An Experimental Study of Sequential Bargaining

By Jack Ochs and Alvin E. Roth*

In a study of alternating offer bargaining with discounting, perfect equilibrium was found to have little predictive power, under the conventional assumption that bargainers' utility is measured by their monetary payoffs. Instead, our data exhibit a first-mover advantage, independent of the equilibrium prediction. However the pattern of rejected offers and counterproposals shows bargainers' utility was not *measured by their monetary payoffs: 81 percent of rejected offers were followed by counterproposals that would earn less money. We also reanalyze data from earlier experiments, finding a similar pattern of rejections and counterproposals.*

Recently there has been a good deal of attention given to models of two-party bargaining in which time is divided into periods, and the opportunity to make an offer alternates between the bargainers. In a given period, one bargainer makes an offer which the other may accept or reject. If the offer is accepted, bargaining ends and the bargainers receive their agreed payoffs. If the offer is declined in any but the last period, then in the next period the other bargainer is the one to make an offer, but the value to the bargainers of any potential agreement shrinks according to some discount factors, which may be different for the two bargainers. The bargaining ends in disagreement if no offer has been accepted by the end of the last period.

Such a game has many strategic equilibria, but most of these can be thought of as involving an attempt by one of the bargainers to threaten a course of action which he would not wish to carry out if his bluff were called. For example, in a two-period game, the player who makes the offer in the first period, player 1, might demand 99 percent of the gains from trade for himself, and threaten that if player 2 refuses to accept this offer, then in the second period he (player 1) will refuse *any* offer, so that disagreement will result and each player will receive nothing. If this threat is believed, player 2's best response is to accept the 1 percent he is offered in the first period. But the threat implies that, if player 2 rejects the offer in the first period, player 1 will reject offers in the next period that he would then prefer to accept. For this reason such threats may not be credible. The class of equilibria which do not involve such threats are called *subgame perfect*.

Specifically, the basic model is the following: two bargainers, 1 and 2, alternate making offers over how to divide some amount k (for example, of money). Time is divided into periods, and in odd-numbered periods t (starting at an initial period $t=1$) player 1 may propose to player 2 any division $(x, k-x)$. If player 2 accepts this proposal then the game ends and player 1 receives a utility of $(\delta_1)^{(t-1)}x$ and 2 receives a utility $(\delta_2)^{(t-1)}(k-x)$, where δ_i is a number between 0 and 1 reflecting player i's cost of delay. (That is, a payoff of y dollars to player i at period t gives him the same utility as a payoff of $\delta_i y$ dollars at period $t-1$). If player 2 does not accept the offer, and if period t is not the final period of the game, then the game proceeds to period $t+1$, and the roles of the two players are reversed. If an offer made in

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the last period of the game is refused, then the game ends with each player receiving 0. A game with a maximum number of periods T will be called a T-period game.[1]

A subgame-perfect equilibrium can be computed by working backward from the last period. An offer made in period T is an ultimatum, and so at such an equilibrium player i (who will receive 0 if he rejects the offer) will accept any nonnegative offer when payoffs are continuously divisible.[2] So at a subgame-perfect equilibrium, player j, who gets to make the proposal in period T, will receive 100 percent of the amount k to be divided, if the game continues to period T. Consequently at period $T-1$ player j will refuse any offer of less than $(\delta_j)k$ but accept any offer of more, so that at equilibrium player i receives the share $k-(\delta_j)k$ if the game goes to period $T-1$, and so at period $T-2$ he must be offered $(\delta_i)(k-(\delta_j)k)$, and so forth. Working back to period 1 in this way, we can compute the equilibrium division: that is, the amount that the theory predicts player 1 should offer to player 2 at period 1, and player 2 should accept. (When payoffs are continuous this equilibrium division is unique). So, when payoffs are continuous, subgame-perfect equilibrium in a two-period game calls for player 1 to offer player 2 the amount $\delta_2 k$ in the first period (and demand $k-\delta_2 k$ for himself), while in a three-period game player 1 offers player 2 the amount $\delta_2(k-\delta_1 k)$ in the first period, and demands $k-\delta_2(k-\delta_1 k)$ for himself.

Recent experimental studies of sequential bargaining problems of this kind have reported markedly different results. Their authors have drawn quite different, sometimes mutually contradictory conclusions about the predictive value of perfect equilibrium models of bargaining, and about the role that experience, limited foresight, or bargainers' beliefs about fairness might play in explaining their observations. (Questions of fairness arise because in some of these experiments many observed agreements give both bargainers 50 percent of the available money).

Each of these recent experiments was designed to correspond to the case that the players have equal discount factors, that is, $\delta_1=\delta_2=\delta$. Following standard practice in the experimental literature when only ordinal utilities are of concern, the utility of the bargainers was assumed to be measured by the amount of money they receive. The costliness of delay in these experiments was implemented by making the amount of money being divided in period $t+1$ equal to δ times the amount available at period t. (So the value of any fixed-percentage share of the pie is multiplied by δ from one period to the next). In a number of these studies the number of periods, T, was identified as the critical variable that distinguishes between the games in these different experiments (and sometimes also within an experiment). The amount of experience that subjects acquire in the experiment (that is, the number of times they bargain) has also been considered. Analysis of the data primarily focused on the accuracy of the perfect equilibrium as a point predictor, that is, on whether the observed outcomes were distributed around the perfect equilibrium division or around some other division of the available money.

This paper reports a new experiment designed to test the predictive accuracy of some of the *qualitative* predictions of the perfect equilibrium in sequential bargaining. Our experiment is implemented in a way that allows the discount factors of the two bargainers to be varied independently. The experimental design allows us to make comparisons between different combinations of discount factors for games of fixed length, as well as between games of different length for given discount factors. The data will also permit us to consider whether the utility of the bargainers is accurately measured by their monetary payoffs, and to consider the effects of experience.

[1] Much of the recent theoretical work using this kind of model follows the treatment by Ariel Rubinstein (1982) of the infinite horizon case. An exploration of various aspects of the finite horizon case is given by Ingolf Stahl (1972).

[2] If payoffs are discrete, so that offers can only be made to the nearest penny, for example, then there are subgame-perfect equilibria at which i refuses to take 0 but accepts the smallest positive offer, for example, one cent.

This experiment was thus designed to make a more comprehensive test of the theory than has previously been attempted. Specifically, it was designed to detect whether changes in the parameters of the game influence the observed outcomes in the predicted direction, even in the case that there might be a systematic error in the point predictions. We will argue that the results of this new experiment also suggest a plausible explanation of why the earlier experiments observed such widely varying results. Before describing and analyzing the new experiment, we set the stage with a brief description of the earlier experiments.

I. The Earlier Experiments

A. *The Experiments of Werner Güth, Rolf Schmittberger, and Bernd Schwarz (1982)*

The first experiment of this study examined one-period ("ultimatum") bargaining games. Players were divided into two groups of equal size, to be matched at random with players of the other group. The players in one of the two groups would always be "Player 1," that is, would always have the first move. Player 1 could propose dividing a fixed sum of k deutsche marks any way he chose, by filling out a form saying "I demand DM $\underline{x}$." Player 2 could either accept, in which case player 1 received x and player 2 got $k-x$, or he could reject, in which case each player received 0 for that game. The perfect equilibrium prediction for such games is that player 1 will receive k and player 2 will receive 0.

There were 21 "naive" interactions (data gathered from inexperienced subjects) and 21 "experienced" interactions (data gathered one week later from the same subjects). (There were three games each with $k = 4,\dots,10$). From the 21 naive interactions, the modal proposal by player 1 (7 times) was for a 50 percent share for himself (and so an equal share for player 2), and the average proposal was for a 65 percent share for player 1. There were two proposals asking for (the equilibrium prediction of) 100 percent for player 1. No other proposal asked for as much as 90 percent. There were two disagreements, one of them in response to a demand for 100 percent. (The other demand for 100 percent was accepted). For the 21 experienced interactions, there were three 50–50 proposals, and one 100-ε (with $\varepsilon =$ DM.01) proposal. No other proposal asked for as much as 90 percent. There were 6 disagreements. The average demand by player 1 was for a 69 percent share. Thus in neither case did the proposals approach the equilibrium prediction for demands of 100 percent.

The authors conclude that "...subjects often rely on what they consider a fair or justified result. Furthermore, the ultimatum aspect cannot be completely exploited since subjects do not hesitate to punish if their opponent asks for 'too much'."

B. *The Experiment of Ken Binmore, Avner Shaked, and John Sutton (1985)*[3]

This work was motivated by the above study. The authors say: "The work of Güth et al. seems to preclude a predictive role for game theory insofar as bargaining behavior is concerned. Our purpose in this note is to report briefly on an experiment that shows that this conclusion is unwarranted... ."[4]

The experiment studied a 2-period bargaining game, whose rules are that player 1 makes a proposal of the form $(x, 100-x)$ to divide 100 pence. If player 2 accepts, this is the result. Otherwise player 2 makes a proposal $(x', 25-x')$ to divide 25 pence. If

[3]Some of the description of this experiment is taken from the original, more detailed report of the experiment contained in the authors' 1984 discussion paper. We are grateful to the authors for explaining to us how to read some of their tables.

[4]They add: "This does not mean that our results are inconsistent with those of Güth et al. Under similar conditions, we obtain similar results. Moreover our full results would seem to refute the more obvious rationalizations of the behavior observed by Güth et al. as 'optimising with complex motivations.' Instead, our results indicate that this behavior is not stable in the sense that it can be easily displaced by simple optimizing behavior, once small changes are made in the playing conditions."

player 1 accepts, this is the result, otherwise each player receives 0. Thus in this game $\delta_1 = \delta_2 = .25$, and (since proposals are constrained to be an integer number of pence) at any subgame-perfect equilibrium player 1 makes an opening demand in the range 74–76 pence, and player 2 accepts any opening demand of 74 pence or less. Subjects played a single game, after which player 2 was invited to play the game again, as player 1. In fact there was no player 2 in this second game, so only the opening demand was observed.

The data for the first game reveal a mode around a first demand near 50 pence. Of 81 observations,[5] only 8 were in the equilibrium interval of 74–76 pence. First demands were rejected 12 times. In the second game (in which only first demands were observed), there was a mode around a first demand just below 75 pence, with 30 of the 81 demands being in the equilibrium interval [74–76]. There was thus a clear shift between the two distributions of first demands, in the direction of the equilibrium demand.

The authors conclude "Our suspicion is that the one-stage ultimatum game is a rather special case, from which it is dangerous to draw general conclusions. In the ultimatum game, the first player might be dissuaded from making an opening demand at, or close to, the 'optimum' level, because his opponent would then incur a negligible cost in making an 'irrational' rejection. In the two-stage game, these considerations are postponed to the second stage, and so their impact is attenuated."

C. *The Experiment of Güth and Reinhard Tietz (1987)*

This paper is a response to Binmore, Shaked, and Sutton (1985). The experiment examined two, two-stage games with discount factors of .9 and .1 respectively. So the subgame-perfect equilibrium predictions (in percentage terms) for the two cases are (10 percent, 90 percent) and (90 and 10 percent), respectively. The authors say "Our hypothesis is that the consistency of experimental observations and game-theoretic predictions observed by Binmore et al. (1985), as well as by Sidney Siegal and Lawrence Fouraker, is solely due to the moderate relation of equilibrium payoffs which makes the game-theoretic solution socially more acceptable."

Subjects played two games, each with a randomly chosen other bargainer. Subjects who played the first game as player 1 played the second game as player 2. One difference from the sequential bargaining games discussed above was that disagreement automatically resulted if player 2 rejected an offer from player 1 but made a counterproposal that would give him less than player 1 had offered him. Note that this rule makes the games more like ultimatum games, since some demands of player 1 (for example, demands of less than 90 percent in games with discount factor of .1) can only be rejected at the cost of disagreement.

In the first game, the average first demand in games with a discount factor of .1 was 76 percent, and in the second game 67 percent. For games with a discount factor of .9, the average first demand in the first game was 70 percent, and in the second game 59 percent. (Recall that when the discount factor is .9, the equilibrium first demand is only 10 percent).

