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Lies, Probabilities, and Threats in Ultimatum Games

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Abstract: Many papers have used the ultimatum game as a means to test bargaining behavior. A subset of experiments has examined the use of cheap talk and asymmetric in bargaining scenarios. Yet, this earlier work leaves out many of the complicated factors that occur in bargaining in the real world. No research has looked at the interaction of probabilities and threats in ultimatum games. The experiment described here makes two novel modifications to the ultimatum game: First: participant's have the ability to send threats. Second: the game uses probabilities to determine whether or not the endowment size becomes revealed. The data show that both threats and probabilities affect the outcomes of the game in interesting ways. Threats affect the amount offered by proposers but do not have a large influence on lying whereas a higher probability of being revealed decreases lying. The implications from this research helps shed light on why we have many institutions dedicated to eliminating uncertainty from a multitude of transactions.

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Introduction

Every day, people make decisions with varying degrees of information. Investors have to chose which stocks they think will do well, someone looking for a used car has to assess an estimate of its worth, a company might need to figure out the long term growth prospects of a potential acquisition. All of these decisions revolve around aspects of bargaining. Economists have trouble studying the nuances of these activities, as they cannot isolate the subtle details involved in these transactions. The ultimatum game gives researchers the tools to study this behavior in a laboratory setting. This experiment is a classic experiment in economics where one person, the proposer, is allocated a certain amount of money, the endowment. The proposer can send any amount of the endowment to the responder. The responder can either accept the proposer's offer or reject it. If the responder rejects the offer both participants get nothing; if the responder accepts, s/he gets the offer while the proposer received the offer subtracted from the endowment. Many experiments have studied how different variables affect offers in ultimatum games, but few have focused on how threats, lies and cheap talk affect the game.

Cheap talk and lying involve communication between players that will not directly alter the payoffs in a game. They also include the ability to either lie or make threats in ultimatum games. For example, will proposers risk rejection by a responder by lying about a pie size? Would threats by responders promote the proposers to be more honest? While these questions have been addressed under simplified settings, no research has studied the proposers' likelihood of lying when they know there is a probability that the lie might be revealed. Given the role that communication and probability interact in bargaining situations, these variables provide important insights into how people behave in reality. Introducing uncertainty, in combination in with communication, helps isolate the effects of these conditions on bargaining behavior.

The best way to model lying behavior in ultimatum games involves the use of asymmetrical information. This methodology also relates to a common real world situation where one party hides vital information from another. One obvious and classic example is the used car market. In this market the dealer knows the true value of the car but the buyer does not. Will the dealer sell a bad car to the buyer with a chance that the seller will not find out? Or does he withhold bad cars from the market in order to protect the reputation of the dealership? Similarly, newly established brands or products face the same issue. The company knows the true quality of the work but the consumer is unaware. Studying incomplete information in ultimatum game can help model the strategies used in the market for used items or newly established brands and products.

The paper will continue as follows. The literature review will start with an overview of the external validity of the ultimatum game using Akerlof's (1970) framing of asymmetric information in markets. Then I will detail the history and substantial changes made to the ultimatum game from its inception to now. Next, I will overview the literature on lying, followed by communication and lastly, asymmetric information. The next sections will describe the design and results. The paper will finish by addressing limitations, a discussion of my findings in context with the literature and lastly a conclusion.

External Validity

Many experimenters often underplay or fail to mention the relevance of their experiments outside of the lab. More articles should focus on how the results connect to the real world where billions of economic decisions occur every day. In terms of uncertainty and incomplete information, Akerlof (1970) provides one of the best theoretical papers by using the example of a used car market. Akerlof envisions a scenario where sellers have a car of some level of quality. The buyers are interested in purchasing one of these used cars but do not know the quality level of the car itself. Akerlof assumes that a car has a quality of good or bad and a probability (q) that the car is good. In this model, the bad cars will cause the owners of the good cars to leave the market, as their good cars will sell for the same price as bad ones due to asymmetric information. Furthermore, sellers want more than the expected value of an average car and buyers are not willing to pay more than the expected value at all price levels, meaning all the cars will fail to sell. This analysis shows how markets surrounding uncertainty work. People need verification of the quality of goods or the market simply fails and no one will buy anything.

While Akerlof uses this theoretical car auction to mathematically illustrate his conclusions, many other industries follow a similar pattern. For instance, insurance providers will never have complete information about people who apply for their plans. Furthermore, Akerlof shows how the theory holds up in practice. He cites data that shows that as people age, they will lose medical insurance. Medicare and, more recently, The Affordable Car Act have taken measures to fix the problem of adverse selection in the healthcare industry. Akerlof also discusses the issue of dishonesty as it applies to developing countries. He asserts that markets in developing countries have a wider range in quality. In this sense, people who have the skills to pick the better items do well as merchants or entrepreneurs. Of course, this dishonesty carries a cost as it drives the expected value of all goods sold on the market down. Akerlof concludes his famous paper by indicating the importance of trust. If people believe others and behave honestly, these types of market failures simply would not exist. However, we do not inhabit such a world and dishonesty enters the equation.

Akerlof's model shows the importance of the research presented in this paper. In reality, buyers can evaluate the item they purchase and will either trust or distrust the seller from that point on. They also have the option to try to punish the seller in some fashion. However, there also exists a probability that the seller will refuse to use deception or that the buyer will never discover the seller's duplicity. My experiment is the first of its kind, as it takes some of the abstractions that exist in reality and quantifies them. Akerlof's analysis gives the perfect framework for extending my results to a common market scenario. Extending Akerlof's initial ideas, reviews on sites like EBay in a sense help shift the expected value of an item through creating a system where the chances of deception decrease. Essentially, my research explores whether these types of measures will actually affect deception. The possibility of reviewing an item acts as an increase in the probability of revealing an actual value of an item. Reviewing exists everywhere; financial institutions constantly review companies and countries for their ability to pay back debt. Car Facts now exists to eliminate uncertainty in the buyer. By modeling these situations in the lab, I can model behavior where a seller may choose to use a site like Craigslist that does not have review or a site like Amazon where thousands of users review products.

History

The ultimatum game was introduced by Guth et al. (1982). The authors took the structure of a bargaining game and limited the play period to a single round. He wanted to go beyond studying repeated play where the strategy involved continuous decision-making. In this situation, participants need to anticipate the behavior of the person with whom they are paired. He ran several trials and varied the amount of money that proposer could divide. Like most authors after them, Guth et al. shows how his results contradict the predictions of economic rationality. Responders in these experiments typically did not accept offers that they found unfair. Essentially the minor penalty of forgoing a small amount appears worth the ability to punish a proposer for his or her selfishness.

Perhaps the most important alteration came from Kahneman, Knetsch, and Thaler (1986) when they presented the results of a variety of experiments. Most importantly, they invented the basic structure for the dictator game. This experiment follows the procedure of the ultimatum game except that responders cannot make a decision and receives whatever the proposer offers them. They first ran a simple dictator game where an allocator could keep $18 and send $2 or split the $20 evenly with $10 going to each player. In this condition 76% of people chose the even option. The researchers tried to eliminate the effect of the responder in order to see whether the results in ultimatum games were the result of fairness or strategic play. While these initial results suggest people favor fair play, this game does not take into account a spectrum of splits like the ultimatum game. Here, participants could pick one of two options whereas the ultimatum game allowed for any offer. Therefore, between a grotesquely unfair option and a completely fair one, people may opt for the even split. Yet, if a proposer could pick any amount to send, the results might differ. Later research covered these alterations (Hoffman et al 1994, Forsythe et al. 1994). Forsythe et al. receive credit for creating the dictator game in its standard form.

Other researchers looked at a variety of factors that influence the results of the game. Roth (1991), for example, looks at both bargaining behavior across countries as well as a multi-person bargaining game that closely resembles the ultimatum game. In his multi-person game, Roth phrased his variation in terms of buyers and sellers. He conducted a ten-round experiment where participants were randomly paid for the results of one round. Buyers essentially acted as proposers; they told sellers the price or offer they would accept for a good. Sellers, therefore, represent responders. The experimenter then wrote the highest offer amongst all proposers on a chalkboard. Responders then accepted or rejected the proposed offer. Interestingly, Roth included one responder in each market with multiple proposers. This experiment presents a situation where multiple proposers try to get a singular responder to accept an offer. This procedure actually shifts the power from the proposer to the responder. With multiple offers, and with the highest offer being the only one accepted or rejected, the proposers will try to offer the responders the highest amount in order to have the opportunity to earn a pay off.

Roth ran this experiment in three different countries, The U.S, Israel, Japan, and Slovenia. In each country, proposers had 1,000 tokens to offer the responders. Across the board, the offers rose in each iteration of the game and eventually offers reached the 1,000 token maximum. While his study does not prove that these effects will happen everywhere, running the game in multiple countries and getting similar results shows how the basic principle of bargaining games can transcend national boundaries and are not strictly cultural.

Along with changing the structure of the game, Roth also ran typical bargaining games in several countries. The bargaining game consists of 10 rounds where two players bargain over offers. If the offer is accepted the proposer keeps the endowment subtracted from the offer and the responder keeps the offer. However, if the responder rejects the offer, both players get nothing for that round. This structure differs from the ultimatum game in that the bargaining game lasts for multiple rounds, compared to the one-shot structure of the ultimatum game. Cross culturally, Japan and Israel experienced lower offers than the other countries. Roth cites Guth et al.'s (1982) ideas about fairness to explain this trend and suggests that each country has notions of fairness. For example, Israel and Japan might have lower offers because people would not expect strangers to exhibit fairness.

