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AN EXPERIMENTAL STUDY OF
DELEGATION AND COMMITMENT***

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ABSTRACT

This paper is about face-to-face bargaining. It attempts to shed some light on the contradictory evidence provided by laboratory face-to-face bargaining experiments and real-world data. The problem is that while face-to-face bargaining seems efficient in the lab, it appears to break down often in the real world. Our explanation for this discrepancy relies on the fact that in the real world face-to-face bargaining is usually conducted not between principals but between the agents of principals, i.e., between fiduciaries like lawyers. Hence while people meet face-to-face and bargain, the fact that these bargainers are acting as agents for others leads to a breakdown of efficiency. In short, the efficiency of laboratory face-to-face bargaining relative to real-world experience is not an artificial aspect of a laboratory environment, but rather a result of the fact that in the real world bargaining is often done through agents and not directly by principals. A set of experiments run to investigate this hypothesis is reported on here. We find a substantial increase in inefficiency when bargaining is conducted through agents rather than through principals and offer an explanation for this rise in inefficiency. As such, this paper helps to shed light on the growing literature on delegation, commitment, and preference distortion games.

1. Introduction

It is a stylized fact of laboratory bargaining experiments that in an incomplete-information environment the efficiency of face-to-face bargaining appears to be much higher than in anonymous bargaining mechanisms in which bargaining is structured by having subjects play some type of non-cooperative game without direct communication. For example, in the experiments on Coasian bargaining performed by Hoffman and Spitzer (1982) and others, face-to-face bargaining seemed to be remarkably efficient in attaining first-best gains from trade while anonymous procedures like those of Roth and Murdigham (1982), where bargainers communicate in a virtually unrestricted manner through computer terminals, were less successful.¹ Radner and Schotter (1989) find the same result comparing face-to-face bargaining with the sealed-bid mechanism.

The relative superiority of face-to-face bargaining in the laboratory does not seem to be supported in the field. For example, Ashenfelter and Currie (1990) report evidence that in a broad variety of situations, including private sector contracts, public sector contracts, collective bargaining agreements, medical malpractice cases, child custody cases, etc., bargaining tends to break down and results in costly and inefficient litigation and strikes. Other investigators, such as Kennan and Wilson (1990), Salop and White (1988) and Card (1990), report similar results. In all of these studies disagreement rates seem to

¹See Roth, A., " Bargaining Experiments", Fothcoming in the Handbook of Experimental Economics, J. Kagel and A. Roth, Editors, Princeton University Press, 1995.

be closer to those of the anonymous laboratory experiments than to those of the face-to-face variety.

The contradictory evidence presented by the lab and the field has led Roth and Prasnikar (1990) to examine whether the supposed superiority of face-to-face bargaining over anonymous structured bargaining is merely an experimental artifact "having to do with social interactions in the laboratory, rather than a more general phenomenon" (p. 1). Roth and Prasnikar argue in particular that "the negligible frequency of disagreements observed in face-to-face bargaining experiments is due to uncontrolled social pressures that have little relation to the substance of the bargaining" (p. 11). They later note that these social pressures are transmitted between experimental subjects not only verbally but also through channels of "social" or "nonverbal communication" (p. 5).

In this paper we comment on this discrepancy between real world and laboratory data. The resolution, we believe, comes from the simple fact that while all of the field studies cited are examples of face-to-face bargaining they are also examples of face-to-face bargaining between fiduciaries (e.g., lawyers, union leaders) or agents and not between the principals themselves. It is our hypothesis here that, because such agents must act on the instructions of their principals and are constrained by their fiduciary relationships, inefficiencies result because of this agency relationship that would not result if the principals could meet by themselves. In fact in a number of disputes the court has dictated that in order to facilitate agreement principals must be present at some of the negotiations between lawyers (their agents). The idea is that their presence is more likely to facilitate settlement.

On another level, one can consider this paper to be an experimental investigation of the theory underlying delegation games studied by Jones (1989a), Burtraw (1990, 1992,

and 1993) and Fershtman and Kalai (1993) amongst others.² Like commitment and preference distortion games³, delegation games have a two-part structure. In stage 1 principals delegate negotiation authority to bargaining agents whose preferences over bargaining outcomes may differ from those of the principals. Typically, the principal attempts to alter the agent's preferences by offering him or her an incentive contract before stage 2 starts. In stage 2, a game is played by the agents which could be either a Nash demand game, a Rubinstein alternating bid bargaining game, or, as in our study, a face-to-face bargaining situation under incomplete information. (Preference distortion games and commitment games have the same basic structure except that in stage 1 agents either report utility functions representing preferences over bargaining outcomes in stage 2 or commit themselves to bargaining positions by making demands from which it is hard to retreat. Obviously, in all three games, stage 1 strategies merely serve the purpose of defining a mapping from preferences to outcomes in stage 2. Principals attempt to get preferential outcomes for themselves in stage 2 by either distortion of their preferences, commitment to bargaining positions, or delegation to agents whose preferences they have attempted to control).

In the experiments we conducted, if an agreement between bargaining agents is reached it is, per se, efficient. Hence we are interested in situations in which delegation leads to inefficiency through its failure to consummate trades. In short we are not interested in explaining inefficient agreements but rather in disagreements. In the

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These are just a few of the papers written in recent years on this subjects. For applications of these ideas to problems in oligopoly theory see Fershtman and Judd (1987) and Sklivas (1987), Fershtman, Judd and Kalai (1991). For an application to the problem of worker firm negotiations see Jones (1989b).

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For some examples of preference distortion games see Crawford and Varian (1979), Sobel (1981). For examples of commitment games see Shelling (1956), Crawford (1982), and Muthoo (1992).

commitment literature, Crawford (1982) has demonstrated that disagreements between bargainers is possible when the cost of breaking a commitment is sufficiently large. The delegation game used in our experiment is isomorphic to a commitment game in the sense that our principals delegate bargaining authority to agents but are able to, if they want, bind or commit these agents not to settle for prices which are less than (or more than) certain critical levels. (They are, of course, free to leave their agents unconstrained if they think that is best.) As a result, the same type of disagreement possibilities described by Crawford (1982) in his commitment model apply here as well. Therefore, our claim is that it is delegation as in real-world bargaining and the possibility for inflexible commitment that is responsible for the breakdown of negotiations in field data and that the lack of such breakdowns in face-to-face experiments without delegation is not an artifact of the laboratory environment. (Put differently, real-world field data on principal-to-principal face-to-face bargaining would have to be presented in order to have a controlled comparison between currently existing laboratory data and field data.)

To test our hypothesis we ran a series of bargaining experiments in which agents, representing, respectively, the buyer and seller of an imaginary commodity, engaged in face-to-face bargaining on behalf of their principals. We refer to such experiments as *indirect* FFBEs, as distinguished from *direct* FFBEs in which bargaining is conducted directly by the principals. The *indirect* FFBE's we ran differed from each other in two ways: the manner through which bargainers were compensated and the frequency with which bargainers were allowed to consult with their principals. In one subset of the experiments, bargainers received their instructions from principals once and only once at the beginning of a three minute bargaining session and could not consult with them again during that round. We will refer to these experiments as the 1-consult indirect FFBE's. In these experiments the bargaining agents were paid in two different ways. In some they were paid a percentage of the profits from any transaction while in the others the agents were paid a fixed fee each time they agreed on a transaction, irrespective of its terms.

In the other *indirect* FFBE's we allowed bargainers the possibility of repeatedly consulting with their agents. We call such experiments the 4-consult *indirect* FFBE's since bargainers were given four opportunities to consult with their principals during the experiment. We ran these experiments in an effort to make our design more closely resemble real-life bargaining where agents (lawyers) can repeatedly consult with their principals (clients) and receive changes in their instructions. We feared that perhaps, the inflexibility of having only one consulting opportunity at the outset of the experiment could have created a too inflexible bargaining environment thereby leading to the inefficiencies we observed.

What we find is that the frequency of inefficient disagreements in 1-consult *indirect* FFBEs is non-negligible when bargaining is conducted by agents rather than directly between principal buyers and sellers. In a comparison of the results of direct and 1-consult indirect FFBEs, we find that disagreement rates are significantly higher when bargaining is conducted through agents and are more similar to the type of disagreement rates observed by Linhart, Radner, and Schotter (1989) in experiments using the sealed-bid mechanism. We interpret our results as evidence that the principals in FFBEs with agents treat the process of giving instructions to their agents as a delegation game with an anonymous sealed-bid mechanism in stage 2. By acting strategically through their agents, the principals distort the bargaining process between agents sufficiently to cause inefficiencies.

With respect to the 4-consult experiments, while one might conjecture that such experiments should be more efficient than their 1-consult counterparts, we find that in fact just the opposite is true. 4-consult *indirect* FFBE's have the greatest incidence of inefficient outcomes. As we will see, this results from the more complex dynamic strategic setting defined by the 4-consult game which leads principals to over-constrain their agents at the outset of the bargaining and then failing to make large enough concessions during

the remainder of the experiment.

Finally, while subjects in the direct and indirect 1-consult FFBE's typically reached an agreement quickly (in the 1-consult treatment the median length of bargaining time for consummated trades was 140 seconds for the Fixed-fee treatment and 110 for the percentage-fee treatment), in the 4-consult experiment subjects often took the full 180 seconds (34 out of 90 trades) while 60 out of the total of 90 trades took place after at least 170 seconds had transpired. (The median number of seconds was 178). This is a dramatic difference and a possible additional source of inefficiency for the 4-consult experiments since in the real world, if the opportunity cost of time is positive, bargaining amongst agents who have the ability to consult may become a lengthy and drawn-out process.⁴

Section II of this paper presents the experimental design used to investigate our hypothesis. Section III presents a discussion of our results while Section IV offers a possible explanation for them. Section V offers some conclusions.

2. The Experiment and Experimental Design

The Experiment

Each experiment was run using New York University undergraduates as subjects and lasted 15 rounds. Subjects arrived in groups of four and were randomly assigned one of the four roles of seller principal, buyer principal, seller bargainer (agent) and buyer bargainer (agent) for the duration of the experiment. The principal and agent of each type (buyer and seller) were then paired for the duration of the experiment. In the 1-consult experiments, after reading the instructions, the seller pair and buyer pair were taken to separate rooms for the first pre-negotiation phase, which lasted two minutes. In the

⁴See Roth (1988) for a discussion of the deadline effect in bargaining.

sellers' room, the cost C of the object to be transacted in the first round was randomly generated from the distribution $G(C) = (C/100)^{0.4}$ defined over the interval $[0,100]$. This distribution skews the random cost realizations toward the low end of the support making it more likely that the seller will have a relatively low cost than a high cost. The seller principal then instructed the agent on the lowest acceptable price that the seller bargainer could accept in the bargaining phase. This price limit was binding on the agent since he was penalized severely if he violated these instructions.⁵ Simultaneously, in the buyers' room, the value V for the first round was drawn from a distribution $F(V) = 1 - [(100 - V)/100]^{0.4}$ defined on the interval $[0,100]$. This distribution skewed the random realizations for the buyer toward the upper end of the interval $[0,100]$, making high realizations more likely than low ones. Given the two prior distributions, the existence of beneficial trade was more the rule in these experiments than the exception. The buyer principal then instructed his agent on the highest acceptable price he or she was willing to pay for the good. (These distributions were identical to the ones used by Radner and Schotter (1989) and are representative of a one-parameter family of distributions.) Each pair was permitted to use any time remaining in the pre-negotiation phase as they wished, e.g., to discuss bargaining strategy, as long as they did not engage in threats or discussion of side payments.

⁵ The exact penalty structure was spelled out in the instructions and read as follows:
Penalties

If the negotiated price exceeds the buyer pair's highest acceptable price, then an amount will be subtracted from the final payoff of the buyer's bargainer equal to the difference between the value and the highest acceptable price (if the negotiated price is less than the value) or the difference between the negotiated price and the highest acceptable price (if the negotiated price exceeds the value). If the negotiated price is less than the seller pair's lowest acceptable price, then an amount will be subtracted from the final payoff of the seller's bargainer equal to the difference between the lowest acceptable price and the negotiated price (if the negotiated price exceeds the cost) or the difference between the lowest acceptable price and the negotiated price (if the negotiated price is less than the cost). These penalties ensure that it can never benefit a bargainer to violate the acceptable price limit specified by his principal."

After the pre-negotiation phase was over, each principal remained in his room while the two bargaining agents met in a third room for the bargaining phase. Bargainers had three minutes to agree on a price, and were permitted to bargain in any manner they wished barring physical threats, discussion of side payments or the disclosure of parameters (V , C or acceptable price limits). After the end of the bargaining phase each bargainer reported the outcome to his principal and round 2 began and was carried out in an identical manner to round 1. There were 15 rounds in each experiment.

Two sets of 1-consult indirect FFBEs were conducted, one in which bargainer payoffs were calculated using a percentage-fee system, and one that used a fixed-fee system. We refer to these experiments as the *percentage-fee FFBE* and the *fixed-fee FFBE*, respectively. In the percentage-fee FFBE, the principal received $2/3$ of the profits generated by the transaction for his pair and bargainers received $1/3$, where the seller-pair profits were $P - C$ and the buyer-pair profits $V - P$ (P is the negotiated price). In the fixed-fee FFBE, the principal received all of the profits and the bargainer received \$1.00 for each round in which a transaction was agreed to by the two bargainers, irrespective of the agreed-upon price. In addition, bargainers were penalized if they violated the price limits they agreed to with their respective principals; these penalties ensured that it was never in a bargainer's interest to violate his agreement with his principal. Payoffs, with the exception of the \$1.00 payments to bargainers under the fixed-fee system, were denominated in "francs"; francs were converted to dollars at the rate of one franc = \$.05 at the end of each experiment. In addition, each subject received a fixed payment at the end of the experiments; the fixed payments were set at levels that ensured that all four subject roles, on the average, were approximately equally remunerative. Appendix A presents the instructions for the indirect face-to-face bargaining experiments conducted here.

The 4-consult experiments were identical to the 1-consult experiments except for two features. First, we only ran the percentage-fee treatment and second, during the

negotiation phase of the experiments, at fixed intervals, bargainers were offered the opportunity to stop bargaining for 45 seconds and return to their principals for further instructions. More precisely, in the pre-negotiation phase, principals were able to instruct their bargainers at the 0 second point of the negotiation phase, before bargaining began. If the bargainer wished they could also consult at the 45 second, 90 second, and 135 second mark. Hence, there were a maximum of four consultations. We limited these consultations for many reasons. First, these are very time consuming experiments to run. The 4-consult experiments lasted almost three hours. Leaving the opportunity to consult as unstructured and unlimited opened the possibility of endless bargaining sessions. In addition, since our aim in data analysis was to look at concession rates, to facilitate comparisons across people and experiments, we wanted the time that concessions were made to come at identical points. Despite these added complications, however, student motivation in these experiments was high with bargaining very animated.

