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Utilitarian, Egalitarian, and Historical Entitlement Theories of Collaborative Bargaining: An Experimental Test

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Abstract: Many public policy debates can be characterized as disagreements among multiple stakeholders about the selection of a public good that has multiple attributes. A common method for resolving such disputes is collaborative decision-making, in which representatives of interest groups negotiate new policies "in the shadow" of government-imposed backstop policies. Although this technique is widely used, little is known about the nature of the policies that it generates. The purpose of this paper is to identify several hypotheses about collaborative decision-making using the Edgeworth box framework, and to test these hypotheses using laboratory experiments. Specifically, we investigate the predictive success of three theories of negotiator motivation: utilitarian (attraction to the Nash bargain), egalitarian (equalization of the parties' payoffs), and historical entitlement (maintenance of the status quo, or "historical" distribution of payoffs). Using a full information, face-to-face bargaining design, we find that parties successfully reach approximately Pareto efficient agreements. When the Nash bargain differs from the efficient outcome at which payoffs are equalized, our subjects are drawn partially towards the latter. When the historical entitlement is also egalitarian, the parties are drawn even further from the Nash toward the efficient equal payoff outcome. Thus, although both utility-maximization and entitlement affect which Pareto efficient outcome is reached, equality is the dominant motivating factor.

Keywords: collaborative decision-making, Nash bargain, utilitarian, egalitarian, historical entitlement, fairness

JEL Classifications: C92, D74, H44, Q58

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1 Introduction

Many public policy debates can be characterized as disagreements among multiple stakeholders about the selection of a public good that has multiple attributes. In environmental policy-making for example, environmentalists, developers, and recreational users might be in conflict not only over the number of acres of public land that are to be set aside to protect endangered species, but also over the degree to which hotels, ski hills, and mining companies will be given access to that land. Similarly, disputes about education provision may be concerned not only with the determination of the number of schools to be built in a district, but also with the development of curriculum, the selection of the pupil-teacher ratio, and the provision of computer laboratories.

Traditionally, the necessary trade-offs among the various attributes of government policy have been made by government agents – either elected officials or their delegated employees – often after consultation with the affected parties. Recently, however, interest has been expressed in an alternative process that is known variously as collaborative decision-making, negotiated rulemaking, deliberative democracy, and consensus-building.[1] In most of these approaches, the government

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[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).

invites representatives from all interested groups to collaborate in person on the development of a new policy. If a consensus is reached among them, the government implements the selected policy; if no consensus is reached, the government implements a “backstop” policy. In some cases the latter is simply a continuation of the existing, or historical policy; in others it is the introduction of a new policy that has been announced by the government.

Although collaborative decision-making has been extensively discussed by scholars of public administration, political science, environmental design, and sociology, among others, economists have been slow to analyze either the efficiency or the equity of this approach. In this paper, we use laboratory experiments to provide some of the first tests of economic theory concerning these factors. Specifically, we employ an Edgeworth box framework to investigate whether two parties, with conflicting payoff functions over two goods (the attributes of a public policy), are able to reach consensus on the allocation of those goods, and to identify what the characteristics of such a consensus will be.

Our benchmark hypothesis, based on axiomatic bargaining theory, is that if subjects are utilitarian, they will select the Nash bargain – the outcome that maximizes the products of their gains relative to the backstop position.[2] However, we also consider two alternative hypotheses that have been proposed in the bargaining literature. First, we investigate the possibility that subjects are egalitarian, favoring efficient outcomes that equalize their payoffs even when that means deviating from

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[2] Note: The prediction that the parties will select the Nash bargain also implies (i) that the parties will reach an agreement; and (ii) that any outcome they choose will be both Pareto superior to the backstop and Pareto efficient. As those hypotheses were tested extensively by Bruce and Clark (2008), they are not the focus of this paper.

the Nash bargain. Second, we test the hypothesis that bargainers' perceptions of the "fairness" of the backstop might be influenced by their relative allocations at the status quo – their historical entitlement. In that case, if the backstop chosen by the government differs from the historical policy, the parties might be drawn away from the Nash bargain towards an efficient outcome that reflects the parties' historical relationship. Importantly, if the latter were to occur, the government's ability to move public policy away from the status quo using collaborative bargaining would be restricted.

We employ four experimental treatments. In Treatment I, the parties' payoffs are equal at the Nash bargain and the government announces that if the parties fail to reach agreement, the status quo will be maintained; (that is, the historical position is also the backstop). In Treatment II, we leave the backstop position and equal payoff Nash bargain unchanged, but begin with a historical policy that has unequal payoffs. In Treatment III, we again align the historical position with the backstop, but choose a payoff structure such that the parties' payoffs are unequal at the Nash bargain – thereby separating the latter from the efficient outcome that equalizes payoffs. Finally, in Treatment IV, we maintain the backstop and unequal Nash bargain from Treatment III, but begin with a more equal historical position.

Our experimental results provide only limited support for the utilitarian hypothesis. Although the parties agree to the Nash bargain when it equalizes payoffs; they are drawn from the former towards the latter when the two diverge. Conversely, strong support is provided for the egalitarian hypothesis: in each of the four treatments, our subjects appear to be drawn towards the efficient equal payoff outcome, even when the latter is Pareto inferior to the backstop. Finally, we find mixed support for the historical entitlement hypothesis. The historical position has no discernible effect on the outcomes chosen by our subjects when it provides a *less* equal allocation of payoffs than the backstop, but it increases the equality of agreement payoffs when it provides a *more* equal allocation.

The remainder of the paper is divided into five sections. In Section 2, we use the Edgeworth box model to describe the nature of the bargaining problem under collaborative decision-making; and we summarise those elements of the existing literature that provide utilitarian, egalitarian and historical entitlement predictions concerning negotiation outcomes. In Section 3, we describe the design of a laboratory experiment used to test the conflicting predictions of the three hypotheses on collaborative decision-making. In Section 4 we present our results. Section 5 concludes the paper with a discussion of the implications of our findings.

2 Modeling Collaborative Decision-Making

The most widely reported applications of collaborative bargaining have occurred in the environmental sector[3], where stakeholder groups have been employed: to develop habitat conservation plans to protect endangered species (Thomas, 2001; Anderson and Yaffee, 1998), zone large tracts of public lands (Bruce, 2006), and develop air and water pollution regulations (Pritzker and Dalton, 1995). But this approach has also been applied to many non-environmental questions. The Negotiated Rulemaking Act in the United States, for example, encourages agencies to employ a process in which the groups that would be affected by a proposed regulation are given the opportunity to develop a consensus-based alternative to that regulation. This technique has been used, for example, to develop rules for financial assistance, to

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[3] See footnote 1.

negotiate regulations concerning workplace safety, and to revise the rules concerning flight times for airline pilots. (Pritzker and Dalton, 1995). Fung and Wright (2001) have argued that many other collaborative processes, including parent-directed school councils and some forms of community-based policing meetings, also satisfy their definition of "empowered deliberative democracy." And political scientists have argued that many types of international negotiations (for example, over the GATT), resemble collaborative bargaining with the status quo as the backstop. (See Steinberg, 2002.)

To model this approach, we develop an example of two interest groups, denoted *environmentalists* (*Env*) and *developers* (*Dev*), in conflict over two dimensions of resource policy: the amount of public land to be protected, A, in acres, and the severity of restrictions to be placed on commercial activity on that land, R. Relative to the current, or historical, allocation, H, environmentalists would prefer more of both A and R, while developers would prefer less of both. We illustrate these assumptions in Figure 1 using a conventional Edgeworth box.

