Games and Economic Behavior **30,** 248–292 (2000)
doi:10.1006/game.1999.0728, available online at http://www.idealibrary.com on IDEAL®

Bargaining Through Agents: An Experimental Study of Delegation and Commitment*

Andrew Schotter

Department of Economics, New York University, 269 Mercer Street, New York, New York 10003
E-mail: schotter@fasecon.econ.nyu.edu

Wei Zheng

William M. Mercer Companies LLC., 1166 Avenue of the Americas, New York, New York 10036
E-mail: wei.cor.zheng@us. wmmercer.com

and

Blaine Snyder†

Department of Economics, New York University, 269 Mercer Street, New York, New York 10003

Received February 17, 1998

While face-to-face bargaining has proven itself to be extremely efficient in the laboratory, it appears to break down often in the real world. This discrepancy, we assert, is explained by the fact that in the real world face-to-face bargaining is usually conducted not between principals but between the agents of principals. 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. *Journal of Economic Literature* Classification Numbers: C78, C92. © 2000 Academic Press

*The authors thank the C.V. Starr Center for Applied Economics at New York University for its financial assistance. Also, early work on this project was funded under National Science Foundation Grant No. SES-8721277 awarded to New York University. We have also benefited from numerous discussions with Roy Radner and Peter Linhart and from the very helpful comments of an anonymous referee. Finally, we would like to thank Antonio Merlo, Alan Corns, Gautam Barua, Alberto Casagrande, Jeff Davis, Karl Rothfuss, and Sara Bertin-Levecq for their assistance in helping us run the experiments.

†Blaine Snyder suffered an untimely death after the first version was completed. We dedicate this paper to his memory.

0899-8256/00 $35.00

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, while Bolton and Katok (1998) demonstrate the ‘‘narcotic effects’’ of having arbitration as a possibility in bargaining situations. In all of these studies, disagreement rates seem to be closer to those of the anonymous laboratory experiments than to those of the face-to-face variety.

The contradictory evidence presented by the laboratory 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

[1] See Roth (1995).

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.[2] (See Ashenfelter and Bloom, 1993, for an alternative explanation of the inefficiencies introduced when lawyers represent principals.)

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, 1993), and Fershtman and Kalai (1997) among others.[3] Like commitment and preference distortion games,[4] 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

[2] Jerome Katz, Esq., private communication.

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

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

consummate trades. In short we are not interested in explaining inefficient agreements but rather in disagreements. In the commitment literature, Crawford (1982) has demonstrated that disagreements between bargainers are 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 the 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* FFBEs 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 3-minute bargaining session and could not consult with them again during that round. We will refer to these experiments as the 1-consult indirect FFBEs. 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* FFBEs we allowed bargainers the possibility of repeatedly consulting with their principals. We call such experiments the 4-consult *indirect* FFBEs 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 *et al.*, (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* FFBEs 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 overconstrain their agents at the outset of the bargaining and then fail to make large enough concessions during the remainder of the experiment.

Section 2 of this paper presents the experimental design used to investigate our hypothesis. Section 3 presents a discussion of our results and offers a possible explanation for them. Section 4 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 2 min. In the 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 or she was penalized severely if he or she 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. These same distributions were used in all of the experiments.

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 or her 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.

After the pre-negotiation phase was over, each principal remained in his or his room while the two bargaining agents met in a third room for the bargaining phase. Bargainers had 3 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 percent-

[5] The exact penalty structure was spelled out in the instructions and reads as follows: Penalties. "If the negotiated price exceeds the buyer pair's highest acceptable price, then an amount will be substracted 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 penalites ensure that it can never benefit a bargainer to violate the acceptable price limit specified by his or her principal."

age-fee FFBE, the principal received 2/3 of the profits generated by the transaction for his or her 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 or her agreement with his or her 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 1 franc = $0.05 at the end of each experiment. In addition, each subject received a fixed payment at the end of the experiment; 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 he or she could also consult at the 45-, 90-, and 135-second mark. Hence, there was 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 3 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 and bargaining was very animated.

Experimental Design

Our experimental design is summarized in Table I. 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 *et al.*, (1990), while three have been performed to demonstrate the point being made here. We wish to make comparisons between the indirect face-to-face

TABLE I
Experimental Design

Experiment	Type of bargaining	Source of data	Number of bargaining pairs	Number of rounds
1	Direct face-to-face	Radner and Schotter (1989)	10	15
2	1-Consult indirect percentage fee	Schotter *et al.* (this article)	10	15
3	1-Consult indirect fixed fee	Schotter *et al.* (this article)	10	15
4	Sealed-bid mechanism	Linhart *et al.* (1990)	10	15
5	4-Consult indirect percentage fee	Schotter *et al.* (this article)	10	15

bargaining mechanisms run here and the direct face-to-face and sealed-bid experiments run by Radner and Schotter (1989) and Linhart *et al.*, (1990). The results of these comparisons are presented in our next section.

3. RESULTS

Descriptive Results[6]

The results of our two indirect FFBEs are summarized in Table II. For purposes of comparison, we also included in Table II 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 *et al.*, (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 to measure 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.

[6]All of the data for these experiments are available from the authors on request.

TABLE II
Disagreement and Inefficiency Rates[a]

Experiment type	Number of observations	Disagreement rate (%) First 7 rounds	Disagreement rate (%) Last 8 rounds	Disagreement rate (%) Total	Inefficiency rate (%) First 7 rounds	Inefficiency rate (%) Last 8 rounds	Inefficiency rate (%) 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

[a]Note that in all of these experiments the values and costs were generated by the same distribution functions: $F(V) = 1 - (100 - V/100)^{0.4}$, $G(C) = (C/100)^{0.4}$

We begin with an examination of the disagreement rates in the two 1-consult and one 4-consult indirect FFBEs 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 *et al.*, (1990).

Table II 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 and fixed-fee FFBEs, respectively, and 25% in the 4-consult indirect FFBE. (Remember that 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 FFBEs indicates a statistically significant difference between disagreement rates in the direct FFBEs and all (1-consult and 4-consult) indirect FFBEs 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 FFBEs approach the 29% disagreement rate observed in the Linhart *et al.*, (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 or her bargainer from being too "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.[7] 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 II, and parallel the comparison of disagreement rates. The introduction of bargaining agents raised the inefficiency rate from 1% in the direct FFBEs 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 *et al.*, (1990) for the sealed-bid mechanism, the

[7]A counterargument 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 himself or herself. On these grounds we might expect differences to persist.

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 pairwise 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 threefold, from 546 in the direct FFBE to 160 and 158 in the 1-consult percentage-fee and fixed-fee FFBEs, respectively, and twofold 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 Fig. 1.

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 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 bargaining 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.