Experimental Design

Our experimental design is summarized in Table 1 below:

Table 1 : Experimental Design

<u>Experiment</u>	<u>Type of Bargaining</u>	<u>Source of Data</u>	<u>Number of Bargaining Pairs</u>	<u>Number of Rounds</u>
1)	Direct Face-to-Face	Radner, Schotter (1989)	10	15
2)	1-Consult Indirect Percentage Fee	Schotter, Snyder, Zheng (1995)	10	15
3)	1-consult Indirect Fixed Fee	Schotter, Snyder, Zheng (1995)	10	15
4)	Sealed-Bid Mechanism	Linhart, Radner, Schotter (1990)	10	15
5)	4-Consult Indirect Percentage Fee	Schotter, Snyder, Zheng (1995)	10	15

Note that two of the five experiments that we will be commenting upon have been performed previously, by either Radner and Schotter (1989) or Linhart, Radner and Schotter (1990), while three have been performed to demonstrate the point being made here. We wish to make comparisons between the indirect face-to-face bargaining mechanisms run here and the direct face-to-face and sealed-bid experiments run by Radner and Schotter(1989) and Linhart, Radner and Schotter (1990). The results of these comparisons are presented in our next section.

3: Results

Descriptive Results

The results of our two indirect FFBEs are summarized in Table 2 below. For purposes of comparison, we also included in Table 2 the results of the direct FFBE and of the anonymous bargaining experiment employing the sealed-bid mechanism (SBM). The data for the latter two experiments are taken from Radner and Schotter (1989) and Linhart, Radner and Schotter (1990), respectively. We will assess and compare the performance of the five bargaining mechanisms using various *inefficiency* measures. To explain these inefficiency measures define the potential gains from trade for a particular round as the difference between the value V to the buyer and the cost C to the seller. Provided that the value exceeds the cost, any price P such that $C < P < V$ would generate positive profits for both parties. One way of measuring the inefficiency of a bargaining experiment is the *disagreement rate*: the ratio of the number of rounds in which no agreement was reached despite positive potential gains from trade, to the total number of rounds in which potential gains were positive. An alternative measure, which we call the *inefficiency rate*, is the ratio of the sum of unrealized positive potential gains from trade to total positive potential gains.

We begin with an examination of the disagreement rates in the two 1-consult and one 4-consult indirect FFBE's and a comparison of their disagreement and inefficiency

rates as well as those of the direct FFBE of Radner and Schotter (1989) and sealed-bid experiments of Linhart, Radner, and Schotter (1990).

Table 2

Table 2 indicates that the introduction of bargaining agents results in a sharp rise in the disagreement rate: from 6% in the direct FFBE to 16% and 14% in the percentage-fee FFBE and fixed-fee FFBE's, respectively, and 25% in the 4-consult indirect FFBE. (Values and costs were generated by the same probability distributions in all of these experiments.) A set of Wilcoxon tests run to test for differences between direct and indirect FFBE's indicates a significant difference between disagreement rates in the direct FFBE and all (1-consult and 4-consult) indirect FFBE's yet finds no difference between any of the indirect FFBE (at the 5% significance level). The 14%, 16%, and 25% rates are also well above the 4% and 6% rates reported for the "ultimatum" games given in Roth and Prasnikar (1990) and for the similar face-to-face bargaining mechanisms in other studies cited there. It is interesting to note that our results for 1 and 4-consult indirect FFBEs fall within the range of disagreement frequencies (11% to 49%) observed in the studies cited by Prasnikar and Roth (1990). However, only the disagreement rates for the 4-consult indirect FFBE's approach the 29% disagreement rate observed in the Linhart, Radner, and Schotter (1990) sealed-bid experiments.

It is somewhat surprising that the 1-consult percentage-fee and fixed-fee indirect bargaining mechanisms had such similar disagreement rates. Under the fixed-fee system, bargainers' payoffs are affected only by the number of transactions completed and not by the gains from trade. Thus, at least if the repeated-game elements of the experiment are ignored, the fixed-fee system creates divergent incentives for the principal and bargainer within each pair. We speculate that, in order to prevent his bargainer from being too

Table 2: Disagreement and Inefficiency Rates

Experiment Type	Number of Observations	Disagreement Rate (%)			Inefficiency Rate (%)		
		1st 7 rounds	Last 8 rounds	Total	1st 7 rounds	Last 8 rounds	Total
4-Consult Indirect FFBE	150	19	31	25	11	17	14
Percentage-fee 1-Consult indirect FFBE	150	19	14	16	8	7	7
Fixed-fee 1-Consult indirect FFBE	150	22	7	14	7	2	4
Direct FFBE	150	6	6	6	1	1	1
Sealed-bid Bargaining	150	25	33	29	13	14	13

"agreeable"—too willing to complete a transaction at prices unfavorable to the principal—the principal would impose a more restrictive price limit than under the percentage-fee system. But the more restrictive are the price limits (the higher the lowest acceptable price, or the lower the highest acceptable price), the more difficult it is for the bargainers to conclude agreements.⁶ On balance, therefore, we expected the disagreement rate to be higher under the fixed-fee system than under the percentage-fee system. In fact, the percentage-fee disagreement rate (16%) is slightly higher than the fixed-fee rate (14%). However, when the hypothesis that the disagreement rates for both 1-consult indirect FFBEs are the same was tested using the Wilcoxon test, this hypothesis could not be rejected at any reasonable significance level.

We now turn to a consideration of the second performance measure for bargaining experiments, the inefficiency rate, or the ratio of unrealized positive potential gains to total positive potential gains. The results for all five experiments are again listed in Table 2, and parallel the comparison of disagreement rates. The introduction of bargaining agents raised the inefficiency rate from 1% in the direct FFBE to 7% and 4% in the 1-consult indirect percentage-fee and fixed-fee experiments, respectively, and to 14% in the 4-consult indirect experiment. While the inefficiency rates for the 1-consult indirect FFBEs are still considerably below the 13% rate reported by Linhart, Radner, and Schotter (1990)

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A counter argument might be that agents bargaining under a fixed fee arrangement might want to build a reputation for getting good deals for their principals as the game is repeated. They would do this in order to get honest reservation prices or instructions from their principals. Consequently, including repeated game elements into our analysis might eliminate or at least diminish the differences expected between the behavior of agents in the fixed-fee and percentage-fee treatments. While this argument might have some merit, we must realize that it is hard for both the buyer and seller agents to simultaneously build strong reputations since the reputation building game is a zero-sum game between them. The principal whose agent has failed to build a strong reputation might be tempted to continue to strategize so as to get favorable outcomes for him or herself. On these ground we might expect differences to persist.

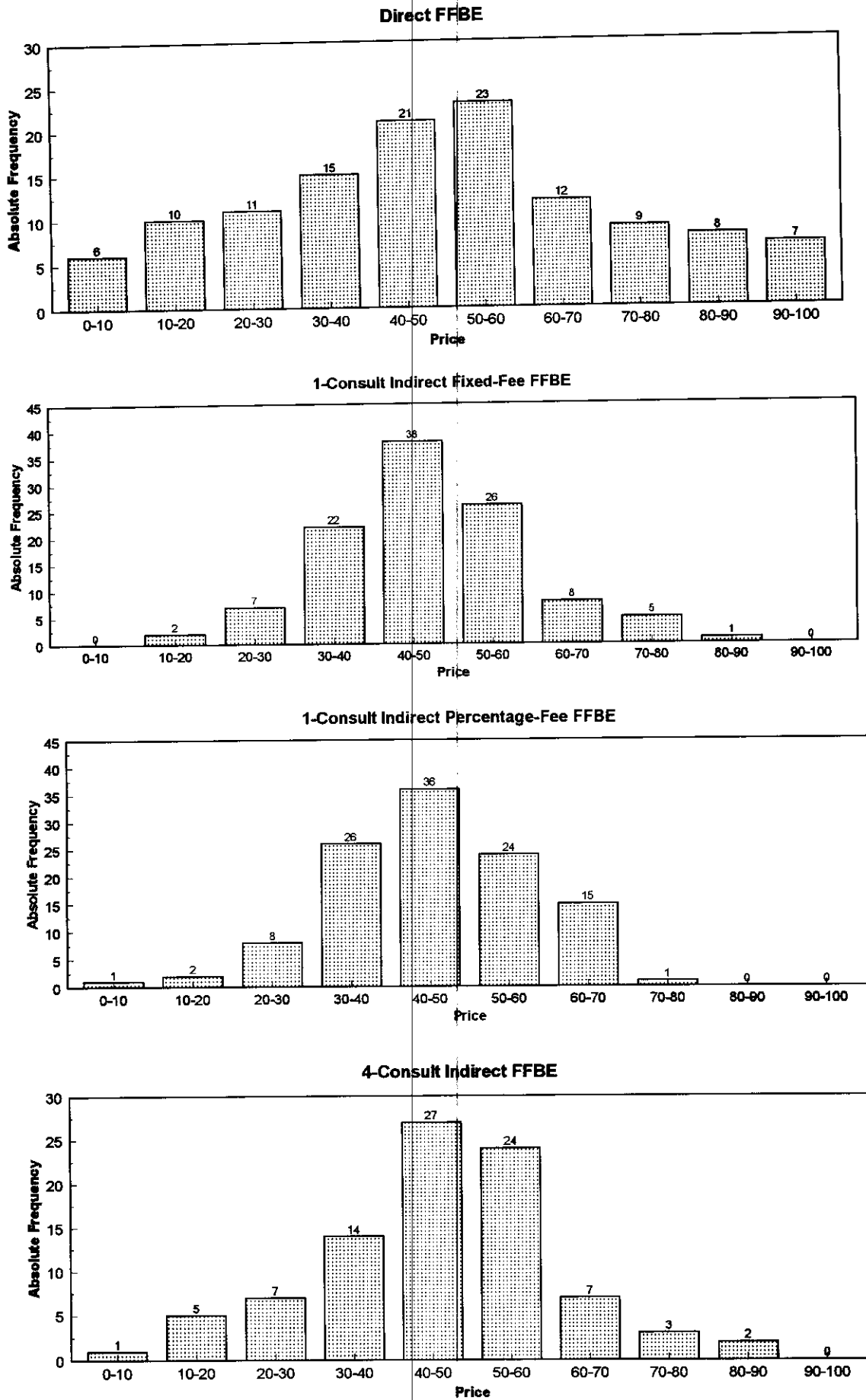
for the sealed-bid mechanism, the inefficiency rate for the 4-consult experiment (14%) was practically identical. This is the first of what will be many similarities between the 4-consult and sealed-bid experiments. Again, at the 5% level of significance while no difference was seen between the inefficiency rates of the 1-consult, 4-consult, and sealed-bid inefficiencies, all of these experiments exhibited inefficiency rates which differed significantly from those seen in the direct FFBE using a series of pair-wise Wilcoxon tests.

An interesting feature of the direct FFBE reported in Radner and Schotter (1989, p. 210) is the high variance of prices formed. However, with the introduction of bargaining agents the price variance declined dramatically (more than three-fold, from 546 in the direct FFBE to 160 and 158 in the 1-consult percentage-fee and fixed-fee FFBEs and two-fold from 546 to 230 in the 4-consult experiment). The range of prices formed was narrowed and the actual prices formed were more concentrated in the center of the interval. This is not very surprising when account is taken of the price limits imposed by the principals; these limits restrict the behavior of the bargainers in such a way as to make any price in either end of the price range highly unlikely. The mean price formed centered around 50: 50.4 for the direct FFBE, 47.2 for the 1-consult indirect percentage-fee FFBE, 48.2 for the 1-consult indirect fixed-fee FFBE, and 47.8 for the 4-consult experiment. The histograms of negotiated prices for the four experiments are presented in Figure 1.

FIGURE 1 HERE

The problem of rationalizing observed inefficiency would be less serious if disagreements generally occurred only when the gains from trade were minimal. Indeed, this is the case in experiments when bargaining agents are absent. In the direct FFBE, among eight non-trade rounds in which the value for the buyer pair is larger than the

Figure 1: Histograms of Negotiated Prices



cost for the seller pair, only two rounds had potential gains greater than 10; the rest had potential gains ranging only from 0.3 to 5.2. However, matters are different in the 1-consult and 4-consult indirect FFBEs. Under each system for bargainer payoffs, most non-trade rounds had potential gains exceeding 10. The highest uncaptured gains were 60 and 39 under the 1-consult percentage-fee and fixed-fee systems, respectively, and 97 for the 4-consult experiment. Hence, while direct face-to-face bargaining fails to make beneficial trades only when the benefits of such trades are minimal, indirect face-to-face bargaining mechanisms are capable of failing even when the gains to trade are substantial.

A Possible Explanation:

1-Consult Experiments

The results discussed above establish that the introduction of bargaining agents into FFBEs results in non-negligible levels of inefficiency comparable to those observed in field studies of real-world bargaining processes. They do not offer an explanation of why this occurs, however. It is our belief that the key to understanding why disagreement rates are higher for indirect FFBEs than direct ones comes from the fact that principals treat these mechanisms as if they were sealed-bid non-cooperative mechanisms and act in a strategic manner when giving instructions to their agents on how to bargain. In short, the sealed bid-mechanism is a type of commitment game in which the bids made by principals are irrevocable commitments which are fed into the second stage price-forming mechanism. Hence, just like the Crawford (1982) model, both models are capable of generating disagreements. It is the incompatible commitments made by principals that accounts for the higher inefficiency and disagreement rates in our experiments. To understand this argument, let us quickly review the sealed-bid bargaining mechanism.

The sealed-bid mechanism

While space prevents us from describing the sealed-bid mechanism in great detail (see Leininger, Linhart, and Radner (1989) for a full analysis of the mechanism) we will

present its major features here. The sealed bid mechanism is a mechanism with an extremely simple structure. Assume that a potential buyer, B , and a potential seller, S , are bargaining over the terms of a possible trade of a single object. If the object is traded, the value to B is V and the cost to S is C . (The seller incurs no cost if there is no trade.) The *sealed-bid mechanism* works as follows: B and S simultaneously choose bids, v and c , respectively. If $v \geq c$, then the trade takes place, and B pays S the price $P = kv + (1-k)c$, $0 \leq k \leq 1$; i.e., the price for the good is the weighted average of the bid and ask made by the buyer and the seller. Note that the greater is k , the more influence the buyer's bid has on the determination of the price of the good given that $v \geq c$. That would imply that high k 's are disadvantageous to the buyer since he or she would prefer as low a price as possible or a price as close to the seller's ask as possible. If $v < c$, then no trade takes place and B pays S nothing.

