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# Social Comparisons in Ultimatum Bargaining*

*Iris Bohnet*

Harvard University, Cambridge, MA 02138, USA
iris_bohnet@harvard.edu

*Richard Zeckhauser*

Harvard University, Cambridge, MA 02138, USA
richard_zeckhauser@harvard.edu

## Abstract

Experiments are used to examine the effects of social comparisons in ultimatum bargaining. We inform responders about the average offer before they decide whether to accept or reject their specific offer. This significantly increases offers and offer-specific rejection probabilities. For comparison, we consider another change in informational conditions: telling responders the total pie is $30—*ex ante* it was either $15 or $30—affects offers and rejection probabilities roughly as much. Our results are consistent with people's dislike for deviations from the norm of equity but inconsistent with fairness theories, where people dislike income disparity between themselves and their referents.

*Keywords*: Experimental bargaining; social comparisons; asymmetric information; ultimatum game

*JEL classification*: *C*91

## I. Introduction

Ample findings from bargaining games show that people typically depart from the standard notions of money-maximizing preferences.[1] In their attempt 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 may make comparisons other than to their bargaining counterpart. In salary negotiations, for example, prospective employees

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[1] For recent surveys of bargaining games, see Camerer (2003) and Roth (1995).

[2] For a recent survey of theories of fairness and reciprocity, see Fehr and Schmidt (2002).

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] The reference group of others in like circumstances has received relatively little attention in the literally thousands of experimental studies on bargaining. The term "social comparisons" refers to information on such "others".

We use the ultimatum game,[4] probably the most studied bargaining game, to investigate the role of social comparisons between responders. A large body of research in psychology, building on Festinger (1954), suggests that social comparisons affect behavior, since they provide information on what is the "right behavior" in a certain context. When social comparisons are given, we expect offers to gravitate towards the "right behavior", and rejection rates to rise where there are deviations from such behavior.

There are a few earlier studies on social comparisons in bargaining. In contrast to them, we inform our responders about the *average offer*, what might be thought of as the social norm, rather than about one other offer; see Knez and Camerer (1995), Cason and Mui (1998), Duffy and Feltovich (1999) and Kagel and Wolfe (2001).[5] Knowledge of one other offer has been found to have only modest effects. According to Cason and Mui (1998, p. 262), a randomly chosen "other" may not "cause an individual to change her belief regarding what constitutes the appropriate or correct behavior", whereas information on average behavior or the social norm may.

Social norms raise questions of coordination. In contrast to earlier studies, we pose that social comparisons in bargaining are linked to problems of coordination. If social comparisons establish a social norm and responders care about this norm, proposers can no longer take advantage of reference-point ambiguity. If all other proposers made a small offer, the reference point would be low and a given proposer could afford to offer less as well—but successful collusion is unlikely within large groups of proposers who do not know each other and cannot communicate. More likely, such proposers

[3] See e.g. Babcock, Wang and Loewenstein (1996) and Frank (1985) for examples in economics addressing the issue.

[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 $\varepsilon$, the smallest possible amount, and for the responder to accept.

[5] Harrison and McCabe's (1996) ultimatum games, which were not designed to study social comparisons, are of interest here as well. In these experiments, subjects were asked to indicate their strategies regarding offers and acceptances "behind the veil of ignorance", before knowing whether they would be in the role of proposer or responder. Information on the complete distribution of strategies behind the veil of ignorance led to a decrease in offers over time.

will tend toward a focal point that comes to 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.[6]

This paper has two objectives. First, it aims to explore the relevance of social comparisons in general and over time, and to distinguish the channels through which social influence is exerted. Second, it seeks to assess the economic significance of these effects. To provide a metric, we compare social comparison effects with another well-established factor in informational conditions, asymmetric information on the size of the pie.[7] We chose to compare effects of social comparison with impacts of pie-size knowledge because we believe that both are important characteristics in real-world bargaining.[8] We pose no hypothesis about the relative magnitudes of the two effects.