Roth ends his paper by discussing that people do not behave purely rationally, no matter which setting they are in. While different nations may experience fairness differently, none of the countries reached the game-theoretic equilibrium for this game. Therefore, Roth expands the discussion on behavioral economics by rejecting the strict assumptions set earlier.

Along with Roth, Hoffman et al. (1996, 1994) produced critical research for understanding ultimatum game behavior. Hoffman et al. (1994) focused on earned roles in ultimatum games. In this design, the role of proposer and responder are not chosen at random. Instead, participants take a general knowledge quiz and those who score better earn the right to make the division. Hoffman et al. ran both ultimatum and dictator games to test this effect. The authors found that when participants earned the role of the proposer, they offered significantly less to the responder than in the control trials. Specifically, in the earned trials only 45% of proposers offered $4 or more compared to 85% in the standard game. In earned-role dictator games, only 4% of dictators gave $4 or more but 20% gave $3 or $4. Overall, while the distribution of all offers in the dictator game decreased with entitlements when compared to a condition without entitlements, most proposers offered more than zero. Hoffman et al. (1994) argue that this result shows how even with earned roles people still act somewhat generously. Lastly, the researchers question whether these ideals of fairness come from social pressure. In order to find out, they ran a double blind version of the dictator game. In double blind experiments, decisions are unknown to the researchers and other participants rather than just other participants. In this trial only 11% of proposers gave $3 or more to receivers. Hoffman et al. (1994) assert that participants act in a way that causes them to behave more generously if the experimenter knows their decisions. In other words, controlling for this effect might have a large impact on the experiment itself. Most importantly, this research shows how fairness can be altered depending greatly on social conditions. Hoffman et al. raise important questions about how future research should carefully consider how fairness is measured. They also explore interesting aspects of human psychology that economists may overlook when attempting to study behavior and decisions making.

Building on the 1994 study, Hoffman et al. (1996) test the effect of anonymity directly. The authors' research has important implications for how future scholars run ultimatum games and economic experiments. In this experiment, they used the dictator game with different states of anonymity to test social pressure and notions of fairness. The essential question behind this study asks the following question: do proposers in the dictator game send offers based on pro-social behavior or whether the experimenter's potential judgment influence sharing behavior? In order to achieve complete anonymity, Hoffman et al. create a design where one subject acts as a monitor and will pass envelopes between the subjects. The key here is that these envelopes have blank pieces of paper as well as $1 bills. This procedure ensures that not even the monitor can tell the amount that the proposers offer. They also ran other trials where the procedure allowed for less anonymity. The results show that, when dictators have more social distance from the experimenters, they offer less to the receiver. However, 62% of dictators still gave $2 or more. Perhaps the reasoning behind this trend lies in reciprocity (Hoffman et al. 1996). Humans may have a strong drive to help others and expect something in return for doing so. Yet, Hoffman et al. fails to go into detail about these principles. It would have strengthened the paper to explore the psychology behind this type of pro-social result. While this research does not rule out ingrained social values, it shows the importance of experimental design and procedure. Researchers should always try to control for as many factors as possible when designing experiments. Hoffman et al (1996) prove that anonymity is one vital factor in behavior during bargaining games.

Social distance is one of many conditions that have an effect on the results of dictator and ultimatum games. The stake size in experimental economics may also greatly affect the results of these games. When stakes are low, participants may not act as selflessly. Specifically, in the ultimatum game, proposers might offer less and responders might have a harder time rejecting lower offers when they have to say no to a larger sum of money. Andersen et al. (2011) attempted measure the effect of the endowment size in the ultimatum game. The authors ran four different trials of the ultimatum game, representing eight different wealth treatments. In one group, participants had wealth before the experiment began; in the non-wealth group participants started the games with nothing. The authors ran the same set of stake increases with both groups. The experiment took place in rural India where the highest wealth treatment ran the game with 20, 200, 2,000 and 20,000 rupees. At the time of the paper the endowment of the highest wealth treatment was worth $410. On average, Andersen et al. (2011) report that villagers earned 100 rupees on average. Clearly, the highest wealth treatment represents an incredibly large sum to these people. Two clear results emerge from this research. First, as the endowment increases, the proportion of the endowment offered decreases. The entire distribution of offers shifts toward zero when the endowment increases. The authors confirm the statistical significance of this pattern. Second, rejection rates decrease drastically when stakes increase. For the wealth groups, rejections decreased from a high of 47% in the 200 rupee treatment to a low of 8% in the 20,000 rupee treatment. In the no wealth group, rejections decreased from a high of 42% in the 200 rupee treatment to 0% with the 20,000 rupee treatment.

These results reveal important aspects of human behavior. As stake sizes get larger, responders proportionately act more in their self-interest. It feels much harder to turn down 20% of 20,000 than 20% of 200. Similarly, proposers will want to keep as much as they possibly can and might offer the bare minimum that they think will not face rejection In the high stakes treatments, they must suspect that they will need to offer a proportionately small amount. Interestingly, Andersen et al. (2011) suggest that the demand for punishment decreases as the money increases. This result indicates that that the demand for punishment slopes downward.

Although this paper does a great job in exploring how drastic increases in stakes change the ultimatum game, the results may not be easily replicable for several reasons. The experiment takes place solely in rural India, meaning the results may not be generalizable to other areas. Furthermore, funding this type of experiment middle-to upper income countries faces budget restraints. Lastly, there are some ethical concerns with providing people with such great windfalls of cash for participating in research. It may appear patronizing to people in areas like rural India to use their inhabitants as a cheap way to test economic behavior.

The last important manipulation of the ultimatum in this review is Binmore et al's (1985) examination of a shrinking endowment size. In this design, Player 1 and Player 2 first bargain over a pie size of 100 pence. This part of the game follows the standard ultimatum game procedure with Player 1 proposing an offer and with Player 2 having the option to either accept or reject it. However, if Player 2 rejects the offer, the game continues into a second stage. In this stage, the endowment shrinks to 25 pence; Player 2 gets to propose an offer and Player 1 can either accept or reject. In stage 2, a rejection results in both players receiving nothing. In terms of game theory, the authors show that the first proposal should demand 74 pence for the proposer. Player 2 cannot possibly obtain more than 25 pence in the second round (Binmore et al. 1985). Unfortunately, the authors presented the results in terms of opening demands and not rejection rates. They presented the demands from two games, Game A and Game B. Game A incorporated all recruited participants. In Game B, participants who filled the role of Player 2 in Game A switched to occupy the Player 1 role in a new trial. In Game A, most proposers in stage 1 offered around 50 of the 100 pence. However, in Game B, the opening demands of Player 2 went up to center around the 75 pence mark. This change shows how Player 2's shifted their behavior to act more in line with the game theoretical predictions. Although the authors unfortunately leave out rejection rates, this paper shows how learning can cause participants to act more line with theory. Player 2's who have played the game before know the best strategy for making money, and they adhere to it more than first timers. The research also presents an interesting way to break down the ultimatum game with changing endowment sizes.

Ultimately, the ultimatum game provides an interesting method for analyzing the economic behavior of people. It provides a model that researchers can easily tweak to suit their interests or needs. Through the game and its modifications, we can understand nuanced aspects of behavior that become difficult to observe and analyze outside of laboratory conditions.

Lying

Asymmetric information creates an opportunity for studying outright deception and lying. The articles reviewed in this section focus on lying as a form of behavior in experimental economic settings. Lundquist et al. (2009) tested lying in a contract game. In this experiment, Player A's have a score taken from a test that determines their payoffs. In some treatments, they can lie in order to try to get Player B's to agree to a contract and receive a higher payoff for themselves. The authors ran several different trials to test how language and communication affected behavior. In a weak promise trial, Player A could send a message that reads, "My score is X". In the strong promise trial Player A's message is "I solemnly promise that my score is X". This difference tests how lying behavior might change depending on the language and format of the experiment. In a third variation, participants could send a free form message to the other player. The authors broke down lies between deceptive lies and white lies. In the structure of the game, white lies would ensure both players would receive higher payoffs. The results showed that all lies decreased from 56% in the weak promise round to 46% in the strong promise and 28% in free form communication. Lastly, Lundquist et al. (2009) also designed a questionnaire to test the perceptions of the participants in the study with regards to lying. Participants predicted with some accuracy the proportions of low talent sellers that actually lied. Interestingly, 93% of participants said that they would be less likely to lie if there was chance of discovery. If this survey is a good measure, then the variation in probability of getting caught should affect lying behavior. Arguably, the most important aspect of this research is that it shows that strong promises and free form communication decreases lying. As such, experimenters should take care to ensure their designs follow neutral wording that will not bias the results.

