Social comparisons in ultimatum bargaining

Iris Bohnet and Richard Zeckhauser*

This paper experimentally examines the effect of social comparisons in ultimatum bargaining. While previous experiments and the theoretical models on fairness focus on the two bargaining counterparts, we address a new reference group—others in like circumstances. We inform responders on how much other responders are offered before they decide whether to accept or reject their specific offer. Such information has a sizable positive effect on offers, rejection and equal split rates. This result is consistent with people disliking deviations from the norm of equity but inconsistent with fairness theories, where people dislike income disparity between themselves and their referents. (JEL C91)

---

* Kennedy School of Government, Harvard University, 79 JFK Street, Cambridge, MA 02138, email Iris_Bohnet@Harvard.edu, Richard_Zeckhauser@Harvard.edu

We thank Simon Gächter for his detailed comments and Miriam Avins, Max Bazerman, Gary Bolton, Suzanne Cooper, Lorenz Götte, Axel Ockenfels and the participants of the CBRSS workshop on experimental research 2002 and the Economic Science Association meetings 2002 for their helpful suggestions. Financial support from the Kennedy School of Government Dean's Research Fund is greatly appreciated.

Ample findings suggest that in bargaining games, people typically depart from the standard notions of money-maximizing preferences.[1] Attempting to explain such results, recent theoretical models on fairness and reciprocity focus on the relationship between the two bargainers.[2] But parties to a negotiation do not always compare themselves to their bargaining counterpart. In salary negotiations, for example, prospective employees typically do not compare their wage (or wage less reservation price) with the surplus the employer reaps from their employ, but rather with the wages of similarly situated employees.[3] This paper addresses a new reference group, so far hardly included in economic studies on bargaining though often considered by real-life bargainers, namely others in like circumstances. The term "social comparisons" refers to information on such "others".

We use the ultimatum game,[4] probably the most studied bargaining game, to investigate experimentally the role of social comparisons between responders. In our version of the game, we also inform the responders of the average offer being made in a given round. People might make social comparisons for two reasons, which would lead to different outcomes in our experiment:

The *Relative Standing Hypothesis* states that responders may not only care about the absolute amount of money they receive but also about their standing relative to other responders.

---

[1] For recent surveys of bargaining games, see Camerer (in press) and Roth (1995).

[2] For a recent survey of theories of fairness and reciprocity, see Fehr and Schmidt (2001).

[3] See e.g., Babcock et al. (1996) and Frank (1985). A large body of research in psychology, building on Festinger 1954, suggests that social comparisons of one's own payoff with the payoff of others considered to be similar are important.

[4] In the standard ultimatum game, a proposer first allocates a fixed amount of money, the "pie," between herself and a responder. The responder can either accept or reject the proposer's offer. If he accepts, the deal stays as proposed; if he rejects, both earn zero. The subgame perfect equilibrium prediction is for the proposer to offer ?, the smallest possible amount, and for the responder to accept.

Among the models on relative income and inequity aversion, the model by Bolton and Ockenfels (2000) most closely reflects the informational conditions in our design.[5] In their model, players care about absolute payoffs, but also how their payoff relates to their referents' average payoff. Responders who care about their standing relative to other responders should never reject a positive offer.[6] By rejecting, they would trade away absolute money to *increase* the disparity between themselves and their referents.

The *Norm Hypothesis* states that responders may care not only about their material payoffs but also about whether a specific offer corresponds to their just deserts. The norm of equity prescribes that equals should be treated equally—so a responder is normatively entitled to earn as much as other people in like circumstances.[7] A responder may reject a positive offer because he dislikes the deviation from the norm more than he values the additional income. We test these two hypotheses against the *Null Hypothesis* that social comparisons do not affect behavior by the responder, implying that behavior by the proposer would not be affected either.

To conduct our tests, we compare the basic ultimatum game with a treatment where we inform responders of the average offer made in a given round, before they decide whether they want to accept or reject the specific offer they received. If information on the average offer induces

[5] Other recent fairness models include Bolton (1991), Fehr and Schmidt (1999) and Kirchsteiger (1994) as well as hybrid models combining fairness and reciprocity such as Charness and Rabin (2002) and Falk and Fischbacher (1999).

[6] Unless responders believe that all other responders reject their offers as well.

[7] Note that our definition of equity differs from the definition used in the reciprocity models based on Rabin (1993) where an equitable payoff is defined as the mean of the highest and the lowest offer a proposer could make. In Rabin's model, a responder's behavior depends on how kind the proposer was to him. Kindness is measured by the difference between the equitable payoff and what the responder expects to receive. While one could imagine

different offers and different rejection rates for given offers than in the basic ultimatum game, we reject the *Null Hypothesis*. If offers and rejection rates are higher with than without social comparisons, we reject the *Relative Standing Hypothesis*.

