How much you talk matters: Cheap talk and collusion in a Bertrand oligopoly game

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Abstract

This study investigates the impact of cheap talk on price in a repeated Bertrand oligopoly experiment. Participants are placed in three-person bidding groups where the lowest bid wins. During the first 10 rounds, participants are not allowed to communicate with each other. All three-person groups show decreasing market prices in the first 10 rounds. We then play another 20 rounds where participants can text with one another using an instant message system. Some groups were allowed to text before every round, some to text before every other round, some to text every third round, some to text every fourth round, and some to text only every fifth round. When texting is allowed, All groups attempt to collude to raise the price after being allowed to text. Whether they are successful depends on the combination of how often they can text and whether all three participants actually text.

Keywords: Bertrand Competition, Experiments, Collusion, Cheap Talk, Amazon MTurk
JEL Classification: C7, C92, K21, L41

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1. Introduction

Collusion is cooperation that reduces competition in markets. Collusion may harm consumers if colluders are able to keep prices above competitive equilibrium prices; it may harm competitors if colluders are able to keep other firms from entering markets. Under the 1890 Sherman Antitrust Act, one of the largest and most important case is lysine price-fixing. During 1992-1995, Archer Daniels Midland and other companies agreed to cooperate and sold lysine at certain prices. They overcharged by at least $70 million, and the related social costs exceed $200 million (Connor 1997). This case raises the questions of how many firms are required for competition and whether trade organizations that encourage "cheap talk" among competitors are anti-competitive (Farrell, 1987; Huck et al., 2004; Horstmann et al., 2018).

Because one of the activities that fosters collusive behavior is communication among competing firms, studies report that companies attempting to coordinate on price meet regularly (Genesove and Mullin, 2008; Harrington Jr., 2006). It is hypothesized that meetings among competing firms often lead to cooperation. These meetings may allow firms to maintain observably higher prices for long periods of time. The observation that some companies succeed in raising prices after meeting with one another encourages other companies to meet to discuss prices and monitor compliance with previously discussed prices (Awaya and Krishna, 2016).

Firms meeting to discuss pricing and compliance pose a problem for social welfare and efficiency. Firms are assumed to be able to keep prices above competitive equilibrium prices and quantities of goods and services sold below competitive equilibrium quantities. The combination of higher prices and lower quantities impose a dead-weight loss on the market, reducing efficiency (Posner, 1975; Fisher, 1985; Charness, 2000).

Hence, it is obvious to most economists that collusion needs to be prohibited. The unanswered question is, can unenforceable "cheap talk" assist coordination sufficiently to allow firms that "talk" with one another to maintain prices and profits above the competitive equilibrium. Theoretical literature published in the last 15 years (Away and Krishna, 2016; Friedman, 2014; Blume and Ortman, 2007; and Fudenburg and Levine, 2007) explores the possibility that firms can use non-binding "cheap talk" to coordinate to raise prices and profits. This paper explores the question, how frequently do firms need to "talk" to coordinate to significantly and sustainably raise prices and profits above the competitive equilibrium?

We use a simple 3-person repeated Bertrand oligopoly experiment to begin to answer this question. Each participant can text, with no restrictions, with two other participants using an instant message system. Each texting opportunity lasts 30 seconds. We first run groups of three subjects through 10 rounds of pricing without allowing cheap talk communication to establish a baseline. For the next 20 rounds, we allow some subjects to text before every round, some to text before every other round, some to text every third round, some to text every fourth round, and some to text only every fifth round. The control groups continue for 20 more rounds with no opportunities to text.

We find that market prices decrease in the first 10 rounds when no texting is allowed. On average, all groups which are allowed to text attempt to collude to raise the price when allowed to text. Average market prices rise in the rounds when discussions are allowed and decline in the rounds when they are not. The opportunity to text frequently helps collusion. The more chances a group has to text, the more likely they will try to raise prices. All three subjects in a group have to actively engage in texting to actually raise average prices.

