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Three Essays on Experimental Economics

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Chapter 1

Introduction

This dissertation consists of three self-contained chapters on experimental economics. All three chapters are motivated by the observation of individual decisions in daily life environments and how their decisions are affected by different information levels. This dissertation's chapters involve applications in three distinct areas: online auctions, charitable giving, and information censorship.

These contexts are particularly interesting for behavioral economists and also other non-economists like platform managers, policymakers. Currently, online shopping becomes much more popular than before. Especially when we are all restricted to stay at home during the COVID-19 pandemic 2020, the way for us to get most of the things that we need is to buy them online. It changes our traditional way of shopping and makes our lives easier, but it also makes rule-breakers hide them in this virtual world. The second chapter of this dissertation focuses on the online auctions market to check how buyers react to seller's shill bidding behavior, in which sellers submit bids to their item to increase its price.

Charitable giving in social preference is an area that has been attracted a lot by behavioral economists because it goes against purely self-interest preference. The motives of giving have been discussed by previous studies in several directions, including fairness concern, equality concern, impure altruism, and social pressure. There has been a surge of studies trying to understand the underlying mechanism through monetary giving experiments. However, few of them have focused on giving preference of non-monetary resources. The third chapter in this dissertation studies whether or not the underlying mechanism of giving time is different from that of giving money and how people make their giving decision of one resource when they have previously given another resource to the same person or the same organization.

Information disclosure is a strategically voluntary behavior to disclose private information to others, which has been studied by many researchers in different perspectives like the contexts of disclosure and the timing of disclosure. (see, e.g., Milgrom (2008), Acharya, DeMarzo, and Kremer (2011) Jin, Luca, and Martin (2015)). There is one unusual way of disclosing information: information censorship. It is to endogenously withhold information which is supposed or regulated to be public to everyone. Information censorship has been observed in many fields. For example, sellers can censor the quality comments of her sold products, and also the government censored negative information about them. In most of these cases, the main concern of a person with a censorship power is the collective decisions made by the information receivers, like a protest against together with the company or the government (See, e.g., King, Pan, and Roberts (2013) Ananyev et al. (2019)). The fourth chapter of this dissertation applied the coordination game to study the effect of information censorship on collective decision-making when one of the players can censor the public information to mislead other players.

The second chapter *Shill Bidding and Information in eBay Auctions: A Laboratory Study* (with Jim Ingebretsen Carlson), is motivated by the fact that sellers have frequently been observed to bid on their item to artificially increase its prices in online auction platforms, like eBay. We find that on eBay, auctions selling similar items usually happen to have a different starting and ending time, similar to the sequential auctions environment. Meanwhile, the bidding history of buyers in new auctions is public to both buyers and sellers. We believe that, under this sequential auctions environment, buyers may take future auctions into their consideration when they are bidding currently (see. e.g., Grether, Porter, and Shum (2015)). To argue this, we are the first to conduct a lab experiment to study the effect of shill bidding by representing the eBay auction in a sequential auctions environment. We also study how shill bidding interacts with different information revelation policies regarding the provision of the bidding history from past auctions to sellers. We find that shill bidding increases the price of auctions and benefits sellers, while different information revelation policies only have little impact on that when shill bidding is allowed. We present the empirical evidence that shill bidders behave much more active at the beginning of auctions by submitting more and earlier bids than buyers. Buyers submit more last minutes bids while sellers do not. This result is consistent with the belief of premium bids that the bids submitted at the very beginning of the auction are believed to be shill bids in some field data studies (see. e.g., Kauffman and Wood (2005)). Our findings can be used by auction platforms to detect shill bidders more precisely in the field.

In the third chapter, *Giving Decision of Time and Money: A Laboratory Experiment*, I investigate whether giving time and giving money are motivated in the same way. Researchers from psychology (see Gino and Mogilner (2014)) find that priming subjects to think about time makes them more moral than priming them to think about money. A recent study by Ottoni-Wilhelm, Vesterlund, and Xie (2017) presents the empirical evidence that the motives for giving are consistent with impure altruism theory Andreoni (1990)) through measuring crowd-out at different output levels. I explore this to demonstrate how those findings of motivating giving money may not be well suited for giving time. Using a within-subject experimental design, I find that, on average, giving time is less and differently crowded out than giving money through measuring crowd-out at different output levels and across different resources (time and money) of others' giving. In particular, my results show that the amount of giving time is sensitive to others' money giving and time giving to the same person, while the amount of giving money does not response that much to other's time giving. Then I also find that asking giving money firstly and then asking complementary giving time can attract more total giving from donors. My findings suggest that pubic policies and fundraising mechanisms that mixes time and money may cause inefficiency.

The last chapter, *Coordination Under Information Censorship*, co-authored with Manwei Liu, mainly focus on the power of censoring information, which has been observed in many countries and also business markets (see. e.g., Ananyev et al. (2019) and Hauser (2020)). To address this issue, we modified a coordination resistance game (Weingast (1997)) and combined it with the signaling game by Cho and Kreps (1987). Our game consists of two different states, including different payoff tables representing a good state and a bad state in terms of total social welfare in real life. The game is between one leader and two subordinates when the leader can censor the information of the true state of nature from their subordinates, and subordinates can coordinate on resisting or cooperating with the leader. This experimental project also argues that the effect of censorship interacts with the possibility of communication among subordinates. So in our experimental design, we applied a strategy method in a two (censorship or not) $\times$ two (communicate or not) treatments to identify the effect of censorship and also the interacted effect between censorship and communication, so that we can understand the underlying mechanism of why and when censorship works. We find that censorship works to affect subordinates' coordination decisions and benefits the leader in a bad state. However, when censorship is not allowed, and communication is allowed, we can get higher total welfare. Interestingly, when the leader has the power to censor the information, but they did not, subordinates increased their willingness to cooperate with the leader. Furthermore, when subordinates can communicate with each other, they showed their social preference concern to the leader and increased their willingness to coordinate on cooperate with the leader.

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Chapter 2

Shill Bidding and Information in eBay Auctions: A Laboratory Study

Abstract[1]

In online auction platforms like eBay, sellers have frequently been observed to bid on their item to artificially increase its price, and this is known as shill bidding. We represent the eBay auction in a sequential auctions environment in lab experiments and study the behavioral consequences of sellers being able to participate as shill bidders and of being informed about buyers' past bidding histories. We find that the possibility of shill bidding in ongoing and future auctions benefits sellers, affects mostly high private-value buyers' bidding strategies, and increases the efficiency of auctions. However, providing sellers with buyers' bidding histories has little impact on their behaviors.

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2.1 Introduction

Online auction platform has been wildly used in our daily life. And eBay is one of the world's largest e-markets and its auction platform is a core business. One of the reasons why eBay has a large share of the online auction market is because of its millions of active users and its' relatively good reputation among her competitors like Amazon, eBid, uBid and, etc [2]. However, in online auctions, it has frequently been observed that sellers bid on their item to artificially increase its price (see, e.g., Engelberg and Williams (2009); Grether, Porter, and Shum (2015); Kauffman and Wood (2005)). This practice is known as shill bidding. Since the purpose of shill bidding is to harms the buyers' surplus, auction houses, such as eBay, have said that they spend money and time to detect and prevent shill bidding. In fact, eBay even has a shill bidding policy where they inform buyers about the existence of shill bidding.[3] However, online shill bidding is easy to organize (e.g. with a large set of paid colluders, a rotating scheme with peer sellers, or through the use of an alternative online identity) and hard to detect. If one auction platform could not protect their users from shill bidding, their users may easily switch to their competitors to play online auctions.

A reason why shill bidding is common in eBay auctions may also be that the price of the item is determined by the second-highest bid that was submitted in the auction. This is known as the second-price rule and it makes it easier for a seller to increase the price of an item, without winning it. Specifically, Sellers can strategically drive up the second-highest bid in the auction by submitting a shill bid that is greater than the current second-highest bid and smaller than the winning bid.

Moreover, at the eBay auction platform, it is common to find similar items that are listed in separate auctions that start and end at different points in time, which is similar to the sequential auctions environment. Since buyers are informed of the presence of possible shill bidding in eBay auctions, this sequential auctions environment introduces a threat of shill bidding in both ongoing and future auctions that buyers may participate in. It is important to address this feature to study shill bidding in the eBay auction, which has not been captured by the previous studies of shill bidding. Since the purpose of shill bidding is to extract more surplus from buyers, the threat of shill bidding in an ongoing and a future auction both ought to affect buyers' behavior in ongoing auctions. For instance, when bidding in the ongoing auctions, buyers may take the threat of shill

[2]eBay is scored as 79 in the most recent American Customer Satisfaction Index (ACSI) for internet retails (see reference at https://www.theacsi.org/).

[3]eBay's shill bidding policy can be found at https://www.ebay.com/help/policies/selling-policies/selling-practices-policy/shill-bidding-policy?id=4353

bidding in future auctions into consideration by raising their ongoing auction bids. They do this to account for the higher expected price in the future auction, which is due to the submitted shill bids, as suggested by Paul. R Milgrom and Robert J. Weber (2000); Gong, Tan, and Xing (2014). Through the threat of shill bidding from future auctions itself, a seller may, thus, also increase earnings in the earlier auctions, even without the actual participation. So, in this paper, we will identify the threat of shill bidding in ongoing auctions and the threat of shill bidding in future auctions separately to study how shill bidding affects buyers' and sellers' strategies.

Furthermore, eBay and other online auction houses make the bidding history from finished and ongoing auctions public.[4] Hence, shill bidding sellers can potentially extract more surplus from buyers through using the bidding information from these past auctions to calibrate their shill bids in ongoing auctions. Previous studies present both theoretical and empirical evidence that the bidding information indeed affects auction players' strategy in different auction environments (see, e.g, Cason, K. N. Kannan, and Siebert (2011), Paul. R Milgrom and Robert J. Weber (2000)), but they mainly focus on the effects of bidding history information on buyers' behavior. However, when sellers are allowed to shill bid in sequential auctions, it is essentially important to study the effects of this bidding information on both buyers and sellers' behavior, while they have been mentioned by neither policymakers at eBay,[5] nor other shill bidding studies. In response, buyers may change their bids to hide their private value information from sellers. This effect is similar to what is typically found in the voluntary information disclosure literature where agents partially disclose private information (See, e.g., Dye (1985); Jin, Luca, and Martin (2017); Jansen and Pollak (2014)). Thus, when sellers are allowed to shill bid in future auctions, and are informed of buyers' ongoing auction bids, the buyers who may participate in both auctions face a trade-off between the needs to increase their ongoing auction bid to win the auction and to lower their ongoing auction bid to hide their private value information. In addition to the threat of shill bidding, as such the public bidding histories may also affect prices and earnings of buyers and sellers.

In this paper, we experimentally investigate the effects of shill bidding in future and

[4]In the eBay bidding history, each buyer is assigned an anonymous ID. However, this ID is kept the same in all auctions, thus making it possible to track individual buyers and their bids.

[5]In an eBay auction, sellers can also choose to set up private listings of their item in which the bids and the name of buyers will be hidden and only the sellers themselves can see this information for the main purpose of hiding the buyer's identity information for high-value items. In this case, the bidding history information cannot be seen by other buyers and other sellers who are selling a similar item. However, if sellers set up several private listing auctions of similar items, then they get access to bidding history, which can be used to calibrate shill bids in the later auctions. Information of private listings can be found at https://www.ebay.com/help/selling/listings/listing-tips/private-listings?id=4161.

ongoing auctions, as well as of publicly disclosing bidding information to sellers in the eBay auction format. In particular, we investigate how buyers in an ongoing auction react to the threat from shill bidding in a future auction, and how this affects outcomes in ongoing and future auctions. In addition to this, we study the impact of providing the sellers with different amounts of buyers' bidding information from the early auction before they decide whether or not to shill bid in the future auctions. Laboratory experiments are well-suited for investigating shill bidding and information disclosure to sellers since they offer control and observability of shill bidders' behavior. These are two characteristics that naturally occurring data typically do not have. The auction format in our experiment was chosen to closely replicate the eBay auction environment.

In our experiment, there are 20 rounds of sequential auctions game played by randomly matched sellers and buyers. The duration for each round is fixed to one minute. In each round of our experiment, two items are sold using two distinct and sequentially carried out auctions, in which the bidders can choose to submit as many bids as they want before the fixed ending time. As on eBay, the auctions use the second price rule: the bidder who wins the item pays a price equal to the second-highest bid submitted by a different bidder. A shill bidding seller can, therefore, increase the price, without winning the item, by submitting the second-highest bid and, thereby, increase earnings. We employ the following between-subjects experimental design with three treatments: The seller cannot shill bid in the first auction in any treatment to identify the threat of shill bidding in future auctions. In the baseline treatment, the seller cannot shill bid in the second auction either. In the other two treatments, the seller can choose to participate and submit shill bids in the second auction. The two shill bidding treatments differ by the amount of information that is provided to the seller. The seller is provided with either the buyers' complete bidding histories or no information at all from the first auction. When provided, the information is given to the sellers before they choose whether or not to participate as shill bidders in the second auction.

Our main contributions to the literature are as below. Firstly, we present the empirical evidence from lab data that the possibility of shill bidding from future and ongoing auctions does hurt buyer's earning and benefits sellers. Prices are higher in the first and second auctions when the sellers can submit shill bids. In particular, the threat of future shill bidding makes buyers bid higher in the early auctions. An interesting finding related to this is that the possibility of shill bidding significantly raises the efficiency of the early auctions, which contributes to higher overall efficiency for both auctions. It suggests that auction platforms may implicitly allow shill bidding while they explicitly forbid this behavior as they said on their website. Their purpose of mentioning shill bidding on their website, which introduces this threat of possible shill bidding, is skeptical. However, we find no significant effect on buyers' behavior of making the bidding history from the first auction public to sellers, between the two shill bidding treatments. Probably because the seller's participation implicitly increases the market size, which causes a higher competition of that auction, and also the bidding information disclosed during the second auction is the information that helps the seller's shill bidding. Both of them may, in the end, increase the final price, instead of the better-calibrated shill bids using the bidding history from the past auctions. It implies that history information may not be very useful in a dynamic game like the eBay auction. Finally, comparing how buyers and sellers bid on an item in the second auction, we find a strong significant difference between their behaviors. It may, thus, be possible for a buyer and auction houses to detect whether or not a seller is submitting shill bids during an auction.

To the best of our knowledge, Kosmopoulou and De Silva (2007) have conducted the only experimental study on shill bidding. The authors investigated shill bidding when a single unit is auctioned in an ascending clock auction format with a common value setting. The study supports their theoretical prediction that shill bidding is harmful to sellers as prices are lower because of the winner's curse. This is different from our study, in which sellers may profit from shill bidding since buyers have private values for the items, by the arguments put forward earlier. The two studies also differ since we replicate the eBay auction environment with buyers participating in sequences of auctions. The issue of information disclosure in auctions is a more studied topic (See, e.g., Dufwenberg and Gneezy (2002); Karthik N. Kannan (2010); Cason, K. N. Kannan, and Siebert (2011); Katuščák, Michelucci, and Zajíček (2015)). However, most of these studies focus on when information from buyers is revealed to buyers, while we focus on studying when information is disclosed from buyers to sellers at the same time. The experimental study by Grebe, Ivanova-Stenzel, and Kröger (2018) involves information disclosed from buyers to sellers and they find that sellers do react to the revealed past bidding histories when deciding on their Buy-It-Now prices.

Lastly, the eBay auction has been studied experimentally by, Ariely, Ockenfels, and Alvin E Roth (2005) and Wang (2006). However, the first study implemented a version of the eBay auction in the lab in which time was discrete. This is different from our auction environment in which the subjects are given one minute to bid for the items. The second study investigated sequential sealed-bid second-price auctions as well as the eBay auction when the two highest bids from the early auctions were revealed to the buyers.[6] The eBay auction environment is similar to the one used in this paper

[6]This auction format is often named an out-cry auction since the buyers can submit any number

and buyers' bids are captured well by the bidding functions that were theoretically derived in the study, which motivated us to have this sequential sealed-bid second-price auctions theory to conjecture the hypotheses for our auction setting.[7]

The outline of this paper is as follows: Section 2.2 explains the experimental design and the hypotheses that we will test using the experimental data. The results from the laboratory experiment are presented and discussed in Section 2.3. Finally, Section 3.6 concludes the paper.

2.2 Experimental design and hypotheses

In this section we explain the design of the experiment and our hypotheses and conjectures. We start by outlining the auction environment, which is used in all treatments of the experiment, in Section 2.2.1. In Section 2.2.2 we explain the different treatments employed in the laboratory study. Then, in Section 2.2.3, we state and discuss our hypotheses and conjectures. Finally, the details of the experiment are presented in Section 2.2.4.

2.2.1 Experimental auction environment

We use the z-Tree software Fischbacher (2007) to program and replicate the eBay auction in the laboratory. An alternative would be to use the eBay interface for a lab-in-the-field experiment. However, using the eBay interface would not serve our purpose since we would not be able to eliminate the possibility of shill bidding and the bidding information in the experiment.

The experimental environment is the following: At the start of each round of the experiment, subjects are randomly matched into groups of five, which consist of one seller and four buyers.[8] Hence, the seller is also a subject. We refer to such a group as a Market. The seller is selling two identical items using two auctions. The two auctions are conducted sequentially, which means that the second auction (SA) is conducted

of bids during the auction. For more studies that implement a version of the out-cry auction see, e.g., Elmaghraby, Katok, and Santamaría (2012); Gonçalves and Hey (2011); Kwasnica and Katok (2007); Sherstyuk and Dulatre (2008); Strecker (2010).

[7] Ockenfels, Reiley, and Sadrieh (2006) also summarized other papers related to shill bidding. But only some of them are the main references for our project.

[8] Four (three in the second auction) buyers are chosen in order to avoid the potential problem of collusion. We follow Dufwenberg and Gneezy (2000), whose experimental evidence suggests that three buyers are enough to avoid collusion.

once the first auction (FA) is completed. Before the start of a round, each buyer receives a private value, which is the same for the two items that are to be sold in the FA and the SA. It is publicly known that the buyers' private values are randomly and independently drawn from a uniform distribution of integers between 0 to 100 experimental currency units (ECUs). A buyer's private value is displayed on their screen during the auctions.[9] Buyers have unit demand, which implies that the winner of the FA will not participate in the SA. All subjects start with a budget of 100 ECUs and they maintain their roles throughout the experiment.

Both the FA and the SA are designed to replicate features of the eBay auction, in which shill bidding has been frequently observed empirically. In our experimental auctions, bidders are allowed to submit any number of bids for the item during one minute.[10] The auctions use the second price rule: the bidder who submits the highest bid, before the end of the one minute, wins the auction and pays a price equal to the second-highest bid that is submitted by a different bidder.[11] A winning buyer receives ECUs equal to his/her private value minus the price of the item and the losing buyers get zero

In order for a bid to be accepted it must be greater than any previously accepted bid that was submitted by that subject. In addition to this, the bid needs to be greater than the current price, which equals the second-highest bid at that moment. The current price starts at 1 ECU in any auction. The current price is displayed on the bidders' screens and is continuously updated as new bids are accepted. The bidding history, consisting of all past current prices from the auction, is displayed to the bidders as well. However, the bids are anonymous.[12] Moreover, the bidding history is continuously updated as bidders submit more bids. If a subject has submitted the highest bid at any moment, then he is informed that heis currently winning the auction. Otherwise, he is informed that he is currently not winning the auction.

After completing any auction, the buyers are informed whether they have won the item or not, of their payoffs and their updated balance in ECU. If a buyer incurred losses after the completion of an auction, a message to warn the buyer of this appears. Between the FA and the SA, buyers will be informed whether or not they will participate in the SA. The sellers receive a payoff equal to the price of the item in one of the FA or SA. A

[9]See Appendix 2.5.4 for screenshots from the different interfaces the subjects were shown during the experiment.

[10]A fixed deadline is chosen to closer replicate the eBay auction.

[11]In the case of several bidders submitting the same highest bid, the bidder who submitted it first wins the item.

[12]This differs from the eBay auction and is done in order to ensure a seller is not to easily detected when submitting a shill bid in the second auction. See 2.5.4 for the screenshot of the bidding screen in our experiment.

lottery with equal probability assigned to each auction is used to determine this payoff. This is done in order to minimize the wealth effect of the FA price on the sellers' shill bidding behavior. If the seller gets the price from the SA as a payoff and won the SA by shill bidding, then as the seller pays the price to himself, the payoff is 0. The sellers will be informed about their payoffs and balances at the end of each round.

2.2.2 Treatments

The experiment has three distinct treatments to which subjects are randomly assigned. All treatment differences are with respect to the sellers and the specifics of a treatment are only told to the buyers of that treatment in the instructions.[13] The treatments differ in whether or not the seller can shill bid in the SA and which information the seller is given from the FA. In all three treatments, the seller is not allowed to participate in or watch the FA. As we are partly investigating how shill bidding in future auctions, and information, affect behavior in ongoing auctions, the seller is only allowed to shill bid in the SA in the shill bidding treatments. Table 2.1 gives an overview of the treatments and their differences.

Table 2.1: Overview of the treatments employed in the experiment

Treatment	Shill bid FA	Information	Shill bid SA
Baseline	No	Yes	No
Treatment 1	No	Yes	Yes
Treatment 2	No	No	Yes

Notes: Deciding to shill bid is the sellers' choice and Information refers to the complete bidding history of all current prices from the FA and whether this is displayed to the seller before deciding to shill bid in the SA.

Baseline treatment: No shill bidding in the SA and information from the FA to the seller.

The sellers cannot actively participate in any of the auctions. During the FA the sellers will be shown a blank screen. Between the FA and the SA, the complete anonymous bidding history of all current prices (any bid that was ever the second-highest bid)

[13]The instructions can be see in Appendix 2.5.3. A questionnaire also checked that the subjects had understood the specifics of the treatment they were in. See Appendix 2.5.4 for more details.

from the FA is provided to the seller. This conveys the most important information to sellers including the distribution of buyers' willingness to pay and the competitiveness between the buyers by seeing how many bids buyers submitted during the auction. The seller watches the bidding live during the SA.

Treatment 1: Shill bidding in the SA and information from the FA to the seller.

During the FA, the sellers will be shown a blank screen. The complete anonymous bidding history of all current prices from the FA is displayed to the seller between the FA and the SA. Before the start of the SA, the sellers can choose to participate in the SA, in which they can choose to submit shill bids. Participating in the SA costs 1 ECU[14] and the rules for bidding, as well as the information displayed, are the same as for the buyers. The buyers are informed that it is the seller's choice whether or not to participate and submit shill bids in the SA. This information is only given to the buyers in the instructions. If a seller chooses not to participate in the SA, then the seller watches the bidding live during the SA.

Treatment 2: Shill bidding in the SA and no information from the FA to the seller.

This treatment is identical to Treatment 1 except that the complete anonymous bidding history of all current prices from the FA is not displayed to the seller between the FA and the SA. Between the FA and the SA, the seller is only informed whether or not the item in the FA was sold.

Treatment 1 is the environment that resembles the situation in online auctions today. We did not conduct a second baseline treatment in which the sellers had no information and in which they could not shill bid. The reason for this was because we believe that the behavior in the Baseline treatment should not differ much from such a second baseline treatment since the only difference is that the non-participating seller is observing the buyers' FA bids. As three other buyers already observe a buyer's bids in the FA, and since these bids are anonymous, we believe that the additional observation effect of the non-participating seller is small. Therefore, when comparing Treatment 2

[14]The cost is introduced in order to more clearly identify sellers who have an intent to shill bid and to reduce possible experimenter demand effects. Based on the real world situation, we believe that the cost of participation is small since sellers only need to make calls to ask for relatives help or just register another account to do this by themselves.

to the baseline we can distinguish a "shill bidding effect" and by comparing Treatment 1 to Treatment 2, it is possible to single out an "information effect" when sellers can shill bid.

2.2.3 Theoretical conjectures and hypotheses

We present our research hypotheses in this section. To have better testable hypotheses, we derive theoretical conjectures, using a sealed-bid assumption, that motivate our hypotheses. For the sake of brevity, we put the complete theoretical model and all proofs in Appendix 2.5.1. Furthermore, similar results as the ones presented here can be found in other papers (see, e.g., Paul. R Milgrom and Robert J. Weber (2000); Katsenos (2010); Gong, Tan, and Xing (2014)). Consequently, proofs in Appendix 2.5.1 are provided for completeness.

Since the exact model of eBay auction is difficult to analyze and also we are more focused on testing the experimental treatments rather than the theory, we start with the simplifying assumption, that the two auctions are of the sealed-bid auction format, to give us some insights about our dynamic eBay auction game. The main difference between the eBay auction and a sealed-bid auction is that the eBay auction allows for dynamic bidding as the bids can be revised upwards during the course of the auction. While a sealed-bid auction is static game, in which bidders only submit one bid and at the same time. At each point of time in the eBay auction, the bidders can submit any bid they would like above the current price, and they do not know how many active bidders are left, only the bids that were ever the current price at some point during the auction. In this sense, the eBay auction is similar to a sealed-bid auction bounded from below by the current price at a given point in time. Furthermore, the eBay auction uses a fixed ending time. Ariely, Ockenfels, and Alvin E Roth (2005) and Alvin E. Roth and Ockenfels (2002) show that bidders frequently "snipe" in eBay auctions, since they submit their bids in the last minute of the auctions.[15] This implies that if all bidders snipe, and submit their final bid at the end of the auction, then the eBay auction essentially becomes a sealed-bid auction. Since we are mostly interested in analyzing buyers' final bids, as they determine prices and outcomes, we believe that results derived from a theoretical model using the sealed-bid assumption will be useful as conjectures for hypotheses regarding behavior and outcomes in the eBay auction.[16]

[15] In their empirical data, they observe that approximately 50% of the bids are sniping bids as they are submitted during the last five minutes of the auctions. In our data, we find that 65.2% of the buyers' final bids are submitted in the last five seconds of the auctions.