In order to resolve this conflict, the government offers to allow the parties to negotiate a revised allocation of resources. In the first of two alternative scenarios, we assume that the government announces that if negotiations succeed, it will implement the policy selected by the parties; whereas if negotiations fail, it will leave in place the historical policy. That is, the backstop, B, is set equal to H.[4] In this circumstance, it has been argued that utility-maximizing negotiators will select the Nash bargain (Nash 1950) - the outcome that maximizes the product of their gains relative to the backstop. (See for

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

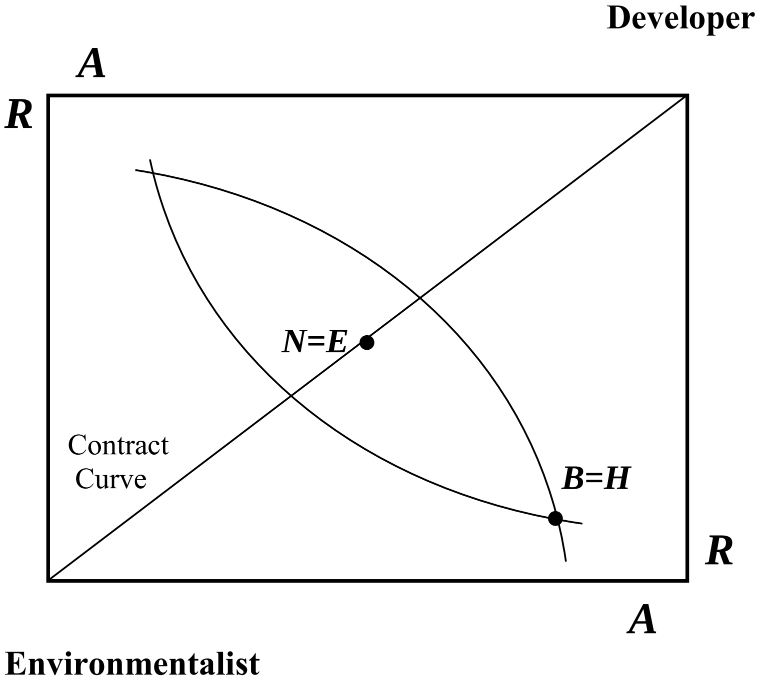

Fig. 1: An Edgeworth Box Representation of Collaborative Decision-making

example, Nydegger and Owen, 1975; and Roth and Malouf, 1979.) Following Hoffman and Spitzer (1985), we refer to this as the *utilitarian* hypothesis. As the Nash bargain, *N*,

must be both Pareto superior to *H* and Pareto efficient, we place *N* on the contract curve, within the bargaining lens formed around *B=H*, in Figure 1.

In a second scenario, the government announces that it proposes to introduce a policy that differs from *H*; but offers interest groups the option of negotiating an alternative outcome, which it promises to implement.[5] In this case, the policy

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[5] This is the effect of the Negotiated Rulemaking Act in the United States. (Pritzker and Dalton, 1995.)

proposed by the government becomes the backstop, *B*, and the Nash bargain is defined relative to this. Now, as indicated in Figure 2, *H* lies outside the bargaining lens formed around *B*.

Although our benchmark hypothesis is that the parties will select the Nash bargain, we also test two other hypotheses that have been proposed in the experimental economics literature. In the first of these, a number of authors - notably, Nydegger and Owen (1975), Roth, Malouf, and Murnighan (1981) and Hoffman and Spitzer (1985), and Shogren (1997) - have tested the conjecture that negotiators behave as if they are *egalitarians*. With the exception of Shogren, they found that their subjects were drawn towards outcomes within the efficient set that offered them equal payoffs.[6] One such outcome has been identified as *E* in Figure 2.

In a second line of research, it has been argued that the relative bargaining power of two negotiators may be influenced by their relative perceptions of "entitlement" to the positions they have taken. In particular, a party which believed that its initial position was "fair" or "deserved" might press more vigorously for maintenance of that position than would a party that did not hold such a belief. Within the experimental literature, this line of argument has taken two forms: the *earned entitlement* and *historical entitlement* approaches.

The earned entitlement approach follows Buchanan (1986) in arguing that individuals will hold more firmly to their positions if they had expended effort to obtain

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[6] Note: the experimental literature has found the outcomes chosen to be efficient in the sense that they lay on the contract curve; *not* in the sense that they were Pareto superior to the backstop.

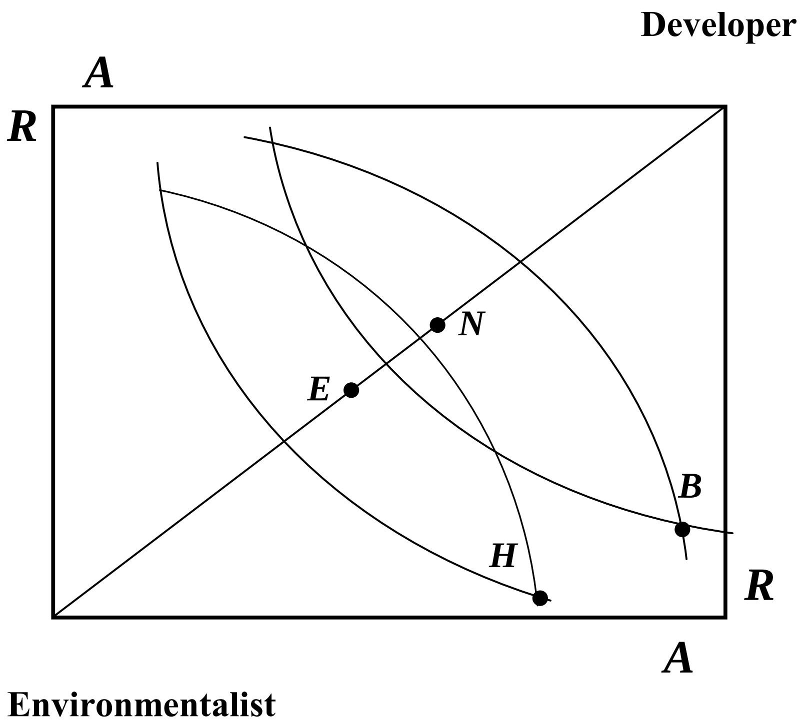

Fig. 2 Edgeworth Box with unequal Nash and Backstop unequal to Historical Entitlement

those positions. A number of experiments – notably Hoffman and Spitzer (1985), Burrows and Loomes (1994), and Gachter and Riedl (2005) – have tested this hypothesis by comparing two sets of negotiations: one in which subjects were randomly assigned unequal initial allocations of resources, and one in which one of the subjects "earned" a preferred initial allocation by winning a simple game. The findings of these studies were consistent with the earned entitlement hypothesis. Subjects who had earned their initial allocations were less likely to negotiate equal divisions of resources than were those who had obtained their initial allocations through random assignment.

In the historical entitlement approach, it is argued that individuals will consider themselves to be entitled to the initial, or "historical" allocation of resources (i) if they had obtained that allocation without the use of threat, fraud, or force (Nozick, 1974; and Zajac, 1995); or (ii) if a "moral authority" had told them that their "….entitlements are rights." (Hoffman and Spitzer, 1985: 266). Both Hoffman and Spitzer (1985) and Roth, Malouf, and Murnighan (1981) have found experimental evidence to support this hypothesis.