The authors conclude that "Our main result is that contrary to Binmore, Shaked, and Sutton, 'gamesmenship' is clearly rejected, that is, the game-theoretic solution has nearly no predictive power."

D. *The Experiment of Janet Neelin, Hugo Sonnenschein, and Matthew Spiegel (1988)*

This paper is also a response to Binmore, Shaked, and Sutton (1985).[6] Two experiments are reported: in the first, 80 students

[5]Or perhaps 82: there is a discrepancy between the histogram in the published version and in the working paper, and the table in the working paper.

[6]Who in turn make a brief reply in Binmore, Shaked, and Sutton (1988).

from a microeconomics class played 2-period, 3-period, and 5-period bargaining games, in order, against different opponents (after a practice game). In the second, 30 students from a similar class played three 5-period games (after a practice game). There was a single discount rate for both players, adjusted across games so that (in experiment 1) the round 1 pie was always worth \$5, and the perfect equilibrium demand was always \$3.75. (This meant that the second period pie was \$1.25, \$2.50, and \$1.70 in the 2-, 3-, and 5-period games, respectively, corresponding to discount factors of .25, .5, and .34, respectively). In experiment 2, the game was the same as the 5-period game of experiment 1, with payoffs multiplied by 3.

The authors summarize their data and conclusions as follows: "Neither the Stahl/Rubinstein nor the equal-split models predict the bargaining behavior observed in our six games. A convenient summary of what we observed is that in each game the sellers offered the buyers the value of the second-round pie." That is, they observe that the data for all their (2-, 3-, and 5-period) games are near the perfect equilibrium prediction for *2*-period games.

II. The New Experimental Design

The new experiment used the 4×2 design shown in Table 1. The two treatment variables were the discount rates δ_1 and δ_2 (the 4-way variable, with values $(\delta_1, \delta_2) = (.4,.4)$, $(.6,.4)$, $(.6,.6)$, and $(.4,.6)$) and the number of periods T (with values $T = 2,3$). In addition, each subject participated in ten consecutive bargaining encounters with the same parameters, against different individuals.

Since some cells of the design require different discount rates for the two bargainers, the discounting could not be implemented as in the previous experiments, by simply reducing the sum to be divided in each period. Instead, in each period, the commodity to be divided consisted of 100 "chips." In period 1 of each game, each chip was worth \$0.30 to each bargainer. In period 2, each chip was worth δ_1(\$0.30) to player 1 and δ_2(\$0.30) to player 2, and in period 3 of the three-period games each chip was worth $(\delta_1)^2$(\$0.30) and $(\delta_2)^2$(\$0.30), respectively.[7] That is, the rate at which subjects were paid for each of the 100 chips that they might receive depended on their discount rate and the period in which agreement was reached. (See the detailed account of procedures below).

Table 1 gives the eight cells of the experiment, and the perfect equilibrium divisions corresponding to the experimental parameters, under the assumption that the bargainers' utility is measured by their monetary payoffs.[8] For convenience, these equilibrium divisions are stated both in chips and in dollar value, and the range of equilibrium divisions is given when there are multiple perfect equilibria due to the discreteness of the medium of exchange.

Note that, aside from the point predictions made by perfect equilibrium, there are also a number of important qualitative predictions.

First, player 1's discount factor only influences the equilibrium division when $T = 3$. When $T = 2$, only player 2's discount factor is predicted to matter, and so the prediction is that the same divisions will be reached in cell 1 as in cell 2, and in cell 3 as in cell 4, and that player 2 will do better in cells 3 and 4 than in cells 1 and 2.

Second, for given discount factors for the two players (i.e., within a row of the table), player 2 is predicted to receive a smaller share when $T = 3$ than when $T = 2$. (When $T = 3$, player 1 not only makes the first offer,

[7] For a player i with $\delta_i = .6$, chips were worth \$0.18 in period 2 and \$0.11 in period 3 (where this latter amount is rounded up from \$0.108, which is the exact value of $(.6)^2$(\$0.30). For a player with $\delta_i = .4$, chips were worth \$0.12 in period 2 and \$0.05 in period 3, where again the latter figure is rounded up from \$0.048.

[8] Since chips could only be divided in integer quantities, there can be multiple perfect equilibrium divisions. In cells 1 and 2 the first-period equilibrium offers to player 2 can be from 39 chips (\$11.70) up to 41 chips (\$12.30), and in cells 3 and 4 from 59 chips (\$17.70) to 61 chips (\$18.30). For the three-period games, we have to take into account the rounding of third-period chip values to the nearest penny, as noted in the previous footnote. This yields unique equilibria in cells 5, 6, and 8 with first-period offers to player 2 of 24, 16, and 35 chips, respectively, and in cell 7 the equilibrium offers to player 2 can be either 23 or 24 chips.

TABLE 1—EXPERIMENTAL DESIGN, AND RANGE OF EQUILIBRIUM DIVISIONS

	Two-Period		Three-Period	
	Chips	Money	Chips	Money
$\delta_1=.4, \delta_2=.4$	Cell 1: (59,41) to (61,39)	(\$17.70,\$12.30) to (\$18.30,\$11.70)	Cell 5: (76,24)	(\$22.80,\$7.20)
$\delta_1=.6, \delta_2=.4$	Cell 2: (59,41) to (61,39)	(\$17.70,\$12.30) to (\$18.30,\$11.70)	Cell 6: (84,16)	(\$25.20,\$4.80)
$\delta_1=.6, \delta_2=.6$	Cell 3: (39,61) to (41,59)	(\$11.70,\$18.30) to (\$12.30,\$17.70)	Cell 7: (77,23) to (76,24)	(\$23.10,\$6.90) to (\$22.80,\$7.20)
$\delta_1=.4, \delta_2=.6$	Cell 4: (39,61) to (41,59)	(\$11.70,\$18.30) to (\$12.30,\$17.70)	Cell 8: (65,35)	(\$19.50,\$10.50)

before discounting takes its toll, but he also has the opportunity to make the last offer).

The theory's predictions include, in addition, all 28 pairwise comparisons between cells.[9] And since each bargainer played 10 games, all in the same cell, against different, anonymous opponents, the design also permits us to investigate the effects that experience may have on the outcome of the bargaining.

A. *Methods*

Subjects were recruited from undergraduate economics classes at the University of Pittsburgh and Carnegie Mellon University. They were told that they would be paid \$5.00 for showing up on time, and that, in addition, they would have an opportunity to bargain over a sum of \$30. Each subject participated in only one cell of the experiment, and all observations for a given cell were conducted in a single session. Participants were assembled in a room and randomly assigned code numbers which determined whether they would be in the position of player 1 or 2 in the subsequent bargaining. (In the instructions, the player 1 and 2 positions were called the "Right" and "Left" positions, respectively). The instructions, which were distributed and read aloud, are presented in Appendix 1. Note that the message form on which offers were exchanged presents the cash value per chip for each player for each period, and that the players were required to keep records which involved computing the cash value of each offer. Following the instructions, a practice game was played, after which all participants were separated into two rooms (so all player 1's were in one room and all player 2's in another) and reseated, in an order determined by the randomly assigned code number. In the subsequent bargaining, each participant bargained consecutively with each of the participants in the other room, without knowing who he was bargaining with in any given round. All subjects knew that they would be bargaining with a different person from round to round during a session. Each round, of course, consisted of either 2 or 3 periods, depending on the cell. Subjects were told that, at the conclusion of the experi-

[9]Note the role that the design plays in facilitating these comparisons. For example, in the experiment of Neelin et al., in which discount factor and game length were varied simultaneously, their individual effects cannot be separated.

ment, one of the rounds would be chosen at random and they would be paid the result of that round.

III. Results of the New Experiment

A. *Observations Related to the Equilibrium Predictions*

(*i*) *Opening Offers*. Figures 1A and 1B display the following data for each cell of our experiment: (1) the number of bargaining pairs per round; (2) the mean of the observed first-period offers to player 2 in each of the 10 rounds; (3) the maximum and minimum first-period offers in each round; (4) plus and minus two standard errors from the mean offer in each round; (5) the number of first-period offers that were rejected in each round. In addition to the data, the perfect equilibrium offer and the equal division offer (which is always \$15) are displayed. The offers made in round 10 of each cell represent the behavior of the most experienced bargainers. As the figures show, the subgame-perfect equilibrium offer is generally a very poor point predictor of the observed outcomes. Cell One is the only cell in which the perfect equilibrium offer is within two standard errors of the observed mean. In no other cell does the perfect equilibrium offer fall within plus or minus two standard errors of the estimated population mean.

The subgame-perfect equilibrium not only fails as a point predictor of observed behavior, it also fails to account for observed qualitative differences. One qualitative prediction of the theory is that a change in player 1's discount factor should have no influence on the proposals made in two-period games. Table 2 presents estimates of the standard error of the distribution of differences in sample means and the 95 percent confidence limits for the difference in expected offers, given the observed difference in the means for each of these comparisons. In neither of these comparisons does the confidence interval on the estimate of the true differences include 0.

A second qualitative prediction of the subgame-perfect equilibrium theory is that, holding discount factors constant, the proposal made to player 2 in the three-period game should be less than the proposal made in the two-period game. Table 3 presents the relevant across-column comparisons. In two of the four comparisons the observed difference in means is in the opposite direction to that implied by the subgame-perfect equilibrium hypothesis. In the other two comparisons, the t-ratios are high enough to reject the null hypothesis of no difference in the means at the 95 percent confidence level but not at the 97.5 percent confidence level.

Another indication of the lack of success of the subgame-perfect equilibrium hypothesis is the fact that Player 2 was only slightly more likely to receive an opening offer for at least 50 percent of the available cash in cells 3 and 4, where the equilibrium offer is for *60 percent* of the cash than in cells 1 and 2, where the equilibrium offer is *40 percent*. Cells 3 and 4 contain 23.7 percent of all of the subjects and only 25.3 percent of all of the opening offers to player 2 which are for 50 percent (or more) of the available cash.

The subgame-perfect equilibrium predicts a qualitative difference in means in 25-pairwise comparisons across the cells in our experiment. (See Table 4). A very weak test of the power of the theory to account for the qualitative properties of the data is whether the success rate in predicting the observed direction of differences in round 10 mean offers is better than could be expected by predictions made on the basis of coin flips. As Table 4 indicates, the direction of the difference in means corresponds to the theoretically predicted direction in 17 of the 25 pairwise comparisons. The probability of getting at least 17 out of 25 answers correct purely by chance is approximately 4.6 percent. Therefore, we can just barely reject the null hypothesis that, as a predictor of the direction of differences in pairwise comparisons of means, the theory does no better than coin flipping.

A slightly more demanding test of the ability of the theory to account for qualitative properties of the data is provided by a test of the correlation between the observed round 10 mean opening offers and the perfect equilibrium offers over the 8 cells of our experiment. Equation (1) reports the regression estimate of the relation between observed mean opening offers in round 10 of each cell and the corresponding theoretical prediction. The value of the coefficient of the theoretical mean is not significantly greater than zero at conventional levels of significance.

$$\text{(1)} \qquad \text{Observed Mean} = 13.944 - .04306 \text{ Theoretical Mean}$$

$$(\text{Std. Error} = .066485).\ R^2 = .06535.$$

In testing the predictive power of the subgame-perfect equilibrium theory we have focused upon the round 10 data since this represents the outcomes of bargaining between the most experienced subjects. While experience makes some difference, as Figures 1A, B show, at the aggregate level round 10 is not very different from other rounds.

Table 5 presents the observed difference in the means of the opening offers to Player 2 between round 1 and round 10 for each of the eight cells in the experiment. As the table shows, at the aggregate level there is no statistically significant difference in offers between rounds 1 and 10 in any cell other than cell 4.

(ii) *Rejected Opening Offers*. So far we have concentrated on first-period offers. The equilibrium prediction is that the first-period offers will be accepted. However, our subjects failed to reach agreement in the first period in 16 percent (125 out of 760) of the bargaining rounds of the experiment. As Figures 1A, 1B show, even in the tenth round, 13 percent (10 of 76) of the first-period offers were rejected.