*(i) Channels of influence*: People might rely on 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. Existing fairness models[9] assume that the proposers are the responders' reference group, and that responders only care about their relative share compared to their proposers. However, models of inequity aversion could easily account for comparisons with other responders if the reference group were redefined.[10] Among the models on relative

[6] While the focal point is an equal split in the standard ultimatum game in most Western societies, this need not be the case. If proposers earned the right to allocate the money, for example, they would feel entitled to keep more than half of the pie, which responders accept; see Hoffman and Spitzer (1985). Depending on the social distance between the proposers and the responders, the focal point may be more or less prominent; see Bohnet and Frey (1999). Substantially different norms have been found to guide behavior in ultimatum games in some non-Western societies; see Henrich *et al*. (2002).

[7] See, for example, Croson (1996), Croson, Boles and Murnighan (2003), Forsythe, Kennan and Sopher (1991), Güth, Huck and Ockenfels (1996), Kagel, Kim and Moser (1996), Mitzkewitz and Nagel (1993), Straub and Murnighan (1995), Rapoport, Sundali and Seale (1996) and Rapoport and Sundali (1996).

[8] Many experimenters choose to study the effect of one institutional change at a time, which makes between-study comparisons challenging, since many variables will change between experiments (subject pool, incentives, information conditions, etc.). Our approach allows us to directly compare the magnitude of two important institutional effects, for specific parameter values. It follows in an experimental tradition that focuses on institutional comparisons; see Ostrom, Gardner and Walker (1994).

[9] Recent inequity-aversion models include Bolton (1991), Fehr and Schmidt (1999) and Bolton and Ockenfels (2000). Rabin (1993), Dufwenberg and Kirchsteiger (1998), Falk and Fischbacher (1998) and Charness and Rabin (2002) present fairness models on how inferences about the proposer's intentions affect decisions.

[10] See Bolton and Ockenfels (forthcoming) for a related argument. They show how inequity-aversion models could be modified to take reference points contingent on the available offers in the game into account.

income and inequity aversion, the model by Bolton and Ockenfels (2000) most closely reflects the informational conditions in our design. In their model, players care about absolute payoffs, but also how their payoff relates to the *average payoff* of their referents. Responders who care about their standing relative to other responders should never reject a positive offer.[11] By rejecting, they would simultaneously give up absolute money and *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 their specific offer provides them 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. A responder may reject a positive offer because he dislikes its 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 proposers would not be affected either. Both standard economic models assuming selfishness and standard fairness models predict that behavior is invariant to information on social comparison.

*(ii) Relative importance*: We measure social comparison effects against effects due to knowledge of the size of the pie. In our ultimatum game, when information is asymmetric, 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 specifies our hypotheses. Section III reports the experimental results and Section IV concludes.

## 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. Table 1 outlines 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 refer to the importance of knowledge of the size of the pie.

[11] This holds unless such responders believe that all other responders reject their offers as well.

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

| | | Responder knows how big the pie is | |
|---|---|---|---|
| | | No | Yes |
| Responder knows comparable offers | No | **A**bsent information | **B**asic information |
| | Yes | **C**omparable information | **D**ouble information |

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 (hoping that the pie would turn out to be large but willing to repeat the session in case we did not get lucky). The size of the pie turned out to be \$30, and this value remained constant during all subsequent sessions. All subjects were informed of this procedure.[12] 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.[13] In treatment C, responders did not know the size of the pie, though they may have been able to infer it over time from information on average offers. In treatment D, the size of pie was common knowledge. Experimental participants played the same game with changing partners five times (stranger treatment) to test for the dynamic effects of comparison and pie-size information.

We test our hypotheses by looking at offers, rejection rates (both unconditional and for given offers), equal-split probabilities and their evolution across rounds. We test the *relative standing* and the *norm hypotheses* against the *null hypothesis* that social comparisons do not

[12] 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 run on different days where the same treatment condition was applied.

