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OUTSIDE OPTIONS AND SOCIAL COMPARISON IN 3-PLAYER ULTIMATUM GAME EXPERIMENTS

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ABSTRACT

We conducted ultimatum games in which a proposer offers a division of $10 to a respondent, who accepts or rejects it. If an offer is rejected, players receive a known outside option. Our proposers made simultaneous offers to two respondents, with outside options of $2 and $4. The rate of rejected offers was higher than in similar studies, around 50%, and persisted across five trials. Outside options seem to make players "egocentrically" apply different interpretations of the amount being divided, which creates persistent disagreement. And half of respondents demand more when they know other respondents are being offered more.

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I. Introduction

Previous experiments on bargaining have shown that many players are willing to reject final offers which are profitable but which they consider unfair. Much of the recent research uses "ultimatum" games, in which one "proposer" player offers a division of a fixed sum to another "respondent", who accepts or rejects it. Roth (1995) and Camerer & Thaler (1995) review recent literature; a very brief summary is given here.

The typical finding is that respondents reject a substantial fraction of low offers (the mean rejected offer is around 20% of the amount being divided) and proposers make rather generous offers, around 40% on average. There are several statistically significant effects which are modest in size, moving average offers by 5-10%, from procedural variables like subject anonymity, instruction wording and context, whether proposers earn their position by winning a contest, whether high-profit proposers compete for the right to play a second ultimatum game, etc. There are similar effects, perhaps smaller and less reliable overall, from demographic variables like national background, gender, race, student major, etc. Raising the stakes, from $5 to $100, does not seem to affect offers or acceptances much (they are similar in percentage terms; two of nine subjects in one study rejected $30 offers from a $100 pie). Playing the game repeatedly-- pairing players together only once, to minimize reputation-building-- appears to have little effect.

When pie sizes are unknown to respondents, proposers make lower offers and respondents accept lower offers. In addition, respondents accept less when uneven offers are generated by a random device than when equally uneven offers are generated by a proposer; respondents appear to punish self-serving un<u>fair</u>ness rather than reject un<u>even</u> allocations, per se.

Our paper extends this work on ultimatum bargaining in two ways.

1. Players had positive "outside options": Even if ultimatum offers were rejected, the proposer and respondent both earned known amounts of money.[1] The presence of these

[1]Giving each respondent different outside options creates an objective reason for the proposer to treat the respondents differently, setting the stage for a social comparison effect.

outside options turned out to have a striking, and surprising effect: Nearly half the offers were rejected, compared to 5-25% in previous studies. The robustness of this outside options effect was explored by including options in both three- and two-person experiments, and allowing five trials of learning in some sessions. High rejection rates persisted. The introduction of outside options seems to create frequent rejections, perhaps by causing players to self-servingly disagree about what constitutes a fair offer.

2. We extended the domain of most previous studies, two-person bargaining, to three players. The results of two-player studies suggest that respondents compare their own payoffs with the greater payoffs of the proposer. Since they care about comparative payoffs-- they dislike earning less-- as well as absolute payoffs (Loewenstein, Thompson, Bazerman 1989; Bolton 1991), or they are willing to punish a player they feel has behaved unfairly (Rabin 1993), they prefer getting nothing to accepting a small offer if the proposer gets a lot more. This interpretation jibes with well-known theories of social evaluation which argue that a player's satisfaction is influenced by the "social comparison" of one's outcome with the outcome of others thought to be similar (e.g. Vroom 1968; Goodman 1977; Bazerman, 1993).

To learn more about these social comparison processes, we study three-player games in which a single proposer makes simultaneous offers to each of two respondents. Each ultimatum game is formally independent because a respondent's decision to accept or reject an offer does not affect the other respondent's decision or monetary payoff. This design tests whether social comparison also occurs between respondents, as it seems to occur between a respondent and a proposer. The 3-player design represents a minimal extension aimed mostly at those who are unfamiliar with the social comparison literature or skeptical of such effects.[2] There are many nuances of social comparison that could be explored in further research.

[2]A related motivation is that organizational scholars cite results of ultimatum games as suggesting that fairness is important in some economic exchanges that occur in organizations (e.g Bazerman, 1993). Extending this insight, a better understanding of the connection between the preference for fair offers

The results of the experiments show that there is a modest social comparison effect for about half of the respondents: Generally, they reject offers more frequently if they are offered less than the other respondent is. But proposers do not adjust their offers in anticipation of this between-respondent social comparison.

II. Experimental Design

In the standard ultimatum game a proposer and a respondent split an amount of money S. The proposer makes an offer $X \leq S$ to the respondent. If the respondent accepts then the proposer receives S - X, and the proposer receives X. If the respondent rejects the offer then both players receive $0.

We conducted 20 3-player ultimatum games with undergraduate students at the University of Chicago. Each player participated in two rounds with the same partners. The first round is essentially a within-subject control group used to evaluate the effect of between-respondent comparison in the second round. The experiments followed these steps (see the Appendix for actual instructions):

Round 1 (No between-respondent comparison)

Step 1A: A proposer makes two offers $X_1 \in$ [$0, $.50, ..., $10] and $X_2 \in$ [$0, $.50, ..., $10] to respondent 1 (R1) and respondent 2 (R2), respectively. The proposer's outside option in each of the two ultimatum games (the amount she earns if an offer is rejected) is $3.00.

Step 1B: While the proposer is determining her two offers each respondent determines the minimum offer he is willing to accept (denoted WTA). R1 has an outside option of

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exhibited by respondents in ultimatum games and behavior in organizational contexts requires richer games that are more like complicated organizations. Three-player games take a small step in this direction.

\$2.00 and R2 has an outside option of \$4.00. They indicate WTAs by circling the minimum offer they would accept from a column of numbers, ranging from \$2 to \$7 for R1 and \$4 to \$7 for R2 (in \$.50 increments). Note that by restricting the range of permissible WTAs, respondents are forced to accept nothing less than their outside option, and to accept nothing more than the maximum a self-interested proposer would offer. (Since the proposer's option is \$3 she shouldn't offer more than \$7).

Step 1C: When all three subjects are finished the proposer's offers are conveyed to their respective respondents. If R1 accepts X_1 then he receives X_1 and the proposer receives \$10 - X_1. If R1 rejects the offer then he receives \$2 and the proposer receives \$3. Similarly, if R2 accepts X_2 then he receives X_2 while the proposer receives \$10 - X_2. If R2 rejects the offer then he receives \$4 and the proposer receives \$3.

The value of each subject's outside option is common knowledge. Note that in this first round each respondent does <u>not</u> know the offer made to the other respondent, so no between-respondent comparison is possible. And the proposer does <u>not</u> learn if her offers have been accepted or rejected until after the second round.

<u>Round 2 (Between-respondent comparison possible)</u>

In round 2 the same three subjects play the 3-player ultimatum game. Each has the same role and outside option. The key procedural difference from round 1 is that the respondents state WTA's which can depend on the offer made to the <u>other</u> respondent, and this fact is common knowledge. The only change in experimental procedure is in the second step, when respondents record WTAs:

Step 2B: Each respondent states a WTA conditional on each of the possible offers made to the other respondent. Respondents circle one number each in a series of columns, where each column heading gives a possible offer to the other respondent (see Table A1 in the

Instructions Appendix). If a respondent does not care how much was offered to the other respondent, he or she will circle the same number in each column.

After the two rounds are concluded, a coin is flipped to determine which one of the two rounds will be used to determine each subject's payoffs.

Special features of the design

Since ultimatum results are sensitive to many aspects of experimental design, some remarks about special features of the design are necessary.

1. Members of a three-subject bargaining group were not anonymous. While subjects only communicated with one another through their offers and WTAs (as described above) each subject could look around the room and see the other two subjects they were grouped with. We did this because we thought the social comparison effect might be small under standard anonymity conditions, and larger if subjects could see each other, and we wanted to give social comparison its best chance to appear. Some two-player games with single-blind anonymity (subjects did not see who they were paired with) are described below; they suggest the lack of anonymity made much difference.

2. The order of rounds was not counterbalanced. The social comparison round 2, in which respondents made WTA's conditional on offers to other respondents, always came after the no-social comparison round 1 (which then serves as a within-subject control group). The order was not reversed because the social comparison round seemed likely to be more confusing. A control group of subjects participating in the social comparison round first, or only in the social comparison round, would establish whether there was a treatment effect in the results we observe from playing the no-social comparison round first.

3. The experiments used the "strategy method" in which respondents were asked to select strategies for each possible moves of the other player in the game (in the form of WTAs). The strategy method gives more information, particularly about the between-

respondent comparison which is our central focus. Like many, we think stating WTAs may lead subjects to reject larger offers than the alternative method, in which respondents face specific offers, but we leave this topic to future research (though see Camerer and Knez, 1995).

4. Our design departs from earlier ultimatum research by adding a second respondent and outside options (which are asymmetric); in addition, the strategy method is used to elicit WTAs. Readers may wonder why we made all these changes at once. There are three answers: First, many of the changes work hand in hand. The strategy method is necessary to efficiently produce data which are informative about social comparison in the 3-player case. (Facing subjects with specific offers would not create enough data to judge reliably how acceptances vary with different offers to others.) The use of asymmetric outside options, instead of symmetric ones, is essential for testing whether proposers realize respondents are comparing their own payoffs (hypothesis C' below).

