ESSAYS IN EXPERIMENTAL ECONOMICS

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ABSTRACT

ESSAYS IN EXPERIMENTAL ECONOMICS

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Judd B. Kessler

This dissertation examines how social preferences and norms affect decision making in a variety of settings using experimental methods.

The first chapter investigates the role of transaction utility on consumer choice. I design two laboratory paradigms to mirror shopping experiences using discounts and mark-ups (Study 1) and coupons (Study 2). In my experiments, participants purchase virtual products, allowing me to isolate transaction utility from inferences of product quality. Results reveal that consumers experience transaction utility even over these virtual products and will sacrifice monetary payoffs for transaction utility. My estimates suggest consumers' marginal rate of substitution between study earnings and transaction utility is: 37-57 cents to gain a dollar of perceived discount and 37-78 cents to avoid a dollar of perceived mark-up.

Chapter 2 investigates the role of gender in negotiations. While conventional wisdom holds that women are "worse" negotiators, we find that men have a disadvantage in a setting with explicit verbal communication relative to a control game without communication. This is driven by men's failure to optimally tailor their negotiation strategy based on their partner's gender. In particular, men are significantly less likely to use tough (but effective) negotiation strategies against female compared to male partners. Male-male pairs have the worst overall joint performance, indicating that women create efficiency gains without losing out on individual payoffs. We suggest these findings may be related to gender-specific social norms influencing communication strategies.

Finally, research from the last three decades suggests that fairness plays an important role

in economic transactions. However, the vast majority of this evidence investigates behavior in a full-information environment. In chapter 3, we develop a new experimental paradigm—which nests the widely used Ultimatum Game—to show that the role of fairness in economic transactions depends fundamentally on the information structure. We find that when transacting agents are less informed inequality increases. In the absence of information, proposers give less-fair offers and report believing that responders will accept them. Responders do, in fact, accept less-fair offers when proposers are uninformed, suggesting that responders are concerned about their social image or proposers' intentions.

TABLE OF CONTENTS

LIST OF TABLES

LIST OF ILLUSTRATIONS

CHAPTER 1 : Transaction Utility and Consumer Choice

1.1. Introduction

Standard theory assumes consumers make purchasing decisions by comparing the perceived value of a good to its selling price. In practice, however, there is robust evidence that consumers respond to whether they believe they are getting a good deal. A notable example comes from JCPenney's short-lived switch from coupons to an "everyday low-pricing" structure in 2012 (Tuttle, 2012). In the first quarter after their new pricing structure was implemented, the company reported a $163 million net loss, suggesting consumers were more likely to buy when getting to use coupons and seeing discounts than when they simply faced low prices. While seeing discounts appears to encourage purchase, a large number of false-advertisement class action lawsuits allege that firms use "fictitious" original prices to create perceptions of discounts and trick consumers into buying.[1]

To explain seemingly "irrational" decisions linked to perceived discounts and mark-ups, Thaler (1985, 1999, 2008) proposed that consumers get two kinds of utility from a purchase: consumption utility, the value of the good obtained relative to its price, and transaction utility, the perceived value of the "deal."[2] Consistent with this theory, marketing research over the past 20 years has shown that "comparative price advertising," such as providing an original price, can distort consumers' purchasing behaviors (for reviews, see Compeau and Grewal (1998); Krishna et al. (2002)). However, this prior work suffers from a potential confound: consumers may use a reference price to update their valuation of the quality of a good (Rao and Monroe, 1989).[3] For example, consumers seeing a bottle of wine selling for

[1] See Berkoff v. Homegoods Inc., United States District Court Central District of California Eastern Division, No. 5:15-cv-01480, July 23, 2015; Berkoff v. Marshalls of CA, LLC, United States District Court Central District of California Eastern Division, No. 5:15-cv-01475, July 23, 2015; Gennock v. Ann Inc., United States District Court Southern District of New York, No. 1:16-cv-03340-JPO, May 5, 2016; Munning v. The Gap, Inc., United States District Court Northern District of California, No. 3:16-cv-03804-TEH, July 7, 2016; Anderson v. Kate Spade and Company, United States District Court Southern District of New York, No. 1:16-cv-07300, September 19, 2016; Holter (2011); Kang (2015).

[2] Thaler (1985, 1999, 2008) refers to consumption utility as "acquisition utility." For the purpose of clearer exposition, I rename this term "consumption utility," but retain the same definition and mathematical structure.

[3] Also see Plassmann et al. (2008); Lewis and Zalan (2014).

$20 might think it is of medium quality; however consumers seeing a bottle of wine with an original price of $40, now selling for $20, might infer that the wine is of high quality, even though it is also selling for $20. Therefore, it is hard to disentangle the effect of transaction utility (i.e., getting a $20 discount) from an inference about product quality (i.e., the wine being high quality). In addition, prior work shows that perceived discounts increase purchase intentions but do not provide evidence that reference prices can lead consumer to make suboptimal purchase decisions.[4]

Understanding whether and how transaction utility distorts consumer choice has important economic implications for firm pricing strategy and for policymakers and regulators, who may worry about the prospect of transaction utility being used to exploit consumers.

The purpose of this paper is to isolate transaction utility from product quality inference and to quantify the value of transaction utility in dollar terms. This paper uses two incentivized experiments, which mirror the shopping experience using discounts and mark-ups (Study 1) and coupons (Study 2), to test whether consumers respond to irrelevant original prices, whether consumers are willing to suffer a monetary loss to capture transaction utility, and how much of a loss consumers are willing to sacrifice for transaction utility. In both experiments, I experimentally control consumers' values by having participants make purchasing decisions over virtual products. Since products are virtual, I am able to exogenously assign participants' valuations for products (in dollars) and there by shut down the quality inference channel. The experiments reveal that participants are willing to suffer a monetary loss in exchange for transaction utility.

The *Discount and Mark-up Game* (Study 1) mirrors the shopping experience of receiving a discount or mark-up by allowing participants to compare the selling price, at which they can transact, to an original price that sets a reference point but does not affect study earnings. In this game, participants were endowed with a valuation for the virtual product, a selling price they had to pay, and an original price that was irrelevant for their monetary payoffs

[4] For example, see Compeau and Grewal (1998); Krishna et al. (2002).

but might reflect a discount or mark-up—and thereby affect their transaction utility—and they had to decide whether they wanted to purchase the virtual product. If participants chose to purchase, they received the difference between their assigned value and the selling price as earnings, and they earned $0 otherwise. I find that participants respond to the "irrelevant" original prices. Participants are more likely to purchase the virtual product if the "irrelevant" original price is above the selling price, suggesting a discount, and significantly less likely to purchase the virtual product if the "irrelevant" original price is below the selling price, suggesting a mark-up. In addition, the rate of purchasing is increasing in the size of the discount and decreasing in the size of the mark-up. In a subsequent study, I stress-test this result by displaying participants' potential earnings on their decision screen. I find that participants respond to "irrelevant" original prices in the same way. This result suggests that consumers receive transaction utility from the terms of the deal itself.

To explore a different channel through which transaction utility may affect choices, I designed a second experiment, the *Coupon Game* (Study 2), which mimics a shopping experience with a coupon. In this game, participants were endowed with preferences for two virtual products, shown the original prices for each product, given a "$5.00 discount coupon valid for one item," and had to decide which product they wanted to purchase. Participants received the difference between their value for the product and the selling price (i.e., the original price after applying the coupon) of the purchased product as study earnings. In the game, one product always generated higher earnings but lower transaction utility and the other always generated higher transaction utility but lower earnings. Thirty percent of the time, participants sacrifice earnings to purchase the good that created higher transaction utility. This result suggests that coupons may induce consumers to purchase a more expensive product—to use more of the coupon and achieve a larger discount—even though it comes at the expense of consumption utility.

In each game, I can price transaction utility. In the *Discount and Mark-up Game*, I use the randomly assigned variation between earnings and perceived markups. In the *Coupon*

Game, I use the randomly assigned variation between earnings and perceived discount. Estimates from the *Discount and Mark-Up Game* suggest that participants are willing to sacrifice 37-78 cents to *avoid a dollar of perceived mark-up*.[5] Estimates from the *Coupon Game* suggest participants are willing to pay 37-57 cents to *gain a dollar of perceived discount*.

These findings have important implications for firm strategy and consumer policy. Results suggest that transaction utility is an important component of demand and therefore should influence firm pricing. Moreover, fictitious original prices can meaningfully distort consumer purchasing behavior, leading to material losses for consumers, which may be of interest to regulators, policymakers, and litigators.

This paper contributes to the existing literature by providing incentive-compatible evidence that consumers care about the terms of the transaction separately from the value of the product itself. This direct evidence in support of transaction utility builds on a large literature about reference dependence and its effects on demand (Tversky and Kahneman, 1991; Thaler, 1999).[6] It also adds to a large set of marketing studies exploring the effect of advertised reference prices on the perceived value of an offer and purchase intention (Berkowitz and Walton, 1980; Urbany et al., 1988; Lichtenstein et al., 1989; Moore and Olshavsky, 1989; Dodds et al., 1991; Grewal et al., 1998; Bitta et al., 1981; Mobley et al., 1988; Scot et al., 1993; Biswas and Burton, 1994; Sinha and Smith, 2000; Muehlbacher

[5] By design, I cannot to directly calculate the trade-off between consumption and transaction utility in the *Discount and Mark-Up Game* when participants are provided with a perceived discount because discounts increase transaction utility and earnings simultaneously.

[6] While reference dependence and loss aversion have been used to explain such deviations in economics (Tversky and Kahneman, 1991), Tversky and Kahneman (1991) and Thaler (1999) rejected the idea that costs incurred by buyers in markets are viewed as losses. Since then, a number of other models incorporating a reference structure have been introduced. These models posit that consumers are loss averse over quantities they were expecting to consume (Kőszegi and Rabin, 2006; Heidhues and Kőszegi, 2014), that a consumer's attention is drawn towards salient attributes of goods (Bordalo et al., 2013), or that a consumer's "disenchantment" towards the firm affects purchasing behavior (Sibly, 2004). These models predict deviations from the traditional rational model, but the theories may be intractable because they rely on endogenous reference prices, which are hard to determine. Furthermore, these models may not always explain pricing patterns observed in the market such as "perpetual" sales from an essentially fictitious "regular price" seen in furniture and rug stores (Heidhues and Kőszegi, 2014).

et al., 2011).[7] I build on this work by disentangling the effect of transaction utility from the effect of inference about product quality that potentially confounds previous studies (Rao and Monroe, 1989; Armstrong and Chen, 2012).[8]

The rest of the paper proceeds as follows. Section 1.2 provides a review of Thaler's theory of transaction utility and a simple model to fix ideas. Section 1.3 presents two studies using the *Discount and Mark-up Game* to demonstrate that an "irrelevant" original price can distort purchasing behavior due to transaction utility. Section 1.4 presents two studies using the *Coupon Game* to provide additional evidence of transaction utility and to show that coupons can trap consumers into purchasing the "wrong" product. Section 1.5 discusses the potential welfare implications associated with "fictitious" sales, future areas of research, and how the findings of this paper affect firm pricing decision and consumer policy.

1.2. A Review of Transaction Utility

To explain why consumers might purchase a product at a price above their valuation for that product or forgo purchasing a product that might seemingly make them better off, Thaler (1985, 1999, 2008) proposed that consumers derive two kinds of utility from a purchase: (1) consumption utility and (2) transaction utility. Thaler defined transaction utility as the perceived value of the "deal," and it is some function of the selling price relative to a reference price. Examples of external reference prices include a seller's cost, the manufacturer's suggested retail price (MSRP), the posted price, and the original price of a good. Furthermore, Thaler posited the most important factor in determining the reference price

[7] Other studies have also looked into the believability of a price offer (Scot et al., 1993; Suter and Burton, 1996), promotion frequency (Krishna et al., 1991; Kalwani and Yim, 1992), and search intentions (Bitta et al., 1981; Urbany et al., 1988; Biswas and Burton, 1994). Also, see Monroe (1973); Peterson et al. (1985); Rao and Monroe (1989); Biswas et al. (1993); Lichtenstein and Bearden (1988); Biswas and Blair (1991); Biswas and Burton (1993); Kaicker et al. (1995); Grewal et al. (1996); Urbany et al. (1997); Darke and Dahl (2003); Darke and Chung (2005); Weaver and Frederick (2012); Ngwe (2017). Other consumer decision and reference point studies have explored the effects of specific product comparison sets (Jahedi, 2010), anchoring (Dodonova and Khoroshilov, 2004), quantity limits (Inman et al., 1997), or time-inconsistencies (Nakamura and Steinsson, 2011) to show that these reference prices affect willingness to pay.

[8] Also see Plassmann et al. (2008); Lewis and Zalan (2014).

is fairness, which depends in large part on the cost to the seller.[9]

To fix ideas, consider a simple model of the consumer's decision problem in a framework similar to Thaler's model, making some simplifying assumptions and notation changes for clarity. I assume that a consumer chooses whether to purchase a single product, $x \in \{\textit{buy=1}, \textit{not buy=0}\}$, to maximize:

$$U_x(P, O; \alpha) = \begin{cases} \underbrace{V - P}_{\text{consumption utility}} + \underbrace{\alpha[O - P]}_{\text{transaction utility}} & \text{if } x = 1 \\ 0 & \text{if } x = 0 \end{cases}$$

Where V is the value obtained from the product, P is the selling price, O is the reference price (e.g. an "original price"), and α is the coefficient of transaction utility (or the marginal rate of substitution between consumption and transaction utility). If the consumer chooses to *not buy*, assume his utility is 0. Consumers will choose to buy the good if $U_1(P, O; \alpha) \geq 0$.

$$\Rightarrow [O - P] \geq \frac{1}{\alpha}[P - V]$$

Incorporating transaction utility into the consumer's decision problem leads to two testable hypotheses. First, all else equal, as the reference price O increases, consumers are more likely to buy the product because it increases the difference between the reference price and the selling price, thus increasing the perceived discount. Second, under certain circumstances, if transaction utility is sufficiently high, consumers will buy the product even if its consumption utility is negative, because the positive gains from getting a good deal will offset any negative consumption utility.[10]

[9] Adding transaction utility into consumers' purchase evaluations can lead to two suboptimal outcomes: (1) some products that would not have been purchased absent transaction utility may be purchased because they are perceived as a good deal, and (2) some products that would have been purchased absent transaction utility may be avoided because they are perceived as a bad deal. See Thaler (1985, 1999, 2008).

[10] We can extend the model of transaction utility to a two-good setting where consumers choose to purchase one of two available goods. Imagine two products where $x \in X = \{X_1, X_2\}$, $\{V_1, V_2\}$ are the values obtained from the products, $\{P_1, P_2\}$ are the selling prices, and $\{O_1, O_2\}$ are the reference-prices. Assuming consumers have to buy one of the two products, consumers will choose X_1 if $U(X_1, P_1, O_1; \alpha) \geq U(X_2, P_2, O_2; \alpha)$.

Furthermore, we can rearrange the equation to predict consumers' willingness to pay for a product taking into account transaction utility:

$$WTP = \frac{V + \alpha O}{1 + \alpha}$$

First order conditions show how the reference price (O) changes the consumer's willingness to pay for the product. In particular, $\frac{\partial WTP}{\partial O} = \frac{\alpha}{1+\alpha}$, suggesting that changes in the consumer's willingness to pay due to changes in the reference price is constant and depends on the weight consumers place on transaction utility (i.e., the consumer's type α).[11] Furthermore, note that a dollar increase in the reference price leads to *less* than a dollar increase in willingness to pay. This is because changing the selling price alters both consumption and transaction utility. As such, a model incorporating transaction utility could imply that consumers are more (selling) price sensitive compared to a model without transaction utility. This suggest that transaction utility, if empirically relevant, could have suboptimal effects on consumer behavior and firm pricing strategy.[12]

Note that these hypotheses deviate from the neoclassical rational model in which consumers make purchasing decisions to maximize consumption utility alone.[13] However, in practice, consumers might infer information from the reference price. One could imagine a model, absent transaction utility, where the valuation of a good is a function of the reference price,

Equivalently, if $[O_1 - P_1] \geq \frac{1}{\alpha}[(V_2 - P_2) - (V_1 - P_1)] + [O_2 - P_2]$, consumers will purchase X_1. Results from this model may differ if transaction utility has diminishing marginal returns. A number of additional factors could also be incorporated into the model, including the credibility of the reference price and the salience of transaction utility.

[11] The first order condition is constant due to the assumption that transaction utility is linear.

[12] Consumers who care about transaction utility could be more price sensitive, meaning that transaction utility could increase the magnitude of the price elasticity of demand. Since firms with market power set mark-ups in proportion to the inverse elasticity of demand, this suggests that transaction utility can have effects on firm pricing strategy. Furthermore, the presence of a well-established reference price could further influence firm pricing. For example, firms in a market with a well-established original price might be encouraged to provide more discounts in order to take advantage of increased demand due to higher consumption and transaction utility as selling prices decrease. On the other hand, a firm that is able to establish or manipulate their own original price would optimally set the original price to the highest credible original price and then provide a discount to stimulate demand.

[13] Under a traditional rational economic model, consumer would purchase the good if $(V - P) \geq 0$. Note that the original price does not factor into the consumer's decision problem.

and this model would derive similar predictions as the transaction utility model.[14] As such, to show that transaction utility exists, I test these hypotheses using an "irrelevant" original price, that is, an original price that contains no information to influence consumption utility but that can alter transaction utility.

In my experiments, participants make purchase decisions about virtual products (i.e., no physical products are used). Using virtual products provides three benefits. First, the experimenter can exogenously assign to participants their valuation of a virtual product, shutting down the quality inference channel. Second, participants can be rewarded in money, based on their purchasing decisions, providing a clear measure of consumption utility. Third, since there are no actual products that participants may have encountered outside of the lab, participants should not have internal reference prices that will influence their purchasing decisions, providing a clean environment to test the effect of transaction utility obtained from "irrelevant" original prices on consumer behavior.

1.3. Study 1: Discount and Mark-up Game

The *Discount and Mark-Up Game* was designed to test how participants respond to perceived discounts and mark-ups produced by an "irrelevant," randomly assigned original price. This game mimics discounts and mark-ups observed in practice, where consumers are shown a selling price and an original price and make comparisons between them to obtain a perceived discount or mark-up.

I ran two versions of the *Discount and Mark-Up Game*, *Study 1A: Baseline* and *Study 1B: With Earnings Displayed*. In *Study 1A: Baseline*, I test whether participants respond to "irrelevant" original prices consistent with predictions from a transaction utility model. In *Study 1B: With Earnings Displayed*, I test the robustness of transaction utility by displaying participants' potential earnings, thereby highlighting their consumption utility. Below, I

[14]Under a model where consumers infer information from the reference price, O, but without transaction utility, consumers would purchase the good if $\mathbb{E}[V|O] - P \geq 0$. Also see Bagwell and Riordan (1991); Armstrong and Chen (2013) for models where high prices signal product quality and change demand.

describe the general design of both versions and then highlight their differences.

1.3.1. Experimental Design

The *Discount and Mark-Up Game* included 358 participants randomly assigned to one of two versions.[15] All participants received a $10 show-up fee for completing the 25-minute study.[16]

In this game, all participants were assigned the role of buyers and decided whether to buy a virtual product at a randomly assigned selling price (P). Since this was a virtual product—that is, no actual product was used—buyers were assigned their value of the virtual product and the seller's cost.[17] All buyers valued each virtual product at $9 and the seller had a cost of $6. To create consumption utility, participants were rewarded in earnings based on their purchasing decision: if the buyer decided to buy the product then the buyer's earnings were the value of the product less the selling price ($\$9 - P$), and the seller's earnings were the selling price less their cost ($P - \$6$), otherwise both received $0. To create transaction utility, buyers were also shown an original price (O) which provided irrelevant information in terms of the buyer's earnings or consumption utility ($\$9 - P$) but it could alter transaction utility ($O - P$), the perceived level of discount obtained.[18]

The *Discount and Mark-Up Game* relies on a 4×8 within-subject design where a participant's selling price and original price were randomly assigned to create variation in two dimensions: the earnings and perceived discount or mark-up from purchasing (see Figure 1.1 for an example).[19]

[15] Participants are students from the University of Pennsylvania across a wide range of disciplines. They participated in 1 of 24 sessions at the Wharton Behavioral Lab in January and February 2018 (12 sessions each month) with 178 participants randomly assigned to *Study 1A: Baseline* and 180 participants randomly assigned to *Study 1B: With Earnings Displayed*.

[16] Individuals participated in a *Coffee Drink Experiment*, the *Discount and Mark-up Game*, and the *Coupon Game* in the 25-minute study. Results from the *Coffee Drink Experiment* are presented in a companion paper (Huang, 2018).

[17] Thaler (2008) posited that the seller's cost is an important component of fairness and reference price formation. The seller's cost is also another way consumers can infer quality. As such, it is important to hold the seller's cost constant to further control for any product quality or other inferences participants may make from the original price.

[18] See Figure 1.1 for examples of the buyer's decision screen.

[19] If participants chose to not purchase the virtual product, their earnings were $0. I assume that if

In total, participants saw four different selling prices $\{\$6.72, \$7.51, \$8.03, \$8.42\}$, which corresponded to four different levels of buyer's earnings: $\{\$2.28, \$1.49, \$0.97, \$0.58\}$.[20] For each possible level of earnings, participants made eight different purchasing decisions. Conditional on each level of buyer's earnings, I manipulated the original price shown to create seven levels of perceived discount or mark-up and a *control* set of decisions where no original price was shown. To facilitate comparisons, original prices were selected such that the selling price as a percentage of the original price was: $\frac{P}{O} \in \{60\%, 80\%, 90\%, 100\%, 110\%, 120\%, 140\%\}$, creating three discount decisions, one without a discount or mark-up decision, and three mark-up decisions for each payoff level.[21]

All participants saw each selling price and original price combination in a random order, for a total of 32 purchasing decisions. At the end of the laboratory session, participants answered a series of demographic questions, and one decision and two participants were randomly selected for payment: one buyer and one seller.[22]

To ensure the quality of participants' responses, all participants had to answer and pass two comprehension questions before proceeding to the game. The comprehension questions consisted of two multiple choice questions, each with 10 choices (see Figure A.5.1 in the Appendix for an example).[23] This ensured that all participants understood the game, and

participants chose not to purchase the virtual product, their perceived discount or mark-up obtained was also $0.

[20]The selling prices were selected to avoid numbers that would be easily remembered. I also allowed for variation in the buyer-seller split of the pie to account for inequity aversion (Fehr and Schmidt, 1999).

[21]Note, the original prices were selected such that the selling price contained a perceived discount or mark-up of 0%, 10%, 20%, or 40%. If an original price was presented, participants were also told "the original price was offered to other participants in this study." To make this a deception-free study, all original prices were offered to a small sample of participants who were excluded from the results below.

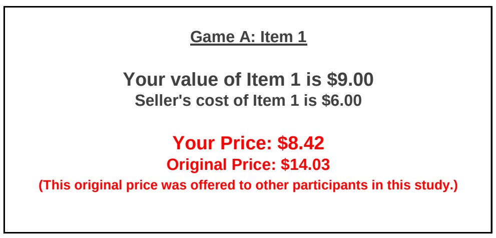
[22]Participants were also told: "You should make all your decisions assuming you are the buyer. Each round is independent of the others. Note that the price may be different from item to item. Please make each of your choices carefully. Remember, you may be selected as the buyer, in which case one of your rounds will be selected for payment. Since only one round is randomly chosen for payment you should treat each round as if it is the one and only round chosen for payment." See section A.5.1 in the Appendix for experimental protocol.

[23]Participants were shown a hypothetical scenario with the same decision screen as the game and were asked: (1) What are your earnings (in dollars) if you choose to purchase the item? (2) What are your earnings (in dollars) if you choose to NOT purchase the item? Participants were told to select their answers from two drop down lists. Each drop down list contained 10 choices ranging from $0.00 to $9.00 in one dollar increments. There is a 1% chance of correctly guessing both comprehension questions.

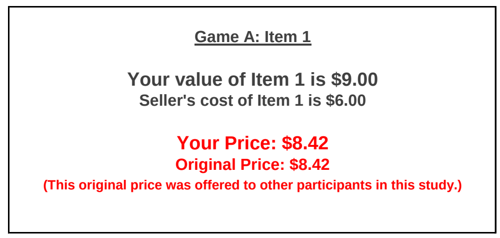
Figure 1.1: Examples of Participant's Decision Screen (Study 1)

Study 1A: Discount and Mark-up Game (Baseline)

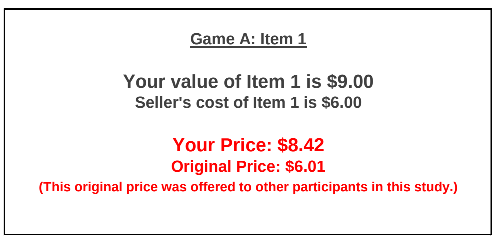
(a) Perceived Discount: $\frac{P}{O} = 60\%$



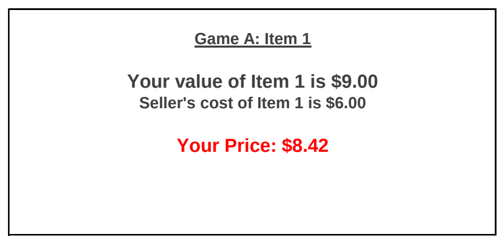
(b) No Discount/Mark-up: $\frac{P}{O} = 100\%$



(c) Perceived Mark-up: $\frac{P}{O} = 140\%$



(d) No Original Price (Control)



Note: In each decision of the *Discount and Mark-up Game*, participants were shown their assigned values of the virtual product and the seller's cost, which were always constant at \$9 and \$6. They were also shown the selling prices and the original prices which were randomly assigned. The four panels in this figure hold the selling price constant at \$8.42 (corresponding to buyer's earnings of \$0.58 if they buyer decided to purchase the virtual product) but vary the original price such that the selling price appeared discounted, equaled to, or marked-up compared to the original price. Panel A shows an example where the selling price appeared 40% discounted; Panel B shows an example where the selling price equaled the original price so there was no discount or mark-up; Panel C shows an example where the selling price appeared 40% marked-up; and Panel D shows an example where participants were not provided with an original price (control) decision.

97% of participants passed both comprehension questions on the first try.[24]

The key feature of this design is that participants were paid their earnings, calculated as the difference between the assigned value of the product and the selling price if they chose to purchase the virtual product. Thus, in terms of payoff, the original price provided irrelevant information.

1.3.1.1. Difference between Study 1A and Study 1B versions

In both versions, *Study 1A: Baseline* and *Study 1B: With Earnings Displayed*, participants played the *Discount and Mark-up Game* with the same incentives and the same 32 purchasing decisions in a random order. The *only* difference between the two versions was that participants' earnings were displayed before making their decisions in *Study 1B: With Earnings Displayed.*

Figure 1.2: Examples of Participant's Decision Screen With and Without Earnings Displayed (Study 1)

Study 1A & 1B: Discount and Mark-up Game

(a) Study 1A: Baseline

(b) Study 1B: With Earnings Displayed

Note: In both versions of the *Discount and Mark-up Game*, participants played the same game with the same incentives and the same 32 purchasing decision in a random order. Panel A shows an example of participants decision screen in *Study 1A: Baseline*, and Panel B shows an example of participants' decision screen in *Study 1B: With Earnings Displayed* where their potential earnings were displayed.

In *Study 1B: With Earnings Displayed*, participants saw the same decision screen as *Study 1A: Baseline* but participants' potential earnings were displayed with the following message: "(If you choose to purchase the item, your earnings are $___)." Figure 1.2 provides examples of participants' decision screens in both versions. *Study 1B: With Earnings Displayed* is

[24] In *Study 1A: Baseline*, 98% of participants passed both comprehension questions on the first try and in *Study 1B: With Earnings Displayed*, 96% of participants passed both comprehension questions on the first try. My results are the same including all participants or only participants who passed the comprehension check the first time.

used to stress-test the robustness of transaction utility. By displaying potential earnings, I encourage each participant to consider his consumption utility; as such, we should expect transaction utility effects to be considerably reduced or eliminated.

In the next subsection, I present the results from the *Discount and Mark-up Game*. First, I present the main results from *Study 1A: Baseline* in Section 1.3.2, then I discuss results from *Study 1B: With Earnings Displayed* in Section 1.3.3.

1.3.2. Showing consumers experience transaction utility

All analyses will focus on the average rate of purchasing the virtual product. I identify the effect of providing an "irrelevant" original price by comparing the presence of an original price that is above, equal to, or below the selling price to decisions where the original price is absent (i.e., control decisions). I do so conditional on the participants' randomly assigned expected earnings if they purchase the virtual good. I also explore the trade-off between earnings and perceived mark-up on a participant's probability of purchasing the virtual product.

Figure 1.3 Panel A shows the average purchase rate when no original price was shown (control) and when the selling price appeared discounted, equaled, or marked-up compared to the original price. Purchase rates are nearly identical in the control decisions and when the selling price equals the original price (82.7% in control and 82.1% when the selling price equals the original price; this difference is not statistically significant in a regression, $p = 0.696$). This result suggests that in the absence of an original price, buyers behave as if the selling price is equal to the original price. Furthermore, while participants' earnings increase if they choose to purchase the virtual product (compared to not purchasing, leading to earnings of \$0), it is not surprising to see a purchase rate lower than 100% due to inequality aversion. Different selling prices lead to different buyer-seller splits of the \$3 surplus. Figure A.1.1 in the Appendix presents the average purchase rate across conditions by selling price and shows that as the buyer-seller split becomes less favorable for the buyer, participants are less likely to purchase the virtual product in the control decisions. This result is consistent with the theory that individuals are inequity averse (Fehr and Schmidt, 1999).

As evidence of transaction utility, Figure 1.3 Panel A shows that providing an "irrelevant" original price can distort purchasing behavior. Participants are more likely to purchase the virtual product when the selling price appears discounted. Providing an original price above the selling prices leads to a 6.8 percentage point increase in the purchasing rate (in a regression controlling for decision order and with robust standard errors clustered at the individual level, $p < 0.01$). Moreover, the larger the perceived discount, the higher the purchasing rate. A perceived discount of 10% leads to a 3.8 percentage point increase while a perceived discount of 40% leads to a 10.7 percentage point increase in the purchasing rate (see Table A.1.1 regression (1) in the Appendix, $p < 0.01$). Similarly, buyers are less likely to purchase the item when the selling price appears marked-up, and the larger the mark-up, the bigger the distortion. Providing an original price below the selling prices leads to a 27.1 percentage point decrease in the purchasing rate (in a regression controlling for decision order and with robust standard errors clustered at the individual level, $p < 0.01$); a 10% perceived mark-up leads to a 20.4 percentage point decrease; and a 40% perceived mark-up leads to a 33.7 percentage point decrease in the purchasing rate (see Table A.1.1 regression (1) in the Appendix, $p < 0.01$).[25] These results are robust and significant with or without controlling for selling price and decision order.[26]

Figure A.1.1 and Table A.1.2 Panel A in the Appendix show the perceived fairness between the buyer-seller split of the pie causes a level shift in the purchase rate. However, identifying

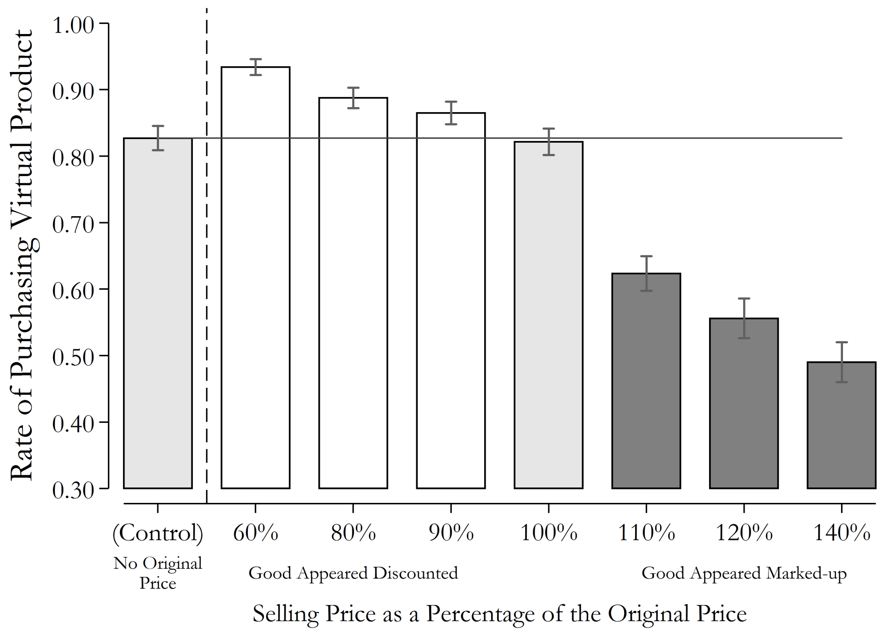
[25] Table A.1.1 in the Appendix presents the results of the *Discount and Mark-up Game* in a regression framework, testing the effect of an original price by percentage below, above, or equal to the selling price on participants' purchasing decisions. Also see Table A.1.2 Panel A regressions (9) and (10) in the Appendix.

[26] Table A.1.3 in the Appendix tests the marginal effect of a perceived percentage change between the selling price and the original price on the purchasing rate. Results are consistent and significant. I find that a perceived 10% change in perceived discount increases the probability of purchasing the virtual product by 2.70 percentage points while a perceived 10% change in mark-up decreases the probability of purchasing the virtual product by 7.00 percentage points. This differential effects between perceived discounts and mark-ups is consistent with prior findings that losses loom larger than gains with a 3:1 ratio (Fehr and Schmidt, 1999).

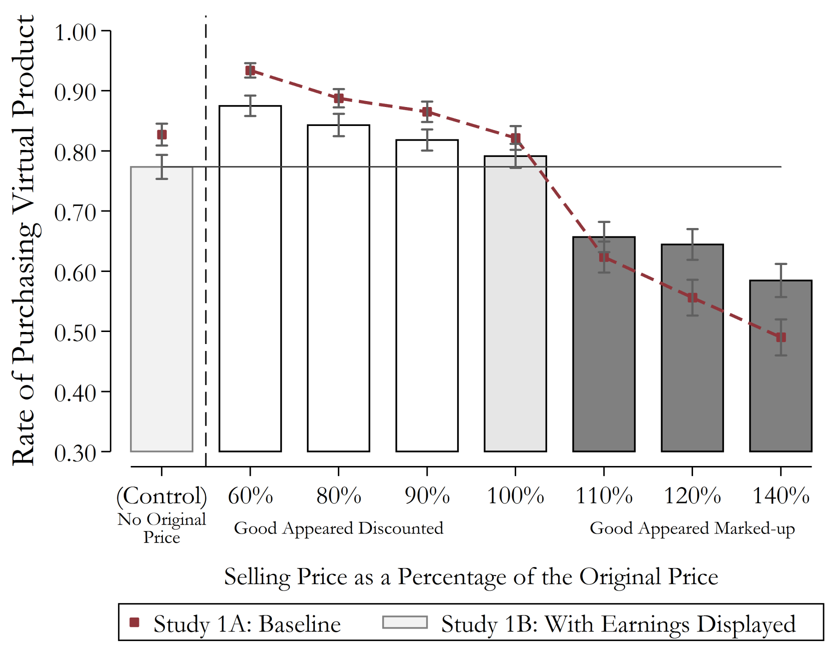
Figure 1.3: Participants Respond to an Irrelevant Original Price

Study 1A & 1B: Discount and Mark-up Game

(a) Study 1A: Baseline



(b) Study 1B: With Earnings Displayed



Notes: Participants' average purchasing rate of the virtual product by selling price as a percentage of the original price. Panel A shows results for *Study 1A: Baseline* and Panel B shows results for *Study 1B: With Earnings Displayed*. Robust standard error bars clustered at the individual level are shown around each mean.

the effect of transaction utility using the slope of purchase rate with respect to discounts and mark-ups across treatment decisions, I find that the effect of transaction utility is stable across the different splits of the pie. The one exception is when the buyer received 76% of the buyer-seller earnings split—that is, the selling price was $6.72 leading to buyer's earnings of $2.28—and participants were already purchasing 97% of the time in the control decisions (i.e., when there was no discount or mark-up). In that case, the slope is estimated to be smaller in magnitude due to a ceiling effect. Importantly, I find the same choice patterns across all earning levels: when the selling price is equal to the original price, purchase rates are nearly identical to the control decisions and participants are more likely to purchase when the selling price appears discounted and less likely to purchase when the selling price appears marked-up compared to the control decisions.

Furthermore, perceived mark-ups have a stronger effect on behavior than perceived discounts. Figure 1.3 Panel A shows that introducing a mark-up of 10% leads to a larger drop in the purchase rate than introducing a discount of 10%. We see this when we compare a 20.4 percentage point decrease in purchases due to a 10% perceived mark-up to a 3.8 percentage point increase due to a 10% perceived discount.[27] This is consistent with a notion of loss aversion over transaction utility, where consumers perceive negative transaction utility as more meaningful than equivalent positive transaction utility (Tversky and Kahneman, 1991; Thaler, 1985).

Compellingly, this variation arose within subject. The same participants who were willing to purchase the virtual product when the original price was absent or equal to the selling price were "over-purchasing" when presented with a perceived discount and "under-purchasing" when presented with a perceived mark-up. These distortions are produced by providing information (i.e., the original price) that should be irrelevant in the participant's earnings calculations.

[27]The asymmetric response to discounts and mark-ups is present even when buyers are earning the minority of the buyer-seller pie (see Figure A.1.1 in the Appendix) and so ceiling effects cannot be driving the result.

I explore the trade-off between participants' earnings and perceived levels of mark-up in dollars on probability of purchase. This analysis parallels Thaler's transaction utility model with some simplifying assumptions. I assume the purchase rate is linear in consumption utility (i.e., earnings) and transaction utility (i.e., the perceived discount or mark-up).

While there was no trade-off when the participants observed a perceived discount, in decisions with a perceived mark-up, participants were trading off higher earning and negative transaction utility when deciding whether to purchase the virtual product.[28] I estimate a linear probability model with the following specification:

$$Purchase_{it} = \alpha + \beta_1 \times Earnings_{it} + \beta_2 \times PerceivedMark\text{-}up_{it} + \epsilon_i$$

where $Purchase_{it}$ is whether participants i decided to purchase the virtual product or not in decision t in the *Discount and Mark-up Game*; $Earnings_{it}$ is the difference in earnings (in dollars) between purchasing the virtual product or not (representing "consumption utility"), and $PerceivedMark\text{-}up_{it}$ is the difference in perceived mark-up, in dollars (representing "transaction (dis)utility"). I control for the decision order and cluster the random error, ϵ_i, at the individual level.

Table 1.1 Panel A presents the trade-off between earnings (consumption utility) and perceived mark-up (transaction disutility) in *Study 1A: Baseline*. Focusing on regression (1), increasing earnings by a dollar increases the purchase rate by 18.4 percentage points. On the other hand, increasing the perceived mark-up by a dollar decreases the purchasing rate by 14.4 percentage points. Regression (2) provides a robustness check using a Probit model and shows marginal effects (holding all other independent variables constant at their means). Results demonstrate that the effect on perceived mark-up is nearly as large as the effect of active earnings. I use the estimates from Table 1.1 to quantify the willingness to pay for transaction utility in Section 1.3.5.

[28]For the purposes of this regression, I ignore inequity aversion.

Table 1.1: Trade-off Between Earnings and Perceived Mark-up

Study 1A & 1B: Discount and Mark-up Game

Panel A: Study 1A Baseline		
	Dependent Variable: Purchase Virtual Product	
	OLS	Probit
	(1)	(2)
Earnings (in $)	0.184***	0.183***
	(0.017)	(0.016)
Perceived Mark-Up (in $)	-0.144***	-0.139***
	(0.011)	(0.010)
Constant	0.538***	
	(0.039)	
Ind. Clusters	178	178
Order Control	Yes	Yes
Observations	2848	2848
R-Squared	0.129	

Panel B: Study 1B With Earnings Displayed		
	Dependent Variable: Purchase Virtual Product	
	OLS	Probit
	(1)	(2)
Earnings (in $)	0.239***	0.242***
	(0.017)	(0.016)
Perceived Mark-Up (in $)	-0.088***	-0.085***
	(0.010)	(0.010)
Constant	0.428***	
	(0.041)	
Ind. Clusters	180	178
Order Control	Yes	Yes
Observations	2880	2880
R-Squared	0.138	

Notes: Trade-off between earnings and perceived mark-up on probability of purchasing the virtual product. Panel A shows results for *Study 1A: Baseline* and Panel B shows results for *Study 1B: With Earnings Displayed*. I control for game period. Robust standard errors in parentheses. Standard errors clustered at the individual level. Significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 1.2 provides robustness checks of these results. Regression (1) reports the baseline specification for comparison. Regressions (2) and (3) subset the data to instances where the buyer-seller earnings split is such that the buyer's share is less than 50%, the cases were inequity aversion might be particularly pronounced.[29] Regression (2) shows the trade-off between earnings and perceived mark-up when the selling price is $7.51, $8.03, and $8.42 and shows that increasing earnings by a dollar increases the purchase rate by 20.0 percentage points. However this estimate incorporates the fact that increasing earnings also reduces inequality. On the other hand, increasing the perceived mark-up by a dollar decreases the purchasing rate by 14.5 percentage points. Regression (3) shows the trade-off between earnings and perceived mark-up when the selling price is $8.03 or $8.42. This comparison reduces the effect of decreasing inequality when the selling price decreases because the inequity difference is smaller, that is, the buyer's share of the pie only changes from 19% to 32%. Results show that increasing earnings by a dollar increases the purchase rate by 11.4 percentage points and increasing the perceived mark-up by a dollar decreases the purchasing rate by 13.0 percentage points. It is important to note the coefficients estimated for a perceived mark-up are stable across regression specifications. As such, inequity aversion does not affect transaction utility.

Note that in Table 1.2 regressions (1) to (3), estimates of coefficients may contain loss aversion over *transaction utility*. Regressions (4) to (6) examine the trade-off between earnings and perceived mark-up "removing" loss aversion by estimating the trade-off using perceived mark-ups of 10%, 20%, and 40%. Regression (4) shows that increasing earnings by a dollar increases the purchase rate by 19.7 percentage points and increasing the perceived mark-up by a dollar decreases the purchase rate by 8.5 percentage points. Regressions (5) and (6) mirror the sample selection of regressions (2) and (3) and show consistent results.

Another way to separate loss aversion and transaction utility effects is by using a kinked

[29]Sub-setting the data provides an "upper" bound for the effect on earnings because decreasing the selling price increases earnings and reduces buyer-seller inequality.

linear regression with the following specification:

$$Purchase_{it} = \alpha + \beta_1 \times Earnings_{it} + \beta_2 \times 1\{Mark\text{-}up_{it}\} + \beta_3 \times PerceivedMark\text{-}up_{it} + \epsilon_i$$

where the only difference compared to the previous specification is that $1\{Mark\text{-}up_{it}\}$ is a dummy variable equaled to 1 for any decision where the perceived mark-up was greater than 0%. This allows for a perceived mark-up to have a differential effect. Estimates from regression (7) show that under this specification, the coefficients for earnings and perceived mark-up are consistent with those in regression (4) and the effect of getting a mark-up reduces the purchase rate by 14.5 percentage points in *Study 1A: Baseline*.[30] Regressions (8) and (9) mimic the sample selection of regressions (2) and (3) and show consistent results.

Table 1.2: Robustness Checks: Trade-off Between Earnings and Perceived Mark-up
Study 1A: Discount and Mark-up Game (Baseline)

	Dependent Variable: Purchase Virtual Product								
	Mark-up 0-40%			Mark-up 10-40% Only			Loss Aversion Adjusted		
Selling Price:	All	≥ \$7.51	≥ \$8.03	All	≥ \$7.51	≥ \$8.03	All	≥ \$7.51	≥ \$8.03
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Earnings (in \$)	0.184***	0.200***	0.114***	0.197***	0.183***	0.064	0.192***	0.209***	0.124***
	(0.017)	(0.026)	(0.038)	(0.018)	(0.028)	(0.045)	(0.017)	(0.026)	(0.038)
Perceived Mark-Up (in \$)	-0.144***	-0.145***	-0.130***	-0.085***	-0.075***	-0.059***	-0.086***	-0.073***	-0.058***
	(0.011)	(0.012)	(0.012)	(0.011)	(0.012)	(0.013)	(0.011)	(0.012)	(0.013)
Mark-Up							-0.145***	-0.185***	-0.189***
							(0.022)	(0.026)	(0.029)
Constant	0.538***	0.530***	0.568***	0.425***	0.435***	0.499***	0.576***	0.581***	0.624***
	(0.039)	(0.045)	(0.050)	(0.046)	(0.051)	(0.059)	(0.038)	(0.045)	(0.050)
Ind. Clusters	178	178	178	178	178	178	178	178	178
Order Control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2848	2136	1424	2136	1602	1068	2848	2136	1424
R-Squared	0.129	0.0874	0.0527	0.0856	0.0314	0.00809	0.136	0.0989	0.0645

Notes: Robustness checks for trade-off between earnings and perceived mark-up on the probability of purchasing the virtual product for *Study 1A: Baseline*. I control for game period. Robust standard errors in parentheses. Standard errors clustered at the individual level. Significance: *** p<0.01, ** p<0.05, * p<0.1.

1.3.3. How robust are transaction utility effects?

While my experimental paradigm is simple for participants (i.e., participants can calculate their earnings by subtracting the selling price from their assigned value), I can further simplify this calculation. To stress-test transaction utility, *Study 1B: With Earnings Displayed* uses the same game but displays participants' potential earnings if they choose to purchase

[30] Comparing the loss aversion effect and the coefficient of a perceived mark-up suggests that loss aversion accounts for over 60% of the change in purchase rate (holding earnings constant).

the virtual product. Not only does this perform the required math for participants, it also makes consumption utility more salient. Note that displaying earnings does not provide new information to participants, as they can easily calculate the earnings themselves. This is a setting where we should not expect distortions due to an "irrelevant" original price, yet finding evidence of transaction utility in this study would show how robust this effect is and provides a lower bound for my estimates.[31]

Figure 1.3 Panel B shows the average purchasing rate by selling price as a percentage of the original price (mirroring Figure 1.3 Panel A) for *Study 1B: With Earnings Displayed*. Results show that displaying participants' potential earnings reduces but does not eliminate transaction utility. While participants exhibit a smaller purchase distortion compared to *Study 1A: Baseline*, they are still significantly more likely to purchase the virtual product under perceived discounts and less likely to purchase under perceived mark-ups compared to the control decisions.[32] This finding, that providing an "irrelevant" original price distorts purchasing behavior even when participants' earnings are displayed—encouraging them to consider their consumption utility—suggests that transaction utility is a strong and robust effect.[33]

Table 1.1 Panel B presents the trade-off between earnings and perceived mark-up in *Study 1B: With Earnings Displayed* (mirroring Table 1.1 Panel A). Focusing on regression (1), increasing earnings by a dollar increases the purchase rate by 23.9 percentage points (com-

[31] *Study 1B: With Earnings Displayed* is a setting difficult to replicate in the field but could be viewed as mapping onto situations where consumers have a rational actor (perhaps a friend) telling them to consider the true "value" they are receiving from a product and to ignore the fact that the product is "on sale."