[13] 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. (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. The experimental instructions are available on request.

affect behavior. If information on the average offer induces different offers and different rejection rates for given offers with than without social comparisons, the *null hypothesis* is rejected. If offers and rejection rates are higher with than without social comparisons, the *relative standing hypothesis* is rejected.

The *norm hypothesis* assumes that social comparisons direct people's attention to a focal point. If the focal point offer is higher than most offers, social comparisons should produce higher offers and greater rejection rates for offers below the focal point. Where the focal point is an equal split, as it is in our experiments, social comparisons should produce more offers of equal splits. We chose a design that induces high-payoff focal points—namely the large pie under all conditions—so as to generate unequivocal predictions.[14] We expect that over time the fairness norm will become better established. Thus, while a focal point may exist faintly in round 1, it will gain importance as more rounds are played. This implies that offers will increase over time in the social comparison treatments C and D.

Asymmetric information poses a coordination problem for proposers when the size of the pie remains constant over time. If proposers could coordinate on small offers, they could successfully pretend that the pie is small. However, as with social comparisons, coordination attempts are likely to fail in our large-group, anonymous context without communication. The more repetitions are run, the more likely that participants will become aware of the size of the pie in treatment A as they update their beliefs based on past offers. In treatment C, due to the help of social comparisons, information on the size of the pie will leak out more swiftly. We thus expect a faster and larger increase in offers in treatment C than in treatment A.

A total of 228 subjects participated in the experiment. Each treatment condition was conducted in two sessions, typically with 14 bargaining pairs per session. Subjects participated in only one of the treatment conditions. They were randomly assigned the role of proposer or responder, and kept that 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 (double-blind). 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.

[14] Had the design been different with the focal point below most offers, we should expect social comparisons to lower offers and offer-specific rejection rates.

## III. Experimental Results

*Result I.* Information on comparable offers and on the size of the pie independently increases offers and rejection rates for given offers, *ceteris paribus*. The likelihood that an equal split is chosen 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 different implications. Our results reject the *relative standing hypothesis* and support the *norm hypothesis*. We discuss how the information given to responders affected the offers they received and the rates of equal splits and rejections. Table 2 lists average and modal offers as well as equal split rates aggregated over all rounds.

Both social comparison and pie-size information substantially increase offers 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, i.e., in treatment D.[15] Rejection rates are low in all treatments and decrease with the size of the offers. Figures 1 to 4 show the number of rejections for an offer of a particular size, over all rounds in each treatment.

To determine the optimal offer (based on EMV) in a given treatment, we calculate a proposer's money-maximizing offer by comparing how often an offer of a given size is accepted.[16] In treatment A, where no information is

Table 2. ***Mean offers,*** modal offers *and equal split rates in all rounds*

| | | Responder knows how big the pie is | | | |
|---|---|---|---|---|---|
| | | No | | Yes | |
| Responder knows comparable offers | No | ***A*** | **8.12**<br>*5*<br>23% | ***B*** | **10.75**<br>*10*<br>25% |
| | Yes | ***C*** | **10.24**<br>*15*<br>24% | ***D*** | **12.46**<br>*15*<br>43% |

[15] 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.
[16] 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, Prasniker, Okuno-Fujiwara and Zamir (1991) and include only offers that were made at least 10 times over all rounds, so we could only include offers of $5, $10 and $15 across our four treatment conditions.

