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Preferences, Property Rights, and Anonymity in Bargaining Games

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Research on ultimatum and dictator games has found that because of "fairness" first movers in such games offer more than noncooperative game theory predicts. We find that if the right to be the first mover is "earned" by scoring high on a general knowledge quiz, then first movers behave in a more self-regarding manner. We also conducted dictator double blind experiments, in which the experimenter could not identify the decision maker. The results yielded by far our largest observed incidence of self-regarding offers, suggesting that offers are due to strategic and expectation considerations. *Journal of Economic Literature* Classification Numbers: C78, C91.

I. INTRODUCTION

The ethnologist Diamond Jenness, who was asked by the Canadian Government in 1913 to join Stefansson's Arctic expedition to study Eskimos for three years, records the following in his diary:

> "Not all the cabins that stood empty had been vacated until the next winter . . . and from two poles dangled a score or more fox skins. It was the latter that particularly caught my attention. Here were what amounted to a year's

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> earnings exposed wide open to the heavens, where the first passerby could appropriate them at his leisure. In reality, of course, they were as safe as in Brower's storeroom, for with a population so small, everyone always knew who was living where, and a pilferer had little or no chance of escaping detection. . . . honesty comes much more easily in a tiny community than it does in a great city, where misconduct always hopes that the multitude of alien tracks will cover up its own footprints." [Jenness, 1957, pp. 128–129]

Noncooperative, nonrepeated game theory is about strangers with no shared history, like the residents of Jenness' "great city." They meet, interact strategically in their individual self-interests according to well specified rules and payoffs, and never meet again. These stark conditions are necessary to assure that the noncooperative, nonrepeated game theoretic prediction for the interaction is not part of a sequence with a past and a future. Thus, repeated games are analyzed differently because now strangers can potentially cooperate by developing their own history and future. Moreover, the outcomes in two-person bargaining games are thought to be particularly sensitive to procedures affecting subject anonymity and the context of bargaining, since it is easy to identify individual actions when there are only two players. Thus, experimental studies of two-person bargaining games regularly take elaborate precautions to guarantee between-subject anonymity. In spite of these precautions, the results of bargaining experiments are generally not consistent with the game theoretic predictions, and they do not always replicate across subject populations, particularly in the absence of monetary rewards (Forsythe *et al.*, 1994).

For example, recent experimental research on ultimatum games has found that first movers in such games tend to offer more to their counterparts than noncooperative game theory would predict. In fact, the modal offer is half the surplus to be divided, although noncooperative game theory would suggest an offer by the first mover of the minimum positive amount that is feasible. In this paper we report the results of nonrepeated ultimatum and dictator game experiments designed to explore the underlying reasons for this apparent taste for "fairness." We found that if the right to be the first mover is earned by scoring high on a general knowledge quiz, and that right is reinforced by the instructions as being earned, then first movers behave in a significantly more self-regarding manner. Because our instructional procedures followed those in the literature and provided for intersubject anonymity as a partial control for the effect of social influences on choice, we conducted Double Blind dictator experiments, in which individual subject decisions could not be known by the experimenter or by anyone else except the decision maker. The results yielded by far our largest observed proportion of self-regarding offers—significantly more than obtained in any of our other treatments, or in any previously reported in the literature. Our interpretation is that offers in ultimatum and dictator games appear to be determined predominantly by strategic and expectations considerations. Other-regarding behavior is primarily an expectations phenomenon—what evolutionary ecologists call "reciprocal altruism" or simply reciprocity (Trivers, 1971; Hawkes, 1991)—rather than the result of an autonomous private preference for equity.

II. Ultimatum and Dictator Games

In an ultimatum game an amount of money M is to be divided between two subjects. One subject, designated the proposer, announces a split of $M - X$ to the proposer and X to the proposer's counterpart. After the proposal is made, the counterpart either accepts or rejects it. If the counterpart accepts, then the proposal is carried out; but if the counterpart rejects, then both the proposer and the counterpart get zero. If the counterpart is rational and nonsatiated in money, then he or she should accept $X = \varepsilon > 0$, where ε is the minimum unit of account. Thus the Nash equilibrium prediction is for the proposer to offer $X = \varepsilon$ and for the counterpart to accept. Experiments on nonrepeated ultimatum games by Guth *et al.* (1982; hereafter, GSS), Kahneman, *et al.* (1986), Forsythe *et al.* (1994; hereafter, FHSS), Roth *et al.* (1991), and others show that first mover proposers in such bargaining games offer more to their counterparts than noncooperative game theory leads one to expect. This tendency toward an equal split is described as being due to "fairness" considerations or to "social norms" of distributive justice. Such terms simply name the observed tendency toward equal outcomes observed in these experiments; they fail to explain the phenomenon in terms of more fundamental considerations that are testable. Bolton (1991) offers a formal model in which distributional considerations are incorporated into the bargainers' utility functions, an approach suggested earlier by Ochs and Roth (1989).

These experimental results are in contrast to those of a game with ultimatum strategic structure reported by Fouraker and Siegel (1963, pp. 34–36, 218–221; hereafter FS). They found strong support for the subgame perfect (Bowley) equilibrium bargaining prediction in the context of a single transaction. The equal-split payoff solution was distinct from the equilibrium point, but none of the 11 bargainers chose the equal split. The procedure used was what today we call a posted offer: the seller begins the process by choosing a price; this price is communicated to the buyer, who then chooses the quantity, thus ending the game. Consequently the seller makes an ultimatum (take-it-or-leave-it) price offer to the buyer. The FS procedures and design differed from the above ultimatum experiments in three ways: (1) all bargaining was described as a buyer/seller transaction; (2) the Nash equilibrium yielded more than an ε payoff to the buyer—in the asymmetric design the Nash equilibrium buyer's payoff was \$2.44, the seller's \$6.44; (3) both sellers and buyers had multiple price/quantity (and payoff) choices available, so that the all-or-none feature of the ultimatum game was not present, but the buyer was free to reject the price offer by choosing a zero quantity. These early FS findings, which helped to motivate the first ultimatum game experiments by GSS, suggest that the results of recent ultimatum games may be due to (1) the different context, or procedures, used; or (2) the fact that the second mover is expected to accept a miniscule reward (ε) at the Nash equilibrium. Thus, the ultimatum game may be a boundary experiment which asks if the Nash prediction still holds when the second mover is required to accept a much smaller payoff then the first mover (GSS, p. 369).

FHSS have also run an important baseline control for strategic behavior in the ultimatum game—the dictator game. In the dictator game the proposer decides on a split of money, M, which is final. The counterpart cannot reject the offer. In the ultimatum game the proposer must form expectations on the reservation value of the counterpart, i.e., the amount X which the counterpart will reject. Thus, concerns for "fairness" are confounded by the proposer's strategic expectations over reservation values. Since, in the dictator game, the proposer's split is final, expectations about the counterpart's reservation values are not assumed to enter into the proposer's decision. Theory predicts that a self-interested, nonsatiated dictator will take M, leaving nothing for the counterpart. FHSS find that proposers in the dictator game take significantly more (where M is either \$5 or \$10) than proposers in the ultimatum game. However, a substantial number, about 20%, still split 50–50. They conclude "that the distribution of proposals in the ultimatum game cannot be fully explained by a taste for fairness among proposers" (FHSS, p. 23). But how do we reconcile the ultimatum data with the dictator data?

A reasonable rational model of the data in both games can be stated in terms of subjects' expectations. In such simple experiments, particularly the dictator game, subjects may ask themselves (unconsciously): What is the experimenter's objective? (1) They may think that their actions in this game will affect the experimenter's decision to have them participate in future experiments. (2) They may think they *will* be chosen to participate in future experiments, but they may be concerned that their current decisions will affect *which* later experiments they are selected for. (3) They may be concerned about appearing greedy and being judged so by the experimenter. Under this latter interpretation "fairness" is not "own" preference, but a derivative of judgement by others. Note that none of these "explanations" requires a personal fairness ethic or utility-of-sharing considerations.

In the ultimatum game the proposer must form expectations about his or her counterpart's reservation value. Thus, a risk averse proposer may give his or her counterpart more than is predicted by noncooperative theory in order to insure acceptance of the proposal. Rational behavior is to choose $X^* = \arg\max u(M - X)F(X)$; where $F(X)$ is the first mover's subjective probability that offer X will be accepted, and captures the expectations of the proposer. But even a subject dictator may still be influenced by expectations about the experimenter's judgment, or future (subject recruiting) behavior, and thus may still give the counterpart a positive amount of money.

Experimenter knowledge of subject expectations is null, and control over them is limited to instructions and pregame treatments. Moreover, certain controls may be inadvertent. For example, in past experiments subjects were randomly assigned a type. Usually, randomization would be justified; when we cannot control for a variable we randomize its effect. But, in the ultimatum experiments, randomization may not be neutral, since it can be interpreted by subjects as an attempt by the experimenter to treat them fairly. Lotteries are often used for the "fair" award of rights such as hunting permits and basketball seats. Thus experimenters may unwittingly induce a "fair response." Subjects may feel that, since the experimenter is being fair to them, they should be fair to each other.

III. Property Rights

A property right is a guarantee allowing action within guidelines defined by the right. The guarantee is against reprisal, in that a property right places restrictions on punishment strategies which might otherwise be used to insure cooperative behavior. Property rights can be viewed as a means by which society legitimizes—makes "fair" (acceptable)—the action of a rights holder. Such rights are taken for granted in private ownership economies, but is this so for the subjects in bargaining experiments?

