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Structured and unstructured bargaining with positive transaction costs: an experimental investigation

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Structured and unstructured bargaining with positive transaction costs:
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Dedicated to my parents, Martha Sarres and Ronald Spencer, and my grandmother, Thelma Whitesell.

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CHAPTER 1:
INTRODUCTION

> Without the concept of transaction costs, which is largely absent from current economic theory, it is my contention that it is impossible to understand the working of the economic system, to analyze many of its problems in a useful way, or to have a basis for determining policy.
> Ronald H. Coase (1988, p.6)

The primary motivation for this thesis comes from Shogren and Herriges (1994). Through (two-person) bilateral bargaining experiments, Shogren and Herriges find that Coasian bargaining [as in Coase (1960)] achieves inefficient results when positive transaction costs exist. This result contrasts the abundance of efficiency results reported in previous Coasian bargaining experiments [e.g., see Hoffman and Spitzer (1982) or Shogren (1992)]. The incorporation of transaction costs represents the fundamental difference between Shogren and Herriges's experiment and the previous experiments. This suggests that the so-called "Coase theorem" does not remain robust when bargaining is costly. The Coase theorem states: if the costs of bargaining are minimal and well-defined property rights exist, two disputing parties will reach a Pareto-efficient solution through a mutually advantageous bargain, and this efficient solution (i.e., optimal allocation of resources) remains independent of the initial assignment of property rights. Most negotiations, however, involve transaction costs. Fact finding, travel, and time costs represent a few of the possible expenses which bargainers might incur. Given the existence and importance of transaction costs, as the above quote from Coase (1988) highlights, and Shogren and Herriges's experimental results, the economics discipline requires further research into costly bargaining. It may be that certain bargaining protocols (e.g., pre-bargaining communication, such as cheap talk) exist which help bargainers achieve efficient and mutually advantageous bargains when bargaining remains costly. This thesis builds on the idea of bargaining protocols.

Specifically, the current study develops several protocol schemes (see Section 6.3) by utilizing various procedures found in both "structured" and "unstructured" bargaining models. Structured bargaining contains process restrictions, where bargainers are not free to make offers and counteroffers whenever they wish. Examples of structured bargaining include sequential offer structures, as in Rubinstein's (1982) strategic model, and simultaneous demand structures, as in Nash's (1953) negotiation model. Unstructured bargaining naturally remains the antonym of structured bargaining; and hence, it imposes no process restrictions--i.e., bargainers are free (usually within a time limit) to make offers and counteroffers whenever they wish. Given this freedom, unstructured bargaining is sometimes referred to as "free form" bargaining. Coasian bargaining and Nash's (1950*a*) axiomatic model both represent examples of unstructured bargaining. As later chapters will discuss, this study, through the use of bilateral bargaining experiments, tests the ability of the bargaining protocols to guide bargainers toward efficient and mutually advantageous bargains, when positive transaction costs exist.

Given the above preview, the thesis proceeds as follows. Chapter 2 reviews the literature on bargaining solutions. This chapter discusses, in more depth, the procedures found in various structured and unstructured bargaining models. Chapter 3 presents some previous theoretical and experimental investigations into Coasian bargaining. The studies reviewed in Chapter 3 investigate the robustness of the Coase theorem upon a relaxation of one or more of the theorem's assumptions. Chapter 4 highlights some of the main issues found in bargaining experiments. This chapter reviews the lengthy fairman-gamesman debate; discusses the concept of focal points; addresses the role of expectations in bargaining; mentions the effect of risk aversion in bargains; and defines cheap talk. Most of the issues in Chapter 4 remain concerns for this paper's experiment.

Next, Chapter 5 presents three measures of costly bargaining, and states the basic hypotheses for this study. In particular, this study considers measures and hypotheses related to efficiency, distribution of wealth, and bargaining time. Chapter 5, also, considers other factors (such as gender effects) which may affect bargaining behavior in this paper's experiment. Chapter 6 explains the entire experimental design. It describes the general bargaining framework which all the bargainers must follow, and it defines the bargaining protocols which separate the experiment into various treatments. [Note, throughout this thesis the terms "treatment" and "protocol" will be used synonymously.] Additionally, Chapter 6 recounts how the experimental subjects were recruited, and how the experiment was implemented. Chapter 7 presents the experimental results and discussion. Specifically, this chapter analyzes the experimental results in terms of the measures presented in Chapter 5, and it draws conclusions regarding the hypotheses developed in Chapter 5. Lastly, Chapter 8 concludes this thesis. It offers an overall summary of this paper's analysis; states the limitations and potential shortcomings of this study; and offers future research ideas.

CHAPTER 2:
BARGAINING SOLUTIONS

Theorists wonder how people behave in bargains and what the outcome entails. These queries represent what is called the bargaining problem. Various theorists offer different solutions. One may classify the different solutions into three categories--axiomatic, strategic, and Coasian bargaining. Though this analysis mainly focuses on Coasian bargaining, much of the bargaining theory and experimental design presented in this analysis draws from the areas of axiomatic and strategic bargaining, too. Thus, one finds it necessary to briefly explain all three approaches.

2.1 Nash's Axiomatic Approach

First, the axiomatic approach, primarily pioneered by John Nash in 1950, offers a solution to the bargaining problem based on a set of assumptions, or axioms, regarding bargainer behavior and economic environment. Nash (1950*a*) begins by developing the utility theory of each individual based on several axioms which von Neuman and Morgenstern present in their 1944 book entitled Theory of Games and Economic Behavior. Additionally, Nash assumes each individual wants to maximize his or her gain in bargaining. Thereupon, the analysis continues by defining a set, S, of feasible expected utility payoffs (Nash uses the concept anticipations instead of payoffs) from which two bargainers bargain for the obtainment of one point. If the bargainers fail to reach an agreement, then a disagreement payoff, d, ensues. This disagreement payoff represents a point in S, or in mathematical terms $d \in S$. For illustration purposes let d describe the point (d_A, d_B) where d_A and d_B correspond to the disagreement payoffs for bargainer A and bargainer B, respectively (note that in Nash's original analysis he assigns the number zero to the disagreement payoff; thus, $d_A = 0$ and $d_B = 0$ in terms of Nash's analysis). Finally, Nash shows that the fulfillment of all the axioms [Pareto optimality, symmetry, invariance of linear transformations of utility, and independence of irrelevant alternatives--see pages 246-247 in Davis and Holt (1993) for a good explanation of these assumptions] requires the solution be a point in S where the product of each bargainer's utility is maximized beyond d. To illustrate, if one lets U_A and U_B represent utility functions for bargainers A and B, respectively, the Nash solution occurs where $(U_A - d_A)(U_B - d_B)$, or in terms of Nash's (1950*a*) analysis $U_A U_B$, is maximized.[1]

In sum, the axiomatic approach makes predictions regarding the outcome of a bargain based on certain assumptions. This approach does not address the use of independent strategies by bargainers. Hence, the axiomatic method remains in the domain of cooperative game theory. To address strategic considerations, one turns to noncooperative game theory.

2.2 The Strategic Approach

Much contemporary work regarding noncooperative game theory follows from Ariel Rubinstein's (1982) strategic model. But before one discusses Rubinstein's

[1] For good examples of the Nash solution involving a duopoloy and a divide-the-dollar game see pages 221-222 in Henderson and Quandt (1980) and Section 5.2 in Davis and Holt (1993), respectively.

work, one must, once again, recall earlier works by John Nash. In the realm of noncooperative game theory, Nash presents two prominent works--see Nash (1950*b* and 1953). At this point, one calls upon Roth (1985*a*, p. 2) to provide a concise explanation of Nash's efforts as follows:

> Nash (1953)...extended his original analysis [recall the axiomatic model in Nash (1950*a*)] in several ways. Perhaps the most significant of these extensions was the proposal of a specific *strategic* model that supported the same conclusions as the general axiomatic model outlined earlier. His approach was to propose one very particular bargaining procedure embodied in a noncooperative game in extensive form....Nash then argued that the predicted outcome of this noncooperative bargaining game would be the same as the outcome predicted by the axiomatic model. To show this, he called on the newly developing theory of noncooperative games to which he had made the seminal contribution of proposing the notion of equilibrium (Nash 1950*b*) that today bears his name. Although the noncooperative game he proposed possessed a multitude (indeed, a continuum) of Nash equilibria, he argued that the one corresponding to the prediction of his axiomatic model had distinguishing characteristics.

(Note, italics appear in original work but the first phrase in brackets is added for clarity.) Actually, Nash's (1953) noncooperative game (which he terms as the "negotiation game") consists of two basic moves. First, each player i (for i = 1, 2) chooses a mixed strategy or threat t_i which player i will use if bargaining ends in disagreement. Second, each player i simultaneously makes a demand d_i. If each player accepts the other's demand, then player 1 receives d_1 and player 2 receives d_2. Otherwise, each player's threat determines the disagreement payoffs. [See pages 130-131 in Nash (1953) for more details.]

Alternatively, Rubinstein (1982) progresses beyond Nash's work and presents a strategic model which incorporates sequential or alternating offers. He presents his model as two players whom must reach an agreement on the partition of a pie of size 1. The players make alternating offers. This essentially forces the players to wait their turn and waiting, as defined by the model, remains costly. Particularly, each player bears an exogeneously determined cost (Rubinstein considers two sources of delay costs which pertain to fixed bargaining costs and fixed discounting factors) for each period. Thus, if one assumes bargainers are cost minimizers or profit maximizers, then the bargainers maintain an incentive to settle sooner rather than later.[2] For the longer bargaining continues, the smaller the proportion of the pie each player faces. "This illustrates that the key to bargaining power here, in Rubinstein's model,... comes from the ability to put the onus of waiting entirely on the other party" [Kreps (1990), p.565].

To continue, Rubinstein advocates that the Nash equilibrium is a weak concept. He illustrates, on page 102 of his 1982 paper, the inadequacy of the concept of a Nash equilibrium in the context of his analysis. Instead, he proposes using the concept of a Subgame Perfect Equilibrium as a remedy. In the end, Rubinstein proves that his model maintains a unique subgame perfect equilibrium. And this equilibrium occurs after the first offer.[3]

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[2] Rubinstein's analysis relies on several assumptions regarding players' preferences,too. [See Rubinstein (1982), pp. 98-99 and 100-101.]

[3] This equilibrium tends to give virtually all the pie to the first mover. However, the payoffs depend on each player's bargaining (delay) cost. Rubinstein (1982, pp. 107-109) proves the following: (1) in the case where both the players have fixed bargaining costs, c_1 and c_2 : (i) if $c_1 > c_2$, then the only P.E.P. (Perfect Equilibrium Partition) gives c_2 to player 1 and (ii) if $c_1 < c_2$, then the only P.E.P. gives all the pie to player 1, and (2) in the case where both the players possess a fixed discounting factor (δ_1 and δ_2), if at least one of the δ_i is strictly less than 1 and at least one of them is strictly positive, then the only P.E.P. is $M = (1-\delta_2)/(1-\delta_1\delta_2)$. (Note, when $\delta_2 = 0$, player 1 maintains the ability to capture all the pie, $M = 1$.) [For a further explanation, with examples, of Rubinstein's model see Section 15.3 in Kreps (1990).]

2.3 The Coasian Approach

In addition to the classifications of cooperative and noncooperative, Nash's axiomatic model depicts free form or unstructured bargaining, while Rubinstein's strategic model depicts structured bargaining. Recall, structured bargaining places restrictions on the bargaining process (e.g., alternating offers and counteroffers), whereas unstructured bargaining imposes no process restrictions. Coasian bargaining represents another example of unstructured bargaining. And although Coasian bargainers may utilize various strategies, any joint agreement between the bargainers requires cooperation; hence, the Coasian approach retains some noncooperative and cooperative game theory elements. This depicts the basic Coasian bargaining environment. The following discussion further elaborates on Coase's (1960) approach towards bargaining and conflict resolution.

In his innovative paper "The Problem of Social Cost" Ronald Coase (1960) proposes an approach to resolve disputes concerning harmful effects (i.e., negative externalities). Coase (1960, pp. 2-8) illustrates that the initial assignment of liability rules over harmful effects remains immaterial for the final allocation of resources if the pricing system is costless. In other words, Coase asserts that regardless of who owns the unilateral property rights, under a costless (zero) transactions market, two bargainers will reach a Pareto-optimal or Pareto-efficient allocation of resources through a mutually advantageous agreement. This is the so-called Coase theorem. Note, one key requirement involves the assignment of property rights. If we could not assign property rights, then we face a commons problem. "The tragedy of the commons", as termed by Garrett Hardin (1968), leads to a misallocation and overexploitation of resources. Hence, one step towards a solution lies in the assignment of property rights. As such, one finds the Coase theorem offers a potential remedy. Coase (1960, p.8) apparently recognizes the commons dilemma as he reiterates his case:

> It is necessary to know whether the damaging business is liable or not for damage caused since without the establishment of this initial delimitation of rights there can be no market transactions to transfer and recombine them. But the ultimate result (which maximises the value of production) is independent of the legal position if the pricing system is assumed to work without cost.

Similar to most expositions concerning Coase's (1960) work, the previous discussion focuses on the Coase theorem. Coase (1960), however, offers an entire proposal, not just a theorem. To start, he makes many arguments in the development of his proposal. Some of his arguments (for further explanations, note the given pages which pertain to Coase's 1960 paper) include:

1) if the externalities problem lies in terms of causation, one must recognize that causation rests with both parties and an optimal allocation of resources requires both parties to consider the costs of the harmful effect--for continuing the activity harms one (e.g., receptor) while legally restricting the activity harms the other (e.g., polluter) [p.13]
2) the courts must consider economic issues (e.g., weigh the gains against the costs of alternative decisions) when making decisions [p.19]
3) the assignment of property rights through government regulation [pp.17-18] or statutory degree [pp.23-24, and 28] may not always promote efficiency; in fact, these techniques may foster the inefficiency in some cases, and

4) the Pigovian tax system fails to achieve optimal conditions because it lacks accountability for all costs imposed on others [pp.41-42].

Notice, all of these arguments rely on some form of benefit/cost analysis. For instance, argument (3) implies that if government regulation proves more costly than the market (or some other mechanism like the courts) in rearranging legal rights, then government regulation achieves suboptimal results; otherwise, government regulation achieves optimal results. Hence, Coase is not necessarily a devout *laissez-faire* supporter, but rather a strict believer in benefit/cost analysis.

Additionally, in contrast to popular belief, Coase strongly acknowledges the existence of positive transaction costs. Coase (1960, p.15) declares,"The argument has proceeded up to this point on the assumption...that there were no costs involved in carrying out market transactions. This is, of course, a very unrealistic assumption." Further, Coase (1960, on p.16) warns that the initial assignment of unilateral property rights matters when positive transaction costs exist. Coase's proposal asserts that when costly transactions exist, we must choose the least costly method for rearranging legal rights. And only a thorough benefit/cost analysis will reveal whether the decisions of courts, governments, or markets achieve a socially optimal arrangement of resources under different situations.[4] Specifically, Coase (1960, p.44) concludes:

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[4] Similarly, Calabresi (1968, p.69) remarks, "...since transactions do cost money, and since substitutes for transactions, be they taxation, liability rules, or structural rules, are also not costless, the 'optimal' result is not necessarily the same as if transactions were costless. Whatever device is used, the question must be asked: Are its costs worth the benefits in better resource allocations it brings about or have we instead approached a false optimum by a series of games which are not worth the candles used?"

> It would clearly be desirable if the only actions performed were those in which what was gained was worth more than what was lost. But in choosing between social arrangements within the context of which individual decisions are made, we have to bear in mind that a change in the existing system which will lead to an improvement in some decisions may well lead to a worsening of others. Furthermore we have to take into account the costs involved in operating the various social arrangements (whether it be the working of a market or of a government department), as well as the costs involved in moving to a new system. In devising and choosing between social arrangements we should have regard for the total effect. This, above all, is the change in approach which I am advocating.

Subsequently, one finds room for extending the Coasian bargaining situation to include transaction costs and the intricacies involved in assigning unilateral property rights. Later, this thesis builds on Coase's ideas and incorporates positive transaction costs into a bilateral bargaining experiment, which adheres to the Coasian ideology of assigning unilateral property rights and allowing for side payments (i.e., transfers) between bargainers. (Chapter 6 contains the experimental design.)

CHAPTER 3:
PREVIOUS STUDIES ON COASIAN BARGAINING

As mentioned in the previous chapter, Coase (1960) posited regardless of the initial assignment of property rights, two people will strike a bargain which is both Pareto-efficient and mutually advantageous, if bargaining remains costless. This represents what George Stigler first coined the "Coase theorem."[5] For over three decades legal scholars, political scientists, sociologists, psychologists, and economists have debated the validity and robustness of this theorem. Many of these philosophers presented theoretical arguments against the theorem. However, starting with Hoffman and Spitzer (1982) and Prudencio (1982), much experimental work has found empirical support for the Coase theorem. In fact, Harrison, Hoffman, Rutström, and Spitzer (1987, p. 388) remarked, "...the Coase Theorem (1960) is behaviorally 'alive and well' in relatively sterile and abstract bargaining environments." This note further investigates some of the theoretical and experimental arguments directed at relaxing one or more of the assumptions in the Coase theorem.

In addition to zero transaction costs, Hoffman and Spitzer (1982) assert that the Coase theorem depends on a set of assumptions. The assumptions are: "(*a*) two agents to each externality (and bargain), (*b*) perfect knowledge of one another's (convex) production and profit or utility functions, (*c*) competitive markets, (*d*) zero transactions costs, (*e*) costless court system, (*f*) profit-maximizing producers and expected utility-maximizing consumers, (*g*) no wealth effects, (*h*) agents will strike

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[5] Coase (1988, p.174) cites George Stigler as the coiner of the term the "Coase theorem."

mutually advantageous bargains in the absence of transactions costs" [Hoffman and Spitzer (1982, p. 73)]. Given a bargaining situation fulfills assumptions (*a*) through (*g*), the Coase theorem implies that the bargainers will act in accordance with assumption (*h*). Assumption (*h*) suggests two behavioral outcomes: (1) a *weak* behavioral outcome: the bargainers will reach a Pareto-efficient agreement and (2) a *strong* behavioral outcome: Pareto-efficient bargains will occur through a mutually advantageous agreement between the bargainers. Various analyses support the weak, strong, or neither behavioral outcome. For example, Hoffman and Spitzer (1982), Prudencio (1982), and Shogren (1992) support the weak outcome while Harrison and McKee (1985) and Hoffman and Spitzer (1985) support the strong outcome. Note, the latter two analyses support both behavioral outcomes, since the strong outcome embodies the weak outcome. In a theoretical argument Regan (1972) supports neither behavioral outcome. Many of these arguments result from an investigation which relaxes one or more of the Coase theorem's assumptions. Therefore, a brief examination of assumptions (*a*) through (*g*) reveals the arguments for and against this theorem.

a) *two agents to each externality* seemingly limits the scope of the Coase theorem's applicability. Baumol (1972) and Baumol and Oates (1988) argue that most environmental problems (e.g., air and water pollution) involve large numbers of people. And while Coase (1960) finds Pigou's solution prohibitive based on enormous data requirements, one finds the Coasian approach impractical when large numbers make bargaining too costly and complex. For instance, Baumol (1972, p.321) notes, "...trading fails to take place not because it is prohibited, but because (as seems characteristic of our most important externalities problems in reality) large numbers make trading virtually impossible to arrange (where have we seen automobile drivers pay one another to cut down their exhaust?)."

Despite Baumol and Oates's strong arguments against the applicability of the Coase theorem, laboratory experimentation and real life experience indicate the potential usefulness of the Coase theorem. Through experimentation Hoffman and Spitzer (1986) find support for the Coase theorem in cases which involve up to thirty-eight people. This brings life to the Coase theorem beyond the two party scenario. In fact, Hoffman and Spitzer list various situations where the Coasian approach may remain practical as a dispute resolution technique. The list includes: "...landowner disputes involving rights to light and air, small nuisances, such as dog kennels, neighborhood aesthetics, such as ugly houses and funeral parlors, and contractual claims involving a number of people" [Hoffman and Spitzer (1986, p.162)].

Furthermore, outside the laboratory setting, Sedjo (1992) reveals a real life situation where Coasian bargaining is helping to preserve wild genetic resources. Sedjo notes that by restricting access and collection countries rich in natural genetic resources can establish collection rights. Interested user groups, such as pharmaceutical companies, can negotiate collection agreements with an endowed country. Collection agreements enable the endowed country to capture a share of the potential resource rents through compensation and royalties [for example, see Sedjo's (1992, p.209-210) discussion of a collection agreement between Costa Rica and Merck pharmaceutical firm]. And as modern technology reduces transaction costs, Coasian bargaining becomes more practical as a means to negotiate collection agreements.

Overall, although many environmental problems are large scale and prohibit the use of Coasian bargaining, still numerous problems remain solvable through Coasian bargaining.

b) perfect knowledge of one another's utility functions implies that bargainers contain full or complete information. In a theoretical argument Samuelson (1985) finds this assumption unrealistic and asserts that Coasian bargaining cannot achieve Pareto-efficient results under partial or incomplete information. Past experimental work, however, opposes this claim. Upon a relaxation of the perfect information assumption experiments by Prudencio (1982) and Hoffman and Spitzer (1982) still support the weak behavioral outcome.

c) competitive markets represent the economically decentralized nature of the Coase theorem. Individuals left alone will reach efficient and mutually advantageous resolutions to their disputes through negotiation. Some speculation exists whether or not the Coase theorem remains consistent with the economic definition of competition. Competition usually implies many market participants whom taken individually maintain no control over the market. This makes assumptions (*a*) and (*c*) contradictory; and furthermore, bargaining agreements depict collusion which is prohibited in competitive markets [Daly (1974) raises these points]. Moreover, one questions whether most bargaining experiments really fulfill assumption (*c*). Two participant bargains appear more characteristic of bilateral monopolies.

Nevertheless, several Coasian experiments capture a competitive environment. First, Harrison, Hoffman, Rutström, and Spitzer [1987] (hereafter, HHRS) test the Coase theorem in a double-oral auction environment. Their experiment extends Charles Plott's (1983) centralized policy experiments to include the decentralized Coasian approach. To explain, Plott (1983), first, establishes that an externality problem exists in his experimental market, then he compares three (centralized) corrective policies--Pigouvian tax, standard, and pollution licenses. Plott (1983) finds the licenses approach most efficient with a range of efficiency from 88.4% to 99.6% over experimental periods. Likewise, HHRS (1987), first, replicate Plott's (1983) externality experiments and establish that an externality problem exists. Then HHRS use a Coasian approach to correct the externality problem. In the end, HHRS find that the Coasian approach yields highly efficient results which are comparable to Plott's (1983) results. Thus, the Coasian approach may remain useful in some competitive markets.

Shogren (1989 and 1994), also, test the Coase theorem in a competitive situation. Although Shogren's experiments do not necessarily represent a market with many buyers and sellers, his experiments create a competitive game situation where many, not just two, people are involved. To expound, Shogren (1989) uses team style Coasian bargains and Shogren (1994) implements Coasian bargaining under a tournament structure. Both experiments yield a high frequency (greater than 83%) of mutually advantageous agreements, but neither experiment yields a high frequency of Pareto-efficient agreements.

In short, though the above discussion does not rectify the conflict between assumptions (*a*) and (*c*), it is apparent that Coasian bargaining can produce efficient or mutually advantageous bargains in competitive environments.

d) *zero transactions costs* remain the most controversial assumption. Most people, especially Ronald Coase [see Section VI in Coase (1960)], realize that positive transactions costs exist in the "real world." While experiments which adhere to assumption (*d*) overwhelmingly produce efficient bargains [for example, see Hoffman and Spitzer (1982, 1985), Harrison and McKee (1985), or Shogren (1992)], this result apparently does not hold when experiments incorporate costly transactions [see Shogren and Herriges (1994)]. Shogren and Herriges's (1994) costly transactions experiment does not support the weak behavioral outcome. Though this preliminary result casts doubt over the Coase theorem's ability to yield efficient solutions in the real world, not all is lost for Coasian bargaining. Silberberg (1978, on p.497) emphasizes that room for negotiation still exists if the gains from transacting exceed the costs, and bargainers can choose among various contracts the one which maximizes mutual benefits. In other words, the maximization problem for bargainers reduces to a choice among contracts. Moreover, one might develop a process or protocol system which helps bargainers choose or create a contract that achieves an efficient solution. A replication of Shogren and Herriges's (1994) experiment with a protocol system might recapture some of the efficiency losses. The current analysis, later, builds on this idea and investigates various protocol schemes.

Overall, although basic economic theory predicts an incomplete exhaustion of the gains from trade when positive transaction costs exist [and Shogren and Herriges's (1994) experiment supports this outcome], a complete loss in efficiency and gains from trade is not always eminent. Despite Shogren and Herriges's (1994) findings, Coasian bargaining may still yield highly (not perfectly) efficient results and some type of protocol system might move bargainers on this semi-optimal path.

e) costless court system implies perfect contract enforcement. Theoretical and experimental investigations into this assumption remain sparse. Shogren and Kask (1992) perform the only, known, experiment which tests this assumption. Upon a relaxation of assumption (*e*) Shogren and Kask (1992) find only moderate support for the weak behavioral outcome and find almost no support for the strong behavioral outcome.

f) profit-maximizing producers and expected utility-maximizing consumers suggest that Coasian bargainers are economically rational. Rationality represents one of the most basic behavioral assumptions found in mainstream economic theory. It embodies the notion that people prefer more to less of a good (except for bad goods like pollution). But, for reasons discussed more clearly in Section 4.1 of the current paper, past experimental work yields mixed opinions regarding the validity of the rationality assumption for Coasian bargainers. Many experiments [e.g., Hoffman and Spitzer (1982) or Shogren (1992)] report irrational behavior where bargainers accept less than mutually advantageous offers (or income maximization offers). Still other experiments [e.g., Shogren (1989; 1994) or Harrison and McKee (1985)] yield rational behavior. Thus, while some experimental results question the validity of assumption (*f*), others signify its validity. (Section 4.1 will address this controversy in more detail.)

g) no wealth effects brings some stability to the Coase theorem when a change in legal rule occurs. The assumption means, as Coase (1988) describes, "...in a regime of zero transaction costs, the allocation of resources remains the same whatever the legal position regarding liability for harmful effects" (p.170). Previous debate over assumption (*g*)'s validity focuses not so much on the short run but in the long run. Hence, the following discussion presents some of the long run arguments.

Two notable arguments favor assumption (*g*). Calabresi (1968) offers the first argument as follows:

> ...if one assumes no transaction costs--including no costs of excluding from the benefits the free loaders, that is, those who would gain from a bargain but who are unwilling to pay to bring it about--and if one assumes, as one must, rationality and no legal impediments to bargaining, Coases's analysis [no wealth effects regardless of liability rule] must hold for the long run as well as the short run. The reason is simply that (on the given assumptions) the same type of transactions which cured the short run misallocation would also occur to cure the long run ones (p.67, note that statement in first brackets is added for clarity).

Nutter (1968) presents another argument, which, unlike Calabresi's (1968) argument, relies on the prior existence of economic rents. Through an arithmetic example, Nutter (1968) illustrates that a change in liability rule causes a change in economic rents, not a change in the allocation of resources (or distribution of wealth). Thus, "The necessary prior existence of rent ensures, then, that the Coase theorem applies to the long as well as the short run" [Nutter (1968, p.507)].

On the other hand, Regan (1972) finds fault with Calabresi's and Nutter's arguments and concludes that a wealth effect will occur from a change in legal rule. Regan's (1972) argument begins by asserting that a change in liability rule will alter participants' profits. To explain, some will no longer be required to pay damages, while others will begin to make side payments which they previously did not pay. This causes a disequilibrium (assuming a perfectly competitive equilibrium existed before the change in liability rule) as the former participants start to earn positive profits and the latter negative profits. The disequilibrium, in turn, sparks a movement of resources from the negative profit sector to the positive profit sector. Thus, Regan (1972, p.432) concludes, "In the long run, then, the allocation of resources will change."

In brief, many theorists accept assumption (*g*) in the short run, but debate exists over its validity in the long run. No, recent, experimental work directly addresses this debate. Future research might gain more insight through an experimental test of this assumption.

Lastly, one might add an additional assumption: *harmful threats are prohibited.* Given Coasian bargaining requires cooperation, unrestricted threats could undermind the negotiation process and lead to a misallocation of resources. In fact, Hoffman and Spitzer (1982) assert that the Coase theorem fails once a threat is carried out. Another concern for threats in Coasian bargaining comes from Mumey (1971) who points out that threats are potential bribe (and hence, revenue) generators. And although a zero transactions costs world can communicate threats without costs and adverse effects on the allocation of resources, Mumey warns that the revenue generating power of threats could entice some bargainers to expend resources on the development of credible threats. This expenditure, in itself, represents a misuse of resources. Thus, one can see how the Coase theorem, without legal restrictions against threats, remains vulnerable. Partly for these reasons most Coasian experiments, including the current paper's experiments in Chapter 6, explicity warn experimental subjects that no physical threats are allowed.

In sum, this chapter presented some of the theoretical and experimental work directed at the Coase theorem. Through a relaxation of one or more of the theorem's assumptions, each work investigated the ability of Coasian bargaining to achieve Pareto-efficient and mutually advantageous bargains. Aside from the inconclusive non-experimental debate, most of the experimental work supported at least one of the behavioral outcomes. In particular, the experimental results overwhelmingly supported the efficiency outcome, except in the case of positive transactions costs [see Shogren and Herriges (1994)].

CHAPTER 4:
MAIN ISSUES IN BARGAINING EXPERIMENTS

Game-theoretic models of bargaining predict solutions based on assumptions. Typically, the fulfillment of the assumptions leads to a particular outcome, or if many equilibriums (outcomes) exist then the most rational and economically efficient outcome prevails. Many experiments which try to test these normative theories, however, report anomalous results. For instance, contrary to the rational predictions of game theory, many experiments on ultimatum games yield equal splits [Thaler (1988)]. Such results seem to either signal the "unrealistic" nature of the theory's assumptions or simply remain products of incorrectly designed experiments.

Overall, this chapter does not attempt to support or oppose the predictions of game theory, rather it reviews some of the issues which persist in many bargaining experiments. Section 4.1 reviews the lengthy debate over why equal splits dominate many bargaining experiments. Focal points remain the subject of Section 4.2 . Next, a discussion of expectations follows in Section 4.3. Section 4.4 discusses how risk aversion affects bargains. Lastly, Section 4.5 digresses and presents a less common issue, called cheap talk, which exhibits promising qualities as a bargaining protocol.

4.1 Equal Splits and the Fairman-Gamesman Debate

Mainstream economic theory assumes that individuals are income maximizers and as such any rational individual prefers more income to less income.

In light of this rationality assumption, game theory predicts that individuals will bargain to a mutually advantageous agreement. To illustrate, in terms of ultimatum games, theory predicts that the proposer will offer a minimal amount to the receiver who, in turn, will accept.[6] More precisely, if we assume the disagreement outcome pays nothing to both players, then surely the receiver (if rational) will accept any offer, c, greater than nothing. Moreover, the proposer realizes this situation; and hence, rationalizes that he or she can obtain virtually all of the distribution (say $1) as long as c > $0. This yields a perfect equilibrium outcome. Similarly, in bilateral Coasian bargains, if a unilateral property rights (UPR) holder (called the controller) can obtain a certain payoff (known as an outside option), without consent from the noncontroller; then theory predicts that the controller will not settle for any agreement which leaves him or her worse off than the outside option [see Hoffman and Spitzer (1982)].[7] Nevertheless, past experimental results deviate from these predictions. Players tend to settle for equal splits which typically leaves one player worse off. In the ultimatum game, an equal split leaves the proposer worse off than the perfect equilibrium outcome, and in the Coasian bargain, this leaves the controller worse off than the outside option, assuming the outside option pays more than the equal split outcome. Thus, in contrast to theoretical predictions, bargainers fail to achieve mutually advantageous agreements.

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[6] Typically, the ultimatum game entails two players whom try to divide an amount of money, say M. One player (the proposer) offers an amount, c, to the other player (the receiver). If the receiver accepts this offer, then the proposer receives M-c and the receiver acquires c. But if the receiver rejects the offer then both players get nothing.

[7] Note, Hoffman and Spitzer (1982) design their experiment so that the outside option pays $11 or $12 depending on whether player A or player B, respectively, is the controller. Additionally, the outside option leaves $0 to the noncontroller in both cases. However, through side payments the players can obtain a joint-maximum of $14. Thus, an equal split, which is Pareto-efficient, pays $7 to both players.