For the purposes of this paper, we focus on historical entitlements. Specifically, we hypothesize that when the backstop chosen by the government differs from the historical position – that is, when it sets a backstop, *B*, that differs from *H*, as in Figure 2 – negotiators will be drawn away from efficient allocations within the lens conditioned on *B*, to those within the lens conditioned on *H.* To the extent that parties do not condition their negotiations on *B*, collaborative bargaining will restrict a government's ability to shift policy away from the status quo.

To summarise, we propose to test three alternative hypotheses concerning the outcomes that negotiators will obtain in collaborative bargaining:

- *Utilitarian*: The parties will negotiate to the Nash bargain, *N* – defined relative to the backstop position *B* announced by the government.
- *Egalitarian*: The parties will be drawn towards the efficient outcome at which payoffs are equalized, *E*. This outcome need not be Pareto superior to the backstop.
- *Historical entitlement*: The parties will be drawn towards efficient outcomes within the bargaining lens conditioned on *H*, rather than those conditioned on *B*. Again, this outcome need not be Pareto superior to the backstop.

3 Experimental Design

3.1 Design Features Across All Treatments

To implement bilateral bargaining over two dimensions of policy, we recruited subjects in groups of 10, and gave each an induced value payoff function over two abstract goods, *X* and *Y*. Five subjects were assigned one payoff function, and five another, based on their prior choice of seat in the room. For exposition, we refer to the two preference types induced by these payoff functions as environmentalists and developers, though the neutral labels "you" and "the other person" were used in the experiment. To generate convex indifference curves for each type over the two goods, we used Cobb Douglas payoff functions:

$$P_{Env} = a_{Env} X_{Env}{}^{\alpha} Y_{Env}{}^{1-\alpha} + b_{Env} \tag{1}$$

$$P_{Dev} = a_{Dev} X_{Dev}{}^{\alpha} Y_{Dev}{}^{1-\alpha} + b_{Dev} \quad . \tag{2}$$

The use of a common exponent, α , for both types implied that the contract curve was a diagonal line. The use of constant returns to scale ensured that total payoffs would be constant along the contract curve, thus controlling for joint payoff efficiency. Each type of individual, *i*, was endowed with a historical allocation of $X_{i,H}$ and $Y_{i,H}$. We set the total quantity of *X* and *Y* each at 20 units, thereby creating 400 potential combinations, in order to minimize the possibility that we would inadvertently create focal points. (See Schelling, 1960.) Across all treatments, we set a non-symmetric backstop outcome 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). This resulted in the portion of the contract curve within the bargaining lens being 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), subjects' risk attitudes were elicited prior to the bargaining instructions using the method of Holt and Laury (2002).[7]

After reading instructions and studying their own payoff tables (and those of their opponents) for as long as any individual wanted, subjects were then placed together in pairs, one environmentalist with one developer. They were then allowed a three minute period of unstructured communication in which they could discuss mutually acceptable allocations of *X* and *Y*. To be accepted as valid, negotiated outcomes had to be technically feasible, or

$$X_{Env} + X_{Dev} \leq X_{Env,B} + X_{Dev,B} = 20 \tag{3}$$

$$Y_{Env} + Y_{Dev} \leq Y_{Env,B} + Y_{Dev,B} = 20 \tag{4}$$

To 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. We prevented subjects from being able to make credible offers of cash side

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[7] The pair average of risk aversion as measured by this instrument was not significant in random effects panel regressions predicting whether agreements were a) in the bargaining lens, b) Pareto efficient, or c) at the Nash bargain. Neither were most pair demographic characteristics we elicited. For brevity, we exclude these results in what follows.

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.

Our mixing protocol over the five rounds resulted in each member of one type being paired serially with all five members of the other type. The experiment was conducted manually. 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 (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 three minute round. They 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 the unstructured, face to face communication introduced "uncontrolled aspects of social interaction" (Roth 1995), it also paralleled the in-person, unstructured negotiation used in collaborative decision making.

3.2 Design Features of Each Treatment

We ran four treatments, varying the location of the historical entitlement and the inequality of the Nash bargain in a 2x2 design. Sessions were run so as to systematically alternate through the four treatments. Returning to our payoff functions (1) and (2), in all treatments we chose the *a*'s, *b*'s and α 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)
2. the backstop allocation *B*: (X_{EnvB}, Y_{EnvB}) = (18, 5) and (X_{DevB}, Y_{DevB})= (2,15).
3. the allocation at the Nash bargain, *N*: (X_{EnvN}, Y_{EnvN}) = (13, 13) and (X_{DevN}, Y_{DevN})= (7,7)
4. the sum of backstop values: $a_{Env}18^{\alpha}7^{1-\alpha} + b_{Env} + a_{Dev}2^{\alpha}13^{1-\alpha} + b_{Dev}$ = \$28.77.
5. the sum of all contract curve values, including the Nash bargain: $a_{Env}13^{\alpha}13^{1-\alpha} + b_{Env} + a_{Dev}7^{\alpha}7^{1-\alpha} + b_{Dev}$ = \$45.50.

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

To simplify the presentation of payoffs, subjects were provided two colored payoff tables showing the specific earnings they and their opponent would receive for all feasible combinations of *X* and *Y*.[8] The parameters for all four treatments are reported in Table 1; the payoff tables presented to the Environmentalists for Treatment II are reproduced in Figure 3. In treatments where *H* and *B* were identical,

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[8] 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. Subjects were given a considerable time to study both payoff tables and a session did not proceed until every subject indicated that he or she had finished looking at the tables. Calculators were provided for each person.

they were identified on a payoff table as a single yellow cell. In treatments where they differed, *H* and *B* were identified by green and red cells, respectively.

Treatment I

Treatment I is our control treatment, with no divergence between the initial allocation *H*, from which subjects began their negotiations, and the backstop allocation *B* that would be imposed if they could not reach agreement ((18,7)/(2,13)). The payoffs for the environmentalist and developer at *B* were approximately equal, at $14.67 and $14.10, respectively. In Treatment I the Nash bargain *N* coincided with the unique allocation *E* on the contract curve that equalized final earnings between the environmentalist and developer at (13,13)/(7,7), with respective payoffs of $22.75 each. Treatment I is approximated by Figure 1 in design and the first panel of Figure 4 in results. In this treatment the utilitarian and egalitarian hypotheses both predict that the parties will bargain within the single bargaining lens created by *B* to a Pareto efficient allocation on the contract curve at *N*. Since *H*=*B*, the historical entitlement hypothesis predicts only that the parties will settle on the contract curve within the lens. Thus, the models set out in Section 2 provide similar predictions concerning the outcome of negotiations in Treatment I.