Gneezy (2005) also examines deception in a simple game with predetermined messages. Player 1 knows the payoffs of two options, A and B. He or she can then send a message that reads, “Option A will earn you more money than Option B” or another message that claims the opposite. Next, Player 2 chooses an option. This game presents a modified version of the ultimatum game with incomplete information. In this experiment, however, responders cannot outright reject but instead they must choose an option that they know nothing about. Gneezy ran three treatments. One round the difference in payouts was only $1 between the options. In another round, option A can give 5 to Player 1 and 15 to Player 2 and action B gave 6 to Player 1 and 5 to Player 2. The last where Option A gives 15 to Player 1 and 5 to Player 2 and B can give 5 to Player 1 and 15 to Player 2. The results showed that as the profit for lying increases the proportion of lying behavior also increases. In third treatment, where the Player 1 can profit directly from lying, 52% of Player 1s lied. Interestingly, 80% of Player 2s chose the options that Player 1 claimed was beneficial for them. Lastly, Gneezy presents a scenario and asks participants whether they think that they are fair or unfair. He uses the example of a seller potentially omitting an issue with a used car. The seller will pass the cost of the repair on to the buyer if the car overheats. Most people thought that the seller not telling the buyer about the issue was "unfair" or "very unfair."

Through his experimental design and survey questions, Gneezy sheds important light on when people lie. He points out in his discussion that, in this experiment, there were no negative consequences to lying. When people might be punished for their deception their behavior could change drastically. The capability of punishment acts as a better model for reality and most likely there will be a chance of the victim discovering a fraudulent claim.

Communication

Many studies have explored how offers by proposers in ultimatum games change by introducing communication as an extra step in the experimental process. Xiao and Houser (2005) set out to see whether the ability for responders to express emotion changed either the proposed split of money or the rejection rates by responders. The authors introduced the ability for responders to send messages to proposers about the split of money with their acceptance or rejection of the offer. They hypothesized that responders, given the ability to signal their emotions with words and not just money, would be more willing to accept lower offers. They ran a traditional ultimatum game for one treatment referred to as NEE, for “no emotional expression”; in the other treatment responders could send emotional messages back to proposers with their decision (referred to as EE). The results showed that there was not a statistically significant difference between the offers made by proposers in either treatment. However, responders in the EE treatment accepted unfair offers at a much higher rate than in the NEE treatment. This research shows that expression of emotion can cause responders to accept lower offers. Many of the messages regarding unfair offers showed that responders will begrudgingly accept lower offers if they can attempt to make the proposer feel bad. In the absence of emotion, people will use tools, in this case money, to express their beliefs. However, once they can relay their feelings, it becomes worth it to accept a smaller offer rather than pass on the chance to earn something. Xiao and Houser (2004) conclude that it is this release of emotion that causes the lowered rejection rate in the treatment with emotional expression.

Similar to Xiao and Houser (2005), Rankin (2003) examined how communication shapes the offers made in ultimatum games. In his experiment, responders could make requests of proposers before proposers submitted their offers. The author ran two treatments. The control treatment consisted of a traditional ultimatum game. The experimental treatment let responders send a message to the proposer requesting a certain division of the money. The results showed that requests by responders both lowered the amount offered by the proposer and, as a consequence, caused higher rejection rates. Rankin (2003) offered no concrete reason why the requests had the effects that they did. However, he suggests that proposers may have interpreted the requests as bluffs because responders would accept offers lower than their requested amounts. This paper interestingly shows that communication may actually deter higher offers. Furthermore, the responder's requests generally suggested a division that gave the responder over half of the money available. Proposers might have felt threatened by what they interpreted as an unfair request and decided to punish the responders by giving them low offers.

Asymmetric Information

The ability for proposers and responders to communicate adds new and interesting elements to the ultimatum game. Moving beyond these designs, other studies show the effects of incomplete information in a multitude of ways. Some studies give the proposer the option to lie. Other studies provide an outside option, not known to the other player. These designs help test how people will take advantage of incomplete information in the experiments to win more money.

Early on, the ultimatum game dealt mostly with studying notions of fairness and not as a means to test the effects of deception. Kagel (1996) was one of the first researchers to develop games that tested asymmetric information and by extension provided the first model in which proposers could implicitly use deceit to their advantage. He set up two different conversion rates for chips to bargain with. For one group, chips had higher values and for the other chips had lower values. In the different sessions, proposers and responders either had chip valued at $.10 or $.30. In the experiment, proposers chose how many chips to offer responders out of the

100-chip endowment. When only proposers knew the monetary payoffs of these chips, they consistently offered far less than a perfectly fair amount of the endowment. Furthermore, responders rejected these unfair offers at a lower rate than in other studies with complete information. In other words, proposers made offers that appeared fair in terms of chips, but in reality, they made off with a much larger take. In the condition where proposers knew the value of the chips and had the low, $.10 payoff, responders rejected 21% of offers. Presumably this result came from the false perception of unfairness.

Schmitt (2004) follows a nearly identical procedure to Kagel, except he takes the experiment a step further and explores the difference in offers when proposers and responders have an outside option. An outside option provides a pay off that does not result from the actions of the players in the game. Essentially, it gives both players less of an incentive to accept low offers as they might benefit from a disagreement. The author set up an ultimatum game that used chips in place of money. Players bargained over 100 chips either valued at $.30 or $.10 and a disagreement payoff of $2.00 or $0, depending on the treatment. In these trials participants all have the same chip value within their role of responders or proposers. In half of the treatment groups, players knew the value of the chips held and whether or not there was a disagreement payoff. In the second half of treatments, players did not have perfect information and only were made aware that the other person's chip values were not the same as their own. Players then played ten rounds as either a proposer or responder and were matched with a different person each round. The results showed that when proposers had a higher chip value and the outside option they offered a lower than even monetary split of the chips. When responders had the outside option with proposers still having higher chip values, the offers, in terms of money, increased when compared to proposers having the outside option. When responders had higher chip values, proposers offered more, in terms of money, compared to the rounds mentioned above. When proposers had the outside option with responders having higher chip values, proposers offered, on average, less, in terms of money than when responders had the outside option.

Next the experimenter repeated the above trials except that the participants were not aware of the other's chip values or any outside options. If responders do not know the value of the chips, they might accept offers that they perceive as fair but in reality heavily favor the proposer. With incomplete information, responders rejected no offer of more than 40 chips (Schmitt 2004). Furthermore, rejections were far less frequent and proposers acted much more strategically. In the first round, proposers seemed to behave altruistically as the average offer was over 40 chips. However, offers drastically decreased in subsequent rounds. Proposers tried to figure out the minimum amount of chips required to ensure that their offer would be accepted. Responders had no monetary reference to judge whether or not offers were fair. Instead they relied on what appeared to be a fair split of chips. In terms of money, this technique worked well when proposers had higher valued chips; monetary and chip offers decreased. However, when responders had higher chip values, the incomplete information worked in responders' favor. Proposers actually offered more both in terms of money and chips when they did not know responders had a higher chip value. Overall, mean offers dropped throughout each round as responders continued to accept lower and lower amounts of chips.

Expanding on the chip procedure developed by Schmitt (2004) and Kagel (1996), Koning et al. (2011) test the effects of cheap talk and deception. The authors set up two experiments to test whether people who have more perceived power use deception more often. In their design, participants played the game with computer-simulated opponents. In all treatments participants played the role of a responder and were paired with a computerized proposer. The authors created an interesting dynamic by multiplying the proposed offer by different multipliers, .9 in one case and .1 in another. The researchers told participants that their chips were worth .08 euros to them and .04 to the proposer. Responders had the option to use deception and claim their chip values were .04 or not use deception and express their chip value honestly at .08. The lower multiplier results in a lower amount of money for the responder. Therefore, responders with low multipliers might feel disadvantaged in this situation. The game gets more complicated as now participants gain the ability to send a message and lie about the relative chip values. Introducing this dynamic brings the possibility of studying a variety of factors relating to cheap talk. Although the experimenters paired the participants with computers, this study shows the value in being able to assess how people can take advantage of their situation. Pairing participants with a computer enabled the study to focus more on isolating the effects of the multiplier variable in isolation with respect the roles within the experiment. Responders who had a lower multiplier acted more deceptively than participants without it. In other words, they lied more frequently about their respective payoffs in the messages they sent. By claiming that they had a lower chip value, responders might hope for proposers to send them more chips and therefore, receive more money. For proposers, multiplier roles had no effect on the use of deception.

While these articles shed light on how incomplete information affects how proposers divide offers in ultimatum games, they share one major flaw. The use of chips, instead of money, while a seemingly good way to incorporate incomplete information, does not translate easily into an actual monetary transaction. Specifically, the most concerning result is that when there was incomplete information, proposers gave responders more than 50% of the even monetary split (Schmitt 2004). This result comes from the fact that responders in the incomplete information trials based their decision not on how much money they received but on the amount of chips proposers offered. Thus, the chips created a false reference point that responders used to judge fairness.

Croson et al. (2003) solve the problem of incomplete information by creating an experiment where the responder has no knowledge of the pie size that the proposer is charged with splitting. The paper also explores how threats and lying affect the offers in the ultimatum game. In addition to an unknown pie size, the researchers also added the possibility that the responders had an unknown outside option in the form of a disagreement payoff. Each proposer and responder was allowed to send messages to each other about the pie size and outside option respectively. Both the proposer and responder could claim that the pie size or outside option was any number between a set of values. First the responder had the opportunity to send a message to the proposer with information about their outside option or a threat. Next, the proposer sent a message with his/her offer. The game then was repeated once with the private information remaining unrevealed. Then, in certain trials, private information was revealed and the participants played a final two rounds. In each round, the participant's kept the same role and the same partner. The researchers created several conditions: In the control both participants knew the values of the pie size and the outside option. In the second treatment they tested a condition in which the responder did not know the pie size with no chance of it being revealed. In a third treatment the responders saw the pie size at the end of the experiment.