At the center of this paradoxical result looms a question of fairness. In particular, some researchers conclude that equal splits represent a clear preference for fairness while other researchers refute the fairness explanation and, instead, offer other explanations. Hence, a diversity of opinion exists, and it remains prudent to discuss these opinions. This discussion begins with the supporting arguments for fairness. Secondly, a discussion of dissenting views follows. Lastly, the discussion investigates views which neither reject nor accept the fairness theory.

The fairness explanation primarily retains its origin in the work "An Experimental Analysis of Ultimatum Bargaining" by Güth, Schmittberger, and Schwarze [1982] (hereafter, GSS). GSS compare the results of "easy" games and "complicated" games. The easy game essentially represents a 2-person ultimatum game where player 1 (the proposer or allocator) declares an amount, a_1, for him/herself. This leaves an amount $M-a_1$, where $M > 0$ is the amount to be divided among the two players, for player 2 (the receiver). If player 2 accepts then player 1 receives a_1 and player 2 receives $M-a_1$; otherwise, both players receive nothing. Similarly, the complicated game retains the same basic structure as the easy games; except, the calculation of demands (and hence, the rational decision behavior) remains more complicated. Player 1 must decide how to divide 5 black and 9 white chips where each chip is worth DM2 (DM stands for the German currency, deutsche mark) for player 1, for player 2 each black chip and white chip is worth DM2 and DM1, respectively. Moreover, player 1's decision requires the proposal of two bundles, I and II. Player 2 chooses between bundle I and II which leaves the unchosen bundle for player 1.

The results of these experiments show a clear deviation from rational behavior. In the easy games players tend to settle towards an equal split. Further, player 2 tends to reject high demands by player 1. GSS (1982, p.384) state that, "...subjects often rely on what they consider a fair or justified result. Furthermore, the ultimatum aspect cannot be completely exploited since subjects do not hesitate to punish if their opponent asks for too much."[8] Additionally, the authors find that player 1 exhibits more rational behavior in complicated games than in easy games. They reason that this result adds further support to their fairness claim. GSS (1982, pp. 381-382) conclude, "This indicates that the subjects did not deviate from the optimal behavior because of their difficulties in solving the game. The main reason seems to be that the rational solution is not considered as socially acceptable or fair." Thaler (1988, p.197) upon reviewing the GSS paper among several other papers, offers additional insight as follows:

> Both the Allocators and the recipients take actions inconsistent with the theory...When a Recipient declines a positive offer, he signals that his utility function has non-monetary arguments...The actions of the Allocators could be explained by either of two motives (or some combination of both). Allocators who make significantly positive offers could either have a taste for fairness, and /or could be worried that unfair offers will be (rationally or mistakenly) rejected. Further experiments reveal that both explanations have some validity.

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[8] In the realm of public goods provision, one finds some similarities. Van Dijk and Grodzka (1992) conduct an experiment involving asymmetric endowments and information levels. They find that subjects informed about the asymmetry consider it fair for High Endowment members to contribute more than Low Endowment members. This treatment shows some similarities with GSS's easy game experiments. GSS's experiments incorporate complete information which resembles van Dijk and Grodzka's informed situation. Additionally, the proposer, in ultimatum (easy) games, maintains a higher endowment than the receiver since the proposer commands a favorable position. This illustrates a similarity between the proposer and the High Endowment member. Given these situational similarities, one draws an analogous conclusion. Both appear willing to forego high amounts, and fairness seems to explain this willingness. Though these experiments maintain some similarities, a direct analogy remains dangerous because both rely on different theories.

Therefore, one sees the possible importance of nonmonetary influences (such as fairness) on bargaining environments. Indeed, Roth (1988, p.988) notes: "... the uniformity, with which 'disadvantageous counterproposals' have appeared in the experiments to date, in contrast to their otherwise quite varied results, suggests that bargaining may be an activity that systematically gives bargainers motivations distinct from simple income maximisation."

Although many researchers acknowledge the importance of nonmonetary influences, many remain skeptical that fairness explains the phenomenon of equal splits. This skepticism ranges from a partial to complete rejection of the significance of fairness. In regards to partial rejection, Kravitz and Gunto (1992) hypothesize that a fear of rejection remains more influential than fairness in determining offers in ultimatum bargains. Kravitz and Gunto assert that bargainers perceive equal splits as fair.[9] Thereupon, bargainers realize extreme offers, as predicted by game theory, will be rejected. This leads to two nonexcludable results. First, proposers find that their expected value reaches a maximum near more equitable offers. Secondly, proposers make more equitable offers because they fear rejection, not because they adhere to a strict social norm of fairness.

Hoffman and Spitzer (1985), however, displace the fairness explanation. They attribute equal splits to a lack of earned and morally justified property rights. Specifically, the authors expand their earlier work, Hoffman and Spitzer (1982), on Coasian bargaining where the authors report 89.5% of the bargains exhibit Pareto-optimality but approximately 60.8% of these bargains are either equal splits or within

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[9] Note, the word "perceive" does not imply "believe." For instance, the proposer may not believe an equal split is fair; but nevertheless, the proposer may perceive that his or her opponent believes an equal split is fair.

$1 from an equal split.[10] Subsequently, Hoffman and Spitzer (1985) seek to explain why this non-rational (or non-self-regarding) behavior occurs.[11] First, the authors call for more competitive methods than a coin-flip in designating the UPR holder.[12] In response, they posit that a game-trigger (in particular, the authors use a hash mark game) creates a heighten notion of earned property rights. Moreover, the authors call for moral justification in assigning property rights. They assert that the use of moral authority instructions (for instance, they use the words "earns the right" instead of "is designated" in the experimental instructions) conveys the notion of a morally justified UPR holder. Ultimately, the authors find that the game-trigger/moral authority experiments yield more rational behavior than the coin-flip/no moral authority experiments [the latter situation replicates Hoffman and Spitzer (1982)]. The game-trigger/moral authority experiments give the UPR holder at least his individual maximum in 68% of the bargains whereas the coin-flip/no moral authority experiments yield approximately 27%. This implies that the former treatment stimulates controllers to make more self-regarding offers, while the latter treatment produces controllers whom make relatively higher offers than game theory would predict. Consequently, Hoffman and Spitzer (1985, p.261) conclude that: "The results suggest that subjects behaved in accord neither with the self-regarding

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[10] These results represent irrational behavior. Recall, Hoffman and Spitzer (1982) design their experiment so that the controller (if player B) can take an outside option which pays him or her $12. But the equal split payoff only yields $7 to the controller. Thus, the controller irrationally foregoes ($12 - $7=) $5 when he or she agrees to an equal split.

[11] Alternatively, Harrison and McKee (1985) offer another response to Hoffman and Spitzer's (1982) irrational behavioral results. For example, Harrison and McKee (1985, p.656) state, "we find that the comparable Hoffman-Spitzer results that are inconsistent with individual rationality are attributable to a lack of understanding by certain subjects of the meaning of UPR."

[12] In Hoffman and Spitzer (1982) the controller was determined by a coin flip. Clearly, this procedure remains uncompetitive.

utilitarian theory nor with the egalitarian theory of distributive justice, but rather in accord with the Lockean theory of earned desert."[13]

To continue, Hoffman, McCabe, Shachat, and Smith [1994] (hereafter, HMSS) combine the UPR ideas of Coasian bargaining with anonymity. Many ultimatum game experiments employ between-subject anonymity. The hypothesis is that anonymity controls for nonmonetary factors, such as fairness and subjects' preferences. Though between-subject anonymity helps control subjects' preferences, the proposer, still, maintains strategic expectations about his/her opponent's reservation value.[14] Subsequently, proposers might find that they can maximize their expected value if they make more equitable offers [for example, see Kravitz and Gunto (1992)]. Given this complication, HMSS, following the lead of Forsythe, Horowitz, Savin, and Sefton [1994] (hereafter, FHSS), look towards dictator games as a remedy. In theory, dictator games should control for strategic expectations since the receiver cannot reject any offer. Nonetheless, HMSS, in accordance with FHSS, report the occurrence of equal splits. At this point, HMSS posit that additional nonmonetary factors influence the bargains. First, in connection

[13] Another interpretation arises if one draws an analogy between this experiment and another experiment conducted by van Dijk and Wilke (1993). Van Dijk and Wilke study the effects of asymmetric interest on the provision of public goods. Participants have either a high or low interest in a public good. These levels of interest are either Justified or Not Justified. Justification requires an asymmetry of effort or input, while the Not Justified condition results from a symmetry of effort or input. The experimental results show that High-interest members only contribute more than Low-interest members in Not Justified conditions, for no differences occur in Justified conditions. These results parallel Hoffman and Spitzer's (1985) findings. To explain, the game-trigger/moral authority treatment (Treat 1) might emphasize an asymmetry of effort or knowledge and the coin flip/no moral authority treatment (Treat 2) might imply a symmetry of effort (for each participant has a 50% chance of being controller). Thus, the controller-noncontroller relationship seems justified in Treat 1 and it appears not justified in Treat 2. This means the controller's action (make a small offer) in Treat 1 parallels the High-interest member's action (contribute less) in the Justified condition. Likewise, the controller's action (make a high offer) in Treat 2 parallels the High-interest member's action (contribute more) in the Not Justified condition. Therefore, we might reinterpret Hoffman and Spitzer's (1985) results as follows: self-regarding behavior occurs only when UPR assignments are justified.

[14] The reservation value represents an amount for which the opponent (receiver) bases his or her decisions. To illustrate, if an offer exceeds the reservation value then the opponent accepts; otherwise, the opponent rejects the offer.

with Hoffman and Spitzer (1985), HMSS propose the inclusion of earned, morally justified unilateral property rights. The authors use a knowledge quiz whereupon the subject with the highest score obtains the UPR, and the experimental instructions emphasize the rights of the UPR holder. Additionally, the authors hypothesize that the subject-experimenter relationship indirectly places expectational considerations on the subjects. For instance, subjects "may be concerned about appearing greedy and being judged so by the experimenter" [HMSS, p.349] In response, the authors suggest the use of Double Blind dictator experiments which ensure subject-experimenter, as well as between-subject, anonymity. In sum, HMSS primarily study two types of experiments--an anonymous dictator experiment with contest UPR assignment and a Double Blind dictator experiment. These experiments illustrate the fact that the authors refute the fairness hypothesis; and instead, attribute equal splits to a lack of: (1) anonymity, (2) properly justified property rights, and (3) control over subjects' expectations.

Overall, the results of these experiments refute the fairness hypothesis. Both the anonymous dictator experiment with contest UPR assignment and the Double Blind dictator experiment show a significant occurrence of self-regarding (rational) behavior. Moreover, the Double Blind dictator experiment yields a high frequency of self-regarding offers; it yields 67% $0 offers and 84% $0 or $1 offers. As a result, HMSS (1994, pp. 371) conclude:

> These Double Blind dictator results (which, so far, are robust), imply that the outcomes in both dictator and ultimatum games should be modeled not primarily in terms of other-regarding preferences (or 'fairness') but primarily in terms of expectations--either explicit strategic expectations as in ultimatum games, or implicit concern for what the experimenter (or others) might think or do in dictator games. These Double Blind experimental results are inconsistent with any notion that

> the key to understanding experimental bargaining outcomes is to be found in subjects' autonomous, private, other-regarding preferences.

Thus far, the discussion illustrates experiments which either advocate fairness or refute fairness. Some experiments imply that bargainers act more like "fairmen" than "gamesmen." In contrast, other experiments attempt to disprove the fairmens claim in favor of the gamesmen explanation, as theory predicts. Thaler (1988, p.205), however, sees this as the wrong approach and states:

> In some experiments most Allocators choose even splits, in others most choose the game-theoretic allocation. Future research should investigate the factors that produce each kind of behavior, rather than attempt to demonstrate that one type of behavior or the other predominates.

Shogren (1989, p.320) answers Thaler's plea: "Realistically, the majority of individuals will have a threshold of loyalty which separates the fairman from the gamesman, in each of us. Explaining this threshold will improve the correspondence between theory and observed phenomena." To expound, Shogren uses team Coasian Bargains where each team contains seven members. Each team member bargains individually with an opponent from another team. During each bilateral bargain, each player tries to score points for his/her team. In the end, the team with the most points wins a large reward, while the losing team wins a small reward.[15] Overall, this experimental design attempts to control for loyalty. It allows one to examine whether the bargainer's loyalty resides with the opponent or with the team. The experimental results, overwhelmingly, support the latter. 85.7% of the

[15] For a further explanation on experimental design, see Shogren (1989).

agreements are mutually advantageous while only 4.8% are equal splits. Subsequently,. Shogren (1989, p.322) concludes that

> ...by explicitly defining a group with which the individual can attach his loyalties, experimental evidence is considerably more consistent with theory. The individual is fair not to the opposing player, but to his team. Clearly, fairness requires a context based on a definition of loyalty.

Moreover, the institutional structure within which bargains take place may explain why some bargains yield equal splits while others produce more rational outcomes [Shogren (1994) motivates this argument]. For instance, Roth (1988) notes that equal splits occur more frequently in face-to-face bargains than in anonymous bargains. He offers two possible hypotheses for this difference. One hypothesis suggests that the difference occurs because nonmonetary incentives affect face-to-face bargains more than anonymous bargains. The second hypothesis is as follows: "Face-to-face bargaining also allows many more channels of communication (e.g. tone of voice and facial expression) than does anonymous bargaining in which subjects may be restricted to written messages, and perhaps the differences are due to this" (p. 990). Though both hypotheses contain some truths about face-to-face bargains, several experiments illustrate situations where face-to-face bargains yield a low frequency of equal splits [e.g., see Harrison and McKee (1985), Shogren (1989; 1994), and Shogren and Herriges (1994)].

In one experiment, Shogren and Herriges (1994) report a low frequency of equal splits in an experiment which incorporates greater monetary incentives than previous face-to-face bargaining experiments. To explain, subjects bargain face-to-face in pairs over the chances to win a 10 token reward (valued at $0.50 per token) in a binary lottery. And similar to other Coasian bargaining experiments one of the subjects (the controller) maintains an outside option. The experimental design creates a heighten awareness for monetary incentives by requiring the subjects to contribute to the reward--the controller, noncontroller, and house (experimental monitor) must contribute 5, 1, and 4 tokens, respectively. Additionally, bargaining remains costly. The 10 token reward shrinks as bargaining time elapses. In the end, Shogren and Herriges's results show that only 22% (50 out of 228) of all bargains were either a 50-50 split or within 50-50 by +/- 10%.

Another experiment which reports a low frequency of equal splits in face-to-face bargains is by Shogren (1994). Similar to Shogren's (1989) team style bargains, Shogren (1994) creates a highly competitive atmosphere. Specifically, subjects bargain under a tournament structure. The tournament contains five rounds and begins with 32 subjects divided equally into two draws. Through an elimination process, the sole survivor from each draw meet in the fifth round to decide the overall winner.[16] The results of the experiment indicate an overwhelming tendency for rational self-interested bargains. 84% of the bargaining agreements were rational while equal splits comprised only 4% of all agreements.[17] Thus, one sees the importance of institutional structure in bargaining. It may explain why certain bargains end in equal splits while others achieve more mutually advantageous outcomes. And for the case of face-to-face bargains, Shogren (1994, p. 5) argues, "Rationality in face-to-face bargains exists in institutional structures that support rather than punish such behavior."

16 Consult Shogren (1994) for details on the experimental design.

17 Note, these results conincide with Shogren's (1989) team experiments, where rational behavior occurred in 85.7% of all bargains and equal splits comprised only 4.8% of the bargains.

To summarize, this section reviews the fairman-gamesman debate in bargaining experiments. The review indicates that many experiments report equal splits, but still others yield mutually advantageous agreements. The former experiments do not prove bargainers are strict fairmen any more than do the latter experiments prove that people strictly behave as gamesmen. Clearly, people exhibit both behaviors and a more descriptive approach might explain the conditions which produce one or the other behavior.[18]

4.2 Focal Points

"Focal points" describe another issue in bargaining experiments. They represent any identifiable solution which coordinates bargainers' behavior or expectations [Roth (1985*b*) and Schelling (1960)]. Focal points can significantly affect the outcome in bargaining [Roth (1985*b*)]. For example, Ashenfelter, Currie, Farber, and Spiegel [1992] (hereafter, ACFS) find that when bargainers are pushed off an established focal point, the number of disagreements increases. Additionally, one might argue that the high frequency of equal splits in bargaining experiments illustrates a focalness for 50-50 splits. And though a 50-50 split usually remains unfavorable for one of the bargainers (recall section 4.1), it coordinates the bargainers' decisions. In general, Schelling (1960, p.60) notes,"...among all the available options, some particular one usually seems to be the focal point for coordinated choice, and the party to whom it is a relatively unfavorable choice quite often takes it simply because he knows that the other will expect him to." This is not

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[18] Shogren and Herriges (1994) and Shogren and Kask (1992) also note that bargaining experiments can yield middle ground agreements, called contrained self-interest agreements, where neither equal splits nor rational behavior dominates.

to say that a focal point guides all bargaining behavior nor does it advocate that all bargainers with a favorable position (e.g., the controller in Coasian bargains) concede to a less favorable outcome. Any particular type of bargaining (e.g., unstructured or structured) may yield various results when it is conducted under different conditions. In brief, the main point of this section notes the potential role of focal points in bargaining experiments; not that noticeable focal points always exist and explain bargaining outcomes.

4.3 Expectations in Negotiations

Different views exist over the importance of expectations in bargaining. And this section briefly reviews some of the supporting and opposing arguments.

Ashenfelter and Currie (1990) offer divergent expectations as one cause of disputes in bargaining. Indeed, Schelling (1960, p. 70) proposes that "...explicit bargaining requires, for an ultimate agreement, some coordination of the participants' expectations." Such conjectures represent the heart of the expectations argument. They suggest that participants typically enter bargains with different expectations. The diversity might range from different expected strategies to different expected outcomes. Whatever the source, successful negotiation requires management of participants' expectations [Miller and Colosi (1989)]. Thus far, the argument establishes convergent expectations as a prerequisite for agreement in bargains. Several economic experiments expand this argument to include expectations as a significant variable in determining the outcome of bargains.

First, Roth and Schoumaker (1983) conducted an experiment where the participants' expectations were manipulated through experience and reputation. To explain, subjects bargained over the chances to win a reward in a binary lottery. The subjects were divided into three groups: subjects in group 1 initially bargained with a computer which was programmed to promote a 20-80 division of lottery tickets, subjects in group 2 initially bargained with a computer which was programmed to promote a 50-50 split, and subjects in group 3 (the control group) never bargained with a commuter.[19] After 15 trials participants in groups 1 and 2 bargained, for 10 more trials, with various people from their group. Additionally, each player's performance (i.e., reputation) from trials 11 through 15 was displayed in the 15th trial.[20] Overall, the experimenters theorized that expectations maintained no impact on bargaining outcomes if the bargains from all three groups converged over time, after trial 15. This hypothesis was rejected. The experimental results showed a divergence in bargaining agreements--agreements in group 1 consistently produced 20-80 splits, agreements in group 2 clustered around 50-50 splits, and agreements in the control group consistently fell between the results in groups 1 and 2. Thus, the experimenters concluded that expectations influenced the outcomes.

Furthermore, Hoffman, McCabe, and Smith [1993] (hereafter, HMS) offer experimental evidence which supports the importance of expectations in bargaining. In particular, they replicate HMSS's (1994) dictator experiments with one minor difference.[21] Unlike the HMSS experiment, HMS add the following two lines to the proposer's experimental instructions: "Before making your choice consider what choice you expect the buyer to make. Also consider what you think the buyer expects you to choose." Note, the statements attempt to directly affect the

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[19] The subjects in groups 1 and 2 were never told that they bargained with a computer program.

[20] Note, subjects did not know, a priori, that their reputations would be displayed in trial 15.

[21] Recall the discuss of HMSS (1994) in Section 4.1.

proposer's strategic expectations about his or her opponent's behavior (i.e., reservation value). In contrast to HMSS's results which report a high percentage of economically rational outcomes, the HMS results show a high tendency for equal splits. HMS (1993, p.12) conclude, "These results shed further light on our hypothesis that the Nash equilibrium in the ultimatum game is a function of the expectations of proposers regarding the behavior of counterparts."[22]

Although the previous discussion advocates the importance of expectations in bargaining, an opposing view exists. Specifically, in bargaining models with private information, Kennan and Wilson (1993) refute the importance of expectations in bargaining. They state, "...divergent expectations are predicted and explained by private information--and found to be inconsequential,..." (p. 91). To explain, this view asserts that divergent expectations arise not from a direct difference between expected and actual payoffs, but from a difference in information which bargainers, in turn, use to calculate their expectations. Hence, the key to bargaining agreements lies in a convergence of bargainers' private information, not a convergence of bargainers' expectations. This raises the question of how can bargainers reduce the information disparity.

As a preliminary note, Plott and Sunder (1988, p. 1116) assert that "some sort of knowledge of others' preferences appears to be a necessary condition for aggregation of diverse information." This adds another dimension: how do participants reveal their preferences to one another? Actually, it does not matter whether bargainers try to convey preferences or information, most communication techniques apply to both. For instance, if a buyer only owns $1.00 and a seller

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[22] Note, also, HMSS (1994) attribute their rational behavior results to expectations. Specifically, they assert that bargainers' expectations, if left unconstrained, explain the phenomenon of 50-50 splits in bargaining experiments.

desires \$1.15, explicit communication could reveal either the seller's preference for \$1.15, the buyer's private information of \$1.00, or both. Thus, the following discussion of communication techniques does not remain excludable to either preferences or information disclosure.

To continue, several methods exist for disseminating information. Kennan and Wilson (1993) advocate the use of delays as a credible method for communicating private information. Indeed, many real life negotiators use delays as a means to convey their bargaining positions (e.g., reservation values). Some of the more visible examples of bargaining delays include teacher union strikes, holdouts by professional football players in contract disputes, and the walkout by United States air traffic controllers in the early 1980's. Though delays are widely used in many real life negotiations, many times delay proves too costly and inefficient. For example, some holdouts end in a loss of jobs without mutual gains from bargaining realized, as in the case when President Ronald Reagan fired the United States air traffic controllers in 1981.

Actually, this issue remains central to the experiment which the current paper analyzes. As discussed in chapter 6, subjects bargain over the chances (i.e., probabilities) to win a reward in a binary lottery. The bargainers are given a time limit of five minutes and as the bargaining time elapses the reward shrinks. In fact, the reward shrinks to nothing within five minutes. Thus, in situations like this where transaction costs remain high, delay may not remain an efficient way to communicate private information.[23] Instead, bargainers may want to disseminate information more directly through explicit communication. Explicit communication of

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[23] As a theoretical note, Hessel (1981) develops a bargaining model which describes when delay is and is not beneficial. Specifically, delay remains disadvantageous if continued bargaining causes strong payoff deterioration (i.e., high bargaining costs). Alternatively, delay may be advantageous if payoffs exhibit weak deterioration (i.e., low bargaining costs) over time.

each other's reservation value or preferences may prove less costly than delays in converging information.

In sum, two opposing views exist over the importance of expectations in bargaining. One view maintains that bargaining agreements occur through a convergence of expectations. On the other hand, the second view suggests that expectations arise from information; and thus, bargaining agreements require a convergence of private information, not expectations. Both theories remain plausible. For example, one might arguably explain Roth and Schoumaker's (1983) experimental results as a product of diverse information. Participants in each treatment apparently formed their expectations through experience and information about each other's reputation. Conversely, HMS's (1993) experimental design shows how expectations can change without private information. Through a minor change in the wording of the experimental instructions, HMS observed a change in bargaining outcomes. Thus, both private information and expectations seem to affect bargainers' behavior.[24]

Moreover, at a higher level, information and expectations may combine to shape bargainers' perceptions. In turn, bargainers' perceptions might represent the final link to bargaining agreements. Thompson (1990, p.518) notes: "The perception process is constructive and selective; that is, perception is influenced by the salience of information and the order in which information is presented, as well as by perceivers' expectations, knowledge, and experience. Finally, people's perceptions influence their behavior." Subsequently, bargainers, more likely than

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[24] An experiment which impliments the ideas of Kennan and Wilson (1993) and HMS (1993) might prove constructive, here. Specifically, one could design an experiment (e.g., a factorial design) which gave participants different information about their opponent's payoffs and included different "thought" instructions to each bargainer. In the end, this experiment's results might offer more insight into the influences of expectations and private information in bargaining.

not, make decisions on the basis of a multitude of factors, not just expectations and private information.

4.4 Risk Aversion

Risk aversion plays a potential role in bargaining. Axiomatic and strategic models of bargaining predict that risk aversion remains disadvantageous [Roth (1985*c*)]. Specifically, an inverse relationship exists between a bargainer's degree of risk aversion and rewards earned in negotiated agreements. Roth (1985*c*) proves this conjecture and formally offers the following theorem: "...more risk averse bargainers get a smaller share at equilibrium than do less risk averse bargainers in the same situation" (p. 208).

As a test of this theorem, Murnighan, Roth, and Schoumaker (1987) conduct experiments where a more risk averse person anonymously bargains, via a computer, with a less risk averse person. The experimental results support the game-theoretic predictions which Roth's theorem embodies. In general, it may be reasonable to assume that people make choices in the "real world" based on their attitudes towards risk. Steven N.S. Cheung (1969) shows that attitudes towards risk help explain the observed contractual choices made between landowners and non-landowners (such as tenants and farm hands) in China's agricultural sector.[25] Moreover, in so far as face-to-face bargains depict real world bargaining environments more accurately than do anonymous bargains, Farber, Neale, and Bazerman (1990) find, through face-to-face bargaining experiments, that bargainers'

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[25] Additionally, Cheung (1969) adds transaction costs to the argument. He states, "...the choice of contractual arrangement is made so as to maximize the gain from risk dispersion subject to the constraint of transaction costs" (p. 25). Thus, transaction costs (if present) and risk preferences interact to shape bargaining behavior.

risk levels affect negotiated settlements--"...the outcome tends to favor the less risk-averse party" (p. 380).

Thus, each bargainer's attitude towards risk may help shape his or her decisions. For example, rather than strive for a higher payoff with uncertain success, a risk averse person may seek a lower payoff with greater certainty.

4.5 Cheap Talk

The previous four sections discuss issues which arise in many bargaining experiments. The issues do not always represent an artifact of the experimental design. Some of them enter through social factors [e.g., focal points, as Roth (1987) notes] while others represent individual attitudes (e.g., risk aversion). Now, the discussion digresses somewhat and presents a less common issue, called cheap talk, which is an artifact of bargaining experiments by design. Recent theoretical and experimental work shows that cheap talk can offer an effective way to help bargainers reach agreements. Hence, the current analysis remains interested in cheap talk as a viable bargaining protocol. Chapter 6 explains how this paper incorporates cheap talk into its experiment. But for now the exposition defines cheap talk, explains its role in bargaining, and discusses some theoretical and procedural notes on cheap talk.

By definition cheap talk is a form of explicit communication which allows bargainers to send costless, nonbinding, payoff irrelevant, nonverifiable messages [see Farrell (1987) or Gibbons (1992)]. Such messages can play two roles in bargaining--a signaling and coordination role [Crawford (1990) and Farrell (1987)]. When bargainers have private information, cheap talk can help signal this information in a costless manner. Here, cheap talk is costless in the sense that it does not directly affect the bargainers' payoffs. Whereas costly signals (e.g., costly delays or payoff relevant actions) directly affect the bargainers' payoffs, cheap talk messages (signals) are simply talk which can (only) directly affect the bargainers' beliefs; however, if these beliefs (in turn) affect the bargainers' actions, then cheap talk can indirectly affect payoffs [see Gibbons (1992, Chapter 4)]. As for the coordination role, cheap talk can help coordinate bargainers' actions in games of complete information. Recent experimental work by Cooper, DeJong, Forsythe, and Ross (1989) shows that cheap talk helps resolve the coordination problem in the "Battle of the Sexes" game. Additionally, Farrell (1987) illustrates how cheap talk can coordinate the actions of potential entrants into a natural-monopoly industry.

Following the works of Farrell (1987), Farrell and Gibbons (1989), and Cooper, De Jong, Forsythe, and Ross (1989), the current paper treats cheap talk as: (i) costless, pre-bargaining communication where (ii) the communication enters via one or more rounds of simultaneous announcements by the bargainers.[26] The announcements might reveal the bargainers' strategies, reservation prices, preferences, or other demands. Once the last round of costless communication ends, the bargainers engage in serious, binding negotiations.

Given cheap talk represents a costless mode of communication, one sees a potential usefulness for cheap talk in costly bargains. To explain, first, specify formal bargaining as being costly over time. This is not an unreasonable specification since many "real life" bargainers encounter real time transaction costs, such as lawyer's fees and child care expenses. Second, specify preplay (informal) bargaining as being nonbinding and free (i.e., cheap talk). Informal bargaining (specified as cheap

[26] Kreps (1990, Chapter 12), also, treats cheap talk as costless, pre-bargaining communication, but he does not specify how the communication takes place.

talk, here) illustrates the situation where future bargainers meet, under informal conditions without their lawyers and other financial obligations, prior to formal negotiation proceedings. For example, if two future bargainers plan to start formal negotiations three weeks from today, then an informal meeting, within three weeks from today, can allow the bargainers to send costless, nonbinding, nonverifiable messages. These cheap talk messages might, in some cases, lead the bargainers to a settlement before the formal negotiations begin. In other cases, the cheap talk messages may elicit a priori information and coordination which leads bargainers to a quick (and hence, less costly) settlement once formal negotiations begin. Naturally, it may also be the case that cheap talk exerts no effect on the bargaining process. The former two scenarios make cheap talk a potentially useful protocol scheme for costly bargains. If cheap talk can reduce transaction costs (e.g., reduce time consuming fact finding activities and coordination costs), then maybe cheap talk can stimulate more efficient bargains. Subsequently, the current paper seeks to take the game theoretic concept of cheap talk and experimentally test its usefulness in stimulating efficient bargains when formal negotiations contain positive transaction costs.[27] But before one proceeds with a cheap talk bargaining experiment, a brief review of two articles helps clarify the mechanics of cheap talk.

To start, Farrell and Gibbons (1989) derive a bargaining model which uses one round of cheap talk. In the model cheap talk is restricted to a simultaneous announcement of "keen" or "not keen" by each bargainer. "Keen" implies that a bargainer is willing to participate in serious and meaningful negotiations. And "not keen" implies the opposite of "keen". Overall, Farrell and Gibbons claim that

[27] Moreover, it may not remain unreasonable to assume that cheap talk bargainers will make rational decisions. Farrell and Gibbons (1989) and Gibbons (1992, Chapter 4) show that cheap talk leads to a perfect Bayesian equilibrium in dynamic games of incomplete information.

announcements of "not keen" by both bargainers precludes serious negotiations. Cheap talk is meaningful and leads to serious negotiations only if one or both parties claims to be "keen". Additionally, one might add the argument: the effectiveness of cheap talk relies on whether bargainers perceive cheap talk messages as credible or not credible. For credible cheap talk leads to successful negotiations while non-credible cheap talk may hinder the negotiation process.

Another article which helps clarify the mechanics of cheap talk is the paper by Cooper, De Jong, Forsythe, and Ross (1989) [hereafter, CDFR]. CDFR conduct experiments which implement multiple rounds of cheap talk prior to serious bargaining. Several interesting design notes exist in the experiments. First, the experiments limit cheap talk to simple announcements about players' intended actions. This prohibits uncontrolled communication which can lead to threats. Threats, in turn, (the authors warn) change the bargaining environment and alter payoffs. Secondly, the experimental design gives the participants the right to remain silent during cheap talk. CDFR consider silence as a viable strategy which signals a bargainer's intentions. For example, the authors state that silence might represent a blatant intention to ignore an opponent's announcement or it might signal a player's intention to choose an action (after cheap talk) which obtains a pure-strategy equilibrium, given what his or her opponent announces.[28] Also, bargainers may remain silent because they do not want to reveal information which their opponent can exploit [Ausubel and Deneckere (1992)]. Lastly, it is noteworthy to mention CDFR's results. They find that cheap talk helps coordinate players' behavior and

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[28] Note, in Coasian bargaining experiments, where one player (the controller) has an outside option, silence might indicate the controller's intention to take the outside option, regardless of the other player's announcement(s).

this coordination ability improves with additional rounds of cheap talk [Farrell (1987) also supports this result].

In brief, cheap talk can yield a significant impact on bargaining. It may offer a way to reduce the efficiency costs of transaction costs and it can help coordinate bargainers' behavior.