The *Norm Hypothesis* assumes that social comparisons tend to establish a social norm, so that proposers can no longer take advantage of reference-point ambiguity. Once they believe that responders care about their just deserts, proposers are confronted with a coordination problem. If all other proposers made a small offer, the reference point would be low and they could afford to offer less as well—but successful collusion is unlikely in large groups of proposers who do not know each other and cannot communicate. More likely, proposers will look for a focal point that may serve as the norm. As the modal offer in the standard ultimatum game is typically an equal division, we expect social comparisons to reinforce the role of this focal point.[8] Thus, the *Norm Hypothesis* predicts that when responders are informed of the average offer, we will see higher average offers and higher rejection rates for given offers as well as a higher concentration of offers at an equal division.

While these comparisons provide information on the statistical significance of potential effects, we also wish insight into the *economic significance of social comparisons*. To provide a metric, we compare social comparison effects with another well-established change in informational conditions, asymmetric information on the size of the pie.[9] In our ultimatum game, when information is asymmetric

---

that information on the average offer affects these expectations, this is outside of the model.

[8] While the focal point is an equal split in the standard ultimatum game, this does not need to be the case. If proposers earned the right to allocate the money, for example, they feel entitled to keep more than half of the pie, which responders accept (Hoffman and Spitzer 1985). Depending on the social distance between the proposers and the responders, the focal point may be more or less prominent (Bohnet and Frey 1999).

[9] See, for example, Croson 1996, Forsythe et al. 1991, Güth et al. 1996, Kagel et al. 1996, Mitzkewitz and Nagel 1993.

responders know only the a priori likelihood of two possible sizes of the pie to be divided (while proposers know the size of the pie). They are informed that there is an equal chance that proposers will have a large or a small pie available for allocation between the two of them.

Our paper is organized as follows: Section II summarizes the experimental design and Section III presents the experimental results. We conclude with Section IV.

II. Experimental Design

We use a simple two-by-two design to test our hypotheses and to measure the relative importance of information on the average offer as compared to information on the size of the pie. That is, responders may or may not know comparable offers and may or may not know the size of the pie. Table 1 presents our design. The four boxes are labeled A, B, C, and D, depending on the responder's information. Vertical comparisons measure the significance of social comparisons. Horizontal comparisons tell the importance of knowledge of the size of the pie.

Table 1: Responder's information for four versions of the ultimatum game

		Responder knows how big the pie is: No	Responder knows how big the pie is: Yes
Responder Knows Comparable Offers	No	**A**bsent information	**B**asic Information
	Yes	**C**omparable information	**D**ouble information

We test our hypotheses by looking at offers, rejection rates for given offers, equal split probabilities, and dynamic effects over time. Experimental participants play the same game with changing partners five times to test for the dynamic effects of comparison and pie size information. Since the size of the pie is constant over time, we expect information on its size to leak out over time. The more repetitions we run, the more likely that participants will be aware of the size of the pie in treatments A and C as they update their beliefs based on past offers. We thus expect asymmetric information on the size of the pie to have its strongest effect in the beginning. By contrast, we expect that information on the average offer will become more important over time as the fairness norm gets established. While a focal point may exist in round 1, it will gain importance with the number of rounds of play. We hypothesize that patterns of offers and acceptances will reflect these dynamics.

228 subjects participated in the experiment. Each treatment condition was conducted in two sessions, typically with 14 bargaining pairs per session. Subjects participated in one of the treatment conditions only. They were randomly assigned the role of proposer or responder, and kept this identity for the duration of the experiment. After the roles were determined, proposers and

responders were separated and responders were escorted to a different room. No conversation or other contact was permitted in either room. The players remained anonymous and were only identified by code numbers. We ran five bargaining rounds, with subjects randomly paired with a counterpart, with no rematch. At the end of the experiment, both proposers and responders were paid according to their earnings in one randomly chosen round. They also received a show-up fee of $10. On average, subjects earned approximately $23 in this experiment, which took about 1 hour.[10]

In the first control treatment, A, only the proposers were informed of the size of the pie. Responders knew that there was an equal chance that the pie was $10 or $30. To determine the size of the pie, we flipped a coin at the beginning of our first session. The size of the pie was determined to be $30, and this value remained constant during all subsequent sessions. All subjects were informed of this procedure.[11] In the second control treatment, B, the standard ultimatum game, both, proposers and responders, knew the size of the pie, $30. In neither A nor B did proposers or responders know about any offers other than their own.

The goal of the two social comparison treatments, C and D, was to determine whether informing responders about average offers would affect the offers and the rejection rates for given offers. To test this, we established the following sequence for each round:

1. Proposers make their offers.
2. The average offer is computed and told to responders.

[10] For a sample of the experimental instructions, see the appendix.

[11] The four sessions with asymmetric information on the size of the pie were conducted on two consecutive days to avoid spillover effects from one session to the other. We did not find any significant differences in behavior between two sessions where the same treatment condition was employed.

3. Responders are given their individual offers.
4. Responders decide whether to accept or reject.
5. Proposers are informed whether their offer was accepted or rejected, but not the experience of other proposers.