In bargaining experiments subjects' expectations may be more compatible, and the first mover less influenced by the possibility of punishment strategies by a counterpart, if the former has earned the right to make use of the advantaged position and the process of right acquisition is common information. Hoffman and Spitzer (1982, 1985; hereafter HS) present experimental data which support this view.[1] In the HS (1982) experiments two persons bargained face to face over the split of $14.

[1] Also see Burrows and Loomes (1989) who investigate further the hypothesis that people behave in a more self-interested manner when they have earned the right to do so. They report support for the hypothesis, but their results also show that people continue to place a value on "fair" outcomes, which is consistent with Hoffman and Spitzer (1985).

Before bargaining began one subject was chosen at random to be the controller. If subjects could not agree on a split the controller would receive \$12; the controller's choice was final. In these experiments 12 out of 12 pairs agreed to split the \$14 evenly even though this gave less to the controller then he or she could obtain by not agreeing. In the HS (1985) experiments, when the controller earned the right in a contest, and this right was reinforced as common knowledge in the instructions, only 4 of 22 bargaining pairs split equally, and on average proposers took \$12.52. Similarly, Guth and Tietz (1986) show that if first and second mover rights in the ultimatum game are auctioned independently to subjects, offers to second movers are much reduced.

Our contest assignment is meant to extend the HS (1985) assignment treatment to ultimatum games.[2] This contest is a current events quiz where subjects are ranked from highest to lowest using the number of correct answers. This assignment technique has been used previously by Binger *et al.* (1991); Cech (1988); and Wellford (1990).[3] If there are ties, each subject's total time in answering the questions is used as the tiebreaker (i.e., shortest time first). In HS (1985) a game of Nim was played by two players to see who would be the controller, but partners were randomly paired. In the contest reported in this paper both the choice of proposer and the pairings of proposers and counterparts are determined by subjects' rankings in the contest.

Except for two control experiments (and two Double Blind experiments) in which we use the FHSS instructions and the subjects' task is to divide \$10, all of our experiments are formulated as an exchange between a buyer and a seller, as in FS. This allows us to test for the effect of Exchange versus Divide \$10. Usually, bargaining is treated as an exchange. This context may itself confer legitimacy and common expectations on a more self-regarding offer by the first mover.[4]

[2] Other experimental treatments might also result in similar changes in the expectations of first movers in ultimatum games. For example, Harrison and McKee (1985) and Burrows and Loomes (1989) essentially replicate the Hoffman and Spitzer (1985) experimental results using different mechanisms for inducing a sense of justification for being the first mover.

[3] Contest software for use on IBM networked personal computers is available on disk by writing author Smith.

[4] Typically experimenters want to infer some conclusion about markets when discussing their experimental results. For example, Kahneman *et al.* [1986, p. 105–106] report experiments in which subjects are asked to reallocate \$10, provisionally allocated to each pair, using simultaneous move rules; i.e., the second mover marks those first mover offers that are acceptable and those that are not before knowing the first mover's decision. They report a strong tendency toward equal split with a substantial portion of the second movers willing to reject positive offers. The authors suggest that such resistance to unfairness "is of the type that might deter a profit-maximizing agent or firm seeking to exploit some profit opportunities (p. 106)." In order to better justify the extension of such results to firms, we hypothesize that it may be important to describe the setting as an exchange between a buyer and a selling firm, and not as one of reallocating \$10 provisionally allocated to each pair.

Seller Chooses

PRICE

Buyer Chooses to

	$0	$1	$2	$3	$4	$5	$6	$7	$8	$9	$10	
BUY	$0	$1	$2	$3	$4	$5	$6	$7	$8	$9	$10	Seller profit
	$10	$9	$8	$7	$6	$5	$4	$3	$2	$1	$0	Buyer profit
NOT BUY	$0	$0	$0	$0	$0	$0	$0	$0	$0	$0	$0	Seller profit
	$0	$0	$0	$0	$0	$0	$0	$0	$0	$0	$0	Buyer profit

FIG. 1. Payoff chart given to subjects.

IV. EXPERIMENTAL DESIGN

In each experimental session, 12 subjects participate simultaneously. Each subject is paid $3 for arriving on time for the experiment. When all subjects have arrived, they first read and then have read to them (by Hoffman) a set of instructions which describe the buy/sell task. In the random assignment treatment, subjects are then randomly assigned the positions of buyer and seller and randomly (and anonymously) paired with one another.[5] In the contest assignment treatment, subjects answer 10 current events questions. The subject ranked No. 1 is the seller, paired with the subject ranked No. 7 as the buyer. The subject ranked No. 2 is paired with the subject ranked No. 8, and so on. No subject is informed of the identity of his or her counterpart and each experimental session involves only one pairing and one decision. Participants earn $0.25 for each correct answer, in addition to their earnings in the subsequent experiment.

After the buyer and seller assignments have been made, each seller chooses a price, given the payoff chart shown in Fig. 1. This payoff chart shows that the game is essentially an ultimatum game embedded in an exchange. There is $10 to divide between the seller and the buyer. If the seller states a price of $9 and the buyer agrees to buy, the seller gets $9 and the buyer gets $1. Similarly, if the seller states a price of $8 and the buyer agrees to buy, the seller gets $8 and the buyer gets $2. As in other ultimatum games, and in FS, if the buyer decides to not buy, both buyer and seller receive $0.

[5] We do not, however, use the word "random" in the instructions to the subjects. We tell them they have been paired anonymously. See the instructions labeled "random" in the Appendix.

TABLE I

NUMBER OF BARGAINING PAIRS BY GAME TYPE AND GAME CONTEXT

Game type:	Ultimatum		Dictator	
Game Context	Divide $10	Exchange	Divide $10	Exchange
FHSS Results	24		24	
Random entitlement, FHSS instructions	24			
Contest entitlement, FHSS instruction	24			
Random entitlement		24		24
Contest entitlement		24		24
Double Blind 1			36	
Double Blind 2			41	

While the sellers are choosing prices, the buyers are answering a questionnaire [labeled Buyer Questionnaire in the Appendix]. The questionnaire serves two purposes. First, it allows us to give a piece of paper to each participant, thus obscuring the identification of the buyers and sellers. Second, the questionnaire asks the buyer to tell us both what price he or she would have chosen and what price he or she expects the seller to choose. These data allow us to test whether expectations are affected by the assignment of the property right.

Once the sellers have chosen prices, we circle the appropriate seller's price choice on each buyer's choice form and ask the buyers to circle BUY or NOT BUY. While the buyers are making their choices, we ask the sellers to answer a questionnaire about their expectations of buyer behavior. Simultaneously passing out the questionnaire also serves the additional purpose of continuing to obscure the identification of buyers and sellers. Once the buyers have made their decisions, we determine each individual subject's earnings, including payment for correct answers in the current events quiz, and pay them individually and privately.

The above procedures are also applied to the dictator game, except that the buyer has no decision to make. In the exchange context, this means that the buyer has a prior commitment to make the purchase whatever the price chosen by the seller.

Table I lists the number of bargaining pairs that participated in all the experiments that we report here. For example, we ran 24 subject pairs in Ultimatum Exchange and in Dictator Exchange, as indicated by the column headings, and with Random Entitlement, as indicated in the row heading. In row 1, for comparison, we list those experiments reported by FHSS which we describe here as the Divide $10 experiments to distinguish them from our Exchange experiments. Thus, in the FHSS instructions subjects are told that "A sum of \$5 (\$10) has been provisionally allocated to each pair . . ." (FHSS, p. 27; also see Kahneman *et al.*, 1986), p. 105). Note particularly that this instruction suggests that neither bargainer has a clear property right to the money; literally, it provisionally belongs to both of them. FHSS paid their subjects a \$3 participation fee in addition to the proceeds of the division of \$10. In all but the double blind experiments reported here we also paid our customary \$3 participation fee in addition to each bargainer's split of the \$10.

As a means of comparing our subjects and procedures with those of FHSS, we conducted one random entitlement and one contest entitlement experiment using the FHSS Divide \$10 instructions. Note, however, that we did not follow FHSS in assigning buyers and sellers to separate rooms because our contest treatment required the same-room (common knowledge) condition and we wanted to maintain comparability with our other experiments. These are not intended as pure replications of FHSS. Rather, we ask if their results are robust with respect to the experimenters, subjects, and same-room condition.

As indicated above, we also have been concerned that subjects in bargaining experiments may be influenced either by (1) imagined use by the experimenter of their decisions to decide whether to recruit or how to use subjects in a later experiment, or by (2) judgments of the subjects' decisions by the experimenter, or by others who see the data, in spite of guarantees of anonymity. The point is that in all of the "anonymous" bargaining experiments known to us the subject knows that the experimenter is fully informed as to who made what decision. "Anonymity" means that neither bargainer in a pair knows the identity of the other subject and that subjects across bargaining pairs do not know one another's identities or decisions, but the experimenter still knows everything.

This particular kind of between-subject anonymity has been standard in private bargaining studies going back to Siegel and Fouraker (1960). This protocol was continued in FS and in all recent private bargaining studies. The procedure has been justified on the grounds that the absence of anonymity, as in face-to-face interactions, brings into potential play all the social experience with which people are endowed, causing the experimenter to risk losing control over preferences (also see Roth, 1990).