The equilibrium prediction is that *if* a proposal is rejected by Player 2, then Player 2 will make a counterproposal that is at least as advantageous to himself as the proposal he just rejected. If the utility of a proposal is determined (only) by its cash value, then the observed pattern of counterproposals is inconsistent with the above prediction. In *101* of the 125 counterproposals offered by Player 2 (81 percent), *less* cash was demanded than had been offered by Player 1 in the rejected initial proposal. Figures 2A–2H display the distribution of first-period offers to Player 2, the distribution of offers which were rejected, and the distribution of the rejected offers which were followed by a disadvantageous counterproposal, that is, one in which player 2's counterproposal would give him a smaller monetary payoff than the proposal he had just rejected.

Note that, after player 1 has made a proposal, player 2 is faced with an individual choice problem. He may accept player 1's offer, in which case his payoff is certain, or he may reject it and make a counterproposal. If he chooses to reject and make a counterproposal, his payoff is uncertain, but will be at most the amount he demands for himself in his counterproposal. So when player 2 rejects player 1's offer and makes the kind of disadvantageous counterproposal we observe so frequently, we know by revealed preference that player 2's utility is *not* measured by his monetary payoff. So the high frequency of disadvantageous counterproposals makes it inappropriate to continue to interpret the monetary payoffs to the bargainers as being equivalent to their utility payoffs.

The pattern of rejections and counterproposals observed in this experiment is quite similar to those in the previous experiments discussed above. Table 6 tabulates the frequency with which first offers are rejected, and the percentage of rejections that are disadvantageous in monetary terms, for each of these experiments.[10] In these dimensions,

[10]These data were not formally analyzed in the reports of the previous experiments, but are derived from tables of the unaggregated data presented in Güth et al. (1982) and Neelin et al. (1988), and in the working paper version of Binmore et al. (1984). We take the opportunity to note what a useful practice it is to include tables of unaggregated data in reports of experimental work, since it permits other investigators to analyze the data from different perspectives. And there is a special place in heaven for journals that allow such tables to be published. (The unaggregated data for rounds one and ten of each cell of the present experiment are presented in Table 9 at the end of the text). The full data set is available from the authors upon request.

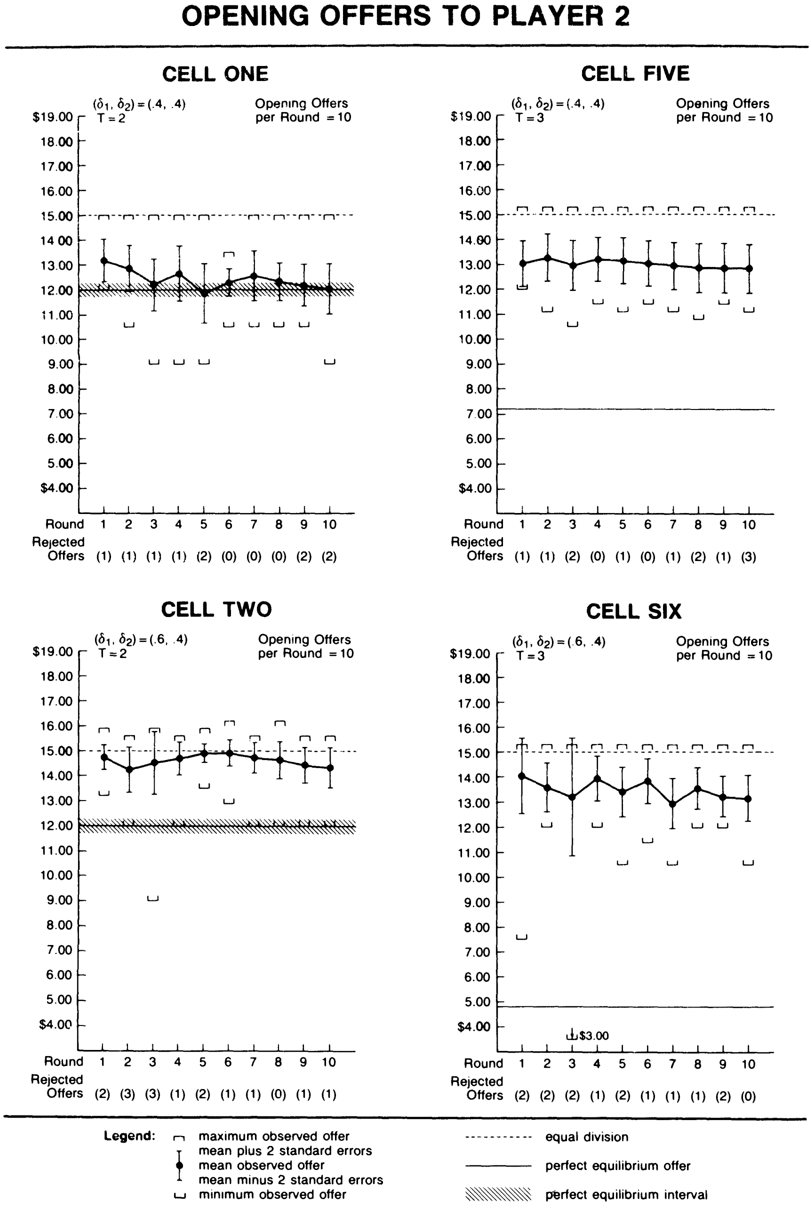

FIGURE 1A. OPENING OFFERS TO PLAYER 2 FOR CELLS ONE, TWO, FIVE, AND SIX

quite a striking similarity is revealed among this whole series of experiments. These similarities are even more striking in view of the differences reported in other aspects of these experiments, (and in view of the different numbers of observations in each experiment). The percentage of first-offer rejections for the multi-period experiments of Binmore et al., Neelin et al., and the present experiment are 15 percent, 14 percent, and 16 percent, respectively, while the percentage of these rejections that were followed by disad-

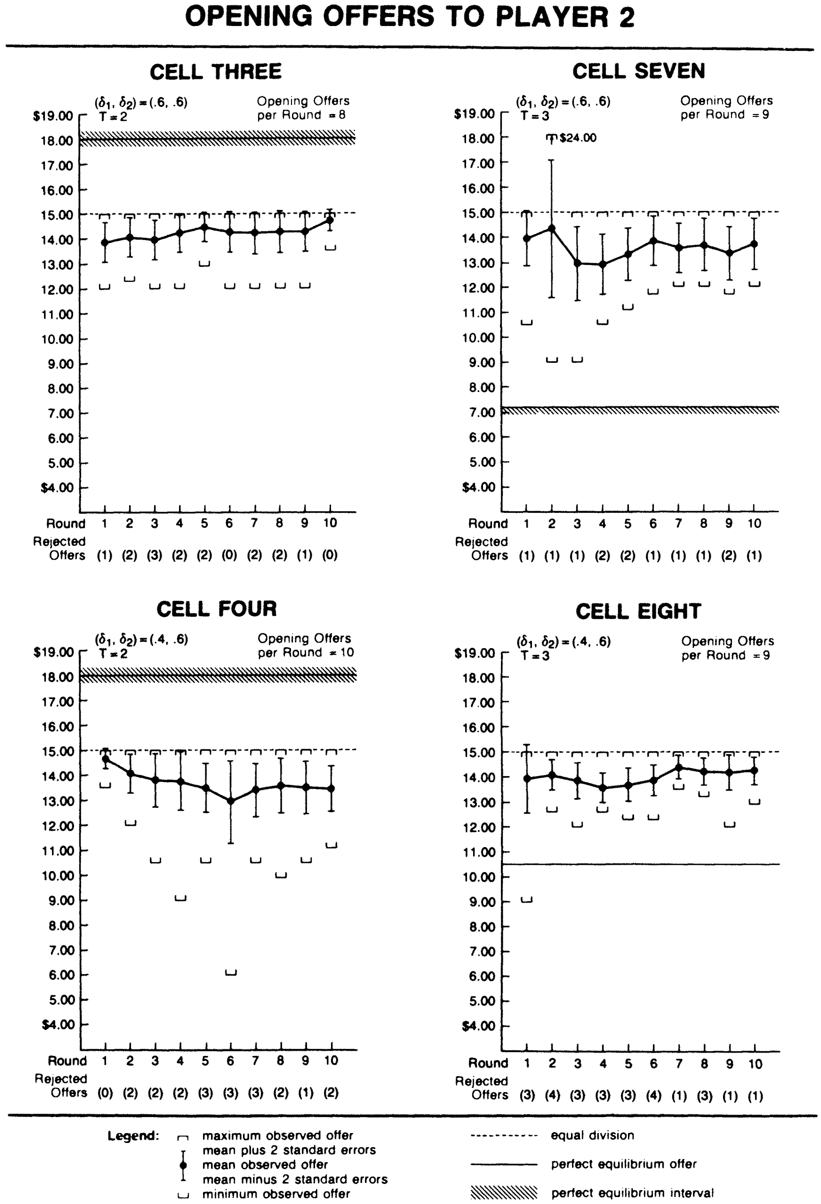

FIGURE 1B. OPENING OFFERS TO PLAYER 2 FOR CELLS THREE, FOUR, SEVEN, AND EIGHT

vantageous counterproposals are 75 percent, 65 percent, and 81 percent, respectively. These are quite close to the corresponding figures for the ultimatum games of Güth et al. (1982), where 19 percent of first offers are rejected, 88 percent disadvantageously. (These latter figures are not fully comparable to those of the multi-period games, since in an ultimatum game any first-offer rejection must lead to disagreement, and so all rejec-

TABLE 2—COMPARISONS ACROSS 2-PERIOD CELLS WITH COMMON DISCOUNT FACTORS (ROUND 10)

Cell A-Cell B	$\bar{x}_A - \bar{x}_B$	$\hat{\sigma}_{\bar{x}_A - \bar{x}_B}$	Degrees of Freedom	95 Percent Confidence Limits
1–2	−2.31	.6488	16	$-3.685 \leq \mu_1 - \mu_2 \leq -.935$
3–4	1.23	.4639	12	$.22 \leq \mu_3 - \mu_4 \leq 2.24$

$$\hat{\sigma}_{\bar{x}_A - \bar{x}_B} = \sqrt{\frac{S_A^2}{N_A} + \frac{S_B^2}{N_B}} \qquad DF \approx \frac{\left[\frac{S_A^2}{N_A} + \frac{S_B^2}{N_B}\right]^2}{\frac{\left(\frac{S_A^2}{N_A}\right)^2}{N_A - 1} + \frac{\left(\frac{S_B^2}{N_B}\right)^2}{N_B - 1}}$$

TABLE 3—DIFFERENCES ACROSS COLUMNS IN MEAN OPENING OFFERS TO PLAYER 2 (ROUND 10)

Cell A-Cell B	$\bar{x}_A - \bar{x}_B$	t-ratio	Degrees of Freedom	$t_{\alpha = .05}$
Cell 1–Cell 5	−.78	−1.173	18	1.734
Cell 2–Cell 6	1.17	2.027	18	1.734
Cell 3–Cell 7	1.0	1.917	10	1.812
Cell 4–Cell 8	−.76	−1.540	14	1.761

$$t = \frac{\bar{x}_A - \bar{x}_B}{\sqrt{\frac{S_A^2}{N_A} + \frac{S_B^2}{N_B}}} \qquad DF \approx \frac{\left[\frac{S_A^2}{N_A} + \frac{S_B^2}{N_B}\right]^2}{\frac{\left(\frac{S_A^2}{N_A}\right)^2}{N_A - 1} + \frac{\left(\frac{S_B^2}{N_B}\right)^2}{N_B - 1}}$$

tions of strictly positive offers are disadvantageous in monetary terms).[11]

So in these previous experiments, as well as in the present one, the monetary payoffs do not capture the utility of the bargainers. We shall argue in Section V that the unobserved element in the bargainers' utility function may have a component related to the perceived "fairness" of a proposal.

B. *Behavior in the Subgames*

When a first-period offer was rejected, the players entered a subgame. There were 65 observations of two-period subgames, corresponding to the 65 rejected first-period offers in the three-period games of cells 5–8. Table 7 presents information on the pattern of offers and responses in these subgames.