CHAPTER 5:

MEASURES OF COSTLY BARGAINING AND BASIC HYPOTHESES

As mentioned earlier, the current analysis investigates bargaining behavior in a positive transaction costs environment. In response to Shogren and Herriges's (1994) costly transactions experiments, where the efficiency proposition of the Coase theorem was strongly rejected, the current analysis tests the ability of various bargaining protocols to improve efficiency while simultaneously promoting rational behavior. Through a combination of arguments found in structured (e.g., ultimatum games) and free form (e.g., Coasian bargaining) bargaining, the analysis defines five protocols plus a base group--see Section 6.3 for a description of each protocol. The base group attempts to emulate Shogren and Herriges's (1994) linear transaction costs treatment. Then, through pairwise comparisons between the base group and each protocol, the current experiment will be able to determine whether or not any protocol achieves higher efficiency results than the base group. But before the analysis presents any formal hypotheses regarding the protocols, one must develop measures or indicators of costly bargaining.

Given this, the current chapter proceeds as follows. Section 5.1 begins the discussion with an explanation of Coasian bargaining in a binary lottery framework. Section 5.2 examines three indicators--efficiency, distribution of wealth, and timing--of costly bargaining, then it develops several hypotheses which utilize the indicators.[29] Lastly, Section 5.3 discusses several factors which may remain influencial in the bargaining experiments.

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[29] Note, much of Sections 5.1 and 5.2 will follow Section 2 in Shogren and Herriges (1994).

5.1 Coasian Bargaining in a Binary Lottery

The experiments, discussed further in Chapter 6, employ costly bargaining between two participants (A and B) over the likelihood of winning a large reward, Z, in a binary lottery. Bargaining remains costly in that the large reward shrinks as bargaining time, t, elapses. To capture this functional relationship, the analysis denotes the large (remaining) reward as $Z(t)$, where $Z(t_{max})$ represents the maximum (initial) reward and, for no time remaining ($t=0$), $Z(0)=0$ denotes the disagreement outcome. Let $L=[P_i Z(t);(1-P_i)z]$, where $P_i \in [0,1]$, represent a binary lottery which gives player i $(i=A,B)$ a P_i probability of winning a large reward, $Z(t)$, and a $(1-P_i)$ probability of winning a small reward, z, where $Z(t)>z=0$ {for $t_{max} \geq t > 0$}. [Note, since the disagreement outcome equals $Z(0)=0=z$, no incentive exists for a risk averse person to hold out for $Z(0)$; thus, setting $z=0$ controls for risk posturing--see Shogren (1992).] Upon normalizing each participant's utility so that $U(Z)=1$ and $U(z)=0$, participant i's expected utility from participating in the binary lottery reduces from

$$E[U_i(L)] = P_i U_i[Z(t)] + (1-P_i)U_i(z)$$

to

$$E[U_i(L)] = P_i U_i[Z(t)] + (1-P_i)(0)$$

$$\Rightarrow \qquad E[U_i(L)] = P_i U_i[Z(t)].$$

Thus, participant i's expected utility from bargaining equals the expected value of the utility of the large reward, $P_i U_i[Z(t)]$.

Furthermore, in accordance with Coasian ideology, the present experiment gives one participant (the controller) unilateral property rights over the lottery. The property rights entitle the controller to an "outside option" which, if the controller chooses not to bargain with the noncontroller, guarantees the controller a specified probability, defined as P^O, of winning the large reward. Notice that the outside option represents a threat point for the controller. The controller, at any time, can threaten to unilaterally end bargaining without consent from the noncontroller and take the outside option. If participant i is the controller, i's expected utility from taking the outside option, with t_a minutes remaining, equals $P^O U_i[Z(t_a)]$. And given the economic assumption that people prefer more to less and the fact that transaction costs decrease $Z(t)$ as time elapses, the utility of $Z(t)$, $U_i[Z(t)]$--and hence, the expected value of the utility from taking the outside option, $P^O U_i[Z(t)]$--must decrease as time elapses. For example, if $t_a > t_b$ then $P^O U_i[Z(t_a)] > P^O U_i[Z(t_b)]$.

5.2 Efficiency, Distribution of Wealth, and Timing in Costly Bargains

Given the binary lottery structure presented in Section 5.1, this paper analyzes costly bargaining in terms of efficiency, distribution of wealth, and timing. The following discussion, first, explains these measures and then develops several hypotheses which utilize the measures.

To start, the discussion shows how transaction costs compromise the traditional measure of efficiency; and instead, presents a more accurate measure. If bargaining is costless, one simply defines efficiency as the maximization of the joint probability of winning a reward. This traditional definition of efficiency asserts that bargainers reach agreements which assure a 100 percent chance that one bargainer will win the reward. For example, if player A bargains with player B in a lottery structure, the result remains traditionally efficient if $\Psi = (P_A + P_B) =$ 100%, where P_A and P_B, respectively, denote player A's and player B's probability of winning the reward. Such a definition, however, remains compromised when positive transaction costs exist. To explain, since transaction costs diminish the reward over time, traditionally efficient bargains (if achieved too late) may guarantee with 100% certainty that all players will win nothing. In other words, the time-independent nature of traditional efficiency can make this measure misleading when transactions are costly over time.

Alternatively, the analysis needs a time-dependent measure of efficiency. Shogren and Herriges (1994) develop such a measure which they call "reward efficiency" and denote as R. R is defined, in Shogren and Herriges (1994, p. 3), as "...the improvement in actual expected gain as a percentage of the potential gain due to bargaining." Symbolically,

$$R = \frac{\{[P_A + P_B]Z(t) - P^O Z\}}{Z - P^O Z}, \tag{1}$$

where P_A and P_B denote the final lottery allotment of players A and B, Z is the maximum (initial) reward [i.e., $Z \equiv Z(t_{max})$], $Z(t)$ represents the remaining reward, and $P^O Z$ depicts the initial expected value of the outside option. The numerator in equation (1) denotes the actual expected gains in efficiency over the initial expected value of the controller's outside option, and the denominator represents the potential gains from bargaining. Several observations emerge from equation (1). First, if the bargainers do not reach a traditionally efficient bargain, then $P_A + P_B < 1$ and a

$P_N = 1-(P_A + P_B)$ probability exists that neither player will win. Higher values of P_N correspond to lower values of R. On the other hand, if traditional efficiency, P_A + P_B = 1, is achieved immediately at t_{max}, reward efficiency remains maximized at $R = 1$. In general, note that R can be negative as well as positive (i.e., $R \leq 1$). R becomes negative whenever the expected value of the remaining reward at time t fails to exceed the initial expected value of the controller's outside option; mathematically, $R < 0$ when $[P_A + P_B]Z(t) < P^O Z$.

Another way to analyze bargaining results is through the distribution of wealth. The distribution of wealth indicates how rational the bargainers behave. A rational controller, who owns unilateral property rights over the lottery, should not settle for any agreement which gives him or her a probability, P_C, of winning less than the outside option probability, P^O. In terms of expected wealth, this means that a rational, self-interested controller should acquire a level of expected wealth which equals or exceeds the initial expected wealth from the outside option; symbolically, rational behavior occurs if $P_C[Z(t)] \geq P^O Z$. Nonetheless, contrary to this game-theoretic prediction, many previous experiments (as mentioned in Section 4.1) report a high frequency of equal splits, even though $P^O > 50\%$. Controllers tend to relinquish their favorable bargaining positions in favor of more equitable agreements. This apparent taste for fairness has sparked the fairman-gamesman debate discussed in Section 4.1. The fairman theorists contend that bargainers are driven by nonmonetary factors, such as socially acceptable focal points and social norms of distributive justice. In contrast, the gamesman theorists explain 50-50 splits as products of the experimental design, not the bargainers' "true" behavior.

Although much of the gamesman-fairman debate remains bipolar, recent research shows that bargainers can exhibit a combination of both behaviors.

Specifically, Shogren and Kask (1992) and, later, Shogren and Herriges (1994) find that many controllers take only partial advantage of their bargaining position. They explain this behavior as self-interest constrained by fairness. Assuming the outside option grants a 90 percent chance of victory, probability splits of 60-40, 80-15, and 70-30 depict examples of constrained self-interest behavior.

Shogren and Kask (1992, p.156) interpret the occurrence of constrained self-interest as follows: "Realistically, we all possess a threshold that separates the gamesperson from the fairperson. Constrained self-interest suggests this threshold is not a point, but rather a continuum in which the individual weights the polar cases of strict profit seeking and equity." Another explanation for constrained self-interest may arise from Roth's (1985*b*) "coordination model" which states, "If both players acquiesce to their less favored focal point, some sort of compromise between the two focal points is the final agreement" (p. 265). For example, if the outside option yields a 90-0 split, a 50-50 split remains a favorable focal point for the noncontroller but not the controller. Likewise, the outside option remains a favorable focal point for the controller but not for the noncontroller. Thus, if both players acquiesce to their less favored focal point, then some solution (à la constrained self-interest) between the outside option and equal split will arise. Either way both explanations suggest that constrained self-interest results from a balance between pure self-interest and equity.

Given the arguments for constrained self-interest, one looks for a way to gauge it. Following Shogren and Herriges (1994), the first step is to define the controller's observed probability of winning, P_C. Mathematically, the controller chooses P_C such that

$$P_C U[Z(t)] = \alpha P^O U(Z) + (1-\alpha)\left[0.5 - \left({}^{P_N}\!/_2\right)\right] U[Z(t)], \qquad (2)$$

where α denotes the weight which the controller places on his or her expected utility from the outside option, and (1- α) represents the weight which the controller places on his or her expected utility from an equal split, $[0.5-(P_N/2)]U[Z(t)]$. Note, equation (2) writes equal splits as $[0.5-(P_N/2)]$ to capture those bargains where some probability exists that neither bargainer will win (i.e., $P_N > 0$), and P_N is shared equally, $P_N/2$. For example, if the bargainers agree to a 45-45 split, then P_N= 10% = 0.10; and hence, $[0.5-(P_N/2)] = 0.45$. To continue, if one lets $EQ = [0.5-(P_N/2)]$ and solves equation (2) for α, one obtains

$$\alpha = \frac{P_C U[Z(t)] - (EQ)U[Z(t)]}{P^O U(Z) - (EQ)U[Z(t)]} \qquad (3).$$

Equation (3) gauges the magnitude of constrained self-interest behavior. If $\alpha \geq 1$, the controller is a strict gamesman driven by pure self-interest as he or she seeks a level of expected utility at least as great as the expected utility from the outside option. If $\alpha = 0$, the controller is a strict fairman who only derives utility from equitable splits; symbolically, this means $P_C U[Z(t)] = (EQ)U[Z(t)]$ or dividing through by $U[Z(t)]$ yields $P_C = EQ$. If 0< α<1, the controller remains a constrained self-interested bargainer, striving to weight pure self-interest with equity. Lastly, if α<0, the controller exhibits a high amount of concern for the noncontroller--one might call this "opponent loyalty." Opponent loyalty is most noticeable when a controller accepts a probability of victory less than EQ; i.e., $P_C < EQ$.[30] [As a final note,

[30] Although α<0 seems highly irrational, 8.3% (5 out of 60) of the bargains in the current paper's experiment exhibit opponent loyalty. See Chapter 7.

equations (1) through (3) and much of their interpretation come from Shogren and Herriges (1994).]

Given the above discussion, two measures of rationality, strict and weak rationality, arise [once again, this follows Shogren and Herriges (1994)]. Strict rationality is a time-dependent measure and as such requires bargainers to consider transaction costs. Since equation (3) incorporates transaction costs, it represents a measure of strict rationality.[31] Bargains depict strict rationality if the controller obtains a level of expected utility at least as great as the initial expected utility from the outside option. Mathematically, this occurs when $P_C U[Z(t)] \geq P^O U(Z)$ and $\alpha \geq 1$. Weak rationality is time-independent; and hence, ignores transaction costs. Equation (3) becomes a weak measure of rationality (denoted as α_I) if one drops the utility terms such that

$$\alpha_I = \frac{P_C - EQ}{P^O - EQ} \qquad (4).$$

Weak rationality exists when the controller secures a probability of victory such that $P_C \geq P^O$ and $\alpha \geq 1$. Note, given equations (3) and (4) differ, it remains possible to simultaneously obtain $\alpha \geq 1$ for equation (4) and $\alpha < 1$ for equation (3). "If we reject strict rationality, weak rationality tests if players ignore the transaction costs and simply bargained over the chances of winning" [Shogren and Herriges (1994, p. 5)].

Lastly, the analysis investigates timing (denoted as $TIME$) in negotiations. A binding time constraint (or "deadline") can remain a significant factor in bargaining [Coursey (1982)]. From a lawyer's perspective, Miller and Colosi (1989, p. 32) make

[31] Equation (3) incorporates transaction costs through $Z(t)$. Recall, $Z(t)$ shrinks over time; or more explicitly, $Z(t) = Z - c(t)$ where Z is the maximum reward and $c(t)$ represents the transaction costs over time.

two remarks about time management: (1) "unmanaged negotiations tend to drag on forever," and (2) "deadlines energize the negotiation process." In line with remark (2), Roth, Murnighan, and Schoumaker [1988] (hereafter, RMS) find that many negotiations exhibit a "deadline effect" where bargainers strike an agreement just moments before the deadline ends. For example, in a comparison of four experiments, RMS (1988, p. 808) report, "...41 percent of the observed agreements were reached in the last 30 seconds of bargaining, and 53 percent of these were reached in the last 5 seconds." Though RMS conclude that the deadline effect appears robust across various bargaining environments, the robustness remains questionable when delay is costly (i.e., positive transaction costs exist). If delay is costly, bargainers can potentially capture higher payoffs from an early agreement as opposed to a late agreement [for example, see Rubinstein (1982)]. This gives bargainers an incentive to reach an early agreement. In fact, unlike RMS, Shogren and Herriges (1994) test the deadline effect in costly bargaining and find a high frequency of early agreements which allows the experimenters to reject the deadline effect hypothesis.[32] The current experiment (see Chapter 6) extends Shogren and Herriges's (1994) costly bargaining experiments and investigates how various bargaining protocols, such as cheap talk and an alternating offer-counteroffer structure, affect the agreement time.

Overall, this paper uses the three measures--- R, α, and $TIME$--to test several hypotheses regarding bargaining protocols in costly bargains. The first hypothesis addresses reward efficiency, R. Given Shogren and Herriges (1994) find

[32] As a side note, face-to-face bargaining may contribute to the rejection of a deadline effect. To explain, Harrison and McKee (1985), like Shogren and Herriges (1994), conduct face-to-face bargains and find no evidence for a deadline effect. However, unlike Shogren and Herriges (1994) but similar to RMS (1988), the Harrison and McKee (1985) experiment uses costless bargaining. Subsequently, one cannot rule out face-to-face bargaining as a potential factor. Future research might investigate what impact (if any) face-to-face bargaining has on agreement time.

that Coasian bargaining yields inefficient results in the presence of transaction costs (especially, linear transaction costs), the current paper tests whether or not this result can be reversed with bargaining protocols. In other words, this paper hypothesizes that at least one of the protocols will show an improvement in efficiency over the base group. Next, this paper investigates rationality. It is hypothesized that bargaining protocols will improve R through mutually advantageous (rational) agreements. This means one expects values of α close to or above 1 across all protocol schemes. Lastly, this paper tests a timing hypothesis. Based on Shogren and Herriges's (1994) rejection of the deadline effect in costly bargaining, the current analysis hypothesizes that no deadline effect will exist across all protocol schemes. Moreover, it is hypothesized that the time of agreement will remain independent of protocol scheme. In summary, the three basic hypotheses are:

Efficiency Hypothesis: Given linear transaction costs, at least one bargaining protocol achieves a higher reward efficiency than the base group.

Rationality Hypothesis: Rational bargainers will secure pure self-interested agreements across all protocol schemes.

Timing Hypothesis: Time of agreement remains independent of protocol scheme, and does not occur near the deadline.

5.3 Potential Factors in Bargaining

Various factors may affect the bargaining outcomes, especially R, α, P_C, and $TIME$. This section briefly examines four potential factors.

First, the analysis considers treatment effects. As will be discussed in Chapter 6, the experiment uses six treatments ($q = 6$), where Treatment 1 represents the base (Coasian bargaining) group and Treatments 2 through 6 correspond to the five different protocol schemes. This paper will address two questions regarding the treatments.

Q(1): Does the average bargaining outcome (response) remain constant across treatments?

Q(2): Does at least one treatment significantly affect the bargaining outcome?

To investigate question (1), the analysis will use an analysis of variance (ANOVA) approach based on the following "single-factor" model:

$$y_{ij} = \mu + \tau_i + \varepsilon_{ij} \qquad \begin{cases} i=1,2,3,\ldots,q=6 \\ j=1,2,3,\ldots,r \end{cases}, \tag{4}$$

where y_{ij} represents the jth observation in treatment i, μ is a parameter common to all treatments called the "overall" mean, τ_i represents the ith treatment effect, and ε_{ij} is the random error term. If one takes the expected value of equation (4), one obtains the mean of the ith treatment, μ_i. Formally, the operation yields

$$E(y_{ij}) \equiv \mu_i = \mu + \tau_i \qquad \{i = 1,2,3,\ldots,6$$

where $E(\varepsilon_{ij}) = 0$. From this result, one can derive a formal hypothesis test for question Q(1). Given Q(1) questions the equality of the average outcome across treatments, this inquiry seeks to test the null hypothesis H_O of equality among the six treatment means versus the alternative hypothesis H_A that not all treatment means are the same. Symbolically, the hypothesis test is

$$H_O\colon\ \mu_1 = \mu_2 = \mu_3 = \mu_4 = \mu_5 = \mu_6$$
$$H_A\colon\ \mu_i \neq \mu_k \text{ for at least one pair } (i,k).$$

Alternatively, one can write the above hypothesis in terms of treatment effects τ_i as follows

$$H_O\colon\ \tau_1 = \tau_2 = \tau_3 = \tau_4 = \tau_5 = \tau_6 = 0$$
$$H_A\colon\ \tau_i \neq 0 \text{ for at least one } i.$$

[In general, see Montogmery (1991, pp. 50-55) for a full explanation of the above procedures.] If the overall F-statistic (or F-value) from the ANOVA is statistically significant, then one can reject H_O and support the claim that not all treatment effects are the same. That is, an acceptance of H_A implies that not all treatments yield the same average bargaining response.

To address question Q(2), the analysis will include the treatments as regressor variables in a multiple regression model. Since the multiple regression model will include other regressors besides the treatments, the analysis defers the model specification until after the other regressors are explained. For now, the main point is that the analysis seeks to discover which treatments (if any) influence the bargaining behavior, given the presence of the other regressor variables. Formally, one tests

$$H_O:\ \beta_i = 0$$
$$H_A:\ \beta_i \neq 0$$

where β_i represents the regression coefficient for Treatment i, H_O implies that Treatment i maintains no effect on the bargaining outcome, and H_A implies that Treatment i affects the bargaining outcome. If the analysis rejects H_O for one or more of the treatments, then the analysis can investigate what impact (holding all other regressor variables constant) the significant treatment(s) has (have) on bargaining behavior. For instance, if we consider the response variable α (constrained self-interest weight), a significant and positive β_i coefficient suggests that α increases when individuals bargain under Treatment (Protocol) i. Likewise, a significant and negative β_i coefficient suggests that Treatment (Protocol) i produces a negative impact on α. The same reasoning applies to the other response variables, R, P_C, and $TIME$, too.[33]

Second, the analysis considers a gender effect. The experiment in Chapter 6 investigates one-shot, bilateral bargaining between two individuals where each pair may consist of a male-male, female-male, or female-female dyad.[34] Given

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33 Note, since the analysis investigates bargaining outcomes in terms of R, P_C, α, and $TIME$ (recall Section 5.2), four response variables exist; and hence, the analysis requires four separate regressions. Chapter 7 discusses this further.

34 Experimental subjects were recruited and paired without regard for gender.

experimental psychology research shows that men and women maintain different perceptions of the bargaining situation, opponent, and self, gender may be related to bargaining behavior [Thompson (1990).] Also, Mason, Phillips, and Redington [1991] (hereafter, MPR) find that men and women tend to behave differently at the beginning of repeated game experiments. This finding remains relevant to the current paper's one-shot bargaining experiments where each dyad bargains only once. That is, the initial stage of MPR's experiments parallel the short run aspect of this paper's one-shot experiments. Therefore, based on perception differences between the sexes [Thompson (1990)] and MPR's (1991) finding of a short run gender effect, this paper considers a gender effect.

Third, this paper tests for a loyalty effect. As discussed in Section 4.1, Shogren (1989) finds that if loyalties between bargainers are well-defined, this may explain the difference between equitable and pure self-interest behavior. This suggests that loyalty may represent an important source of explanatory power in bargains, if one knows where each bargainer's loyalty resides. To test this hypothesis, the current analysis explicitly asks the bargainers, through a post experimental questionnaire, who they felt loyal to during the bargaining session. Specifically, the question (see the Post Experimental Questionnaire in Appendix A) reads:

During the bargaining session I felt loyal to _______.

a) myself

b) the other player

c) the monitor

d) someone or something else (please indicate) _______.

A response of a) implies pure self-interest while a response of b) implies opponent loyalty. Choice c) addresses HMSS's (1994) concern for a subject-experimenter effect [recall the discussion of HMSS (1994) in Section 4.1.]. The last choice, d), allows for open-ended responses. Given the bargainers' responses to the above question, the analysis econometrically tests for a loyalty effect. Chapter 7 contains the results.

Lastly, the analysis considers an initial wealth effect. To explain, before bargaining begins participants in each bargaining pair earn tokens (valued at $2 per token) by competing in a memorization game. The participant who wins the most tokens becomes the controller for that bargaining pair (for a further explanation see Chapter 6). The analysis investigates what effect (if any) the difference in initial wealth (tokens) between the controller and noncontroller exerts on bargaining behavior. For example, greater differences in initial wealth may make controllers more sympathetic towards noncontrollers; and subsequently, yield a negative impact on the controller's observed probability of winning, P_C. Alternatively, greater differences in initial wealth may provoke greater incentives for profit seeking, and hence, produce a positive effect on P_C. Through the use of an econometric model which regresses P_C on several predictor variables, including the difference in initial wealth, this paper will determine which of the above examples (if any) seems plausible.

Overall, this analysis considers four factors--treatments, gender, loyalty, and differences in initial wealth--which may affect bargaining behavior. To test which factor or factors remain influential, the analysis uses the following regression model

$$y = \beta_0 + \beta_1 DTOK + \sum_{i=2}^{6} \beta_i \tau_i + \beta_7 G_{FM} + \beta_8 G_{MF} + \beta_9 G_{FF} + \beta_{10} LOYA + \varepsilon \qquad (5)$$

where y = one of the response variables, either R, α, P_C, or $TIME$

$DTOK$ = difference in initial wealth between the controller and noncontroller

τ_i = 1 if the current treatment is Treatment i (i=2,3,4,5,6) and $=0$ otherwise (note that Treatment 1 is the base level)

G_{ik} =1 if the genders of the current bargaining pair consist of a j (j= M for male and = F for female) controller and a k (k= M for male and = F for female) noncontroller, and $G_{jk} = 0$ otherwise

$LOYA$ = 1 if the controller's loyalty response from the Post Experimental Questionnaire is "a", and = 0 otherwise (note, Chapter 7 considers another variation of the loyalty variable)

and ε denotes the random error term.[35] Note, since the analysis considers four response variables, the analysis will run four separate regressions with each pertaining to one of the four response variables--either R, α, P_C, or $TIME$.

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[35] Additionally, in Chapter 7, the analysis partitions the treatments into groups and defines another regression model based on the partitions. Sections 6.3 and 7.2 will clarify the basis for the partitions.

CHAPTER 6:
EXPERIMENTAL DESIGN

Several key elements highlighted the experimental design. First, the basic structure of the experimental design involved face-to-face, one-shot bargaining between two people. Each dyad bargained over the chances to win a reward in a binary lottery. Additionally, not all participants bargained under the same conditions. Six different bargaining treatments existed. For instance, some dyads bargained under a free form framework while others bargained under a structured framework. The assignment of any dyad to a treatment remained completely random. Moreover, each participant bargained only once. This eliminated any learning or reputation effects.[36] Lastly, similar to Shogren and Herriges (1994), this experiment incorporated real-time transaction costs. The transaction costs shrunk the reward over time.

Overall, the above description summarizes the experimental design. The remainder of this section presents the details. Section 6.1 explains the general bargaining framework. Section 6.2 investigates the Nash solution and the controller. Section 6.3 describes the six treatments and Section 6.4 elaborates on transaction costs. Next, Section 6.5 explains the choice of experimental design, a completely randomized design. Section 6.6 mentions several aspects about the experimental

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[36] The use of repeated decision-making experiments may or may not offer more insight than one-shot decision-making experiments. For instance, Thaler (1987, pp. 121-122) states, "There is every reason to believe that an initial response in the laboratory will most likely be the one a subject will make in a similar real-life problem. The response after several learning trials may be no more general than the response students give on exam questions." Moreover, he argues, "Is there any reason to believe that the real world teaches people to choose and judge rationally? Unfortunately, one must be skeptical about this prospect. First, many decisions are made infrequently...Second, even for repeated decisions, the quality of the learning depends crucially on the quality of the feedback... Third, even studies of expert decision making have revealed numerous biases" (p. 122).

subjects and it recounts the experimental procedures. Finally, Section 6.7 concludes with a few additional design notes.

6.1 General Bargaining Framework

Each bargaining session involved the following two choices:

(1) picking a number which establishes an initial allocation of lottery tickets, and
(2) deciding how to allocate the lottery tickets.

In essence, these choices represented two types of agreements or contracts. Choice (1) depicted a number contract while choice (2) represented a transfer contract.[37] A brief overview explains both contracts.

First, the number contract required each bargaining dyad to select a number from 1 to 6 on a Lottery Distribution Schedule. This number specified the initial allocation of lottery tickets. In other words, the bargainers decided how to allocate their initial chances of winning a large reward, $Z(t)$.[38] For example, consider the Lottery Distribution Schedule on page 160 in Appendix A. If the bargainers select number 5, then bargainer A owns 80 out of 100 lottery tickets which gives A an 80% chance to win $Z(t)$, bargainer B owns 15 lottery tickets which gives him or her a 15%

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[37] This terminology follows Shogren and Herriges (1994).

[38] $Z(t)$ implies that the large reward, Z, remains a function of time, t. As bargaining time decreases, the large reward shrinks. For more explanation, see Section 6.4.

chance to win $Z(t)$. Additionally, note that 5 lottery tickets belong neither to A nor to B ; thus, a 5% chance exists that neither bargainer will win the large reward. [39]

Secondly, the transfer contract specified how to allocate the chances of winning $Z(t)$. Once the bargainers chose a number from the Lottery Distribution Schedule, they could decide how to transfer their lottery tickets. To illustrate, suppose the bargainers selected number 5, as in the above example. If A agreed to transfer 75 lottery tickets to B, then A retained a 5% chance to win $Z(t)$ while B's chance to win $Z(t)$ increased to 90%. Upon any joint agreement, the bargainers were required to sign a contract (or known as an "Agreement Form"). The contract stated which number was chosen and how many lottery tickets would be transferred from one bargainer to the other bargainer.[40] Only those joint agreements which were signed remained binding and enforceable. Throughout the experiments all contracts were enforced with 100 percent certainty.

In short, this defines the basic bargaining framework which remains common to all bargains in this experiment. Later, Section 6.3 describes how the bargaining framework differs among the experimental treatments.

6.2 The Nash Solution and Controller

Before a discussion of the treatments ensues, it behooves the writer to explain the Nash Solution and Controller. Section 2.1 discusses the Nash solution to the bargaining problem. Now one attempts to apply this concept in light of this experiment. The solution is not a 50-50 split of 100 lottery tickets. This remains true

[39] Following the terminology of Shogren and Herriges (1994), one says that the house maintains a 5% chance to retain the large reward, $Z(t)$.

[40] The experimental instructions in Appendix A contain a copy of the Agreement Form.

when one bargainer maintains a unilateral property right over the distribution of lottery tickets. To explain, the discussion introduces the concept of a "controller".

Prior to bargaining, each experimental unit (dyad) plays a matching game (a type of memorization game) with cards. The person who finds the most matches wins the game and earns the right to be the controller. This yields a situation where each experimental unit consists of a controller and noncontroller. The controller maintains the right to unilaterally pick a number off the Lottery Distribution Schedule and end the session. No consent from the noncontroller or signatures are required if the controller chooses this option.[41] Assuming the controller is a rational income maximizing individual and faces the Lottery Distribution Schedule in Appendix A, the controller will not settle for any allocation which gives him or her less than 90% of the lottery tickets--this represents the controller's so-called outside option. If bargainer A is the controller, number 6 yields the controller's outside option while number 1 yields the controller's outside option when bargainer B is the controller.[42] Moreover, the allowance of side payments, as in a Coasian bargain, enables the noncontroller to influence the controller to reach a mutually acceptable agreement. For instance, the noncontroller may offer to give some of his or her lottery tickets to the controller.

This returns the discussion back to the bargaining problem and the Nash solution. Note, if the controller takes the 90% outside option, the noncontroller receives 0% and the outcome is inefficient. Inefficiency results because not all lottery tickets (resources) are used.[43] Instead, the Nash solution offers an efficient

[41] The experimental instructions in Appendix A contain a further explanation of the matching game and the controller's rights.

[42] Note that all bargains throughtout this experiment use the Lottery Distribution Schedule in Appendix A; thus, the 90% outside option remains common over all treatments.

[43] Note, this explanation of inefficiency is time independent. The level of inefficiency associated with a 90-0 split may be greater if one considers a time dependent explanation of inefficiency (see Section 5.2).

and rational solution. It requires the bargainers to split the remaining 10% of lottery tickets so that the controller receives 95% and the noncontroller 5%. This result depicts a Pareto-optimal improvement over the outside option. Figure 1 illustrates the argument.[44]

Figures 1a and 1b highlight the bargaining problem when players *B* and *A*, respectively, are the controller. For each figure, let the outside option represent an outcome d such that $d \equiv (d_A, d_B)$. The downward sloping line in each figure illustrates an efficient allotment (of lottery tickets) frontier. Every point, defined as (U_A, U_B), on the frontier depicts an efficient improvement over any point within the frontier. A Nash solution maximizes the product of *A* and *B*'s allotment, U_A and U_B, beyond d [mathematically one expresses this as maximum $(U_A - d_A)(U_B - d_B)$]. This occurs at the midpoint of either figure's allotment frontier where $(U_A - d_A)(U_B - d_B)$ = 25. For instance, suppose *A* is the controller as in Figure 1b. At point E, $(U_A - d_A)(U_B - d_B)$ = (98-90)(2-0) = 16 and at point F, $(U_A - d_A)(U_B - d_B)$ = (91-90)(9-0) = 9. Clearly, no point other than the midpoint (95,5) yields a higher value than 25.

To complete the analysis, one transposes figures 1a and 1b into figure 1c. The solid lines at either end of figure 1c's allotment frontier portray efficient and rational outcomes depending on whether *A* or *B* is the controller. The dashed line represents only efficient outcomes. Points marked as asterisks (*) plot the allotment possibilities for the lottery schedule in Appendix A. Note, all but one asterisk lies inside the frontier. Since every point within the frontier remains inefficient, one finds that the allowance of side payments enables the bargainers to achieve efficient improvements. Hence, the efficient allotment frontier relies on side payments.