In treatment C, responders did not know the size of the pie, though they may have been able to infer it from average offers. In treatment D, the size of pie was common knowledge.

III. Experimental Results

Result I:

Information on comparable offers and on the size of the pie independently increase offers and rejection rates for given offers, ceteris paribus. The likelihood that a proposer offers an equal split is higher if responders learn both types of information.

Our results reject the *Null Hypothesis* that social comparisons have no effect. Our two alternative hypotheses have significantly different implications. Our results reject the *Relative Standing Hypothesis* and support the *Norm Hypothesis*. We first discuss how the information given to responders affected the offers they received, and then we discuss the equal split and rejection rates. Table 2 presents average and modal offers aggregated over all rounds.

Table 2: Mean and (*modal)* offers in all rounds

		Responder knows how big the pie is: No	Responder knows how big the pie is: Yes
Responder knows comparable offers	No	**A** 8.12 *(5)*	**B** 10.75 *(10)*
	Yes	**C** 10.24 *(15)*	**D** 12.46 *(15)*

Both social comparison and pie size information make a difference over all rounds. As the data from different rounds of the same session are not independent, we disaggregate the data by rounds to test whether these differences are significant (see Table A.1 in the Appendix for the data). A Wilcoxon test reveals significant differences between the offers in treatment A and treatment C as well as between treatment B and treatment D in later rounds.[12] Average offers are significantly higher in treatment B than in treatment A and in treatment D than in treatment C in earlier rounds.[13]

Over all rounds, proposers are most likely to offer an equal split of $15 when responders know both the size of the pie and the average offer. In treatment D, 43% of all offers are $15 while only 25% of proposers offer an equal division in the basic game B, 24% in treatment C, when responders know the average offer but not the pie size, and 23% in treatment A, when responders do

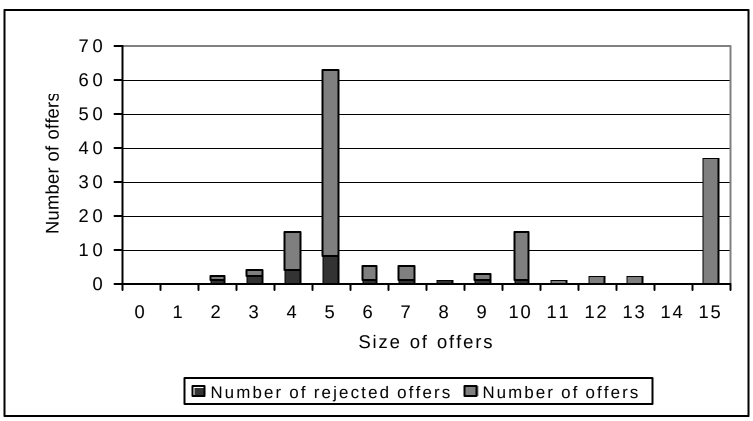
[12] For treatments A and C, we find for rounds 1: Z=-1.21, p=0.23, 2: Z=-2.28, p=0.02, 3: Z=-1.99, p=0.06, 4: Z=-2.46, p=0.01 and 5: Z=-2.55, p=0.01. For treatments B and D, we find for rounds 1: Z=-0.35, p=0.73, 2: Z=-1.23, p=0.22, 3: Z=-2.50, p=0.01, 4: Z=-2.74, p<0.01 and 5: Z=-2.43, p=0.02.

[13] For treatments A and B, we find significant differences for rounds 1 (Z=-3.54, p<0.01), 2 (Z=-3.31, p<0.01) and 3 (Z=-2.24, p=0.03), and marginally significant differences for rounds 4 (Z=-1.95, p=0.051) and 5 (Z=-1.93, p=0.053).

not know the pie size or the average offer.[14] Round-by-round comparisons reveal significant differences between treatment D and each of the other three treatments starting in round 3 (see Table A.2 in the Appendix for the data by round).

Unconditional rejection rates are low in all treatments (see Table A.3 in the Appendix for the unconditional rejection rates by round). A more detailed analysis of rejections implies taking into account how often an offer of a given size is rejected. In all treatments, rejection rates decrease with the size of the offer. Figures 1 to 4 present the number of rejections for an offer of a particular size, over all rounds in each treatment.

Figure 1: Distribution of offers and rejections in treatment A



For treatments C and D, we find significant differences for rounds 1 (Z=-2.39, p=0.02), 2 (Z=-2.32, p=0.02) and 3 (Z=-2.57, p=0.01) but not for rounds 4 (Z=-1.84, p=0.07) and 5 (Z=-1.41, p=0.16).

[14] On the other hand, many more people offer $5, the seemingly fair offer, in treatment A than in any of the other treatment conditions. 41% offer $5 in treatment A, 15% in C, 12% in D, and 10% in B.