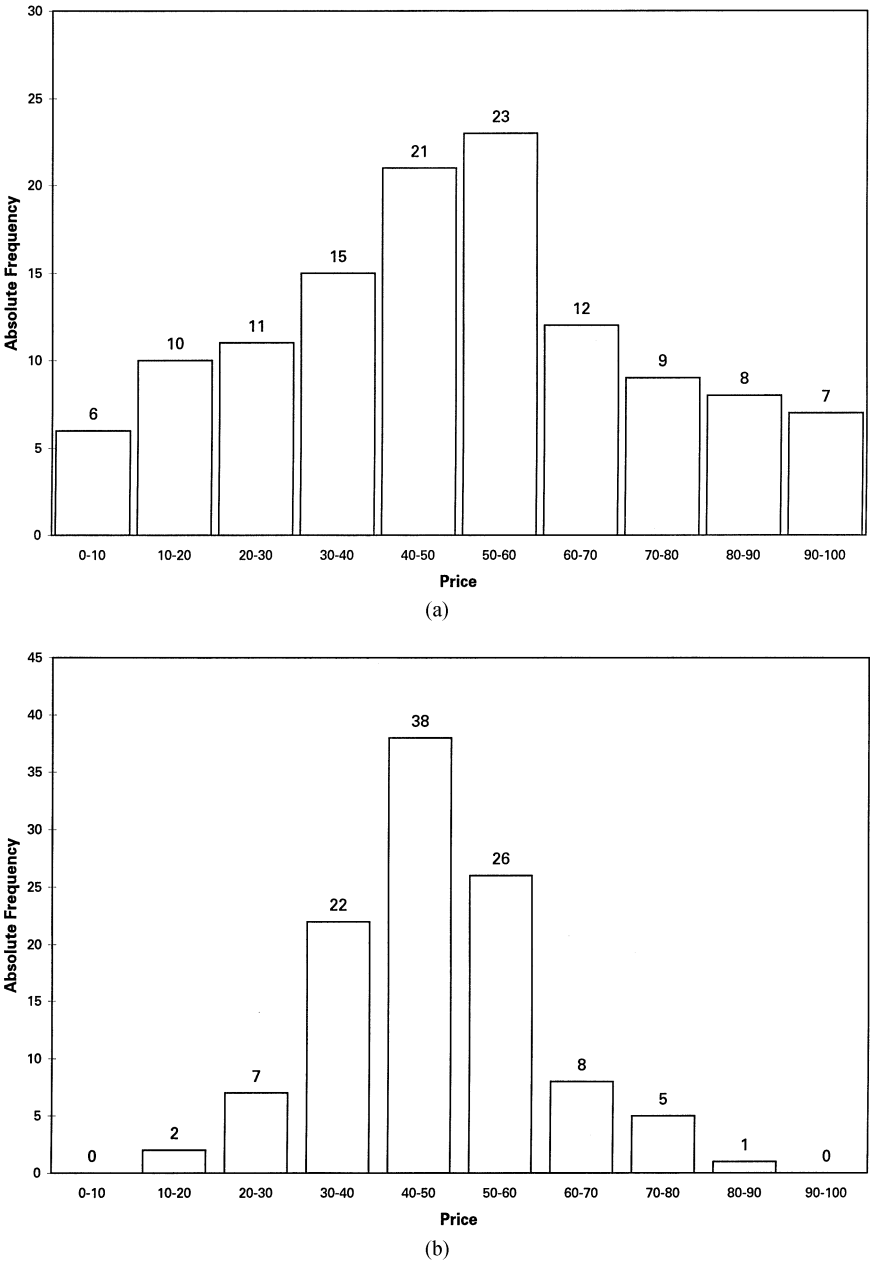


FIG. 1. Histograms of negotiated price. (a) Direct FFBE; (b) 1-consult indirect fixed-fee FFBE; (c) 1-consult indirect percentage-fee FFBE; (d) 4-consult indirect FFBE.

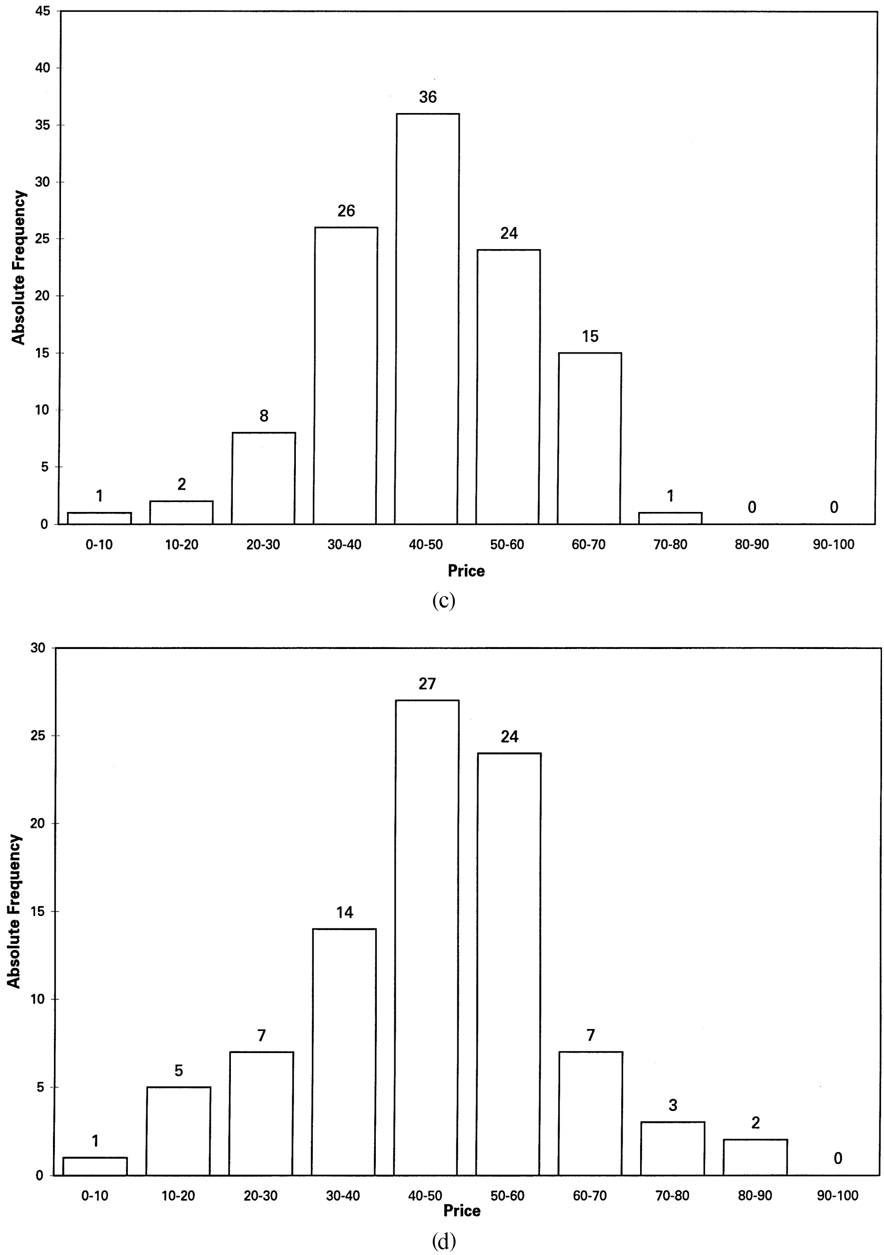


FIGURE 1—*Continued*

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 account 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 *et al.*, 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 $P = kv + (1 - k)c$, $0 \geq k \geq 1$; i.e., the price for the good is a 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 ks 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 or her bid v for each value of V, and S's strategy is a function γ that determines his or her 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, when, again, $k = 0.5$,

$$v = \begin{cases} V, & V < 36, \\ 20.25 + 0.438\,V, & V \geq 36, \end{cases}$$
$$c = \begin{cases} C, & C > 64, \\ 36.00 + 0.438\,C, & C \leq 64. \end{cases} \tag{3.2}$$

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 short,[8] 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 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 *et al.*, (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).