Second, we thought adding one feature at a time was too conservative and time-consuming. If all these simultaneous changes still produce standard results, we can guess that none of the changes are substantial (assuming their effects do not work in opposite directions and cancel) and move on. Since the results are not standard-- WTAs are larger than offers and rejection rates are high-- we are left wondering which specific changes cause the departures. One step is taken backward toward answering this question in section V by reverting to a standard 2-player design and adding asymmetric outside options.

Third, moving from two players to three necessarily opens a (small) Pandora's box of design choices: Should the respondents behave independently? Together? If together, should they respond simultaneously or sequentially? So running a thorough set of 3-player experiments, even neglecting outside options, requires either a very large design or narrow choices. Since no particular treatment seems a better place to start than the others, we picked one and added options as well.

III. Hypotheses

The data produced by each triple of subjects (proposer, R1, and R2) is a pair of observations, one in each round. Each observation consists of two offers by the proposer (to R1 and R2) and two WTA functions. In the first round, the WTA function for each respondent is simply a demand (the minimum acceptable offer); in the second round, the WTA may specify a different demand for each possible offer to the other respondent.

Three sets of hypotheses, and predictions they imply, will serve as benchmarks against which to compare results. The hypotheses assume self-interest (S), fairness (F), and social comparison (C). Each hypothesis about basic preferences or knowledge, coupled with trembling-hand perfection or iterated elimination of weakly dominated strategies, leads directly to predictions about offers and WTAs.

Self-interest and knowledge of self-interest:

Hypothesis S: Respondents are purely self-interested[3]
$\Rightarrow$ $WTA_1 = \$2.50$, $WTA_2 = 4.50$.

Hypothesis S': Proposer believes S and Proposer is purely self-interested
$\Rightarrow X_1 = \$2.50$, $X_2 = \$4.50$.

The two hypotheses are stated separately because one might be true and the other false, and our data enable us to test that possibility. For example, proposers could be self-interested and think respondents are too (S' holds), so they make small offers. But if respondents are not purely self-interested (S is false) then small offers will be rejected often.

The second two hypotheses, following Loewenstein, Bazerman & Thompson (1989), Bolton (1991), Rabin (1993), and others, express the possibility that players dislike low

[3]For simplicity, we assume respondents will not accept offers equal to their outside options, and proposers know this, though a few do state WTAs equal to their outside options (as Tables 1-2 show).

relative payoffs or being treated unfairly and therefore proposers make generous offers and respondents reject small offers.

Fairness and knowledge of fairness:

Hypothesis F: Respondents dislike negative relative payoffs between themselves and the proposer $\Rightarrow WTA_1(X_1) \geq \$2.50$, $WTA_2(X_2) \geq 4.50$.

Hypothesis F': Proposers dislike negative relative payoffs between themselves and the respondent $\Rightarrow X_1 \geq \$2.50$, $X_2 \geq 4.50$.

Hypothesis F": Proposer is self-interested but believes F $\Rightarrow X_1 \geq \$2.50$, $X_2 \geq \$4.50$.

Note that hypotheses F' and F" are fundamentally different-- F' posits fair-minded proposers, and F" posits strategic proposers who believe respondents are fair-minded-- but are observationally equivalent. Our design cannot discriminate between these hypotheses but a large literature tries to do so, mostly using "dictator" games (e.g., Forsythe et al 1994), and reports mixed results.[4]

The next two hypotheses express the possibility that respondents compare their payoffs with those of other respondents, as well as with the proposer's payoff.

Between-respondent social comparison, and knowledge of social comparison:

Hypothesis C: Respondents care about relative payoffs between themselves and the other respondent $\Rightarrow WTA_i(X_i,X_j)$ is increasing in X_j, $i = 1,2$, $i \neq j$.

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[4]Researchers have tested F' versus F" in dictator games in which proposers make offers that cannot be rejected. If F' is true offers will be generous: if F" is true offers will be zero. Since dictator offers are clearly lower than ultimatum offers, and close to zero in some conditions, researchers have concluded that F" is true, and is presumably true in ultimatum games too. But note that standards of fair behavior may vary across dictator and ultimatum games. For example, proposers may shift somewhat from F' in dictator games to F" in ultimatum games, because proposers feel obligated to treat respondents fairly in dictator games but feel free to behave self-interestedly in ultimatum games, because respondents can stick up for themselves in utimatums. If this is true, then data supporting F' in dictators is of little relevance for testing F' versus F" in ultimatums. Then F" would have to be tested directly by eliciting proposers' beliefs about the distribution of WTAs to see if ultimatum offers are expected-profit-maximizing given their beliefs.

Hypothesis C': Proposer believes C

$$\Rightarrow X_2(1) - X_1(1) \geq X_2(2) - X_1(2),$$

where $X_i(t)$ is the offer made to respondent $i = 1,2$ in stage $t = 1,2$.

Hypothesis C states that each respondent's WTA increases in the offer made to the other respondent, because respondents dislike earning less than other respondents do. The prediction is consistent with theories of social comparison based on similarity of "referents" (see Goodman, 1977). Since respondents 1 and 2 are in similar roles (relative to the proposer) they will use the offer the other respondent receives as a point of comparison in determining the fairness of the offer they receive. Note that hypothesis C could be true for different reasons. Respondents could be envious of other respondents (and require higher offers to appease their envy). Or respondents could think that proposers are behaving unfairly, and hence deserve punishment by having a higher offer of X rejected, if they offer X to one respondent but not another. Other preferences underlying hypothesis C could be imagined. The first step is to see if the social comparison hypothesis C is true. If it is, further research could try to distinguish different reasons underlying the effect.[5]

If the proposer believes C, then he or she should reduce the difference between the respondent offers in round 2 in order to lower the risk of rejection and increase expected profit, leading to hypothesis C'. (Note that we assume $X_1 < X_2$, which is always true in our data). Note that we used asymmetric outside options to create a sensible reason why offers might differ in round 1, in order to create an offer gap that proposers obeying hypothesis C' would reduce in round 2.

IV. Experimental Results: 3-player games

[5] For example, a referee suggested a 4-person design in which two 2-player ultimatum games are played, but respondent 1 can condition her WTA on the offer to respondent 2. In this design, a between-respondent effect is due to pure envy, rather than the desire to punish an unfair proposer, because each respondent only gets offers from a different proposer. In our design envy and unfairness could both produce comparison.

A. Between-respondent Social Comparison: Round 2 Results

WTAs: The analysis begins by looking at round 2 results, which capture any between-respondent social comparison because respondents can condition their WTAs on offers made to the other respondent. Each respondent's *conditional* WTAs are described in tables 1 (for R1) and 2 (R2). Each row of table1 gives the conditional WTA of one of the twenty subjects in the role of R1, and each column represents a potential offer made to R2. For example, the minimum offer subject 1 will accept if R2 receives an offer of $4.00 is $3.50, while the minimum offer he will accept if R2 receives an offer of $7.00 is $6.50.

The social comparison effect (hypothesis C) predicts that respondents' stated WTA's are increasing in the offer made to the other respondent. The far right column gives the slope of each subject's conditional WTA function, b, fitted with an ordinary least-squares regression of conditional WTA on offer-to-other-respondent, $WTA_1(X_1,X_2)=a+bX_2+e$. Hypothesis C predicts a positive slope b.

Entries in the table are ordered by the estimate of b. Seven of the 20 R1s-- the first seven rows of Table 1-- exhibit a substantial social comparison effect ($b \geq .5$); 11 of the 20 subjects are completely indifferent to R2's offer (b=0).

Table 2 gives the conditional WTA's for the respondent R2s. Only five subjects (the top five rows) display a strictly positive social comparison slope. Several, at the bottom of the table, appear to have negative slopes: If R1's are offered less, they demand more. Five subjects are completely indifferent.

Interestingly, on average the R2s appear to care negatively about social comparison with R1 in some range of offers, and positively in others. For example, subject 7 demanded $6 if $3.50-$4.50 was offered to R1, but raised his demand to $7 if R1 was offered less than $3.50 or more than $4.50. This can still be social comparison, with the focus of comparison switching for very low and high offers to R1: When the offer to R1 is low the proposer gets more; R2 subjects compare their offers with the proposer's total share from the two games and demand some of the proposer's windfall. When the offer to R1 is high, R2s compare themselves with the well-off R1 and also demand more. Note that this dual-comparison account predicts a U-shaped pattern of WTAs (across R1 offer levels), which is only apparent for two subjects (subjects 6-7 in Table 2). But since many subjects seem to care about R1's high offers, and others care about the proposer taking advantage of R1 with a low offer, a composite of these subjects creates a U-shaped pattern in the average WTA.

This interpretation of the differing results for R1 and R2 may sound convoluted. We think the results are complicated because there are two natural foci for comparison-- the other respondent, and the proposer. Differing option values for R1 and R2 allow foci to differ between them, and in ways that vary with offers. If the data are consistent with a single organizing principle, perhaps it is this: Subjects dislike negative relative payoffs (and raise their WTAs as compensation), and compute relative payoffs by comparing their absolute payoff with the payoff of the player whose absolute earnings are closest to theirs. For R1s, the referent is usually R2 and we observe positive slopes or zero slopes in the regression results. For R2s, the referent is the proposer when the offer to R1 is low and R2 when her offer is high. (Note that adding more respondents, with various option values, would provide an interesting test of how well this general principle predicts.)