We agree with this assessment, but propose that it also applies to the experimenter as a potential socializing factor. To eliminate observation by the experimenter, we designed a new set of Divide \$10 experiments, in which subjects are guaranteed anonymity with respect to everyone: other subjects, the experimenter, and anyone who might view their decisions. Since subject decisions and payoffs are anonymous with respect to both the experimenters and the subjects we call this treatment Double Blind.[6]

In the Double Blind 1 experiments reported in this paper, 15 people are recruited to room A and 14 to room B. The same instructions are read by each subject, and then read orally by an experimenter in each room (A, McCabe; B, Smith). All subjects are paid a $5 show-up fee (now standard in our lab, this experiment being one of the first). One of the subjects in room A is voluntarily selected to be the monitor in the experiment. The monitor is paid $10. The instructions state that 14 plain white unmarked opaque envelopes contain the following: 2 of the envelopes contain 20 blank slips of paper each, and 12 contain 10 blank slips and 10 one-dollar bills each. Each subject is given an envelope by the monitor, proceeds to the back of the room, and opens the envelope inside a large cardboard box which maintains his/her strict privacy. The subject keeps 0 to 10 of the one-dollar bills and 10 to 0 of the blank slips of paper, so that the number of bills plus slips of paper add up to 10. For the envelopes with 20 blank slips, 10 are returned to the envelope. (In this way all returned envelopes feel equally thick. Moreover, each person in room A knows that if his/her counterpart in room B receives an envelope with 10 slips of blank paper, it could be because there was no money in the envelope originally. Thus, it is really true that "no one can know".)

After everyone is finished in room A, the monitor goes to room B, sits outside the room, and calls each person out one at a time. The person selects an envelope, opens it, and keeps its contents, which are recorded by the monitor on a blank sheet of paper containing no names. The experimenter accompanies the monitor to answer any questions that arise, but does not particpate in this process. These procedures are intended to make it transparent that room A subjects are on their own in deciding how much to leave their counterparts in room B, and that no one can possibly know how much they left their counterparts. The use of a monitor minimizes experimenter involvement and guarantees that someone from room A besides the experimenter can verify that there is actually a room B with 14 subjects, as stated in the instructions.

The above procedures represent a substantial departure from those used

[6] The conventional use of "double blind" in medical experiments means that neither the subject nor the experimenters know which subject is receiving which treatment. This use of the term is not appropriate in economics experiments since the subject must necessarily know the treatment (instructions and situation) in order to perform the task. We propose a more appropriate use for the term "Double Blind" in economics. Moreover, our use of the term has already been adopted by others (Davis and Holt, 1992). And ours is not the only use of the term to denote anonymity from two points of view (Blank, 1991).

in our other experiments. This was deliberate; we wanted to do a step-out experiment that would include everything we thought might be important in protecting the subjects' total anonymity. The results, as we discuss below, were dramatic in reducing the distribution of offers. Which of these procedures are most important? The use of a monitor who is paid \$10 may help to suggest that the subjects in room A should take all the money. Contrary to this, the examples used in the instructions (giving \$2 and \$9; see Appendix) suggest that something should be given. Again, having two envelopes with only blank slips may suggest that giving nothing is in order. Using envelopes actually containing the money is itself a departure from having subjects choose numbers, then paying them afterwards. Any or all of these features could be important.

As a first step among many variations that we intend to study we conducted a series of Double Blind 2 experiments (41 pairs). In these experiments we eliminated the paid subject monitor; that function was performed by the room A experimenter. Second, we eliminated the use of the envelopes with 20 blank slips of paper and did three replications, each using 14 envelopes containing 10 one-dollars bills and 10 blank sheets of paper each. (In one session we had only 13 subjects in each of the rooms.)

V. Ultimatum Game First-Mover Results

FHSS evaluate the power of five non-parametric tests to distinguish between different sample distributions: the Cramer–von Mises, Anderson Darling (AD), Kolmorogov–Smirnov, Wilcoxon rank-sum, and Epps–Singleton (ES) test. They find that the AD and ES tests have the most statistical power in the context of ultimatum games. They also note that the ES test has the added advantage of not requiring the distributions being tested to be continuous. Epps and Singleton (1986) also investigate the power of the ES test versus the Anderson–Darling, Cramer–Von Mises, and Kolmorogov–Smirnov tests. Epps and Singleton find that the power of the ES test is superior to the other tests in distinguishing between different continuous distributions. Furthermore, the difference is even more pronounced when the distributions being compared are discrete.

The ES test is based upon characteristic functions. It compares the difference between the characteristic functions of two samples to test the null hypothesis that the characteristic functions, and hence the distributions, are equal. In Table II we report the results of pairwise comparisons

TABLE II

PAIRWISE EPPS–SINGLETON TESTS

χ^2 Statistic (Probability significance level)	FHSS results Divide $10 Dictator	Random entitlement Divide $10 FHSS Instructions Ultimatum	Contest entitlement Divide $10 FHSS Instructions Ultimatum	Random entitlement Ultimatum exchange	Random entitlement Dictator exchange	Contest entitlement Ultimatum exchange	Contest entitlement Dictator exchange	Double Blind 1 Divides $10 Dictator
FHSS results, Divide $10 Ultimatum	30.9 (0.00)	5.2 (0.27)	30.5 (0.00)	22.4 (0.00)		35.5 (0.00)		238.4 (0.00)
FHSS results, Divide $10 Dictator					7.3 (0.12)		5.7 (0.22)	23.6 (0.00)
Random entitlement, Divide $10 FHSS instructions, Ultimatum			10.4 (0.03)	10.4 (0.03)		19.3 (0.00)		304.6 (0.00)
Contest entitlement, FHSS instructions, Ultimatum				2.7 (0.60)		6.1 (0.19)		283.7 (0.00)
Random entitlement, Ultimatum exchange					6.4 (0.17)	4.9 (0.29)		144.4 (0.00)
Random entitlement, Dictator exchange							13.2 (0.01)	44.9 (0.00)
Contest entitlement, Ultimatum exchange								114.4 (0.00)
Contest entitlement, Dictator exchange								11.8 (0.02)
Double Blind 2, Divide $10 Dictator								3.0 (0.56)

using the ES test with the small sample correction.[7] Since comparisons using the ES test are not based only on the first moment of the distribution, we include charts of the data that show how our data shift with the treatments and Table III which reports results from the Wilcoxon rank sum test (also called Mann–Whitney; the test was discovered at least five

[7] The tests are conducted in the following manner. The first step is to form a vector representing the real and imaginary parts of the characteristic function for each sample (treatment):

$$g(X_{km}) = (\cos \hat{t}_1 X_{km}, \sin \hat{t}_1 X_{km}, \cos \hat{t}_2 X_{km}, \sin \hat{t}_2 X_{km}),$$

where $\hat{t} = t/\hat{\sigma}$ and t is a real number,

$$g_k = n_{k^{-1}} \sum_m g(X_{km}),$$

m is an index on the observation within a specific sample, and k is an index on samples.

Epps and Singleton (1986), ES, provide calculations to determine the power-maximizing values for t_1 and t_2. $\hat{\sigma}$ is a scaling measure for t and is calculated as

$$\hat{\sigma} = 0.5\left[\frac{(Y_U + Y_{U-1})}{2} + \frac{(Y_L + Y_{L+1})}{2}\right],$$

where $\{Y_i\}$ is sample 1 and sample 2 combined and then placed in ascending order; L is the greatest integer in $(n_1 + n_2)/4$; and U is $n_1 + n_2 - L$. The test statistic is W_1, given by

$$W_1 \equiv N(g_1 - g_2)' \hat{\Omega}^{-1}(g_1 - g_2),$$

where

$$\hat{\Omega} = (\hat{S}_1 + \hat{S}_2)\, N[(n_1^{-1} + n_2^{-1})]/2,$$

and

$$\hat{S}_k = n_k^{-1} \sum_{m-1}^{n_k} g(X_{km})\, g(X_{km})' - g_k g_k'.$$

If the null hypothesis that the characteristic functions are the same is true, then the test statistic is distributed as a chi square with 4 degrees of freedom.

ES also derive a small sample correction that improves the power of the test in small samples. The small sample correction is given as

$$\hat{C}(n_1, n_2) = [1 + (n_1 + n_2)^{-0.45} + 10.1\,(n_1^{-1.7} + n_2^{-1.7})]^{-1}$$
$$W_2 = W_1 \cdot \hat{C}.$$

The results reported in Table 1 include the small sample correction.