[8] Myerson and Satterthwaite (1983) 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.

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 placed 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 5 out of 22 non-trade rounds in the percentage-fee FFBE and in only 1 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 *et al.*, (1990), or that they are even close to the theoretical linear bid functions predicted, in part, by the theory. In Linhart *et al.*, (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 piecewise linear bid function of the type depicted in Eq. (3.2) above. (In Linhart *et al.*, there were 75 rounds.) Second, in the Linhart *et al.*, (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 and these are outlined below. 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.[9] 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 simply to 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, in a space where we place the value of a buyer (cost of the seller) on the x-axis and the bid (ask) on the vertical axis, principal–agent theory would predict instruction functions which are linear with a 0 intercept and a slope of 1 in the percentage-fee experiments but with a slope less than 1 in the fixed-fee experiments. In other words, in the percentage-fee experiments, buyer principals should simply allow their agents to pay up to their value for a good purchased if they have to while seller principals would permit selling at cost. For the fixed-fee experiments, however, we might expect an upper limit on the price paid (price accepted) by the buyer's (seller's) agent in order to prevent them from maximizing the probability of sales rather than their profitability. (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 IIIa presents estimates of the following linear regressions run for each buyer and seller in each 1-consult indirect experiment,

$$v_i = \alpha_1 + \beta_1 V_i + \epsilon_i,$$

$$c_i = \alpha_2 + \beta_2 C_i + \epsilon_i,$$

[9] When we refer to principal–agent theory 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 III
Linear Regression Results

(a) 1-Consult face-to-face bargaining experiment

Pair	Seller pairs α	Seller pairs β	Seller pairs R^2	Buyer pairs α	Buyer pairs β	Buyer pairs R^2
1-Consult percentage-fee FFBE						
1	12.13 (2.27)	0.86 (0.06)	0.94	1.31 (3.11)	0.92 (0.04)	0.98
2	10.32 (0.92)	0.86 (0.02)	0.99	3.87 (11.92)	0.76 (0.15)	0.65
3	2.91 (0.60)	0.96 (0.02)	1.00	11.23 (10.26)	0.52 (0.12)	0.58
4	27.88 (2.50)	0.60 (0.08)	0.80	−2.47 (1.04)	0.92 (0.01)	1.00
5	45.10 (1.80)	0.29 (0.08)	0.53	4.50 (1.28)	0.81 (0.02)	1.00
6	14.83 (4.30)	0.85 (0.12)	0.80	3.44 (5.27)	0.77 (0.07)	0.91
7	46.95 (1.50)	0.30 (0.04)	0.81	−1.00 (0.00)	1.00 (0.00)	1.00
8	2.90 (1.22)	0.97 (0.03)	0.99	8.45 (12.12)	0.61 (0.16)	0.54
9	15.18 (3.89)	0.80 (0.11)	0.80	3.24 (3.74)	0.83 (0.05)	0.96
10	3.57 (1.30)	0.95 (0.04)	0.98	4.19 (2.87)	0.63 (0.04)	0.96
Pooled	18.22 (1.48)	0.76 (0.04)	0.69	2.05 (2.91)	0.80 (0.04)	0.77
1-Consult fixed-fee FFBE						
1	12.13 (1.79)	0.85 (0.04)	0.98	10.89 (4.73)	0.63 (0.06)	0.89
2	30.37 (3.06)	0.51 (0.11)	0.63	26.19 (5.32)	0.36 (0.07)	0.64
3	10.12 (2.51)	0.80 (0.07)	0.90	5.88 (5.04)	0.76 (0.06)	0.92
4	32.69 (2.41)	0.45 (0.15)	0.41	5.51 (7.50)	0.77 (0.11)	0.79
5	27.30 (2.05)	0.68 (0.05)	0.93	6.30 (3.85)	0.71 (0.06)	0.93
6	20.78 (1.72)	0.71 (0.04)	0.95	15.12 (4.99)	0.58 (0.07)	0.84
7	25.25 (3.45)	0.69 (0.07)	0.87	−2.35 (4.72)	0.89 (0.06)	0.95
8	18.34 (1.63)	0.71 (0.06)	0.92	7.95 (1.99)	0.54 (0.03)	0.96
9	26.90 (3.34)	0.71 (0.07)	0.90	4.85 (3.24)	0.83 (0.04)	0.97
10	19.87 (1.56)	0.72 (0.06)	0.91	15.39 (7.60)	0.56 (0.09)	0.74
Pooled	22.76 (0.95)	0.70 (0.03)	0.83	7.60 (2.02)	0.70 (0.03)	0.83

(b) 4-Consult indirect face-to-face bargaining experiment

Group	Parameter	Seller pairs 0 s	Seller pairs 45 s	Seller pairs 90 s	Seller pairs 135 s	Buyer pairs 0 s	Buyer pairs 45 s	Buyer pairs 90 s	Buyer pairs 135 s
1	α	32.10	28.87	23.77	16.01	−8.07	−7.50	−9.09	−3.87
		(5.89)	(5.80)	(3.98)	(4.09)	(14.48)	(18.34)	(16.28)	(8.66)
	β	0.66	0.64	0.71	0.85	0.39	0.50	0.55	0.81
		(0.14)	(0.14)	(0.09)	(0.10)	(0.17)	(0.22)	(0.19)	(0.10)
	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
		(5.05)	(3.30)	(2.84)	(3.58)	(9.93)	(10.58)	(10.01)	(8.94)
	β	0.58	0.47	0.44	0.44	0.09	0.16	0.19	0.30
		(0.15)	(0.10)	(0.08)	(0.11)	(0.12)	(0.13)	(0.12)	(0.11)
	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
		(3.99)	(4.28)	(3.71)	(3.71)	(9.44)	(6.80)	(7.97)	(7.97)
	β	0.98	0.98	0.98	0.98	0.46	0.42	0.50	0.50