Treatment II

In Treatment II, we wished to separate the historical allocation *H* that subjects were given at the start of a round from the backstop position, *B*. We did this by setting the

Table 1: Parameters Used Across Treatments

Treatment I: (Historical = Backstop Allocation, Nash Bargain Equalizes Payoffs)

	Environmentalist	**Developer**
Payoff Function:	$U_{Env}(X,Y) = 4.55X^{1/2}Y^{1/2} - 36.40$	$U_{Dev}(X,Y) = 4.55X^{1/2}Y^{1/2} - 9.10$
At *H* & *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: (Historical ≠ Backstop Allocation, Nash Bargain Equalizes Payoffs)

	Environmentalist	**Developer**
Payoff Function:	See Treatment I	See Treatment I
At *H*:	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: (Historical = Backstop, Nash Bargain Does Not Equalize Payoffs)

	Environmentalist	**Developer**
Payoff	$U_{Env}(X,Y) = 4.55X^{1/2}Y^{1/2} - 22.75$	
$U_{Dev}(X,Y) = 4.55X^{1/2}Y^{1/2} - 22.75$		
At *H* & *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: (Historical ≠ Backstop, Nash Bargain Does Not Equalize Payoffs)

	Environmentalist	**Developer**
Payoff Function:	See Treatment III	See Treatment III
At *H*:	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)

Figure 3 Payoff Tables Given to an Environmentalist in Treatment II[9]

ID _______

PART II: YOUR DOLLAR EARNINGS FROM YOUR FINAL HOLDINGS OF X AND Y

	20																					
	19					3.27	7.95	12.18	16.07	19.70	23.10	26.32	29.38	32.30	35.11	37.81	40.41	42.93				
	18					2.21	6.77	10.88	14.67	18.20	21.51	24.64	27.62	30.47	33.20	35.83	38.36	40.82	43.19	45.50		
	17					1.12	5.55	9.55	13.23	16.66	19.88	22.92	25.82	28.59	31.24	33.79	36.26	38.64	40.95	43.19		
	16					0.00	4.30	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	42.93	
	15						3.00	6.77	10.22	13.44	16.47	19.33	22.05	24.64	27.14	29.54	31.85	34.09	36.26	38.36	40.41	
YOUR	14						1.67	5.30	8.64	11.75	14.67	17.44	20.06	22.57	24.98	27.30	29.54	31.70	33.79	35.83	37.81	
	13						0.28	3.78	7.00	10.00	12.82	15.48	18.01	20.43	22.75	24.98	27.14	29.22	31.24	33.20	35.11	
FINAL	12							2.21	5.30	8.18	10.88	13.44	15.88	18.20	20.43	22.57	24.64	26.65	28.59	30.47	32.30	
	11							0.56	3.53	6.28	8.87	11.32	13.65	15.88	18.01	20.06	22.05	23.96	25.82	27.62	29.38	
HOLDINGS	10								1.67	4.30	6.77	9.10	11.32	13.44	15.48	17.44	19.33	21.15	22.92	24.64	26.32	
	9									2.21	4.55	6.77	8.87	10.88	12.82	14.67	16.47	18.20	19.88	21.51	23.10	
OF Y	8									0.00	2.21	4.30	6.28	8.18	10.00	11.75	13.44	15.08	16.66	18.20	19.70	
	7											1.67	3.53	5.30	7.00	8.64	10.22	11.75	13.23	14.67	16.07	
	6												0.56	2.21	3.78	5.30	6.77	8.18	9.55	10.88	12.18	
	5														0.28	1.67	3.00	4.30	5.55	6.77	7.95	
	4																	0.00	1.12	2.21	3.27	
	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

PART II: OTHER'S DOLLAR EARNINGS FROM HIS/HER FINAL HOLDINGS OF X AND Y

	20																					
	19																					
	18																					
	17																					
	16		9.10	16.64	22.42	27.30																
	15		8.52	15.82	21.42	26.14	30.30	34.07	37.52													
OTHER'S	14		7.92	14.98	20.39	24.95	28.97	32.60	35.94	39.05	41.97											
	13		7.31	14.10	19.31	23.71	27.58	31.08	34.30	37.30	40.12	42.78										
FINAL	12		6.66	13.19	18.20	22.42	26.14	29.51	32.60	35.48	38.18	40.74	43.18	45.50								
	11		5.99	12.24	17.04	21.08	24.64	27.86	30.83	33.58	36.17	38.62	40.95	43.18								
HOLDINGS	10		5.29	11.25	15.82	19.68	23.07	26.14	28.97	31.60	34.07	36.40	38.62	40.74	42.78							
	9		4.55	10.20	14.54	18.20	21.42	24.34	27.01	29.51	31.85	34.07	36.17	38.18	40.12	41.97						
OF Y	8		3.77	9.10	13.19	16.64	19.68	22.42	24.95	27.30	29.51	31.60	33.58	35.48	37.30	39.05						
	7		2.94	7.92	11.75	14.98	17.82	20.39	22.75	24.95	27.01	28.97	30.83	32.60	34.30	35.94	37.52					
	6		2.05	6.66	10.20	13.19	15.82	18.20	20.39	22.42	24.34	26.14	27.86	29.51	31.08	32.60	34.07					
	5		1.07	5.29	8.52	11.25	13.65	15.82	17.82	19.68	21.42	23.07	24.64	26.14	27.58	28.97	30.30					
	4		0.00	3.77	6.66	9.10	11.25	13.19	14.98	16.64	18.20	19.68	21.08	22.42	23.71	24.95	26.14	27.30				
	3			2.05	4.55	6.66	8.52	10.20	11.75	13.19	14.54	15.82	17.04	18.20	19.31	20.39	21.42	22.42				
	2			0.00	2.05	3.77	5.29	6.66	7.92	9.10	10.20	11.25	12.24	13.19	14.10	14.98	15.82	16.64				
	1					0.00	1.07	2.05	2.94	3.77	4.55	5.29	5.99	6.66	7.31	7.92	8.52	9.10				
	0																					
		0	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20

OTHER'S FINAL HOLDINGS OF X

OTHER'S PAYOFF FUNCTION: $\$4.55 X^{1/2} Y^{1/2} - \$9.10$

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[9] 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.

environmentalists' and developers' initial allocations at (16,4)/(4,16), (valued at $0.00 and $27.30 respectively), but leaving all of our other assumptions from Treatment I unchanged.[10] That is, the only difference between Treatments I and II was that, in the latter, the initial allocation now lay outside the bargaining lens - *B*, *N* and *E* were all unchanged between treatments. This separation of *H* from *B* implied: (i) that payoffs were divided more equally within the bargaining lens associated with *B* than the lens associated with *H*; and (ii) that the environmentalists were better off at every point within the bargaining lens associated with *B* than they were at *H* (and the developers worse off except for allocations where the two lenses overlapped). As in Treatment I, both the utilitarian and egalitarian hypotheses predict that the parties will select the *N*=*E* outcome within the bargaining lens formed around *B*. The historical entitlement hypothesis, however, predicts that the negotiated outcomes will move south-west along the contract curve to reflect at least some of the inequality favouring the Developer that had been present at *H*.

Treatments III and IV

Our goal in Treatments III and IV was to reproduce Treatments I and II, respectively, but with *N* separated from *E*. Our approach was to leave the physical locations of *H*, *B* and *N* as in the first two treatments; but to change the underlying payoff functions to move *E*. This resulted in three changes. First, the parties' payoffs were now unequal at *B* - $28.32 and $0.45, respectively.[11] Second, with the *N* resulting from this *B* now

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[10] 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.

[11] The degree of inequality and low absolute earnings threatened for developers in Treatments III and IV made us concerned *ex ante* that some subjects might leave the

also unequal at \$36.40 and \$9.10, respectively, *E* shifted south west to be outside the bargaining lens defined by *B* to (10,10)/(10,10), with payoffs of \$22.75 each. Thirdly, in Treatment IV alone, where *H* diverged from the (unequal) *B*, the payoffs at *H* became equal at \$13.65 each. Treatment IV is approximated by Figure 2 in design and the fourth panel of Figure 4 in results.

With only one exception, all of the predictions that were made with respect to Treatment I apply also to Treatment III – the parties are expected to negotiate an outcome on the contract curve, within the bargaining lens formed around *B*, at *N*. The exception is that if the parties are egalitarian, they can be expected to be drawn from the *N* allocation of (13,13)/(7,7) towards the *E* allocation of (10,10)/(10,10).[12]

The only difference between Treatments III and IV is that in the latter we moved *H* to a position southwest of *B*, to the same physical location as used in Treatment II. The historical entitlement hypothesis suggests that this divergence between *H* and *B* would result in bargained outcomes that were further southwest along the contract curve in Treatment IV than they had been in Treatment III.