TABLE III

PAIRWISE WILCOXON RANK SUM TESTS

Wilcoxon W (Probability significance level)	FHSS results Divide $10 Dictator	Random entitlement Divide $10 FHSS Instructions Ultimatum	Contest entitlement Divide $10 FHSS Instructions Ultimatum	Random entitlement Ultimatum exchange	Random entitlement Dictator exchange	Contest entitlement Ultimatum exchange	Contest entitlement Dictator exchange	Double Blind 1 Divide $10 Dictator
FHSS results, Divide $10 Ultimatum	−4.61 (0.00)	−1.74 (0.08)	−4.33 (0.00)	−3.63 (0.00)		−4.83 (0.00)		−6.04 (0.00)
FHSS results, Divide $10 Dictator					−0.822 (0.41)		−2.20 (0.03)	−3.52 (0.00)
Random entitlement, Divide $10 FHSS instructions, Ultimatum			−3.09 (0.00)	−2.69 (0.00)		−4.09 (0.00)		−5.88 (0.00)
Contest entitlement, Divide $10 FHSS instructions, Ultimatum						−1.92 (0.02)		−5.58 (0.00)
Random entitlement, Ultimatum exchange					−2.06 (0.04)	−1.66 (0.09)		−5.86 (0.00)
Random entitlement, Dictator exchange							−3.08 (0.00)	−3.86 (0.00)
Contest entitlement, Ultimatum exchange							−4.28 (0.00)	−5.25 (0.00)
Contest entitlement, Dictator exchange								−1.71 (0.09)
Double Blind 2, Divide $10 Dictator								−.52 (0.61)

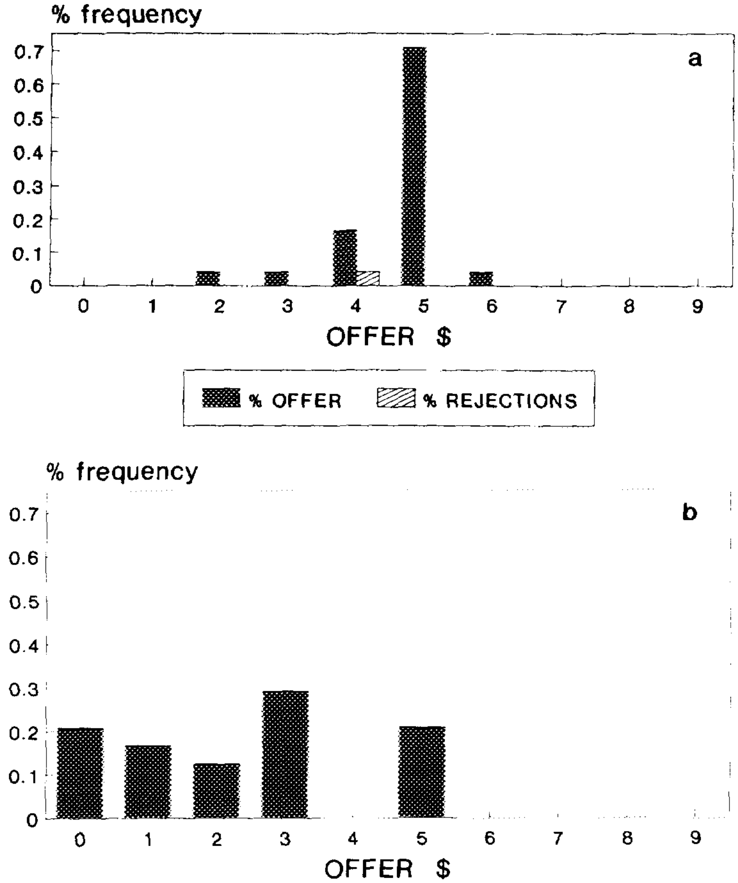

FIG. 2. Results from the FHSS experiments: (a) Ultimatum, Divide \$10 experiment, $N = 24$; (b) Dictator, Divide \$10 experiment, $N = 24$.

times going back to 1914; see Kruskal, 1957). Comparing Tables II and III, all comparisons that are significantly different using the ES test are also significant for only the rank-sum measure of a shift in the distribution. However, the "good" results (lower significance levels) in many of the comparisons using the Wilcoxon test are qualified because of this test's poor power characteristics.

Figure 2 charts the data from FHSS for their \$10 ultimatum experiments and their \$10 dictator experiments. Since the FHSS paper and data provided one of the three major motivations for the present study (the second being HS, 1985; and the third being FS, 1963), Fig. 2 sets the stage for reporting our results. In the comparison shown in Fig. 2, FHSS report that the dictator results are significantly different (more self-regarding) than the ultimatum data (also see Tables II and III).

Figure 3 provides a four-way comparison among our Divide \$10, Random and Contest, experiments using FHSS instructions, and the parallel experiments presented as a buyer/seller exchange. Note first that our Divide \$10 ultimatum experiments (Fig. 3a) replicate those of FHSS (Fig. 2a): i.e., different subjects, different experimenters, and "same room" conditions yield results that are not significantly different from the FHSS results (p is 0.27 in Table II, 0.08 in Table III). Comparing Random versus Contest when presented as Divide \$10 (Figs. 3a and 3b), we observe a statistically significant ($p = 0.03$ in Table II) shift toward lower offers in the contest treatment. In the experiments presenting the task as a buyer/seller exchange, the contest entitlement also shifts the offers to a lower level as compared with the random entitlement (Figs. 3c and 3d); the difference is not significant by ES ($p = 0.22$ in Table II), but is significant by Wilcoxon. Comparing Divide \$10 versus Exchange under Random entitlement (Figs. 3a and 3c), Exchange shifts the offers to a significantly lower level ($p = 0.03$ in Table II). With contest entitlements (Figs. 3b and 3d); however, the lowering of offers as a result of the exchange treatment is not significant ($p = 0.22$ in Table II). Comparing the combined effect of exchange and contest (Fig. 3d with Fig. 3a), we observe a highly significant ($p = 0.00$, Tables II and III) shift toward self-regarding offers.[8] Much of this shift, however, is due to the effect of *exchange alone*, which helps to account for the strong results reported by FS.

The results of our seller questionnaire, providing data on seller expectations of buyer (or second mover) acceptance behavior, were as follows. Using the FHSS (Divide \$10) instructions, every first mover expected his/her offer to be accepted under both the Random and Contest entitlements; also, all sellers stated that they would have accepted if they had been buyers. Under the Random/Exchange treatment one seller stated that he/she did not expect the buyer, who in fact did buy; all would have bought if they had been buyers. Under the Contest/Exchange treatment all sellers expected their buyers to accept; however, one would not have accepted his own offer of \$2 if he had been the buyer. It was accepted anyway.

[8] By "self-regarding offers" we mean simply lower offers. One reader has interpreted it as meaning that realized expected utility, $U(M - X^*)F(X^*)$, increases with the exchange and contest treatments, and stated that this was *not* the case. This is incorrect. Since our first mover results show that X^* decreases significantly, while our second mover results (Section VI below) show that the net rejection (and acceptance) rates, $F(X^*)$, do not alter significantly, it follows that $U(M - X^*)F(X^*)$ increases with our treatments. First movers are made subjectively better off, on balance, with the exchange and contest treatments, and can therefore be said, in the expected utility sense, to have made more self-regarding offers.

Overwhelmingly, across all treatments, first movers expected their offers to be accepted. This is consistent with the high observed acceptance rate discussed in the next section. It is also consistent with self-regarding offer motivation tempered by expectations (the risk of rejection), as indicated by the analysis of Section II above.

VI. Ultimatum Game Second Mover Results

The hatched bars in Figs. 3a–3d provide frequency data on the second-mover rejection rates for each of our treatments. These rates are very low: 2/24 (8.3%) in Random/Divide \$10; 0/24 in Contest/Divide \$10; 2/24 (8.3%) in Random/Exchange; and 3/24 (12.5%) in Contest/Exchange. None of these rates is significantly different from any other, nor from that of FHSS (Fig. 2a). The buyer (second-mover) questionnaires for all treatments yielded only one buyer (in the Contest/Exchange treatment) who thought the seller expected his/her offer to be rejected. That buyer, in fact, accepted the offer.

The importance of these results is indicated by the fact that most ulimatum game experimenters (see below) have emphasized that a substantial proportion of positive offers are rejected, and that this is taken into account by first movers. Since all of our procedures (and those of FHSS that used a \$10 pie) resulted in little or no, and in any case no significant, differences in rejection rates, we concluded that our treatments were highly successful in inducing common expectations on the bargaining pairs. In particular, our contest and Exchange treatments not only produced significantly lower offers relative to Random/Divide \$10, but this was accomplished without any detectable increase in the rejection rates. Thus, *first movers accurately gauged the willingness of second movers to accept lower offers as we shifted to treatments eliciting lower offers*. It is therefore appropriate to say that in these treatments the self interests of first movers were served not only in offering less, but also in their expectations that their risks of rejection would not rise accordingly. Statistically, the risk of rejection of the lower offers remained unchanged.

VII. Dictator Game Results

Figures 4a and 4b chart the frequency distributions of the data for our dictator games under the Random and Contest entitlements, respectively. The contest treatment lowers the offer distribution and the difference is significant ($p = 0.01$ in Table II). In Fig. 4c we chart the distribution of our Double Blind 1 dictator data. From the latter it is clear that the Double Blind treatment is by far our most potent. When no one can know what the