TABLE III—*Continued*

(b) 4-Consult indirect face-to-face bargaining experiment									
		Seller pairs				Buyer pairs			
Group	Parameter	0 s	45 s	90 s	135 s	0 s	45 s	90 s	135 s
		(0.07)	(0.08)	(0.07)	(0.07)	(0.12)	(0.09)	(0.10)	(0.10)
	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
		(4.99)	(4.84)	(4.70)	(6.25)	(3.29)	(3.72)	(2.77)	(5.01)
	β	0.23	0.08	0.22	0.47	0.35	0.46	0.62	0.85
		(0.15)	(0.15)	(0.14)	(0.19)	(0.05)	(0.06)	(0.04)	(0.07)
	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
		(5.25)	(5.30)	(5.34)	(5.34)	(3.51)	(5.23)	(5.23)	(5.73)
	β	0.81	0.78	0.78	0.78	0.52	0.65	0.65	0.71
		(0.10)	(0.10)	(0.10)	(0.10)	(0.05)	(0.07)	(0.07)	(0.07)
	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
		(3.56)	(2.81)	(2.89)	(3.47)	(16.91)	(21.32)	(15.32)	(15.32)
	β	0.17	0.22	0.41	0.47	0.45	0.76	0.71	0.71
		(0.09)	(0.07)	(0.08)	(0.09)	(0.20)	(0.25)	(0.18)	(0.18)
	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
		(2.52)	(2.76)	(3.32)	(2.97)	(9.93)	(10.86)	(9.85)	(8.86)
	β	0.10	0.23	0.25	0.57	0.23	0.31	0.37	0.47
		(0.05)	(0.05)	(0.06)	(0.06)	(0.12)	(0.13)	(0.12)	(0.11)
	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
		(2.22)	(2.42)	(2.36)	(2.73)	(6.48)	(7.35)	(7.11)	(12.21)
	β	0.75	0.76	0.75	0.87	−0.03	0.05	0.08	0.04
		(0.06)	(0.06)	(0.06)	(0.07)	(0.07)	(0.08)	(0.08)	(0.14)
	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
		(4.59)	(3.01)	(2.95)	(2.95)	(4.61)	(6.34)	(6.34)	(8.81)
	β	0.37	0.54	0.60	0.60	0.34	0.52	0.56	0.62
		(0.13)	(0.09)	(0.09)	(0.09)	(0.06)	(0.09)	(0.09)	(0.12)
	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
		(5.23)	(5.35)	(4.43)	(3.59)	(3.45)	(3.89)	(3.82)	(3.86)
	β	0.41	0.41	0.47	0.51	0.72	0.72	0.72	0.69
		(0.16)	(0.17)	(0.14)	(0.11)	(0.04)	(0.05)	(0.05)	(0.05)
	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	7.7	8.23	8.19
		(2.27)	(2.08)	(1.82)	(1.65)	(3.97)	(4.38)	(4.20)	(3.83)
	β	0.47	0.49	0.54	0.64	0.33	0.44	0.48	0.57
		(0.05)	(0.05)	(0.04)	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)
	R^2	0.34	0.41	0.53	0.65	0.23	0.31	0.36	0.49

TABLE III—*Continued*

(c) Sealed-Bid bargaining

	Sellers			Buyers		
Pair	α	β	R^2	α	β	R^2
1	51.89 (2.87)	0.41 (0.06)	0.79	−1.17 (1.18)	0.96 (0.02)	1.00
2	50.71 (4.22)	0.45 (0.09)	0.67	8.13 (12.00)	0.63 (0.14)	0.60
3	4.58 (1.99)	1.03 (0.07)	0.94	13.42 (7.24)	0.30 (0.10)	0.39
4	39.67 (3.05)	0.49 (0.08)	0.75	11.09 (5.43)	0.56 (0.07)	0.83
5	29.66 (2.42)	0.54 (0.07)	0.81	1.79 (3.88)	0.65 (0.05)	0.93
6	31.18 (2.69)	0.77 (0.08)	0.88	−1.27 (7.48)	0.89 (0.10)	0.86
7	22.96 (10.57)	0.58 (0.25)	0.30	15.99 (6.32)	0.67 (0.09)	0.82
8	35.59 (2.77)	0.46 (0.06)	0.80	44.08 (9.98)	0.01 (0.12)	0.00
9	30.08 (1.88)	0.56 (0.07)	0.84	−0.92 (8.24)	0.69 (0.10)	0.79
10	8.94 (0.84)	0.90 (0.02)	1.00	5.54 (6.89)	0.38 (0.08)	0.62
Pooled	30.30 (1.76)	0.62 (0.04)	0.57	8.25 (3.68)	0.59 (0.05)	0.51

Note. The number in parentheses are standard errors.

where ϵ_i are iid disturbance terms with mean 0 and standard deviation 1. We present linear regressions here after performing a Ramsey specification test to test if a higher order specification led to significantly different results. What we find is that in the fixed-fee experiment for only 6 of the 20 subjects (buyers or sellers) could we reject the linear hypothesis, while in the percentage-fee experiment we could reject the linear specification for only 4 subjects. All of these results are at the 5% level. The results of the Ramsey test are given in Table IV.

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 0.68 and 0.66 for seller and buyer subjects in the fixed-fee experiment and 0.74 and 0.78 for sellers and buyers in the percentage-fee experiment. Finally, when we test whether these intercepts and slopes are different from regressions fit using the data of Linhart *et al.*, (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 (see Appendix B), 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

TABLE IV
Results of Ramsey Test: F-Statistics

	Fixed-fee experiment		Percentage-fee experiment	
Subject No.	Buyer F-stat (Prob)	Seller F-stat (Prob)	Buyer F-stat (Prob)	Seller F-stat (Prob)
1	6.38 (0.02)	0.76 (0.39)	1.08 (0.31)	12.81 (0.00)
2	5.39 (0.03)	5.10 (0.04)	1.69 (0.21)	10.04 (0.00)
3	0.24 (0.62)	2.49 (0.14)	1.94 (0.18)	7.30 (0.01)
4	2.02 (0.17)	0.12 (0.73)	1.13 (0.30)	1.13 (0.30)
5	0.51 (0.48)	17.26 (0.00)	0.35 (0.56)	1.51 (0.24)
6	0.00 (0.99)	11.66 (0.00)	1.56 (0.23)	0.27 (0.60)
7	1.82 (0.20)	0.74 (0.40)	0.00 (1.00)	3.55 (0.08)
8	17.35 (0.00)	3.12 (0.10)	0.00 (0.97)	0.23 (0.63)
9	0.02 (0.92)	0.03 (0.85)	2.88 (0.11)	0.00 (0.95)
10	0.03 (0.85)	0.03 (0.85)	2.59 (0.13)	0.12 (0.73)

of significance.[10] 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 0, 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 buyer-seller pair in each experiment, we then estimated the average k

[10] This is true of all comparisons of slopes and intercepts the buyers and sellers made between the Linhart *et al.* (1990) sealed-bid auction experiments and the 1-consult fixed- and percentage-fee experiments run here, except for the comparison made between the slopes of the buyers in the percentage-fee 1-consult experiments and the sealed-bid auctions.

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 C 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.

A Possible Explanation: 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 0-second instant can be changed at 45 seconds, 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 (i.e., gave instructions which lead to exceedingly low probabilities of trade) at the 0-second mark and then did not sufficiently relax these constraints as time went on to sufficiently increase the probability of trade as time ran out. As a consequence, at the 135-second mark the bargainers were still too 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 was 140-second 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 s 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 "time is money," bargaining among agents who have the ability to consult may become a lengthy, drawn out, and costly process. Second, the efficiencies in the 4-consult experiment were almost identical with those of the sealed-bid experiment of Linhart *et al.*, (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-, 45-, 90-, 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 markups and markdowns 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.

For illustrative purposes only (we do our statistical analysis using individual and not pooled data) Fig. 2a and b presents the estimated 0-, 45-, 90-, and 135-second aggregate instruction functions for buyers and sellers in the 4-consult indirect FFBE along with those of the sealed-bid experiment (Radner and Schotter, 1989). These instruction functions were estimated using a pooled sample of subject buyer and seller principals. (Table IIIb and c presents the actual individual and pooled 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 or her bargainer to not buy for a price greater than 40 at the 0-s mark while at the 135-s mark he or she 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 *et al.*, (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 II indicate.