The remainder of this paper proceeds as follows. **Section 2** presents and summarizes the previous articles related to this topic. **Section 3** describes how we design the experiment. **Section 4** describes the way we collect the data, the characteristics of the data, and an in-depth analysis of the data. **Section 5** describes significant findings on pricing behavior. The final section offers some concluding statements.

2. Communication in Experiments

Communication is an essential tool for collusive pricing behavior, as well as for coordination and cooperation in general. Jiménez and Cosano (2021) provide a review of the literature going back to the 1990a. They state that the literature concludes that pre-play communication enhances the probability that non-equilibrium strategies that promote cooperation (collusion) to achieve higher profits can be achieved. One of the most commonly used forms of pre-play communication is non-binding talk, often referred to as "cheap talk" (Holt and Davis, 1990; Cooper et al., 1992; Cason, 1995).

Some experiments using various types of games show that communication leads to cooperation. Some papers show the impact of communication in a repeated double auction, posted offer, sealed bid-offer, and Cournot market (Isaac and Plott, 1981; Isaac et al., 1984; Isaac and Walker, 1985; Gomez-Martinez et al., 2016). These studies find that subjects may be able to attain higher profits for some periods (collusion), but subjects are not able to sustain those higher profits with repetition. Two-stage static game and Hotelling competition are also used to investigate the effect of communication (Brown-Kruse et al., 1993; Brown-Kruse and Schenk, 2000; Andersson and Wengström, 2012). They report that messages have a positive and

significant effect on cooperation and lead to higher profits. There are a number of studies specifically studying Bertrand competition with communication. Using a Bertrand duopoly design, Andersson and Wengström (2007) find that costly communication decreases the number of messages but enhances the stability of collusive agreements compared to no-cost communication. Fonseca and Normann (2012, 2014) also point out that communication works and fosters cartel formation.

Some studies focus on free-form communication (Charness and Dufwenberg, 2006; Goeree and Yariv, 2011; Agranov and Tergiman, 2014). The articles investigate the effect of leniency in antitrust used chat as a treatment, but their focus is not the communication itself (Hinloopen and Soetevent, 2008; Bigoni et al., 2012; Andres et al., 2021). Cooper and Kühn (2014) conduct a repeated two-period Bertrand game with five different communication treatments. They find that communication works as a coordinating device improving cooperation. Harrington Jr. et al. (2016) conclude that the efficacy of communication depends on market structure. They also compare price announcements and unrestricted written communication. They report that free-form communication leads to higher prices in all market structures.

However, to our knowledge, there is no systematic study of how much free-form communication is required to maintain price collusion. This paper studies repeated Bertrand Oligopoly competition using an instant message system that allows participants to send messages without any limitation. We vary the frequency of communication allowed. We also consider the difference between groups allowed to communicate who do not communicate, and groups in which all subjects take advantage of their ability to communicate.

3. Experimental Design

We examine Bertrand oligopoly markets with inelastic demand and a constant marginal cost of production (Holt et al., 1986). Participants are placed in groups of three according to their arrival times. Each participant is endowed with one unit of a hypothetical good each round. The goal of each round is to sell each unit at the highest possible price in an auction where the lowest price wins. The constant marginal cost is 0 for all. Each participant privately sets her or his price between 0 and 100. The lowest price in the group wins, and the participant who names the lowest price earns that price as private profit. In the case of a tie for the lowest price, the profit is evenly divided among those naming the lowest price.

Since each participant makes either the lowest price (possibly shared) or 0 as profit, the incentive is to set a price lower than either of the participant's two competitors. Each participant plays this game for 30 rounds with the same two other participants. The equilibrium of this game is for everyone to name a price of 0 and for everyone to earn 0 profit. On the other hand, if subjects can agree on a higher price and actually set that higher price, the profit-maximizing strategy is for every subject to set a price of 100 and for each subject to earn 33.3 each round.