Suppose that at the time of bidding, B knows V but not C , and S knows C but not V . The situation is modelled by supposing that V and C are random variables drawn independently from the distributions $F(V)$ and $G(C)$ described in Section 2 ($F(V) = 1 - [(100 - V)/100]^{0.4}$ and $G(C) = (C/100)^{0.4}$) defined over the interval $[0,100]$. B 's strategy is a function β that determines his bid v for each value of V , and S 's strategy is a function γ that determines his bid c for each value of C . Thus

$$\begin{aligned} v &= \beta(V), \\ c &= \gamma(C). \end{aligned} \tag{3.1}$$

With the skewed prior distributions used in our experiments, the linear equilibrium bid functions are as follows when, again, $k = 0.5$.

$$v = \begin{cases} V, & V < 36, \\ 20.25 + 0.438V, & V \geq 36. \end{cases}$$

$$c = \begin{cases} C, & C > 64, \\ 36.00 + 0.438C, & C \leq 64. \end{cases} \quad (3.2)$$

It is our claim here that principals in indirect FFBEs view their situation as strategically identical to that faced by bidders in the sealed-bid mechanism. In the sealed-bid mechanism a strategy is a function from the set of random values (costs) to the set of bids (asks). Trade fails to be realized when the bids made by the buyer are less than the offers made by the seller and since this may occur when there are actually positive gains from trade existing (i.e., when the true value is greater than the true cost), the sealed bid-mechanism yields efficiencies which are typically less than first-best optimal efficiencies. In short⁷, the sealed-bid mechanism can be viewed as a black box into which bids and asks are placed and, depending upon the k used by the mechanism, out of which comes a price at which successful trade will occur.

Explaining 1-Consult Experimental Results

For principals, the indirect face-to-face bargaining mechanism appears to be identical to the sealed-bid mechanism. For them a strategy is an instruction function mapping their value (cost) realizations into the price limits (commitments) they place on their agents. Prices are formed in the black box of negotiations conducted by the agents. The assessment of the bargaining ability of their agents strictly defines the k which the principal believes characterizes the price formation process of the negotiations. If our

⁷Myerson and Satterthwaite have proven that the linear equilibrium to the sealed-bid mechanism yields the second-best efficiency levels which may be substantially below the first-best optimal levels.

analogy between the indirect bargaining mechanism and the sealed-bid mechanism is correct, we should observe two patterns in the data we have generated. First, we should observe trade inefficiencies in the indirect mechanism which mimic the patterns observed in the sealed-bid mechanism experiments of Linhart, Radner and Schotter (1990) in the sense that when inefficiencies occur they should do so because the instruction functions lead to sets of instructions for which no trade can be negotiated despite the fact that benefits from trade do exist (i.e., the highest acceptable price for the buyer's agent is below the lowest acceptable price for the seller's agent despite the fact that the buyer's value is greater than the seller's cost). Further, the instruction function observed should appear similar, at least qualitatively, to those bid functions observed in that experiment as well. Let us examine these two pieces of evidence one at a time.

In the sealed-bid mechanism inefficient disagreements can only occur when the bid submitted by the buyer is less than the offer made by the seller. In indirect FFBEs, inefficiencies can occur, despite potential gains from trade, for two reasons. First, the price limits placed on the buyer's agent may be less than the price limit place on the seller's agent (despite the fact that the true value may be greater than the true cost). As a result, unless agents violate the instructions of their principals, there is no room for trade to take place. This failure process is identical to the one that created inefficiencies in the sealed-bid mechanism. The other reason why beneficial trade may fail to take place in indirect face-to-face mechanisms is that, despite price limits that allow mutually beneficial trade to take place, negotiations break down for seemingly irrational reasons.

If our analogy between the sealed bid mechanism and indirect bargaining mechanisms is correct, then we should see breakdowns in efficiency of the first type only or at least in the vast majority of cases. An examination of the pattern of disagreements in the two indirect FFBEs offers support for our conjecture, since a substantial majority of disagreements, especially in the fixed-fee FFBE, occur when the price constraints are

binding. In fact, in 17 out of the 22 non-trade rounds with positive potential gains in the percentage-fee FFBE, and in 17 out of 18 such rounds in the fixed-fee FFBE, the seller's lowest acceptable price exceeded the buyer's highest acceptable price. That is, in only five out of 22 non-trade rounds in the percentage-fee FFBE and in only one out of 18 in the fixed-fee FFBE, would it have been possible for the bargainers to have concluded transactions without incurring penalties. (In the 4-consult experiments the same statistic is 10 out of 30.) This pattern is consistent with our hypothesis that inefficiencies result primarily because of the instruction behavior of principals rather than the bargaining behavior of agents.

It is harder to establish that the instruction functions used by our principals are similar to the bid functions observed by Linhart, Radner and Schotter (1990), or that they are even close to the theoretical linear bid functions predicted, in part, by the theory. In Linhart, Radner, and Schotter (1990) it was observed that bidders employed linear bid functions similar to those predicted by the theory although systematic deviations were found. The reason for these difficulties has to do with the fact that our current experiments only contain 15 observations per bargaining pair (they repeated the experiment only 15 times). These are skimpy data upon which to estimate a piece-wise linear bid function of the type depicted in equation 3.2 above. (In Linhart, Radner and Schotter there were 75 rounds.) Second, in the Linhart, Radner and Schotter (1990) experiments subjects knew the k that was being used to set prices while in our experiments principals would have had to estimate it and that estimate might come too late in the experiment to be observed in the pattern of instructions.

Despite these reservations there are some facts about the manner in which our 1-consult subject principals gave instructions which are consistent with our hypothesis. First, principal-agent theory would indicate that subject principals would give price limits to their agents which were identical to their true values and costs in the percentage-fee

indirect bargaining experiment but not in the fixed-fee experiments.⁸ More precisely, since the incentives of agents in fixed-fee experiments are simply aimed at making trades no matter what their price, we might expect principals to try to tie their hands by placing strict limits on the prices acceptable to them. In the percentage-fee experiments, however, the incentives of principals and agents are aligned and hence we would expect the principals to simply set their agents' price limits equal to their values or costs and instruct their agents to get the best price they can from the other agents. Therefore, principal-agent theory would predict instruction functions which are linear and with slope 1 in the percentage-fee experiments but whose slope is less than 1 in the fixed-fee experiments. (These instruction functions in the percentage-fee experiments need not, of course, be linear.) Second, our sealed-bid bidding hypothesis is consistent with instruction functions which have two properties: they are not of slope 1 (rather, they have slopes less than 1 if they are linear and at least have the property that principals constrain the prices they will allow their agents to accept) and these instruction functions should vary depending on the principal's estimate of the agent's bargaining strength. These features are expected to be true of both fixed-fee and percentage-fee experiments.

Table 3a presents estimates of linear regressions run for each buyer and seller in each 1-consult indirect experiment along with the mean of the estimated coefficients for these subjects.

TABLE 3 HERE

⁸When we refer to principal-agent theory here we mean the static theory governing a one-shot version of this experiment. There may be repeated game considerations here involving reputation building, that might alter these results.

Table 3a: 1-Consult Indirect Face-to-Face Bargaining: Linear Regression Results

1-Consult Percentage-Fee Indirect Face-to-Face Bargaining Experiment								
Pair Number	Seller Pairs				Buyer Pairs			
	α	β	Std Err	R ²	α	β	Std Err	R ²
1	12.13	0.86	0.06	0.94	1.31	0.92	0.04	0.98
2	10.32	0.86	0.03	0.99	3.87	0.76	0.15	0.65
3	2.91	0.96	0.02	0.99	11.23	0.52	0.12	0.58
4	27.88	0.60	0.08	0.80	-2.47	0.92	0.01	0.99
5	45.1	0.29	0.08	0.53	4.50	0.81	0.02	0.99
6	14.83	0.86	0.12	0.80	3.44	0.77	0.07	0.91
7	46.95	0.30	0.04	0.81	-1.00	1.00	0.00	1.00
8	2.90	0.97	0.03	0.99	8.45	0.61	0.16	0.54
9	15.18	0.80	0.11	0.80	3.24	0.83	0.05	0.96
10	3.57	0.95	0.04	0.98	4.19	0.63	0.04	0.96
Pooled	18.22	0.76	0.04	0.69	2.05	0.80	0.04	0.77
Mean	18.18	0.74	0.06	0.86	3.68	0.78	0.07	0.86
1-Consult Fixed-Fee Indirect Face-to-Face Bargaining Experiment								
Pair Number	Seller Pairs				Buyer Pairs			
	α	β	Std Err	R ²	α	β	Std Err	R ²
1	12.13	0.85	0.04	0.98	10.89	0.63	0.06	0.89
2	30.37	0.51	0.11	0.63	26.19	0.36	0.07	0.64
3	10.12	0.80	0.07	0.90	5.88	0.76	0.06	0.92
4	32.69	0.45	0.15	0.41	5.51	0.77	0.11	0.79
5	27.30	0.68	0.05	0.93	6.30	0.71	0.06	0.93
6	20.78	0.71	0.04	0.95	15.12	0.58	0.07	0.84
7	25.25	0.69	0.07	0.87	-2.35	0.89	0.06	0.95
8	18.34	0.71	0.06	0.92	7.95	0.54	0.03	0.96
9	26.90	0.71	0.07	0.90	4.85	0.83	0.04	0.97
10	19.87	0.72	0.06	0.91	15.39	0.56	0.09	0.74
Pooled	22.76	0.70	0.03	0.83	7.60	0.70	0.03	0.83
Mean	22.38	0.68	0.07	0.84	9.57	0.66	0.06	0.86

Table 3b: 4-Consult Indirect Face-to-Face Bargaining: Linear Regression Results

Group	Parameter	Seller Pairs				Buyer Pairs			
		0 Sec	45 Sec	90 Sec	135 Sec	0 Sec	45 Sec	90 Sec	135 Sec
1	α	32.10	28.87	23.77	16.01	-8.07	-7.50	-9.09	-3.87
	β	0.66	0.64	0.71	0.85	0.39	0.50	0.55	0.81
	R^2	0.63	0.63	0.81	0.86	0.28	0.28	0.38	0.82
2	α	58.03	48.54	42.31	32.70	15.55	16.21	20.50	28.69
	β	0.58	0.47	0.44	0.44	0.09	0.16	0.19	0.30
	R^2	0.54	0.65	0.68	0.57	0.04	0.10	0.16	0.37
3	α	5.13	5.25	4.67	4.67	2.51	14.46	12.30	12.30
	β	0.98	0.98	0.98	0.98	0.46	0.42	0.50	0.50
	R^2	0.94	0.93	0.94	0.94	0.53	0.64	0.66	0.66
4	α	73.51	70.04	60.16	46.17	4.40	8.85	5.61	2.39
	β	0.23	0.08	0.22	0.47	0.35	0.46	0.62	0.85
	R^2	0.16	0.02	0.17	0.34	0.81	0.85	0.95	0.91
5	α	19.18	17.50	17.17	17.17	9.06	4.47	4.47	2.61
	β	0.81	0.78	0.78	0.78	0.52	0.65	0.65	0.71
	R^2	0.84	0.83	0.82	0.82	0.91	0.88	0.88	0.87
6	α	61.98	59.34	46.58	40.42	-0.69	-1.01	8.84	8.84
	β	0.17	0.22	0.41	0.47	0.45	0.76	0.71	0.71
	R^2	0.21	0.39	0.69	0.67	0.28	0.41	0.54	0.54
7	α	77.04	65.33	61.65	37.46	6.51	14.97	13.18	8.78
	β	0.10	0.23	0.25	0.57	0.23	0.31	0.37	0.47
	R^2	0.25	0.61	0.55	0.89	0.23	0.31	0.45	0.62
8	α	30.25	26.62	24.41	15.24	17.99	16.14	14.62	24.46
	β	0.75	0.76	0.75	0.87	-0.03	0.05	0.08	0.04
	R^2	0.93	0.92	0.92	0.92	0.02	0.02	0.06	0.01
9	α	42.81	32.63	27.72	27.72	10.62	8.06	9.80	7.81
	β	0.37	0.54	0.60	0.60	0.34	0.52	0.56	0.62
	R^2	0.39	0.76	0.80	0.80	0.70	0.74	0.77	0.68
10	α	58.54	57.82	50.66	46.30	-0.27	0.62	1.26	4.09
	β	0.41	0.41	0.47	0.51	0.72	0.72	0.72	0.69
	R^2	0.32	0.31	0.47	0.61	0.95	0.94	0.94	0.94
Pooled	α	47.88	42.95	37.21	29.58	6.48	7.70	8.23	8.19
	β	0.47	0.49	0.54	0.64	0.33	0.44	0.48	0.57
	R^2	0.34	0.41	0.53	0.65	0.23	0.31	0.36	0.49
Mean	α	45.86	41.19	35.91	28.39	5.76	7.53	8.15	9.61
	β	0.51	0.51	0.56	0.65	0.35	0.45	0.49	0.57
	R^2	0.51	0.58	0.67	0.74	0.48	0.51	0.57	0.63

Table 3c: Sealed-Bid Bargaining: Linear Regression Results

Pair Number	Sellers				Buyers			
	α	β	Std Err	R ²	α	β	Std Err	R ²
1	51.89	0.41	8.01	0.79	-1.17	0.96	2.21	1.00
2	50.71	0.45	10.68	0.67	8.13	0.63	12.58	0.60
3	4.58	1.03	5.84	0.94	13.42	0.30	12.86	0.39
4	39.67	0.49	9.37	0.75	11.09	0.56	6.90	0.83
5	29.66	0.54	6.65	0.81	1.79	0.65	5.15	0.93
6	31.18	0.77	7.38	0.88	-1.27	0.89	10.50	0.86
7	22.96	0.58	27.93	0.30	15.99	0.67	7.61	0.82
8	35.59	0.46	7.25	0.80	44.08	0.01	8.65	0.00
9	30.08	0.56	5.64	0.84	-0.92	0.69	7.91	0.79
10	8.94	0.90	1.99	1.00	5.54	0.38	6.87	0.62
Pooled	30.30	0.62	15.53	0.57	8.25	0.59	15.34	0.51
Mean	30.53	0.62	9.07	0.78	9.67	0.57	8.12	0.68

As can be seen, in both 1-consult experiments the observed bid functions have slopes which are significantly different from unity. This is as true in the percentage-fee experiments as it is in the fixed-fee experiments, with mean slopes of .68 and .66 for seller and buyer subjects in the fixed fee experiment and .74 and .78 for sellers and buyers in the percentage-fee experiment. Note also that in the overwhelming majority of cases, the R^2 of these regressions is rather high indicating that linearity is an assumption that helps explain much of the variation in the data. Finally, when one tests whether these intercepts and slopes were different from regressions fit using the data of Linhart, Radner and Schotter (1990), where sealed-bid experiments were similarly run with 15 round horizons, and employing the same distribution functions as we used here (with $k = 0.5$), we find, using a Wilcoxon test, that there is no significant difference between either the slopes or intercepts of these instruction functions and the bid functions observed there at the 5% level of significance. From looking at the data one would not be able to recognize the institution which generated them.