[16] An alternative would be to model the eBay auction as an English ascending auction. The main reason for this is that the English auction also allows for dynamic bidding. However, in the English

Consequently, we start with a static game that each auction is conducted as a second price sealed-bid auction. A seller is selling two items by means of two sequential sealed-bid auctions with $n \geq 3$ participating buyers. The buyers have unit demand and the winner of the FA will therefore not participate in the SA. Each buyer i has a private value, v_i, which is the same for both items. The buyers' private values are randomly and independently drawn from a uniform distribution on (0,1). Let $\beta_1(v_i)$ be a bidding function determining how much a buyer with private value v_i bids in the FA. Similarly, $\beta_2(v_i)$ is a bidding function for the SA. We assume that $\beta_1(v_i)$ and $\beta_2(v_i)$ are symmetric and strictly increasing. This implies that the buyer with the highest private value wins the FA and the buyer with the second-highest private value wins the SA. The seller's private values for the items are assumed to be 0. Starting from the SA, the buyers have a dominant strategy to submit a bid equal to their private value. This is unaffected by whether or not the seller is shill bidding in the SA, since for any shill bid submitted by the seller, the buyers can do no better than bidding their private value, as long as this is not lower than the current price (see Graham, Marshall, and Richard (1990); Izmalkov (2004); Riley and Samuelson (1981)). Consequently, $\beta_2(v_i) = v_i$ in our three treatments. Turning to the FA, a buyer is trading off the possibility of winning an item now or waiting to possibly acquire an item in the SA. If the seller cannot shill bid, then the buyers' optimal strategy is:

$$\beta_1(v_i) = \frac{n-2}{n-1} * v_i \tag{2.1}$$

This implies that the buyers who participate in the Baseline treatment should bid below their private value in the FA to account for less competition in the SA. Allowing the seller to submit a shill bid, s, in the SA, but not displaying the complete bidding history from the FA to the seller, gives rise to a "shill bidding effect". The buyers should still submit a FA bid equal to the expected payment in the SA, but now they take into account that the seller's possible shill bid raises the expected price. Moreover, for any bidder with $v_i \leq s$, the FA is essentially the last auction, which means that they will submit a bid equal to their private value in the FA. The buyers optimal strategy

ascending auction there is a clock that increases the price by a fixed amount at some predetermined time interval and at each price the bidders can only choose to either stay in the auction or drop out. In the eBay auction on the other hand, the submitted bids can be any number greater than the current price and this is determined by another bidder's bid and not by a clock. Differently to the eBay auction, the English ascending auction is typically modeled with the bidders knowing the number of active bidders at any price (see , e.g., Paul R Milgrom and Robert J Weber (1982); Paul. R Milgrom and Robert J. Weber (2000)). When there is only one active bidder left, the auction stops, and this bidder buys the item at the current price. However, if the English ascending auction is modeled without the buyers knowing the number of active bidders during any step in the auction, then the derived bidding functions coincide with the ones presented in this section.

in Treatment 2 is:

$$\beta_1(v_i) = \begin{cases} \frac{n-2}{n-1} * v_i + \frac{s^{n-1}}{v_i^{n-2}(n-1)} & \text{if } v_i > s \\ v_i & \text{if } v_i \leq s \end{cases} \tag{2.2}$$

When the seller chooses the shill bid s in the SA, it has an obvious effect on the seller's expected payoff in the SA. However, a higher shill bid also raises the expected FA price and payoff since it raises the buyers' FA bids. Taking both these effects into account, we find that the seller's optimal shill bid in Treatment 2, s^*_{T2}, is:

$$s^*_{T2} = \frac{1}{2} \tag{2.3}$$

Now we turn to the situation in Treatment 1 in which, before submitting a shill bid in the SA, the seller can observe all bids that were ever the current price in the FA. It turns out that there cannot exist any symmetric and strictly increasing $\beta_1(v_i)$ in this case. The reason is that if the buyers follow such a bidding function, then the seller knows this and can infer the private values of the buyers from the FA bidding history by inverting the bidding function. Therefore, the buyers expect a payoff of 0 in the SA in this case. Consequently, the buyers have an incentive to under-report their private value in order to increase their possible earnings in the SA. Proposition 1 states the result.

Proposition 1 *If the seller shill bids and is given the complete bidding history from the FA, then there does not exist a strictly increasing symmetric bidding function $\beta_1(v_i)$ for any buyer i.*

The derived bidding functions in Equation 2.1 and Equation 2.2 give rise to conjectures regarding prices and earnings. Table 2.2 displays the conjectures adapted to our experimental setting.

We base our first four hypotheses on the theoretically derived conjectures in Table 2.2:

Hypothesis 2.1 ***The shill-bidding effect on final price:*** *When the sellers are allowed to shill bid in the SA auction, but are not given the FA bidding history, then the final prices are higher in both the FA and the SA.*

Table 2.2: Theoretically derived conjectures using sealed-bid assumption

	Baseline	Treatment 1	Treatment 2
FA buyer bids	$\frac{2}{3} * v_i$	-	$\frac{2}{3} * v_i + \frac{S^3}{v_i^2 * 3}$ if $v_i > s$ v_i if $v_i \leq s$
SA buyer bids	v_i	v_i	v_i
SA seller bid	-	-	50
FA price	40	-	50
SA price	40	-	51.25
Seller FA payoff	40	-	50
Seller SA payoff	40	-	38.75
Buyer FA payoff	10		7.5
Buyer SA payoff	5	-	3.44

Notes: Entries are multiplied by 100 to match the outcomes in the experiment.

Buyer earnings refer to winning buyer's earnings.

Hypothesis 2.2 ***The shill-bidding effect on sellers' payoff****: When the sellers are allowed to shill bid in the SA, but are not given the FA bidding history, then the FA payoff is higher and the SA payoff is lower for sellers compared to when the sellers are not allowed to shill bid in the SA.*

Hypothesis 2.3 ***The shill-bidding effect on buyers' payoff****: When the sellers are allowed to shill bid in the SA, but are not given the FA bidding history, then the buyers' FA and SA payoffs are lower than when the sellers are not allowed to shill bid in the SA.*

As $n = 4$ in all our experimental treatments, we expect all buyers to bid $\frac{2}{3} * v_i$ in the Baseline. In Treatment 2, we expect buyers to bid $\frac{2}{3} * v_i + \frac{0.5^3}{v_i^2 * 3}$ if $v_i > 0.5$ and to submit a bid equal to v_i if $v_i \leq 0.5$. Consequently, we expect that the FA bids are higher in Treatment 2 than in the Baseline.

Hypothesis 2.4 ***The shill-bidding effect on buyers' FA bids:*** *When the sellers are allowed to shill bid in the SA, but are not given the FA bidding history, then the buyers' FA final bids are higher than when the sellers are not allowed to shill bid in the SA.*

Since we have not been able to characterize the mixed strategy equilibria in Treatment 1, we do not have any theoretically derived conjecture of the buyers' behavior in this treatment. However, based on the results from the literature on voluntary information disclosure (See, e.g., Teoh (1997); Denker et al. (2014); Ertac, Gumren, and Kockesen (2017); Guttman, Kremer, and Skrzypacz (2014); Jansen and Pollak (2014); Jin, Luca, and Martin (2017)) and the buyers' incentive to under-report their private value, we believe that revealing the FA bidding history to the sellers makes buyers decrease their FA bids. Consequently, we conjecture that buyers will bid lower in Treatment 1 than in Treatment 2.

Conjecture 2.1 ***The information effect on buyers' FA bids:*** *When the sellers are allowed to shill bid in the SA and are given the FA bidding history, then the buyers' FA final bids are lower than when the shill bidding seller has no such information.*

In addition to the buyers, the sellers themselves may be affected by being shown the bidding history from the FA. Our experimental setting allows us to analyze the effects of information on sellers' behavior since the sellers are subjects and not programmed by a computer. The study by Grebe, Ivanova-Stenzel, and Kröger (2018) shows that sellers respond strategically to bidding information when choosing Buy-It-Now prices. Therefore, we conjecture that there is a higher correlation between the price in the FA and the sellers' SA bids in Treatment 1 than in Treatment 2:

Conjecture 2.2 ***The information effect on sellers' bids****: There is a higher correlation between the price in the FA and the sellers' final SA bid when the sellers are given the FA bidding history than when shill bidding sellers are not provided with this information.*

2.2.4 Details of the experiment

We ran nine sessions during April 2017 at the Autonomous University of Barcelona. Each session consisted of 20 participants and we had 180 participants in total. Participants were students at the Autonomous University of Barcelona and were recruited using the Online Recruitment System for Economic Experiments (ORSEE). All sessions were computerized and programmed using the z-Tree software Fischbacher (2007). Instructions were read aloud, questions were answered in private, and no communication was allowed between subjects. Before starting the experiment, subjects had to pass a comprehension test and complete two test rounds (See Appendix 2.5.3 for the instructions and Appendix 2.5.4 for the comprehension test). The experiment consisted of 20 rounds, for which the subjects were paid. To increase the number of independent observations, we created, within each session, two groups of ten subjects with two sellers and eight buyers in each. Throughout the paper, we will refer to any such independent observation as a Group. After the end of an experimental round, subjects were randomly re-matched within their Group while maintaining their roles. Subjects were told that they were re-matched, but not about the specifics of the rematching procedure. At the end of the experiment, 100 ECU was converted to 1 euro for buyers and 0.4 euros for sellers.[17] Average earnings were 15.68 euros and subjects spent approximately two hours in the lab. Table 2.3 summarizes the structure of our experimental design.

Table 2.3: Overview of experimental design

Treatment	**Independent Groups**	**Markets/ Group**	**Subjects/ Market**	**Number of Subjects**	**Number of Periods**
Baseline (No shillbid & Info)	6	2	5	60	20
Treatment 1 (Shillbid & Info)	6	2	5	60	20
Treatment 2 (Shillbid & No info)	6	2	5	60	20
Total Number of Subjects	180				

2.3 Experimental Results

In this section, we present the experimental results of our study. We start by analyzing the effects of shill bidding on prices, payoffs and efficiency in Section 2.3.1. Then, we continue by exploring buyer behavior in Section 2.3.2 and seller behavior in Section 2.3.3. Finally, we compare buyers and sellers in Section 2.3.4. We will sometimes refer to the Baseline as B, Treatment 1 as T1 and Treatment 2 as T2. When we test for differences in means across the treatments, we will use the Mann-Whitney U rank sum test on the independent Group averages. The p-values for this test will be denoted by p_{MWU}. We will also test for mean differences within treatments, then we will use the Wilcoxon matched-pairs signed-ranks test on the independent Group averages. The p-values for this test will be denoted by p_{SR}. Finally, complementary regression analysis can be found in Appendix 2.5.2.[18]

[17] This was done for two purposes: Firstly, in order to try to keep the average earnings of both roles relatively close to each other. Secondly, to decrease the wealth effect of sellers as this may affect the incentive to shill bid in the SA.

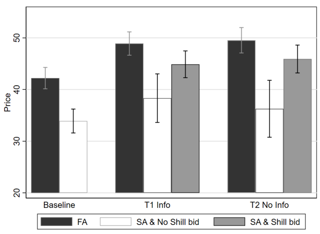
[18] A complete description of all variables that have been used in the regression analysis can be found in Table 2.7 and Table 2.8 of Appendix 2.5.1.3.

2.3.1 Prices, payoffs and efficiency

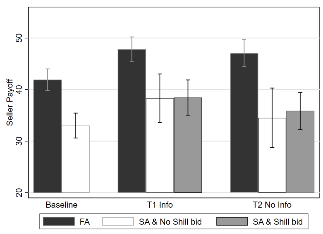
We present results on prices, payoffs and efficiency in this section. Figure 2.1 shows, by treatment, the average final prices, seller payoffs and buyer payoffs in the FA and the SA, divided by whether or not the seller shill bid, as well as average efficiency in the FA, SA and both auctions together. Table 2.9 in Appendix 2.5.2 shows the summary statistics and expected outcomes for these variables.

Final Prices: By looking at Figure 1a we can conclude that shill bidding affects the average final prices in both the FA and the SA. The FA average final prices are significantly higher in the shill bidding treatments than in the Baseline (B vs T1, $p_{MWU} = 0.0374$ and B vs T2, $p_{MWU} = 0.0782$). The same is true for the SA average final prices (B vs T1, $p_{MWU} = 0.0303$ and B vs T2, $p_{MWU} = 0.0547$). The price increase in the shill bidding treatments is around 17% in the FA and 27% in the SA. Consequently, we find support for Hypothesis 2.1 that prices are higher in both the FA and SA when the sellers are allowed to shill bid.

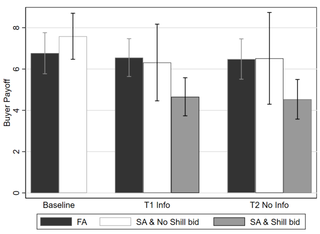
Figure 2.1: Overview of treatment effects



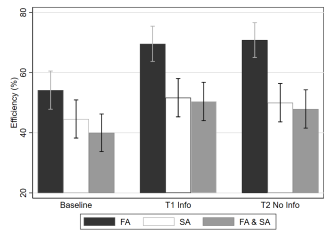
(a) Final prices



(b) Seller payoff



(c) Buyer payoff



(d) Efficiency

Notes: Figure 1a, 1b, 1c and 1d display the average final prices, seller payoff and buyer payoff in the FA and SA. The SA is divided by whether or not the seller submitted shill bids in the auction. Figure 1d shows the average efficiency in the FA, SA and for both auctions together. The FA (SA) is efficient if the bidder with the highest (second highest) private value in that Market wins the item and both the FA and SA are efficient together in a Market if the two previous conditions hold simultaneously. The lines show 95% confidence intervals.

Average SA final prices are also higher in both shill bidding treatments when the seller shill bids compared to the average SA final price in the Baseline (B vs T1, $p_{MWU} = 0.025$ and B vs T2, $p_{MWU} = 0.0104$).[19] However, there are no statistically significant differences in SA final prices between the Baseline and the shill bidding treatments when the seller did not choose to shill bid. This implies that the participation of shill bidding sellers in the SA affects prices. Furthermore, comparing the two shill bidding treatments, average final prices are not different. This is particularly interesting in the SA, as having access to previous bidding history does not increase the ability of the shill bidding seller to increase the final price. Finally, the prices are higher in the FA than in the SA for each independent Group. Thus, as Ashenfelter (1989); Preston and Daniel (1993) and others, we find evidence of the declining price anomaly in all three treatments. In relation to this, we find that the average FA price in the Baseline and T2 are close to the theoretically expected prices of 40 and 50, while the average SA prices are lower than expected in theory.[20]

Result 2.1 *Prices: **(i)** Shill bidding in the second auction increases prices in both the first and the second auctions. **(ii)** In the second auction, prices are higher when the sellers shill bid compared to when they do not. **(iii)** Prices are higher in the first auction than in the second auction in all three treatments.*

Seller Payoff: As a seller's payoff in the FA equals the FA price, the seller earns more from the FA in the two shill bidding treatments than in the Baseline.[21] However, even though Figure 1a displays higher SA auction prices in the two shill bidding treatments, the sellers' average payoff from the SA is not different across any of the three treatments. This can be seen in Figure 1b that displays average seller payoff. The reason for this may be that, when shill bidding, the sellers won the item in 14.6% of the cases in T1 and 17.4% in T2, in which case the payoff is 0.[22] However, shill bidding does not decrease the sellers' payoff on average. Thus, we partly find support for Hypothesis 2.2 since the sellers earn more in the FA, but not less in the SA, in T2 than in the Baseline. If we only look at the SAs in which the sellers shill bid, then the average SA final prices are not different when the sellers win the SA compared to when they do not win the SA. Therefore, shill bidding raises the SA payoff when the seller does not win the item in the SA in both shill bidding treatments (B vs T1, $p_{MWU} = 0.025$ and

[19] The sellers submitted at least one shill bid in 71.7% of the SAs in T1 and 76.7% in T2.

[20] In Table 2.10 of Appendix 2.5.2, we provide the results from OLS regressions, which confirm these results.

[21] In the analysis of seller payoff we include the payoff of the sellers from both auctions and thus disregard that the sellers were only paid one of the two prices, which was chosen by a lottery at the end of the round.

[22] In the experiment, there was a cost of 1 ECU for sellers participating as a shill bidder. This cost is disregarded in this analysis.

B vs T2, $p_{MWU} = 0.0104$).[23]

Buyer Payoff: In Figure 1c surprisingly, we can see that in spite of the higher FA average prices in the shill bidding treatments, buyers' FA payoffs are not different between any of the three treatments. As expected, however, the buyers' average SA payoffs are lower in the shill bidding treatments compared to the Baseline (B vs T1, $p_{MWU} = 0.025$ and B vs T2, $p_{MWU} = 0.0104$). Hence, we do not find full support for Hypothesis 2.3 as the buyers' payoff is only lower in the SA, and not in the FA, in T2 compared to the Baseline. Analyzing the effects of shill bidding on the buyers' SA payoffs, we find that payoffs are lower in the shill bidding treatments when the sellers shill bid compared to the Baseline (B vs T1, $p_{MWU} = 0.0163$ and B vs T2, $p_{MWU} = 0.0104$). Comparing when the sellers do not shill bid, there are no differences across the treatments.[24]

Finally, as prices are decreasing over time within a round, sellers earn more in the FA than in the SA. Buyers also earn more in the FA than in the SA, but only in T1 ($p_{SR} = 0.0277$). Taking the results on sellers' and buyers' payoff together, we can conclude that the sellers are are the winners from shill bidding:

Result 2.2 *Payoffs: Due to shill bidding in the second auction,* ***(i)*** *sellers' payoffs are increased in the first auction and are not lowered in the second auction; and* ***(ii)*** *buyers' payoffs are not affected in the first auction, but their second auction payoffs are lower.*

Efficiency: The somewhat surprising result that buyers' FA payoffs are not lower in the shill bidding treatments compared to the Baseline can be explained by differences in efficiency across the different treatments. We define the FA (SA) to be efficient if the buyer with the highest (second highest) private value wins the item. The FA and SA are both together efficient if the two previous requirements both hold true. As can be seen in Figure 1d the average efficiency is higher in the FA for the shill bidding treatments compared to the Baseline (B vs T1, $p_{MWU} = 0.0159$ and B vs T2, $p_{MWU} = 0.0127$).[25] The difference is size-able as while the average FA efficiency is 54.2% in the Baseline, it is 69.6% in T1 and 70.8% in T2. As buyers with the highest private value win the FA

[23] GLS random effects regressions confirming these results are shown in Table 2.11 of Appendix 2.5.2.

[24] Table 2.12 in Appendix 2.5.2 presents the GLS random effects regressions results regarding buyer payoffs.The GLS random effects regressions confirm the results. However, in models (4) and (6), with buyer SA payoff as a dependent variable, the coefficients of the shill bidding treatments are negative but not statistically significant.

[25] The averages shown in Figure 2.2d include all SAs in which the sellers win the item and efficiency is never lower in any of the two shill bidding treatments compared to the Baseline.

to a larger extent in the shill bidding treatments than in the Baseline, this counteracts the increase of the average final prices in the shill bidding treatments on the buyers' average earnings in the FA. Even though average efficiency is higher in the SA, and in both the FA and SA, for the shill bidding treatments compared to the Baseline, none of these differences are statistically significant.[26]

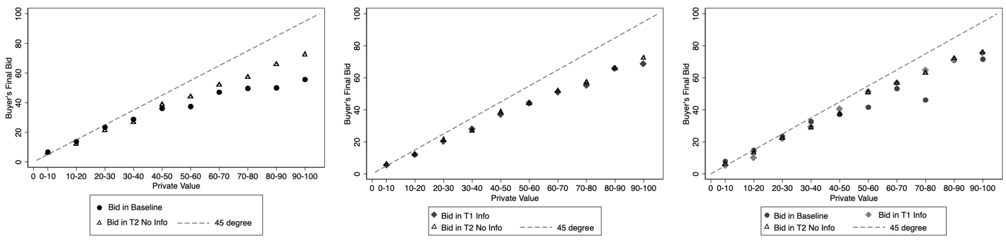
Result 2.3 *Efficiency: The possibility of Shill bidding in the second auction,* ***(i)*** *increases efficiency in the first auction; and* ***(ii)*** *does not lower efficiency in the second auction nor both auctions together.*

2.3.2 Buyer behavior

In this section we look deeper into how the buyers behaved in the experiment.

Figure 2.2: Buyers' average final bids by private value

(a) Shill bidding effect - FA (b) Information effect - FA (c) Final bids - SA



Notes: We divided buyers into 10 groups based on their private value. The data in each figure is the average of the final bids for all buyers in the same private value group. Figure 2.2a compares the average final bid in the FA between Baseline and T2 (with shill bid and No info). Figure 2.2b shows the average final bid in the FA between T1(with shill bid and Info) and T2. Figure 2.2c shows the average SA bid for all treatments.

Treatment Differences: We start by investigating if there are any between-treatment differences. We note that even though the average FA bid is higher in the shill bidding treatments (39.9 in both) compared to the Baseline (35.8), the Mann-Whitney U tests on the Group averages show that there are no statistically significant treatment differences. Thus, we do not find support for either Hypothesis 2.4 or Conjecture 2.1 on average. One reason for this might be that buyers with a low private value may be unable to submit their desired final bid since other buyers may push the current

[26] We also tried an alternative measurement of efficiency based on our purpose. As you can see from the Figure 2.5 in Appendix 2.5.2, we also measured the efficiency by the ratio of the winner's private value and the current highest private value in that auction. We still found a similar result that when shill bidding is possible, the efficiency of FA auctions is significantly increased, so does the overall efficiency.

price above this bid before it was submitted. Therefore, we create Figure 2.2, which shows the buyers' average final FA and SA bids conditional on their private value. Figure 2.2a and Figure 2.2b show FA bids for the Baseline and T2 and T1 and T2 respectively, while Figure 2.2c displays SA bids for all treatments. Looking at Figure 2.2a, we can see that a shill bidding effect exists for buyers with a private value above 50 since their final FA bids are higher in T2 than in the Baseline. Moreover, the buyers with a private value of 50 and above are less likely to be unable to submit their desired final bid.[27] Consequently, we partly find support for a shill bidding effect in line with Hypothesis 2.4. By inspecting Figure 2.2b, we can see that providing the shill bidding sellers with the FA bidding history does not have an effect on the buyers' final FA bids for any private value group. Consequently, we do not find an information effect on buyers FA bids and, thus, no support for Conjecture 2.1.[28] We believe that the absence of an information effect on buyers FA bids may be caused by buyers being boundedly rational. In Table 2.13 of Appendix 2.5.2, we provide results from GLS regressions that confirm these two results. [29]

Turning to the final SA bids, Figure 2.2c paints a somewhat similar picture since it looks as if buyers with a higher private value bid higher in the shill bidding treatments compared to the Baseline. However, the results from GLS regressions, displayed in Table 2.14 in Appendix 2.5.2, suggest that this effect is much weaker and non-existent. This is to be expected as the buyers have the same dominant strategy in all three treatments in the SA.

Estimating bidding functions: Now we estimate the bidding functions, which were theoretically conjectured using the sealed-bid assumption, of buyers' final bids using Maximum Likelihood and assuming normally distributed errors. The results are shown in Table 2.4. For the Baseline we report the estimate of the coefficient β_1 when $Bid_{FA} = \beta_1 * v_i + \epsilon$. The estimate equals 0.674 and we cannot reject that this is

[27]A crude measure of this is to count the number of buyers for whom the FA final price is higher than or equal to their private value and who do not submit the highest or second-highest bid. By doing this, only 9.5% of the buyers with a private value above 50 are unable to submit their desired bid. For the buyers with a private value of 50 or less, this number is 73.5%.

[28]We do find that buyers overbid less in the FA in T1 than in T2. Table 2.17 and Figure 2.6 in Appendix 2.5.2 shows the Probit regression on buyers' probability of submitting an overbid and the graph for comparing the overbidding behavior across treatments, which confirms this result.

[29]We also checked that if there is a censoring problem of their final bid strategies that buyers will not reveal her maximum bid if others already pushed the price higher than this maximum bid. We find that, since overbidding is allowed, all the low private value buyers submitted some bids and the average of their final bid is slightly higher than their private value (with a ratio of 1.5 on average). If one buyer's private value is too low, we don't think that they are the ones that we want to study. At the same time, for these buyers with a higher private value, we find that they do have a symmetric strategy of sniping, in this case, they have this intention to submit their intended bids in the end of each auction. This censoring problem may not be an essential problem in our experiment.

significantly different from $\frac{2}{3}$, which is what is expected from the theoretical conjecture. For T2 we estimate β_2 when $Bid_{FA} = \frac{2}{3} * v_i + \frac{\beta_2 3}{v_i^2 * 3} + \epsilon$ for $v_i > \beta_2$ and $Bid_{FA} = v_i + \epsilon$ for $v_i \leq \beta_2$. Hence, β_2 determines the shape of the bidding functions for the buyers with a private value larger than β_2 and the cut-off of buyers who bid as if the FA is the last auction they participate in and, therefore, bid their private value. Recall that the cut-off is determined by the expected shill bid in the SA. The estimate equals 45.7 and it is not significantly different from the theoretically conjectured value of 50. For sake of comparison, we estimate β_2 for T1 as well and the estimated parameters are not significantly different between T1 and T2.

Table 2.4: MLE of buyers' bidding functions.

	Baseline		Treatment 1		Treatment 2	
	Expected	Observed	Expected	Observed	Expected	Observed
FA	$\beta_1 : 0.678$	0.674*** [0.036]	-	41.157*** [1.947]	$S : 50$	45.697*** [3.326]
SA	$\beta_2 : 1$	0.785 [0.068]	$\beta_2 : 1$	0.863 [0.029]	$\beta_2 : 1$	0.868 [0.031]

Notes: Parameters of buyer bids are estimated using maximum likelihood.
The test in Treatment 1 is compared with theory of Treatment 2.
Standard errors clustered on Group level in brackets.

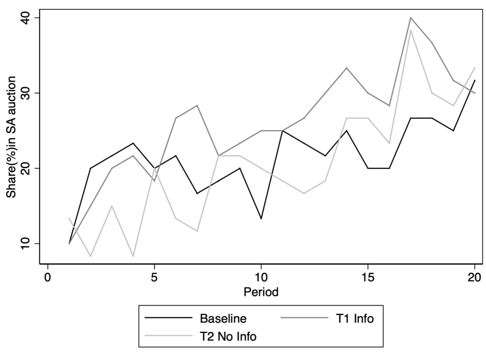
In summary, we find support for Hypothesis 2.4, at least for the buyers with a private value above average, but not for Conjecture 2.1. Furthermore, we can conclude that buyers' behavior can be explained fairly well, at least in the FA, by the theoretically conjectured bidding functions in spite of the sealed-bid assumption. A possible reason for this is that buyers sniped a lot and submitted final bids just before the fixed ending time.[30]

Result 2.4 *Buyers' first auction final bid:* ***(i)*** *Partly in line with the prediction of Hypothesis 1, shill bidding in the second auction increases the first auction final bid, at least for buyers with a private value greater than average.* ***(ii)*** *Contrary to Hypothesis 2, providing the seller with the complete bidding history from the first auction does not lower the buyers' first auction final bid.* ***(iii)*** *On average, buyers' FA final bids are predicted fairly well by the theoretically derived conjectures, in spite of the sealed-bid assumption.*

[30] See Figure 2.4b and Figure 2.4c in Appendix 2.5.2 for the amount of sniping in the experiment. Table 2.16 and Table 2.15 show that buyers' sniping behavior are not significantly affected by the treatments and that sniping occurs very often.