4 The Results

Sixteen experiment sessions with 10 subjects each were run at the University of Canterbury between April and May of 2008. Four sessions were run per treatment, so that each treatment contained 40 people who provided 20 paired bargaining outcomes

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experiment early. To our relief, this did not happen. Our subsequent results may explain why.

[12] The parties are only predicted to settle exactly at E if the equity motive dominates all other motives.

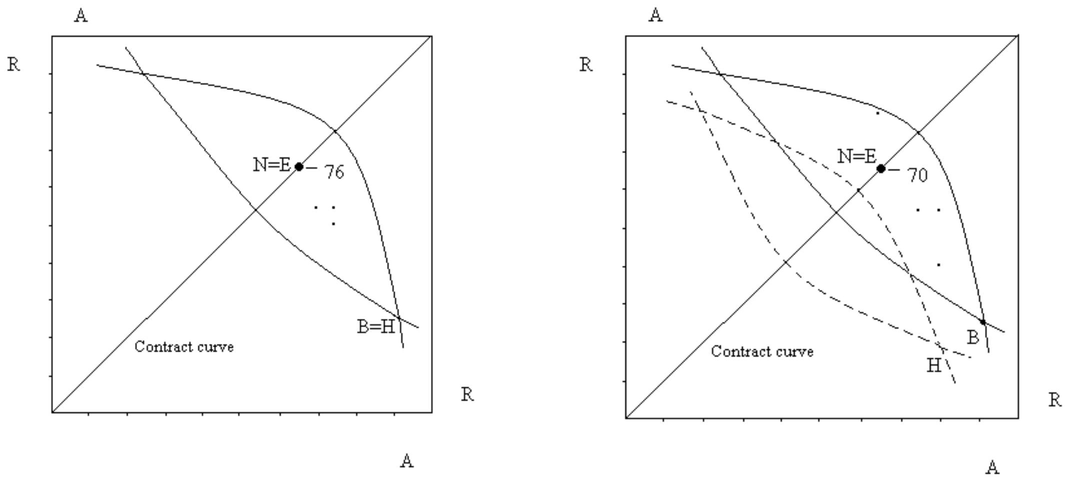
per round over five bargaining rounds. Each bargaining outcome consisted of the physical allocation of X and Y between the Environmentalist and Developer, $(X_{Env}, Y_{Env})/(X_{Dev}, Y_{Dev})$, and the parties' resulting respective earnings. Each session took roughly 90 minutes, and subjects earned on average NZ $24.49 (1.00NZ$ = 0.78US$).

We divide our discussion of the results from the experiment as follows. Since all three of our predictive models assume that subjects reach Pareto efficient agreements, we begin by comparing agreement rates and proximity to the contract curve across all treatments. We then briefly summarize how agreed outcomes change across treatments, and then test whether the utilitarian, egalitarian, or historical entitlement hypotheses can explain the changes observed.

4.1 Agreement rates and Proximity to the Contract Curve

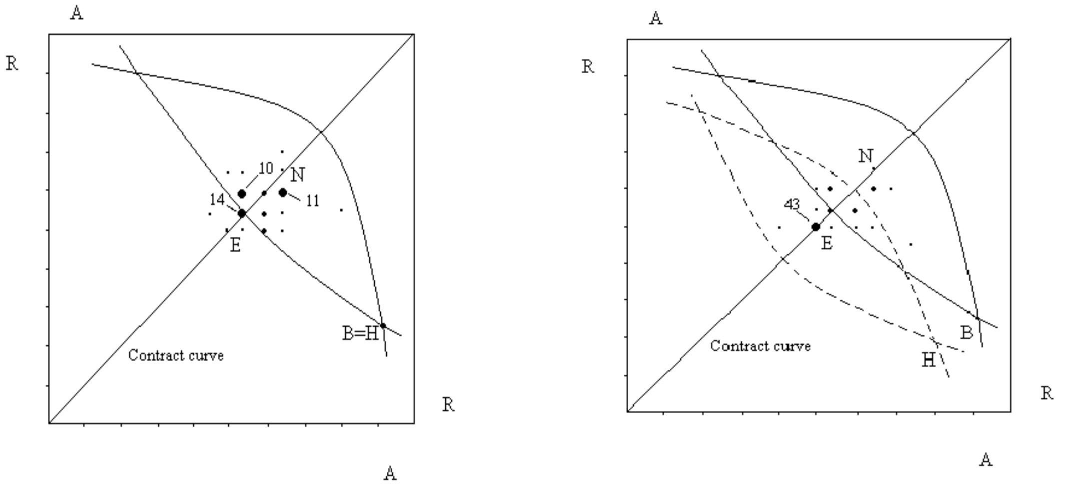
To provide intuition for the results to follow, Figure 4 illustrates all agreements and disagreements for our bargaining pairs for the final four rounds pooled for each of our four treatments. Corresponding descriptive statistics for all five rounds are provided in Table 2. As is apparent from the table, initially subjects found it easiest to reach agreement in Treatment I ($N=E$, $H=B$). Here 100% of pairs agreed to a new outcome even in Round 1. Round 1 agreements were almost as common in Treatment II ($N=E$, $H\neq B$) at 85%, and in Treatment IV ($N\neq E$, $H\neq B$) at 80%. The initial agreement rate was lowest, at 50%, in Treatment III ($N\neq E$, $H=B$), where both utilitarianism and historical entitlement produced very unequal payoffs. By rounds 4 and 5, however, agreement rates had converged to or near 100% in all treatments. Comparison of mean agreement rates round by round, either for the effect of separating H from B (I vs. II, III vs. IV) or of separating E from N (I vs. III, II vs. IV) found no significant differences in Round 2, 4 or

Figure 4 Observations from Rounds 2 through 5, Treatments I through IV



Treatment I: **B=H; N=E**

Treatment II: **B≠H; N=E**



Treatment III: **B=H; N≠E**

Treatment IV: **B≠H; N≠E**

Note: Number of observations in each treatment: 80

Legend:

- · 1-4 observations
- • 5-8 observations
- ● 9+ observations

Table 2: Descriptive Statistics of Physical Bargaining Outcomes

			Round					
		Pair N	1	2	3	4	5	Ave.
Agreement Rates								
T I:	$H = B, E = N$	20[1]	1.00	.95	1.00	1.00	1.00	.99
T II:	$H \neq B, E = N$	20	.85	.95	.95	.90	1.00	.93
T III:	$H = B, E \neq N$	20	.50	.85	.80	1.00	.95	.82
T IV:	$H \neq B, E \neq N$	20	.80	.95	1.00	1.00	1.00	.95
Proportion in Bargaining Lens:								
T I:	$H = B, E = N$	20[1]	1.00	1.00	1.00	1.00	1.00	1.00
T II:	$H \neq B, E = N$	20	1.00	1.00	1.00	1.00	1.00	1.00
T III:	$H = B, E \neq N$	20	.70	.70	.70	.75	.70	.71
T IV:	$H \neq B, E \neq N$	20	.35	.30	.30	.30	.20	.29
Contingent on Reaching Agreement:								
Proportion exactly on the Contract Curve:								
T I:	$H = B, E = N$		.65	1.00	1.00	.90	.95	.90[2]
T II:	$H \neq B, E = N$		.65	.89	.84	1.00	1.00	.88
T III:	$H = B, E \neq N$		.70	.35	.25	.35	.32	.37
T IV:	$H \neq B, E \neq N$		.63	.68	.60	.60	.75	.65
Proportion exactly at the Nash Bargain (13,13)/(7,7):								
T I:	$H = B, E = N$		.65	1.00	1.00	.90	.95	.90[2]
T II:	$H \neq B, E = N$		.59	.84	.84	1.00	1.00	.86
T III:	$H = B, E \neq N$		.10	.06	.00	.05	.00	.04
T IV:	$H \neq B, E \neq N$		.00	.05	.05	.00	.05	.03
Proportion exactly at (10,10)/(10,10) (Equalizes Earnings in III, IV):								
T I:	$H = B, E = N$		.00	.00	.00	.00	.00	.00[2]
T II:	$H \neq B, E = N$		.00	.00	.00	.00	.00	.00
T III:	$H = B, E \neq N$		.40	.12	.06	.05	.00	.10
T IV:	$H \neq B, E \neq N$		.63	.53	.50	.55	.60	.56

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[1] N = 19 pairs for Round 5 of Treatment I, where a technically inefficient agreement is omitted.