[32] I note that participants are also less likely to purchase in the control decisions compared to *Study 1A: Baseline*. One plausible explanation for this effect is that displaying earnings makes the unequal split between the buyer's and seller's share of the surplus more salient. Figure A.1.2 and Table A.1.2 Panel B show the results by buyer's share of the surplus. Results are consistent with the baseline setting where participants are not explicitly shown their earnings.

[33] Table A.1.4 in the Appendix pools data from *Study 1A: Baseline* and *Study 1B: With Earnings Displayed* to interact the perceived percentage change with *earnings displayed* and show its effect on participants' purchasing decisions. Regression (5) shows, conditional on selling price, a perceived 10% change in mark-up when earnings are displayed increases the purchasing rate by 4.6 percentage points but this is more than offset by the coefficient of observing a perceived 10% change in mark-up (decreasing purchasing rate by 8.1 percentage points). Similarly, a perceived 10% change in discount when earnings are displayed marginally decreases the purchasing distortion by 1.2 percentage points but this is more than offset by the coefficient of observing a perceived 10% change in discount (increasing purchasing rate by 3.0 percentage points).

pared to 18.4 percentage points in the previous study). On the other hand, increasing the perceived mark-up by a dollar decreases the purchase rate by only 8.8 percentage points (compared to 14.4 percentage points in the previous study). Regression (2) provides a robustness check using a Probit model and shows marginal effects (holding all other independent variables at their means). Table A.1.5 in the Appendix provides robustness checks of these results (mirroring Table 1.2). Results follow the same pattern as those in *Study 1A: Baseline* and coefficient estimates are consistent.

Displaying earnings encourages participants to consider their earnings, which should make consumption utility more salient. As such, the presence of transaction utility effects under this stress-test shows how important the "value of the deal" is to consumers and provides a lower bound for transaction utility.

1.3.4. Heterogeneity of transaction utility effect

While the sample of participants is fairly homogeneous, I am able to explore whether specific consumer traits attenuate or amplify transaction utility effects. In particular, I look at gender and whether a participant has taken a marketing 101 course. I also explore participants' self-reported measures of caring about discounts and being averse to mark-ups. To do this, I use a difference-in-differences regression that interacts the selling price as a percentage of the original price ($\frac{P}{O}$) and a binary (or categorical) measure of the four heterogeneity groups (see Table A.1.6 Panels A and B in the Appendix).

First, gender stereotypes depict women as enjoying shopping and buying things at discounts more than men. As such, we might expect women to be more susceptible to transaction utility effects compared to men. Using data from *Study 1A: Baseline*, I find that each 10 percentage point increase in the selling price (as a percent of the original price) decreases the probability of purchasing the virtual product by an additional 2.8 percentage points for female participants compared to male participants. This difference is statistically significant and meaningful. The slope of purchase rate with respect to $\frac{P}{O}$ for women is 67% steeper than the slope for men.[34] Effects are consistent when participants were shown their potential earnings in *Study 1B: With Earnings Displayed*.[35]

Second, we might expect students who have previously taken a marketing 101 course to know about discounts and promotion tactics that companies use to drive sales. As such, we might expect transaction utility effects to be attenuated for those who have taken a marketing course. Indeed, I find that participants who have taken a marketing 101 course exhibit a less steep slope. In *Study 1A: Baseline* I find that the slope for students who have taken a marketing 101 class is 35% flatter than those who have never taken marketing 101, a difference that is significant at the 1% level.[36]

Third, in a post-survey, participants were asked to self-report how important discounts were to them and how averse they were to surge pricing on a 5-point Likert scale.[37] I find that results are correlated with these self-reported measures of caring about discounts and being averse to mark-ups. In *Study 1A: Baseline*, participants who stated that they cared more discounts or were more averse to surge pricing displayed steeper slopes than participants who stated they did not care about discounts or mark-ups, a difference that is significant at the 1% level.[38]

1.3.5. MRS of consumption and transaction utility and WTP

Since I estimate reactions to earnings and transaction utility in dollar terms, I can estimate participants' willingness to pay for a $1 gain or loss of transaction utility. I use Table 1.1

[34]See Table A.1.6 Panel A regression (1) in the Appendix.

[35]See Table A.1.6 Panel B regression (1) in the Appendix.

[36]See Table A.1.6 Panel A regression (2). Results are weaker but directionally consistent in *Study 1B: With Earnings Displayed*. See Table A.1.6 Panel B regression (2) in the Appendix.

[37]Participants were asked: "How important is it that your purchases contain a discounted price (that is, a mark-down on the original price)? (Examples of sales or discounts include: store discount codes, purchase rewards, or sales)". Participants self-reported this measure using a 5-point Likert scale from "extremely important" to "not at all important". Participants were also asked: "How averse are you to make a purchase knowing that the price is inflated (that is, a mark-up on the original price)? (Examples of inflated pricing include: ride share (Uber or Lyft) services at rush hour, hotels prices during peak season, airfare ticket prices during specific times, or sports events during high demand)". Participants self-reported this measure using a 5-point Likert scale from "extremely averse" to "not at all averse".

[38]See Table A.1.6 Panel A regression (3) and (4) in the Appendix. Results from *Study 1B: With Earnings Displayed* show that coefficients are directionally consistent but not significant or marginally significant (see Table A.1.6 Panel B regression (3) and (4) in the Appendix).

Panel A and compare the coefficients between earnings and perceived mark-up to estimate a marginal rate of substitution between consumption and transaction utility. I find that participants are willing to pay 78 cents to avoid a dollar of perceived mark-up.[39] I use Table 1.1 Panel B, which provides the effect of earnings and perceived mark-up in *Study 1B: With Earnings Displayed*, to estimate a lower bound for the marginal rate of substitution. Displaying earnings to participants prior to making their decision reduces this exchange rate to 37 cents per dollar.

Using the simple model shown in Section 1.2, a change in the original prices leads to a $\frac{\alpha}{1+\alpha}$ change in willingness to pay, where α is the marginal rate of substitution. By plugging in the estimate from *Study 1A: Baseline*, I find that participants' willingness to pay changed by 44 cents for a dollar change in the original price.[40] This suggests that firms that are able to manipulate the original price of a product (e.g. by providing a inflated original price) may be able to significantly manipulate consumers' willingness to pay for a product.

1.3.6. Alternative Explanations?

The *Discount and Mark-up Game* shows that consumers gain transaction utility from discounts and transaction disutility from mark-ups. These effects are robust to a setting where consumption utility is made salient by showing study participants their potential earnings prior to making a decision. Consumers appear to care about the terms of the deal *separately* from the value of the product and selling price (that is, their consumption utility). In this subsection, I explore, and rule out, other potential explanations for my experimental results: complexity of the design, experience, anchoring, signaling, expectations, and experimenter demand.

[39] Ngwe (2017) compares the change in purchasing probability between a dollar increase in fake sale or a dollar decrease in the real price using sales data from a luxury handbag store and finds a similar estimate, though this estimate is for perceived discounts and could potential contain a quality inference confound.

[40] Using the marginal rate of substitution from *Study 1B: With Earnings Displayed*, I estimate a lower bound effect: a dollar change in the original price leads to a 27 cents change in willingness to pay for a product.

1.3.6.1. The role of complexity

While the experimental design is simple and participants' earnings are easy to calculate, one could imagine that participants may not have understood how to calculate their earnings. However, all participants had to answer two comprehension questions which consisted of calculating their earnings if they chose to purchase the virtual product or not, and 97% of participants passed both comprehension questions on the first try. This suggest that individuals are capable of doing the correct earnings calculations, and this is a relatively simple calculation. As expected, results are consistent when I restrict my sample to only the 97% of participants who passed the comprehension questions on the first try. Importantly, participants' purchasing rate is responding to both the selling price and the "irrelevant" original price as predicted by the transaction utility model.

As an extreme way to help participants with calculations, *Study 1B: With Earnings Displayed* actually does the required math for participants by displaying potential earnings. *Study 1B: With Earnings Displayed* shows that displaying earnings information reduces the effect but does not eliminate it, suggesting my transaction utility result is not simply a result of complexity.

1.3.6.2. The role of experience

I identified the effect of transaction utility within an individual, so I can explore whether distortions in purchasing behavior are reduced as participants gain experience over the 32 decisions in the *Discount and Mark-up Game*. To do this, I divide my sample into the first and last 16 decisions of the game and use a difference-in-differences regression that interacts the selling price as a percentage of the original price ($\frac{P}{O}$). I find no significant differences between the first and second half of the game in *Study 1A: Baseline* or *Study 1B: With Earnings Displayed*, suggesting that transaction utility is not mitigated with experience.[41]

[41] See Table A.1.6 Panels A and B regression (6) in the Appendix.

1.3.6.3. The role of anchoring, signaling, and expectations

One might posit that participants anchor to an original price or that the original price signals something about social norms associated with the transaction. Prior research has shown that anchoring effects are stronger under judgment uncertainty (Kahneman, 1992).[42] In my study, judgment uncertainty is removed or minimized by holding the buyer's value and seller's cost constant. As such, anchoring effects should be minimal. However, if transaction utility exists, then individuals need a reference point to derive positive or negative transaction utility. Since I provide virtual products with no prior reference price, participants might "anchor" to the original price as the reference price to evaluate *transaction utility.* In any case, this could mean that the formation of reference points and their effects on consumer's decisions may not be fully understood and this research seeks to further understand the importance of reference points in the presence of transaction utility.

Moreover, if there are any additional signals from the original price, we might expect to see a differential effect in the first half compared to the second half of the study—that is, after participants have seen a large set of randomly assigned original prices, they should realize that any signal from the original price is just noise. As previously stated, I find that the effects in the first and second halves of the study are nearly identical.[43]

I can also rule out that participants make their purchasing decisions based on expectations formed from perceived discounts or mark-ups seen in prior decisions.[44] A regression controlling for the perceived discount or mark-up seen in up to three prior decisions shows that the effect of prior discounts and mark-ups is not significant.

[42]For example, when you are uncertain about the quality of wine you might anchor to the last two digits of your Social Security Number or when you are uncertain about the number of countries in Africa you might anchor to the (random) previous number you saw (Kahneman, 1992).

[43]See Table A.1.6 Panels A and B regression (6) in the Appendix.

[44]Note that all participants were told to treat each decision independently since only one decision would chosen for payment if the participant was selected to be the buyer.

1.3.6.4. The role of experimenter demand

Finally, one might worry that experimenter demand might be driving these results. Again, I find this is not the case. First, we could expect participants who are regular lab participants to be differentially susceptible to experimenter demand. I divide my sample into participants who have below or above median participation in lab sessions at the Wharton Behavioral Lab.[45] I find no significant differences by this median split in either *Study 1A: Baseline* or *Study 1B: With Earnings Displayed*.[46]

Moreover, in the spirit of de Quidt et al. (2018), where experimenter demand can be bounded by "deliberately inducing demand in a structured way," *Study 1B: With Earnings Displayed* provides a test of experimenter demand. In *Study 1B: With Earnings Displayed*, participants are displayed their potential earnings prior to making their decisions. This could be thought of as producing experimenter demand for participants to place more value on their earnings. That transaction utility effects persisting in this setting provides a lower bound for this effect.[47]

[45] In *Study 1A: Baseline*, participants' number of lab sessions attended ranges from 1 to 160 and in *Study 1B: With Earnings Displayed*, participants' number of lab sessions attended ranges from 1 to 126. In both studies, the median number of lab sessions completed is 13.

[46] See Table A.1.6 Panels A and B regression (5) in the Appendix. Furthermore, Kessler and Meier (2014) ran an experiment in the same lab using the same population, students from the University of Pennsylvania, and show that of participants being asked to return some portion of a $3 endowment to the experimenter, on average, only 10% of funds are returned and 75-80% of participants gave nothing back to the experimenter. This suggests that few participants are willing to give money back to the experimenter even under an explicit request.

[47] Participants were also asked to rate their selling price on a scale from 1-5 where 1 is a "very bad deal" and 5 is a "very good deal". This question was asked on the same decision screen where buyers made their purchasing decisions and is used to construct a mediation analysis (in Figure A.1.3 and Table A.1.7 in the Appendix). If asking participants to rate their selling price hinted to participants that this was a study about deal perception, a counterargument could be that asking participants to rate the deal allowed them to tell the experimenter that they know a particular selling price was a bad deal and *still* choose to purchase the virtual product to get positive earnings without feeling foolish. This could attenuate the transaction utility effect. In fact, I find that even participants who, conditional on earnings, always purchased the virtual product would still rate the price as a bad deal when they observed a perceived mark-up. Figure A.5.2 in Appendix A.5.1 shows an example of the rating question.

1.3.7. Summary of Study 1

Results from the *Discount and Mark-up Game* suggest that consumers care about the terms of the deal, it can distort purchasing behavior, and it can deter the purchase of products that lead to higher consumption utility. I argue that alternative explanations are not sufficient to explain the experimental results and finding significant heterogeneity effects consistent with our expectations further strengthens this argument. Moreover, quantifying participant's marginal rate of substitution between consumption and transaction utility (i.e., their willingness to pay to avoid a dollar worth of perceived mark-up) suggests that actual monetary losses due to transaction utility are non-trivial.[48]

Next, I use the *Coupon Game*, which mimics the shopping experience using a coupon, to show that transaction utility is robust to a variety of settings and that it can lead consumers to buy the "wrong" product (i.e, a product that leads to lower consumption utility).

1.4. Study 2: Coupon Game

Imagine walking to a store with a \$5 coupon valid for one of two products at the store. The first product is priced at \$1 and the second product is priced at \$8. Which product would you choose to buy using your \$5 coupon? While there could be a number of reasons to purchase the \$8 product, did it seem silly to use a \$5 coupon on a product that was priced at \$1? What if you would have been better off buying the product priced at \$1?

The usage of coupons presents a different (but common) way in which consumers can acquire transaction utility.[49] The *Coupon Game* mimics a shopping experience with a coupon, mirroring the scenario above, and was designed to show that distortions due to transaction utility are present in a very different context and that consumers may be manipulated into not only spending more but also buying the "wrong" product.

[48] See Appendix A.1 for additional results on monotonicity and mediation.

[49] In 2016, the total value of coupons distributed in the United States was \$307 billion. (See https://www.statista.com/statistics/630086/total-number-of-coupons-distributed-in-the-us/)

I ran two versions of the *Coupon Game*, *Study 2A: Baseline* and *Study 2B: With Earnings Displayed*. In *Study 2A: Baseline*, I test whether participants respond to coupons achieving different levels of transaction utility. In *Study 2B: With Earnings Displayed*, I test the robustness of transaction utility effects in this setting by displaying participants' potential earnings to highlight their consumption utility prior to their decision. Below, I describe the general design of both versions and then highlight their differences.

1.4.1. Experimental Design

The *Coupon Game* included 204 participants randomly assigned to one of two versions.[50] In addition to earning a $10 show-up fee, one decision and one participant were randomly selected for payment in each session.[51]

In this game, all participants were assigned the role of buyers and decided between one of two virtual products to buy using a "$5.00 off the original price discount coupon valid for one item." Again, since both items were virtual products—that is, no actual products were used—buyers were assigned their values of both virtual products: item Y and item Z. All buyers always valued item Y at $6 and item Z at $8. Participants were also provided with the (randomly assigned) original prices (O_Y and O_Z) of both virtual products. To create consumption utility, participants were rewarded in earnings based on their purchasing decision: buyers' earnings were the value of the virtual product less the price they pay (that is, the original price less the coupon value) for the chosen virtual product.[52] Notice that participants obtained transaction utility measured by the amount of coupon value used or the discount (in dollars) gained. Crucially, all participants knew they were allowed to purchase only one of the two virtual products. All participants were also told that if the value of the coupon exceeded the original price of the chosen virtual product, they would

[50] Individuals participated in 1 of 12 sessions at the Wharton Behavioral Lab in January 2018 with 101 participants randomly assigned to *Study 2A: Baseline* and 103 participants randomly assigned to *Study 2B: With Earnings Displayed*.

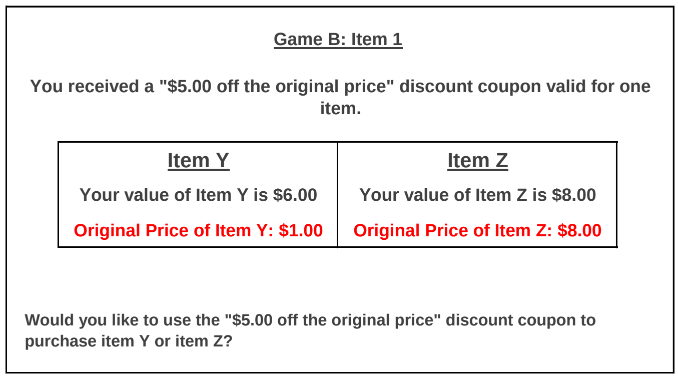
[51] See section A.5.2 in the Appendix for experimental protocol.

[52] If a buyer chose to purchase Item Y, his earnings were $6 value - [$O_Y$ - "$5 coupon"] and if a buyer chose to purchase Item Z, his earnings were $8 value - [$O_Z$ - "$5 coupon"].

Figure 1.4: Example of Participant's Decision Screen With and Without Earnings Displayed (Study 2)

Study 2A & 2B: Coupon Game

(a) Study 2A: Baseline



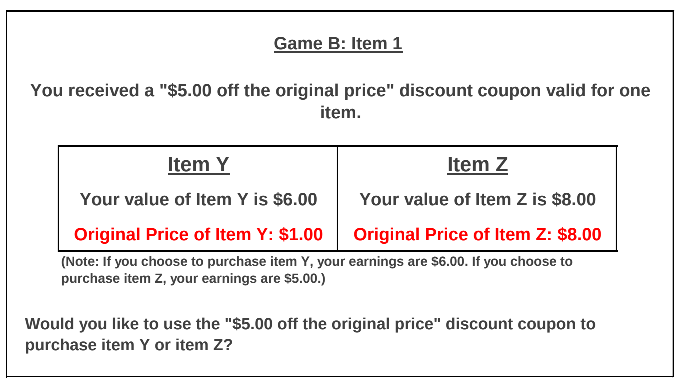
Game B: Item 1

You received a "$5.00 off the original price" discount coupon valid for one item.

Item Y	Item Z
Your value of Item Y is $6.00	Your value of Item Z is $8.00
Original Price of Item Y: $1.00	Original Price of Item Z: $8.00

Would you like to use the "$5.00 off the original price" discount coupon to purchase item Y or item Z?

(b) Study 2B: With Earnings Displayed



Game B: Item 1

You received a "$5.00 off the original price" discount coupon valid for one item.

Item Y	Item Z
Your value of Item Y is $6.00	Your value of Item Z is $8.00
Original Price of Item Y: $1.00	Original Price of Item Z: $8.00

(Note: If you choose to purchase item Y, your earnings are $6.00. If you choose to purchase item Z, your earnings are $5.00.)

Would you like to use the "$5.00 off the original price" discount coupon to purchase item Y or item Z?

Note: In each decision of the *Coupon Game*, participants were reminded they received a "$5.00 off the original price" discount coupon valid for one item and shown their assigned values and original prices for each virtual product. Panel A shows an example of the participant's decision screen in *Study 2A: Baseline*, and Panel B shows an example for *Study 2B: With Earnings Displayed*.

not receive credit back (that is, they forfeit the remainder of the coupon value).

Figure 1.4 Panel A shows an example of the participant's decision screen for the *Coupon Game*. In this example, the participant values item Y at \$6 and it has an original price of \$1; and the participant values item Z at \$8 and it has an original price of \$8. Choosing to purchase item Y leads to \$6 in earnings but only \$1 worth of transaction utility (i.e., a \$1 discount).[53] On the other hand, choosing to purchase item Z leads to \$5 in earnings, but \$5 of transaction utility (i.e., a \$5 discount).[54]

The *Coupon Game* relies on a 2×3 within-subject design where participants' original price of item Y and item Z were randomly assigned to create variation in two dimensions: (1) the difference in earnings and (2) the difference in discount gained between the two virtual products. Participants observed 6 original price combinations in a random order: $(O_Y, O_Z) \in \{(\$6, \$1), (\$6, \$3), (\$6, \$4.50), (\$1, \$8), (\$3, \$8), (\$4.50, \$8)\}$. Notice that, for all six purchasing decisions, the original price of one virtual product is always greater than \$5 and one is always less than \$5. This was constructed such that one virtual product always led to a \$5 discount (but lower earnings) and one item always led to less than \$5 in discount (but higher earnings). Which virtual product led to a \$5 discount, by using the full value of the coupon to offset the original price, was randomly assigned. For the purpose of clear exposition, I define the following terms:

- The ***coupon-trap* good** is the virtual product that led to a \$5 discount but which, by design, led to lower earnings.
- The ***alternative* good** is the other available virtual product that led to less than \$5 in discount but led to higher earnings.

To achieve the two key sources of variation in my experiment, I selected the randomly assigned original prices for each virtual product such that purchasing the *coupon-trap* good led to either \$1 or \$3 *less* in earnings compared to purchasing the *alternative* good. I also

[53] Earnings from item Y: \$6 value - [\$1 - "\$5 coupon" = \$0] = \$6; discount gained is \$1.
[54] Earnings from item Z: \$8 value - [\$8 - "\$5 coupon" = \$3] = \$5; discount gained is \$5.

independently varied the extra discount from purchasing the *coupon-trap* good (compared to the *alternative* good), such that purchasing the *coupon-trap* good led to \$0.50, \$2.00, or \$4.00 in extra discount.

All participants saw each combination of original prices, for a total of 6 purchasing decisions, in a random order. One decision and one participant were randomly selected for payment in each session. At the end of the laboratory sessions, participants answered a series of demographic questions.

To ensure the quality of participants' responses, all participants had to answer and pass two comprehension questions before proceeding to the game. The comprehension questions consisted of two multiple choice questions, each with 11 choices (see Figure A.5.3 in the Appendix for an example).[55] There is less than 1% chance of correctly guessing both comprehension questions by random chance. If participants failed any comprehension question, they had to try again until they passed both questions.[56] This ensured that all participants understood how to calculate their earnings by the time they played the game.

Calculating earnings for the *Coupon Game* required additional (and more complicated) math than calculating earnings in the *Discount and Mark-up Game*. In this case, participants first had to calculate the selling price they would be paying after applying the coupon.[57] Then, participants had to subtract the selling price from their assigned value to get to their earnings. Indeed, I find that participants found this math more difficult: only 50% of participants passed both comprehension questions on the first try.[58] However, the remaining 50% who did not pass on their first attempt had the opportunity to re-answer the

[55]Participants were shown a hypothetical scenario with the same decision screen as the game and were asked: (1) What are your earnings if you choose to <u>purchase</u> the item Y? (2) What are your earnings if you choose to <u>purchase</u> the item Z? Participants were told to select their answer from two drop down lists. Each drop down list contained 11 choices ranging from \$0.00 to \$10.00 in one dollar increments.

[56]Participants were not told which of the two questions they answered correctly and were asked to select the correct answer for both questions until they passed.

[57]The selling price was the maximum of the difference between the original price less the coupon value and zero. In math, $\textit{selling price} = max\{\textit{original price} - \textit{coupon value}, 0\}$.

[58]In *Study 2A: Baseline*, 52% of participants passed both comprehension questions on the first try and in *Study 2B: With Earnings Displayed*, 48% of participants passed both comprehension questions on the first try.

comprehension questions until they passed. Thus, all participants learned and were capable of calculating earnings by the time they played the game.[59]

The key feature of this design is that participants were paid their earnings—calculated as the difference between the assigned value and the price they pay (that is, the original price after applying the coupon) for their chosen virtual product. Thus, absent transaction utility, the coupon should only matter as a way to increase participants' earnings by reducing the selling price. By construction, if participants only care about consumption utility, they should always purchase the *alternative* good, leading to higher earnings.

1.4.1.1. Difference between Study 2A and Study 2B

In both versions, *Study 2A: Baseline* and *Study 2B: With Earnings Displayed*, participants played the *Coupon Game* with the same incentives and the same 6 purchasing decisions in a random order. The *only* difference between the two versions was that participants' earnings were displayed before making their decisions in *Study 2B: With Earnings Displayed*.

In *Study 2B: With Earnings Displayed*, participants saw the same decision screen as *Study 2A: Baseline* but participants' potential earnings were displayed with the following message: "(Note: If you choose to purchase the item Y, your earnings are $___. If you choose to purchase item Z, your earnings are $___.)" Figure 1.4 provides examples of participants' decision screens in both versions. As in Study 1, *Study 2B: With Earnings Displayed* is used to attempt to stress-test the robustness of transaction utility. By displaying potential earnings, I encourage participants to consider their consumption utility; as such, we should expect transaction utility effects to be considerably reduced or eliminated in *Study 2B: With Earnings Displayed*.

In the following subsection, I present results from the *Coupon Game*. First, I present the main results from *Study 2A: Baseline* in Section 1.4.2, then I discuss results from *Study 2B: With Earnings Displayed* in Section 1.4.3.

[59]My results are qualitatively the same including all participants or only participants who passed the comprehension check on the first try.

1.4.2. Consumers get transaction utility from using coupons

All analyses will focus on the average rate of purchasing the *coupon-trap* good. I look at the effect of (randomly assigned) extra earnings and of extra discount if consumers were to purchase the *coupon-trap* good (compared to the *alternative* good) on participants' purchasing decisions.[60] I also explore the trade-off between extra earnings and extra discount gained from purchasing the *coupon-trap* good (compared to the *alternative* good) on a participant's probability of purchasing the *coupon-trap* good.[61]

Figure 1.5 Panel A shows participants' average purchase rate of the *coupon-trap* good by the extra discount if the consumer purchased the *coupon-trap* good. I find that participants are willing to sacrifice higher earnings to obtain a higher discount almost 30% of the time. First, participants are willing to give up a dollar in earnings to gain an additional 50 cents of discount 28.5 percent of the time. Conditional on earnings, when the *coupon-trap* good leads to an extra \$2.00 of discount, buyers are 2.5 percentage points more likely to purchase the *coupon-trap* good (not significant). However, when the *coupon-trap* good leads to an extra \$4.00 of discount, buyers are significantly more likely to purchase the *coupon-trap* good by 6.9 percentage points (see Table A.2.1 regression (1) in the Appendix, $p < 0.05$). Moreover, as the earnings loss associated with purchasing the *coupon-trap* good increases from \$1 to \$3, the purchase rate of the *coupon-trap* good decreases by 6.6 percentage points (see Table A.2.1 regression (1) in the Appendix, $p < 0.01$).

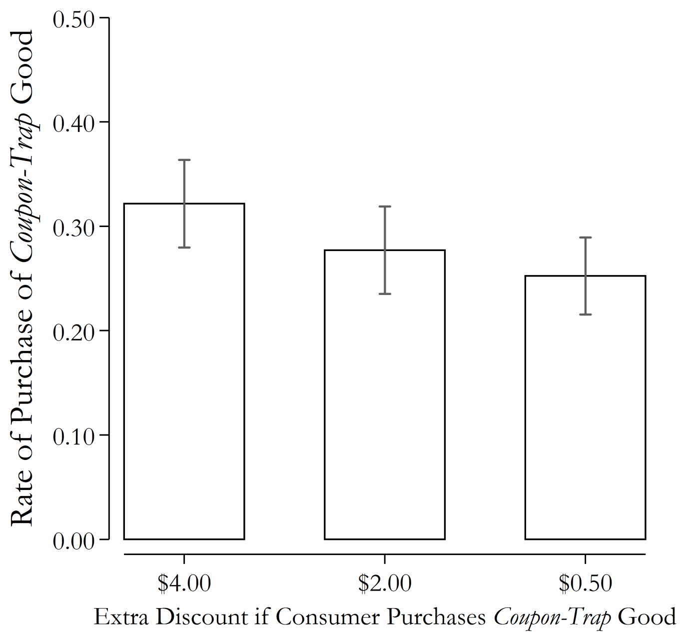
To verify that participants are behaving consistent with predictions from a model with transaction utility, I use the slope of the effect of extra discount if the consumer purchased the *coupon-trap* good. Table A.2.2 regression (1) in the Appendix shows that participants are more likely to purchase the *coupon-trap* good as the extra discount from buying the *coupon-trap* good increases (slope coefficient of 0.020 is significant at the 5% level). More-

[60]This parallels the analysis in the *Discount and Mark-up Game* where I examined the effect of perceived discount and mark-up achieved from choosing to purchase (compared to not purchase).

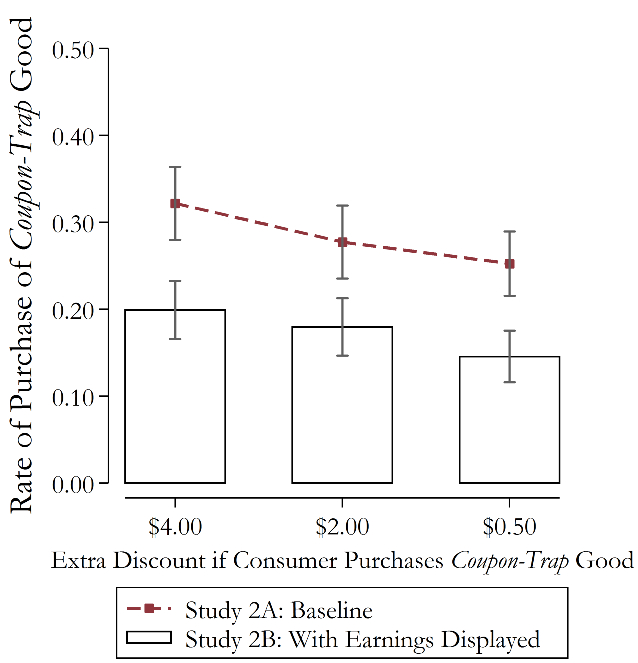
[61]This parallels the analysis in the *Discount and Mark-up Game* where I examined the trade-off between earnings and perceived mark-up on a participant's probability of purchasing the virtual product.

Figure 1.5: Rate of Purchasing the *Coupon-Trap* Good
Study 2A & 2B: Coupon Game

(a) Study 2A: Baseline



(b) Study 2B: With Earnings Displayed



Note: Purchase rate of the *coupon-trap* good by extra discount from purchasing the *coupon-trap* good. The *coupon-trap* good is the virtual product that led to a $5 discount which, by design, led to lower earnings. Panel A shows results for *Study 2A: Baseline* and Panel B shows results for *Study 2B: With Earnings Displayed.* Robust standard error bars clustered at the individual level are shown around each mean.

over, splitting the sample by extra earnings lost, we can see that this effect is primarily driven by decisions when extra earnings lost is \$1 (i.e., rather than \$3).[62]

Table 1.3 Panel A presents the results of a regression that exploits the variation in extra earnings and extra discount gained if a consumer purchases the *coupon-trap* compared to the *alternative* good in *Study 2A: Baseline*. The regression specification estimated is as follows:

$$Purchase_{it} = \alpha + \beta_1 \times Extra\ Earnings_{it} + \beta_2 \times Extra\ Discount_{it} + \epsilon_i$$

where $Purchase_{it}$ is whether participant i decided to purchase the *coupon-trap* good in decision t in the *Coupon Game*; $Extra\ Earnings_{it}$ is the extra earnings (in dollars) achieved if the consumer purchases the *coupon-trap* (compared to the *alternative* good); and $Extra\ Discount_{it}$ is the extra discount (in dollars) if the consumer purchases the *coupon-trap* good. I control for the decision order and cluster the random error, ϵ_i, at the individual level.

Results from regression (1), show that increasing extra earnings lost by a dollar decreases the purchase rate of the *coupon-trap* good by 3.3 percentage points. On the other hand, increasing the extra discount gained by a dollar increases the purchase rate of the *coupon-trap* good by 1.9 percentage points. As a robustness check, regression (2) estimates a Probit model and presents marginal effects (holding all other independent variables constant at their means) and shows consistent results.[63]

Conditional on earnings, results suggest individuals are responding to changes in extra discount gained. Notably, this was a within-subject design. Thus, the same participants who bought the *alternative* good, revealing that they did value their earnings, "switched" their purchase decisions when the extra discount from the *coupon-trap* good was sufficiently high. That is, they were willing to give up actual money for transaction utility.

[62] See Table A.2.2 regressions (2) and (3) in the Appendix.

[63] See Table A.2.3 regressions (1) and (2) in the Appendix for estimates using only participants who passed the comprehension questions on the first try.

Table 1.3: Trade-off between Extra Earnings and Extra Perceived Discount Realized by Purchasing the *Coupon-Trap* Good
Study 2A & 2B: Coupon Game

Panel A: Study 2A Baseline		
	Dependent Variable: Purchase of *Coupon-Trap* Good	
	OLS	Probit
	(1)	(2)
Extra Earnings (in \$)	-0.033***	-0.033***
	(0.011)	(0.011)
Extra Discount (in \$)	0.019**	0.019**
	(0.009)	(0.009)
Constant	0.336***	
	(0.052)	
Ind. Clusters	101	101
Order Control	Yes	Yes
Observations	606	606
R-Squared	0.0103	

Panel B: Study 2B With Earnings Displayed		
	Dependent Variable: Purchase of *Coupon-Trap* Good	
	OLS	Probit
	(1)	(2)
Extra Earnings (in \$)	-0.022**	-0.021**
	(0.010)	(0.010)
Extra Discount (in \$)	0.015	0.015
	(0.010)	(0.010)
Constant	0.229***	
	(0.054)	
Ind. Clusters	103	103
Order Control	Yes	Yes
Observations	618	618
R-Squared	0.00981	

Notes: Trade-off between extra earnings and extra discount if a consumer purchases the *coupon-trap* good on participants' purchasing decisions. The *coupon-trap* good is the virtual product that led to a $5 discount gained which, by design, led to lower earnings. Panel A shows results for *Study 2A: Baseline* and Panel B shows results for *Study 2B: With Earnings Displayed*. I control for the game period. Robust standard errors in parentheses. Standard errors clustered at the individual level. Significance: *** p<0.01, ** p<0.05, * p<0.1.

1.4.3. How robust are transaction utility effects?

I use *Study 2B: With Earnings Displayed* to stress-test the effect of transaction utility. Recall that displaying earnings does not give the participant new information, as he can calculate his earnings with the value, coupon, and original price provided. However, we should expect smaller transaction utility effects in this setting since showing earnings may encourage participants to think about their earnings, making consumption utility more salient.

Figure 1.5 Panel B shows the average purchase rate of the *coupon-trap* good by the extra discount if a consumer purchases the *coupon-trap* good compared to the *alternative* good (mirroring Figure 1.5 Panel A) for *Study 2B: With Earnings Displayed.* Results are directionally consistent with a setting where participants are not explicitly shown their earnings but differences between conditions are no longer significant in a regression (see Table A.2.1 regressions (5) and (6) in the Appendix). However, notice that participants are *still* choosing to purchase the *coupon-trap* good 15-20% of the time. Limiting the data to the "smarter" cohort of participants—participants who passed the comprehension questions on their first attempt—shows results are marginally significant (see Table A.2.1 regressions (7) and (8) in the Appendix).[64]

Focusing on the slope of extra discount if a consumer purchases the *coupon-trap* good, Table A.2.2 regression (4) in the Appendix shows increasing the extra discount by a dollar directionally increases the purchase rate by 1.5 percentage points (p-value=0.13). Similar to the baseline study, splitting the sample by extra earnings lost, shows that this effect is primarily driven by decisions where the extra earnings lost is \$1 compared to \$3. When extra earnings lost from purchasing the *coupon-trap* good is \$1, increasing the extra discount by a dollar increases participants' purchase rate of the *coupon-trap* good by 2.7 percentage points, and this is significant at the 10% level.[65]

[64] Also see Figure A.2.2 Panel B in the Appendix.
[65] See Table A.2.2 regressions (2) and (3) in the Appendix.

Table 1.3 Panel B presents the trade-off between extra earnings and extra discount if a consumer purchases the *coupon-trap* good (mirroring Table 1.3 Panel A) for participants in *Study 2B: With Earnings Displayed.* Estimates are directionally consistent with those in the baseline study where earnings were not displayed. Increasing the earnings lost from purchasing the *coupon-trap* good decreases its purchase rate by 2.2 percentage points. On the other hand, increasing the extra discount by a dollar increases the probability of purchasing the *coupon-trap* good by 1.5 percentage points but this is not. Regression (2) uses a Probit model, and reports marginal effects (holding all other independent variables at their mean), and shows a consistent story.[66]

1.4.4. Heterogeneity of transaction utility

Following Study 1, I explore consumer heterogeneity in the *Coupon Game*. Looking at the same four groups as before: gender, whether a participant has taken a marketing 101 course, and participants' self-reported measures of caring about discounts and being averse to mark-ups, I use a difference-in-differences regression that interacts the extra discount if consumers purchase the *coupon-trap* good and a binary (or categorical) measure of the four heterogeneity groups. I do not find statistically significant differences between those groups, possibly due to my lack of statistical power in this game with only 6 choices per subject (see Table A.2.4 in the Appendix).

1.4.5. MRS of consumption and transaction utility and WTP

Using the *Coupon Game*, I perform the same coefficients comparison as before to estimate participants' willingness to pay for a \$1 gain in perceived discount (that is, transaction utility). I use Table 1.3 Panel A and compare the coefficients between extra earnings and extra discount if a consumer purchases the *coupon-trap* good to estimate a marginal rate of substitution between consumption and transaction utility. I find that participants are willing to pay 57 cents to gain a dollar of transaction utility. Coefficients in Table 1.3

[66] See Table A.2.3 regressions (1) and (2) in the Appendix for estimates using only participants who passed the comprehension questions on the first try.

Panel B are noisy. As such, I use coefficients from the “smarter” cohort of participants in Table A.2.3 to estimate a lower bound for the marginal rate of substitution: participants are willing to pay a minimum of 37 cents to gain a dollar of transaction utility. Notice, this willingness to pay is similar to the 37-78 cents estimated in the *Discount and Mark-up Game*. While we might expect individuals to be willing to pay less for a perceived discount than for a perceived mark-up, in accordance with loss aversion, it is possible that coupons make transaction utility more salient.

Using the marginal rate of substitution of 57 cents suggests that increasing the original price by a dollar increases participants’ willingness to pay for a product by 36 cents. Once again, this suggests that coupons’ effectiveness may be due in large part to its effects on transaction utility.

1.4.6. Alternative Explanations?

The *Coupon Game*, which mirrors buying products with coupons, showed that participants respond to changes in discount gained in a manner consistent with a theory of transaction utility. This suggests that coupons may trap consumers into purchasing more expensive products to obtain higher discounts at the expense of consumption utility.[67] Following the approach in Study 1, I discuss and rule out alternative explanations for these experimental results.

1.4.6.1. The role of complexity

As before, we might question the complexity of the *Coupon Game* and whether participants understood how to calculate their earnings for each virtual product. Similar to Study 1, all participants had to correctly answer two comprehension questions which consisted of calculating their earnings if they chose to buy item Y or item Z before proceeding to the game. While it appears that this earnings calculation is difficult, with only 50% of

[67] This latter result is consistent with prior work showing that coupons may increase spending by consumers (Milkman and Beshears, 2009); however, I show that it not only increases spending but may also lead to suboptimal decisions, that is, it leads to lower consumption utility.

participants passing both comprehension question on the first try, participants had to pass the comprehension questions to proceed. As such, they are capable of doing the correct earnings calculations. One could imagine that participants who passed the comprehension questions on their first attempt are a “smarter” cohort. As a robustness check, I present results for only participants who passed the comprehension questions on the first try in the Appendix (see Figure A.2.2 and Table A.2.1). Results are robust to this subset of “smarter” participants.[68] Importantly, participants’ purchase rate of the *coupon-trap* good responds to both changes in the extra earnings and to extra discount gained as predicted by the transaction utility model.

Moreover, *Study 2B: With Earnings Displayed* actually does the required math for participants by displaying potential earnings prior to making their decisions. However, even in this game participants still sacrifice higher earnings for a larger discount 15-20% of the time and results on the slope of *coupon-trap* good purchase with respect to discount are marginally significant when isolating to decisions where the earnings lost is $1.

1.4.6.2. The role of anchoring

As stated in Section 1.3.6.3, judgment uncertainty is removed or minimized by assigning the buyer’s value of the two products, so anchoring effects should be minimal. However, if transaction utility exists, then individuals may anchor to the $5 value of the coupon as the target amount of transaction utility to achieve. In a companion paper (Huang, 2018), I provide suggestive evidence that coupon values may act as targets, increasing the weight placed on transaction utility.

1.4.6.3. The role of experimenter demand

As in Study 1, one might worry about experimenter demand driving these results. Again, I find this is not the case. First, I again find that there is no difference in transaction utility

[68] While there is a level change across all six purchasing decisions, effects due to changes in earnings and perceived discounts if a consumer purchases the *coupon-trap* good persist.

effects between rookie and veteran lab experiment participants.[69]

Moreover, in the spirit of de Quidt et al. (2018), *Study 2B: With Earnings Displayed*, where participants are displayed their potential earnings prior to making their decision, could again be thought of as producing experimenter demand for participants to place more value on their earnings. In *Study 2B: With Earnings Displayed*, while the slope of extra discount on the purchase rate of the *coupon-trap* good is only directionally consistent, participants are still choosing to purchase the *coupon-trap* good 15-20% of the time (see Figure 1.5 Panel B).[70]

1.4.7. Summary of Study 2

Results from the *Coupon Game* provide further evidence that consumers care about the terms of the deal, it can distort purchasing behavior, and it can induce the purchase of products that lead to lower consumption utility. I again argue that alternative explanations are not sufficient to explain the experimental results. Moreover, quantifying participant's marginal rate of substitution between consumption and transaction utility (i.e., their willingness to pay to gain a dollar worth of perceived discount) suggests that actual monetary losses due to transaction utility are non-trivial.[71]

1.5. General Discussion

Do consumers place value on the perceived quality of a "deal" and, if so, can it lead them to sacrifice consumption utility? This paper provides evidence that the answer is yes. Two incentive-compatible laboratory experiments, mirroring the shopping experience with discounts and mark-ups (Study 1) and coupons (Study 2), demonstrate that transaction utility can shift demand and lead to perverse changes in consumption decisions. I find that consumers gain utility from discounts and disutility from mark-ups, following past

[69]See Table A.2.4 regression (5) in the Appendix.

[70]Furthermore, focusing on only the smarter cohort of participants who passed the comprehension check on the first try in *Study 2B: With Earnings Displayed*, I find that participants are 9.4 percentage points more likely to buy the *coupon-trap* good when the extra perceived discount is $4.00 compared to $0.50 and this is a marginally significant (see Table A.2.1 regression (7) and (8)).

[71]See Appendix A.2 for additional results on monotonicity.

research on "transaction utility" (Thaler, 1985, 1999, 2008). Thus, individuals may choose to purchase a discounted, inferior good, or refuse to purchase a marked-up, superior good, to gain transaction utility or avoid transaction disutility. Indeed, my estimates suggest the marginal rate of substitution between earnings and transaction utility is 57 to 78 cents.[72] Moreover, these estimates suggest that a dollar change in the reference price (i.e., the original price) can change consumer's willingness to pay for a product by 36 to 44 cents. These estimates suggest that transaction utility is highly relevant across a wide array of consumer decisions and purchasing behaviors.

Importantly, results from both experiments were not driven by product quality inference. If consumers are inferring quality from an original price, then updating of beliefs may play a role in explaining distortions. However this is not possible for my study participants because they are making decisions over virtual products with assigned valuations. Moreover, participants had to successfully answer two comprehension questions to proceed to each game, thus they had the ability to do the math. Finally, effects persisted to varying degrees even after displaying potential earnings before participants made their decision. This suggests results were driven by transaction utility.

Future research should further explore the effects of attention and prior beliefs on transaction utility. Attention to "irrelevant" original prices may depend on the transaction context and consumers' prior experience.[73] Well-informed consumers may respond less to "irrelevant" original prices or consumers may respond more to "irrelevant" original prices for rarely purchased goods (Bagwell and Riordan, 1991; Armstrong and Chen, 2013). For example, consumers rarely buy mattresses or rugs. Thus perceived sales from "irrelevant" original prices may be more believable and effective for these products. On the other hand, consumers may be less likely to be fooled by "irrelevant" original prices for products they

[72] Consumers are willing to pay 57 cents to gain a dollar of perceived discount and 78 cents to avoid a dollar of perceived mark-up.

[73] Prior models have theorized the effect of references prices on consumers' demand for a product using signaling (Bagwell and Riordan, 1991; Armstrong and Chen, 2013), attention (Gabaix et al., 2006; Koszegi and Szeidl, 2013), and bargain-hunting (Armstrong and Chen, 2013). These models suggest attention to prior prices and experience could mediate transaction utility effects.

purchase everyday.[74] When faced with "irrelevant" original prices, consumers may find some perceived discounts and mark-ups more believable than others, based on prior experience and recall. Attention to "irrelevant" original prices may produce stronger responses from less-informed and inexperienced consumers than more-informed and experienced ones.

Moreover, distortions of purchasing behavior due to transaction utility produced by "irrelevant" original prices may only last as long as the "irrelevant" original prices are believable. Extremely high discounts and consistent sales may lead consumers to believe that the transaction utility gained is "fake" and thus diminish its utility. Further research is needed to estimate consumers' sensitivity to consistent sales as well as the credibility of "fictitious" original prices. Additional research is also needed to compare different sales tactics and explore how they perform relative to one another. Finally, future research should also focus on understanding the "half-life" of transaction utility. How long-lasting is the experience of getting a good deal? What is the depreciation rate of transaction utility? Understanding how permanent transaction utility is could have important implications for consumer's welfare and long-term consumer policy and firm pricing regulations.

In general, understanding how transaction utility can affect demand is of great importance for firm pricing strategy as virtually every retail firm engages in some form of discount or sales tactic. For example, years after JCPenney's pricing debacle, the company's sales have yet to recover fully, suggesting transaction utility could play an important role in short and long-term firm pricing strategy (Mourdoukoutas, 2017).

Policymakers and regulators will also be interested in how transaction utility can be manipulated to exploit consumers. For example, research has shown an increase in the use of inflated "fictitious" original prices by retailers. Some of the most egregious pricing practices had items offered "on sale" more than 75% of the time.[75] Evidence from a large set of

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[74] Although some consumers may be less likely to remember prior selling prices (Dickson and Sawyer, 1990).

[75] Beginning March 2017, Consumer's Checkbook tracked the prices offered by 19 national chains for 20 big-ticket items at each store for 44 weeks. They found the use of inflated original prices and discounts more widespread compared to similar research performed in 2014 and 2015. See Brasler (2018). Con-

class action lawsuits alleging that firms use "fictitious" original prices to create perception of discounts and trick consumers into purchasing suggests firms are exploiting consumers using transaction utility.