For the bidding functions of the SA final bids, we estimate β_2 in $Bid_{SA} = \beta_2 * v_i + \epsilon$ assuming normally distributed errors. While F-tests indicate that the estimated parameters are not significantly different between treatments in the SA, we find that β_2 is significantly lower than the conjectured value of 1 in all treatments when using Wald tests ($p < 0.01$). However, we find that buyers seem to learn over time to use their optimal strategy, which is to submit a bid equal to their private value. Figure 2.3 shows the share of buyers in the SA who submitted a bid equal to their private value by treatment and period. While only 18.7% of the buyers submitted a bid equal to their private value in the first period, 52.8% did so in the last period[31].

Figure 2.3: Learning effect in the SA



Notes: The share of subjects who submit a bid equal to their private value in the SA are plotted over time and by each treatment.

Finally, we find that buyers bid higher relative to their private value in the SA than in the FA as expected. In particular, if we look at the buyers who participated in both the FA and the SA, all Group averages for SA bids are higher than their FA counterparts.

Result 2.5 *Buyers' second auction final bid:* ***(i)*** *In line with what is expected, there are no treatment differences in buyers' SA final bids.* ***(ii)*** *Buyers seem to learn, as they use their optimal strategy, which is to submit a final bid equal to their private value, more over time. However, the majority of buyers does not use this strategy.*

2.3.3 Seller behavior

We start by noting that the sellers chose to participate as bidders and bid fairly often in the SA. The sellers chose to submit at least one shill bid in 71.7% of the SAs in

[31] In the FA, buyers submit a bid equal to their private value more often in the later auctions in T1 and T2, but not in the Baseline. This could be interpreted as the buyers learning about the threat of shill bidding in the SA and, therefore, bid high in the FA in order to avoid participating in the SA.

T1 and 76.7% in T2 [32] and there is no significant difference between the treatments. Table 2.5 presents mean percentages of SAs in which the seller shill bid, won the SA and successfully raised the SA price, by the two shill bidding treatments. Even though the sellers in Treatment 1 seem to perform better as they won the SA less often and placed relatively more successful shill bids, we do not find any statistically significant differences between the two treatments. Consequently, giving the sellers the complete bidding history from FA, as in Treatment 1, does not increase their performance.[33]

Table 2.5: Means of descriptive statistics for sellers by the two shill bidding treatments

Variables	Treatment 1	Treatment 2
Seller shill bids SA (%)	71.67	76.67
	[45.16]	[42.38]
Seller wins SA (%)	14.53	17.39
	[35.35]	[38.01]
Successful shill bid (%)	30.23	25
	[46.06]	[43.42]

Notes: The percentage of Seller wins SA and Successful shill bid are calculated conditional on the seller shill bidding

Bidding Strategy: Turning to the sellers' bidding strategy, Table 2.6 shows the average seller SA final bid and the correlation between the FA price and the sellers' SA final bids. While there are no treatment differences in the magnitude of final bids, sellers bid lower than 50 in T2, which is the theoretical prediction ($p_{SR} = 0.028$). This may be due to sellers being risk-averse, but more interesting is the fact that the optimal shill bid, when only taking the SA into account, is 33.33. Therefore, it may be that sellers don't take into account that their SA shill bids affect buyers FA bids. However, since the FA has finished once the sellers submit shill bids, the actual shill bids can no longer affect the buyers' FA bids. Realizing this may be what makes sellers submit shill bids closer to 33.33 than 50. Moreover, from the Spearman rank-order correlation coefficients between the sellers' SA bid and the price in the FA, we can conclude that sellers in T1 react to the information they are shown. The correlation is significantly stronger in T1 than in T2.

[32]This is lower than expected, at least in T2, where we expect 100% participation in theory since shill bidding is profitable even when there is a risk of winning the auction.

[33]Table 2.18, in Appendix 2.5.2, shows results from a probit regression that the sellers' probability of participating as a shill bidder does not differ by treatments.

Table 2.6: Sellers' bidding strategy

	Average seller bids		Correlation ρ (info vs bids)	
	Treatment 1	Treatment 2	Treatment 1	Treatment 2
SA	37.81 [18.80]	35.19 [21.92]	0.469***	0.103
Test if $s = 50$	Reject	Reject	T1 > T2***	

Notes: The test for Treatment 1 is compared with the theory of Treatment 2.
Standard deviation in brackets.

Consequently, we find support for Conjecture 2.2:

Result 2.6 *Information and sellers: Sellers react to the information since by providing the sellers with the complete bidding history from the first auction there is a positive correlation between the FA price and the sellers' SA bid.*

2.3.4 Do sellers and buyers behave differently?

Since shill bidding may hurt buyers, it is interesting to know if it is possible for buyers, and auction houses, to detect when a seller is shill bidding. Therefore, we compare the behavior of buyers and sellers in different respects in this section. Figure 2.4 summarizes the differences in behavior that we observed between buyers and sellers in our experimental data. We test the differences by using the Wilcoxon matched-pairs signed-ranks test on the independent Group averages with a hypothesis of no difference between their behaviors. All data presented in this section is from the SA since sellers can only submit bids in this auction.[34]

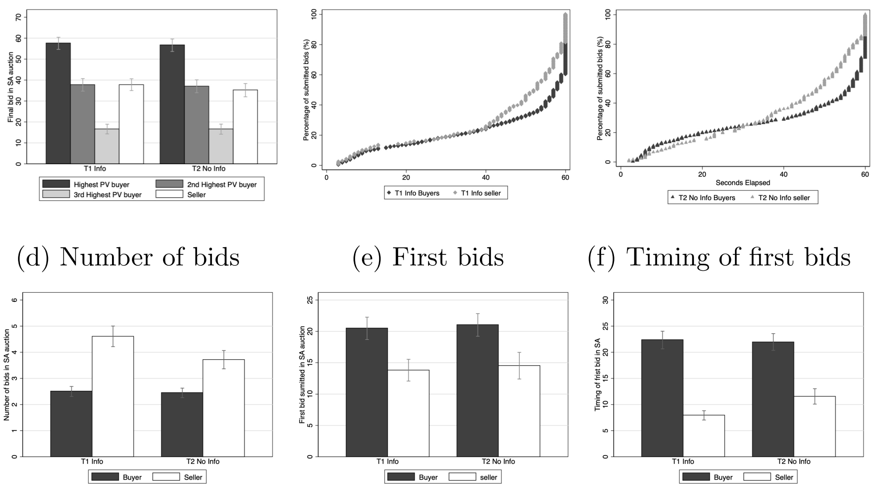
Final bids: In Figure 2.4a, we ranked the three buyers who participated in a SA into three levels based on their private value (PV). As we can see from the graph, the sellers' final bids are smaller than those from the highest PV buyer, close to those from the 2nd highest PV buyer and greater than those from the 3rd highest PV buyer. We found acceptance between the bids submitted by the 2nd highest PV buyer and the sellers ($p_{SR} = 0.735$ in T1 and $p_{SR} = 0.600$ in T2). For the other comparisons, we reject equality and the difference goes in the direction that we observe in Figure 2.4a ($p_{SR} = 0.028$ in each of the four tests). It, thus, seems that sellers final bids are essentially equal to the bids submitted by the second-highest PV bidders.

[34]The treatment differences for these variables, for both buyers and sellers in the SA using GLS regressions, are shown in Table 2.19 and Table 2.20 in Appendix 2.5.2. They confirm the different bidding behavior between buyers and sellers, and also imply that they are affected differently by shill bidding and information.

Figure 2.4: Behavior between buyers and sellers

(a) Final bids (b) Sniping in T1 Info (c) Sniping in T2 No Info

(d) Number of bids (e) First bids (f) Timing of first bids



Notes: In Figure 2.4a (final bids), we rank the three buyers who participated in a SA by their private value into highest, second-highest and third-highest private value (PV) buyer. We only include the sellers who chose to participate in the SA auction. In Figure 2.4b and 2.4c, the CDFs of the timing of the final SA bid for both buyers and sellers in each shill bidding treatment are plotted. Figure 2.4d, 2.4e and 2.4f show the average for each variable divided by seller and buyer. The lines show 95% confidence intervals.

Sniping: Following Ariely, Ockenfels, and Alvin E Roth (2005) and Alvin E. Roth and Ockenfels (2002), who find that last-minute bidding, also known as sniping, frequently occurs in eBay auctions, we plot the CDFs of the timing of the final bid for buyers and sellers in Figure 2.4b and Figure 2.4c. There is a significant difference between the share of buyers and sellers who submit their final bid in the last 5 seconds of the SA ($p_{SR} = 0.075$ in T1 and $p_{SR} = 0.035$ in T2) in both shill bidding treatments. We observe that 65.2% of buyers submitted their last bid within the last 5 seconds of the SA, while 43.7% of the sellers did the same.[35] Consequently, we conclude that buyers snipe more than sellers.

Number of bids: With the purpose of seeing how active sellers and buyers are in the auctions, we display Figure 2.4d to see how many bids that they submit during the SA. Figure 2.4d suggests that sellers are much more active as they submit more bids ($p_{SR} = 0.028$ in both T1 and T2) than buyers.

First bids: In Figure 2.4e and 2.4f, which show the average of the size and timing of the first bids, we can see that sellers submit their first bids earlier and that they are lower than buyers' first bids ($p_{SR} = 0.028$ in all four tests).

By comparing these variables, we can conclude that buyers and sellers behave differently

[35]In Figure 2.7 in Appendix 2.5.2, we also show the CDFs of the timing of the last submitted bid for buyers and compare these between treatments. It suggests that there are no treatment differences, which means that buyers snipe a lot even when there is no threat of shill bidding.

in many respects when acting as bidders.[36] Thus, if buyers see a small early bid followed by many other bids, then they can suspect that a shill bidding seller is active in the auction.

2.4 Conclusion

We conducted a laboratory experiment to test the effects of shill bidding in sequential eBay auctions environment. The experiment replicates a real-world issue in online auctions that has been observed by previous researchers. Using two eBay auctions, we investigate the effects of shill bidding in ongoing and future auctions, and the impact of disclosing past buyer bidding histories to sellers, on prices, earnings as well as buyers' and sellers' bidding behavior.

We find that the possibility of shill bidding affects outcomes in both auctions. Shill bidding causes prices to be higher in both auctions and sellers are the winners of shill bidding as they earn more in the first auction, while buyers earn less in the second auction. Consequently, these results provide further evidence that behaviors of shill bidding do hurt buyers with a higher willingness to pay and benefit sellers in the end, so online auction platforms should figure out how to protect their buyers from shill bidding to increase their trust in the auction house. However, we find that the threat introduced by the possibility of shill bidding increases the efficiency of auctions because more current highest private value buyers win the current auctions. This implies a conflict between protecting buyers by preventing shill bidding or getting higher efficiency by implicitly allowing shill bidding. It suggests that even though preventing shill bidding is what online auction platforms should do, they may not have a strong incentive to do so. However, providing the seller with the bidding history from the previous auctions has very little effect on behavior and outcomes. This suggests that such information disclosure may not be an issue for auction houses as long as buyers know that there is a possibility of future shill bidding. Importantly, we find that buyers and sellers behave quite differently when acting as bidders. If this difference also carries over to the real-world, then it makes it possible for buyers and auction houses to detect and prevent shill bidding sellers. Interestingly, even though sellers shill bid lower than expected, buyers react strongly to shill bidding in the first auction. Consequently, it seems that buyers truly react to the threat of shill bidding in future auctions.

[36] We also compared buyers' behavior, including the baseline, which can be found in Figure 2.8 in Appendix 2.5.2. It suggest that there are no significant differences in buyers' behavior between any of the three treatments.

Our experiment suggests a few open questions for future research. A natural extension to our experimental design is to compare shill bidding to the case in which the seller chooses a reservation price before the start of the second auction. In this case, we hypothesize that buyers' behavior will be less affected compared to shill bidding since the sellers only act before the auctions start. We also believe that giving the seller the bidding history from the first auction will, at least, have more impact on the behavior and outcomes of the sellers. Another path would be to compare the effects of seller participation in sequential auctions using different auction formats such as eBay, sealed-bid, and English auctions. In reality, shill bidding sellers who win their own item have the possibility to resell it. Accommodating this into an experimental design would lower the cost of shill bidding and we would, consequently, expect more shill bidding. Nevertheless, except eBay, we do also have some other online auction platforms like Amazon, eBid, etc,. Most of them do have a similar auction environment like sequential auctions, possible shill bidding but sometimes different rules about pricing and housing fee. So it would also be good to check how these different settings in online auctions can contribute to less shill bidding in the future studies.

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2.5 Appendix

2.5.1 Theoretical model

We make the simplifying assumption that each auction is conducted as a sealed-bid second-price auction. In each round two items are sold by a seller who uses two sequential sealed-bid auctions with $n \geq 3$ bidders. The bidders have unit demand and the winner of the FA will therefore not participate in the SA. Each bidder i has a private value, v_i, which is the same for both items. The private value is drawn from a uniform distribution on (0,1). For any bidder i, let $Y_1, \ldots, Y_{n-1}$ denote the private values of the other $n-1$ bidders, where $Y_1 > Y_2 > \ldots > Y_{n-1}$. Let $\beta_1(v_i)$ be a bidding function determining how much a bidder with private value v_i bids in the FA. Similarly, $\beta_2(v_i)$ is a bidding function for the SA. We assume that $\beta_1(v_i)$ and $\beta_2(v_i)$ are symmetric and strictly increasing. This implies that the bidder with the highest private value wins the FA and the bidder with the second-highest private value wins the SA. The seller's private value for the items is assumed to be 0.

2.5.1.1 Buyer behavior

We start by considering the case in which the seller cannot shill bid. This is equal to the standard model of Paul. R Milgrom and Robert J. Weber (2000). As the SA is equal to a standard sealed-bid second-price auction, the bidders have a dominant strategy of submitting a bid equal to their private value. Thus, $\beta_2(v_i) = v_i$. Turning to the FA, the bidder is trading off the possibility of winning an item now or waiting to possibly acquire an item in the SA. We will consider what happens if a bidder bids $\beta_1(w)$,where $w \neq v_i$ and then assume truthful bidding to drive the optimal bidding function $\beta_1(v_i)$. Then we will return to the cases where the bidder deviates and show that this makes him strictly worse off. Assume that the bidder bids $w \geq v_i$, then the expected payoff, $U_i(w, v_i)$ for a bidder in the FA is:

$$U_i(w, v_i) = w^{n-1} \left(v_i - E[\beta_1(Y_1)|Y_1 < w]\right) \tag{2.4}$$

$$+(n-1)(1-w)\, v_i^{n-2} \left(v_i - E[\beta_2(Y_2)|Y_2 < v_i < Y_1\right),$$

where the first term results from the event that $Y_1 < w$ and the second from $Y_2 < v_i < w < Y_1$ and since the bidders valuations are independently drawn from a uniform distribution on (0,1), v_i^{n-1} is the probability that bidder i wins the FA.[37] With uniformly distributed private values and since $\beta_2(v_i) = v_i$ 2.4 becomes:

$$U_i(w, v_i) = w^{n-1}\left(v_i - \frac{n-1}{n}\beta_1(w)\right) + (n-1)(1-w)\, v_i^{n-2}\left(v_i - \tfrac{n-2}{n-1}v_i\right), \tag{2.5}$$

Taking the derivate of Equation 2.5 w.r.t w and setting it to 0 yields:

$$\begin{aligned}\frac{\partial}{\partial w}U_i(w, v_i) =&(n-1)w^{n-2}\left(v_i - \frac{n-1}{n}\beta_1(w)\right) - w^{n-1}\frac{n-1}{n}\beta_1'(w)\\ &-(n-1)v_i^{n-2}\left(v_i - \frac{n-2}{n-1}v_i\right) = 0,\end{aligned} \tag{2.6}$$

In equilibrium $w = v_i$, which gives:

$$\beta_1'(v_i) = \frac{(n-2)n}{n-1} - \beta_1(v_i)\frac{n-1}{v_i} \tag{2.7}$$

The solution to the differential equation 2.7 is:

$$\beta_1(v_i) = \frac{n-2}{n-1} \times v_i \tag{2.8}$$

Now assume that the bidder bids $w < v_i$, then the expected payoff, $U_i(w, v_i)$ for a bidder in the FA is:

$$\begin{aligned}U_i(w, v_i) =&w^{n-1}\left(v_i - \frac{n-1}{n}\beta_1(w)\right)\\ &+(n-1)(1-v_i)\int_0^{v_i}(v_i - p)p^{n-3}f(p)\partial p\\ &+(n-1)\int_w^{v}(n-2)\int_0^{y_1}(v_i - p)p^{n-3}f(p)\partial p f(y_1)\partial y_1\end{aligned} \tag{2.9}$$

where the first term results from the event that $Y_1 < w$, the second from $Y_2 < v_i < Y_1$and the third from $w < Y_1 < v_i$. By taking the derivative of Equation 2.9 and setting it to 0 we get:

[37] $(n-1)(1-w)\, v_i^{n-2}$ is the probability that bidder i loses the FA and wins the SA.

$$\begin{aligned}\frac{\partial}{\partial w}U_i(w,v_i) =&(n-1)w^{n-2}\left(v_i-\frac{n-1}{n}\beta_1(w)\right)\\ &-w^{n-1}\frac{n-1}{n}\beta_1'(w)-(n-1)w^{n-2}\left(v_i-\frac{n-2}{n}w\right)\\ &+w^{n-1}\frac{n-2}{n}=0,\end{aligned} \tag{2.10}$$

Equation 2.10 together with the equilibrium requirement that $w = v_i$,gives Equation 2.7 and, thus, the optimal strategy given in Equation 2.8.

Now we will check that the bidders have no incentive to deviate from the optimal strategy by bidding $\beta_1(w)$, where $w \neq v_i$. To do this we will check that Equation 2.6 and 2.10 are maximized at $w = v_i$and that $\frac{\partial}{\partial w}U_i(w,v_i) \leq 0$ when $w > v_i$and $\frac{\partial}{\partial w}U_i(w,v_i) \geq 0$ when $w < v_i$for $\beta_1(w) = \frac{n-2}{n-1} \times w$. Using the optimal strategy, and after some manipulations, Equation 2.6 becomes:

$$v_i - \frac{n-2}{n-1}w - \left(\frac{v_i}{w}\right)^{n-2}\frac{v_i}{n-1} = 0, \tag{2.11}$$

which holds true for $w = v_i$ and is less than equal to 0 for any $w > v_i$,which is what we need.

Similarly, by replacing $\beta_1(w) = \frac{n-2}{n-1} \times w$in Equation 2.10 and perfoming some manipulations we get $0 = 0$, which concludes the proof.

Now we introduce the possibility for the seller to submit a shill bid in the SA. We let $s \in [0, 1]$ denote the shill bid submitted by the seller in the SA. However, for now, the seller does not have any information regarding the bids submitted in the FA. The bidders' dominant strategy in the SA does not change due to the sellers' ability to shill bid. Regardless of whatever shill bid the seller submits, it is still optimal for the bidders to bid their private value in SA .

We will consider what happens if a bidder bids $\beta_1(w)$,where $w \neq v_i$ and then assume truthful bidding to drive the optimal bidding function $\beta_1(v_i)$. Then we will return to the cases where the bidder deviates and show that this makes him strictly worse off. For the bidders whose private value is less than s, they view the FA as the last auction they participate in and hence submit a bid of v_i. For any bidder for whom $v_i \geq s$, assume that the bidder bids $w \geq v_i$, then the expected payoff, $U_i(w, v_i)$ in the FA is:

$$U_i(w, v_i) = w^{n-1}\left(v_i - \frac{n-1}{n}\beta_1(w)\right) + (n-1)(1-w)\left[\int_s^v (n-2)(v_i-p)p^{n-3}\partial p + (v_i - s)s^{n-2}\right], \tag{2.12}$$

which is equal to:

$$U_i(w, v_i) = w^{n-1}\left(v_i - \frac{n-1}{n}\beta_1(w)\right) + (n-1)(1-w)\left[v_i^{n-1} - \left(\frac{v_i^{n-1}(n-2)+s^{n-1}}{(n-1)}\right)\right], \tag{2.13}$$

Taking the derivate of Equation 2.13 w.r.t w and setting it to 0 yields:

$$\frac{\partial}{\partial w}U_i(w, v_i) = (n-1)w^{n-2}\left(v_i - \frac{n-1}{n}\beta_1(w)\right) - w^{n-1}\frac{n-1}{n}\beta_1'(w) - (n-1)\left[v_i^{n-1} - \left(\frac{v_i^{n-1}(n-2)+s^{n-1}}{(n-1)}\right)\right] = 0, \tag{2.14}$$

In equilibrium $w = v_i$, which gives:

$$\beta_1'(v_i) = \frac{(v_i^{n-1}(n-2)+s^{n-1})n}{v_i^{n-1}(n-1)} - \beta_1(v_i)\frac{n-1}{v_i} \tag{2.15}$$

The solution to this differential equation is:

$$\beta_1(v_i) = \frac{v_i^{n-1}(n-2)+s^{n-1}}{v_i^{n-2}(n-1)} \tag{2.16}$$

Now assume that the bidder bids $w < v_i$, then the expected payoff, $U_i(w, v_i)$ for a bidder in the FA is:

$$\begin{aligned} U_i(w, v_i) =& w^{n-1}\left(v_i - \frac{n-1}{n}\beta_1(w)\right) \\ &+(n-1)\,(1-v_i)\left(\int_s^v (n-2)(v_i-p)p^{n-3}\partial p + (v_i - s)s^{n-2}\right) \\ &+(n-1)\int_w^v (n-2)\int_s^{y_1} (v_i-p)p^{n-3}f(p)\partial p + (v_i-s)s^{n-2}f(y_1)\partial y_1, \end{aligned} \tag{2.17}$$

which is equal to:

$$\begin{aligned} U_i(w, v_i) =& w^{n-1}\left(v_i - \frac{n-1}{n}\beta_1(w)\right) \\ &+(n-1)\,(1-v_i)\left(\int_s^v (n-2)(v_i-p)p^{n-3}\partial p + (v_i - s)s^{n-2}\right) \\ &+v^{n-1}(v_i - \frac{n-2}{n}v_i) - s^{n-1}v_i - w^{n-1}(v_i - \frac{n-2}{n}w) + s^{n-1}w, \end{aligned} \tag{2.18}$$

By taking the derivative of Equation 2.17 and setting it to 0 we get:

$$\begin{aligned} \frac{\partial}{\partial w}U_i(w, v_i) =& (n-1)w^{n-2}\left(v_i - \frac{n-1}{n}\beta_1(w)\right) - w^{n-1}\frac{n-1}{n}\beta_1'(w) \\ &-(n-1)w_i^{n-2}(v_i - \frac{n-2}{n}w) + w^{n-1}\frac{n-2}{n} + s^{n-1} = 0, \end{aligned} \tag{2.19}$$

Equation 2.19 together with the equilibrium requirement that $w = v_i$,gives Equation 2.15 and, thus, the optimal strategy given in Equation 2.16.

Now we will check that the bidders have no incentive to deviate from the optimal strategy by bidding $\beta_1(w)$, where $w \neq v_i$. To do this we will check that Equation 2.14 and 2.19 are maximized at $w = v_i$and that $\frac{\partial}{\partial w}U_i(w, v_i) \leq 0$ when $w > v_i$and $\frac{\partial}{\partial w}U_i(w, v_i) \geq 0$ when $w < v_i$for $\beta_1(w) = \frac{w^{n-1}(n-2)+s^{n-1}}{w^{n-2}(n-1)}$. Using the optimal strategy, and after some manipulations, Equation 2.14 becomes:

$$v_i - \left(\frac{v_i}{w}\right)^{n-2} v_i + \frac{(v_i^{n-1} - w^{n-1})(n-2)}{w^{n-2}(n-1)} = 0, \tag{2.20}$$

which holds true for $w = v_i$ and is less than 0 for any $w > v_i$.

Similarly, by replacing $\beta_1(w) = \frac{n-2}{n-1} \times w$ in Equation 2.19 and performing some manipulations we get that $0 = 0$, which concludes the proof.

Now we turn to the situation in which, before submitting a shill bid in the SA, the seller can observe all except the winning bid from the FA. Since the bidders are assumed to use $\beta_1(\cdot)$, the seller can perfectly infer the private values of the bidders who will participate in the SA from the bids they submitted in the FA by inverting $\beta_1(\cdot)$. Therefore, it is optimal for the seller to submit a shill bid equal to the private value of the bidder who submitted the second-highest bid in the SA and then the payment to the bidder who wins the SA is equal to this bidder's private value. While leaving the dominant strategy in the SA unchanged, this gives the bidders incentives to report a low private value in the FA. Consequently, $\beta_1(\cdot)$ does not exist in this scenario:

Proposition 2 *If the seller shill bids and is informed of the losing bidders' bids in the FA, then there does not exist a strictly increasing symmetric bidding function $\beta_1(v_i)$ for any bidder i.*

Therefore, we expect that giving the seller the complete anonymous bidding history from the FA, as in Treatment 1, will decrease the bidders' bids in the FA compared to Treatment 2. The proof of 2 is similar to the proofs of Proposition 9 and Proposition 10 in Katsenos (2010). The proof is mainly given here for completeness. The proof builds on the idea that since the seller is informed of the bidders' bids in the FA and the bidders follow a symmetric strategy, the seller can extract all surplus from the bidders in the SA. This gives incentives for the bidders to report a lower private value as this makes it possible to expect a positive surplus also from the SA. If a bidder i follows the symmetric strategy $\beta_1(v_i)$ and misrepresents his private value by $w \geq v_i$ in the FA, then his expected payoff is:

$$U(w, v_i) = w^{n-1}\left(v_i - \frac{n-1}{n}\beta_1(w)\right) \tag{2.21}$$

Note that if the bidder wins the SA, then he gets zero. This is because the bidder has reported a higher private value, which leads the seller to submit a shill bid that is higher than the bidder's private value and the seller will therefore win the item in the SA.