The results showed that when the responder exaggerated the size of their outside option, they received significantly higher offers from proposers. Similarly, responder threats slightly increased offers but increased rejection rates as well. When proposers lied about their pie size, they gave much lower offers without a lower rejection rate by responders. The article also examines the long-term effects of lying across multiple rounds. When responder lies were revealed, offers decreased only slightly. However, when proposer lies were revealed, subsequent offers were significantly higher across the remaining rounds.

Other studies also followed this model to address more nuanced components of games with asymmetric information. Besancenot et al. (2013) present an experimental design very similar to my own. The authors follow a comparable procedure to Croson et al. (2003). Besancenot et al. gave proposers the ability to send a potentially deceptive message to the responder regarding the size of the endowment. In these trials, the responder could not verify whether the proposers told the truth or not. The authors prove theoretically that proposers should understate the endowment amount. In doing so, they can make it appear like the responders receive a larger chunk of the pie than in actuality. They found that the models accurately predict proposer behavior. The vast majority of proposers (88.5 percent) lied about how much money they received. Furthermore, proposers chose to understate their endowments by 20.5% on average. They also quantify the payoff from misrepresenting the endowment. A one Euro gap between the stated and real endowment led to a .19 Euro lower offer by proposer on average. Lastly, the authors mention that proposers believe that the responders will take their messages at face value.

Besancenot et al. (2013) show the role that strategy can play in asymmetric information. Other authors (Kagel 1996, Schmitt 2004), created scenarios where proposers maintained an advantage over responders, but could not express it except with the size of their offers. By including the ability for proposers to contact responders, the authors added a whole new element into the ultimatum game: how the option to misinform directly affects the behavior of the proposer in terms offers and deceit. Veselý (2014) also tested deception in the ultimatum game. Proposers in his design knew the size of the endowment. Along with an offer, proposers sent a message that stated the size of the pie to the responders. Veselý then collected the offers and redistributed to the same participants randomly. Therefore, the same people played both of the roles as responder and proposer. The procedure allowed for the pie size to fluctuate between 20 and 200 in multiples of 20. Veselý reports similar results to Besancenot et al. (2013). Proposers consistently understated the endowment. Proposers received an average of 108.06 and stated they received 59.06 experimental Korunas on average. In this case, 96% of proposers deceived the responders in at least one round and 43% acted dishonestly in all three consecutive rounds. These results confirm the trends observed in Besancenot et al.'s study. When given the option to lower offers through lying, proposers will seize the opportunity and act in their self-interest. Kriss et al. (2013) also looked at deceptive proposer behavior by giving proposers the option to send a message in an ultimatum game. The authors compared deception across high endowment and low endowment treatments. However, they also added a step where proposers sent a promise to the responder regarding the accuracy of their message. When proposers had to make promises regarding their messages, they used less deception in the messages they sent. However, the ability to send a message without a promise increases lying behavior in proposers. Specifically, 88.5% of proposers misrepresented the endowment in the message treatment and 65% lied when they sent a promise along with their message. The threats in my experiment might cause proposers to have some sympathy or feel like they need to act more honestly. However, a personal promise differs greatly from the threat of a stranger.

Demographics

Researchers cannot address all variables of importance by altering the test design. In fact, demographic variables, specifically gender and personal characteristics, can have an effect on the propensity to lie. Childs (2012) conducted an experiment to directly test gendered behavior in a variation of the ultimatum game. For half of the subjects, proposers could either keep C$15 dollars and send C$5 or keep C$5 and send C$5. For the other half, proposers could keep C$5 and send C$15 or keep C$5 and send C$5. The crux of the experiment is that proposers could send one of two pre-written messages. If in the second half, the proposer claims that the option of both participants receiving 5 is best for the responder, then the proposer lied as another option gives the responder 15 and the proposer 5. The message supposedly informs the responder about which option will earn them more money. Thus, the proposer could claim that they chose an option that would earn the responder more money. In actuality, the proposer could choose either option regardless of the message they send. Childs found that 57.3% of the participants lied about the option that would benefit the responder most. Most importantly, the author's results do not show a significant difference between the behavior of men and women.

In contrast to these results, Veselý's (2014) study found definitive differences between genders. Women not only made lower offers, but they also engaged in more deception than their male counterparts. These results might contradict Childs' for several reasons. Childs' experiment had an incredibly straightforward design that offered a choice between two options. In Veselý's design, proposers have the option to lie about the endowment by any amount and then to act on that lie in terms of an offer. In other words, the more complex procedure might draw out differences that did not appear obvious in the experiment conducted by Childs.

Garcia-Gallego et al. (2012) also explored gendered aspects of the ultimatum game. The authors study whether risk preferences cause differences in results or whether other factors come into play. The authors use an established lottery game to measure their participant's attitudes toward risk. In order to avoid abstraction, the authors frame the game from the perspective of an employer offering a value and an employee having the ability to accept or reject it. They also control using the regular wording in two out of the three trials. In two of three trials the pairing is random and in trial three it remains fixed. Participants also filled out an evaluation of the game about their expectations of the pay offs before the monetary trials began. The results show that with regards to risk preferences, women exhibit more risk aversion. While women reject more throughout the experiment, this result was especially prevalent in the employer treatment. Also, women offered significantly less in the employer treatment than men. Lastly, the authors show that the gender differences in the ultimatum game do not stem from differences in risk aversion. Generally, risk-averse players will offer more in order to reduce the chance of a rejection. However, as we saw in the employer game, women offered less and in the other trials the gender difference was minimal.

This research helps eliminate one of the theories behind gender differences. However, this is a divided topic and different authors present different cases (Veselý 2014, Childs 2012). Seeing as how gender effects emerged only in the employer trial, language and framing might have a significant impact. If researchers across the board use varying languages, the confusion could stem from these nuanced and subtle differences. Ultimately, these papers show that no conclusive gender difference exists in the ultimatum game.

In addition to gender, other research examined how personal characteristics affect deception. Irlenbusch, and Ter Meer (2013) focused on how personal characteristics affect lying behavior in a public goods game. Specifically, the authors managed to separate nice players from those who act slyly. Briefly, a public goods game involves a number of participants who all divide an amount of money between a public or private good in repeated rounds. The money spent on the public good becomes multiplied by a certain factor (in this case 1.6) and then spread amongst the entire group. A participant can keep all of the money spent on the private good. Lastly, each member of the group has 10 punish points that they can give to another member. Each punish point reduces the earnings of a participant by 3 units. The experiment adds punish points to total earnings. Therefore, the group eventually moves to a pattern where everyone contributes in order to avoid punishment. The authors use this procedure for one treatment. In the other treatment, after they make their respective contributions and before administering punishment, participants can tell others how many points they contributed. They do not have to report these points honestly. After the public goods game concludes, the authors evaluated the participant's social preferences through a questionnaire. The authors show that people who score higher on their social values tend to believe the reported scores at a higher rate and contribute more themselves. The authors do not go into specific detail regarding how the survey measures social values but they suggest it includes the desire to benefit a group over an individual. People with lower social values can take advantage of this situation by recognizing that they can profit by announcing higher contributions and contributing with lower amounts. Moreover, those with lower social values managed to earn significantly more than people with higher value scores. One limitation of this result might come from the nature of the experimental setting. People may act more deceiving in an experiment than in the real world. Regardless, this paper sheds light on how behavior in experiments might reflect personal characteristics. While this result is exhibited through a public goods game, it has important implications for my study. Specifically, those who have certain inherent characteristics with regards to social preferences or ethics might have an advantage and might exploit or profit from "nice guys."

Experimental Design

My procedure is built off a similar procedure to Besancenot et al.'s (2013) design for allowing for incomplete information. However, my own experiment contains five consecutive rounds with different experimental conditions in each round. Furthermore, no experiment, to my knowledge, has dealt with the possibility of revealing the endowment size based on probability. Previous studies lose external validity in real bargaining scenarios because in real life situations people put their credibility on the line for the sake of a lie. For example, when people buy used items or buy from retailers they may not have knowledge of the true quality of the good that they purchase. After some time with the item, a consumer might discover that a firm misrepresented through branding or false advertising. Ultimately, if sellers can convince buyers that their goods have a higher value than in actuality, they can profit greatly and therefore may want to deceive the public.

The examples given above show how my experiment more accurately models real-world transactions by introducing to both participants the possibility that the endowment size would be revealed. The results of Schmitt (2004) and Croson et al. (2003) help explore how participants might behave during my design. Croson et al. show the effects of cheap talk and how people respond to lies and threats in bargaining games. Schmitt (2004) asserts that when there is incomplete information, proposers will act more selfishly. In other words, strategic minded proposers will look for the minimum amount that will ensure that the responder accepts their offer. The current literature shows that proposers often take advantage of the ability to misinform responders. Yet, their behavior might change if a there is a probability that the responder might catch the proposer in the act.