[11]The data from Güth and Teitz (1987) are not included in the table because it was incomparable in another way: recall from the description of that experiment that disadvantageous counterproposals were expressly forbidden by the rules of the game. Nevertheless, out of 42 observations, 17 first-period offers were refused (40 percent), of which 6 (35 percent) were disadvantageous counterproposals, *in spite of* the rule that such counterproposals would not be acted upon, but would simply count as disagreements.

TABLE 4—HYPOTHESIZED VS. OBSERVED DIFFERENCES IN OPENING OFFERS TO PLAYER 2 (ROUND 10)

Hypothesis	Observed Difference	Agreement in Direction
$\mu_1 = \mu_2$[a]	-2.31	–
$\mu_1 < \mu_3$	-2.66	Yes
$\mu_1 < \mu_4$	-1.44	Yes
$\mu_1 > \mu_5$	$-.78$	No
$\mu_1 > \mu_6$	-1.14	No
$\mu_1 > \mu_7$	-1.67	No
$\mu_1 > \mu_8$	-2.20	No
$\mu_2 < \mu_3$	$-.36$	Yes
$\mu_2 < \mu_4$	.87	No
$\mu_2 > \mu_5$	1.53	Yes
$\mu_2 > \mu_6$	1.17	Yes
$\mu_2 > \mu_7$	1.17	Yes
$\mu_2 > \mu_8$	.11	Yes
$\mu_3 = \mu_4$	1.23	–
$\mu_3 > \mu_5$	1.89	Yes
$\mu_3 > \mu_6$	1.53	Yes
$\mu_3 > \mu_7$	1.00	Yes
$\mu_3 > \mu_8$	.47	Yes
$\mu_4 > \mu_5$	.66	Yes
$\mu_4 > \mu_6$	.30	Yes
$\mu_4 > \mu_7$	$-.23$	No
$\mu_4 > \mu_8$	$-.76$	No
$\mu_5 > \mu_6$	$-.36$	No
$\mu_5 = \mu_7$	$-.87$	–
$\mu_5 < \mu_8$	-1.42	Yes
$\mu_6 < \mu_7$	$-.53$	Yes
$\mu_6 < \mu_8$	-1.06	Yes
$\mu_7 < \mu_8$	$-.53$	Yes

[a] μ_i = predicted (perfect equilibrium) offer in cell i.

TABLE 5—DIFFERENCES IN ROUND 1 AND ROUND 10 OPENING OFFERS BY CELL

Cell	$\bar{x}_1 - \bar{x}_{10}$	$\hat{\sigma}_{\bar{x}_1 - \bar{x}_{10}}$	t-ratio	$t_{\alpha=.05}$
One	1.14	.6248	1.825	2.262
Two	.39	.3178	1.227	2.262
Three	$-.825$	.413	-1.996	2.365
Four	1.12	.463	2.419	2.262
Five	.21	.637	.330	2.262
Six	.87	.831	1.047	2.262
Seven	.223	.706	.330	2.306
Eight	$-.33$	.653	.505	2.306

$$\hat{\sigma}_{\bar{x}_1 - \bar{x}_{10}} = \sqrt{\frac{s_1^2 + s_{10}^2}{N}} \qquad t = \frac{\bar{x}_1 - \bar{x}_{10}}{\hat{\sigma}_{\bar{x}_1 - \bar{x}_{10}}}$$

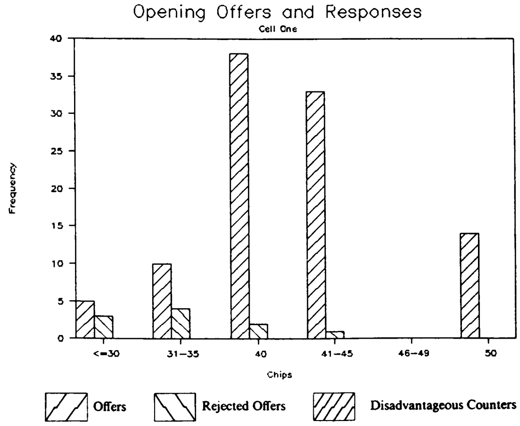

FIGURE 2A. OPENING OFFERS AND RESPONSES FOR CELL ONE

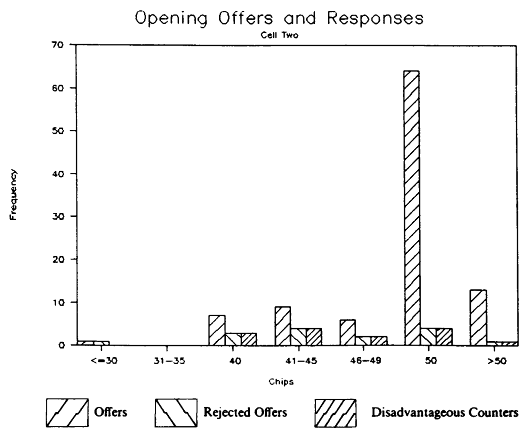

FIGURE 2B. OPENING OFFERS AND RESPONSES FOR CELL TWO

Note first that 24 of the 65 opening offers in these subgames were rejected and that in 16 (67 percent) of these cases player 1's subsequent cash demand was for less than the amount the player had just rejected. Second, like the observed first-period offers, these offers reflect a perceived first-mover advantage in that the maximum offer made by player 2 (the period 2 proposer) to player 1 never exceeded an equal division of cash offer even though in two of these four cases the perfect equilibrium offer exceeded the equal division offer. Third, in Cells Five and Eight, where the perfect equilibrium offer is less than the equal division offer, the average offer is above the perfect equilibrium offer. Conversely, in Cells Six and Seven, where the perfect equilibrium offer is above the

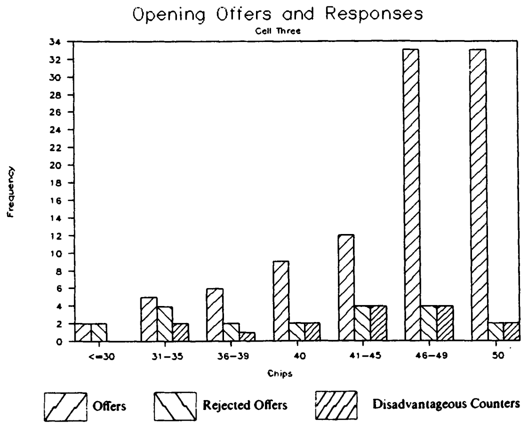

FIGURE 2D. OPENING OFFERS AND RESPONSES FOR CELL THREE

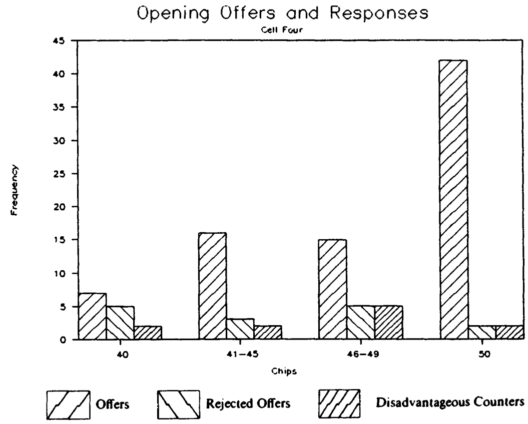

FIGURE 2C. OPENING OFFERS AND RESPONSES FOR CELL FOUR

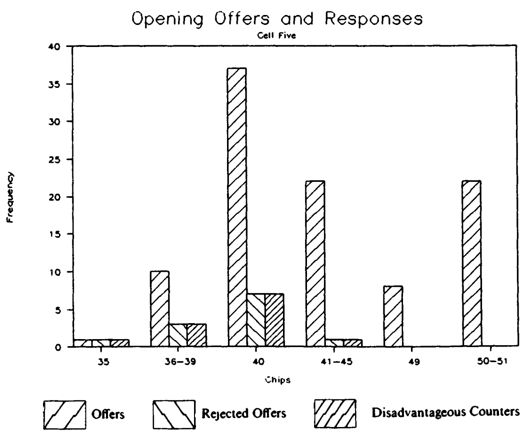

FIGURE 2E. OPENING OFFERS AND RESPONSES FOR CELL FIVE

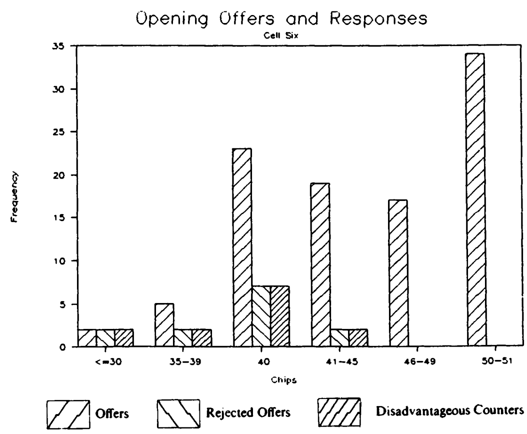

FIGURE 2F. OPENING OFFERS AND RESPONSES FOR CELL SIX

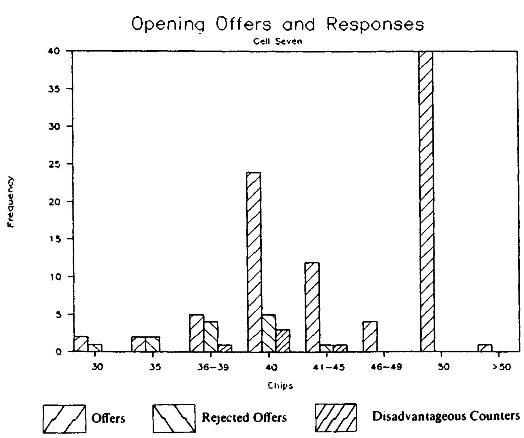

FIGURE 2G. OPENING OFFERS AND RESPONSES FOR CELL SEVEN

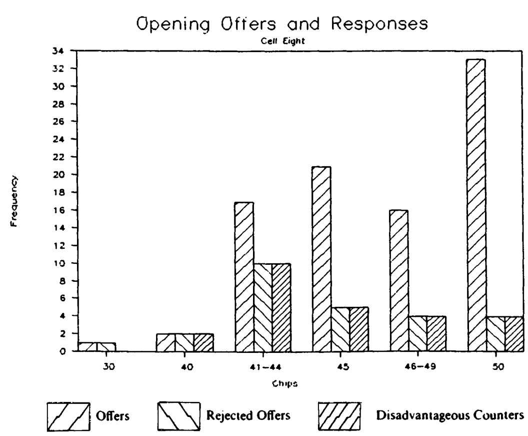

FIGURE 2H. OPENING OFFERS AND RESPONSES FOR CELL EIGHT

TABLE 6—FIRST-OFFER REJECTIONS AND DISADVANTAGEOUS COUNTERPROPOSALS

	Observations	First-Offer Rejections	Disadvantageous Counterproposals
Ochs and Roth	760	$\left(\frac{125}{760}\right)$ 16 percent	$\left(\frac{101}{125}\right)$ 81 percent
Binmore, Shaked, and Sutton			
Game 1	81	$\left(\frac{12}{81}\right)$ 15 percent	$\left(\frac{9}{12}\right)$ 75 percent
Game 2		NA[a]	
Neelin, Sonnenschein, and Spiegel			
Experiment 1	120	$\left(\frac{16}{120}\right)$ 13 percent	$\left(\frac{9}{16}\right)$ 56 percent
Experiment 2	45	$\left(\frac{7}{45}\right)$ 16 percent	$\left(\frac{6}{7}\right)$ 86 percent
Experiments 1 and 2	165	$\left(\frac{23}{165}\right)$ 14 percent	$\left(\frac{15}{23}\right)$ 65 percent
Güth, Schmittberger, and Schwarz, 1982 (Ultimatum Games)			
"Naive"	21	$\left(\frac{2}{21}\right)$ 10 percent	$\left(\frac{1}{2}\right)^{b}$ 50 percent
"Experienced"	21	$\left(\frac{6}{21}\right)$ 29 percent	$\left(\frac{6}{6}\right)$ 100 percent
Naive and Experienced	42	$\left(\frac{8}{42}\right)$ 19 percent	$\left(\frac{7}{8}\right)^{b}$ 88 percent

[a]There was no second player in this game.
[b]One of the rejections was of a (100,0) division, so the rejection was not disadvantageous.