Finally, one would expect that the instruction function used by our 1-consult bidders would vary according to their estimate of the k defined by their agents. For example, when k is estimated to be equal to 1 price is determined strictly by buyer's principal price instruction. If both buyer and seller principals share this estimate of k , then it is a dominant strategy for the seller to use an instruction function which is linear with a constant of 0 and a slope equal to 1, while the equilibrium instruction function for the buyer would involve shaving one's value and setting a price limit less than the value. This is true because such a price-setting mechanism shares all the properties of truth-revealing mechanisms for the seller in the sense that his or her bid does not affect the price of the transaction but only the probability of trade. Hence, truthful instructions are dominant. As k moves toward zero, the opposite occurs. Hence, we would expect an inverse relationship between the size of k and the slope of the buyer's instruction function and a positive

relationship between k and the slope of the seller's instruction function. (Note that k can be estimated for any pair of buyers and sellers by observing the price data knowing the bid and ask instructions of the principals. More precisely, we can estimate k by solving the equation $P = kv + (1-k)c$ for k , yielding $k = (P-c)/(v-c)$. For each experiment, we then estimated the average k by substituting the average negotiated price (over the 15 rounds of the experiment) for P , the average lowest acceptable price for c , and the average highest acceptable price for v . Appendix B presents our estimates of the average k for each pair of agents.)

Computation of this correlation substantiates our hypothesis. The correlation coefficient depicting the relationship of k and the buyers' bid function was -0.81 (significant at the 0% level) in the percentage-fee experiment and -0.14 (significant at the 72% level) in the fixed fee experiment. The correlation coefficients for the sellers were 0.77 (significant at the 1% level) and 0.32 (significant at the 37% level) for the percentage and fixed-fee experiments respectively. As we can see, there is a strong correlation between the slope coefficients of the subjects' instruction functions and the estimated mean value of k for each pair using the percentage fee mechanism. While the strength of the correlation is weaker in the fixed-fee case, in both cases the sign of the correlation is consistent with the logic presented above since the sign of the correlation between the buyer's bid function and k is negative while it is positive for the sellers.

On the basis of the data discussed above, there appears to be at least qualitative support for the hypothesis we have put forward that subjects in indirect bargaining experiments tend to behave in a manner that is similar to that observed in previous sealed-bid experiments.

4-Consult Experiments

Dynamic Instruction Functions and the Probability of Trade

While the 1-consult game played by the principals is basically static, the 4-consult game is inherently dynamic. Instructions given at the zero-second instant can be

changed at 45 seconds, and those given at 45 seconds can be changed at 90 seconds etc. Hence, in order to correctly analyze what happened in the 4-consult experiments we must look at the time path of instructions given and at the concession rates they imply. It is our hypothesis here, that the reason why the 4-consult experiment was so inefficient was that principals overly constrained their bargainers at the 0-second mark and then did not sufficiently relax these constraint as time went on. As a consequence, at the 135 second mark the bargainers were still to constrained to make a sufficient number of beneficial trades.

To be successful, this hypothesis must be consistent with two stylized facts presented by the data of this experiment. The first is that while in the 1-consult treatment the median length of time until a trade was consummated when one was 140 seconds for the Fixed-fee treatment and 110 for the percentage-fee treatment, in the 4-consult experiment subjects often took the full 180 seconds (34 out of 90 trades) while 60 out of the total of 90 trades took place after at least 170 seconds had transpired . (The median number of seconds was 178). This is a dramatic difference and a possible additional source of inefficiency for the 4-consult experiments since in the real world, if the opportunity cost of time is positive, bargaining amongst agents who have the ability to consult may become a lengthy and drawn-out process. Second, the efficiencies in the 4-consult experiment were almost identical with those of the sealed-bid experiment of Linhart, Radner and Schotter (1990) indicating that the instruction functions for the 135-180 second interval (where almost all trades took place) should be close to those seen in the sealed-bid experiment.

To substantiate our hypothesis we will proceed by first presenting the instruction functions for the buyer and seller principals in this experiment at the 0 second, 45 second, 90 second, and 135 second marks. Using these instruction functions, we will then illustrate how their shape and the manner in which they change over the horizon of the experiment

help explain the two-stylized facts stated above. When this analysis is over, we will present a table of what can be called mark-ups and mark-downs for all indirect experiments indicating the average concession that sellers and buyers were willing to make during the experiment. These markups and their change over time represent an alternative (yet consistent) analysis of our experiments results.

Figures 2a and 2b present the estimated 0-second, 45-second, 90-second, and 135-second instruction functions for buyers and sellers in the 4-consult indirect FFBE along with those of the sealed-bid experiment (Radner-Schotter (1989)). These instruction functions were estimated using a pooled sample of subject buyer and seller principals. (Table 3b and 3c present the actual regression results). Looking only at the 4-consult regressions for the moment, we clearly see that on average, buyer and seller principals place extremely tight constraints on their bargainers at the outset (0 seconds) of their negotiations, and then relax these constraints as time goes on. For example, a buyer with a value of 100 would, on average, instruct his bargainer to not buy for a price greater than 40 at the 0-second mark while at the 135 second mark he would relax this constraint to allow a price of approximately 65. Similarly, the seller's principal with a cost of zero would demand a price no less than 48 at the 0-second mark and would relax this constraint to permit a sale at about 30 at the 135 second mark. Note, however, how close the 135-second instruction function is to the sealed-bid instruction function of Linhart, Radner, and Schotter (1990). This indicates that at the end of the experiment, where almost all trades took place and where the 135 second instruction function is relevant, we should expect efficiencies in the 4-consult experiments to be almost identical to those of the sealed-bid experiment as our data in Table 2 indicates.

Figures 2a and 2b here

Figure 2a

Instruction Functions: 4-Consult and Sealed Bid Experiments
Buyers

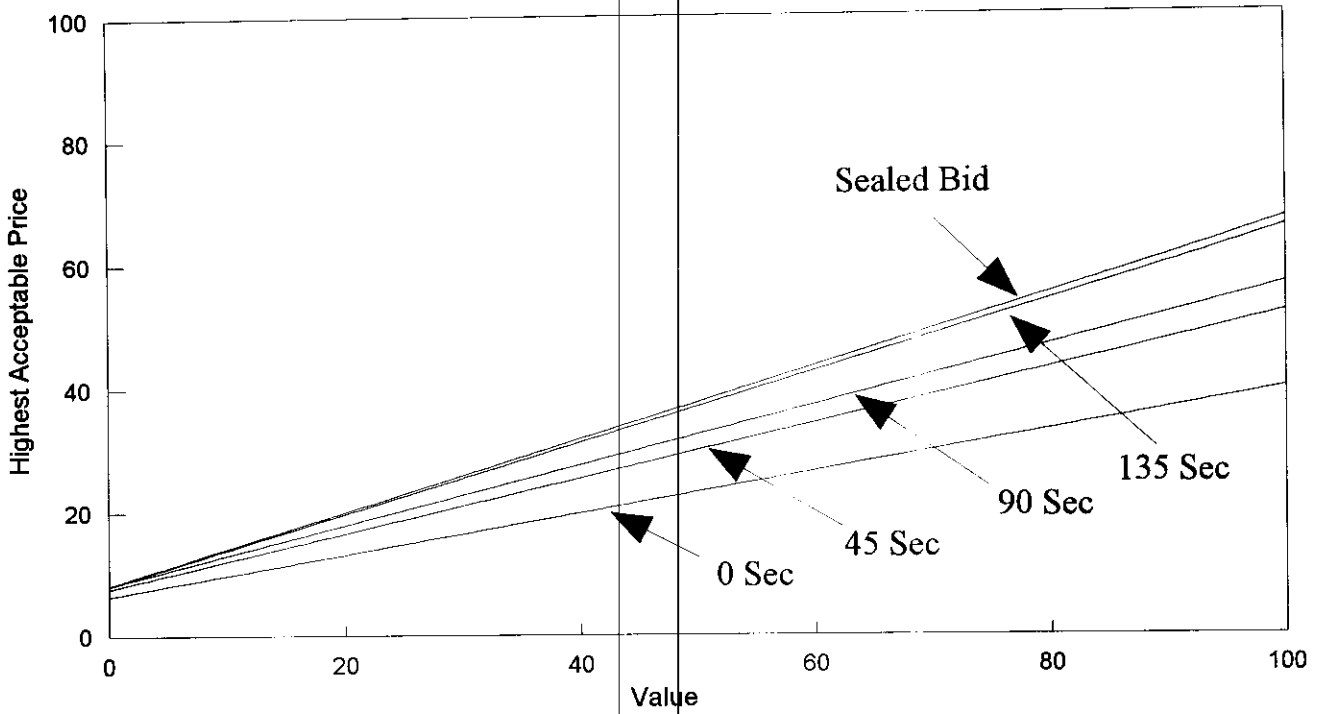
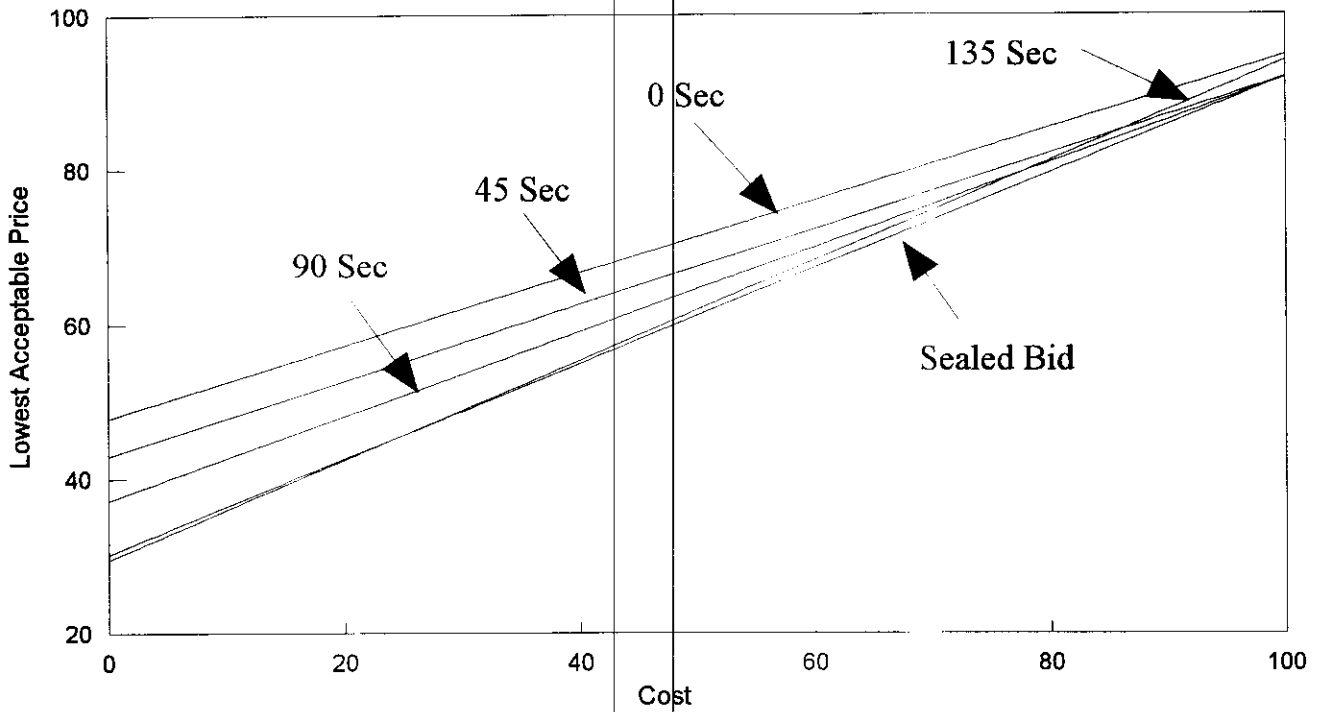


Figure 2b

Instruction Functions: 4-Consult and Sealed Bid Experiments
Sellers



To see the impact of these constraints on trade, we present Figure 3a -- 3d where we take the 0 second, 45 second, 90 second and 135 second instruction functions for buyer and seller principals and place them on the same graph. If trade occurred according to these instruction function, then we can clearly calculate the probability of trade during each time interval. For example, take the 0-second instruction functions presented in Figure 3a. Here we see that given the constraints of the buyer and the seller principal, no trade can occur since even if the seller drew a cost of zero and the buyer and value of 100, given these instructions, the buyer's maximum price would be lower than the seller's minimally acceptable price. To more carefully characterize the possibilities of trade given these instruction functions note that the best chance of trade occurring is when either the seller draws a cost of 0 or the buyer draws a value of 100. Knowing this, define V^* as the minimal value that a buyer could draw given that their seller opponent has drawn a 0 cost for which trade is possible. At 45 seconds (Figure 3b), given the instruction functions, we see that trade is only possible if either the buyer draws a value greater than 80 ($V^* = 80$). As we move to 90 seconds (Figure 3c) and 135 seconds (Figure 3d), we see that these crucial values (V^*) become approximately 61 and 35 for buyer. (similar calculations can be done for the seller by defining a comparable C^* , but for expository simplicity we will concentrate on the buyer). These critical values help us understand why the consummation of trade was delayed in these experiments as compared to the 1-consult experiments since it took time until there was even a sufficient feasible likelihood of trade.