Finally, from a litigation perspective, evidence of consumers being deceived and suffering economic harm (i.e., welfare loss) due to "fictitious" original prices is a necessary condition in court rulings. However, typical rulings favor the advertisers and sellers because assessing material damages associated with fictitious pricing is difficult (Friedman, 2016). If firms are using fake prices to trick consumers into purchasing then transaction utility achieved is not only a non-pecuniary, intangible gain, but it but could also be considered as "fake transaction utility." Showing evidence that "fictitious" original prices distort consumer behavior and lead to material losses is pertinent to such lawsuits. Moreover, quantifying participant's willingness to pay to gain a dollar of perceived discount or avoid a dollar of perceived mark-up provides a benchmark for economic harm.

sumer's Checkbook is an independent, non-profit consumer organization founded in 1974 to provide survey information to consumers about vendors and service providers (see http://www.checkbook.org).

CHAPTER 2 : The Myth of the Male Negotiator: Gender's Effect on Negotiation Strategies and Outcomes

2.1. Introduction

Conventional wisdom holds that men outperform women in negotiations. However, this popular tale has surprisingly little empirical support. We know that women negotiate *less* than men (Leibbrandt and List, 2015; Small et al., 2007; Exley et al., 2016). We also know that outside observers rate their performance worse (Bowles et al., 2007; Tinsley et al., 2009; Bowles, 2012; Bowles and Babcock, 2013; Amanatullah and Tinsley, 2013). But do they actually get less at the negotiating table?

Apparent evidence for this gender gap in negotiation performance actually comes from either one-shot bargaining games (i.e., ultimatum or dictator games) that explicitly do not have a communication feature (Eckel and Grossman, 2001; Solnick, 2001; Sutter et al., 2009; Ridgon, 2012; Demiral and Mollerstrom, 2017; Eckel et al., 2008) or alternating bargaining with numeric offers only (Dittrich et al., 2012; Andersen et al., 2015; Hernandez-Arenaz and Iriberri, 2016, 2018). If verbal communication is the essence of negotiation in practice, we lack incentivized evidence of relative performance in this crucial domain.[1]

In this paper, we fill this gap by measuring relative outcomes in an incentivized verbal negotiation. We might expect that the presence of verbal communication would exacerbate the differences found in the no-communication bargaining literature, since men might exploit women's gender to target them with more aggressive communication strategies, mirroring the finding in non-communication games where men play more "hawkishly" toward female partners, anticipating a more "dovish" response (Eckel and Grossman, 2001; Holm, 2000; Ben-Ner et al., 2004; Houser and Schunk, 2009).[2] What we find is precisely the opposite.

[1]There is evidence from other fields that examines performance in scenarios with no monetary incentives, such as classroom negotiation exercises (Bowles et al., 2005; Kray et al., 2002, 2001; Walters et al., 1998; Stuhlmacher and Walters, 1999; Mazei et al., 2015).

[2]Moreover, the fact that women have been shown to be more generous, community-minded, and inequality-averse in experimental games (Bolton and Katok, 1995; Eckel and Grossman, 1998; Andreoni

Relative to a control game where we replicate the literature's finding of a male advantage, we find that the introduction of verbal communication disadvantages men, leading to lower payoffs.

Furthermore, by randomly assigning whether gender information is revealed or not, we show that this wedge between the negotiation game and control game is created by the use of gender information to tailor one's strategies. In the non-communication game, men optimally exploit gender information to be more hawkish toward female partners and less so toward male partners, exactly as the literature would suggest. In the negotiation game, however, strategy tailoring is *inverted*. By analyzing the natural language data created by the chat negotiations, we find that while there are no significant gender differences in communication strategy in the absence of gender information, with gender information, men tailor their communication strategies in the negotiation game in exactly the opposite direction one might expect based on the one-shot bargaining literature. Men are starkly more aggressive toward men than women. For example, men issue ultimatums to male partners 121% more frequently than toward female partners.

This behavior is not explained by a difference in optimal strategies between the negotiation and the non-communication game. In fact, payoffs show that men's use of aggressive and yielding strategies appear mis-paired with whom they are most effective against. The use of tough strategies, used much more with men, reduces payoffs against male, but not female, partners. Similarly, yielding strategies, used far more frequently against women, increases payoffs against male, but not female partners.

We thus posit that complying with social norms or non-pecuniary values may explain this paradoxical result. Men may feel constrained in their use of aggressive strategies toward women, or, in other words, derive negative utility from using aggressive communication toward women. Similarly, they may derive some social or other non-pecuniary benefit from

and Vesterlund, 2001; Heinz et al., 2012; Croson and Gneezy, 2009) provides further evidence that targeting them with more hawkish behavior could pay off.

using such aggressive communication against men, even when it reduces their monetary payoff.

This latter result could be referred to as a manifestation of "toxic masculinity." In the non-communication game, men curtail their naturally more aggressive play when they know they are facing a male partner. However, in the negotiation game, men persist in their use of aggressive strategies toward men, despite it increasing negotiation breakdown. This leads to a startling result: Male-male pairs are the least efficient in the negotiation game, earning \$15.60 in joint payoffs compared to over \$18 for other pair-types (and a possible \$20 at a fully efficient equilibrium). That is, adding a woman to the negotiation increases joint payoffs, by around \$3 out of \$20.

Our experiment shows that situations with communication may be fundamentally different than games with no interaction, and thus there are limits in the external validity of bargaining games to what we think of as "negotiation" in the business or legal world. In doing so, we poke holes in the myth of the "great" male negotiator. We show that, in our setting, women perform equally well as men in the negotiation game with no gender information, while introducing gender information, men perform worse in the negotiation game relative to the control game. Moreover, having women at the negotiating table increases efficiency of negotiations.

We additionally provide a methodological contribution to the literature, providing an incentivized game that can be used to easily measure outcomes in verbal negotiations without confounding from overly complex negotiation scenarios. Our negotiation scenario can also be easily implemented as a non-communication game, allowing comparability across communication settings. also be outcomes to settings without communication. Lastly, we design a neutral framing negotiation game which mirrors the payoffs from Battle of the Sexes, creating an incentive compatible (and replicable) game as well as.

The remainder of the paper proceeds as follows: Section 2.2 presents the experimental design, Section 2.3 shows the impact of gender information in settings with and without communication based on our quantitative and qualitative data, Section 2.4 provides plausible mechanisms, and Section 2.5 concludes.

2.2. Experimental Design and Hypothesis

Our experiment investigates the role of gender in negotiations using a neutral frame and incentive compatible negotiation game.[3] Participants are matched in pairs in a given round and negotiate how to divide \$20. Each participant could choose either \$15 for themselves or \$5 for themselves. If they agree, meaning one chooses \$15 while one chooses \$5, the split is implemented and participants receive their respective shares as earnings. If they fail to agree, that is, both choose \$15 or \$5, then they both gets \$0. Notice, these payoffs mirror the payoffs from a standard Battle of the Sexes game.

		Participant 2	
		A	B
Participant 1	A	(15, 5)	(0,0)
	B	(0, 0)	(5, 15)

The experimental design varies the availability of verbal communication and partner gender information. In the negotiation game, negotiating partners were allowed to communicate via unstructured chat for two and a half minutes. After the expiration of the chat period, participants simultaneously made their choices without further communication. All participants also played a control game without communication, this allows us to separately identify the effect of verbal communication and compare it to outcomes in one-shot bargaining literature. In the control game, participants play the same game but simply make their choice simultaneously with their partner.

We also randomized whether participants were informed or not of their partner's gender information (at the session level).[4] In order inform participants of their partner's gender

[3]The experiment was conducted using z-Tree (Fischbacher, 2007).
[4]Under the informed condition, gender information is public.

without making it overly salient (and cuing this as a gender study), *all* negotiating pairs were shown a partner information sheet with five plausibly relevant, but actually substantively meaningless, partner characteristics prior to making their choices.[5] When gender information was public, an additional line containing their partner's gender was simply inserted as the first characteristic (see Appendix Figure A.6.3).

In total, we have four conditions: (1) an informed negotiation game, (2) a uninformed negotiation game, (3) an informed control (non-communication) game, and (4) a uninformed control (non-communication) game.[6]

The informed negotiation game can be thought of as the closest stand-in for real negotiations, since, in practice, individuals rarely negotiate without actual communication or are able to "hide" their gender. While the control game serves as a baseline on behavior that mirrors the literature, the uninformed negotiation game serves to isolate the role of gender information in creating the dynamics observed.

The full experiment proceeded as follows. Subjects played a total of eight rounds with their partner randomly assigned in each round. First, subjects played four rounds of the control game, followed by four rounds of the negotiation game. After all eight rounds, one round is randomly selected and subjects received their earnings from that round (in addition to a show-up fee). No information about the outcomes of each round was revealed until the end to limit learning effects.[7]

In the control game, there are two pure strategy equilibria and a mixed strategy equilibrium. In the pure strategy equilibria, either Participant 1 chooses \$15 for themselves and Partici-

[5] The five characteristics revealed in the partner information sheet were: if their partner (1) is left- or right-handed; (2) is an only child; (3) their month of birth; (4) could roll their tongue; and (5) had hitchhiker thumbs. This partner information sheet was shown to subjects for 15 seconds before proceeding to the "choice" window, and was also displayed on the "choice" window. This information was designed to seem potentially relevant, and plausibly related to some other research objectives, but highly unlikely to actually influence what a person would choose in the game, and thus have little effect on strategic behavior.

[6] See Appendix A.6 for experimental protocol.

[7] Prior to the eight game rounds, subjects played two practice rounds of the control game with the same payoffs against a computer to understand the game, this also minimizes in-game learning (and we control for order effects). At the end of the study, subjects also answered a post-survey.

pant 2 chooses \$5 for themselves or Participant 1 chooses \$5 for themselves and Participant 2 chooses \$15 for themselves, leading to those payoffs, respectively. In the mixed strategy equilibrium, each participant chooses \$15 for themselves (i.e., their preferred choice) 75% of the time, leading to an expected payoff of \$3.75, which is a lower payoff than one would achieve choosing randomly.

Like a typical negotiation, payoffs in our game are set up such that both participants prefer an agreement to their outside option of \$0, but there is disagreement over whom the agreement favors (that is, who will choose \$15 for themselves). Moreover, there is no strong theoretical predictions for which one of the pure strategy equilibria will be selected, and thus there is scope for this ultimate outcome to depend on the effectiveness of each party's communication.

Moreover, gender information can distort these predictions. We know from prior literature that in the absence of communication (i.e, the control game) partner gender can act as a coordination device with men choosing \$15 for themselves more often (and women choosing \$5) leading to higher coordination in mixed gender pairs and men reaping the benefits of this increased coordination. However it is unclear how the role of gender information will impact payoffs in the negotiation game.

2.3. Results

A total of 232 subjects participated in the experiment yielding over 1800 observations.[8] All individuals participated in both the control and negotiation games, thus the subject pool is identical. Subjects in the informed and uninformed conditions are balanced on all characteristics with the exception of being a US citizen.[9]

[8] Participants are students from the University of Pennsylvania across a wide range of disciplines. They participated in 1 of 21 sessions at the Wharton Behavioral Lab in October 2016. We restricted only an equal number of women and men to play the game, in order to have sufficient observations for male-female pairs. If there were additional women or men in the session, these extra subjects were diverted to a separate game, and excluded from our sample. The WBL subject pool skews female, and thus these exclusions were entirely female (and randomly selected). We exclude data from three sessions that had only one male participant.

[9] See Appendix Table A.3.1. Our results are robust to controlling for a number of individual controls, including being a US citizen, and session controls.

2.3.1. Negotiation Payoffs by Gender

Our principal outcome is the payoff in the negotiation game—the number of dollars a player ends each game with, whether \$0, \$5, or \$15. We want to analyze the impact of gender and the impact of verbal communication on this outcome. We thus run the following regression specification:

$$Payoff_i = \beta_0 + \beta_1\, male_i \times negotiation_i + \beta_2\, male_i + \beta_3\, negotiation_i + \beta_4 X_i + \epsilon_i$$

where $negotiation_i$ represents whether the payoff is from the communication game or the control game and $male_i$ reflects whether the subject is male, and X_i reflects controls for session timing, round order, and subject characteristics, added in even columns (odd columns are raw data). We cluster standard errors at the individual level, since individuals play multiple games.

Table 2.1, regressions (1) and (2), shows the results of this specification on the pooled informed and uninformed treatments. These results show that men perform worse relative to women in the negotiation game compared to the control game. Regression (1) shows that men earn \$0.88 more than women in the control game, but in the negotiation game, this effect is more than reversed: their relative payoff is reduced by \$1.25. Thus, the presence of verbal communication disadvantages men.

Figure 2.1 shows the breakdown of the payoffs for men and women that underlay these results, splitting them into the game with gender information and without. This breakdown shows that this inversion of results between the communication and control games appears in the treatment where gender information is provided.

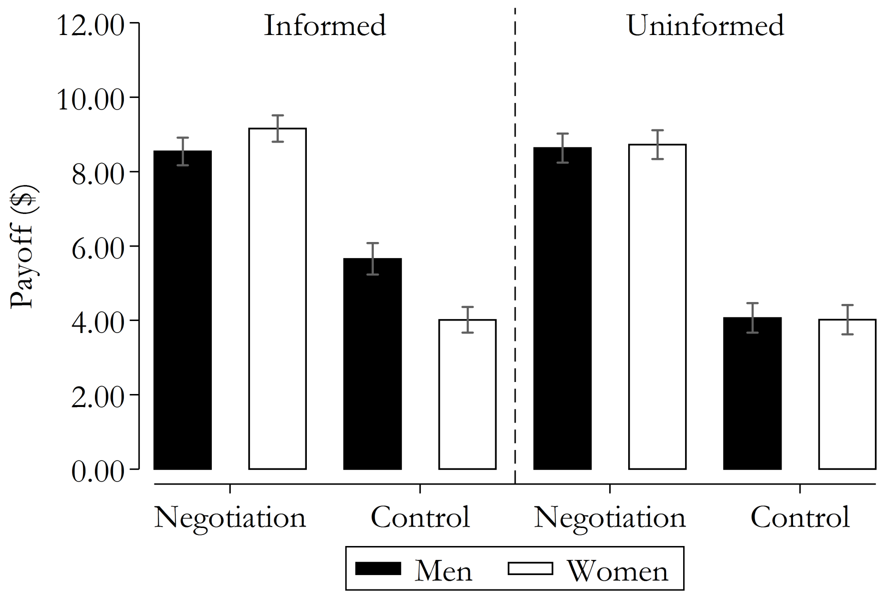
When informed of gender, men outperform in the control game, but (more than) lose this advantage in the negotiation game. Payoffs are naturally higher in the game with negotiation, as coordination is increased versus the one-shot game, but this reverses, rather

Table 2.1: Payoff by Treatment and Gender

	Dependent variable: Payoff							
	All		Informed		Uninformed		All	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Male	0.884**	1.080***	1.639***	1.964***	0.0455	0.177	0.0455	0.166
	(0.397)	(0.396)	(0.561)	(0.531)	(0.542)	(0.539)	(0.541)	(0.555)
Male × Negotiation	-1.250**	-1.304**	-2.254***	-2.361***	-0.136	-0.136	-0.136	-0.136
	(0.527)	(0.527)	(0.697)	(0.697)	(0.784)	(0.789)	(0.783)	(0.785)
Male × Negotiation × Informed							-2.118**	-2.224**
							(1.047)	(1.048)
Male × Informed							1.594**	1.740**
							(0.779)	(0.772)
Chat × Informed							0.439	0.545
							(0.681)	(0.680)
Informed							-0.00633	0.0676
							(0.519)	(0.561)
Negotiation	4.935***	5.271***	5.143***	5.655***	4.705***	4.850***	4.705***	4.986***
	(0.340)	(0.614)	(0.466)	(0.877)	(0.498)	(0.860)	(0.497)	(0.695)
Constant	4.019***	5.769***	4.016***	5.362***	4.023***	6.533***	4.023***	5.305***
	(0.260)	(1.156)	(0.373)	(1.559)	(0.363)	(1.781)	(0.362)	(1.235)
Ind. Clusters	232	231	122	121	110	110	232	231
Controls		YES		YES		YES		YES
Observations	1856	1848	976	968	880	880	1856	1848
R-Squared	0.122	0.131	0.114	0.138	0.138	0.144	0.127	0.136

Notes: Robust standard errors clustered at the individual level are in parentheses. Session controls include day of the week, within day trend, and game round. Individual controls include subject's age, being nonwhite, begin politically liberal, being a US citizen, being a native English speaker, employment status, and the number of sessions completed. Significance: *** p<0.01, ** p<0.05, * p<0.1

Figure 2.1: Payoff by Treatment and Gender



Notes: Average payoff by treatment and gender. Standard error bars are shown around each mean.

than widens, the gender gap in payoffs. Meanwhile, when uninformed, men and women perform equally well in both games. This suggests that differences in payoff between men and women are not driven by the setup of the game but rather the availability of partner gender information. Moreover, the role of gender information results in inverse payoff patterns in the presence of verbal communication.

Regressions (3) and (4) show results under the informed treatment whereas regressions (5) and (6) restrict to the uninformed treatment. This analysis demonstrates that the effect is entirely driven by the public gender information setting, which is the setting. When informed of gender, men outperform women in the non-communication (control) game, earning on average \$1.64 more (a substantial effect when average payoffs are around \$4). But this advantage is more than reversed in the negotiation game, with men earning \$2.25 less in the negotiation game relative to the control game. Meanwhile, the uninformed treatment shows shows that in both games men have no inherent edge over women—both genders perform equally well in terms of payoff.

Regressions (7) and (8) uses all the data and adds interactions between the two games and information conditions, to confirm that the difference between the informed and uninformed treatments is statistically significant. Indeed, communication does not impact payoffs for men when partner gender is unknown, while significantly decreasing payoffs when gender is known. This interaction suggests that, contrary to prior results in one-shot bargaining games, verbal communication in a negotiation significantly reduces the value of gender information to male participants. While men take advantage of gender information to increase payoffs in the control game, gender information does not provide an advantage in the negotiation game. This demonstrates that non-communication bargaining games are limited in their external validity as a proxy for "real-world" negotiations, which involve direct communication.[10]

2.3.2. Tailoring by Partner Gender

In this section, we explore the drivers of men's apparent negotiation disadvantage, and in particular their use of partner gender information to tailor their strategy. We show that men's payoff reversal between the negotiation and the control games is due to men failure to tailor their actions and communication strategies based on partner gender in the negotiation game.

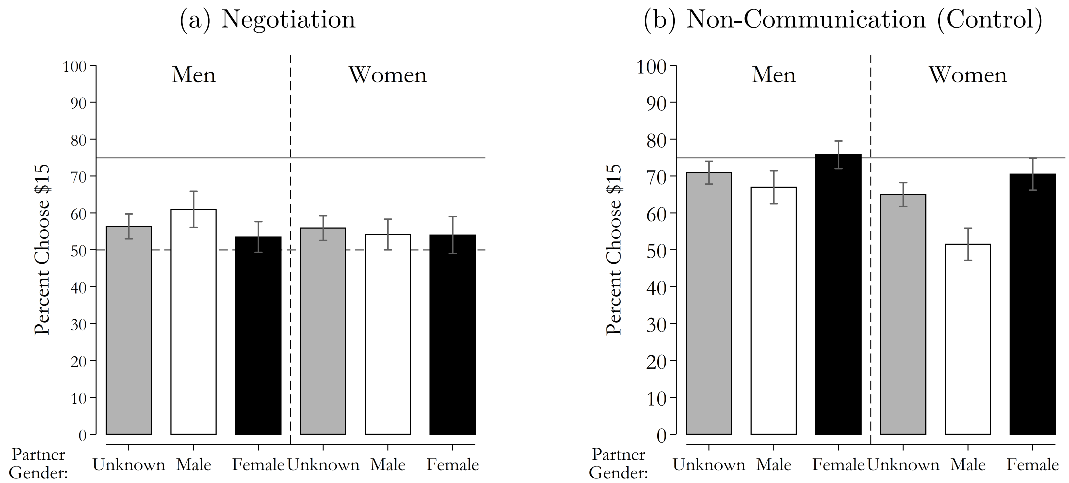
2.3.2.1. Actions by Partner Gender

First we explore men's probability of choosing $15 for themselves (i.e. their preferred choice) in both games with and without gender information. As shown in Figure 2.2, in the informed control game, both men and women choose $15 for themselves less often against male compared to female partners. The tailoring by men is optimal from a payoff perspective, because women choose $15 less often against male partners thus leading to

[10] Appendix Figure A.3.1 shows the average joint payoffs received by different gender-pair types in each treatment. In the informed control game, men do better both against other male and female partners. In particular, we observe gender information increasing coordination (and thus payoffs) with men in mixed-gender pairs reaping the benefits and obtaining higher payoffs compared to their female partners. However, in the informed negotiation game, men do not have an advantage against women, that is, we do not observe the unequal split in payoffs in mixed-gendered pairs, and perform worse against male partners.

increased coordination and payoffs.[11]

Figure 2.2: Choose $15 by Treatment and Gender-Pair



Notes: Average rate of choosing $15 for themselves by communication, information condition, and gender pair-type. The gray bars are for subjects who are uninformed of their partner's gender, the white bars are for subjects who are informed that their partner's gender is male, and the black bars are for subjects who are informed that their partner's gender is female. The solid horizontal gray line marks the theoretical mixed strategy equilibrium which is picking $15 for themselves ($5 for their partner) 75 percent of the time. The dashed horizontal gray line marks 50 percent probability which denotes equal split and full coordination. Standard errors bars are shown around each mean.

By contrast, in the informed negotiation game, men's action when gender is known appears to invert from the previously observed optimal tailoring. Men choose $15 for themselves *less* often against female compared to male partners. These effects are absent when gender is unknown, indicating that men's negotiation disadvantage is driven by their inability to capitalize on gender information and tailor their behavior optimally.

2.3.2.2. Communication Strategy by Partner Gender

Our experiment yielded 464 negotiation conversations giving us a rich dataset to further understand how the role of gender information affects the *way* men and women communicate. Below, we first describe our textual analysis methodology and then present our results.

[11]See Appendix Table A.3.2 for results in a regression framework.

Negotiation transcript coding

To analyze the negotiation transcripts, we used 310 Amazon Mechanical Turk (MTurk) workers to classify chat transcripts according to definitions we provided.[12] MTurk workers were blind to the gender of participants, whether participants were informed of their partner's gender, and the overall objective of the study. On average, five different MTurk workers classified each negotiation transcript. We use the average score given by the MTurk workers for each communication measure in each negotiation.

We defined two specific strategies used and two negotiation styles as our key communication measures.[13] On one side of the communication strategy spectrum, our two key "aggressive" communication measures are *hard commitment* (a strategy) and *tough talker* (a style). We defined *hard commitment* as one negotiating party intransigently insisting they are choosing \$15, and refusing to entertain any discussion to the contrary.[14] If credible, this makes the other party's best response to choose \$5, or face mismatch and thus \$0, essentially turning two-way communication into one-way communication.[15] Of course, *hard commitment* is not always effective. It can be met with countervailing "commitment" from the other partner, or may destroy goodwill in the negotiation and result in mismatch. A *tough talker* was defined as a "pushy" or "mean" negotiator that fights for the \$15 and actively tries to convince the other person to choose \$5, but does not necessarily lay out the "all or nothing" ultimatum implied by *hard commitment*.[16]

[12]See Appendix A.7 for negotiation transcript coding protocol.

[13]We defined several other metrics to examine the mechanisms behind paradoxical gender tailoring and the robustness of our results. Usage rates for these secondary measures can be found in Appendix Table A.3.7.

[14]Some participants described this trade-off explicitly to their partners, saying, "I'm choosing 15 no matter what. So if you want anything you only have one option."

[15]Previous work on coordination games has shown that while one-way communication can be very effective, two-way communication can sometimes fail to resolve the issue, and becomes, in a sense, no communication. In the presence of one-way communication, if one side communicates their move, the other side has a clear best response to choose the coordinating move. However, with two-way communication, a tussle can develop over who receives their preferred outcome (Cooper et al., 1989).

[16]For specific definition given to MTurk workers see Appendix A.7. We defined several other metrics to examine the mechanisms behind paradoxical gender tailoring and the robustness of our results. Usage rates for these secondary measures can be found in Appendix Table A.3.7.

On the other side of the communication strategy spectrum, our two key "yielding" communication measures are *offer $15* (strategy) and *friendly negotiator* (style). We defined *offer $15* as individuals who outright offer that their negotiating partner can take the higher payoff of $15, thus guaranteeing coordination but yielding a lower payoff than the average in the session. A *friendly negotiator* is a negotiator who is trying to build up-front rapport, and acts friendly towards their negotiating partner."

Paradoxical tailoring of communication strategies

We demonstrate that the paradoxical tailoring of men's actions in the negotiation game is an extension of their gendered communication strategy within the negotiation itself.

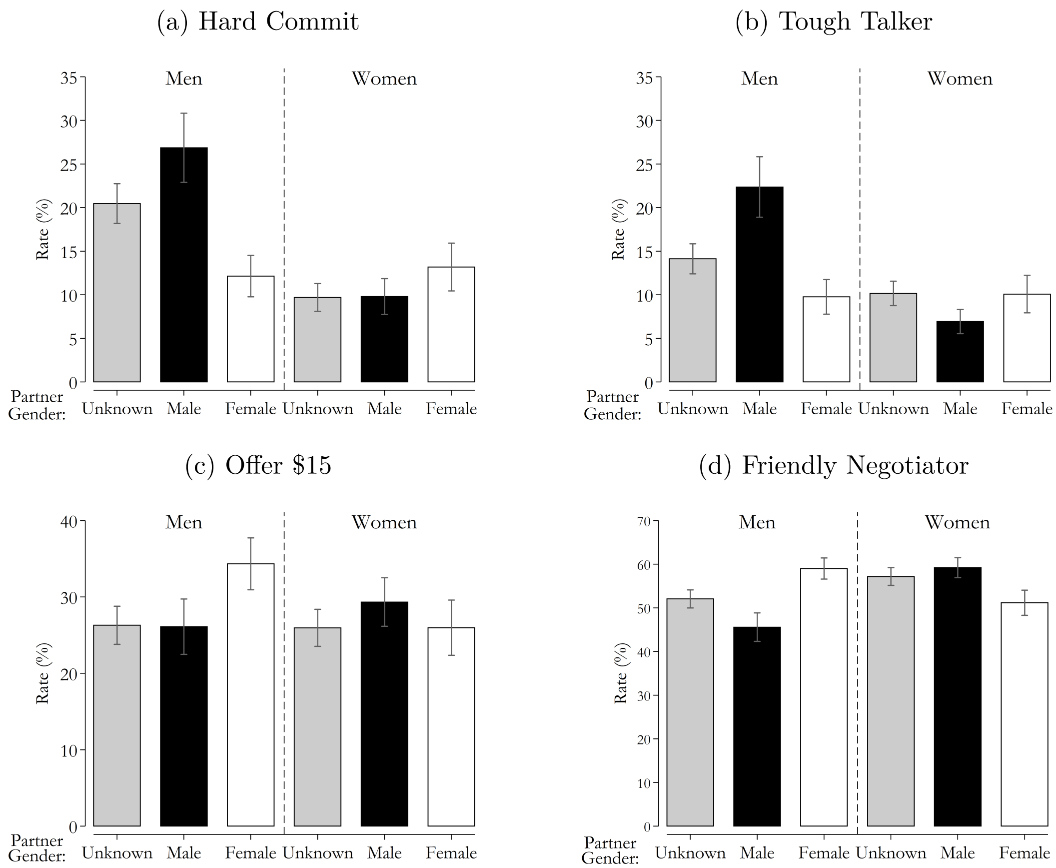
Based on both the literature and the non-communication game, we might expect men to be more "hawkish" in their communication toward women, expecting a more "dovish" response, just as their approach to the control game appears to anticipate less aggressive action by women.

However, we find that men's tailoring of their communication strategies goes in the opposite direction. Figure 2.3 presents the average rate men and women used our four key communication measures by partner gender and show that men are substantially less likely to use aggressive communication strategies against female compared to male partners. Likewise, we find that men are more likely to use yielding communication strategies against female compared to male partners.

Figure 2.3 Panel A shows that men are 14.7 percentage points less likely to use *hard commitment* against known female compared to male partners, that is, men are 121% more likely to use *hard commitment* against known male partners compared to known female partners. Similarly, Figure 2.3 Panel B shows that men are 12.6 percentage points less likely to be *tough talkers* towards female compared to male partners, that is, 129% more likely to be a *tough talker* against known male partners.[17]

[17] Appendix Table A.3.3 Panel A shows that results are statistically significant at the 1% level for *hard commitment* and *tough talker*. Men's tailoring in response to gender information is so strong that it eliminates

Figure 2.3: Aggressive and Yielding Communication Strategies
by Treatment and Gender-Pair



Notes: Average rate that men and women use aggressive and yielding communication measures by information condition and gender pair-types. The gray bars are for subjects who are uninformed of their partner's gender, the white bars are for subjects who are informed that their partner's gender is male, and the black bars are for subjects who are informed that their partner's gender is female. Standard errors bars are shown around each mean.

Figure 2.3 Panel C shows that men are directionally more likely to *offer $15* to a known female compared to male partner. Similarly, Figure 2.3 Panel D shows that men are 13.4 percentage points more likely to be *friendly negotiators* against known female partners compared to male partners (that is, a 22% increase).[18]

One might worry that this behavior is not reflective of men's responsiveness to gender information, but rather a joint product of men's and women's behavior in the negotiation. We show this is not the case, focusing on participants in the uninformed treatment we find there is no significant tailoring of men's strategies (see Appendix Table A.3.3 Panel B). Moreover, we show that men's use of *hard commitment*, being a *tough talker*, and being a *friendly negotiator* toward women is significantly different when informed versus uninformed.[19] In other words, men only decreased their use of aggressive strategies and increased their use of yielding strategies toward women when informed of their partner's gender. Our results are robust to limiting to those who hard commit as a first action, further ruling out that it is a response to the other player's behavior.[20]

2.4. Potential Explanations

In this section explore potential mechanisms for this paradoxical tailoring. We provide suggestive evidence that tailoring in the negotiation game is suboptimal. We also rule out the possibility of gender-specific altruism. Instead, we provide a third explanation: men may be constrained in their strategy by gendered social norms.

the original gender effect in negotiation behavior—when men play against known female partners, they tailor their communication behavior to the point of behaving like female players. Perhaps for this reason, we find very little tailoring in women's behavior (if anything, it goes in the opposite direction, with women acting more aggressively toward other women), since, to them, male and female partners use aggressive strategies at similar rates. Prior literature has shown that women are more responsive and tailor their negotiation "persistence" more than men (Bowles and Flynn, 2010). However, in our case, male and female negotiators behave very similarly when playing against known female partners, because of men's paradoxical tailoring, thus women may not need to tailor their behavior.

[18] Appendix Table A.3.3 Panel A shows that results are directionally consistent for *offer $15* and statistically significant at the 1% level for *friendly negotiator*. These findings are also supported by a subjective rating of negotiator aggressiveness by MTurk workers, where men are rated as much more aggressive toward other men than women, and a subjective measure of whether an agreement was reached at the end of the negotiation, which is much lower in male-male pairs (shown in Appendix Table A.3.7).

[19] See Appendix Table A.3.4.

[20] See Appendix Table A.3.7.

2.4.1. Optimal Strategy?

One explanation for this behavior is that it is the optimal strategy based on beliefs about who is more receptive to which type of strategy. First, this would run counter to evidence in the literature that women are expected to be more "dovish." Second, in the control game, men in the informed treatment chose to be more aggressive towards female partners (believing they would behave more dovishly).[21] Recall the type of game played was varied *within* subject, thus, this explanation would contradict the beliefs of the same men who chose to be aggressive towards women in the absence of communication (i.e., the control game) and yielding in the presence of explicit verbal communication.

It is possible that the optimal tailoring approach based on gender could be different in a setting with explicit verbal communication.[22] For example, women might respond negatively toward aggressive strategies when they know their partner is male, or it might pay to try to "convince" other men aggressively, because the gain in getting the higher payoff might balance out the loss from lower coordination. Thus we look directly at whether these approaches are optimal given partner responses.

We find stark evidence that the inverse tailoring observed in the negotiation game is not optimal from a payoff perspective. Table 2.2 shows the payoff for men using our four key communication strategies against female compared to male partners. We note that evidence from payoffs based on communication strategy is only suggestive since results are conditional on men using the strategy (and those who use versus do not use a strategy may differ). First, aggressive communication measures appear to perform far better against female versus male partners. Regressions (1) and (2) show that men using *hard commitment* against a male partner decreases payoffs by over \$3, while using it against a female partner directionally increases payoffs. Regression (4) shows directionally consistent evidence that being *tough talkers* against male partners reduces payoffs, whereas this loss is canceled out when using

[21] See Figure 2.2 and Appendix Table A.3.2.

[22] In the negotiation game, there is opportunity for interaction, whereas in the control game (without communication) there is just one shot to try to match responses.

it against female partners. This suggests that, when using aggressive negotiation strategies against male partners, the benefit of "convincing" the other party more often does not appear to outweighs the negative impacts of negotiation breakdown.

Not only do male negotiators appear to not tailor their aggressive strategies optimally to their partner's gender, but male-male pairs leave significant value on the table due to negotiation breakdown. Appendix Figure A.3.1 shows that male-male pairs are the least efficient in the negotiation game when gender is known. In fact, male-male pairs' joint payoff is only \$15.60 (out of a possible \$20), and compared to an average of over \$18 for other pair types.[23] This combined with evidence that men are more aggressive against those players who are less likely to be receptive (i.e., other men) and that their payoff goes down when gender information is introduced, suggests men are failing to maximize their possible monetary payoff.

Table 2.2: Payoff by Aggressive and Yielding Communication Strategies and Partner Gender
(Informed Men Only)

	Dependent variable: Payoff							
	Aggressive Strategies				Yielding Strategies			
Communication Measure:	Hard Commitment		Tough Talker		Offer \$15		Friendly Negotiator	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Strategy	-3.029*	-3.384**	-3.073	-3.553*	-3.791***	-3.309**	3.912*	4.499**
	(1.813)	(1.659)	(1.886)	(1.855)	(1.199)	(1.283)	(2.006)	(1.931)
Strategy × Partner Female	4.753*	5.272*	2.818	3.266	-3.219**	-3.678**	-5.268**	-5.998**
	(2.692)	(2.916)	(2.604)	(2.933)	(1.506)	(1.577)	(2.401)	(2.415)
Partner Female	0.239	0.185	0.600	0.567	2.680***	2.868***	3.847**	4.218***
	(0.754)	(0.797)	(0.803)	(0.844)	(0.988)	(1.006)	(1.521)	(1.507)
Constant	8.614***	7.975**	8.487***	7.720**	8.790***	8.314**	6.017***	4.251
	(0.642)	(3.491)	(0.636)	(3.526)	(0.827)	(3.475)	(1.191)	(3.477)
Ind. Cluster	61	61	61	61	61	61	61	61
Controls		YES		YES		YES		YES
Observations	244	244	244	244	244	244	244	244
R-Squared	0.033	0.072	0.025	0.064	0.176	0.195	0.034	0.074

Notes: Robust standard errors clustered at the individual level are in parenthesis. Session controls include day of the week, within day trend, and game round. Individual controls include subject's age, being nonwhite, begin liberal, being a US citizen, being a native English speaker, employment status, and the number of sessions completed. Significance: *** p<0.01, ** p<0.05, * p<0.1

[23] On its own, this is not clear evidence that male players are mis-optimizing, since it may be individually rational to be aggressive in order to try to get the higher payoff, even if it means risking negotiation breakdown.

In contrast, we find that yielding communication measures perform significantly better against male partners. Table 2.2 regressions (5) and (6) show that *offering \$15* to one's partner helps reduce negotiation breakdown in male-male pairs. While, naturally, *offering \$15* reduces payoffs against either gender, since it virtually guarantees the below-average \$5 payoff, the negative payoff impact is statistically significantly much greater, by over \$3, when using the strategy against female partners. Regressions (7) and (8) shows that being a *friendly negotiator* actually increases payoffs against male partners. Moreover, the interaction coefficient for using it against female partners more than cancels out this effect. That is, against male partners, something as simple as opening with a friendly greeting is correlated with higher payoffs by almost \$4, showing just how sub-optimal overly aggressive communication can be.

Moreover, we find that the presence of women in the negotiation significantly improves efficiency. Table A.3.5 compared the joint payoff in the negotiation game by gender-pair types with at least one female negotiator (i.e., male-female and female-female pairs). Regressions (1) and (2) use all negotiation game data (with and without gender information) and shows that having at least one women leads ot \$1.92 increase in joint payoff. Regressions (3) and (4) show this effect is primarily driven by the informed treatment where negotiating pairs with at least one women earn \$2.65 more compared to male-male pairs (a 17% increase). Regressions (5) and (6) limit the data to uninformed treatment only and shows directionally consistent results, indicating that men's tendency towards more aggressive communication is also associated with lower coordination in this setting. As a result, the two settings are not statistically significantly different from one another (see regressions (7) and (8)). It appears that the presence of gender information exacerbates men's aggressive tendencies toward one another, and the fact that it is associated with decreased payoffs shows this is a response to the partner's gender.

Evidence of communication strategy effectiveness is only suggestive as they may be being used by the most effective people, against the most effective targets. It is possible that such strategies would lose effectiveness if more people employed them. However, the large effect sizes indicate that there should be some marginal people who could benefit, monetarily, from switching strategies. It also seems unlikely that men are making a mistake, and trying to use the best strategy from a purely payoff perspective, since men demonstrate they *are* aware of who is likely to be more or less aggressive in the control game. These findings show that men could likely improve their payoffs by choosing to use less aggressive strategies against male partners, and more aggressive strategies against female partners.

2.4.2. Gender-specific Altruism?

Another possible explanation is that men have a preference for giving female partners the higher payoff. In other words, the negotiation setting created gender-specific altruism. However, this explanation is inconsistent with a number of facts. First, altruistic preferences toward women would not explain the inefficient outcome in male-male pairs, and the apparent "over-use" of aggressive strategies toward men. Secondly, altruism towards women appears inconsistent with men's behavior in the non-communication game, where they are more hawkish toward women.[24]

More compellingly, if men truly had altruistic preferences, then in the presence of communication, they could easily simply grant the higher payoff to women more often. However, men and women split the payoff approximately equally in the negotiation game with and without gender information (see Appendix Figure A.3.1). If it were only *some* men with altruistic preferences, and this effect was perhaps canceled out by women being worse negotiators when they encounter men without these preferences, then we would expect a less favorable split without gender information, where men could not correctly target their altruism (but

[24] In the control game coordination increases payoffs for both players versus mismatching. Thus, it is possible that even with altruistic preferences toward women, men would want to behave more hawkishly based on their beliefs about women's actions, in order to create more coordination. Moreover, the difference between our results in the negotiation and control game do not come from order effects or learning. We control for game round, and find an insignificant impact of round on behavior (thus the huge switch from the non-communication (control) game to the negotiation game cannot be caused by the negotiation rounds coming later). Our results are also robust to restricting to the first period only, to limit the potential impact of previous rounds on behavior (even though payoffs are not revealed). Results available upon request.

women's inferior negotiating would still be active).

Finally, we find suggestive evidence that men do try to get the higher payoff for themselves against known female partners, but merely through non-confrontational means. Appendix Table A.3.7 shows that men are significantly more likely to mention their previous choices against female versus male partners. This is often used by players as a way to get the higher payoff by appealing to a sense of fairness, saying they got $5 last time, and so should be allowed to take $15 this time.[25] Additionally, men are marginally significantly more likely to claim to be alternating their choice as their strategy against female partners, which could be another way to try to get them to agree to go with the lower payoff.

2.4.3. Constrained by gender norms?

If participants are not optimally responding to partner gender information and have not developed gender-specific altruism, what could explain this behavior? We propose that negotiations (which involve verbal communication) carry with it social norms, which then constrain men's behavior. That is, gender norms might encourage men to be more chivalrous toward women in their communication behavior and show bravado toward other men.

In sections 2.4.1 and 2.4.2 we showed that men indeed *prefer* the higher payoff, even against female partners, but dislike using aggressive communication strategies against female partners, and like using these same strategies against male partners, even to the detriment of their own payoffs. This is consistent with a model of an additional social reward or punishment from complying with gender norms dictating more chivalrous behavior toward women and tough behavior toward men. Here, men might have an aversion to feeling like they are "beating up" on female participants, in violation of gender norms. Similarly, they might enjoy the "pissing match" against other male participants.[26] Complying with these norms comes at a cost: the money left on the table in male-male pairs demonstrates the cost of

[25]Mentioning previous choices is positively correlated with the strategy of asking for $15 directly, and negatively correlated with offering $15 at the outset.

[26]Similar to Niederle and Vesterlund's (2007) finding that men choose to over-compete due to a preference for competition itself.

over-competitiveness, and potentially "toxic masculinity."[27]

This is consistent with a body of literature showing that complying with norms, rules, and social considerations can create utility that might offset losses from non-payoff maximizing behavior. For example, lab participants stopping at (meaningless) "red lights" in a timed game (Kimbrough and Vostroknutov, 2016), participants rejecting low or unfair offers in an ultimatum game (Kahneman et al., 1986a; Thaler, 1988), and participants contributing more to public goods when identified to other players (Kessler et al., 2017).

In our study, the utility obtained from complying with social norms is asymmetric based on partner: chivalry toward women and bravado towards men. This shows that men do not view male and female partners as equivalent, and thus is still a form gender inequality. While here the outcome is perhaps positive (or at least neutral) for women, such "benevolent" sexism has been linked to overall sexist beliefs.[28] Moreover, Huang and Low (2017) shows that norms of "appropriate" behavior toward each gender might be sensitive to role model or leader effects, and could potentially flip to "hostile" sexism under the right conditions.

2.5. General Discussion

In this paper, we developed an incentivized negotiation experiment to study the impact of gender on negotiation strategies and payoffs. We find that situations with communication may be fundamentally different than games without verbal interaction. Relative to a control game with no communication, men do worse compared to women in the same game with communication (i.e., the negotiation game). This result is driven by men sub-optimally tailoring their negotiation strategy in response to partner gender.

We show that men are more aggressive toward male than female partners, using *hard commitments* towards male negotiating partners more than twice as often as they do with female negotiating partners. Similarly, men are more likely to use friendly or yielding strategies

[27]We cannot say for certain if men could do better against women if they did not choose the paradoxical tailoring of their approach.

[28]Chivalry has been described by psychologists as "benevolent sexism," since women are still treated differently due to their gender (Glick and Fiske, 1996).

toward female partners than male partners. We present evidence from payoffs that men's use of aggressive and yielding strategies appear mis-paired with whom they are most effective against, suggesting that the optimal tailoring of strategies has not changed in the negotiation game. Instead, we posit that men are constrained by social norms in their communication strategy, leading them to be more chivalrous to women and tough toward men, at the expense of their own payoff.

While chivalrous behavior may seem innocuous, such gendered behavior might reflect underlying sexist views. Importantly, the chivalry we show is about how women are to be treated (via the types of communication strategies used) but does not appear to reflect an underlying desire to actually give women the higher payoff. Moreover, a social norm predicated on differential treatment by gender might be sensitive to broader societal context. Huang and Low (2017) showed that immediately following the 2016 Presidential election, hostile behavior toward women strikingly increased, perhaps indicating a reduction in the chivalry norm. Future research should examine how exogenously varying expected social behavior can influence gendered interactions in the short and long run.

We also find manifestations of "toxic masculinity" in the negotiation game which can lead to increasing negotiation breakdown. In our study, male-male pairs are the least efficient in the negotiation game. In fact, adding a woman to a negotiation pair increases the pair's efficiency by \$2.65 to \$3.48 (a 17-24% increase). This behavior could have substantial implications outside the lab. For example, toxic masculinity and over-competitiveness in trading markets may lead to over-investments in risky options, something popular press articles linked to the financial crisis.[29]

[29]For example, in response to a journalist's question on women's strength in times of crisis, Christine Lagarde said: "if Lehman Brothers had been Lehman Sisters," today's economic crisis clearly would look quite different." See (Dealbook, 2010; NPR, 2014).

CHAPTER 3 : The Value of Information and the Role of Fairness in Bargaining

3.1. Introduction

Research from the last 30 years suggests an important role of fairness in economic transactions. The Ultimatum Game — which models the last round of a negotiation between a buyer (proposer) and a seller (responder) — has been a workhorse of this literature. Results from Ultimatum Games are far from the equilibrium prediction based on selfish agents, suggesting that agents have powerful fairness concerns. These results helped spawn literature exploring social preferences (Fehr and Schmidt, 1999; Ockenfels and Bolton, 2000) and the 50-50 norm (Andreoni and Bernheim, 2009; Charness and Rabin, 2002). Simultaneously, however, other research argued that fairness concerns were overblown, and the debate persists (Levitt and List, 2007).[1] In this paper, we demonstrate that the extent to which fairness concern will be relevant in economic decisions of interest depends fundamentally on the information setting. We find that fairness concerns are mitigated as agents become less informed and explore the mechanisms for this result.

While the literature from the past three decades has reported results from a variety of tweaks on the original Ultimatum Game, all the research — with fewer than a handful of exceptions — has kept one feature of the original design: both agents have common knowledge about the size of the pie and thus the share of the pie that is going to each agent in the transaction.[2] This focus on a setting with complete information is surprising given that many transactions are made without common knowledge of the size of the pie. For example, sellers rarely (if ever) know a buyer's true willingness to pay for a good and buyers are unlikely to know a seller's cost of providing a good as costs of production are rarely broadcast.

[1] Critiques on the extent to which fairness concerns should be incorporated into theoretical predictions started as early as the first results from Ultimatum Games (Binmore et al., 1985; Neelin et al., 1988; Ochs and Roth, 1989; Prasnikar and Roth, 1992; Güth and van Damme, 1998).

[2] In the full-information environment, variations of the Ultimatum Game include: set fair and unfair splits (Kahneman et al., 1986b), with punishment (Kahneman et al., 1986b), two-stage games (Binmore et al., 1985), differing proposer "property right" (Hoffman and Spitzer, 1982), multi-stage games with various lengths (Neelin et al., 1988) and different random partners (Ochs and Roth, 1989).

How relevant are fairness concerns to outcomes in the (more realistic) environment with less information? What is the role and value of information to either bargaining party? The few examples from the literature that eliminate common knowledge of the size of the pie have either given the buyer full information and restricted the information of the seller (Mitzkewitz and Nagel, 1993; Croson, 1996; Kagel et al., 1996) or eliminated information from both parties (Schmitt, 2004). These papers have generally found evidence of lower offers and higher acceptance rates. Because each paper has a different setting, however, it is not possible to establish the effect of information of each party on the outcome of the Ultimatum Game or to fully explore the mechanisms driving the relationship between information and fairness.

In this study, we introduce a new experimental paradigm that nests the standard full-information Ultimatum Game and allows us to systematically vary the information of each transacting party. We provide experimental results from bargaining games in which neither party, only the buyer, only the seller, or both parties (i.e, the standard Ultimatum Game) have information about the size of the pie. This design allows us to identify the value of information and its effect on the role of fairness in a variety of information environments.

We find that fairness concerns are significantly muted in settings when less information is available. This suggests that the standard full-information Ultimatum Game, which has been the workhorse of this literature and has highlighted the importance of fairness in bargaining, is a special case at the far end of the spectrum on the impact of fairness on bargaining outcomes.