Table 1 Structure of Experiments

Treatment	**Round**																					
	1-10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30
Every		v	v	v	v	v	v	v	v	v	v	v	v	v	v	v	v	v	v	v	v
Every2		v		v		v		v		v		v		v		v		v		v	
Every3		v			v			v			v			v			v			v	
Every4		v				v				v				v				v			
Every5		v					v					v					v				
Never																					

NOTE: "v" represents a round where participants can text each other.

Participants cannot communicate with each other for the first 10 rounds; however, they may text each other, beginning with the 11th round. The frequency of communication varies according to the treatment level to which a participant is assigned. The control groups (Never) can never text. There are five treatment groups: texting each round (Every), every other round (Every2), every three rounds (Every3), every four rounds (Every4), and every five rounds (Every5). Each conversation lasts for 30 seconds, and messages are sent through the Instant Messenger (IM) built into the game. **Table 1** summarizes the experimental structure.

The topic of the conversation can be anything and does not have to be related to price. We advise participants to protect their own privacy and keep the discussion respectful. Participants also have the right to skip the conversation by clicking the "Next" button provided in the IM. Unlike other experiments, communication is not forced. The lowest price is disclosed to everyone at the end of each round, but the participant who has chosen the lowest price is not revealed. In most other cartel-related experiments, the full vector of prices is revealed. However, we disclose only selling prices. This gives participants more temptation to deviate from a collusive agreement than if we revealed each participant's price. Communication is required to keep participants in the line and reiterate threats of punishment.

4. Data

The experiment was done on Amazon MTurk via oTree (Chen et al., 2016) (IRB ID 19-189, Iowa State University). MTurk workers are generally more diverse than student subjects, but seem to respond to experimental stimuli in a manner consistent with the results of prior research (Cassese et al., 2013; Hoffman et al., 2020). We prevent retaking and set regional

restrictions to the US so that participants can text each other smoothly. To conform to the schedules that most MTurk subjects work, the experiments were open from 9 AM CT through 8 PM CT. Two groups belonging to the same treatment were allowed to participate each hour, and different treatments were provided every hour. The first treatment of the day started with the least collected treatment from the day before.

From August 2020 to September 2020, we ran 30-round experiments with 360 participants, including 20 groups per treatment and control groups. We also imposed qualifications that are commonly used by other research using MTurk. The experiment required participants to have completed more than 10 approved Human Intelligence Tasks (HIT), with approval scores of more than 95%. They were paid a $2 completion fee, and 3000 points were worth $7.5. We also excluded all automatically submitted tasks suspected of bots and did not pay. Completing the experiment took 25-30 minutes. Subjects interacted for 30 rounds and each round lasted at most 40 seconds. All 30 rounds were played with the same group of 3 individuals.

Table 2 shows the characteristics of participants who reported their personal information. Although not every participant provided his or her personal characteristics, we observe that they are randomly assigned to their tasks. Their education levels are (1) High school, (2) Some college, (3) Associate degree, (4) Bachelor's degree, (5) Master's degree, and (6) Doctoral degree. Their average age is 35-38 years old, education level is 3.9-4.2, and the Female ratio is 27-42%. The result of a Jonckheere-Terpstra Test shows that all variables show no priori ordering of the populations (cannot reject the null hypothesis).

Table 2 Characteristics of Participants

Never

Variable	Obs	Mean	Std. Dev.	Min	Max
Age	57	35.39	8.76	22	63
Education	57	4.18	0.68	2	5
Female	57	0.42	0.50	0	1

Every5

Variable	Obs	Mean	Std. Dev.	Min	Max
Age	59	37.86	10.58	22	70
Education	59	4.19	0.92	1	6
Female	59	0.27	0.45	0	1

Every4

Variable	Obs	Mean	Std. Dev.	Min	Max
Age	58	35.64	10.80	22	66
Education	58	4.00	1.06	1	5
Female	58	0.33	0.47	0	1

Every3

Variable	Obs	Mean	Std. Dev.	Min	Max
Age	60	37.22	10.88	25	70
Education	60	3.88	1.15	1	5
Female	60	0.20	0.40	0	1

Every2

Variable	Obs	Mean	Std. Dev.	Min	Max
Age	59	36.02	9.41	20	65
Education	59	3.92	1.06	1	5
Female	59	0.39	0.49	0	1

Every

Variable	Obs	Mean	Std. Dev.	Min	Max
Age	59	36.88	11.59	20	65
Education	59	4.12	0.79	2	5
Female	59	0.41	0.50	0	1

NOTE: This tables exclude participants who did not report their characteristics.