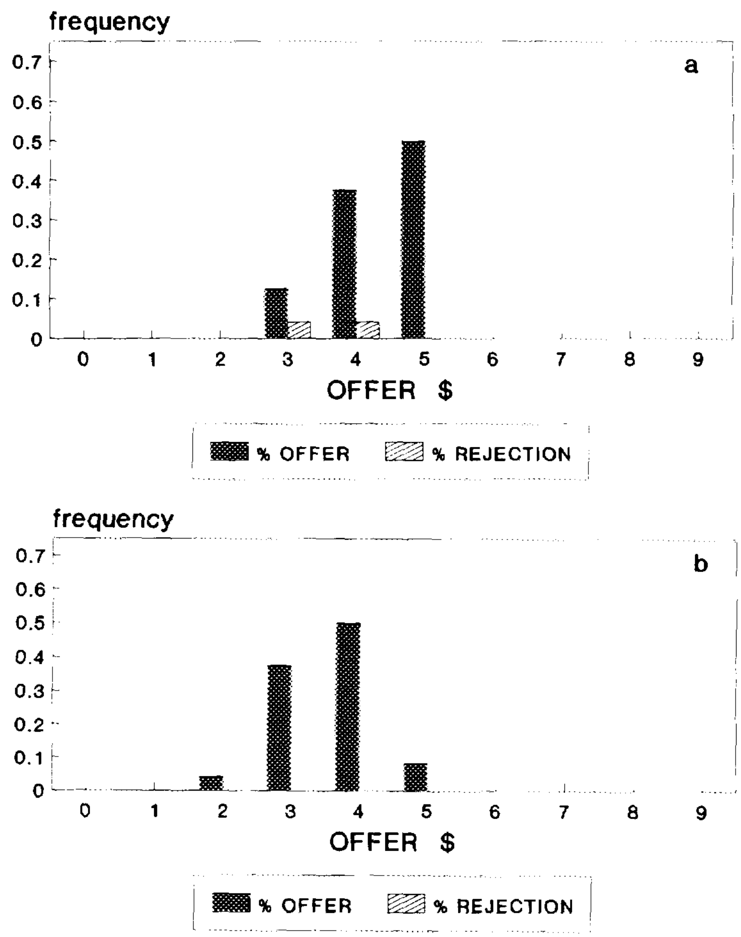

FIG. 3. Comparison of results obtained from Divide $10 experiments presented using the FHSS instructions and those obtained from experiments presented asa buyer/seller exchange. N = 24 in all cases. (a) Ultimatum, random entitlement, FHSS instructions, Divide $10; (b) ultimatum, context entitlement, FHSS instructions, Divide $10; (c) ultimatum, random entitlement, exchange; (d) ultimatum, contest entitlement, exchange.

first mover offers his/her counterpart, the offer distribution is dramatically lowered relative to all dictator and ultimatum treatments. With one exception (Double Blind 2), the Double Blind 1 dictator results are significantly different (lower offers) as compared to all our other treatments and the FHSS treatments.

If we look just at the proportion of dictators giving $0 to their counter-

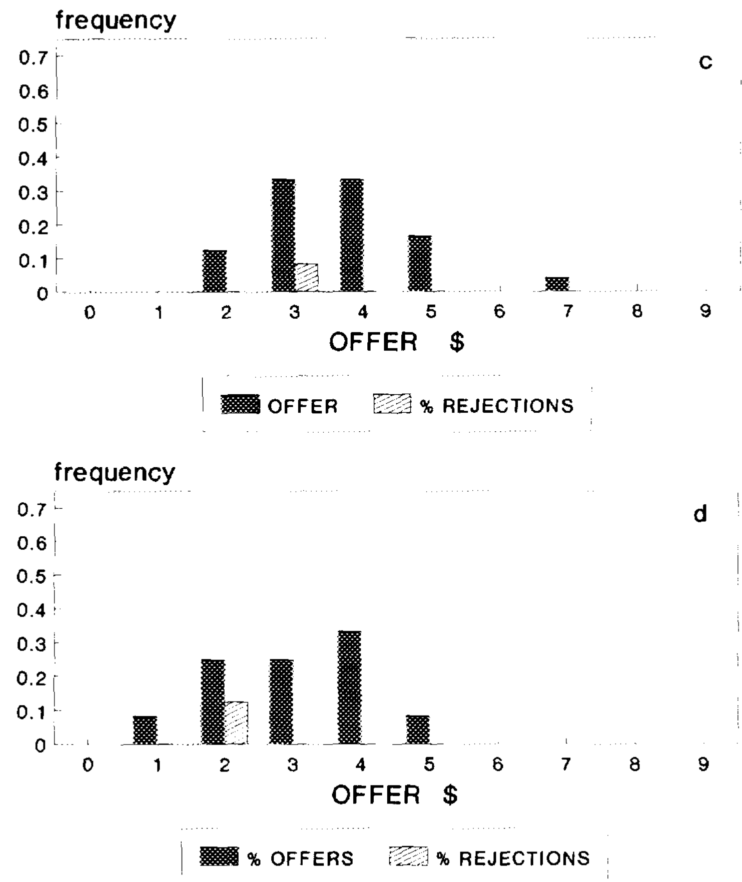

FIG. 3—*Continued*

parts, the differences become quite clear. In the FHSS and Random/Exchange treatment, only about 20% offer \$0; a similar proportion still offers \$5. In the Contest/Exchange treatment about 40% offer \$0; another 40% offer \$1 or \$2. In the Double Blind 1 dictator treatment, over two-thirds of the first movers now offer \$0 and 84% offer \$0 or \$1; only 2 of 36 subjects offer \$5.

In Fig. 4d we chart the Double Blind 2 data. Comparing Figs. 4c and 4d shows the robustness of Double Blind with respect to the use of a paid monitor, and the use of two padded, no money, envelopes. Although the offers are slightly higher, the statistical comparisons in Tables II and III

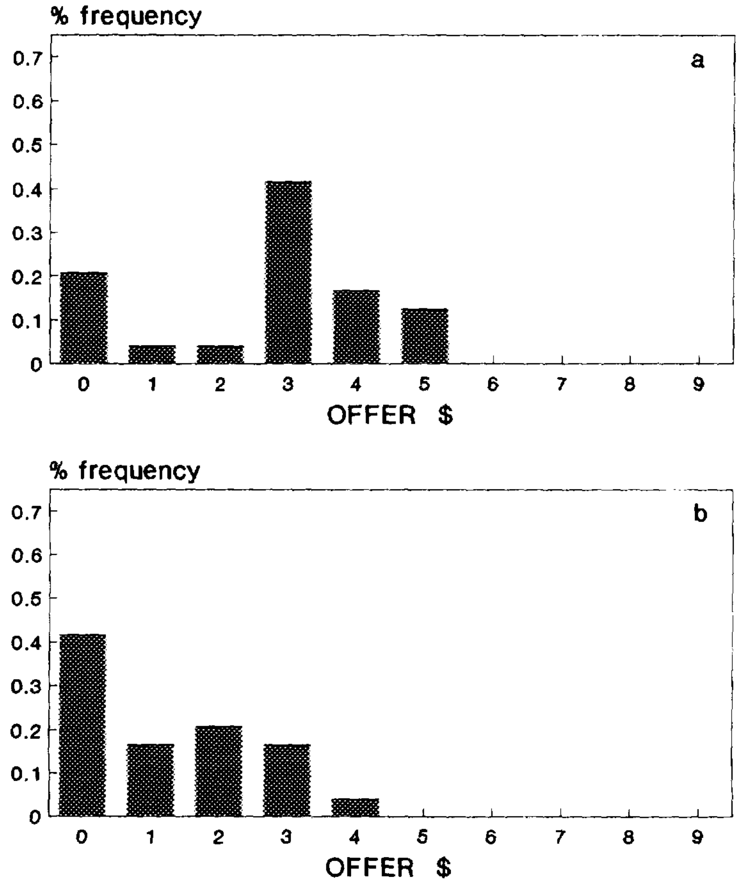

FIG. 4. Results for Dictator, Double Blind 1, and Double Blind 2 experiments. (a) Dictator, random entitlement, exchange, $N = 24$; (b) Dictator, context entitlement, exchange, $N = 24$; (c) Dictator, random entitlement, Divide \$10, Double Blind 1, $N = 36$; (d) Dictator, random entitlement, Divide \$10, Double Blind 2, $N = 41$.

show that the Double Blind results are robust to the indicated procedural factors ($p = 0.56$ and 0.61). Clearly, Double Blind 1 and 2 are both powerful treatments relative to all our other experiments. The results also further reinforce our general conclusion that the class of games we are considering is sensitive to the procedural, contractural, and instructional setting of the experiment.[9]

[9] The use of an experimenter to monitor the Double Blind 2 experiments allows an impressionistic "observation" to be made: some subjects in room A did not seal their envelopes as instructed, and we could not help but observe that such subjects tended predominantly

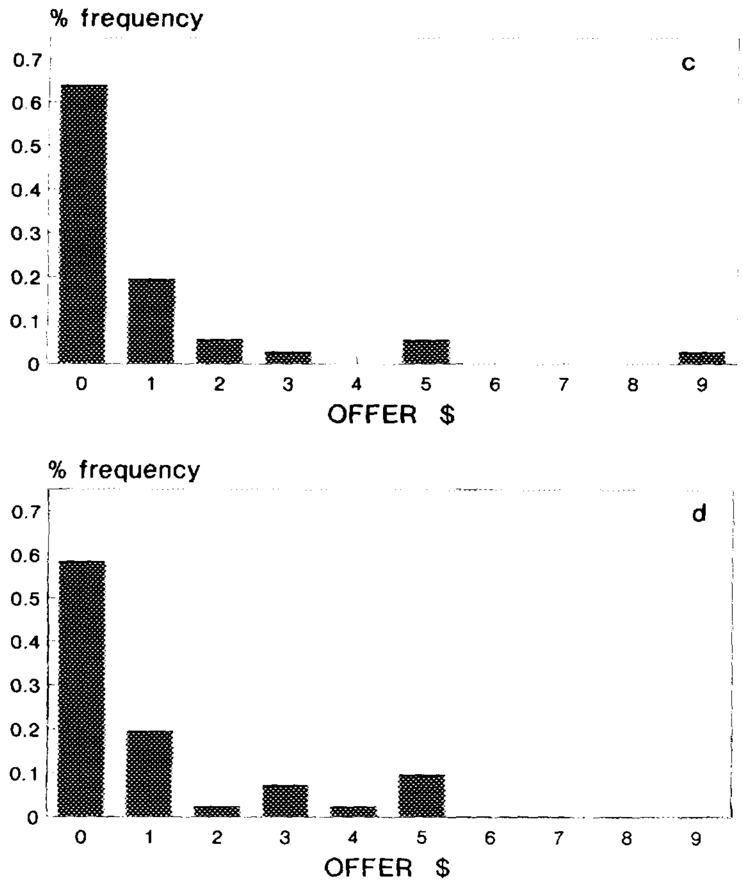

FIG. 4—*Continued*

VIII. Relation of Results to the Theoretical and Experimental Literature

Bolton (1991) has proposed a formal extension of standard bargaining theory in which a bargainer's welfare depends upon her own monetary payoff, and upon a comparison of her own earnings with that of her

to be the ones who left positive amounts in their envelope. It would appear that people feel ambivalent toward completely anonymous giving. As we were unprepared for this we did not record any observations. What would have been the effect on giving if we had insisted that all envelopes be sealed before the subjects left the privacy of their boxes?

counterpart with whom the pie is shared. Potential counterparts prefer more equal splits, including the possibility of (0, 0), to proposals which offer them substantially less than the proposer. First movers must take this fact strategically into account in deciding how much to offer. Bolton's model is thus a mixture of fairness considerations in the sense of tastes, and strategic considerations in the sense of a subgame perfect equilibrium as in the standard ultimatum game. The difference is that payoff and relative payoff are substitutes in bargainer utility functions. It is important to emphasize that the model was directly motivated by data from previous ultimatum game experiments, but has new testable implications which hold up well in the new experiments reported.