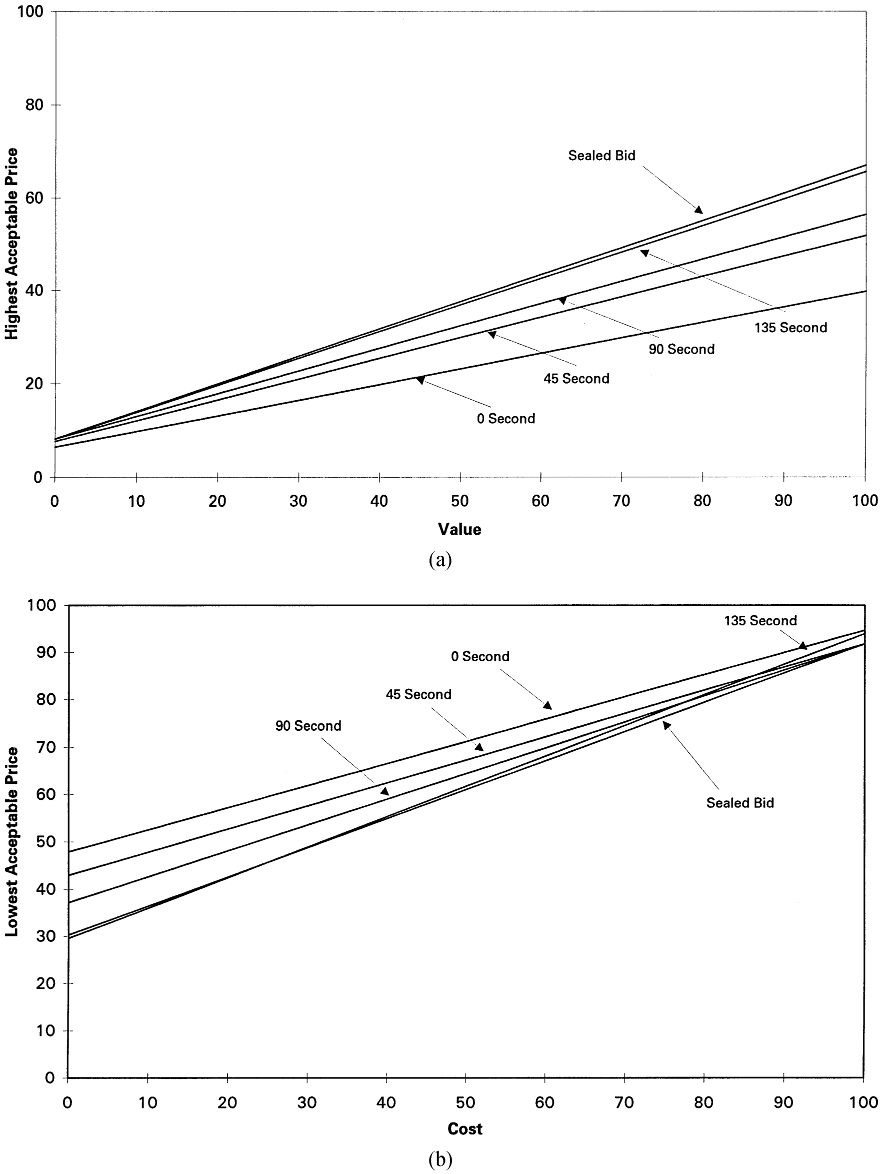


FIG. 2. Instruction functions: 4-consult and sealed-bid experiments: (a) buyers; (b) sellers.

To illustrate how such constraints on trade would affect the probability of trade, we present Fig. 3a–d which presents the 0-, 45-, 90- and 135-second aggregate instruction functions for buyer and seller principals and places them on the same graph. If trade occurred according to these instruction functions, then we can clearly calculate the probability of trade during each time interval. For example, take the 0-second instruction functions presented in Fig. 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 0 and the buyer drew a 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 his or her seller opponent has drawn a 0 cost for which trade is possible. At 45 seconds (Fig. 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 (Fig. 3c) and 135 seconds (Fig. 3d), we see that these crucial values (V^*) become approximately 61 and 35 for the 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.

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 seller drew his or her cost from the distribution $(C/100)^{0.4}$ while the buyer drew his or 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 or 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

$$(\text{probability of trade} \mid V) = \int_0^{a+bV} (0.4/100)(c/100)^{-0.6}\, dC$$

$$\text{where } a = (\alpha_1 - \alpha_2)/\beta_2 \text{ and } b = \beta_1/\beta_2$$