Table 3 shows the texting patterns in experiments. In **Table 3**, 36% of treated groups who can text messages to other participants in the same market actually do text and only 30% do not text at all. For example, 1.65 participants in the Every treatment text each other on average, while only 0.95 participants per group text in the Every5 treatment. In order to investigate the effect of communication and the opportunity for communication, we compare the full sample and a restricted sample. The restricted sample includes only groups of participants who text when they can. Tests using the unrestricted sample allow us to study the effects of treatment and texting separately. Moreover, we find below that the results are similar in the full sample and the restricted sample, but the treatment effects are larger in the restricted sample.

Table 3 Texting patterns in Experiments

Frequency distribution of subjects in groups allowed to text who do text	
0	30%
1	16%
2	18%
3	36%

Average Number of participants who text by treatment	
Every5	0.95
Every4	1.2
Every3	2.05
Every2	2.15
Every	1.65

NOTE: We exclude the control group for this analysis

5. Result

Table 4 shows the average individual payoff by treatment level. Each row represents a treatment effect. Never is the control group. Compared to Never, all other treatments earn higher payoffs on average, which means that communication among the three participants is associated with higher earnings. Moreover, payoffs to Never are significantly lower than payoffs to participants in any other groups (*t* test result, p-value < 0.0001). The average payoff of Every is nearly double the Never. The fact that the participants in the Every are able to text before each of the second 20 rounds may be responsible for the difference.

Table 4 Average Individual Profit by Treatment

	Mean	SD	Min	Max
Never	176.72	212.71	5	960
Every5	343.28	325.73	26	1166
Every4	281.78	286.47	19	1213
Every3	303.63	253.1	10	1274
Every2	395.07	330.72	10	1428
Every	395.4	290.45	27	1092

Figure 1-1 presents the average winning price for all treatments and the control group.[1]The horizontal axis represents the round, and the vertical axis is the average market price. There is a declining trend for every group in the first ten rounds. What happens in rounds 11-30 depends on the frequency of texting. In the control group (Never), the price decline continues, converging toward the Nash equilibrium. Every5 (texting before rounds 11, 16, 21, and 26), average prices rise at rounds 11, 16, 21, and 26, but then decline until the next text. In Every4 (texting before rounds 11, 15, 19, 23, and 27), prices increase at those rounds, then fall after. In Every3 (texting before rounds 11, 14, 17, 20, 23, 27, and 30), prices rise for each of those rounds and then decrease. In Every2 (texting at rounds 11, 13, 15, 17, 19, 21, 23, 25, 27, and 29), average prices rise in each of those rounds and decline between. In Every (texting before each round from 11-30) subjects set price similar to Every2. The difference between Every & Every2 and the other treatments demonstrates the power of frequent texting. **Figure 1-2** shows the graphs of the restricted samples which are consistent with our unrestricted findings: higher average market prices in the 11-30th treated round**s** than in rounds 1-10.

[1] We also have the median winning price graphs for each group, and they show almost the same result.

