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The efficiency of direct public involvement in
environmental policymaking: An experimental test

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Abstract: In collaborative negotiation, representatives of interested parties are charged with the responsibility of developing a set of public policies that is mutually. Although this approach has become popular in environmental negotiations, little is known about the characteristics of the outcomes that are reached. We use laboratory experiments to test whether a number of *axiomatic* models of bargaining can cast light on this question. As most such conflicts are multi-dimensional, we ask pairs of subjects to negotiate over two goods, with no cash side payments. We examine: whether parties with an initial endowment that is Pareto inefficient will make trades until they reach an efficient allocation; whether parties reach the Nash bargain when it coincides with or conflicts with outcomes that maximise the parties' joint payoffs and with outcomes at which the parties' receive equal payoffs; and whether behavior varies as subjects are provided with varying amounts of information.

Keywords: environment, decision-making, resource allocation, collaboration, experiment

JEL Classifications: C90, D74, H44, Q58

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1 Introduction

In this paper, we investigate the manner by which policy debates are resolved when multiple stakeholders are in disagreement over the selection of a public good that has multiple attributes. In the development of environmental policy, for example, environmentalists, developers, logging companies, and recreational users might be in conflict not only over the number of acres of public land that should be set aside to protect endangered species, but also over the degree to which hotels, ski hills, loggers, and hikers should be given access to that land.

Traditionally, the trade-offs among the various attributes of government policy have been made by government agents – either elected officials or their delegated employees. Recently, however, increased interest has been expressed in an alternative process that is known variously as consensus-building, mediation, conflict resolution, environmental dispute settlement, and collaborative policy making. In these approaches: negotiation processes are open to all parties, the participants are expected to reach a consensus on the policies they recommend, and the government signals that such agreements will be implemented.

Although collaborative decision making has been discussed extensively by scholars of public administration, political science, environmental design, and

forestry, among others¹, economists have been slow to analyze either the efficiency or the equity of this approach. In this paper, we attempt to rectify this lacuna both through the application of bargaining theory and through the development of laboratory experiments that compare outcomes of simulated collaboration processes under alternative rules and preferences.

Given the multi-party, multi-attribute nature of public policymaking, we begin by developing a simple Edgeworth box model to investigate the nature of the bargaining process between two parties (representing competing stakeholder groups) with conflicting payoff functions over two goods (the attributes of a public policy). Specifically, we ask whether the parties can be expected to reach consensus on the allocation of those goods; and, if so, whether any such agreements will be efficient or equitable.

Because the hypotheses that arise from our model are difficult to test in the field, our second step is to develop, and implement, a set of laboratory experiments that mimic some of the most important characteristics of environmental negotiation. For this purpose, we give each of two subjects a payoff table across all possible combinations of twenty units of X and twenty units of Y. These payoff tables differ between the subjects, but both have convex iso-payoff curves. Individual A is assigned the initial allocation (X_A, Y_A) and B the allocation $(20-X_A, 20-Y_A)$. Subjects are then given a limited amount of time to negotiate a mutually-agreeable (re)allocation of X and Y.

¹ Some of the more important contributions to this literature include Aengst et. al. (1997), Amy (1985), Coglianese (1997), Crowfoot and Wondolleck (1990), Harter (1982), Pritzker and Dalton (1995), and Wondolleck and Yaffee (2000).

The preliminary results from these experiments are encouraging. Most importantly, we find that even though our subjects had to choose from over two hundred possible outcomes, and were given only two minutes to bargain, by the time they had played the game twice, over ninety percent of pairs reached an agreement, the majority of which were both Pareto improving and either “on” or “near” the contract curve. These results give us some confidence that our experimental design offers a practical technique to study multi-issue bargaining behavior. Accordingly, in supplementary treatments, payoff tables are altered to present subjects with alternative focal points, and the amount of information about the opponent’s payoff table is also varied. Here we find evidence that the outcome that is both Pareto efficient and equitable is (slightly) preferred to the Nash bargain and to the outcome that maximized joint payoffs; and we further find, as might be expected, that information was important.

The paper is divided into five sections. In Section 2, we develop a multidimensional bargaining model based on the Edgeworth box² and use it to pose several predictions concerning the outcomes that negotiators can be expected to reach. Section 3 reviews the relevant experimental literature on bargaining and argues that if experiments are to provide information about the conduct of environmental negotiations, they must ask subjects to negotiate over more than one good. Section 4 describes such an experiment, which we use to test our predictions. In Section 5, we report the results from our experiments. Section 6 concludes with a summary and a discussion of some implications of our findings.

² Spence and Gopalkrishnan (2001) and Bruce (2006) also employ the Edgeworth box to model environmental policymaking.

2 A model of interest group negotiation

In most cases, environmental policymaking involves the development of plans to manage publicly owned natural resources. Examples include decisions about public lands, such as national forests (Tableman 1990); riparian areas, such as watersheds (Koontz et al 2004); and endangered species, such as grouse (Clifford 2002). Universally, these decisions are multi-dimensional. The policy question with respect to grouse is not: “should we have more, or fewer, grouse?” Rather, the issue is more likely to be: “should society place restrictions on the use of public (and private) lands in order to decrease the probability that grouse will become extinct; and, if so, what types of restrictions should be placed and how strict should those restrictions be?”

The multi-dimensionality of such problems argues, we suggest, for use of a bargaining model in which negotiators are required to make a number of decisions simultaneously, and thus “trade” with one another. For this purpose, we develop an Edgeworth Box model involving the government, two parties, and two characteristics. We employ that model to illustrate several predictions from bargaining theory that might be expected in public policy making.