Figure 2: Distribution of offers and rejections in treatment B

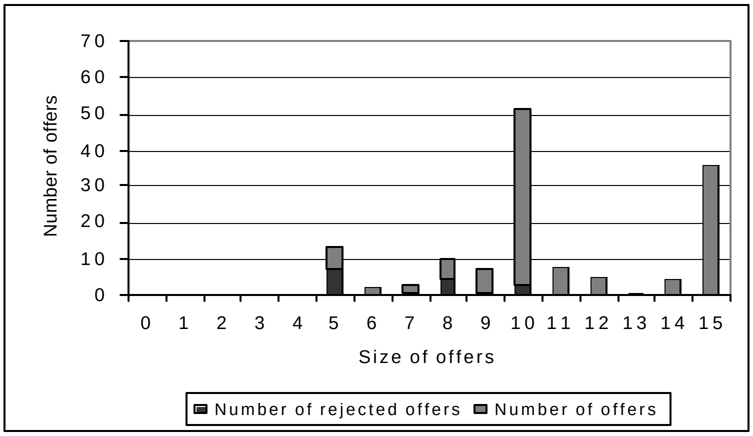


Figure 3: Distribution of offers and rejections in treatment C

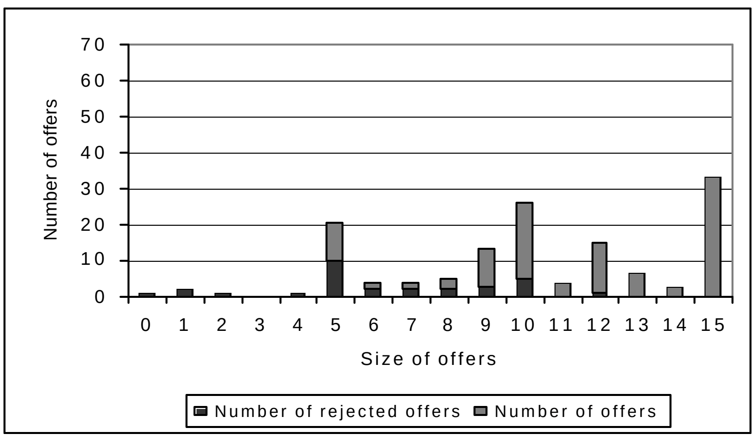
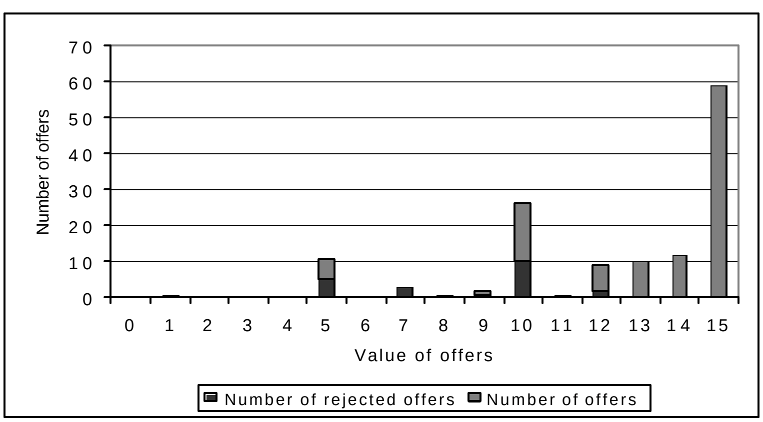


Figure 4: Distribution of offers and rejections in treatment D



To determine the optimal offer in a given treatment, we calculate a proposer's money-maximizing offer by comparing how often an offer of a given size is accepted.[15] In treatment A where no information is provided, it is \$5 ($EV_{prop.}$=\$21.83); in treatment B, where only the pie size is known, it is \$10 ($EV_{prop.}$=\$18.82); in treatment C where comparable offers are known, the offer that maximizes the expected payoff for the proposer is \$10 (EV_{prop}.=\$16.16); and in treatment D where responders know both the size of the pie and the average offer, the money-maximizing offer for the proposer is \$15 ($EV_{prop.}$=\$15).

To estimate the treatment effects more precisely and to determine the relative importance of knowing the average offer compared to knowing the size of the pie, we run a multiple regression where

Pie size known = 1 if responder knows the size of the pie (treatments B and D)

= 0 otherwise (treatments A and C)

Social comparisons known = 1 if responder knows the average offer (treatments C and D)

= 0 otherwise (treatments A and B)

We run OLS regressions for offers and Probit regressions for rejection rates and equal split probabilities, controlling for possible round and session effects. Table A.4 in the Appendix presents the regression results. We find that social comparison and pie information each increases offers by more than \$2 and increases rejection rates for a given offer by more than 10 percent, ceteris paribus.

[15] Comparisons across treatments are somewhat complicated because the number of offers of a given size varies greatly in the different treatments. For example, while a number of proposers offered less than \$5 in treatment A, there were hardly any offers below \$5 when responders knew the size of the pie. We follow Roth et al. (1991) and

Rejection rates fall by almost 4 percentage points with every additional dollar offered. The probability of an equal split is only higher if responders have both, information on the average offer and on the size of the pie (treatment D).