The experiment consists of a one shot ultimatum game repeated across five treatments. Participants were of 84 Connecticut College students and received $7 for taking part in the experiment with the addition of any money made throughout. The experiment was conducted through using the program Z-Tree (Fischbacher, 2007). Participants were paired anonymously and sent all messages and offers through the program. Across all five sessions, I varied the order of the treatments between two different orders to eliminate any potential effect the order might have on the results. Table 1 gives an overview of the treatments. One treatment acts as a control treatment. In this round proposers have the ability to lie without the chance of the responders discovering their deception. In the second treatment (low probability or LP), proposers have the option to tell the responders that they received any amount between 80-160 experimental currency units (ECUs) with a 25% chance that the actual pie size will be revealed to the responder before they make the choice to accept or reject the offer. Both the proposer and the responder know (1) that the proposer has the ability to misinform the responder and (2) the exact odds that the computer will show the actual endowment size to the responder. The third treatment (high probability or HP) follows the same procedure as the second except the chances of the program revealing the endowment change to 75%. The fourth treatment (low probability threat or LPT) is the same as the second except now responders can threaten proposers. Threats involve a single one-way message from the responder to the proposer outlining what the responder plans to do if the offer is too low or he/she finds out that the proposer lied. The responder can tell the proposer both the minimum percentage of the endowment that s/he will accept and her/his reaction to misinformation. The wording for minimum was "What is the minimum proportion of the endowment that you will accept in percent?" The responder could also threaten to not accept any offer, if they proposer lied. The question asked, "If the stated endowment differs from the actual endowment will you accept the offer?" The responder could fill in a button with the options "will" or "will not". The responder did not have to act on the threats sent to the proposer. The fifth treatment (high probability threat or HPT) is the same as the third with the addition of the ability for responders to send threats. Table 1 gives a summary of all the treatments in terms of probability and threats.

Table 1: Breakdown of Treatments

Round	Probability of a Reveal	Threat Option
Control	0% (no reveal)	No
LP	25%	No
HP	75%	No
HPT	75%	Yes
LPT	25%	Yes

The order of the game is as follows. First, responders will have the option to send threats in the appropriate treatments. Next, proposers will send an offer with a proposed split and a claim as to the endowment size in all treatments. After the proposal, the actual pie size is revealed to the responder with the probabilities given above in specific treatments. Lastly, responders will either accept or reject the proposer's offers.

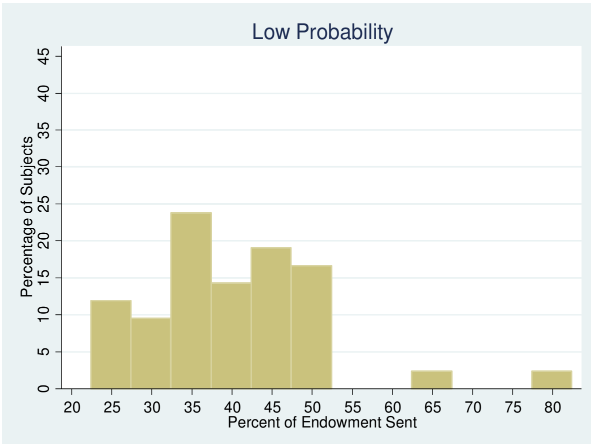
All 84 participants participated in each treatment. Participants sat at computers in a single room and before beginning the game read instructions and completed a brief quiz to ensure comprehension of the rules. The instructions for each round and quiz are presented in Appendix B. Before each treatment, participants were reminded of the rules of the upcoming round on their computer screens. At the end of the experiment, participants received payment based on a randomly chosen round, revealed at the end of the session.

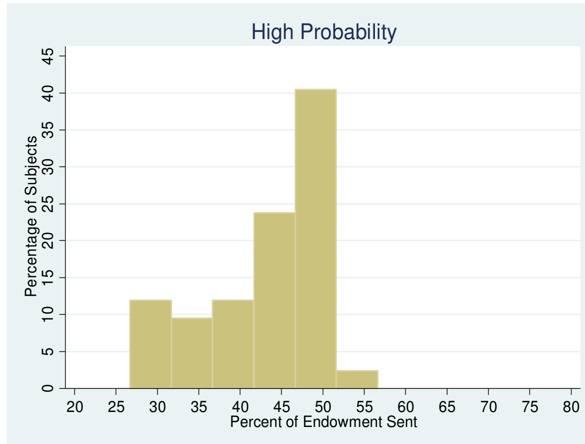
Previous studies suggest that when proposers gain an informational advantage, they use it and lie to the responders (Croson et al., 2003; Besancenot et al. 2013; Veselý 2014). Therefore, I predict that the treatment without threats will show similar results to the Besancenot and Veselý experiments. Proposers will lie and understate their endowment to increase their earnings. Secondly, just like how responders in Besancenot et al.'s study did not believe lies from proposers, in my study, cheap talk from responders should not influence proposer decisions. On the other hand, the ability to communicate threats might increase offers and result in less lying. Lastly, in rounds with higher probabilities, proposers will not lie at all or will lower the magnitude of their lie in order to increase the odds of a responder accepting their offer.

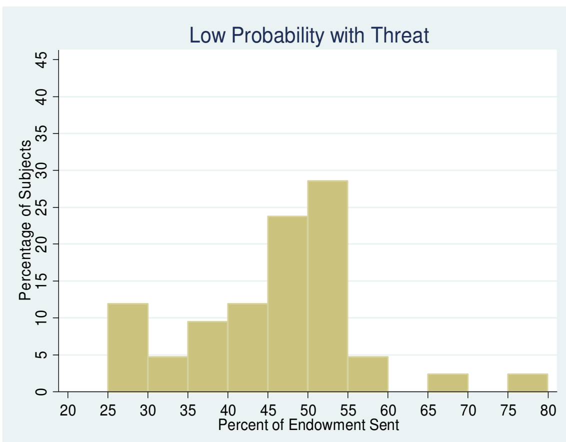
Results

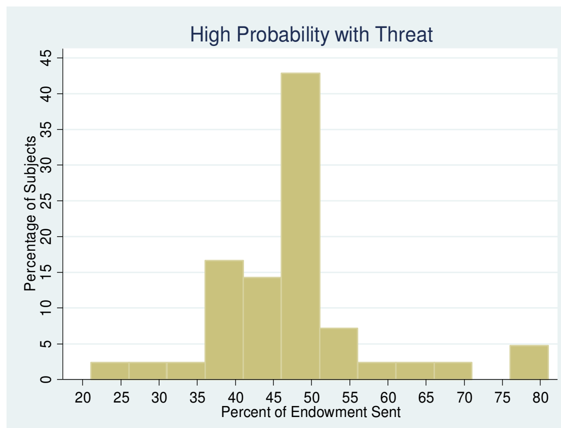
As expected, the data show a large difference in lying between the high probability and low probability treatments. I calculated the results using STATA versions 12 and 13. %Sent represents the proportion of the endowment offered by proposers. %Reported measures lying; it is calculated as the proportion of the fake endowment to the real one, meaning that if the endowment was 100 ECUs and the proposer claimed they had 80 ECUs then the %Reported equals 80. A higher %Reported indicates an overall decrease in lying. The histograms show that high probability caused a large shift toward more truth telling while the addition of threats had a lesser affect. However, threats caused a larger increase in the distribution %Sent than the change caused by high and low probabilities.

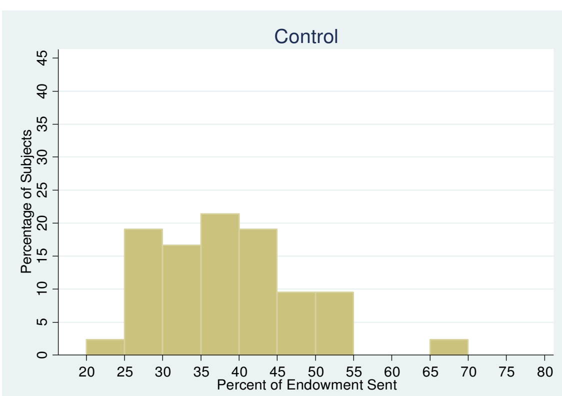
Histograms for %Sent:



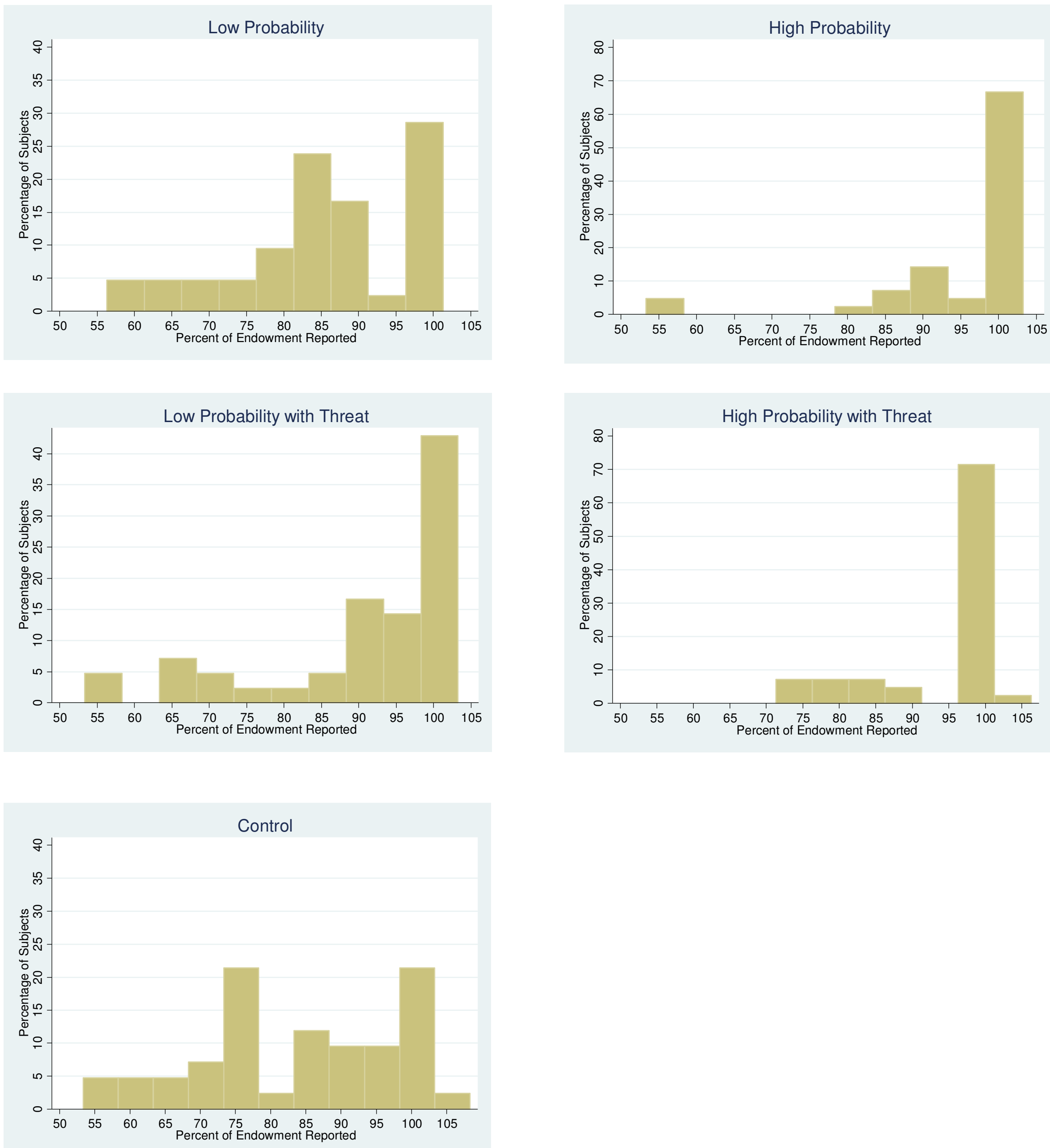








Histograms for %Reported:



In order to test the means across the treatments, I used an OLS[1] model to compare the percent reported and percent sent across all treatments. Since all the

[1] All relevant models were also run with the endowment size in the regression. The results can be found in Appendix A

proposers received different endowments, %Reported acts as the best measure of deceit. Model 1 includes only the treatments. Model 2 adds number of economics courses taken and female as binary variables. To use this regression analysis to calculate %Reported in a given treatment, add the coefficient to the constant, or control value. Specifically, the constant is the average in the control treatment and the coefficients show the differences in the other conditions, when compared to the control. For example in HPT, proposer reported that they received 95.47% of the actual endowment.

The first two specifications look at the %Reported in each treatment compared to the control. Specification 2 adds demographic variables. In each treatment, except for LP, proposers reported proportionally more of the endowment. Specifications 3 and 4 use %Sent as the dependent variable. The findings are identical to the patterns seen with %Reported. In every treatment, proposers also sent more with the exception of LP. These results indicate proposers lie significantly less and send significantly more in all treatments compared to the control. Perhaps the most interesting result that comes from examining this table is that there does not seem to be a difference when comparing lying between the HPT and HP treatments yet proposers send 5% more of their endowment in HPT compared to HP. The differences in the coefficients between threat and non-threat treatments with regards to %Sent are far larger than differences the coefficients with regards to %Reported. Threats may influence the amount sent more than they change lying behavior.

Table 2: %Reported and %Sent across treatments

	(1) Model 1 %Reported	(2) Model 2 %Reported	(3) Model 1 %Sent	(4) Model 2 %Sent
LPT	6.628*	6.628*	7.465**	7.465**
	(2.960)	(2.967)	(2.103)	(2.101)
HPT	11.25**	11.25**	9.885**	9.885**
	(2.533)	(2.540)	(2.105)	(2.108)
HP	11.43**	11.43**	4.746**	4.746**
	(2.659)	(2.670)	(1.774)	(1.801)
LP	1.997	1.997	2.078	2.078
	(2.860)	(2.865)	(2.146)	(2.167)
Female		-0.533		1.232
		(1.775)		(1.258)
Num.of Econ. Courses		-0.220		0.0485
		(0.315)		(0.259)
Constant	83.56**	84.22**	37.98**	37.12**
	(2.120)	(2.552)	(1.338)	(1.563)
Observations	210	210	210	210
R-squared	0.136	0.138	0.121	0.125

Robust standard errors in parentheses
** p<0.01, * p<0.05

There are, however, issues with using this method to calculate significance. OLS assumes the data comes from a normal distribution. Mann-Whitney tests avoid this problem, as the test does not make assumptions about the distribution. Instead, their tests examine whether the two sets of data come from the same population. If not, the samples are significantly different from each other and the test statistic is large enough to reject the null hypothesis that differences in treatments stem from chance. Table 2 and 3 compare the W-values of a series Mann-Whitney pairwise tests. Table 2 looks at pairwise tests comparing % Reported in the relevant rounds. The variables on the left column were in all cases lower than the variables in the headers. For this reason, the W-scores reported remain positive in all cases. In the case of %Reported, treatments HPT, HP, and LPT all show reduced lying when compared to the control. LP has a lower reported value when compared to HP and LPT respectively. Lastly, LPT has less lying than the HPT treatment. These results align with the OLS data shown above and, expand on them, as individual treatments are compared against one another.

Table 3: %Reported Wilcoxon. Left column distributions lower than headers

Wilcoxon W (Probability Significance level)	HPT	HP	LPT	LP
Control	4.133 (.0000)**	4.273 (.0000)**	2.329 (.0198)*	.625 (.5317)
LP		3.863 (.0001)**	2.019 (.0435)*	
HP	.207 (.8362)			
LPT	2.135 (.0328)*			

** p<0.01, * p<0.05

Similarly, table 3 shows the results of pairwise tests when looking at the %Sent variable. Once again, all rounds except LP show a significantly higher distribution than the control. When compared to LP, the %Sent increased in the LPT treatment. Lastly, proposers sent more in the HPT round when compared to the HP round.

Table 4: %Sent Wilcoxon. Left column distributions lower than headers

Wilcoxon W (Probability Significance level)	HPT	HP	LPT	LP
Control	4.595 (.0000)**	2.775 (.0055)**	3.528 (.0004)**	.850 (.3953)
LP		1.933 (.0532)	2.909 (.0036)**	
HP	2.831 (.0173)*			
LPT	.887 (.3753)			

** p<0.01, * p<0.05

This result shows that the ability to threaten increases the amount sent when the probability of getting caught remains constant.

Next, I isolated the effect of the threat conditions, both LPT and HPT, on the %Sent and %Reported. The threat conditions contained variables that only pertained to those treatments. Therefore, by analyzing these rounds separately, the nuances of the responder's threats emerge. The previous regressions show that the ability to threaten matters. Using the second set of regressions, it becomes clear exactly how the behavior of responders alters on the %Sent and %Reported. I used a standard OLS model to test how the different variables in threat conditions affected proposer behavior. Table 5 gives the results of the regressions. The "Will Not" variable is a dummy variable that shows whether responder told proposers they would not accept in the instance of a lie. Specifications 1 and 2 show the effect of the chosen variables on %Reported. Specification 2 adds controls for gender and the number of economics courses taken. The results show that the binary threat and the minimum demanded do not have an effect on %Reported in either specification. Specifications 3 and 4 look at how the same variables affect %Sent. Minimum and gender both caused an increase in %Sent. Specifically, a one-percentage increase in the amount demanded led to a 0.44 percentage increase in the offered amount. Women, in the threat conditions, on average, offered 5.48% more of the endowment than men. The data show that the binary threat in the form of "will" or "will not" does not change %Reported or %Sent Interestingly, when responders demanded more, proposers reported the same amount. Meaning that lying did not change with regards to the responder's demands. In these treatments, both responders and proposers knew that the responder did not have to follow through on either aspect of their threat.

These results help explain the patterns seen in the earlier regressions and the Mann-Whitney tests. It appears that the reason for an increase in %Sent, in threat treatments, comes from the demands of the responder. Proposers might have used that information to determine their offers while continuing the same lying behavior. Furthermore, they back up the contention that the threat options affect %Sent more than %Reported. As the previous regression shows, the threats, in the form of a demand, increase %Sent and not %Reported.

Table 5: OLS Models for threat rounds only

	(1) Model 1 %Reported	(2) Model 2 %Reported	(3) Model 1 %Sent	(4) Model 2 %Sent
High	4.563	4.550	0.0146	-0.0499
	(2.620)	(2.654)	(1.829)	(1.855)
Will Not	-0.450	-0.239	-0.558	-1.106
	(2.466)	(2.463)	(2.234)	(2.263)
Minimum	0.0115	0.0129	0.430**	0.444**
	(0.117)	(0.123)	(0.120)	(0.110)
Female		-0.0292		5.475**
		(2.791)		(1.735)
Num. Econ. Courses		-0.244		0.296
		(0.498)		(0.454)
Constant	89.95**	90.12**	26.03**	21.80**
	(6.028)	(7.003)	(5.866)	(5.270)
Observations	84	84	84	84
R-squared	0.041	0.044	0.262	0.331

Robust standard errors in parentheses
** p<0.01, * p<0.05

The previous regression results show how threats and probabilities interact to change both the percentage sent and reported. Along with studying proposer's decisions, the experimental design allows for an examination of the factors that cause a responder to accept or reject an offer. In order to run a regression on a binary variable, I used a logit model and then converted the coefficients to odds ratios. Model 1 contained three basic variables. Model 2 accounted for gender and the amount of economics courses taken.