For expository purposes, suppose that the government wishes to allocate a parcel of public land between commercial uses – such as grazing, logging, or mining – and public uses – such as recreation and wildlife preservation. Within those allocations, various restrictions may be placed on permissible uses: for example, ranchers might be required to prevent their cattle from disturbing riparian ecosystems; or limits might be set on the number of miles of roads that can be constructed. We assume that these alternatives can be captured by two characteristics: (i) the number

of acres³ of public land to be set aside as environmental reserve, A , and (ii) the level of (environmental) restrictions to be placed on the commercial and recreational use of each acre of land placed in that reserve, R .

There are three actors in the model: the *government* and two interest groups, *environmentalists* and *developers*. The government's goal is assumed to be the maximization of the sum of the interest groups' utilities. Its ability to achieve this, however, is constrained by its lack of information about the parties' true utility functions. Accordingly, the government uses its best estimate of the parties' preferences to select a backstop policy G , ($= G(A_g, R_g)$), but recognizes that G may be Pareto inefficient.⁴ In an effort to identify a superior outcome, it establishes a process in which environmentalists and developers are invited to construct their own proposal that the government would then implement. It is assumed that G will be imposed if the parties fail to reach a joint agreement. Finally, we assume that the parties are constrained to bargain only over A and R : there is no third commodity such as money that could be used to make side payments.

Environmentalists and developers can thus be said to bargain with one another "in the shadow" of policy G . Figure 1, a conventional Edgeworth Box, represents the preference functions of the two parties to this bargaining process. The horizontal axis measures the number of acres of public lands devoted to an environmental reserve, while the vertical axis measures the strength of the restrictions on each acre of land placed in that reserve. Environmentalists' indifference curves are convex to the origin

³ Alternatively, A may be thought of as the percentage of all public lands that are devoted to the environmental reserve.

⁴ In the literature on policy-making, G is sometimes referred to as the BATNA, or "best alternative to a negotiated agreement" (Fisher and Ury, (1991).

and increasing in utility with both A and R ; while developers' indifference curves are concave to the origin and increase in utility as both A and R decrease. The set of outcomes that is Pareto *superior* to G is represented by the bargaining lens between the indifference curves that intersect at G in Figure 1. The set of outcomes that is Pareto *efficient* is represented by the contract curve; whereas the subset of efficient points that lies within the bargaining lens is represented by segment LM .

If the government's objective is to maximize the sum of the parties' utilities, (that is, if it has a utilitarian, or Benthamite social welfare function), the social optimum must also be Pareto efficient and, therefore, will lie on the contract curve. However, that optimum need not fall within the bargaining lens. If G has been chosen inappropriately, it is possible that one of the parties may be worse off at the social optimum⁵ than at the backstop allocation. One such outcome has been identified as S in Figure 1.

Ideally, the government would like to know whether the parties will be able to bargain to a mutually-satisfactory agreement and, if so, whether that agreement (i) will represent an improvement on G and (ii) will approach the social optimum, S . Four predictions concerning the negotiated outcome are commonly made in the bargaining literature:

1. *Pareto superiority*: In standard versions of the Edgeworth box model, it is predicted that the outcome reached by bargaining parties will be Pareto superior to the backstop position. Note that, in the absence of side payments, this excludes the possibility that the parties will reach a joint utility-

⁵ What we have called the "social optimum" is often referred to as the Kaldor-Hicks efficient outcome. We have chosen our terminology in order to avoid confusion between Kaldor-Hicks and Pareto efficiency.

maximising outcome if the latter lies outside the bargaining lens, such as S in Figure 1.

2. *Pareto efficiency*: More specifically, it is usually argued that, in the absence of impediments, the negotiated outcome will be Pareto efficient; that is, in Figure 1, the parties are predicted to agree to an outcome on LM .
3. *The Nash bargain*: John Nash (1950) studied bargaining games in which each party was assumed to be both individually rational and well-informed about the opponent's utility function. In these cases, he showed that if the parties' behavior satisfied four intuitive axioms, they would choose an outcome that maximised the product of their respective net gains relative to the backstop, here G .⁶ This outcome is called the Nash bargain, or Nash product.
4. *Equality of outcomes*: Experimental studies often find that at least some individuals seek allocations that divide benefits in such a way as to reduce or eliminate inequality of final outcomes (e.g. Nydegger and Owen 1975 and Clark 1998).

The experiments that we report in Sections 4 and 5 are designed to test these predictions. We will focus on whether our subjects negotiate Pareto efficient agreements at the Nash bargain, particularly when (i) they are given incomplete information about their opponent's payoff function; (ii) the outcome that maximises their joint payoffs differs significantly from the Nash bargain; and (iii) the outcome at which the parties' receive equal payoffs differs from the Nash bargain.

⁶ The four axioms stated that this outcome should be (i) Pareto efficient, (ii) symmetrical, (iii) independent of irrelevant alternatives, and (iv) independent of equivalent utility representations. Nash's axioms are discussed in detail in Nydegger and Owen (1975) and Montet and Serra (2003).

3 Insights from previous bargaining experiments

While economic models of bargaining provide predictions of the outcomes to which parties will negotiate, they are difficult to test empirically, primarily because the preference orderings of the parties to real world negotiations are not public knowledge - either to the other party, or to observers of the negotiations. Laboratory experiments, although artificial, provide a means for circumventing this problem, as the (material) benefits parties receive can be induced by the experimenter. Information conditions can also be controlled, either to match those assumed in theory, or relaxed to approximate real world conditions. Experiments can also provide a standardized framework from which the effect of different negotiation rules or backstop policies can be evaluated for their effects on settlement rates, efficiency, and distribution of gains.