Figure 1-1 Average Market Price by Treatment

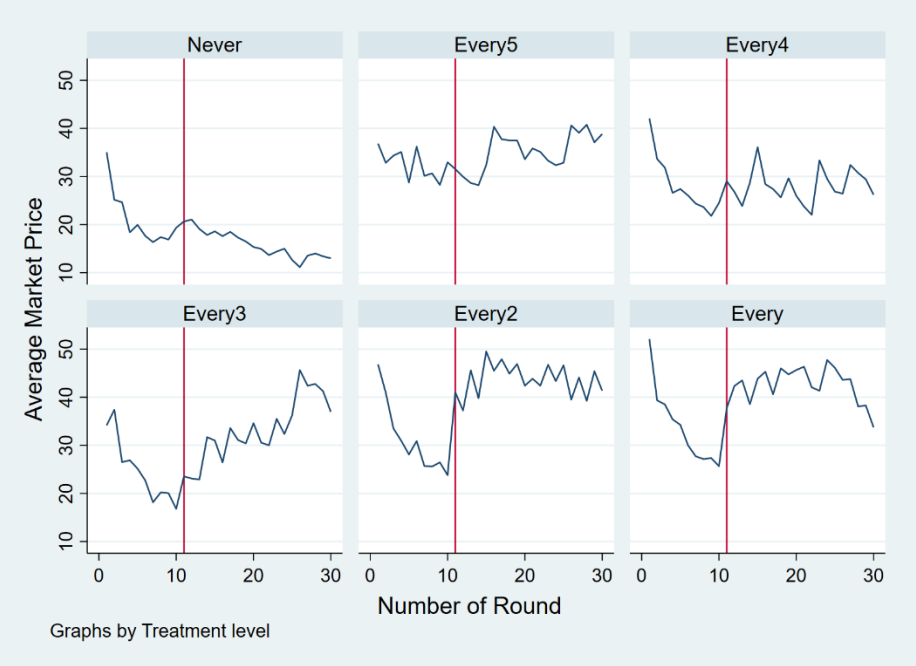

Figure 1-2 Average Market Price by Treatment (Restricted)

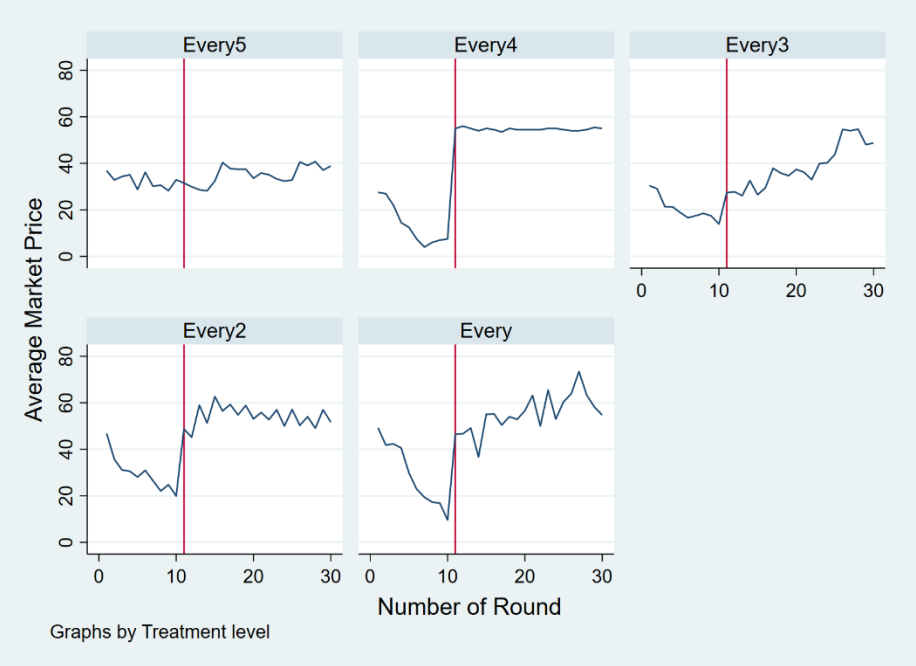

Table 5 reports the OLS estimates of the effect of communication on pricing behavior. The dependent variable is the market price of each group in each round. *Treatment* is a categorical variable equal to 0 if the group is in Never, 1 if the group is in Every5, 2 if the group is in Every4, 3 if the group is in Every3, 4 if the group is in Every2, and 5 if the group is in Every. *Post* is a dummy variable equal to 1 when the round is greater than 10. The regression was run separately on each sample. Each column represents the OLS result for each group. The first column shows the result for the full sample, and the second column is for the restricted sample. Standard errors are corrected for clustering at the group level.