Looking at the last 10 rounds of the bargaining game, we find that the difference in earnings between the buyer and the seller (i.e., a measure of inequity in outcomes that is 0% for 50-50 splits and 100% in the equilibrium prediction of selfish agents) is smallest in the Ultimatum Game (10.22% of the possible surplus).[3] Removing information from the buyer doubles

[3] We document significant evidence of learning over the course of our 30 round game, and so focus on the last 10 rounds for our primary results. Results are very similar when we use all 30 rounds. See Appendix A.4.2.

this measure to 20.14% of the possible surplus. Removing information from the seller also doubles this inequality measure to 20.60% of the possible surplus. Finally, removing information from both the buyer and seller rises it to 24.98% of the possible surplus, inequality that is nearly 150% larger than in the Ultimatum Game.

These large increases in inequality when one or both parties are uninformed arise from both: (1) the buyer (proposer) making less-fair offers and (2) the seller (responder) accepting less-fair offers when less information is available. We find evidence that buyers behave strategically based on reported beliefs that a seller will accept a given offer. That is, when a seller is uninformed, buyers report believing that they are more likely to accept a low offer. Buyers with a high value (i.e., a large pie) respond strategically to their belief by mimicking the offers of buyers with a low value (i.e., small pie). On the other hand, uninformed sellers are more likely to accept a given offer when buyers are uninformed suggesting a role for caring about the intentions of the buyer or for social image concerns of not wanting to accept unfair splits when the buyer is aware of the inequity.

In addition, we find that the value of information for the parties is asymmetric. Sellers earn more when they have information about the buyer's value (and thus the size of the pie): The share of possible surplus going to the seller increases by 10% when the seller is informed compared to uninformed. Buyers, meanwhile, do not gain from additional information, and they do worse with information if the seller is also informed. This suggests that the seller has an incentive to get information, but the buyer has an incentive to remain uninformed. In a setting where information is available and there is common knowledge of information acquisition, the outcome in which the seller is informed and the buyer is not informed is an "absorbing state," which might be particularly likely to arise. This particular information structure has not previously been studied experimentally.

The rest of the paper proceeds as follows: Section 3.2 describes the experimental design. Section 3.3 shows our main results: removing information increases inequality, the two major reasons driving this pattern, and the asymmetric value of information. Finally, Section 3.4

concludes.

3.2. Experimental Design and Hypothesis

In this section, we describe a new experimental paradigm designed to investigate the role of information on behavior and outcomes in a bargaining game. This game allows us to vary what information players have and explore the effect of fairness concerns in different information settings. We also provide the subgame perfect equilibrium of this bargaining game assuming agents have no fairness concerns and care only about their own earnings.

3.2.1. Experimental Design

In this experiment, buyers (proposers) and sellers (responders) meet in a given round with the opportunity to trade. It is common knowledge that the buyer's value for the good V is either 70 or 90 experimental units (each with 50% chance) and that the seller's cost to produce the good C is either 10 or 30 experimental units (each with 50% chance).[4] In each round, the buyer makes a take-it-or-leave-it offer P to the seller to purchase the good. If the offer is accepted, the buyer receives $\pi_B = V - P$ and the seller receives $\pi_S = P - C$, where π_B and π_S are denoted in experimental units. If the offer is rejected, both players get 0 experimental units, so $\pi_B = 0$ and $\pi_S = 0$. The seller cannot accept an offer for which $P < C$.

The experimental design varies what information players have about the realization of the value V and cost C. The buyer always knows her value and the seller always knows his cost. In the *Incomplete Information* treatment, this is all the players know. In additional treatments, we provide additional information (about V and C) to either: the buyer, the seller, or both. In all treatments, the information structure is common knowledge. In total, we have four information treatments, shown in Table 3.1. Our design embeds a version of a standard Ultimatum Game as the *Complete Information* treatment in which both players are informed of the buyer's value V and the seller's cost C and thus the size of the pie.

[4]The experiment was conducted using z-Tree (Fischbacher, 2007).

Table 3.1: Treatments of Experiment

		Buyer (Proposer) Knows	
		Value	Value & Cost
Seller (Responder) Knows	Cost	Incomplete Information (II) (46 Buyers & 46 Sellers)	Buyer Knows (BK) (49 Buyers & 49 Sellers)
	Value & Cost	Seller Knows (SK) (47 Buyers & 47 Sellers)	Complete Information (CI) (45 Buyers & 45 Sellers)

Notes: The experimental design contains four treatments differing only in the information available to buyers and sellers. Our design embeds a version of a standard Ultimatum Game as the *Complete Information* treatment in which both players are informed of the buyer's value V and the seller's cost C and thus the size of the pie.

Subjects play 30 rounds of the bargaining game as either a buyer who makes proposals or a seller who responds to proposals and are randomly assigned to a new partner of the opposite type in each round. Subjects play all 30 rounds in the same information treatment. Subjects are informed or reminded at the end of each round only of their own action and the action of their partner, as well as the information they had in that round. Subjects were paid their earnings for a randomly selected round with each experimental unit worth \$0.50.

As shown in Table A.4.1, after the 30 rounds, subjects answer additional questions that depend on their treatment and role. In this paper, we will focus on one of these questions. Buyers in all information conditions were asked in an incentive compatible way to report the probability that offers made by other buyers in their treatment were accepted by sellers. These questions allow us to generate a distribution of beliefs for buyers.

3.2.2. Hypothesis

Before presenting our results, we solve for the subgame perfect equilibrium assuming agents have no fairness concerns and care only about their own earnings. While a large literature suggests that we should not expect the point predictions of such a model to hold, the comparative static predictions might still be illuminating. In this model, when the buyer has information on the seller's cost, C, the buyer offers $P = C$, which is accepted by the seller. This yields essentially perfectly unequal payoffs with the buyer receiving 100% of the surplus. When the buyer has no information on seller's cost, C, the buyer's profit maximizing offer is $P = 30$ (the larger possible value of C). These offers are accepted by all sellers.[5] In this treatment, the surplus of the buyer is 100% of total surplus when $C = 30$, 75% when $V = 90$ and $C = 10$ and 67% when $V = 70$ and $C = 10$. Therefore, we expect all offers to be accepted, and we expect, overall, the share of the surplus for the buyers to be 85.4% for the seller to be 14.6%, a difference of 70.8% of the surplus.[6]

Therefore, the self-interested subgame perfect equilibrium predicts that: (1) information has no value to the seller, (2) the buyer's share of surplus increases with information, and (3) inequality is highest when the buyer has information. In what follows, we find that all three of these comparative statics are wrong.

It is worth noting that the design of our Ultimatum Game (i.e., the Complete Information treatment) differs from the standard Ultimatum Game in several ways. First, the size of the pie is determined by random draws of V and C that are associated with the buyer and seller. It could be that this design feature on its own affects the fair outcome, as players may feel entitled to keep part of the value generated if they get a good draw (i.e., a high value V or a low cost C).[7] Second, framing the game as a market transaction may affect results and increase the inequality between the buyer and the seller.[8] However, these two factors are held constant across treatments, so any differences between our treatments can nevertheless be attributed to informational differences. In addition, as we will show in the next section, we replicate the standard Ultimatum Game results in our Complete Information treatment, suggesting these concerns are not first order.

[5] For any value V, the buyer essentially decides between offering a price P of 10 or 30. An offer of $P = 10$ generates an automatic 50% rejection rate (whenever the cost of the seller is 30) and hence an expected profit of $\frac{V}{2}$. An offer of $P = 30$ is accepted with certainty and generates profits $V - 30$, which is larger than $\frac{V}{2}$ for $V > 60$.

[6] Note these outcomes are nearly identical if we assume sellers reject offers of $P = C$, in which case buyers offer P that are 1 unit higher and the shares of surplus going to each player change only slightly.

[7] While it is hard to move behavior in the Ultimatum Game away from fairly equal splits, entitlements to parts of the pie can play a role, see Kerschbamer and Kirchsteiger (1997).

[8] See Liberman et al. (2004).

3.3. Results

We organize our findings about how information interacts with fairness concerns, answering three questions. First, how does information affect fairness concerns? Second, what is the mechanism? Third, what is the value of information for each party?

All our results will use the condition with more information as control and the condition with less information as our comparison group. In section 3.3.1 we establish a measure of inequality and show that removing information increases inequality. In section 3.3.2 and 3.3.3 we show two major reasons for this result: (1) buyers give lower offers to uninformed sellers and (2) uninformed sellers are more likely to accept when buyers are uninformed. Moreover, this is consistent with buyers behaving strategically based on their beliefs and sellers having social image of intention concerns. Finally, in section 3.3.4 we look at the value of information for buyers and sellers and find that the value of information is asymmetric.

In total, we analyze data from 3,740 decisions made by 374 subjects (focusing on the last 10 rounds of decisions out of a total of 30 rounds). Subjects were randomly assigned to one of the four treatments.[9]

3.3.1. Removing information increase inequality

While prior literature has argued that fairness concerns significantly alter bargaining outcomes, most evidence comes from the full-information setting. Our results show that the information structure significantly impacts both efficiency and the distribution of profits. In particular, we find that removing information decreases the role of fairness in outcomes.

Before we assess the role of information on fairness, we define a key measure of inequity, which we call the *inequality share*. Given the theoretical prediction that the buyer should end up with all the surplus, the ideal measure should get larger as the buyer's earning increase relative to the seller's earnings and be minimized when outcomes are equal. Note

[9]Results are from 20 experimental sessions — 12 run at the Wharton Behavioral Lab and 8 run at the Stanford Economics Research Lab — during May 2012 (at Stanford) and June 2012 (at Wharton).

that this leaves open the question of what to do when a buyer makes an offer that leaves the majority of the surplus to the seller. This could be thought of as increasing inequality at the transaction level (i.e., outcomes are less fair than an equal split for this particular buyer and seller) or it could be thought of as decreasing inequality at the market level (i.e., since buyers usually earn more than sellers, these overly generous offers help equalize average earnings).[10] In what follows, we take as our primary measure a third path that treats these offers as neither increasing or decrease inequality relative to a fair split, and we explore the other two cases in Appendix A.4.3. In particular, we define *inequality share* as the max of the difference between the buyer and seller's earnings and zero divided by the total surplus: $\frac{max\{\text{Buyer's earnings}-\text{Seller's earnings},0\}}{V-C}$. This measure satisfies our criteria of increasing as the difference between buyer's earnings and seller's earnings increases from zero and treats all other outcomes as being equal.[11]

Table 3.2 shows this *inequality share* as well as the average buyer and seller earnings as a percentage of possible surplus available in a given round (i.e., $V-C$), and the *total share* of possible surplus realized, a measure of efficiency achieved for each of the four treatments. The top panel shows the data for all 30 rounds, the middle panel shows the data for the first 10 rounds, and the bottom panel shows the data for the last 10 rounds.

We note that there is significant learning and experience effects between the first 10 rounds and the last 10 rounds. Not only does the *inequality share* change but the *total share* changes as well. This suggest that buyers are learning to make offers that are more likely to be accepted and more unfair.[12] Throughout the paper, we limit our analysis to the last

[10] In appendix Table A.4.2 we show that most "generous" offers occur when the buyer is uninformed of the seller's cost. When the buyer is informed, "generous" offers make up less than 1% of total transactions.

[11] Note that this definition is about transaction specific inequality. We replicate our results using the *absolute inequality share*: $\frac{|\text{Buyer's earnings}-\text{Seller's earnings}|}{V-C}$, which is the absolute difference between the buyer and seller's earnings divided by the total surplus, as well as the *difference inequality share*: $\frac{\text{Buyer earnings}-\text{Seller earnings}}{V-C}$, which is a measure of average inequality among all buyers and sellers. See appendix Tables A.4.8 and A.4.9 and A.4.10.

[12] In a regression framework, Table A.4.3 in the appendix shows that there are significant differences in *inequality share*, buyer earnings, and seller earnings between the first 10 rounds and the last 10 rounds. In particular, Table A.4.3 shows that in the final 10 rounds, inequality significantly increases when buyers or sellers are uninformed; however, the interaction between uninformed buyers or sellers and experience (the last 10 rounds) shows that there is a further statistically significant increase in inequality. Moreover, when

Table 3.2: Surplus, Inequality and Total Shares

	All 30 Rounds			
Treatment:	**Buyer Share**	**Seller Share**	**Inequality Share**	**Total Share**
Incomplete Information	51.67	31.67	23.23	83.33
Seller Knows	51.51	33.88	19.38	85.39
Buyer Knows	51.81	32.13	20.60	83.95
Complete Information	49.00	36.77	12.38	85.78
	First 10 Rounds			
Treatment:	**Buyer Share**	**Seller Share**	**Inequality Share**	**Total Share**
Incomplete Information	47.26	31.87	20.09	79.13
Seller Knows	49.57	32.98	18.72	82.55
Buyer Knows	49.86	30.14	21.69	80.00
Complete Information	48.16	33.40	14.86	81.56
	Last 10 Rounds			
Treatment:	**Buyer Share**	**Seller Share**	**Inequality Share**	**Total Share**
Incomplete Information	54.19	31.90	24.98	86.09
Seller Knows	53.83	35.11	20.14	88.94
Buyer Knows	54.89	34.50	20.60	89.39
Complete Information	49.22	39.00	10.22	88.22

Notes: Table 3.2 reports the share of total surplus (Value - Cost) that goes to the buyer and the seller. Inequality Share is the difference in buyer share and seller share (i.e., Buyer earnings - Seller earnings) divided by the total surplus. Total Share is the sum of buyer share and seller share. Total share is less than 100% since some offers are rejected by the seller and lead to both players getting 0.

10 rounds since we are interested in the extent to which our results persist in the presence of this learning or experience. As a robustness check, we replicate all analysis using all 30 rounds and find consistent results (see Appendix A.4.2).

In the last 10 rounds of the Complete Information condition, the *inequality share* is 10.22%. Removing information to one party, either the buyer or seller, doubles the *inequality share* to about 20%. In addition, removing information from both parties drastically increases the *inequality share* to 24.98%. This is a 14.76 percentage point or a 144% increase in inequality compared to the Complete Information condition. Furthermore, note that total surplus is constant across treatments, suggesting that information does not increase the likelihood of trade, even though it dramatically changes how gains from trade are split.

Table 3.3: Inequality Shares, Seller Shares, and Buyer Shares

Dependent variable:	**Inequality Shares**			**Seller Shares**			**Buyer Shares**		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Seller No Info	0.074***		0.104***	-0.038***		-0.045***	0.030**		0.057***
	(0.016)		(0.021)	(0.010)		(0.013)	(0.013)		(0.017)
Buyer No Info		0.069***	0.099***		-0.031***	-0.039***		0.018	0.046***
		(0.017)	(0.021)		(0.010)	(0.014)		(0.013)	(0.018)
Both No Info			-0.055*			0.013			-0.053**
			(0.031)			(0.019)			(0.025)
Constant	0.153***	0.156***	0.102***	0.370***	0.367***	0.390***	0.516***	0.522***	0.492***
	(0.012)	(0.012)	(0.016)	(0.007)	(0.007)	(0.010)	(0.009)	(0.009)	(0.013)
Incomplete Information			0.148***			-0.071***			0.050**
			(0.025)			(0.014)			(0.020)
Test CI=II p-value			0.000			0.000			0.013
Number of Buyers (Clusters)	187	187	187	187	187	187	187	187	187
Observations	1870	1870	1870	1870	1870	1870	1870	1870	1870
R-Squared	0.0299	0.0258	0.0614	0.0124	0.00855	0.0219	0.00442	0.00168	0.00976

Notes: Seller No Info denotes when a seller does not have information on the buyer's valuation (in BK and II). Buyer No Info denotes when the buyer does not have information on the seller's cost (in SK and II). Both No Info denotes when both the buyer and seller do not have information (II). Regression analysis shows Complete Information has the least inequality in payoffs, while removing information from either the buyer or the seller leads to more inequality. Similarly, removing information from either buyer or the seller leads a lower share of the surplus for sellers. On the other hand, removing information from sellers increases buyer's share. Robust standard errors in parentheses. Standard errors clustered at the individual level. There are 187 clusters. Significance: *** p<0.01, ** p<0.05, * p<0.1.

comparing the Complete Information treatment to the Incomplete Information treatment, having uninformed buyers and sellers increases the *inequality share* by 5.2 percentage points during the first 10 rounds but the interaction between Incomplete Information and experience shows that it increases *inequality share* by an additional 9.5 percentage points in the latter 10 rounds. Furthermore, looking at buyer and seller earnings between the Incomplete Information treatment and the Complete Information treatment, we find that while there is an increase in buyer and seller earnings in the first 10 rounds, this is not significant. On the other hand, we see that the Incomplete Information setting significantly increases buyer earnings by an additional 4.10 experimental units and significantly reduces the sellers earnings by 3.23 experimental units in the second half of the experiment. This suggest that there is significant learning and from the first half to the second half of the experiment.

Table 3.3 uses a regression framework to analyze how removing information from sellers and buyers affects *inequality share* in regressions (1) to (3).[13] We use dummy variables to denote when a seller does not have information on the buyer's valuation, when the buyer does not have information on the seller's cost, and when neither party has information. Regressions (1) and (2), show that removing information from either buyers or sellers increases inequality by 7 percentage points. In addition, in regression (3) we split this effect by information setting and find that removing information from either buyers or sellers significantly increases inequality independently. The last row of regression (3) shows that the added effect associated with removing information from both reduces the *inequality share* by a total of 5.5 percentage points and this is marginally significant but does not offset the large gains in inequality from removing information from buyers or sellers. Thus, Complete Information (i.e., the standard Ultimatum Game) has the least inequality in payoffs, and removing information from either the buyer or the seller leads to more inequality. These results demonstrate that removing information can reduce the impact of fairness concerns on outcomes dramatically. Below, we explore why information reduces inequality.

3.3.2. Two major reasons information reduces inequality

Our experimental design allows us to explore why removing information increases inequality by looking at offers and acceptance rates across our treatments. In this subsection, we report on two factors driving this pattern. First, buyers, particularly high-value buyers, make lower offers when sellers are uninformed. Second, conditional on a given offer, sellers are more likely to accept when buyers or sellers are uninformed.

Below, we analyze our data by exploring four main comparisons: (1) SK vs CI: removing buyers information when the seller is informed, (2) II vs BK: removing buyers information when the seller is uninformed, (3) BK vs CI: removing sellers information when the buyer is informed, and (4) II vs SK: removing sellers information when the buyer is uninformed. All

[13]Regressions (4) to (9) focus on the seller's and buyer's share of the surplus and will be discussed in section 3.3.4.

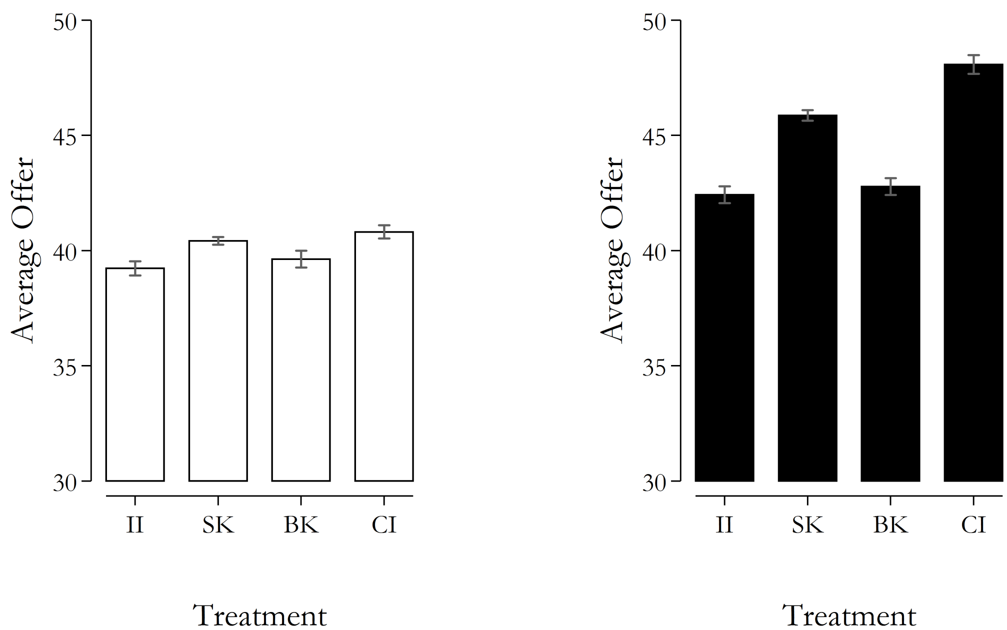
analysis is performed using the last 10 rounds only. As a robustness check, we perform the same analysis using data from all 30 rounds in Appendix A.4.2 and find consistent results.

We find that removing buyers information when the seller is informed and removing sellers information, regardless whether the buyer is informed or not, significantly lowers buyer's offers. Moreover, conditional on offers, seller's acceptance rate directionally increase, thus, inequality rises through lower offers. In contrast, removing buyers information when sellers are uninformed only directionally decreases buyer's offers, however, conditional on offers, uninformed sellers are significantly more likely to accept when the buyer is uninformed, thus increasing inequality through more acceptances.

Lower offers to uninformed sellers

First, we show that buyers make lower offers when sellers are uninformed and that this is primarily driven by high-value buyers. Figure 3.1 shows the average offer by treatment for buyer with $V = 70$ (Panel A) and buyers with $V = 90$ (Panel B). Note that buyers do not know the seller's cost in treatment II and SK and buyers know the seller's cost in BK and CI. Buyers make lower offers when sellers are uninformed (II and BK). While there is a small decrease in offers when $V = 70$ (Panel A), the decrease is substantially larger when $V = 90$ (Panel B). Moreover, when sellers are not informed (in II and BK), the average offers by high-value buyers are similar to (generous) offers of low-value buyers. This amounts to high-value buyers mimicking offers of low-value buyers when sellers are not informed, a result we will explore in greater detail below.

Tables 3.4 analyzes the results in Figure 3.1 in a regression framework. First, we show the results for all buyers and then show the effect conditional on the buyer's value. Panel A shows the effect of removing information from buyers when the seller is informed (on the left) and when the seller is uninformed (on the right). We find that removing buyers information significantly decreases buyers' offers but only when sellers are informed (i.e., comparing SK vs CI). Regression (1) shows that when buyers are uninformed, offers decrease by 1.94



(a) Value=70

(b) Value=90

Figure 3.1: Offer by Treatment and Value

Notes: Panel A shows the average offer by treatment for buyers with a value of 70 while Panel B shows the average offer by treatment for buyers with a value of 90. Buyers do not have information on seller's cost in II and SK but know the seller's cost in BK and CI. Buyers, particularly high-value buyers, make lower offers when sellers are uninformed. This amounts to high-value buyers mimicking offers of low-value buyers when sellers are uninformed. Standard error bars are shown around each mean.

experimental units compared to a full-information setting (and this is partially significant). Moreover, regression (3) shows that reduction in offers are driven by high-value buyers. On average, removing information from high-value buyers decreases offers by 4.08 experimental units when the seller is informed compared the full-information condition. Meanwhile, regressions (4) to (6) shows that removing buyer information when the seller is uninformed directionally decreases offers (i.e., comparing II vs BK).

Table 3.4: Offer Conditional on Value: Effect of Removing Information

Panel A: Removing Buyer's Information | Seller's Information Setting

When Seller Informed:	All	**SK vs CI** Value=70	Value=90	**When Seller Uninformed:**	All	**II vs BK** Value=70	Value=90
	(1)	(2)	(3)		(4)	(5)	(6)
Seller Knows	-1.941*	-0.681	-4.077***	Incomplete Information	-1.387	-0.957	-1.766
	(0.982)	(0.909)	(1.178)		(1.291)	(1.263)	(1.603)
Constant	45.520***	41.544***	50.490***	Constant	42.263***	40.657***	43.720***
	(0.798)	(0.735)	(0.882)		(0.713)	(0.759)	(0.804)
Number of Buyers (Clusters)	92	91	92	Number of Buyers (Clusters)	95	95	95
Observations	920	490	430	Observations	950	453	497
R-Squared	0.0179	0.00351	0.0864	R-Squared	0.00676	0.00375	0.0101

Panel B: Removing Seller's Information | Buyer's Information Setting

When Buyer Informed:	All	**BK vs CI** Value=70	Value=90	**When Buyer Uninformed:**	All	**II vs SK** Value=70	Value=90
	(1)	(2)	(3)		(4)	(5)	(6)
Buyer Knows	-3.257***	-0.887	-6.770***	Incomplete Information	-2.703**	-1.163	-4.459***
	(1.070)	(1.056)	(1.194)		(1.219)	(1.143)	(1.591)
Constant	45.520***	41.544***	50.490***	Constant	43.579***	40.862***	46.413***
	(0.798)	(0.735)	(0.882)		(0.572)	(0.535)	(0.781)
Number of Buyers (Clusters)	94	93	94	Number of Buyers (Clusters)	93	93	93
Observations	940	483	457	Observations	930	460	470
R-Squared	0.0335	0.00324	0.137	R-Squared	0.0382	0.0106	0.0885

Notes: Panel A the shows that removing information from buyers when the seller is informed significantly decreases buyers' offers, in particular high-value buyers are more likely to give lower offers. Similarly, removing information from buyers when the seller is uninformed directionally decreases buyer's offers. Panel B shows that removing information from sellers regardless of whether the buyer is informed or not significantly decreases buyers' offers, in particular high-value buyers are more likely to give lower offers. Robust standard errors in parenthesis. Standard errors clustered at the individual level. Significance: *** p<0.01, ** p<0.05, * p<0.1.

Table 3.4 Panel B shows the effect of removing information from sellers when the buyer is informed (on the left) and when the buyer is uninformed (on the right). We find that removing seller information significantly decreases buyers' offers regardless whether the buyer is informed or not. Regression (1) shows that when sellers are uninformed, offers significantly decrease by 3.26 experimental units compared to a full-information setting (i.e., BK vs CI). Consistent with previous results, regression (3) shows that reduction in offers are driven by high-value buyers. On average, removing information from high-value buyers decreases offers by 6.77 experimental units when the seller is uninformed compared the full-information condition (a 13.4% decrease). Similarly, regressions (4) and (6) shows that when buyers and sellers are uninformed (that is, in the Incomplete Information condition), offers significantly decrease by 2.70 experimental units and, again, these results are drive by high-value buyers, who reduce offers by 4.46 experimental units compared to when the seller is informed (i.e., II vs SK).

Perhaps a more practical comparison is to look at the Incomplete Information setting, which mimics bargaining conditions in practice, to the Complete Information (i.e., Ultimatum Game) results we have seen in the literature. To do this, we also perform a fully stacked regression with treatment dummies and show that removing information from both parties (i.e., the Incomplete Information treatment) decreases offers by 4.6 experimental units compared to Complete Information and this is significant at the 1 percent level.[14]

Uninformed sellers are less likely to accept when buyers are informed

Second, we find that conditional on a given offer, removing information from buyers significantly increases seller's acceptance rate when sellers are uninformed and this effect is driven by high-cost sellers. We posit this is due to a combination of social image and intention effects. Moreover, conditional on offers, removing buyer's information when the seller is informed or removing seller's information, regardless whether the buyer is informed or not, directionally increases sellers' acceptance rate.

Tables 3.5 Panels A and B presents these results in a regression framework controlling for the offer. First we shows the results for all sellers and then show the effect conditional on the seller's cost. Panel A shows the effect of removing information from buyers when the seller is informed (on the left) and when the seller is uninformed (on the right). We find that removing buyers information significantly increases seller's acceptance rate but only when sellers are uninformed. Regressions (1) to (3) shows that when buyers are uninformed,

[14] Regression available upon request.

sellers are directionally more likely to accept a given offer compared to a full-information setting. Moreover, regression (4) shows that removing buyer information when the seller is uninformed significantly increases acceptance rate for a given offer by 7.2 percentage points compared to when the buyer is informed. This effect is driven by high-cost sellers who are 14.2 percentage points more likely to accept in the Incomplete Information treatment compared to the Buyer Knows treatment (see regression (6)).

Table 3.5: Acceptance Rates Conditional on Offer: Effect of Removing Information

Panel A: Removing Buyer's Information | Seller's Information Setting

When Seller Informed:	**SK vs CI**			**When Seller Uninformed:**	**II vs BK**		
	All	Cost=10	Cost=30		All	Cost=10	Cost=30
	(1)	(2)	(3)		(4)	(5)	(6)
Seller Knows	0.028	0.048	0.005	Incomplete Information	0.072**	0.025	0.142**
	(0.041)	(0.034)	(0.067)		(0.032)	(0.031)	(0.066)
Number of Buyers (Clusters)	92	92	92	Number of Buyers (Clusters)	95	95	95
Observations	920	450	470	Observations	950	480	470
R-Squared	0.270	0.312	0.231	R-Squared	0.293	0.283	0.259

Panel B: Removing Seller's Information | Buyer's Information Setting

When Buyer Informed:	**BK vs CI**			**When Buyer Uninformed:**	**II vs SK**		
	All	Cost=10	Cost=30		All	Cost=10	Cost=30
	(1)	(2)	(3)		(4)	(5)	(6)
Buyer Knows	0.024	0.065	-0.012	Incomplete Information	0.034	0.015	0.055
	(0.033)	(0.040)	(0.041)		(0.036)	(0.018)	(0.067)
Number of Buyers (Clusters)	94	94	94	Number of Buyers (Clusters)	93	93	93
Observations	940	461	479	Observations	930	469	461
R-Squared	0.235	0.238	0.235	R-Squared	0.302	0.304	0.222

Notes: Panel A the shows that removing information from buyers when the seller is informed directionally increases sellers' acceptance rate. Moreover, removing information from buyers when the seller is uninformed significantly increases sellers' acceptance rate. Panel B shows that removing information from sellers, regardless of whether the buyer is informed or not, directionally increases sellers' acceptance rate. Robust standard errors in parenthesis. Standard errors clustered at the individual level. Significance: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 3.5 Panel B shows the effect of removing information from sellers when the buyer is informed (on the left) and when the buyer is uninformed (on the right). Consistent with previous results, we find that removing seller information directionally increases seller's acceptance rate regardless whether the buyer is informed or not.

As before, we also compare Incomplete Information setting to the Complete Information (i.e., Ultimatum Game) results we have seen in the literature in a fully stacked regression with treatment dummies and show that, conditional on a given offer, removing information from both parties (i.e., the Incomplete Information treatment) decreases seller's acceptance rate by 9.6 percentage points compared to Complete Information and this is significant at the 1 percent level.[15]

Note that previously we showed that buyer's offers change under different information settings, as such, it is hard to compare seller's rejection rates due to the different offer distributions across treatment. That is, treatments with more information have less instances with less-fair offers. In appendix A.4.5 we control for differences in distribution of offers using sample weights and replicate the analysis on seller's acceptance rate and show that controlling for the distribution of offers in each comparison, the magnitude of change in acceptance rate increases (that is, sellers accept more when there is less information).

In the following subsection, we use additional belief data collected during the experiment to explore the underlying behavioral phenomenon generating the results we just described.

3.3.3. Beliefs and Strategic Behavior

In section 3.3.2, we showed that when sellers are uninformed, buyers make lower offers and sellers are directionally more likely to accept a given offer. We use belief data to show that these patterns arise from buyers anticipating that sellers will accept lower offers when uninformed.[16] Furthermore, we posit that sellers accept these lower offers when buyers are uninformed due to social image or intention concerns.

Buyer's beliefs

We use buyer's beliefs to show that buyers are behaving strategically in response to sellers being uninformed. We asked buyers in each treatment, in an incentive compatible way, to report the probability that the seller would accept a given offer. In particular, each buyer was randomly assigned one round from the last 10 rounds and asked about the probability

[15] Regression available upon request.

[16] In this section, we will focus on buyer and sellers' beliefs conditional on offers between 20 and 55 experimental units — where most of our data appears to be concentrated. Figure A.4.1 in the appendix presents the CDF of offers used to elicit beliefs and shows that more than 90% of the mass of the offers lies between 20 and 60 experimental units.

an offer was accepted for each of the buyer-seller pairs in that round. The buyer was shown, for each buyer-seller pair, the value of the buyer in that round, the information the buyer had in that round (i.e., cost of the seller in BK and CI treatments), and the offer the buyer made in that round.[17]

Figure A.4.2 Panel A reports the buyer's belief probability that an offer is accepted when the seller's cost is 10 for the Buyer Knows and Complete Information treatments. Buyers in the BK treatment recognize that sellers' acceptance probability is unlikely to differ with the buyer's value (i.e., the gray and black solid lines practically overlap with K-S test p-value=0.972). In addition, the reported probability of acceptance are closer to beliefs in CI when $V = 70$. Panel B shows the actual acceptance rate by offer and shows that buyers hold accurate beliefs.

In addition, buyers behave strategically based on these beliefs. Distributions of offers by treatment and value show that buyers with a high value mimic offers of low-value buyers when sellers are uninformed. Figure A.4.2 Panel C shows that when sellers are uninformed (in BK), the high-value buyers' offer distribution almost directly overlaps with the low-value buyers' offer distribution (both mean and standard deviation) and both lie on top of the offer distribution of low-value buyers in CI. On other hand, when sellers are informed, buyers' offer distribution is shifted to the right (black dashed line) when they have a high value. This strategic behavior is consistent with buyers optimally responding to their belief that a seller will accept a given offer.

We find very similar results when we compare Buyer Knows and Complete Information treatments when the seller's cost is 30. Figure A.4.3 Panel A shows that buyers in BK correctly believe that uninformed sellers cannot condition acceptance on the buyer's value (the solid lines are on top of each other). Again, the BK probabilities are nearly identical

[17]To ensure incentive compatibility, we used a Becker-DeGroot-Marschak (BDM) method for binary outcomes (see Niehaus et al. (2012)). Buyers were explicitly instructed: "This payment method ensures that to maximize your earnings, you should report the number that is exactly your best guess of the probability the seller accepted the buyer?s offer."

to beliefs in CI for $V = 70$, supporting buyers' belief that sellers in BK give buyers the benefit of the doubt.

We also find similar evidence that buyers behave strategically based on this belief: high-value buyers mimic offers of low-value buyers when the seller is uninformed, as shown in Figure A.4.3 Panel C, when $C = 30$. When sellers are uninformed (in BK), high-value buyers' offer distribution is more closely aligned with the low-value buyers' offer distribution (smaller difference in means) and low-value BK buyers' offer distribution lies on top of the offer distribution of low-value buyers in CI. However, once again, we find that when the sellers are informed (in CI), buyers' offer distribution is shifted more heavily towards the right (black dashed line) when they have a high value. One difference we observe here, is that high-value Buyers in BK paired with high-cost sellers will less aggressively mimic low-value informed buyers (i.e., the BK $V = 90$ distribution is right of the BK $V = 70$ distribution). This may arise because high-value buyers have more to lose if an offer is rejected. This evidence further supports the notion that buyers behave strategically assuming seller's acceptance rate.

We also find supporting evidence when we compare the Incomplete Information and Seller Knows conditions. From Figure A.4.4 Panel A we can see that when offers are in the range of 30 to 40 experimental units, buyers believe that when sellers are informed, that is in the Seller Knows treatment, low offers from high-value buyers are less likely to be accepted (compare gray and black dashed lines). On the other hand, in the Incomplete Information condition, we actually find the opposite with a higher acceptance rate for the same offer from high-value buyers (compare gray and black solid lines for offers). Actual acceptance rates in the offer ranges of 30 to 40 experimental units does not show a distinct difference in acceptance rate. However, buyers are still adjusting their offers based on their beliefs.

Figure A.4.4 Panel C shows that buyers are not only aware that informed sellers respond differently compared to uninformed sellers but actually high-value buyers behave strategically based on this belief by mimic offers of low-value buyers when the seller does not have information. Furthermore, we note that there is a larger spread in offers when for buyers in the Incomplete Information condition (which is not seen in other treatments). This may be due to buyers not fully realizing that having uninformed sellers (and possibly being uninformed buyers) is actually beneficial for their outcomes.

Seller's concerns

We also elicited and explore seller's beliefs to try to explain why sellers are directionally more likely to accept an offer when uninformed. After subjects played all 30 rounds of the bargaining game, we incentivized sellers in the Incomplete Information and Buyer Knows conditions (i.e., in each treatment where they did not know the buyer's value) to report the probability that the buyer had $V = 70$ (i.e., the low value), one by one for each offer they received in the last 10 rounds of the game.[18] Evidence from our belief elicitation suggests that there might be simultaneity bias. Since sellers are asked to state their belief over offers they may have accepted or rejected, they might ex-post adjust their beliefs of the buyer's value to justify their actions. As such, we do not use this data.

Instead, we posit that uninformed sellers are more likely to accept when buyers are uninformed due to a combination of social image and intention effects. That is, seller's may care about buyer's intentions, accepting less-fair offers when the buyer is uninformed because "they did not know the size of the pie", or sellers may care about social image, unwilling to accept less-fair offers when the buyer is aware of the equity.[19]

3.3.4. Value of Information

We have found that fairness concerns are significantly muted in settings when parties are uninformed about the size of the pie. This result suggests that the Ultimatum Game — the workhorse that has attracted significant attention and highlighted the importance of

[18]To ensure incentive compatibility, we used a Becker-DeGroot-Marschak (BDM) method for binary outcomes (see Niehaus et al. (2012)). Sellers were explicitly instructed: "This payment method ensures that to maximize your earnings, you should report the number that is exactly your best guess of the probability the buyer had a value of 70 experimental units."

[19]We are not able to separate these two effects.

fairness in bargaining — is a special case at the far end of the spectrum on the impact of fairness on bargaining outcomes.

These large increases in inequality when one or both parties are uninformed arise from both: (1) the buyer (proposer) making less-fair offers and (2) the seller (responder) accepting less-fair offers when less information is available. We find evidence that buyers behave strategically based on reported beliefs that a seller will accept a given offer. That is, when a seller is uninformed, buyers report believing that they are more likely to accept a low offer. Buyers with a high value (i.e., a large pie) respond strategically to their belief by mimicking the offers of buyers with a low value (i.e., small pie) when the seller is uninformed. That uninformed sellers are more likely to accept a given offer when buyers are uninformed suggests a role for caring about the intentions of the buyer or for social image concerns of not wanting to accept unfair splits when the buyer is aware of the inequity.

Given the evidence, we look at the value of information for buyers and sellers and find that the value of information is asymmetric. From Table 3.2 in section 3.3.1 we can see that sellers always earn more when they have information about the buyer's value (and thus the size of the pie) — seller earnings decrease by 10% when they are uninformed (compared to II). The buyers, meanwhile, do not gain from additional information about the size of the pie, and they do worse with this information if the seller is also informed.

Table 3.3 investigates the effect of information on the seller's share of possible surplus using a regression framework. Regressions (4) to (6) show that sellers earn less when there is less information for the seller but also with less information for the buyer. Regression (4) shows that when the seller is uninformed, the seller's share of possible surplus decreases by 3.8 percentage. Regression (5) shows that when the buyer is uninformed, the seller's share of possible surplus also decreases by 3.1 percentage points. Moreover, comparing the Complete Information and Incomplete Information treatment, we find that when neither party is informed seller's share of the possible surplus decrease by 7.1 percentage points.

In contrast, information is not valuable and can actually harm buyers. Table 3.2 shows the buyer's share of possible surplus across the four treatment conditions. Buyers earns less in Complete Information compared to all the other conditions, which are basically identical. Table 3.3 regressions (7) to (9) show the effect of information on the buyer's share of the possible surplus in a regression framework. In regression (7) we find a gain of 3 percentage points when the seller is not informed. From regression (9), note that removing buyer or seller information lead to similar gains in profit for buyers, 4.6 and 5.7 percentage points, respectively. We do find that when neither party is informed, buyers lose 5 percentage points of the total surplus but this loss is offset by the gains from removing buyer and seller information. In fact, under Incomplete Information, the buyer's share of possible surplus increases by 5 percentage points compared to the Complete Information treatment. Thus, there is no gain of having information for the buyer and information can actually make the buyer worse off. As shown in section 3.3.2, this may arise because sellers are more forgiving of low offers when the buyer is uninformed.

The asymmetrical value of information for buyers and sellers suggest that, in a setting like ours where there is common knowledge of which parties are informed, the seller has an incentive to get information but the buyer has an incentive to remain uninformed. This pattern makes the setting in which the seller is informed and the buyer is not informed — which has not previously been studied — an "absorbing state" that might be likely to arise if sellers and buyers had the opportunity to get additional information. As we note above, ours is a case where who has what is common knowledge, so buyers are aware when sellers are informed and sellers are aware when buyers are informed. We leave the case where information acquisition is private information for future work.

3.4. General Discussion

In this paper, we introduce a new experimental paradigm that nests the standard full-information Ultimatum Game and allows us to systematically vary the information of either party to investigate the role of information on behavior and outcomes in a bargaining game. We find that fairness concerns are significantly muted when one or both parties are uninformed. This suggests that the Ultimatum Game, which has attracted significant attention and has highlighted the importance of fairness in bargaining, is a special case at the far end of the spectrum on the impact of fairness on bargaining outcomes.

These large increases in inequality when one or both parties are uninformed arise from both: (1) the buyer (proposer) making less-fair offers and (2) the seller (responder) accepting less-fair offers when less information is available. We find evidence that buyers behave strategically based on reported beliefs that a seller will accept a given offer. That is, when a seller is uninformed, buyers report believing that they are more likely to accept a low offer. Buyers with a high value (i.e., a large pie) respond strategically to their belief by mimicking the offers of buyers with a low value (i.e., small pie) when the seller is uninformed. That uninformed sellers are more likely to accept a given offer when buyers are uninformed suggests a role for caring about the intentions of the buyer or for social image concerns of not wanting to accept unfair splits when the buyer is aware of the inequity.

Finally, we find that the value of information is different for buyers and sellers. Sellers earn more when informed, but buyers do not. The asymmetrical value of information for buyers and sellers suggest that, in a setting like ours where there is common knowledge of which parties are informed, the seller has an incentive to get information but the buyer has an incentive to remain uninformed. This pattern makes the setting in which the seller is informed and the buyer is not informed — which has not previously been studied — an "absorbing state" that might be likely to arise if sellers and buyers had the opportunity to get additional information.

APPENDIX

A.1. Transaction Utility and Consumer Choice: Discount and Mark-up Game

A.1.1. Supplemental Results

Table A.1.1 tests the effect of an original price by percentage below, above, or equal to the selling price on buyer's purchasing decision. The basic regression specification is:

$$\begin{aligned} Purchase_i =& \alpha + \sum_{n=1}^{7} \beta_n \times Selling\ Price\ as\ \%\ of\ Original\ Price\ Dummies_{n,i} \\ &+ \sum_{j=1}^{4} \beta_{n+j} \times Selling\ Price\ Dummy_{j,i} + \epsilon_i \end{aligned}$$

where $Purchase_i$ is whether participants i decided to purchase the virtual product or not in the *Discount and Mark-up Game*; $Selling\ Price\ as\ \%\ of\ Original\ Price\ Dummies_{n,i}$ is a dummy for being in each discount or mark-up percentage bin; and $Selling\ Price\ Dummy_{j,i}$ is a dummy for each of the selling prices. I cluster the random error, ϵ_i, at the individual level.

Figure A.1.1 and Figure A.1.2 presents the average purchasing rate by selling price as a percentage of the original price by earnings earnings in the *Discount and Mark-Up Game*. Figure A.1.1 shows the results for *Study 1A: Baseline* and Figure A.1.2 shows the results for *Study 1B: With Earnings Displayed*.

Tables A.1.2 Panels A and B tests effect of the selling price as a percentage of the original price ($\frac{P}{O}$) on buyer's purchasing decision by selling price for the *Study 1A: Baseline* and *Study 1B: With Earnings Displayed*, respectively.

Table A.1.3 presents the results of the *Discount and Mark-Up Game* in a regression framework, testing the marginal effect of a perceived percentage change between the selling price

Table A.1.1: Rate of Purchasing Good by Selling Price as a Percentage of the Original Price

Study 1A & 1B: Discount and Mark-up Game

	Dependent Variable: Purchase Virtual Product							
	Study 1A Baseline				**Study 1B With Earnings Displayed**			
	All Participants		Pass First Try		All Participants		Pass First Try	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
60% Of Original Price	0.107***	0.107***	0.105***	0.105***	0.102***	0.102***	0.103***	0.103***
	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)
80% Of Original Price	0.060***	0.060***	0.057***	0.058***	0.070***	0.070***	0.071***	0.071***
	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)
90% Of Original Price	0.038***	0.038***	0.036***	0.036***	0.044***	0.044***	0.045***	0.045***
	(0.013)	(0.013)	(0.013)	(0.013)	(0.014)	(0.014)	(0.015)	(0.015)
100% Of Original Price	-0.006	-0.006	-0.006	-0.006	0.018	0.018	0.016	0.016
	(0.014)	(0.014)	(0.014)	(0.014)	(0.015)	(0.015)	(0.016)	(0.016)
110% Of Original Price	-0.204***	-0.204***	-0.194***	-0.194***	-0.116***	-0.116***	-0.122***	-0.122***
	(0.020)	(0.020)	(0.020)	(0.020)	(0.019)	(0.019)	(0.019)	(0.019)
120% Of Original Price	-0.271***	-0.271***	-0.264***	-0.264***	-0.130***	-0.130***	-0.141***	-0.141***
	(0.024)	(0.024)	(0.024)	(0.024)	(0.020)	(0.020)	(0.020)	(0.020)
140% Of Original Price	-0.337***	-0.337***	-0.330***	-0.330***	-0.189***	-0.189***	-0.198***	-0.198***
	(0.025)	(0.025)	(0.025)	(0.025)	(0.022)	(0.022)	(0.022)	(0.022)
Constant	0.829***	0.976***	0.828***	0.974***	0.765***	0.944***	0.777***	0.944***
	(0.020)	(0.016)	(0.020)	(0.017)	(0.022)	(0.016)	(0.022)	(0.017)
Selling Price: $7.51		-0.086***		-0.084***		-0.083***		-0.074***
		(0.012)		(0.012)		(0.014)		(0.013)
Selling Price: $8.03		-0.221***		-0.222***		-0.276***		-0.259***
		(0.021)		(0.021)		(0.024)		(0.023)
Selling Price: $8.42		-0.273***		-0.274***		-0.365***		-0.345***
		(0.023)		(0.024)		(0.026)		(0.026)
Ind. Clusters	178	178	174	174	180	180	173	173
Price Dummies		Yes		Yes		Yes		Yes
Order Control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5696	5696	5568	5568	5760	5760	5536	5536
R-Squared	0.132	0.195	0.126	0.190	0.0521	0.165	0.0585	0.163

Notes: Participants' average purchase rate of the virtual product conditional on the selling price. I control for game period. Robust standard errors in parentheses. Standard errors clustered at the individual level. Significance: *** p<0.01, ** p<0.05, * p<0.1.