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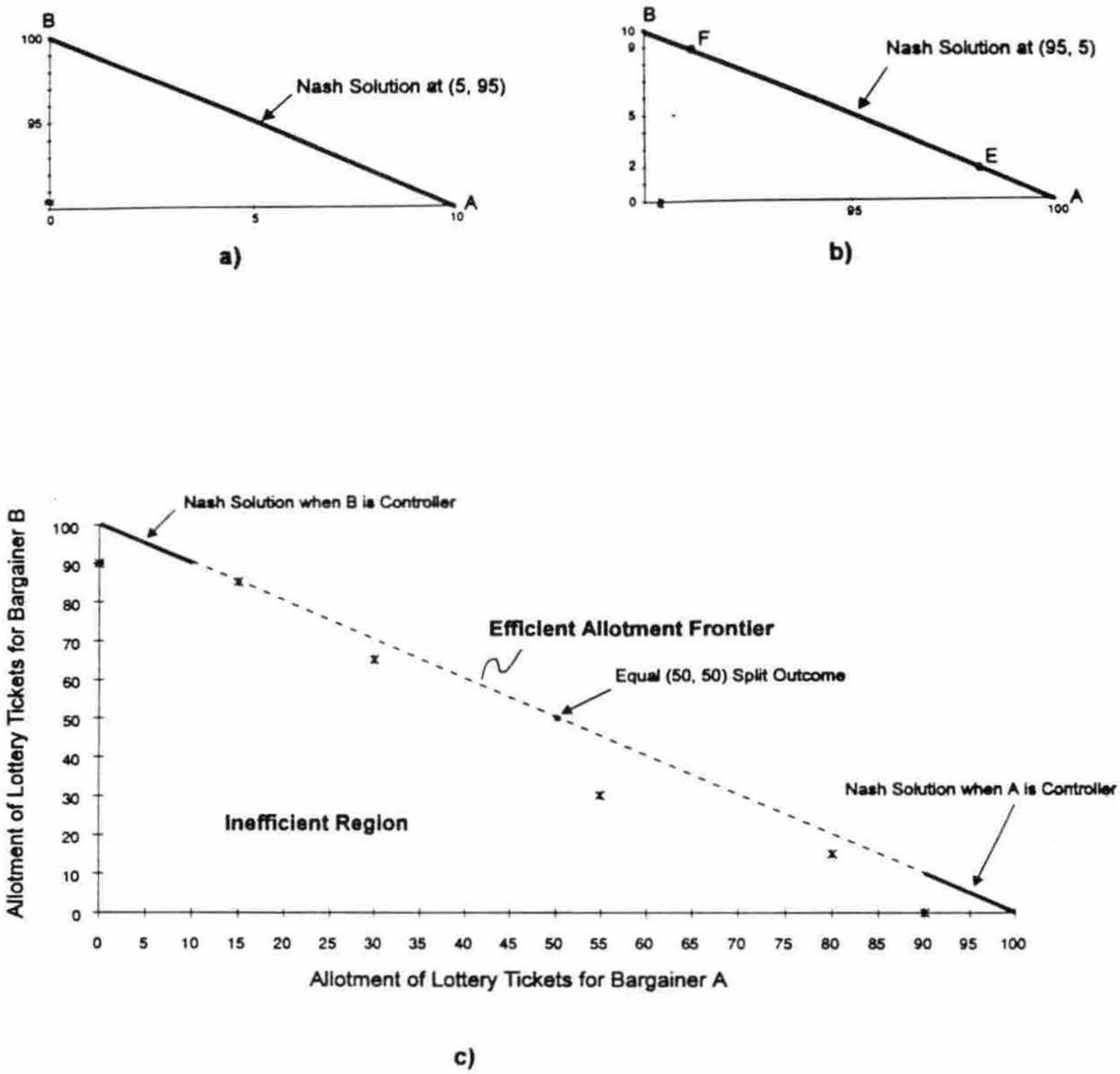
44 The idea for this figure comes from Davis and Holt (1993, p. 259).



x Represents allotment points without side payments.

Note: (0, 90) is Player B's outside option.
(90, 0) is Player A's outside option.

Figure 1. The Nash Solution to the Bargaining Problem

Up to this point, the discussion illustrates how the controller is chosen and how the controller's unilateral property rights over the lottery distribution schedule affect the Nash solution. Another, more psychological issue persists here. The experimental design must instill a belief in the controller that he or she maintains this unilateral property right. This issue falls back to the subject matter of Section 4.1. Section 4.1 addresses experimental techniques which help induce such beliefs. Among the various techniques presented, this experiment utilizes three--competitive methods (i.e., game-triggers), moral justification, and a monetary incentive.

Similar to game-triggers found in Hoffman and Spitzer (1985) and Shogren (1992) or the knowledge quiz as in HMSS (1994), the matching game offers a competitive way to determine the controller. Unlike a simple coin flip or an arbitrary designation, the matching game requires a certain level of memorization skills; and hence, may induce an earned or warranted feeling of achievement. Additionally, this experiment implements morally justified wording. As in Hoffman and Spitzer (1985), the experimental instructions state "earns the right" instead of "is designated" whenever a reference exists towards the determination of the controller. Lastly, following Shogren and Herriges (1994), the design uses a monetary incentive. To explain, the maximum reward, Z, consists of 10 tokens valued at $2 per token. Z is derived from contributions by the controller, noncontroller, and the house (monitor). The controller must contribute 5 tokens, the noncontroller 1 token, and the house provides the remaining 4 tokens (5+1+4=10). This develops a situation where the controller potentially stands more to lose than the noncontroller. Participants earn tokens during the matching game. Each time a participant finds a match he or she earns one token valued at $2. The matching game maintains a design which yields the controller (the winner of the game) at least 5 tokens.[45] Overall, the monetary incentive technique, in combination with the matching game and moral justification wording, tries to elicit rational, self-regarding behavior from the controller.

In summary, the Coasian nature of the experiment, which defines a unilateral property right holder and allows side payments, yields a Nash solution consisting of a 95-5 split. Such a solution is rational and efficient. Indeed, any split which gives the controller 90% or more of the lottery tickets while utilizing all 100 lottery tickets remains rational and efficient.[46] Moreover, the experiment attempts to induce rational behavior through the use of competitive methods, moral justification, and a monetary incentive.

6.3 Treatments (Protocol Schemes)

This experiment examines bilateral bargaining under six different situations, known as treatments or protocols. Treatment 1 represents the baseline group where participants bargain in a costly Coasian environment. Treatments 2 through 6 incorporate some type of protocol scheme where bargainers must follow more rules than in pure Coasian bargaining. Given this, the following discussion begins with a further explanation of Treatment 1 and continues by explaining the protocol scheme in Treatments 2 through 6.

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[45] The matiching game uses ordinary playing cards. The game contains nine matches (or pairs). Thus, the winner will always obtain at least 5 matches which yields 5 tokens. For further details, see Experimental Instructions in Appendix A. [In the advent that a player finds zero matches (this implies a noncontroller with zero tokens), the house contributes 5 tokens. This occurred once during the experiment.]

[46] The argument ignores transaction costs; and hence, discusses efficiency in the traditional sense. As noted in Section 5.2, transaction costs can compromise this definition of efficiency.

Treatment 1 (T1) employs free form (Coasian) bargaining. This represents the control or baseline group. The term free form arises because the bargains remain unstructured. Bargainers are free to make offers and counteroffers whenever they wish, within the specified time limit for bargaining. This lies in contrast to a Rubinstein model where the bargainers must take turns in a sequential manner. Moreover, T1 allows verbal communication between the two bargainers; however, any verbal offers remain nonbinding. The creation of any obligations must occur through written offers. This facet exists in all treatments.

Similarly, Treatment 2 (T2) uses the free form format, except T2 allows nonverbal pre-bargain communication. The experiment employs "cheap talk" as the pre-bargain communication. Recall, cheap talk remains nonbinding and costless. The bargainers participate in two rounds of cheap talk. In each round, the bargainers have ten seconds to write a response (in terms of lottery tickets) on a piece of paper which reads:

"The <u>minimum</u> I am willing to <u>accept</u> is _______."
"The <u>maximum</u> I am willing to <u>offer</u> is _______."

When ten seconds elapse, the bargainers simultaneously exchange papers. This format allows each bargainer to simultaneously announce their reservation prices or strategy. If a participant chooses not to respond, then he or she must write "NR" in the spaces. This requirement eliminates the potential revalation of a no response strategy before the round ends. To explain, since the bargains occur face-to-face, writing nothing might reveal a no response strategy. This, in turn, may influence the other bargainer's response. Thus, writing nothing undermines the simultaneous nature of the cheap talk rounds. Finally, bargaining begins immediately after the second round.

While Treatments 1 and 2 use free form bargaining, Treatments 3 through 6 employ structured bargaining. The structured format entails alternating offers. As in a Rubinstein model, bargainers must wait their turn to make offers and counteroffers and waiting remains costly. No restrictions, other than the bargaining session time limit of five minutes, exist on the number of offer and counteroffers allowable and on the amount of time for making an offer. However, unlike the Rubinstein model but similar to Coasian ideology, the controller at any time (whether in turn or out of turn) maintains the right to unilaterally pick a number and end the session.

To continue, structured bargaining includes several other components. All treatments with structured bargaining do not allow verbal communication between bargainers. This represents another difference between free form bargains and structured bargains. The allowance of verbal communication does not give an experimenter control over a sequential offer structure. Moreover, given the controller maintains a bargaining advantage (the outside option) over the noncontroller, one finds an interest in the potential effects of who moves first--the controller or noncontroller. Baik and Shogren (1992) show that it is advantageous for the "underdog" to move first. Although the current experimental design presents no true test of Baik and Shogren's theory, the design draws on their results and investigates the effects of the controller (noncontroller) moving first. Specifically, here lies the difference between Treatments 3 and 5 and Treatments 4 and 6.

Treatment 3 (T3) represents structured bargaining with the controller as the first mover. In contrast, Treatment 4 (T4) gives the first move to the noncontroller. Thus, T3 and T4 differ only in who moves first.

Treatments 5 and 6 mirror Treatments 3 and 4, respectively, except the former two treatments incorporate two rounds of pre-bargain cheap talk as does T2. Specifically, Treatment 5 (T5) uses structured bargaining with cheap talk and the controller as first mover. Treatment 6 (T6) utilizes the same bargaining format as T5, but T6 yields the first move to the noncontroller. Thus, the difference between Treatments 3 and 4 and Treatments 5 and 6 is that Treatments 5 and 6 embody cheap talk.

In brief, this experiment uses three protocols-- cheap talk, a formal offer-counteroffer structure, and specific first-move restrictions where either the controller or noncontroller moves first. Each protocol appears in at least one treatment. Table 1 summarizes the treatments.[47] Note, three major comparisons exist. First, Treatments 1 and 2 use free form bargaining while Treatments 3 through 6 employ structured bargaining. Second, Treatments 2, 5, and 6 utilize cheap talk whereas Treatments 1, 3, and 4 do not. Thirdly, Treatments 3 and 5 allow the controller to move first while Treatments 4 and 6 restrict the first move to the noncontroller. These comparisons yield treatment contrasts which remain testable in an analysis of variance. Chapter 7 presents the results.

6.4 Transaction Costs

All bargains maintain a 5 minute time limit. The large reward, $Z(t)$, remains a function of time. $Z(t)$ shrinks as bargaining time elapses, where $Z(5) = 10$ tokens represents the maximum reward. The rate at which $Z(t)$ shrinks remains constant; thus, the experimental design uses linear transaction costs. This continuous time

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[47] For a further explanation of the treatments, see the Specific Instructions in Appendix B.

Table 1. Summary of Treatments (Protocols)

Protocol	Cheap Talk	Bargaining Structure	First Mover
1	No	Free Form	N/A[a]
2	Yes	Free Form	N/A
3	No	Alternating Offers	Controller
4	No	Alternating Offers	Noncontroller
5	Yes	Alternating Offers	Controller
6	Yes	Alternating Offers	Noncontroller

[a] N/A implies "not applicable" which means that the given description remains irrelevant.

framework appears in Table 2 and Figure 2. Table 2 lists the "Reward Schedule" while Figure 2 plots this schedule. Note, if 2 minutes elapse, then 3 minutes remain and the reward is worth 6 tokens [$Z(3) = 6$ tokens]. If 2.25 minutes elapse, then the reward shrinks to 5.5 tokens [$Z(2.75) = 5.5$ tokens]. And if 5 minutes elapse without an agreement, $Z(0) = 0$ tokens--this defines the disagreement outcome.

Prior to bargaining each subject was given a Reward Schedule. Additionally, each bargaining dyad had a digital timer which showed the time remaining (this meant the timers were designed to count down from 5 minutes). In the advent of a joint agreement, both parties were required to sign the Agreement Form before their timer could be stopped. If the controller chose to unilaterally end the session, he or she could push the stop button without the noncontroller's consent. In either case, the time remaining and the remaining reward was noted by the monitor after the timer stopped.

6.5 A Completely Randomized Design

Overall, the experimental design remains a completely randomized (CR) design. Three distinct properties characterize a CR design: (1) the experimental units or materials are homogeneous, (2) each experimental unit maintains an equal chance of being assigned to any particular treatment, and (3) the experiment proceeds in a random order. The present experiment contains these properties.

First, the present experiment satisfied the homogeneity condition. The subject pool (which made up the experimental units) remained fairly homogeneous. 94.17% of the 120 subjects were college students, at the University of Rhode Island (URI), between the ages of 17 and 23 years old. The majority of the remaining

Table 2. Listing of the Reward Schedule

Time (min.) Remaining	Reward
5.0	10
4.5	9
4.0	8
3.5	7
3.0	6
2.5	5
2.0	4
1.5	3
1.0	2
0.5	1
0.0	0

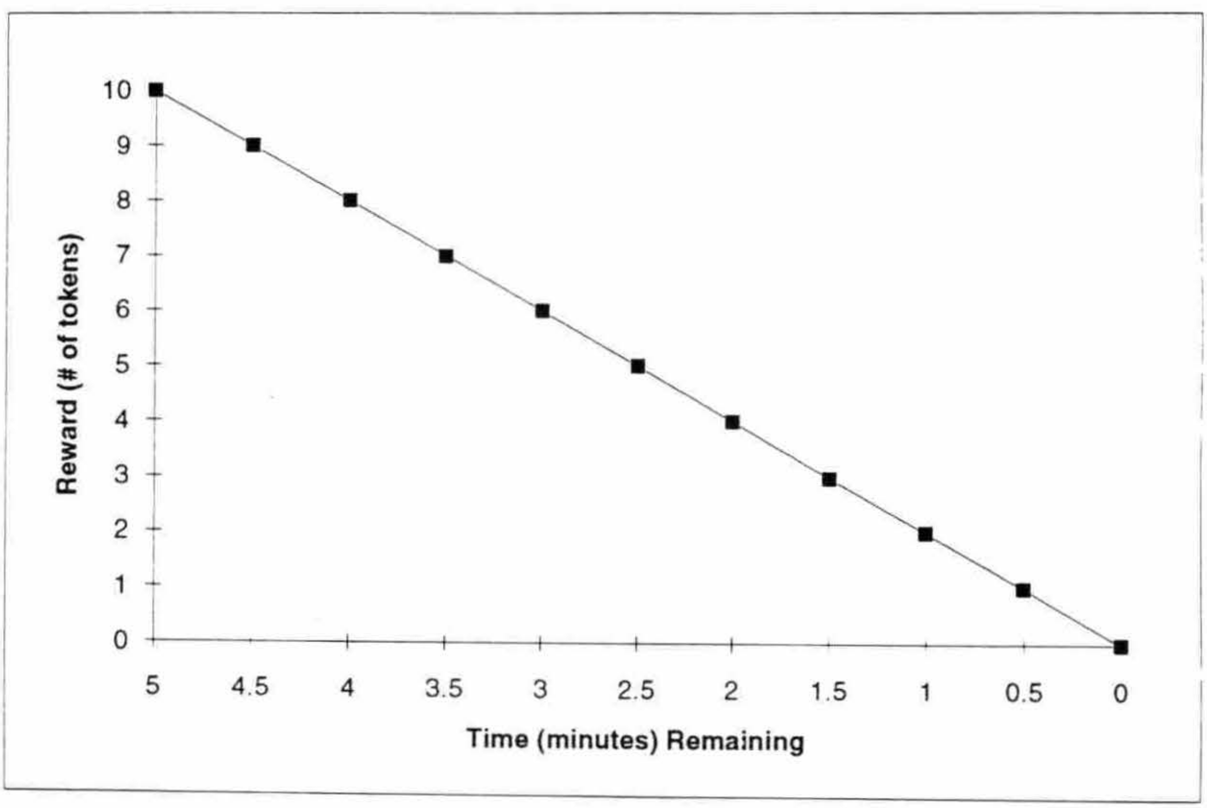

Figure 2. Graph of Reward Schedule

5.83% were, also, URI students, but these students' ages exceeded 23 years old. Furthermore, the subjects used the same, basic experimental materials throughout the experiment. Each bargaining dyad kept time with a digital timer, all matching games utilized ordinary playing cards, all tokens were standard poker chips, and all bargaining pairs used the same reward schedule. The experimental units maintained similar characteristics--student status and common school-- and used the same basic materials.

As mentioned earlier, the experiment employs six treatments. Since the researcher remains equally interested in all treatments, each treatment is equally replicated. In fact, the experiment replicates each treatment 10 times (r =10 for each treatment). Replication offers an important element in any experiment. "It allows the experimenter to obtain an estimate of the experimental error" [Montgomery (1991), p.8]. Thus, large r values remain desirable. But many times feasibility and budget constraints restrict one's choice of r. This experiment represents no exception, for the choice of $r = 10$ appears reasonable under the experiment's time and budget constraints. Given six treatments ($q = 6$) and the choice of $r = 10$, the total number of observations, N, is 60 [i.e., $N = qr = (6)(10) = 60$]. Each observation represents a bargaining outcome between two people, where every bargaining dyad depicts an experimental unit. This information leads to a discussion of the second characteristic of CR designs.

At this point, most CR designs would number the experimental units from 1 to 60, then obtain a random order of these numbers and assign the first r numbers to treatment 1, the second r numbers to treatment 2, and so on. This randomization scheme ensures that the experimental units are allotted to the treatments by chance. Unfortunately, such a randomization scheme remains implausible for the current experiment. To explain, the experiment requires nine experimental sessions, three per day for three days.[48] Since the experimenter cannot force subjects to attend specific experimental sessions, the time constraint prohibits a priori assignments of experimental units to treatments. Thus, the experiment needs a different randomization plan.

Specifically, the alternative randomization plan entailed several steps. Step one involved the randomization of treatments among bargaining pairs. The Statistical Analysis System (SAS) was used to randomly assign the six treatments to pairs 1 through 60 (recall, $N = 60$). Step two was performed before each experimental session. This step randomly assigned subjects to a bargaining pair number. To explain, after the subjects entered the laboratory and seated themselves, a bag which contained slips of paper was passed among the subjects. Each subject withdrew a slip of paper from the bag. This paper contained one of two letters, "*A*" or "*B*", and a number. The number matched subjects into pairs and the letter identified subjects in each pair. For example, subjects *A*22 and *B*22 represented bargainers *A* and *B* for the 22nd bargaining pair. Table 3 summarizes the randomization plan. Each "T" followed by a number represented the treatment assigned to the bargaining pair number in parentheses. For instance, treatment 6 (T6) was randomly assigned to bargaining pair 22.

Lastly, the present experiment fulfilled the third property of CR designs. In accordance with Table 3, the experimental sessions followed the bargaining pair numbers. To explain, the first two experimental sessions observed pairs 1 through 12 and the last session observed pairs 50 through 60. Though the experiment

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48 Sessions were held at 9 a.m., 12 noon, and 2:30 p.m. each of the three days. The 9 a.m. sessions contained an average of 3 pairs while the 12 noon and 2:30 p.m. sessions contained an average of 8 pairs. Due to the comparatively low attendance at 9 a.m., future experiments might avoid morning sessions.

Table 3. Summary of Randomization Plan

T1 (1)	T4 (2)	T3 (3)	T1 (4)	T6 (5)	T4 (6)	T2 (7)	T4 (8)	T2 (9)	T1 (10)
T4 (11)	T3 (12)	T6 (13)	T3 (14)	T1 (15)	T5 (16)	T4 (17)	T2 (18)	T1 (19)	T2 (20)
T5 (21)	T6 (22)	T3 (23)	T5 (24)	T2 (25)	T5 (26)	T5 (27)	T6 (28)	T4 (29)	T3 (30)
T5 (31)	T4 (32)	T5 (33)	T6 (34)	T5 (35)	T5 (36)	T1 (37)	T2 (38)	T2 (39)	T6 (40)
T6 (41)	T4 (42)	T4 (43)	T6 (44)	T2 (45)	T3 (46)	T2 (47)	T3 (48)	T3 (49)	T2 (50)
T3 (51)	T5 (52)	T6 (53)	T1 (54)	T3 (55)	T1 (56)	T1 (57)	T6 (58)	T4 (59)	T1 (60)

Note: the top element in each box indicates the treatment and the numbers in parentheses represent the bargaining pair number.

observed the pairs in numerical order, the treatments were randomly assigned to the bargaining pairs. This meant that none of the experimental sessions employed only one treatment. For instance, the first ten observations (or bargaining pairs) did not solely utilize treatment 1, observations 11 to 20 did not solely use treatment 2, and so on. The treatments were used in a random order; and thus, the experiment proceeded in a random order.

To recap, the experimental design remains a completely randomized design. The experimental units appear homogeneous; each experimental unit maintains an equal chance of bargaining under any particular treatment; and the experiment proceeds in a random order.

As a final note, it remains noteworthy to mention the importance of randomization in all experimental designs, not just CR designs. Randomization ensures against those biases caused by extraneous factors or sources of variation. It usually validates the statistical assumption which requires the observations or experimental errors to be independently distributed random variables [Montgomery (1991, p. 9)]. Furthermore, Cochran and Cox (1957, p. 8) note,

> Randomization is somewhat analogous to insurance, in that it is a precaution against disturbances that may or may not occur and that may or may not be serious if they do occur. It is generally advisable to take the trouble to randomize even when it is not expected that there will be any serious bias from failure to randomize. The experimenter is thus protected against unusual events that upset his expectations. ... It should be realized ... that failure to randomize at any stage may introduce bias unless either the variation introduced in that stage is negligible or the experiment effectively randomizes itself.

6.6 The Subjects and Experimental Procedures

Sections 6.1 to 6.5 explain the operational components of the experimental design. This section (6.6) discusses how the experimental subjects were recruited, who the subjects were, and it recounts the experimental procedures.

120 subjects were recruited campus-wide at the University of Rhode Island. The recruitment methods involved advertisements in the school newspaper, distribution of a flyer, visits to various classrooms, and a sign-up booth located in a popular meeting place (the Memorial Union) on campus. During the recruitment process, special care was taken not to divulge detailed information about the experiments.[49] Potential recruits were told nothing beyond what appeared on the flyer (see page 162 in Appendix A for a copy of the flyer). In other words, the information on the flyer served as standard information given to potential recruits regardless of recruitment method.

Since the recruitment methods covered the entire campus, the subject pool represented a mixture of undergraduate and graduate students from a variety of academic disciplines. Moreover, as mentioned in Section 6.5, 94.17% of the 120 subjects were between 17 and 23 years old while the remaining 5.83% were older than 23. Overall, this experiment tried to obtain a sample which remained representative of the URI's population.

Given a description of how the subjects were recruited and who the subjects were, the discussion, now, turns to an outline of the experimental procedures. The experimental procedures follow sixteen steps. Table 4 summarizes these steps.

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[49] Any pre-experimental disclosure about experimental design or theoretical expectations can introduce bias into experiments. Davis and Holt (1993, pp. 58-60) offer suggestions on how to recruit subjects without introducing biased expectations.

Table 4. Summary of Experimental Procedures

Step	Description of Event
1	Randomly assign I.D. numbers to subjects.
2	Read "General Instructions" aloud to subjects.
3	Complete the "Pre-Experimental Questionnaire."
4	Move subjects to bargaining tables.
5	Play the matching game to determine the controller.
6	Distribute tokens to each subject.
7	Let subjects silently read their "Specific Instructions".
8	Explain the "Offer Sheet".
9	Distribute the "Agreement Forms".
10	Setup reward--controller, noncontroller, and house contribute 5, 1, and 4 tokens, respectively.
11	Ask if everyone understands.
12	Distribute Lottery Schedules to cheap talk pairs and start the cheap talk phase.
13	Distribute Lottery Schedules to all other pairs and start bargaining time for every pair.
14	Monitors note remaining time and reward, plus they randomly determine, based on the distribution of lottery tickets, who wins the reward.
15	Complete the "Post-Experimental Questionnaire."
16	Pay subjects--hourly payment ($2/hour), the reward (if won), and any tokens (valued at $2/token) left over.

Though many of the steps in Table 4 remain self explanatory, the following discussion recounts the procedures and offers explanatory notes for each step.

After the subjects entered the laboratory and seated themselves, step 1 began. The subjects were randomly assigned an identification number which consisted of either an "*A*" or a "*B*" letter followed by a number. As explained in section 6.5, the letter identified each subject as either an *A* or a *B* player and the number matched subjects into bargaining pairs--for example, subject *A*17 was the *A* player for bargaining pair 17.[50] In step 2, the experimenter read the General Instructions (see Appendix A) aloud. This method (reading the instructions aloud) was used to create common knowledge among the subjects. Next, step 3 tested the subjects' understanding of the instructions through the use of a pre-experimental questionnaire (see questionnaire in Appendix A). When the subjects completed the questionnaire, the experimenter went over the answers and clarified any remaining questions from the subjects. Once the subjects understood the general instructions, the subjects moved to the bargaining tables (step 4)[51] and played the matching game to determine the controller (step 5).[52] Based on the results of the matching game, the monitors, in step 6, distributed the tokens to each subject.

In contrast to step 2, step 7 let the subjects read their Specific Instructions in silence. Since each experimental session involved three to twelve pairs which bargained under different treatments, no particular set of instructions could be

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50 Refer back to Section 6.5 for details on how the identification numbers were randomly assigned.

51 Prior to step 4 the subjects sat in chairs at the front of the laboratory. In step 4, the subjects not only moved to the bargaining tables, they discovered who their partner was. At this point, the experimenter asked if anyone was paired with a friend. If so, the pair(s) was split-up and each bargainer was assigned a new partner.

52 Refer to Appendix A for the logistics of the matching game.

anonymously read aloud.[53] Each subject was given a set of specific instructions and a questionnaire followed by an answer key. The questionnaire elicited each subject's understanding of their specific instructions. After the subjects checked their answers, the experimenter and monitors visited each bargaining table to make sure that no remaining questions existed.

To continue, the experimenter, in step 8, verbally explained the Offer Sheet. The subjects made offers and counteroffers through the Offer Sheet (see Appendix A for a copy of the Offer Sheet). After a player wrote an offer, the other player could either accept (circle "Yes") or reject (circle "No") it. If the player rejected the offer, he or she could make a counteroffer, and so on. The Offer Sheet remained the only mode of communicating offers and counteroffers in the structured treatments. In the free form treatments, players could make both verbal and written offers, but only written offers remained binding. Following step 8, the monitors distributed the Agreement Forms (see Appendix A for a copy of this form). Next, step 10 setup the 10 token reward. The controller, noncontroller, and house contributed 5, 1, and 4 tokens, respectively.[54]

At this point (step 11), the experimenter asked if everyone understood the instructions. The subjects were told, "This is it. You're only going to do this once. Are there anymore questions?" Upon the exhaustion of all questions, the experiment proceeded to step 12. The monitors distributed, face-down, the Lottery Schedule to each player in the cheap talk pairs.[55] Then the bargainers turned over

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53 Appendix B contains the Specific Instructions for all the treatments.

54 See Section 6.2 and the General Instructions in Appendix A for more details on the reward.

55 Note that the Reward Schedule appeared with the Lottery Schedule on the same sheet of paper--see Appendix A.

their Lottery Schedules as they simultaneously began the cheap talk phase.[56] While the cheap talk phase ensued, the monitors gave out, face-down, the Lottery Schedule to each player in all of the other pairs. The moment the cheap talk phase ended, the experimenter and monitors started the bargaining times for all groups. This marked the start of step 13.

As each experimental unit finished bargaining, they were instructed to remain seated and quite. When all bargains were completed or the five minute time limit expired, step 13 ended and step 14 began.[57] In step 14, the monitors noted the remaining time and calculated the remaining reward, for each experimental unit. Additionally, the monitors, based on the distribution of lottery tickets, randomly determined who won the reward. To explain, the monitors had a bag which contained 100 pieces of paper numbered 1 to 100. If, for example, a bargaining pair agreed to a 30-65 split (where player A owned 30 tickets and player B owned 65 tickets), player A won if the monitor drew any number from 1 to 30, player B won if any number from 31 to 95 was drawn, and the house retained the reward if a number from 96 to 100 was withdrawn. At the completion of all lottery draws, the final two steps followed.

Step 15 involved the completion of a post-experimental questionnaire (see the Post-Experimental Questionnaire in Appendix A). This questionnaire elicited personal traits, such as sex, age, and major, and it directly asked where each

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56 One cannot say enough for the importance of giving out the Lottery Schedule just before bargaining begins. If the monitors distribute this schedule too soon (say around step 7), many players will start to bargain before the bargaining time begins. (Actually, this problem was discovered in a practice session.) However, the experimenter maintains more control over the experimental session if he or she distributes the Lottery Schedule just before bargaining begins.

57 Actually, no experimental unit exhausted the entire five minute limit. All experimental units either settled on a joint agreement or the controller took the outside option. See Chapter 7 for more details on the experimental results.

subject's loyalty lied. Given Shogren's (1989) loyalty argument, the experimenter implemented the loyalty question in an attempt to help explain each subject's bargaining behavior.[58] Once the subjects completed a post-experimental questionnaire, the final step occurred. Each subject received a cash payment which consisted of an hourly payment ($2 per hour), the reward (if won), and any tokens (valued at $2 per token) left over.

6.7 Additional Design Notes

This section concludes chapter 6 with four additional notes. Specifically, this section includes a discussion of: (1) the laboratory, (2) the prohibition of physical threats, (3) the exclusion of cueing words, and (4) the justification for face-to-face bargains.

First, the size and location of the laboratory helped simplify several logistical concerns. A large room in the URI's Memorial Union (MU) served as the laboratory. The lab was spacious enough to allow twelve pairs to bargain without the pairs disturbing one another.[59] Additionally, since the lab remained situated in the MU, most subjects had no trouble finding it.[60]

Another facet of the experimental design was the prohibition of physical threats. Like other cooperative models (such as cartels), negotiations remain crippled when threats exist. In fact, Hoffman and Spitzer (1982, footnote 8, p. 75)

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58 Section 5.3 addresses this question in more detail.

59 Twelve individual tables were setup and dispersed throughout the lab. This meant that the pairs could be separated and alloted to individual tables.

60 The MU is a common gathering place on campus at the URI. It contains food services, a mini-market, a bank, an ATM machine, a video arcade, a hair salon, the URI bookstore, and a host of other services.

note, "once a threat is carried out the Coase Theorem has failed." Subsequently, the current experiment's general and specific instructions clearly stated that "no physical threats are allowed". Thirdly, the experimental design excluded any words which might have suggested how the subjects were expected to behave.[61] For example, the experimental instructions avoided the words "partner" and "opponent" and instead used the neutral phrase "other player". Also, no technical terminology, such as income maximizer, fairman, or gamesman, was used.

Lastly, the experimental design used face-to-face bargains. Unlike the anonymity experiments conducted by HMSS (1994), this experiment strived for a more realistic environment. To explain, many (if not most) negotiations, from the environmental talks at the Earth Summit in Rio de Janeiro, Brazil during June 1992 to the haggling over souvenir prices in Taipei, Taiwan, occur face-to-face. And though it may be argued that face-to-face bargains retain less control over nonmonetary incentives than anonymous bargains, bargaining models should be robust enough to capture many aspects of reality [see footnote 12 in Shogren and Herriges (1994)].

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[61] Binmore, Shaked, and Sutton (1988) discuss how the use of such cues can bias the subjects' behavior towards theoretical expectations.

CHAPTER 7:
RESULTS AND DISCUSSION

Recall, Chapter 5 develops the response variables and hypotheses which remain applicable for the experiment presented in Chapter 6. The current chapter utilizes this information to analyze the experimental results (Appendix C contains a list of the data set). As a preliminary analysis, Section 7.1 begins this chapter by summarizing and discussing the descriptive statistics which result from the experiment. Several observational implications regarding reward efficiency, distribution of wealth, and timing emerge from the descriptive statistics. Next, Section 7.2 presents a test procedure, called the analysis of variance, and uses this procedure to statistically test for differences among the treatment means. Section 7.3 analyzes the experimental results from an econometric perspective. Lastly, Section 7.4 summarizes the key results and discusses the economic implications.

7. 1 Summary Statistic Results

As a preliminary analysis, this section analyzes the descriptive statistics from the experiment. Table 5 summarizes these statistics. Several implications regarding efficiency, distribution of wealth, and timing emerge from Table 5.

In regards to reward efficiency, R, Table 5 shows low levels of efficiency across all treatments. Every treatment contains a negative mean R and the overall mean R is -1.370. This implies that, on average, bargainers let the expected value

Table 5. Summary Statistics of Experimental Results

		Efficiency							Distribution of Wealth (risk neutral measure, α_N)[c]				Timing (seconds remaining)			
		R[a]			Ψ[b]											
Treatment (Protocol Scheme)	r	Mean	Median	Std. Dev.	Mean	Median	Std. Dev.	Ave. Payoff, $\overline{Z(t)}$ (tokens)	Pure Self-Interest ($\alpha \geq 1$)	Pure Equity ($\alpha = 0$)	Constrained Self-Interest ($0 < \alpha < 1$)	Opponent Loyalty ($\alpha < 0$)	Mean	Std. Dev.	Range Min.	Range Max.
1	10	-1.525	-1.175	1.303	0.970	1.000	0.063	7.70	0	3	7	0	232.5	37.79	181.0	275.0
2	10	-0.528	0.000	1.188	0.985	1.000	0.024	8.60	0	4	4	2	256.5	34.74	177.0	286.0
3	10	-1.400	-0.750	1.482	0.985	1.000	0.034	7.70	0	5	5	0	229.2	44.05	145.0	267.0
4	10	-1.563	-1.688	1.198	0.975	1.000	0.035	7.65	0	1	8	1	229.9	39.90	180.0	282.0
5	10	-2.330	-3.000	1.494	0.930	0.950	0.054	7.20	1	1	6	2	216.3	46.39	157.0	293.0
6	10	-0.873	-0.500	1.589	0.985	1.000	0.034	8.25	1	4	5	0	246.9	49.24	128.0	298.0
Overall Values	60	-1.370	-0.963	1.443	0.972	1.000	0.045	7.85	2	18	35	5	235.2	42.57	128.0	298.0

[a] Reward Efficiency: $R = \{[P_A + P_B]Z(t) - P^O Z\} / (Z - P^O Z)$, where P_A and P_B , denote the final lottery allotment of players A and B, Z is the maximum reward (10 tokens), $Z(t)$ represents the remaining reward, and $P^O Z$ depicts the initial expected value of the outside option. Reward efficiency is maximized when $R = 1$.