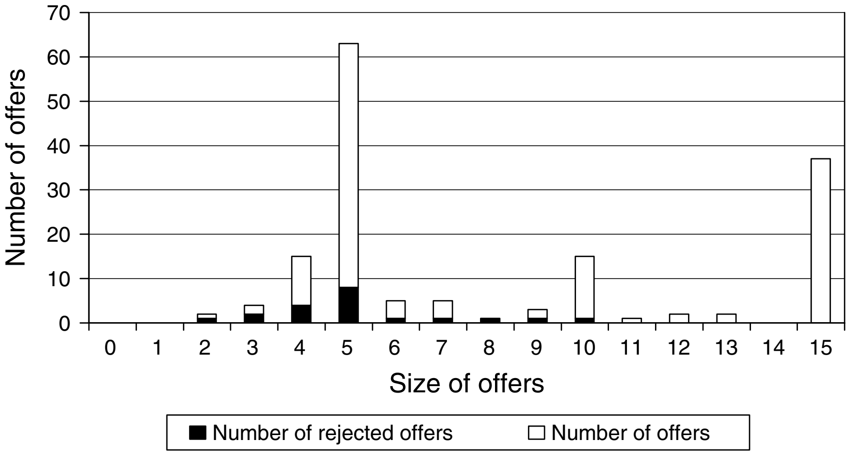


*Fig. 1.* Distribution of offers and rejections in treatment A ($N = 155$)

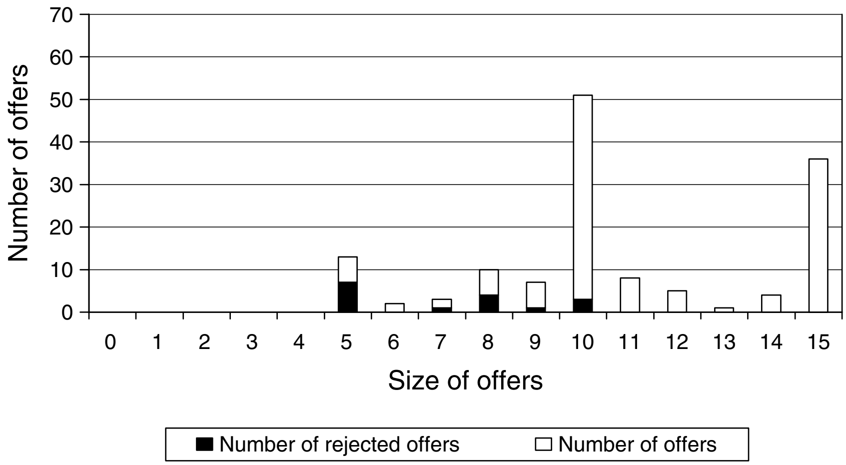


*Fig. 2.* Distribution of offers and rejections in treatment B ($N = 140$)

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 ran a multiple regression where

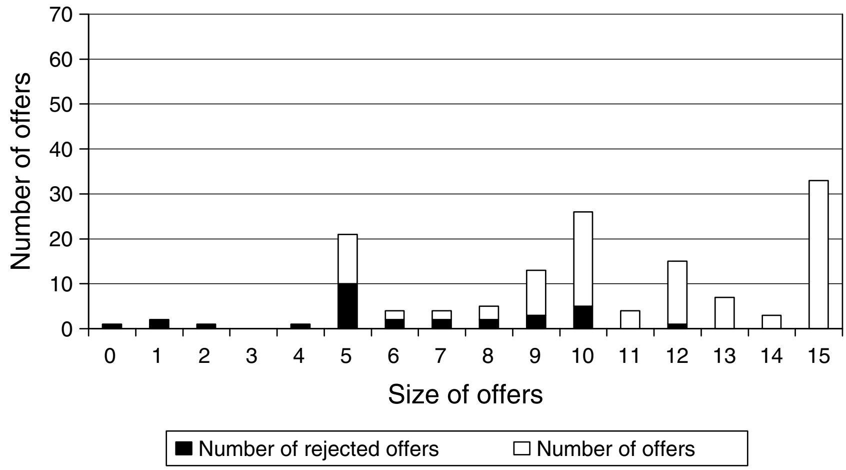


*Fig. 3.* Distribution of offers and rejections in treatment C ($N = 140$)