Table 5 Regression of Market Price on Treatment

	Full Sample	Restricted Sample
VARIABLES	Market price	
Treatment	1.598*	-1.003
	(0.865)	(1.574)
Post	-3.215	-1.199
	(2.855)	(7.292)
Post#Treatment	3.067**	6.233**
	(1.250)	(2.793)
Constant	24.63***	30.99***
	(2.302)	(4.655)
Observations	3,600	1,530
R-squared	0.063	0.090

NOTE: *Post* is a dummy variable equal to 1 when round >10. *Treatment* is a categorical variable equal to 0 if a group is in *Never* and 1-5 if a group is treated. Standard errors in parentheses and are clustered at the group level.
*** p<0.01, ** p<0.05, * p<0.1

In order to compare the effect of treatment, we include *Treatment*, *Post* and an interaction term. The interaction term takes on a value of 0 for the first 10 rounds, regardless of treatment, and a value equal to the value assigned by treatment for 11-30th rounds. The coefficients for the

interaction term *Post#Treatment* are always statistically significant and positive. Positive coefficients mean that the higher market prices are the result of treatment, which is communication, and the market prices would decrease without the treatments.

In the full sample, regression results show that the market prices in periods 11-30 increase an average of 3.067 for each additional texting opportunity. Restricting the samples to only groups that do text when they can results in increases in average market price of 6.233. The results summarized in **Table 5** support the conclusion that the ability to text allows groups to sustain higher market prices. Actually taking advantage of the opportunity to text allows a group to sustain even higher average market prices.

To investigate whether each group has the same distribution of winning prices, several nonparametric tests are used. **Table 6** shows the p-values of each test. Each row of **Table 6** reports the p-value of each test comparing the means or the distributions of market prices. The first 5 rows present the results of the Wilcoxon–Mann–Whitney test, comparing each treatment to the control. The distribution of market prices in each treatment is statistically significantly higher than the distribution of control market prices (Never). This result gets stronger when we restrict the sample. We find the same results when we compare the distribution of winning prices using medians. In particular, p-values are also smaller for the restricted sample. The last part of **Table 6** includes the Jonckheere-Terpstra test, which is a test for an ascending ordered alternative hypothesis. It confirms that the distributions of market prices in each treatment, ordered from Every5 to Never is statistically distinct and higher than the next lower treatment ($p<0.0001$ for rounds 11-30).

Table 6 Nonparametric Test

Wilcoxon–Mann–Whitney test

Full Sample		Restricted	
Round 11-30		Round 11-30	
	Never		Never
Every5	<0.0001	Every5	<0.0001
Every4	<0.0001	Every4	<0.0001
Every3	<0.0001	Every3	<0.0001
Every2	<0.0001	Every2	<0.0001
Every	<0.0001	Every	<0.0001

Nonparametric Equality-of-Medians Test

Full Sample		Restricted	
Round 11-30		Round 11-30	
	Never		Never
Every5	<0.0001	Every5	<0.0001
Every4	<0.0001	Every4	<0.0001
Every3	<0.0001	Every3	<0.0001
Every2	<0.0001	Every2	<0.0001
Every	<0.0001	Every	<0.0001

Jonckheere-Terpstra Test for Ascending Ordered Alternatives

Full Sample		Restricted	
Round 11-30		Round 11-30	
By Treatment	<0.0001	By Treatment	<0.0001

6. Discussion

This paper presents the results of an experimental test of a repeated Bertrand oligopoly with free-form texting (cheap talk). This study compares different pricing behaviors between 1-10 and 11-30th rounds, where participants can text in rounds 11-30. We find that average market prices are higher when communication is allowed. This result confirms a commonsense notion that the existence of communication in a market leads to a collusive outcome. Communication works as a mediator and proctor. Then, we restrict the sample to the groups in which all participants actively participate in the communication when allowed. We find that the opportunity to have a conversation is important, but only some participants take advantage of that opportunity when it is offered. When they do, participants set higher average market prices.