Figure A.1.1: Buyer's Purchase Rate by Selling Price

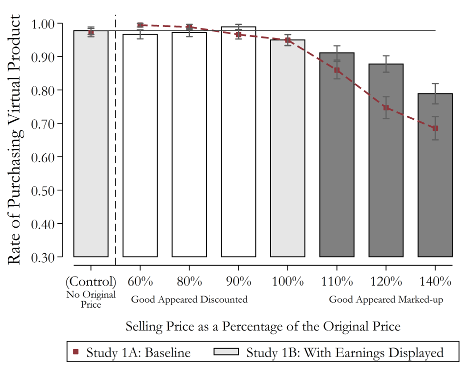
Study 1A: Discount and Mark-up Game (Baseline)

Note: Participants' average purchase rate of the virtual product conditional on the selling price. Robust standard error bars clustered at the individual level are shown around each mean.

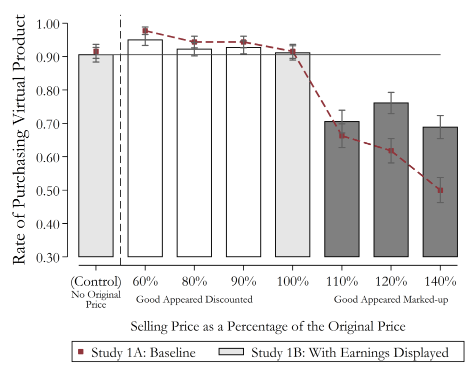
Figure A.1.2: Buyer's Purchase Rate by Selling Price

Study 1B: Discount and Mark-up Game (With Earnings Displayed)

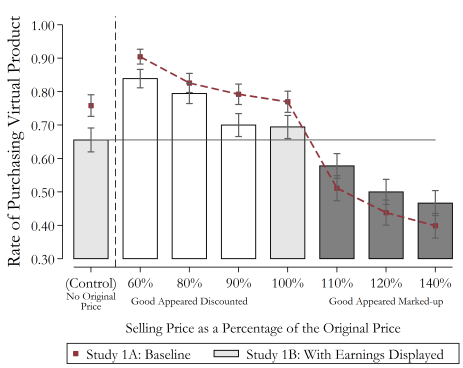
(a) Selling Price: $6.72



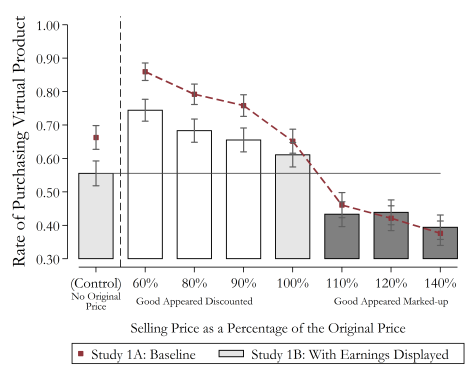
(b) Selling Price: $7.51



(c) Selling Price: $8.03



(d) Selling Price: $8.42



Note: Participants' average purchase rate of the virtual product conditional on earnings. Robust standard error bars clustered at the individual level are shown around each mean.

Table A.1.2: Buyer's Purchase Rate by Selling Price

Study 1A & 1B: Discount and Mark-up Game

Panel A: Study 1A Baseline										
	Dependent Variable: Purchase Virtual Product									
Buyer's Selling Price:	\$6.72		\$7.51		\$8.03		\$8.42		All	
Buyer's Earnings:	\$2.28		\$1.49		\$0.97		\$0.58			
Buyer's Share:	76%		50%		32%		19%			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$\frac{P}{O}$	-0.435***	-0.435***	-0.677***	-0.676***	-0.733***	-0.735***	-0.708***	-0.707***	-0.638***	-0.638***
	(0.045)	(0.045)	(0.056)	(0.056)	(0.054)	(0.055)	(0.055)	(0.055)	(0.044)	(0.044)
Constant	1.319***	1.316***	1.471***	1.484***	1.396***	1.405***	1.325***	1.337***	1.522***	1.529***
	(0.035)	(0.040)	(0.044)	(0.046)	(0.050)	(0.057)	(0.051)	(0.055)	(0.037)	(0.039)
Ind. Clusters	178	178	178	178	178	178	178	178	178	178
Price Dummies										Yes
Order Control		Yes		Yes		Yes		Yes		Yes
Observations	1246	1246	1246	1246	1246	1246	1246	1246	4984	4984
R-Squared	0.111	0.111	0.168	0.169	0.144	0.144	0.127	0.127	0.185	0.185
Panel B: Study 1B With Earnings Displayed										
	Dependent Variable: Purchase Virtual Product									
Buyer's Selling Price:	\$6.72		\$7.51		\$8.03		\$8.42		All	
Buyer's Earnings:	\$2.28		\$1.49		\$0.97		\$0.58			
Buyer's Share:	76%		50%		32%		19%			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$\frac{P}{O}$	-0.233***	-0.233***	-0.378***	-0.380***	-0.524***	-0.528***	-0.503***	-0.503***	-0.409***	-0.410***
	(0.041)	(0.041)	(0.050)	(0.050)	(0.051)	(0.051)	(0.051)	(0.051)	(0.040)	(0.040)
Constant	1.155***	1.149***	1.216***	1.203***	1.177***	1.151***	1.069***	1.078***	1.332***	1.322***
	(0.036)	(0.039)	(0.043)	(0.045)	(0.049)	(0.054)	(0.055)	(0.058)	(0.036)	(0.038)
Ind. Clusters	180	180	180	180	180	180	180	180	180	180
Price Dummies										Yes
Order Control		Yes		Yes		Yes		Yes		Yes
Observations	1260	1260	1260	1260	1260	1260	1260	1260	5040	5040
R-Squared	0.0453	0.0455	0.0633	0.0638	0.0727	0.0740	0.0617	0.0618	0.159	0.159

Notes: Participants' average purchase rate of the virtual product conditional on the earnings. I control for game period. Robust standard errors in parentheses. Standard errors clustered at the individual level. Significance: *** p<0.01, ** p<0.05, * p<0.1.

and the original price on the purchasing rate. The basic regression specification is:

$$Purchase_i = \alpha + \beta_1 \times \textit{Perceived \% Change}_i + \beta_2 \times \textit{Perceived \% Change} \times \textit{Mark-up}_i$$
$$+ \beta_3 \times \textit{Mark-up}_i + \beta_4 \times \textit{Selling Price}_i + \epsilon_i$$

where $Purchase_i$ is whether participant i decided to purchase the item or not in the *Discount and Mark-up Game*; $\textit{Perceived \% Change}_i$ is the perceived discount (in percentage); and $\textit{Perceived \% Change} \times \textit{Mark-up}_i$ is the perceived mark-up (in percentage). I control for the game period and use dummies to control for the selling price. I cluster the random error, ϵ_i at the individual level.

Table A.1.3: Perceived Percentage Change Effect on Purchasing Rate
Study 1A: Discount and Mark-up Game (Baseline)

	Dependent Variable: Purchase Virtual Product				
Buyer's Selling Price:	\$6.72	\$7.51	\$8.03	\$8.42	
Buyer's Earnings:	\$2.28	\$1.49	\$0.97	\$0.58	All
Buyer's Share:	76%	50%	32%	19%	
	(1)	(2)	(3)	(4)	(5)
Perceived % Change	0.1126***	0.1411***	0.3459***	0.4841***	0.2708***
	(0.0402)	(0.0523)	(0.0742)	(0.0875)	(0.0435)
Perceived % Change × Mark-Up	-0.6557***	-0.6892***	-0.6950***	-0.7540***	-0.6981***
	(0.1284)	(0.1128)	(0.1262)	(0.1186)	(0.0732)
Mark-Up	-0.0643**	-0.1986***	-0.2321***	-0.1981***	-0.1734***
	(0.0316)	(0.0353)	(0.0357)	(0.0322)	(0.0211)
Constant	0.9514***	0.9348***	0.7695***	0.6940***	0.9805***
	(0.0216)	(0.0234)	(0.0339)	(0.0378)	(0.0173)
Selling Price: \$7.51					-0.0900***
					(0.0125)
Selling Price: \$8.03					-0.2215***
					(0.0206)
Selling Price: \$8.42					-0.2674***
					(0.0232)
Ind. Clusters	178	178	178	178	178
Price Dummies					Yes
Order Control	Yes	Yes	Yes	Yes	Yes
Observations	1246	1246	1246	1246	4984
R-Squared	0.127	0.200	0.163	0.139	0.200

Notes: Effect of a one percent increase in perceived discount or mark-up on participants' purchase rate, conditional on earnings. I control of game period and selling price. Robust standard errors in parentheses. Standard errors clustered at the individual level. Significance: *** p<0.01, ** p<0.05, * p<0.1.

Table A.1.4 pools the data from *Study 1A: Baseline* and *Study 1B: With Earnings Displayed* and interacts the perceived percentage change with *earnings displayed* to shows its effect on participants' purchasing decisions. The basic regression specification is:

$$
\begin{aligned}
Purchase_i = \alpha &+ \beta_1 \times \textit{Perceived \% Change} \times \textit{Mark-up} \times \textit{Earnings Displayed}_i \\
&+ \beta_2 \times \textit{Perceived \% Change} \times \textit{Mark-up}_i \\
&+ \beta_3 \times \textit{Perceived \% Change} \times \textit{Earnings Displayed}_i \\
&+ \beta_4 \times \textit{Earnings Displayed}_i \\
&+ \beta_5 \times \textit{Perceived \% Change}_i \\
&+ \beta_6 \times \textit{Mark-up}_i \\
&+ \beta_7 \times \textit{Selling Price}_i + \epsilon_i
\end{aligned}
$$

Table A.1.4: Displaying Earnings Increases Consumption Utility Salience
Study 1A & 1B: Discount and Mark-up Game

	Dependent Variable: Purchase Virtual Product				
Buyer's Selling Price:	\$6.72	\$7.51	\$8.03	\$8.42	
Buyer's Earnings:	\$2.28	\$1.49	\$0.97	\$0.58	All
Buyer's Share:	76%	50%	32%	19%	
	(1)	(2)	(3)	(4)	(5)
Perceived % Change × Mark-Up × Earnings Displayed	0.4046***	0.5970***	0.4098***	0.4093***	0.4570***
	(0.1221)	(0.1503)	(0.1494)	(0.1505)	(0.1186)
Perceived % Change × Mark-Up	-0.7462***	-0.7393***	-0.9191***	-0.8170***	-0.8065***
	(0.1071)	(0.1109)	(0.1153)	(0.1173)	(0.0801)
Perceived % Change × Earnings Displayed	-0.1475**	-0.0805	-0.0741	-0.1954*	-0.1249*
	(0.0645)	(0.0849)	(0.1092)	(0.1154)	(0.0666)
Earnings Displayed	0.0269	-0.0001	-0.0356	-0.0529	-0.0157
	(0.0191)	(0.0281)	(0.0424)	(0.0442)	(0.0267)
Perceived % Change	0.1384***	0.1562***	0.4048***	0.5018***	0.3010***
	(0.0420)	(0.0571)	(0.0731)	(0.0872)	(0.0456)
Mark-Up	-0.0373*	-0.1844***	-0.1625***	-0.1793***	-0.1409***
	(0.0204)	(0.0242)	(0.0243)	(0.0240)	(0.0143)
Constant	0.9420***	0.9154***	0.7301***	0.6860***	0.9796***
	(0.0181)	(0.0219)	(0.0318)	(0.0345)	(0.0181)
Selling Price: \$7.51					-0.0870***
					(0.0096)
Selling Price: \$8.03					-0.2454***
					(0.0157)
Selling Price: \$8.42					-0.3120***
					(0.0177)
Ind. Clusters	358	358	358	358	358
Price Dummies					Yes
Order Control	Yes	Yes	Yes	Yes	Yes
Observations	2506	2506	2506	2506	10024
R-Squared	0.101	0.148	0.118	0.105	0.180

Notes: Effect of displaying earnings and a one percent increase in perceived discount or mark-up on participants' purchase rate, conditional on earnings. I control of game period and selling price. Robust standard errors in parentheses. Standard errors clustered at the individual level. Significance: *** p<0.01, ** p<0.05, * p<0.1.

Table A.1.5: Robustness Check: Lower Bound of Trade-off Between Earnings and Perceived Mark-up

Study 1B: Discount and Mark-up Game (With Earnings Displayed)

	Dependent Variable: Purchase Virtual Product								
	Mark-up 0-40%			Mark-up 10-40% Only			Loss Aversion Adjusted		
Selling Price:	All	$\geq$ \$7.51	$\geq$ \$8.03	All	$\geq$ \$7.51	$\geq$ \$8.03	All	$\geq$ \$7.51	$\geq$ \$8.03
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Earnings (in \$)	0.239***	0.318***	0.220***	0.256***	0.322***	0.228***	0.244***	0.325***	0.226***
	(0.017)	(0.030)	(0.047)	(0.019)	(0.032)	(0.051)	(0.018)	(0.030)	(0.047)
Perceived Mark-Up (in \$)	-0.088***	-0.089***	-0.090***	-0.048***	-0.037***	-0.045***	-0.051***	-0.037***	-0.045***
	(0.010)	(0.010)	(0.011)	(0.010)	(0.011)	(0.012)	(0.011)	(0.011)	(0.012)
Mark-Up							-0.093***	-0.134***	-0.117***
							(0.021)	(0.025)	(0.029)
Constant	0.428***	0.355***	0.424***	0.345***	0.264***	0.335***	0.452***	0.390***	0.456***
	(0.041)	(0.048)	(0.055)	(0.046)	(0.052)	(0.061)	(0.041)	(0.049)	(0.056)
Ind. Clusters	180	180	180	180	180	180	180	180	180
Order Control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2880	2160	1440	2160	1620	1080	2880	2160	1440
R-Squared	0.138	0.0865	0.0325	0.126	0.0635	0.0130	0.142	0.0927	0.0370

Notes: Trade-off between earnings and perceived mark-up. I control for game period. Robust standard errors in parentheses. Standard errors clustered at the individual level. Significance: *** p<0.01, ** p<0.05, * p<0.1.

A.1.2. Regression Specification?

I note that estimates of willingness to pay may be different depending on the concavity of the transaction utility function. In practice, a number of factors could also moderate this behavior including the credibility of the reference price or the salience of transaction utility. Visual analysis of Figure 1.3 suggest that the effect or transaction utility is fairly linear in the experimental data, with the exception of a kink going from no discount or mark-up to a perceived mark-up. As such, my trade-off analysis uses a linear regression specification.

While a willingness to sacrifice 78 cents to avoid a dollar of perceived mark-up may seem high, note that this value may contain loss aversion over *transaction utility*. Using Table 1.2 regressions (4) and (7) I estimate participant's marginal rate of substitution "removing" loss aversion. This exercise shows that participants are willing to sacrifice \$0.43 in order to avoid a dollar of perceived mark-up in *Study 1A: Baseline*. Doing the same analysis using *Study 1B: With Earnings Displayed*, when participants are shown their potential earnings the exchange rate is reduced to \$0.19 cents (see Appendix Table A.1.5, regressions (4) and (7)).[1]

[1] Estimates show that getting a mark-up reduces the purchase rate by 14.5 percentage points in *Study 1A: Baseline* and 9.3 percentage points in *Study 1B: With Earnings Displayed*. See Table 1.2 regression (7)

A.1.3. Monotonic Preferences Over Transaction Utility

Given the same buyers made decisions over different levels of transaction utility, I can explore consumers preferences over perceived discounts and mark-ups. Pooling data from *Study 1A: Baseline* and *Study 1B: With Earnings Displayed*, I find that, conditional on selling price, 4.6% of participants never buy the virtual product. About 45.5% percent of participants exhibit fully rational behavior and always buy regardless of the perceived discount or mark-up. Conditional on earnings, 30.5% of participants have a single switching point from buying to not buying exhibit monotonic preferences over perceived discounts and mark-ups. The remaining 19.5% of participants have multiple switching points. Furthermore, focusing on participants who had a single switching point, I find that 30% of participants switched at the 0% to 10% mark-up range.

A.1.4. Heterogeneity of Transaction Utility Effect

Table A.1.6 Panels A and B shows the heterogeneity of transaction utility effects in a regression framework. I am able to explore whether specific consumer traits attenuate or amplify transaction utility effects. In particular, I look at gender and whether a participant has taken a marketing 101 course. I also explore participants' self-reported measures of caring about discounts and being averse to mark-ups. To do this, I use a difference-in-differences regression that interacts the selling price as a percentage of the original price ($\frac{P}{O}$) and a binary (or categorical) measure of the four heterogeneity groups.

A.1.5. Mediation: Participants' Perception of the Deal

In *Study 1A: Baseline* and *Study 1B: With Earnings Displayed*, I also asked participants to rate their selling price on a scale from 1-5 where 1 is a "very bad deal" and 5 is a "very good deal".[2] While this measure was not incentivized, I use participants' reported perceptions of getting a good deal or a bad deal to do a mediation analysis. Figure A.1.3 in the Appendix

and Table A.1.5 regression (7) in the Appendix.

[2]This questions was asked on the same decision screen the buyer where buyers made their purchasing decision. Figure A.5.2 in Appendix A.5.1 shows an example.

Table A.1.6: Heterogeneity of Buyer's Purchase Rate

Study 1A & 1B: Discount and Mark-up Game

Panel A: Study 1A Baseline

	Dependent Variable: Purchase Virtual Product					
Group:	**Female Participant**	**Taken Marketing**	**Care Discount**	**Averse Surge**	**Lab Experience**	**Last 16 Rounds**
	(1)	(2)	(3)	(4)	(5)	(6)
Group × $\frac{P}{O}$	-0.2811***	0.2522***	-0.1206***	-0.1279***	-0.0454	-0.0293
	(0.1030)	(0.0944)	(0.0410)	(0.0464)	(0.0888)	(0.0512)
Group	0.1546*	-0.1357*	0.0765**	0.0779**	0.0172	0.0196
	(0.0820)	(0.0764)	(0.0341)	(0.0376)	(0.0710)	(0.0540)
$\frac{P}{O}$	-0.4170***	-0.7296***	-0.3433***	-0.3105**	-0.6128***	-0.6230***
	(0.0905)	(0.0620)	(0.1062)	(0.1254)	(0.0660)	(0.0493)
Constant	1.4059***	1.5807***	1.3426***	1.3303***	1.5190***	1.5112***
	(0.0738)	(0.0541)	(0.0885)	(0.1024)	(0.0548)	(0.0422)
Ind. Clusters	178	153	178	178	178	178
Price Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Order Control	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4984	4284	4984	4984	4984	4984
R-Squared	0.203	0.203	0.200	0.202	0.186	0.185

Panel B: Study 1B With Earnings Displayed

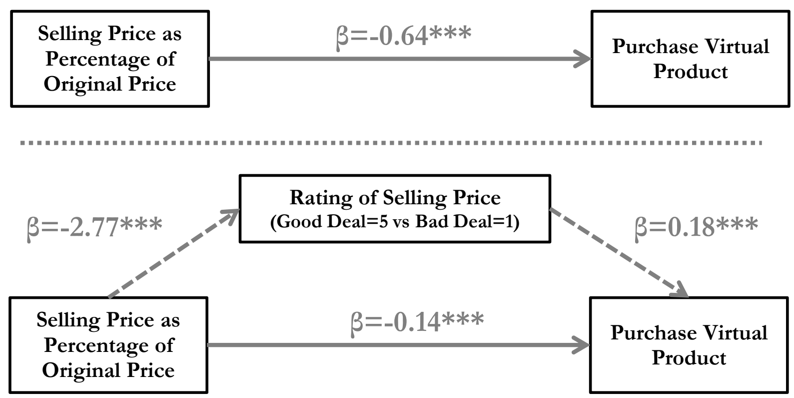
	Dependent Variable: Purchase Virtual Product					
Group:	**Female Participant**	**Taken Marketing**	**Care Discount**	**Averse Surge**	**Lab Experience**	**Last 16 Rounds**
	(1)	(2)	(3)	(4)	(5)	(6)
Group × $\frac{P}{O}$	-0.1792**	0.0486	-0.0623	-0.0772*	0.1262	-0.0100
	(0.0884)	(0.1018)	(0.0411)	(0.0410)	(0.0797)	(0.0453)
Group	0.1080	0.0086	0.0324	0.0405	-0.0608	-0.0034
	(0.0800)	(0.0901)	(0.0351)	(0.0370)	(0.0725)	(0.0472)
$\frac{P}{O}$	-0.2796***	-0.4311***	-0.2603***	-0.2025*	-0.4801***	-0.4050***
	(0.0755)	(0.0472)	(0.0924)	(0.1061)	(0.0603)	(0.0430)
Constant	1.2436***	1.3258***	1.2440***	1.2137***	1.3564***	1.3132***
	(0.0690)	(0.0444)	(0.0786)	(0.0969)	(0.0569)	(0.0419)
Ind. Clusters	180	156	180	180	180	180
Price Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Order Control	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5040	4368	5040	5040	5040	5040
R-Squared	0.166	0.165	0.164	0.168	0.166	0.159

Notes: Participants' average purchase rate of the virtual product conditional on the earnings. I control for game period and selling price. Robust standard errors in parentheses. Standard errors clustered at the individual level. Significance: *** p<0.01, ** p<0.05, * p<0.1.

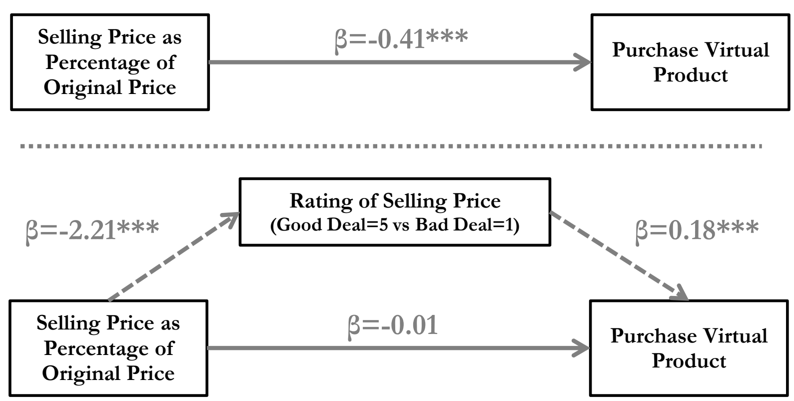
Figure A.1.3: Mediation Analysis

Study 1A & 1B: Discount and Mark-up Game

(a) Study 1A: Baseline



(b) Study 1B: With Earnings Displayed



Note: Mediation analysis: using participant's rating of the selling price. I control for game period and selling price. Standard errors clustered at the individual level. Significance: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

shows that in both *Study 1A: Baseline* and *Study 1B: With Earnings Displayed* the perceived discount or mark-up is a significant predictor of the participants' perception of the deal: as the selling price as a percentage of the original price ($\frac{P}{O}$) increase, participant's rating of the price decreases and this is significant (regression coefficient in *Study 1A: Baseline* is -2.77 and coefficient in *Study 1B: With Earnings Displayed* is -2.21).[3] Moreover, including participant's rating of the deal reduces the effect of selling price as a percentage of the original price ($\frac{P}{O}$). In *Study 1A: Baseline* the coefficient of $\frac{P}{O}$ is reduced to -13.9 percentage points (compared to 63.8 percentage points in a regression without the mediator). In *Study 1B: With Earnings Displayed* the coefficient of $\frac{P}{O}$ is reduced to -0.8 percentage points and not significant (compared to 41.1 percentage points in a regression without the mediator). This suggest, that discounts and mark-ups alter participant's perceptions of the terms of the transactions, even when consumption utility is constant, and this in turn distorts their purchasing behavior.

Table A.1.7: Buyer's Purchase Rate by Mediated by Perceived Deal
Study 1A & 1B: Discount and Mark-up Game

	Study 1A Baseline			Study 1B Earnings Displayed		
Dependent Variable:	**Purchase Virtual Product**	**Rate Deal**	**Purchase Virtual Product**	**Purchase Virtual Product**	**Rate Deal**	**Purchase Virtual Product**
	(1)	(2)	(3)	(4)	(5)	(6)
Rate Deal			0.1803***			0.1815***
			(0.0147)			(0.0142)
$\frac{P}{O}$	-0.6380***	-2.7685***	-0.1388***	-0.4101***	-2.2136***	-0.0082
	(0.0442)	(0.1052)	(0.0380)	(0.0396)	(0.1149)	(0.0337)
Constant	1.5286***	6.3858***	0.3770***	1.3218***	6.0147***	0.2299***
	(0.0393)	(0.0953)	(0.0838)	(0.0379)	(0.1080)	(0.0792)
Ind. Clusters	178	178	178	180	180	180
Price Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Order Control	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4984	4984	4984	5040	5040	5040
R-Squared	0.185	0.468	0.290	0.159	0.389	0.288

Notes: Participants' average purchase rate of the item conditional on the earnings. I control for game period and selling price. Robust standard errors in parentheses. Standard errors clustered at the individual level. Significance: *** p<0.01, ** p<0.05, * p<0.1.

[3] Also see Table A.1.7 regressions (2) and (5) in the Appendix.

A.2. Transaction Utility and Consumer Choice: Coupon Game

A.2.1. Supplemental Results

Table A.2.1 presents the results of the *Coupon Game* in a regression framework, testing the effect of extra earnings and extra discount gained from purchasing the *coupon-trap* good on participants' purchase decision. The basic regression specification is:

$$\begin{aligned} Purchase_i = &\alpha + \beta_1 \times \textit{\$4.00 Extra Discount Dummy}_i \\ &+ \beta_2 \times \textit{\$2.00 Extra Discount Dummy}_i \\ &+ \beta_3 \times \textit{-\$3.00 Extra Earnings}_i + \epsilon_i \end{aligned}$$

where $Purchase_i$ is whether participant i decided to purchase the *coupon-trap* good or not; *\$4.00 Extra Discount Dummy*$_i$ is a dummy for observing a decision where purchasing the *coupon-trap* good leads to an extra \$4.00 of discount gained; *\$2.00 Extra Discount Dummy*$_i$ is a dummy for observing decision where purchasing the *coupon-trap* good leads to using an extra \$2.00 of discount gained; *-\$3.00 Extra Earnings*$_i$ is a dummy for observing a decision where purchasing the *coupon-trap* good leads to -\$3.00 in extra earnings (compared to -\$1.00 in extra earnings). I cluster the random error, ϵ_i, at the individual level.

A.2.2. Robustness Check: "Smarter" Cohort Only

One can consider participants who passed the comprehension check on the first try as the "smarter" cohort of participants. Figure A.2.2 presents the average purchase rate of the *coupon-trap* good for individuals who passed the comprehension questions on the first attempt. Table A.2.3 presents trade-off between earnings loss and discount gain for individuals who passed the comprehension questions on the first attempt. I find results are the same including everyone or only participants who passed the comprehensions check the first time.

Table A.2.1: Rate of Purchasing the *Coupon-Trap* Good
Study 2A & 2B: Coupon Game

	Dependent Variable: Purchase *Coupon-Trap* Good							
	Study 2A Baseline				**Study 2B With Earnings Displayed**			
	All Participants		Pass Comprehension		All Participants		Pass Comprehension	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
\$4.00 Extra Discount	0.069**	0.067**	0.066*	0.063*	0.053	0.054	0.094*	0.094*
	(0.031)	(0.031)	(0.033)	(0.033)	(0.035)	(0.035)	(0.049)	(0.049)
\$2.00 Extra Discount	0.025	0.023	0.009	0.005	0.034	0.032	0.021	0.021
	(0.025)	(0.025)	(0.029)	(0.028)	(0.028)	(0.028)	(0.033)	(0.033)
Constant	0.285***	0.315***	0.239***	0.287***	0.168***	0.211***	0.087**	0.085
	(0.038)	(0.048)	(0.051)	(0.063)	(0.031)	(0.048)	(0.035)	(0.052)
-\$3.00 Extra Earnings	-0.066***	-0.066***	-0.101***	-0.103***	-0.045**	-0.044**	-0.007	-0.007
	(0.022)	(0.022)	(0.028)	(0.029)	(0.020)	(0.020)	(0.021)	(0.021)
Ind. Clusters	101	101	53	53	103	103	48	48
Order Control		Yes		Yes		Yes		Yes
Observations	606	606	318	318	618	618	288	288
R-Squared	0.00940	0.0104	0.0201	0.0229	0.00694	0.00994	0.0152	0.0153

Notes: Rate of purchasing the *coupon-trap* good, the virtual product that led to a \$5 discount gained which, by design, led to lower earnings. Regressions (3-4) and (7-8) show results including only participants who passed the comprehension question on the first try. I control of the game period. Robust standard errors in parentheses. Standard errors clustered at the individual level. Significance: *** p<0.01, ** p<0.05, * p<0.1.

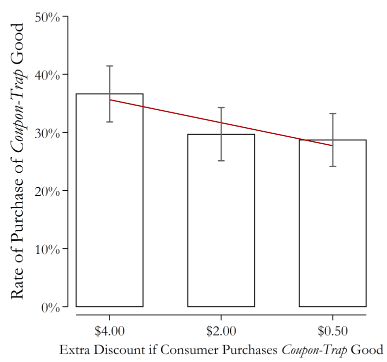
Table A.2.2: Rate of Purchasing the *Coupon-Trap* By Extra Earnings Lost
Study 2A & 2B: Coupon Game

	Dependent Variable: Purchase *Coupon-Trap* Good					
	Study 2A Baseline			**Study 2B With Earnings Displayed**		
Earnings Lost:	All	\$1.00	\$3.00	All	\$1.00	\$3.00
	(1)	(2)	(3)	(4)	(5)	(6)
Extra Discount (in \$)	0.020**	0.023*	0.017	0.015	0.027*	0.003
	(0.009)	(0.014)	(0.011)	(0.010)	(0.015)	(0.012)
Constant	0.241***	0.266***	0.215***	0.142***	0.138***	0.146***
	(0.040)	(0.049)	(0.042)	(0.033)	(0.041)	(0.038)
Ind. Clusters	101	101	101	103	103	103
Observations	606	303	303	618	309	309
R-Squared	0.00402	0.00515	0.00301	0.00319	0.00966	0.000110

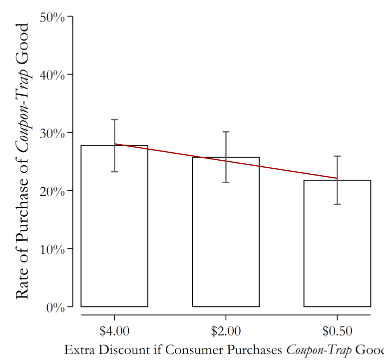
Notes: Rate of purchasing the *coupon-trap* good, the virtual product that led to a \$5 discount gained which, by design, led to lower earnings, by extra earnings lost. Robust standard errors in parentheses. Standard errors clustered at the individual level. Significance: *** p<0.01, ** p<0.05, * p<0.1.

Figure A.2.1: Rate of Purchasing the *Coupon-Trap* Good
(By Extra Earnings Lost from Purchasing the *Coupon-Trap* Good)
Study 2A & 2B: Coupon Game

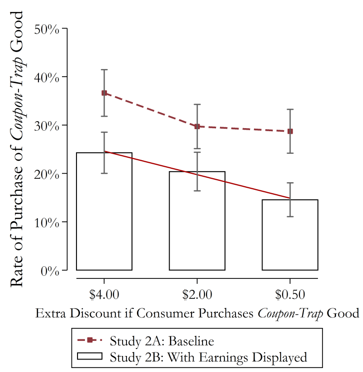
(a) Study 2A: Baseline
Earnings Lost: $1



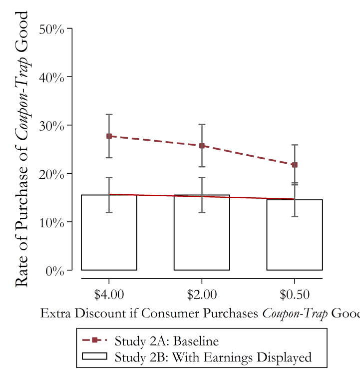
(b) Study 2A: Baseline
Earnings Lost: $3



(c) Study 2B: Earnings Displayed
Earnings Lost: $1



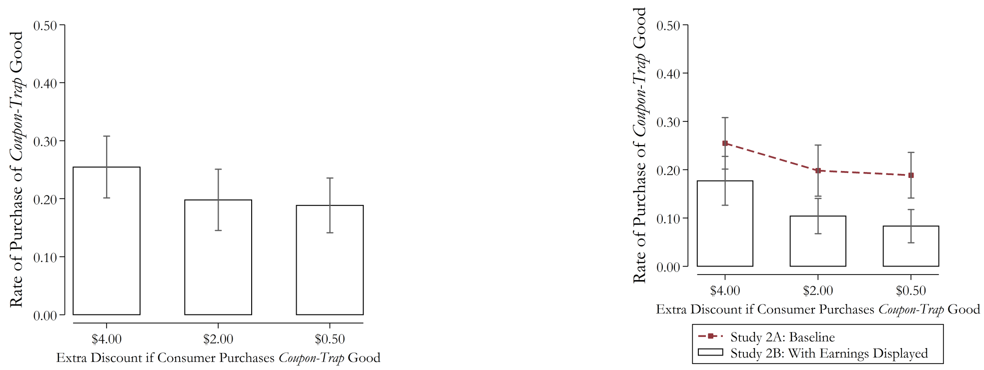
(d) Study 2B: Earnings Displayed
Earnings Lost: $3



Note: Rate of purchasing the *coupon-trap* good, the virtual product that led to a $5 discount gained which, by design, led to lower earnings, by extra earnings lost. Robust standard error bars clustered at the individual level are shown around each mean.

Figure A.2.2: Rate of Purchasing the *Coupon-Trap* Good
(Smarter Cohort: Pass Comprehension on First Try Only)
Study 2A & 2B: Coupon Game

(a) Study 2A: Baseline (b) Study 2B: Earnings Displayed



Note: Rate of purchasing the *coupon-trap* good, the virtual product that led to a $5 discount gained which, by design, led to lower earnings. Results includes only participants who passed the comprehension question on the first try only. Results are consistent with using all data. Robust standard error bars clustered at the individual level are shown around each mean.

Table A.2.3: Rate of Purchasing the *Coupon-Trap* Good
(Smarter Cohort)
Study 2A & 2B: Coupon Game

Dependent Variable: Purchase of *Coupon-Trap* Good				
	Study 2A Baseline		**Study 2B With Earnings Displayed**	
	OLS	Probit	OLS	Probit
	(1)	(2)	(3)	(4)
Extra Earnings	-0.051***	-0.051***	-0.003	-0.003
	(0.014)	(0.014)	(0.011)	(0.011)
Extra Discount	0.019*	0.018*	0.027*	0.027*
	(0.010)	(0.009)	(0.014)	(0.014)
Constant	0.320***		0.067	
	(0.073)		(0.059)	
Ind. Clusters	53	53	48	48
Order Control	Yes	Yes	Yes	Yes
Observations	318	318	288	288

Notes: Rate of purchasing the *coupon-trap* good, the item that uses up the entire value of the coupon which, by design, leads to a lower earnings. Results includes only participants who passed the comprehension question on the first try only. Results are consistent with using all data. I control of the game period. Robust standard errors in parentheses. Standard errors clustered at the individual level. Significance: *** p<0.01, ** p<0.05, * p<0.1.

A.2.3. Monotonic Preferences Over Transaction Utility

Given the same buyers made decisions over different levels of transaction utility, I can explore consumers' preferences over different levels of extra discount gained from purchasing the *coupon-trap* good. Pooling data from *Study 2A: Baseline* and *Study 2B: With Earnings Displayed*, I find that, conditional on earnings, 63% of participants exhibit fully rational behavior and never buy the *coupon-trap* good. Conditional on earnings, 15% of participants buy the *coupon-trap* good once, 11% of participants buy the *coupon-trap* good twice, and 10% of participants always buy the *coupon-trap* good. Moreover, of the 26% of participants who choose to buy the *coupon-trap* good at least once, 81% of them exhibit monotonic preferences over extra discount gained. The remaining 19% of participants have multiple switching points.

A.2.4. Heterogeneity of Transaction Utility Effect

Following Study 1, I explore consumer heterogeneity in the *Coupon Game* in Table A.2.4. Looking at the same four groups as before: gender, whether a participant has taken a marketing 101 course, and participants' self-reported measures of caring about discounts and being averse to mark-ups, I use a difference-in-differences regression that interacts the extra discount if consumers purchase the *coupon-trap* good and a binary (or categorical) measure of the four heterogeneity groups. I do not find statistically significant differences between those groups, possibly due to my lack of statistical power in this game with only 6 choices per subject.

Table A.2.4: Heterogeneity of Buyer's Purchase Rate of *Coupon-Trap* Good
Study 2A & 2B: Coupon Game

	Dependent Variable: Purchase of *Coupon-Trap* Good					
Group:	**Female Participant**	**Taken Marketing**	**Care Discount**	**Averse Surge**	**Lab Experience**	**Last 3 Rounds**
	(1)	(2)	(3)	(4)	(5)	(6)
Group × Extra Discount	-0.0043	-0.0026	0.0000	0.0061	-0.0091	-0.0009
	(0.0151)	(0.0140)	(0.0085)	(0.0071)	(0.0138)	(0.0149)
Extra Discount	0.0203	0.0179**	0.0170	0.0011	0.0210***	0.0176*
	(0.0130)	(0.0082)	(0.0232)	(0.0197)	(0.0076)	(0.0103)
Group	-0.0076	0.0689	0.0113	-0.0272	0.0446	0.0164
	(0.0637)	(0.0573)	(0.0294)	(0.0307)	(0.0524)	(0.0466)
-$3.00 Extra Earnings	-0.0546***	-0.0545***	-0.0545***	-0.0546***	-0.0545***	-0.0542***
	(0.0148)	(0.0148)	(0.0148)	(0.0147)	(0.0148)	(0.0149)
Display Earnings	-0.1092**	-0.0975**	-0.1081**	-0.1061**	-0.1098**	-0.1090**
	(0.0453)	(0.0446)	(0.0451)	(0.0451)	(0.0453)	(0.0452)
Constant	0.3159***	0.2807***	0.2814***	0.3793***	0.2911***	0.3146***
	(0.0684)	(0.0472)	(0.0900)	(0.0962)	(0.0473)	(0.0470)
Ind. Clusters	204	204	204	204	204	204
Order Control	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1224	1224	1224	1224	1224	1224
R-Squared	0.0268	0.0315	0.0271	0.0281	0.0276	0.0266

Notes: Rate of purchasing the *coupon-trap* good, the item that uses up the entire value of the coupon which, by design, leads to a lower earnings. I control of the game period. Robust standard errors in parentheses. Standard errors clustered at the individual level. Significance: *** p<0.01, ** p<0.05, * p<0.1.

A.3. The Myth of the Male Negotiator: Gender's Effect on Negotiation Strategies and Outcomes

Participants were randomly assigned to the Informed and Uninformed treatment at the session level. Table A.3.1 provides summary statistics of participant's characteristics between these two treatments. Fifty-five men and 55 women were uninformed of their negotiating partner's gender and 61 men and 61 women were informed of their negotiating partner's gender. We find these two groups are balanced on all characteristics with the exception of being a US citizen. Our results are robust to controlling for a number of individual controls, including being a US citizen, and session controls.

Table A.3.1: Summary Statistics

	Informed	**Uninformed**	***p*-value**
N:	**122**	**110**	
Male	.5	.5	1
	(.045)	(.048)	
Age	21.066	20.736	.584
	(.428)	(.421)	
Non-white	.721	.627	.129
	(.041)	(.046)	
Employment Status	.364	.382	.777
	(.044)	(.047)	
Native English Speaker	.843	.9	.195
	(.033)	(.029)	
US Citizen	.785	.882	.048
	(.037)	(.031)	
Politically Liberal	.861	.882	.632
	(.031)	(.031)	

Notes: This table presents the summary statistics of participant's characteristics. Fifty-five men and 55 women were uninformed of their negotiating partner's gender and 61 men and 61 women were informed of their negotiating partner's gender. When the balance test is restricted to men only, there is a marginally significant difference in being a US citizen and a native English speaker between informed and uninformed conditions. Standard deviations in parentheses.

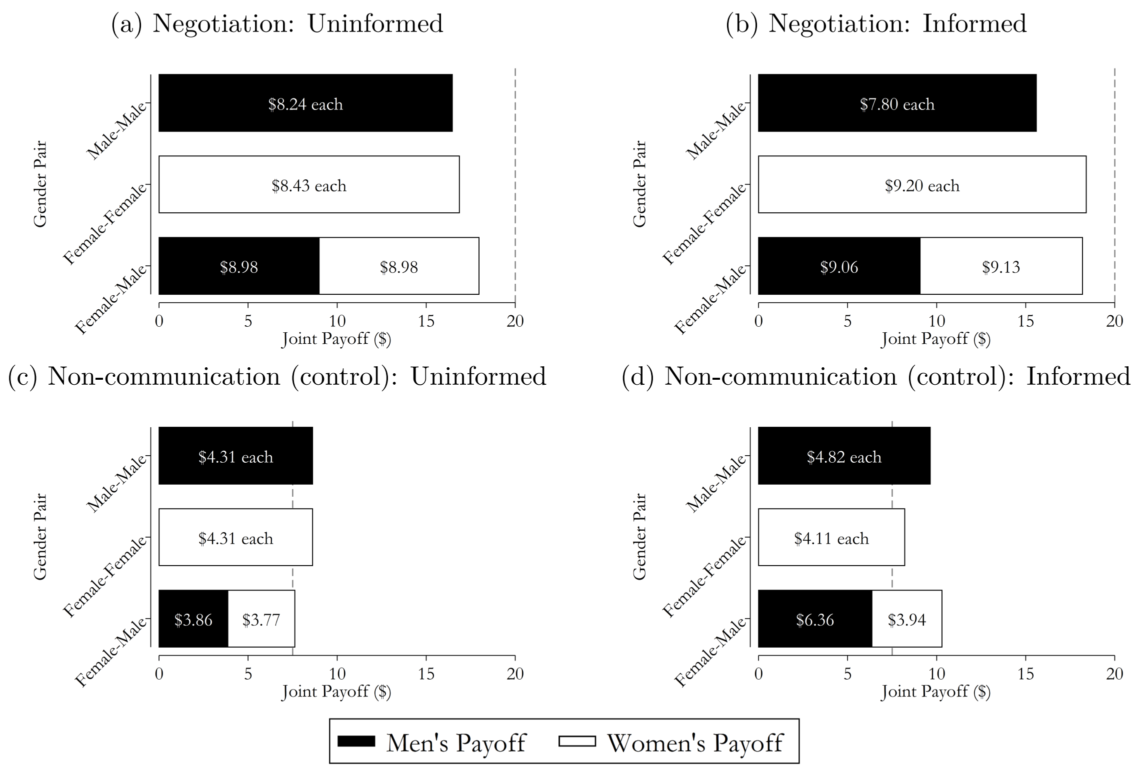
Figure A.3.1 summarizes the joint payoffs results in all treatments. Panel A shows results from the control game when participants are uninformed about gender. We find that the joint payoff is close to the mixed strategy equilibrium. Male-male and female-female pairs payoffs are slightly higher than mixed-gender pair payoff, but not significantly so. In mixed gender pairs, the joint payoff is exactly divided between the two partners.

Panel B presents results when gender information is introduced in the control game. The joint payoff of mixed-gender pairs increases sharply, but it is men who reap the rewards from this increase. Men play more "hawkishly" when they know they're playing female partners, as previous literature has shown. Because women also play more dovishly against men, this results in both more coordination (higher joint payoff) and higher payoffs for men. Men also play slightly *less* hawkishly against known male partners, which increases payoffs to male-male pairs.

Panels D show that results of introducing gender information in the negotiation game are starkly different. As a baseline, in Panel C, when participants are uninformed, coordination is markedly higher with communication, as expected, although not quite at the fully efficient level of 100% coordination. Furthermore, there are no notable gender asymmetries in payoffs, indicating that one gender does not appear "better" at negotiating in this game than the other. Importantly, in Panel D, introducing gender information does not change to the payoff allocation between male and female partners in mixed gender pairs, in contrast to the control game without communication. Moreover, when informed, male-male pairs perform notably worse than all other pair types, achieving only $15 out of a joint payoff of a possible $20.

In both the control and negotiation game, participants face three possible action choice pairs: ($15, $5), ($15, $15), and ($5, $5). In Figure A.3.2 we plot the frequency of each action pair by gender-pair type for the negotiation game when partners are informed (in Panel A) versus uninformed (in Panel B) of their partner's gender. As expected, we find high coordination across all gender pairs with and without gender information and mis-matching on ($5, $5) is reduced to almost zero. Furthermore more, in mixed gender pairs, frequency of man and women choosing $15 for themselves when their partner chooses $5 (i.e., the ($15, $5) action choice pair) is equal between the two genders. We find gender information does not change coordination in mixed-gender pairs, and increases coordination in female-female

Figure A.3.1: Joint Payoff by Treatment and Gender-Pair Type



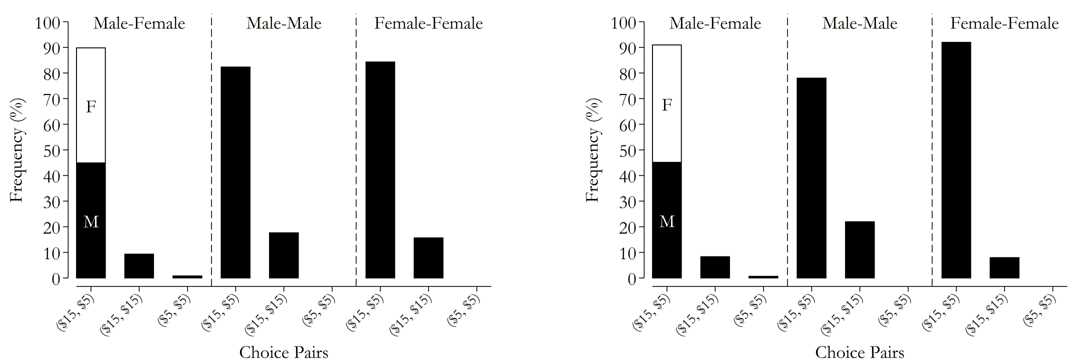
Notes: Average joint payoff by treatment and gender-pair type. The payoff split between male and female partners is shown in mixed-gender pairs.

pairs. Regardless of gender information, mixed-gender pairs coordinate about 90% of the time, mis-coordinate on ($15, $5) about 9% of the time, and mis-coordinate on ($5, $5) less than 1% of the time.

In contrast, we find that gender information reduced coordination for male-male pairs and increases coordination for female-female pairs. When male-male pairs are uninformed, they coordinate 82% of the time and mis-coordination on ($15, $15) about 18% of the time. However, introducing gender information reduced coordination for male-male pairs: choosing ($15, $5) only 78% of the time and increasing mis-coordination on ($15, $15) about 22% of the time. Meanwhile, uninformed female-female pairs coordinate 84% of the time and mis-coordination on ($15, $15) about 15% of the time and introducing gender information increases coordination to 92% of the time and decreases mis-coordination on ($15, $15) to 8% of the time.

Figure A.3.2: Frequency of Action Choice Pair by Gender-Pair

(a) Negotiation, Uninformed (b) Negotiation, Informed



Notes: Frequency of choice pairs by gender pair type. For mixed-gender pairs we separate the pair choice by the frequency the men or women chose the higher payoff in the case of ($15, $5) action outcome.