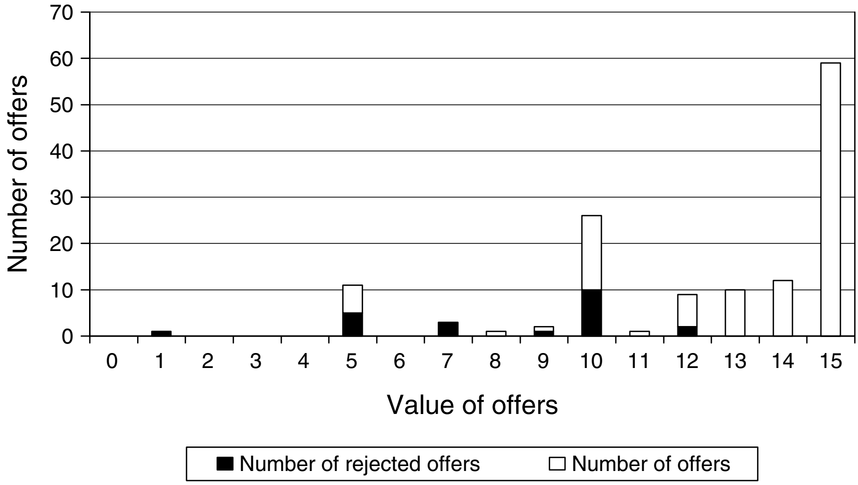


*Fig. 4.* Distribution of offers and rejections in treatment D ($N = 135$)

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)

Pie size and social comparisons known = 1 if responders know average offer and pie size (D)
= 0 otherwise (A, B, C).

We ran OLS regressions for offers and Probit regressions for rejection rates and equal split probabilities, controlling for possible round and subject effects. Table A1 in the Appendix reports the regression results. It shows that social comparison and pie information each increases offers by more than $2 and each increases rejection rates for a given offer by more than 10 percent, *ceteris paribus*. Rejection rates fall by almost 4 percentage points with every additional dollar offered. Over five 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. The probability of an equal split is not higher unless responders have both: information on the average offer and on the size of the pie (treatment D). In the other social comparison treatment, C, there are competing focal points, an equal split of the large and an equal split of the small pie. Moreover, offers generally increase over time, with significantly larger offers in round 5 than in round 1. However, dynamic effects may vary in the different treatments.

*Result II*. The difference between offers in treatments with and without social comparisons increases over time. Equal splits become more likely over time in treatment D. Unconditional rejection rates do not vary over time.

Tables A2–A4 in the Appendix report offers, equal split frequencies and (unconditional) rejection rates for each round. A Wilcoxon test reveals significantly larger offers in later rounds for treatments with (C and D) than without social comparisons (A and B).[17] Equal splits are more likely in treatment D than in treatment B, starting in round 3.[18] There are no significant differences in unconditional rejection rates in any of the rounds.

We expected information about pie size to leak over time, more quickly with social comparisons. Offers in treatment A remain low in all rounds, with only a slight, non-significant increase in offers over time. Average offers are significantly higher in treatment B than in A in all rounds, while the differences between treatments D and C are only significant in early rounds.[19]

[17] 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: $Z = -2.48$, $p = 0.01$; 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: $Z = -2.43$, $p = 0.02$.

[18] For treatments B and D, we find for rounds 3: $chi^2 = 4.70$, $p = 0.03$; 4: $chi^2 = 3.62$, $p = 0.05$; 5: $chi^2 = 4.89$, $p = 0.03$.

[19] 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 ($Z = -1.99$, $p = 0.05$) and 5 ($Z = -1.96$, $p = 0.05$). 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 ($Z = -1.75$, $p = 0.09$) and 5 ($Z = -1.41$, $p = 0.16$).

In fact, by the fifth round, responders in treatment C have almost caught up with responders in D. More strikingly, they earn more than responders in treatment B, who know the pie size. While this difference between treatments C and B does not reach significance ($Z = -1.625$, $p = 0.10$), it is in line with our previous finding when comparing treatments D and B: knowledge of comparable offers provides more than pie-size information.