[2] Average across rounds weighted by the number of agreements per round.

5.[13] Thus, after only a few rounds of experience, bargainers learned to reach agreement even in very challenging environments, as predicted under all three bargaining hypotheses.

Were the agreements reached Pareto efficient? Table 2 reports the proportion of agreements that were precisely on the contract curve. We think, however, that a better gauge of support for Pareto efficiency can be found by measuring the physical or financial deviation of agreements from the contract curve. This is because some (*X*,*Y*) allocations immediately adjacent to the contract curve offered bargainers additional options for dividing payoffs, with joint earnings that were almost as high. Beginning first with physical deviations, we measure the geometric distance of agreements to the nearest allocation on the contract curve.[14] To illustrate magnitudes, an agreement one or two diagonal units from the contract curve would have distance measures of 1.41 or 2.83 units away from it, respectively, while *B* (at (18,7)/(2,13)) would be 7.78 units away. As reported in Table 3, we find that subjects who reach

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[13] In Round 3, two tailed t tests found that agreement rates were significantly higher in Treatment I than Treatment III (p value .04), and significantly higher in Treatment IV than in Treatment III (p value .04). The results were similar in round 1, where agreements were again more likely if the Nash equalized earnings (p value .05), or if historical entitlements equalized earnings (p value .00).

[14] If the closest allocation (in geometric distance) on the contract curve is defined as ($X_{env,cc}, Y_{env,cc}$), the distance of an agreement is $((X_{env} - X_{env,cc})^2 + (Y_{env} - Y_{env,cc})^2)^{1/2}$. If an outcome was equidistant to two cells on the contract curve, distance was measured to the averaged coordinates.

Table 3: Geometric Distance and Difference in Joint Earnings Between Agreements and the Nearest Point on the Contract Curve

		Round			
Treatment	**1**	**2**	**3**	**4**	**5**
I Mean Distance	.813	0	0	.318	.112
St. dev.	1.262	0	0	.986	.487
Mean Difference	.55	0	0	.26	.06
St. dev.	.96	0	0	.81	.25
N	20	19	20	20	19
II Mean Distance	.666	.558	.595	0	0
St. dev.	1.211	1.865	1.532	0	0
Mean Difference	.45	.99	.67	0	0
St. dev.	1.13	3.84	1.95	0	0
N	17	19	19	18	20
III Mean Distance	.283	.749	.707	.601	.707
St. dev.	.494	.769	.577	.770	.816
Mean Difference	.07	.26	.19	.24	.29
St. dev.	.15	.39	.27	.73	.75
N	10	17	16	20	19
IV Mean Distance	.884	.484	.354	.318	.177
St. dev.	2.153	1.029	.487	.428	.314
Mean Difference	1.28	.30	.08	.07	.03
St. dev.	4.53	1.00	.14	.12	.05
N	16	19	20	20	20

agreement do so close to or on the contract curve in all treatments. Average distance ranged from 0.28 to 0.88 units across treatments in Round 1, and from 0 to 0.71 units by Round 5.

Perhaps more importantly from an efficiency perspective, subjects achieved joint earnings indistinguishably close to that available on the contract curve ($45.50). Again to illustrate magnitudes, an agreement one diagonal unit from the contract curve would reduce joint earnings by $0.46 - $0.51 depending on where in the box it occurred, while an agreement two units away would cost $1.84-$2.03. Failing to reach agreement, so that *B* would be imposed, would always cost the pair $16.73. We find in Table 3 that the average joint earnings shortfall of agreements ranged from $0.07 to $1.28 in Round 1, narrowing to $0.00 to $0.29 by Round 5. More formally, Table 4 reports p values from t tests that compare the joint earnings shortfall of agreements from the contract curve across treatments round by round. We find that separating *H* from *B* (I vs. II, III vs. IV) had no significant effect on earnings shortfall in any round. Separating *E* from *N* had no effect by Rounds 4 and 5 without historical divergence (I vs. III). With historical divergence (II vs IV), separating *E* from *N* did significantly increase the mean joint earnings shortfall, but the magnitude of the difference was trivial. The mean shortfall was $0.00 for Rounds 4 and 5 in Treatment 2, and $0.07 and $0.03 in Treatment IV. We interpret these results to suggest that, with limited experience, support for reaching agreement, and Pareto efficient agreements in particular, was strong across all four treatments. With our three bargaining models' common predictions out of the way, we turn now to look at the changes in agreements across treatments.

Table 4: Cross Treatment Comparisons of Distance and Difference in Joint Earnings Between Agreements and the Contract Curve

P Values From Between-Sample T Tests

		Round				
		1	**2**	**3**	**4**	**5**
Distance to Contract Curve:						
I vs. II	(H=B→H≠B; N=E)	0.719[1]	0.208	0.108	0.165	0.331
III vs.IV	(H=B→H≠B; N≠E)	0.298	0.385	0.060	0.162	0.014
I vs. III	(N=E→N≠E; H=B)	0.112	**0.001**	**0.000**	0.319	**0.011**
II vs. IV	(N=E→N≠E; H≠B)	0.725	0.880	0.518	**0.004**	**0.021**
Difference in Joint Earnings:						
I vs. II	(H=B→H≠B; N=E)	0.787	0.277	0.154	0.172	0.331
III vs. IV	(H=B→H≠B; N≠E)	0.302	0.885	0.158	0.317	0.150
I vs. III	(N=E→N≠E; H=B)	**0.041**	**0.015**	**0.013**	0.932	0.221
II vs. IV	(N=E→N≠E; H≠B)	0.487	0.457	0.209	**0.024**	**0.021**

[1] All tests are two tailed, with equal variance not assumed. Significant values are in bold.