Another way to measure the effect of social comparison is by studying the rate of rejection of a particular offer-- say, an offer to R1-- conditional on the offer to R2. Figures 1 and 2 give the conditional rejection rates for R1s and R2s, respectively. Conditional rejection rates were calculated by determining the probability that an offer would be rejected given a specific offer made to the other respondent. For example, in Figure 1, if an offer of $4.50 is made to respondent 2, then there is a 50 percent chance that an offer of $3.50 to respondent 1 will be rejected. Increasing conditional rejection rates are consistent with a social comparison effect between respondents. The biggest effect occurs for offers of $3.50 or $4.00. For each of these cases a $.50 increase in the offer made to respondent 2 leads to a roughly 10 percent increase in the rejection rate.

The conditional rejection rates for R2s, shown in Figure 2, show much less social comparison effect. The social comparison effect does not kick in unless R1 receives offers greater than R2. For example, if R2 receives an offer of $5.50, then an increase in R1's offer from $5.50 to $6.00 doubles the rejection rate from about 15% to 30%. Finally, the downward sloping rejection rates at relatively high offers to R2, but low offers to R1, again suggests that the R2s are making relative comparisons at that point between themselves and the proposer, not R1.

In sum: Tables 1-2 suggest that about half the respondents care about other respondents' offers, and R1s and R2s are apt to make different types of social comparisons when determining their WTA's. R1s can be roughly divided into two groups: Half do not care about R2's offer at all (b=0); several others do care, and demand more when R2 is offered more (b>0). R2's can be roughly divided into those two groups as well-- five don't care (b=0) and six demand more when R1 gets more (b>0)-- and there is a third group of six or so who compare themselves to proposers when R1's offer is low (b<0). Overall, there is mixed evidence for hypothesis C.

Psychologists have expressed surprise at how little social comparison our data show, compared to other studies (e.g., Bazerman, Loewenstein & White 1992). There are three replies to this conclusion. First, note that a small majority (60%) do exhibit <u>some</u> apparent social comparison. Second, the high WTAs indicate a very strong apparent comparison between respondents and <u>proposers</u>. The strength of this proposer-respondent comparison may divert respondents' attention away from comparisons with other respondents, or limit how large the between-respondent effect could be. Third, our rating scale expresses WTAs to the nearest $.50. Subjects who care a little bit about other respondents' offers (less than $.50 variation in their own WTA) will look as if they do not care at all, so our method understates the number of subjects who do social comparison.

<u>Offers</u>: Figure 3 gives a three-dimensional histogram showing the frequency of offer-pairs (X_1,X_2) made in round 2. On average, proposers offered $3.78 to R1 and $5.08 to

R2. The mode is the self-interest point (2.50, 4.50), with six of twenty pairs (which is a very large number of self-interested offers compared to other studies). Four proposers offered both respondents equal splits (5,5).

It is useful to compare these actual offers with the offers that proposers should have made if they (i) self-interestedly strove to maximize their own expected profits; and (ii) knew the actual distributions of WTAs. The level of expected profits for each possible offer-pair is displayed in figure 4. The optimal offer-pair is shown by the tallest box, at ($5, $5). Note that offering too little to respondents lowers expected payoffs (the bars get shorter) because rejection is likely; offering too much lowers expected payoffs because it lowers the proposer's share.

Comparing Figures 3 and 4, it is clear that many proposers erred on the side of self-interest, choosing lower offers yielding lower expected profits (about $1 less) than the profit-maximizing equal split ($5,$5). This result is unusual because many previous studies show that offers are very close to the expected-profit-maximizing point, even in the first period of repeated play (e.g. Roth et al 1991).[6] Since the three-player game with options is more complicated than in earlier studies, one might argue that proposers of limited rationality cannot figure out how to profit-maximize. But note that the profit-maximizing offer pair is as simple as can be-- offer two equal splits.

<u>Comparing round 1 and round 2 offers</u>: Recall that the analysis of Tables 1-2 showed mixed evidence for the social comparison hypothesis C. Proposers who anticipated this result, obeying hypothesis C', should reduce the difference in the offers made to the two respondents in round 2, relative to round 1, because in round 2 the respondents' know each others' offers and are more likely to reject offers that are far apart. In fact, hypothesis C' is rejected because the round 1 and 2 offers are very close together. Ten proposers

[6] A referee suggested subjects may be viewing the 3-player ultimatum game as a single game between the proposers and the two respondents combined. Under this interpretation the equal-split implies the proposer receives a total of $10.00 and the remaining $10.00 is somehow divided between the two respondents. In round 1 eight of the twenty offers are consistent with this hypothesis, and five out of twenty are consistent in round 2. However, all these offer pairs are (5,5) or (4,6), which are about equally common in the two-player experiments we report in section IV.

made exactly the same offers in the two rounds. Five others made different offers, but changed R1 and R2's offers by the same amount. The statistic for testing hypothesis C',the difference in offer gaps, $[X_2(1)-X_1(1)]-[X_2(2)-X_1(2)]$, has a mean of -.025 (t=-.16).[7]

B. Round 1 Offers and WTAs

In round 1, respondents gave a single WTA and did not know what other respondents were offered, so between-respondent social comparison is controlled away. Round 1 offers were used in the last section as a control for whether proposers alter their round 2 offers in anticipation of social comparison. The round 1 data are also interesting because our design includes positive outside options, which previous ultimatum studies did not.[8]

From the perspective of orthodox game theory, if the self-interest hypotheses S and S' are true the only effect of positive outside options should be to raise offers by the amount of the respondent's option. Respondents should accept any positive level of surplus rather than exercising their outside options. Proposers, anticipating this, should offer a minimal amount above the respondents' options.

The left two data columns of Table 3 give summary statistics for offers and WTAs, for R1 and R2, from round 1. For R1s the mean WTA of $3.86 is greater than the mean offer

[7]A referee points out that each responder's WTA in period 2 may be affected by the offer they observe in period 1. In order to test for such an effect we can compare each respondent's first round WTA with their second round WTA, for those respondents whose WTAs in round 2 are independent of the offer made to the other respondent (i.e. the state the same WTA for each of the possible offers made to the other respondents). This is of course not a perfect test since there may be a level effect in round 2 which is independent of the first round offer. Pooling across respondent 1's and respondent 2's yields sixteen cases of players stating the same round 2 WTA for each of the possible offers made to the other respondents. In five of these cases respondents rejected the first round offer. Three of these subjects stated the same WTA in round 2, and two lowered their WTAs. Eleven accepted round 1 offers; of those, eight stated the same WTA and two lowered theirs. Hence, there is a modest, insignificant, relation between round 1 acceptance/rejection and lowering WTAs. Also, if we regress WTA in round 2 on offer in round 1 for the same subsample as above there is no relationship (t=-.043).

[8]Binmore, Shaked & Sutton (1989) also studied the influence of outside options in alternating-offer bargaining and found that with some experience, subjects learned to ignore options if and only if, in equilibrium, they would not be exercised. It is hard to know whether options increase disagreement in their setting because there is not a large body of no-option evidence to compare their data with.

of $3.63, leading to a rejection rate of 45%.[9] For R2s the mean WTA of $5.28 is again greater than the mean offer of $4.90, creating 55% rejections. Hypotheses S and S' are clearly rejected in the direction of the fairness hypotheses F and F' or F".

The fact that respondents ask for more than proposers offer, on average, is remarkable; no other ultimatum study reports mean WTAs consistently above mean offers. The result is that rejection rates are very high, around 50%, roughly double the rejection rates of 5-30% or so observed in previous experiments.

Egocentric assessments of fairness: Our interpretation is that average WTAs exceed average offers, and rejection rates around 50% result, because the presence of outside options cause players to have differing "egocentric" interpretations of what offer is fair.

For example, suppose for sake of argument that both the proposer and respondent (implicitly) agree that it is fair for the respondent to get, say, 35% of the surplus being divided. Focus on respondent 1 (with the $2 option) for simplicity. Because of the outside options, there are two ways to define surplus and compute the fair offer. If the surplus to be divided is $10.00, then the respondent should receive $3.50. Alternatively, the surplus could be defined as the gains from exchange above and beyond the option values, which is $10-$2-$3 or $5. Then the fair offer is 35% of $5 beyond R1's $2 option, a total of $3.75.

Note that the two "frames" or interpretations of surplus are both plausible (cf. Binmore, Shaked & Sutton 1989). Players could reasonably have preferences over which way to view the situation and hence, over what is fair. If players egocentrically choose the view which benefits them most, proposers will pick the $10 surplus definition and offer $3.50; respondents will pick the $5 definition and ask for $3.75. These numbers match fairly closely the mean offers and WTAs observed in our experiment ($3.63 and $3.86). (Of course, the 35% figure was picked to fit the data, but note that the mean offer and mean

9The rejection rate is measured by pairing every offer with every other WTA, so it may be slightly different than the empirical rejection rate (which paired one offer with exactly one WTA). But since subjects had no opportunity to learn from direct experience, this difference shouldn't matter.

WTA, two statistics, were closely fit by using just one parameter and the obvious two ways to define surplus.)