We like Bolton's methodological approach, which is to (1) modify the conventional model, rather than abandon it prematurely, retaining the assumptions of maximizing, perfect equilibrium behavior; and (2) test it. The model is not, however, consistent with either the FHSS or our results for the dictator game. This is beause Bolton's bargainers desire fair treatment only for *themselves*, and first movers must take this into account in deciding their ultimatum offers. In the dictator game. Bolton's decision maker should ignore the concerns of his/her counterpart and take the entire pie. Most subjects do not do this except in our Double Blind dictator games. Consequently, the latter approaches consistency with the Bolton model.[10]

Guth and Tietz (1986) determine the bargaining positions in the ultimatum game by independent auctions of the right to be the first or second mover. For example (Guth and Tietz, 1986, Table 2), a subject paid DM 25 for the right to move first in an ultimatum game for splitting DM 55. The subject was matched with another subject who paid DM 5 for the right to move second. Finally, the first mover offered DM 20 to the second, who accepted it. The first mover's net payoff was DM 10, and the second DM 15. Although data for only 12 subjects are reported (each participating in 3 rounds of ultimatum games), the results are consistent with the outcomes of our Contest/Exchange treatment. Guth and Tietz (1986, Table 12) report that in only 2 of 36 rounds do first movers offer as much as half of their pie. In our Contest/Exchange treatment only 2 in 24 subjects offer this much. Auctions tend to increase the incidence of self-regarding offers for two reasons: (1) they serve to legitimize the right to exploit the first mover advantage, but (2) what constitutes an equal outcome is redefined as the original pie net of auction prices.

Thus, in the above example, the first mover demands DM 35, asking

[10] But that a taste for fairness is not necessary to comprehend the ultimatum results has been demonstrated using a model of *rivalrous* bargainers and incomplete information on the part of the first mover (Burnell *et al.*, 1992).

for a profit of only DM 10 over his/her auction price of DM 25. The offer of DM 20 is two-thirds of the pie net of auction price (DM 30). We like the auction approach to allocating first mover rights, but our Contest/Exchange treatment is designed to examine legitimacy ("earning" rights) *without altering* the net pie to be divided. First movers in the Guth and Tietz (1986) experiments offer very liberal percentages of the pie net of auction price, whereas our subjects offer comparatively low percentages of the pie under Contest/Exchange.[11]

Roth *et al.* (1991) have conducted the most extensive and ambitious study of ultimatum bargaining behavior yet attempted: an international comparison of behavior in ultimatum bilateral and multilateral (market) bargaining. In their market experiments nine buyers with value M each state a buying price (offer of a split of the pie). The seller, with zero cost, can accept or reject the highest of these offers. If the seller accepts a buyer's offer, the pie is divided between the seller and that buyer according to the terms of the offer and all the other buyers get zero. If the seller rejects all offers, all get zero. Each player learns whether a transaction occurred and the price. The process is repeated for 10 periods with a changing population of buyers. Viewed as a market, the supply is inelastic at one unit with willingness-to-accept of zero. The maximum willingness-to-pay demand is constant at M per unit, the amount of the pie to be divided, up to nine units. The excess supply is constant at eight units for all prices from zero up to the equilibrium price of M-ε (actually any price for M-ε to M is an equilibrium). This environment is known in the experimental literature as a "swastika" supply and demand design, and has been studied extensively, under both complete and private information (see, for example, Smith, 1965, 1976, 1980; Smith and Williams, 1990; Cason and Williams, 1990). This is known to be an extremely competitive environment (unless selling and buying capacities are equal (Smith and Williams, 1990) or nearly equal (in Smith, 1965, the case with only two excess sellers is among those examined)); such markets converge easily in less than 10 periods. Roth *et al.* (1991, pp. 1075–1082) nicely replicate these results in the context of what is customarily called the (buyers) posted price trading institution. They then compare the market results to the results of the ultimatum bargaining experiments, which show consistently lower first-mover prices.

We agree with their interpretation that first movers in both the market

[11] Since the auction winners in Guth and Tietz (1986) did not know the prices paid by their counterparts, they faced considerable additional uncertainty, above that in the known-pie ultimatum experiments, as to how much their counterparts would find acceptable. Our contest/exchange treatment creates a common-knowledge property right in a certain amount of money. Our results could be compared with those of an auction procedure that allows both bargainers knowledge of the net pie to be divided.

and the bilateral bargaining environments are making offers that are directed to maximizing expected return. This is true in the markets because those buyers posting prices less than the equilibrium get rejected; it is true in bilateral bargaining because the first mover believes (correctly) that less than equilibrium prices are accepted with high enough probability to yield an acceptable risk–reward tradeoff.[12]

Turning to the bargaining data in Roth *et al.* (1991), the overwhelming difference in their results vis-à-vis ours is the very large rejection rates by their second movers. They report rejection rates that vary from 10% to a remarkable high of 44%, with averages (across rounds) varying from 22 to 29%. These high rejection rates indicate a substantial divergence of expectations between bargainers. The standard game theoretic assumption of common expectations ("knowledge") is clearly not satisfied.

Why do our results yield such low rejection rates, while the Roth *et al.* results yield such high rejections rates? We offer three reasons. First, in the light of our Exchange treatment results, and the buyer/seller results of FS, the choice of the buyer instead of the seller to be the first mover in Roth *et al.* is problematic.[13] Culturally, almost everywhere, it is the seller who is thought to be justified in naming a price, not the buyer. Casting the buyer in this role creates the likelihood of a considerable role-reversal conflict in both subjects' behavior. Second, the use of rounds introduces the possibility that punishment strategies will be invoked because subjects' treat them as socially relevant, not appreciating the game theoretic assumption that punishment is not supposed to be viable when you never get matched twice with the same partner. McCabe *et al.* (1992) report a surprising incidence of strategies clearly intended to punish noncooperative behavior, although the bargaining pairs are rerandomized after

[12] However, Roth *et al.* suggest that the "competitive pressures" (behavior away from equilibrium) in the market need not be due to income-maximization, and they give an example (p. 1093) based on fairness preferences in which a buyer punishes other buyers by bidding above them. This contradicts earlier studies showing [Smith, 1976, 1980] that convergence to equilibrium is *more* rapid under private than under complete information: under private information fairness preferences cannot enter the explanation; only income maximization and competitive considerations can apply. Consequently, based on all the data the interpretation is as follows: under complete information, income maximization overwhelms any preference for equity, but the presence of the latter *could* be a factor (it could also be due to strategic behavior) in retarding the convergence rate relative to that under private information. Thus equity considerations cannot explain convergence in the Roth *et al.* market experiments, although they might possibly tell us why convergence was not still faster than that observed.

[13] Note that in the published version of their paper, the bargaining/ultimatum game is discussed as divide-the-prize. In their working paper, which includes instructions, it is clear that they use an "Exchange" context. This indicates the importance of having instructions when trying to understand specific experimental results.

each play. To control for this, play should not be repeated in ultimatum games. Third, subjects were actually paid on only one of the ten rounds, the round being chosen at random. This makes expected payoffs quite low, and greatly reduces the lost profit consequence to rejecting the first mover's offer. The FHSS ultimatum data show that rejection rates decrease as rewards increase: with no reward, rejections are 8/48 (16.6%); with \$5 pies rejections are 3–43 (6.98%); with \$10 pies rejections fall to 1/24 (4.17%). All of our experiments used a pie of \$10. In Roth *et al.* (1991) expected payoffs in a round are (1/10) \$10 = \$1 and (1/10) \$30 = \$3.00 for the U.S. data.

IX. Conclusion and Discussion

Here is a brief summary and interpretation of our primary findings.

(1) In ultimatum games first mover offers are sensitive to the instructional (property right and contractual) setting of the experiment. In particular, offers are smaller if the context is that of an exchange between a seller and a buyer instead of a Divide \$10 task, or if the first mover earns the (instructionally reinforced) right to his/her role instead of having it assigned in a random manner. When an earned entitlement is combined with exchange, less than 45% of the first movers offer \$4 or more. When we combine Random Entitlement with Divide \$10, more than 85% offer \$4 or more, in line with previously reported ultimatum game outcomes. But the strategic/expectational character of ultimatum games makes it impossible to conclude from offer data alone whether offers in excess of \$1 are due to other-regarding preferences or to the first mover's concern that his/her offer might be rejected unless it is deemed satisfactory by the second mover. The dictator game proposed by FHSS controls for strategic considerations in the ultimatum game.