[b] Traditional Measure of Efficiency: $\Psi = (P_A + P_B)$, where P_A and P_B denote the final lottery allotment for players A and B. Traditional efficiency is maximized when $\Psi = 100\% = 1$.

[c] Distribution of Wealth: $\alpha_N = \{P_C[Z(t)] - (EQ)[Z(t)]\} / \{P^O Z - (EQ)[Z(t)]\}$, where $EQ = [0.5 - (P_N / 2)]$, P_C represents the controller's observed probablity of winning, and P_N (>0) represents the probability that neither player will win.

of the remaining reward fall below the expected value of the outside option at t_{max} minutes remaining; i.e., $[P_A + P_B]Z(t) < P^O Z$. Figure 3 highlights the difference in mean reward efficiency across treatments. The figure shows that Treatment 5 contains the lowest mean R at -2.330 while Treatment 2 contains the highest mean R at -0.528. Though all treatments indicate negative reward efficiency, several protocols report higher reward efficiency results than the base group, Coasian bargaining with linear transaction costs, which yields $\overline{R}_1$ = -1.525.[62] [Note, $\overline{R}_i$ (i=1,2,3,...6) denotes the mean reward efficiency for Treatment i.] Protocol schemes (Treatments) 2,3, and 6 apparently maintain higher mean R values than Treatment 1.

As a side note, Table 5 shows that a high level of efficiency exists if one considers the traditional measure of efficiency, $\Psi = (P_A + P_B)$. The overall mean Ψ is 0.972. Protocols 2, 3, and 6 contain the highest mean Ψ equal to 0.985 and Protocol 5 contains the lowest mean Ψ at 0.930. These results suggest that bargainers may have ignored transaction costs; and instead, strove for an efficient distribution of lottery tickets which left little to no chance for a house victory. This explanation, however, implies that bargainers do not value the remaining reward. In other words, the bargainers maintain minimal concern for the opportunity cost of time. If this remains true, one might not expect a high frequency of early agreements.

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[62] Note, the directional sign of this result remains consistent with the result of Shogren and Herriges's (1994) linear costs experiment which reports a negative mean R of -0.331. Although -0.331 > -1.525 implies a magnitudinal inconsistency, this inconsistency may not remain significant when one considers how the two experiments differ. For instance, Shogren and Herriges's (1994) experiment, unlike the current one, uses each participant in more than one bargain (thus, the experiment is not immune to a potential learning effect), and obtains a larger sample size (the current experiment obtains 10 observations for the base group while Shogren and Herriges's experiment obtains 61 observations for their linear transaction costs treatment). Thus, the above magnitudinal difference may or may not render the two results completely inconsistent.

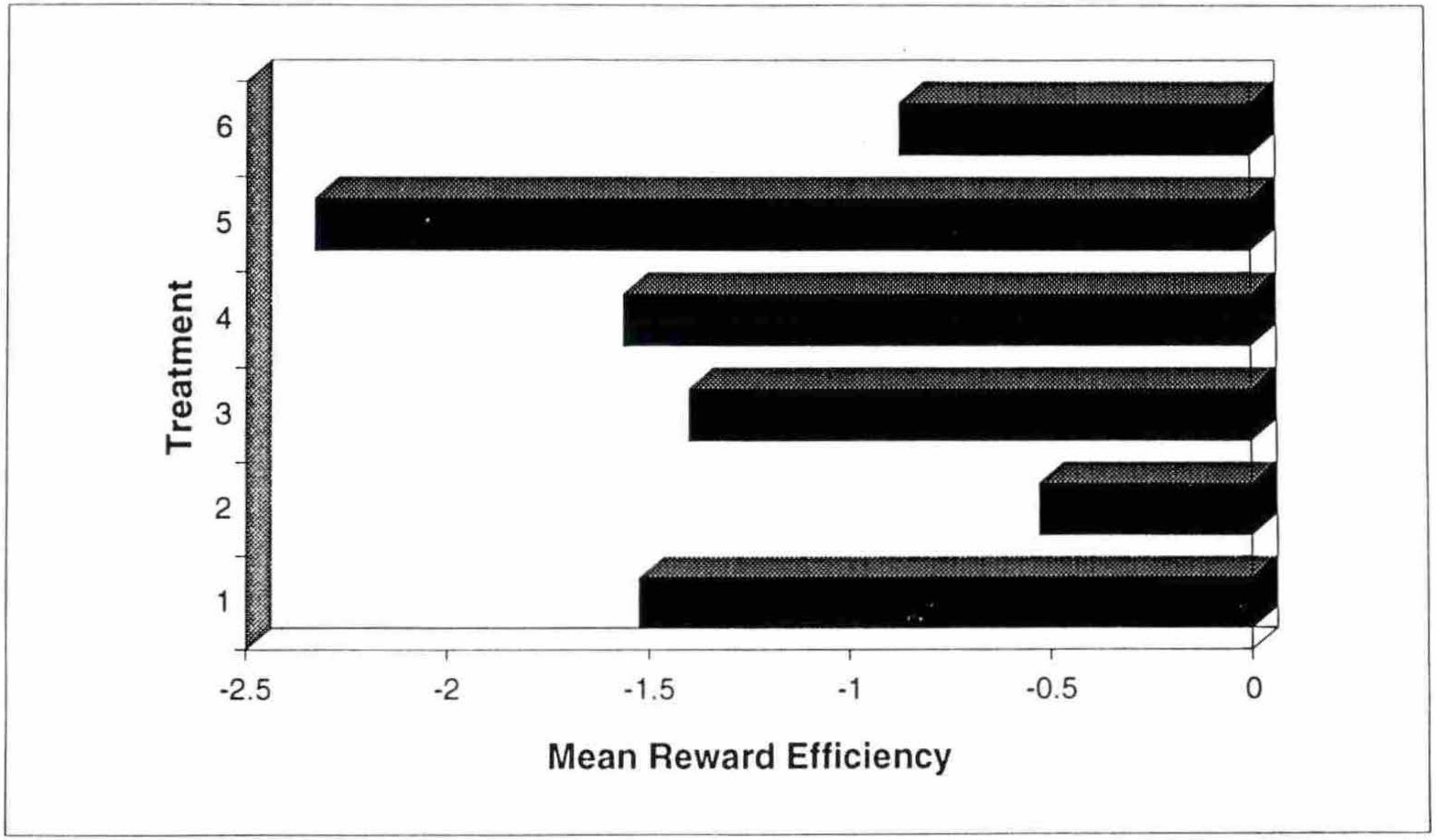

Figure 3. Mean Reward Efficiency by Treatment

To continue, several implications arise from Table 5 with regards to the distribution of wealth.[63] First, neither pure equity nor pure self-interest dominates the bargains. Table 5 shows that only 3.3% (2 out of 60) of the bargains exhibit pure self-interest and only 30% (18 out of 60) exhibit pure equity. Instead, constrained self-interest dominates the bargains. 58.3% (35 out of 60) of the bargainers balanced the two extremes of pure self-interest and equity.[64] Figure 4 illustrates the distribution of wealth by treatment. This figure reveals that constrained self-interest strongly dominates Treatments 1, 4, and 5 and captures exactly 50% of the bargains in Treatments 3 and 6. In general, these results remain consistent with Shogren and Kask (1992) and Shogren and Herriges (1994) where constrained self-interest dominates 52.3% and 80.8%, respectively, of the bargains. Both investigations, along with the current one, offer substantial empirical support for the existence of constrained self-interest behavior. This suggests that bargainers, more realistically, are driven by both monetary and nonmonetary incentives.

Another interesting observation from Table 5 is that 8.3% (5 out of 60) of the bargainers remained extremely loyal to their opponent. 5 out of 60 controllers accepted a probability of victory, P_C, less than the equal split outcome; i.e., $P_C < EQ$ results in α<0. Several reasons might have explained this behavior. One possible reason may have been that the controller knew the noncontroller. The experimenter, however, tried to avoid this possibility by asking all dyads if they were paired with a friend or someone they knew. If so, the dyad was separated and each bargainer was assigned a new partner. Another reason may have involved some type of "chivalry effect". A closer look reveals that 3 out of the 5 opponent loyalty bargains

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63 Note, Table 5 uses a risk neutral measure of rationality. The discussion will ellaborate on risk neutrality, shortly.

64 Additionally, note that no disagreement outcomes occurred.

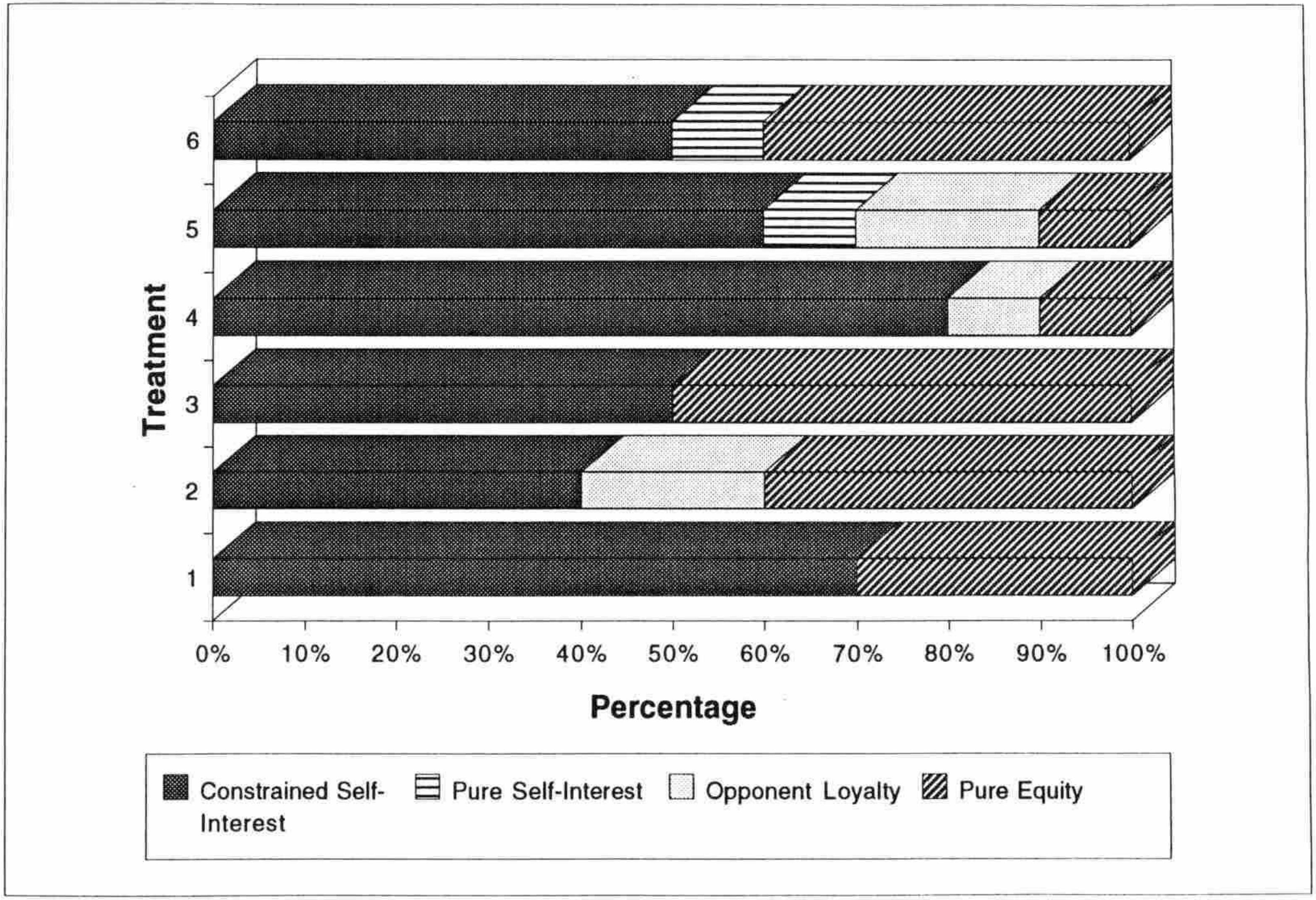

Figure 4. Distribution of Wealth by Treatment

involved a male controller and a female noncontroller.[65] The current analysis cannot infer either of the above reasons. Perhaps a more inquisitive post experimental questionnaire might have revealed the "true" reason behind the occurrence of opponent loyalty.

To further analyze the distribution of wealth, the discussion considers strict and weak rationality. Initially, consider strict rationality which is a time-dependent measure. Recall equation (3) represents a measure of strict rationality. Given the outside option and an equal split leave the controller with a 10% and 50%, respectively, chance to lose the lottery, the discussion considers three specifications for the bargainer's utility function in equation (3)--risk neutrality, "low" risk aversion where the controller is willing to pay a 10% risk premium, and "high" risk aversion where the controller is willing to pay a 50% risk premium. For risk neutrality $U[Z(t)] = Z(t)$ and one defines α as

$$\alpha_N = \frac{P_C[Z(t)] - (EQ)[Z(t)]}{P^O(Z) - (EQ)[Z(t)]}. \tag{6}$$

For the risk averse specifications, the discussion assumes that each bargainer's preferences toward risk follow a CARA (constant absolute risk aversion) utility function of the form $U[Z(t)] = 1 - e^{-A[Z(t)]}$, where A is the CARA coefficient. Following Babcock, Choi, and Feinerman [1993] (hereafter, BCF), one defines A as

$$A = \frac{\ln[(1+2\rho)/(1-2\rho)]}{Z} \tag{7}$$

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65 The other two cases involved 1) a female controller and male noncontroller and 2) a female-female dyad.

where Z is the maximum potential gain from bargaining, ρ denotes the probability premium, and $\ln[\cdot] \equiv \log_e[\cdot]$. By using Table 2 (to find values of probability premiums, ρ, associated with risk premiums, θ) in BCF (1993, p.22) and equation (7) above, one can calculate values for A. For instance, in the case of low risk aversion, Table 2 in BCF associates θ = 10% with ρ = 0.050167 and given Z = 10 tokens = \$20, one calculates A as follows

$$A = \frac{\ln\{[1+2(0.050167)]/[1-2(0.050167)]\}}{20}$$
$$= 0.01.$$

Thus, for low risk aversion one defines α as

$$\alpha_{10\%} = \frac{P_C\{1-e^{-0.01[Z(t)]}\}-(EQ)\{1-e^{-0.01[Z(t)]}\}}{P^O[1-e^{-0.01(Z)}]-(EQ)\{1-e^{-0.01[Z(t)]}\}}. \tag{8}$$

Similarly, for high risk aversion, θ = 50% is associated with ρ = 0.271844; and subsequently, equation (7) yields A = 0.06. Given this, one defines α for high risk aversion as

$$\alpha_{50\%} = \frac{P_C\{1-e^{-0.06[Z(t)]}\}-(EQ)\{1-e^{-0.06[Z(t)]}\}}{P^O[1-e^{-0.06(Z)}]-(EQ)\{1-e^{-0.06[Z(t)]}\}}. \tag{9}$$

Finally, weak rationality is a time-independent measure and is defined in equation (4).

Table 6 gives the mean, median, and standard deviation for the four α measures by treatment. For weak rationality, Table 6 shows that Treatment 1

Table 6. Constrained Self-Interest Weight, α

		Weak Rationality (time-independent, α_I)			High Risk Aversion (50% risk premium, $\alpha_{50\%}$)			Low Risk Aversion (10% risk premium, $\alpha_{10\%}$)			Risk Neutral (α_N)		
Treatment (Protocol Scheme)	r	Mean	Median	Standard Deviation	Mean	Median	Standard Deviation	Mean	Median	Standard Deviation	Mean	Median	Standard Deviation
1	10	0.5068	0.4651	0.4449	0.3536	0.3781	0.3094	0.3266	0.3419	0.2862	0.3211	0.3348	0.2815
2	10	-0.0074	0.0000	0.3945	-0.0204	0.0000	0.3311	-0.0213	0.0000	0.3176	-0.0215	0.0000	0.3148
3	10	0.1720	0.0294	0.2846	0.1166	0.0115	0.2125	0.1079	0.0098	0.1991	0.1061	0.0094	0.1963
4	10	0.4750	0.3934	0.4446	0.3385	0.2245	0.3628	0.3155	0.2005	0.3473	0.3109	0.1957	0.3442
5	10	0.3191	0.2132	0.4547	0.2292	0.1222	0.3508	0.2167	0.1105	0.3400	0.2143	0.1082	0.3380
6	10	0.4640	0.5074	0.4462	0.3686	0.2277	0.3988	0.3548	0.2012	0.3938	0.3520	0.1960	0.3928

Note: α_I is a time-independent measure while $\alpha_{50\%}$, $\alpha_{10\%}$, and α_N are time-dependent measures. Also, in accordance with theoretical expectations, $\overline{\alpha_I} > \overline{\alpha_{50\%}} > \overline{\alpha_{10\%}} > \overline{\alpha_N}$ within each treatment (where $\overline{\alpha} \equiv$ mean of α).

maintains the highest mean α ($\overline{\alpha}$) at 0.5068 and Treatment 2 reports the lowest at $\overline{\alpha}$= -0.0074. For the time-dependent (strict) measures of rationality, Treatment 6 contains the highest $\overline{\alpha}$ and Treatment 2, once again, holds the lowest $\overline{\alpha}$.[66] By and large, whether one considers the mean or median, one major observation emerges from Table 6. Except for Treatment 2, constrained self-interest (i.e., 0<α<1) clearly dominates the treatments across all measures of rationality. That is, constrained self-interest appears robust across both time-independent and time-dependent measures of rationality. This means that neither strict nor weak rationality (where $\alpha \geq 1$ for both) explains the bargainers' behavior. These results remain consistent with the observations from Table 5 where the risk neutral measure indicates that 58.3% of the bargainers weight the two extremes of pure self-interest and equity. Figure 5 further illustrates the dominance of constrained self-interest behavior. This figure shows the cumulative frequency distribution of the risk neutral measure of rationality (α_N) for each treatment. Similar to Figure 4, the cumulative frequency distributions indicate that at least 50% of the bargains, in each treatment [excluding Treatment 2's (T2) 40%], fall within the range of constrained self-interest (0<α<1).

So far, the discussion uses R and α to describe how the bargainers behaved in the experiment. Since the discussion maintains a special interest in the controller's behavior, the controller's observed probability of winning the lottery, Pr(controller wins) = P_C, naturally offers another way to analyze this experiment. Table 7 summarizes the descriptive statistics for P_C by treatment. This table reveals that only 11.7% (7 out of 60) of the controllers obtained a probability of victory at

66 Recall, when $P_C < EQ$, α<0 (see Section 5.2). The negative mean α values under Treatment (Protocol) 2 result from the fact that two controllers accepted an extremely low probability of victory where $P_C < EQ$. In fact, one controller accepted P_C=15% (see Table 7). Further comments regarding these P_C values will occur, shortly, when the discussion analyzes Table 7.

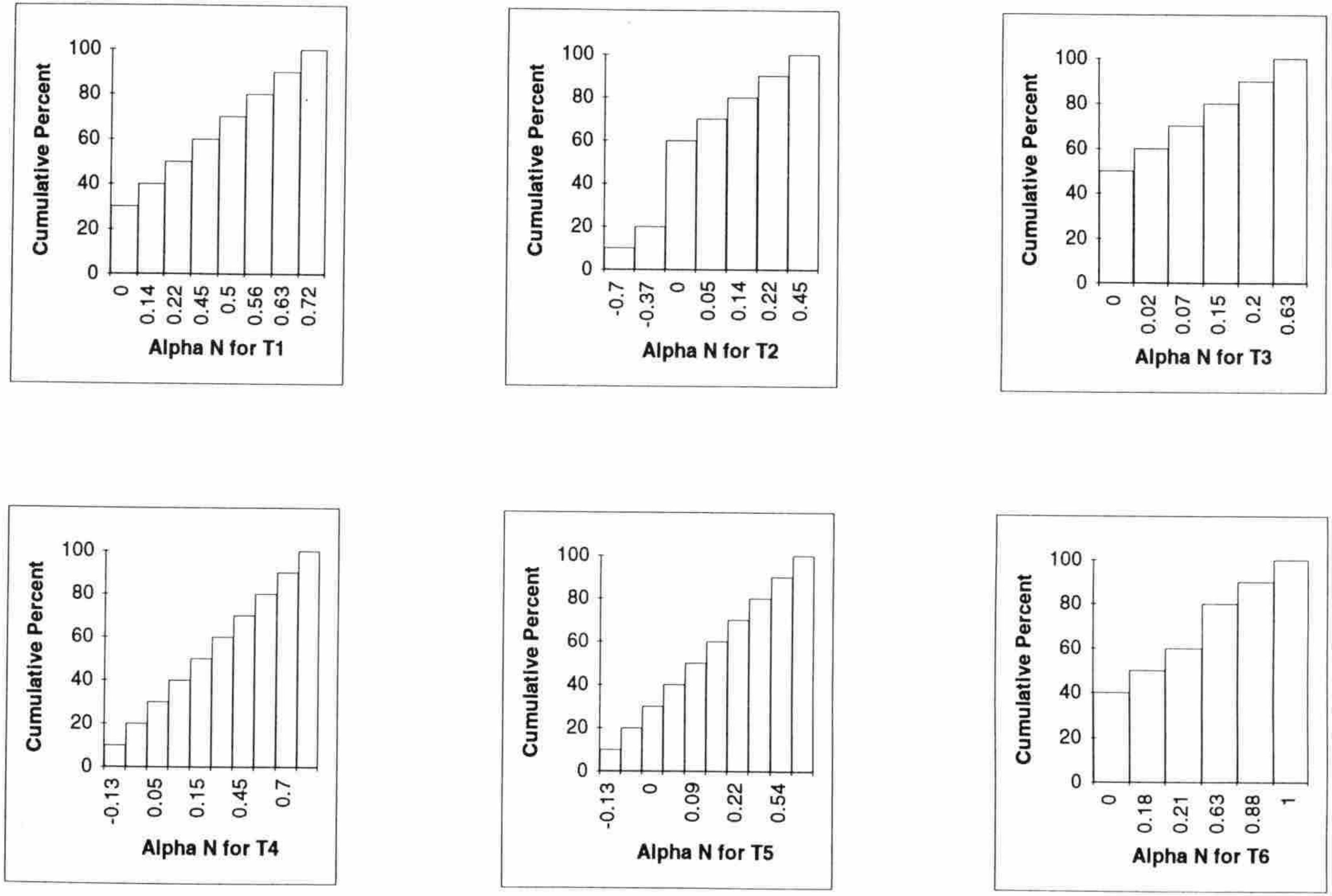

Figure 5. Cumulative Frequency Distribution of the Risk Neutral Constrained Self-Interest Weight (Alpha N) for Each Protocol (Treatment) Scheme

Table 7. Controller's Observed Probability of Winning, P_c

Treatment (Protocol Scheme)	r	# of controllers who obtained a Pr(win) such that $P_c \geq P^O$.	Mean	Median	Standard Deviation	Standard Error	Minimum Value	Maximum Value
1	10	2	0.692	0.660	0.185	0.059	0.500	0.950
2	10	0	0.490	0.500	0.160	0.050	0.150	0.700
3	10	0	0.562	0.500	0.117	0.037	0.500	0.850
4	10	2	0.685	0.650	0.183	0.058	0.400	0.950
5	10	2	0.605	0.550	0.202	0.064	0.300	0.900
6	10	1	0.685	0.700	0.178	0.056	0.500	0.900

least as great as the outside option (P^O). The base group (T1) holds the highest mean P_C at 0.692. Protocols 4 and 6 closely follow with a mean P_C value of 0.685. Protocol 2 contains the lowest mean value at 0.490. A mean $P_C < EQ = 0.500$ is not surprising for Protocol 2 given this protocol reports a negative mean α as seen in Table 6. An explanation for Protocol 2's highly irrational results rests in the fact that not only did two controllers accept a $P_C < 50\%$ (refer back to Table 5) but one of them accepted an extremely low P_C equal to 15% (see Table 7). From an observation of the latter controller's post experimental questionnaire, one finds that this controller (a male) did not know his opponent (a female), but for the loyalty question he marked "b" which indicates he felt loyal to the noncontroller. A response of "b" supports the controller's observed opponent loyalty behavior.[67] Overall, aside from Protocol 2, it appears that most controllers only take partial advantage of their bargaining position, such that $50\% < P_C < P^O = 90\%$.

Lastly, several implications emerge from Table 5 with respect to time. First, no deadline effect exists. The overall mean time remaining is 235.2 seconds. Note, since this analysis measures time in terms of seconds remaining, a mean time of 235.2 seconds implies that bargainers, on average, reached an agreement after 64.8 seconds (recall t_{max} = 5 minutes = 300 seconds, so that 300 - 235.2 = 64.8) of bargaining. Moreover, an inspection of the mean time remaining by treatment reveals no apparent deadline effect. Figure 6 charts the mean times by treatment. This figure implies that Treatment 2, on average, yields the quickest bargains at 256.5 seconds remaining and Treatment 5 yields the slowest bargains at 216.3

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[67] One might simply tag this observattion (P_C=15%) as an outlier and throw it out. However, given this controller's responses to the post experimental questionnaire, one cannot find any meaningful reasons for throwing it out. The controller acknowledges his loyalty to the other player and he acts on it.

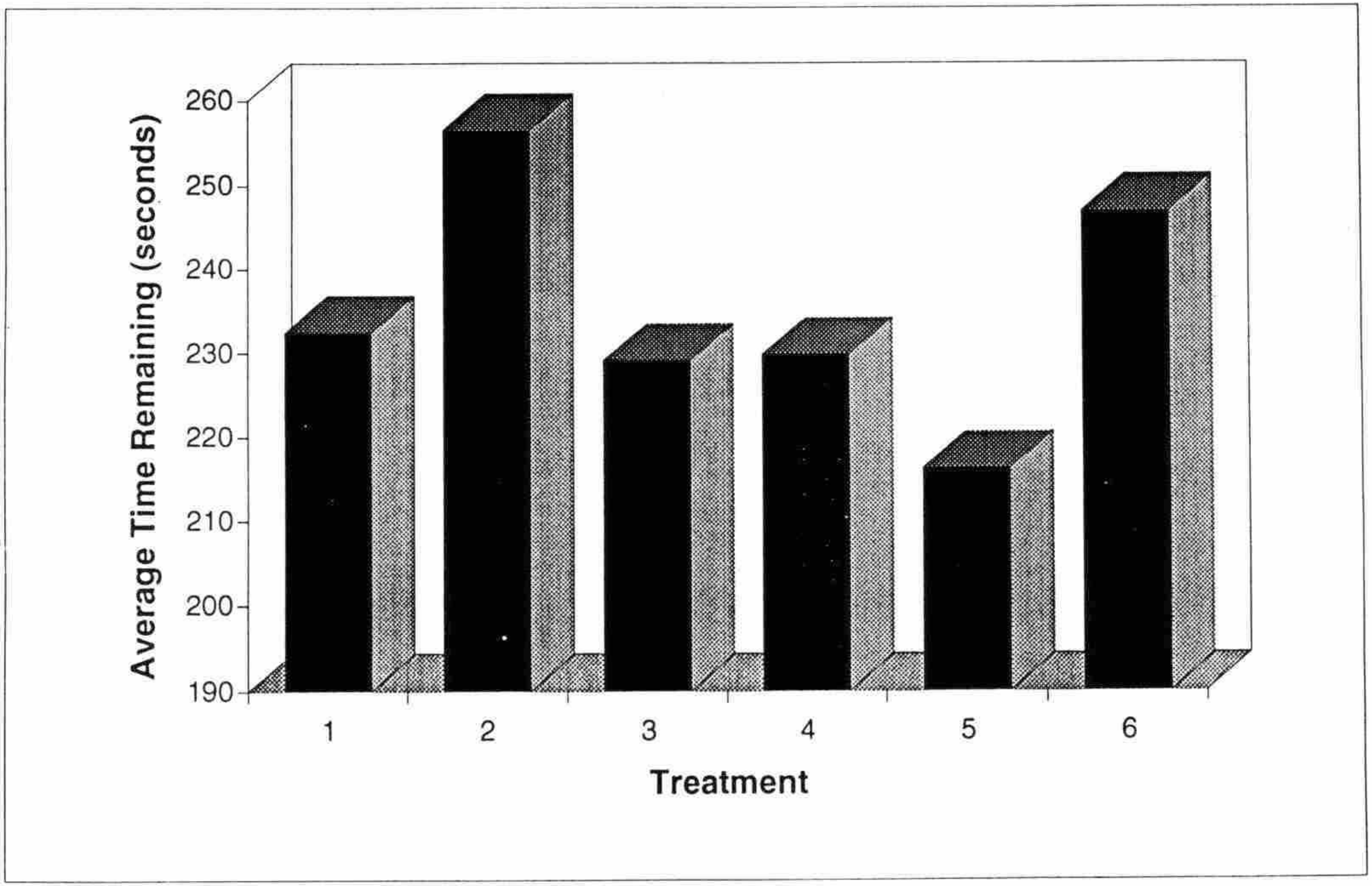

Figure 6. Average Time Remaining in Each Treatment

seconds remaining. Neither of these results lies close to the deadline of 0 seconds remaining. Generally speaking, the timing results of this experiment remain consistent with Shogren and Herriges's (1994) timing results which report an overall mean time of 255.2 (i.e., 300 - 44.8) seconds remaining. Thus, when bargainers face costly transactions, RMS's (1988) deadline effect does not hold.

Second, this experiment's timing results imply that delay, in general, is not a preferred strategy among the bargainers. This implication remains more visible through an inspection of Figure 7 which illustrates the relative frequency distribution of times remaining for each treatment. Note, Figure 7 reveals that early bargains dominate. The majority of agreement times fall within the range of 211 to 300 seconds remaining. This means that most bargainers took only 0 to 89 seconds to reach a decision. Thus, bargainers did not, in general, use bargaining delays to signal information, such as reservation prices and preferences.

Finally, the above timing results suggest that most bargainers maintain some appreciation for the opportunity cost of time. The relatively high frequency of early decisions seems to indicate that bargainers associated an earlier bargain with a higher $Z(t)$ value. Thus, although the bargainers obtained, approximately, traditionally efficient bargains (recall the overall mean $\Psi = 0.972$) and not reward efficient bargains (recall the overall mean $R = -1.370$), the bargainers did not necessarily ignore the transaction costs and simply bargain over the chances to win $Z(t)$.