Figure 5[10] shows histograms of offers and WTAs which illustrate in more detail how egocentric interpretations of fairness could account for the surprisingly high rejection rate. For R1 (top histogram) nearly half the respondents demanded $5 of the $10 pie, or $4.50, but only a third of the proposers offered that much. A large cluster of proposers offered $2.50-$3, just above R1's $2 option value, but few respondents were willing to accept that little. The same kind of shift is evident for R2, though less pronounced: For example, a third of respondents demanded more than half the $10 pie-- note that $5.50 is the equal split of the beyond-option surplus plus R2's option-- but only 10% of the proposers offered that much.

So if proposers and respondents roughly agree on what percentage offer is fair, but self-servingly pick the definition of surplus which benefits them most, then a significant disparity in offers and WTAs follows, and high rejection rates. This effect was not predicted, but it corroborates many other studied documenting egocentric assessments of fairness (some are mentioned below) and perhaps deserves further study.

V: Experimental results: 2-player games

Because the level of rejection rates in these experiments was unusually high compared to earlier studies, it is natural to wonder whether the two-respondent structure, the presence of outside options, or some other unusual design feature (e.g., the lack of single-blind anonymity) was the source. So we ran a second set of experiments on 2-player ultimatum games with outside options using University of Chicago MBA students. Thirty 2-player games were conducted with the standard single-blind anonymity condition (subjects sat in a room, and knew they were paired with someone in the room but did not know who) and

[10]The figure pools some statistically similar data from 2-person games, described below in section V.

the same conditions as our 3-player games (round 1), except each proposer makes an offer to only one respondent.

The two-player results are summarized in the middle part of Table 3. (The slightly different "POR games" are discussed separately, below.) Two-player offers are a little lower than in the 3-player case, and WTAs are a bit more compressed between high- and low-option respondents R1 and R2. But the essential features of the three-player game results are the same: Mean and median WTAs are still larger than offers, and rejection rates are still around 50%.[11]

We use two nonparametric hypothesis tests to check for distributional differences between 2- and 3-player results: (i) The Kolmogorov-Smirnov (KS) test, which uses the maximum difference in two sample cumulative distribution functions as a test statistic (and is simple to compute and interpret); and (ii) the more complicated Epps-Singleton (1986) ES test (with J=2 and t=.4), which uses the empirical characteristic function and generally has more power than KS. (Forsythe et al, 1994, give helpful details and a useful comparison of these tests with two others.) Table 4 shows these two test statistics for the comparison between 2- and 3-player outcomes. Statistics for offers are shown in the upper right of each table and WTA statistics are shown in the lower left. Only the WTAs in the R2 ($4 option) condition differ significantly, by only one of the two tests (ES).

<u>POR condition:</u> One procedural change was made in further two-player sessions, as a loose test of our egocentric assessment interpretation of the high rejection rate. In the 3-player experiments and most 2-player sessions, respondents were asked to circle a WTA in the range of feasible offers circumscribed by the outside options (R1 picked a WTA from $2 to $7, and R2 from $4 to $7). But proposers were asked to simply write down an

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[11]Note that in two cases proposers made $10.00 offers. One proposer told the experimenters that she did not completely understand the instructions, and hence, felt compelled to give the whole $10.00 away. There appears to have been a foreign language problem. These two offers were dropped from the analysis. Including them does not impact the results greatly; e.g., the median offers are the same.

offer.[12] A referee suggested this procedure may encourage differing assessments of fairness because respondents see only a restricted range while proposers do not (and may think about the full $10 range). To test this, some sessions were run using a "proposer offer range" (POR) treatment in which proposers made offers by circling a number from the feasible range, just as respondents did. If the offer range they are presented with influences players' preference for how to define surplus, then the POR treatment should make offers rise and rejections fall.

The rightmost column of Table 3 shows the POR results.[13] POR offers do appear to be higher. The frequent offers of $2.50 and $3.00 observed in the standard condition, visible in the Figure 5 histogram, almost disappear in the POR treatment (one offers $2.50 and another $3.50) and the number of $4 offers rises sharply. Furthermore, the POR and pooled 2- and 3-player offer distributions are significantly different by the KS and ES tests (Table 4).

The WTA's appear to be slightly lower in the POR games, but are insignificantly different from those in the 2- and 3-player games. The rejection rate in the POR treatment is still substantial, 38%. (If POR offers are matched with the higher two-player R1 WTAs, averaging $4.27, the rejection rate rises to 44%.) The effect of POR shows that which surplus interpretation subjects use can, to some extent, be affected by experimental procedure. In effect, the POR treatment induces proposers to see the situation less egocentrically, more like respondents do, and raises offers.

A purely economic explanation of high rejection rates is that subjects are not highly motivated to agree because the outside options mean that the marginal gain from an agreement is only $3 or $5, not $10. (Hence, a rejection is a less costly mistake in our games than in games with $10 pies and no options.) This explanation seems unlikely

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[12]We allowed this difference because we did not want respondents to be confused about the role of their outside options by making it look acceptable to select WTA's which were less than their outside options. We were less worried that proposers would be confused.

[13] We excluded two outlying $7 offers from the analysis of POR offers. The strength of our conclusions is only increased by including them, except the rejection rate falls slightly.

because studies have varied actual pie sizes from $1 to $100 with little variation in rejection rates except for hypothetical-payoff sessions (see Hoffman et al in press). Nonetheless, a very careful control requires using our methods, subject pool, etc., with pie sizes of $5 (corresponding to R1 surplus) and $3 (R2 surplus) and no options. If rejection rates are low in such an experiment, then we can conclude that the presence of outside options, not lower motivation due to reduction in surplus, cause high rejection rates.[14]

VI. Experimental Results: Repetition and learning in 2-player games

It is natural to wonder whether the frequent disagreements our subjects exhibit are expressions of preference-- subjects don't mind losing money from disagreement because they think others are behaving unfairly-- or mistakes in judgment about what others will do. If disagreement results from errors in judgment, the disagreement rate should shrink if the game is repeated, as subjects learn. Learning is tested in five-round games where subjects played the outside option ultimatum game with a new partner in each round. (The learning sessions used the original instructions rather than the POR instructions.)

Three experimental sessions were conducted with ten subjects in each session. In each session five of the subjects were randomly assigned to be proposers and five of the subjects are randomly assigned to be respondents. Each session consisted of five rounds of play. In each round subjects would play the respondent 1 ultimatum game where the proposer has an outside option of $3.00 and the respondent has an outside option of $2.00.

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[14] A weak control is reported by Camerer and Loewenstein (1993), who conducted a series of no-option ultimatum bargaining experiments involving several pie sizes. On average, their subjects' offers from $3 and $5 pies were $1.34 and $2.23, compared to our results of $.66 and $1.80 (adjusting offers by subtracting respondent option values). Their average WTAs were $.85 and $1.46, compared to $.96 and $2.27. Our subjects offered less and asked more. (These differences are highly significant: A comparison of the pooled WTA and offer distributions for $2 options, or $5 pies, yields KS statistics of .49 and .40, both significant at p=.025, and ES statistics of 10.43, p<.05, and 1068, p<.001.) As a result, our disagreement rates, 45% and 48% for $3 and $5 pies, were much higher than their rates of 15% and 14%. Their disagreement rates are very close to rates we observed with Chicago MBAs in other $10 experiments without options (Camerer and Knez, 1995). Furthermore, we are virtually certain that a suitable control with a $5 pie and no options would yield a significantly different offer distribution, because the Camerer-Loewenstein and Forsythe et all (1994) $5 pie data show 50-70% of the offers at the equal split $2.50, but only 2 of 35 offers (6%) in our option conditions were at the option-adjusted equivalent of $4.50.

Subjects remained in the same role throughout the five rounds of play, and were paired with a new partner in each round. Subjects never played with the same partner twice and this was common knowledge. At the end of each round respondents observed their offers, and proposers learned if their offers were accepted or rejected. Finally, at the end of the fifth round one of the five rounds was randomly selected as the round determining each subject's payoffs.

Results of the learning trials are shown in Table 5. First note that average WTAs and offers are roughly similar to those from the other sessions we have reported (compare Tables 3 and 5). Both the round 1 and round 5 results are statistically indistinguishable from the pooled 2- and 3-player R1 results (see Table 4).

The primary question is whether WTA's fall over time or offers rise (or both), leading to an overall reduction in the rejection rate. Results are shown in Table 5. First note that the rejection rate of 47% in round 5 is identical to the rejection rate in round 1, and is close to the 45% rejection rate observed in our single-shot, R1 ultimatum games (see Table 3). So five rounds of repetition do not lower the high rejection rate. However, the average offer rises steadily over time, from $3.43 in round 1 to $3.90 in round 5. Under a paired-comparison t-test this difference is significant (t=-2.17, p<.025), and the two-sample tests indicates significantly different offers too (see Table 4).. In eighteen cases proposers increased their offers between rounds. In fifteen of these eighteen cases the proposer's offer was rejected in the previous round.[15]

While offers rise a bit over the five trials, there is no decrease in average WTAs between rounds 1 and 5 (t=-.28, p>.38, and see Tabe 4). There are twelve cases where a respondent lowers his WTA, and in ten of these cases the respondent rejected the offer they received in the previous round. But in six of the twelve cases respondents later raise their WTAs. Hence, respondents appear to stick more to their guns, or temporarily retreat, while

[15]The increase in offers is consistent with results on repeated ultimatum games reported by Prasnikar and Roth (1992). It would be interesting to know whether the learning dynamics can be captured with simple reinforcement or adaptive models, as in Roth & Erev (1995).

proposers slowly give in to the respondents' demands. It is natural to think that this asymmetry in learning occurs because a rejection usually costs a proposer more than it costs a respondent (cf. Roth & Erev 1995). The rate of change is too slow and erratic to lower the overall rejection rate of 50% within five periods, but it would be interesting to know whether many more periods of learning (e.g. 10-20) reduces the rejection rate to typical levels.