Experimentalists have modeled bargaining processes using either sequential games, in which the parties' only form of communication is offer and counteroffer, or continuous-time games, in which the parties are free to negotiate the terms of an agreement. In this section, we summarize only the continuous time literature as the negotiations of interest to us are conducted under those conditions.⁷

In the earliest of these experiments, pairs of subjects were told that if they could agree how to divide a fixed sum between them, they could keep the amounts that had been agreed upon, but would otherwise receive nothing. Nydegger and Owen (1975) (henceforth N&O), for example, presented subjects with three treatments. In the first, pairs were simply asked to divide one dollar. In the second, they were again

⁷ For surveys of the non-continuous time literature, see Davis and Holt (1993), Roth (1995), and Camerer (2003).

asked to divide a dollar, but with the constraint that Player I could not receive more than sixty cents. In the third, players were asked to divide sixty poker chips, which could later be cashed in at the rate of two cents per chip for Player I and one cent per chip for Player II. In the first two treatments, the Nash bargain was a 50/50 split of the dollar, whereas in the third it was a 50/50 split of chips.

As all of the bargaining pairs chose the 50/50 solution in the first two treatments, N&O concluded that support was provided for the prediction that the parties would select a Pareto efficient outcome, at the Nash bargain. However, support for this conclusion must be considered to be weak as *every* division of a fixed sum, here a dollar, is Pareto efficient: the only inefficient outcome offered to the subjects in these experiments was disagreement. The outcome of the third treatment in N&O's study was inconsistent with Nash's axiom concerning "independence of equivalent utility representations," as the parties divided the chips 20/40, resulting in equal dollar payoffs.

A number of authors (Roth and Malouf 1979, 1981, Hoffman and Spitzer 1986, and Rhoads and Shogren, 2003) have argued that the parties' apparent preference for equity over efficiency in N&O's third treatment may have arisen because they were knowledgeable about one another's utility functions, unlike in real world bargaining. Specifically, they predicted that the division of a fixed sum would approach the Nash bargain if the bargaining parties had information only about their own payoff functions (private information), and approach an equal division of the payoffs if each of the parties had full information about their own and their opponent's payoff functions. Experimental results have generally been consistent with this hypothesis.

Subsequently, a number of authors – particularly Wondolleck and Yaffee (2000) and Murningham and Roth (1988) – investigated the possibility that some of the early results arose, not because bargainers acted in accordance with Nash’s predictions, but because they were drawn towards certain “focal” outcomes (a term attributed to Schelling 1960). For example, in N&O, the Nash bargain in all three treatments was a 50/50 split. Thus, it was not possible – and is not possible in most versions of the fixed-sum game – to distinguish whether the parties who chose the 50/50 split were maximising the Nash product or simply choosing a “fair” or “reasonable” outcome.

A second stream of continuous-time experiments was initiated by Hoffman and Spitzer (1982, 1985, 1986: henceforth H&S), primarily to test the Coase Theorem. Their experiments asked subjects to make two sequential decisions. In the first stage, they were to agree on a payment option chosen from a pre-determined set, usually consisting of between six and eight pairs. (These payment pairs may be thought of as reflecting the choices facing farmers and ranchers in Coase’s classic example.) If they could agree on one of these pairs, subject A would be entitled to the first number in the pair (in dollars) and subject B would be entitled to the second. If they failed to agree, they would receive an option chosen unilaterally by one of the subjects (chosen in advance) called the “controller.” In the second stage, the subjects were directed to split the sum of their payoffs from the first round in any way that they chose.

The Coase theorem experiments may be seen as taking a first step towards the multi-dimensional bargaining that we wish to test in this paper. Implicitly, by asking the parties to choose both a bargaining outcome *and* a distribution of income, H&S created a situation in which the parties could “trade” outcomes for income. Both H&S

and Harrison and McKee (1985) found that the parties generally chose the efficient outcome in the first stage. In the second stage, however, subjects exhibited a strong tendency to divide the sum of their payoffs in such a way as to equalize earnings, rather than maximize efficiency.⁸

Like H&S and H&M, we believe that increasing the complexity of the bargaining process, by adding a second dimension to the environmental bargaining experiment, makes the results more applicable to real world situations. Our model differs from theirs in three important ways, however. First, the payoff functions in both H&S and H&M each offer only one efficient outcome. As discussed with respect to fixed sum games, this may focus bargainers' attention on that outcome, resulting in only a weak test of the hypothesis that bargaining yields efficient allocations of resources. We argue that a stronger test of the predictions set out in Section 2 could be achieved if subjects were offered a broad set of outcomes, both efficient and inefficient, from which to choose. Second, because H&S and H&M have only one efficient outcome, that outcome is also the Nash bargain. Hence, they are unable to test whether the Nash bargain would have been selected from among a set of efficient outcomes. In our experiments, the Nash bargain will be one of a large number of efficient outcomes. Third, to capture the manner in which citizen groups actually negotiate over public land use policy, we constrain the ability of the parties to make credible side payments in return for concessions on the issues that are being negotiated. Hence, our second dimension is not money, but a policy issue of interest to both parties.

⁸ H&S (1985) presented evidence that controllers behaved like "Lockeans;" that is, they were more likely to adopt a self-serving position, the greater was their feeling that they had "deserved" their position of authority.

4 Experimental design

4.1 Design features across all treatments

In our experiment, subjects were recruited in groups of ten, and each given an induced value payoff function over two abstract goods, X and Y . Half of the subjects were assigned one payoff function, and the other half a second payoff function. We shall refer to the two preference types induced by these payoff functions as environmentalists, E , and developers, D , though neutral labels were used in the experiment. We selected Cobb Douglas payoff functions for the two types to generate convex indifference curves:

$$P_E = A_E X_E^a Y_E^a + B_E \quad (1)$$

$$P_D = A_D X_D^a Y_D^a + B_D \quad (2)$$

The use of a common exponent, a , for both goods and both types implied that the contract curve would be a diagonal line. Each type of individual, i , was endowed with an initial backstop allocation of $X_{i,G}$ and $Y_{i,G}$. In order to offer our subjects sufficient numbers of choices that they would not be implicitly “directed” to the outcomes we predicted (that is, to combinations along the contract curve), we set the sums $X_{D,G} + X_{E,G}$ and $Y_{D,G} + Y_{E,G}$ each equal to twenty. We next chose a non-symmetric endowment backstop of $(X_{E,G}, Y_{E,G}) = (18, 5)$ and $(X_{D,G}, Y_{D,G}) = (2, 15)$. In all treatments, this implied that the contract curve was located roughly between $(X_E, Y_E) = (9.5, 9.5)$ and $(X_E, Y_E) = (14.5, 14.5)$.

