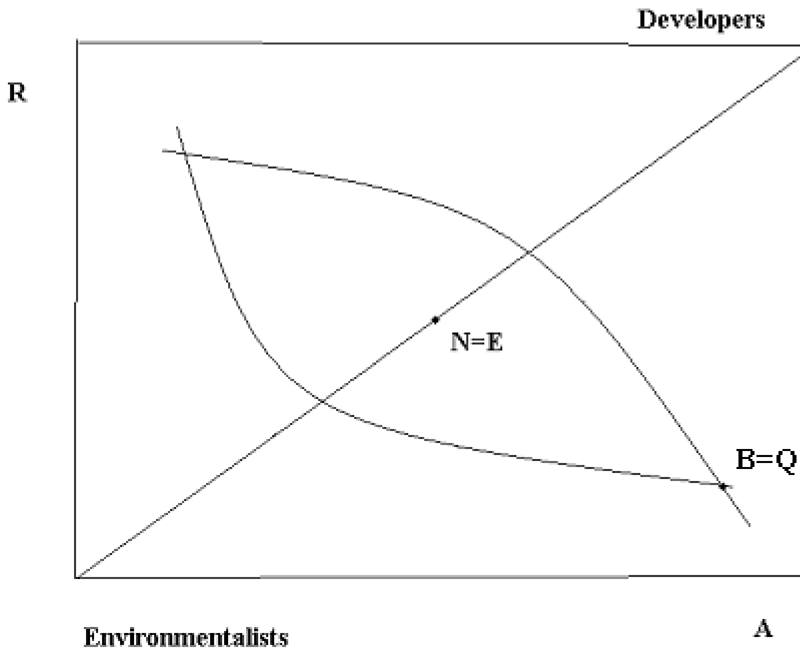

FIGURE 2 Collaborative bargaining with *Q≠B* and *N≠E*

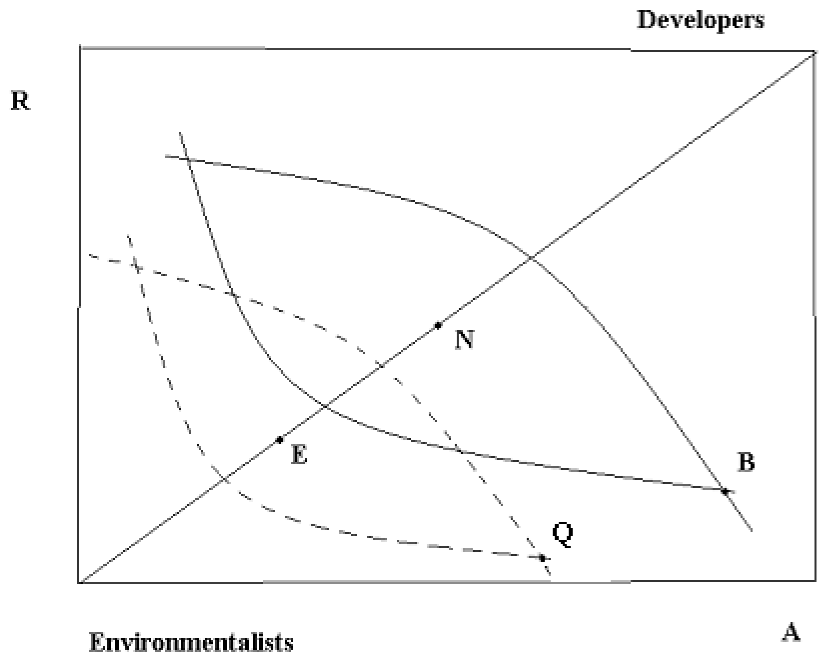

FIGURE 3 A Payoff Table Denominated in Experimental Currency (Environmentalist, Treatment II)

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

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 Environmentalists' 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 Environmentalists' 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 Environmentalists' 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 Environmentalists' 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 (N = 16 session averages for each treatment)	**Comparing Treatment Coefficients** 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.