Figures 3a-3d here

In fact, we can easily calculate the expected probability of trade at each stage given the instruction functions of the buyers and sellers and the assumed distribution functions from which values and costs are drawn. For example, in our experiments the

Figure 3a

Instruction Functions: 4-Consult Indirect FFBE at 0 Second

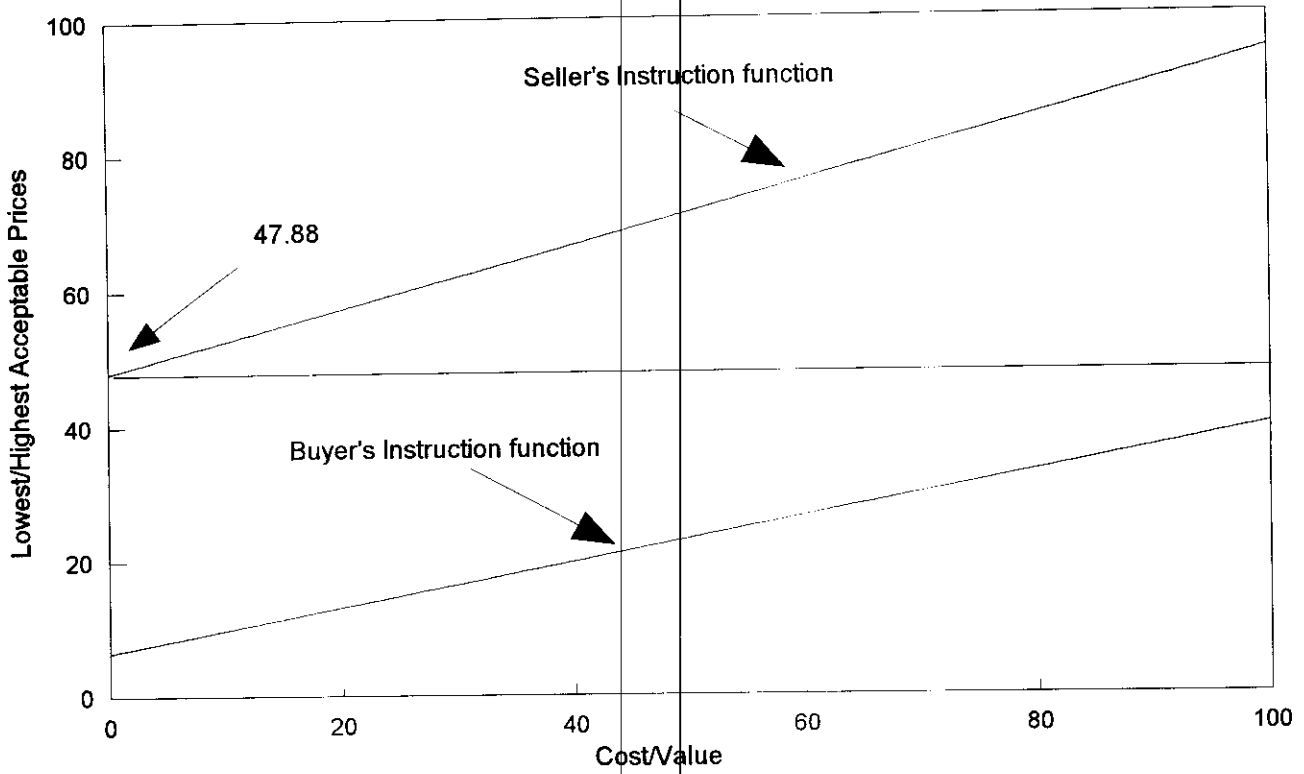


Figure 3b

Instruction Functions: 4-Consult Indirect FFBE at 45 Seconds

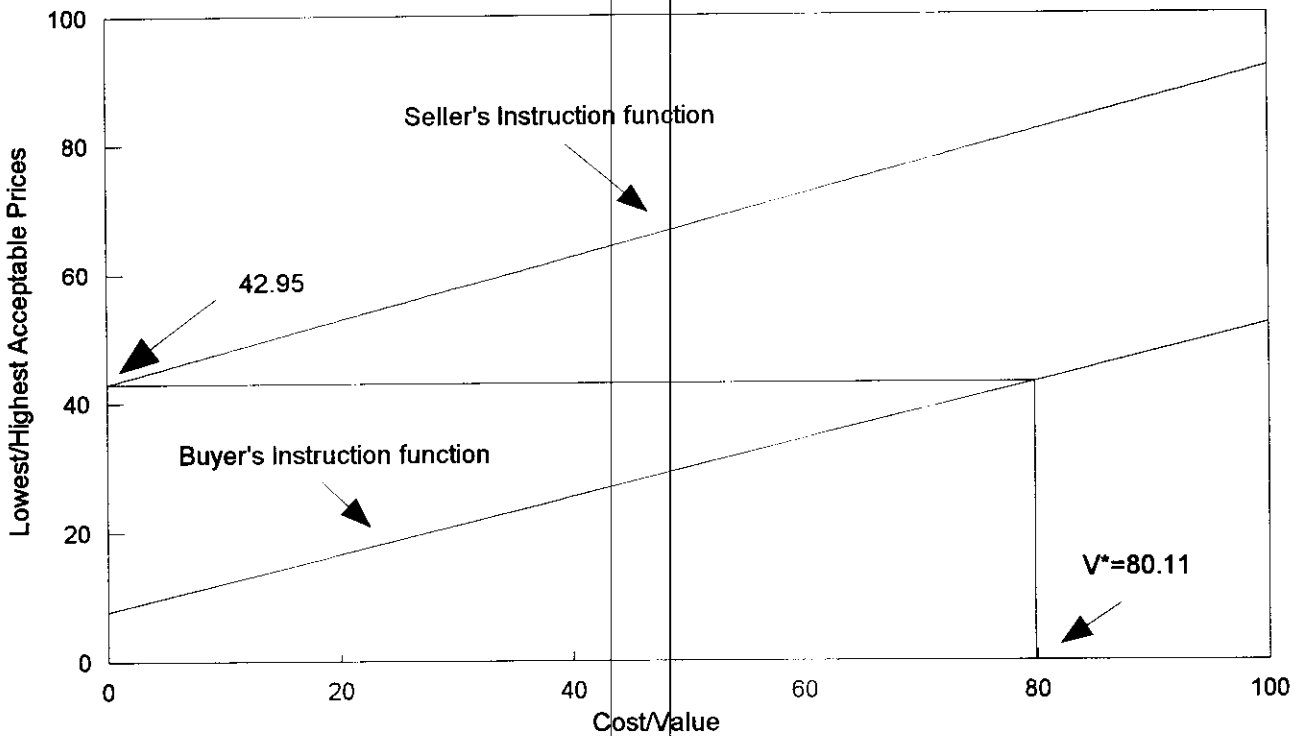


Figure 3c

Instruction Functions: 4-Consult Indirect FFBE at 90 Seconds

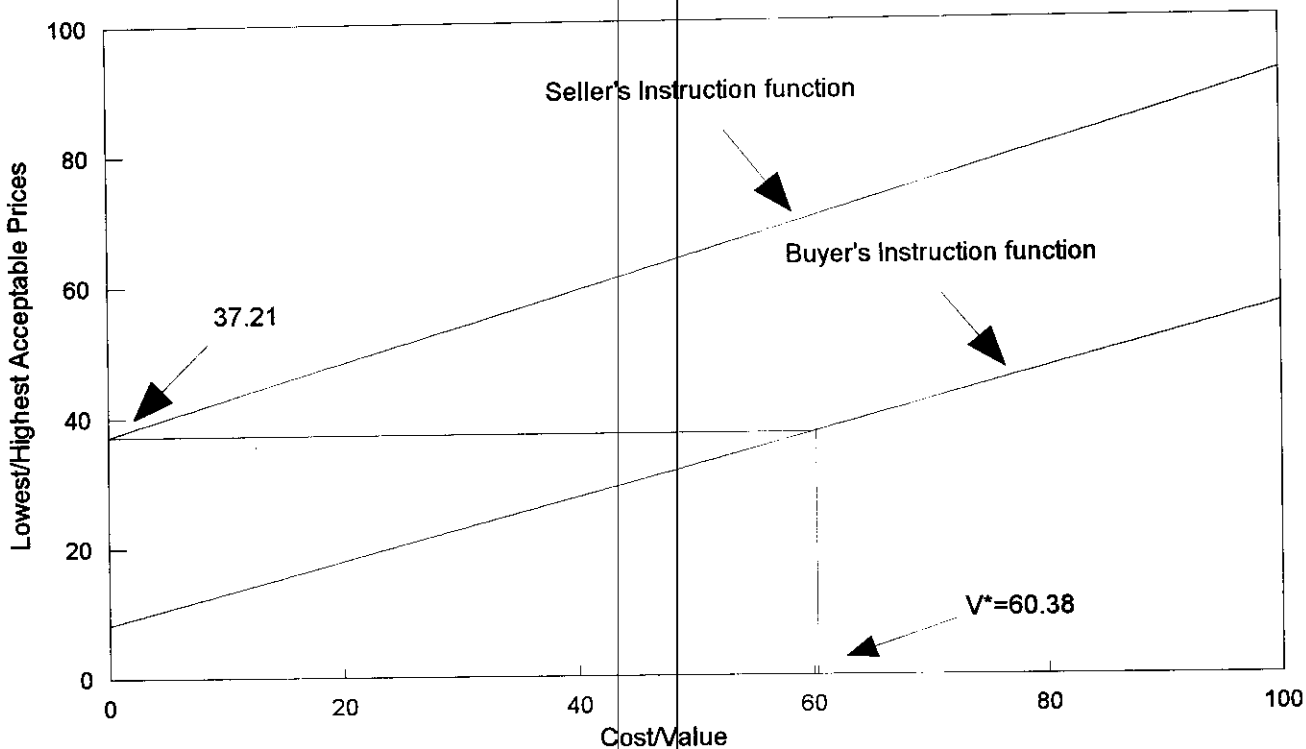
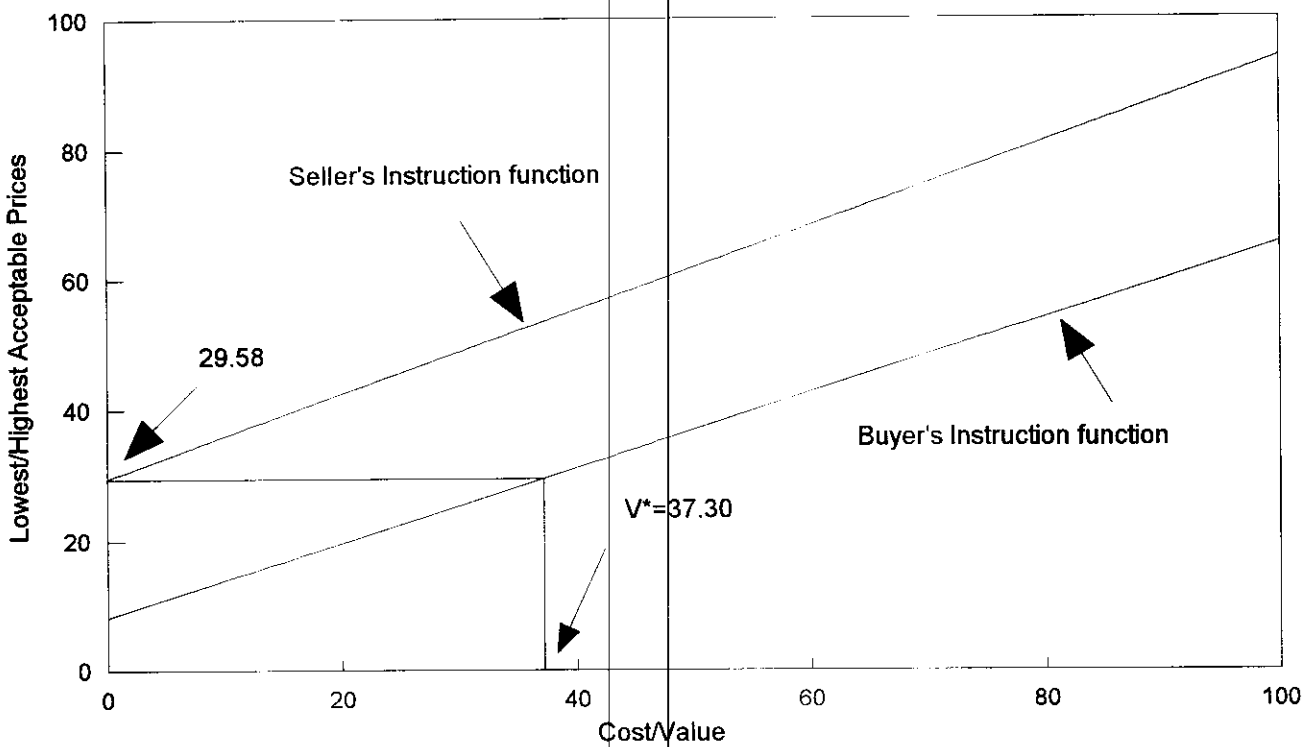


Figure 3d

Instruction Functions: 4-Consult Indirect FFBE at 135 Seconds



seller drew his/her cost from the distribution $(C/100)^{0.4}$ while the buyer drew his/her value from the distribution $(1 - (100-V)/100)^{0.4}$. Using the pooled estimated instruction functions, $v = \alpha_1 + \beta_1 V$ for the buyer and $c = \alpha_2 + \beta_2 C$ for the seller, we first calculate the conditional probability of trade at 0, 45, 90, and 135 seconds, respectively, conditional on the buyer drawing any value $V > V^*$. V^* is the lowest value for which the probability of trade is positive since it equates the instruction that a seller would give when he/she receives a zero cost to the instruction given by a buyer with value V^* i.e V^* equates $c = \alpha_2 + \beta_2 0$ with $v = \alpha_1 + \beta_1 V^*$ or $V^* = [(\alpha_2 - \alpha_1)/\beta_1]$. Hence the probability of trade at any $V > V^*$ is,

$$\begin{aligned} (\text{Probability of trade} | V) &= \int_0^{a+bV} (0.4/100)(c/100)^{-0.6} dC, & (4.1) \\ &\text{where } a = (\alpha_1 - \alpha_2)/\beta_2 \text{ and } b = \beta_2/\beta_1. \\ &= ((a+bV)/100)^4. \end{aligned}$$

The total probability of trade is then defined by integrating (4.1) with respect to V from V^* to 100 to yield (4.2):

$$(\text{Total Probability of Trade}) = \int_{V^*}^{100} ((a+bV)/100)^4 (0.4/100) ((100-V)/100)^{-0.6} dV \quad (4.2)$$

Table 4 presents these calculations by presenting the theoretical probability of trade at each time interval of the 4-consult experiment along with the same probabilities for the 1-consult and sealed bid experiments. In making these calculations, we used the estimates of the coefficients for α_1 , α_2 , β_1 , and β_2 presented using the pooled set of data for these experiments.

Table 4
Theoretical Probability of Trade

	Sealed-Bid Mechanism		1-Consult		4-Consult			
	Truth-Telling	Linear Equil.	Fixed-Fee	%-Fee	0-Sec.	45-Sec.	90-Sec.	135 Sec.
Theoretical Probability of Trade Given Instr. Functions	.85	.57	.66	.70	0	.22	.38	.55
Actual Probability of Trade	NA	.63 (95/150)	.73 (109/150)	.75 (113/150)	.03 (4/148)	.05 (7/148)	.07 (11/148)	.53 (78/148)

This table presents the theoretical probability of trade that would have occurred if subjects used the instruction functions estimated using the pooled set of observations generated by our 1-consult and 4-consult experiments and actually made trades whenever the instruction for the buyer was greater than that of the seller. For comparison we have also included the probability of trade for the Sealed-Bid Mechanism when bidders use either their linear equilibrium strategies or a truth-telling strategy. As we see, while trade could have occurred before the 135 second interval in the 4-consult experiments it tended not to. (Later we will attribute this failure of trade to the fact that subjects may have treated these rounds as cheap-talk rounds or posturing rounds and any instructions made here were not seriously considered.) However, we clearly see that our theory does predict monotonically increasing probabilities of trade as the 4-consult experiment progresses and probabilities in the last 135-180 second time interval which were not far away from those actually occurring (55% versus 53%). Hence, our hypothesis, that principals were over zealous at the 0-second mark and then insufficiently relaxed their constraints on bargainers as the experiment progressed is capable of explaining the two most salient stylized facts of the 4-consult experiment: That practically no trades took place in the [0,45] second

interval and the observed similarity between the sealed-bid inefficiencies and the results in the [135,180] second interval where the 135-second instruction function was used.