For a deviation to not be profitable, it must be that $\partial \Delta U(w, v_i)/\partial w \leq 0$.

$$\frac{\partial}{\partial w}U_i(w, v_i) = (n-1)w^{n-2}\left(v_i - \frac{n-1}{n}\beta_1(w)\right) - w^{n-1}\frac{n-1}{n}\beta_1'(w) \leq 0 \tag{2.22}$$

If the bidder instead reports a lower private value $w \leq v_i$, then the expected payoff of the bidder is:

$$
\begin{aligned}
U_i(w, v_i) =& w^{n-1}\left(v_i - \frac{n-1}{n}\beta_1(w)\right) + (n-1)(1-v_i)\int_0^{v_i}(v_i - p)p^{n-3}f(p)\partial p \\
&+(n-1)(1-w)w^{n-2}(v_i - w) \\
&+(n-1)\int_w^{v}(n-2)\int_0^{y_1}(v_i - p)p^{n-3}f(p)\partial p f(y_1)\partial y_1,
\end{aligned}
\tag{2.23}
$$

where the first term results from the event $Y_1 < w$, the second from $w < Y_2 < v_i < Y_1$, the third from $Y_2 < w < Y_1$and the fourth from $w < Y_2 < Y_1 < v_i$ For a deviation not to be profitable, it must be that $\partial \Delta U(w, v_i)/\partial w \geq 0$.

$$
\begin{aligned}
\frac{\partial}{\partial w}U_i(w, v_i) =& (n-1)w^{n-2}\left(v_i - \frac{n-1}{n}\beta_1(w)\right) - w^{n-1}\frac{n-1}{n}\beta_1'(w) \\
&+(n-1)(n-2)w^{n-3}(v_i - w) - (n-1)^2 w^{n-2}(v_i - w) \\
&-(n-1)(w^{n-2} - w^{n-1}) - (n-1)(v_i - \frac{n-2}{n}w) + w^{n-1}\frac{n-2}{n} \geq 0
\end{aligned}
\tag{2.24}
$$

Combining Equation (2.22) and (2.24) we get:

$$
(n-2)w^{n-3}(v_i - w) - (n-1)w^{n-2}(v_i - w) - w^{n-2} \tag{2.25}
$$

$+w^{n-1} - (v_i - \frac{n-2}{n}w) + w^{n-1}\frac{n-2}{(n-1)n} \geq 0,$

which equals:

$$
\left[v_i\left[(n-2)w^{n-3} - (n-1)w^{n-2} - 1\right] + w\left[\frac{n-2}{n}\right]\right] \tag{2.26}
$$

$+\left[\frac{n^2+n-2}{(n-1)n}w^{n-1} - \frac{n^3-2n^2+2n}{(n-1)n}w^{n-2}\right] \geq 0$

Equation (2.26) can never be true as both terms are less than 0 for any $w < v_i$ and $n \geq 3$.

2.5.1.2 Seller behavior

We start by considering the setting in which the seller can submit shill bids, but have no information about the FA bids. In order to derive the optimal shill bid we consider the expected payment of a bidder in the SA in this scenario, which is:

$$(n-1)(1-v_i)\left(\int_s^{v_i}(n-2)pp^{n-3}\partial p + ss^{n-2}\right). \tag{2.27}$$

The seller knows that the bidders' private values are uniformly distributed between 0 and 1. Therefore, the seller's expected revenue in the SA is:

$$n\left[\int_s^1 (n-1)(1-v_i)\int_s^{v_i}(n-2)pp^{n-3}f(p)\partial p f(v_i)\partial dv_i + \int_s^1 (n-1)(1-v_i)ss^{n-2}f(v_i)\partial dv_i\right], \tag{2.28}$$

which is equal to:

$$\frac{n-2}{n+1}+\frac{n}{2}s^{n-1}-2(n-1)s^n+\frac{3(n-1)n}{2(n+1)}s^{n+1} \tag{2.29}$$

However, the seller also needs to take into account the effect of the shill bid on the bidders' FA bids. When the bidders use the optimal strategy in Equation 2.16, then the expected payment of a bidder for whom $v_i \geq s$ in the FA is:

$$(n-1)\int_s^{v_i}\left(\frac{n-2}{n-1}+\frac{s^{n-1}}{p^{n-1}(n-1)}\right)pp^{n-2}f(p)\partial p + (n-1)\int_0^s pp^{n-2}f(p)\partial p, \tag{2.30}$$

which equals:

$$\frac{n-2}{n}v_i^n + s^{n-1}v_i - \frac{n-1}{n}s^n \tag{2.31}$$

For any bidder for whom $v_i < s$, the expected payment in the FA is:

$$(n-1)\int_0^{v_i} pp^{n-2}f(p)\partial p = \frac{n-1}{n}v_i^n \tag{2.32}$$

The seller's expected revenue in the FA becomes:

$$n\left[\int_s^1 \frac{n-2}{n}v_i^n + s^{n-1}v_i - \frac{n-1}{n}s^n f(v_i)\partial v_i + \int_0^s \frac{n-1}{n}v_i^n f(v_i)\partial v_i\right], \tag{2.33}$$

which equals:

$$\frac{n-2}{(n+1)} + \frac{n}{2}s^{n-1} - (n-1)s^n + \frac{n(n-1)}{2(n+1)}s^{n+1} \tag{2.34}$$

To derive the optimal s,we combine Equation 2.29 and Equation 2.34, which is the expected payment from the FA and SA, and take the derivative of this w.r.t s and setting it equal to 0. This gives us:

$$s^2 - \frac{3}{2}s + \frac{1}{2} = 0. \tag{2.35}$$

From Equation 2.35 it follows that the seller's revenue is maximized when $s = \frac{1}{2}$.

2.5.1.3 Description and construction of variables used in regressions

Table 2.7: Description of variables used in regression analysis

Variable	Description and construction of the variables
Ave private value	Ave private value is the average private value among the participating bidders in an auction.
Buyer	A dummy variable that takes the value 1 if the subject is a buyer and 0 otherwise.
FA (SA) payoff (bidders)	Bidder's payoff is their private value minus the final price if they are the winner of an auction and it is 0 if they lose the auction. There is one payoff for the first auction (FA payoff) and one for the second auction (SA payoff).
FA (SA) payoff (sellers)	A seller's payoff in an auction equals the final price of that auction. There is one payoff for the first auction (FA payoff) and one for the second auction (SA payoff)
FA (SA) price	The FA (SA) price is the final price that the winning bidder pays to the seller, which is determined by the second-highest bid submitted before the end of that auction. There is one price for the first auction (FA price) and one price for the second auction (SA price).
FA (SA) 50 and FA (SA) 55	These variables measure late bidding by subjects and are divided by if the last submitted bid was submitted in the last 10 seconds (FA50 and SA50) or last 5 seconds (FA55 and SA55) in each auction. FA and SA refers to the first auction and the second auction.
Final FA (SA) bid	Final bid takes the value of the last bid that bidders and sellers successfully submitted before the end of each auction. Final bid is divided by first (Final FA bid) and second auction (Final SA bid).
First bid SA	This variable measures the magnitude of bidders' and sellers' first bid in the second auction.
Last period balance	This is the experimental currency balance of the last period. This is showed to the subjects at the end of each auction for bidders and each round for sellers.
Last period shill bid	This is a dummy variable that takes the value of 1 if the seller submitted at least one shill bid in the second auction of the previous round and zero otherwise.
Nr bids FA (SA)	The Nr bids variables summarize the number of bids that a bidder or a seller submitted in each auction. This variable is divided by the first (Nr bids FA) and second auction (Nr bids SA).

Table 2.8: Description of variables used in regression analysis continued

Variable	Description and construction of the variables
Nr of previous shill bidding SA	This variable represents the number of previous rounds in which the sellers submitted at least one shill bid in the second auction.
Period	Period takes the value of the current round that subjects are participating in. There are 20 periods in total.
Pr first bid SA	The probability that a subject submits the first bid in the second auction.
Private value	The private value is assigned randomly to each bidder at the beginning of each round from a uniform distribution of 0 to 100.
Seller	A dummy variable that takes the value of 1 if the subject is a seller and 0 otherwise.
Seller shill bids	Is a dummy variable that takes the value of 1 if the seller submits at least one shill bid in the second auction of that round and 0 otherwise.
Seller wins SA	This is a dummy variable that takes the value of 1 if the seller shill bids and wins the item and zero otherwise.
T. first bid	This variable records the timing, in seconds, that subjects submit their first bid in the second auction.
T1	A dummy variable that takes the value of 1 if the subject is in Treatment 1 in which the seller can shill bid and is given the complete bidding history from the first auction.
T2	A dummy variable that takes the value of 1 if the subject is in Treatment 2 in which the seller can shill bid, but is not given the complete bidding history from the first auction.
% of earlier wins FA (SA)	This is defined as the percentage of the auctions that a bidder or seller won in the previous rounds. There is one variable for the first auction (% of earlier wins FA) and one for the second auction (% of earlier wins SA).

2.5.2 Additional analysis and regression tables

Table 2.9 shows the theory driven variables and the data observed statistics. Here in the following, we will explain how we compute those theory driven numbers in details. The derived bidding functions in Equation 2.1 and 2.2 give rise to hypotheses regarding prices, efficiency and earnings 2.9. With buyer private values uniformly distributed between $(0, 1)$ it follows that the expected first, second and third order statistics of this distribution equals 0.8, 0.6 and 0.4 respectively. In the Baseline, it follows that the expected FA price is $\frac{2}{3} * 0.6 = 0.4$, which is equal to the expected SA price of 0.4. These are also the seller's expected earnings. As the bidders are assumed to follow $\beta_1(v_i)$ and $\beta_2(v_i)$ we expect full efficiency in the baseline in the sense that the buyer with the highest private value wins the FA and the buyer with the second-highest private value wins the SA. From this it follows that we expect the winning buyer's earnings to be 0.4 in the FA and 0.2 in the SA. Therefore, the average buyer earnings are 0.1 and 0.05 in the FA and SA respectively

In Treatment 2 it turns out that the seller submits a shill bid that makes her forego some profit from the SA. By submitting a shill bid of 0.5, Equation 2.34 and 2.29 in Appendix 2.5.1 give that the seller expects to earn 0.5 in the FA and 0.3875 in the SA. The expected price in the FA is 0.5, but the expected SA price is 0.5125. The reason for this is that the seller sometimes wins the SA and the expected price for this event is 0.125. We expect full efficiency in the FA of Treatment 2, but since there is a 12.5% probability that the seller wins the SA when submitting a shill bid of 0.5 we expect SA efficiency to be 87.5%. Finally, the winning buyer's earnings are expected to be 0.3 in the FA and 0.1375 in the SA, so average buyer earnings are predicted to be 0.075 in the FA and 0.0344 in the SA.

Table 2.9: Other theory driven variables vs data observed statistics

	Theory Driven			Data Observed		
	Baseline	Treatment 1	Treatment 2	Baseline	Treatment 1	Treatment 2
FA price	40	-	50	42.20	48.89	49.53
		-		[17.77]	[16.26]	[19.32]
SA price	40	-	51.25	33.90	43.02	43.65
		-		[18.06]	[18.19]	[19.42]
Ave buyer FA earnings	10	-	7.5	6.72	6.54	6.23
		-		[15.62]	[14.44]	[14.8]
Ave buyer SA earnings	5	-	3.44	7.46	5.08	4.77
		-		[17.3]	[13.29]	[13.32]
FA seller earnings	40	-	50	42.20	48.89	49.53
		-		[16.26]	[17.77]	[19.32]
SA seller earnings	40	-	38.75	33.90	38.64	37.33
		-		[18.19]	[21.64]	[23.55]
FA efficiency	100%	-	100%	54.17%	69.58%	70.83%
		-		[49.93]	[46.10]	[45.55]
SA efficiency	100%	-	87.5%	44.58%	51.67%	50.00%
		-		[49.81]	[50.08]	[50.10]

Notes: Entries are multiplied by 100 to match the outcomes in the experiment.

Buyer earnings refer to winning buyer's earnings.

We don't have theory for Treatment 1, so there is no theory driven for this.

Table 2.10: OLS regressions with FA and SA final prices as dependent variables

	(1)	(2)	(3)	(4)	(5)
	FA price	FA price	SA price	SA price	SA price
T1	6.688**	6.031**	9.117**	4.864	4.484
	[2.982]	[2.589]	[3.447]	[4.129]	[3.683]
T2	7.325**	7.430**	9.750***	2.364	4.140
	[3.443]	[3.115]	[3.282]	[4.517]	[3.068]
Seller shill bids * T1				5.969**	7.437***
				[2.684]	[1.791]
Seller shill bids * T2				9.634**	9.328***
				[3.662]	[1.177]
Period		0.239**			0.257**
		[0.0990]			[0.103]
Ave private value		0.821***			0.799***
		[0.0353]			[0.0446]
Constant	42.20***	-1.610	33.90***	33.90***	-3.955
	[2.096]	[3.012]	[2.669]	[2.673]	[2.608]
Observations	720	720	720	720	720
R^2	0.034	0.472	0.055	0.077	0.549
Clusters	18	18	18	18	18

Notes: Robust standard errors clustered on Group level in brackets

*** p<0.01, ** p<0.05, * p<0.1

Table 2.11: GLS random effects models with sellers' FA and SA payoff as dependent variable

	(1)	(2)	(3)	(4)	(5)	(6)
	FA payoff	FA payoff	SA payoff	SA payoff	SA payoff	SA payoff
T1	6.688**	4.790*	4.733	5.680	4.429	3.339
	[2.982]	[2.628]	[2.903]	[4.501]	[3.574]	[3.639]
T2	7.325**	6.156*	3.429	2.017	4.032	3.770
	[3.443]	[3.421]	[3.221]	[4.354]	[3.021]	[2.749]
Seller shill bids * T1				-1.329	0.203	4.588*
				[4.618]	[3.321]	[2.545]
Seller shill bids * T2				1.841	0.531	4.695***
				[3.031]	[2.002]	[1.528]
Ave private value		0.820***			0.892***	0.774***
		[0.0347]			[0.0444]	[0.0382]
Last period balance		0.0220**			0.0124	0.0159
		[0.00979]			[0.0138]	[0.0103]
Last period shill bid		-0.794			-0.0302	-0.321
		[1.988]			[2.434]	[1.286]
Nr bids SA					0.139	0.444
					[0.409]	[0.290]
% of earlier wins SA		8.642			-0.808	6.612
		[5.900]			[6.512]	[4.241]
Period		-0.637			-0.188	-0.361
		[0.406]			[0.546]	[0.426]
Seller wins SA						-36.50***
						[2.342]
Constant	42.20***	-2.197	33.90***	33.90***	-8.942***	-3.460*
	[2.096]	[2.911]	[2.669]	[2.673]	[2.294]	[2.066]
Observations	720	720	720	720	720	720
Number of Subject	36	36	36	36	36	36
Clusters	18	18	18	18	18	18

Notes: Robust standard errors clustered at Group level in brackets

*** p<0.01, ** p<0.05, * p<0.1

Table 2.12: GLS random effects models with buyers' FA and SA payoff as dependent variable

	(1)	(2)	(3)	(4)	(5)	(6)
	FA payoff	FA payoff	SA payoff	SA payoff	SA payoff	SA payoff
T1	-0.175	0.596	-2.386***	-1.186	-1.130	-0.624
	[0.443]	[0.836]	[0.780]	[1.160]	[0.784]	[1.218]
T2	-0.491	0.608	-2.690***	-1.232	-1.614*	-0.583
	[0.708]	[1.239]	[0.746]	[1.095]	[0.891]	[1.058]
Seller shill bids * T1					-1.764***	-0.966
					[0.537]	[0.735]
Seller shill bids * T2					-1.404*	-1.024
					[0.739]	[0.795]
Nr of previous shill bidding SA		-0.175		-0.133		-0.114
		[0.124]		[0.0983]		[0.104]
Private value		0.250***		0.191***		0.191***
		[0.0126]		[0.0150]		[0.0149]
Nr bids FA		-0.236**				
		[0.111]				
% of earlier wins FA		4.436***		-0.304		-0.319
		[1.468]		[1.753]		[1.755]
% of earlier wins SA		0.345		0.389		0.390
		[1.951]		[1.740]		[1.748]
Last period balance		-0.00772**		0.0137***		0.0136***
		[0.00311]		[0.00421]		[0.00429]
Period		0.188**		-0.146*		-0.151*
		[0.0943]		[0.0827]		[0.0828]
Nr bids SA				1.037***		1.031***
				[0.134]		[0.135]
Constant	6.717***	-6.445***	7.461***	-5.931***	7.461***	-5.817***
	[0.353]	[1.093]	[0.680]	[1.668]	[0.681]	[1.706]
Observations	2,880	2,880	2,880	2,880	2,880	2,880
Number of Subject	144	144	144	144	144	144
Clusters	18	18	18	18	18	18

Notes: Robust standard errors clustered at Group level in brackets

*** p<0.01, ** p<0.05, * p<0.1

Figure 2.5: Alternative measurement of efficiency

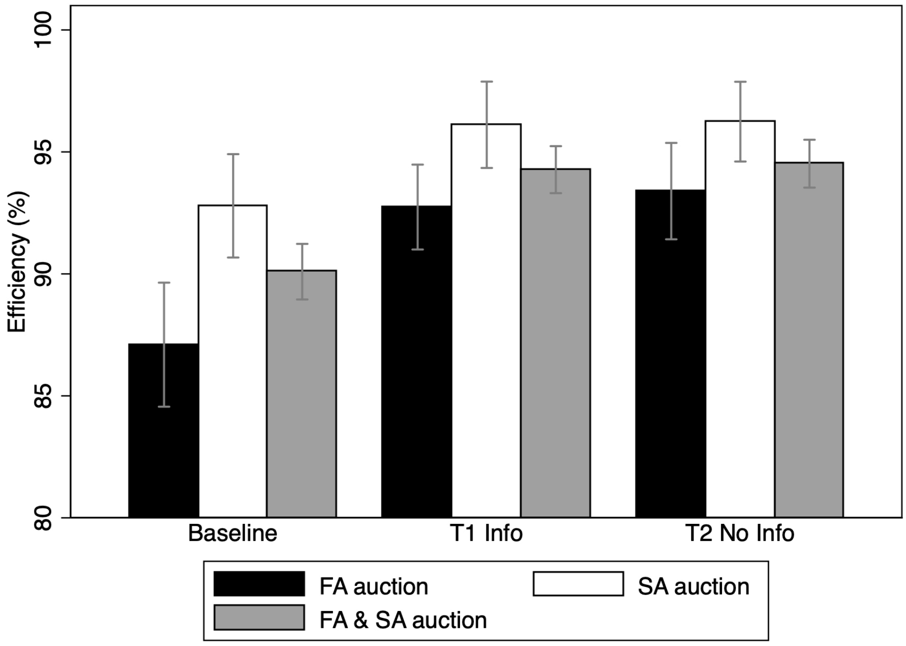


Table 2.13: GLS random effects regressions with buyers' FA final bids as dependent variable

	(1)	(2)	(3)	(4)
	Final FA bid	Final FA bid	Final FA bid	Final FA bid
T1	4.148*	1.625	-5.552**	-7.555**
	[2.294]	[2.179]	[2.630]	[3.386]
T2	4.144*	2.453	-5.123**	-7.436**
	[2.511]	[2.633]	[2.119]	[3.092]
Private value * T1			0.181***	0.184***
			[0.0612]	[0.0576]
Private value * T2			0.186***	0.199***
			[0.0528]	[0.0512]
Private value		0.662***	0.567***	0.533***
		[0.0328]	[0.0426]	[0.0378]
Nr of previous shill bidding SA		0.290		0.272
		[0.199]		[0.202]
Nr bids FA		1.288***		1.324***
		[0.188]		[0.198]
% of earlier wins FA		3.887*		4.160*
		[2.129]		[2.325]
% of earlier wins SA		3.258		2.848
		[2.054]		[2.043]
Last period balance		-0.0123		-0.0127
		[0.00863]		[0.00901]
Period		0.235		0.252
		[0.218]		[0.225]
Constant	35.72***	-2.849	7.223***	3.505
	[1.695]	[2.644]	[2.035]	[2.933]
Observations	2,880	2,880	2,880	2,880
Number of Subject	144	144	144	144
Clusters	18	18	18	18

Notes: Robust standard errors clustered at Group level in brackets

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 2.14: GLS random effects regressions with buyers' SA final bid as dependent variable

	(1)	(2)	(3)	(4)	(5)
	Final SA bid	Final SA bid	Final SA bid	Final SA bid	Final SA bid
T1	1.334	3.838	-4.433**	-2.247	-0.350
	[2.725]	[3.152]	[1.917]	[2.024]	[2.464]
T2	0.702	4.124	-3.893**	-1.675	2.299
	[3.038]	[3.368]	[1.693]	[1.964]	[2.131]
Seller shill bids * T1					-2.870
					[2.706]
Seller shill bids * T2					-5.519**
					[2.215]
Seller shill bids * Private value * T1					0.0856
					[0.0698]
Seller shill bids * Private value * T2					0.142***
					[0.0507]
Private value * T1			0.147*	0.138*	0.0789
			[0.0859]	[0.0820]	[0.0956]
Private value * T2			0.138	0.132	0.0259
			[0.0886]	[0.0879]	[0.0958]
Private value		0.784***	0.711***	0.697***	0.697***
		[0.0375]	[0.0778]	[0.0765]	[0.0767]
Nr of previous shill bidding SA		-0.167		-0.154	-0.137
		[0.168]		[0.161]	[0.159]
Nr bids SA		1.198***		1.172***	1.164***
		[0.238]		[0.228]	[0.228]
% of earlier wins FA		-0.205		-0.0454	0.129
		[2.253]		[2.294]	[2.268]
% of earlier wins SA		0.495		0.391	0.303
		[2.631]		[2.563]	[2.526]
Last period balance		0.0214***		0.0214***	0.0214***
		[0.00535]		[0.00561]	[0.00551]
Period		0.0969		0.0896	0.0761
		[0.123]		[0.121]	[0.119]
Constant	36.10***	-7.536***	5.112***	-3.584*	-3.459*
	[2.348]	[2.578]	[1.503]	[1.967]	[1.957]
Observations	2,164	2,164	2,164	2,164	2,164
Number of Subject	144	144	144	144	144
Clusters	18	18	18	18	18

Notes: Robust standard errors clustered at Group level in brackets

*** p<0.01, ** p<0.05, * p<0.1

Table 2.15: Probit regressions with the buyer's probability of submitting a late final SA bid in the last 10 and 5 seconds as dependent variable

	(1)	(2)	(3)	(4)
	SA 50	SA 50	SA 55	SA 55
T1	-0.220	-0.0232	-0.249	-0.0317
	[0.191]	[0.284]	[0.153]	[0.251]
T2	-0.352*	-0.245	-0.372**	-0.179
	[0.206]	[0.302]	[0.169]	[0.304]
Seller shill bids * T1		-0.360**		-0.313***
		[0.162]		[0.111]
Seller shill bids * T2		-0.213		-0.242
		[0.137]		[0.171]
Nr of previous shill bidding SA		0.0141		0.00346
		[0.0196]		[0.0216]
Private value		0.0190***		0.0142***
		[0.00170]		[0.00143]
Nr bids SA		0.145***		0.0958***
		[0.0237]		[0.0198]
% of earlier wins FA		-0.150		-0.0558
		[0.270]		[0.264]
% of earlier wins SA		-0.0128		0.0199
		[0.199]		[0.221]
Last period balance		0.00162**		0.00118*
		[0.000667]		[0.000665]
Period		-0.00929		0.00255
		[0.0170]		[0.0174]
Constant	0.383**	-1.009***	0.0853	-1.083***
	[0.179]	[0.225]	[0.137]	[0.183]
Observations	2,164	2,164	2,164	2,164
Clusters	18	18	18	18

Notes: Robust standard errors clustered at Group level in brackets

*** p<0.01, ** p<0.05, * p<0.1

Table 2.16: Probit regressions on the probability of submitting a late final FA bid in the last 10 and 5 seconds as dependent variable for buyers

	(1)	(2)	(3)	(4)
	FA 50	FA 50	FA 55	FA 55
T1	0.169	0.0700	0.126	-0.000638
	[0.170]	[0.221]	[0.173]	[0.216]
T2	-0.0316	-0.134	-0.0555	-0.181
	[0.174]	[0.225]	[0.185]	[0.224]
Nr of previous shill bidding SA		0.0197		0.0208
		[0.0158]		[0.0167]
Private value		0.0165***		0.0143***
		[0.00130]		[0.00123]
Nr bids FA		0.136***		0.0772***
		[0.0350]		[0.0226]
% of earlier wins FA		-0.131		0.0666
		[0.219]		[0.196]
% of earlier wins SA		-0.0833		-0.00911
		[0.243]		[0.224]
Last period balance		-0.000166		8.76e-05
		[0.000939]		[0.000880]
Period		-0.00171		-0.00971
		[0.0170]		[0.0167]
Constant	0.152	-0.962***	-0.131	-1.039***
	[0.162]	[0.182]	[0.166]	[0.206]
Observations	2,880	2,880	2,880	2,880
Clusters	18	18	18	18

Notes: Robust standard errors clustered at Group level in brackets

*** p<0.01, ** p<0.05, * p<0.1

Table 2.17: Probit regression on share of overbid in FA and SA auction for buyers

	(1)	(2)
	overbidFA	overbidSA
T1	-0.324***	-0.191*
	(-3.84)	(-2.31)
T2	0.0178	0.0419
	(0.23)	(0.54)
Private value	-0.0104***	-0.0130***
	(-8.69)	(-10.51)
Nr bids FA	0.0399***	
	(3.43)	
Nr bids SA		0.100***
		(8.22)
% of wins FA in the past	0.0124	-0.0200
	(0.07)	(-0.12)
Last period balance	-0.000439	0.000354
	(-1.30)	(1.09)
constant	-0.737***	-0.905***
	(-6.70)	(-8.14)
Observations	2879	2879

Notes: t statistics in parentheses

*** p<0.001, ** p<0.01, * p<0.05

Table 2.18: Probit regression on sellers' shill bid

	(1)	(2)	(3)	(4)
	P. Shill bid (Within T1)			P. Shill bid (T1 vs T2)
FA final price	-0.00377	-0.00454	-0.00438	
	[-0.79]	[-0.95]	[-0.90]	
Accumulated Earning		0.000449	0.000474	0.000369
		[1.36]	[1.25]	[1.26]
P.Won Past Rounds			-0.321	0.641
			[-0.26]	[0.94]
T1				-0.147
				[-0.58]
Constant	0.759***	0.562**	0.571***	0.466
	[3.40]	[3.24]	[2.78]	[1.71]
Observation N	240	240	240	240