Over 5 rounds, the effects of both informational conditions—average offer and pie size—are statistically and economically significant and of comparable size. Responders are significantly better off if they have either type of information available to them. However, the relative importance of the two pieces of information may vary over time, a subject we turn to in the next section.

Result II:

Asymmetric information on the size of the pie drives behavior at the outset. Social comparisons are the primary force in the end.

The results support our compound hypothesis about *development over time*.

For the first round, Tables A.1 and A.3 in the Appendix suggest no difference in offers and unconditional rejection rates with and without social comparisons but substantially lower offers and unconditional rejection rates in treatments where responders do not know the pie size, whether or not social comparisons are provided. The regression results in Table A.5 support this conjecture. When responders know the size of the pie, in the first round proposers offer almost $4 more, and rejection rates for a given offer are about 13 percent higher than when responders are uninformed on pie size.

On the other hand, last-round offers and rejection rates are significantly higher in treatments where social comparisons are given, whether or not responders are told the size of the pie. In the last

---

include only offers that have been made at least 10 times over all rounds, so we could only include offers of $5, $10 round, offers are almost $3 higher and rejection rates for given offers about 20 percent higher with than without social comparisons. No such differences can be found where social comparisons are held constant and the information on the size of the pie varies.[16]

IV. Discussion and Conclusions

When deciding whether to accept or to reject a specific offer, responders take into account information on offers to other responders—and proposers anticipate this. In our ultimatum game experiments, proposers made higher offers and responders were more likely to reject a given offer when social comparisons were provided than when they were not. This rejects the *Null Hypothesis,* where responders are concerned about inequity relative to their proposers only, and also rejects the *Relative Standing Hypothesis,* where responders dislike payoff differences between themselves and other responders. Our findings support the *Norm Hypothesis,* where responders dislike deviations from the norm of equity.

We find that social comparisons activate the norm of equity: Responders expect to be treated like others in like circumstances. In an ultimatum game, where there is no economic context that suggests what an appropriate or equitable division of a surplus might be, the equal division point will be significantly advantaged as a possible focal point. Thus, we expected and found that social comparisons made offers of an equal split of the pie more likely if responders knew the size of the pie.

---

and $15 across our four treatment conditions.

[16] The latter finding is not surprising as the size of the pie is constant over time and responders will have figured it out by the end.

Previous experimental evidence and recent fairness theories show that concerns other than absolute payoffs motivate responders' rejections, and that these concerns are understood by proposers. Our results suggest that while concerns about others in like circumstances have widely been neglected so far, they should be incorporated into models on bargaining behavior.[17] Players do not care merely about how well they do compared to their bargaining counterpart (whether in terms of fairness or reciprocity); they are also concerned about the norm of equity. They want to be treated like comparable others.

Our preliminary conclusion, based on a limited number of experiments, is that social comparisons facilitate attention to norms. Whether such comparisons favor proposers or responders, employers or workers, sellers or buyers, will depend on a variety of contextual factors that help establish norms. Retailers lure buyers by offering a discount off what they hope to establish as the norm, the manufacturer's list price; salesmen seek to convey the impression that you are getting a special low price; and proposers of marriage try to convey the impression that the responder is regarded more highly and loved more dearly than anyone the proposer has ever met. Both players know that if the proposal is viewed as being favorable relative to the norm, the prospects for acceptance are considerably enhanced.

[17] While the impact of information on the average offer has not been tested in previous experiments, alternative designs include the three-party design by Knez and Camerer (1995), who acknowledge that they cannot differentiate between the relative standing hypothesis (which they call "envy") and reciprocity; Duffy and Feltovich (1999), who inform proposers and responders on the actions and payoffs of one randomly chosen pair in the previous round and are aware that their results could be partly due to reputation-building; and the three-party design with a dummy by Kagel and Wolfe (2001), who write that their data could be reconciled with inequity aversion if the responder considered only the proposer and not the dummy responder a referent.

References

Babcock, Linda; Wang, Xianghong and Loewenstein, George. "Choosing the Wrong Pond: Social Comparisons in Negotiations that Reflect a Self-serving Bias." *Quarterly Journal of Economics,* 1996, *CXI*(1), pp. 1-19.

Bohnet, Iris and Frey, Bruno S. "Social Distance and Other-Regarding Behavior in Dictator Games: Comment." *American Economic Review*, March 1999, *89*(1), pp. 335-339.

Bolton, Gary. "A Comparative Model of Bargaining: Theory and Evidence." *American Economic Review*, 1991, *81*(5), pp. 1096-1136.

Bolton, Gary and Ockenfels, Axel. "ERC: A Theory of Equity, Reciprocity, and Competition." *American Economic Review*, March 2000, *90*(1), pp. 166-193.

Camerer, Colin. *Behavioral game theory: Experiments on strategic interaction*. Princeton: Princeton University Press, in press.

Charness, Gary and Rabin, Matthew. "Understanding Social Preferences with Simple Tests." *Quarterly Journal of Economics,* 2002, *CXVII*(3), pp. 817-870.