TABLE 7—TWO-PERIOD SUBGAMES

	Cell Five	Cell Six	Cell Seven	Cell Eight
No. of 2-Period Subgames	12	14	13	26
No. of Rejected Opening Offers	2	6	7	9
No. of Disadvantageous Counteroffers	2	3	3	8
Minimum Opening Offer	\$4.80	\$3.60	\$2.88	\$3.60
Maximum Opening Offer	\$6.00	\$7.20	\$8.01	\$7.20
Average Opening Offer	\$5.50	\$6.146	\$6.065	\$5.88
Variance of Opening Offers	\$0.1868	\$0.9575	\$4.5329	\$1.057
Minimum Accepted Offer	\$4.80	\$5.94	\$6.30	\$4.56
Perfect Equilibrium Offer	\$5.04	\$11.16	\$11.16	\$5.04
Equal Division of Cash Offer	\$6.00	\$7.20	\$9.00	\$7.20

TABLE 8—ULTIMATUM SUBGAMES

	Cell (1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
No. of Games	10	15	15	20	2	6	7	9
No. End in Disagreement	7	7	6	6	1	4	4	4
No. End in Agreement	3	8	9	14	1	2	3	5
Minimum Offer	.12	.18	3.60	3.60	1.00	0	1.10	2.20
Maximum Offer	6.00	9.00	9.00	7.20	2.50	3.45	5.50	4.40
Average Offer	2.844	5.184	7.14	5.556	1.75	1.025	4.02	3.434
Variance in Offers	4.335	5.759	3.264	.664	.5625	1.544	3.014	.319
Minimum Accepted Offer	4.20	5.04	4.50	4.80	2.50	3.45	4.40	3.30
Equal Division Offer	6.00	7.20	9.00	7.20	2.50	3.41–3.45	5.50	3.41–3.45

TABLE 9—UNAGGREGATED DATA FOR ROUNDS ONE AND TEN

idr =identification # of Player I;
idl =identification # of Player II;
p1. rc=period-one demand of player I, in cash;
p1. lc=period-one offer to player II, in cash;
p1. a="a" if period-one offer is accepted; ="r" if rejected;
p2. rc=period-two offer to player I, in cash;
p2. lc=period-two demand by player II, in cash;
p2. a="a" if period-two offer is accepted; ="r" if rejected; ="d" if period-one offer was accepted;
p3. rc=period-three demand of player I, in cash;
p3. lc=period-three offer to player II, in cash;
p3. a="a" if period-three offer is accepted; ="r" if rejected; ="d" if an earlier period proposal was accepted.

Cell One, Player I's Discount Factor =.4,
Player II's Discount Factor =.4, Two Periods

Round 1

idr	idl	p1. rc	p1. lc	p1. a	p2. rc	p2. lc	p2. a
0	9	15.	15.	a			d
1	8	17.7	12.3	a			d
2	7	15.	15.	a			d
3	6	18.	12.	a			d
4	5	15.	15.	a			d
5	4	18.	12.	a			d
6	3	18.	12.	a			d
7	2	18.	12.	a			d
8	1	17.1	12.9	r	6.	6.	a
9	0	16.5	13.5	a			d

Round 10

idr	idl	p1. rc	p1. lc	p1. a	p2. rc	p2. lc	p2. a
0	8	19.5	10.5	a			d
1	7	17.7	12.3	a			d
2	6	18.	12.	r	3.6	8.4	r
3	5	21.	9.	r	1.2	10.8	r
4	4	15.	15.	a			d
5	3	18.	12.	a			d
6	2	18.	12.	a			d
7	1	18.	12.	a			d
8	0	16.5	13.5	a			d
9	9	18.	12.	a			d

TABLE 9—CONTINUED

Cell Two, Player I's Discount Factor = .6,
Player II's Discount Factor = .4, Two Periods

Round 1

idr	idl	p1. rc	p1. lc	p1. a	p2. rc	p2. lc	p2. a
0	0	16.8	13.2	a			d
1	9	15.	15.	a			d
2	8	15.	15.	r	5.4	8.4	a
3	7	14.1	15.9	a			d
4	6	15.	15.	a			d
5	5	15.	15.	a			d
6	4	15.	15.	a			d
7	3	15.3	14.7	a			d
8	2	15.	15.	a			d
9	1	16.5	13.5	r	9.	6.	a

Round 10

idr	idl	p1. rc	p1. lc	p1. a	p2. rc	p2. lc	p2. a
0	9	15.	15.	a			d
1	8	15.	15.	a			d
2	7	15.	15.	a			d
3	6	17.7	12.3	a			d
4	5	15.	15.	a			d
5	4	18.	12.	r	7.2	7.2	a
6	3	15.	15.	a			d
7	2	14.4	15.6	a			d
8	1	15.	15.	a			d
9	0	16.5	13.5	a			d

Cell 3, Player I's Discount Factor = .6,
Player II's Discount Factor = .6, Two Periods

Round 1

idr	idl	p1. rc	p1. lc	p1. a	p2. rc	p2. lc	p2. a
0	7	15.	15.	a			d
1	6	15.	15.	a			d
2	5	15.9	14.1	a			d
3	4	17.1	12.9	a			d
4	3	15.	15.	a			d
5	2	16.5	13.5	a			d
6	1	18.	12.	r	9.	9.	a
7	0	16.5	13.5	a			d

Round 10

idr	idl	p1. rc	p1. lc	p1. a	p2. rc	p2. lc	p2. a
0	6	15.	15.	a			d
1	5	15.	15.	a			d
2	4	15.	15.	a			d
3	3	15.9	14.1	a			d
4	2	15.	15.	a			d
5	1	15.	15.	a			d
6	0	15.	15.	a			d
7	7	16.5	13.5	a			d

TABLE 9—CONTINUED

Cell 4, Player I's Discount Factor = .4,
Player II's Discount Factor = .6, Two Periods

Round 1

idr	idl	p1. rc	p1. lc	p1. a	p2. rc	p2. lc	p2.a
0	9	15.	15.	a			d
1	8	16.5	13.5	a			d
2	7	15.	15.	a			d
3	6	15.	15.	a			d
4	5	15.	15.	a			d
5	4	15.	15.	a			d
6	3	16.5	13.5	a			d
7	2	15.	15.	a			d
8	1	15.	15.	a			d
9	0	15.3	14.7	a			d

Round 10

idr	idl	p1. rc	p1. lc	p1. a	p2. rc	p2. lc	p2. a
0	8	15.9	14.1	a			d
1	7	16.5	13.5	a			d
2	6	18.	12.	r	4.8	10.8	r
3	5	15.3	14.7	a			d
4	4	15.9	14.1	a			d
5	3	15.	15.	a			d
6	2	18.3	11.7	a			d
7	1	16.5	13.5	a			d
8	0	15.	15.	a			d
9	9	18.9	11.1	r	6.	9.	a

Cell 5, Player I's Discount Factor = .4,
Player II's Discount Factor = .4, Three Periods

Round 1

idr	idl	p1. rc	p1. lc	p1. a	p2. rc	p2. lc	p2. a	p3. rc	p3. lc	p3. a
0	9	17.4	12.6	r	5.76	6.24	a			d
1	8	18.	12.	a			d			d
2	7	17.7	12.3	a			d			d
3	6	18.	12.	a			d			d
4	5	18.	12.	a			d			d
5	4	14.7	15.3	a			d			d
6	3	15.	15.	a			d			d
7	2	18.	12.	a			d			d
8	1	15.	15.	a			d			d
9	0	18.	12.	a			d			d

Round 10

idr	idl	p1. rc	p1. lc	p1. a	p2. rc	p2. lc	p2. a	p3. rc	p3. lc	p3. a
0	8	17.7	12.3	a			d			d
1	7	18.	12.	a			d			d
2	6	18.9	11.1	r	6.	6.	a			d
3	5	18.	12.	a			d			d
4	4	18.3	11.7	r	5.04	6.96	a			d
5	3	14.7	15.3	a			d			d
6	2	15.3	14.7	a			d			d
7	1	18.	12.	a			d			d
8	0	15.	15.	a			d			d
9	9	18.	12.	r	5.76	6.24	a			d

Table 9—Continued

Cell 6, Player I's Discount Factor = .6,
Player II's Discount Factor = .4, Three Periods

Round 1

idr	idl	p1. rc	p1. lc	p1. a	p2. rc	p2. lc	p2. a	p3. rc	p3. lc	p3. a
0	9	16.5	13.5	r	6.3	7.8	a			d
1	8	15.	15.	a			d			d
2	7	15.	15.	a			d			d
3	6	15.	15.	a			d			d
4	5	15.9	14.1	a			d			d
5	4	15.	15.	a			d			d
6	3	14.7	15.3	a			d			d
7	2	15.	15.	a			d			d
8	1	15.	15.	a			d			d
9	0	22.5	7.5	r	3.6	9.6	r	6.05	2.25	r

Round 10

idr	idl	p1. rc	p1. lc	p1. a	p2. rc	p2. lc	p2. a	p3. rc	p3. lc	p3. a
0	8	16.5	13.5	a			d			d
1	7	16.5	13.5	a			d			d
2	6	15.	15.	a			d			d
3	5	19.5	10.5	a			d			d
4	4	17.4	12.6	a			d			d
5	3	16.2	13.8	a			d			d
6	2	14.7	15.3	a			d			d
7	1	18.	12.	a			d			d
8	0	16.5	13.5	a			d			d
9	9	18.	12.	a			d			d

Cell 7, Player I's Discount Factor = .6,
Player II's Discount Factor = .6, Three Periods

Round 1

idr	idl	p1. rc	p1. lc	p1. a	p2. rc	p2. lc	p2. a	p3. rc	p3. lc	p3. a
0	8	15.	15.	a			d			d
1	7	15.	15.	a			d			d
2	6	15.	15.	a			d			d
3	5	15.9	14.1	a			d			d
4	4	18.	12.	a			d			d
5	3	16.2	13.8	a			d			d
6	2	15.	15.	a			d			d
7	1	15.	15.	a			d			d
8	0	19.5	10.5	r	6.3	11.7	a			d

Round 10

idr	idl	p1. rc	p1. lc	p1. a	p2. rc	p2. lc	p2. a	p3. rc	p3. lc	p3. a
0	7	15.	15.	a			d			d
1	6	15.	15.	a			d			d
2	5	15.	15.	a			d			d
3	4	17.7	12.3	a			d			d
4	3	18.	12.	r	8.1	9.9	a			d
5	2	18.	12.	a			d			d
6	1	15.	15.	a			d			d
7	0	15.	15.	a			d			d
8	8	18.	12.	a			d			d

TABLE 9—CONTINUED

Cell 8, Player I's Discount Factor = .4,
Player II's Discount Factor = .6, Three Periods

Round 1

idr	idl	p1. rc	p1. lc	p1. a	p2. rc	p2. lc	p2. a	p3. rc	p3. lc	p3. a
0	8	15.	15.	a			d			d
1	7	17.4	12.6	a			d			d
2	6	21.	9.	r	6.	9.	a			d
3	5	15.	15.	a			d			d
4	4	15.	15.	a			d			d
5	3	15.	15.	a			d			d
6	2	16.5	13.5	r	4.8	10.8	r	3.5	3.3	r
7	1	15.	15.	r	5.76	9.36	a			d
8	0	15.	15.	a			d			d

Round 10

idr	idl	p1. rc	p1. lc	p1. a	p2. rc	p2. lc	p2. a	p3. rc	p3. lc	p3. a
0	7	15.	15.	a			d			d
1	6	15.9	14.1	a			d			d
2	5	17.1	12.9	r	4.56	11.16	a			d
3	4	15.9	14.1	a			d			d
4	3	15.	15.	a			d			d
5	2	15.	15.	a			d			d
6	1	16.5	13.5	a			d			d
7	0	16.5	13.5	a			d			d
8	8	15.	15.	a			d			d

idr = identification # of Player I;
idl = identification # of Player II;
p1. rc = period-one demand of player I, in cash;
p1. lc = period-one offer to player II, in cash;
p1. a = "a" if period-one offer is accepted; = "r" if rejected;
p2. rc = period-two offer to player I, in cash;
p2. lc = period-two demand by player II, in cash;
p2. a = "a" if period-two offer is accepted; = "r" if rejected; = "d" if period-one offer was accepted;
p3. rc = period-three demand of player I, in cash;
p3. lc = period-three offer to player II, in cash;
p3. a = "a" if period-three offer is accepted; = "r" if rejected; = "d" if an earlier period proposal was accepted.

equal division offer, the average offer is below the perfect equilibrium offer (and below the equal division offer). Therefore, as in the case of the observed first-period offers, the deviation of the average offer from the perfect equilibrium offer is always in the direction of equal division. Fourth, minimally acceptable offers tended to be positively related to the cash value of the equal division of cash offer.