Because risk preference is thought to influence bargaining outcomes (Murningham and Roth 1988), subjects’ risk attitudes were elicited prior to the bargaining instructions using the method of Holt and Laury (2002).

After studying their own payoff tables (and those of their opponents in the “full information” treatments) for as long as any individual wanted, subjects were then placed together in pairs, one environmentalist with one developer, and allowed a two minute period of unstructured communication in which they might agree to any alternative allocation of X and Y . To successfully register a negotiated outcome other than the backstop, one of the bargaining pair had to describe the allocation on a form, and the other had to tick a box signifying agreement.

To control for the effects of accumulating income on risk preference, only one of the five rounds was implemented at the end of experiment, chosen by the throw of a die. To prevent subjects from being able to make credible offers of side payments after the experiment, a different random draw was carried out for each person in privacy when being paid to determine which round to count.

Our mixing protocol ensured that each member of one type was paired serially with all five members of the other type during the five rounds. The experiment was conducted manually. Our design is unusual in that subjects were allowed full, unrestricted communication with their opponents during each two minute round. Subjects were provided with the following written warning:

“Please note that **all discussion must stop at the end of the 2 minute round, without exception. No threatening or abusive language will be tolerated.** People who do not respect these requirements will be asked to leave the experiment.”

We hope that the “uncontrolled aspects of social interaction” (Roth 1995) introduced by unstructured face-to-face communication were more than compensated by the increased parallelism between our design and actual face-to-face negotiations that take place between stakeholders in public policy making.

4.2 Design features of each treatment

In all treatments, we chose the A 's, B 's and a 's of the payoff functions in such a way as to keep constant the following:

1. the size of the Edgeworth Box: $X_E + X_D = 20$ and $Y_E + Y_D = 20$
2. the size of the bargaining lens: 89 cells
3. the endowments: $(X_{EG}, Y_{EG}) = (18, 5)$ and $(X_{DG}, Y_{DG}) = (2, 15)$.
4. the allocation at the Nash bargain, N: $(X_{EG}, Y_{EG}) = (12, 12)$ and $(X_{DG}, Y_{DG}) = (8, 8)$
5. the sum of backstop values: $A_E 18^a 5^a + B_E + A_D 2^a 15^a + B_D = \20 .
6. the sum of Nash bargain values: $A_E 12^a 12^a + B_E + A_D 8^a 8^a + B_D = \36.20 .

In addition, we set the parameters to ensure that the value of the joint payoff (i.e. the sum of the parties' payoffs) was substantially higher at N than at G (the backstop); and substantially higher at the joint payoff maximum than at N for treatments where these diverged. Finally, our treatments are ordered in such a way as to place increasingly stringent tests on the hypothesis that the parties would select the Nash bargain.

Treatment I

In Treatment I, we chose the parameters in such a way that the joint payoff was maximised at a single outcome (developers receive (12, 12)/environmentalists receive (8, 8)) that was also the Nash bargain *and* an allocation that equalized the money payoffs to the parties, at \$18.10 each. To simplify the presentation, subjects were provided colored payoff tables showing the specific earnings that both they and their opponent would receive from the experimenter for all feasible combinations of X and

Y.⁹ The parameters for Treatment I (and for the other treatments) are reported in Table 1; and the payoff tables presented to the Environmentalists for Treatment I are reproduced in Figure 2. Note that we did not include allocations yielding negative values for either party in the payoff tables, so that only 223 of the 400 cells in each table were feasible outcomes.¹⁰

In one sense, it might not have been difficult for the parties to reach the Nash bargain in Treatment I because they only needed to identify the (unique) outcome at which their joint payoff was maximized and earnings equalized. Even here, however, the challenge facing the parties was much more complex than that presented to subjects in the variable sum experiments that were described in Section 3, as subjects now had over two hundred outcomes from which to choose, instead of the eight or so that were offered in previous experiments.

Treatment II

In Treatment II we set the parameters in such a way that the joint payoff at every point on the contract curve now equaled the maximum, of \$36.20 – in order to address

⁹ Subjects were given a considerable amount of time to study both payoff tables, and the experiment did not proceed until every subject indicated that he or she had finished looking at the tables. Calculators were provided at each table.

¹⁰ The corresponding number of feasible allocations in Treatments II – V were 237, 201, 201 and 197, respectively. Negative cell earnings were a by-product of creating a differential between the payoff at the backstop and at the efficient outcomes that was both relatively and absolutely large. Given our other constraints, this required setting a negative value for B_i , (see equations (1) and (2)), which in turn created negative values for one or other party at the outlying allocations.

the possibility that the unique joint payoff maximum in Treatment I may have acted as a focal point. As in Treatment I, however, the parties received equal payoffs at the Nash bargain (\$18.10) and both parties were given full information about their opponent's payoffs.

This treatment may be thought of as a two-dimensional transformation of N&O's one-dimensional "divide-the-pie" game. As all points along the contract curve share the same joint payoff, movements along that curve simply reallocate that amount between the parties. Treatment II differs from N&O's game in a number of dimensions, however. First, it requires that the parties calculate the value of the maximum payoff to be divided – not a trivial task when they are given two payoff tables with over 200 entries in each. Second, unlike N&O's game, in which all but one outcome is efficient, Treatment II presents subjects with hundreds of outcomes, of which only a handful are efficient. Hence, we offer a much more stringent test of the hypothesis that subjects will reach an efficient outcome. Finally, whereas subjects in N&O's experiments were simply asked to divide a fixed sum, subjects in Treatment II could reach the joint payoff maximum only by "trading" units of X and Y – a more complex problem.