Croson, Rachel T.A. "Information in ultimatum games: An experimental study." *Journal of Economic Behavior and Organization*, 1996, *30*, pp. 197-212.

Duffy, John and Feltovich, Nick. "Does observation of others affect learning in strategic environments? An experimental study." *International Journal of Game Theory*, 1999, *28*, pp.131-152.

Fehr, Ernst and Schmidt, Klaus. "Theories of Fairness and Reciprocity -- Evidence and Economic Applications." Institute for Empirical Research in Economics Working paper #75, 2001, University of Zürich.

Fehr, Ernst and Schmidt, Klaus. "A Theory of Fairness, Competition, and Cooperation." *Quarterly Journal of Economics*, August 1999, *114*(3), pp. 817-868.

Festinger, Leon. "A theory of social comparison processes." *Human Relations,* 1954, *7*, pp. 117-140.

Forsythe, Robert, Kennan, John and Sopher, Barry. "An experimental analysis of bargaining and strikes with one sided private information." *American Economic Review*, 1991, *81,* pp. 253-278.

Frank, Robert. *Choosing the Right Pond. Human Behavior and the Quest for Status*. 1985, Oxford: Oxford University Press.

Güth, Werner; Huck, Steffen and Ockenfels, Peter. "Two-level ultimatum bargaining with incomplete information*." Economic Journal*, 1996, *106*, pp. 593-604.

Hoffman, Elizabeth and Spitzer, Matthew L. "Entitlements, rights and fairness: An experimental examination of subjects' concepts of distributive justice." *Journal of Legal Studies*, 1985, *14*, pp. 259-297.

Kagel, John H. and Wolfe, Katherine W. (2001). "Tests of Fairness Models on Equity Considerations in a Three-Person Ultimatum Game." *Experimental Economics,* 2001, *4,* pp. 203-219.

Kagel, John H.; Kim, Chung and Moser, Donald. “Fairness in Ultimatum Games with Asymmetric Information and Asymmetric Payoffs.” *Games and Economic Behavior*, March 1996, 13(1), pp. 100-110.

Kirchsteiger, Georg. “The role of envy in ultimatum games.” *Journal of Economic Behavior and Organization*, 1994, *25*, pp. 373-389.

Knez, Marc J. and Camerer, Colin F. “Outside Options and Social Comparison in Three-Player Ultimatum Game Experiments.” *Games and Economic Behavior*, 1995, *10*, pp. 65-94.

Mitzkewitz, Michael and Nagel, Rosemarie. “Envy, greed and anticipation in ultimatum games with incomplete information: An experimental study. *International Journal of Game Theory*, 1993, *22*, pp. 171-198.

Rabin, Matthew. “Incorporating Fairness into Game Theory and Economics.” *American Economic Review*, December 1993, *83*(5), pp. 1281-1302.

Roth, Alvin E.; Prasnikar, Vesna, Okuno-Fujiwara, Masahiro and Zamir, Shmuel. Bargaining and market behavior in Jerusalem, Ljubliana, Pittsburgh, and Tokyo: An experimental study. *American Economic Review*, 1991, 81, pp. 1068-1095.

Roth, Alvin E. “Bargaining experiments.” In: Kagel, John H. and Roth, Alvin E. (eds.). *Handbook of Experimental Economics*, 1995, Princeton: Princeton University Press, pp. 253-348.

Appendix

Sample instructions (for the Double-UG)

Welcome to this research project!

You are participating in a study that gives you a chance to earn cash. The actual amount you earn will depend upon your choices and the choices of the other persons in the study. The study will consist of five rounds. At the end, one round will be randomly selected and the amount you earned in that round will be added to your show-up fee of $10 as the amount you take home.

In this study, there are two roles, "A" and "B", and you will be randomly assigned to one of these roles at the beginning of the study. You will remain in the same role, A or B, throughout.

In addition to these instructions, everyone will receive an envelope containing

- a Code Number Form
- an Earnings Form

Those of you who will be randomly assigned the role of A in addition will receive:

- 5 "Checks", each worth $X
- 5 Decision Forms

What the study is about:

The study addresses how people decide. Each decision pair consists of one A and one B. The As and Bs are randomly matched each round. In a round, each A will receive a check worth $X and will have to decide how much (if any) of the $X s/he wants to give to his/her B. Bs receive no money except from their counterparts. Bs can either accept or reject the amount of money offered (but cannot make a counteroffer). If B accepts, the deal is the one proposed by A. If B rejects, both, A and B, earn $0.

Note:

- The study is repeated five times. In each of the 5 rounds, each A will receive a new check worth $X and will be randomly matched with a new B.
- The value of X is the same in all rounds. X is either $30 or $10. There is a 50%-chance that X will be $30 and a 50%-chance that it will be $10. Before the start of the study, we will flip a coin to determine X.