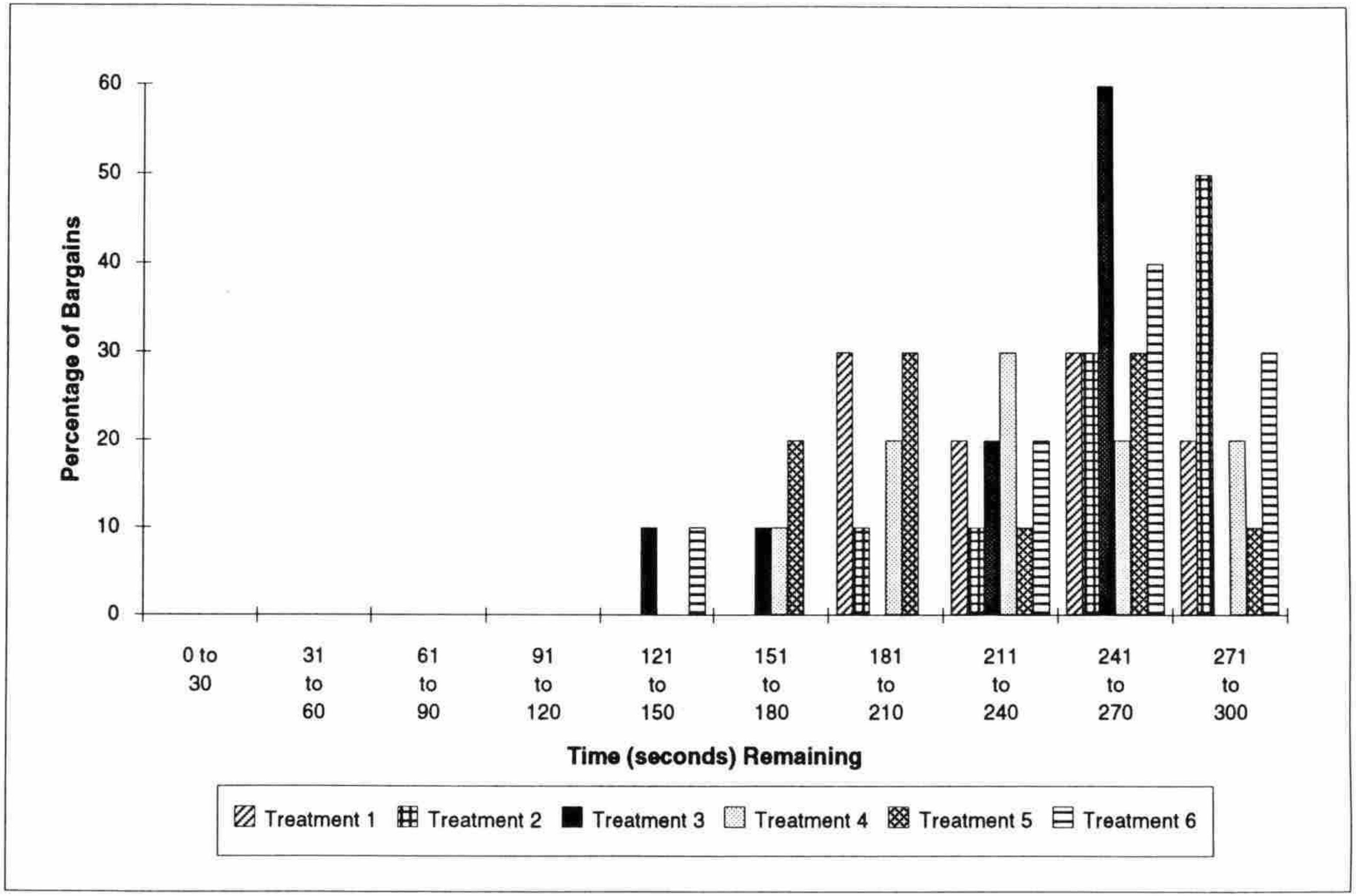

Figure 7. The Relative Frequency Distribution of the Time Remaining in Each Treatment

7.2 Analysis of Variance Results

Section 7.1 summarizes the descriptive statistics which emerge from this experiment. Though an observation of these statistics sparks several economic implications regarding R, α, $P_{C,}$ and $TIME$ one cannot make statistical inferences from these observations alone. For example, although Section 7.1 implies that the mean time remaining may vary by treatment (recall Figure 6), the observed differences may not be statistically significant. To statistically infer that differences exist among the six treatments, one must use an appropriate test procedure. The analysis of variance (ANOVA) remains an appropriate procedure for the task at hand, which is to determine whether or not the observed differences in treatment means, as seen in Section 7.1, remain statistically significant. With this, the discussion proceeds as follows. First, the discussion defines the basic ANOVA for this experiment. Next, the discussion defines two additional techniques, the *least significant difference* method and *treatment contrasts*, which compliment the basic ANOVA. Lastly, the discussion presents the experimental results from all of these techniques.

Given this experiment uses a completely randomized design, one bases the ANOVA on the single factor model depicted in equation (4) of Section 5.3. But since the discussion analyzes the experimental results in terms of four response variables, one redefines equation (4) as

$$y_{(ijlk)} = \mu_k + \tau_{(ilk)} + \varepsilon_{(ijlk)} \qquad \begin{cases} i = 1,2,\ldots,6 \\ j = 1,2,\ldots,10 \\ k = 1,2,3,4 \end{cases}$$

where $y_{(ij|k)}$ represents the jth observation for treatment i given the response variable k (= 1 for R, = 2 for α, = 3 for P_C, and = 4 for $TIME$), μ_k, $\tau_{(i|k)}$, and $\varepsilon_{(ij|k)}$ are as defined in Section 5.3 [for equation (4)] given response variable k. This means that the discussion, separately, applies the ANOVA to the following four models:

$$y_{(ij|1)} \equiv R_{ij} = \mu + \tau_i + \varepsilon_{ij} \tag{10}$$

$$y_{(ij|2)} \equiv \alpha_{ij} = \mu + \tau_i + \varepsilon_{ij} \tag{11}$$

$$y_{(ij|3)} \equiv P_{C(ij)} = \mu + \tau_i + \varepsilon_{ij} \tag{12}$$

$$y_{(ij|4)} \equiv TIME_{ij} = \mu + \tau_i + \varepsilon_{ij} \tag{13}$$

for i=1,2,3,...,6 and j=1,2,3,...,10. Table 8 illustrates the structure for the data sets which the above models describe. Note, $y_{i.}$ equals the total sum of the observations in Treatment i ($y_{i.} = \sum_{j=1}^{10} y_{ij}$), $y_{..} = \sum_{i=1}^{6} y_{i.}$, $\bar{y}_{i.} = \left(\sum_{j=1}^{10} y_{ij}\right)/10$, and $\bar{y}_{..} = y_{..}/60$ [where $N = qr = (6)(10) = 60$]. Table 9 presents the basic structure of the ANOVA for models of the type $y_{ij} = \mu + \tau_i + \varepsilon_{ij}$.

Following the discussion in Section 5.3, for each of the models depicted in equations (10) through (13) the ANOVA tests

$$H_O\colon\ \mu_1 = \mu_2 = \mu_3 = \mu_4 = \mu_5 = \mu_6$$

$$H_A\colon\ \mu_i \neq \mu_j \quad \text{for at least one pair } (i,\ j).$$

If the overall F-statistic [= $MS(Tr)/MSE$] is sufficiently large, then one can reject H_O and support the claim that not all treatment means (or effects) are the same. A rejection of H_O, however, infers nothing about which μ_i's differ. To find which treatment means differ, one can apply the least significant difference (LSD) method.

Table 8. Basic Structure of Data Sets

Treatment	Replications (Observations)					Totals	Averages
1	$y_{1,1}$	$y_{1,2}$	$y_{1,3}$	$\cdots$	$y_{1,10}$	$y_{1\cdot}$	$\bar{y}_{1\cdot}$
2	$y_{2,1}$	$y_{2,2}$	$y_{2,3}$	$\cdots$	$y_{2,10}$	$y_{2\cdot}$	$\bar{y}_{2\cdot}$
3	$y_{3,1}$	$y_{3,2}$	$y_{3,3}$	$\cdots$	$y_{3,10}$	$y_{3\cdot}$	$\bar{y}_{3\cdot}$
4	$y_{4,1}$	$y_{4,2}$	$y_{4,3}$	$\cdots$	$y_{4,10}$	$y_{4\cdot}$	$\bar{y}_{4\cdot}$
5	$y_{5,1}$	$y_{5,2}$	$y_{5,3}$	$\cdots$	$y_{5,10}$	$y_{5\cdot}$	$\bar{y}_{5\cdot}$
6	$y_{6,1}$	$y_{6,2}$	$y_{6,3}$	$\cdots$	$y_{6,10}$	$y_{6\cdot}$	$\bar{y}_{6\cdot}$
						$y_{\cdot\cdot}$	$\bar{y}_{\cdot\cdot}$

Table 9. Structure of ANOVA for Models of the Type $y_{ij} = \mu + \tau_i + \varepsilon_{ij}$

Source	Degrees of Freedom	Sum of Squares	Mean Square	F-Statistic
Treatments	$q-1$	$SS(Tr) = \sum_{i=1}^{q} \frac{y_{i.}^2}{r} - \frac{y_{..}^2}{N}$	$MS(Tr) = \frac{SS(Tr)}{(q-1)}$	$F = \frac{MS(Tr)}{MSE}$
Error	$N-q$	$SSE = SST - SS(Tr)$	$MSE = \frac{SSE}{(N-q)}$	
Corrected Total	$N-1$	$SST = \sum_{i=1}^{q} \sum_{j=1}^{r} y_{ij}^2 - \frac{y_{..}^2}{N}$		

That is, given a rejection of the above null hypothesis for equality of the μ_i's, LSD tests H_O: $\mu_i = \mu_j$ for all $i \neq j$. To explain, one develops the LSD test criterion from the following test statistic

$$t_o = \frac{\bar{y}_{i\cdot} - \bar{y}_{j\cdot}}{\sqrt{MSE\left(\frac{1}{r_i} + \frac{1}{r_j}\right)}}$$

where $\bar{y}_{i\cdot} = \hat{\mu}_i = \hat{\mu} + \hat{\tau}_i$ is a point estimator for the ith treatment mean (μ_i), $\bar{y}_{j\cdot} = \hat{\mu}_j = \hat{\mu} + \hat{\tau}_j$ is a point estimator for the jth treatment mean (μ_j), MSE is the mean square error from the ANOVA, and r_i and r_j are the number of observations in Treatments i and j, respectively. [Notice, since the current experiment replicates all treatments the same amount ($r = 10$) of times, $r_i = r_j = r$ and the denominator in t_o reduces to $\sqrt{2(MSE)/r}$.] If H_O: $\mu_i = \mu_j$ is true, then t_o follows a t-distribution with N-q degrees of freedom. Within this framework, one defines the LSD test criteria for H_O: $\mu_i = \mu_j$ $\forall$ $i \neq j$ as follows

$$\text{reject } H_O \text{ if } \left|\bar{y}_{i\cdot} - \bar{y}_{j\cdot}\right| > t_{\alpha/2, N-q}\sqrt{\frac{2(MSE)}{r}} = \text{LSD}$$

where α, here, denotes the level of significance, not the constrained self-interest weight.

Though the LSD method allows one to determine (given a rejection of H_O: $\mu_1 = \mu_2 = \cdots = \mu_q$ from an ANOVA F-test) which treatment means differ, it is limited to pairwise comparisons. Another statistical technique, called treatment contrasts, enables one to compare groups of treatment means. This latter technique remains especially useful for this experiment because it allows one to compare the mean (or effects) of : (1) cheap talk versus no cheap talk, (2) free form versus structured bargaining, and (3) controller moves first versus noncontroller moves first.[68]

To help facilitate an explanation of treatment contrasts consider comparison (2), above. Here one wishes to test the equality of the average of Treatments 1 and 2 versus the average of Treatments 3,4,5, and 6. In terms of formal hypotheses, this comparison tests

$$H_O:\ \tfrac{1}{2}(\mu_1+\mu_2)=\tfrac{1}{4}(\mu_3+\mu_4+\mu_5+\mu_6)$$
$$H_A:\ \tfrac{1}{2}(\mu_1+\mu_2)\neq\tfrac{1}{4}(\mu_3+\mu_4+\mu_5+\mu_6)$$

or multiplying through by 4 one writes

$$H_O:\ 2(\mu_1+\mu_2)=(\mu_3+\mu_4+\mu_5+\mu_6)$$
$$H_A:\ 2(\mu_1+\mu_2)\neq(\mu_3+\mu_4+\mu_5+\mu_6).$$

One can state the null hypothesis in terms of a linear combination, L_2, of treatment sample means, where

$$L_2=2\bar{y}_{1\cdot}+2\bar{y}_{2\cdot}-\bar{y}_{3\cdot}-\bar{y}_{4\cdot}-\bar{y}_{5\cdot}-\bar{y}_{6\cdot}.$$

If one denotes the coefficients on the $\bar{y}_{i\cdot}$ terms as c_i, L_2 is a treatment contrast if $\sum_{i=1}^{6}c_i=0$. Since $\sum_{i=1}^{6}c_i=2+2-1-1-1-1=0$, L_2 is a treatment contrast. In general, if

68 Recall that Table 1 illustrates these comparisons.

$$\sum_{i=1}^{q} c_i = 0$$

then

$$L = \sum_{i=1}^{q} c_i \overline{y}_{i \cdot}$$

is a treatment contrast.[69] Given this definition, it follows that comparison (1), which corresponds to H_O: $(\mu_2 + \mu_5 + \mu_6) = (\mu_1 + \mu_3 + \mu_4)$, and comparison (3), which corresponds to H_O: $(\mu_3 + \mu_5) = (\mu_4 + \mu_6)$, are treatment contrasts, too.

One can test the hypotheses based on the treatment contrasts through the ANOVA. The procedure is as follows. Assign 1 degree of freedom to each treatment contrast and calculate a sum of squares for each treatment contrast, SSL_n, as follows

$$SSL_n = \frac{L_n^2}{\sum_{i=1}^{6} \frac{c_i^2}{r_i}} \tag{14}$$

where n=1,..., h denotes h different treatment contrast sum of squares. Next, use the SSL_n's to partition the treatment sum of squares, $SS(Tr)$, in the ANOVA. But note, since each SSL_n is associated with 1 degree of freedom, there can be no more than $q-1$ (which equals the degrees of freedom for treatments in the ANOVA)

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69 As a technical note, if one defines L in terms of treatment totals such that $L^* = \sum_{i=1}^{q} c_i y_{i \cdot}$, then L^* is a treatment constrast if $\sum_{i=1}^{q} c_i r_i = 0$; or if the experiment equally replicates all the treatments (i.e., $r_1 = \cdots = r_q = r$) then this condition reduces to $\sum_{i=1}^{q} c_i = 0$.

treatment contrasts; i.e., $h \le (q-1)$. Additionally, in order to establish and conduct independent tests on each of the treatment contrasts, the contrasts must be mutually orthogonal. In general, if two treatment contrasts, L_a and L_b, contain the set of coefficients $\{c_i\}$ and $\{d_i\}$, respectively, then L_a and L_b are orthogonal if

$$\sum_{i=1}^{q} \frac{c_i d_i}{r_i} = 0$$

or if the treatments are equally replicated then the orthogonality condition reduces to

$$\sum_{i=1}^{q} c_i d_i = 0.$$[70]

For this experiment the treatment contrasts are:

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70 Alternatively, if one defines L in terms of treatment totals such that $L^* = \sum_{i=1}^{q} c_i y_{i\cdot}$, then L_a^* and L_b^* are orthogonal if $\sum_{i=1}^{q} c_i d_i r_i = 0$, or given equally replicated treatments the condition reduces to $\sum_{i=1}^{q} c_i d_i = 0$. Whether one defines L in terms of $\bar{y}_{i\cdot}$ or $y_{i\cdot}$ makes no difference because both yield equivalent results. However, one point of caution remains here: if the treatments are not equally replicated (e.g., $r_i \ne r_j$ for Treatments i and j) then the coefficients in L and L^* will be different, so that $L = \sum_{i=1}^{q} c_i \bar{y}_{i\cdot}$ and $L^* = \sum_{i=1}^{q} f_i y_{i\cdot}$. This difference remains important for estimation purposes because some statistical packages, like SAS, calculate SSL in terms of $\bar{y}_{i\cdot}$ [i.e., SAS uses equation (14), above]. Thus, plugging in coefficients related to L^* will yield incorrect estimates in SAS. To calculate the sum of squares for L^*, one must use the equation:

$$SSL^* = \frac{(L^*)^2}{\sum_{i=1}^{q} f_i^2 r_i}.$$

$$L_1 = -\bar{y}_{1.} + \bar{y}_{2.} - \bar{y}_{3.} - \bar{y}_{4.} + \bar{y}_{5.} + \bar{y}_{6.}$$
$$L_2 = 2\bar{y}_{1.} + 2\bar{y}_{2.} - \bar{y}_{3.} - \bar{y}_{4.} - \bar{y}_{5.} - \bar{y}_{6.}$$
$$L_3 = \bar{y}_{3.} - \bar{y}_{4.} + \bar{y}_{5.} - \bar{y}_{6.} \ .$$

If one defines the set of coefficients for each treatment contrast as $\{c_i\} \equiv \{-1,1,-1,-1,1,1\}$ for L_1, $\{d_i\} \equiv \{2,2,-1,-1,-1,-1\}$ for L_2, and $\{f_i\} \equiv \{0,0,1,-1,1,-1\}$ for L_3, then one sees that

$$\sum_{i=1}^{6} c_i d_i = 0 \Rightarrow L_1 \text{ and } L_2 \text{ are orthogonal,}$$
$$\sum_{i=1}^{6} c_i f_i = 0 \Rightarrow L_1 \text{ and } L_3 \text{ are orthogonal, and}$$
$$\sum_{i=1}^{6} d_i f_i = 0 \Rightarrow L_2 \text{ and } L_3 \text{ are orthogonal.}$$

Thus, L_1, L_2, and L_3 are mutually orthogonal contrasts. Table 10 illustrates the structure of the ANOVA given these contrasts. If F_n is sufficiently large, then one rejects H_O for L_n, where $n = 1,2,3$. And note, if the number of contrasts $h<(q-1)$, one completes the partition of the $SS(Tr)$ with a category called "Others", where the sum of squares for Others (SSO) equals the remainder of $SS(Tr) - \sum_{n=1}^{h} SSL_n$. Although a F-test on Others may yield valuable information in some instances, this experiment finds no meaningful interpretation for Others; and hence, excludes this test. In sum, the ANOVA provides a F-test for hypotheses involving group comparisons (contrasts) of treatments.

Based on the preceding discussion, the analysis, now, turns to a presentation of the results. The presentation begins with the results for the response variables R,

Table 10. Structure of ANOVA Given Orthogonal Treatment Contrasts

Source	Degrees of Freedom	Sum of Squares	Mean Square	F-Statistic
Treatments	$q-1$	$SS(Tr)$	$MS(Tr)$	$F = MS(Tr)/MSE$
<u>OrthogonalContrasts</u>:				
L_1	1	SSL_1	$MSL_1 = SSL_1/1$	$F_1 = MSL_1/MSE$
L_2	1	SSL_2	$MSL_2 = SSL_2/1$	$F_2 = MSL_2/MSE$
L_3	1	SSL_3	$MSL_3 = SSL_3/1$	$F_3 = MSL_3/MSE$
Others	$(q-1)-3$	SSO	$MSO = SSO/(q-1)-3$	
Error	$N-q$	SSE	MSE	
Corrected Total	$N-1$	SST		

P_C, and $TIME$. Afterwards, a presentation for the constrained self-interest weight, α, follows.

Table 11 contains the ANOVA results for R, P_C, and $TIME$. The overall F-value ($= MS(Tr)/MSE$) for R ($F = 2.03$) and P_C ($F = 2.28$) remains moderately significant given that $0.05 < p$-value <0.10 in both cases. For $TIME$ the overall F-value equals 1.14 and remains statistically insignificant since the p-value = 0.3516 >> 0.10. This means that one can reject the null hypothesis of equality of treatment means (i.e., reject H_O: $\mu_1 = \mu_2 = \cdots = \mu_6$) for R and P_C, but not for $TIME$. These test results suggest that the observed differences in average times remaining across treatments, as seen in Figure 6, remain statistically insignificant, while the observed differences in mean R and mean P_C across treatments, as seen in Tables 5 and 7, respectively, remain statistically significant for at least one pair (i, j) of treatments.

As mentioned earlier, the LSD method allows one to determine which treatment (protocol) pairs differ, given a rejection of H_O for the equality of treatment means. Figure 8 summarizes the LSD results (at a 5% level of significance) for R and P_C. (Note, since one cannot reject H_O: $\mu_1 = \mu_2 = \cdots = \mu_6$ for $TIME$, one cannot remain confident about LSD results for $TIME$; thus, this analysis excludes the LSD approach for $TIME$.) The figure presents the treatment means in descending order. Treatment pairs not connected by a line remain statistically different. Given this, Figure 8 illustrates that μ_2 and μ_5 differ and μ_5 and μ_6 differ in terms of R. For P_C, μ_1 and μ_2 differ, μ_2 and μ_4 differ, and μ_2 and μ_6 differ. Since $\overline{R}_2 > \overline{R}_5$ and $\overline{R}_6 > \overline{R}_5$, the LSD results suggest that, on average, free form/cheap talk bargaining (Protocol 2) and structured/cheap talk bargaining with the noncontroller as first mover (Protocol 6) both yield more efficient bargains than structured/cheap talk bargaining with the controller as first mover (Protocol 5). These results indicate that pairwise

Table 11. Analysis of Variance for Three Response Variables, R, P_C, $TIME$

		Reward Efficiency (R)				Pr(controller wins) (P_C)				Time Remaining ($TIME$)			
Source of Variation	Degrees of Freedom	Sum of Squares	Mean Square	F-value	Pr > F (p-value)	Sum of Squares	Mean Square	F-value	Pr > F (p-value)	Sum of Squares	Mean Square	F-value	Pr > F (p-value)
Treatments	5	19.4089	3.8818	2.03	0.0896	0.3412	0.0682	2.28	0.0590	10191.68	2038.34	1.14	0.3516
Orthogonal Contrasts:													
(T2, T5, T6) vs. (T1, T3, T4)[a]	1	0.9563	0.9563	0.50	0.4830	0.0421	0.0421	1.41	0.2402	1316.02	1316.02	0.73	0.3951
(T1, T2) vs. (T3, T4, T5, T6)[b]	1	3.5363	3.5363	1.85	0.1800	0.0249	0.0249	0.83	0.3650	2585.41	2585.41	1.44	0.2348
(T3, T5) vs. (T4, T6)[c]	1	4.1926	4.1926	2.19	0.1449	0.1030	0.1030	3.45	0.0688	2449.23	2449.23	1.37	0.2474
Others	2	10.7237	5.3619	2.80		0.1712	0.0856	2.86		3841.02	1920.51	1.07	
Error	54	103.4799	1.9163			1.6135	0.0299			96704.50	1790.82		
Corrected Total	59	122.8889				1.9547				106896.18			

[a] (T2, T5, T6) vs. (T1, T3, T4) $\equiv L_1 \Rightarrow$ cheap talk versus no cheap talk.

[b] (T1, T2) vs. (T3, T4, T5, T6) $\equiv L_2 \Rightarrow$ free form bargaining versus structured bargaining.

[c] (T3, T5) vs. (T4, T6) $\equiv L_3 \Rightarrow$ controller moves first versus noncontroller moves first.

Treatment (Protocol)	2	6	3	1	4	5
Mean, $\overline{R}_i$	-0.528	-0.873	-1.400	-1.525	-1.563	-2.330

Treatment (Protocol)	1	4	6	5	3	2
Mean, $\overline{P}_{C(i)}$	0.692	0.685	0.685	0.605	0.562	0.490

Figure 8. LSD Results for R and P_C

efficiency differences only exist *among* the cheap talk protocols, not *between* cheap talk and non-cheap talk protocols, and not *among* non-cheap talk protocols. Based on this information, one may reject the Efficiency Hypothesis (as stated in Section 5.2), since no pairwise comparisons between the base group (Protocol 1) and the other protocols remains statistically significant (i.e., no protocol scheme exhibits a significantly higher efficiency level than Protocol 1). Finally, since $\overline{P}_{C(2)} < \overline{P}_{C(6)} = \overline{P}_{C(4)} < \overline{P}_{C(1)}$, the LSD results for P_C suggest that controllers, on average, secure higher probabilities of victory under Protocols 1, 4, and 6 than under Protocol 2. Given a higher P_C depicts more, economically, rational behavior than a lower P_C, the LSD results for P , also, imply that controllers under protocols 1,4, and 6 behave more rationally than the controllers in Protocol 2.

To further analyze the treatment effects, one can turn back to the ANOVA in Table 11 and conduct F-tests on the three treatment contrasts which were derived earlier. For the response variable R none of the contrasts remain statistically significant at either the 5% or 10% level of significance. In terms of L_1, this means that cheap talk bargainers as a group do not, on average, bargain any more or less efficiently than non-cheap talk bargainers. For P_C contrasts L_1 and L_2 remain insignificant, as their large p-values indicate, but contrast L_3 remains significant at the 10% level (since the p-value = 0.0688 < 0.10). This suggests that P_C results, on average, differ depending on who moves first in structured bargaining. Lastly, since the overall F-test, for $TIME$, fails to reject H_O: $\mu_1 = \mu_2 = \cdots = \mu_6$, a F-test on the treatment contrasts for $TIME$ remains meaningless. Nevertheless, Table 11 reports the F-values for each contrast under $TIME$ and as one might suspect they remain highly insignificant, as their large p-values indicate. This completes the ANOVA for

R, P_C, and $TIME$. Now the discussion turns to an analysis of the constrained self-interest weight, α.

Recall that this analysis considers three time-dependent measures of α ($\alpha_{50\%}$, $\alpha_{10\%}$, and α_N) and one time-independent measure (α_I). Table 12 presents the ANOVA results for each measure of α. The overall F-value for α_I ($F = 2.41$) remains significant at the 5% level (since the p-value = 0.0479 < 0.05) and the overall F-values for $\alpha_{50\%}$ ($F = 2.20$), $\alpha_{10\%}$ ($F = 2.14$), and α_N ($F = 2.13$) remain significant at the 10% level (since the 0.05 < p-value < 0.10 in all three cases). This allows one to reasonably reject H_O: $\mu_1 = \mu_2 = \cdots = \mu_6$ across all measures of α. Given the overall F-test for each α measure suggests that differences exist among the treatments for at least one pair (i, j), the analysis, once again, uses the LSD method to determine pairwise differences among the treatments. Figure 9 summarizes the LSD results (at a 5% level of significance) for each α measure. For α_I Figure 9 illustrates that μ_1 and μ_2 differ, μ_2 and μ_4 differ, and μ_2 and μ_6 differ. Similarly, for $\alpha_{50\%}$, $\alpha_{10\%}$, and α_N Figure 9 shows that μ_1 and μ_2 differ, μ_2 and μ_4 differ, and μ_2 and μ_6 differ. Since $\overline{\alpha}_{I(1)} > \overline{\alpha}_{I(4)} > \overline{\alpha}_{I(6)} > \overline{\alpha}_{I(2)}$, the LSD results suggest that Protocols 1, 4, and 6 yield more rational behavior than Protocol 2 when one considers time-independence. The three time-dependent measures yield similar conclusions. Since $\overline{\alpha}_6 > \overline{\alpha}_1 > \overline{\alpha}_4 > \overline{\alpha}_2$ across all time-dependent measures, the LSD results suggest that Protocols 1, 4, and 6 yield more rational behavior than Protocol 2.

Lastly, turn back to Table 12 and note the individual F-tests for each treatment contrast. Across all measures of α contrasts L_1 and L_2 remain highly insignificant (since the p-value >> 0.10 for all α measures), but contrast L_3 remains significant at the 10% level (since the p-value < 0.10 for each α measure). This

Table 12. Analysis of Variance for the Constrained Self-Interest Weights

		Weak Rationality (time independent, α_I)				**High Risk Aversion** (50% risk premium, $\alpha_{50\%}$)				**Low Risk Aversion** (10% risk premium, $\alpha_{10\%}$)				**Risk Neutral** (α_N)			
Source of Variation	D.F.	S.S.	M.S.	F-value	Pr > F	S.S.	M.S.	F-value	Pr > F	S.S.	M.S.	F-value	Pr > F	S.S.	M.S.	F-value	Pr > F
Treatments	5	2.087	0.417	2.41	0.0479	1.218	0.244	2.20	0.0677	1.094	0.219	2.14	0.0747	1.070	0.214	2.13	0.0763
<u>Orthogonal Contrasts</u>:																	
(T2, T5, T6) vs. (T1, T2, T4)[a]	1	0.238	0.238	1.38	0.2457	0.089	0.089	0.81	0.3735	0.067	0.067	0.65	0.4234	0.062	0.062	0.62	0.4349
(T1, T2) vs. (T3, T4, T5, T6)[b]	1	0.155	0.155	0.90	0.3483	0.125	0.125	1.12	0.2936	0.123	0.123	1.20	0.2774	0.123	0.123	1.22	0.2741
(T3, T5) vs. (T4, T6)[c]	1	0.501	0.501	2.90	0.0944	0.326	0.326	2.95	0.0918	0.299	0.299	2.92	0.0932	0.293	0.293	2.91	0.0936
Others	2	1.193	0.597	3.45		0.678	0.339	3.05		0.605	0.303	2.97		0.592	0.296	2.93	
Error	54	9.342	0.173			5.979	0.111			5.524	0.102			5.435	0.101		
Corrected Total	59	11.429				7.197				6.618				6.505			

[a] (T2, T5, T6) vs. (T1, T3, T4) $\equiv L_1 \Rightarrow$ cheap talk versus no cheap talk.

[b] (T1, T2) vs. (T3, T4, T5, T6) $\equiv L_2 \Rightarrow$ free form bargaining versus structured bargaining.

[c] (T3, T5) vs. (T4, T6) $\equiv L_3 \Rightarrow$ controller moves first versus noncontroller moves first.

Treatment (Protocol)	1	4	6	5	3	2
Mean, $\overline{\alpha}_{I(i)}$	0.5068	0.4750	0.4640	0.3191	0.1720	-0.0074

Treatment (Protocol)	6	1	4	5	3	2
Mean, $\overline{\alpha}_{50\%(i)}$	0.3686	0.3536	0.3385	0.2292	0.1166	-0.0204

Treatment (Protocol)	6	1	4	5	3	2
Mean, $\overline{\alpha}_{10\%(i)}$	0.3548	0.3266	0.3155	0.2167	0.1079	-0.0213

Treatment (Protocol)	6	1	4	5	3	2
Mean, $\overline{\alpha}_{N(i)}$	0.3520	0.3211	0.3109	0.2143	0.1061	-0.0215

Figure 9. LSD Results for α_I, $\alpha_{50\%}$, $\alpha_{10\%}$, and α_N

suggests that cheap talk bargainers as a group do not, on average, behave any more or less rational than non-cheap talk bargainers, and such an inference remains robust across both time-dependent and time-independent measures of α. The same suggestion, also, applies for a comparison of free form bargainers versus structured bargainers. However, when one considers who moves first in structured bargaining, the preceding suggestion does not apply. The significance of L_3 suggests that rational behavior differs in structured bargaining depending on who, the controller or noncontroller, moves first. More precisely, one can determine which group bargains more rationally by calculating the point estimate of L_3 $(=\bar{y}_{3.}+\bar{y}_{5.}-\bar{y}_{4.}-\bar{y}_{6.})$. A negative estimate indicates that the noncontroller/first mover group behaves more rationally than the controller/first mover group [i.e., $(\mu_3+\mu_5)<(\mu_4+\mu_6)$]; conversely, a positive estimate indicates the opposite result [i.e., $(\mu_3+\mu_5)>(\mu_4+\mu_6)$]. Across all measures of α, the point estimate for L_3 is negative which implies that sequential (structured) bargaining yields more rational behavior when the noncontroller, as opposed to the controller, moves first.

In short, the preceding results lead to conclusions regarding the three hypotheses-- Efficiency, Rationality, and Timing-- which appear in Section 5.2. First, the LSD results for reward efficiency (R) do not allow one to reject H_O: $\mu_1=\mu_i$ $\forall$ $i\neq 1$; thus, one rejects the Efficiency Hypothesis. No protocol scheme yields a significantly higher level of efficiency than the base group (Protocol 1). Second, none of the α measures, on average, exhibit a constrained self-interest weight near a value of 1 (see Figure 9). This means that bargainers, on average, did not secure pure self-interested agreements across all protocol schemes. Hence, one rejects the Rationality Hypothesis. Lastly, for the response variable $TIME$, a test of H_O: $\mu_1=\mu_2=\cdots=\mu_6$ cannot be rejected (recall Table 11). This suggests that agreement times (defined as time remaining in seconds) remain independent of protocol scheme. Additionally, the experiment exhibits a high frequency of early agreements (recall Figure 7). Since the agreement times remain independent of the protocol scheme and do not occur near the deadline, this analysis fails to reject the Timing Hypothesis.[71]

7.3 Econometric Results

Finally, to better understand the bargaining behavior in this paper's experiment, the analysis considers four multiple regression models. Equation (5) in Section 5.3 represents **model 1** which, for convenience, is

$$y = \beta_0 + \beta_1 DTOK + \sum_{i=2}^{6} \beta_i \tau_i + \beta_7 G_{FM} + \beta_8 G_{MF} + \beta_9 G_{FF} + \beta_{10} LOYA + \varepsilon .$$

Model 2 mimics model 1 except model 2 replaces $LOYA$ with $LOYD$ as follows

$$y = \beta_0 + \beta_1 DTOK + \sum_{i=2}^{6} \beta_i \tau_i + \beta_7 G_{FM} + \beta_8 G_{MF} + \beta_9 G_{FF} + \beta_{10} LOYD + \varepsilon$$

---

[71] As a final note, it may seem reasonable to conduct a multivariate analysis of variance (MANOVA) which includes the four response variables, R, α, P_C, and $TIME$. Such an analysis reveals information about the correlation (if any) between the response (dependent) variables [see Stevens (1992)]. This type of information, however, does not remain too critical for the present study because several of the response variables remain functions of each other. For example, all measures of α include P_C, and R depends on $TIME$.; hence, the analyst already knows which variables remain correlated.

where $LOYD = 1$ if the controller's loyalty response from the Post Experimental Questionnaire is "d", and = 0 otherwise.