$$= ((a + bV)/100)^{0.4}. \tag{4.1}$$

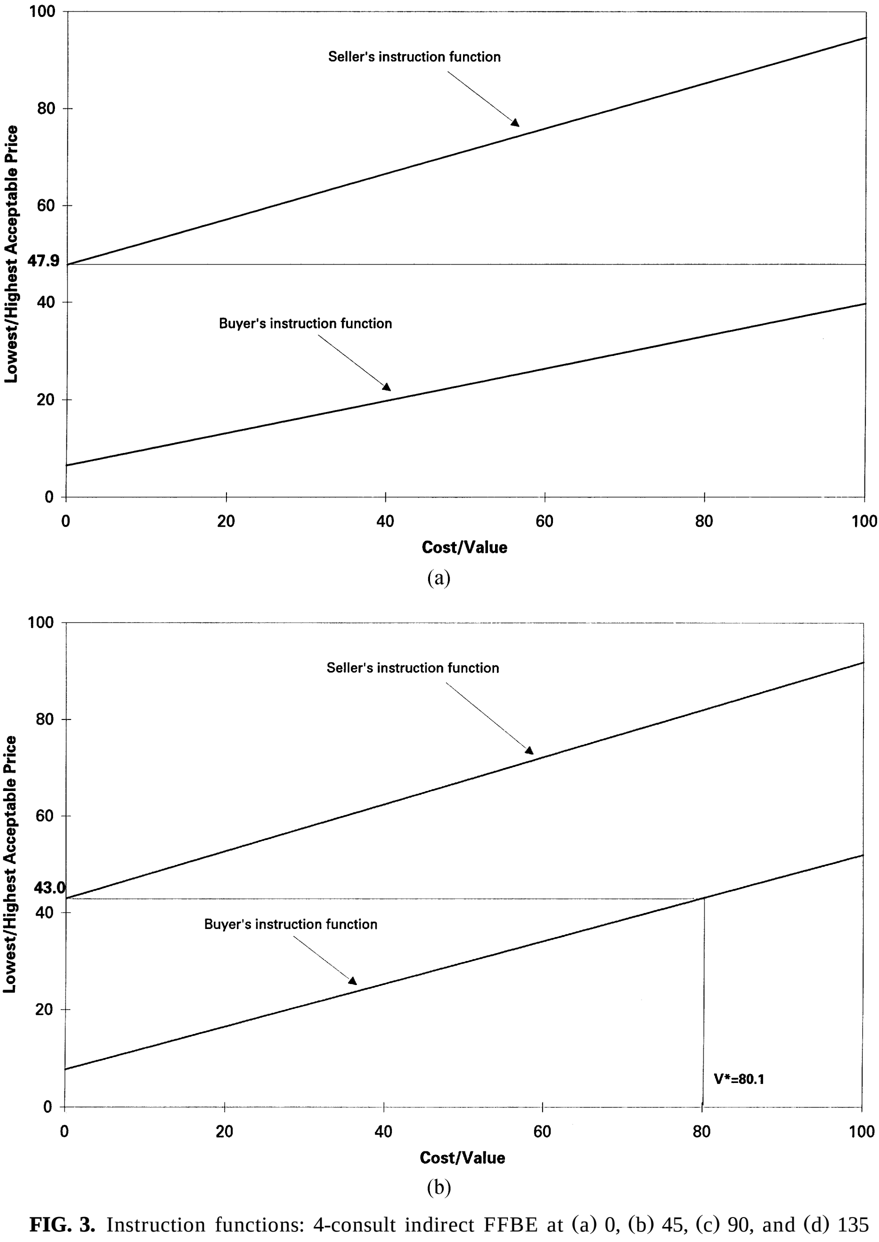
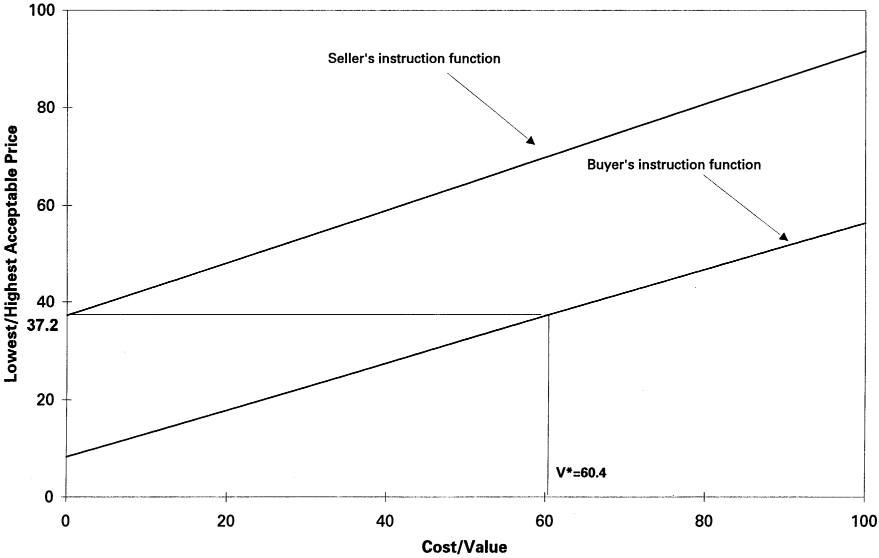
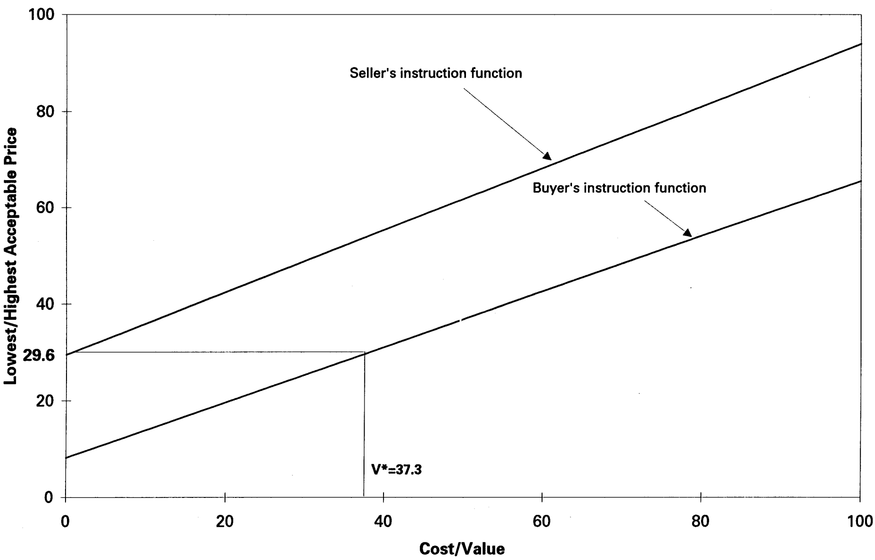


FIG. 3. Instruction functions: 4-consult indirect FFBE at (a) 0, (b) 45, (c) 90, and (d) 135 seconds.



(c)



(d)

FIGURE 3—*Continued*

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

$$\text{(total probability of trade)} = \int_{V^*}^{100} ((a + bV)/100)^{0.4}/(0.4/100)((100 - V)/100)^{-0.6}\, dV. \tag{4.2}$$

Table V presents a set of identical calculations done using individual instruction functions rather than the aggregate ones presented in Fig. 3. These calculations present the mean theoretical probability of trade at each time interval of the 4-consult experiment. In making these calculations, we calculated the probability of trade subject pair-by-subject pair and then averaged these probabilities time interval-by-time interval.

This table presents the mean theoretical probability of trade that would have occurred if subjects used their individual instruction functions estimated in Table IIIb and actually made trades whenever the instruction for the buyer was greater than that of the seller. Here the mean is taken over the individual-by-individual probability of trade for each time interval and does not use the pooled instruction functions used above to illustrate how our calculation could be made. In addition, it includes the actual probability of trade observed. For comparison purposes, we include the same calculations for the static sealed-bid mechanism where linear equilibrium strategies are employed.

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 instruc-

TABLE V
Theoretical Probability of Trade

	4-Consult experiment				Sealed-bid
	0 s	45 s	90 s	135 s	linear equilibrium
Theoretical probability of trade given instr. functions	0.16	0.31	0.40	0.66	0.57
Actual probability of trade	0.03 (4/148)	0.05 (7/144)	0.08 (11/137)	0.62 (78/126)	0.63

tions 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 s time interval which were not far away from those actually occurring (65 versus 61%). Hence, our hypothesis that principals were overzealous at the 0-s 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]-s interval and the observed similarity between the sealed-bid inefficiencies and the results in the [135, 180]-s interval where the 135-s instruction function was used.

Markups, Markdowns, and Cheap Talk

Markups and Markdowns

Finally, we present an alternative presentation of our hypothesis using the notions of a markup (MU) and markdown (MD). We define a markup for seller j in period or round t as the percentage above cost demanded by the principal or $\text{MU} = (c_{jt} - C_{jt})/C_{jt}$, while the markdown for buyer i in period or round t is the percentage below his or her value demanded or $\text{MD} = (V_{it} - v_{it})/V_{it}$. These markdowns and markups 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 $0 \leq \text{MD} \leq 1$, the markup is unbounded since c is unlimited. In fact, since demands greater than $V = 100$ can never be met, we would expect $0 \leq \text{MU} \leq (100 - C_j)/C_j$. Finally note that the markup is undefined when $C = 0$. Because of this, we will use in our calculations the average markup and markdown for buyers and sellers defined as $\text{AMU} = \Sigma_t[(c_{jt} - C_{jt})/t] \cdot \Sigma_t C_{jt}/t$ and $\text{AMD} = \Sigma_t(V_{it} - v_{it})t/\Sigma_t V_{it}/t$, where $t = 1, 2, \ldots, 15$ is the time period of an experiment and i (j) is the buyer (seller).