- All participants are informed on the value of X.
- Participants in the role of B are informed on the average value of the offers made in each round. That is, each B learns about the average offer before s/he decides whether to accept or reject the specific offer s/he received. As are not informed on the value of the average offer.
- Participants are only identified by "code numbers". In order to guarantee privacy and anonymity, do not show anyone your code number!

START

1. Everyone picks an envelope. You will learn whether you are in the role of A or of B.
2. A random device will determine the value of X. All participants will be informed on the value of X.
3. All Bs will be escorted to a different room (called "room B").
4. All As stay in this room (called "room A").

Round 1:

5. Each A makes his/her first decision and indicates on the Decision Form #1 how much money (if any) s/he wants to offer his/her counterpart B. Decision Forms #1 are then put back into the envelope and into the box that is passed around.
6. The envelopes are taken to room B where the average offer will be calculated. All Bs are informed on the value of the average offer. Then, envelopes are randomly given to the Bs in room B.
7. Bs decide whether they want to accept or reject their counterpart's offer. Bs indicate on the Decision Form whether they accept or reject, and then put the Decision Form back into the envelope and into the box. Bs then note on their Earnings Form how much money they earned in this round.
8. The envelopes are given back to the respective As in room A. The As are asked to take the envelope marked with their code number out of the box and note their earnings in round #1 on their Earnings Form. End of round #1.

The following rounds:

X remains constant in all rounds. The exact same procedure as in Round #1 will be repeated 4 additional times. You will be paired with a new person in each round. The study concludes after round #5. We then randomly decide which round is relevant for your payment. You are informed on this. For your own records, please write down how much you earned in this study. **Please put the decision forms and the earning forms back into the envelope and then into the box. Keep your code number form!**

END OF THIS STUDY. You are invited to collect your earnings right after the experiment.

If you have any questions, please address them to Iris_Bohnet@Harvard.edu

We thank you for participating in the study.

Iris Bohnet and Richard Zeckhauser

Table A.1: Size of offers

	Absent (N=31)			**B**asic (N=28)		
	Mean	Median	Mode	Mean	Median	Mode
Round 1	7.71	5	5	11.64	10.5	15
Round 2	7.84	5	5	11.39	10.5	15
Round 3	8.26	5	5	10.46	10	10
Round 4	8.35	5	5	10.14	10	10
Round 5	8.42	6	5	10.14	10	10

	Comparable (N=28)			**D**ouble (N=27)		
	Mean	Median	Mode	Mean	Median	Mode
Round 1	8.82	9	10	11.74	14	15
Round 2	9.89	9.5	15	12.48	14	15
Round 3	10.04	10	15	12.78	15	15
Round 4	11.07	10.5	15	12.59	14	15
Round 5	11.39	12	15	12.52	13	15

Table A.2: Percent of proposers choosing an equal split of the large pie ($30)

	Absent (N=31)	**B**asic (N=28)
Round 1	22.58%	32.14%
Round 2	22.58%	32.14%
Round 3	25.81%	21.43%
Round 4	22.58%	21.43%
Round 5	22.58%	17.86%

	Comparable (N=28)	**D**ouble (N=27)
Round 1	25.00%	40.74%
Round 2	25.00%	37.04%
Round 3	21.43%	48.12%
Round 4	25.00%	44.44%
Round 5	21.43%	44.44%

Table A.3: Unconditional rejection rates

	Absent (N=31)	**B**asic (N=28)
Round 1	19.35%	15.66%
Round 2	16.12%	7.14%
Round 3	6.45%	10.71%
Round 4	9.68%	10.71%
Round 5	12.90%	7.14%

	Comparable (N=31)	**D**ouble (N=28)
Round 1	28.57%	14.81%
Round 2	21.43%	18.52%
Round 3	21.43%	14.81%
Round 4	21.43%	14.81%
Round 5	17.86%	18.52%

Table A.4: The influence of social comparisons and pie-size information on offers, equal split and rejection rates

	Offer	Prob. of equal split	Rejection rate
Social comparisons known	2.100 **	-0.002	0.173 **
	(0.440)	(0.055)	(0.038)
Pie size known	2.644 **	0.019	0.110 **
	(0.438)	(0.054)	(0.038)
Social comparisons x pie size known	-0.439	0.173 *	-0.053
	(0.631)	(0.084)	(0.034)
Offer			-0.036 **
			(0.004)
Round 2	0.412	-0.008	-0.006
	(0.498)	(0.059)	(0.034)
Round 3	0.404	-0.010	-0.012
	(0.498)	(0.059)	(0.033)
Round 4	0.561	-0.018	-0.008
	(0.498)	(0.059)	(0.034)
Round 5	0.640	-0.018	-0.003
	(0.498)	(0.059)	(0.035)
Constant	7.604 **		
	(0.466)		
Pseudo R-squared		0.025	0.261
R-squared	0.151		
Observations	570	570	570

* significant at 5% level, ** significant at 1% level, standard errors in parentheses
OLS regressions for offer and probit regressions for rejection rate and prob. of equal split, clustered for session

Table A.5: The influence of social comparisons and pie-size information on first-round outcomes