In Cells One through Four, games which did not reach agreement in the first period continued into an ultimatum (one-period) subgame. There were 60 such games which entered into an ultimatum stage. In Cells Five through Eight, 24 games failed to reach agreement by the end of period two and entered into an ultimatum subgame. Table 8 displays the data for these ultimatum subgames. Notice that 38 of these 84 ultimatum subgames ended in disagreement. Both the average offer made and the minimum offer accepted in these subgames is consistently below the offer that represents an equal division of the cash between players 1 and 2. Furthermore, in Cells Five through Eight, where the total amount of cash potentially available to be divided was the smallest, tolerable deviations (i.e., those that were accepted) from an equal division were smallest.

C. *Other Observed Regularities*

In addition to these results which are at odds with the subgame-perfect equilibrium prediction, there are several regularities in the data to which we call the reader's attention.

In six of the eight cells in the experiment at least half of the first round period-one offers to Player 2 were for between 45 and 50 percent of the available cash. Experience did not significantly diminish the incidence of 50–50 offers. In round ten, at least 50 percent of the opening offers to Player 2 fell in this range for the same six cells. Furthermore, in four of these six cells there were always at least half of the first-period offers in this range. And in all but Cell 2, the maximum offer in each round was almost always very close to equal division (see Figures 1A, 1B).

Three main types of individual behavior over rounds are reflected in the data. There is one type of player 1 who *never* offers player 2 less than 50 percent of the chips in period one. Sixteen of the 76 subjects (21 percent) who had the role of player 1 in our experiment behaved in this way. A second type of behavior is of the variety where the period-one offer made to player 2 in round one is not the smallest opening offer ever made and where the opening offer made in round $t+1$ is never greater than the opening offer made in round t unless the round t opening offer was rejected. Twenty-eight of the individuals (36.8 percent of the total) who had the role of player 1 exhibited this type of behavior. The third main type of behavior is characterized by making a round-one opening offer which is both an offer of less than 50 percent of the chips and is also the smallest opening offer the individual ever makes. There were 14 (18.4 percent of all subjects) of our subjects whose opening offers displayed this pattern. The first type of behavior has no apparent learning component to it. The second type might be characterized as a cautious search for the lowest acceptable offer. The third type of behavior is exhibited by individuals who are apparently optimistic at the outset that they can exploit what they believe to be a "first-mover" advantage and either never increase their opening offer or who respond to rejection by increasing their opening offer. Because both type-two and type-three behavior are exhibited in the same groups, the aggregate data (Table 5) mask the volume of adaptive behavior which was exhibited by a substantial proportion of the subjects in our experiment.

Proposals which offered Player 2 at least 50 percent of the available money were almost never rejected. There were 296 such proposals, only 13 of which were rejected. Player 2 was slightly more likely to reject an offer when the subgame-perfect equilibrium required that he get 60 percent of the available cash than when the equilibrium required that he get 40 percent or less. In cells 3 and 4, 19.4 percent of the 180 offers made to Player 2 were rejected while in the other cells 15.5 percent of the 580 opening offers were rejected.

It was not profitable to be aggressive in making counterproposals. There were only 20 counterproposals in which Player 2 demanded at least \$11. Sixteen of these 20 counterproposals were rejected. Altogether, 50 of the 125 counterproposals were rejected. In only 12 of these rejected counterproposals was Player 1 offered at least 67 percent of the cash demanded by Player 2. The mean cash demand of Player 2 in the rejected counterproposals was \$10.50 while the mean cash demand by Player 2 of the accepted counterproposals was only \$8.44.

Even though Player 1 had a theoretical strategic advantage in all cells other than cells three and four, aggressive exploitation of this advantage was not, in fact, profitable to Player 1. Figures 3A–3H display the relationship between Player 1's average cash earnings per round and the average cash value of his opening demands for each of the cells in the experiment. Notice that in every cell the highest average earnings are associated with an individual who made less than the highest cash demands. In each cell, the player with the highest average opening demand had average earnings below the average earnings for the cell as a whole. In four of the eight cells, the player with the highest average demand had the *lowest* average earnings.

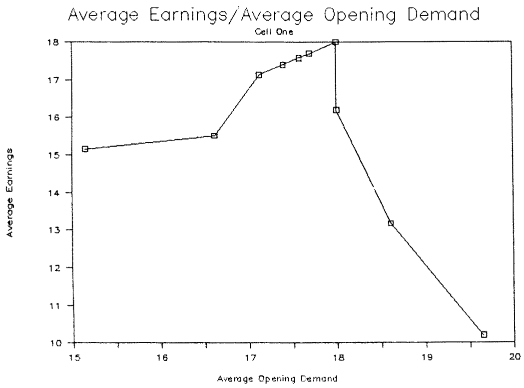

FIGURE 3A. AVERAGE EARNINGS/AVERAGE OPENING DEMAND FOR CELL ONE

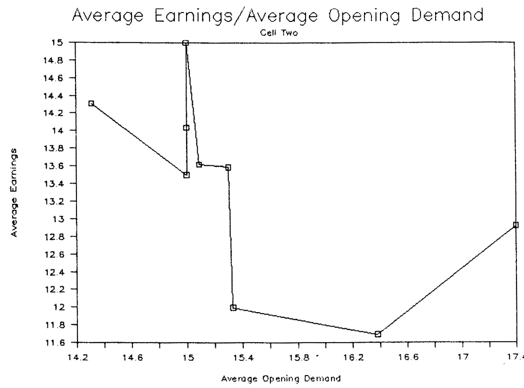

FIGURE 3B. AVERAGE EARNINGS/AVERAGE OPENING DEMAND FOR CELL TWO

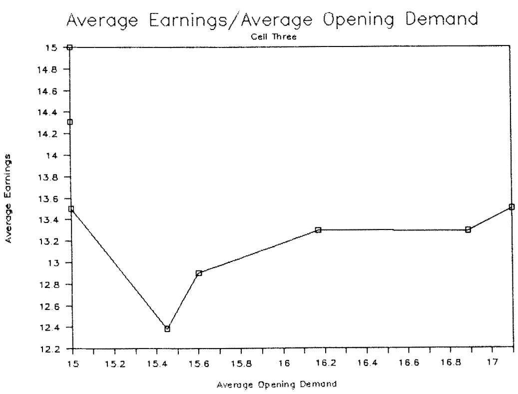

FIGURE 3C. AVERAGE EARNINGS/AVERAGE OPENING DEMAND FOR CELL THREE

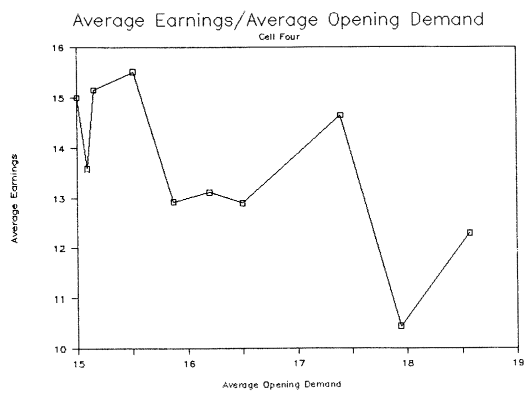

FIGURE 3D. AVERAGE EARNINGS/AVERAGE OPENING DEMAND FOR CELL FOUR

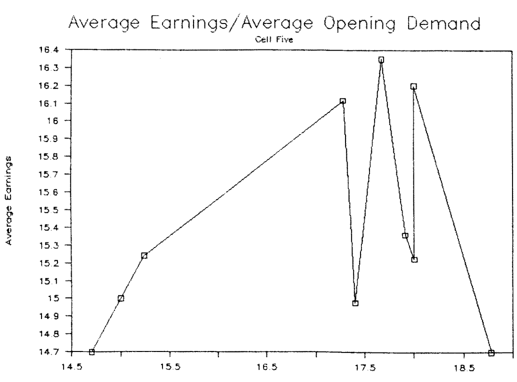

FIGURE 3E. AVERAGE EARNINGS/AVERAGE OPENING DEMAND FOR CELL FIVE

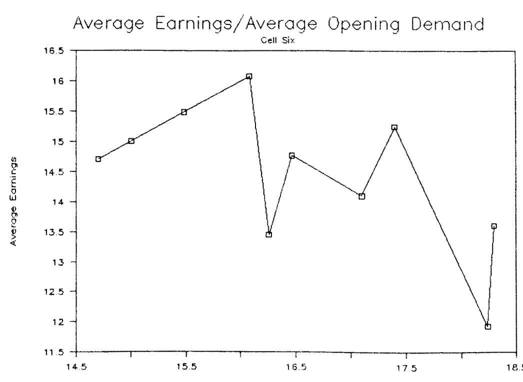

FIGURE 3F. AVERAGE EARNINGS/AVERAGE OPENING DEMAND FOR CELL SIX

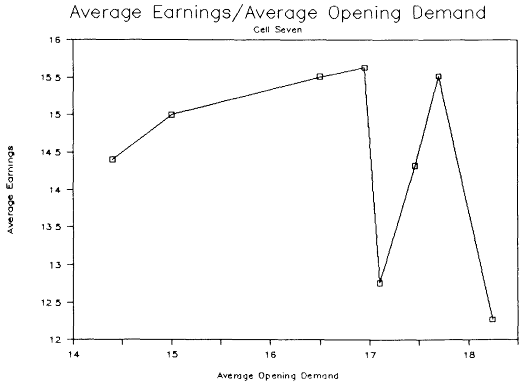

FIGURE 3G. AVERAGE EARNINGS/AVERAGE OPENING DEMAND FOR CELL SEVEN

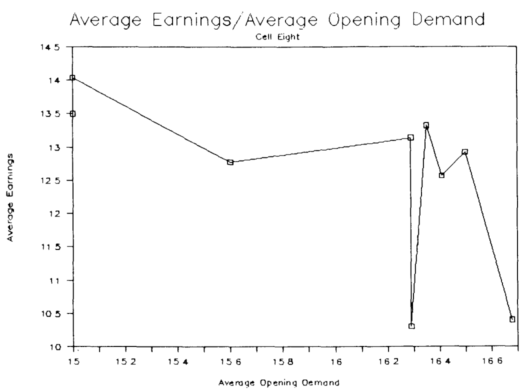

FIGURE 3H. AVERAGE EARNINGS/AVERAGE OPENING DEMAND FOR CELL EIGHT

IV. Making Sense of the Data

The high frequency of disadvantageous counterproposals makes clear that there are nonmonetary arguments in the bargainers' utility functions. As this phenomenon seems to occur in the data of a variety of experiments (Table 6), it merits serious attention.

Of course, any important nonmonetary components of the bargainers' utility functions could account for the failure of the perfect equilibrium predictions, since these are made under the assumption that the bargainers' utilities are identical to their monetary payoffs. (In this sense, we conclude that these experiments fail to test perfect equilibrium per se). But not just any nonmonetary components could account for the specific regularities we observe in our data. We turn now to consider what kinds of nonmonetary arguments can account both for the failure of the perfect equilibrium predictions and for the observed regularities. At this point these considerations must necessarily be somewhat speculative, since these nonmonetary arguments are neither observed nor controlled for in either these or the previous experiments.