(2) In dictator games reported by FHSS, where the task is to divide \$10 (Random Entitlement), only about 20% of the first movers offer \$4 or more. Our replication of FHSS reinforces their results, although our subjects are in the same room.

(3) We find that dictator games are also sensitive to the instructional (property right and contractual) settings of the experiment. When exchange is combined with the contest entitlement, only 4% of first movers offer \$4, none offer \$5. But over 20% give \$3 or \$4; so these results also show that some incidence of other-regarding behavior cannot be ignored entirely as an element in either dictator games or ultimatum games under the usual anonymity conditions.

(4) What is the nature of this other-regarding behavior? We only begin to answer this question with the Double Blind dictator experimental results summarized in this paper. In this design, subjects are guaranteed anonymity not only with respect to other subjects, but also with respect to the experimenters and everyone else—only the first mover can possibly know his/her offer. In our Double Blind 1 experimental results only 4 in 36 subjects, or 11%, give $3 or more to their counterparts. This may *approach the appropriate indicator of fairness as a pure preference phenomenon.* The full data are not significantly altered in our Double Blind 2 experiments, which eliminate the paid monitors, and the use of two in fourteen envelopes padded with blank white paper and including no money. But other aspects of the double blind procedures require experimental examination to identify what is driving the outcome; an envelope containing the cash might be an important factor.

(5) These Double Blind dictator results (which, so far, are robust) imply that the outcomes in both dictator and ultimatum games should be modeled not primarily in terms of other-regarding preferences (or "fairness") but primarily in terms of expectations—either explicit strategic expectations as in ultimatum games, or implicit concern for what the experimenter (or others) might think or do in dictator games. These Double Blind experimental results are inconsistent with any notion that the key to understanding experimental bargaining outcomes is to be found in subjects' autonomous, private, other-regarding preferences. At the very minimum, these results suggest that other-regarding preferences may have an overwhelming social, what-do-others-know, component, and therefore should be *derived* formally from more elementary expectational considerations. These results also suggest that the argument for the use of anonymity in bargaining experiments as a means of controlling for social influences on preferences has not gone far enough. The presence of the experimenter, as one who knows subjects' bargaining outcomes, can be one of the most significant of all treatments for reducing the incidence of self-regarding behavior.

The results of these Double Blind experiments also appear to raise fundamental questions regarding the nature and origins of other-regarding behavior in our society. The results suggest that such behavior may be due not to a taste for "fairness" (other-regarding preferences), but rather to a social concern for what others may think, and for being held in high regard by others. If this view is correct, other-regarding behavior can be interpreted as a form of social exchange, in which I share some of my resource claims with others, in return for their esteem and good offices (and thus in return for shares of their resource claims, as is frequently observed in aboriginal tribes).[14] This interpretation accords with our opening quotation from Jenness wherein the Eskimo were found to be oblivious to possible theft.[15] Jenness did not say this was *due* to "honesty," which was just a word for the phenomenon, not an explanation; instead he attributes it to the close community ties which allow monitoring and community discovery of theft. The contemporary ethnology literature, which is much focused on understanding sharing traditions in aboriginal societies, uses the term reciprocity (Hawkes, 1991, and Trivers, 1971, call it "reciprocal altruism") for the phenomenon in which individuals incur short term costs for their sharing in exchange for delayed benefits from others' sharing. Consequently, repayment from reciprocators provides a net benefit in the self-interest. Participants in this exchange process discriminate against those who do not return favors. Social traits like honesty and sharing are best, rational, policies. Such actions are possible in close-knit communities because each individual can "keep score" and punish free-riders with sanctions. Such relations break down where sociability is pushed to the edge of credibility, as in our Double Blind experiments.[16] But this simply shows how very strong is the game theoretic assumption that bargainers have no social relationship before or beyond the single instance in which they interact in their self-interest. That theory, together with this experimental testing, has, however, served an important function in explicating these diehard social issues.

APPENDIX: Instructions for Experiments

FHSS REPLICATION, RANDOM ENTITLEMENTS

Instructions

You have been asked to participate in an economics experiment. In addition to the $3 you already received for participation, you may earn an additional amount of money, which will be paid to you at the end of the experiment.

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[14] This interpretation predicts that truly anonymous gift giving, where the contribution is not even known to family members or close friends, would be rare. For example, it is customary at church services to pass collection plates in public, during the service.

[15] It is further illustrated by Hughes's (1982) account of the closing of the seasonal Alaskan salmon cannery where he worked as an accountant in the summer of 1951. The half-Eskimo watchman was removing locks and chains from various items of equipment; when asked why, he stated that locks and chains are not needed now that the Christians are gone.

[16] It is also well known that they break down under conditions of catastrophic destruction: riots, floods, earthquakes, and war devastation. Such conditions produce mini-doomsdays in which, temporarily, there is no tomorrow like yesterday, and one observes looting by normally law abiding citizens. Reciprocity must be reestablished in the aftermath of the event. In the interim the police are often reinforced with troops such as the national guard.

In this experiment each of you will be paired with a different person in this room. You will not be told who that person is either during or after the experiment, and he or she will not be told who you are either during or after the experiment.

The experiment is conducted as follows: a sum of $10 has been provisionally allocated to each pair, and person A in each pair can propose how much of this each person is to receive. To do this, person A will fill out a "Proposal Form." The proposal consists of an amount that person B is to receive and the amount that person A is to receive. The amount that person A is to receive is simply the amount to be divided, $10, minus the amount that person B is to receive.

When each person A has made a proposal, the proposal forms will be distributed to the appropriate B's and each person B will be given a change to accept or reject the proposal made by his or her counterpart person A. If person B accepts the proposal, then the amount of money will be divided as specified in the proposal. If person B wishes to accept the proposal he or she should check "accept" on the proposal form. If person B does not wish to accept the proposal he or she should check "reject" on the proposal form. If person B rejects the proposal, both A and B will be paid nothing.

After all the B's have accepted or rejected the proposal made by the A's each person will be paid according to the terms of the proposal.

Are there any questions?

FHSS REPLICATION, CONTEST ENTITLEMENTS

Instruction Changes

[The first two paragraphs and the first sentence of the third paragraph are identical to the random instructions.]

[Paragraph three continues . . .] The positions of persons A and B in each pairing will be determined by your scores on a general knowledge quiz. The quiz will be given concurrently to 12 participants. Each of you will be asked to answer the same set of 10 questions, selected from a large data bank of questions. Your quiz score will be the number of questions you answer correctly. Quiz scores will be ranked from highest to lowest and ties will be decided by giving a higher ranking to the person who finishes the quiz in the shortest amount of time. Note #1 is the highest rank while 12 is the lowest. Once the complete ranking of participants is determined, those ranked 1–6 will have *earned* the right to be A's. Notice being an A and making the proposal is a definite advantage in this experiment. The other six participants will be B's. The A with the highest rank (A1) will be paired with the highest ranking B (B7), the A with the second-highest rank (A2) will be paired with the second-highest ranking B (B8), and so on. Your total score will not be publicized under your name.

Once person A has *earned* the right to be an A, he or she will fill out a "Proposal Form." The proposal consists of an amount that person B is to receive and the amount that person A is to receive. The amount that person A is to receive is simply the amount to be divided, $10, minus the amount that person B is to receive. [The rest of the instructions are identical to the random instructions.]

FHSS REPLICATION

Proposal Form

(1) Identification Number __A__
(2) Paired With __B__
(3) Amount to divide ________
(4) Person B receives ________
(5) Person A receives (3)–(4) ________
(6) Accept _____ Reject _____

ULTIMATUM, BUY-SELL, RANDOM ENTITLEMENTS

Instructions

In this experiment you have been paired anonymously with another person. One of you will be the seller, the other the buyer. The seller chooses the selling PRICE. Then the seller's choice is presented to the buyer who chooses to BUY or NOT BUY. In the following table each cell shows the possible profit, in dollars, in the upper right corner for the seller, and in the lower left corner for the buyer. For example, if the seller chooses PRICE = $8, and then the buyer chooses BUY, the seller will be paid $8 and the buyer will be paid $2. If the seller chooses PRICE = $1, and then the buyer chooses BUY, the seller makes $1, and the buyer $9. If the buyer chooses NOT BUY, each of you will be paid nothing, whatever might have been the seller's choice of PRICE. The seller will be given a choice form. After he/she has circled a PRICE choice, the experimenter will circle this PRICE on the buyer's choice form, and the buyer will choose BUY or NOT BUY.

ULTIMATUM, BUY-SELL, CONTEST ENTITLEMENTS

Instruction Changes

In this experiment you will be paired with another person. One of you will be the seller, the other the buyer. The positions of buyer and seller in each pairing will be determined by your scores on a general knowledge quiz. The quiz will be given concurrently to 12 participants. Each of you will be asked to answer the same set of 10 questions, selected from a large data bank of questions. Your quiz score will be the number of questions you answer correctly. Quiz scores will be ranked from highest to lowest and ties will be decided by giving a higher ranking to the person who finishes the quiz in the shortest amount of time. Note #1 is the highest rank while 12 is the lowest. Once the complete ranking of participants is determined, those ranked 1–6 will have earned the right to be sellers. Notice being a seller and choosing price is a definite advantage in this experiment. The other six participants will be buyers. The seller with the highest rank (seller 1) will be paired with the highest ranking buyer (buyer 7), the seller with the second highest rank (seller 2) will be paired with the second-highest ranking buyer (buyer 8), and so on. Your total score will not be publicized under your name.