A common concern about the strategy method is that stated WTAs and actual accept/reject decisions, given specific offers, might differ because WTAs are inflated. Repeated play offers some clues suggesting this is false. Of the twenty-eight rejections which occur in the first four rounds, only ten lead to a reduction in the respondent's WTA in the next round. And in five of these ten cases the respondents raise their WTA's in subsequent rounds. Hence, there is not much evidence that respondents pick high WTAs and permanently revise them downward after rejections.

VII. Conclusions

In this paper we studied a 3-player ultimatum bargaining game in which a proposer made offers dividing $10 with each of two respondents. Unlike in most other studies, all players had positive outside options which they earned if the proposer's offer was rejected.

Respondents in standard two-player ultimatum games often reject positive offers, which suggests they compare their payoffs with the proposers' -- which psychologists call "social comparison"-- and sometimes prefer getting nothing to getting much less than the proposer does. Our design had two different respondents, each with different outside option values ($2 and $4), to see if the respondents would compare their offers with other respondents' offers as well as with the proposer's share.

Our findings and related research

In this section we summarize our two main findings and relate them to previous research.

Frequent disagreement and egocentric assessment of fairness

Our first main finding is that introducing outside options led to high rates of rejection of offers, around 50%, which were much larger than most previous studies.We suggest that self-serving or egocentric interpretations of the amount of surplus being divided provides an explanation for this effect. Of course, replication across different subject pools, using a more conservative one-treatment-at-a-time extension of the zero-option design, are necessary to establish whether this finding is robust.

Manipulating common information about monetary payoffs is another way to study egocentric assessment. For example, in Kagel, Kim, and Moser (in press), players ultimatum-bargain over divisions of 100 chips which have different monetary conversion values for different players (following Roth & Murnighan, 1982). When the proposers have high chip values and respondents have low values (and both know this), the setting is ripe for egocentric assessment: Proposers offer a fair share of chips while respondents think they should get their fair share of dollars (which means lots more chips). Indeed, the rejection rate in this condition was 39%, close to our high rates.

A second batch of papers report evidence of egocentric assessment of fairness in non-ultimatum bargaining (see Camerer & Loewenstein 1993). For example, Loewenstein & Thompson (1992) studied a two-person wage bargaining problem with escalating per-period delay costs. Babcock, Loewenstein, Issacharoff, and Camerer (in press) used an actual lawsuit adapted for experimental use, in which the facts of the case were sufficiently rich, and ambiguous, that both sides could come to believe the facts favored them. In both of these studies, the difference between two players' self-reported perceptions of what agreement is fair-- a measured gap in fairness-- was a significant predictor of the delay until settlement (and delay was financially costly to subjects).

These tales of egocentric assessment will sound ad hoc to many readers. Yet there are many demonstrations of such effects in psychology experiments (see Babcock et al, in press)-- and in readers' own lives?-- and it would be fruitful to develop the systematic principles underlying them more formally. The recipe seems to be: Construct a situation in which there are multiple ways of describing or "framing" the game. (Outside options provide two ways of defining surplus in our study, using chips provide two ways of valuing choices-- as chips or dollars-- in Kagel, Kim & Moser, and diverse information whose meaning or importance can be argued creates competing interpretations in Babcock et al). Now even if subjects agree on the same choice function, mapping alternative frames into a vector of outcomes, players' differing values for outcomes induce different preferences for frames. In a sense, players do not agree on the game they play and hence, standard arguments about when rational play leads to efficiency do not apply. Obviously, formal modeling along these lines would probably require replacing the Harsanyi "common prior" assumption or replacing the assumption that players update beliefs about states, based on new information, independently of the consequences of those states for them.

Our results also tentatively suggest that previous research on ultimatum games, which always used zero outside options, may vastly understate disagreement rates in natural settings with outside options. Low disagreement rates are often used to infer that while respondents may act "irrationally" (against self-interest) by rejecting small offers, proposers rationally make generous offers in order to maximize expected profits (obeying our hypothesis F"). Our data contradict this view: Most proposers offered too little, sacrificing a modest amount of expected profit by doing so, and only learned to raise their offers slowly across five trials of experience. Further studies of richer settings with more scope for egocentric assessment could help determine whether proposers' suboptimal behavior is really as common as in our studies, and how experience affects it.

Social comparison

Our second main finding is that roughly half the subjects exhibited some kind of between-respondent social comparison, altering the amount they would accept from the proposer in response to the offer made to the other respondent. Proposers did not appear to expect these social comparisons, or did not care about them, since their offers hardly changed between the round 1 condition where respondents did not know offers made to others (so no between-respondent comparison was possible) and the round 2 condition in which they did.

Some previous studies addressed the effects of social comparison indirectly, by varying information and the number of players. For example, Croson (1994) and Straub and Murnighan (in press) found that respondents accepted less when they did not know how much the proposers earned. These data, which are hardly surprising, show that respondents' social comparison with proposers' earnings causes rejections of offers.

Güth and van Damme (1994) studied three-player bargaining in which a proposer divides a stake three ways, between herself, an active respondent A, and a passive ("dummy") respondent D. Only the active respondent A decides whether to accept or reject the offer. They varied what A knew about the offers to each player. Their study mixes elements of ultimatum and dictator games, since A can reject the offer but D cannot, and represents an interesting contrast to ours.

When A knew all three players' shares or just her own offered share, proposers offered about 30% of the stake to A and much less, 5-10%, to D. When A knew only the offer to D, but not her own, the proposers tricked A by leaving 15-20% for D and much less for A. The proposers acted like A's would use the offer to D as a clue about the proposers' generosity, and the A's did. In all cases rejection rates were low (7% in total).

These data are consistent with our general finding that respondents who care at all about other offers generally demand more (less) when other offers are higher (lower). The fact that A accepts generous offers to herself, knowing D gets very little, suggests A's satisfaction is increased by the low offers to D.

Roth et al (1991) study two-player ultimatum bargaining, and compare it with a monopoly market game in which nine buyers compete by making offers to a single seller, who either takes the most favorable offer or rejects it and earns nothing. In the two-player ultimatum games, respondents reject offers about half the time when 30% or less is offered. But in the nine-buyer market games, buyers compete vigorously by offering to accept only 20% of the surplus or, after ten rounds of repetition, less than 5%.[16] Why do buyers accept so much less when competing with other buyers?

A plausible answer is that the social comparisons differ in the two settings.[17] In ultimatum bargaining, the single buyer can only compare herself with the seller, and rejects low offers because she dislikes comparing unfavorably with a seller who gets a lot more. But in the nine-buyer setting, each buyer can also compare herself with the other eight buyers, who earn nothing if their offers are rejected. Buyers then accept smaller surplus shares because knowing that eight other buyers earned nothing takes the sting out of earning much less than the seller did.

Further Research

There are many avenues for further interesting work. A natural extension is to methodically increase the number of players beyond three, offering more foci for social comparison, to see how people integrate multiple comparisons. The market game reported in Roth et al (1991) suggest that players will accept smaller and smaller shares of surplus if they know others like them are earning small shares (or earning nothing at all). Note how competitive, self-interested outcomes could then result if fair-minded players take solace in

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[16] A related precursor result in economic experiments on monopoly pricing (e.g., Smith & Williams, 1990) is that buyers attempt to drive prices down by withholding demand-- foregoing profitable acceptance of high prices posted by a monopoly seller. Sometimes such "boycotts" decay over time (as in Roth et al); other times buyers wear down the monopolist so prices and quantities approach the competitive point.

[17] A related fairness-based explanation is that buyers in the ultimatum game punish unfair sellers by rejecting small offers, but buyers in the market game punish <u>other buyers</u> who behaved unfairly by undercutting them, by making still lower offers (e.g., Roth et al (p 1093, esp. fn 23). This explanation is plausible because the buyers' market offers fall across ten rounds, whereas respondent behavior does not generally change much across ten rounds in two-player experiments. Note how this sort of explanation can explain competitive, self-interested behavior resulting from fair-minded players.

the fact that many others are treated equally unfairly. A different interpretation of this effect is that making many social comparisons quickly becomes complicated as the number of possible comparisons rises. People may solve this problem by ignoring everyone else and focussing on self-interest. (In that view, own-payoff maximization is a heuristic decision rule which approximates a more rational rule that is too hard to figure out.)