Mark-Ups, Mark-Downs, and Cheap Talk

Mark-Ups and Mark-Downs

Finally, we present an alternative presentation of our hypothesis using the notions of a mark-up and mark-down. We define a mark-up for seller j in period or round t as the percentage above cost demanded by the principal or $MU = (c_{jt} - C_{jt})/C_{jt}$, while the mark-down for buyer i in period or round t is the percentage below his value demanded or $MD = (V_{it} - v_{it})/V_{it}$. These mark-downs and mark-ups represent, in some sense, the toughness of the buyer and seller principals since they indicate how much below value or above cost they will allow their bargainers to settle for. Note that while $0 \leq MD \leq 1$, the mark-up is unbounded since c is unlimited. In fact, since demands greater than $V = 100$ can never be met, we would expect $0 \leq MU \leq (100 - C_j)/C_j$. Finally note that the mark-up is undefined when $C = 0$. Because of this, we will use in our calculations the average mark-up and mark-down for buyers and sellers defined as $AMU = \sum_t [(c_{jt} - C_{jt})/t] \cdot \sum_t C_{jt}/t$ and $AMD = \sum_t (V_{it} - v_{it})/t / \sum_t V_{it}/t$, where $t = 1, 2, \dots, 15$ is the time period of an experiment and i (j) is the buyer (seller).

Table 5 presents the average markups and mark-downs for sellers and buyers in the 4-consult experiment and the two 1-consult experiments.

Table 5
Average Mark-Ups and Mark-Downs:
4-Consult Indirect FFBE's

Experi- ment	Seller Mark-Up				Buyer Mark-Down			
	S0	S45	S90	S135	B0	B45	B90	B135
1	0.62	0.51	0.42	0.33	0.71	0.59	0.56	0.24
2	1.97	1.47	1.18	0.78	0.72	0.64	0.55	0.33
3	0.10	0.10	0.08	0.08	0.51	0.39	0.33	0.33
4	2.01	1.77	1.53	1.23	0.57	0.39	0.28	0.12
5	0.27	0.21	0.19	0.19	0.35	0.28	0.28	0.26
6	1.87	1.80	1.43	1.23	0.56	0.25	0.18	0.18
7	1.06	0.87	0.80	0.51	0.69	0.52	0.48	0.44
8	1.19	1.04	0.92	0.60	0.83	0.77	0.76	0.68
9	0.73	0.52	0.40	0.40	0.50	0.37	0.30	0.27
10	2.42	2.39	2.08	1.89	0.28	0.27	0.27	0.25
Average	1.22	1.07	0.90	0.73	0.57	0.45	0.40	0.31

Note: S0, S45, S90, S135 are the average markups for sellers at the 0, 45,90, and 135 second mark, while
 B0, B45, B90, B35 are the average markdowns for buyers at the 0, 45,90, and 135 second mark

Table 6
Average Mark-Up and Mark-Downs:
1-Consult Indirect FFBE's

Experiment	Fixed Fee Experiment Seller Mark-Up	Buyer Mark-Down
1	0.19	0.22
2	1.12	0.26
3	0.23	0.17
4	4.51*	0.13
5	0.93	0.19
6	0.52	0.20
7	0.47	0.14
8	0.62	0.33
9	0.40	0.11
10	1.01	0.24
Average	0.61*	0.20

Experiment	Percentage-Fee Experiment Seller Mark-Up	Buyer Mark-Down
1	0.28	0.50
2	0.23	0.54
3	0.08	0.69
4	1.01	0.43
5	2.12	0.37
6	0.43	0.48
7	0.97	0.35
8	0.07	0.59
9	0.42	0.55
10	0.09	0.63
Average	0.57	0.51

Note: * This observation is treated as an outlier in this sample and the average registered at the bottom omits it from its calculation.

Looking at the sellers first note that the toughest sellers in any experiments were the 0-second seller principals whose average mark-up was 1.22 as compared to the average mark-up in the fixed fee and percentage-fee experiments of 0.61 and 0.57 respectively. Over time these mark-ups decreased yet at 135 second mark, the average seller markup of the 4-consult principals was still higher than both the fixed-fee and percentage fee principals in the 1-consult experiment (.73 as compared to .61 and .57 respectively). For buyers the situation is slightly different. While the 4-consult buyers start out being tougher than their counterparts in the 1-consult experiments with an average mark-down of 0.57 as opposed to 0.20

and 0.51 for the fixed fee and percentage fee experiments, respectively, by the 135 second mark the 4-consult buyer principals were tougher than the fixed-fee buyer principals yet weaker than those of the percentage-fee experiment. Despite this fact, the 4-consult sellers principals were sufficiently tough at the end of their experiment to keep efficiency levels down.

Cheap Talk

Finally, since practically all trades in our 4-consult experiment occurred at the 180 second mark, one might be tempted to treat all instructions prior to that time as cheap talk or simple posturing. While this hypothesis can explain the delay in reaching a deal, it can not, of course, help us in explaining why the final inefficiency rate was similar to that of the sealed-bid experiment. In any case, to test this hypothesis we ran a Probit regression where the left hand variable was the probability of trade for a given pair and the right hand variables were the cost and value of the buyer and seller as well as their concessions (i.e for buyers, the difference between their value and their instruction (maximum acceptable price) or, for sellers, the difference between their cost and their instruction (minimal acceptable price) at the four concession intervals. The results of these regressions are presented in Table 7:

Table 7
Probit Regression Results:
Probability of Trade

Variable	Coefficient	Std Error	t-Statistic	Prob.
C	-0.505	0.738	-0.683	0.496
Con0B	-0.002	0.016	-0.144	0.886
Con45B	0.075	0.038	1.956	0.053
Con90B	-0.078	0.040	-1.957	0.053
Con135B	-0.027	0.016	-1.698	0.092
Value	0.051	0.014	3.702	0.000
Con0S	0.011	0.023	0.481	0.631
Con45S	0.021	0.034	0.634	0.328
Con90S	-0.024	0.032	-0.755	0.452
Con135S	-0.052	0.025	-2.080	0.040
Cost	-0.062	0.013	-4.866	0.000
Log Likelihood	-39.78375			
Obs with Dep = 1	90			
Obs with Dep = 0	30			

This cheap-talk hypothesis finds strong support for sellers where the only variable significantly affecting the probability of trade are the costs of sellers and their concessions at the 135 second mark. This obviously implies that whatever instructions they received prior to that point were irrelevant. For buyers, however, the concessions made at the 45 and 90 second marks appear significant at approximately the 5% level while the concessions made at the 135 second mark are only marginally significant at the 10% level. Clearly, since all concessions were monotonic, if on average, buyers conceded early in the experiment they need not go much further later in the experiment to be able to make a deal. In this sense it appears that sellers were tougher than buyers.

4: Conclusions

This paper has attempted to shed some light on the contradictory evidence of laboratory face-to-face bargaining experiments and real world experience with this same mechanism. The problem is that while face-to-face bargaining seems efficient in the lab, it appears to break down often in the real world. Our explanation for this discrepancy relies on

the fact that in the real world face to face bargaining is usually conducted not between principals but between the agents of principals, i.e., between fiduciaries like lawyers. Hence while people meet face to face and bargain, the fact that these bargainers are acting as agents for others leads to a breakdown of efficiency. In short, the efficiency of laboratory face-to-face bargaining, relative to real-world experience, is not an artificial aspect of a laboratory environment, but rather a result of the fact that in the real world bargaining is often done through agents and not directly by principals.

In addition, in the experiments we ran where agents could repeatedly consult with their principals, we noticed a dramatic lengthening of the time needed to consummate deals. This result implies that not only is bargaining through agents inefficient in terms of causing economic agents to miss otherwise profitable deals, but also tends to exhibit large transaction costs than bargaining where principals bargain face-to-face or where agents bargain with only one opportunity to consult. In the real world, such transaction costs can be substantial for principals when the hourly billing rates of lawyers are calculated.

To explain how this principal-agent relationship interferes with efficiency, we hypothesize that subject principals in our experiments treat the experiment as if it were a two-stage delegation game in which in stage 1 they instruct agents on how to play the stage-2 sealed-bid game. As a result, the principal placed restrictions on their bargaining that got in the way of efficiency. This hypothesis is supported by data on the type of bargaining breakdowns that occur as well as the shape of instruction functions each principal uses to instruct his agent. The greater inefficiency observed when agents have many opportunities to consult with their principals is explained in dynamic terms by observing that early on in such negotiations principals tended to over-constrain their bargainers leaving them little room to make beneficial deals until late in the bargain process, during the last 45 seconds.

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APPENDIX A

Percentage-Fee Indirect Face-to-Face Bargaining Experiment: INSTRUCTIONS

1-Consult

Introduction

You are about to engage in an experiment concerning decision-making. Various research institutions have provided funds for these experiments and if you make appropriate decisions you will earn a good monetary payment.

The Experiment

As you arrived, two things occurred. First, you were randomly arranged in pairs, and within each pair one of you has been designated as the **bargainer** and the other has been called the **principal**. Second, each pair has also been designated as either a **buyer pair** or a **seller pair**. Hence you may be either a **buyer principal**, a **buyer's bargainer**, a **seller principal**, or a **seller's bargainer**. Check the top of your instructions to see which one you are. You will be matched with the other member of your pair for the duration of the experiment. The buyer pair and seller pair will bargain repeatedly over the course of the experiment. In each round the buyer pair will be bargaining to buy an fictitious object from the seller pair; the seller pair will be bargaining to sell this object to the buyer pair. The fictitious object will be completely described by two numbers: its **value** to the buyer pair, and its **cost** to the seller pair. These numbers will be generated randomly, and independently in each round, in a manner to be described below.

After you read these instructions and the procedures of the experiment are described to you, the buyer pair will be placed in a separate room from the seller pair. Although the principals in these pairs will not meet again until the experiment is over, the bargainers will meet repeatedly to bargain with each other in a pre-designated third room. The experiment

will last for 15 rounds and each round will have two phases which we will call the **pre-negotiation phase** and the **bargaining phase**. The way the experiment works is as follows.

The Pre-Negotiation Phase

The first step is the generation of the random numbers defining the value of the object to the buyer pair and its cost to the seller pair. In each round, in the privacy of your room, the principal in each pair will be asked to choose a number from one to five, and then another number from one to 75. The experimental administrator will then read out the number from one of five lists each containing 75 numbers. The buyer pair's random number (value) represents the amount of a fictitious currency called "francs" we will pay the buyer principal if the buyer's bargainer is successful in purchasing the object during the bargaining phase of that round. The buyer pair's random number is the buyer principal's "redemption value" for the object. The seller pair's random number (cost) represents how much the seller principal will have to pay in order to obtain the object for sale to the buyer pair. The seller pair's random number is the fictitious object's "production cost" to the seller principal. Note that only your pair knows your random number and that you will not be told the random number of the other pair. After you observe your number, the principal will write it in column 1 of his worksheet.

After the buyer pair's value and the seller pair's cost are known to them, each pair will discuss the way the bargainer in that pair is to conduct the negotiations with the other bargainer during the bargaining phase. In this discussion the buyer principal and buyer's bargainer must agree on a **highest acceptable price**, or a price "ceiling". In the subsequent bargaining phase, the buyer's bargainer may agree on any price less than or equal to the highest acceptable price, but if the agreed-upon price is higher than the highest acceptable price then the buyer's bargainer will be penalized by having money subtracted from his final payoff. Similarly, the seller principal and seller's bargainer must agree on a **lowest acceptable price**, or a price "floor". The seller's bargainer, in the bargaining phase, may agree

on any price higher than or equal to the lowest acceptable price, but he will be penalized if he agrees to a price that is lower than the lowest acceptable price. These penalties ensure that it will never benefit a bargainer to violate the instructions of his principal. These acceptable prices will be written in column 2 of the principal's worksheet. Note that the bargainer on each pair will be bargaining for the principal and must follow the principal's instructions in order to make a positive payoff. If either the seller pair or the buyer pair cannot agree on an acceptable price within the two-minute pre-negotiation phase, that round of the experiment will end and all payoffs to all people will be zero for that round. The bargainer's payoff at the end of the experiment will not be identical to the principal's payoff. (Payoffs are explained in detail below.) The pre-negotiation phase will last two minutes and at this point the bargaining phase will begin.

The Bargaining Phase

After the pre-negotiation phase is over, the bargaining phase will begin by having the bargainers of each pair leave their respective rooms and go to a third room in which they will bargain. The bargaining rules are as follows. After each bargainer has observed the random number of his principal, and discussed bargaining strategy with his principal, the two bargainers will be given three minutes to discuss whether they want to make a transaction in this period of the experiment and if so at what price. They will be told when there are three minutes, two minutes, one minute and 30 seconds left in the period. The experimental administrator will also count down the seconds left from 10 to 0.

Payoffs

If a price is agreed on within the three minutes, the franc payoff to the buyer pair will be

$$\text{Buyer franc payoff} = \text{value} - \text{price},$$

while the seller pair's franc payoff will be

$$\text{Seller franc payoff} = \text{price} - \text{cost}.$$

For example, say the buyer pair receives a value of 60 while the seller pair receives a cost of 20. If a price of 50 is negotiated, the payoffs will be

$60 - 50 = 10$ for the buyer pair, and

$50 - 20 = 30$ for the seller pair.

If the price agreed on is 30, then the payoffs will be

$60 - 30 = 30$ for the buyer pair, and

$30 - 20 = 10$ for the seller pair.

Note that the payoff to a buyer pair can be negative if and only if the buyer's bargainer agrees on a price which is greater than the value. Similarly, the payoff to a seller pair can be negative if and only if the seller's bargainer agrees on a price which is less than the cost.

If no price is agreed on during the three minutes, i.e. if time runs out before a price is agreed on, all players' payoffs will be zero for that period. We will wait until the three minutes are over before proceeding to the next round if no price is agreed on. If both the buyer's bargainer and the seller's bargainer agree on a price, you must tell the experimental administrator sitting with you that an agreement is reached.

For each round in which a price agreement is reached, and in which the negotiated price is not greater than the highest acceptable price determined in the buyer pair's pre-negotiation phase, the buyer principal will receive $2/3$ of the payoff to the buyer pair and the buyer's bargainer will receive the remaining $1/3$. If the negotiated price exceeds the highest acceptable price, the buyer principal will receive all of the payoff to the buyer pair for that round; the buyer's bargainer will receive no payoff for that round, and in addition the buyer's bargainer will have a penalty subtracted from his final payoff at the end of the experiment. Similarly, for each round in which a price agreement is reached and in which the negotiated price is not less than the lowest acceptable price determined in the seller pair's pre-negotiation phase, the seller principal will receive $2/3$ of the payoff to the seller pair and the seller's bargainer will receive the remaining $1/3$. If the negotiated price is less than the lowest acceptable price, the seller principal will receive all of the payoff to the seller pair for that round; the seller's bargainer will receive no payoff for that round and will have a penalty subtracted from his

final payoff. The formula for these penalties is given below. The important point, however, is simply that it will never be in the interest of a bargainer to violate the instructions of his principal.

Once either a trade has been made or the three minutes of the round are over, this round will end. The bargainers will then return to their principals and report to them whether or not a trade was made and if so at what price. The principal will write in column 3 of his worksheet whether a transaction has been agreed to in period 1 by writing yes or no. In column 4 he will write the price and in column 5 the payoff. If no trade occurred, zeros should appear in columns 4 and 5.

After period 1 is over, you will proceed to period 2 by generating your new random numbers (cost for the sellers, value for the buyers), and then engage in another two-minute pre-negotiation phase and another three-minute bargaining phase. There will be 15 rounds. At the end we will convert your franc payoff into dollar payoffs in a manner to be described below.