Notes: Robust standard errors clustered at Group level in brackets

*** p<0.01, ** p<0.05, * p<0.1

Table 2.19: Probit regressions with probability of submitting a late bid and of submitting the first bid as dependent variables for both buyers and sellers

	(1)	(2)	(3)	(4)	(5)	(6)
	SA 50	SA 50	SA 55	SA 55	Pr first bid SA	Pr first bid SA
Buyer * T1	0.132	0.119	0.124	0.116	-0.145**	-0.165**
	[0.125]	[0.148]	[0.123]	[0.144]	[0.0706]	[0.0797]
Seller * T1	-0.163	-0.693**	-0.388*	-0.691***	0.951***	0.471**
	[0.228]	[0.303]	[0.216]	[0.258]	[0.201]	[0.202]
Seller * T2	-0.293	-0.688***	-0.551***	-0.768***	0.513**	0.143
	[0.200]	[0.226]	[0.163]	[0.220]	[0.239]	[0.242]
Ave private value		0.00942***		0.00519*		-0.000915
		[0.00207]		[0.00270]		[0.00131]
Period		0.0115		0.0124		-0.0275**
		[0.00792]		[0.0127]		[0.0117]
Last period balance		0.000493		0.000264		0.000714
		[0.000329]		[0.000519]		[0.000521]
Nr bids SA		0.177***		0.0971***		0.155***
		[0.0283]		[0.0182]		[0.0232]
Constant	0.0311	-1.006***	-0.287***	-0.938***	-0.540***	-0.763***
	[0.104]	[0.162]	[0.101]	[0.144]	[0.0399]	[0.0565]
Observations	1,799	1,799	1,799	1,799	1,424	1,424
Clusters	12	12	12	12	12	12

Notes: Robust standard errors clustered at Group level in brackets

*** p<0.01, ** p<0.05, * p<0.1

Figure 2.6: Buyers' overbidding behavior across treatments

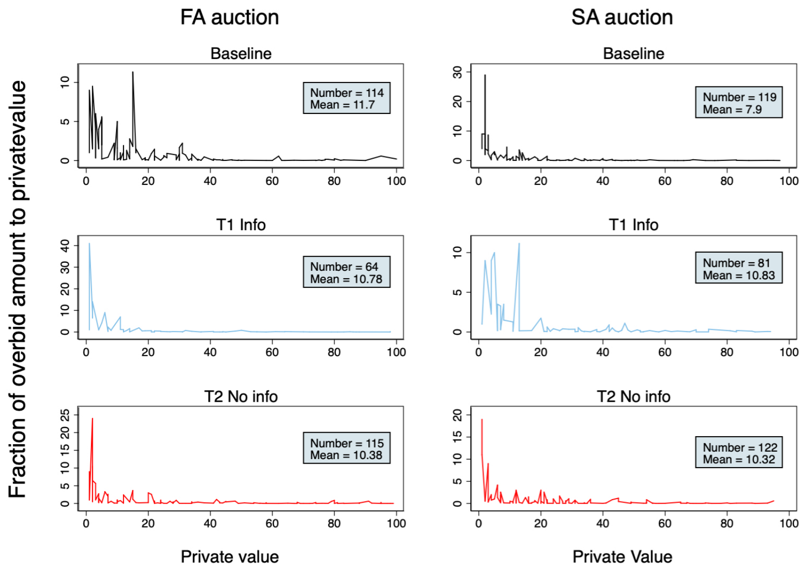


Table 2.20: GLS random effects models with Number of bids, Final bid, magnitude of first bid and timing of first bid for both buyers and sellers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Nr bids SA	Nr bids SA	Final SA bid	Final SA bid	First bid SA	First bid SA	T.first bid	T. first bid
Buyer * T1	0.0855	0.0686	0.630	-0.328	-0.728	-1.040	0.333	0.315
	[0.316]	[0.314]	[2.411]	[2.158]	[2.810]	[2.359]	[2.290]	[1.946]
Seller * T1	1.915***	1.522***	-0.151	-4.306	-6.560***	0.0514	-16.56***	-14.09***
	[0.196]	[0.129]	[3.446]	[2.999]	[2.261]	[2.154]	[1.412]	[1.233]
Seller * T2	1.245***	0.835**	-2.709	-5.542	-7.738***	-3.323	-14.41***	-12.85***
	[0.416]	[0.375]	[4.105]	[4.495]	[2.144]	[2.728]	[1.433]	[2.142]
Ave private value		0.0148***		0.680***		0.435***		0.111***
		[0.00480]		[0.0269]		[0.0443]		[0.0368]
Period		-0.0336***	0.116	0.230		0.415***		0.515***
		[0.0123]	[0.102]	[0.184]		[0.0992]		[0.109]
Last period balance		0.00150***		-0.00290		-0.00619		0.000743
		[0.000559]		[0.00967]		[0.00473]		[0.00459]
Nr bids SA				2.723***		-2.245***		-1.505***
				[0.330]		[0.344]		[0.268]
Constant	2.397***	1.805***	35.58***	-0.168	21.22***	5.297***	22.04***	15.41***
	[0.220]	[0.220]	[2.237]	[2.328]	[2.668]	[1.790]	[1.803]	[2.689]
Observations	1,799	1,799	1,799	1,799	1,637	1,637	1,637	1,637
Number of Subject	120	120	120	120	120	120	120	120
Clusters	12	12	12	12	12	12	12	12

Notes: Robust standard errors clustered at Group level in brackets

*** p<0.01, ** p<0.05, * p<0.1

Figure 2.7: Cumulative distributions of buyers' submitted final bids over time in first and second auctions

(a) First auction (b) Second auction

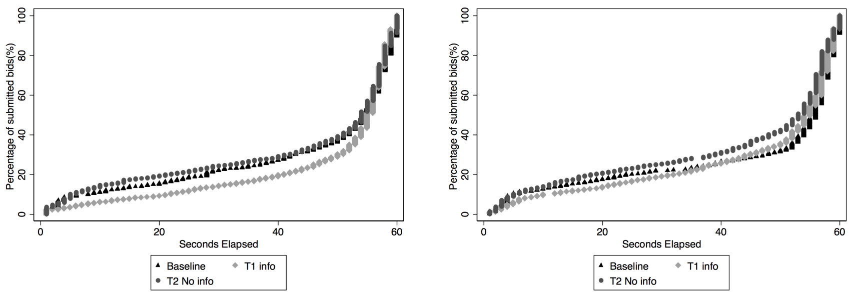
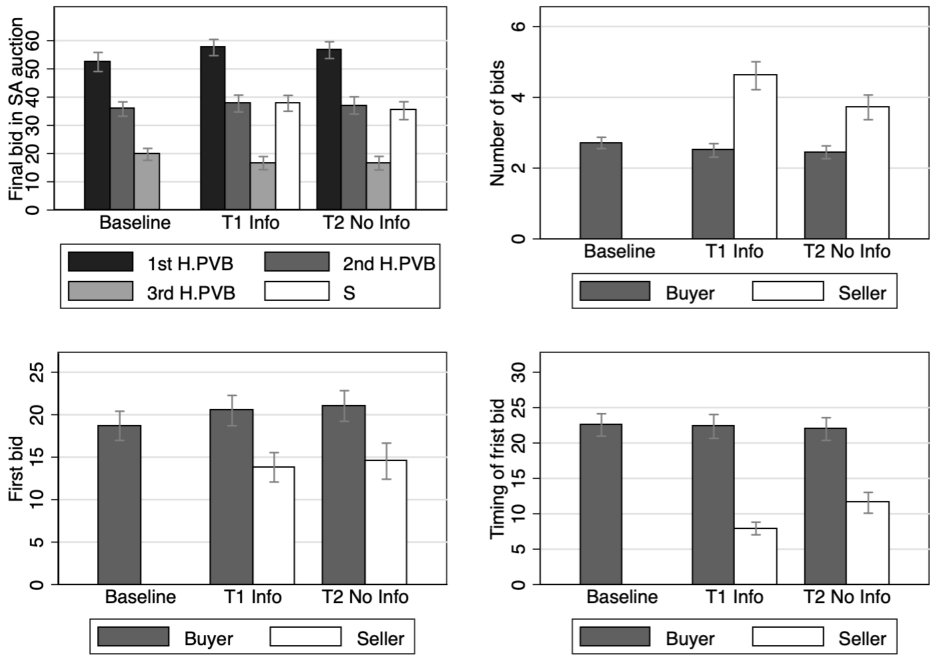


Figure 2.8: Buyers and sellers behavior comparing (including baseline)



2.5.3 Instructions for the experiment

These instructions are for the all the treatments together and have been translated from Spanish .

I N S T R U C T I O N S

Welcome to the experiment! The purpose of this experiment is to study how individuals make decisions in a certain context. The experiment consists of several rounds of auctions. The instructions are simple and if you follow them carefully you will at the end of the session earn money. The amount of money you earn depends on the decisions you and others make in the experiment. If you have any questions, please do not hesitate to ask us by raising your hand. Apart from these questions, any kind of communication is prohibited and may lead to your immediate exclusion from the experiment.

Overview:

In this experiment you will make decisions in a number of auctions. At the beginning of the experiment you will be randomly assigned a role as either a bidder or a seller. You will keep the same role throughout the whole experiment.

The experiment consists of 20 rounds and in each round you will participate in two auctions. At the beginning of each round, you are randomly matched in a group of five participants including one seller and four bidders. In each round, the seller has two identical items which will be sold in two separated auctions. The two auctions will be conducted sequentially meaning that the second auction starts when the first auction is finished. Bidders are only allowed to buy one item in each round. Therefore, the bidder who wins the first auction will not participate in the second auction of that round. However, once a new round starts they can bid again.

At the beginning of every round, each bidder will be given a private value which is how much the bidder values the two items that are to be sold in this round. This private value will be an integer between 0 and 100 which is randomly picked by a computer. Any integer in this interval is equally likely to be given to a bidder. A bidder's private value is always displayed on the bidder's screen. Furthermore, a bidder's private value is not known by any other participants in the experiment.

Once a round is completed and before the start of a new round, you will be randomly re-matched into a new group of one seller and four bidders, while maintaining your role. Moreover, the bidders will be given a new private value. Given that no participant will be given any identity number, all the actions that you take during the experiment will be absolutely anonymous.

Each participant receives a 5 euros show up fee. The additional payment depends on the outcome of the auctions. All participants start with a balance of 100 Experimental Currency Unit (ECU) to which gains and losses will be added and subtracted during the course of the experiment.

The auction environment:

To Buyers:

Each round consists of two auctions and before the start of the first auction, bidders will be shown their private value on the screen. In order to proceed to the first auction, all participants must press the "Continue" button.

In each auction one item is to be sold by the seller. An auction lasts 60 seconds during which it is possible for you to submit bids for the item. A bid is submitted by writing an integer number in the "Your bid" box followed by clicking the "Make bid" button.The winner of the item is the bidder who has submitted the highest bid before the auction ends. In case several bidders submit the same highest bid, the bidder who submitted the bid first will win the item. However, the winner only needs to pay the second-highest bid which was submitted by one of the other bidders before the auction ended. The final second-highest bid is thus the price of the item. A bidder who wins an auction will receive an amount of ECUs equal to his/her private value minus the price of the item. A bidder who does not win an auction gets 0 ECU.

You can start bidding and/or react to other bidders bids at any time during the 60 seconds.If you submit a bid which at that moment is the highest bid, the words "Winning" appears on the screen. Otherwise the words "Not winning" will be shown. Furthermore, the highest bid will never be shown on the screen of any participant. However, when some other bidder submits a higher bid, then the old highest bid becomes the second-highest bid and is displayed on the screen as the current price.

Furthermore, you are allowed to submit any number of bids. However, there are two restrictions:

1) Any submitted bid must be greater than the current price. At the beginning of the auction, the current price is set to 1 (ECU). During the auction, the current price will be updated in accordance with the submitted bids and it will always be shown on the screen. Remember, the final price of the item is determined by the final second-highest bid.

2) Any submitted bid of yours must be greater than the your previously submitted bids.

During an auction the following information will be displayed on a bidder's screen: Your private value, the time left before the auction ends, if you are the current winner of the item or not, the current price and a history of all bids which were the current price at some point during the auction. In the history of prices, no information regarding the ID of the bidder who submitted the bid will be displayed. Furthermore, the history of current prices is updated as new second-highest bids are submitted.

After the end of the first auction the bidders are informed of their payoff in ECUs and whether they will be able to bid in the second auction or not. Only the bidder who wins the item in the first auction will not be able to bid in the second auction. In order to proceed to the second auction, all participants must press the "Continue" button.

Payoff: After the second auction is finished, all participants will be displayed their payoff from the current round and their updated balance in ECUs. Note that a bidder may lose money by submitting a bid which is higher than his/her private value. If a bidder has lost money in a round, a message warning the bidders will appear. When all participants have pressed the "Continue" button, a new round starts unless you have completed all 20 rounds. When the 20 rounds are finished, the accumulated ECUs from all auctions will be converted into euros at a rate of 100 ECU = 2 euros for the bidders. If a bidder has a negative balance when the 20 rounds are finished, then the bidder will only earn the show up fee.

To Sellers:

Baseline: Sellers have no active part in the auction. During the first auction, the seller will be shown a blank screen. Between the first and the second auction, the seller will be shown the complete history of all the current prices from the recently finished first auction of the same round. During the second auction, the seller will be able to view the auction live. Specifically, the current price and the constantly updating history of current prices will be shown to the seller.

Payoff: The seller's payoff is only displayed once the second auction is finished. The seller gets a payoff in ECUs which is equal to the final price in one of the two auctions. A random draw from the computer, with 50 % probability assigned to each of the two auctions, determines which final price is paid out to the seller.

Treatment 1: During the first auction, the seller will be shown a blank screen. Between the first and the second auction, the seller will be shown the complete history of all the current prices from the recently finished first auction of the same round. Before the second auction starts, the seller can choose to join the second auction, which gives him/her the possibility to bid on his/her own item. Joining the auction costs 1 ECU and the seller will then be presented with the same screen as the bidders where the seller can submit bids. The rules for bidding are the same for the seller as for the bidders. If the seller does not join the second auction, the seller will be able to view the second auction live. This includes the information of the current price and the constantly updating history of current prices. The bidders are not told whether the seller decided to join the auction or not.

Payoff: The seller's payoff is only displayed once the second auction is finished. The seller gets a payoff in ECUs which is equal to the final price in one of the two auctions. A random draw from the computer, with 50 % probability assigned to each of the two auctions, determines which price is paid out to the seller. Hence, only one of the two prices is paid out to the seller in each round. Moreover, if the seller decided to join the second auction, 1 ECU is deducted from the payoff. Furthermore, if the seller wins the auction by bidding on his own item, he/she gets a payoff of -1 as the cost of joining the auction is paid.

Treatment 2: During the first auction the seller will be shown a blank screen. Between the first and the second auction, the seller will be shown a blank screen as well. Before the second auction starts, the seller can choose to join the second auction, which gives him/her the possibility to bid on his/her own item. Joining the auction costs 1 ECU and the seller will then be presented with the same screen as the bidders where the seller can submit bids. The rules for bidding are the same for the seller as for the bidders. If the seller does not join the second auction, the seller will be able to view the second auction live. This includes the information of the current price and the constantly updating history of prices. The bidders are not told whether the seller decided to join the auction or not.

Payoff: The seller's payoff is only displayed once the second auction is finished. The seller gets a payoff in ECUs which is equal to the final price in one of the two auctions. A random draw from the computer, with 50 % probability assigned to each of the two auctions, determines which final price is paid out to the seller. Hence, only one of the two prices is paid out to the seller in each round. Moreover, if the seller decided to join the second auction, 1 ECU is deducted from the payoff. Furthermore, if the seller wins the auction by bidding on his own item, he/she gets a payoff of -1 as the cost of joining the auction is paid.

After the second auction is finished, all participants will be displayed their payoff from the current round and their updated balance in ECUs. When the 20 rounds are finished, the accumulated ECUs from all auctions will be converted into euros at a rate of 100 ECU = 1 euro for the sellers. You need to fill the blank in the receipt paper on your table and sign it. An experiment assistant who has your payment information will give your earnings in cash after your filled out receipt has been turned in to him/her.

Good luck!

Example

Before starting the experiment, please go over an example of one round to ensure that you understand how your payment is determined. Assume that the winner of the first auction has a private value 90. We will now consider three different final prices in order to illustrate how these affects the winner's payoff. Assume that the three different final second-highest bids (prices) submitted are 20, 50 and 70. The earnings for the winning bidder in this round, by varying second-highest bid (SHB), are as shown in the table below:

Private Vvalue	*SHB*	*First*	*Second*	*Earnings in this round*
90	20	$90 - 20 = 70$	0	70
90	50	$90 - 50 = 40$	0	40
90	70	$90 - 70 = 20$	0	20

Notice that the winning bidder in the first auction earns 0 in the second auction since he/she is not allowed to participate in it.

Then another bidder wins the second auction and assume that this bidder's private value is 84. Once again we consider the same three different second-highest bids (prices) of 20, 50 and 70. So the earning for this bidder in this round by varying the second-highest bid (SHB) are like the table below:

Private Vvalue	*SHB*	*First*	*Second*	*Earnings in this round*
84	20	0	$84 - 20 = 64$	64
84	50	0	$84 - 50 = 34$	34
84	70	0	$84 - 70 = 14$	14

Notice that since the bidder did not win the first auction, he/she gets 0 from first auction.

Now lets look at the earnings of the seller in this round. Since the computer will randomly pick a final price in one of the two auctions in this round to be paid out to the seller, the seller either get the final SHB of the First auction or the final SHB of the second auction. Hence, the seller earns on average more if the items in both auctions are sold by a high final price.

2.5.4 Comprehension test and experimental screenshots and post-experiment questionnaire

Comprehension Test

Q1. Suppose that you are a bidder and that your private value is 60. Moreover, you bid 55 and win the second auction and the current second-highest bid is 45. What is your earnings in this auction?

Q2. What is the seller's earnings if the outcome of the auction in question 1. is paid out?

Q3. Suppose that the seller joins the second auction in question 1 and submits a bid equal to 53 and that this bid becomes the final second-highest bid. If this auction is paid out, how much does the seller earn?

Q4. Before starting the second auction, the seller will see the bidders' bidding history from the first auction of the same round. (True=1 or False=0)\

Q5. The seller can affect the final price of an item in the first auction by bidding. (True=1 or False =0)

Q6. The seller can affect the final price of an item in the second auction by bidding. (True=1 or False=0)

Q7. I will face the same bidders and sellers in all the rounds. (True=1 or False=0)

Q8. It is only possible to bid once in an auction. (True=1 or False=0)

Q9. How many bidders will participate in the first auction?

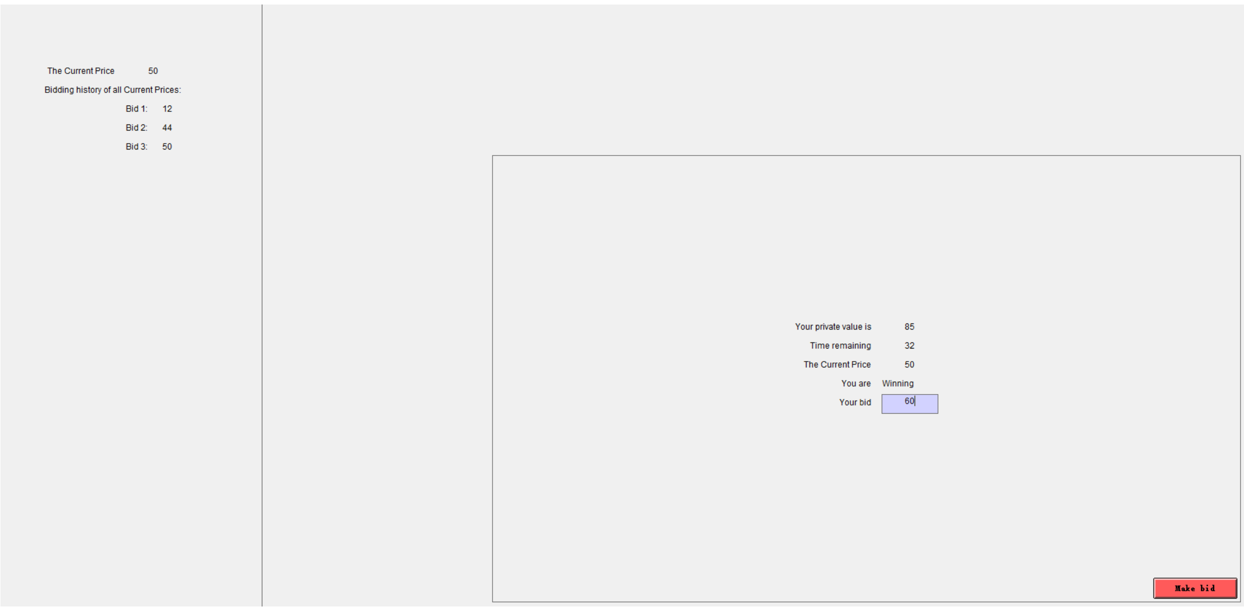
Q10. If the first item is sold, how many bidders will participate in the second auction?

Q11. How many auctions are there in each round?

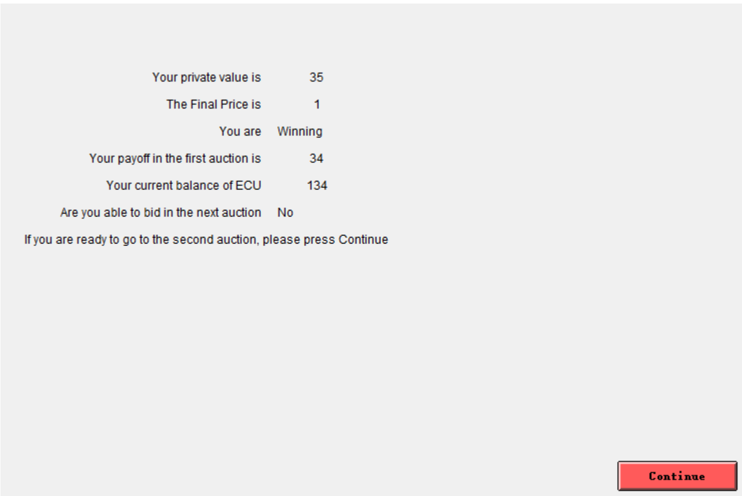
Q12. What is the total number of auctions that you will participate in during the entire experiment?

The second part of this appendix reports the main screenshots used during the experiment. The screen have been translated from Spanish to English.

Screen 1 : Bidding Screen

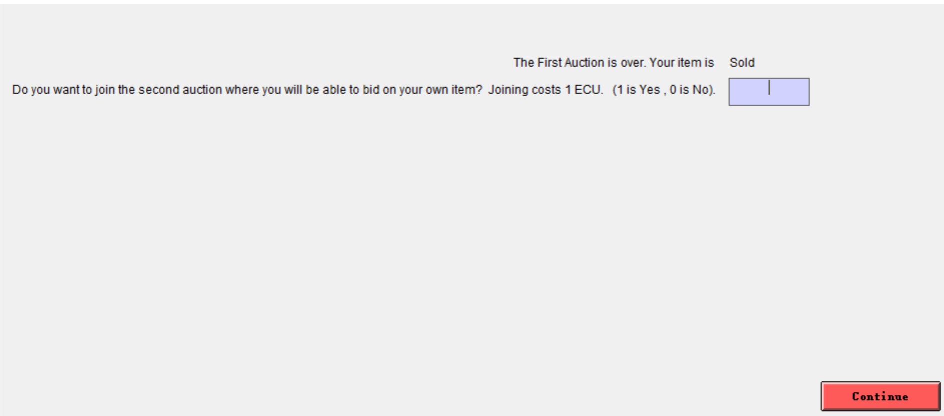


Screen 2 : Information stage for bidders



Screen 3: Information stage for sellers in Full information Treatment 1

Screen 4: Information stage for sellers in No Information Treatment 2



Screen 5 : Post-questionnaire stage

Chapter 3

Giving Decision of Time and Money: A Laboratory Experiment

Abstract[1]

When studying the preference for giving, most researchers have centered on giving money. However, time, as the other principal resource, has been understudied. Existing evidence from psychology suggests that when people are focused on time, they behave in a less self-interested way than when they are focused on money. We compare giving time and giving money by measuring crowd-out at different levels and across different resources of others' giving. Using a within-subject experiment, I find that, consistent with the psychologists' evidence, the generosity level of giving time is significantly higher that of giving money. On average, time giving is sensitive to both others' time giving and money giving while money giving does not respond much to others' time giving

[1]I am thankful for the constructive comments received from Jordi Brandts, James Andreoni, Lise Vesterlund, Jim Ingebretsen Carlson, Maja Adena, Pedro Rey Biel and seminar participants at Universitat Autònoma de Barcelona, IMEBESS 2019 in Netherlands and SERC conference 2019 in Singapore. I also thank Adrià Bronchal and Yulianna Valencia for helping us in conducting the experimental sessions. I gratefully acknowledge financial support from the Spanish Ministry of Economics and Competitiveness through Grant: ECO2017-88130 and through the Severo Ochoa Program for Centers of Excellence in R&D (SEV2015-0563), the Generalitat de Catalunya (Grant: 2017 SGR 1136).

3.1 Introduction

People constantly engage in activities that are costly to themselves but benefit others. In the year of 2016, money donations amounted to a value of more than 390 billion dollars in just the U.S. To understand the motives of why and how people make giving decisions is quite essential, especially for the optimal designs of public policy and fundraising mechanisms. In previous studies, the motives for giving have been explained in several ways, including altruism, fairness, and inequality aversion, etc. (see, e.g., Fehr and K. M. Schmidt (1999) Andreoni and Miller (2002); Bénabou and Tirole (2006) and Ellingsen and Johannesson (2009)). Most of these studies examine the motives to share money with others, often studying behavior in laboratory dictator games or natural settings like charitable giving. Focusing on sharing money is understandable, as monetary donations are easily measurable and more economically efficient since the opportunity cost of giving the same value of money is equal or less than that of giving the same value of time.[2]

While sharing money constitutes an appropriate focus for economic research on the motives of giving, it is surprising that alternative resources of costly giving behavior have been relatively neglected in economics, like time.[3] Money and time are both principal resources that individuals encounter daily, as they continuously manage how to spend and save their dollars and hours. Time giving is also highly related to moral labor markets, which recently have attracted much attention. However, whether or not the motives for giving time, especially for leisure time, to help others are the same as the motives for giving money has not been fully studied. Most researchers and policymakers do not treat them differently. By ignoring this possible difference, public policies and fundraising mechanisms that mix these two resources may be inefficient.