Treatment III

In Treatment III, we wished to investigate the effect of separating the joint payoff maximum from the Nash bargain. Our goal was to determine whether the parties might be drawn towards the "social optimum" when it was not included in the bargaining lens. The parameters chosen for this treatment left the individual payoffs equal to one another at the Nash bargain (\$18.10 each at (12,12)/(8,8), as in the first two treatments) but moved the maximum joint payoff to a point in the "northeast" portion of the contract curve. Specifically, joint earnings were maximized at \$47.37 at

the allocation (19,19)/(1,1). Again, the parties were given full information about one another's payoff tables.

Recognizing that subjects might also care about initial inequality, ideally we would have liked them to have started with identical endowment incomes across all three treatments. This turned out to be infeasible in Treatment III, and we settled for the second best of setting the *sum* of endowment earnings equal to the value that had been used in the first two treatments, \$20.¹¹

Treatment IV

To test for the effect of information about the opponent's preferences, Treatment IV reproduced Treatment III in all respects, except that subjects were given only their own type's payoff table. They were instructed verbally that they were under no obligation to share the information on their payoff table with the other party during negotiations, but were given the option to reveal or not reveal that information as they chose.

Treatment V

In Treatments I through IV, the Nash bargain occurred at an outcome at which the parties received equal payoffs (\$18.10 each). To test whether they would be attracted to the Nash bargain even if it offered different payoffs to the two parties, we introduced Treatment V, in which the Nash bargain still occurred within the bargaining lens at (12,12)/(8,8) and joint earnings were still \$36.20, but individual earnings there were now unequal, at \$11.97 and \$24.23. In contrast, the only equal-

¹¹ Reducing the inequality of the endowment value in Treatment III would have come at the expense of making the total earnings at the social optimum little higher than at the Nash bargain, removing joint payoff dominance.

payoff outcome along the contract curve occurred at (14,14)/(6,6) where each party earned \$20.36, (\$40.72 in total). As in Treatments III and IV, the joint payoff maximum continued to be located at (19,19)/(1,1), where total earnings remained \$47.37. And, as in Treatment IV, the subjects were given only their own payoff tables.

Finally, subjects in all five treatments completed a questionnaire supplying demographic information and explaining what they tried to achieve during bargaining.

5 The results

Twenty-one experiment sessions with ten subjects each were run at the University of Canterbury.¹² Four sessions were run per treatment, with an additional session run for Treatment IV that will be described shortly. Thus, each treatment contained forty people who provided twenty paired bargaining outcomes per round over five bargaining rounds. Each bargaining outcome consisted of the physical allocation of X and Y between an Environmentalist and a Developer, and their resulting respective earnings. Each session took roughly 90 minutes, and subjects earned on average approximately 25.00NZ\$ (1.00NZ\$ = 0.72US\$).

A complication arose with the intended final (fourth) session of Treatment IV. A subject who had participated previously in a session of Treatment I (where the Nash bargain is a prominent solution) participated again in this session, where the Nash solution was not as prominent. The potential contamination of this session's results meant that a replacement final session of Treatment IV was run. As its results were similar to those for the pairs that excluded the repeat participant in the contaminated

¹² The sessions for Treatments I-IV were run between March and May, 2007 and the sessions for Treatment V were run in September, 2007.

session, we report the results for Treatment IV using the four “clean” sessions *plus* the bargaining outcomes in the contaminated session that did not directly involve the repeat subject.¹³

We divide our discussion of the results from these experiments into six sections. In the first of these we investigate whether the parties were able to reach a mutually-acceptable agreement (other than the backstop). In the second and third, we ask whether agreements were, respectively, Pareto superior to the backstop and Pareto efficient. In the fourth and fifth sections we ask whether the parties selected outcomes that: maximised the sum of the parties’ payoffs; maximised the Nash product (i.e. chose the Nash bargain); or divided the joint payoff equally. Finally, we report on the effect of information concerning the other party’s payoff table.

5.1 Agreement rates

Despite the complexity of the task facing them, most subjects were able to agree to an outcome other than the backstop. It is seen in Table 2 that, even in the first round of negotiations, when subjects had no experience, 70 percent of the pairs who had information about their opponents’ payoff functions (Treatments I, II, and III) agreed to a new allocation. After just one round, these pairs were able to agree more than 90 percent of the time. Those who were *not* provided with information about their opponents’ payoffs (Treatments IV and V), initially found it difficult to reach agreement, with rates being only 36 and 35 percent, respectively, in the first round. Subsequently, however, agreement rates rose quickly, approaching 90 to 100 percent by subjects’ third bargaining round.

¹³ The results of all univariate tests to follow were not substantively affected by whether the contaminated session of Treatment IV was included or not.

5.2 Pareto superiority

The first prediction from Section 2 is that the bargained outcome will be Pareto superior to the backstop position; that is, that subjects will choose an outcome within the bargaining lens. This prediction was strongly supported across all five treatments. Counting outcomes at the backstop as being within the lens, Table 2 reports that the overall rates across all five rounds lay between 98 and 100 percent. For Treatments III - V these results do more than confirm elementary rationality as, in those treatments, the parties could have (jointly) gained as much as \$11.17 (relative to the Nash bargain) by settling outside the bargaining lens.