Once the seller has earned the right to be the seller, he/she chooses the selling PRICE. [The rest follows from sentence two of the random instructions.]

ULTIMATUM, BUY-SELL, RANDOM ENTITLEMENTS

Seller Choice

You are the seller. Please circle your choice of PRICE in the top row of the following profit table.

ULTIMATUM, BUY-SELL, CONTEST ENTITLEMENTS

Seller Choice (Changes)

You have earned the right to be the seller.
[Sentence two of random seller choice follows.]

ULTIMATUM, BUY-SELL, RANDOM ENTITLEMENTS

Buyer Questionnaire

1. If you had been the seller in this experiment what PRICE would you have chosen? ________ (write in the PRICE).
2. What PRICE do you expect the seller to choose? ________ (write in the PRICE).
3. What choice do you think the seller expected you to make? BUY ____ NOT BUY ____ (check your answer).

ULTIMATUM, BUY–SELL, CONTEST ENTITLEMENTS

Buyer Questionnaire (Changes)

1. If you had earned the right to be a seller in this experiment what PRICE would you have chosen? ________ (write in the PRICE).

[Questions 2 and 3 are the same as for the random entitlement.]

ULTIMATUM, BUY–SELL, RANDOM ENTITLEMENTS

Buyer Choice

You are the buyer. The price chosen by the seller is shown circled in the top row of the table below.

Please circle your choice of BUY or NOT BUY in the left column of the column of the following profit table.

ULTIMATUM, BUY–SELL, CONTEST ENTITLEMENTS

Buyer Choice (Changes)

[Add following sentence at end of 1st paragraph]: Recall that the seller earned the right to be a seller.

[Rest is the same as for the random entitlements.)

ULTIMATUM, BUY–SELL, RANDOM AND CONTEST ENTITLEMENTS

Seller Questionnaire

1. If you had been the buyer in this experiment would you have chosen to BUY ____, or NOT BUY ____? (check your answer).
2. What did you expect the buyer to choose? BUY ____, or NOT BUY ____ (check your answer).
3. What PRICE do you think the buyer expected you to choose? ________ (write in the PRICE)

ULTIMATUM, BUY–SELL, RANDOM AND CONTEST ENTITLEMENTS

Choice Form

Seller Chooses

PRICE

		$0	$1	$2	$3	$4	$5	$6	$7	$8	$9	$10	
Buyer Chooses to	BUY	$0	$1	$2	$3	$4	$5	$6	$7	$8	$9	$10	Seller profit
		$10	$9	$8	$7	$6	$5	$4	$3	$2	$1	$0	Buyer profit
	NOT BUY	$0	$0	$0	$0	$0	$0	$0	$0	$0	$0	$0	Seller profit
		$0	$0	$0	$0	$0	$0	$0	$0	$0	$0	$0	Buyer profit

DICTATOR, BUY–SELL, RANDOM ENTITLEMENTS

Instructions

In this experiment you have been paired anonymously with another person. One of you will be the seller the other the buyer. The seller chooses the selling PRICE, and the buyer must buy at that price. This determines the profits of both the seller and the buyer. In the following table each cell shows the possible profit, in dollars, in the upper right corner for the seller, and in the lower left corner for the buyer. For example, if the seller chooses PRICE = \$8, the seller will be paid \$8 and the buyer will be paid \$2. If the seller chooses PRICE = \$1, the seller makes \$1 and the buyer \$9. The seller will be given a choice form. After he/she has circled a PRICE choice, the experimenter will collect the forms.

DICTATOR, BUY–SELL, CONTEST ENTITLEMENTS

Instruction Changes

In this experiment you will be paired with another person. One of you will be the seller the other the buyer. The positions of buyer and seller in each pairing will be determined by your scores on a general knowledge quiz. The quiz will be given concurrently to 12 participants. Each of you will be asked to answer the same set of 10 questions, selected from a large data bank of questions. Your quiz score will be the number of questions you answer correctly. Quiz scores will be ranked from highest to lowest and ties will be decided by giving a higher ranking to the person who finishes the quiz in the shortest amount of time. Note #1 is the highest rank while 12 is the lowest. Once the complete ranking of participants is determined, those ranked 1–6 will have <u>earned</u> the right to be sellers. Notice being a seller and choosing price is a definite advantage in this experiment. The other six participants will be buyers. The seller with the highest rank (seller 1) will be paired with the highest ranking buyer (buyer 7), the seller with the second-highest rank (seller 2) will be paired with the second-highest ranking buyer (buyer 8), and so on. Your total score will not be publicized under your name.

Once the seller has <u>earned</u> the right to be the seller, he/she chooses the selling PRICE, and the buyer must buy at that price. This determines the profits of both the seller and the buyer. [The rest follows from sentence three of the random instructions.]

DICTATOR, BUY–SELL, RANDOM ENTITLEMENTS

Seller Choice

You are the seller. Please circle your choice of PRICE in the top row of the following profit table.

DICTATOR, BUY–SELL, CONTEST ENTITLEMENTS

Seller Choice (Changes)

You have <u>earned</u> the right to be the seller. [The 2nd sentence is the same.]

DICTATOR, BUY–SELL, RANDOM ENTITLEMENTS

Buyer Questionnaire

1. If you had been the seller in this experiment what PRICE would you have chosen? ________ (write in the PRICE).
2. What PRICE do you <u>expect</u> the seller to choose? ________(write in the PRICE).

DICTATOR, BUY-SELL, CONTEST ENTITLEMENTS

Buyer Questionnaire (Changes)

1. If you had earned the right to be a seller in this experiment what PRICE would you have chosen? ________ (write in the PRICE).

[Same question 2]

DICTATOR, BUY-SELL, RANDOM AND CONTEST ENTITLEMENTS

Buyer Form

You are the buyer. The price chosen by the seller is shown circled in the top row of the table below.

DICTATOR, BUY-SELL, RANDOM AND CONTEST ENTITLEMENTS CHOICE FORM

Seller Chooses

PRICE

$0	$1	$2	$3	$4	$5	$6	$7	$8	$9	$10	
$0	$1	$2	$3	$4	$5	$6	$7	$8	$9	$10	Seller profit
$10	$9	$8	$7	$6	$5	$4	$3	$2	$1	$0	Buyer profit

DICTATOR, DIVIDE $10, DOUBLE BLIND 1

Instructions[17]

You have been asked to participate in an economics experiment. For your participation today we have paid you $5 in cash. You may earn an additional amount of money, which will also be paid to you in cash at the end of the experiment.

In this experiment each of you will be paired with a different person who is in another room. You will not be told who these people are either during or after the experiment. This is room A.

You will notice that there are other people in the same room with you who are also participating in the experiment. You will not be paired with any of these people.

One of the persons in room A will be chosen to be the monitor for today's experiment. The monitor will be paid $10 in addition to the $5 already paid. The monitor will be in charge of the envelopes as explained below. In addition the monitor will verify that the instructions have been followed as they appear here.

The experiment is conducted as follows: Fourteen unmarked envelopes have been placed in a box. Twelve of these envelopes contain 10 one dollar bills and 10 blank slips of paper. The remaining 2 envelopes contain 20 blank slips of paper. The monitor will be given a list of names of people in the room. He or she will call one person at a time to the back of the

[17] The instructor emphasized orally that the subjects were to remain quiet and ask only procedural questions. This is important to keep subjects from communicating editorial comments about the experiment.

room, and hand each person an envelope from the box. The person who was called will then go to one of the seats, with a large box on top, in the back of the room.[18] The envelope will then be opened privately inside the box. Only the person who was given the envelope will know what the envelope contains.

Each person in room A must decide how many dollar bills (if any) and how many slips of paper to put in the envelope. The number of dollar bills plus the number of slips of paper must add up to 10. The person then pockets the remaining dollar bills and slips of paper. Examples: (1) Put $2 and 8 slips in the envelope, pocket $8 and 2 slips. (2) Put $9 and 1 slip in the envelope, pocket $1 and 9 slips. These are examples only, the actual decision is up to each person. If the envelope has 20 blank slips, put 10 blank slips in the envelope and pocket the other 10. This is done in private and we ask that you tell no one of your decision. Notice that each envelope returned will look exactly the same. Also note that no one else, including the experimenter will know the personal decisions of people in room A.

Once you have made your decision you will seal your envelope and place it in the box marked return envelopes.[19] You may then leave the room.

After all fourteen envelopes have been returned the monitor will take the box to room B. There are 14 people in room B. Each of these persons has been paid $5 to participate. The monitor will be given a list of names of people in room B. The monitor will then call up the people in room B. The monitor will choose an envelope from the box, open the envelope, record its contents, and give the contents of the envelope to the person called up. They are then free to leave. The monitor will continue until all the envelopes have been handed out and everyone else has left the room. The experiment is then over.

DICTATOR, DIVIDE $10, DOUBLE BLIND 2

Instruction Changes

Same as Double Blind 1, except all references to a student monitor and to envelopes containing 20 blank slips of paper are removed.

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[18] The experimenter maintained a substantial distance between himself and the subjects using the box. Also, two boxes, well separated, were used to speed up the process.

[19] Not all subjects sealed their envelopes. The experimenter should reinforce this instruction with a reminder as the envelopes are passed out.

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