Previous studies from psychology find that people react to time and money differently (see, e.g., Zauberman and Lynch (2005) Sanford E DeVoe and Pfeffer (2007) Sanford E DeVoe (2019) Mogilner (2010) J. L. Aaker, Rudd, and Mogilner (2011) and Whillans and Dunn (2015)). Notably, Gino and Mogilner (2014) suggests that priming subjects think about money makes them less moral than priming them to think about time. Because merely thinking about money leads people to be less helpful and fair in their

[2]Based on the paper by Andreoni (1990), they assume that there is no intrinsic preference for giving time and giving money and the imputed wage of giving time is equal or less than the wage of their professional work, because if not, you would not stay in your current professional work in the equilibrium. Then the opportunity cost of giving the same value of money is equal or less than giving the same value of time, so people would prefer always to give money to gain the same level of utility because it is more efficient than giving the same level of time.

[3]There are other resources related to giving behaviors, like giving your life to save others, organ donations, etc. However, these resources are relatively rare on a daily basis.

dealing with others since they are more self-interested and focusing on calculating the balance of their decision's cost and gain towards optimizing their economic profit. While time is equally ubiquitous in daily lives, it tends to absorb less attention. For instance, When people focus on time, they make their decisions based more on reflecting their personalities like what kind of person they want to be, instead of only optimizing their economic profit. This evidence from psychology suggests that economic behavior related to time, including giving decisions, is motivated differently and less self-interest-driven than money.

As for the motives for giving, the recent study by Ottoni-Wilhelm, Vesterlund, and Xie (2017) measures crowd-out at different output levels to identify whether giving is motivated by altruism and/or warm-glow. The altruism theory they use was proposed by Andreoni (1990) with the extension to charitable giving to analyze people's preference for giving. Pure altruism included in the theory assumes that the sole motive for giving is the utility derived from the output of charity. This altruistic motive implies that gifts are only valued because they increase the total output of charity. Giving, driven by pure altruism, respond sensitively to other's giving to the same charity, which produces an extreme prediction that one unit from others added to increase the output of charity decrease the donor's contribution unit-for-unit. While, impure altruism implies that in addition to getting utility from the increased total output of charity, donors also get warm-glow from the act of giving itself. Warm-glow is a more private benefit to donors like the happiness of giving, which may correlate with an individual's morality, personality, or belief, etc. The implication is that giving, driven by warm-glow, will not respond to others' giving. Through measuring crowd-out, Ottoni-Wilhelm, Vesterlund, and Xie (2017) focus on the preference for giving money and present the first empirical evidence that, consistent with impure altruism, crowd-out decreases with output. Our project will demonstrate that these findings for giving money may not be well suited for giving time.

The methodology of measuring crowd-out to test their intrinsic preference of giving creates a clean environment of decision-makers. Most previous papers using crowding out effect to study giving decisions focus on money donations crowded out by others' money donations. However, others' giving in daily lives mixes with time and money. For example, when a severe earthquake happened in SiChuan, China, in 2008, the public transfer from the government to the victims included both direct money transfer and workers hired by the government to help them build up new homes. Moreover, this information reached many people in China; Organizations like charities and NGOs often publish their funding information online including both money donated and volunteer time given; For a more routine example, when your neighbors or colleagues ask help from you, it would typically include both money help and time help. At the same time, you may know if there is someone else also helping or the leisure time and money this person has spent since you are from the same community.

This project concentrates on understanding the difference between the pure preference for giving time and the pure preference for giving money. The charitable giving environment is sophisticated to identify this preference. Because it usually includes the reputations/purposes of charities and donation receiver's type like needy children, victims of the earthquake and so on., We introduce an experiment of a simple giving game between one donor and one recipient, both played by the students from the same pool, to measure crowd-out at different output levels and across different resources of others' giving. To approach our research purpose, we start by studying the overall generosity levels in giving time and giving money. Then we move to the question of how giving time and giving money are crowded out by others' giving differently? However, the crowd-out we measured here is the crowd-out by others plus crowd-out across resources. To identify the effect of crowd-out across resources, we then check how people's giving is crowded out by their previous giving.[4] By answering all these three questions, we hope to understand the intrinsic preference for giving time and giving money in more details and perspectives.

In our laboratory experiment, each subject is assigned a role as either a donor or a recipient. One donor is randomly paired with one recipient to play a dictator game.[5] Donor subjects are endowed with some Money and Time to allocate between their paired recipient and themselves, while recipient subjects do not have such endowments.[6] In the first stage of the experiment, all donors can make five giving decisions, including the combinations of two resources of others' giving (Money and Time) and two levels of others' giving (low and high) for each giving resource (Money and Time). The actual giving from donors to their recipient in this stage will be randomly picked from these final ten decisions that they made, either for Time giving or Money giving. The second stage of the experiment is a surprise stage about which participants will be informed of

[4]Since we are more interested in this interacted effect between giving time and giving money, we focus more on the identification of crowd-out across resources by your giving instead of the crowd-out by your giving within one resource. Furthermore, Brown, Meer, and Williams (2018), Breman (2011), and Andreoni, Rao, and Trachtman (2011)already test a similar effect as the crowd-out by your giving within resource in different ways.

[5]In the field, it would be hard to control the time giving. The time giving decisions they make highly correlates with the work type, the social connection you are with the person in need, and different believes about the exchange rate between time and money for each individual. The complicated environment contributes to the difficulties of singling out the pure preference of giving time. The Lab experiment would be a proper methodology for this.

[6]We will explain the specific money and time used in our experiment as Money and Time using this capital letter to differentiate with the general time and money.

the details once they have finished the first stage. Donors make decisions about their willingness to give complementary Time(Money) to the same paired recipient to whom they already gave some Money(Time). They can choose to enter this stage to make a giving decision or just skip this stage. Two incentivized post-experiment surveys are then provided to all of the participants. Once they have finished their surveys, all of them enter into a fixed 30 minutes Time period where the specific activity they are involved in depends on their role and their Time resource. Time in our experiment is the free time that subjects can surf online/read books but remaining seated quietly during this Time period. If recipients do not have any Time given, they will have to do a tedious and annoying "Constrained Time" task for all the 30 minutes, which consists in clicking on a warning message every 10 seconds.[7] It means that if donors choose to give an amount of Time to their recipient, they need to click the same amount of "Constrained Time" for their recipient. Meanwhile, their recipient can enjoy the same amount of Time given by their donor instead of doing that amount of "Constrained Time" of clicking. Money in our experiment is the cash that they will receive when leaving the Lab.

The result from the experiment suggests that the generosity level of giving time is significantly higher than that of giving money. We also find that, on average, time giving is sensitive to both others' time giving and money giving while money giving does not respond much to others' time giving. Finally, we observe that complementary time giving to their previous money giving can raise more funds from donors.

The outline of this paper is as follows: Section 4.1.2 presents the literature review related to our study. Section 3.3 explains the theory behind the crowding out effect. Section 3.4 shows the experimental design and the hypotheses. The results from the experiment are presented and discussed in Section 3.5. Finally, Section 3.6 concludes and discusses the results of the paper.

3.2 Literature review

The motives of pro-social behavior have been explained as altruism, fairness, social reputation, and inequity aversion in the previous literature. None of these motives can invalidate another, and each of them captures some critical aspects of individual behavior. Considering the pure and intrinsic preference of giving with altruism motives

[7]To decrease their possible preference for one specific work, we choose this waiting time in the Lab. Clicking for every 10 seconds lets them feel more annoying to be differentiated with the Time, which is a kind of freedom. Furthermore, this clicking helps them to count the time.

is a clean and straightforward environment without social concern. However, most of them did not isolate the motives for time giving from the motives for money giving. For our paper, we focus on altruism as the primary reason for giving and analyze the difference between giving money and giving time to a stranger. Andreoni (1990) lays out a model with the extent of charitable giving, in which an agent can not just gain utility from helping to increase the output of charity but also from the act of donating itself, so-called "warm-glow ". This model explained that the donating behavior is not just pure altruism driven but also gaining happiness itself. Fehr and K. M. Schmidt (1999) assume that in addition to purely self-interested people, there is a fraction of them motivated also by fairness considerations. The fairness in their model is self-centered inequity aversion, which means that people resist inequitable outcomes. Bénabou and Tirole (2006) develop a theory of prosocial behavior that combines heterogeneity in individual altruism and greed with social reputation concerns. They show that the extrinsic incentives of rewards and punishments can induce the "overjustif--ication effect". They check whether or not stigma or honor is the dominant reputational concern and the socially optimal level of incentives (external). These paper and their derived other studies summarized the fundamental motives of giving behavior, most of them including both money and volunteer hours. Altruism and fairness is more self-related intrinsic preference with different definition and the social reputation is an extrinsic incentive. Since this paper wants to understand the intrinsic preference of giving, we would mainly focus on altruism-related motives.

In terms of studying the difference between time and money, the psychologist literature compares them in many different ways and find somehow similar consistent results like the morality related study by Gino and Mogilner (2014). Time is less socialized, while money is more economic concerned. Zauberman and Lynch (2005) demonstrated that the expectation of growth of slack in the future is more pronounced for time than for money, in which the slack is the perceived surplus of a given resource available to complete a focal task. Sanford E DeVoe and Pfeffer (2007) suggests that the interaction of prior experience with whether or not participants calculated an hourly wage in predicting participants' willingness to trade more time for money fully mediated by the salience of the equivalence of time and money in participants' decision-making. Again Sanford E. DeVoe and House (2012) Mogilner (2010)J. L. Aaker, Rudd, and Mogilner (2011) both investigated thinking about time, rather than money, influence how effectively individuals pursue personal happiness. They used different methods and find a similar result that thinking in time is associated with greater happiness than thinking in money. Whillans and Dunn (2015)used large-scale survey data and experimental methodology to show that making the economic value of time salient reduces environmental intentions and behavior. This occurs in part because thinking about the economic value of time creates awareness of the opportunity costs associated with environmental behavior. Finally, Sanford E DeVoe (2019) reviewed the recent literature examining the psychological consequences of thinking about time in terms of money. It suggests that putting a monetary price on time can have powerful psychological consequences and a clear need for future research to ascertain these possible consequences.

Since time and money are the two principal resources that we encounter in daily life. Not just psychologists, economists, especially behavioral economists, also studied this to understand their difference in pro-social behavior. However, recent economic studies often observed this "volunteer puzzle" also (see, Handy and Katz (2008)) where agents prefer volunteer time to donate money when monetary donations are, ceteris paribus, more efficient for providing resources to charity. Liu and J. Aaker (2008) analyzed this difference in charitable giving in a survey experiment. They found that asking people about their intentions to donate time to work for an organization leads to a significant increase in the actual contribution compared with asking them to donate money. Ellingsen and Johannesson (2009) ran a bargaining game with a demand for a compensation to imply that people are less likely to demand compensation for their time investment, while almost all subjects demand equally costly monetary investments. This implies that people may have a higher loss aversion of money. Brown, Meer, and Williams (2018) showed that when people work directly for charity (volunteer), they behave more generously rather than working for themselves and donate part of their earning. All of those showed a possible difference in the altruism level between monetary giving and volunteer time giving.

However, Feldman (2010) tested how the tax policy in giving money affects people decide on giving time and giving money, and they found that money and time are substitutes. Vilares, Dam, and Kording (2011) used a trust game to test the reciprocity level in the money and effort domain, and they found that money and effort are the same. Bauer, Bredtmann, and C. M. Schmidt (2013) found that when their time offered to the market increases, the higher substituted time donation by monetary donation through the Europe Social Survey data. Furthermore, those showed different results to prove this substitution relationship between money donation and volunteer hours. This variance may occur because of their different way of manipulating the volunteer hours in the lab or the empirical studies. People's joy of giving volunteer hours may not just be caused by their altruism, also by their preference for the type of this volunteer work, the accomplishment feeling for finishing that specific work, and also the social interaction when you are giving volunteer time. The volunteer time we observed from the field measured not only their intrinsic preference of giving time but also their preference for that work type and social interaction. If we do not ask them to work in one specific task and get rid of social interaction with people during volunteer hours, then we can revise the word ”volunteer work time” into just ”volunteer time”. Based on the suggestions form psychologists, we may still observe the difference between giving decisions of time and money. Our project is designed to have a clean and straightforward environment without social connection and no meaningful work to represent time. By applying this, we could measure their intrinsic preference of giving time and money in a more precise way.

3.3 Theory background of crowding out effect

Here in this project, we focus on measuring the crowding- out effect on the intrinsic preference for giving behavior. Our hypotheses will base on the impure altruism model from Andreoni (1990) to assumes that individual i will gain utility from the decision of consumption x_i, the total value added to the charity $m_i + w'v_i$ and their warm-glow of giving either money m_i or timev_i like below :

$$u(x_i, m + w'v_i, m_i, v_i)$$

$$s.t \quad x_i + m_i \leq w(1 - v_i)$$

There are two main assumptions in this paper:

Assumption 1 (A1): No intrinsic preference of giving type :

$$\frac{\partial U}{\partial m} \mid_{m=w'v_i} = \frac{\partial U}{\partial v'} \mid_{m=w'v_i}$$

Assumption 2 (A2): $w' < w_i$, the market value of the volunteer hours is not greater than their professional work wage. [8]

[8] These two assumptions come from the model in James Andreoni's paper. In the economic concerns, that time can finally change into money by multiplying their wage (or we say the opportunity cost). Then in this optimization problem, there should be no difference between them since there exits this equivalency. Without lose of generality, this assumption should be satisfied. As for the assumption 1, if the volunteer work's wage is higher than their own wage, then they are in their wrong professional work and overqualified by the charity organization. The market value $w'v$ of the volunteer hours can be explained like that charity can hire someone else to work in this volunteer job by paying them the market price for this type of work.

If we assume that the equilibrium of giving is m^* if subjects donate money only and v_i^* if subjects give time only. Based on those two assumptions, we can see that $m^* = w'v_i^*$ as the solution for this optimization problem. However, the cost of giving money m^* is m but the cost of giving v_i^* hours is wv_i^* which is greater than the $w'v_i^*$. then we have $m = m^* = w'v_i^* < wv_i$. If the cost of giving the same amount of money is less than giving the same amount of time then people would prefer always to give money to gain the same level of utility. To understand the preference of giving time and money, we keep these two wages the same as $w'v_i^* = wv_i$ and use the Assumption A1 to consturct the crowding out variablesas follows.

Based on the model from the James Anderoni's Philanthropy chapter, Bénabou and Tirole (2006) and Ottoni-Wilhelm, Vesterlund, and Xie (2017), Here we developed the theory about how this increases of the pubic good crowd out their giving decision in the case that there are two types of giving resources Time v_i and Money m_i , and what variables we can use to test the difference between the preference of giving.

In pure altruism model, Subjects only care about the output of public good $G = \sum_{i=1}^{n} G_i$ which is accumulated by all people's donation G_i in the society including themselves. This donation including both money donation and volunteer hours donation. Since we say money donation can be used by the charity organization to hire someone to work for them, so in fact they can be substituted by each other in the terms of economic concern. To avoid the volunteer work's market price elasticity' effect on their decision of giving time, we take the extreme case that the market price of the volunteer is equal to their own wage $w' = w_i$. In other words, they only volunteer their professional work to the charity and this is the most efficient volunteer too.[9] Subjects can decided to donate money or time and their personal consumption level to maximize their utility under their wage w_i and total working time is 1. The optimization problem described above for individual i can also be written as:

$$U(x_i, G)$$

$$s.t.\ x_i + g_i = w_i$$

$$g_i = m_i + w_i v_i$$

[9]This is good to assume like this and also control like this, how people measure their giving time by the opportunity cost or by the value added to the charity remains unknown. To decrease this effect here in analyzing their driven of giving time by pure altruism or by warm-glow here, it is better to set them be equal which also satisfy the original assumption from the model that we mentioned above $w' \leq w$.

$$G = \sum_{i=1}^{n} G_i = \sum_{i=1}^{n} (m_i + w_i v_i)$$

Here since we have the assumption that people are indifferent with the donation gift by others G_{-i} and by themselves g_i to increase the charity's output. Then under the Nash assumption, each person i treats G_{-i} as independent of g_i when solving the problem. It is very intuitive that when other's making the decision to donate, you are not donate yet to affect their decision. To add this donation from others to the budget constraint, then we have optimization problem with each individual choosing G rather than g_i :

$$\underset{x_i, G}{Max}\ u(x_i, G)$$

$$s.t.\ x_i + G = w_i + G_{-i}$$

$$G \geq G_{-i}$$

which becomes the same problem with the James and Ottoni-Wilhelm, Vesterlund, and Xie (2017) and we also follow the first note by Becker (1974) to assume that each individual acts as through their "Social income" which were $w_i + G_{-i}$. In other words that w_i and G_{-i} have the same marginal effect on an individual's optimal G. To find the solution from those equation above then set the marginal rate of substitution equal to 1, which is $(\partial u_i/\partial G)/(\partial u_i/\partial x_i) = 1$. So we have this supply function of $G = f_i(w_i + G_{-i})$. Since in our assumption that the charitable good given by others is the same given by themselves in pure altruism model. Then based on the Philanthropy chapter in the book Kolm and Ythier (2006), we can observe the complete crowd out

$$\frac{dg_i^*}{dG_{-i}}|_{dw_i = -dG_{-i}} = -1$$

Based on Assumption A1,people are indifferent of giving time v_i and the monetary giving m_i. So here in this case there will be an special case that if we only allow them to give time v_i but the others donation G_{-i} is money, we called G_{-i}^m. If Assumption A1 holds then, we can also observe this complete crowd out in the pure altruism model:

$$\frac{dw' v_i}{dG_{-i}^m}|_{dw_i = -dG_{-i}, w' = w_i} = -1 \quad (1)$$

In the warm-glow model , since here subjects' decision is not just depends on their willingness to increase the charity's output but also their joy of giving itself . In this case we change our model into below :

$$U(x_i, G, g_i)$$

$$s,t\ x_i + g_i = w$$

$$g_i = m_i + w_i v_i$$

$$G = \sum_{i=1}^{n} G_i = \sum_{i=1}^{n} (m_i + w_i v_i)$$

Like above we have the $G_{-i} = G - g_i$, then when we change our model into below with the same purpose like above

$$\underset{x_i, G, G-G_{-i}}{Max}\ u(x_i, G, G - G_{-i})$$

$$s.t.\ x_i + G = w_i + G_{-i}$$

Then their donation decision become

$$g_i^* = f_i(w_i + G_{-i}, G_{-i}) - G_{-i}$$

then we can do the derivative respect to the others donation

$$\frac{dg_i^*}{dG_{-i}}|_{dw_i=0} = -1 + q_1$$

In which that q_1 is the derivative with respect to the warm-glow term. It is assumed to be positive, so $q_1 > 0$ which means that— $\frac{dg_i^*}{dG_{-i}}|_{dw_i=0}| = | - 1 + q_1| < 1$ the crowd out amount in impure altruism model is smaller than the pure altruism .

Also here since we care about the money term to crowd out the time , like that in the pure altruism model , When we change the others donation into money donation and the subjects themselves only can donate time , then this crowd out effect becomes like below :

$$\frac{dw'v_i}{dG^m_{-i}}|_{dw_{i=0},w'=w_i} = -1 + q_1$$

If the assumption A1 holds then this crowd-out relationship from money to time holds too. Then if we keep $m^* = w'v_i^* = wv_i^*$, then we have $\frac{dw'v_i}{dG^v_{-i}}|_{dw_{i=0},w'=w_i} = \frac{dm_i}{dG^m_{-i}}|_{dw_{i=0},w'=w_i} = -1 + q_1$ and also $\frac{dm_i}{dG^{v_i}_{-i}}|_{dw_{i=0},w'=w_i} = \frac{dw'v_i}{dG^v_{-i}}|_{dw_{i=0},w'=w_i}$. Based on this thoery behind, we can measure the crowding-out effect in different enviorment to understand the preference for giving different resources. To check the generosity level within-subject when there is no others' giving, we can compare $g_i^M|_{G_{-i}=0}$ *and* $g_i^T|_{G_{-i}=0}$.To see how their giving decisions being crowded out by the same resources from others, we construct $\frac{dm_i}{dG^m_{-i}}|_{dw_{i=0},w'=w_i} = -1 + q_1^{MM}$ (1) and $\frac{dw'v_i}{dG^v_{-i}}|_{dw_{i=0},w'=w_i} = -1 + q_1^{TT}$ (2) to test if the $q_1^{MM} = q_1^{TT}$. Then when others' giving to increase the charity output is a different resource to your giving resource, will you treat the others' different resource given as the others' same resource given by comparing $\frac{dm_i}{dG^{v_i}_{-i}}|_{dw_{i=0},w'=w_i}? = -1 + q_1^{MT}$ (3) , $\frac{dw'v_i}{dG^m_{-i}}|_{dw_{i=0},w'=w_i}? = -1 + q_1^{TM}$ (4) to (1), (2), respectively. These cases above can only measure the crowding out effect by other's giving, how about the crowding out effect within their own giving. So we also measure whether or not they are willingness to give once they already gave some amount of the resources to see how agents treat differently between their giving and other's giving. By measuring all the variables we can answer our main research questions:

RQ1: Generosity level: Do people behave more/less generously for giving time than giving money?

RQ2: Warm-glow or pure altruism: Are giving time and giving money crowded out by others' giving in a dierent way ? If so, by what amount and by what resources (time/money) of others' giving?

RQ3: Crowding-out to your own decision: Will people choose to make complementary decisions of giving Time (Money) when they already gave some Money(Time) ?

3.4 Experimental design and hypotheses

In this section we explain the design of the experiment and our hypotheses. We start by outlining treatment design employed in our laboratory study to address our research purpose. In Section 3.4.1we explain the procedure of our experiment. Then, in Section 3.4.2, we state and discuss our hypotheses.

3.4.1 Experiment procedure

This experimental environment is as follows: At the start of the experiment, subjects are randomly assigned as a role and matched into groups of two, consisting of one donor, so-called Odd player in the experiment, and one recipient, named as Even player. Each donor is endowed with an amount of Money and an amount of Time to be able to allocate between himself and his paired recipient, while their paired recipient is not endowed with Money and Time. The recipients will end up with no money earned (except the participation fee) and have to work for a fixed 30 minutes "Constrained Time" before the end of this experiment if their paired donor does not share anything with them. Meanwhile, during the recipient's "Constrained Time" work, the donor can surf online to enjoy the 30 minutes of leisure in the Lab if they decide to allocate some positive amount of their endowment to their recipient. The Money that they allocated refers to the cash that their recipient will get. The Time that they allocated refers to the "Constrained Time" that the donor helps to work for their recipient. Since the recipient received the help of Time from their donor, instead of doing the "Constrained Time" work, they can enjoy the Time given by their donor to surf online. It is public that how much endowments that donors have in this experiment. To better capture the real world, the Money they allocate in this experiment is euros directly. To avoid any preference of real work, the "Constrained Time" work that the donor helps to do is to click a warning message every 10 seconds, which is just like counting down the time.

There are two stages of decisions that the donor needs to make. In the first stage of the decision, called Decision Task, the donor has to make in a total of 10 decisions. Instead of mixing decisions of giving money and giving time together, we separate those ten decisions in two blocks and 5 in each. One block is about making decisions to allocate their Money (Block Money), and the other block is to make decisions of giving their time to help their recipient (Block Time). Those two blocks will be displayed to the donors sequentially with a random order, which means that donor has to finish making all five decisions of Block Time (Money) to enter the Block Money (Time). Within blocks, they will see five budgets about their money and time's endowments and the amount that the computer chooses to give to their recipient. Based on the information given by each budget, the donor can make their giving decision by pulling the slide bar between 0% to 100 % to choose the percentage of their endowment that they want to give to their recipient. To make their decisions compared to the computer's giving and also be aware of the exact number of their giving in terms of euros in Block Money and minutes in Block Time, the donor can slide the bar and click "check" to have a look of the converted euros/minutes of the percentage. If they do not like the number, they can adjust their decisions until they like it and then click "submit". At the end of this stage, only one of the ten decisions will be randomly selected as final giving in the Decision Task stage.

Once they are informed with the randomly selected final giving, they will enter the second stage of giving decisions - X stage. At the beginning of this experiment, participants will only be informed about the existence of this stage, but nothing about the context of this stage. In this stage, they can make complementary of giving Time(Money) knowing the final Money(Time) giving selected from Decision Task. To measure their willingness to make a complementary giving decision, they can choose to enter the X stage by clicking "Yes" or do not enter this X stage by clicking "Skip". Once they entered the stage, then they can make their complementary decision in a similar giving window under the information of their final giving in Decision Task and their endowment.

After they made their decisions, they need to fill two post-surveys. From this stage, both donor and recipient can fill the surveys. The post-survey one is about how much recipients expect to receive from their donors. Recipients need to answer their expectations firstly, and then donors are asked to guess this number for which they will get the proportion of 1 euro by how their guess is close to the average of the recipients' answer. This is for controlling their beliefs of the demand of giving, which may drive their generosity based on the Andreoni's charitable giving model. Recipients need to guess the actual amount that donors have given.The Studies of Bauman and Rose (2011) Maren Elise Bachke and Wik (2014) and Jyoti Khanna (1995) suggest that the factor of gender, age, religion, and Econ-student may affect agent's generosity level differently, together with their past pro-social behavior and anti-social behavior. Our post-survey two is a demographic questionnaire including questions about gender, age, religion and Econ-student, and their past pro-social experience and anti-social experience,[10] which will be reward one euro if they choose to fill this survey. To better capture their generosity level and change, we also include control variables that may also affect their giving decision based on previous literature. Once they finished all the surveys, depending on their Time payoff, they will enter the fixed 30 minutes Time task to work for "Constrained Time" or enjoy the Time.

[10]This measurement of past pro-social experience and anti-social experience measurement based on the study of Carlo and A.Randall (2002) and Young I.Cho and Widaman (2010) separately.

3.4.2 Treatments and hypotheses

This experiment has two × three treatments within-subject design, including two different giving resources (time and money) and three different others' giving type. To maintain the robustness, we add one sub-treatment in Treatment 1 and Treatment 2, which contains one low level of other's giving and one high level of other's giving. All the differences are respect to the donors' decision in the second stage, but the recipient will also know the existence in the instruction. Table 3.1 gives an overview of the principal treatments.

Table 3.1: Overview of Treatments 2 × 3

		Donor's giving Decision	
		Time	Money
	T. No	No	No
Others' giving	T. Same	Time	Money
	T. Diff	Money	Time

Notes : "Others" refers to the computer's giving to increase the output of recipients.