A finding from previous studies is that when respondents do not know the proposer's share, so clear social comparison is impossible, they tend to accept less (often zero). Together, the combined effects of increasing the number of players and reducing information about their outcomes may provide a psychologically-plausible answer to an important puzzle: Why do bargaining experiments exhibit a lot of fairness preference and substantial disagreement, while in market experiments efficiency is high and fairness appears to play no role in perturbing competitive equilibrium or inhibiting convergence? One answer is that in most market experiments, there are many other traders to compare one's earnings with, and (typically) little information about how much others are earning. Connecting these two domains-- two-person bargaining, and markets-- and exploring the separate influences of multiple social comparison and information in generating fairness preference, is a fundamental issue that lies at the heart of much economic analysis.

Another direction is to expand the methods used to investigate fairness and its roots in social comparison. For example, a referee suggests asking subjects to express a preference for how they view the amount of surplus being divided (cf. Fischhoff 1983), to see if proposers and respondents really express different preferred views. One could also ask respondents whether they consider other respondents, or the proposer, more similar to them, or somehow measure the focus of their social comparison attention (perhaps using a computerized information display device, e.g., Camerer et al 1994). It cannot hurt to use some measurement techniques or treatment variables from social psychology in economic settings, adding them to standard experimental economics designs when it is easy and harmless to do so, to test theories of social comparison more precisely.

Finally, our results are tentative because our design has many departures from the standard ultimatum design. Each change-- using WTAs, adding outside options, making the outside options asymmetric-- may increase the rejection rate a bit for different reasons, leading to a large overall increase in rejections. We are most curious to study 3-player games with no options, and 2-player games with symmetric options, and use additional designs (e.g. footnote 5) to further isolate the source of social comparison effects.

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Appendix: Instructions for 3-player Ultimatum Games

Round 1 Instructions: In this experiment you will be either a *proposer* or a *responder*. The proposer has to decide how to divide up two ten dollar bills, one between responder 1 and him or herself, and the other between responder 2 and him or herself.

The proposer makes an offer of X dollars, where X is divisible by 50 cents. If a responder accepts then the responder receives \$X and the proposer receives \$10 - \$X.

If the responder rejects the offer then both the responder and the proposer receive their outside options. The proposer's outside option is equal to \$3.00 for each of the 10 dollar bills. (That is, if either one of the proposer's offers is rejected then the proposer receives \$3.00; if both offers are rejected then the proposer receives \$6.00. Responder 1 has an outside option equal to \$2.00, while responder 2's outside option is equal to \$4.00.

One responder's acceptance or rejection of the offer he or she receives from the proposer does not affect the outcome of the offer made to the other responder.

Please do not talk during the experiment (except to ask the experimenter questions). The proposer has two record sheets, one for responder 1 and another for responder 2. On the appropriate space the proposer will privately record his or her offer. While the proposer is deciding how much to offer the respondents should fill out their own sheets. On the respondent's sheets, you should indicate the minimum offer you would accept from the proposer. Then the experimenter will collect the proposer's offer sheet and hand each sheet to the appropriate respondent. If you are a respondent, you should check whether the offer the proposer makes is above or below the minimum offer you said you would accept. If the offer is above your minimum (or equal to it) you should circle "ACCEPT" on your sheet. If the offer is below your minimum you should circle "REJECT". The experimenter will then pick-up the record sheets. The offer that each responder receives and their decision to accept or reject should be kept private.

There are two rounds in the experiment. Round 2 involves the same monetary stakes as round 1, but the rules are slightly different. (We will explain the difference in rules when we reach round 2.) Each responder's acceptance decision will not be revealed to their respective proposer until the end of round 2. At the end of round 2 a coin will be flipped to determine whether you receive your payoffs from round 1 or round 2. You will receive the payoffs from one of the two rounds. The decisions you make may affect the money you earn substantially, so ask questions now if you have them.

Round 2 Instructions: Round 2 is identical to round 1 except that the proposer's offers to each of the respective responders is public. That is, on the round 2 record sheet the proposer must record both of his or her offers on each of the responder record sheets. So each responder will know the value of the offer made to the other responder.

Also, each responder must specify the minimum offer they will accept for each of the possible offers made to the other responder. If you are a responder you have received a table of numbers [see Table A1]. Each column in the table represents a possible offer made to the other responder, while each row represents a possible offer made to you. You should circle the number in each column which represents the minimum offer you will accept from the proposer given the offer made to the other responder, as represented by that column.

Once the proposer makes his or her offers and you have determined the minimum offer you will accept for each of the possible offers made to the other responder, you will learn both the offer made to you and the offer made to the other responder. The number you circled in the column corresponding to the offer the other responder received determines your decision to accept or reject. If the offer is greater than or equal to the number you circled then you accept, if it is less then you reject. If you accept then you receive the value of the offer, if you reject you receive your outside option - $2.00 if you are responder 1, $4.00 if you are responder 2. Note that the other responder's decision to accept or reject their offer does not affect your payoffs.

At the end of the round a coin will be flipped to determined whether your final earnings in the experiment correspond to your round 1 earnings or your round 2 earnings.

Table
A1

Offer Made to Respondent 2

	4	4.5	5	5.5	6	6.5	7
	2	2	2	2	2	2	2
	2.5	2.5	2.5	2.5	2.5	2.5	2.5
	3	3	3	3	3	3	3
OFFER	3.5	3.5	3.5	3.5	3.5	3.5	3.5
MADE	4	4	4	4	4	4	4
TO	4.5	4.5	4.5	4.5	4.5	4.5	4.5
YOU	5	5	5	5	5	5	5
	5.5	5.5	5.5	5.5	5.5	5.5	5.5
	6	6	6	6	6	6	6
	6.5	6.5	6.5	6.5	6.5	6.5	6.5
	7	7	7	7	7	7	7

Each column represents an offer the proposer may make to respondent 2. Each row represents an offer the proposer may make to you. For each of the possible offers the proposer may offer repondent 2 circle the minimum offer you would be willing to accept from the proposer. So you should circle one number in each column.

When you receive an offer the number you circled in the column corresponding to the offer respondent 2 received will determine you decision to accept or reject. If the offer is greater than or equal to the number you circled then you accept, if is less then you reject.

Table 1

Subj.	4	4.5	5	5.5	6	6.5	7	
	Offer Made to Respondent 2							
1	3.5	4	4.5	5	5.5	6	6.5	1 *
2	3	3.5	4	4.5	5	5.5	6	1 *
3	3	3.5	4	4.5	5	5.5	6	1 *
4	2	2.5	3	3.5	4	4.5	5	1 *
5	2.5	3	3.5	4	4.5	5	5.5	1 *
6	2.5	2.5	3	3.5	4	4.5	5	0.89*
7	3.5	4	4.5	4.5	5	5	5	0.5*
8	4	4.5	5	5	5	4.5	4.5	0.1
9	2.5	2.5	2.5	2.5	2.5	2.5	2.5	0
10	4	4	4	4	4	4	4	0
11	2	2	2	2	2	2	2	0
12	5	5	5	5	5	5	5	0
13	5	5	5	5	5	5	5	0
14	4.5	4.5	4.5	4.5	4.5	4.5	4.5	0
15	2.5	2.5	2.5	2.5	2.5	2.5	2.5	0
16	4	4	4	4	4	4	4	0
17	2.5	2.5	2.5	2.5	2.5	2.5	2.5	0
18	2.5	2.5	2.5	2.5	2.5	2.5	2.5	0
19	4	4	4	4	4	4	4	0
20	6	5.5	5	4.5	4	4	4	-0.7*
mean	3.425	3.575	3.75	3.875	4.025	4.15	4.3	

Each row corresponds to a particular respondent 1's WTA conditional on an offer made to respondent 2, as designated by each column.

The far right column is the slope coefficient from the regression WTA = a + bOffer.
* $p<.01$

Table 2

	subj.	Offer Made to Respondent 1: 2	2.5	3	3.5	4	4.5	5	5.5	6	6.5	7	b
	1	4	4	4	4	4	4.5	5	5.5	6	6.5	7	0.64*
	2	4	4	4.5	4.5	5	5	5	5	6	6.5	7	0.55*
	3	4	5	5	6	6	6	6.5	6.5	6.5	6.5	6.5	0.44*
	4	5	5	5	5	5	5	5	5.5	6	6.5	7	0.36*
	5	5	5	5	5	5	5	5	5.5	6	6.5	7	0.36*
	6	4.5	4.5	4.5	5	5	5	5.5	5.5	5	5	6	0.23*
W	7	7	7	7	6	6	6	7	7	7	7	7	0.05
T	8	5.5	5	5	5	5	5	5	5	5	5	6	0.05
A	9	4	4	4	4	4	4	4	4	4	4	4	0
	10	4.5	4.5	4.5	4.5	4.5	4.5	4.5	4.5	4.5	4.5	4.5	0
	11	5	5	5	5	5	5	5	5	5	5	5	0
	12	4.5	4.5	4.5	4.5	4.5	4.5	4.5	4.5	4.5	4.5	4.5	0
	13	5	5	5	5	5	5	5	5	5	5	5	0
	14	5	4.5	5.5	5	5	5.5	5	4.5	4	4.5	5	-0.1
	15	5	5	5	5	4.5	4.5	4.5	4.5	4.5	4.5	4.5	-0.12*
	16	6	6	6	6	5.5	5.5	5.5	5.5	5.5	5	5	-0.21*
	17	6.5	6	5.5	5	5	5	5	5	5	5	5	-0.24*
	18	7	7	7	6.5	6.5	6.5	6.5	6.5	6	6	5.5	-0.26*
	19	7	6.5	6	5.5	5	5	5	5	5	5	5	-0.36*
	20	7	7	6	6	6	6	5	5	5	5	5	-0.44*
	mean	5.275	5.225	5.2	5.125	5.075	5.125	5.175	5.225	5.275	5.375	5.575	

Each row corresponds to a particular respondent 2's WTA conditional on an offer made to respondent 1, as designated by each column.