In the discussions of the bargainers certain rules will be strictly enforced by the experimental administrator sitting with you. If they are violated, the experiment will be terminated and the violating person given a minimal payoff.

(1) A bargainer must not announce his random number (cost or value) or price limit to the other bargainer, nor may he take a piece of paper on which these numbers are written, into the bargaining room.

(2) No personal threats or abusive language will be tolerated, nor any disparaging remarks about the actions of other persons engaged in the experiment.

(3) In each period of the experiment you may only discuss the price of the fictitious good being sold in *that* period. For instance, no price arrangements about objects to be sold in later rounds may be discussed. You must discuss each object separately.

(4) Finally, no discussion of payments outside of the experiment will be tolerated. For instance, you may not say "if you agree to such and such price in this period I will pay you \$X after we leave the experiment."

Penalties

If the negotiated price exceeds the buyer pair's highest acceptable price, then an amount will be subtracted from the final payoff of the buyer's bargainer equal to the difference between the value and the highest acceptable price (if the negotiated price is less than the value) or the difference between the negotiated price and the highest acceptable price (if the negotiated price exceeds the value). If the negotiated price is less than the seller pair's lowest acceptable price, then an amount will be subtracted from the final payoff of the seller's bargainer equal to the difference between the lowest acceptable price and the negotiated price (if the negotiated price exceeds the cost) or the difference between the lowest acceptable price and the negotiated price (if the negotiated price is less than the cost). These penalties ensure that it can never benefit a bargainer to violate the acceptable price limit specified by his principal.

Final Payoffs

When the 15 rounds of the experiment are over, the final payoffs will be determined by adding all of the francs accumulated over the 15 rounds of the experiment. These francs will be converted into dollars at the rate of 1 franc = \$.05. Each bargainer will also receive a fixed payment of \$6.00 for participating, while each principal will receive a fixed payment of \$3.00. If by the end of the experiment a bargainer's total franc payoff is negative, these losses will be subtracted from the fixed payment of the principal. You will be paid when you leave. If any rules are violated, the violating party will receive the fixed payment of \$3.00 minus any losses that have occurred while the other person will be paid by calculating his average payoff up until the violation and then multiplying it by 15.

Random Numbers

The random numbers on the lists held by the experimental administrators were generated independently depending upon whether you are a buyer or a seller. What we mean by a "random number" can be explained by looking at Figures 4 and 5.

FIGURE 4 HERE

Figure 4 describes how the random values of the buyers were generated. The graph shows possible values between 0 and 100 on the horizontal axis and a probability number on the vertical axis. The probability number tells you how likely it is that a buyer will receive a random number (value) less than or equal to any particular number. For instance, this graph indicates that the probability that a buyer will receive a value less than or equal to, say, 25 is .108 (while the probability that the buyer receives a value *greater* than 25 is .892). Similarly, the probability that the buyer receives a value less than or equal to 50 is .242 (while the probability that the buyer receives a value greater than 50 is .758). The probability that the buyer receives a value of 75 or less is .425 (the probability of receiving a value greater than 75 is .575). Table 5 gives you more information about the way your random values are generated if you are a buyer. A point to note is that if you are a buyer the likelihood of getting a "high" (greater than 50) value is substantially greater than the probability of getting a "low" value (i.e. a value less than 50). In fact, the probability that a buyer will receive a value less than 50 is only .24.

TABLE 5 HERE

For a seller, just the opposite is true, as Figure 5 illustrates. Here we place all possible seller costs between 0 and 100 on the horizontal axis and a probability number of the vertical axis. Again the probability number defines the probability that if you are a seller you will receive a cost less than or equal to any particular cost. For instance, the probabilities that a seller will have a cost less than either 25, 50, or 75 are .575, .758, and .891, respectively. Table 6 below lists other such probabilities.

FIGURE 5 HERE

TABLE 6 HERE

A point to note is that, as opposed to the buyers, sellers have a greater chance of getting a low number (i.e., a cost less than or equal to 50) than a high number (greater than 50). In fact, the probability that a seller will receive a cost less than 50 is .757.

To give you a feel for these numbers the experimental administrator will generate a few illustrative random numbers from the sellers' and buyers' lists, respectively. Since the numbers on the lists were generated in a manner consistent with the graphs described above, they should familiarize you with the random numbers you could draw. After this is done, we will begin our experiment.

Instructions for Fixed-Fee Indirect Face-to-Face Bargaining Experiment:

The instructions for the fixed-fee FFBE are identical to those for the percentage-fee FFBE except for the following changes:

(1) In place of the section entitled "Payoffs" in the percentage-fee instructions, the fixed-fee instructions contain two sections entitled "Payoffs to Principals" and "Payoffs to Bargainers."

(2) The "Payoffs to Principals" section of the fixed-fee instructions is identical to the first four paragraphs of the "Payoffs" section of the percentage-fee instructions, except that references to the "buyer pair" or "seller pair" are replaced by references to the "buyer principal" or "seller principal," respectively.

(3) The first paragraph of the "Payoffs to Bargainers" section of the fixed-fee instructions reads as follows:

"For each round in which a price agreement is reached, the buyer's bargainer and the seller's bargainer will each receive a fixed payment of \$1.00, provided that the agreed-upon price does not violate the acceptable price limits specified by the principals for that round.

Otherwise, the bargainer who violated his principal's price limit will receive no payment for that round and will also have penalties subtracted from his final payoff. The formula for these penalties is given below. The important point, however, is simply that it will never be in the interest of a bargainer to violate the instructions of his principal."

(4) The remainder of the "Payoffs to Bargainers" section of the fixed-fee instructions is identical to the last seven paragraphs of the "Payoffs" section of the percentage-fee section.

(5) In place of the first two sentences of the section entitled "Final Payoffs" in the percentage-fee instructions, the corresponding section of the fixed-fee instructions contains the following three sentences:

"When the 15 rounds of the experiment are over, the principals' final payoffs will be determined by adding all of the francs accumulated over the 15 rounds of the experiment. These francs will be converted into dollars at the rate of 1 franc = \$.04. Each of the bargainers will receive \$1.00 for each round in which an acceptable price was agreed upon."

INSTRUCTIONS

4-Consult Indirect Face-To-Face Bargaining

Introduction

You are about to engage in an experiment concerning decision-making. Various research institutions have provided funds for these experiments and if you make appropriate decisions you will earn a good monetary payment.

The Experiment

As you arrived, two things occurred. First, you were randomly arranged in pairs, and within each pair one of you has been designated as the **bargainer** and the other has been called the **principal**. Second, each pair has also been designated as either a **buyer pair** or a **seller pair**. Hence you may be either a **buyer principal**, a **buyer's bargainer**, a **seller principal**, or a **seller's bargainer**. Check the top of your instructions to see which one you are. You will be matched with the other member of your pair for the duration of the experiment. The buyer pair and seller pair will bargain repeatedly over the course of the experiment. In each round the buyer pair will be bargaining to buy an fictitious object from the seller pair; the seller pair will be bargaining to sell this object to the buyer pair. The fictitious object will be completely described by two numbers: its **value** to the buyer pair, and its **cost** to the seller pair. These numbers will be generated randomly, and independently in each round, in a manner to be described below.

After you read these instructions and the procedures of the experiment are described to you, the buyer pair will be placed in a separate room from the seller pair. Although the principals in these pairs will not meet again until the experiment is over, the bargainers will meet repeatedly to bargain with each other in a pre-designated third room. The experiment will last for

15 rounds and each round will have two phases which we will call the **pre-negotiation phase** and the **bargaining phase**. The way the experiment works is as follows.

The Pre-Negotiation Phase

The first step is the generation of the random numbers defining the value of the object to the buyer pair and its cost to the seller pair. In each round, in the privacy of your room, the principal in each pair will be asked to choose a number from one to five, and then another number from one to 75. The experimental administrator will then read out the number from one of five lists each containing 75 numbers. The buyer pair's random number (value) represents the amount of a fictitious currency called "francs" we will pay the buyer principal if the buyer's bargainer is successful in purchasing the object during the bargaining phase of that round. The buyer pair's random number is the buyer principal's "redemption value" for the object. The seller pair's random number (cost) represents how much the seller principal will have to pay in order to obtain the object for sale to the buyer pair. The seller pair's random number is the fictitious object's "production cost" to the seller principal. Note that only your pair knows your random number and that you will not be told the random number of the other pair. After you observe your number, the principal will write it in column 2 of his worksheet.

After the buyer pair's value and the seller pair's cost are known to them, each pair will discuss the way the bargainer in that pair is to conduct the negotiations with the other bargainer during the bargaining phase. In this discussion the buyer principal and buyer's bargainer must agree on a **highest acceptable price**, or a price "ceiling" which will be relevant during the first 45 seconds of the bargaining phase of the experiment. During this first 45 seconds of the bargaining phase, the buyer's bargainer may agree on any price less than or equal to the highest acceptable price, but if the agreed-upon

price is higher than the highest acceptable price then the buyer's bargainer will be penalized by having money subtracted from his final payoff. Similarly, the seller principal and seller's bargainer must agree on a **lowest acceptable price**, or a price "floor". The seller's bargainer, in the bargaining phase, may agree on any price higher than or equal to the lowest acceptable price, but he will be penalized if he agrees to a price that is lower than the lowest acceptable price. These penalties ensure that it will never benefit a bargainer to violate the instructions of his principal. These acceptable prices will be written in column 3 of the principal's worksheet where it says (0 sec), "Lowest Acceptable Price" or (0 sec. "Highest Acceptable Price", depending upon whether you are a buyer or a seller. Note that the bargainer on each pair will be bargaining for the principal and must follow the principal's instructions in order to make a positive payoff. If either the seller pair or the buyer pair cannot agree on an acceptable price within the two-minute pre-negotiation phase, that round of the experiment will end and all payoffs to all people will be zero for that round. The bargainer's payoff at the end of the experiment will not be identical to the principal's payoff. (Payoffs are explained in detail below.) The pre-negotiation phase will last two minutes and at this point the bargaining phase will begin.

The Bargaining Phase

After the pre-negotiation phase is over, the bargaining phase will begin by having the bargainers of each pair leave their respective rooms and go to a third room in which they will bargain. The bargaining rules are as follows. After the pre-negotiations phase is over, the two bargainers will be given three minutes to discuss whether they want to make a transaction in this period of the experiment and if so at what price. If they reach an agreement before 45 seconds is up, they will each get a payoff as described in the

section below. If no agreement is reached, then at the 45 second mark the experimental administrator will give each bargainer the option of going back to their respective principals and consulting about the negotiations. At that point the principal and the bargainer can, if they wish, re-set their price ceilings or price floors. If either bargainer wants to consult or renegotiate their floor or ceiling, they will simply say so and the bargaining phase will stop for 45 seconds during which both bargainers are free to go back to the rooms of their principals and consult. If a new price floor or ceiling is determined, it will be written in column 4 of the principal's worksheet under (45 sec.) "Lowest Acceptable Price" etc.. If the floor or ceiling stays the same, write the word "same" or if your agent did not choose to consult you about new instructions, write "not consulted".

If neither bargainer wants to stop the negotiations, they will continue for another 45 seconds at which point if no agreement has been reached, they will again be offered a chance to stop and consult again and reset their price floor and ceiling. In total there will be four 45 second sub-periods in the bargaining phase with potential consulting periods at the 45 second, 90 second, and 135 second points. If no agreement is reached within 180 seconds (3 minutes) the bargaining phase will end and all payoffs will be zero.

Payoffs

If a price is agreed on at any time within the three minutes, the franc payoff to the buyer pair will be

Buyer franc payoff = value - price,

while the seller pair's franc payoff will be

Seller franc payoff = price - cost.

For example, say the buyer pair receives a value of 60 while the seller pair receives a cost of 20. If a price of 50 is negotiated, the payoffs will be

60 - 50 = 10 for the buyer pair, and

50 - 20 = 30 for the seller pair.

If the price agreed on is 30, then the payoffs will be

60 - 30 = 30 for the buyer pair, and

30 - 20 = 10 for the seller pair.

Note that the payoff to a buyer pair can be negative if and only if the buyer's bargainer agrees on a price which is greater than the value.

Similarly, the payoff to a seller pair can be negative if and only if the seller's bargainer agrees on a price which is less than the cost.

If no price is agreed on during the three minutes, i.e. if time runs out before a price is agreed on, all players' payoffs will be zero for that period. We will wait until the three minutes are over before proceeding to the next round if no price is agreed on. If both the buyer's bargainer and the seller's bargainer agree on a price, you must tell the experimental administrator sitting with you that an agreement is reached.

For each round in which a price agreement is reached, and in which the negotiated price is not greater than the highest acceptable price determined in the buyer pair's pre-negotiation phase, the buyer principal will receive 2/3 of the payoff to the buyer pair and the buyer's bargainer will receive the remaining 1/3. If the negotiated price exceeds the highest acceptable price, the buyer principal will receive all of the payoff to the buyer pair for that round; the buyer's bargainer will receive no payoff for that round, and in addition the buyer's bargainer will have a penalty subtracted from his final payoff at the end of the experiment. Similarly, for each round in which a price agreement is reached and in which the negotiated price is not less than the lowest acceptable price determined in the seller pair's pre-negotiation phase, the seller principal will receive 2/3 of the payoff to the seller pair and the seller's bargainer will receive the remaining 1/3. If the negotiated

price is less than the lowest acceptable price, the seller principal will receive all of the payoff to the seller pair for that round; the seller's bargainer will receive no payoff for that round and will have a penalty subtracted from his final payoff. The formula for these penalties is given below. The important point, however, is simply that it will never be in the interest of a bargainer to violate the instructions of his principal.

Once either a trade has been made or the three minutes of the round are over, this round will end. The bargainers will then return to their principals and report to them whether or not a trade was made and if so at what price. The principal will write in column 7 of his worksheet whether a transaction has been agreed to in period 1 by writing yes or no. In column 8 he will write the price and in column 9 the payoff. If no trade occurred, zeros should appear in columns 8 and 9.

After period 1 is over, you will proceed to period 2 by generating your new random numbers (cost for the sellers, value for the buyers), and then engage in another two-minute pre-negotiation phase and another three-minute bargaining phase. There will be 15 rounds. At the end we will convert your franc payoff into dollar payoffs in a manner to be described below.

In the discussions of the bargainers certain rules will be strictly enforced by the experimental administrator sitting with you. If they are violated, the experiment will be terminated and the violating person given a minimal payoff.

(1) A bargainer must not announce his random number (cost or value) or price limit to the other bargainer, nor may he take a piece of paper on which these numbers are written, into the bargaining room.

(2) No personal threats or abusive language will be tolerated, nor any disparaging remarks about the actions of other persons engaged in the experiment.

(3) In each period of the experiment you may only discuss the price of the fictitious good being sold in *that* period. For instance, no price arrangements about objects to be sold in later rounds may be discussed. You must discuss each object separately.