Figure 2.2 in section 2.3.2.1 showed that in the informed negotiation game men's action when gender is known appears to invert compared to the optimal tailoring observed in the control game. Table A.3.2 presents these results in a regression framework. Regressions (1) and (2) present results for male participants in the informed treatment. We find that in the negotiation game, men's play when gender is known appears to invert from the optimal tailoring: they choose \$15 for themselves more often against men, and less often against women. This reversal is marginally significant without controls (see regression (1)), and highly significant in the regression with controls (see regression (2)). Regressions (3) and (4) show that these effects are absent in the setting with no gender information, indicating that it is men being unable to capitalize on gender information to tailor behavior in the negotiation game that drives their negotiation disadvantage.

Table A.3.2: Choosing \$15 by Treatment and Partner Gender (Men Only)

	Dependent variable: Choosing \$15			
	Informed		Uninformed	
	(1)	(2)	(3)	(4)
Negotiation × Partner Female	-0.163*	-0.232***	0.0213	0.0375
	(0.0935)	(0.0870)	(0.0971)	(0.0965)
Partner Female	0.0879	0.124**	-0.0671	-0.0854
	(0.0584)	(0.0545)	(0.0702)	(0.0702)
Negotiation	-0.0596	-0.161*	-0.157**	-0.183**
	(0.0669)	(0.0886)	(0.0595)	(0.0808)
Constant	0.670***	0.745***	0.745***	0.570*
	(0.0490)	(0.216)	(0.0477)	(0.293)
Ind. Clusters	61	61	55	55
Controls		YES		YES
Observations	488	488	440	440
R-Squared	0.032	0.107	0.026	0.081

Notes: Robust standard errors clustered at the individual level are in parentheses. Session controls include day of the week, within day trend, and game round. Individual controls include subject's age, being nonwhite, begin liberal, being a US citizen, being a native English speaker, employment status, and the number of sessions completed. Significance: *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Figure 2.3 in section 2.3.2.2 shows that men's tailoring of negotiation approach also goes in the opposite direction than expected: men are substantially less likely to use aggressive communication strategies against female compared to male partners and much more likely to use yielding communication strategies against female compare to male partners. Table A.3.3 shows these results in a regression framework when participants are informed (in Panel A) and uninformed (in Panel B) of their partner's gender. Panel A shows that men are much less likely to use the *hard commitment* strategy and be a *tough talker* against

Table A.3.3: Tailoring of Communication Strategies by Gender and Partner Gender

Panel A: Informed Only

	Aggressive Strategies				**Yielding Strategies**			
Dependent variable:	Hard Commitment		Tough Talker		Offer $15		Friendly Negotiator	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Male × Partner Female	-0.181***	-0.184***	-0.157***	-0.154***	0.116*	0.099	0.215***	0.199***
	(0.055)	(0.056)	(0.052)	(0.051)	(0.067)	(0.064)	(0.053)	(0.056)
Male	0.171***	0.181***	0.154***	0.154***	-0.032	-0.035	-0.136***	-0.136***
	(0.060)	(0.063)	(0.048)	(0.047)	(0.056)	(0.053)	(0.046)	(0.049)
Partner Female	0.034	0.030	0.032	0.026	-0.034	-0.015	-0.080**	-0.067*
	(0.029)	(0.032)	(0.030)	(0.031)	(0.047)	(0.046)	(0.034)	(0.037)
Constant	0.098***	-0.065	0.069***	-0.024	0.293***	0.205	0.592***	0.695***
	(0.026)	(0.165)	(0.015)	(0.137)	(0.037)	(0.186)	(0.027)	(0.153)
Ind. Cluster	122	121	122	121	122	121	122	121
Controls		YES		YES		YES		YES
Observations	488	484	488	484	488	484	488	484
R-Squared	0	0	0	0	0	0	0	0

Panel B: Uninformed Only

	Aggressive Strategies				**Yielding Strategies**			
Dependent variable:	Hard Commitment		Tough Talker		Offer $15		Friendly Negotiator	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Male × Partner Female	-0.079	-0.081	-0.055	-0.059	0.006	-0.015	0.036	0.043
	(0.052)	(0.051)	(0.044)	(0.045)	(0.062)	(0.064)	(0.056)	(0.053)
Male	0.147***	0.127***	0.065**	0.049	-0.002	-0.005	-0.070	-0.059
	(0.048)	(0.046)	(0.032)	(0.031)	(0.057)	(0.057)	(0.049)	(0.046)
Partner Female	0.039	0.036	0.056*	0.053*	0.024	0.034	-0.006	-0.001
	(0.028)	(0.031)	(0.031)	(0.032)	(0.041)	(0.044)	(0.037)	(0.037)
Constant	0.079***	-0.135	0.076***	-0.104	0.248***	0.029	0.575***	0.621***
	(0.020)	(0.252)	(0.019)	(0.171)	(0.040)	(0.211)	(0.033)	(0.197)
Ind. Cluster	110	110	110	110	110	110	110	110
Controls		YES		YES		YES		YES
Observations	440	440	440	440	440	440	440	440
R-Squared	0	0	0	0	0	0	0	0

Notes: Robust standard errors clustered at the individual level are in parenthesis. Session controls include day of the week, within day trend, and game round. Individual controls include subject's age, being nonwhite, begin politically liberal, being a US citizen, being a native English speaker, employment status, and the number of sessions completed. Significance: *** p<0.01, ** p<0.05, * p<0.1

female partners and this is significant at the 1% level. Moreover, we find that results are directionally consistent for *offer $15* and statistically significant at the 1% level for *friendly negotiator*. In Panel B, we check that when participants are uninformed there is no significant tailoring of men's strategies.

Moreover, Table A.3.4 provides a further robustness check. Here we show that men's tailoring toward women is significantly different when informed versus uninformed for *hard commitment*, being a *tough talker*, and being a *friendly negotiator*. This suggest that the paradoxical tailor is driven by gender information since only when men are informed does the decreased use of aggressive strategies and increased use of yielding strategies toward women appear.

Table A.3.4: Use of Aggressive and Yielding Communication Strategies by Information Treatment (Men Only)

	Aggressive Strategies				**Yielding Strategies**			
Dependent variable:	Hard Commitment		Tough Talker		Offer $15		Friendly Negotiator	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Partner Female × Informed	-0.107*	-0.135**	-0.127**	-0.135***	0.052	0.072	0.104*	0.114**
	(0.064)	(0.061)	(0.053)	(0.051)	(0.066)	(0.066)	(0.058)	(0.057)
Informed	0.042	0.026	0.083	0.070	0.015	0.034	-0.048	-0.032
	(0.070)	(0.081)	(0.052)	(0.061)	(0.058)	(0.055)	(0.053)	(0.055)
Partner Female	-0.040	-0.037	0.001	-0.002	0.031	0.026	0.030	0.032
	(0.043)	(0.039)	(0.032)	(0.030)	(0.046)	(0.047)	(0.042)	(0.038)
Constant	0.226***	0.209	0.141***	0.155	0.247***	0.263	0.504***	0.491***
	(0.044)	(0.241)	(0.025)	(0.194)	(0.040)	(0.203)	(0.036)	(0.179)
Ind. Cluster	116	116	116	116	116	116	116	116
Controls		YES		YES		YES		YES
Observations	464	464	464	464	464	464	464	464
R-Squared	0	0	0	0	0	0	0	0

Notes: Robust standard errors clustered at the individual level are in parentheses. Session controls include day of the week, within day trend, and game round. Individual controls include subject's age, being nonwhite, begin liberal, being a US citizen, being a native English speaker, employment status, and the number of sessions completed. Significance: *** p<0.01, ** p<0.05, * p<0.1

Table A.3.5 shows that the presence of women in the negotiation significantly improves efficiency. Having a woman in the negotiation (either on one side or both sides), in the combined sample with both gender information treatments, leads to an approximately $2 increase in joint payoff. This effect is primarily driven by the public gender information setting, shown in regressions (3) and (4) where negotiating pairs with at least one woman (female-male or female-female) earn around $3 more compared to male-male pairs. However, even in the no gender information setting, in regressions (5) and (6) the results are directionally consistent, indicating that men's tendency toward more aggressive communication is also associated with lower coordination in this setting. As a result, the two settings are not statistically significantly different from one another in regressions (7) and (8). Nevertheless, it appears that the presence of gender information exacerbates men's aggressive tendencies toward one another. And, the fact that it is more strongly present when gender information is known, and associated with decreased payoffs, show that this is not just about the optimal deployment of "driving a hard bargain," but rather a response to the partner's gender.

Table A.3.5: Efficiency Impacts of Women (Negotiation Only)

	Dependent variable: Payoff | Negotiation							
	All		Informed		Uninformed		All	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Any Women	1.922**	2.134**	2.647**	3.478**	1.163	1.239	1.163	1.183
	(0.856)	(0.898)	(1.245)	(1.446)	(1.183)	(1.211)	(1.183)	(1.186)
Any Women × Informed							1.485	1.935
							(1.718)	(1.833)
Informed							-0.871	-1.212
							(1.592)	(1.699)
Constant	16.040***	18.129***	15.600***	14.651***	16.471***	31.807***	16.471***	18.175***
	(0.795)	(4.235)	(1.176)	(5.097)	(1.073)	(9.776)	(1.072)	(4.181)
Pair Clusters	464	464	244	244	220	220	464	464
Controls		YES		YES		YES		YES
Observations	464	464	244	244	220	220	464	464
R-Squared	0.015	0.056	0.028	0.082	0.005	0.108	0.017	0.060

Notes: Robust standard errors clustered at the individual level are in parenthesis. Session controls include day of the week, within day trend, and game round. Individual controls include subject's age, being nonwhite, begin liberal, being a US citizen, being a native English speaker, employment status, and the number of sessions completed. Significance: *** p<0.01, ** p<0.05, * p<0.1

Table A.3.6 presents the probability of matching and getting the preferred payoff (that is $15) conditional on matching by communication strategy. First, in Panel A, we look at how aggressive and yielding communication strategies affect coordination in the negotiation game when gender is known. We find that using the *hard commitment* strategy or being a tough talker decreases the probability of coordinating by 60 percentage points. However, this is less so when used againt female partners. In contrast, choosing to yield and *offer $15* to their partner increases matching by 40 percentage points and being a *friendly negotiator* increases matching by 58 percentage points. With the effect attenuated but still increasing coordination when used against female partners.

In Panel B, we explor how using these strategies affects getting the $15 payoff conditional on matching. First, we find that using conditional on matching, using the *hard commitment* strategy and being a *touch talker* is effective. However, notices that the benefits of geing the higher payoff are exactly offset by the probability of negotiation breakdown from using these strategies. Focusing on yielding strategies, we find that offer $15, obviously, reducing the probabiliy of getting the higher payoff by almost 90 percentage points. Finally, being a friendly negotiator appears very effective. While the probability of getting the $15 payoff is reduced by 17-27 percentage points, this is a much smaller effect compared to the gains of being a friendly negotiator and increasing effiency by 58 percentage points.

We used MTurk workers to classify the negotiations according to several definitions we provided. Appendix A.7 provides details on the negotiation transcript coding protocol. Table A.3.7 provides the definition and average usage rate for all strategies coded (no including the ones presented in the main paper) by gender and partner gender. We also perform a two-sided t-test between difference in tailoring of strategies by men and women based on partner's gender when participants are informed.

Table A.3.6: Matching and Getting $15 Payoff | Matching by Aggressive and Yielding Communication Measures

Panel A: Matching

	Aggressive Strategies				**Yielding Strategies**			
Dependent variable:	Hard Commitment		Tough Talker		Offer $15		Friendly Negotiator	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Strategy	-0.577***	-0.569***	-0.595***	-0.603***	0.398***	0.397***	0.579***	0.561***
	(0.117)	(0.107)	(0.121)	(0.118)	(0.081)	(0.092)	(0.108)	(0.111)
Strategy × Partner Female	0.334**	0.334**	0.227	0.239	-0.354***	-0.340***	-0.402***	-0.396***
	(0.158)	(0.163)	(0.157)	(0.166)	(0.112)	(0.103)	(0.119)	(0.121)
Partner Female	0.004	0.023	0.033	0.051	0.218***	0.234***	0.289***	0.306***
	(0.044)	(0.042)	(0.042)	(0.041)	(0.062)	(0.065)	(0.093)	(0.092)
Constant	0.935***	0.835***	0.913***	0.808***	0.676***	0.527***	0.516***	0.405**
	(0.040)	(0.164)	(0.039)	(0.169)	(0.062)	(0.190)	(0.081)	(0.187)
Ind. Cluster	61	61	61	61	61	61	61	61
Controls		YES		YES		YES		YES
Observations	244	244	244	244	244	244	244	244
R-Squared	0	0	0	0	0	0	0	0

Panel B: $15 Payoff | Matching

	Aggressive Strategies				**Yielding Strategies**			
Dependent variable:	Hard Commitment		Tough Talker		Offer $15		Friendly Negotiator	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Strategy	0.601***	0.529***	0.614***	0.542***	-0.895***	-0.852***	-0.273	-0.172
	(0.117)	(0.120)	(0.114)	(0.126)	(0.081)	(0.091)	(0.206)	(0.199)
Strategy × Partner Female	-0.018	0.078	-0.103	-0.030	0.070	0.024	-0.073	-0.180
	(0.148)	(0.183)	(0.201)	(0.239)	(0.111)	(0.118)	(0.238)	(0.235)
Partner Female	0.030	0.002	0.040	0.011	-0.007	-0.011	0.060	0.096
	(0.074)	(0.077)	(0.076)	(0.078)	(0.074)	(0.075)	(0.141)	(0.136)
Constant	0.408***	0.433	0.418***	0.465	0.793***	0.933***	0.646***	0.550
	(0.063)	(0.323)	(0.063)	(0.328)	(0.061)	(0.301)	(0.126)	(0.370)
Ind. Cluster	61	61	61	61	61	61	61	61
Controls		YES		YES		YES		YES
Observations	209	209	209	209	209	209	209	209
R-Squared	0	0	0	0	0	0	0	0

Notes: Robust standard errors clustered at the individual level are in parentheses. Session controls include day of the week, within day trend, and game round. Individual controls include subject's age, being nonwhite, begin liberal, being a US citizen, being a native English speaker, employment status, and the number of sessions completed. Significance: *** p<0.01, ** p<0.05, * p<0.1

Table A.3.7: Other Communication Measures
Informed Treatment Only

Strategy	Definition	Percent Use Measure							
		Men				Women			
		Unknown Partner	Male Partner	Female Partner		Unknown Partner	Male Partner	Female Partner	
Hard Commit (First Mover)	This is the first person who used a hard commitment strategy.	11.86 (1.61)	15.04 (2.82)	6.84 (1.65)	**	3.64 (.96)	4.94 (1.35)	7.15 (2.04)	
Ask $15	This is when a person asks the other person if they can take the $15 at any point in the conversation.	17.8 (1.95)	16.11 (2.88)	14.03 (2.23)		21.16 (2.24)	19.95 (2.82)	22.33 (3.36)	
Offer $15 (First Mover)	This is when a person starts the conversation (not including saying or other pleasantries) by offering the $15 to the other person or stating that they will take $5.	17.36 (2.03)	15.75 (2.76)	19.28 (2.45)		15.74 (1.98)	19.98 (2.71)	16.23 (2.75)	
Gave in	This is when the person gives in to the other person's ask or demands after there is an initial negotiation or back-and-forth.	19.76 (1.84)	15.12 (2.65)	21.68 (2.29)	*	23.63 (2.16)	23.34 (2.48)	21.58 (3.02)	
Started Negotiation	This is the person that starts the negotiations on how to split the money, not including saying or other pleasantries.	33.15 (2.12)	33.28 (2.92)	34.97 (2.6)		37.83 (2.32)	43.59 (2.63)	34.07 (3.14)	***
Used the Word Fair	This is when the person mentions anything about trying to make a fair split.	4.67 (1.14)	4.28 (1.16)	4.1 (1.26)		4.49 (.92)	3.46 (1.05)	2.9 (.91)	
Mentioned Previous Choices	This is when the person mentions what they previously chose. Individuals had to negotiate with multiple people, so sometimes they will mention what their previous choice was.	34.52 (2.75)	15.32 (2.99)	36.06 (3.38)	***	35.58 (2.78)	30.73 (3.33)	35.75 (4.09)	
Random Game	This is the person that introduces a random game such as playing rock/paper/scissor (), guessing a number, using trivia questions, using birthday dates, or other similar games to choose who picks $15 for themselves.	8.17 (1.6)	6.82 (2.18)	9.32 (2.04)		10.05 (1.76)	8.43 (2.03)	7.6 (2.11)	
Alternating Strategy	This is when the person claims to be alternating between 5 and 15 and that this is their strategy.	12.07 (1.61)	9 (2.15)	13.82 (2.14)	*	16.02 (1.93)	10.43 (1.64)	14.8 (2.51)	
Sad Story	This is a person that uses their current (unfortunate) situation to gain sympathy from the other person and tries to get the $15.	8.27 (1.42)	5.82 (1.84)	3.95 (.99)		11.46 (1.7)	3.81 (.91)	8.82 (2.17)	*
Happy Emojis	This is when a person uses any sort of happy or smiley faces.	5.28 (1.15)	4.68 (1.7)	8.15 (1.74)	*	7.96 (1.37)	10.36 (2.06)	8.95 (2.3)	
Sad Emojis	This is when a person uses any sort of sad or angry .	2 (.48)	1.35 (.58)	1.64 (.6)		5.23 (1.13)	1.86 (.61)	1.87 (.92)	
Aggressive Score	Normalized friendly to aggressive score given to each participant by the MTurk worker based on the conversation transcript.	26.82 (1.37)	33.32 (2.72)	21.75 (1.73)	***	22.96 (1.2)	19.96 (1.38)	21.66 (1.7)	
Reached Agreement	Mturk worker's perception that the negotiation was successful?	80.14 (2.47)	73.27 (4.26)	90.97 (2.01)	***	82.38 (2.28)	90.97 (2.01)	90.3 (2.65)	

Notes: Average rate men and women use these communication measures in percent. *Hard commit (first mover)* and *offer $15 (first mover)* are robustness checks to the corresponding primary measures in the main paper. *Ask $15* and *gave in* are secondary measures for aggressive and yielding communication strategies, respectively. *Started negotiation, used the word fair, mentioned previous choices, random game,* and *alternating strategy* are different "neutral" mechanisms. *Sad story, happy emojis,* and *sad emojis* are "emotion" based strategies and styles. Finally, *aggressive score* and *reached agreement* are scored provided from the MTurk worker's perception of the negotiation as a "third party" observer. Robust standard errors clustered at the individual level in parenthesis. Stars denote significant difference in tailoring of strategies by men or women based on partner's gender. Significance: *** $p<0.01$, ** $p<0.05$, * $p<0.1$

A.4. The Value of Information and the Role of Fairness in Bargaining

A.4.1. Supplemental Results

After subjects played the 30 rounds of the bargaining game, they were asked to answer a number of additional questions, depending on their treatment and role. Table A.4.1 shows the additional questions that each type of subject answered. Data from sellers in Part 2 is not analyzed in this paper.

Table A.4.1: Additional Experimental Questions

		Buyer				**Seller**			
		II	**SK**	**BK**	**CI**	**II**	**SK**	**BK**	**CI**
Part 1	Bargaining Game (30 Rounds) (30 Rounds)	✓	✓	✓	✓	✓	✓	✓	✓
Part 2	Given available information, report the probability offers got accepted. (10 Rounds)	✓	✓	✓	✓				
	For each cost and offer, report the probability that the buyer had $V = 70$. (10 Rounds)					✓		✓	
	Questions to assess seller's perception of a fair offer. (10 Questions)						✓		✓

Notes: After subjects played the 30 rounds of the bargaining game, they were asked to answer a number of additional questions, depending on their treatment and role. This table shows the additional questions that each type of subject answered. Data from sellers in Part 2 is not analyzed in this paper.

Offers made by buyers can be classified into three categories: unfair, fair, and generous. We define a fair offer as an equal split given common knowledge with a range of ± 2 experimental units around the equal split. For example, if cost is 10 and value is 90 (giving a surplus of 70 experimental units), the equal split is 50 and the range for a fair offer is 48-52 (inclusive). All offers below the fair range are labeled as "unfair" offers and all offers above the range are labeled as "generous" offers. Table A.4.2 presents the rate of unfair, fair, and generous offers by treatment. We know that a small portion of buyers give “generous” offers and most generous offers come from treatments were the buyer is not informed. We note that in the last 10 rounds less than 1% of offers are generous offers when the buyer is informed. This suggests that generous offers made when the buyer is informed were a “mistake” (i.e., trembling hand).

Table A.4.2: Unfair, Fair, and Generous Transactions

	All Rounds		
Treatment:	**Unfair Offers Share**	**Fair Offers Share**	**Generous Offers Share**
Incomplete Information	71.38	19.28	9.35
Seller Knows	66.60	25.32	8.09
Buyer Knows	71.90	25.99	2.11
Complete Information	55.70	43.78	0.52
	First 10 Rounds		
Treatment:	**Unfair Offers Share**	**Fair Offers Share**	**Generous Offers Share**
Incomplete Information	69.78	18.70	11.52
Seller Knows	68.51	22.55	8.94
Buyer Knows	73.67	22.24	4.08
Complete Information	61.11	38.22	0.67
	Last 10 Rounds		
Treatment:	**Unfair Offers Share**	**Fair Offers Share**	**Generous Offers Share**
Incomplete Information	71.52	19.57	8.91
Seller Knows	64.26	28.30	7.45
Buyer Knows	71.02	28.16	0.82
Complete Information	50.44	49.56	0.00

Notes: A fair offer is defined as an equal split given common knowledge with a range of ±2 around the equal split. For example, if cost is 10 and value is 90, the equal split is 50 and the range for a fair offer is 48-52 (inclusive). All offers below the fair range are "Unfair" offers and all offers above the range are "Generous" offers.

We note that there is significant learning and experience effects between the first 10 rounds and the last 10 rounds suggesting that buyers are learning to make offers that are more likely to be accepted and more unfair. Table A.4.3 shows this in a regression framework. We find significant differences in *inequality share*, buyer earnings, and seller earnings between the first 10 rounds and the last 10 rounds. In particular, in the final 10 rounds, inequality significantly increases when buyers or sellers are uninformed; however, the interaction between uninformed buyers or sellers and experience (the last 10 rounds) shows that there is a further statistically significant increase in inequality. Moreover, when comparing the Complete Information treatment to the Incomplete Information treatment, having uninformed buyers and sellers increases the *inequality share* by 5.2 percentage points during the first 10 rounds but the interaction between Incomplete Information and experience shows that it increases *inequality share* by an additional 9.5 percentage points in the latter 10 rounds. Furthermore, looking at buyer and seller earnings between the Incomplete Information treatment and the Complete Information treatment, we find that while there is an increase in buyer and seller earnings in the first 10 rounds, this is not significant. On the other hand, we see that the Incomplete Information setting significantly increases buyer earnings by an additional 4.10 experimental units and significantly reduces the sellers earnings by 3.23 experimental units in the second half of the experiment. This suggest that there is significant learning and from the first half to the second half of the experiment.

We know that removing information from buyers or sellers decreases offers and increases acceptance rate. To compare the effect of buyers' and sellers' actions on the *inequality share*, we compare the ex-ante and ex-post *inequality share* across treatments to the subgame-perfect equilibrium predictions (in the absence of fairness concerns). We define the ex-ante *inequality share* as the possible *inequality share* produced by the buyer's offer (but before the seller accepts or rejects it) and the ex-post *inequality share* is realized *inequality share* after the seller has responded to the offer. We focus our analysis on the last 10 rounds. Recall that subgame-perfect equilibrium predicts the *inequality share* is 100% when the buyer is informed (in BK and CI) and 70.8% when the buyer is uninformed (in II and SK). First,

Table A.4.3: Learning and Experience (First vs Last 10 Rounds)

Dependent variable:	**Inequality Share**			**Seller Earnings**			**Buyer Earnings**		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Seller No Info × Last 10 Round	0.033**		0.035	-1.048		-0.964	2.161**		2.644*
	(0.016)		(0.023)	(0.764)		(0.901)	(1.009)		(1.450)
Buyer No Info × Last 10 Round		0.059***	0.061***		-2.096***	-1.982*		1.825*	2.326
		(0.016)	(0.022)		(0.752)	(1.089)		(1.009)	(1.526)
Both No Info × Last 10 Round			-0.001			-0.283			-0.870
			(0.031)			(1.500)			(1.998)
Seller No Info	0.041**		0.068***	-1.004		-1.417	0.997		2.492**
	(0.016)		(0.025)	(0.679)		(0.930)	(0.840)		(1.095)
Buyer No Info		0.010	0.039*		1.594**	1.104		-0.579	0.989
		(0.017)	(0.023)		(0.675)	(0.882)		(0.846)	(1.047)
Both No Info			-0.055*			0.913			-3.035*
			(0.033)			(1.337)			(1.666)
Last 10 Round	-0.015	-0.028**	-0.046***	2.418***	2.929***	3.431***	1.668**	1.859**	0.480
	(0.011)	(0.011)	(0.016)	(0.556)	(0.450)	(0.692)	(0.771)	(0.733)	(1.113)
Constant	0.168***	0.184***	0.149***	20.017***	18.715***	19.453***	28.874***	29.668***	28.369***
	(0.012)	(0.013)	(0.019)	(0.445)	(0.474)	(0.598)	(0.528)	(0.568)	(0.678)
Incomplete Information			0.052***			0.601			0.446
			(0.025)			(0.927)			(1.184)
Test CI=II p-value			0.038			0.517			0.707
Incomplete Information × Last 10 Round			0.095***			-3.229***			4.100***
			(0.022)			(1.102)			(1.428)
Test CI=II p-value			0.000			0.004			0.005
Number of Buyers (Clusters)	187	187	187	187	187	187	187	187	187
Observations	3740	3740	3740	3740	3740	3740	3740	3740	3740
R-Squared	0.0183	0.0123	0.0352	0.00975	0.00783	0.0122	0.0118	0.00772	0.0155

Notes: Table A.4.3 shows that buyers and sellers are learning across rounds. In particular, in the final 10 rounds, inequality significantly increases when buyers or sellers are uninformed; however, the interaction between uninformed buyers or sellers and experience (the last 10 rounds) shows that there is a further statistically significant increase in inequality. We find similar results for seller's and buyer's earnings. Significance: *** p<0.01, ** p<0.05, * p<0.1.

under Complete Information, we know that the final *inequality share* is 89.8 percentage points lower than predicted. We find that 95% of this reduction is due to buyer's more fair offers and 5% is due to the seller's response. When we remove information from the seller (i.e., in BK) we find that the final *inequality share* is 79.4 percentage points lower than predicted and, again, 95% of this reduction is due to buyer's more fair offers and 5% is due to the seller's response. In contrast, when we remove information from the buyer (i.e., in SK) we find that the final *inequality share* is 50.66 percentage points lower than predicted and 90% of this reduction is due to the buyer and 10% is due to the seller. Finally, in the Incomplete Information treatment, the final *inequality share* is only 45.8 percentage points lower than predicted and only 76% of this reduction is due to the buyer and while 24% is due to the seller.

Table A.4.4: Ex-ante and Ex-post Inequality Shares

	All Rounds		
Treatment:	**Ex-Ante Share**	**Ex-Post Share**	**Difference**
Incomplete Information	35.99	23.23	12.76
Seller Knows	27.46	19.38	8.08
Buyer Knows	27.47	20.60	6.88
Complete Information	18.48	12.38	6.11
	First 10 Rounds		
Treatment:	**Ex-Ante Share**	**Ex-Post Share**	**Difference**
Incomplete Information	35.27	20.09	15.18
Seller Knows	29.48	18.72	10.76
Buyer Knows	31.86	21.69	10.17
Complete Information	24.20	14.86	9.34
	Last 10 Rounds		
Treatment:	**Ex-Ante Share**	**Ex-Post Share**	**Difference**
Incomplete Information	35.79	24.98	10.82
Seller Knows	25.29	20.14	5.15
Buyer Knows	24.60	20.60	4.00
Complete Information	14.25	10.22	4.03

Notes: Table A.4.4 reports the ex-ante and ex-post inequality share. We define the ex-ante *inequality share* as the possible *inequality share* produced by the buyer's offer (but before the seller accepts or rejects it) and the ex-post *inequality share* is realized *inequality share* after the seller has responded to the offer.

A.4.2. Robustness check: analysis using all 30 rounds

Subjects played 30 rounds of the bargaining game. We note that there is significant learning and experience effects between the first 10 rounds and the last 10 rounds suggesting that buyers are learning to make offers that are more likely to be accepted and more unfair. Throughout the paper, we limit our analysis to the last 10 rounds since we are interested in the extent to which our results persist in the presence of this learning or experience. As a robustness check, we replicate all analysis using all 30 rounds and find consistent results.

Table A.4.5: Inequality Shares, Seller Shares, and Buyer Shares (All 30 Rounds)

Dependent variable:	**Inequality Shares**			**Seller Shares**			**Buyer Shares**		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Seller No Info	0.059***		0.082***	-0.034***		-0.046***	0.015		0.028**
	(0.014)		(0.019)	(0.009)		(0.013)	(0.010)		(0.011)
Buyer No Info		0.046***	0.070***		-0.016*	-0.029**		0.011	0.025*
		(0.014)	(0.018)		(0.009)	(0.011)		(0.010)	(0.013)
Both No Info			-0.044			0.024			-0.026
			(0.027)			(0.017)			(0.020)
Constant	0.160***	0.167***	0.124***	0.353***	0.344***	0.368***	0.503***	0.505***	0.490***
	(0.010)	(0.011)	(0.015)	(0.006)	(0.007)	(0.008)	(0.007)	(0.006)	(0.008)
Incomplete Information			0.109***			-0.051***			0.027*
			(0.022)			(0.012)			(0.015)
Test CI=II p-value			0.000			0.000			0.081
Number of Buyers (Clusters)	187	187	187	187	187	187	187	187	187
Observations	5610	5610	5610	5610	5610	5610	5610	5610	5610
R-Squared	0.0179	0.0109	0.0320	0.00836	0.00179	0.0114	0.000869	0.000509	0.00213

Notes: Seller No Info denotes when a seller does not have information on the buyer's valuation (in BK and II). Buyer No Info denotes when the buyer does not have information on the seller's cost (in SK and II). Both No Info denotes when both the buyer and seller do not have information (II). Regression analysis shows Complete Information has the least inequality in payoffs, while removing information from either the buyer or the seller leads to more inequality. Robust standard errors in parentheses. Standard errors clustered at the individual level. There are 187 clusters. Significance: *** p<0.01, ** p<0.05, * p<0.1.

Table A.4.6: Offer Conditional on Value: Effect of Removing Information (All 30 Rounds)

Panel A: Removing Buyer's Information | Seller's Information Setting

When Seller Informed:	**SK vs CI**			**When Seller Uninformed:**	**II vs BK**		
	All	Value=70	Value=90		All	Value=70	Value=90
	(1)	(2)	(3)		(4)	(5)	(6)
Seller Knows	-1.287	-0.390	-2.210*	Incomplete Information	-0.392	-0.401	-0.353
	(0.916)	(0.800)	(1.212)		(1.272)	(1.189)	(1.525)
Constant	44.236***	40.814***	48.079***	Constant	41.225***	39.632***	42.780***
	(0.772)	(0.617)	(1.041)		(0.634)	(0.693)	(0.731)
Number of Buyers (Clusters)	92	92	92	Number of Buyers (Clusters)	95	95	95
Observations	2760	1470	1290	Observations	2850	1414	1436
R-Squared	0.00637	0.000936	0.0171	R-Squared	0.000418	0.000492	0.000323

Panel B: Removing Seller's Information | Buyer's Information Setting

When Buyer Informed:	**BK vs CI**			**When Buyer Uninformed:**	**II vs SK**		
	All	Value=70	Value=90		All	Value=70	Value=90
	(1)	(2)	(3)		(4)	(5)	(6)
Buyer Knows	-3.011***	-1.181	-5.299***	Incomplete Information	-2.116*	-1.192	-3.442**
	(0.999)	(0.927)	(1.272)		(1.209)	(1.093)	(1.475)
Constant	44.236***	40.814***	48.079***	Constant	42.949***	40.423***	45.869***
	(0.772)	(0.617)	(1.041)		(0.494)	(0.510)	(0.620)
Number of Buyers (Clusters)	94	94	94	Number of Buyers (Clusters)	93	93	93
Observations	2820	1440	1380	Observations	2790	1444	1346
R-Squared	0.0226	0.00438	0.0639	R-Squared	0.0188	0.00830	0.0435

Notes: Panel A the shows that removing information from buyers when the seller is informed directionally decreases buyers' offers, in particular high-value buyers are more likely to give lower offers (marginally significant). Similarly, removing information from buyers when the seller is uninformed directionally decreases buyer's offers. Panel B shows that removing information from sellers regardless of whether the buyer is informed or not significantly decreases buyers' offers, in particular high-value buyers are more likely to give lower offers. Robust standard errors in parenthesis. Standard errors clustered at the individual level. Significance: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A.4.7: Acceptance Rates Conditional on Offer: Effect of Removing Information (All 30 Rounds)

Panel A: Removing Buyer's Information | Seller Information Setting

When Seller Informed:	SK vs CI			**When Seller Uninformed:**	II vs BK		
	All	Cost=10	Cost=30		All	Cost=10	Cost=30
	(1)	(2)	(3)		(4)	(5)	(6)
Seller Knows	0.025	0.024	0.026	Incomplete Information	0.074**	0.022	0.136***
	(0.034)	(0.025)	(0.049)		(0.029)	(0.024)	(0.047)
Number of Buyers (Clusters)	92	92	92	Number of Buyers (Clusters)	95	95	95
Observations	2760	1371	1389	Observations	2850	1478	1372
R-Squared	0.258	0.259	0.234	R-Squared	0.265	0.248	0.245

Panel B: Removing Seller's Information | Buyer Information Setting

When Buyer Informed:	BK vs CI			**When Buyer Uninformed:**	II vs SK		
	All	Cost=10	Cost=30		All	Cost=10	Cost=30
	(1)	(2)	(3)		(4)	(5)	(6)
Buyer Knows	0.010	0.062*	-0.043	Incomplete Information	0.044	0.006	0.083
	(0.035)	(0.036)	(0.040)		(0.032)	(0.020)	(0.053)
Number of Buyers (Clusters)	94	94	94	Number of Buyers (Clusters)	93	93	93
Observations	2820	1436	1384	Observations	2790	1413	1377
R-Squared	0.200	0.238	0.171	R-Squared	0.306	0.172	0.254

Notes: Panel A the shows that removing information from buyers when the seller is informed directionally increases sellers' acceptance rate. Moreover, removing information from buyers when the seller is uninformed significantly increases sellers' acceptance rate. Panel B shows that removing information from sellers, regardless of whether the buyer is informed or not, directionally increases sellers' acceptance rate. Robust standard errors in parenthesis. Standard errors clustered at the individual level. Significance: *** p<0.01, ** p<0.05, * p<0.1.

A.4.3. Alternate Measures of Inequality

We find that a small number of buyers are "generous" and give the seller a larger portion of the shares. Table A.4.2 shows that these "generous" offers occur mainly when the buyer is not informed of the seller's cost and occur less than 1% of the times when the buyer is informed. Generous offers from buyers can be seen as increasing inequality at the transactional level or reducing inequality (against sellers) at the market level. Here we define two alternative inequality share measures to accounts for those two scenarios.

First, *absolute inequality share* is defined as the absolute difference between the buyer and seller's earnings divided by the total surplus: $\frac{|\text{Buyer's earnings} - \text{Seller's earnings}|}{V-C}$. Second, *difference inequality share* is defined as the of average inequality among all buyers and sellers: $\frac{\text{Buyer earnings} - \text{Seller earnings}}{V-C}$.

As a robustness check, we replicate all analysis using these two alternate definitions and find consistent results: removing information increases inequality, is robust to both alternative definitions. The *absolute inequality share* provides a measure of inequality at the transaction level; however, because it treats any unequal split — whether for or against the seller — as inequality, it introduces generous buyers as inequality, increasing effects. On the other hand, the *difference inequality share* measure reduces inequality at the market level — as inequality working for the seller cancels out some of the inequality working against the seller — but not at the transaction level, thus it downward biases our results.

Table A.4.8: Alternate Measures of Inequality Shares

	All 30 Rounds		
Treatment:	**Inequality Share**	**Absolute Inequality Share**	**Difference Inequality Share**
Incomplete Information	23.23	26.46	20.00
Seller Knows	19.38	21.14	17.62
Buyer Knows	20.60	21.51	19.68
Complete Information	12.38	12.52	12.23
	First 10 Rounds		
Treatment:	**Inequality Share**	**Absolute Inequality Share**	**Difference Inequality Share**
Incomplete Information	20.09	24.79	15.39
Seller Knows	18.72	20.84	16.59
Buyer Knows	21.69	23.67	19.71
Complete Information	14.86	14.95	14.76
	Last 10 Rounds		
Treatment:	**Inequality Share**	**Absolute Inequality Share**	**Difference Inequality Share**
Incomplete Information	24.98	27.66	22.29
Seller Knows	20.14	21.56	18.72
Buyer Knows	20.60	20.82	20.39
Complete Information	10.22	10.22	10.22

Notes: Table A.4.8 provides the key and alternative measures of inequity. We define our key measure of inequity, the *inequality share*, as the maximum of the difference between the buyer and seller's earnings or zero divided by the total surplus: $\frac{max\{\text{Buyer earnings}-\text{Seller earnings},0\}}{V-C}$. This measure allows for only (unfair) inequality towards the buyer — so the best the seller can do to reduce inequality is reject an offer — thus allowing us to compare the equity of outcomes across information conditions. Alternate measures of inequality share are (1) the *absolute inequality share*: $\frac{|\text{Buyer earnings}-\text{Seller earnings}|}{V-C}$, which is the difference between the buyer and seller's earnings divided by the total surplus, and (2) the *difference inequality share*: $\frac{\text{Buyer earnings}-\text{Seller earnings}}{V-C}$, which is a measure of average inequality among all buyers and sellers. The *absolute inequality share* provides a measure of inequality at the transaction level; however, because it treats any unequal split — whether for or against the seller — as inequality, it introduces generous buyers as inequality, increasing effects. On the other hand, the *difference inequality share* measure reduces inequality at the market level — as inequality working for the seller cancels out some of the inequality working against the seller — but not at the transaction level, thus it downward biases our results.

Table A.4.9: Absolute Inequality Shares = $\frac{|BuyerEarnings - SellerEarnings|}{V-C}$

	Dependent variable: Absolute Inequality Share					
	Last 10 Rounds			All 30 Rounds		
	(1)	(2)	(3)	(4)	(5)	(6)
Seller No Info	0.081***		0.106***	0.070***		0.090***
	(0.016)		(0.021)	(0.014)		(0.020)
Buyer No Info		0.088***	0.113***		0.066***	0.086***
		(0.015)	(0.020)		(0.014)	(0.017)
Both No Info			-0.045			-0.037
			(0.028)			(0.026)
Constant	0.160***	0.157***	0.102***	0.169***	0.172***	0.125***
	(0.011)	(0.012)	(0.016)	(0.010)	(0.011)	(0.015)
Incomplete Information			0.174***			0.139***
			(0.022)			(0.020)
Test CI=II p-value			0.000			0.000
Number of Buyers (Clusters)	187	187	187	187	187	187
Observations	1870	1870	1870	5610	5610	5610
R-Squared	0.0366	0.0433	0.0849	0.0246	0.0218	0.0494

Notes: Regression analysis shows Complete Information has the least inequality in payoffs while removing information from either the buyer or the seller leads to more inequality. The *absolute inequality share* provides a measure of inequality at the transaction level; however, because it treats any unequal split — whether for or against the seller — as inequality, it introduces generous buyers as inequality, increasing effects compared to our key measure of *inequality share*. Standard errors clustered at the individual level. There are 187 clusters. Significance: *** p<0.01, ** p<0.05, * p<0.1.

Table A.4.10: Difference Inequality Shares = $\frac{BuyerEarnings - SellerEarnings}{V-C}$

	Dependent variable: Difference Inequality Share					
	Last 10 Rounds			All 30 Rounds		
	(1)	(2)	(3)	(4)	(5)	(6)
Seller No Info	0.067***		0.102***	0.048***		0.074***
	(0.019)		(0.021)	(0.017)		(0.020)
Buyer No Info		0.050**	0.085***		0.027	0.054***
		(0.019)	(0.022)		(0.017)	(0.019)
Both No Info			-0.066*			-0.051
			(0.037)			(0.034)
Constant	0.146***	0.155***	0.102***	0.150***	0.161***	0.122***
	(0.012)	(0.012)	(0.016)	(0.010)	(0.011)	(0.015)
Incomplete Information			0.121***			0.078***
			(0.031)			(0.029)
Test CI=II p-value			0.000			0.008
Number of Buyers (Clusters)	187	187	187	187	187	187
Observations	1870	1870	1870	5610	5610	5610
R-Squared	0.0214	0.0116	0.0389	0.00965	0.00296	0.0155

Notes: Regression analysis shows Complete Information has the least inequality in payoffs while removing information from either the buyer or the seller directionally leads to more inequality. The *difference inequality share* measure reduces inequality at the market level — as inequality working for the seller cancels out some of the inequality working against the seller — but not at the transaction level, thus it downward biases our results compared to our key measure of *inequality share*. Standard errors clustered at the individual level. There are 187 clusters. Significance: *** p<0.01, ** p<0.05, * p<0.1.

A.4.4. Buyers' Beliefs and Strategic Behavior

We use buyer's beliefs to show that buyers are behaving strategically in response to sellers being uninformed. We asked buyers in each treatment, in an incentive compatible way, to report the probability that the seller would accept a given offer. In particular, each buyer was randomly assigned one round from the last 10 rounds and asked about the probability an offer was accepted for each of the buyer-seller pairs in that round. This provides a "clean" measure of probability belief of acceptance since buyers are making judgment on offers by other buyers reducing the possibility of simultaneity bias in beliefs and offers.

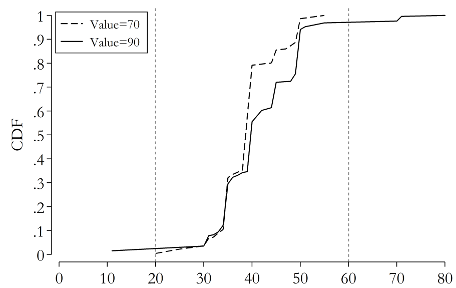
Buyers report believing that when a seller is uninformed, they are much more likely to accept an offer. In fact, the belief probability is similar to the belief probability if the seller were informed and the buyer had a low value. High-value buyers respond to this belief strategically by mimicking offers of low-value buyers when the seller is uninformed.

Figure A.4.1 presents the CDF of offers used to elicit beliefs and shows that more than 90% of the mass of the offers lies between 20 and 60 experimental units.

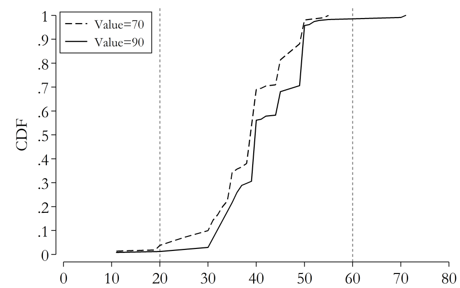
We find that removing sellers information when the buyer is informed changes the belief probability that an offer will be accept based on buyer's value. Figure A.4.2 and A.4.3 reports the buyer's belief that an offer was accepted in Panel A; the actual rate that an offer was accepted in Panel B; and the normal density function of offers in Panel C; for low-cost and high-cost sellers, respectively.

Panel A shows that buyers report believing that an offer is more likely to be accepted when the seller is uninformed (see solid grey and black lines). When the seller is uninformed, buyers (correctly) believe that seller's acceptance rate will not change based on buyers value (see solid grey and black lines overlap). When the seller is uninformed, buyers belief that seller's acceptance rate is closer to the belief probability when the seller is informed but the buyer has a low value (see solid lines are closer to the grey dash line). Panel C

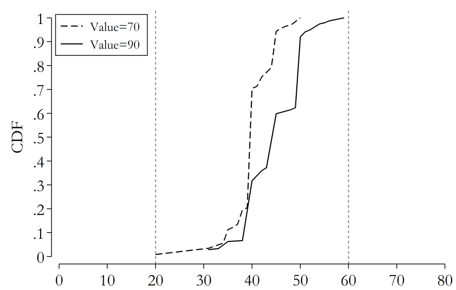
Figure A.4.1: CDF of Offers Used to Elicit Beliefs



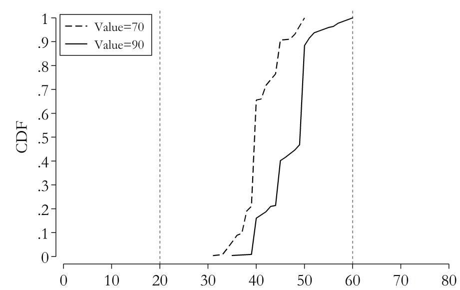
(a) Incomplete Information, Cost=10



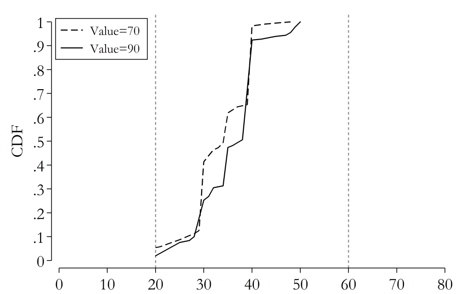
(b) Incomplete Information, Cost=30



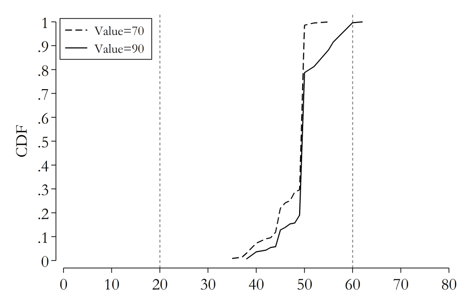
(c) Seller Knows, Cost=10



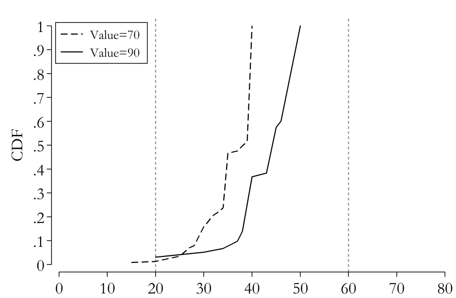
(d) Seller Knows, Cost=30



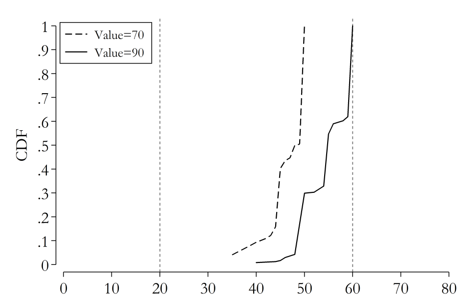
(e) Buyer Knows, Cost=10



(f) Buyer Knows, Cost=30



(g) Complete Information, Cost=10



(h) Complete Information, Cost=30

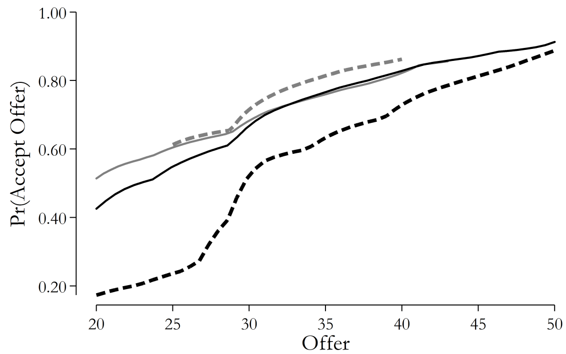
Notes: Table A.4.1 presents the CDF of offers used to elicit beliefs and shows that more than 90% of the mass of the offers lies between 20 and 60 experimental units.
Source: Experimental Data.

presents the normal density function of offers by value and suggests that high-value buyers mimic the offers of low-value buyer when sellers are uninformed. Notice, when the seller is uninformed, buyers' offer distribution overlap, regardless of value and they also overlap the offers of low-value buyers when sellers are informed (see solid grey and black lines are on top of each other and on top of dashed grey line). When the seller is informed, buyers' offer distribution shifts to the right but only for high-value buyers (see dashed black line).