4.2 Results Across the Four Treatments

Table 5 reports a number of statistics concerning the deviations of the negotiated outcomes from the two points of greatest interest to us: the Nash bargain (13,13)/(7,7), and the efficient outcome that equalized payoffs in Treatments III and IV, (10,10)/(10,10). Our first two measures report the *geometric* distances between our observed agreements and the two key allocations. As before, a one diagonal unit of deviation from an allocation of interest results in a distance of 1.41 units, and two results in 2.83 units. Our third measure in Table 5 provides a single measure of the *pecuniary* distance between observed agreements and the two key allocations. This financial measure takes the absolute value of the difference between the environmentalist's share

Table 5: Mean Distance and Deviation in Environmentalist's Share of Earnings Between Agreements and the Allocations at (13,13)/(7,7) and (10,10)/(10,10)

Treatment		Round				
		1	2	3	4	5
I $N=E=(13,13)/(7,7)$ $H=B$	Distance to (13,13)	.84	0	0	.32	.12
	St. dev.	1.28	0	0	1.00	.51
	Distance to (10,10)	4.38	4.24	4.24	4.32	4.24
	St. dev.	.39	0	0	.25	.03
	Deviation in Env.'s	.29	.30	.30	.30	.30
	Share of Earnings[1]	.04	0	0	.02	.02
II $N=E=(13,13)/(7,7)$ $H \neq B$	Distance to (13,13)	.87	.63	.66	0	0
	St. dev.	1.30	1.88	1.63	0	0
	Distance to (10,10)	4.13	4.44	4.58	4.24	4.24
	St. dev.	.70	1.07	.81	0	0
	Deviation in Env.'s	.25	.29	.29	.30	.30
	Share of Earnings[1]	.09	.05	.05	0	0
III $N=(13,13)/(7,7)$, $E=(10,10)/(10,10)$ $H=B$	Distance to (13,13)	3.02	2.43	2.42	2.06	2.08
	St. dev.	1.53	1.05	1.10	1.08	.91
	Distance to (10,10)	1.44	2.30	2.35	2.63	2.72
	St. dev.	1.47	1.17	1.26	1.34	1.26
	Deviation in Env.'s	-.12	-.00	-.00	.05	.06
	Share of Earnings[1]	.22	.16	.18	.16	.15
IV $N=(13,13)/(7,7)$, $E=(10,10)/(10,10)$ $H \neq B$	Distance to (13,13)	4.13	3.59	3.13	3.34	3.52
	St. dev.	1.77	1.29	1.41	1.21	1.14
	Distance to (10,10)	1.27	1.20	1.30	1.09	.79
	St. dev.	2.28	1.55	1.53	1.47	1.18
	Deviation in Env.'s	-.23	-.17	-.13	-.16	-.20
	Share of Earnings[1]	.15	.19	.21	.20	.16

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[1] Ranges from -0.3, indicating the environmentalist's share of earnings corresponds to that at the allocation (10,10) (10,10), to +0.3, corresponding to his share at (13,13) (7,7). Standard deviation appears immediately below.

of earnings at the agreement and what it would have been at (13,13)/(7,7), and subtracts from it the absolute value of the difference between the environmentalist's share at the agreement and what it would have been at (10,10)/(10,10). In all treatments this measure can range in value from -0.3, indicating that a pair settled exactly at (10,10)/(7,7), to +0.3, indicating that a pair settled exactly at the Nash allocation (13,13)/(7,7). A measure of 0 indicates that the environmentalist's share was half way between what it would have been at the two allocations.[15]

Treatment I

As Table 5 illustrates, the agreements in Treatment I were at or very near *N* even in the first round. This is true whether the measure is mean physical distance of agreements from *N* (0 by Round 2), or mean environmentalist's share of earnings (+0.3 by Round 2). Since the utilitarian, egalitarian and historical entitlement models all predict, or are consistent with, this outcome, Treatment I provides a reassuring baseline from which to make cross-treatment comparisons.

Treatment II

Recall that between Treatments I and II, the only change is that *H* diverges from the roughly equal distribution of initial income at *B*, ($14.67, $14.10), to a very unequal one in which the environmentalist starts with nothing ($0, $27.30). Our results indicate that subjects ignored this unequal *H* and any bargaining lens it might have created. As illustrated in Table 4, the agreements in Treatment II appear very similar to those in Treatment I, particularly from Round 2 on.

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[15] A simpler measure of pecuniary distance, such as comparing the difference in joint earnings at observed agreements from joint earnings at key allocations, was not feasible because joint earnings were identical at all such allocations along the contract curve.

Treatment III

Recall that in Treatment III the physical locations of *H*, *B* and *N* were all left unchanged from Treatment I; but the payoff functions were altered in such a way as to separate *E* from *N*. Thus, in this treatment, the payoffs were unequal at both *N*, ($36.40, $9.10), and *B*, ($28.32, $0.45); while *E* -, (10,10)/(10,10) at ($22.75, $22.75) - lay outside the bargaining lens formed around *B*.

As illustrated in Table 5, the agreements in Treatment III were more dispersed than previously, but were on average a compromise between *E* and *N*, both in physical distance and earnings share. Agreements began slightly closer to *E* in Rounds 1 and 2, and became slightly closer to *N* by Rounds 4 and 5. In Round 5, the modal agreement was at (11,11)/(9,9), generating earnings of ($27.30, $18.20). This outcome was just outside the bargaining lens, making the environmentalist $1.02 worse off than by forgoing agreement. However most agreements were closer than this to *N*, and were within the bargaining lens.

Treatment IV

Recall that the only change between Treatments III and IV was that *H*, in the latter, was separated from *B*, to an allocation that equalized initial payoffs at ($13.65, $13.65). This value of *H* defined a "historical bargaining lens" that included the Pareto efficient allocation *E* (10,10)/(10,10), which as in Treatment III generated equal earnings of ($22.75, $22.75). As illustrated in Table 5, the agreements in Treatment IV were again on average a compromise between *E* and *N*, but now far more heavily tilted towards *E*. Agreements on average were physically closer to *E* than to *N*, and the Environmentalist's share of earnings was closer to *E* than to *N* in all five rounds. Indeed, the modal outcome was at (10,10)/(10,10) for all five rounds. At this outcome, the environmentalists agreed to leave the bargaining lens defined by *B*, and earn \$5.57 less than they could have by forgoing agreement. This tendency remained as strong in Round 5 as in Round 1.

4.3 The Three Way Horse Race

With this summary of results, we are now in a position to compare the predictive power of the utilitarian, egalitarian and historical entitlement models of bargaining. Starting with utilitarianism, the Nash bargain remained at (13,13)/(7,7) in all four treatments, so that the distance or deviation in environmentalist's share of earnings at agreements should not change as we introduce historical divergence (II vs. I, IV vs. III), or unequal Nash bargains (III vs. I, IV vs II). Table 6 provides the p values of two tailed t tests comparing the mean distance and earnings share deviation of agreements from *N* and from (10,10)/(10,10) for the four relevant pair-wise comparisons, round by round. Quite strikingly, the utilitarian approach failed to predict the significant movements away from *N* when *E* diverged from *N* between Treatments I and III, and Treatments II and IV. There were significant differences in distance from *N* and earnings share in all five rounds for both treatment comparisons. And, although the utilitarian approach did successfully predict that separating *H* from *B* would have no significant effect on the distance of agreements from *N* when N=E, (that is, between Treatments I and II), it failed to predict the significant movement of agreements away from *N* when $N \neq E$, (that is, between Treatments III and IV.) In the latter case, there were significant differences in distance from *N* and earnings share in Rounds 2, 4 and 5. Thus, in our face-to-face, full information bargaining design, utilitarianism succeeded in its predictions in only one of four cross treatment comparisons.