5.3 Pareto efficiency

In Table 3, we report t-tests of the null hypothesis that, when the parties reached an agreement, the mean deviation of the agreed outcomes from Pareto efficiency (i.e. from the contract curve) was zero.¹⁴ There, it is seen that this hypothesis is rejected at the five percent level only for Treatment IV in Round 1. Remarkably, when faced with two payoff tables, each of which contained roughly two hundred cells, our subjects were able to reach one of a handful of efficient (or approximately efficient) outcomes by the second round of bargaining – even when those outcomes did not

¹⁴ Because we imposed the assumption of declining marginal utility, the sum of the parties' payoffs did not vary appreciably as outcomes deviated from the contract curve. To impose a strict test of efficiency, we used geometric values to measure the distances between the bargained outcomes and the closest points on the contract curve.

maximise their joint payoffs and even when the parties were not provided with information about their opponents' payoff functions.

5.4 Maximization of joint payoffs

In Treatments III, IV, and V, the Nash bargain was separated from the socially optimal outcome (the allocation at which the sum of payoffs was maximized). In those treatments, the latter became \$47.37 - at (19,19)/(1,1) – while the former remained \$36.20 - at (12,12)/(8,8). The results from these three treatments suggest strongly that subjects were *not* drawn to outcomes that maximised joint payoffs when these lay outside the bargaining lens (and side payments were not available). Only 1 of 100 outcomes in Treatment III was at the joint maximum; and in Treatments IV and V, the corresponding rates were 1 of 120 and 0 of 100, respectively¹⁵. This suggests that if the government sets an “inappropriate” backstop policy, creating a bargaining lens that excludes the joint payoff maximum, stakeholders in public policy making may be unable to reach that maximum through subsequent bargaining.

5.5 Nash bargain and equal payoff outcomes

One of the primary goals of this paper was to test the hypothesis that subjects would select the Nash bargain, N, when faced with a large set of options, allocated over multiple characteristics. In one sense, Treatments I, II, and III provided strong support for this hypothesis. As can be seen in Table 2 and Figure 3, in Rounds 2 – 5, 82.3 percent of bargaining pairs reached N *exactly*. However, in those treatments, N

¹⁵ Furthermore, although there were outcomes that would have increased the parties' joint payoffs, relative to the Nash bargain and the equal payoff outcome, and that lay “inside” the bargaining lens, in no case was such an outcome chosen.

equaled E, the outcome at which the parties' payoffs were equalized. Hence, it was possible that it was the latter to which parties had been drawn.

To deal with this, we developed Treatment V, in which E was separated from N. The impact was dramatic. First, the percentage of outcomes that occurred exactly at either E or N (in Rounds 2 – 5) declined from 82.3 to 32.5, or 26 out of 80. Second, as can be seen in Figure 4, of those 26 outcomes, 23 occurred at E (and only three at N), suggesting that it had been E, and not N, to which subjects had been drawn in Treatments I, II, and III. The latter conclusion is further supported by the observation that whereas 46 outcomes were less than 1.5 geometric units from E, only eleven were within that distance from N. Finally, as is reported in Table 4, the mean geometric differences between the bargained outcomes and N are greater than the comparable differences between those outcomes and E in every round of Treatment V; and these differences are statistically significant (at the 6 percent level) in Rounds 3 and 4¹⁶. Thus, it appears that the tendency of our subjects to settle at or “near” N in the first four treatments resulted in part from the correspondence between N and E in those treatments.¹⁷ Once the outcome at which earnings were equalized was separated from the Nash bargain, the latter no longer appeared to be a focal point in negotiations. The apparent support for N in Treatments I-III may thus have been

¹⁶ For the purpose of this calculation, we excluded outcomes in which the subjects had failed to reach an agreement.

¹⁷ The preference for E over N in Treatment V may, in part, have arisen because joint payoffs were higher at the former (\$40.72) than at the latter (\$36.20). However, it should be noted that subjects did not show a preference for outcomes with relatively high joint earnings in Treatments III and IV.

driven in large part by equity considerations, as observed by Nydegger and Owen (1975), rather than by self-interest, as postulated by Nash.

5.6 Full versus private information

Given the complexity of the task that faced our subjects, we anticipated that they would be less able to reach efficient outcomes when they had information only about their own payoff tables than when they were also informed about their opponent's tables. Treatments III and IV, which differed only in the amount of information that was provided, offer evidence concerning this hypothesis.

First, simple visual inspection of the outcomes depicted in Figure 3 reveals that the dispersion of bargained outcomes was much greater in Treatment IV than in Treatment III. Furthermore, the data reported in Table 5 indicate that the mean geometric distance between the bargained outcomes and the contract curve was significantly less in the full information treatment, Treatment III, than in the private information treatment, Treatment IV.

At the same time, however, it must be recognized that 68 of the 90 outcomes reached in Treatment IV lay within one geometric unit of the contract curve. It might be argued that this is unremarkable as, although the subjects were not given one another's payoff tables, there was nothing to prevent them from sharing the information in those tables during the bargaining process. But this argument fails to take account of the fact that, whereas in Treatment III our subjects were given time to study both payoff tables *before* negotiations began; in Treatment IV, subjects would only have been able to see their opponents' tables *during* bargaining, and bargaining proceeded for only two minutes.

In short, although bargainers were less likely to reach efficient outcomes in the private information than in the full information treatments, the lack of information in the former did not prevent the parties from approaching the efficient set.

6 Summary and conclusions

In this paper we have set out to represent the process of public policymaking over multiple dimensions of public land use as a bargaining problem, in the context of a classical Edgeworth Box diagram. We have designed and run laboratory experiments to test whether the hypotheses drawn from standard bargaining models in economics – with respect to Pareto superiority, Pareto efficiency, Nash bargaining, and equity – might successfully predict the outcomes of such public policymaking negotiations.

In designing our experiments, we sought to create an analogy to the case in which a government sets a backstop policy prior to negotiations that the stakeholders – environmentalists and developers – must live with if negotiations fail. First, we implemented three treatments corresponding to the cases in which the backstop policy creates a bargaining lens that (i) contains the outcome that maximises the sum of the parties' payoffs, (ii) contains many outcomes that all maximise the sum of payoffs, or (iii) does not contain the payoff maximum. Cash side payments were not permitted and full information concerning all parties' payoffs was provided, to correspond to information conditions assumed by axiomatic bargaining theories. Second, we implemented a fourth treatment to model the more realistic case of private information concerning payoffs - and allowed the parties to choose whether to reveal that information during negotiations. Finally, we introduced a treatment in which the Nash bargain was separated from the outcome at which the parties' payoffs were equal.