An examination of the open-ended responses to choice "d" ($LOYD$), from the Post Experimental Questionnaire, reveals that 21 out of 60 controllers marked "d", and of these 21 controllers 9 wrote "to be fair" and 9 wrote "a & b" or "both." Clearly, these responses retain the same meaning; and hence, this analysis considers response "d" as a loyalty towards fairness. Lastly, except for the loyalty variable, all other variables in models (1) and (2) maintain the same definition as given in Section 5.3 for equation (5).

Additionally, rather than use each treatment separately as a regressor, models 3 and 4 partition the treatments into three groups [which coincide with the three treatment contrasts found in Section 7.2]. Specifically, **model 3** is

$$y = \beta_0 + \beta_1 DTOK + \beta_2 CT + \beta_3 STRUCT + \beta_4 NC + \beta_5 G_{FM} + \beta_6 G_{MF} + \beta_7 G_{FF} + \beta_8 LOYA + \varepsilon$$

where CT = 1 if cheap talk and = 0 if not

$STRUCT$ = 1 if structured bargaining and = 0 if free form bargaining

NC = 1 if noncontroller (in structured bargaining) moves first, and = 0 if controller (in structured bargaining) moves first.

All other variables retain the same definition as given in Section 5.3 for equation (5). **Model 4** mirrors model 3 except model 4 replaces $LOYA$ with $LOYD$.

Table 13 summarizes the regression results for the response variables R, P_C, and $TIME$, under models 1 and 2. In this table, except for the overall F-values, all

Table 13. Regression Results for Three Response Variables, R, P_C, $TIME$: Models 1 and 2

	Reward Efficiency (R)		Pr(Controller Wins) (P_C)		Time Remaining ($TIME$)	
Terms	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2
Intercept, β_0	-1.0052 (0.8577)	-0.9920 (0.8354)	0.6924 * (0.0914)	0.9089 * (0.0888)	246.5828 * (26.1816)	252.0045 * (25.3702)
Difference in inital wealth, *DTOK*	-0.0373 (0.1024)	-0.0389 (0.1019)	-0.0008 (0.0109)	-0.0145 (0.0108)	-2.3165 (3.1254)	-2.6830 (3.0944)
Treatment 2, τ_2	0.9248 (0.6625)	1.0179 (0.6465)	-0.1718 * (0.0706)	-0.2033 * (0.0688)	23.2984 (20.2212)	25.4682 (19.6331)
Treatment 3, τ_3	0.1805 (0.6702)	0.2360 (0.6661)	-0.1161 (0.0714)	-0.1279 ** (0.0708)	1.0812 (20.4581)	2.5368 (20.2297)
Treatment 4, τ_4	-0.1158 (0.6622)	0.0069 (0.6730)	-0.0022 (0.0706)	0.0192 (0.0716)	-1.9553 (20.2138)	2.3617 (20.4393)
Treatment 5, τ_5	-0.7531 (0.6561)	-0.8115 (0.6635)	-0.1390 ** (0.0699)	-0.1724 * (0.0706)	-13.1175 (20.0258)	-15.7094 (20.1506)
Treatment 6, τ_6	0.5058 (0.6498)	0.4589 (0.6586)	-0.0109 (0.0692)	-0.0651 (0.0700)	9.2346 (19.8344)	6.5201 (20.0011)
Female-Male Genders, G_{FM}	-0.5364 (0.6526)	-0.5513 (0.6533)	0.0124 (0.0695)	-0.0309 (0.0695)	-16.7561 (19.9201)	-18.2238 (19.8388)
Male-Female Genders, G_{MF}	-0.0345 (0.6622)	-0.0434 (0.6610)	-0.0592 (0.0706)	-0.1154 (0.0703)	0.6642 (20.2126)	-0.9142 (20.0745)
Female-Female Genders, G_{FF}	-0.5919 (0.6654)	-0.5936 (0.6578)	-0.1023 (0.0709)	-0.1954 * (0.0700)	-11.0691 (20.3109)	-13.2772 (19.9762)
Loyalty A, *LOYA*	-0.1625 (0.4405)		0.1596 * (0.0469)		-1.3671 (13.4472)	
Loyalty D, *LOYD*		-0.2226 (0.4424)		-0.1626 * (0.0471)		-10.6910 (13.4361)
Overall F-value for model	1.172	1.187	3.605 *	3.659 *	0.769	0.841

Note: the top number for each term represents the estimated coefficient and the bottom number in parentheses represents the estimate's standard error.

* significant at the 5% level.

** significant at the 10% level.

non-parenthetical numbers represent parameter estimates and all numbers in parentheses represent standard errors associated with the parameter estimates. For R and $TIME$, none of the regression coefficients remain significant (i.e., one cannot reject H_O: $\beta_j = 0$ for each regressor) under models 1 and 2. Given this, it remains no surprise that a test for significance of regression (i.e., test H_O: $\beta_1 = \beta_2 = \cdots = \beta_{10} = 0$) based on the overall F-value for models 1 and 2 under both R and $TIME$ cannot be rejected, either.

For P_C, the overall F-value is 3.605 and 3.659 for models 1 and 2, respectively. Both of these F-values remain significant at the 5% level. This means that at least one regressor variable in each model contributes to the significance of the model. Indeed, t-tests on the individual regression coefficients reveal several significant effects. First, a loyalty effect exists across both models, where the coefficients on $LOYA$ and $LOYD$ both remain significantly different from zero at the 5% level. The significantly positive coefficient on $LOYA$ indicates that P_C increases when controllers remain loyal to themselves, while the significantly negative coefficient on $LOYD$ implies that P_C declines when controllers remain partially loyal to both themselves and the noncontroller. Second, Treatments 2 and 5 both exhibit significantly negative effects on P_C, across both models. Apparently, neither cheap talk/free form bargaining (τ_2) nor cheap talk/structured (with the controller as first mover) bargaining (τ_5) induce the controller to secure higher probabilities of victory. Thirdly, model 2 contains a gender effect. P_C tends to decline when female controllers bargain with female noncontrollers (see the significantly negative coefficient on the female-female gender variable).

Table 14 summarizes the regression results across all measures of the constrained self-interest weight, α, for models 1 and 2. As in Table 13, except for

Table 14. Regression Results for the Constrained Self-Interest Weights: Models 1 and 2

	Weak Rationality (time independent, α_I)		High Risk Aversion (50% risk premium, $\alpha_{50\%}$)		Low Risk Aversion (10% risk premium, $\alpha_{10\%}$)		Risk Neutral (α_N)	
Terms	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2
Intercept, β_0	0.4951 * (0.2209)	1.0313 * (0.2139)	0.3791 * (0.1789)	0.7993 * (0.1720)	0.3531 * (0.1725)	0.7543 * (0.1664)	0.3477 * (0.1712)	0.7450 * (0.1653)
Difference in inital wealth, *DTOK*	-0.0059 (0.0264)	-0.0398 (0.0261)	-0.0049 (0.0214)	-0.0314 (0.0210)	-0.0044 (0.0206)	-0.0298 (0.0203)	-0.0043 (0.0204)	-0.0294 (0.0202)
Treatment 2, τ_2	-0.4364 * (0.1706)	-0.5128 * (0.1655)	-0.3135 * (0.1382)	-0.3710 * (0.1331)	-0.2890 * (0.1332)	-0.3448 * (0.1288)	-0.2840 * (0.1322)	-0.3394 * (0.1279)
Treatment 3, τ_3	-0.2894 ** (0.1726)	-0.3177 ** (0.1706)	-0.2001 (0.1398)	-0.2209 (0.1372)	-0.1830 (0.1348)	-0.2033 (0.1327)	-0.1795 (0.1338)	-0.1998 (0.1318)
Treatment 4, τ_4	-0.0093 (0.1705)	0.0455 (0.1723)	-0.0010 (0.1381)	0.0451 (0.1386)	0.0030 (0.1332)	0.0458 (0.1340)	0.0038 (0.1322)	0.0459 (0.1332)
Treatment 5, τ_5	-0.3055 ** (0.1690)	-0.3891 * (0.1699)	-0.2087 (0.1369)	-0.2756 * (0.1366)	-0.1883 (0.1319)	-0.2517 ** (0.1322)	-0.1841 (0.1310)	-0.2468 ** (0.1313)
Treatment 6, τ_6	-0.0551 (0.1673)	-0.1900 (0.1686)	-0.0030 (0.1356)	-0.1098 (0.1356)	0.0102 (0.1307)	-0.0914 (0.1312)	0.0129 (0.1297)	-0.0876 (0.1303)
Female-Male Genders, G_{FM}	0.0340 (0.1681)	-0.0736 (0.1673)	-0.0327 (0.1361)	-0.1173 (0.1345)	-0.0409 (0.1312)	-0.1216 (0.1301)	-0.0425 (0.1303)	-0.1224 (0.1292)
Male-Female Genders, G_{MF}	-0.1224 (0.1705)	-0.2616 (0.1693)	-0.1100 (0.1381)	-0.2193 (0.1361)	-0.1066 (0.1332)	-0.2108 (0.1317)	-0.1058 (0.1322)	-0.2090 (0.1308)
Female-Female Genders, G_{FF}	-0.2194 (0.1714)	-0.4500 * (0.1684)	-0.2090 (0.1388)	-0.3896 * (0.1355)	-0.2017 (0.1338)	-0.3742 * (0.1310)	-0.2001 (0.1328)	-0.3710 * (0.1301)
Loyalty A, *LOYA*	0.3926 * (0.1134)		0.3033 * (0.0919)		0.2911 * (0.0886)		0.2887 * (0.0879)	
Loyalty D, *LOYD*		-0.4057 * (0.1133)		-0.3234 * (0.0911)		-0.3068 * (0.0881)		-0.3033 * (0.0875)
Overall *F*-value for model	3.615 *	3.734 *	3.271 *	3.504 *	3.184 *	3.363 *	3.165 *	3.332 *

Note: the top number for each term represents the estimated coefficient and the bottom number in parentheses represents the estimate's standard error.

* significant at the 5% level.

** significant at the 10% level.

the overall F-values, all non-parenthetical numbers represent parameter estimates and all numbers in parentheses represent standard errors associated with the parameter estimates. Since the overall F-value remains significant at the 5% level for models 1 and 2 across all measures of α, one can reject H_O: $\beta_1 = \beta_2 = \cdots = \beta_{10} = 0$ for all the models in Table 14. These F-test results suggest that at least one regressor variable, in each model, makes a significant contribution to the model. And once again, the analysis uses t-tests on the individual regression coefficients to find which regressors make significant contributions. For the time-independent measure, α_I, Table 14 reveals several ignificant effects. First, similar to P_C, a loyalty effect exists across both models for α_I, where $LOYA$ and $LOYD$ show a significantly positive and negative effect, respectively, on rational behavior. This implies that rational behavior increases when controllers remain loyal to themselves, while rational behavior decreases when a controller's loyalty lies partially with him/herself and the noncontroller. Such results remain consistent with theoretical expectations since one would expect self-interested feelings to increase rational behavior, and fairness feelings to decrease rational behavior. Second, Treatments 2, 3, and 5 yield significantly negative effects on α_I, in both models. This suggests that Protocols (Treatments) 2,3, and 5 lead controllers toward less rational behavior. Given Protocols 3 and 5 both involve structured bargaining with the *controller* as first mover, one might speculate that having the controller move first induces some type of awareness (or sympathy) for the noncontroller's disadvantageous bargaining position; and hence, leads the controller towards less rational behavior. Similarly, Protocol 2 may induce, through its various modes of communication (e.g., verbal, written, and cheap talk), some type of intrinsic awareness for the noncontroller's well-being. Such explanations for the negative effects of Protocols 2, 3, and 5 on rational behavior are not unrealistic if these Protocols cause the controller to consider how his/her actions affect the noncontroller's well-being (or expected utility).[72] Lastly, Table 14 indicates that the coefficient on the female-female gender variable is negative and significant at the 5% level. This suggests that rational behavior declines when female controllers bargain with female noncontrollers.

For the time-dependent measures of α ($\alpha_{50\%}$, $\alpha_{10\%}$ and α_N), Table 14 reports many of the same effects as found for α_I. Specifically, the *LOYA* and *LOYD* effects and the female-female gender effect all appear robust across both time-independent and time-dependent measures of α. As for the treatment (protocol) effects, a slight difference occurs between α_I and the time-dependent measures of α. Protocol 2 and Protocol 5 (only for model 2) remain significant across all time-dependent measures, while Protocol 3 becomes insignificant with the incorporation of time.

Tables 13 and 14 summarize the regression results corresponding to models 1 and 2. For the regression results pertaining to models 3 and 4, the discussion, now, focuses on Tables 15 and 16. Table 15 summarizes the regression results for the response variables R, P_C, and *TIME* under models 3 and 4. The overall *F*-values for R and *TIME* under models 3 and 4 are statistically insignificant. This suggests that models 3 and 4 offer no help in predicting R and *TIME*.

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[72] Note, actions based on sympathy may not remain irrational. Sen (1977, p. 326) remarks, "It can be argued that behavior based on sympathy is in an important sense egoistic, for one is oneself pleased at others' pleasure and pained at others' pain, and the pursuit of one's own utility may thus be helped by sympathetic action. " An alternative explanation for Protocols 2,3, and 5's negative effect on rational behavior might stem from what Sen (1977) calls "commitment." "If the knowledge of torture of others...does not make you feel personally worse off, but you think it is wrong and you are ready to do something to stop it, it is a case of commitment" [Sen (1977, p. 326)]. Even if a controller does not feel sympathetic towards the noncontroller, Protocols 2,3, and 5 might present environments which cause a controller to perceive the noncontroller's disadvantageous position as unjust; and hence, the controller may make a choice, based on commitment, which yields him/her a lower expected level of utility than the outside option. Such an explanation, however, would suggest that the experiment failed to induce a belief of morally justified unilateral property rights.

Table 15. Regression Results for Three Response Variables, R, P_C, $TIME$: Models 3 and 4

	Reward Efficiency (R)		Pr(Controller Wins) (P_C)		Time Remaining ($TIME$)	
Terms	Model 3	Model 4	Model 3	Model 4	Model 3	Model 4
Intercept, β_0	-0.4852 (0.7795)	-0.6017 (0.8350)	0.6183 * (0.0819)	0.8731 * (0.0865)	258.4745 * (23.1623)	260.9889 * (24.6472)
Difference in inital wealth, $DTOK$	-0.0344 (0.1048)	-0.0184 (0.1037)	-0.0018 (0.0110)	-0.0171 (0.0107)	-2.2263 (3.1141)	-2.1264 (3.0618)
Ceap Talk, CT	0.2296 (0.3944)	0.2088 (0.4124)	-0.0710 ** (0.0414)	-0.1194 * (0.0427)	7.3255 (11.7205)	5.6626 (12.1727)
Structured Bargaining, $STRUCT$	-0.7380 (0.4813)	-0.7924 (0.4868)	-0.0391 (0.0506)	-0.0482 (0.0504)	-17.5982 (14.3026)	-19.3095 (14.3681)
Noncontroller Moves First, NC	0.4682 (0.4827)	0.5121 (0.4875)	0.1190 * (0.0507)	0.1272 * (0.0505)	9.5268 (14.3434)	10.9266 (14.3895)
Female-Male Genders, G_{FM}	-0.7275 (0.6621)	-0.7176 (0.6702)	0.0256 (0.0696)	-0.0217 (0.0695)	-20.5210 (19.6752)	-21.3118 (19.7826)
Male-Female Genders, G_{MF}	-0.1821 (0.6649)	-0.1681 (0.6772)	-0.0342 (0.0699)	-0.1020 (0.0702)	-2.8807 (19.7567)	-4.0199 (19.9901)
Female-Female Genders, G_{FF}	-0.6870 (0.6698)	-0.6159 (0.6776)	-0.0793 (0.0704)	-0.1902 * (0.0702)	-13.6502 (19.9039)	-14.1831 (20.0004)
Loyalty A, $LOYA$	-0.3487 (0.4316)		0.1835 * (0.0454)		-5.5122 (12.8268)	
Loyalty D, $LOYD$		-0.0775 (0.4264)		-0.1844 * (0.0442)		-6.2948 (12.5879)
Overall F-value for model	0.849	0.762	4.033 *	4.195 *	0.741	0.750

Note: the top number for each term represents the estimated coefficient and the bottom number in parentheses represents the estimate's standard error.

* significant at the 5% level.

** significant at the 10% level.

For P_C, the overall F-values under models 3 and 4 are statistically significant at the 5% level. This suggests that models 3 and 4 remain useful in predicting P_C. Additionally, individual t-tests on each coefficient in both models reveal several significant effects. First, cheap talk yields a significantly negative effect on P_C in both models--see the negative coefficients on cheap talk in models 3 and 4. This implies that P_C tends to decrease when bargainers exchange costless messages (in terms of lottery tickets) prior to bargaining. Second, a first mover effect exists in structured bargaining. The coefficient on the noncontroller variable is positive and significant (at the 5% level) for both models. This suggests that P_C increases when noncontrollers (as opposed to controllers) move first in structured bargaining. Third, similar to models 1 and 2, a loyalty effect exists in models 3 and 4. $LOYA$ yields a positive and significant effect on P_C, while $LOYD$ yields a negative and significant effect on P_C. Lastly, model 4 contains a female-female gender effect. The significantly negative coefficient on G_{FF} suggests that P_C declines when female controllers bargain with female noncontrollers.

Table 16 summarizes the regression results across all measures of the constrained self-interest weight, α, for models 3 and 4. The overall F-value remains significant at the 5% level for models 3 and 4 across all measures of α. This suggests that at least one regressor variable, in each model, makes a significant contribution to the model. Individual t-tests on the regression coefficients reveal which regressor variable(s) significantly affects α. First, model 4, across all α measures, contains an initial wealth effect. The coefficient on the difference in initial wealth variable is negative and significant (at the 10% level) across all α measures. This suggests that as the difference in initial wealth [i.e., $DTOK$ = (Controller's initial wealth) - (Noncontroller's initial wealth), and $DTOK > 0$] increases, rational behavior

Table 16. Regression Results for the Constrained Self-Interest Weights: Models 3 and 4

	Weak Rationality (time independent, α_I)		High Risk Aversion (50% risk premium, $\alpha_{50\%}$)		Low Risk Aversion (10% risk premium, $\alpha_{10\%}$)		Risk Neutral (α_N)	
Terms	Model 3	Model 4	Model 3	Model 4	Model 3	Model 4	Model 3	Model 4
Intercept, β_0	0.3023 (0.1988)	0.9358 * (0.2098)	0.2326 (0.1604)	0.7295 * (0.1677)	0.2153 (0.1544)	0.6885 * (0.1619)	0.2117 (0.1532)	0.6800 * (0.1608)
Difference in inital wealth, *DTOK*	-0.0083 (0.0267)	-0.0462 ** (0.0261)	-0.0068 (0.0216)	-0.0363 ** (0.0208)	-0.0062 (0.0208)	-0.0344 ** (0.0201)	-0.0061 (0.0206)	-0.0340 ** (0.0200)
Cheap Talk, *CT*	-0.1746 ** (0.1006)	-0.2949 * (0.1036)	-0.1144 (0.0812)	-0.2099 * (0.0828)	-0.1017 (0.0781)	-0.1923 * (0.0800)	-0.0991 (0.0775)	-0.1887 * (0.0794)
Structured Bargaining, *STRUCT*	-0.0738 (0.1228)	-0.0966 (0.1223)	-0.0430 (0.0990)	-0.0623 (0.0978)	-0.0366 (0.0953)	-0.0547 (0.0944)	-0.0353 (0.0946)	-0.0531 (0.0938)
Noncontroller Moves First, *NC*	0.2610 * (0.1231)	0.2816 * (0.1225)	0.1987 ** (0.0993)	0.2161 * (0.0979)	0.1887 ** (0.0956)	0.2048 * (0.0945)	0.1866 ** (0.0949)	0.2025 * (0.0939)
Female-Male Genders, G_{FM}	0.0723 (0.1689)	-0.0453 (0.1684)	-0.0053 (0.1362)	-0.0978 (0.1346)	-0.0155 (0.1312)	-0.1036 (0.1300)	-0.0175 (0.1301)	-0.1047 (0.1291)
Male-Female Genders, G_{MF}	-0.0585 (0.1696)	-0.2271 (0.1702)	-0.0610 (0.1368)	-0.1937 (0.1360)	-0.0604 (0.1317)	-0.1866 (0.1313)	-0.0602 (0.1307)	-0.1851 (0.1304)
Female-Female Genders, G_{FF}	-0.1622 (0.1709)	-0.4380 * (0.1703)	-0.1644 (0.1378)	-0.3802 * (0.1361)	-0.1595 (0.1327)	-0.3651 * (0.1314)	-0.1584 (0.1317)	-0.3620 * (0.1305)
Loyalty A, *LOYA*	0.4556 * (0.1101)		0.3508 * (0.0888)		0.3358 * (0.0855)		0.3328 * (0.0848)	
Loyalty D, *LOYD*		-0.4589 * (0.1072)		-0.3640 * (0.0857)		-0.3455 * (0.0827)		-0.3417 * (0.0821)
Overall F-value for model	3.950 *	4.135 *	3.617 *	3.986 *	3.539 *	3.842 *	3.522 *	3.809 *

Note: the top number for each term represents the estimated coefficient and the bottom number in parentheses represents the estimate's standard error.

* significant at the 5% level.

** significant at the 10% level.

decreases. Such behavior is inconsistent with the concept of wealth maximization, but it is not inconsistent with the concept of sympathy. Second, cheap talk exerts a negative and significant effect on all measures of α. The implies that rational behavior declines when bargainers engage in costless, pre-bargain communication. A possible explanation for this result is that the noncontroller's cheap talk messages influence (in a negative direction) the controller's reservation price (i.e., lowest acceptable probability of victory, P_C).[73] A lower P_C yields a lower α level, in terms of both weak and strict rationality. Third, a first mover effect, in structured bargaining, appears. The coefficient on the noncontroller moves first variable is positive and statistically significant across all measures of rationality. This implies that rational behavior, in structured bargaining, increases when noncontrollers move first.[74] Fourth, similar to models 1 and 2, a loyalty effect exists in models 3 and 4, across all measures of α. *LOYA* and *LOYD* yield significantly positive and negative, respectively, effects on all measures of rationality. Lastly, a negative and statistically significant female-female gender effect exists for model 4 across all measures of rationality.

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[73] Given cheap talk yields a significantly negative effect on P_C (see the regression results for P_C in Table 15), this explanation remains plausible.

[74] Note, this result remains consistent with the treatment contrast results for L_3 under all α measures (see Section 7.2), where the point estimates for L_3 across all α measures indicate that the noncontroller moves first group obtains a higher level of rationality (α) than the controller moves first group.

7.4 Summary of Key Results and Economic Implications

Sections 7.1 to 7.3 analyze the experimental results in terms of efficiency, distribution of wealth (i.e., rationality), and time remaining. This section summarizes the key results and notes the economic implications of the results.

First, none of the protocols help the bargainers choose or create contracts which are more reward efficient than contracts created under the Coasian base group. All protocols yield an average efficiency ($\overline{R}$) which is negative and not statistically different from the base group average; hence, one rejects the Efficiency Hypothesis. The linear transaction costs severely inhibit the bargainers from capturing a positive proportion of the potential gains due to bargaining, and none of the protocols help counteract this effect.[75] Thus, the implementation of stricter bargaining rules appears ineffective in reducing the efficiency costs of linear transaction costs.

Second, the majority of bargainers acted neither as gamesmen nor fairmen. Constrained self-interest dominated the majority of bargains (recall Figures 4 and 5); hence, one rejects the Rationality Hypothesis. This suggests that bargainers are driven by both monetary and nonmonetary concerns, as they balance the two extremes of pure self-interest and equity. In fact, the econometric results (in Section 7.3) indicate what factors increase and decrease rational behavior.

For instance, model 4's results (see Table 16) suggest that higher differences in initial wealth, between the controller and noncontroller, lead to lower levels of rationality. This remains consistent with the concept of sympathy, not wealth maximization. Additionally, cheap talk exerts a negative effect on rational play.

[75] Note, since the bargainers struck high traditional efficiency agreements (recall the Ψ results in Table 5), one can attribute the negative reward efficiency results to the linear transaction costs.

Controllers who bargain under cheap talk appear willing to accept lower probabilities of victory. A possible explanation for this effect might lie in the wording of the cheap talk messages which explicitly elicit "willingness to accept" and "willingness to offer" from both players. This wording might, somehow, enable the noncontroller to persuade the controller to accept a lower probability of victory. Furthermore, rational behavior tends to decline when female controllers bargain with female noncontrollers. This female-female gender effect appears consistent with social-psychological research [see Thompson (1990)] which finds that women are less concerned, than are men, about winning and maximizing their outcome. A final factor which reduces rational behavior is loyalty towards fairness (recall the *LOYD* effect in Tables 14 and 16). Consistent with theoretical expectations, rational behavior declines when a controller remains partially loyal to both him/herself and the noncontroller.

In contrast to the above factors, two other factors increase rational behavior. Loyalty towards oneself represents one factor. As expected, rational behavior rises when a controller feels loyal to him/herself (recall the *LOYD* effect in Tables 14 and 16). The second factor depends on who moves first in structured bargaining. Rational behavior increases, in structured bargaining, when the noncontroller (as opposed to the controller) moves first (see Table 16). One possible explanation for this effect is: a noncontroller moves first *rule* helps justify the controller's unilateral property right (assuming the bargainers regard a first move as advantageous) and, in turn, reduces the possibility of actions based on sympathy, commitment (see footnote 72), or some other nonmonetary factor.

Lastly, the time remaining results suggest that no deadline effect exists. The bargainers did not wait till the last minute before striking a bargain (see Figures 6 and 7). This suggests that lengthy bargaining delays are not preferred strategies among bargainers when bargaining is costly over time. Additionally, the timing results remain independent of protocol scheme (see Section 7.2). This implies that none of the protocols (including the base group) guide bargainers to quicker agreements than any other protocol scheme. Thus, given no deadline effect exists and the time remaining results remain independent of protocol scheme, this analysis cannot reject the Timing Hypothesis.

In sum, this analysis rejects both the Efficiency and Rationality Hypotheses; however, it cannot reject the Timing Hypothesis.

CHAPTER 8:
CONCLUSION

The main objective of this thesis was to further economic research into costly bargaining. Given the high prevalence of positive transaction costs in many "real life" bargaining situations [as Coase (1960) strongly acknowledges], and Shogren and Herriges's (1994) inefficiency results from their costly bargaining experiments, this thesis investigated the usefulness of several bargaining protocols to improve bargaining efficiency through mutually advantageous agreements in a positive transaction costs environment. The protocols (treatments) considered were: (1) free form (Coasian) bargaining which represented the base group, (2) free form (Coasian) bargaining with cheap talk, (3) structured bargaining with the controller as the first mover, (4) structured bargaining with the noncontroller as the first mover, (5) the same format as protocol 3 but with cheap talk added, and (6) the same format as protocol 4 but with cheap talk added. Through the use of a (two-person) bilateral bargaining experiment, this thesis measured the performance of each protocol in terms of reward efficiency (R), distribution of wealth (α), and time remaining ($TIME$). Additionally, the analysis investigated what probability of victory (P_C) the controllers secured across all protocol schemes.

This chapter offers some concluding remarks with respect to the above investigation. Specifically, Section 8.1 summarizes the experimental results and draws several conclusions. Section 8.2 highlights this investigation's limitations and potential shortcomings. Lastly, Section 8.3 offers some ideas for future research.

8.1 Overall Summary

Overall, the bargaining protocols did not foster efficient and mutually advantageous bargains. The experimental results (recall Chapter 7) revealed a negative mean R across all protocol schemes which were not statistically different from one another, and none of the protocols achieved an average α measure near 1. Since none of the protocols guided bargainers to more reward efficient agreements than the Coasian base group, this analysis rejected Section 5.2's Efficiency Hypothesis. This meant that the protocols appeared ineffective in reducing the efficiency costs of positive transaction costs. As for the rationality results, constrained self-interest, where $0<\alpha<1$, described the distribution of wealth in the majority of the bargains. That is, the majority of the controllers behaved neither as pure gamesmen nor as pure fairmen, but rather as rational persons constrained by fairness; hence, this experiment rejected the Rationality Hypothesis, as stated in Section 5.2. Moreover, the bargainers reached early agreements, and the times remaining remained independent of the protocol scheme. This meant that no deadline effect occurred, and costly bargaining delays did not remain a choice of strategy among the bargainers. Subsequently, the timing results could not reject Section 5.2's Timing Hypothesis.

In addition to the above results, the econometric results suggested that several factors affected rational play. Among the factors which significantly reduced rational play were: greater differences in initial wealth, cheap talk, and a female-female gender effect. Rational play also declined under Protocols 3 and 5 (recall Table 14), where the bargaining rule (protocol) designated the controller as first mover in structured bargaining. In contrast, rational play increased, in structured bargaining, when the bargaining rule designated the noncontroller as first mover (recall Table 16). Lastly, the econometric results showed that rational behavior increased when controllers remained loyal to themselves, while rational behavior declined when controllers remained partially loyal to both themselves and the noncontroller. [76]

8.2 Limitations and Potential Shortcomings

Similar to many other studies, this study is not immune to potential shortcomings, and it contains limitations.

First, one might criticize the one-shot (i.e., nonrepeated) nature of this experiment's bargains. Other bargaining experiments typically use repeated trials. This allows subjects to gain a better understanding of the experimental procedures through experience. Such experience, however, can create a learning effect, and many experiments which use repeated trials do not account for this. As mentioned earlier, this experiment uses one-shot bargains to avoid a learning effect. This avoidance, nevertheless, may come at a cost in terms of a loss in understanding of the experimental instructions among the subjects. By including examples and a pre-experimental questionnaire in the experimental instructions (recall Section 6.6), the current experiment hopefully evaded this potential tradeoff between understanding the procedures and eliminating (via one-shot bargains) the potential for a learning effect. Future research might replicate this experiment with repeated trials, but the experimental design should allow the experimenter to check for potential learning

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[76] Note, the latter two loyalty effects support Shogren's (1989) loyalty explanation for the divergence between gamesman and fairman behavior.

effects. For example, some type of block or factorial design might explain any potential learning effects.

Second, though this experiment and Shogren and Herriges's (1994) experiment indicate that no deadline effect exists in costly bargaining, the current investigation cannot attribute this occurrence entirely to the positive transaction costs. Other factors may exist. For example, Shogren and Herriges (1994) attribute their early agreement results not only to transaction costs, but to the controller's right (via the outside option) to unilaterally end the bargain. This explanation remains relevant for the current experiment, too. Another contributing factor may be the face-to-face bargaining format. To explain, Harrison and McKee (1985) study face-to-face bargaining in a costless transactions environment, and they report, "Very few of the observed outcomes were decided 'at the wire', and this time limit did not appear to be a binding constraint on negotiation behavior" (p. 658). At this point, note that three perspectives on the deadline effect exist: (1) RMS (1988) find a deadline effect based on anonymous bargaining in a costless transactions environment, (2) Harrison and McKee (1985) find no deadline effect in face-to-face bargaining under a costless transactions environment, and (3) the current investigation and Shogren and Herriges (1994) find no deadline effect in face-to-face bargaining under a positive transaction costs environment. A comparison of perspectives (2) and (3) suggests that face-to-face bargaining leads to early agreements, regardless of transaction costs, while a comparison of perspectives (1) and (2) suggests that anonymous bargaining yields slower agreements than face-to-face bargaining. The latter comparison may simply result because communication is, in general, more limited or difficult under anonymous bargaining than under face-to-face bargaining. Although these comparisons highlight the potential influence of face-to-face bargaining on early agreements, one must stress that these comparisons merely represent suggestions and not decisive assertions. An experiment on costly bargaining with deadlines, which uses both anonymous bargaining and face-to-face bargaining treatments, might reveal a better understanding of deadlines in costly bargains.

Lastly, this investigation contains several limitations. First, one cannot generalize this investigation beyond the two-person scenario. Though many negotiations involve disputes among two people [e.g., child custody battles], many others involve disputes among multitudes of people [e.g., environmental and natural resource disputes]. A more comprehensive study of the usefulness of protocols in costly bargains might involve multilateral, as well as bilateral team, bargains. Second, the results of this investigation evolve from linear transaction costs. Given Shogren and Herriges (1994) report that bargaining behavior differs across various cost structures [e.g., linear versus nonlinear costs], this investigation's protocols may or may not foster efficient and mutually advantageous bargains in other cost structures. Third, the cheap talk phase only lasts for 20 seconds [i.e., two 10 second rounds]. Ten seconds may not be long enough for bargainers to think clearly about their responses. Longer cheap talk rounds would give bargainers more time to think which, in turn, might bring about greater pre-bargain coordination; and hence, trigger quicker and less costly bargains. Lastly, this experiment's memorization game may not remain an appropriate way to distribute property rights. Future research might use an auction (e.g., a second-price sealed bid auction) to distribute property rights.