Table 6: Cross Treatment Comparisons of Distance and Deviation in Environmentalist's Share of Joint Earnings

P Values From Between Sample T Tests

		Round				
		1	**2**	**3**	**4**	**5**
Distance to (13,13) (7,7):						
I vs. II	(H=B→H≠B; N=E)	0.940[1]	0.158	0.097	0.166	0.331
III vs. IV	(H=B→H≠B; N≠E)	0.104	**0.005**	0.098	**0.001**	**0.000**
I vs. III	(N=E→N≠E; H=B)	**0.001**	**0.000**	**0.000**	**0.000**	**0.000**
II vs. IV	(N=E→N≠E; H≠B)	**0.000**	**0.000**	**0.000**	**0.000**	**0.000**
Distance to (10,10) (10,10):						
I vs. II	(H=B→H≠B; N=E)	0.200	0.431	0.085	0.163	0.331
III vs. IV	(H=B→H≠B; N≠E)	0.827	**0.021**	**0.030**	**0.001**	**0.000**
I vs. III	(N=E→N≠E; H=B)	**0.000**	**0.000**	**0.000**	**0.000**	**0.000**
II vs. IV	(N=E→N≠E; H≠B)	**0.000**	**0.000**	**0.000**	**0.000**	**0.000**
Deviation in Environmentalist's Share:						
I vs. II	(H=B→H≠B; N=E)	0.190	0.331	0.331	0.330	0.331
III vs. IV	(H=B→H≠B; N≠E)	0.199	**0.008**	0.063	**0.001**	**0.000**
I vs. III	(N=E→N≠E; H=B)	**0.000**	**0.000**	**0.000**	**0.000**	**0.000**
II vs. IV	(N=E→N≠E; H≠B)	**0.000**	**0.000**	**0.000**	**0.000**	**0.000**

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[1] All tests are two tailed, with equal variance not assumed. Significant values are in bold.

In contrast, the egalitarian hypothesis alone successfully predicted most of the cross treatment effects. Like utilitarianism, it correctly predicted no difference in agreement outcomes between Treatments I and II, since *E* remained at *N*. Unlike the utilitarian approach, however, it correctly predicted that agreements would move away from (13,13)/(7,7) towards (10,10)/(10,10) between Treatments I and III, and between Treatments II and IV. These movements, whether measured in physical or earnings share deviations, were significantly different at the 5% level or better for all five rounds in between-sample t tests. The only flaw in the predictive success of the egalitarian approach concerned the comparison of agreements in Treatment III and IV, where *H* diverged from *B*. Since *E* (and *B* and *N*) remained unchanged between these treatments, the egalitarian prediction was that agreements would remain unchanged. Yet between-sample t tests find that agreements moved significantly further away from *N* and towards *E* in both distance and earnings share deviation in Rounds 2, 4 and 5. The fact that the allocation (10,10)/(10,10) became not only equal but an allocation on the contract curve within the bargaining lens defined by *H* significantly increased the support it received. The egalitarian approach alone cannot explain this. In short, egalitarianism succeeded in its predictions in three of four cross treatment comparisons.

Finally, the historical entitlement hypothesis alone failed to predict most of the cross treatment effects. It successfully predicted that a divergence of an equal *H* from a very unequal *B* (and resulting *N*) between Treatments III and IV would significantly pull agreements further away from *N* towards the *E* allocation in Rounds 2, 4 and 5. While the *E* allocation (10,10)/(10,10) equalized efficient earnings in both III and IV, it was in IV alone that it lay on the contract curve within the bargaining lens defined by *H*. In all other cross treatment comparisons, however, historical entitlement's predictions were not born out. Agreements did not change in distance or earnings deviation from *N* or *E* between Treatments I and II when *H* diverged from *B* for any round. Conversely, agreements *did* change significantly in distance or earnings deviations from *N* and *E* between Treatments I and III and between Treatments II and IV, when the location of *H* was constant. In short, historical entitlement alone joined the utilitarian hypothesis in successfully predicting only one of four cross treatment comparisons.

5 Discussion and Conclusion

Many public policy debates can be characterized as disputes among multiple interest groups over policies that are composed of multiple attributes. In this paper, we analyze one method of resolving these disputes that has been widely discussed by non-economists: collaborative decision-making. Specifically, we use laboratory experiments to determine whether competing groups would be able to reach agreement under this approach and, if so, to test two factors that might affect the nature of those agreements.

Subjects in our experiments were provided with induced payoff functions over combinations of two goods; and were asked to negotiate trades among those combinations, subject to the condition that if they failed to reach an agreement, a backstop outcome would be imposed. Our subjects were faced with a much more complex set of problems than had been posed in previous bargaining experiments – they could trade up to twenty units of each of two items, with approximately two hundred possible payoff outcomes. It was thus a reassuring finding that our subjects were quickly able to negotiate Pareto efficient (or nearly Pareto efficient) agreements, even when the payoffs imposed at the backstop allocation were very unequal ($28.32, $0.45) or differed substantially from the payoffs at the initial (historical) allocation. By the final round of negotiations, agreement rates were never less than 95%, and never achieved less than 99% of the total payoffs available on the contract curve.

Admittedly, the fact that there were no transactions costs to negotiations, full information as to opponents' payoffs, and face-to-face unrestricted communication likely contributed to this success. Nevertheless, as a starting point, our results are encouraging with respect to the ability of collaborative bargaining to achieve Pareto efficient agreements relative to backstop policies that governments set, even in very “unfair” conditions.

The second result found in our implementation was that the utilitarian Nash bargain alone was a poor guide to the *specific* Pareto efficient allocation parties would select when it resulted in unequal payoffs. By the final round of negotiations, 95 to 100 percent of our subjects chose the Nash bargain when it equalized payoffs, but only 0 to 5 percent did so when it yielded unequal payoffs.

In contrast, we found that egalitarian principles provided an excellent, though imperfect, guide to the efficient allocations that our parties selected. When the efficient outcome that offered equal payoffs to our parties differed from the Nash bargain, their agreements were drawn away from the latter towards the former. This movement was reinforced when the historical allocation of resources (that is, the status quo) was also separated from the backstop in the same direction as the equal payoff allocation was separated from the Nash bargain. In this latter case, by the fifth round of bargaining 60 percent of subjects (12/20) chose the efficient outcome that equalized payoffs, whereas only 5 percent (1/20) chose the Nash bargain. It appears that bargainers were drawn to a compromise between the utilitarian and egalitarian outcomes when these diverged; but that the extent of this compromise was affected by the parties’ historical entitlement. Egalitarianism alone would not have predicted this finding.

Finally, the effect of historical entitlements on outcomes was decidedly asymmetric. In our first two treatments, in which the Nash bargain offered equal payoffs to the parties, the separation of the historical allocation from the backstop had no effect on agreement locations. In particular, a historical allocation that would have pointed bargainers towards more unequal efficient agreements was ignored. In contrast, in our second two treatments, in which the egalitarian outcome diverged from the Nash bargain, a historical allocation that pointed bargainers towards more equal agreements was decidedly influential. It appears that the entitlement effect was too weak to draw parties away from a Nash bargain that equalized payoffs; but strong enough to draw parties further away from a Nash bargain that did not equalize payoffs. Of course, one could argue that it is impressive that historical entitlement had even this asymmetric effect considering the weakness of its implementation in our design: initial bargainer types and their associated endowments were assigned randomly and subjects were not told they were "deserved."

In future work, it would be interesting to see whether collaborative bargaining would be as successful in achieving Pareto efficient agreements, and egalitarian agreements in particular, if information about payoffs was private rather than public, and if unequal starting or backstop allocations were earned rather than randomly assigned. Other experiments in bargaining have found that private information about payoffs can strengthen support for utilitarian predictions at the expense of egalitarian ones (Roth and Malouf, 1979, Roth, Malouf, and Murninghan, 1981, Hoffman and Spitzer, 1986, and Rhoads and Shogren, 2003). It has similarly been found that assigning bargainer roles by contest rather than randomly similarly strengthens support for utilitarian rather than egalitarian outcomes (see for example, Davis and Holt (1993: 265-267), Hoffman et al. (1994) and Oxoby and Spraggon (2008)). The effect of these design changes on the relative support for historical entitlement predictions remains an open question.

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