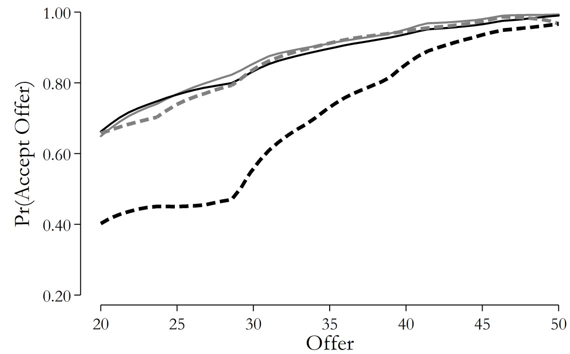
We also find supporting evidence when we compare the Incomplete Information and Seller Knows conditions. Figure A.4.4 reports the buyer's belief that an offer was accepted in Panel A; the actual rate that an offer was accepted in Panel B; and the normal density function of offers in Panel C.

Panel A we can see that when offers are in the range of 30 to 40 experimental units, buyers believe that when sellers are informed, that is in the Seller Knows treatment, low offers from high-value buyers are less likely to be accepted (compare gray and black dashed lines). On the other hand, in the Incomplete Information condition, we actually find the opposite with a higher acceptance rate for the same offer from high-value buyers (compare gray and black solid lines for offers). Actual acceptance rates in the offer ranges of 30 to 40 experimental units does not show a distinct difference in acceptance rate. However, buyers are still adjusting their offers based on their beliefs. Panel C shows that buyers are not only aware that informed sellers respond differently compared to uninformed sellers but actually high-value buyers behave strategically based on this belief by mimic offers of low-value buyers when the seller does not have information. Furthermore, we note that there is a larger spread in offers when for buyers in the Incomplete Information condition (which is not seen in other treatments). This may be due to buyers not fully realizing that having uninformed sellers (and possibly being uninformed buyers) is actually beneficial for their outcomes.

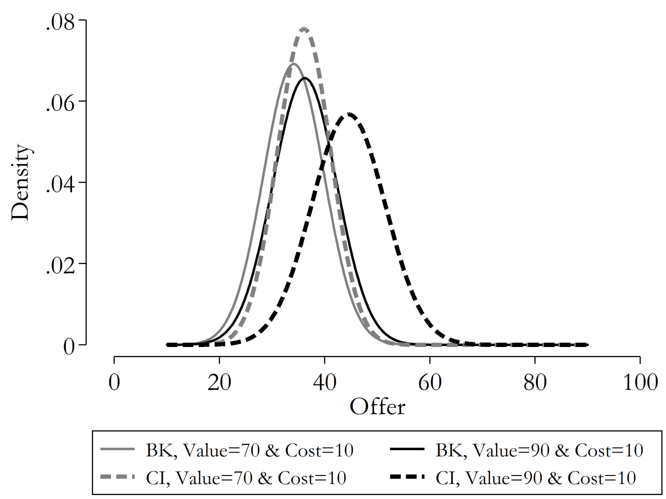
Figure A.4.2: Buyer's Belief and Strategic Behavior
(BK vs CI | Cost=10)



(a) Buyer Belief Offer was Accepted



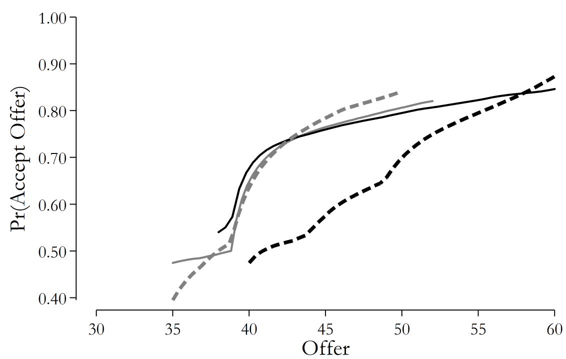
(b) Actual Rate Offer was Accepted



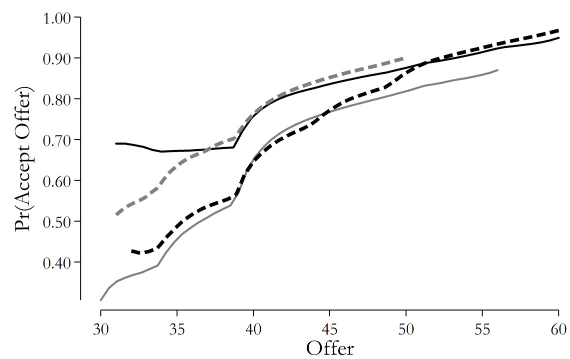
(c) Normal Density Function of Offers by Value

Notes: Panel A reports the buyer's belief probability that an offer is accepted when the seller's cost is 10 for the Buyer Knows and Complete Information treatments. Buyers in the BK treatment recognize that sellers' acceptance probability is unlikely to differ with the buyer's value (i.e., the gray and black solid lines practically overlap with K-S test p-value=0.972). In addition, the reported probability of acceptance are nearly identical to beliefs in CI when $V = 70$. This latter result suggests buyers believe sellers in BK give buyers the "benefit of the doubt," thinking like they always have $V = 70$. Panel C, shows that buyers are not only aware that informed sellers respond differently compared to uninformed sellers but also behave strategically based on this belief. Distribution of offers by treatment and value show that buyers with a high value mimic offers of low-value buyers when sellers are uninformed.
Source: Experimental Data.

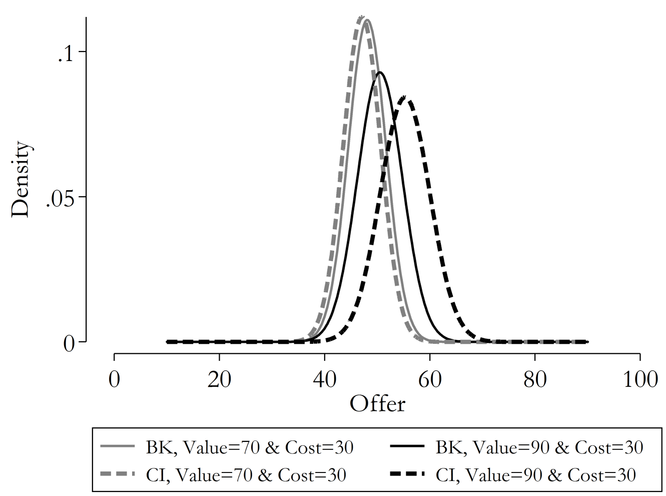
Figure A.4.3: Buyer's Belief and Strategic Behavior
(BK vs CI | Cost=30)



(a) Buyer Belief Offer was Accepted



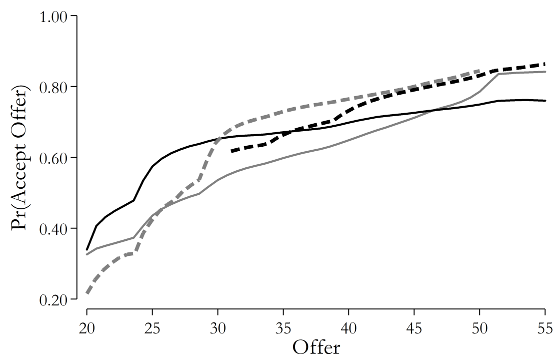
(b) Actual Rate Offer was Accepted



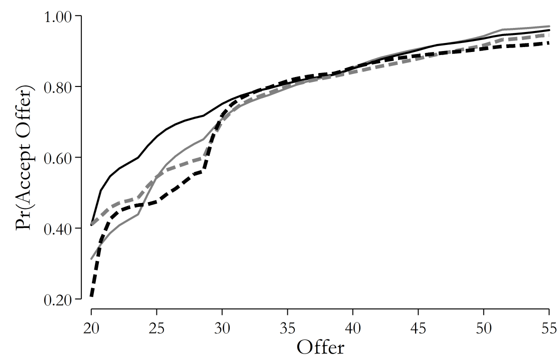
(c) Normal Density Function of Offers by Value

Notes: Panel A compare Buyer Knows and Complete Information treatments conditional on the seller's cost being 30. Buyers in BK correctly realize that uninformed sellers cannot condition acceptance on the buyer's value (the solid lines are on top of each other) and the BK probabilities are nearly identical to beliefs in CI for $V = 70$, supporting buyers' belief that sellers in BK give buyers the benefit of the doubt. Panel C shows that buyers behave strategically based on this belief: high-value buyers mimic offers of low-value buyers when the seller is uninformed.

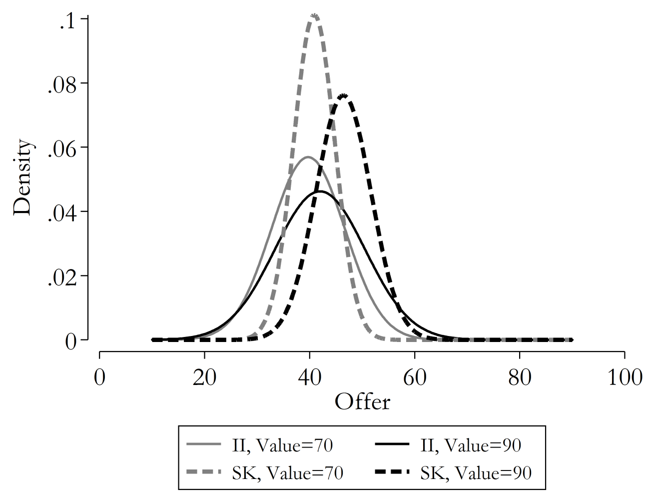
Figure A.4.4: Buyer's Belief and Strategic Behavior
(II vs SK)



(a) Buyer Belief Offer was Accepted



(b) Actual Rate Offer was Accepted



(c) Normal Density Function of Offers by Value

Notes: Panel A we can see that when offers are in the range of 30 to 40 experimental units, buyers believe that when sellers are informed, that is in the Seller Knows treatment, low offers from high-value buyers are less likely to be accepted (compare gray and black dashed lines). On the other hand, in the Incomplete Information condition, we actually find the opposite with a higher acceptance rate for the same offer from high-value buyers (compare gray and black solid lines for offers). Panel C shows that buyers are not only aware that informed sellers respond differently compared to uninformed sellers but actually high-value buyers behave strategically based on this belief by mimic offers of low-value buyers when the seller does not have information.

A.4.5. Seller's Acceptance Rate Weighted by Offers

We know that giving information to buyers and sellers can change the buyer's offer. As such, it is hard to compare the seller's rejection rates when offer distributions are not the same across treatment. In this section, we use sample weights to control for this. Below I describe each method in more detail and show previous results are amplified after controlling for offer distribution differences.

A.4.5.1. Using Sampling Weights by Offer

First, in order to control the offer distributions, we construct a sampling weight consisting of the ratio of the percentage of a given offer, conditional on buyer's value and seller's cost, in the treatment with less information over the percentage of specific offer, conditional on buyer's value and seller's cost, in the treatment with more information. This implies that all observations in the "control" group (that is, the treatment with less information) have a sampling weight of 1, observations that appears more often in the treatment with more information compared to the control group receive a weight less than 1, and observations that appeared less often in the treatment with more information compared to the control group receive a weight greater than 1.

For example, let us assume that we are comparing II and SK and there are 100 observations in each treatment conditional on value and cost, and an offer of 15 experimental units appeared 25 times in II and 10 times in SK. This means that, conditional on value and cost, all observations with an offer of 15 experimental units receive a weight=1 in II and weight= $\frac{0.25}{0.1} = 2.5$ in SK.

Then we run the exact same regressions specified in Table 5 and Table 6 controlling for the sampling weights. Note that this method creates "missing" weights for any offer that appeared in one treatment but not the other, essentially dropping it from the regression. It also might place a lot of weight on the outcome of a single observation. For example, if an offer of 20 experimental units appeared 10 times in II where it was accepted half the time and rejected half the time and 1 time in SK where it was accepted, then the sampling weight will put a weight of 10 on the observation in SK, making it appear as if it is an offer of 20 experimental units is accepted more often in SK. In order to minimize the number of observation dropped and overweighting outcomes from observations that appeared very infrequently, we use offer "bins" to construct sampling weights (see Tables D3 and D4).

Table A.4.11: Acceptance Rates Conditional on Offer: Effect of Removing Information (Controlling for Sampling Weights By Offer Conditional on Value and Cost)

Panel A: Removing Buyer Information | Seller Information Setting

When Seller Informed:	**SK vs CI** All	Cost=10	Cost=30	**When Seller Uninformed:**	**II vs BK** All	Cost=10	Cost=30
	(1)	(2)	(3)		(4)	(5)	(6)
Seller Knows	0.081	0.037	0.134	Incomplete Information	0.124***	0.029	0.256***
	(0.050)	(0.032)	(0.095)		(0.042)	(0.032)	(0.083)
Number of Buyers (Clusters)	92	92	92	Number of Buyers (Clusters)	95	95	95
Observations	902	434	468	Observations	934	471	463
R-Squared	0.242	0.135	0.193	R-Squared	0.386	0.214	0.341

Panel B: Removing Seller Information | Buyer Information Setting

When Buyer Informed:	**BK vs CI** All	Cost=10	Cost=30	**When Buyer Uninformed:**	**II vs SK** All	Cost=10	Cost=30
	(1)	(2)	(3)		(4)	(5)	(6)
Buyer Knows	0.036	0.081*	-0.001	Incomplete Information	0.053	0.008	0.104
	(0.034)	(0.045)	(0.044)		(0.037)	(0.018)	(0.073)
Number of Buyers (Clusters)	94	94	94	Number of Buyers (Clusters)	93	93	93
Observations	932	457	475	Observations	912	457	455
R-Squared	0.248	0.284	0.224	R-Squared	0.379	0.260	0.301

Notes: Panel A the shows that removing information from buyers when the seller is informed directionally increases sellers' acceptance rate. Moreover, removing information from buyers when the seller is uninformed significantly increases sellers' acceptance rate. Panel B shows that removing information from sellers, regardless of whether the buyer is informed or not, directionally increases sellers' acceptance rate. Robust standard errors in parenthesis. Standard errors clustered at the individual level. Significance: *** p<0.01, ** p<0.05, * p<0.1.

A.4.5.2. Using Sampling Weights by Offer Bins

In order to construct sampling weights based on offer bins, we first generate a new variable that allocates each offer into a bin, where bins are defined in 5 unit increments (that is, offers of 10-15 units are in bin 0, offers of 16-20 units are in bin 1, and so on) and then construct sampling weights in the same method as previously described.

Table A.4.12: Acceptance Rates Conditional on Offer: Effect of Removing Information (Controlling for Sampling Weights By Offer Bins Conditional on Value and Cost)

Panel A: Removing Buyer Information | Seller Information Setting

When Seller Informed:	**SK vs CI**		
	All	Cost=10	Cost=30
	(1)	(2)	(3)
Seller Knows	0.085*	0.048	0.133
	(0.050)	(0.035)	(0.096)
Number of Buyers (Clusters)	92	92	92
Observations	915	445	470
R-Squared	0.243	0.163	0.195

When Seller Uninformed:	**II vs BK**		
	All	Cost=10	Cost=30
	(4)	(5)	(6)
Incomplete Information	0.144***	0.033	0.287***
	(0.042)	(0.032)	(0.082)
Number of Buyers (Clusters)	95	95	95
Observations	950	480	470
R-Squared	0.416	0.238	0.368

Panel B: Removing Seller Information | Buyer Information Setting

When Buyer Informed:	**BK vs CI**		
	All	Cost=10	Cost=30
	(1)	(2)	(3)
Buyer Knows	0.040	0.091*	-0.000
	(0.036)	(0.050)	(0.045)
Number of Buyers (Clusters)	94	94	94
Observations	940	461	479
R-Squared	0.245	0.275	0.226

When Buyer Uninformed:	**II vs SK**		
	All	Cost=10	Cost=30
	(4)	(5)	(6)
Incomplete Information	0.045	0.006	0.089
	(0.037)	(0.021)	(0.072)
Number of Buyers (Clusters)	93	93	93
Observations	930	469	461
R-Squared	0.355	0.286	0.278

Notes: Panel A the shows that removing information from buyers when the seller is informed directionally increases sellers' acceptance rate. Moreover, removing information from buyers when the seller is uninformed significantly increases sellers' acceptance rate. Panel B shows that removing information from sellers, regardless of whether the buyer is informed or not, directionally increases sellers' acceptance rate. Robust standard errors in parenthesis. Standard errors clustered at the individual level. Significance: *** p<0.01, ** p<0.05, * p<0.1.

A.5. Transaction Utility and Consumer Choice Experimental Protocol

After consenting to participate in this study, each participant is informed that they might receive a free drink of choice and additional bonus payment on top of their $10 participation fee. Participants read instructions and participate in the *Coffee Drink Experiment* followed by the *Discount and Mark-up Game* and the *Coupon Game*. Below are the instructions for the *Discount and Mark-up Game* and the *Coupon Game* used in this paper.

<u>General Instructions</u>

This is part 2 of the Decision Making Study. In this part of the study you will be participating in two games (Game A and Game B) in which you play the role of a buyer. Below is a brief description of each game.

- <u>Game A:</u> As a buyer in Game A, you will encounter a store. You will review the price of 32 items in the store and decide if you would like to purchase the item or not. As such, you will be playing a total of 32 rounds in Game A.

- <u>Game B:</u> As a buyer in Game B, you will review the prices of two items and decide which item you would like to purchase. You will be playing a total of 6 rounds in Game B.

At the end of the session, the lab coordinator will randomly select one participant to be the buyer for each of the two games. If you are randomly selected to be the buyer, then one of your rounds in that game will be randomly selected for bonus payment.

Please click "Next" to read the instructions for Game A.

A.5.1. Discount and Mark-up Game

<u>Game A: Instructions</u>

In this game you play the role of a buyer. As a buyer, you will encounter a store. You will review the price of 32 items in the store and decide if you would like to purchase the item or not. As such, you will be playing a total of 32 rounds in Game A.

In this game, you will value each item at the store at $9.00 while the seller has a cost of $6.00.

For each item, you will be told how much you value (V) the item. Then the computer will determine the price (P) of the item for you. The computer will give you either a price with a discount (that is, a mark-down on the original price), a price with an inflated price (that is, a mark-up on the original price), or the original price (that is, neither a discounted nor marked-up price). The computer may also show you the original price other participants in this study have been offered for each of the items. You will decide whether or not you want to purchase the each item at the price the computer selected for you.

Game A: Earnings and Bonus Payment

Your earnings will be determined in the following way: if you, as the buyer, decide to purchase the item, you will earn the difference between your value of the item and the price the computer selected for you, that is your earnings will equal V-P, and the seller will earn the price (P) you accepted to pay minus their cost (C). If you decide to not purchase the item, both you and the seller will earn $0.00.

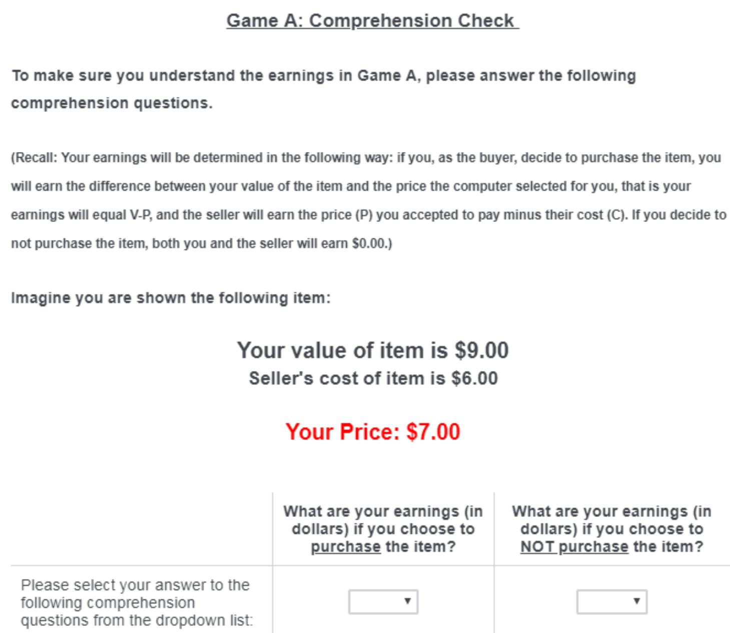
At the end of the session, the lab coordinator will randomly select one participant to be the buyer in Game A. If you are randomly selected to be the buyer, then one of your rounds in Game A will be randomly selected for bonus payment. You will receive your earnings from that round. The lab coordinator will also randomly select a different participant to be the seller. If you are randomly selected to be the seller, your earnings will not depend on your decisions.

Note: you should make all your decisions assuming you are the buyer.

Each round is independent of the others. Note that the price may be different from item to item. Please make each of your choices carefully. Remember, you may be selected as the buyer, in which case one of your rounds will be selected for payment. Since only one round is randomly chosen for payment you should treat each round as if it is the one and only round chosen for payment.

Please click “Next”.

Figure A.5.1: Comprehension Questions
Study 1A & 1B: Discount and Mark-up Game



Game A: Comprehension Check

To make sure you understand the earnings in Game A, please answer the following comprehension questions.

(Recall: Your earnings will be determined in the following way: if you, as the buyer, decide to purchase the item, you will earn the difference between your value of the item and the price the computer selected for you, that is your earnings will equal V-P, and the seller will earn the price (P) you accepted to pay minus their cost (C). If you decide to not purchase the item, both you and the seller will earn $0.00.)

Imagine you are shown the following item:

Your value of item is $9.00
Seller's cost of item is $6.00

Your Price: $7.00

	What are your earnings (in dollars) if you choose to purchase the item?	What are your earnings (in dollars) if you choose to NOT purchase the item?
Please select your answer to the following comprehension questions from the dropdown list:		

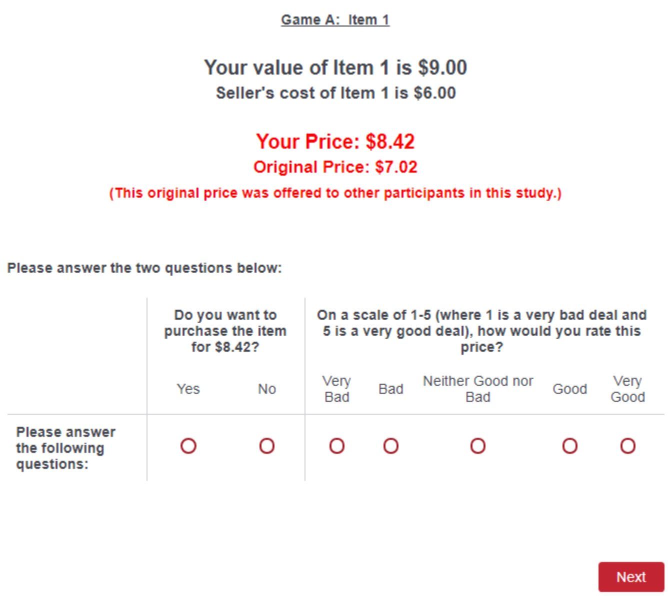
Game A

You have entered Game A. In this game, the computer may show you the original price that other participants in this study have been offered for each of the items, your value (V) of the item (that is, how much you value the item), and the price (P) the computer selected for you. The computer will give you either a price with a discount (that is, a mark-down on the original price), a price with an inflated price (that is, a mark-up on the original price), or the original price (that is, neither a discounted nor marked-up price). You will decide whether or not you want to purchase the each item at the price the computer selected for you.

You will be asked whether you want to buy each of the 32 different items.

Figure A.5.2: Example of Study 1 Participant's Decision Screen
Study 1A & 1B: Discount and Mark-up Game

(a) Study 1A: Baseline



Game A: Item 1

Your value of Item 1 is $9.00
Seller's cost of Item 1 is $6.00

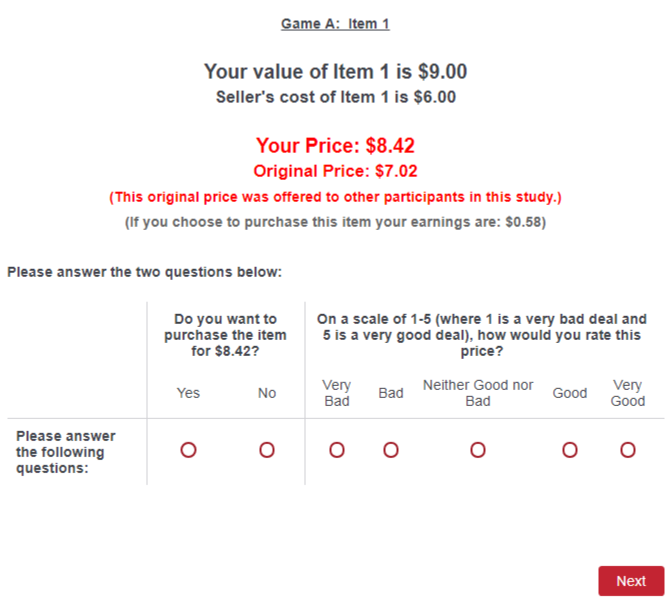
Your Price: $8.42
Original Price: $7.02
(This original price was offered to other participants in this study.)

Please answer the two questions below:

	Do you want to purchase the item for $8.42?		On a scale of 1-5 (where 1 is a very bad deal and 5 is a very good deal), how would you rate this price?				
	Yes	No	Very Bad	Bad	Neither Good nor Bad	Good	Very Good
Please answer the following questions:	O	O	O	O	O	O	O

Next

(b) Study 1B: With Earnings Displayed



Game A: Item 1

Your value of Item 1 is $9.00
Seller's cost of Item 1 is $6.00

Your Price: $8.42
Original Price: $7.02
(This original price was offered to other participants in this study.)
(If you choose to purchase this item your earnings are: $0.58)

Please answer the two questions below:

	Do you want to purchase the item for $8.42?		On a scale of 1-5 (where 1 is a very bad deal and 5 is a very good deal), how would you rate this price?				
	Yes	No	Very Bad	Bad	Neither Good nor Bad	Good	Very Good
Please answer the following questions:	O	O	O	O	O	O	O

Next

A.5.2. Coupon Game

Game B: Instructions

In this game you play the role of a buyer. As a buyer, you will review the original prices of two items and decide which item you would like to purchase. You can only buy one item in each round. You will be playing a total of 6 rounds in Game B.

In this game, you will value item Y at \$6.00 and item Z at \$8.00. You have received a “\$5.00 off the original price” discount coupon valid for one item.

For both, item Y and item Z, the computer will show you how much you value each item. You will also be shown the original price of item Y and the original price of item Z. Since you have received a “\$5.00 off the original price” discount coupon valid for one item, the price that you pay will be the original price of the item minus \$5.00. You will decide whether you want to use the “\$5.00 off the original price” discount coupon to purchase item Y or item Z.

Game B: Earnings and Bonus Payment

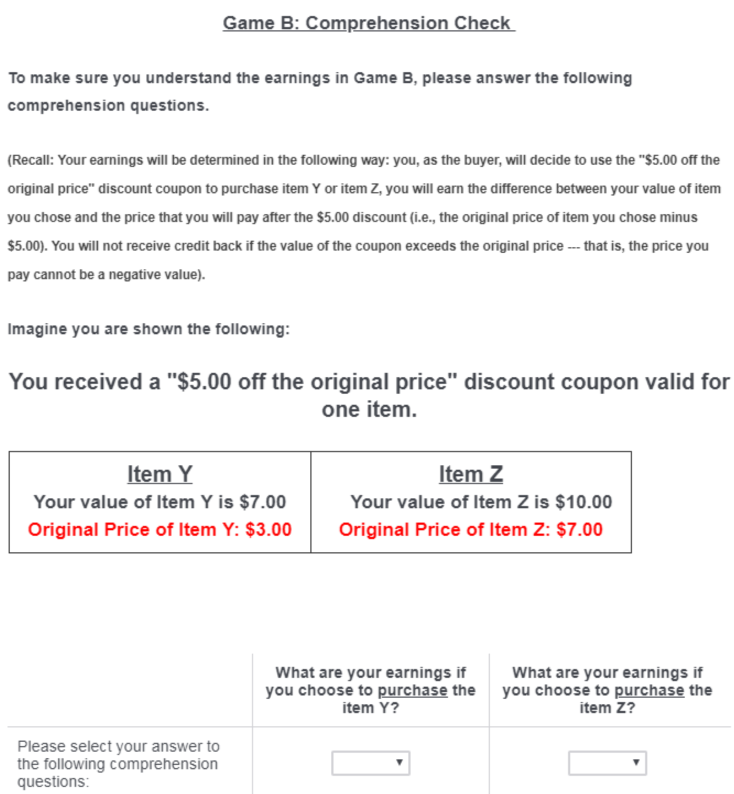
Your earnings will be determined in the following way: if you, as the buyer, decide to use the “\$5.00 off the original price” discount coupon to purchase item Y, you will earn the difference between your value of item Y and the price that you will pay after the \$5.00 discount (i.e., the original price of item Y minus \$5.00). You will not receive credit back if the value of the coupon exceeds the original price (that is, the price you pay cannot be a negative value). Similarly, if you decide to use the “\$5.00 off the original price” discount coupon to purchase item Z, you will earn the difference between your value of item Z and the price that you will pay after the \$5.00 discount (i.e., the original price of item Z minus \$5.00). You will not receive credit back if the value of the coupon exceeds the original price (that is, the price you pay cannot be a negative value).

At the end of the session, the lab coordinator will randomly select one participant to be the buyer in Game B. If you are randomly selected to be the buyer, then one of your rounds in Game B will be randomly selected for bonus payment. You will receive your earnings from that round.

Each round is independent of the others. Note that the price may be different from item to item. Please make each of your choices carefully. Remember, you may be selected as the buyer, in which case one of your rounds will be selected for payment. Since only one round is randomly chosen for payment you should treat each round as if it is the one and only round chosen for payment.

Please click "Next" to Begin.

Figure A.5.3: Comprehension Questions
Study 2A & 2B: Coupon Game


Game B: Comprehension Check

To make sure you understand the earnings in Game B, please answer the following comprehension questions.

(Recall: Your earnings will be determined in the following way: you, as the buyer, will decide to use the "$5.00 off the original price" discount coupon to purchase item Y or item Z, you will earn the difference between your value of item you chose and the price that you will pay after the $5.00 discount (i.e., the original price of item you chose minus $5.00). You will not receive credit back if the value of the coupon exceeds the original price --- that is, the price you pay cannot be a negative value).

Imagine you are shown the following:

You received a "$5.00 off the original price" discount coupon valid for one item.

Item Y	Item Z
Your value of Item Y is $7.00	Your value of Item Z is $10.00
Original Price of Item Y: $3.00	Original Price of Item Z: $7.00

	What are your earnings if you choose to purchase the item Y?	What are your earnings if you choose to purchase the item Z?
Please select your answer to the following comprehension questions:		

Figure A.5.4: Example of Study 2 Participants' Decision Screen

Study 2A & 2B: Coupon Game

(a) Study 2A: Baseline

Game B: Round 1

You received a "$5.00 off the original price" discount coupon valid for one item.

Item Y	Item Z
Your value of Item Y is $6.00	Your value of Item Z is $8.00
Original Price of Item Y: $3.00	Original Price of Item Z: $8.00

Would you like to use the "$5.00 off the original price" discount coupon to purchase item Y or item Z?

Item Z

Item Y

(b) Study 2B: With Earnings Displayed

Game B: Round 1

You received a "$5.00 off the original price" discount coupon valid for one item.

Item Y	Item Z
Your value of Item Y is $6.00	Your value of Item Z is $8.00
Original Price of Item Y: $3.00	Original Price of Item Z: $8.00

(Note: If you choose to purchase item Y, your earnings are $6.00. If you choose to purchase item Z, your earnings are $5.00.)

Would you like to use the "$5.00 off the original price" discount coupon to purchase item Y or item Z?

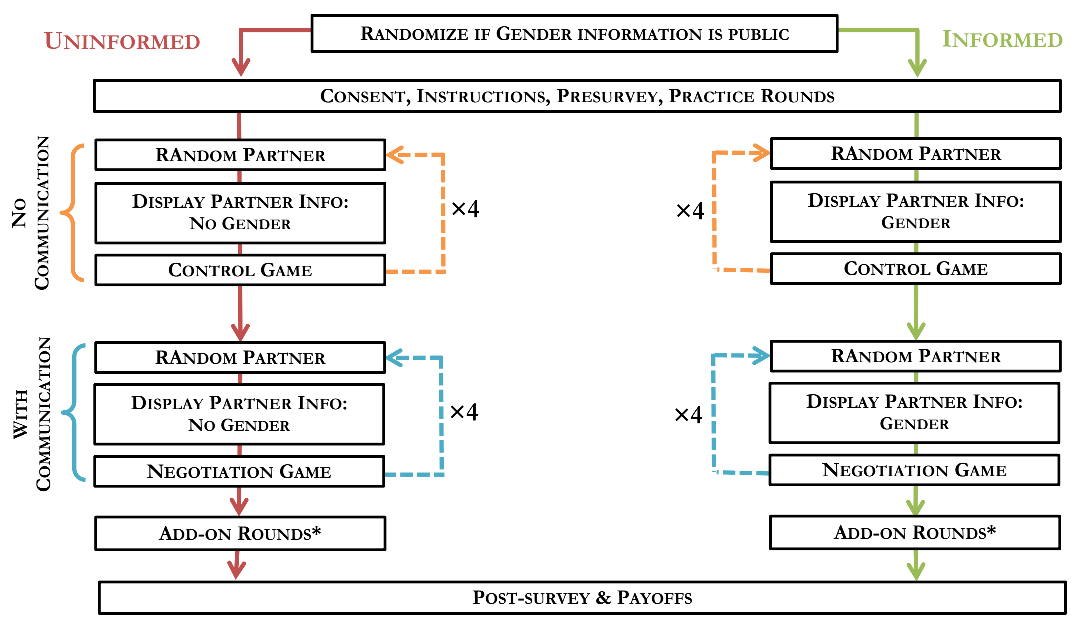
Item Z

Item Y

A.6. The Myth of the Male Negotiator Experimental Protocol

The full experimental protocol was as follows. Randomization of the informed and uninformed treatment was at the session level. Figure A.6.1 shows the timeline of events. After consenting to participate in this study, each participant read the general instructions, went through two practice rounds with the computer to understand the game and proceeded to the actual control and negotiation games. All subjects participated in four rounds of the control game followed by four rounds of the negotiation game. In addition, participants also went through two "add-on" rounds. Results from the add-on round are not included in this paper but instructions have been included for completeness. Below are the instructions used in this paper.

Figure A.6.1: Full Experimental Design



<u>Experimental Instructions: Choice Study</u>

We will now go through the instructions for the experiment in more detail. Note that **this study involves bonus payment, and so understanding the instructions carefully will allow you to maximize your earnings.**

Experiment policies

This experiment is being done by economists Corinne Low and Zheng Jai Huang. It is the policy of economists that **participants cannot be deceived at any point throughout the experiment**. Therefore, the instructions described are exactly the way the experiment will proceed, and you will be paid.

Please don't talk or gesture to any participants in the lab, nor should you do anything on the computers other than the experiment, as this could interfere with the validity of the results. We greatly appreciate you offering your full attention for the duration of the experiment.

Experiment stages

This experiment will have three stages. There will be an opportunity to earn bonus pay at each of the stages.

1. First, you will take a brief survey.

2. Then you will be asked to make 10 rounds of decisions. You will be randomly assigned a different partner in the lab in each round. At the end of the study, a computer will randomly determine which round will count towards your bonus pay.

3. Finally, you will take a brief post-survey.

Stage 1

We will now complete a survey. Please answer the questions truthfully to the best of your ability.

Please note, some of this information will be shared with your partners, anonymously, in the next stage of the experiment.

Please you click next, and you will begin the survey.

(Note from the experimenter: Participants saw the following questions. Each questions was in its own window. Included are the choices provided to the participant when applicable.)

- What is your major?
- Why do you participate in WBL experiments? (Check ALL that apply.) [Answer options: (1) They're interesting; (2) To make extra money; (3) Course Credit; (4) Other, please specify.]
- What is your gender? [Answer choices: male, female]
- Are you right- or left-handed? [Answer choices: left, right]
- Please type this word as quickly and accurately as you can in the box below:

  shenanigans

  Hit "OK" immediately after finishing.
- What month were you born in? [Answer choices: January to December]
- Lay your right hand flat on the table. Is your **index finger** (next to thumb) or your **ring finger** (next to pinky) longer? [Answer choices: (1) Index is longer; (2) Ring is longer; (3) Same length]
- **Please answer yes/no to the following:**
  - Can you roll your tongue (shape tongue into "u" shape)?
  - Do you have a "hitchhiker" thumb (extend thumb as far as you can - you have hitchhiker thumbs if the top segment bends past 45 degrees)?
  - Are you an only child?
- What is your favorite color?

- Finally as bonus payment for this part of the study, you will receive $1.00. You can either take that dollar as-is, or put some portion of it into a lottery. Money placed in the lottery will be worth **0 with 50% probability and 2.5x its value with 50% probability** (decided randomly by the computer). How much of your $1.00 would you like to place in the lottery? [Answer choices: $0 to $1.00 in 5 cents increments.]

General Instructions

You will play 10 different rounds and will be randomly assigned a **different** partner in each round. Results from each round will be saved and stored in the system.

One of the 10 rounds will be randomly chosen by the computer and you will receive the full payoff of that round as your bonus payment. **Thus, you should play each round as though you will be paid for that round, as it may be selected at the end.**

In each rounds, you and your partner will choose how to split $20, with the caveat that there are only two possible ways to split it: Either you can take $15, and your partner takes $5, OR you can take $5, and your partner takes $15. But, if you do not agree on how to split it, you each get $0.

Please click "Page 2 of Instructions".

General Instructions (continued)

To clarify further, in each round, you and your partner will be shown the same two choices:

- $15 for yourself ($5 for partner)
- $5 for yourself ($15 for partner)

If one of you chooses $15, and one chooses $5, you will each receive this payoff. If both of you choose the same amount for yourself, however, you will each get $0.

Let's review each possible scenario:

- If you choose \$15 for yourself (\$5 for partner)
  - ...And your partner "agrees," by choosing \$5 for themselves, **you get \$15** (partner gets \$5)
  - ...And your partner "disagrees," by also choosing \$15 for themselves, **you each get \$0**
- If you choose \$5 for yourself (\$15 for partner)
  - ...And your partner "agrees," by choosing \$15 for themselves, **you get \$5** (partner gets \$15)
  - ...And your partner "disagrees," by also choosing \$5 for themselves, **you each get \$0**

You and your partner must make this choice simultaneously, so you cannot see what they are choosing while you make your choice.

For whichever round is randomly chosen for payment, you will receive the entire amount of the game's outcome, either \$15, \$5, or \$0.

Please click next, and we will give you a chance to practice the game.

<u>Practice Rounds</u>

We will now do two practice rounds. In these rounds, you will not have a real partner; instead, the computer will choose your "partner's" choices randomly.

The payoffs from the practice rounds will NOT count towards your final earnings.

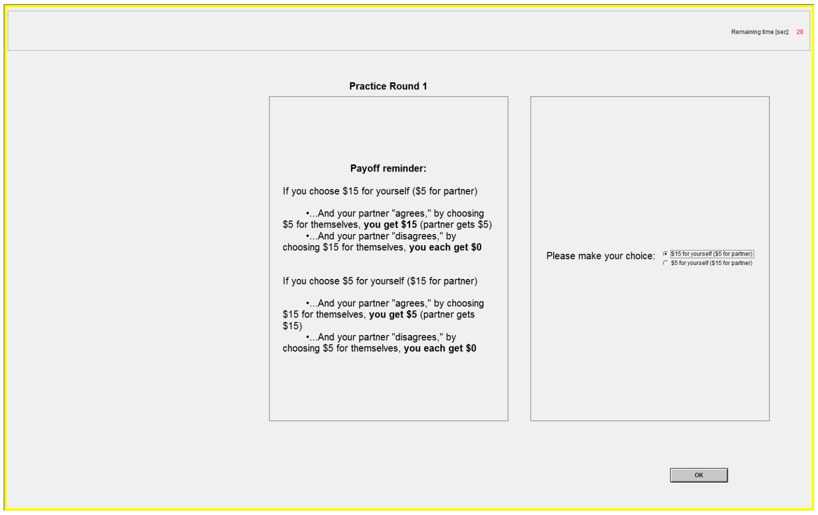
When you hit next, you will be taken to the practice rounds.

(Note to experimenter: To help participants understand the game, outcomes from the practice rounds were shown after each round. Figure A.6.2 Panel A shows an example of the

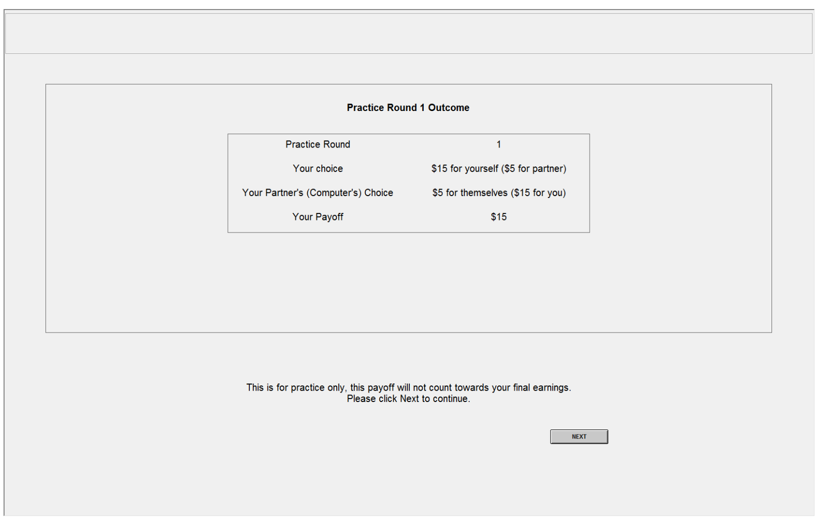
participant's choice screen during the practice rounds. Panel B shows an example of the outcome screen during the practice rounds.)

Figure A.6.2: Example of Practice Round

(a) Choice Screen



(b) Outcome Screen



<u>Instructions: Rounds 1-4</u>

Now that we have practiced, you will be paired with a partner and we will start the experiment.

In the next 4 rounds, you will be shown some information about your partner first, then you will be shown the decision screen where you can make your choice. You will each pick simultaneously whether to choose \$15 or \$5 for yourself without knowing what the other person is choosing.

The outcome of these rounds will be stored in the system, and you will only be told your results when all 10 rounds are completed. Remember, any round could be randomly chosen to determine your bonus payment, so you would play each round as though real money is on the table.

Note that you will be randomly paired with a DIFFERENT partner in each round.

Please click Next to begin.

(Note to experimenter: Below are the two sequential screens participants saw: (1) participants saw the partner information screen for 15 seconds; (2) participants saw the choice screen. Gender information was randomized at the session level. Participants either saw the partner information sheet with or without gender for all rounds. Figure A.6.3 Panels A and B shows an example of the partner information sheet with and without gender information. Note that the only difference between the two screens is that gender is show as the first characteristic in the Informed treatment. Figure A.6.4 shows an example of the choice window in the control game. Note that the partner information sheet and payoff rules are restated.)

<u>Instructions: Rounds 5-8</u>

In the next 4 rounds, Rounds 5-8, you will be allowed to communicate with your partner prior to making your decision. This time, after you view the information about your partner, you will have the opportunity to discuss your choice with your partner for 2.5 minutes before you each choose.

Figure A.6.3: Example of Partner Information Screen with and without Partner Gender

(a) Informed Condition

(b) Uninformed Condition

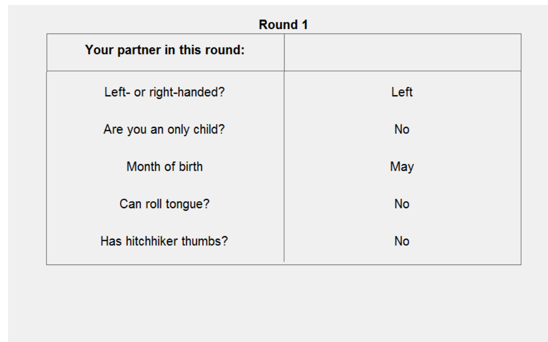
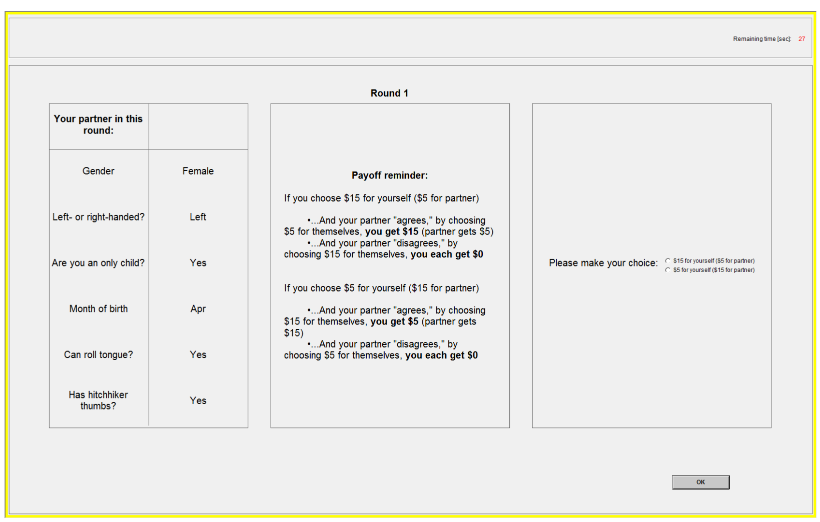


Figure A.6.4: Example of Choice Window



When the 2.5 minutes are up, you will each pick simultaneously whether to choose \$15 or \$5 out of the \$20 for yourself without knowing what the other person is choosing.