8.3 Future Research Ideas

Although the protocols, considered here, exhibit little success in improving the efficiency results of Coasian bargaining under positive transaction costs, the case for protocols is far from closed. This investigation only scratches the surface. Other protocols may exist which lessen the negative effects of transaction costs. This section offers some ideas for future research into bargaining protocols.

The first idea involves the use of an arbitrator. An arbitrator is a neutral third party who reviews each parties' arguments, and then makes a binding decision which settles the dispute. Ashenfelter and Currie (1990) and Ashenfelter, Currie, Farber, and Spiegel [1992] (hereafter, ACFS) define three types of arbitration systems:

> (i) conventional arbitration (CA) where the arbitrator can impose any settlement he/she prefers,
> (ii) final-offer arbitration (FOA) where the arbitrator must choose one of the final offers of the parties without any compromise, and
> (iii) tri-offer arbitration (TRI) where the arbitrator must choose one outcome from three possibilities--one or the other of the parties' final offers or the recommendation of a neutral fact-finder.

Typically, the negotiation process involves arbitration only if an impasse exists at a designated deadline. Given this, any experimental test of the usefulness of arbitration, as a viable protocol, in costly bargaining must consider several issues. First, if the transaction costs remain relatively high, such that the potential gains from bargaining diminish to zero or some small amount by the deadline, then an arbitration settlement will not yield an efficient result. In this situation, the experimenter might consider different time mechanisms for implementing arbitration. Potential mechanisms might include sending negotiations to arbitration after X minutes of an impasse or after X amount of offers and counteroffers occur without an agreement. Another issue involves the choice of arbitration system(s). Mixed results exist on the effectiveness of alternative arbitration systems to stimulate settlements. ACFS (1992) find, through computer aided experiments, that the anticipation of CA, FOA, and TRI lead to higher dispute rates than bargaining without arbitration. These results support the so-called "chilling effect", which asserts that anticipated arbitration leads to higher dispute rates because it increases the incentives for bargainers to make extreme offers. In contrast, experiments by Grigsby and Bigoness (1982) do not support the chilling effect for FOA. Additionally, Coleman, Jennings, and McLaughlin (1993) present a mathematical model and empirical evidence which illustrate that anticipated FOA leads to a convergence of offers; and hence, exhibits no chilling effect. Overall, the case for arbitration, as a useful protocol in costly bargaining, involves defining an optimal time mechanism for intervention and an effective arbitration system.

Another protocol idea involves the use of a mediator. Similar to an arbitrator, a mediator is a neutral third party who reviews each parties' arguments. However, unlike an arbitrator, a mediator cannot make binding decisions regarding a settlement. The key role of a mediator is to help bargainers reach a mutually acceptable resolution to a dispute [see Mernitz (1980, pp. 39-42) for more details]. Additionally, Evarts (1988) views mediation as a variant of Coasian bargaining: "Mediation is a variant of the negotiation processes discussed by the great economist Ronald Coase, who relies on the assumption that, in certain disputes, the parties can discover their mutual interdependence and resolve their own disagreements without resorting to court processes" (p.70). Given a Coasian solution relies on a discovery of mutual interdependence among the disputing parties, one can envision the potential usefulness for mediation (as a bargaining protocol) in costly negotiations. In particular, mediation could be useful if it can help bargainers realize their mutual interdependence in a more efficient and timely manner than if the bargainers are left alone.

Implementation of a mediation protocol, however, requires several considerations. First, since mediator services are not free [Mernitz (1980)], one must consider who will pay the mediator. Should both parties or just one party bear the costs, or can one expect an outside source (such as a state or local government) to pay the mediator's bill? For environmental disputes, Bingham (1986) reports that, during the 1970's, mediator bills were typically paid by outside sources, but the future trend remains unclear on who will be willing to pay for the mediator. Second, one must consider the entry time for a mediator. In economic terms, one might suggest that the optimal entry time exists where the marginal costs of mediation equal the marginal benefits of mediation. Though this economic rule appears clear, no universal agreement exists concerning the optimal entry time for a mediator. For example, Hiltrop (1985) advocates late entry, Prudencio (1982) and Mernitz (1980) advocate entry when negotiations reach an impasse, and Simkin (1971) advocates that the optimal entry time exists in an "optimum time spectrum" which begins after trouble seems certain to the negotiators and ends before a deadlock on issues is firm. A third concern in mediation involves mediator bias. For instance, laboratory work conducted by Welton and Pruitt (1987) shows that, "Disputants who perceived the mediator as biased against their side were less accepting of the mediator and less influenced by the mediator than disputants who perceived the mediator as neutral" (p. 123). Lastly, future research on mediation protocols might address the effectiveness of mediation when property rights are well-defined.

In sum, this section offers two ideas for future research into bargaining protocols. Either one might prove effective in guiding bargainers towards efficient and mutually advantageous agreements, in a positive transaction costs environment. And since the laboratory can offer a "realistic" test of social behavior [Smith (1976)], the laboratory represents a possible method for testing these (and other) protocols.

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APPENDIX A:

GENERAL INSTRUCTIONS AND RELATED MATERIALS

Event Summary Sheet

Step	Event
1	Random pairing--you will be randomly paired with another person.
2	Play matching game to determine the controller [note: you receive 1 token (valued at $2/token) for each match].
3	Before bargaining place the required tokens (controller 5, non-controller 1, monitor 4) on the table.
4	Bargain--recall that you have 5 minutes.
5	After bargaining there will be a random draw based on the distribution of lottery tickets to determine the winner.
6	Complete the "Post Experimental Questionnaire".
7	Receive payment consisting of hourly payment ($2/hour), reward (if won), and any tokens left over.
8	End

EXPERIMENTAL INSTRUCTIONS

INTRODUCTION

You are about to participate in an experiment in decision making. The purpose of this experiment is to gain insight into certain features of economic processes. If you follow the instructions carefully, you might earn a considerable amount of money. You will be paid in cash at the end of the experiment. An hourly payment ($2/hour) will be paid regardless of your success in bargaining.

GENERAL INSTRUCTIONS

This experiment involves several basic steps (see the handout entitled "Event Summary Sheet"). First, you will be randomly paired with another person. Secondly, you and this other person will play a matching game to determine who earns the right to be the controller--an explanation of the controller and the matching game occurs below. This yields a situation where one player is the controller and the other player is the non-controller.

Next, you will participate in one bargaining session with the other player. The session involves two choices—(1) selecting a number that represents the chance to win a reward, and (2) deciding how to distribute the chances between the two players. For example, consider the lottery schedule on the blackboard. If number 3 is selected, then player A has 50 out of 100 lottery tickets giving him or her a 50% chance to win the reward; and player B has 30 lottery tickets giving him or her a 30% chance to win the reward. Note that there is also a 20% chance neither party will win.

The size of the reward depends on the time spent bargaining. Although you have 5 minutes to complete each bargain, the reward decreases as bargaining time increases. The rate at which the reward decreases will be explained below.

Finally, the last few steps include a determination of the winner (by a random draw as explained below) and a cash payment to each player.

CONTROLLER

One of you will earn the right to be the "controller" at the outset of the session. The controller has the right to unilaterally select a number by himself or herself and inform the monitor, who will stop the bargaining session. The other participant can attempt to influence the controller to reach a mutually acceptable joint decision; i.e., the other participant may offer to give either part or all of his or her chances of winning (lottery tickets) to the controller. How the controller is determined is explained below.

HOW TO DETERMINE THE CONTROLLER

After you are paired with another person, you will play a matching game to determine who will be the controller. Nineteen cards lay on the table. Nine matching pairs exist (9x2 = 18 cards) with one card left over (18+1 = 19 cards). Player A starts first by flipping over two cards so both players can see them. If they match, player A keeps the cards and earns 1 token valued at $2; otherwise, player A turns the cards face down. Player B then turns over two cards; and so on. (Note that player A and player B alternate in taking turns.) The game ends when one card is left on the table. The person with the most matches wins the game, and has earned the right to be the controller. **Remember that each match is worth 1 token valued at $2.**

For example, if player A finds 5 pairs and player B finds 4 pairs, A earns the right to be the controller. In addition, player A has 5 tokens worth $10 and player B has 4 tokens worth $8.

AGREEMENT OUTCOME

After the controller has been determined, the bargaining session begins. During the bargaining session, you may arrive at two agreements with the other player:

(1) which number to choose,

(2) how to allocate the chances (**lottery tickets**) of winning the reward.

(Note: you are not allocating the reward.)

If a joint agreement is reached, both parties must sign an agreement form stating both the chosen number and how many lottery tickets will be transferred from one participant to the other. <u>No physical threats are allowed</u>. If a joint agreement is made and the form is signed, the monitor will terminate the session. The signed agreement will be upheld in all cases.

DISAGREEMENT OUTCOME

If you cannot come to an agreement before the end of the session both participants receive zero payoff.

CONTROLLER'S RIGHT

Remember that the controller can end the session at any time without the agreement of the other party. The controller can select a number and inform the monitor, who will then end the session. No signatures are necessary if the controller ends the session.

THE REWARD

The maximum reward for bargaining will be 10 tokens. Each token is worth $2.00. The 10 token reward is derived as follows—the controller contributes 5 tokens, the noncontroller contributes 1 token, and the monitor contributes 4 tokens (5+1+4 = 10). The controller and non-controller must in all cases contribute the required tokens.

BARGAINING TIME

The bargaining session lasts 5 minutes. The maximum 10 token reward decreases over time. If no agreement is reached after 5 minutes, the reward equals zero. The 10 token reward decreases according to the reward schedule below. For example, if it took 1 minute to reach an agreement, then 4 minutes would be left and the reward would be worth 8 tokens.

Reward Schedule

Time (min.) Remaining	Reward
5.0	10
4.5	9
4.0	8
3.5	7
3.0	6
2.5	5
2.0	4
1.5	3
1.0	2
0.5	1
0.0	0

As soon as both parties sign the agreement form, or the controller chooses to end the session, the time and the value of the decreasing reward is noted by the monitor. [Note: since each token is worth $2, the $20 (=10 tokens X $2) reward declines by $1 every 15 seconds.]

EXAMPLE

Assume that A is the controller and that A and B have the following schedule of lottery tickets reflecting the chances of winning the reward:

Example of Lottery Ticket Distribution Schedule

Number	A's Chance to Win (%)	B's Chance to Win (%)
1	80	0
2	70	20
3	50	30
4	40	50
5	30	70
6	0	80

You and the other participant may arrive at two agreements:

(1) which number to choose, and

(2) how to allocate the lottery tickets.

Referring to the example schedule above, first A and B agree to select a number. If A and B agree to select number 3, then A has 50 chances out of 100 (50%) to win, B has 30 chances out of 100 (30%) to win, and there are 20 chances out of 100 (20%) that neither A nor B will win. If they agree to select number 5, A has 30 chances out of 100 (30%) to win and B has 70 chances out of 100 (70%) to win, and there is 0 chances (0%) neither A nor B will win. Suppose A and B agree to select number 5.

Second, A and B then agree how to allocate the lottery tickets. Suppose A and B agree that B will transfer 40 lottery tickets to A, then A has 70 tickets and B has 30 tickets. If they agree that B will transfer 20 lottery tickets to A, then both A and B have 50 tickets. If they agree that B will transfer 60 tickets, then A will have 90 tickets and B will have 10 tickets.

After the bargaining session, the monitor will determine the winner by drawing a lottery ticket. The composition of lottery tickets will correspond to the agree-upon distribution of lottery tickets. The amount of the reward will depend on the bargaining time. **Recall that if the time elapses (5 minutes) before the agreement is signed, then it will be considered that you have not reached an agreement, and the reward is zero for both parties.**

<u>Agreement Form (Example)</u>

A and B agree to set the number at ________.

A and B agree that _______ lottery tickets will be transferred from ______ to _____.

Signed: A: ____________________
B: ____________________

From the matching game: Player A found _____ matches and Player B found _____ matches.

Pair number : _________

Time Remaining : _________

Reward : _________

Pre-Experimental Questions

I.D. # ______

You will be more successful in bargaining and have higher chances of winning larger rewards if you thoroughly understand the experiment. Please, do not hesitate to ask questions before the bargaining session! Use the example schedule on the blackboard to answer the following questions.

1. Number _____ gives me the greatest chance to win.
2. Number _____ gives me the smallest chance to win.
3. If the other participant is the controller and he picks number 3, (a) I have _____ chances out of _____ to win. (b) I must contribute _____ tokens if I am the noncontroller.
4. If I am the controller and I select number 4, (a) there are _____ chances out of _____ that neither party will win. (b) I must contribute _____ tokens if I am the controller.
5. If the other participant and I agree to select Number 2 and I agree to transfer 10 lottery tickets to the other party: (a) I have _____ chances out of _____ to earn 10 tokens; I have _____ chances out of _____ not to earn 10 tokens.
6. The reward will increase or decrease as bargaining time elapses? __________.
7. I am indifferent between receiving (circle one) 0 1 2 3 4 5 6 7 8 9 10 tokens with 100% certainty and playing a lottery where there is an 80% chance of winning 10 tokens and a 20% chance of winning 0 tokens. (Note there is no "correct" answer to this question. Please just give your best response.)
8. What do you think is the most likely outcome (state answer in terms of a split in lottery tickets)?__________ And what do you think is the probability of its occurrence?_____ (Note there is no "correct" answer to this question. Please just give your best response.)
9. The time limit for bargaining is _____ minutes.

Agreement Form

A and B agree to set the number at ________.

A and B agree that _______ lottery tickets will be transferred from ______ to _____.

Signed: A: ____________________
B: ____________________

From the matching game: Player A found _____ matches and Player B found _____ matches.

Pair number : _________

Time Remaining : _________

Reward : _________

OFFER SHEET

Player _____ is the controller.

Player Making the Offer			Number	Lottery Tickets A	Lottery Tickets B	Accept Offer?	
1.	A	B	_______	_______	_______	Yes	No
2.	A	B	_______	_______	_______	Yes	No
3.	A	B	_______	_______	_______	Yes	No
4.	A	B	_______	_______	_______	Yes	No
5.	A	B	_______	_______	_______	Yes	No
6.	A	B	_______	_______	_______	Yes	No
7.	A	B	_______	_______	_______	Yes	No
8.	A	B	_______	_______	_______	Yes	No
9.	A	B	_______	_______	_______	Yes	No
10.	A	B	_______	_______	_______	Yes	No
11.	A	B	_______	_______	_______	Yes	No
12.	A	B	_______	_______	_______	Yes	No
13.	A	B	_______	_______	_______	Yes	No
14.	A	B	_______	_______	_______	Yes	No
15.	A	B	_______	_______	_______	Yes	No
16.	A	B	_______	_______	_______	Yes	No
17.	A	B	_______	_______	_______	Yes	No
18.	A	B	_______	_______	_______	Yes	No
19.	A	B	_______	_______	_______	Yes	No
20.	A	B	_______	_______	_______	Yes	No
21.	A	B	_______	_______	_______	Yes	No

Lottery Schedule

Number	A's Chance to Win (%)	B's Chance to Win (%)
1	0	90
2	15	85
3	30	65
4	55	30
5	80	15
6	90	0

Reward Schedule

Time (Min.) Remaining	Reward
5.0	10
4.5	9
4.0	8
3.5	7
3.0	6
2.5	5
2.0	4
1.5	3
1.0	2
0.5	1
0.0	0

Post Experimental Questionnaire

Please answer the following questions.

1. I was player (e.g., **A1**) ________.
2. Sex ________
3. Age ________
4. Major ________
5. Did you know the other person? ________
6. During the bargaining session I felt loyal to ________.
    a) myself
    b) the other player
    c) the monitor
    d) someone or something else (please indicate) ________

Thank you for your participation.

$$$ Earn Extra CASH by Participating in an Economic Experiment $$$

Features:

~involves **NO** physical discomfort (such as electric shocks)!

~ requires about 1 hour or 1 & a half hours (approximately)

~each participant has the opportunity to earn up to $28

~additionally, get $2/hr. for participating

~experiments will be held on campus in Atrium I (room 220) on the 2nd floor of Memorial Union

~experiments will take place at three different times (9:00, 12:00, and 2:30)on Friday 12/3, Monday 12/6, and Wednesday 12/8

~work around your class schedule--choose one of the above times and date

~we need 120 people to finish the experiment

~we prefer people to sign-up in advance, but walk-ins are welcome

Please come to Room #318 of Lippitt Hall or call 792-0386 after 6:00 pm. Ask for Michael. Also look for our sign-up booth in Memorial Union on 12/1, 12/2, and 12/3 from 9:00a.m. to 1:00p.m.

APPENDIX B:

SPECIFIC INSTRUCTIONS

SPECIFIC INSTRUCTIONS FOR TREATMENT 1

You and the other player may make written offers at any time. Bargaining procedure remains at the discretion of the bargainers. The only restrictions include the time limit (5 minutes) and the prohibition of physical threats. You can talk, but verbal offers are not binding.

There is no time limit for making an offer. However, note that the entire bargaining session maintains a five minute limit and the controller at any time can end the bargaining. Again, no physical threats are allowed.

Questions for Specific Instructions (Treament 1)

I can make an offer at any time, true or false? __________

The time limit for bargaining is __________ minutes.

__________ offers are binding.

(a) Verbal

(b) Written

SPECIFIC INSTRUCTIONS FOR TREATMENT 2

You and the other player may make written offers at any time. Bargaining procedure remains at the discretion of the bargainers. The only restrictions include the time limit (5 minutes) and the prohibition of physical threats. You can talk, but verbal offers are not binding.

There is no time limit for making an offer. However, note that the entire bargaining session maintains a five minute limit and the controller at any time can end the bargaining. Again, no physical threats are allowed.

Before bargaining begins, you will be given the chance to communicate (nonverbally) with the other player. This communication will occur in two rounds. First, each of you will have a slip of paper which reads:

1st Round Response:

"The minimum I am willing to accept is _____."

"The maximum I am willing to offer is _____."

2nd Round Response:

"The minimum I am willing to accept is _____."

"The maximum I am willing to offer is _____."

You will have ten seconds to write your first response. Write this response in the appropriate space provided under the section entitled "1st Round Response". If you choose not to respond, simply write "NR" in either of these spaces. Do not write any other comments on the paper. When ten seconds have elapsed, the monitor will announce "switch". At this time, exchange papers with the other player. This marks the end of round 1 and the beginning of round 2.

Once again, you will have ten seconds to respond. Write your response in the appropriate space provided under the section entitled "2nd Round Response". If you choose not to respond, simply write "NR" in either of these spaces. Any other comments are strictly prohibited. When ten seconds have elapsed, the monitor will announce "switch". At this time, exchange papers with the other player. This ends the communication rounds, and marks the start of the bargaining session. Recall you have five minutes to bargain.

Questions for Specific Instructions (Treatment 2)

I can make an offer at any time, true or false? __________

__________ offers are binding.

(a) Verbal

(b) Written

I can verbally communicate with the other player before bargaining begins, true or false?_________

Pre-bargaining communication will occur in _________ rounds and each round lasts for _________ seconds.

If I choose not to respond in one of the pre-bargaining communication rounds, then I _________.

(a) leave the paper blank

(b) write "NR"

Bargaining begins _________ after the last round of pre-bargaining communication.

(a) 10 seconds

(b) immediately

SPECIFIC INSTRUCTIONS FOR TREATMENT 3

You and the other player will make alternating offers in writing. No talking is allowed. The bargaining session begins with the controller making the first offer. The noncontroller can either accept this offer, or reject the offer and counter with a new offer. The number of offer and counteroffers is unrestricted within the 5 minute time limit.

There is no time limit for making an offer. However, note that the entire bargaining session maintains a five minute limit and the controller at any time (whether in turn or out of turn) can end the bargaining. Again, no physical threats are allowed.

Questions for Specific Instructions (Treatment 3)

I can talk to the other player, true or false? _________

If I am the controller then I can unilaterally select a number at any time and inform the monitor, who will stop the bargaining session. However, I must wait my turn when I make offers to the other player, true or false? _________

When the bargaining session begins, the _________ makes the first offer.

(a) controller

(b) non-controller

When receiving an offer, I can _________.

(a) accept it

(b) reject it and make a counter-offer

(c) do nothing

(d) any of the above

SPECIFIC INSTRUCTIONS FOR TREATMENT 4

You and the other player will make alternating offers in writing. No talking is allowed. The bargaining session begins with the noncontroller making the first offer. The controller can either accept this offer, or reject the offer and counter with a new offer. The number of offer and counteroffers is unrestricted within the 5 minute time limit.

There is no time limit for making an offer. However, note that the entire bargaining session maintains a five minute limit and the controller at any time (whether in turn or out of turn) can end the bargaining. Again, no physical threats are allowed.

Questions for Specific Instructions (Treatment 4)

I can talk to the other player, true or false? _________

If I am the controller then I can unilaterally select a number at any time and inform the monitor, who will stop the bargaining session. However, I must wait my turn when I make offers to the other player, true or false? _________

When the bargaining session begins, the _________ makes the first offer.

(a) controller

(b) non-controller

When receiving an offer, I can _________.

(a) accept it

(b) reject it and make a counter-offer

(c) do nothing

(d) any of the above

SPECIFIC INSTRUCTIONS FOR TREATMENT 5

You and the other player will make alternating offers in writing. No talking is allowed. The bargaining session begins with the controller making the first offer. The noncontroller can either accept this offer, or reject the offer and counter with a new offer. The number of offer and counteroffers is unrestricted within the 5 minute time limit.

There is no time limit for making an offer. However, note that the entire bargaining session maintains a five minute limit and the controller at any time (whether in turn or out of turn) can end the bargaining. Again, no physical threats are allowed.

Before bargaining begins, you will be given the chance to communicate (nonverbally) with the other player. This communication will occur in two rounds. First, each of you will have a slip of paper which reads:

1st Round Response:

"The minimum I am willing to accept is _____."

"The maximum I am willing to offer is _____."

2nd Round Response:

"The minimum I am willing to accept is _____."

"The maximum I am willing to offer is _____."

You will have ten seconds to write your first response. Write this response in the appropriate space provided under the section entitled "1st Round Response". If you choose not to respond, simply write "NR" in either of these spaces. Do not write any other comments on the paper. When ten seconds have elapsed, the monitor will announce "switch". At this time, exchange papers with the other player. This marks the end of round 1 and the beginning of round 2.

Once again, you will have ten seconds to respond. Write your response in the appropriate space provided under the section entitled "2nd Round Response". If you choose not to respond, simply write "NR" in either of these spaces. Any other comments are strictly prohibited. When ten seconds have elapsed, the monitor will announce "switch". At this time, exchange papers with the other player. This ends the communication rounds, and marks the start of the bargaining session. Recall you have five minutes to bargain.

Questions for Specific Instructions (Treatment 5)

I can make an offer at any time, true or false? __________

__________ offers are binding.
- (a) Verbal
- (b) Written

I can verbally communicate with the other player before bargaining begins, true or false?__________

Pre-bargaining communication will occur in __________ rounds and each round lasts for __________ seconds.

If I choose not to respond in one of the pre-bargaining communication rounds, then I __________.
- (a) leave the paper blank
- (b) write "NR"

Bargaining begins __________ after the last round of pre-bargaining communication.
- (a) 10 seconds
- (b) immediately

I can talk to the other player, true or false? __________

If I am the controller then I can unilaterally select a number at any time and inform the monitor, who will stop the bargaining session. However, I must wait my turn when I make offers to the other player, true or false? __________

When the bargaining session begins, the __________ makes the first offer.
- (a) controller
- (b) non-controller

When receiving an offer, I can __________.
- (a) accept it
- (b) reject it and make a counter-offer
- (c) do nothing
- (d) any of the above

SPECIFIC INSTRUCTIONS FOR TREATMENT 6

You and the other player will make alternating offers in writing. No talking is allowed. The bargaining session begins with the noncontroller making the first offer. The controller can either accept this offer, or reject the offer and counter with a new offer. The number of offer and counteroffers is unrestricted within the 5 minute time limit.

There is no time limit for making an offer. However, note that the entire bargaining session maintains a five minute limit and the controller at any time (whether in turn or out of turn) can end the bargaining. Again, no physical threats are allowed.

Before bargaining begins, you will be given the chance to communicate (nonverbally) with the other player. This communication will occur in two rounds. First, each of you will have a slip of paper which reads:

1st Round Response:

"The minimum I am willing to accept is _____."

"The maximum I am willing to offer is _____."

2nd Round Response:

"The minimum I am willing to accept is _____."

"The maximum I am willing to offer is _____."

You will have ten seconds to write your first response. Write this response in the appropriate space provided under the section entitled "1st Round Response". If you choose not to respond, simply write "NR" in either of these spaces. Do not write any other comments on the paper. When ten seconds have elapsed, the monitor will announce "switch". At this time, exchange papers with the other player. This marks the end of round 1 and the beginning of round 2.

Once again, you will have ten seconds to respond. Write your response in the appropriate space provided under the section entitled "2nd Round Response". If you choose not to respond, simply write "NR" in either of these spaces. Any other comments are strictly prohibited. When ten seconds have elapsed, the monitor will announce "switch". At this time, exchange papers with the other player. This ends the communication rounds, and marks the start of the bargaining session. Recall you have five minutes to bargain.

Questions for Specific Instructions (Treatment 6)

I can make an offer at any time, true or false? __________

__________ offers are binding.
- (a) Verbal
- (b) Written

I can verbally communicate with the other player before bargaining begins, true or false?__________

Pre-bargaining communication will occur in _________ rounds and each round lasts for _________ seconds.

If I choose not to respond in one of the pre-bargaining communication rounds, then I _________.
- (a) leave the paper blank
- (b) write "NR"

Bargaining begins _________ after the last round of pre-bargaining communication.
- (a) 10 seconds
- (b) immediately

I can talk to the other player, true or false? _________

If I am the controller then I can unilaterally select a number at any time and inform the monitor, who will stop the bargaining session. However, I must wait my turn when I make offers to the other player, true or false? _________

When the bargaining session begins, the _________ makes the first offer.
- (a) controller
- (b) non-controller

When receiving an offer, I can _________.
- (a) accept it
- (b) reject it and make a counter-offer
- (c) do nothing
- (d) any of the above

APPENDIX C:

DATA SET

Definitions of notation used in list of data set:

PA ≡ Pr(A wins)

PB ≡ Pr(B wins)

PQ ≡ Pr(house wins)

PCONT ≡ Pr(controller wins)

TIME ≡ Time (Seconds) Remaining

GA ≡ A's amount of tokens

GB ≡ B's amount of tokens

TREAT ≡ Treatment (Protocol)

SA ≡ A's sex

SB ≡ B's sex

LA ≡ A's loyalty response

LB ≡ B's loyalty response

NET ≡ Remaining Reward, $Z(t)$

List of Data Set

OBS	PA	PB	PQ	PCONT	TIME	GA	GB	TREAT	SA	SB	LA	LB	NET
1	0.57	0.28	0.15	0.57	183	5	4	1	F	F	A	B	6.0
2	0.30	0.65	0.05	0.65	229	4	5	4	F	M	A	A	7.5
3	0.50	0.50	0.00	0.50	267	4	5	3	F	F	D	D	9.0
4	0.15	0.85	0.00	0.85	261	3	6	1	M	F	C	A	8.5
5	0.15	0.85	0.00	0.85	297	4	5	6	F	M	A	A	10.0
6	0.80	0.15	0.05	0.80	219	6	3	4	M	F	D	D	7.5
7	0.50	0.50	0.00	0.50	272	5	4	2	M	M	D	A	9.0
8	0.40	0.60	0.00	0.60	182	4	5	4	M	M	D	D	6.0
9	0.30	0.65	0.05	0.65	215	4	5	2	F	F	A	A	7.0
10	0.50	0.50	0.00	0.50	181	1	8	1	F	F	A	D	6.0
11	0.15	0.85	0.00	0.85	270	3	6	4	F	M	D	D	9.0
12	0.67	0.33	0.00	0.67	158	6	3	3	F	M	D	D	5.5
13	0.60	0.40	0.00	0.60	254	5	4	6	F	M	D	D	8.5
14	0.50	0.50	0.00	0.50	242	2	7	3	F	F	A	B	8.0
15	0.95	0.05	0.00	0.95	240	6	3	1	F	M	A	A	8.0
16	0.00	0.90	0.10	0.90	216	4	5	5	M	F	B	B	7.0
17	0.35	0.65	0.00	0.65	180	1	8	4	M	F	D	D	6.0
18	0.50	0.50	0.00	0.50	286	2	7	2	F	F	A	B	9.5
19	0.10	0.90	0.00	0.90	195	3	6	1	F	M	A	A	6.5
20	0.30	0.70	0.00	0.70	282	3	6	2	F	F	A	B	9.5
21	0.40	0.55	0.05	0.55	254	4	5	5	F	M	A	A	8.5
22	0.50	0.50	0.00	0.50	214	5	4	6	F	F	B	B	7.0
23	0.50	0.50	0.00	0.50	253	6	3	3	M	M	D	C	8.5
24	0.55	0.30	0.15	0.30	190	2	7	5	F	M	D	B	6.5
25	0.30	0.65	0.05	0.30	281	5	4	2	M	F	D	B	9.5
26	0.40	0.55	0.05	0.40	184	6	3	5	F	M	A	A	6.0
27	0.40	0.60	0.00	0.60	172	3	6	5	M	F	A	A	5.5
28	0.15	0.85	0.00	0.85	257	4	5	6	F	M	A	B	8.5
29	0.55	0.45	0.00	0.55	181	8	1	4	M	M	D	A	6.0
30	0.50	0.45	0.05	0.50	145	5	4	3	F	M	D	D	5.0
31	0.90	0.00	0.10	0.90	293	7	2	5	M	M	A	A	10.0
32	0.95	0.05	0.00	0.95	224	8	1	4	F	M	B	B	7.5
33	0.55	0.30	0.15	0.55	266	5	4	5	F	F	B	A	9.0
34	0.50	0.50	0.00	0.50	271	6	3	6	M	F	B	B	9.0
35	0.80	0.15	0.05	0.80	181	6	3	5	F	M	A	C	6.0
36	0.50	0.50	0.00	0.50	250	6	3	5	F	M	D	D	8.5
37	0.50	0.50	0.00	0.50	266	4	5	1	F	F	B	B	9.0
38	0.40	0.55	0.05	0.55	266	4	5	2	M	F	B	B	9.0
39	0.50	0.50	0.00	0.50	249	6	3	2	F	M	B	B	8.5
40	0.50	0.50	0.00	0.50	229	6	3	6	M	F	B	B	7.5
41	0.50	0.50	0.00	0.50	266	6	3	6	F	F	C	B	9.0
42	0.50	0.50	0.00	0.50	272	5	4	4	F	F	D	D	9.0
43	0.40	0.55	0.05	0.40	260	5	4	4	F	F	D	D	8.5
44	0.90	0.00	0.10	0.90	298	5	4	6	M	F	A	A	10.0
45	0.15	0.85	0.00	0.15	272	6	3	2	M	F	D	D	9.0
46	0.15	0.85	0.00	0.85	255	3	6	3	M	M	A	A	8.5
47	0.55	0.45	0.00	0.55	177	6	3	2	M	F	D	A	6.0
48	0.50	0.50	0.00	0.50	260	0	9	3	M	F	B	B	8.5
49	0.50	0.50	0.00	0.50	225	1	8	3	F	F	B	B	7.5
50	0.50	0.50	0.00	0.50	265	4	5	2	M	F	D	D	9.0
51	0.40	0.50	0.10	0.50	220	3	6	3	F	F	B	B	7.5
52	0.40	0.55	0.05	0.55	157	4	5	5	F	F	A	C	5.0
53	0.15	0.85	0.00	0.85	255	4	5	6	M	M	A	A	8.5
54	0.15	0.85	0.00	0.85	242	3	6	1	F	M	B	B	8.0
55	0.60	0.40	0.00	0.60	267	6	3	3	M	F	A	B	9.0
56	0.55	0.30	0.15	0.55	275	5	4	1	F	F	D	D	9.0
57	0.75	0.25	0.00	0.75	275	5	4	1	M	F	D	D	9.0
58	0.80	0.15	0.05	0.80	128	5	4	6	F	F	A	C	4.5
59	0.90	0.00	0.10	0.90	282	5	4	4	F	M	A	D	9.5
60	0.50	0.50	0.00	0.50	207	3	6	1	F	M	B	B	7.0