If an offer was revealed and the reported amount increased then the proposer lied less, the responder was 1.091 times more likely to accept an offer rather than reject it if the percentage reported increased by one. Meaning that, as %Reported increased by one, and if the endowment was revealed, the responder was 9% more likely to accept. Reveals drastically reduced the likelihood of an acceptance by responders. This result most likely comes from the fact that responders have no reason to reject an offer that might look fair. However, if the endowment size gets revealed responders might punish lying or unfairness or both.

Table 6: Odds ratios on accept

Accept	(Odds Ratios) Model 1	(Odds Ratios) Model 2
Reveal	0.000167**	0.000244**
	(0.000509)	(0.000742)
%Reported	0.991	0.993
	(0.0195)	(0.0199)
RevealX%Reported	1.096**	1.091**
	(0.0372)	(0.0368)
Female		2.053
		(0.839)
Num. Econ. Courses		1.297
		(0.274)
Constant	12.93	6.048
	(22.87)	(11.09)
Observations[2]	201	201

Robust standard errors in parentheses
** p<0.01, * p<0.05

[2] There are fewer observations in this regression because 9 accepts results had to be dropped because of a coding error caught early on.

Lastly, I created to variables to look at fairness and pure truth telling. Truth equals 1 if the proposer acted with complete honesty, meaning that if reported is 100 truth has a value of 1 and 0 if otherwise. The variable fair follows a similar process except using %Sent instead. I made the threshold for the fair variable 40%. Therefore, fair was equal to 1 if %Sent was greater than or equal to 40 and 0 if otherwise.

Table 7: Odds Ratios on Truth and Fair

VARIABLES	(1) Truth Odds Ratios	(2) Fair Odds Ratios
High Probability	6.277**	2.712*
	(3.060)	(1.149)
Threat Condition	1.537	2.621**
	(0.509)	(0.913)
Low Probability	1.837	1.441
	(0.903)	(0.606)
Female	0.460*	0.854
	(0.148)	(0.269)
Num. Econ. Courses	1.016	1.013
	(0.0630)	(0.0592)
Constant	0.363*	0.738
	(0.158)	(0.283)
Observations	210	210

Robust standard errors in parentheses
** p<0.01, * p<0.05

These regressions continue to confirm the results presented earlier. High probability rounds increase complete truth telling by a factor of 6.3 and increase the likelihood of a fair offer by a factor of 2.7. Threat conditions also increase the odds of receiving a fair offer. These data continue to show how probability affects lying more than offers and how threats influence offers but have a lesser or no effect on lying. The reasoning behind this trend may come from the demands of responder influencing the proposers to send more. Surprisingly, women told absolute truths less than men although the variable Truth might not be as indicative of lying as %Reported. Women may lie by the same amount as men, as the other regressions show, but are less likely to report the full endowment size.

Limitations

The biggest limitation for any experimental study is the issue of external validity. The real world contains many factors that researchers simply cannot measure in laboratory settings. With regards to my design, threats and exact probabilities simplify more complex phenomena. Bargaining often includes many steps of negotiation, and demands ebb and flow. The minimum demanded and will/will not variables model how people make demands in real world scenarios. Similarly, when dealing with unknown conditions, reality is far more complex than 25% and 75% odds. People in bargaining situations take steps to help improve the chances of getting a good deal but ultimately, they will not know probability to the same degree of certainty as they did in the experiment. Ultimately, these simplifications become necessary to create models of reality. Yet, they may hinder the ability for experiments of any kind to explain real life patterns.

Lastly, using a within subjects design always carries unavoidable issues. I created two different treatment orders to control for any learning or treatment effects. However, a random treatment order may have been best but was not feasible given the limitations of time and Z-Tree's features.

Discussion

First I will go over how my results compare to the asymmetric information studies presented earlier. Then, I will discuss how these results expand on this literature and also raise questions that future studies should test. In the control the %Reported's average of 84% compares to Besancenot et al.'s (2013) value where the authors found that proposers understated the endowment by 20.5%. Also, in accordance with other studies, my data show that proposers take advantage of being in a position of power through prevalent lying behavior (Veselý 2014, Besancenot et al. 2013). These data also support my predictions that proposers would understate their endowments and that the lying would decrease as probability increases.

In terms of the communications, there are two main possibilities for the decrease in lying in the threat treatments. First, the enhanced communication might have led proposers to offer more out of a sense of empathy. The ability for the responder to communicate might have caused the proposer to consider them in more human terms. Unlike Rankin's (2003) findings, the results from this experiment suggest that demands by responders increase offers rather than decrease them. Rankin used an ultimatum game with complete information where responders could make demands of proposers. The demands caused proposers to lower their offers and therefore increased rejections rates. Rankin suggested that proposers interpreted the demands as bluffs and may have acted purposefully against them. However, his reasoning about bluffs does not hold up in conjunction with my results as both proposers and responders knew that the responder did not have to abide by their threats. The key difference between my experiment and his comes from complete vs. incomplete information. Demands may operate differently in ultimatum games with complete information compared to designs like mine that incorporate asymmetric information. Yet, the significance of the minimum demand might also make more sense than Rankin's reasoning about bluffs as risk aversion will cause proposers to send more in the hopes of a responder accepting. However, as mentioned, my experiment incorporated asymmetric information and Rankin's results may hold true only in a conventional ultimatum game setting. Either way, future research should deconstruct how demands by responder function in cases of asymmetric compared to complete information. While I was correct in my assertion that the communication rounds would result in higher offers, the results contradict my predictions and the findings of Rankin (2003) with regards to cheap talk by showing that proposers respond to cheap talk. The conflation between cheap talk and communication might create an interesting topic for another experiment designed to separate communication and cheap talk.

When looking at gender, the results show no differences except in the case of women sending more in the threat treatments and women are less likely to tell absolute truth. Unlike, Garcia-Gallego et al. (2012), my results do not show women offering more than men or women rejecting offers more than men. Their results may have come from Garcia-Gallego et al. (2012) framing the ultimatum game as a job interview. Also my results do not support Veselý's (2014) findings either. Veselý found that women made lower offers and engaged in more deception. Yet, my results show that men and women lied by the same proportion. While women had lower odds of telling the outright truth to responders, as mentioned earlier, %Reported is a better variable to judge the amount of lying.

All of the experiments on gender have followed different procedures and experimental conditions. My results help expand the literature on gender by showing a meaningful gender difference only in the threat conditions. In this case, women may be acting in a less risky manner than men. Yet as Garcia-Gallego et al. (2012) show, this difference might not mean that women are inherently more risk averse than men.

In addition to expanding upon previous research, my study also expands the current literature on ultimatum games. No experiment, to my knowledge, has tested probability as a factor on lying behavior. The results clearly indicate that probability has a salient effect on how proposers decide to lie. A low probability of getting caught seems not to influence lying but a high probability drastically reduces the amount the proposers lie by and, as the histograms show, the frequency of people who lie. These results suggest that a threshold must exist where proposers decide it is no longer advantageous to underrepresent the endowment at the base rate. In terms of threats, lying behavior remained unchanged between the HP and HPT treatments but decreased between LP and LPT. The increased probability of being caught in a lie must have caused proposers to change their behavior such that the addition of the threat had only a trivial effect. Yet, when the probability was low, the threat had more room to influence the decisions of proposers.

As mentioned earlier, the threat rounds may have changed behavior through two mechanisms. First, it increased communication between the proposer and responder. Second, it provided proposers with a demand. The most interesting result when examining threats comes from the minimum demanded by responder in threat treatments and the frequency of fair offers in threat treatments. Proposers actively increased the amount that they sent as responders demanded more. However, proposers may have used the demand to make the offer appear slightly better; ultimately, the responder has no way knowing except when the endowment was revealed. At the very least, the response by proposers shows that they may have believed that the cheap talk was credible. With regards to the Fair variable, responders received consistently higher proportions of the endowment in threat conditions. This result implies that cheap talk has a profound effect on the actions of proposers. Responders may have been willing to accept lower offers if they came in. Yet, proposers yielded to the demands made through their increased offers.

Conclusion

Ultimately, we can see that the higher probability decreases lying while the threats increase the amount sent. Perhaps future research could examine the distinctions and interactions in lying and threats. Although no experiment can perfectly model reality, my findings show a far more complex nature of bargaining than existed previously, as it takes into account demands in the forms of threats and probabilities. Real bargaining scenarios constantly involve these factors in some, qualitative, if not quantitative manner. Therefore, the findings help explore the nuances of interactions under asymmetric information that occur millions of times every day.