We will concentrate on five observed, unpredicted regularities (see Figures 1A, B):

1. A consistent first-mover advantage was observed in all the cells of this experiment (both in the first period and in the subgames).
2. The discount factor of player 1 was observed to influence the outcome even in the two-period games.
3. A substantial percentage of first offers were rejected.
4. The observed mean agreements deviate from the equilibrium predictions in the direction of equal division.
5. A substantial percentage of rejected offers were followed by disadvantageous counterproposals.

A. *A (Too) Simple Model of Minimum Acceptance Thresholds*

We begin with a model simple enough to allow us to solve for perfect equilibria under alternative assumptions about bargainers' utilities. This will allow us to illustrate how nonmonetary components of utility can enter the model in a way that can account for the first three of the above five unpredicted regularities. But the fourth and fifth regularities will force us to consider more complicated kinds of utility functions.

The motivation is the following. Suppose agents regard some offers as "insultingly low," and that there is a disutility to accepting such offers. This utility could take many forms, but for simplicity we suppose here that it takes the form of a simple monetary threshold: each player i has some threshold t_i, in dollars, such that he will refuse offers of less than $\$t_i$. That is, a bargainer's utility function is such that the disutility of accepting a low offer is greater than the utility of increasing his wealth by less than $\$t_i$.

In this case, in contrast to the case when the minimum acceptable offer at the last period is taken to be equal to the $0 disagreement payoff, the discount factors of both bargainers matter even in 2-period games. For example, when player 1 will not accept less than $3.00, the perfect equilibrium payoff is ($15,$15) in cell 3 ($\delta_1 = .6, \delta_2 = .6$), but is ($16.5,$13.5) in cell 4 ($\delta_1 = .4, \delta_2 = .6$).[12] These payoffs are rather close to the mean observed agreements in round 10 of cells 3 and 4 (see Figures 1A, 1B.)

Furthermore, minimum acceptance thresholds of this magnitude are consistent with a first-mover advantage at perfect equilibrium in all eight cells of this experiment. Finally, if bargainers' threshold levels are private information, the bargainers are playing a game of incomplete information, in which case theory is consistent with the prediction that not all first-period offers will be accepted (see, for example, some of the models in Roth, 1985).

So, if we looked only at the first three observed regularities, we might hope that the uncontrolled elements in the utility of the players would simply involve minimum acceptance thresholds of this kind and magnitude. But when we look at the last two of the above-mentioned regularities, cells 1 and 2 show that matters are not so simple.

Consider cells 1 and 2, with discount factors ($\delta_1 = .4, \delta_2 = .4$) and ($\delta_1 = .6, \delta_2 = .4$), respectively. When utility can be measured by the monetary payoff, the perfect equilibrium prediction is that player 2 will receive $12 in each cell, and in fact we observe (see Figures 1A, B) that the mean first-period offer to player 2 is greater than this in all rounds of cell 2, and in all rounds but round 5 of cell 1. If player 1 will not accept less than $3.00, then the perfect equilibrium predictions become ($21,$9) for cell 1, and ($19.80,$10.20) for cell 2. While this is consistent with the observation that player 2 does better in cell 2 than in cell 1, these predictions are *further* from equal division than are the standard equilibrium predictions, while the observed outcomes were *closer* to equal division.

So, while the "minimum acceptance threshold theory" of bargainers' utilities is at least roughly consistent with observations in six of the eight cells of this experiment, in two of the cells it fails to account for one of the clear regularities observed in both this experiment and many earlier experiments, namely, that many observed offers and agreements are approximately equal divisions.[13] And in all of the cells it fails to account for the high percentage of disadvantageous counterproposals, since rejections caused by a minimum monetary threshold would always be followed by a counterproposal demanding more than the threshold. Thus, while there is a lot of intuitive plausibility to the notion that this kind of threshold may play some role in bargaining,[14] we are forced to conclude that it is not sufficient by itself to account for the observed regularities.

So we might speculate that the uncontrolled elements of utility include some component that measures "unfairness" as deviations from equal division, for example, by imposing a minimum acceptance threshold which takes the form of a minimum *percent-*

[12] The computation works as follows. In cell 3, player 1's chips are worth $0.18 each in the second period, so in the second period player 2 must offer him 17 chips ($3.06) to meet the minimal acceptable amount of $3.00, leaving 83 chips (worth $14.94) to player 2. So in period 1, player 1 must offer player 2 50 chips, worth $15, in order to have him accept rather than reject and go to period 2. In cell 4, player 1's chips are worth only $0.12 in the second period, so player 2 must offer him 25 chips ($3.00), leaving 75 chips (worth $13.50) to player 2. So in the first period player 1 must offer player 2 45 chips, worth $13.50.

[13] It could of course be argued that six out of eight cells is not too bad, and that perhaps random variation accounted for the fact that the observed outcomes deviated from the direction predicted by the "minimum threshold theory" in two cells. This argument fails to take into account that the preponderance of equal and near equal divisions is one of the most consistent regularities in both this and previous experiments.

[14] For example, the back cover of the December 1986 issue of the *Journal of Political Economy* contained a brief account of an Israeli taxicab driver who, insulted by being offered an unexpectedly low fare at the end of an unmetered journey, took his (economist) passengers back to their starting point.

age of the available commodities. This would hamper the ability of player 1 to fully exploit the standard perfect equilibrium arithmetic, not to mention the even larger first-mover advantage that appears when a minimum acceptable monetary threshold is introduced. We consider below the consistency of such utility functions with the data.

B. *When Deviations from 50–50 Are Important*

Suppose players 2 would tolerate only some maximum deviation of an opening offer from an equal division of the available cash. This threshold will vary across individuals and can be empirically estimated for those Player 2s whose maximum rejected opening offer is less than their respective minimum accepted opening offer. There were 48 different Player 2 subjects who rejected at least one opening offer. Of this group there were 35 for whom the lowest first offer they ever accepted was no less than the highest first offer they ever rejected. Suppose we set the estimated "deviation threshold" for each of these individuals to be the mean of these two numbers. The level of these thresholds appears to have a systematic effect on the opening offers of experienced subjects. This is reflected in the regression between the mean round-ten opening offers to Player 2 across the cells of our experiment and the median threshold levels across these cells. Equation (2) presents this regression.

$$(2) \quad \text{Observed Mean Offer} = 3.378 + .8287 \text{ Median Threshold}$$

$$R^2 = .7126 \text{ (Std. Error} = .2148).$$

Furthermore, there is some evidence that second-period proposals made by players 2 who rejected a first-period proposal were sensitive to how "unfair" the initial proposal had been. If a proposal which contains equal cash values for both players is "fair," then Player 2 made counterproposals that were as "unfair" as the initial proposals of Player 1.

It is easy to verify in addition that the first four observed regularities discussed above are all consistent with a model in which bargainers reject offers that deviate too much from equal division, as of course is the additional observed regularity regarding the frequency of disadvantageous counterproposals in this and previous experiments (Table 6).

V. Conclusions

Figures 1A, 1B and Table 6 convey much of what has been learned here. Figures 1A, 1B make clear that the subgame-perfect equilibrium predictions that come from assuming that players' monetary payoffs are a good proxy for their utility payoffs are not at all descriptive of the results we observed. This is true not merely of the point predictions, as has been observed by some of the earlier experimenters to investigate this kind of bargaining, but also of the qualitative predictions about how the results in different cells should be related. But there is a great deal of regularity in the observed behavior, and indeed there is much more similarity among the observed outcomes in the eight cells than there is in the perfect equilibrium predictions for those cells.

There is also a high frequency of disadvantageous counterproposals (Figure 2 and Tables 7 and 8), and Table 6 shows that this is true of the previous experiments also. This previously overlooked feature of the data is central to our conclusion that the monetary payoffs are not a good proxy for players' utilities. We have shown how many of the observed regularities in the data can be reconciled with a theory in which bargainers incorporate distributional concerns (namely, comparisons of how large a proportion of the available wealth is received by each bargainer) directly into their utility functions.

To the extent that players may have distributional concerns in their utility functions, both the behavioral regularities observed within these various experiments, and perhaps some of the marked differences between them, may share a common cause. The reason is that individual's ideas about "fairness" seem to be both clear (see, for example, Daniel Kahneman, Jack Knetch, and Richard Thaler (1986a, b)) and highly sensitive to the context in which the issue arises (on which point see the excellent study of Menachem Yaari and Maya Bar-Hillel, 1984). If ideas about fairness play a significant role in players' utility functions, their clarity would help account for the regular behavior often observed within each of the previous experiments discussed here, as well as in our own. But the sensitivity of these ideas to specific contexts could well mean that the differences in experimental environments, subject pools, and instructions[15] employed in different experiments[16] could have much larger effects than would be anticipated if bargainers' own monetary payoffs were the only determinant of their utility.

All this is not to suggest that all or even most of the similarities and differences in the interpretations of earlier experiments can be traced to uncontrolled elements of bargainers' utilities. In this regard, note that many parts of the data gathered in the present experiment are consistent with observations made in earlier experiments, but that because of the somewhat larger experimental design employed here, we interpret the data differently.[17]

Notice also that we do not conclude that players "try to be fair." It is enough to suppose that they try to estimate the utilities of the player they are bargaining with, and that, as discussed in the previous section, at least some agents incorporate distributional considerations in their utility functions. Since offers (not to mention agreements) reflect a bargainer's estimate of his opponent's behavior, they do not directly reveal anything about the utility of either individual.[18] However, the data on rejections and counterproposals are at least in part data about individual choice, and Table 6 shows that, both in this experiment and in the previous ones we have discussed, the utilities cannot simply be assumed to be equal to the monetary payoffs of the players. The extent to which this would remain true if the bargaining concerned much larger monetary payoffs is of course an empirical question, but we see no obvious reason to jump to the conclusion that the very consistent pattern of behavior observed here would disappear as the stakes become larger, particularly when they become large for both bargainers. This is particularly so since there is clear evidence of strategic considerations in the present data, both in the consistent first-mover advantage, and in the fact that in most cases the equal division offer is (also) outside of the 95 percent confidence interval for the observed mean offers (see Figures 1A, 1B).

Regardless of how important distributional considerations turn out to be on bargaining domains involving much larger stakes, the consistency of these considerations across experiments demonstrated in

[15]Indeed, just such sensitivity to experimental instructions has been observed in a related context by Elizabeth Hoffman and Matthew Spitzer (1982, 1985). See also the ultimatum games reported by Kahneman et al. (1986a) for manipulations directly motivated by considerations of fairness.

[16]Among the many differences in how the experiments reviewed here were conducted, we note, for example, the following. The experiments of Güth et al. used German graduate students of economics attending a seminar to get credit for the final exams. Each participant could see all the others. In the experiment of Binmore et al., pairs of subjects bargained via linked microcomputers. They were not informed until after the first game had been played that player 2 would play another game as player 1. Their instructions include the statement "YOU WILL BE DOING US A FAVOUR IF YOU SIMPLY SET OUT TO MAXIMIZE YOUR WINNINGS." (All capital letters in original). The subjects for the Neelin et al. study were the members of an economics class. In their instructions is the phrase "You will be discussing the theory this experiment is designed to test in class."

[17]For example, if we had looked only at Cell 1 our conclusions might have been similar to those of Binmore et al., since the data for that cell look as if after one or two periods of experience, the players settle down to perfect equilibrium proposals (see Figures 1A, 1B.) And if we had looked only at Cells 1 and 5, our conclusions might have been similar to those of Neelin et al., since in those two cells both the two- and three-period games yield observations near the two-period predictions (again, see Figure 1). And if we had looked only at Cells 5 and 6, we might have concluded, like Güth and Teitz, that the phenomena observed here was closely related to the relatively extreme equilibrium predictions in those cells.

[18]For example, we cannot conclude even from the striking relationship observed between maximum offers and equal division (in all but Cell 2: see Figures 1A, 1B) that there were almost always some players 1 who preferred an equal division to a more unequal division. These players may simply have judged the risk of rejection of a more unequal offer to have outweighed the benefits.