The increasing differences in offers with and without social comparisons over time are driven by identifiable forces: an increase in offers from round 1 to round 5 in treatment C, where responders learn about the size of the pie as they learn about comparable offers, and a decrease in offers in treatment B, the basic ultimatum game. Offers also increase over time in the other social comparison treatment D, but the differences between the first and the last rounds are only significant in B and in C.[20] The decrease in offers over time in the basic ultimatum game is in line with standard results. Most studies find a slight tendency of offers to decrease over time; see Camerer (2003). We expected an increase in offers in both social comparison treatments, C and D, but found the increase to be significant in C only. Based on this experiment, we conclude that social comparisons lead to an especially large increase over time if they allow responders to learn what comparable others earn as well as the size of the pie.

## 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. Proposers anticipate this when making their offers. 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* that social comparisons do not affect behavior. It also rejects the *relative standing hypothesis*, under which 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 our ultimatum game, there is no economic argument regarding what division of the surplus is appropriate. Nor do proposers have a means to coordinate. Thus the focal-point properties of the equal-division point will come into play. In particular, when social comparisons are available, this point will be significantly

[20] The results of a Wilcoxon signed ranks test, comparing rounds 1 and 5, are for treatment B: $Z = -2.139$, $p = 0.032$, and for treatment C: $Z = -3.029$, $p < 0.01$.

advantaged in becoming established as the norm. 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.

Knowing comparable offers increases offers by a similar magnitude as does knowing the pie size. Social comparisons gain in importance over time, especially when responders do not know the size of the pie. They enable responders to hold proposers accountable to the norm of equity, with respect to both other responders and the share their proposers keep. Our results do not suggest that responders do not care about their bargaining counterpart, but they do suggest that in addition to these considerations, responders are also concerned that they be treated equivalently to others in like circumstances, and that proposers are aware of this.

Our preliminary conclusion, based on a limited number of experiments, is that social comparisons facilitate attention to the social norm. In our design, the social norm, an equal split, improved the responders' lot. More generally, social comparisons decrease the distance between an offer and the norm. Whether social 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 their buyer of the moment is 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 favorable relative to the norm, the prospects for acceptance are considerably enhanced.

## Appendix

Table A1. *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 | 2.127** | −0.003 | 0.172*** |
| | (0.957) | (0.113) | (0.042) |
| Pie size known | 2.641*** | 0.020 | 0.110*** |
| | (0.890) | (0.108) | (0.0391) |
| Social comparisons × pie size known | −0.462 | 0.174* | −0.053 |
| | (1.200) | (0.103) | (0.036) |
| Offer | | | −0.037*** |
| | | | (0.004) |
| Round 2 | 0.412 | −0.008 | −0.006 |
| | (0.282) | (0.036) | (0.034) |

Table A1. *(Continued)*

| | Offer | Prob. of equal split | Rejection rate |
|---|---|---|---|
| Round 3 | 0.404 | −0.010 | −0.012 |
| | (0.349) | (0.042) | (0.030) |
| Round 4 | 0.561 | −0.018 | −0.007 |
| | (0.352) | (0.035) | (0.034) |
| Round 5 | 0.640* | −0.018 | −0.002 |
| | (0.373) | (0.036) | (0.034) |
| Constant | 7.713*** | | |
| | (0.768) | | |
| Pseudo $R$-squared | | 0.025 | 0.261 |
| $R$-squared | 0.151 | | |
| Observations | 570 | 570 | 570 |

*Notes*: *significant at 10% level, **significant at 5% level, ***significant at 1% level; standard errors in parentheses; OLS regressions for offer and probit regressions for rejection rate and probability of equal split, clustered for subjects.

Table A2. *Size of offers*

| | Absent ($N=31$) | | | Basic ($N=28$) | | |
|---|---|---|---|---|---|---|
| | 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 ($N=28$) | | | Double ($N=27$) | | |
|---|---|---|---|---|---|---|
| | 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 A3. *Percent of proposers choosing an equal split of the large pie ($30)*

| | **A**bsent ($N = 31$) | **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% |

| | **C**omparable ($N = 28$) | **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 A4. *Unconditional rejection rates*

| | **A**bsent ($N = 31$) | **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% |

| | **C**omparable ($N = 31$) | **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% |

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