Tables VI and VII present the average markups and markdowns for sellers and buyers in the 4-consult experiment and the two 1-consult experiments.

Looking at the sellers first note that the toughest sellers in any experiments were the 0-s seller principals whose average markup was 1.22 as compared to the average markup in the fixed-fee and percentage-fee experiments of 0.61 and 0.57, respectively. Over time these markups decreased, yet at the 135-s mark, the average seller markup of the 4-consult principals was still higher than that of both the fixed-fee and percentage-fee principals in the 1-consult experiment (0.73 as compared to 0.61 and 0.57, respectively). For buyers the situation is slightly different.

TABLE VI
Average Markups and Markdowns: 4-Consult Indirect FFBEs[a]

	Seller markup				Buyer markdown			
Experiment	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

[a] S0, S45, S90, S135 are the average markups for sellers at the 0-, 45-, 90-, and 135-s mark, while B0, B45, B90, B35 are the average markdowns for buyers at the 0-, 45-, 90-, and 135-s mark.

While the 4-consult buyers start out being tougher than their counterparts in the 1-consult experiments with an average markdown of 0.57 as opposed to 0.20 and 0.51 for the fixed-fee and percentage-fee experiments, respectively, by the 135-s 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 seller 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-s 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 cannot, 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 a discrete $0-1$ variable indicating whether a trade was made or not and the right-hand variables were the cost and value of the seller and buyer as well as their concessions (i.e., for buyers, the difference between their value and their instructions (maximum acceptable price) or, for sellers, the difference between their cost and their instructions (minimal acceptable price) at the four concession intervals). The results of these regressions are presented in Table VIII.

TABLE VII
Average Markup and Markdowns: 1-Consult Indirect FFBEs

Experiment	Seller markup	Buyer markdown
Fixed-Fee Experiment		
1	0.19	0.22
2	1.12	0.26
3	0.23	0.17
4	4.51[a]	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
Percentage-fee experiment		
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

[a] This observation is treated as an outlier in this sample and the average registered at the bottom omits it from its calculation.

This cheap-talk hypothesis finds strong support for sellers where the only variables 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.

TABLE VIII
Probit Regression Results: Probability of Trade

Variable	Coefficient	Std. error	t-Statistic	P-value
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	−39.78			
Obs. with Dep. = 1	90			
Obs. with Dep. = 0	30			

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 laboratory, 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 it also tends to exhibit larger 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 principals 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 or her 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 overconstrain their bargainers leaving them little room to make beneficial deals until late in the bargaining process, during the last 45 seconds.

APPENDIX A

Instructions for the Percentage-Fee Indirect Face-to-Face Bargaining Experiment

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 a 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 1 to 5, and then another number from 1 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 or her 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 the 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 or her 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 or she will be penalized if he or she 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 or her 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 2-min 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 2 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 or her principal, and discussed bargaining strategy with his or her principal, the two bargainers will be given 3 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 3 minutes, 2 minutes, 1 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 3 min, 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

$$60 - 50 = 10 \qquad \text{for the buyer pair,}$$
$$50 - 20 = 30 \qquad \text{for the seller pair.}$$

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

$$60 - 30 = 30 \qquad \text{for the buyer pair,}$$
$$30 - 20 = 10 \qquad \text{for the seller pair.}$$

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 3 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 3 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 or her 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 or her 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 or her principal.

Once either a trade has been made or the 3 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 or her worksheet whether a transaction has been agreed to in period 1 by writing yes or no. In column 4 he or she 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 2-minute pre-negotiation phase and another 3-min 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 will be given a minimal payoff.

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

(2) No personal threats, abusive language or disparaging remarks about the actions of other persons engaged in the experiment will be tolerated.

(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 or her 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 = $0.05. 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 or her 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 Figs. 4 and 5.

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 0.108 (while the probability that the buyer receives a value *greater* than 25 is 0.892). Similarly, the probability that the buyer receives a value less than or equal to 50 is 0.242 (while the probability that the buyer receives a value greater than 50 is 0.758). The probability that the buyer receives a value of 75 or less is 0.425 (the probability of receiving a value greater than 75 is 0.575). Table IX 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

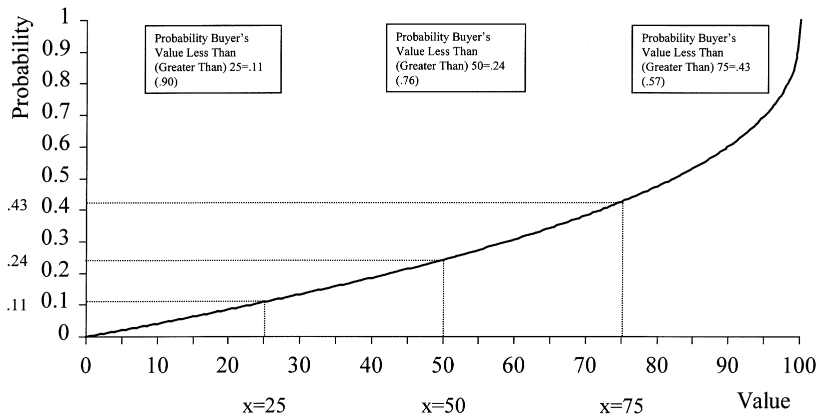


FIG. 4. Probability that buyer receives a value less than (greater than) X.

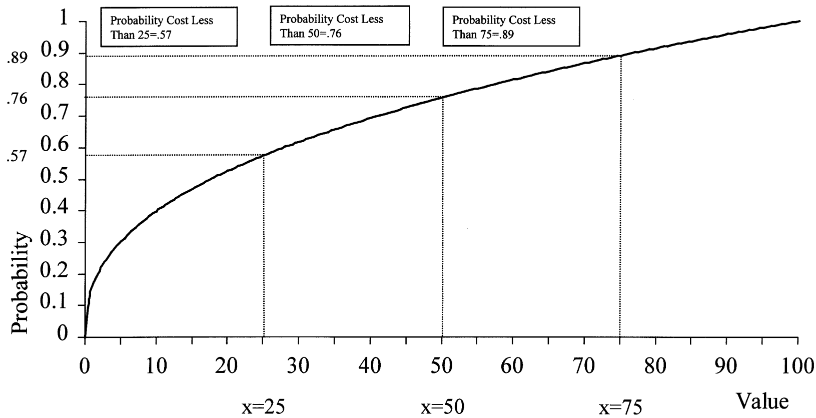


FIG. 5. Probability that seller receives a cost less than X.

TABLE IX
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	0.757856
55	0.273417	0.726582
60	0.306855	0.693144
65	0.342906	0.657093
70	0.352199	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

TABLE X

Cost in points (X)	Probability cost les than X	Probability 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

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 0.24.

For a seller, just the opposite is true, as Fig. 5 illustrates. Here we place all possible seller costs between 0 and 100 on the horizontal axis and a probability number on 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 0.575, 0.758, and 0.891, respectively. Table X lists other such probabilities.

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 0.757.

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 the 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 titled "Payoffs" in the percentage-fee instructions, the fixed-fee instructions contain two sections titled "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 or her principal's price limit will receive no payment for that round and will also have penalties subtracted from his or her 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 or her 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 titled "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 = \$0.04. Each of the bargainers will receive \$1.00 for each round in which an acceptable price was agreed upon."