The far right column is the slope coefficient from the regression WTA = a + bOffer.
* p<.01

Table 3: Offers and WTAs in 3- and 2-player ultimatum games

	Offers in 3-player games (n=20)		Offers in 2-player games (n=15)		Offers in 2-player POR games (n=23)
Statistic	to R1	to R2	to R1	to R2	to R1
mean	3.63	4.90	3.80	4.66	4.37
median	3.00	5.00	4.00	4.50	4.00
std.dev.	1.10	.45	1.20	.49	.68

	WTAs in 3-player game (n = 20)		WTAs in 2-player game (n = 15)		WTAs in 2-player POR games (n=24)
Statistic	R1 WTA	R2 WTA	R1 WTA	R2 WTA	R1 WTA
mean	3.86	5.28	4.27	4.96	3.88
median	4.00	5.00	4.50	5.00	4.00
std. dev.	1.26	.63	.92	.61	1.19
rejection rate	45%	55%	45%	48%	38%

Table 4: Statistics Testing Hypotheses of Distributional Equality
(offers upper right, WTAs lower left)

	R1 ($2 option) number of players 3	R1 ($2 option) number of players 2	R4 ($4 option) number of players 3	R4 ($4 option) number of players 2
3 players	----	.13,.23	---	.18,2.54
2 players	26,1.55	----	.18,18.02[c]	----

R1 ($2 option) only

	2+3-player pooled	POR	learning trials Round 1	learning trials Round 5
2+3 pooled	----	.45[b],53.00[c]	.22,1.28	.25,3.79
POR	.15,5.73	----	.60[c],31.67[c]	.37,16.10[c]
Round 1	.17,1.38	.04,.01	----	.40[a],71.20[c]
Round 5	.15,.01	.09,15.19[c]	.07,.00	----

[a]$p<.10$, [b]$p<.05$, [c]$p<.01$

Note: Table gives Kolmogorov-Smirnov test-statistic (largest distribution of sample cdf's) first, followed by Epps-Singleton statistic (distributed chi-squared with 4 dof, using J=2 and t=.4).

Table 5
Repeated 2-player Ultimatum games*
Respondent 1 Case

	round 1 WTA	round 1 offer	round 2 WTA	round 2 offer	round 3 WTA	round 3 offer	round 4 WTA	round 4 offer	round 5 WTA	round 5 offer
mean	3.87	3.43	3.73	3.43	3.97	3.67	3.77	3.73	3.97	3.9
median	4	3	4	3	4.5	3.5	3.5	4	4.5	4
std.dev.	1.25	0.84	1.07	0.75	1.22	0.84	1.24	0.68	1.25	0.71
%rej.	0.47		0.4		0.66		0.47		0.47	

*15 pairs in each round

Figure 1

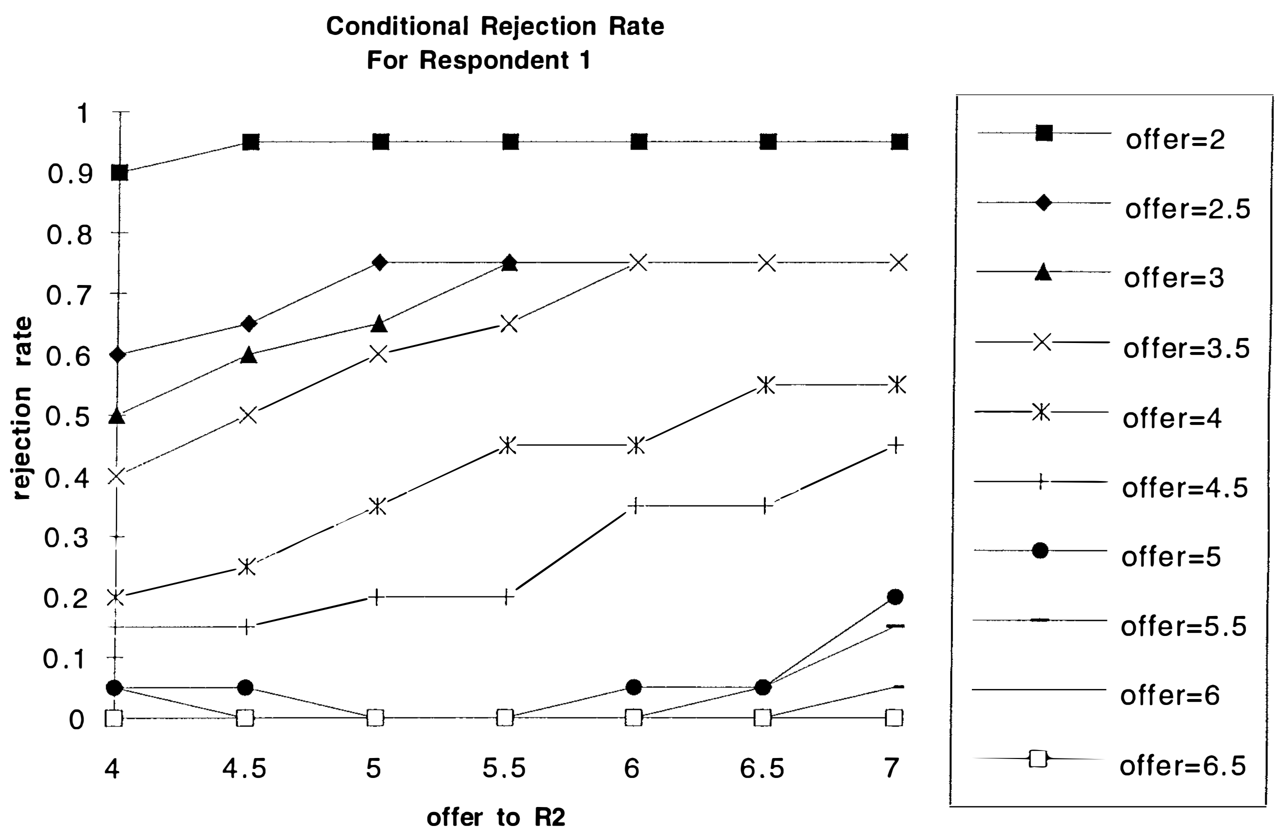

Conditional Rejection Rate
For Respondent 1
1
0.9
0.8
0.7
0.6
0.5
0.4
0.3
0.2
0.1
0
rejection rate
4
4.5
5
5.5
6
6.5
7
offer to R2
offer=2
offer=2.5
offer=3
offer=3.5
offer=4
offer=4.5
offer=5
offer=5.5
offer=6
offer=6.5