Table 6 implies that experimenters ignore them at their peril. This is so not merely for experiments concerning bargaining with sequential offers and counteroffers of the kind considered here, but for all bargaining experiments (including those designed to control for cardinal aspects of the bargainers' utilities).[19] In this respect, perhaps our main (albeit imprecise) conclusion is this: Bargaining is a complex social phenomenon, which gives bargainers systematic motivations distinct from simple income maximization. This means that special care must be taken in designing, conducting, and interpreting bargaining experiments (and also in interpreting nonexperimental bargaining data).

We remark in closing that we reach this conclusion (that explanation of at least some bargaining phenomena must be sought in the utility functions of the bargainers) with the very greatest caution, and hope that it will be received in the same cautious spirit. If we were to take the point of view that *any* outcome of bargaining could be "explained" by an unobserved component of bargainers' utilities, we would have robbed the theory of content. However, the data on disadvantageous counterproposals seem to us to clearly rule out the hypothesis that all the bargainers in these experiments can be modeled as maximizing their own monetary payoffs. So some cautious appraisal of how particular bargaining processes and environments might influence bargainers' utilities seems called for.

[19]A series of experiments, reviewed in Roth (1987), have players bargain over probabilities of winning some amount of money in "binary lottery games," in order to control for the predictions made by theories expressed in terms of bargainers' expected utility. Those experiments also observe concentrations of agreements that seem to bear some relation to socially recognized notions of fairness. In Roth, Michael Malouf, and J. Keith Murnighan (1981), it was suggested that these concentrations arose as some kind of coordination equilibrium. The possibility that agents' utility functions themselves incorporate significant distributional concerns suggests another mechanism by which such notions of fairness might enter into the bargaining. The results of the present experiment thus suggest some ways in which the results of those quite different experiments might be reevaluated. There are respects in which this involves modeling issues at least as much as clear-cut empirical issues: to the extent that bargaining itself may engender changes in utility involving comparisons between the bargainers, it may still be most fruitful to model this as part of the bargaining theory, rather than directly in the utility functions of the bargainers, so that the underlying economic data of the problem should be measurable independently of the course of the bargaining. And those experiments employed very different rules of bargaining (for example, bargaining was not restricted to alternating offers and counteroffers) which may influence the bargainers' utilities differently. In this last regard it is nevertheless worthwhile to note that the substantial percentage of first-offer rejections observed in the sequential bargaining experiments is reminiscent of the substantial percentage of costly disagreements observed in these other experiments (see Roth, 1987; Roth, Murnighan, and Francoise Schoumaker, 1988).

APPENDIX 1

Instructions

General. The purpose of this experiment is to study how people behave in bargaining situations. During this experiment you will participate in several bargaining rounds. At the end of the experiment, one of the bargaining rounds you participated in will be chosen at random, and you will be paid in cash what you earned in that round.

A bargaining round involves the division of 100 chips between two bargainers. Both bargainers must agree on the division, otherwise neither side receives any chips for the round. A round lasts, at most, two periods. The *cash value* of the chips distributed to an individual depends on the period in which agreement is reached. The cash value of a chip will also generally be different for individuals who occupy different bargaining positions. These cash values will be written on a message form which is used to transmit proposals from one bargaining partner to the other. Are there any questions so far?

At the end of the instruction period you will be assigned to either the Left bargaining position or to the Right bargaining position. These assignments have been made randomly prior to your arrival. Your assignment is designated on the folder you received after you entered this room.

In your folder is a card with your ID#. (Don't take it out right now.) You are to reveal your ID# to no one other than a monitor during or after the experiment.

The Conduct of a Round. A bargaining round proceeds as follows: The Right position partner takes out of a pre-numbered Message Form, such as the one reproduced on the next page. Let's look at that form. Notice that there are cash values per chip for both Left and Right bargainers. Notice that these cash values diminish as the periods proceed. In period 1 an agreement is worth *at most* \$30 to either Left or Right. If agreement is not reached in period 1 we go on to period 2. In period 2 an agreement is worth *at most* \$12 to Left and *at most* \$12 to Right.

Here's how a round proceeds. The Right partner checks that his or her ID# on the form is correct and then makes a proposal for period 1. The proposal is of the form Left gets ____ chips; Right gets ____ chips.

ROUND # Sample
Message Form

Left	Right
ID# ________	ID# ________
Left's cash value/chip	Right's cash value/chip
Period 1 \$.30/chip	Period 1 \$.30/chip
Period 2 \$.12/chip	Period 2 \$.12/chip

Each proposal must add up to no more than 100 chips.

Period One

Right Proposes: Left gets _ chips; Right gets _ chips.
End of Right's message.
Left Responds: accept reject
(circle one)

If Left accepts, draw a line through the remainder of form. No other marks are to be made on the form.

Period Two

Left Proposes: Left gets _ chips; Right gets _ chips.
End of Left's message.
Right Responds: accept reject
(circle one)

The form is collected and given to a predesignated Left-position player. The Left-position player enters his/her ID# and then enters a response.

If Left's response is *Accept*, the round ends and each bargainer is credited with the *Period 1* cash value of the chips agreed upon.

If Left's response is *Reject*, the round continues into Period 2.

The Left-side bargainer begins period 2 by making a proposal. The proposal is of the form Left gets ____ chips; Right gets ____ chips. The message form is returned to the Right-side bargainer. If Right *accepts* Left's proposal then the round ends and each bargainer is credited with his/her respective *Period 2* cash value of the chips agreed upon. If Right *rejects* Left's proposal, the round ends without anyone's earning anything.

At the end of a round the message forms are collected and you are assigned a new bargaining partner for the next round. Any questions?

Admissible Messages. No communication is allowed except that indicated on the Message Form. A Proposal must be written as two nonnegative whole numbers (which sum to no more than 100) on the places indicated on the message form. A response is to be indicated by circling either "Accept" or "Reject." Nothing else is to be written on the Message Form.

Once the proposal is accepted no other messages are to be written, even though the Message Form is sent back and forth between bargainers.

If you violate these rules your agreements will be void and you will not be paid anything for the round.

Work Pad. All proposals are made in chips. Notice that there are always 100 *chips* to be divided. However, the *cash value* of chips differs from individual to individual and from period to period. Therefore, a work pad is provided so that you may calculate the cash value of any proposal you might make or accept before you actually send any message.

Personal History Forms. You have a set of Personal History forms. There is one form for each round. You *must* fill out this personal history form for each period of each round. These forms will provide you with a history of the chip proposals made, their cash values to you and to your bargaining partner and which proposal, if any, was accepted. You may wish to review the history of your previous bargaining rounds when developing a strategy in later rounds.

All the information on these forms is strictly private. Do not show this form to any other participant.

Method of Payment. At the end of this session we will randomly select one round from the rounds played and pay each person the cash value of the chips that person earned that round. Payment will be made in the Right Room first. We will then repeat the selection procedure in the Left Room and pay the Lefts. (Since selection is random, Lefts may not be paid on the basis of the same round as Rights.)

Final Comments. You will be bargaining with a different person each round. Your ID# is your own private information. Do not reveal your number to anyone during or after the session. What you earn is your own business. It is in your interest to earn as much cash credit each round as you can. Any questions?

Practice Round. We will now go through a practice round together. Feel free to ask questions at any point during this practice round. Put your instructions back in your folder. Those who have an *R* on your folders will please go to the right side of this room. Those with an *L* please go to the left side. Those with the *R* are *Right* players. Those with the *L* are *Left* players. After the practice round, the Right players will go to the adjoining room. Please take your pens, work pads, and the Personal History sheet marked "Practice Round" out of your folders. Please use only the pen provided. Right players, please take out the Message Form marked "Practice Round." They will make the Round-One proposal.

Proposers. Consider your period-one proposal. Notice the cash value/chip for both yourself and your bargaining partner in the first and second periods. Use your work pad to calculate the cash values of different proposals you might make. Remember, if your proposal is accepted, the period <u>1</u> cash values will apply. Don't write on anything other than your work pad until you have decided on a proposal. When you have decided on a proposal enter it on your Personal History Form for this round and fill in on your history form the cash values you and your bargaining partner will receive if your proposal is accepted. Next, enter your proposal on the message form. *Do not write on the message form until you are certain of the proposal you wish to make*. You may not change a message once you have written it without the permission of the monitor. When you have finished writing your entry on the message form, place it on the desk beside you so that the monitor can collect them.

Remember each proposal must sum to no more than 100 chips.

Tear off practice round message form.

When all of the Right-side players have made their proposals they will be collected and delivered to the Left side.

Left-Side Players: Write the proposal you have just received on your *Personal History Form* for this round. Notice the cash value/chip for both yourself and your bargaining partner. Calculate the cash values. Remember, if you accept Right's period 1 proposal, the period 1 cash values will apply. If you reject and make a period 2 proposal that Right accepts, then the period 2 cash values will apply. Use your work pad to calculate the cash values of different proposals you might make. Decide whether to accept or reject the proposal you have just received. Indicate your decision on your Personal History sheet and then on your message form. If you reject, write your new proposal on your Personal History sheet. Next enter your new proposal on your message form. *Do not write on the message form until you are certain what you wish to do.* You may not change a message form once you have written it without the permission of the monitor.

Remember each proposal must sum to no more than 100 chips.

Remember, if you circled *accept* to Right's first-round proposal then you must also draw a line through the remainder of the message form.

When all of the Left players have responded the forms are returned to the Right.

Right: Update your Personal History sheet. If your period-one proposal was accepted make no further marks on the Message Form. Otherwise, write the proposal you have just received on your Personal History Form. Notice the cash value/chip for both yourself and your bargaining partner. Calculate the cash values. Remember, if you accept Left's period 2 proposal, period 2 cash value/chip will apply. If you reject neither partner earns anything for the round. *Do not write on the message form until you are certain what you wish to do.* You may not change a message form once you have written it without the permission of the monitor.

The message forms are collected and returned to the Left.

Left: Update your Personal History sheet.

The Message Forms are collected and returned to the Right.

The round is now over. The monitors will collect the Message Forms.

We have now completed a practice round. In the bargaining that is about to begin you will engage in several such rounds against *different* bargainers. *One* of these rounds will be chosen at random to determine your payoff. Any questions?

Please place your materials back in your folders.

Rights, please proceed to the next room.

Lefts, stay here. You will be assigned new seats presently.

Two-Period Personal History Form

Round # ________ ID# ________

Personal History

Left's cash value/chip	Right's cash value/chip
Period 1 $.30/chip	Period 1 $.30/chip
Period 2 $.12/chip	Period 2 $.12/chip

The Period 1 proposal is
Left gets ___ chips; Right gets ___ chips.
Cash value of proposal
Left: ___ chips × $.30/chip = $___.
Right: ___ chips × $.30/chip = $___.

The Period 1 proposal was: ________ accepted
________ rejected

If the Period 1 proposal was rejected then

The Period 2 proposal is
Left gets ___ chips; Right gets ___ chips.
Cash values of proposal
Left: ___ chips × $.12/chip = $___
Right: ___ chips × $.12/chip = $___

The Period 2 proposal was: ________ accepted
________ rejected

Three-Period Personal History Form

ROUND #___ ID# ________

Personal History

Left's cash value/chip — Right's cash value/chip

Period 1 \$.30/chip — Period 1 \$.30/chip
Period 2 \$.12/chip — Period 2 \$.12/chip
Period 3 \$.05/chip — Period 3 \$.05/chip

The Period 1 proposal is
Left gets ___ chips; Right gets ___ chips.
Cash value of proposal
Left: ___ chips × \$.30/chips = \$___
Right: ___ chips × \$.30/chips = \$___
The Period 1 proposal was: ___ accepted
___ rejected
If the Period 1 proposal was rejected then
The Period 2 proposal is
Left gets ___ chips; Right gets ___ chips.
Cash values of proposal
Left: ___ chips × \$.12/chip = \$___
Right: ___ chips × \$.12/chip = \$___
The Period 2 proposal was: ___ accepted
___ rejected
If the Period 2 proposal was rejected the Period 3 proposal is
Left gets ___ chips; Right gets ___ chips.
Cash values of proposal
Left: ___ chip × \$.05/chip = \$___
Right: ___ chip × \$.05/chip = \$___
The Period 3 proposal was: ___ accepted
___ rejected

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