	Offer	Probability of equal split	Rejection rate
Social comparisons known	1.085	0.028	0.099
	(0.980)	(0.122)	(0.065)
Pie size known	3.937 **	0.097	0.132 *
	(0.979)	(0.119)	(0.073)
Social comparisons x pie size known	-0.991	0.051	-0.078
	(1.409)	(0.176)	(0.055)
Offer			-0.036 **
			(0.004)
Round 2	0.129	0.000	-0.010
	(0.954)	(0.119)	(0.053)
Round 3	0.548	0.036	-0.081
	(0.954)	(0.122)	(0.036)
Round 4	0.645	0.000	-0.045
	(0.954)	(0.119)	(0.044)
Round 5	0.710	0.036	-0.025
	(0.954)	(0.122)	(0.050)
Round 2 x social comparisons	0.942	-0.001	0.016
	(1.384)	(0.171)	(0.094)
Round 3 x social comparisons	0.666	-0.071	0.201
	(1.384)	(0.153)	(0.204)
Round 4 x social comparisons	1.605	0.000	0.174
	(1.384)	(0.171)	(0.181)
Round 5 x social comparisons	1.862	-0.071	0.124
	(1.384)	(0.152)	(0.158)
Round 2 x pie size	-0.379	0.000	-0.055
	(1.384)	(0.168)	(0.048)
Round 3 x pie size	-1.727	-0.129	0.149
	(1.384)	(0.131)	(0.187)
Round 4 x pie size	-2.145	-0.102	-0.023
	(1.384)	(0.142)	(0.074)
Round 5 x pie size	-2.210	-0.130	-0.066
	(1.384)	(0.131)	(0.039)
Round 2 x social comparisons x pie size	0.048	-0.032	0.222
	(1.992)	(0.226)	(0.322)
Round 3 x social comparisons x pie size	1.550	0.279	-0.044
	(1.992)	(0.280)	(0.072)
Round 4 x social comparisons x pie size	0.747	0.156	0.016
	(1.992)	(0.278)	(0.151)
Round 5 x social comparisons x pie size	0.416	0.243	0.183
	(1.992)	(0.282)	(0.300)
Constant	7.601 **		
	(0.694)		
Observations	570	570	570
Pseudo R-squared		0.029	0.283
R-squared	0.168		

* significant at 5% level, ** significant at 1% level, standard errors in parentheses

OLS regressions for offer and probit regressions for rejection rate and prob. of equal split, clustered for session

Table A.6: The influence of social comparisons and pie-size information on last-round outcomes

	Offer	Probability of equal split	Rejection rate
Social comparisons known	2.947 **	-0.047	0.202 **
	(0.980)	(0.121)	(0.081)
Pie size known	1.727	-0.049	0.025
	(0.979)	(0.121)	(0.077)
Social comparisons x pie size known	-0.575	0.289	0.014
	(1.409)	(0.191)	(0.107)
Offer			-0.036 **
			(0.004)
Round 1	-0.710	-0.035	0.029
	(0.954)	(0.113)	(0.070)
Round 2	-0.581	-0.034	0.018
	(0.954)	(0.113)	(0.066)
Round 3	-0.161	0.000	-0.066
	(0.954)	(0.116)	(0.043)
Round 4	-0.065	-0.034	-0.024
	(0.954)	(0.113)	(0.055)
Round 1 x social comparisons	-1.862	0.079	-0.061
	(1.384)	(0.187)	(0.038)
Round 2 x social comparisons	-0.919	0.078	-0.054
	(1.384)	(0.187)	(0.045)
Round 3 x social comparisons	-1.196	0.000	0.044
	(1.384)	(0.172)	(0.129)
Round 4 x social comparisons	-0.257	0.078	0.028
	(1.384)	(0.187)	(0.110)
Round 1 x pie size	2.210	0.158	0.148
	(1.384)	(0.194)	(0.191)
Round 2 x pie size	1.831	0.158	0.026
	(1.384)	(0.194)	(0.129)
Round 3 x pie size	0.483	0.001	0.387
	(1.384)	(0.172)	(0.273)
Round 4 x pie size	0.065	0.036	0.100
	(1.384)	(0.181)	(0.174)
Round 1 x social comparisons x pie size	-0.416	-0.176	-0.066
	(1.992)	(0.145)	(0.039)
Round 2 x social comparisons x pie size	-0.368	-0.194	0.019
	(1.992)	(0.131)	(0.165)
Round 3 x social comparisons x pie size	1.134	0.032	-0.078
	(1.992)	(0.254)	(0.023)
Round 4 x social comparisons x pie size	0.331	-0.070	-0.062
	(1.992)	(0.212)	(0.047)
Constant	8.311 **		
	0.694		
Observations	570	570	570
Pseudo R-squared		0.029	0.283
R-squared	0.170		

*significant at 5% level, ** significant at 1% level, standard errors in parentheses

OLS regressions for offer and probit regressions for rejection rate and prob. of equal split, clustered for session