Baseline treatment: There is no others' giving

In this treatment, donors need to make their giving decisions to their recipients when there is no giving to their paired recipients from computers.

Treatment 1: Others' giving is positive and the same type as donors' giving

In this treatment, donors need to make their giving decisions to their recipient when the computer also chooses to give their paired recipient some endowment from the same type. Computers' giving will be from two categories: resource (time and money) and level (one high level and one low level).

Treatment 2: Others' giving is positive and the different type as donors' giving

In this treatment, donors need to make their giving decisions to their recipient when the computer chooses to give their paired recipient some endowment from the different types. When donors are making decisions about giving money(time), the computer's giving to their recipient will be time(money). Computers' giving will be from two categories: resource (time and money) and level (one high level and one low level).

Baseline resembles the situation in real life: when someone needs help, you are the only one who can help. This measures the overall generosity level related to our first research question. Based on the impure altruism model from Andreoni (1990), the overall generosity level will be affected by two incentives of giving, pure altruism, and warm-glow. The pure altruism motivated giving will be crowded out by others' giving to the same person. However, warm-glow motivated giving will not. Treatment 1 with others' giving makes it possible to measure whether or not their giving behavior is partly driven by warm-glow through checking how much their giving is crowded out by different levels of others' giving. For checking the substitution relationship between your giving and others' giving, we can measure how people make decisions through the two incentives. However, in real life, others' giving will not always be the same type as yours. Then the substitution relationship between money and time (different types) matters to understand how people give these two different endowments. Conditional on the substitution between yours and others' giving, treatment 2 captures the substitution relationship by measuring how much of their time(money) can crowd out others' giving of money(time). This singles out that the substitution relationship between your giving time(money) and others' giving money(time). In case, if other's time(money) and your money(time) are not substituted by each other, to fully understand their relationship, the X stage design captures the substitution between your money(time) and your time(money). With our design, we can fully understand how giving time is different from giving money in terms of generosity and substitution. To employ the treatments in our experiment, we used five budgets condition for donors to make their giving decisions. Table 3.2 shows the details of those five budgets.

To simplify the income effect,[11] in all budgets, donors will be endowed with the same

[11]The other setting would also include the income effect in which we need to add one more budget with different endowment levels to our current setting for each treatment. Furthermore, this will need six more budgets. Donors have to make six more decisions conditional the ten decisions we have already. We think this will be overwhelming in terms of agents' mental accounting since we already have a different level of others' giving and different types of others' giving. Meanwhile, we are more interested in how they change their giving decisions along with the change of others' giving instead of measuring the pure altruism parameter and warm -glow parameter in the utility function. So we

Table 3.2: Experimental budgets employed to the treatments

Budget	Computer's giving (G_{-i})	Donor's Time (Money) endowment (w_i)	Donor's social income ($G_{-i} + w_i$)
B1	0	20 mins (10 €)	20 mins (10 €)
B2	4.3 mins	20 mins (10 €)	24.3 mins (10 €)
B3	9.2 mins	20 mins (10 €)	29.2 mins (10 €)
B4	2.15€	20 mins (10 €)	20 mins (12.15 €)
B5	4.6€	20 mins (10 €)	20 mins (14.6 €)

amount of money (10 €) and time (20 minutes) to allocate to their recipients. Budget 1 (B1) fits the baseline. B2 and B3 serve the treatment 1 for giving time and treatment 2 for giving money, while the B4 and B5 serve treatment 1 for giving money and treatment 2 for giving time. The reason to choose those numbers as others' low and high level giving and the budget design is inspired by the study of Ottoni-Wilhelm, Vesterlund, and Xie (2017) in which they have the design of a similar budget to analyze how people give money. Nevertheless, they have more budgets for a different endowment to capture the income effect. Also, in the previous literature of giving decisions, the average of donors' giving amount is around 50% of their endowment. For observing the crowding out in our experiment, the high level of others' giving we set here is close to that average (50%), which will crowd out all of the donor's giving if there is giving and if they are fully incentivized by pure altruism. Thus, if pure altruism is the only one driven by any type of giving, we will have this possibility to observe the full crowding out on average. The low level of others' giving is just slightly less than half of the high level in both time and money[12]. In our experiment, there is no actual wage for the time[13], since they are not paying for working this non-meaningful "Constrained Time". As suggested by psychologists, time and money essentially are different things

did not add this different budget in our experiment. Based on our research purpose, there is no loss to have no income effect.

[12]This number could not be too small. Then there would be no possibility to observe a significant change.

[13]As for the equivalency problem between time and money, this would be different in real life since everyone's working hour has a different wage rate, but the money is the same to everyone in numbers at least. However, we think that for each individual, how much money they have may even gain different utility to them than how much time they have. In other words, each individual may value the time and money to their own differently. For example, the wage for woking one hour is less valued to have one leisure hour, and in this case, the money you earned from this hour is different from the value from one hour. However, in our experiment, there is a fixed impute wage for the time, which is 20 minutes vs. 10 euros from their endowment, which means they only have the.

which can not be easily transformed. For comparison between time and money giving, we only compare how much percentage of your endowment that you want to give to your recipient considering the others' giving. We also considered these social income variables in the table, which can be used to measure how many resources the society has as a control variable to analyze their giving behavior.

For our hypotheses, we mainly based on the mind-set explanation to predict how they give time and give money in our setting based on the different settings that we have to other previous studies. Since in the study Gino and Mogilner (2014), they find that by priming the time mind-set, agents behave with a higher morality level in the form of cheating decision. The time mind-set is more related to self-reflection. Self-reflection is similar to the warm-glow in terms of the emotion that your behavior is more based on your personal feeling rather than other external drives. Motivated by this mind-set explanation, our first two hypotheses are:

Hypothesis 3.1 ***Geneosity Level****: Within each budget across all treatments, subjects give more time than give money.*

Hypothesis 3.2 ***Warm-glow or Pure altrism-1****: Comparing giving time and giving money within treatment 1 conditional on their giving in baseline, giving time is less crowded out than giving money.*

Based on the study from working paper from Alexander L. Davis and Weber (2015) they find a weaker relationship between individuals' allocation decisions across monetary and non-monetary contexts than allocations that constantly hold the monetary nature of the context. We believe that this may also hold for giving money and giving time in our case. The same reason also for the complementary decision to give. Since they are different contexts, they will then treat the Time (Money) given is different from the complementary decision to give Money (Time). We hypothesize that they will make a positive complementary giving decision in our case. Based on the moral level of Time mind-set is higher, we also hypothesize on the amount of their giving time is higher than giving money.

Hypothesis 3.3 ***Warm-glow or Pure altrism-2****: Comparing giving time and giving money between Treatment 1 and Treatment 2 conditional on their giving in baseline, giving time(money) crowded out by others' giving of money(time) is less than giving time(money) crowded out by others' giving of time(money).*

Hypothesis 3.4 ***Crowd-out to your own giving***: *Comparing their complementary decision of giving time and giving money, subjects make positive decisions of complementary giving no matter in domain of time or money. But they give more amount of time once they entered the X stage.*

3.5 Experimental details and results

In this section, we present the experimental results of our study. We start by showing the details of our experiment about the participate pool. Then, we continue by exploring donors' giving decisions in terms of their overall giving across treatments, which represents their overall generosity. After this, we go further to analyze the crowding-out effect of the donor's giving decision. Finally, we checked their complementary decision of giving. We will sometimes refer to budget 1 as B1, budget 2 as B2, budget 3 as B3, budget 4 as B4. Also, we will refer to the time and money that we are using in the experiment as Time and Money with this first capital letter to distinguish with other money or time we may mention in the paper. In our decision design experiment, each donor is one independent variable. For testing the mean difference within treatments, we use the Wilcoxon matched-pairs signed-ranks test on the individual data. Finally, some complementary regression analysis can be found in Appendix 3.7.3

3.5.1 Experimental details

We ran our experiment during June and July in 2019 among the students in Universitat Autònoma de Barcelona (UAB) and Universitat Pompeu Fabra (UPF). There are 138 participants for eight sessions, half in UAB and half in UPF. Our experiment has two roles, including donors and recipients. 69 participants as donors made giving decisions, and the other 69 participants as recipients paired with them received giving. To avoid the framing effect, we call donor players as Odd players and recipient players as Even players in our experiment. Each one has a 5 euros show-up fee. To be more precisely capture their preference of time, they are asked to left all of their belongings to the table next to the door before they entered the Lab room. To fully participate in our experiment, each subject has to sign the consent form by knowing a brief introduction to our experiment. Our RA will read aloud the instruction, which is the same to both donors and recipients, and answer participates' questions in private. Subjects were separated with separators with which it would be impossible for them to see the computer screen of others' computers and no communication was allowed between them.

Before starting the experiment, subjects had to practice one round of the decision and one minute of the "Constrained Time" to understand how to make decisions and the Time experience that they may encounter in our experiment. To be informed about their roles and enter the decision stage, everyone has to pass the comprehension test (See Appendix 3.7.1 for the instruction and the quiz for comprehension test). Subjects will be paid by one randomly selected decision from the donors' giving decision plus the complementary decision if there is and also the bonus from the surveys. At the end of our experiment and before they finish their "Constrained Time" or Time, their monetary payoff will be in cash stored in one envelope and distributed to them separately. Average earnings were 12.40 euros, and subjects spent approximately 1.5 hours in the lab. Table 3.3 summarizes the statistic data of our participates.[14]

Table 3.3: Overview of participants statistics

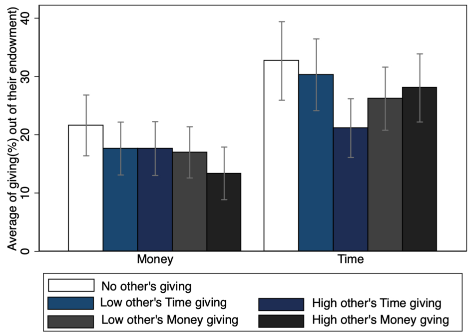
Variables	Data
Participants	138
Gender	57% Female, 43% Male
Age	Average 21.9 (Sd 4.25) from 18 to 53
Religion	60% with religions
	(including Catholicism, Buddhism, Muslim, etc)
Econ-student	24.7% Econ-related subjects
Positive giving (B1)	71.6%
Positive Crowd-out	73.64%

3.5.2 Experimental results - Overall giving

We present the overall giving here to examine the donor's generosity level across the treatments. Figure 3.1 shows the average giving amount in terms of the percentage (%) to their endowment between giving time and giving money in all budgets. By looking at this figure, we can conclude that the giving level in the domain of time is much higher than the giving of money in our data in almost all the budgets. The giving time is almost 10% higher than the giving money in budget 1 in which there is no others' giving, and it is only around 20% giving itself. We can also see that there is variance across treatments, and the variance looks different between giving time and giving money, which means that we may find the different crowding-out effect between giving time and giving money.

[14]The study by Ottoni-Wilhelm, Vesterlund, and Xie (2017) published on *American Economic Review* 2017 has a simliar decision and 85 subjects to make in total 510 decisions.

Figure 3.1: Overall giving in all treatments



The non-parametric test suggests that except the comparison between budget B1 and B2 is not significant, the other givings are significantly different from their giving in B1 (See Appendix 3.7.1 Table 3.8). Furthermore, by comparing treatment 1 when the other's giving is the same type as donors', both giving time and giving money have significant changes (P-value =0.007 to Time vs. Other's Time and 0.027 to Money vs. Others' Money). While comparing treatment 2, when the others' giving is a different type like donors, we did not observe the significant results. However, in our average data, some subjects may do not giving anything even in budget B1, so including those subjects will cause biased results for crowding out purpose. We will check this in the next section about the crowding-out effect in detail.

Table 3.4: Linear regression on giving across Treatments

	(1) Giving	(2) Giving	(3) Giving	(4) Giving
Block (1: Time, 0: Money)	10.22***	10.23***	10.23***	2.263
	(5.95)	(6.06)	(6.12)	(1.20)
Gender		4.251*	5.089**	3.483
		(2.21)	(2.66)	(1.89)
Age		-0.320	-0.546	-0.767**
		(-1.14)	(-1.92)	(-2.81)
Religion		-3.487	-3.650	-2.848
		(-1.70)	(-1.80)	(-1.46)
Pro-social index		0.426	0.468	0.631*
		(1.66)	(1.84)	(2.58)
Anti-social index		-0.513**	-0.426*	-0.290
		(-3.02)	(-2.52)	(-1.78)
Econ-students			8.466***	5.975**
			(4.02)	(2.93)
Donor's belief				1.852***
				(8.02)
Constant	17.44***	19.23***	19.70***	14.69***
	(14.37)	(8.94)	(9.25)	(6.89)
Observations	700	700	700	700

t statistics in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The linear regression result suggests that giving time increase their giving decision by 10.22% like what we can see from the Figure 3.1 even we controlled with other influential factors. By looking at the control variables, we can find that women are more likely to give no matter what endowment, which is robust with many previous studies like Linda Babcock and Weingart (2017). However, in our data, Eon-students seem giving more than non-Econ-students. Furthermore, antisocial measurement also captures their pro-social behavior in the Lab. However, once we add the control variables of the donor's belief about their recipients' demand, we find the donor's belief explains more changes in their giving.

Table 3.5: Linear Regression on donor's expectation of giving

	(1) Donorguess	(2) Donorguess(T)	(3) Donorguess(M)
Block (1: Time, 0: Money)	4.292***		
	(6.61)		
Gender	0.150	-0.0211	1.461
	(0.20)	(-0.02)	(1.84)
Age	0.263**	0.563**	-0.0492
	(2.68)	(3.28)	(-0.48)
Religion	-1.261	-3.170*	0.431
	(-1.55)	(-2.12)	(0.48)
Pro-social Index	-0.106	-0.261	-0.000547
	(-1.28)	(-1.85)	(-0.01)
Anti-social Index	-0.108	-0.149	0.0292
	(-1.25)	(-1.06)	(0.34)
Econ-students	1.044	1.305	0.398
	(1.30)	(0.96)	(0.48)
Constant	1.914*	5.417***	3.179***
	(2.36)	(4.49)	(4.36)
Observations	92	45	45

t statistics in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Then we did this linear regression on the donor's belief of their recipients' demand. We find that their belief in the high demand of receiving is significantly higher in giving time. To be more precise, older and non-religion subjects may think that the demand of time from their recipient is higher.

Result 3.1 *Giving: (i) Subjects are more generous in giving time than giving money (H1); (ii) Donors believe that their recipients demand higher time giving than money giving which may be the causality for the result (i); (iii) Women are more likely to give, no matter giving time or giving money.*

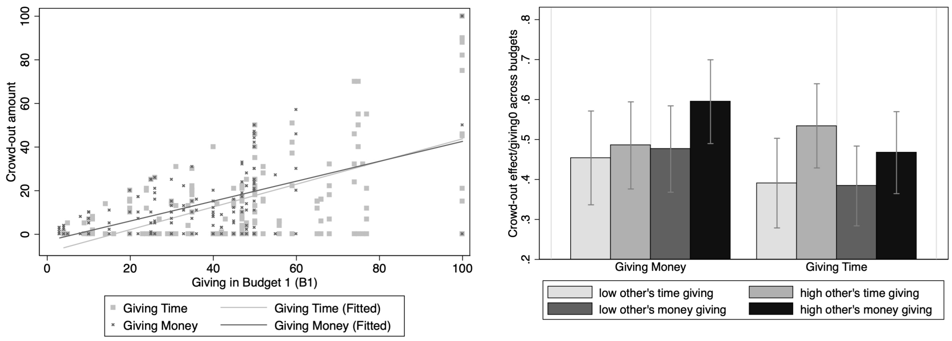
3.5.3 Experimental results - Crowding out effect

In this section, we will talk about the crowding out effect. In all the analysis here, the subjects who did not give in the budget B1 is excluded, since it is biased with the crowding out effect purpose. The crowding out amount is the reduced amount from their giving in budget B1 to their giving in other budgets. Figure 3.2a scattered subjects' giving conditional on their giving in budget B1 when there is no others' giving.

It suggests their crowding out is conditional on their generosity level when there is no others' giving. Then we standardized our crowding out data by dividing their giving in B1. Figure 3.2b shows the crowding out conditional on their giving in B1 across giving type and budgets.

Figure 3.2: Crowd-out effect across treatment

(a) Crowd-out to giving in B1 (b) Crowd-out across treatments



In this figure, it looks like that giving time is less crowded out by others' giving, at least when others' giving is low. Also, we can see that giving money is not crowded out that much by others' time giving than other's money giving while giving time can be crowded out by both time and money from others.

Table 3.6: Linear regression for Crowd-out effect across treatments

	(1) Overall	(2) Overall	(3) G.Time	(4) G.Time	(5) G.Money	(6) G.Money
Block (1: Time, 0: Money)	0.0586	0.0541				
	(1.65)	(1.80)				
$G_{-i}\|_{G_{-i}+w_i}$		-1.047***	-1.028***	-0.508*	-1.080***	-0.458
		(-12.75)	(-9.26)	(-2.26)	(-8.84)	(-1.97)
Gender		0.0303	0.0177	0.0365	0.0532	0.0537
		(0.89)	(0.40)	(0.64)	(1.00)	(0.81)
Age		-0.00833	0.00436	0.00810	-0.0165*	-0.0201*
		(-1.54)	(0.41)	(1.80)	(-2.39)	(-2.33)
Religion		-0.0445	-0.0821	-0.107	0.00515	0.0147
		(-1.29)	(-1.81)	(-1.84)	(0.10)	(0.22)
Econ		-0.0259	-0.0396	-0.0418	-0.0110	-0.0445
		(-0.70)	(-0.82)	(-0.67)	(-0.19)	(-0.61)
Pro-socia Index		0.00955*	0.00124		0.0125*	0.0161*
		(1.97)	(0.12)		(2.14)	(2.22)
Anti-social Index		-0.00882*	-0.00733	-0.00966	-0.00937	-0.0128
		(-2.21)	(-1.44)	(-1.49)	(-1.47)	(-1.57)
$G_{-i}\|_{G_{-i}+w_i} \times DTM$				0.141		0.210
				(0.97)		(1.40)
_cons	-0.388***	-0.0718	-0.0794	-0.330**	-0.00854	-0.269*
	(-15.33)	(-1.54)	(-1.31)	(-3.07)	(-0.13)	(-2.52)
N	403	403	203	150	200	153

t statistics in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The linear regression from Table 3.6 suggests that conditional on their giving in B1, their respective crowding out amount is not significant between giving time and giving money. The others' giving of time can crowd out subjects' money giving while the others' giving of money can also crowd out subjects' time giving. The result is also confirmed in the non-parametric test, which can be found in Appendix 3.7.1 Table 3.9.

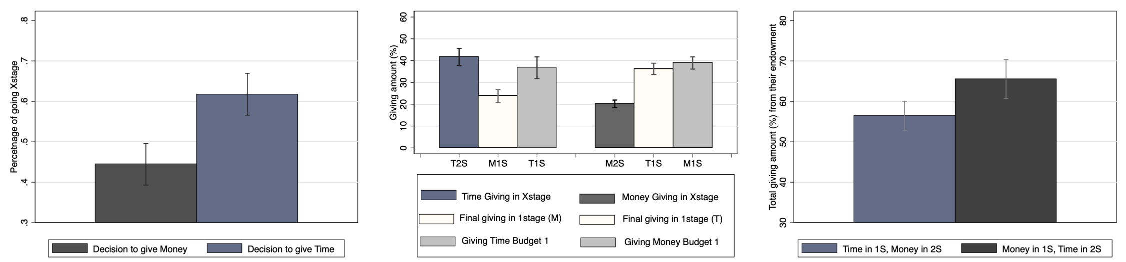
Result 3.2 *Crowd-Out Effect: (i) Across time and money treatment, there is no significant difference; (ii) when other's giving is low, giving Time is less crowded out than giving money; (iii) Others' giving of time could not significantly crowd out the giving of money, while others' giving of money can crowd out the giving of time.*

3.5.4 Experimental results - X stage

In this section, we will focus on the entry decision and giving the amount of the X stage. Figure 3.3 summarized the overview of all analyses we have done for the complementary giving decision. Figure3.3a shows the decision to enter the X stage to make a complementary giving decision. 60% of donors whose final selected giving decision from Stage 1 is giving money choose to entry the X stage to make their complementary giving decision of time, while only around 40% of donors whose final selected giving decision from Stage 1 is giving time choose to entry X stage to give money. In Table 3.7a, we can find that this difference is significant with the Probit regression on their decision of entry across making complementary giving decisions of time and that of money.

Figure 3.3: Overview of donor's complementary decision of giving

(a) Decision of entry X stage (b) Giving amount in X stage (c) Total giving (1S + 2S)



For complementary decisions, all the control variables affect it. Notably, women are more likely to make a complementary decision of giving time if they have given some money before but not to give a complementary decision of giving money. Older subjects are not less likely to make a complementary giving decision, no matter it is time or money. Econ students are more likely to make complementary of giving time too[15].

[15] I am not sure so far how much Econ-students are women in our data. However, based on the regression we have now, it seems no perfect correlation problem here.

Table 3.7: Regression tables for analyzing complementary giving

(a) Probit regression

	(1) DecisionX	(2) DecisionX(T-M)	(3) DecisionX(M-T)	(4) DecisionX
Block in 1stage (1: Time, 0: Money)	-0.642***			-1.032***
	(-6.03)			(-6.49)
Gender	0.330**	0.122	0.883***	0.177
	(2.94)	(0.76)	(4.19)	(1.51)
Age	-0.0424***	-0.0774***	-0.0208*	-0.0439***
	(-6.01)	(-6.04)	(-2.07)	(-6.05)
Religion	-0.416***	0.110	-1.351***	-0.422***
	(-3.65)	(0.68)	(-6.36)	(-3.59)
Econ-students	0.548***	0.229	1.240***	0.569***
	(4.43)	(1.34)	(5.53)	(4.42)
Giving from 1stage(GF1)		0.0244***	0.0143***	0.0116***
		(7.20)	(4.46)	(3.90)
Block in 1st × GF1				0.0100*
				(2.26)
Constant	1.217***	0.395	0.912***	1.062***
	(8.36)	(1.82)	(4.69)	(6.58)
Observations	700	360	340	700

t statistics in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

(b) Regression on giving

	(1) Givingx	(2) Givingx(M)	(3) Givingx(M)	(4) Givingx(T)	(5) Givingx(T)
Block in Xstage (1: Time; 2 Money)	19.20***				
	(8.25)				
Giving in 1Stage	-0.0186	0.295***	0.291***	-0.0946	-0.0925
	(-0.33)	(6.39)	(4.43)	(-1.12)	(-0.65)
Gender	-5.244*	14.00***	14.14***	-13.37**	-13.38*
	(-2.07)	(8.30)	(5.88)	(-3.09)	(-2.13)
Age	0.966***	-0.439***	-0.356*	1.154***	1.155***
	(7.97)	(-3.96)	(-2.16)	(6.39)	(4.41)
Religion	-0.904	3.931*	3.543	5.448	5.378
	(-0.32)	(1.98)	(1.22)	(1.18)	(0.76)
Econ	0.514	-5.100*	-5.070	-0.451	-0.376
	(0.18)	(-2.56)	(-1.77)	(-0.10)	(-0.05)
Giving Money in B1			-0.153		
			(-1.83)		
Giving Time in B1					-0.00348
					(-0.03)
Constant	8.266**	9.565***	14.54***	25.00***	25.09***
	(2.86)	(5.11)	(3.90)	(7.59)	(4.45)
Observations	370	160	80	210	105

t statistics in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Final selected giving from the last stage also increases the possibility of entering the X stage. However, this may be caused by those subjects with no giving even in Budget 1. Those subjects who have positive giving in 1 Stage 1 are those who are willing to give. So it suggests that generous people are still generous when they already give something to benefit others.

Figure 3.3b shows how much they give conditional on the entry decision of the X stage. As we can see in the figure, their complementary decision to give money is significantly lower than their giving decision of money in budget B1 from stage 1 on average. While their complementary decision of giving time is close to their giving decision of time in budget B1 from stage 1 on average, it looks like that giving time is not substituted that much by their own given money while giving money is substituted more by their own given time. Table 3.7b confirms the figure result that giving extra money is much more related to their giving decision of time in the f stage 1. While giving time is not. Furthermore, it confirms the finding that subjects give more complementary of giving time. Figure 3.3c summarized the first two results, and it suggests that, on average, giving money firstly and giving time later can attract more final giving from donors' endowment than giving time firstly and giving money secondly.

Result 3.3 *Complementary decision: (i) Decision to entry X stage: if it is the decision to make complementary decision of giving time after gave money in 1 stage, the probability of subjects' entry is higher(H3); (ii) It looks like that giving money can be more substituted by given time in 1 stage than that of giving time; (iii) The order of asking giving affects the final total amount of giving received. Asking giving money firstly and* *then ask time giving can attract more giving from donors;*

3.6 Conclusion

We conducted a laboratory experiment of a dictator game to identify the preference for giving behavior with the extent to charitable giving. The experiment excludes the social reputation concern and the preference of volunteer work type, which can be easily found in real life of giving volunteer time to focus on the difference between the intrinsic preference of giving time and giving money. By measuring the crowding-out effect based on the impure altruism model from Andreoni (1990), we compared subjects' giving preference conditional on different levels and across different resources (time or money) of other's giving. Meanwhile, we added one surprise stage in our experiment to see whether or not their giving behavior of Time(Money) can be crowded out by their own previous Money(Time) giving.

We have three main results. Firstly, We find that giving time is more generous than giving money on average. The difference may be caused by the donor's different beliefs about their recipients' demands of time and money. Donors think that their recipients will demand more time than money, and recipients also think in the same direction that they expect more giving of time from their donors than the giving of money. Then in the crowding-out effect analysis, we find that giving time is more sensitive to others' giving of time and money, while monetary giving is not very sensitive to others' giving of time. In our results, there is no significant difference between the crowding-out effect of giving time and giving money. However, giving time is less crowded out by a low level of others' giving of time and money both. For giving money, the change of other's giving time will not affect their crowding-out amount that much. While giving time, they can be crowded out by the change of others' giving time and giving money both. The implication of this would be related to the charity or moral labor market's information disclosing the strategy. Finally, giving time is less substituted by their own given money in 1 Stage than that of giving money, when comparing their complementary giving decision in surprise stage. So asking giving money first and then asking complementary giving time can attract more total giving from donors. The implication of this would be related to the fundraising strategies in charity and moral labor market.