To repeat, the pattern is:

1. View information about partner
2. Communicate with partner via chat for 2.5 minutes
3. Make choice

Important note about chat communication: The chat window allows you to discuss your choice with your partner. However, you may not:

- Reveal identifiable information about yourself
- Ask others to reveal identifiable information
- Make arrangements to discuss or meet outside the lab.

(Failure to comply with this will affect your future ability to participate in WBL studies)

The outcome of these rounds will be stored in the system, and you will only be told your results when all games are completed. Remember, any round could be randomly chosen to determine your bonus payment, so you would play each round as though real money is on the table.

Note that you will be randomly paired with a DIFFERENT partner in each round.

Please click Next to begin.

(Note to experimenter: Below are the three sequential screens participants saw: (1) partner information screen; (2) chat window; (3) choice window. Figure A.6.5 Panels A, B, and C shows these three windows sequentially. Notice that the screens (and thus the game) is

identical to the control game with the exception that participants are able to (free-form) chat and negotiate with their partners for 2.5 minutes before proceeding the choice window where decisions are made simultaneously.)

(Note to experimenter: Results from Rounds 9 and 10 (the "add-on" rounds) are not included in this paper, but instructions are included for completion.)

Instructions: Round 9

We will now proceed to Round 9.

In this round, you will have the opportunity to choose whether you would like to communicate with your partner or not. This time, after you view the information about your partner, you will choose whether you would like to:

- Not communicate with your partner (like in rounds 1-4)
- Communicate with your partner (like in rounds 5-8)

Both you and your partner will make this choice. Then, the computer will choose randomly whether *your* choice your *your partner's* choice will be used to determine the game ou will actually play. With a 50% chance, your choice will be used, and you will play the game you have chosen. And with a 50% chance your partner's choice will be used, and you will play the game they have chosen.

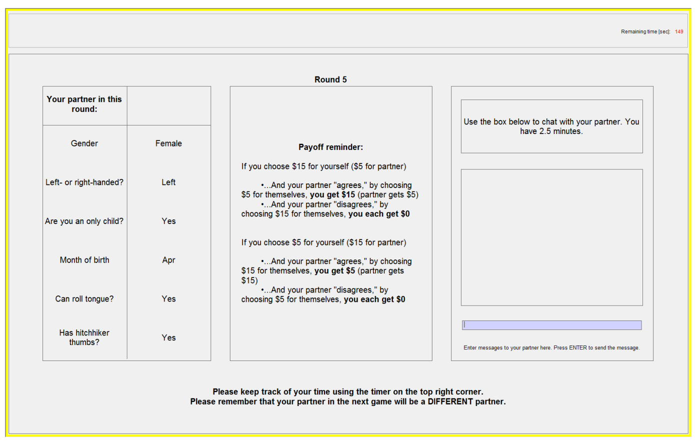
The sequence of the round will be:

1. Participants are matched randomly with a partner.
2. You will be shown some information about your partner.
3. Each partner chooses whether they would like to communicate or not communicate with their partner.

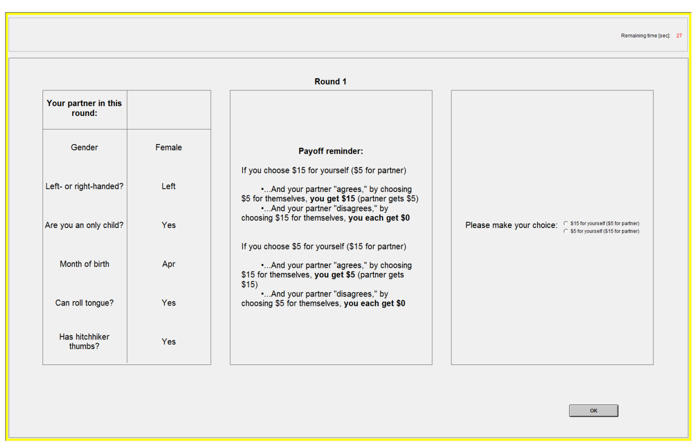
Figure A.6.5: Example of Negotiation Game Screen Sequence

(a) Partner Information

(b) Chat Window



(c) Choice Window



4. A computer will randomly decided if you or your partner's choice of game will be used.

5. You will play the chosen round.

As in the previous rounds, you will be shown the decision screen where you can make your choice. You will each pick simultaneously whether to choose $15 or $5 out of the $20 for yourself without knowing what the other person is choosing.

The outcome of these rounds will be stored in the system, and you will only be told your results when all games are completed. Remember, any round could be randomly chosen to determine your bonus payment, so you should play each round as though real money is on the table.

Note that you will be randomly paired with a DIFFERENT partner in each round.

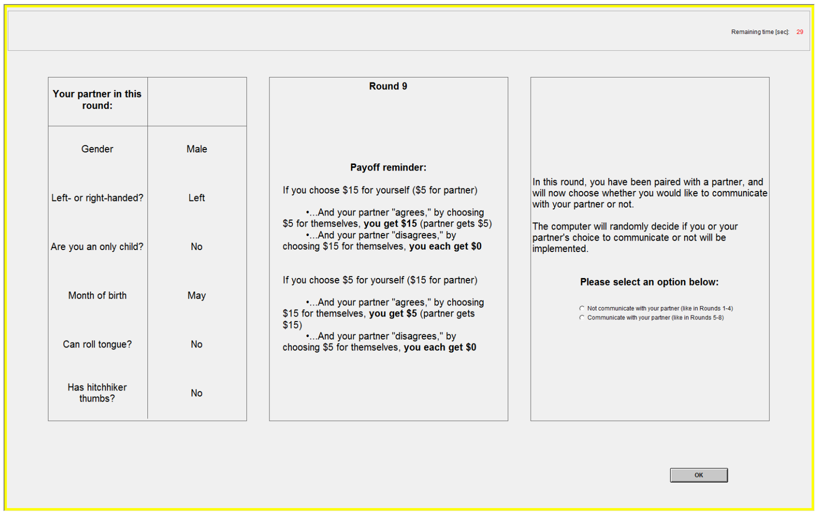
Please click Next to begin.

Note to experimenter: Participants saw three sequential screens: (1) participants saw the partner information screen for 15 seconds; (2) participants choose whether they want to play the control or negotiation game with their randomly assigned partner; (3) participants are told whether they are playing the control or negotiation game and proceed accordingly. Below is an example of the screen where participants choose which game they wished to play with their partner. Figure A.6.6 shows an example of the screen where participants are able to choose if they want to play the negotiation game or the control game (i.e., step 2).

Instructions: Round 10

For the 10th round, you will choose one of your previous rounds to "count" an extra time, and therefore have an extra chance of being randomly drawn for payment. You get to choose whether you want a random round from rounds 1-4 (with no communication) or rounds 5-8 (with communication) to fill this extra "slot." This round will be saved in the

Figure A.6.6: Example of Game Choice Window



system and may be randomly picked as your bonus payment. Please choose if you would like this random round to be picked from:

- Rounds 1-4 (with no communication)
- Rounds 5-8 (with communication)

Note to experimenter: Figure A.6.7 shows an example of the screen where participants are able to choose if they want their random round (which will count twice) to come from the negotiation game or the control game.

(Notes to the experimenter: after all 10 rounds were played, participants saw the outcome of all 10 rounds. Afterwards they answered a post-survey. Figure A.6.8 shows an example of this screen with the outcome of all 10 rounds.)

Figure A.6.7: Example of Round 10 Choice Window

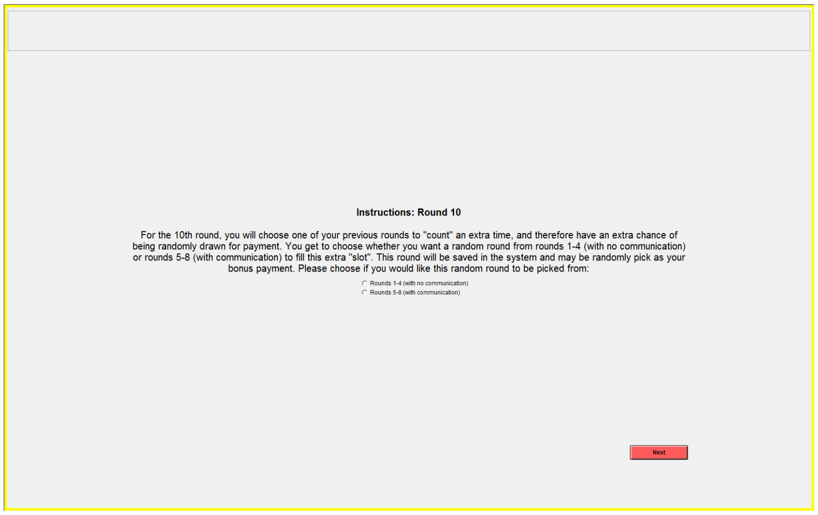


Figure A.6.8: Example of Control and Negotiation Games Outcome Table

Round	Your choice	Your Partner's Choice	Your Payoff
1	$15 for yourself ($5 for partner)	$5 for themselves ($15 for you)	$15
2	$5 for yourself ($15 for partner)	$5 for themselves ($15 for you)	$0
3	$15 for yourself ($5 for partner)	$5 for themselves ($15 for you)	$15
4	$5 for yourself ($15 for partner)	$5 for themselves ($15 for you)	$0
5	$15 for yourself ($5 for partner)	$5 for themselves ($15 for you)	$15
6	$5 for yourself ($15 for partner)	$5 for themselves ($15 for you)	$0
7	$5 for yourself ($15 for partner)	$15 for themselves ($5 for you)	$5
8	$15 for yourself ($5 for partner)	$5 for themselves ($15 for you)	$15
9	$5 for yourself ($15 for partner)	$15 for themselves ($5 for you)	$5
10	$5 for yourself ($15 for partner)	$5 for themselves ($15 for you)	$0

Post-Survey

You will now be asked to complete a brief post-survey, and then will learn you final earnings. Remember, a computer will randomly choose one of the 10 rounds you played and the payoffs in that round will be your bonus earnings for this lab session.

Please click next to be taken to the post-survey.

- What did you think the experiment was about?
- In rounds 1-9, you were partnered with someone in the lab. How many of your partners do you think were women? [Answer choices from 0 to 9.]
- Please answer the following questions from Strongly Agree to Strongly Disagree. [Answer choices were on a 5-point likert scale: Strongly Agree, Agree, Neither Agree Not Disagree, Disagree, Strongly Disagree.]
  - Many women are actually seeking special favors, such as hiring policies, that favor them over men, under the guise of asking for "equality."
  - In a disaster, women ought not necessarily to be rescued before men.
  - Women are too easily offended.
  - Women should be cherished and protected by men.
  - When women lose to men in a fair competition, they typically complain about being discriminated against.
  - Men should be willing to sacrifice their own well being in order to provide financially for the women in their lives.
- For each of the following information you learned about your partners, say how much it influenced your interactions: [Answer choices were on a 5-point likert scale: Influenced

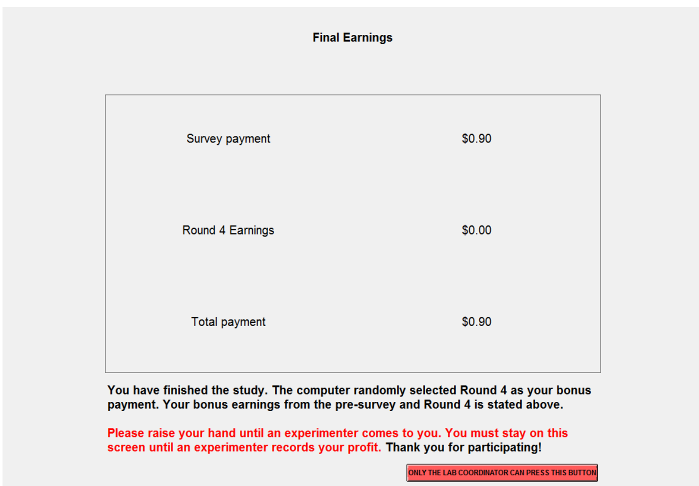
a lot, Influenced a little, Influenced in some period, Did no influence very much, Did not influence at all.]

- Gender
- Dominant hand
- Only Child
- Month of birth
- Ability to roll tongue
- "Hitchhiker" thumb

- Did your mother work full-time outside the home when you were growing up? [Answer choices: yes, no]
- Do you have any other comments about this study?

(Notes to the experimenter: after the post-survey, the computer randomly selected the round for bonus payment and final payoffs were revealed. Figure A.6.9 shows an example of the final payoff screen.)

Figure A.6.9: Example of Final Payoff Window



A.7. Negotiation Transcript Classification Protocol

After all sessions were finished, we had MTurk workers classify different communication strategies. Each MTurk worker reviewed 15 randomly selected negotiation transcripts. To ensure high quality of work, MTurk workers reviewed the communication strategy definitions and had to answer all 8 comprehension questions correctly to continue. Additionally, workers where also asked an attention question and if any worker failed to pass the attention question we discarded their work. Below are the specific instructions provided to MTurk workers.

General Instructions:

In this survey you will be asked to read through 16 different conversations and answer some questions regarding each conversation. Each conversation has only two people: Person A and Person B. Person A and Person B are negotiating over how to split $20 dollars, BUT there are only two possible way to split it: $15 for one person and $5 for the other, or the reverse. If they do not agree, then they will both get $0.

Note: In this survey you will be asked comprehension questions and attention questions. Please read all instructions and materials carefully. If you fail the comprehension question, you will not be allowed to continue with the HIT. If you fail the attention questions, your work will be rejected. There is a probability that we will check a randomly selected question. If we find your work was not completed accurately, we may reject your work.

Definitions:

Before you begin, we will define some negotiation strategies that you will help identify in the 16 conversations. Please read through each of the definitions and examples carefully. (Do not worry if you are unable to memorize or remember all the definitions, these will be provided again later.)

Hard commitment: this is when a person starts the conversation (not including saying "hi" or other pleasantries) stating that they will pick $15 for themselves regardless of what the other person is choosing. They have set their mind to this outcome and will not change.

- *Example 1: "Hi, I'm always choosing 15 not matter what, that is my strategy."*
- *Example 2: "I'm letting you know that I'm picking 15 regardless of what you do."*

Tough talker: this is when person is a tough negotiator and fights for the $15. They are trying hard to convince the other person to take $5. This may happen at any point in the conversation. They will use a strong tone and may seem "pushy" or "mean." (Note: Someone using a tough talker strategy may also be playing a hard commitment strategy.)

- *Example 1: "It's my turn to take $15, I let the other person have theirs"*

Asked for the $15: this is when a person asks the other person if they can take the $15 at any point in the conversation.

- *Example 1: "Can I pick $15?"*
- *Example 2: "Would it be ok if I pick 15?"*

Led with a concession: this is when a person starts the conversation (not including saying "hi" or other pleasantries) by offering the $15 to the other person or stating that they will take $5.

- *Example 1: "Hi, you can take the $15" or "Hi, I'll pick $5"*
- *Example 2: "Hi, you can pick which one you want, I'll pick the other option."*

Offered the $15: this is when a person offers $15 to the other person or offers to pick the $5 at any point in the conversation. (Note: someone offering $15 may also be doing a "led with a concession" strategy.)

- *Example 1: "Ok, you can pick $15"*

- *Example 2: "I'll just take the $5"*

Friendly negotiator: this is when the person tries to be friendly and build a relationship with the other person in order to gain their trust. We provided each person some information about the other person (e.g., birthday month, can they roll their tongue, do they have hitchhiker thumbs, etc) – many times, the person will comment on one of these traits.

- *Example 1: "Hi, how is your day going?"*

- *Example 2: "Happy birthday month! Oh look, we both have hitchhiker thumbs!"*

âĂŃ **Started negotiations:** this is the person that starts the negotiations on how to split the money, not including saying "hi" or other pleasantries.

- *Example 1: "We should discuss this so we can cooperate and get something. How do you want to split this?"*

- *Example 2: "Any ideas on what we should do?"*

Random game: this is the person that introduces a random game such as playing rock/paper/scissor ("rps"), guessing a number, using trivia questions, using birthday dates, or other similar games to choose who picks $15 for themselves.

- *Example 1: "What if we play rps?"*

- *Example 2: "How about I think of a number, 0 or 1, and if you guess it you get $15. I promise to tell the truth"*

- *Example 3: "We were both born in June! Let's do birth dates, the closest one wins $15?"*

Sad story: this is a person that uses their current (unfortunate) situation to gain sympathy from the other person and tries to get the $15.

- *Example 1: "I really need the money, my fridge broke so I need to buy food."*
- *Example 2: "I'm having a terrible day, I just failed my midterm."*
- *Example 3: "I'm poor, I need the money for food and to pay for college."*

Happy emojis: this is when a person uses any sort of happy "emojis" or smiley faces.

- *Example 1: ":)"*
- *Example 2: ":D"*

Sad/angry emojis: this is when a person uses any sort of sad or angry "emojis".

- *Example 1: ":("*
- *Example 2: ">:("*

Mentioned the word fair: this is when the person mentions anything about trying to make a fair split

- *Example 1: "How can we do this fairly?"*
- *Example 2: "I'll pick $5, its fair since you picked $5 before"*
- *Example 3: "I don't know how to split this in a fair way."*

Mentioned previous choices/outcomes: this is when the person mentions what they previously chose. Individuals had to negotiate with multiple people, so sometimes they will mention what their previous choice was.

- *Example 1: "I chose 5, 15, 5 in the previous rounds"*

- *Example 2: "But I've picked 5 in the last 2 rounds too"*

Alternating strategy: this is when the person claims to be alternating between 5 and 15 and that this is their strategy.

- *Example 1: "I'm alternating between 5 and 15"*
- *Example 2: "My strategy is to pick 5 on even rounds and 15 on odd rounds"*

Gave-in: this is when the person gives in to the other person's ask or demands after there is an initial negotiation or back-and-forth.

- *Example 1: Person A: "Can I pick 15?"*
  - *Person A: "Can I pick 15?"*
  - *Person B: "I would like 15 as well. Why do you want 15?"*
  - *Person A: "Honestly, I need it to buy food."*
  - *Person B: "Oh, me too. This is hard...Ok, you can take the $15."*
- *(In this example, Person B is "giving in".)*

Comprehension Check:

Before you begin, we will let you practice. Please read the conversation below and answer the questions carefully. If you have any question, please refer to the definitions and examples above.

If you are unable to correctly identify the negotiation strategies in the following practice conversation, you will not be allowed to proceed with the task and you will not receive payment for this HIT.

(Notes for the experimenter: In order to ensure the highest quality of data, MTurk workers had to pass both comprehension checks to proceed to the actual classification. Figures A.7.1 and A.7.2 shows a screenshot of both comprehension questions. Failure to pass the comprehension check meant the MTurk worker would not be allowed to proceed and would have to "return" the task. MTurk workers who passed the comprehension question proceeded to the actual negotiation analysis (see Figure A.7.3). Each MTurk worker analyzed 7 negotiations (see Figure A.7.4 for example). Then they saw an attention check question (disguised as an 8th conversation) followed by 8 more actual negotiations. Figure A.7.5 show an example of the attention check question.)

Figure A.7.1: Example of Comprehension Check 1

Practice 1:
Please read the conversation below and answer the questions.

Conversation:

Person A: i want the $15 no matter what
Person B: Im gonna choose it regardless
Person B: so you can either take $0 or $5
Person A: i'm also gonna choose it regardless

For all items, check if Person A did it or if Person B did it. If both did it, check both. If neither did, check neither.
(Please refer back to the conversation shown above if you are unsure. Definitions of the different strategies defined earlier are available after each question in parentheses.)

Remember, you will only be allowed to proceed with the HIT (and get paid) if you answer these practice questions correctly.

	Person A	Person B	Both	Neither
Used a hard commitment strategy? (This is when a person starts the conversation (not including saying "hi" or other pleasantries) stating that they will pick $15 for themselves regardless of what the other person is choosing. They have set their mind to this outcome and will not change.)				
If a hard commitment was used by both Person A and Person B, who announced it first? (If only one person used the hard commitment strategy or neither used the hard commitment strategy then check neither)				
Led with a concession? (This is when a person starts the conversation (not including saying "hi" or other pleasantries) by offering the $15 to the other person or stating that they will take $5.)				
Offered the $15 to the other person? (This is when a person offers $15 to the other person or offers to pick the $5 at any point in the conversation. (Note: someone offering $15 may also be doing a "led with a concession" strategy.))				

(Notes for the experimenter: At the end, MTurk workers answered a demographics question. Once MTurker workers submitted the completed task, the attention question was checked and any worker who failed the attention check was told so and their work was discarded.)

Figure A.7.2: Example of Comprehension Check 2

Practice 2:
Please read the conversation below and answer the questions.

Conversation:

Person A: Hi
Person B: you can take the $15, I'll pick $5
Person B: haha
Person A: haha are you sure?
Person B: yeah its cool

For all items, check if Person A did it or if Person B did it. If both did it, check both. If neither did, check neither.
(Please refer back to the conversation shown above if you are unsure. Definitions of the different strategies defined earlier are available after each question in parentheses.)

Remember, you will only be allowed to proceed with the HIT (and get paid) if you answer this practice questions correctly.

	Person A	Person B	Both	Neither
Used a hard commitment strategy? (This is when a person starts the conversation (not including saying "hi" or other pleasantries) stating that they will pick $15 for themselves regardless of what the other person is choosing. They have set their mind to this outcome and will not change.)	○	○	○	○
If a hard commitment was used by both Person A and Person B, who announced it first? (If only one person used the hard commitment strategy or neither used the hard commitment strategy then check neither)	○	○	○	○
Led with a concession? (This is when a person starts the conversation (not including saying "hi" or other pleasantries) by offering the $15 to the other person or stating that they will take $5.)	○	○	○	○
Offered the $15 to the other person? (This is when a person offers $15 to the other person or offers to pick the $5 at any point in the conversation. (Note: someone offering $15 may also be doing a "led with a concession" strategy.))	○	○	○	○

Figure A.7.3: Example of Comprehension Check Passing/Failure Message

(a) Comprehension Check: Failure Message

You did NOT pass the comprehension check.

Unfortunately, you did not correctly identify the negotiation strategies in the practice questions. You will not be able to continue with this HIT.

Please return the HIT.

(b) Comprehension Check: Passing Message

You correctly identified the negotiation strategies in the practice questions.

Please click next and you will start classifying actual negotiation conversations.

Figure A.7.4: Example of Negotiation Transcript Classification

Please read the conversation below and answer the questions.

Conversation ID: 13001-130013-10

Conversation:

Person A: U choose the split, I dont care
Person B: Cool if I keep the 15 this time? Not feeling very generous :P
Person A: Yeah, go ahead
Person B: Gotcha.
Person B: tough decisions I swear
Person B: Feel like I'm in vegas
Person A: This is a really interesting game theory problem. I should really take game theory next year
Person B: Sounds fun!
Person B: Hope you get an A
Person A: thanks
Person A: it was much harder when we couldnt chat lol

For all items, check if Person A did it or if Person B did it. If both did it, check both. If neither did, check neither.
(Please refer back to the conversation shown above if you are unsure. Definitions of the different strategies defined earlier are available after each question in parentheses.)

	Person A	Person B	Both	Neither
Used a hard commitment strategy? (This is when a person starts the conversation (not including saying "hi" or other pleasantries) stating that they will pick $15 for themselves regardless of what the other person is choosing. They have set their mind to this outcome and will not change.)				
If a hard commitment was used by both Person A and Person B, who announced it first? (If only one person used the hard commitment strategy or neither used the hard commitment strategy then check neither)				
Was a tough talker? (This is when person is a tough negotiator and fights for the $15. They are trying hard to convince the other person to take $5. This may happen at any point in the conversation. (Note: Someone using a tough talker strategy may also be playing a hard commitment strategy.))				
Asked for the $15? (This is when a person asks the other person if they can take the $15 at any point in the conversation.)				
Led with a concession? (This is when a person starts the conversation (not including saying "hi" or other pleasantries) by offering the $15 to the other person or stating that they will take $5.)				
Offered the $15 to the other person? (This is when a person offers $15 to the other person or offers to pick the $5 at any point in the conversation. (Note: someone offering $15 may also be doing a "led with a concession" strategy.))				
	Person A	**Person B**	**Both**	**Neither**
Was a friendly negotiator? (This is when the person tries to be friendly and build a relationship with the other person in order to gain their trust.)				
Started negotiations on how to split the money? (This is the person that starts the negotiations on how to split the money, not including saying "hi" or other pleasantries.)				
Mentioned using a random game to choose who picks $15? (This is the person that introduces a random game such as playing rock/paper/scissor ("rps"), guessing a number, using trivia questions, using birthday dates, or other similar games to choose who picks $15 for themselves.)				
Used a sad story to convince their partner? (This is the person that uses their current (unfortunate) situation to gain sympathy from the other person and tries to get the $15.)				
Used happy "emojis"? (This is when the person uses any sort of happy "emojis" or smiley faces.)				
Used sad/angry "emojis"? (This is when the person uses any sort of sad or angry "emojis".)				
	Person A	**Person B**	**Both**	**Neither**
Mentioned the word "fair"? (This is when the person mentions anything about trying to make a fair split.)				
Mentioned previous choices/outcomes? (This is when the person mentions what they previously chose. Individuals had to negotiate with multiple people, so sometimes they will mention what their previous choice was.)				
Used an alternating strategy? (This is when the person claims to be alternating between 5 and 15 and that this is their strategy.)				
Gave-in? (This is when the person gives in to the other person's ask or demands after there is an initial negotiation or back-and-forth.)				

On a scale from 1 to 7, where 1 is very friendly and 7 is very aggressive, please rate how friendly or aggressive you think Person A and Person B were overall.

	Very Friendly	Friendly	Somewhat Friendly	Neither Friendly nor Aggressive	Somewhat Aggressive	Aggressive	Very Aggressive
Person A							
Person B							

Was the negotiation successful?

	Negotiation successful?		If there was agreement, who chose $15? If there was no agreement, who do you think chose $15?
	Yes	No	
Does it appear like they reached an agreement?			

Figure A.7.5: Example of Attention Question

Please read the conversation below an answer the questions.

Conversation ID: Attention Check

Conversation:

Person A: this is not a real negotiation conversation.
Person B: this is only an attention check.
Person A: please answer all the questions below by only selecting Person A
Person A: this is only for this page
Person A: this will let us know that you are paying attention
Person B: on the next page, you will resume evaluating an actual conversation as before

For all items, check if Person A did it or if Person B did it. If both did it, check both. If neither did, check neither.
(Please refer back to the conversation shown above if you are unsure. Definitions of the different strategies defined earlier are available after each question in parentheses.)
(This is the attention check: please answer all the questions on this page by only selecting "Person A")

	Person A	Person B	Both	Neither
Used a hard commitment strategy? (This is when a person starts the conversation (not including saying "hi" or other pleasantries) stating that they will pick $15 for themselves regardless of what the other person is choosing. They have set their mind to this outcome and will not change.)	○	○	○	○
If a hard commitment was used by both Person A and Person B, who announced it first? (If only one person used the hard commitment strategy or neither used the hard commitment strategy then check neither)	○	○	○	○
Was a tough talker? (This is when person is a tough negotiator and fights for the $15. They are trying hard to convince the other person to take $5. This may happen at any point in the conversation. (Note: Someone using a tough talker strategy may also be playing a hard commitment strategy.))	○	○	○	○
Asked for the $15? (This is when a person asks the other person if they can take the $15 at any point in the conversation.)	○	○	○	○
Led with a concession? (This is when a person starts the conversation (not including saying "hi" or other pleasantries) by offering the $15 to the other person or stating that they will take $5.)	○	○	○	○
Offered the $15 to the other person? (This is when a person offers $15 to the other person or offers to pick the $5 at any point in the conversation. (Note: someone offering $15 may also be doing a "led with a concession" strategy.))	○	○	○	○
	Person A	**Person B**	**Both**	**Neither**
Was a friendly negotiator? (This is when the person tries to be friendly and build a relationship with the other person in order to gain their trust.)	○	○	○	○
Started negotiations on how to split the money? (This is the person that starts the negotiations on how to split the money, not including saying "hi" or other pleasantries.)	○	○	○	○
Mentioned using a random game to choose who picks $15? (This is the person that introduces a random game such as playing rock/paper/scissor ("rps"), guessing a number, using trivia questions, using birthday dates, or other similar games to choose who picks $15 for themselves.)	○	○	○	○
Used a sad story to convince their partner? (This is the person that uses their current (unfortunate) situation to gain sympathy from the other person and tries to get the $15.)	○	○	○	○
Used happy "emojis"? (This is when the person uses any sort of happy "emojis" or smiley faces.)	○	○	○	○
Used sad/angry "emojis"? (This is when the person uses any sort of sad or angry "emojis".)	○	○	○	○
	Person A	**Person B**	**Both**	**Neither**
Mentioned the word "fair"? (This is when the person mentions anything about trying to make a fair split.)	○	○	○	○
Mentioned previous choices/outcomes? (This is when the person mentions what they previously chose. Individuals had to negotiate with multiple people, so sometimes they will mention what their previous choice was.)	○	○	○	○
Used an alternating strategy? (This is when the person claims to be alternating between 5 and 15 and that this is their strategy.)	○	○	○	○
Gave-in? (This is when the person gives in to the other person's ask or demands after there is an initial negotiation or back-and-forth.)	○	○	○	○

MTurk Worker Demographics Survey:

You have now completed identifying all 16 negotiations conversations. Please answer the following questions. Afterwards, you will be told your completion code.

- What is your gender? [Answer choices: male, female]
- What is your year of birth?
- What is your employment status? [Answer choices: (1) unemployed; (2) full-time employment (3) part-time employment]
- Are you a native English speaker? [Answer choices: yes, no]
- Please choose the answer that best describes your political ideology. [Answer choices: (1) very liberal; (2) somewhat liberal; (3) slightly liberal; (4) Neither liberal nor conservative; (5) slightly conservative; (6) somewhat conservative; (7) very conservative]
- What is the highest degree or level of school you have completed? If currently enrolled, highest degree received. [Answer choices: (1) Some high school, no diploma; (2) High school graduate, diploma or equivalent (for example, GED); (3) Some college credit, no degree; (4) Trade/technical/vocational training; (5) Associate degree; (6) Bachelor's degree; (7) Master's degree; (8) Professional degree (for example, JD or MD); (8) Doctorate degree (Ph.D)]
- Please specify your ethnicity. [Answer choices: (1) Caucasian; (2) Hispanic or Latino; (3) Black/African American; (4) Native American/American Indian; (5) Asian/Pacific Islander; (6) Middle Eastern; (7) Other (please specify)]
- Please let us know what you thought of the survey. Was anything confusing?

A.8. The Value of Information and Role of Fairness in Bargaining Experimental Protocol

After consenting to participate in this study, the experimenter read the instructions out loud and participants followed along. Buyers (proposers) and sellers (responders) meet in a given round with the opportunity to trade. It is common knowledge that the buyer's value for the good V is either 70 or 90 experimental units (each with 50% chance) and that the seller's cost to produce the good C is either 10 or 30 experimental units (each with 50% chance). In each round, the buyer makes a take-it-or-leave-it offer P to the seller to purchase the good. If the offer is accepted, the buyer receives $\pi_B = V - P$ and the seller receives $\pi_S = P - C$, where π_B and π_S are denoted in experimental units. If the offer is rejected, both players get 0 experimental units, so $\pi_B = 0$ and $\pi_S = 0$. The seller cannot accept an offer for which $P < C$.

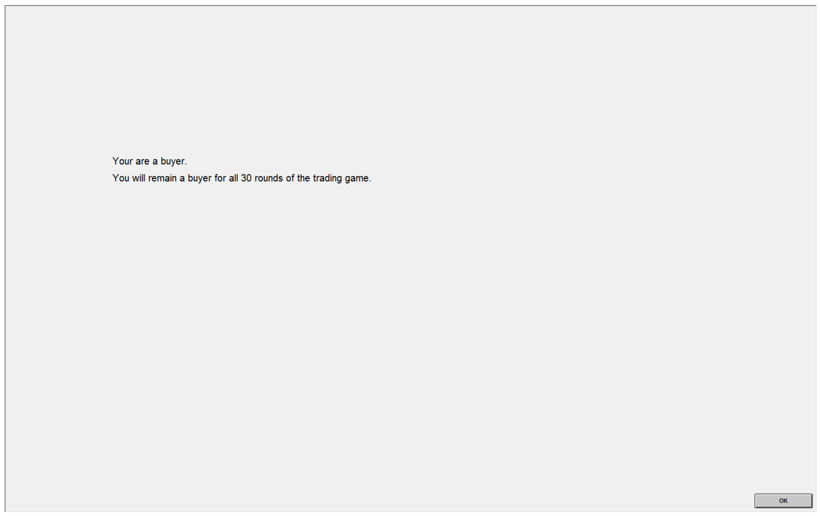
Afterwards, participants were randomly assigned to be a buyer or a seller. They maintain this role for the remainder of the experiment. Figure A.8.1 shows how participants' screens for role assignment.

The experimental design varies what information players have about the realization of the value V and cost C. Randomization of treatment was performed at the session level. The buyer always knows her value and the seller always knows his cost. In the *Incomplete Information* treatment, this is all the players know. In additional treatments, we provide additional information (about V and C) to either: the buyer, the seller, or both. In all treatments, the information structure is common knowledge. Figure A.8.2 shows an example of the buyer's offer screen and the seller's acceptance screen for the Complete Information treatment. Figure A.8.3 shows an example of the buyer's offer screen and the seller's acceptance screen for the Incomplete Information treatment.

Subjects receive the outcome of bargaining game at the end of each round. Subjects are informed or reminded at the end of each round only of their own action and the action

Figure A.8.1: Example of Role Assignment

(a) Buyer's Screen



(b) Seller's Screen

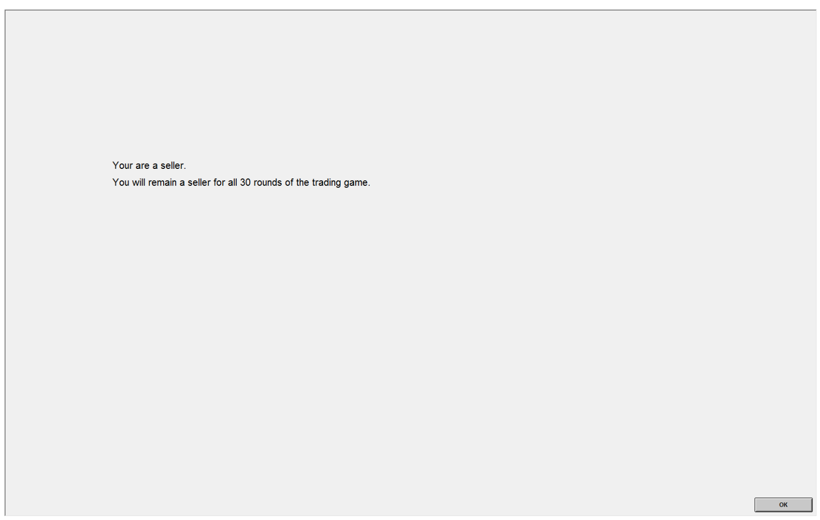
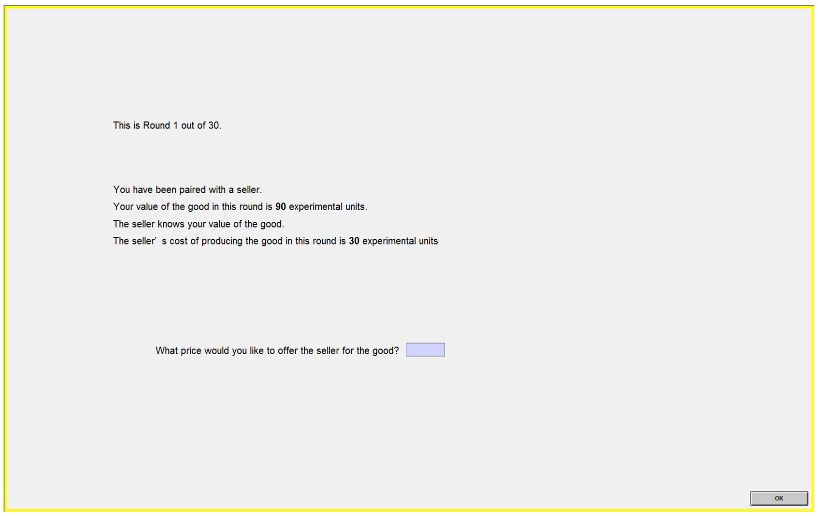


Figure A.8.2: Example of Complete Information (CI) Buyer and Seller Screens

(a) Buyer's Offer Decision Screen



(b) Seller's Accept Decision Screen

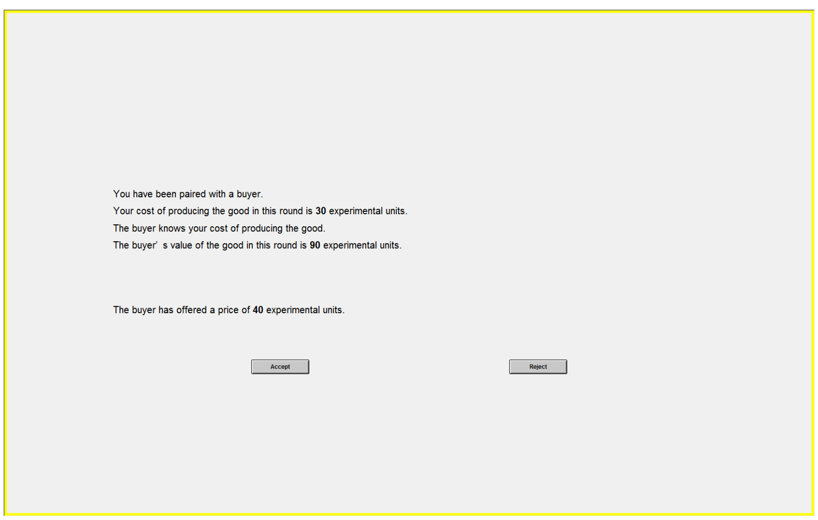
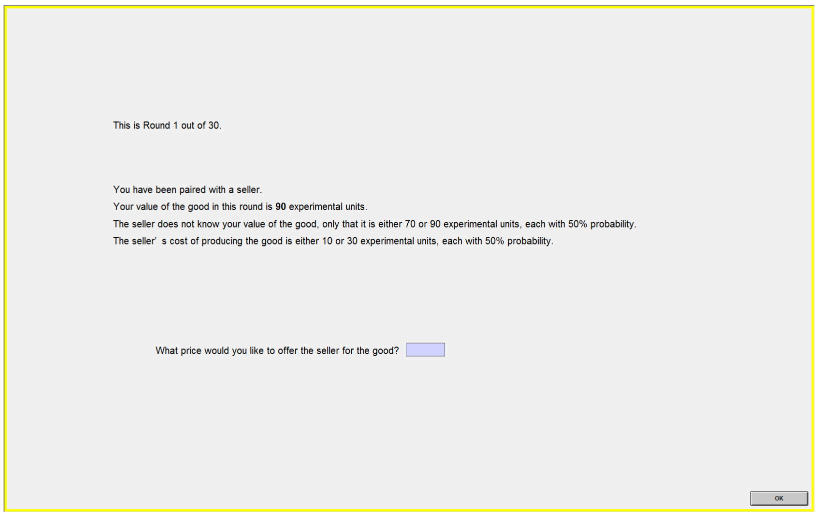
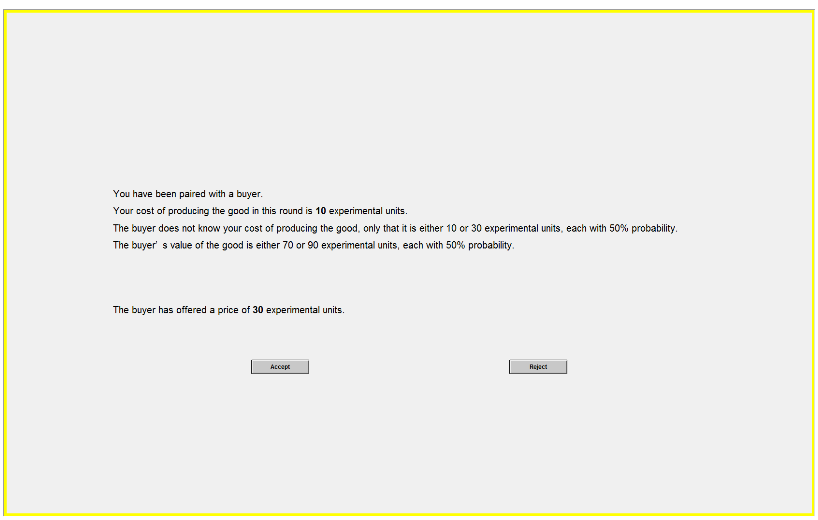


Figure A.8.3: Example of Incomplete Information (II) Buyer and Seller Screens

(a) Buyer's Offer Decision Screen



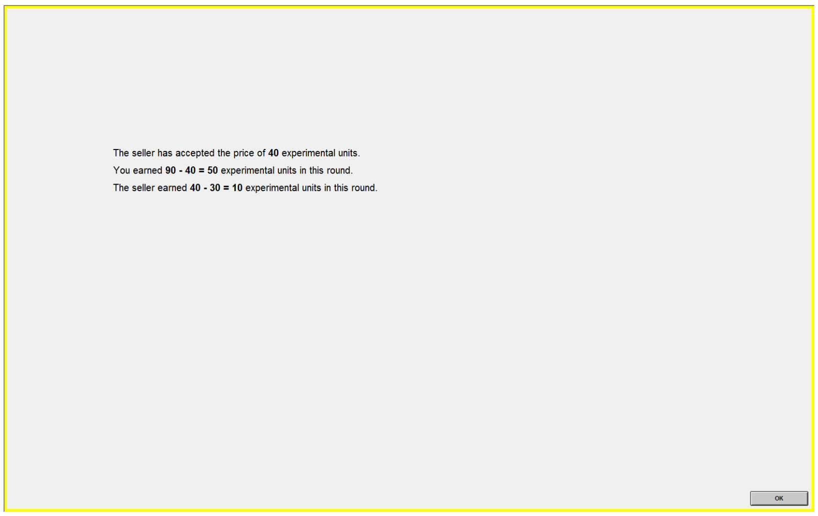
(b) Seller's Accept Decision Screen



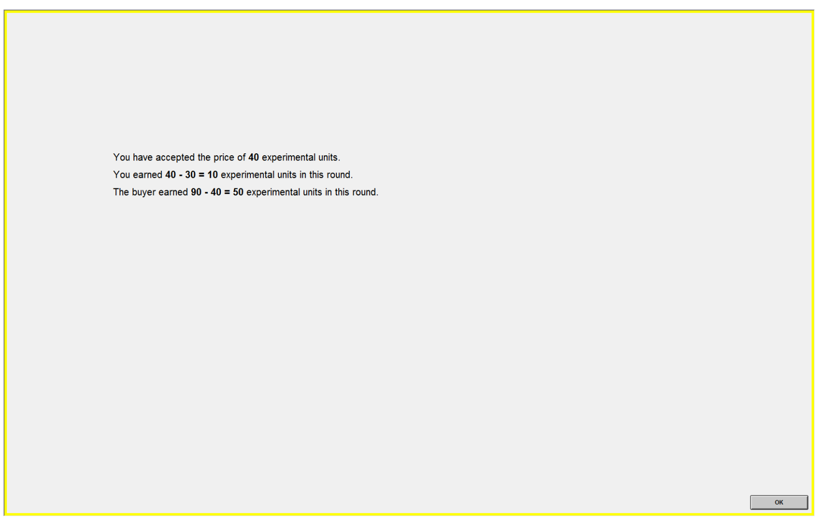
of their partner, as well as the information they had in that round. Figure A.8.4 shows an example of the buyer's and seller's outcome screen for that round for the Complete Information treatment. Figure A.8.5 shows an example of the buyer's and seller's outcome screen for the Incomplete Information treatment.

Figure A.8.4: Example of Complete Information (CI) Outcome Screens

(a) Buyer's Outcome Screen



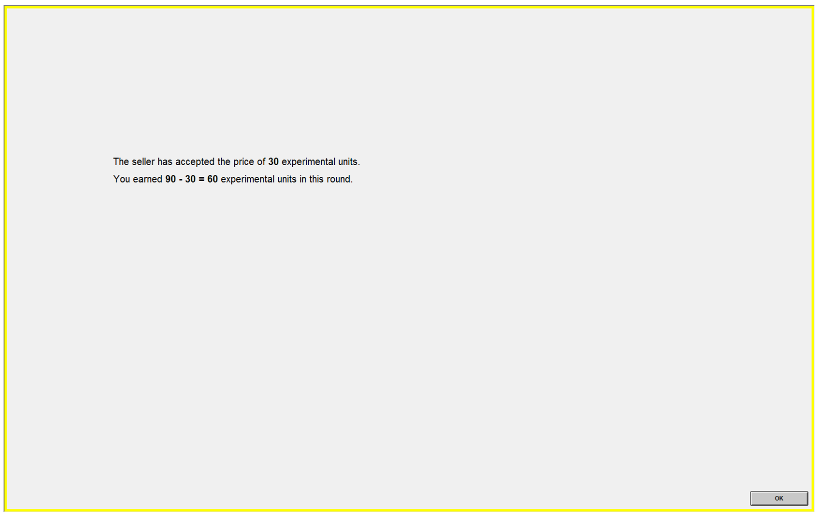
(b) Seller's Outcome Screen



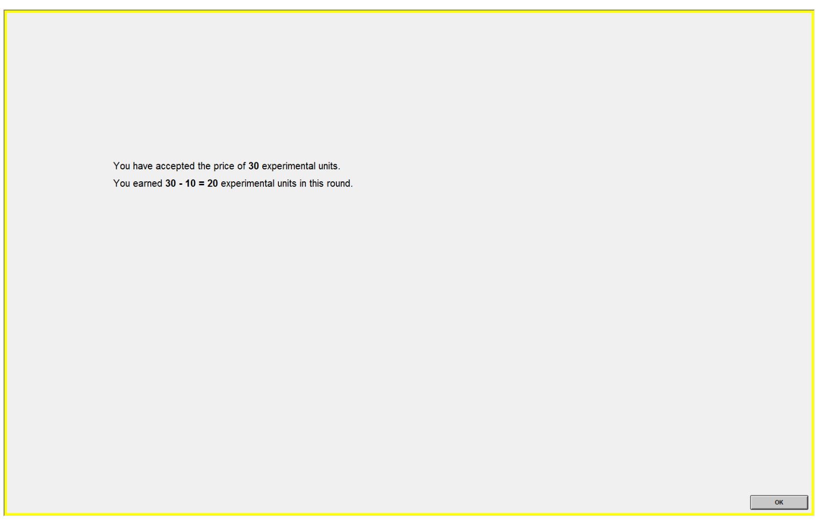
Subjects play a total of 30 rounds of the bargaining game. After the 30 rounds, subjects answer additional questions that depend on their treatment and role. In this paper, we will focus on one of these questions. Buyers in all information conditions were asked in an

Figure A.8.5: Example of Incomplete Information (II) Outcome Screens

(a) Buyer's Outcome Screen



(b) Seller's Outcome Screen



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