APPENDIX B: WILCOXON SIGNED-RANK TEST

Percentage-Fee versus Sealed-Bid

Seller

α *%-fee* vs α *sealed-bid*: $z = -1.580$, $p = 0.114$;

β *%-fee* vs β *sealed-bid*: $z = 1.274$, $p = 0.202$.

Buyer

α *%-fee* vs α *sealed-bid*: $z = -0.866$, $p = 0.386$;

β *%-fee* vs β *sealed-bid*: $z = 2.497$, $p = 0.012$*.

Fixed-Fee versus Sealed-Bid

Seller

α *%-fee* vs α *sealed-bid*: $z = -1.580$, $p = 0.114$;

β *%-fee* vs β *sealed-bid*: $z = 0.918$, $p = 0.358$.

Buyer

α *%-fee* vs α *sealed-bid*: $z = 0.255$, $p = 0.798$;

β *%-fee* vs β *sealed-bid*: $z = 0.663$, $p = 0.507$.

Note: The asterisk (*) indicates comparison for which we can reject the hypothesis of eqaulity in slopes at the 5% level of significance.

APPENDIX C

The Estimates of k for Each Subject Group

Subject Group	Average LAP[1]	Average HAP[2]	Average of C.2 + C.3	Average Price	C.5 − C.4	k[3]
(a) 1-Consult Percentage-Fee Indirect Face-to-Face Bargaining Experiment						
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
(b) 1-Consult Fixed-Fee Indirect Face-to-Face Bargaining Experiment						
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

Note. 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 actural price for P, the average lowest acceptable price for c, and the average highest acceptable price for v.

REFERENCES

Ashenfelter, O., and Bloom, D. (1993). "Lawyers as Agents of the Devil in a Prisoners' Dilemma Game," working paper 4447, National Bureau of Economic Research, Cambridge MA.

Ashenfelter, O., and Currie, J. (1990). "Negotiator Behavior and the Occurrence of Disputes," *Amer. Econom. Rev., Papers and Proc.*, **80**, 416−420.

Bolton, G., and Katok, E. (1998). "Reinterpreting Arbitration's Narcotic Effect: An Experimental Study of Learning in Repeated Bargaining," *Games Econom. Behav.*, **25**, 1–33.

Burtraw, D. (1990). "The Incentive Contract for Strategic Delegation in Bargaining." discussion paper QE90-19, Resources for the Future.

Burtraw, D. (1992). "Strategic Delegation in Bargaining," *Econom. Lett.* **38**, 181–185.

Burtraw, D. (1993). "Bargaining with Noisy Delegation." *Rand J. Econom.*, **24**, 40–55.

Card, D. (1990). "Strikes and Bargaining: A Survey of Recent Empirical Literature," *Amer. Econom. Rev. Papers and Pro.* **80**, 405–415.

Chatterjee, K., and Samuelson, W. (1983). "Bargaining Under Incomplete Information," *Operations Res.* **31**, 835–851.

Crawford, V. P. (1982). "A Theory of Disagreement in Bargaining," *Econometrica* **50**, 607–637.

Crawford, V., and Varian, H. (1979). "Distortion of Preferences and the Nash Theory of Bargaining," *Econom. Lett.*, **3** 203–206.

Fershtman, C. K., and Judd, K. (1987). "Equilibrium Incentives in Oligopoly," *Amer. Econom. Rev.* **77**, 927–940.

Fershtman, C. K., Judd, K., and Kalai, E. (1991). "Observable Contracts: Strategic Delegation and Cooperation," *Internat. Econom. Rev.* **3**, 551–560.

Fershtman, C. K., and Kalai, E. (1997). "Unobserved Delegation," *Internat. Econom. Rev.* **38**, 763–774.

Hoffman, E., and Spitzer, M. (1982). "The Coase Theorem: Some Experimental Tests," *J. Law Econom.*, **25**, 93–98.

Jones, S. (1989a). "Have Your Lawyer Call My Lawyer: Bilateral Delegation in Bargaining Situations." *J. Econom. Behav. Organization*, **11**, 159–174.

Jones, S. (1989b). "The Role of Negotiators in Union–Firm Bargaining." *Canad. J. Econom.*, **22**, 631–642.

Katz, M. (1991). "Game Playing Agents: Contracts as Pre-Commitments," *Rand J. Econom.* **22**, 307–327.

Kennan, J., and Wilson, R. (1990). "Can Strategic Bargaining Models Explain Collective Bargaining Data," *Amer. Econom. Rev.* **80**, 405–409.

Leininger, W., Linhart, P., and Radner, R. (1989). "The Sealed-Bid Mechanism for Bargaining with Incomplete Information," *J. Econom. Theory*, **48**, 63–107.

Linhart, P., Radner, R., and Schotter, A. (1990). "Behavior and Efficiency in the Sealed-Bid Mechanism," Economic Research Report No. 90–51, C.V. Starr Center for Applied Economics, New York University.

Muthoo, A. (1992). "Revocable Commitment and Sequential Bargaining," *Econom. J.*, **102**, 378–387.

Myerson, R., and Satterthwaite, M. (1983). "Efficient Mechanisms for Bilateral Trading," *J. Econom. Theory* **29**, 265–281.

Radner, R., and Schotter, A. (1989). "The Sealed–Bid Mechanism: An Experimental Study," *J. Econom. Theory* **48**, 179–221.

Roth, A. (1995). "Bargaining Experiments," in *Handbook of Experimental Economics* (J. Kagel and A. Roth, Eds.) pp. 253–292. Princeton, NJ: Princeton Univ. Press.

Roth, A., and Murnighan, K. (1982). "The Role of Information in Bargaining." *Econometrica*, **50**, 1123–1142.

Roth, A., Murnighan, K., and Schoumaker, F. (1988). "The Deadline Effect in Bargaining: An Experimental Study," *Amer. Econom. Rev.*, **78**, 806–823.

Roth, A., and Prasnikar, V. (1990). "Face to Face versus Anonymous Bargaining Experiments: A Cautionary Note on the Theoretical Implications of Observed Differences," memo, July 24.

Salop, S., and White, L. (1988). "Private Anti-Trust Litigation: An Introduction and Framework," in *Private Anti-Trust Litigation: New Evidence, New Learning* (L. J. White, ed), Cambridge: MIT Press.

Schelling, T. (1956). "An Essay on Bargaining," *Amer. Econom. Rev.* **46**, 281–306.

Schotter, A., Snyder, B., and Zheng, W. (1992). "Bargaining Through Agents: An Experimental Study," Economic Research Report No. 92–48, C.V. Starr Center for Applied Economics, New York University.

Sklivas, S. (1987). "The Strategic Choice of Managerial Incentives." *Rand J. Econom.* **18**, 452–458.

Sobel, J. (1981). "Distortion of Utilities and the Bargaining Problem." *Econometrica* **49**, 597–619.

Stuart, A., and Ord, J. K. (Eds.) (1979). *Kendall's Advanced Statistics Theory*, Vol. II, 4th Ed. London: Griffin.

Vickers, J. (1985). "Delegation and the Theory of the Firm," *Econom. J. Supple.* **95**, 138–147.