Despite the small stakes and limited time available in these experiments, subjects reached negotiated outcomes that approached the Pareto efficient level, even when they were not provided with information about their opponent's payoff functions. We also found some evidence that subjects were drawn more to the equal payoffs outcome than to the Nash bargain, in keeping with the results of previous bargaining experiments.

Furthermore, our results indicate that when subjects are presented with a very broad array of choices, in a loosely-structured face-to-face bargaining situation, they are able to negotiate efficient agreements in a short period of time. This suggests, we believe, that the two-party, two-good experimental approach developed in this paper could provide a valuable framework within which a variety of questions about environmental consensus-building could be studied.

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Table 1: Parameters Used Across Treatments

Treatment I: (NB = Equal Payoffs = unique Social Optimum, Full Information)

	Environmental	Developer
Payoff	$U_E(X,Y) = 10.53X^{1/3}Y^{1/3} - 37.10$	$U_D(X,Y) = 9.18X^{1/3}Y^{1/3} - 18.62$
At G:	Gets \$10.10 from (18,5)	Gets \$9.90 from (2,15)
At NB:	Gets \$18.10 from (12,12)	Gets \$18.10 from (8,8)
At SO:	Gets \$18.10 from (12,12)	Gets \$18.10 from (8,8)

Treatment II: (NB = Equal Payoffs $\subset$ Social Optimum, Full Information)

	Environmental	Developer
Payoff	$U_E(X,Y) = 3.22X^{1/2}Y^{1/2} - 20.50$	$U_D(X,Y) = 3.22X^{1/2}Y^{1/2} - 7.64$
At G:	Gets \$10.02 from (18,5)	Gets \$9.98 from (2,15)
At NB:	Gets \$18.10 from (12,12)	Gets \$18.10 from (8,8)
At SO:	Jointly receive \$36.20 from any allocation (X_E, X_E) , $(20-X_E, 20-X_E)$.	

Treatment III: (NB = Equal Payoffs $\neq$ unique Social Optimal Allocation Outside Bargaining Lens, Full Information)

	Environmental	Developer
Payoff	$U_E(X,Y) = 14.80X^{1/3}Y^{1/3} - 59.45$	$U_D(X,Y) = 5.54X^{1/3}Y^{1/3} - 4.08$
At G:	Gets \$6.85 from (18,5)	Gets \$13.15 from (2,15)
At NB:	Gets \$18.10 from (12,12)	Gets \$18.10 from (8,8)
At SO:	Gets \$45.90 from (19,19)	Gets \$1.47 from (1,1)

Treatment IV: Same as Treatment III, with Partial Information

Treatment V: (NB $\neq$ Equal Payoffs $\neq$ unique Social Optimal Allocation Outside Bargaining Lens, Private Information)

	Environmental	Developer
Payoff	$U(E) = 14.80(XY)^{0.33} - 65.60$	$U(D) = 5.54(XY)^{0.33} + 2.05$
At G:	Gets \$0.72 from (18,5)	Gets \$19.28 from (2,15)
At NB:	Gets \$11.97 from (12,12)	Gets \$24.23 from (8,8)
At SO:	Gets \$39.77 from (19,19)	Gets \$7.60 from (1,1)
Equal earn.	Gets \$22.36 from (14,14)	Gets \$22.36 from (6, 6)

Table 2 Descriptive Statistics of Physical Bargaining Outcomes¹

	Round					
	1	2	3	4	5	Avg.
Agreement Rates						
Treatment I	.70	1.00	1.00	.95	1.00	.93
Treatment II	.70	.90	.95	1.00	.90	.89
Treatment III	.70	.95	.90	1.00	.95	.90
Treatment IV	.36	.85	.91	1.00	1.00	.82
Treatment V	.35	.75	.80	.90	.95	.75
Proportion in Bargaining Lens²:						
Treatment I	1.00**	1.00**	1.00**	1.00**	1.00**	1.00
Treatment II	1.00**	1.00**	1.00**	.95*	.95*	.98
Treatment III	1.00**	.95*	1.00**	1.00**	.95*	.98
Treatment IV	1.00**	1.00**	1.00**	1.00**	.96*	.99
Treatment V	.95*	1.00**	1.00**	1.00**	1.00**	.99
Proportion on the Contract Curve²						
Treatment I	.40*	.90*	1.00**	.95*	1.00**	.85
Treatment II	.45*	.60*	.80*	.89*	.85*	.72
Treatment III	.50*	.79*	.70*	.90*	.90*	.76
Treatment IV	.09	.25*	.17*	.35*	.42*	.26
Treatment V	.10	.30*	.40*	.40*	.45*	.33
Proportion at the Nash Bargain²						
Treatment I	.35*	.85*	.95*	.90*	1.00**	.81
Treatment II	.40*	.60*	.80*	.89*	.85*	.71
Treatment III	.45*	.74*	.65*	.85*	.80*	.70
Treatment IV	.00	.10*	.13*	.35*	.38*	.19
Treatment V	.00	.00	.05	.05	.05	.03

¹ Only technically efficient bargaining allocations included.

² 95% exact two tailed binomial confidence intervals are constructed around the sample means for each round for this bargaining prediction using Stata 10.0.

** refers to sample proportion for which the null that every bargaining outcome satisfies the theory cannot be rejected (the “strong test.”). This requires 100% consistency with the theory.