Figure 2

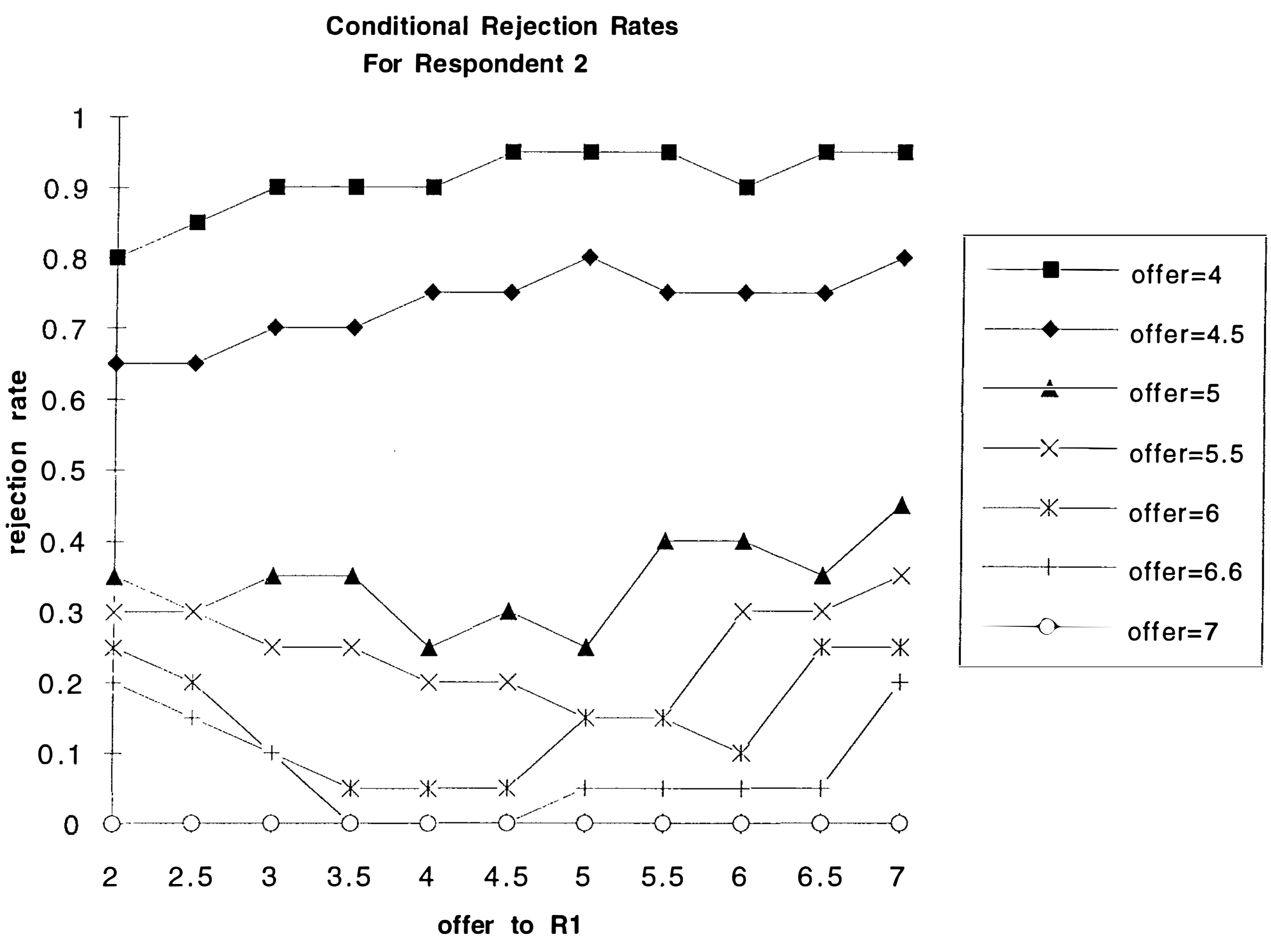

Conditional Rejection Rates
For Respondent 2
rejection rate
1
0.9
0.8
0.7
0.6
0.5
0.4
0.3
0.2
0.1
0
2
2.5
3
3.5
4
4.5
5
5.5
6
6.5
7
offer to R1
offer=4
offer=4.5
offer=5
offer=5.5
offer=6
offer=6.6
offer=7

Figure 3

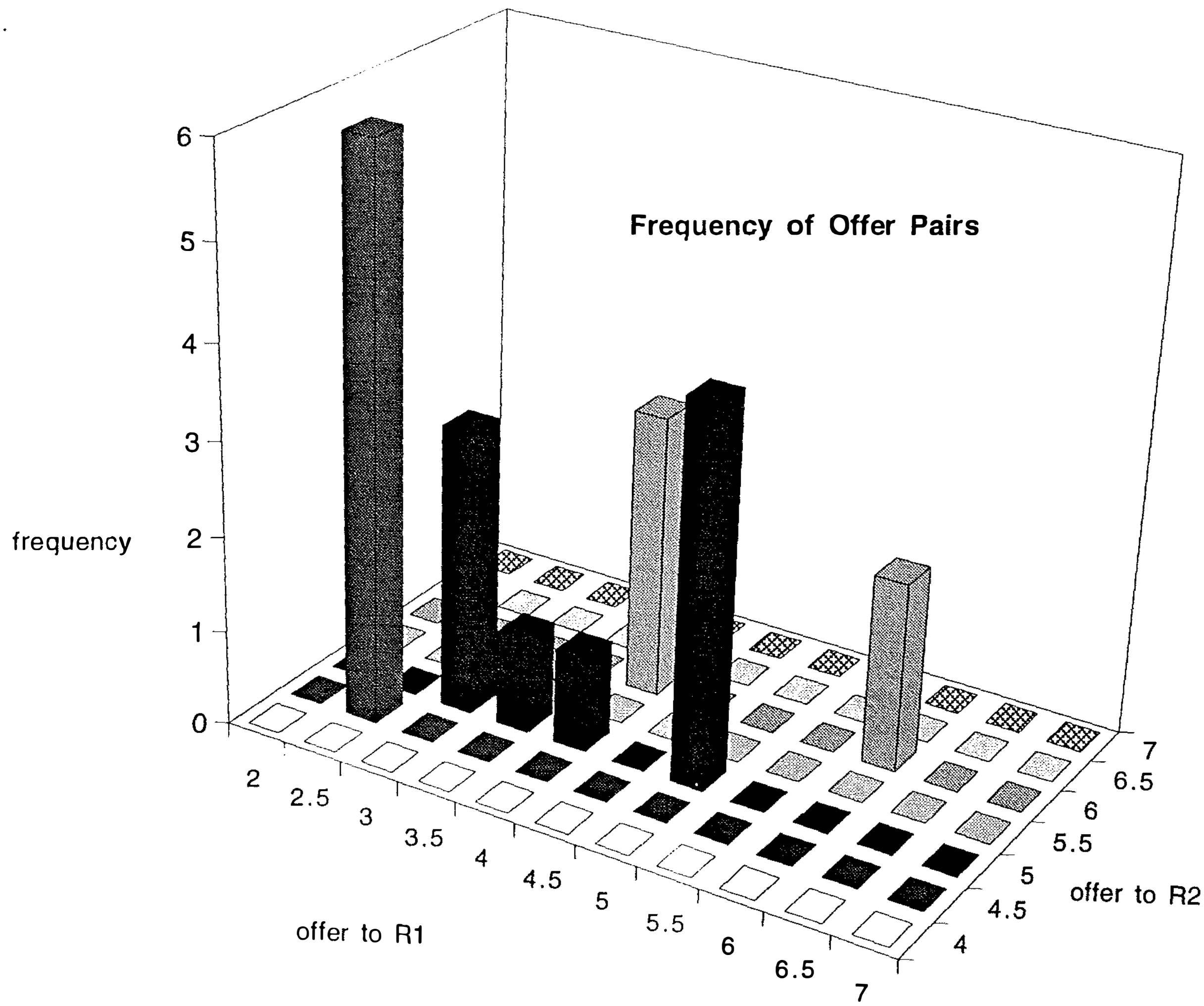
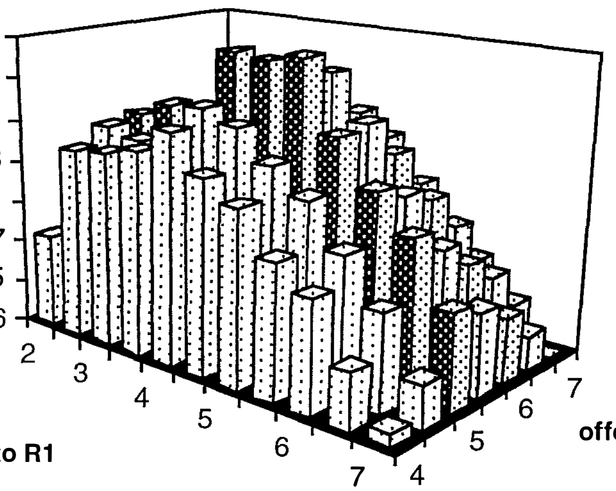

Frequency of Offer Pairs
frequency
0
1
2
3
4
5
6
offer to R1
2
2.5
3
3.5
4
4.5
5
5.5
6
6.5
7
offer to R2
4
4.5
5
5.5
6
6.5
7

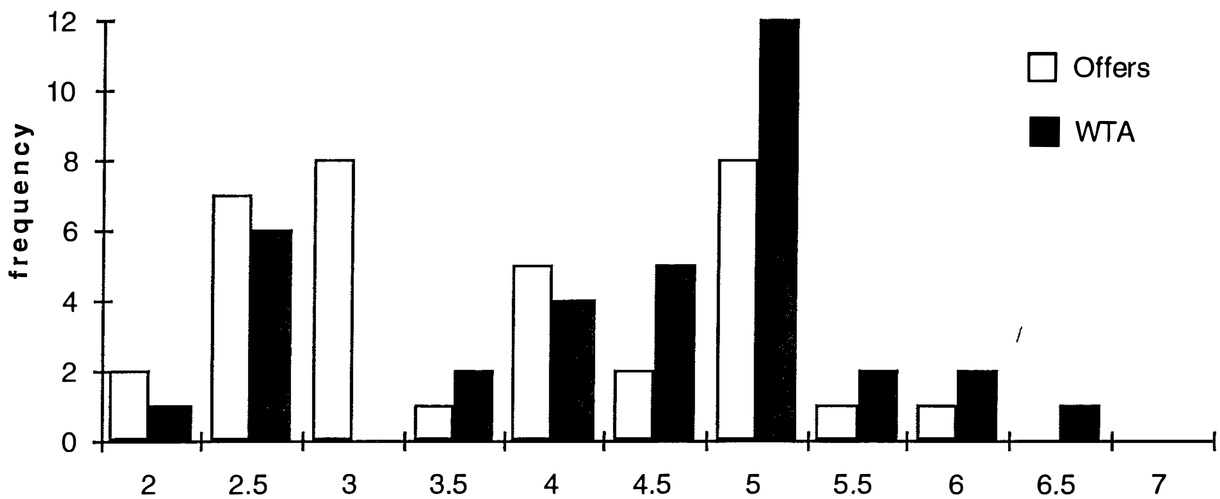
Figure 4: Expected Profits



Each bar gives the expected profits
for a particular offer pair, given the
conditional rejection rates.

Figure 5

Frequency of Offers and WTAs for R1



Frequency of Offers and WTAs for R2