As we have seen, threats have the largest effects on fair allocations while probabilities change lying behavior. It seems natural, almost necessary, to break down bargaining into these two aspects: the demands of one party vs. the honesty of the other. Arguably, most proposers acted more honestly in fear of a rejection or a lower pay off. The best analogy for this game is to think of the proposer as a seller of a good with an unknown quality and the responder as a potential buyer of this item. In the real world, sellers of goods, companies, stocks and other merchandise have their reputations on the line. As such, the cost of being revealed as a fraud increases greatly from maybe a few dollars to the collapse of a company. As the stakes rise a small increase in the probability of being caught might dissuade deceptive behavior by a much larger amount. On the other side of the equation, there are the bargainers who will try to extract as much from the seller as possible. They will demand a specific price for the item. Apparently, if the buyer in this case issues such a statement, the seller would rather give into the buyer's demand in order to ensure the closure of the deal rather than let the buyer walk away. Of course, in reality, sellers can attempt to find another buyer. In the experiment, the one shot nature of the game put more pressure on proposer to take the demands of responders seriously.

Lastly, as I mentioned earlier, my experiment helps shed light on why review sites and customer reviews for an assortment of items matter. Essentially, these evaluations take guessing out of the equation for the buyer. The buyer knows the quality of the item that they hope to attain and the seller has a motivation to display that item honestly. Reviews greatly increase the probability of a fake item of any kind, being revealed to the public. As the results show, probability has a large effect on lying behavior. Customer reviews, in this manner, give power to the buyers and help to ensure that they receive a fair trade.

This research merely scratches the surface when exploring the interactions of probabilities and threats. It provides one of the most complete and realistic examinations of the ultimatum game to date. Future research needs to explore this topic in greater depth as important questions remain. For instance, it is not clear whether threats influence proposers because of an increase in communication or because proposers believe the cheap talk. Also, a future experiment may want to examine in greater detail how probabilities and threats interact to change lying and offers respectively.

Appendix A: Endowment Size Regressions

OLS comparison

VARIABLES	(1) Model 1 Reported	(2) Model 2 %Reported	(3) Model %Sent	(4) Model 2 %Sent
LPT	6.628*	6.573*	7.465**	7.445**
	(2.960)	(2.599)	(2.103)	(2.107)
HPT	11.25**	11.54**	9.885**	9.997**
	(2.533)	(2.217)	(2.105)	(2.067)
HP	11.43**	12.57**	4.746**	5.175**
	(2.659)	(2.458)	(1.774)	(1.800)
LP	1.997	2.960	2.078	2.441
	(2.860)	(2.478)	(2.146)	(2.164)
Female		0.824		1.743
		(1.625)		(1.268)
Num. Econ. Courses		-0.309		0.0150
		(0.302)		(0.255)
Endowment Size		-0.212**		-0.0798**
		(0.0337)		(0.0283)
Constant	83.56**	108.5**	37.98**	46.29**
	(2.120)	(4.076)	(1.338)	(3.648)
Observations	210	210	210	210
R-squared	0.136	0.285	0.121	0.157

Robust standard errors in parentheses
** p<0.01, * p<0.05

OLS Threat Conditions

VARIABLES	(1) Model 1 %Reported	(2) Model 2 %Reported	(3) Model 1 %Sent	(4) Model 2 %Sent
High	4.563	5.029*	0.0146	0.177
	(2.620)	(2.481)	(1.829)	(1.819)
Will Not	-0.450	-0.0565	-0.558	-1.019
	(2.466)	(2.259)	(2.234)	(2.227)
Minimum	0.0115	-0.0147	0.430**	0.431**
	(0.117)	(0.108)	(0.120)	(0.102)
Female		1.923		6.400**
		(2.619)		(1.649)
Num. Econ. Courses		-0.511		0.170
		(0.389)		(0.439)
Endowment Size		-0.197**		-0.0934*
		(0.0505)		(0.0418)
Constant	89.95**	113.7**	26.03**	32.97**
	(6.028)	(7.286)	(5.866)	(7.102)
Observations	84	84	84	84
R-squared	0.041	0.204	0.262	0.375

Robust standard errors in parentheses
** p<0.01, * p<0.05

Truth and Fair

VARIABLES	(1) Truth Odds Ratios	(2) Fair Odds Ratios
High Probability	7.134***	3.128**
	(3.504)	(1.399)
Threat Condition	1.470	2.519***
	(0.500)	(0.883)
Low Probability	1.957	1.582
	(0.959)	(0.702)
Female	0.499**	0.941
	(0.163)	(0.305)
Num. Econ. Courses	1.005	1.009
	(0.0668)	(0.0646)
Endowment Size	0.984**	0.982**
	(0.00649)	(0.00689)
Constant	2.217	5.722**
	(1.941)	(4.965)
Observations	210	210

Robust standard errors in parentheses
** p<0.01, * p<0.05

Appendix B: Instructions and Quiz

Instructions:

This is an experiment in the economics of decision-making. You will be paid cash earnings based on decisions that you make and decisions that other people in the experiment make.

During the experiment, you will make decisions that involve ECUs, which stand for experimental currency units. ECUs will be exchanged for dollars at a rate of 10-to-1, which means that 10 ECUs will equal $1. This exchange rate is written on the board behind me.

The experiment will last for 5 rounds. After the five rounds are complete, the computer will randomly select one of these rounds. The one selected round will determine how much money you make in the experiment. So note that you will be paid based on the outcome of one of the five rounds, but you will not know which round this will be when you are making your decisions.

At this point, please make sure that your cell phones are silenced. Please do not talk to any other participants or look at their screens. Your decisions in
the experiment will be private, and no other participant will know what choices you make. If you have a question at any point during the experiment, quietly raise your hand and somebody will come to assist you. You will be paid in cash at the end of the experiment.

There are two possible roles in the experiment, and you will occupy the same role throughout the entire experiment. The two roles are Proposers and Responders, so you will either be a Proposer or a Responder throughout the entire experiment.

In each round, you will make decisions that involve ECUs, which stand for experimental currency units. At the end of the experiment, ECUs will be exchanged for dollars at a rate of 10-1. This means that you will receive $1 for every 10 ECUs that you acquire rounded to the nearest dollar.

At the start of each round, the Proposer will receive between 80 and 160 ECUs. The Proposer will then suggest an offer in which they chose how much of the money to allocate to the Responder.

The Responder can either accept or reject the Proposer's offer. If the Responder accepts the offer, the Responder receives the offer and the Proposer receives the endowment subtracted from the offer. However, if the Responder rejects the offer, both participants receive nothing.

This experiment will consist of 5 rounds.

Round 1:

The Proposer will receive an endowment between 80 and 160 ECUs and make an offer to the Responder
The Proposer can tell the Responder they received any amount of ECUs between these two values.

The Responder will not know the size of the endowment.

Round 2:

The Responder will have the ability to threaten the Proposer with how they will react to the decisions made by the Proposer.

The Responder will tell the Proposer the minimum percentage of the endowment they will accept. They will also inform the Proposer whether they will or will not accept an offer if the Proposer is caught misrepresenting the size of the endowment.

The Proposer will receive an endowment between 80 and 160 ECUs. Before making his or her offer, the Proposer must send a message to the Responder, indicating the amount of the initial endowment that she or he received.

The Proposer can tell the Responder they received any amount of ECUs between these two values.

However, there is a 25% chance that the Responder will find out the actual size of the endowment.

In Round 3:

The Responder will have the ability to threaten the Proposer with how they will react to the decisions made by the Proposer.

The Responder will tell the Proposer the minimum percentage of the endowment they will accept. They will also inform the Proposer whether they will or will not accept an offer if the Proposer is caught misrepresenting the size of the endowment.

The Proposer will receive an endowment between 80 and 160 ECUs. Before making his or her offer, the Proposer must send a message to the Responder, indicating the amount of the initial endowment that she or he received.

The Proposer can tell the Responder they received any amount of ECUs between these two values.

However, there is a 75% chance that the Responder will find out the actual size of the endowment.

In round 4:

The Proposer will receive an endowment between 80 and 160 ECUs. Before making his or her offer, the Proposer must send a message to the Responder, indicating the amount of the initial endowment that she or he received.

However, there is a 75% chance that the Responder will find out the actual size of the endowment.

Round 5:

The Proposer will receive an endowment between 80 and 160 ECUs. Before making his or her offer, the Proposer must send a message to the Responder, indicating the amount of the initial endowment that she or he received.

The Proposer can tell the Responder they received any amount of ECUs between these two values.

However, there is a 25% chance that the Responder will find out the actual size of the endowment.

Before each round, you will be reminded of the conditions that apply in the upcoming round.

Quiz:

Question 1:

Suppose that you are a Proposer and a Responder starts the round by telling you that s/he will not accept less than 30% of the endowment and will outright reject the offer if you misrepresent the endowment.

You receive an endowment of 45 ECUs and offer the Responder 15 and tell the Responder you received 30 ECUs. You are told that there is a 75% chance that the Responder will learn the actual size of the endowment.

The Responder finds out that you misrepresented the endowment and rejects your offer.

How many ECUs does each person receive in this scenario?

Question 2:

Suppose that you are a Responder and a Proposer tells you s/he receives an endowment of 56 ECUs and offers you 25 ECUs.

You then find out the actual endowment was 80 ECUs but decide to accept the offer anyway.

How many ECUs does each person receive in this scenario?

Question 3:

Suppose that the Proposer is told that there is a 10% chance that the the[SIC] pie will be revealed. Does this mean that the Responder is likely or unlikely to learn the actual size of the Proposer's endowment?

Question 4:

Suppose that the Proposer is told that there is a 90% chance that the the[SIC] pie will be revealed. Does this mean that the the[SIC] Responder is likely or unlikely to learn the actual size of the Proposer's endowment?

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