It is important that the communication is not binding or restricted in any way. The only punishments allowed in the experiment are denunciation (a threat) or lowering of the bid price, which may make the participant who does lower bid price money in the short run, while potentially destroying cooperation in the longer run. Our result shows that collusive behavior can be achieved without a legal agreement. All it takes is the opportunity to talk about prices and the willingness to engage in conversation.

We understand that the experiment is an abstraction from reality, but the results support the argument that firms are able to fix prices through non-binding conversations. Our experiment may help initiate an antitrust investigation. The fact that competitors hold a meeting is a sufficient information to presume they are trying to collude. It is not necessary to analyze the conversations at their meeting.

Our future research question is to investigate the effect of face-to-face communications, such as Facetime, Skype, or Zoom, instead of texting. Face-to-face communication is more dynamic and fluid with verbal and facial expressions. Such communication also allows individuals to more accurately address important issues and concerns (Balliet, 2010). Moreover, there is experimental evidence that face-to-face discussion further increases cooperation (Hoffman and Spitzer, 1982, 1985, 1986; Bochet et al., 2006; Baranski and Kagel, 2015).

A limitation of this experiment is that we could not control the participants' previous experiences playing economic games. Although we limit them to play only once in our experiment, it would also be helpful to know whether some participants have more experience playing economic games than others. Such experience might affect the results.

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<Appendix A>

Instructions for participants

You have been randomly and anonymously combined with other participants. Each of you will represent a firm. Each firm manufactures one unit of the same product at no cost.

Each of you privately sets your price, anything from 0 to 100 points. The buyer in the market will always buy one unit of the product at the lowest price. In case of a tie, you will get evenly divided profit.

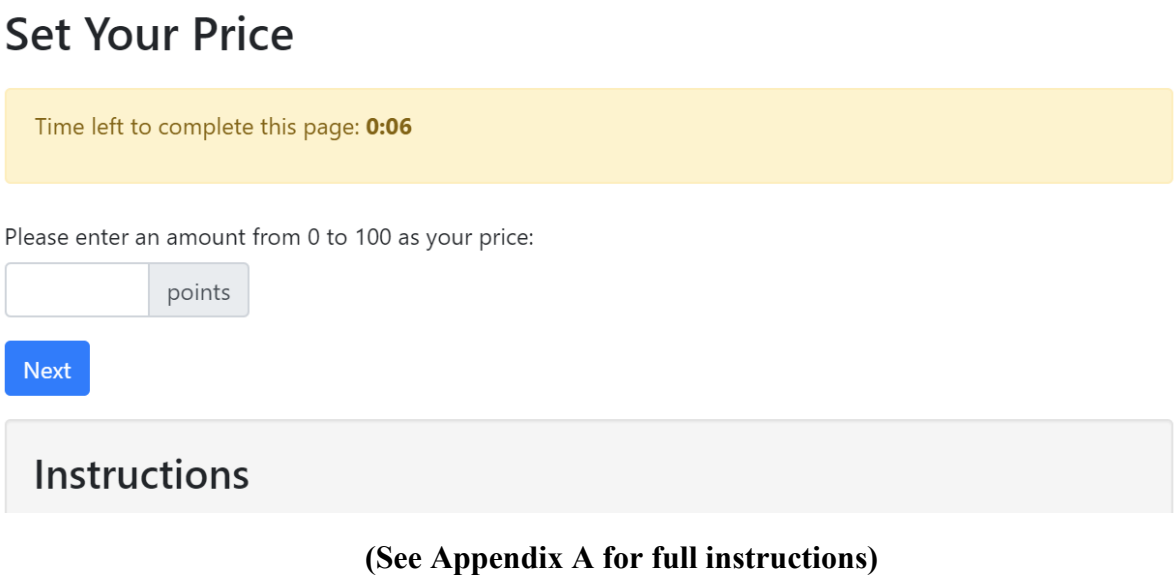
Your profit is your price if your product is sold and zero otherwise. You will play this game for 30 rounds. In the second 20 rounds, you can chat with other two participants before choosing prices. Please make sure not to share your personal information while chatting and keep discussion respectful. If one of the participants left a game early, the price of the participant would be automatically 100 points so that others can keep playing the game.