* refers to sample proportion for which the null that bargaining outcomes were random (equally likely across the feasible Edgeworth Box) can be rejected at the 5% level in a two tailed test (the “weak test.”) In Lens: TI: $H_0=.40$, TII: $H_0=.38$, TIII, IV: $H_0=.44$, TV: $H_0=.45$. On Contract Curve: TI, II, III, IV and V: $H_0=.02$. At Nash Bargain: TI, II: $H_0=.004$, TIII, IV, V: $H_0 = .005$.

Table 3 Geometric Distance of Outcomes from Contract Curve

Treatment	Round 1	Round 2	Round 3	Round 4	Round 5
I mean	.4040714	.03535	0	0	0
N	14	20	20	19	20
P value	0.4296	0.6663	..	..	..
II mean	.6565714	.3142222	.4093684	0	.0392778
N	14	18	19	19	18
P value	0.0966	0.1192	0.1498	1.0000	0.3313
III mean	.404	.1963889	.1571111	.03535	.0372105
N	14	18	18	20	19
P value	0.0554	0.1355	0.3605	0.6663	0.3306
IV mean	1.41425	.3327647	-.2356667	.3381739	.3535
N	8	17	21	23	24
P value	0.0456	0.3322	0.3392	0.2294	0.0830
V mean	.707	.0942667	-.1325	-.3535	.1861053
N	7	15	16	18	19
P value	0.1106	0.7090	0.7095	0.1659	0.6351

Bold terms: reject null hypothesis that mean of distribution equals Pareto efficient outcome (i.e.a point on contract curve).

Table 4 T tests of whether private information affected the probability that subjects reached the contract curve.

	Round 1	Round 2	Round 3	Round 4	Round 5
TIII Mean	.5	.7894737	.7	.9	.9
N	20	19	20	20	20
TIV Mean	.0909091	.25	.173913	.3478261	.4166667
N	22	20	23	23	24
P value	.0039	0.0004	0.0003	0.0001	0.0004

All p values from two sided tests. Equal variance of two samples not assumed. Tests that reject H_0 at 5% level in bold.

Table 5 Descriptive Statistics of Financial Bargaining Outcomes¹

	Round					Ave
	1	2	3	4	5	
Mean Absolute Value of Deviations Of Joint Earnings From Nearest Pareto Efficient Joint Earnings (NZ\$) ² :						
Treatment I (NB=EE=SO, full info):	5.23	.02*	.00**	.81*	.00**	1.21
Treatment II (NB=EE $\subset$ SO, full info):	5.12	1.73*	1.05*	.01*	1.62*	1.91
Treatment III (NB=EE $\neq$ SO, full info):	4.54	.82*	1.56*	.02*	.75*	1.54
Treatment IV (NB=EE $\neq$ SO, private):	9.73 [⊥]	2.48	1.45*	.25*	.14*	2.81
Treatment V (NB $\neq$ EE $\neq$ SO, private):	9.77 [⊥]	3.82	3.20	1.64*	1.14*	3.91
Mean Absolute Value of Deviations Of Joint Earnings From Nash Bargain Joint Earnings (NZ\$) ¹ :						
Treatment I:	5.27	.04*	.02*	.83*	.00**	1.23
Treatment II:	5.12	1.73*	1.05*	.01*	1.62*	1.91
Treatment III:	5.18	1.53*	2.07*	.21*	1.56*	2.11
Treatment IV:	11.50 [⊥]	4.59	2.40*	1.15*	1.24*	4.18
Treatment V	12.15 [⊥]	6.80	5.80	4.67	3.93*	6.67

¹ Only technically efficient bargaining outcomes included.

²t tests are conducted to test whether the sample means for each round differ from what would be expected under the prediction of Pareto efficient or Nash bargaining using Stata 10.0.

** refers to sample mean for which the null that every bargaining outcome has zero deviation from the predicted joint earnings cannot be rejected (the “strong test”). This requires zero deviations in earnings for all bargaining pairs.

* refers to sample mean for which the null that joint earnings deviations were those that would result from random bargaining (with equally likely outcomes across the feasible Edgeworth Box) can be rejected at the 5% level in a two tailed test (the “weak test”). The deviations from Pareto efficient joint earnings that could be expected under random bargaining were: TI: \$5.34, TII: \$5.23, TIII: \$4.09, TIV: \$4.09 and TV: \$5.49. The deviations from Nash bargain joint earnings that could be expected under random bargaining were TI: \$6.02, TII: \$5.23, TIII: \$6.92, TIV: \$6.92, TV: \$6.53.

[⊥] refers to sample means for which the null of random bargaining can be rejected, but because joint earnings deviations from Pareto efficient or Nash levels are *greater* than would be predicted, rather than less. (This arose from high disagreement rates in early rounds).

Figure 1: An Edgeworth Box Representation of Land Use Conflict

ID _____

[illegible][illegible]

OTHER'S PAYOFF FUNCTION: $\$9.18 X^{1/3} Y^{1/3} - \18.62

¹⁸ Note that, when given to participants, these tables each took a full sheet of paper, and that the “Other’s” payoff table was printed on differently colored paper.

**Figure 3 Bargained Outcomes in Round 5 of Treatments I - IV:
Subjects' Payoffs Equal at Nash Bargain**

(a) I: Nash bargain occurs at social optimum

(b) II: All points on contract curve socially optimal

(c) III: Social optimum differs from

**(d) IV: Treatment III with private
Nash bargain information**

Legend N: Nash bargain; E: parties obtain equal payoffs; S: social optimum (parties' joint payoffs maximised). Each dot refers to an outcome bargained by subjects in round 5 of the respective treatments. Dots with numbers refer to outcomes reached by more than one pair. There are twenty outcomes in each of Treatments I, II, and III, and twenty four outcomes in Treatment IV.

Figure 4 **Bargained Outcomes in Round 5 of Treatment V:
Subjects' Payoffs not Equal at Nash Bargain**

Legend See Table 3.