(4) Finally, no discussion of payments outside of the experiment will be tolerated. For instance, you may not say "if you agree to such and such price in this period I will pay you \$X after we leave the experiment."

Penalties

If the negotiated price exceeds the buyer pair's highest acceptable price, then an amount will be subtracted from the final payoff of the buyer's bargainer equal to the difference between the value and the highest acceptable price (if the negotiated price is less than the value) or the difference between the negotiated price and the highest acceptable price (if the negotiated price exceeds the value). If the negotiated price is less than the seller pair's lowest acceptable price, then an amount will be subtracted from the final payoff of the seller's bargainer equal to the difference between the lowest acceptable price and the negotiated price (if the negotiated price exceeds the cost) or the difference between the lowest acceptable price and the negotiated price (if the negotiated price is less than the cost). These penalties ensure that it can never benefit a bargainer to violate the acceptable price limit specified by his principal.

Final Payoffs

When the 15 rounds of the experiment are over, the final payoffs will be determined by adding all of the francs accumulated over the 15 rounds of the experiment. These francs will be converted into dollars at the rate of 1 franc = \$.05. Each bargainer will also receive a fixed payment of \$6.00 for participating, while each principal will receive a fixed payment of \$3.00. If by the end of the experiment a bargainer's total franc payoff is negative,

these losses will be subtracted from the fixed payment of the principal. You will be paid when you leave. If any rules are violated, the violating party will receive the fixed payment of \$3.00 minus any losses that have occurred while the other person will be paid by calculating his average payoff up until the violation and then multiplying it by 15.

Random Numbers

The random numbers on the lists held by the experimental administrators were generated independently depending upon whether you are a buyer or a seller. What we mean by a "random number" can be explained by looking at Figures 4 and 5.

FIGURE 4 HERE

Figure 4 describes how the random values of the buyers were generated. The graph shows possible values between 0 and 100 on the horizontal axis and a probability number on the vertical axis. The probability number tells you how likely it is that a buyer will receive a random number (value) less than or equal to any particular number. For instance, this graph indicates that the probability that a buyer will receive a value less than or equal to, say, 25 is .108 (while the probability that the buyer receives a value greater than 25 is .892). Similarly, the probability that the buyer receives a value less than or equal to 50 is .242 (while the probability that the buyer receives a value greater than 50 is .758). The probability that the buyer receives a value of 75 or less is .425 (the probability of receiving a value greater than 75 is .575). Table 5 gives you more information about the way your random values are generated if you are a buyer. A point to note is that if you are a buyer the likelihood of getting a "high" (greater than 50) value is substantially greater than the probability of getting a "low" value (i.e. a value less than 50). In fact, the probability that a buyer will receive a value less than 50 is only .24.

Table 5

Buyer Value Probability

Value in Points (X)	Probability Value Less Than X	Probability Value Greater Than X
1	0.004012	0.995987
5	0.020308	0.979691
10	0.041268	0.958731
15	0.062939	0.937060
20	0.085389	0.914610
25	0.108698	0.891301
30	0.132959	0.867040
35	0.158284	0.841715
40	0.184806	0.815193
45	0.212691	0.787308
50	0.242141	0.757858
55	0.273417	0.726582
60	0.306855	0.693144
65	0.342906	0.657093
70	0.382199	0.617800
75	0.425650	0.574349
80	0.474694	0.525305
85	0.531794	0.468205
90	0.601892	0.398107
95	0.698291	0.301708
100	1	0

Figure 4 that Buyer receives a Value less than (greater than) X

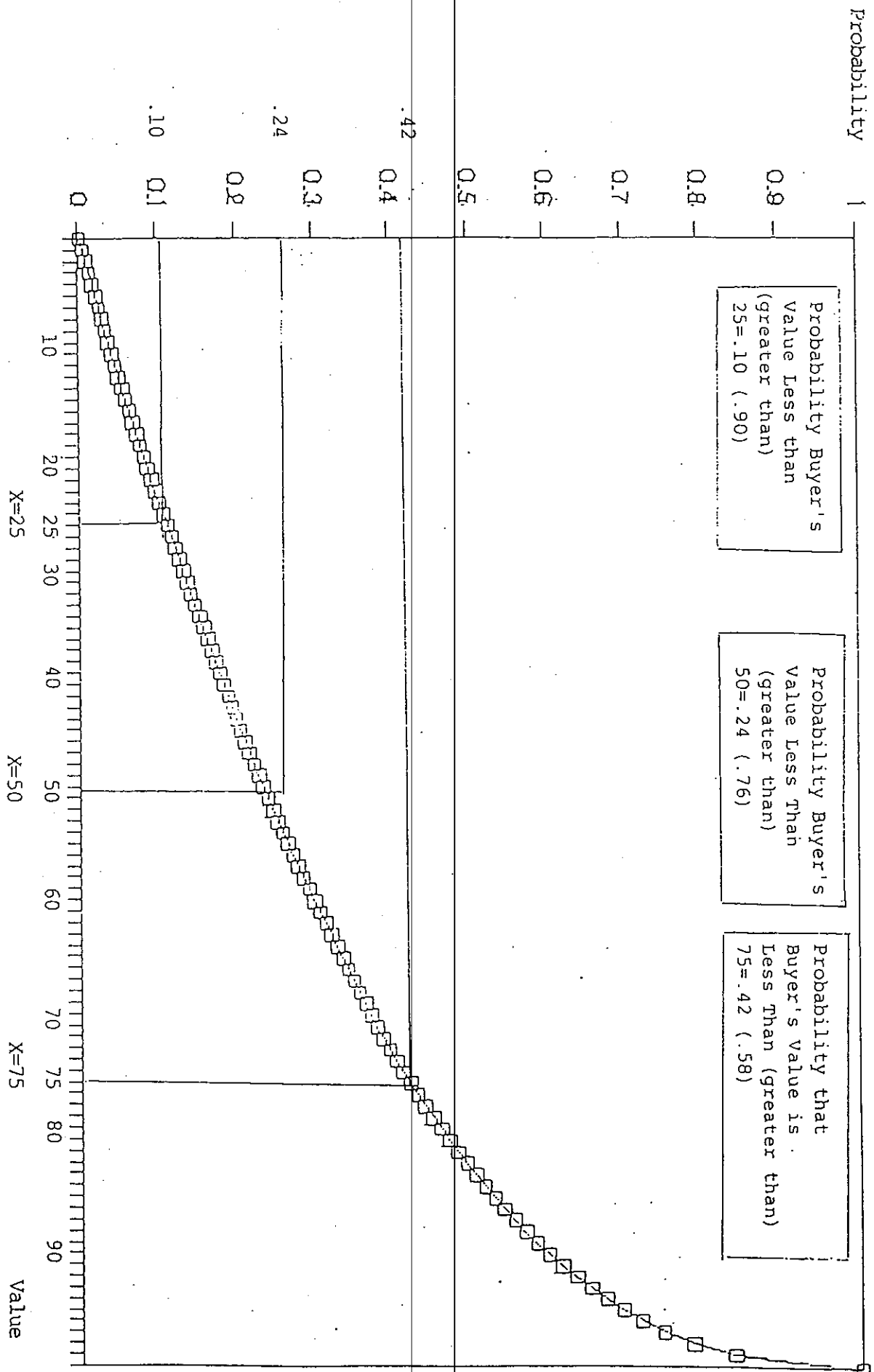


Figure 5
 Probability That Seller Receives
 A Cost Less Than X

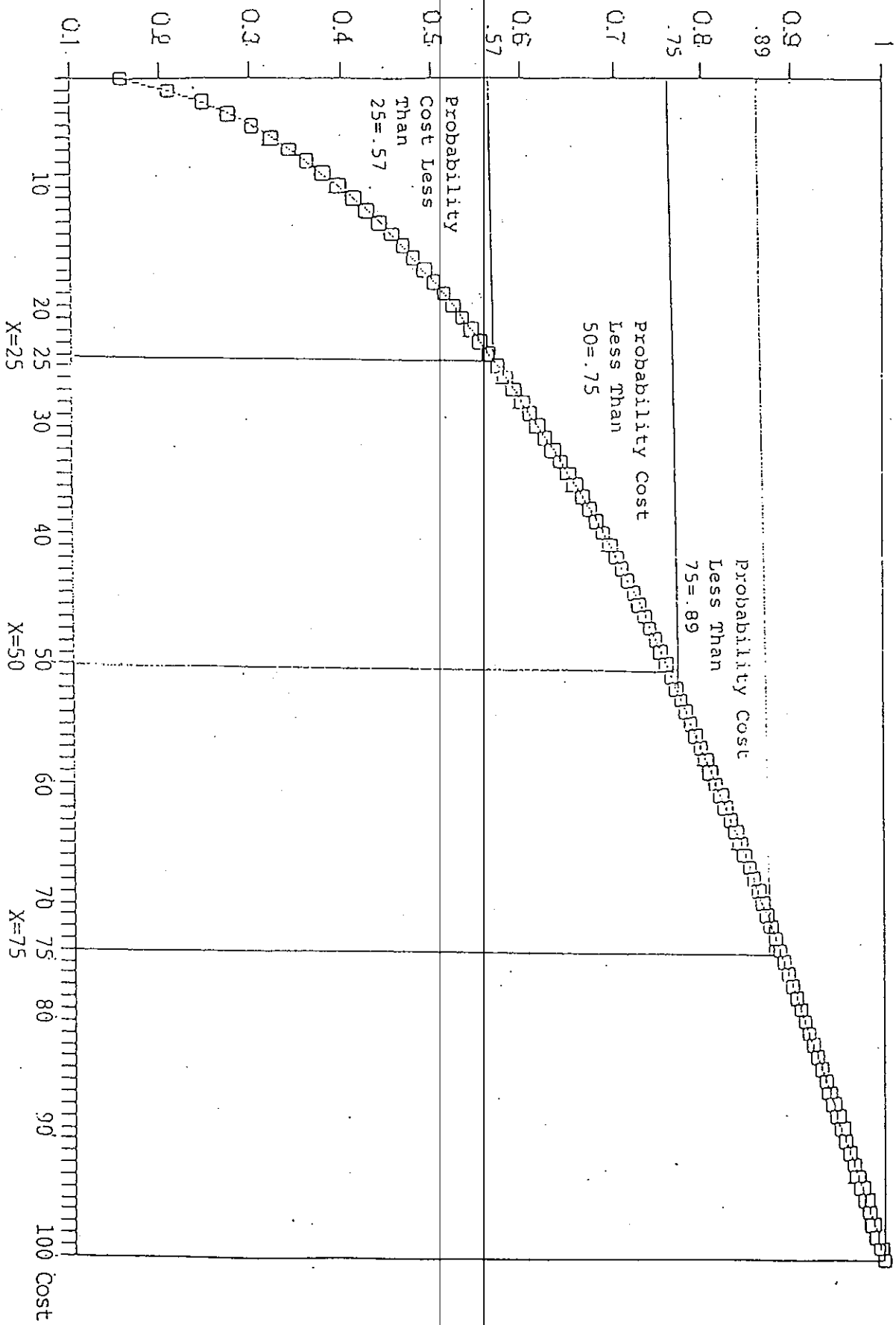


Table 6

Cost in
Points (X)Probability Cost
Less Than XProbability Cost
Greater Than X

1	0.158489	0.841510
5	0.301708	0.698291
10	0.398107	0.601892
15	0.468205	0.531794
20	0.525305	0.474694
25	0.574349	0.425650
30	0.617800	0.382199
35	0.657093	0.342906
40	0.693144	0.306855
45	0.726582	0.273417
50	0.757858	0.242141
55	0.787308	0.212691
60	0.815193	0.184806
65	0.841715	0.158284
70	0.867040	0.132959
75	0.891301	0.108698
80	0.914610	0.085389
85	0.937060	0.062939
90	0.958731	0.041268
95	0.979691	0.020308
100	1	0

TABLE 5 HERE

For a seller, just the opposite is true, as Figure 5 illustrates. Here we place all possible seller costs between 0 and 100 on the horizontal axis and a probability number of the vertical axis. Again the probability number defines the probability that if you are a seller you will receive a cost less than or equal to any particular cost. For instance, the probabilities that a seller will have a cost less than either 25, 50, or 75 are .575, .758, and .891, respectively. Table 6 below lists other such probabilities.

FIGURE 5 HERE

TABLE 6 HERE

A point to note is that, as opposed to the buyers, sellers have a greater chance of getting a low number (i.e., a cost less than or equal to 50) than a high number (greater than 50). In fact, the probability that a seller will receive a cost less than 50 is .757.

To give you a feel for these numbers the experimental administrator will generate a few illustrative random numbers from the sellers' and buyers' lists, respectively. Since the numbers on the lists were generated in a manner consistent with the graphs described above, they should familiarize you with the random numbers you could draw. After this is done, we will begin our experiment.

APPENDIX B

The Estimates of k for Each Subject Group

1-Consult Percentage-Fee Indirect Face-to-Face Bargaining Experiment						
Subject Group	Average LAP ¹	Average HAP ²	Average of C.2 + C.3	Average Price	C. 5 - C. 4	k ³
1	25.00	86.70	55.85	49.20	-6.65	0.39
2	28.85	61.38	45.12	41.44	-3.68	0.35
3	6.82	51.91	29.36	48.64	19.27	0.79
4	35.50	82.00	58.75	38.50	-20.25	0.46
5	48.00	70.92	59.46	51.88	-7.58	0.70
6	23.56	74.44	49.00	43.56	-5.44	0.41
7	56.00	91.75	73.88	59.67	-14.21	0.14
8	21.55	57.91	39.73	47.36	7.64	0.54
9	28.58	77.42	53.00	57.00	4.00	0.34
10	20.38	53.38	36.88	33.66	-3.27	0.62
Total	29.73	70.25	49.99	47.11	-2.88	0.52
1-Consult Fixed-Fee Indirect Face-to-Face Bargaining Experiment						
Subject Group	Average LAP ¹	Average HAP ²	Average of C. 2 + C.3	Average Price	C. 5 - C. 4	k ³
1	23.44	68.67	46.06	46.22	0.17	0.40
2	34.09	52.91	43.50	39.82	-3.68	0.38
3	20.50	72.58	46.54	60.92	14.38	0.92
4	32.83	63.58	48.21	47.71	-0.50	0.08
5	31.36	60.09	45.73	51.91	6.18	0.16
6	31.25	56.08	49.67	43.08	-0.58	0.41
7	38.55	75.91	57.23	44.00	-13.23	0.11
8	25.75	52.50	39.13	40.50	1.38	0.65
9	45.80	83.80	64.80	57.30	-7.50	0.59
10	26.15	60.38	43.27	48.46	5.19	0.44
Total	30.95	64.64	49.80	48.22	0.43	0.45

Notes: 1. LAP refers to lowest acceptable price. 2. HAP refers to highest acceptable price. 3. These estimated k's were obtained by solving the equation $P = kv + (1 - k)c$ for k after substituting the average actual price for P, the average lowest acceptable price for c, and the average highest acceptable price for v.