For your time and efforts, participants will earn between $2 and $9.5 in the study, including $1.99 competition fee and $0.01 participation fee. If you leave the game early, you will get a participation fee and the profit you made. The 3000 points is equal to $7.5 at the end. Your final compensation will vary depending on your aggregated profits.

(Underlined statements are only applied to treated groups)

<Appendix B>

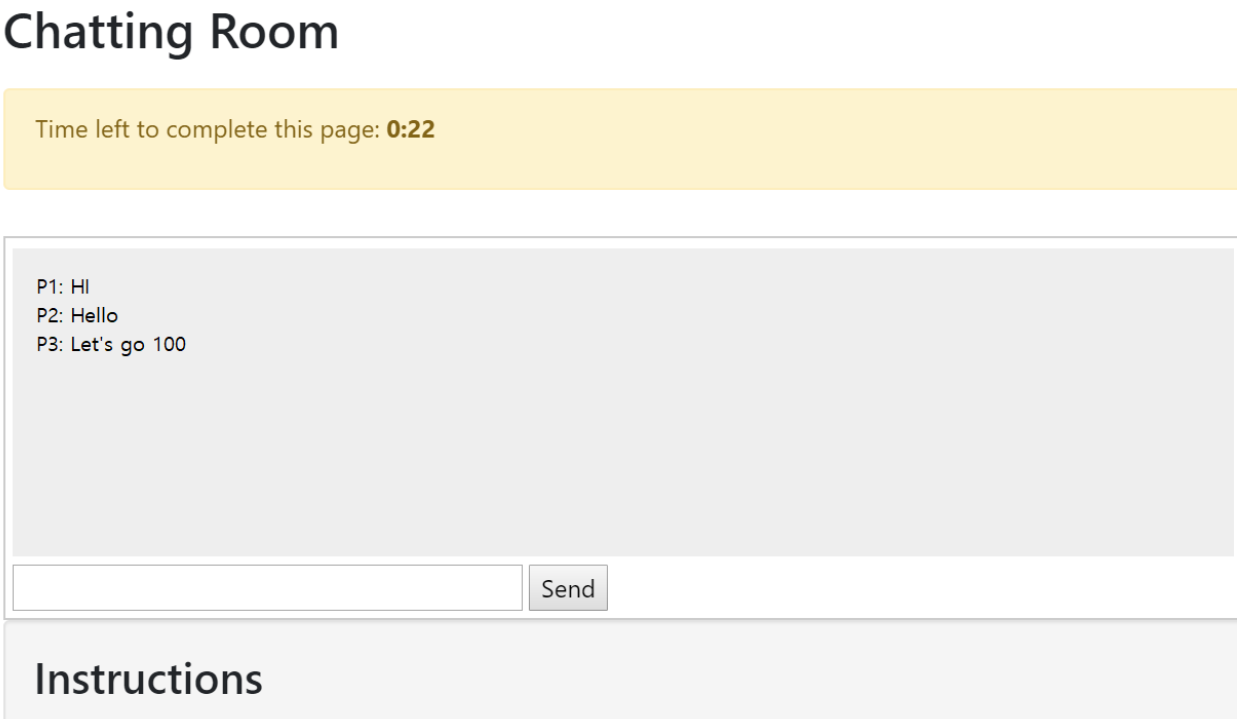
Screenshots of experiments: Choosing prices



(See Appendix A for full instructions)

<Appendix B>

Screenshots of experiments: Texting space



(See Appendix A for full instructions)

<Appendix B>

Screenshots of experiments: Summary of a round

Round 2 of 30

Time left to complete this page: **0:03**

Your price	100 points
Lowest price	100 points
Was your product sold?	Yes
Your payoff	33 points
Your Cumulative payoff	66 points