Further studies related to this project can be in several directions. We can add the social pressure in our experiment to check how their crowding out effect can be affected by social pressure so that we can measure this parameter of social pressure to rich the charitable giving model. Except for the difference between money and time, we also have other giving resources like goods, blood, and other possible things. Giving behavior may be contingent on the giving resources (see the working paper by Alexander L. Davis and Weber (2015)), we can also classify the resources that we can give and extend this experiment with more resources to find out the principle of giving preference across resources.

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3.7 Appendix

3.7.1 Appendix A

Additional analysis and regression tables

Table 3.8: P-value table for all non-parametric test for all treatments

G.Time > G.Money	Between Budgets	
Overall: < 0.001	Giving Time	Giving Money
B1: 0.009	B1 vs B2: 0.399	B1 vs B2: 0.020
B2: < 0.001	B1 vs B3: 0.002	B1 vs B3: 0.010
B3: 0.092	B1 vs B4: 0.017	B1 vs B4: 0.018
B4: < 0.001	B1 vs B5: 0.129	B1 vs B5: < 0.001
B5: < 0.001		
Between Others		
Other's giving	Donor's Time	Donor's Money
Time	0.007	0.929
Money	0.326	0.027

Table 3.9: P-value table for the crowd-out effect

Between Budgets (T vs M)		Across Budgets (T vs M)	
Over all	Positive, $P - value$: 0.1351		
B2 vs B2:	0.3050	B2 vs B4:	0.4608
B3 vs B3:	0.0025	B3 vs B5:	0.5861
B4 vs B4:	0.1771	B4 vs B2:	0.3940
B4 vs B5:	0.4827	B5 vs B3:	0.1274

3.7.2 Appendix B

I N S T R U C T I O N S

Welcome! Thank you for participating in our study. The purpose of this experiment is to study how individuals make decisions in a certain context. The instructions are simple and if you follow them carefully you will earn money which will be paid to you in cash at the end of this session. The amount of money you earn depends on the decisions you make in the experiment and there is 5 euros show-up fee in our experiment. If you have any questions, please do not hesitate to ask us by raising up your hand. Apart from these questions, any kind of communication is prohibited and may lead to your immediate exclusion from the experiment.

Your Identity

Your identity is secret. You will never be asked to reveal it to anyone during the whole study. Your name will never be associated with your decisions or with your answers on the survey about your belief. Neither the assistants nor the other participants will be able to link you to any of the decisions you make. In order to keep your decisions private, please do not reveal your choices to any other participant even after this experiment.

Experiment ID

The ID Number that you picked up on the table of entrance is your unique number for us to maintain secrecy about your decisions, payment, and identity. You will present your Experiment ID to an assistant who does not know the context of this experiment at the end of the study to receive your cash payment.

We ran this experiment to analysis how people make decisions. Please give us your serious decisions. Like what we just mentioned above, your decision together with your ID won't be disclosed to anyone.We also hope that you do not talk about your decision and also the context of our experiment afterwards. To keep it as private information is our respect to academic research. We thank you for your cooperation and your help for participating in our research.

Experiment Environment

In this experiment, each of you will be assigned a role of either Even player or Odd player based on your random given Experiment ID. You , with different roles, entered in those two Lab rooms separately. Meanwhile, each of you in this room will be randomly paired with one participant in the other room. You are going to play a game with your paired partner, but your identity and your paired partner's identity will not be disclosed to each other. There are three different stages in total in our experiment which are Decision stage, X stage and Survey stage. Before you start your stages, each of your will need to do some practices to understand some key elements in our experiment. Only when every one correctly go through the practices, you will be informed of your role and enter the actual stages. After those three stages, you will enter into a fixed 30 minutes "Constrained Time" task. During the "Constrained Time" , you need to click a warning message every 10 seconds to reduce the 30 minutes “Constrained Time ” until it reaches 0.

There are two important items that will appear in those stages for you to make decisions. They are named “Money” and “Time”. “Money” refers to the cash in euros. “Time” refers to the time that you can enjoy to surf online or do whatever you like but remain seated quietly in front of your computer in the Lab. If you have some “Time ” then some of your “Constrained Time” will be replaced by them but the total amount remains the same which is “Time” + Left “Constrained Time” = 30 minutes. Each of you will need to play some different games depending on your role. The difference between those two roles is explained as below in each stage.

Decision Stage :

To Even Number player :

You are an even number player. In this experiment,you are endowed with 0 “Time” and 0 “Money” .Your paired partner has some “Money” and “Time” . He/She maybe willingness to share part of their endowments with you. If they choose to share, then you will be informed before your “Constrained Time”, meanwhile the computer may randomly give you some bonus that maybe “Money” or “Time”. This will be added to your paired partner’s giving to you as your payoff for this stage.

To Odd Number player :

You are an odd number player. In this experiment , you are endowed with 20 minutes “Time” and 10 euros “Money”. You will need to make 10 decisions included in two blocks (5 questions for each block) about how to allocate your endowments based on the information that we provide to you . The order of those two blocks is random, so does those 5 questions within the blocks. After all of your decisions, the computer will randomly pick one decision from all the 10 decisions that you made as the payoff that you and your partner receive.

X stage :
You will be given the information of how to play when you reach this stage.

Survey stage :

For both of you, There are two surveys you can fill before you enter your "Constrained Time". By filling the survey, you will possibly get a bonus up to 2 euros. The exact information for that will be disclosed once you reach there. Please follow the information we provide to submit your answers. Then you will be informed of your final payoff information including both "Money" and "Time" to enter your "Constrained Time". Once you finished, please fill the form on your desk. Then you can show your Experiment ID to the RA seating next to the door, He/she will give you your bags and your "Money" payoff envelope. We will show you one example question and how to calculate your final payoff in the next chapter.

Sample Decisions :

Here is an example of the decision. Since only Odd number players' decision will affect each of your payoffs, we will use Odd number players' decision as example to show you how to calculate your payoffs. The example is not meant to guide your decision in any way. On the actual decision sheets we want you to select the allocation that you like best.

(To Odd number) : You have been given 10 euros to allocate between you and your paired partner. There is no news from the computer. Please decide how much money you want to give to your partner and how much you want to keep to yourself ? in 0.5 increments.

Odd number player's payoff from this decision would be :

"Money": 5 euros (showing-up fee) + Your decision of keeping + Survey Bonus;

"Constrained Time": 30 minutes

Even number player's payoff from this decision would be :

"Money" : 5 euros (showing-up fee) + Your decision of giving + Survey Bonus

"Constrained Time" : 30 minutes

Here above is the example for "Money". The decision for "Time" would be the same but the changes in payoff is like :

"Money": 5 euros (showing-up fee) + Survey Bonus;

"Constrained Time": 30 minutes - Decision of "Time"

Enjoy !

3.7.3 Appendix C

Post-Survey and Questionnaire

First part questions: 1, Gender : A Female ; B, Male; 2, Age; 3, Study; 4 Religion

Second part questions (Pro-social index and anti-social index):

You will answer those questions from 0-4 scale measurement. All questions will be random mixed.

Never (0); few times(one or two times ,1); usually do it (three-four times, 2); often do it (five to six times 3); very often(more than six times,4)

Antisocial Measurement:

During the past 6 months, how often have you:

1, Cut classes or stayed away from school without permission;

2, Taken a car or other vehicle without the owner's permission, just to drive around;

3, Snatched someone's purse or wallet without hurting them;

4, Been drunk in a public place;

5, Broke in or tried to break into a building just for fun or to look around;

6, Broke in or tried to break into a building to steal or damage something;

7, Thrown objects such as rocks or bottles at people to hurt or scare them;

8, Set fire to a building or field or something like that just for fun;

9, Sneaked into a movie, ballgame or something like that without paying;

10, Steal money or take something that did not belong to you;

11, Beat up on someone or fought someone physically because they made you angry;

12, Purposely damaged or destroyed property that did not belong to you;

13, Attack someone with a weapon trying to seriously hurt them;

14, Sold illegal drugs such as pot, grass, has, LSD, cocaine, or other drug;

15, Used a weapon, force or strong arm methods to get money or things from someone;

16, Drive a car recklessly;

17, Cheat at school or other places;

18, Tell lies to people;

19, Sell stolen goods;

20,Write bad checks Use someone else's credit card without permission.

Prosocial Tendency Measurement (PTM):

During the past 6 months, how often have you:

1, Comfort someone I know after they experience a hardship;

2, Care for someone's child, animal , or home for free;

3, Offer to help someone I know with a difficult project;

4, Mentor a younger or less experienced person;

5, Do a task/choice for my friend, family member , colleague, etc;

6, Help care for a sick friends or relative;

7, Volunteer for a fundraiser;

8, Help out in a school/religious/community organization;

9, Volunteer for a philanthropic organization;

10, Donate old books, clothes, toys, etc to charity;

11, Donate my change to a charity;

12, Assist a stranger with a small task (e.g help them carry groceries, watch their things while use the restroom);

13, Help someone I know move their belongings to a new residence;

14, Make efforts to help a stranger who endured a hardship (send money or a kind message);

15, Send someone I know a supportive message;

16, Help a stranger find something they lost, like a piece of jewelry or pet;

17, Give directions to a stranger;

18, Lend someone an item that I care about, like a car or a favorite jacket;

19, Participate in a social/political movement;

20, Recycle.

3.7.4 Screen shot of Z-tree

Figure 3.4: Screen shot of Quiz problems

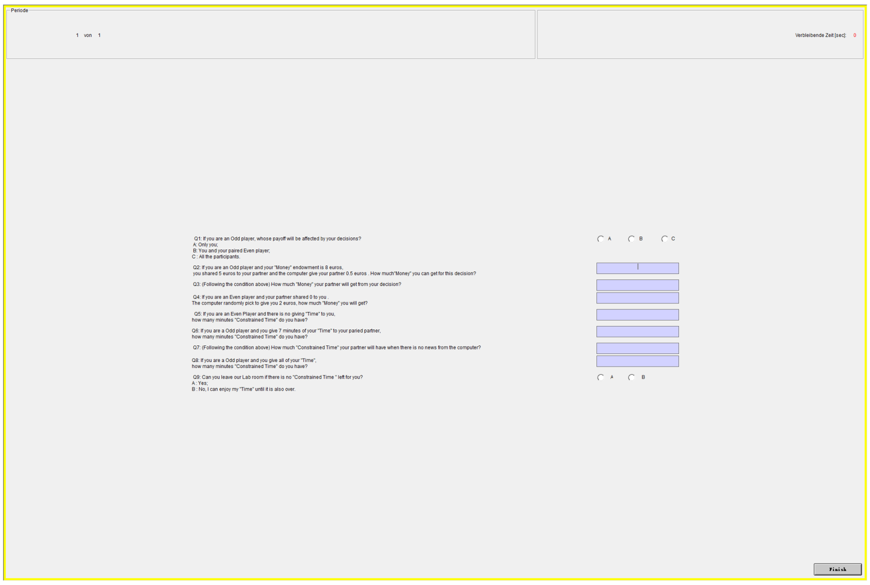


Figure 3.5: Screen shot of block information

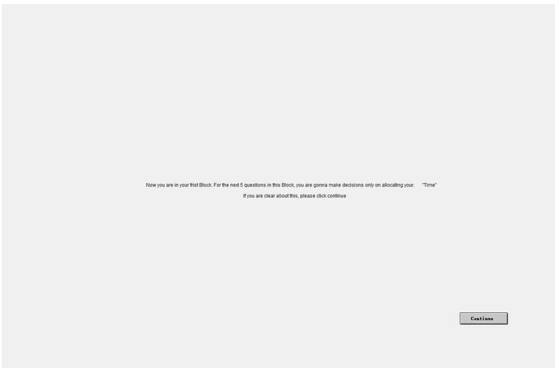


Figure 3.6: Screen shot of giving decision with other's giving (Time as example)

Figure 3.7: Screen shot of giving decision with out other's giving (Money as example)

Figure 3.8: Screen shot of X stage decision

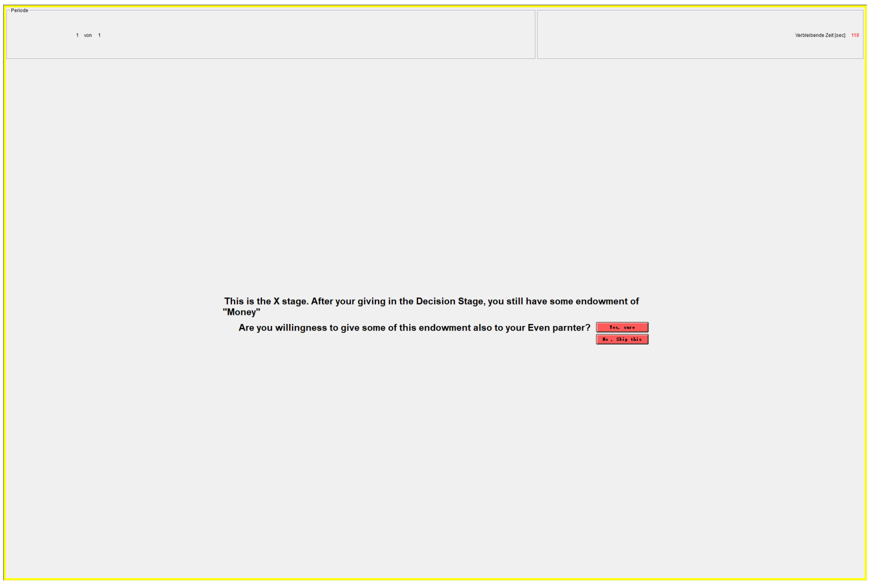


Figure 3.9: Screen shot of X stage giving decision

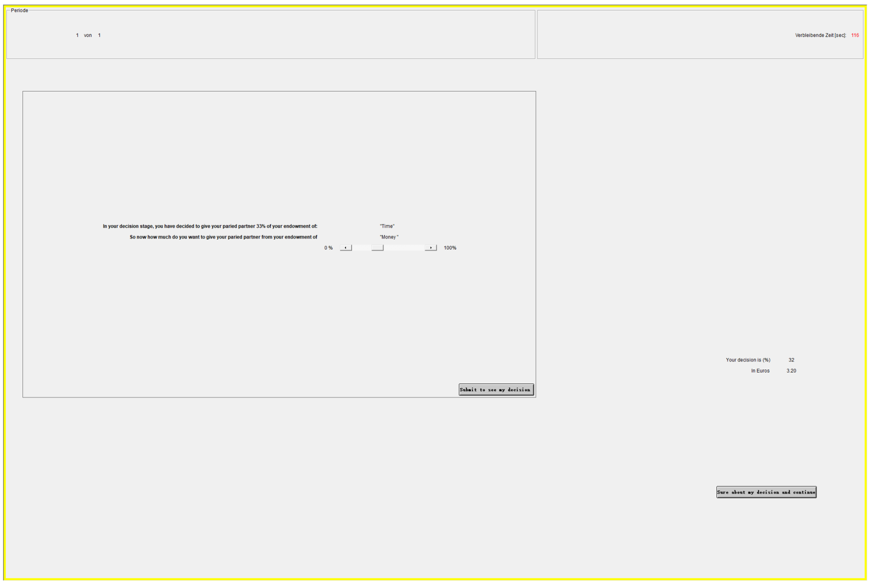


Figure 3.10: Screen shot of question to ask donors' belief

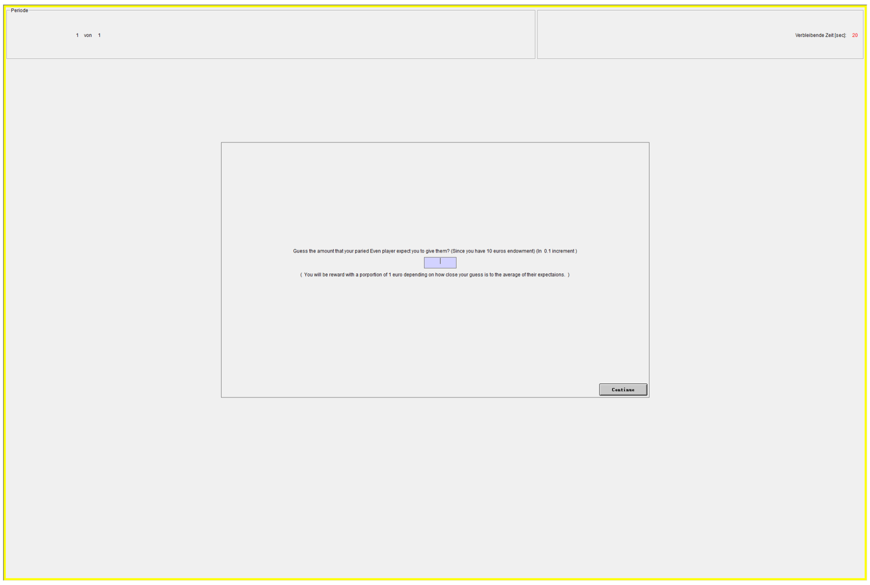


Figure 3.11: Screen shot of Constrained Time

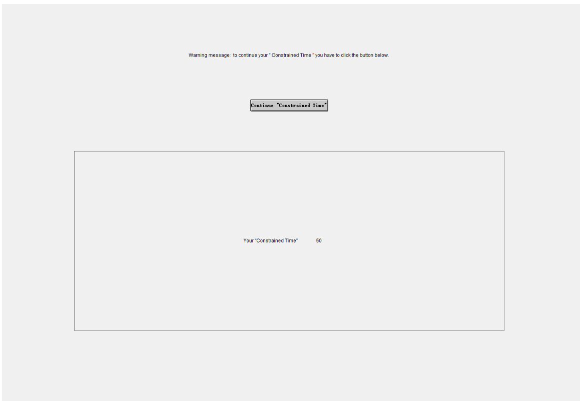
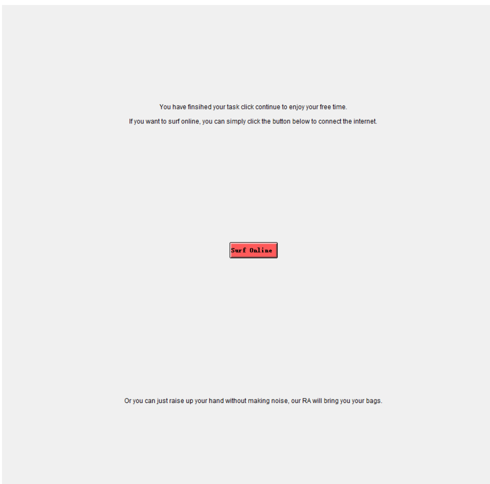


Figure 3.12: Screen shot of Time to enjoy in Time task



Chapter 4

Coordination Under Information Censorship

Abstract[1]

Leaders have the power to censor undesired information from subordinates to maintain power and extract surplus in the presence of the conflict of interests. This experimental study investigates the effect of censorship on collective decision-making and how it interacts with the possibility of communication among subordinates using a modified Coordinated Resistance (CR) game. In our game, payoffs are contingent on the state of nature. The leader is capable of censoring the state-related information before it reaches the subordinates, while the subordinates can coordinate on their decisions to resist or cooperate with their leader. We find that censorship works to affect subordinates' coordination decisions and benefit the leader in a bad state. However, when censorship is not allowed, and communication is allowed, we can get a higher total welfare.

[1]We are thankful for the constructive comments received from Jordi Brandts, Matthew Ellman. We gratefully acknowledge financial support from the Spanish Ministry of Economics and Competitiveness through Grant: ECO2017-88130 and through the Severo Ochoa Program for Centers of Excellence in R&D (SEV2015-0563) and the Generalitat de Catalunya (Grant: 2017 SGR 1136)

4.1 Introduction

4.1.1 Background

Authorities and autocratic regimes often use censorship to maintain their power and governance. As documented in the literature, two main censorship methods: content censorship and coordination censorship (see Ananyev et al. (2019)) are used quite often. The former suppress bad news such as an economic recession, which may undermine the public faith and, in turn, trigger collective action against the authority. The latter shuts down coordination among the public to forestall collective action.

Although censorship is mostly carried out by the government, it can also be used in other fields. For example, sellers are observed to delete the published negative feedback of their products, which other buyers are supposed to see. Censorship can happen when someone (some group) has the power to decide whether to censor the information which is supposed to be public to others. Censorship works mostly because of the uncertainty of unseen to protect the powerful person/group's interest, which might be undermined by the publication of the particular information.

In many cases, the existence of censorship is not a secret. What seems puzzling is that while the public is aware that the information they receive is tailored to the authorities' needs or not? Censorship remains to be a widely used and somewhat effective tool. To keep simplicity and generality, we mainly focus on investigating the censorship of information, also called as content censorship by Ananyev et al. (2019). While the other studies focused on the strategy of censorship from the side of the governor, this project would be also concentrated on the behavior of the respondent of the censored/non-censored information. We study the underlying mechanism that makes censorship work even when censorship is public knowledge through an interactive experiment.

The main information that they want to censor contains bad news to their interest in purpose. The optimal strategy for a leader is to choose an accurate report to enhance better cooperation between them and their subordinates in a good state and choose censorship to avoid the coordinated resistance decision of subordinates in a bad state (see Lorentzen (2014)). It would be easy for subordinates to infer true state through backward induction, which is that no news is equal to bad news (see Jin, Luca, and Martin (2015)). VPN[2] exists, but it is not officially allowed to use in most of these less democratic countries. Furthermore, there is a cost for them to get uncensored internet.

[2]One virtual IP which makes it possible to access censored internet.

However, the naive player might fail to consider no information because of naivety. This may also be affected by individuals' abilities of k-level thinking of the reason why leaders want to do this and their ignorance intention (see Enke (2015)). These contribute to the naivety of the subordinates, but they may also be uncertain about the governor's strategy. In this case, governor wants to censor the related information anyway. Nevertheless, the exploitation of this type of player will quickly disappear if one rational player is allowed to talk to each other to make this coordination decision (see R. Cooper et al. (1992)). In the real world, with a big population, at least one rational player can infer the purpose of the governor. Once they are allowed to talk, then everybody will become a rational player in the long run. Thus, we have two main research questions in this study: (1) How does information censorship work in a coordination game? (2)How this effect interacts with the possibility of communication between subordinates.

To address this censorship issue and give an answer to our research questions, we modified a Coordinated Resistance (CR) game by Weingast (1997) and combined it with a signaling game by Cho and Kreps (1987) based on our purpose. In this game, we have one leader and two subordinates to play a coordination game in two different states of nature: state X and state Y. Their payoffs are contingent on the state of nature. The leader player has the power to censor this state information before it reaches the subordinates players. The subordinates' players need to choose between cooperate with the leader or resist to the leader. The leader has an incentive to censor the information strategically. Because all players are interested in cooperation in state X, while there is a conflict of interest between them in state Y, in which subordinate players would prefer to coordinate on resisting the leader player, and the leader gets the lowest payment if subordinates successfully do so.

Following the structure of our modified CR game, we designed one-two (Censor or Not Censor) × two (Communication or Not Communication) treatments experiment. In particular, we name them as *NoCensorMute* treatment, *NoCensorChat* treatment, *CensorMute* treatment and *CensorChat* treatment, respectively. By comparing *NoCen- -sorMute* treatment and *CensorMute* treatment, we can single out the pure censorship effect when there is no possibility of communication. To identify the effect of communica- -tion on censorship, we can analyze the difference between *CensorMute* treatment and *CensorChat* treatment while take the *NoCensorChat* treatment as a control group for the effect of communication. In our experiment, we use the strategy method[3] to

[3]Brandts and Charness (2011) to present the first survey of the literature regarding the strategy method. Finally, no case do they find that a treatment effect found with the strategy method is not observed with the direct-response method.

get subordinates' decisions in three situations: situation X, situation Y and situation Unknown, and leaders' decisions in two situations: State X and State Y. For subordinates, situation X shows that they can receive the state information of X while in situation Y, they can observe the state information of Y. In the situation Unknown, subordinates can not see any information about the state. We will finally randomly pick one situation to pay them.

Our main results show that when there is no possibility of censorship, subordinates' behaviors follow the equilibrium, which is more likely to coordinate on cooperation with the leader in state X and coordinate on resistance with the leader in state Y. Meanwhile, they coordinated more on cooperation with the leader when they do not know the state information for a higher expected payoff by doing so. When communication is allowed, we find that it significantly increases their willingness to coordinate on cooperation in situation Y when the leader can not censor any information. However, the possibility of censorship drives them to less likely to cooperate if they could not see any information in this case which follows the refinement sequential equilibrium. Interestingly, if they can see the state information of Y when the leader has the power to censor, they are more likely to cooperate instead of resisting with the leader. Our results also suggest that censorship benefits the leader to earn more in state Y when subordinates can not communicate with each other. Because fewer subordinates successfully coordinate in state Y in this case. Nevertheless, when censorship is not allowed, and communication is allowed, we can get the highest total welfare.

The outline of this paper is as follows: Section 4.1.2 presents the literature review related to our study. Section 4.3 explains the details of our Coordination Resistance game. Section 4.3 shows the experimental design and the hypotheses. The results from the experiment are presented and discussed in Section 4.4. Finally, Section 4.5 concludes and discusses the results of the paper.

4.1.2 Literature review

In this section, we discuss previous literature related to censorship. From the very beginning, censorship may be treated as one type of information disclosure. However, there is an essential difference between these two. The information disclosure is a strategically voluntary behavior to choose whether or not to disclose the private information, while censorship is to withhold information which is supposed or regulated to be public to everyone with this concern. Many researchers have studied strategical information disclosure, in particular the context of the disclosure, timing of disclosure (see, e.g., Milgrom (2008), Acharya, DeMarzo, and Kremer (2011) Jin, Luca, and Martin (2015)).

More than half of censorship can be found in political activities since it closely fits some authorities' purposes. For example, Shadmehr and Bernhardt (2015) characterize a ruler's decision of whether to censor media reports that convey information to citizens who decide whether to revolt. They find that a ruler gains under some specific conditions and a bad ruler prefers very strong media. In a recent study by Ananyev et al. (2019) classify censorship into content censorship and coordination censorship based on what authorities censored. They provide evidence that when citizens use information and communication technologies for protest coordination, Governments can respond by restricting (censor) access to these technologies used for coordination, which can be found in Egypt, Russia, and China. Furthermore, [King, Pan, and Roberts (2013)] suggests that the censorship program is aimed at curtailing collective action by silencing comments using a large scale of media data from China, which means that the resistance decision we should consider is a coordination decision and successful coordination is highly related to effective communication. Roberts (2018) focused on the censorship in China and compared to information manipulation in other countries. It summarizes the existence, the method, the discussion, and the effect of this censorship in many aspects.

Apart from political activities, Smirnov and Starkov (2018) shows the existence of information censorship in the business market in which sellers often have the power to censor the reviews of their products. They find that bad reviews are worth revealing and allow the high-type seller to use them as a costly signal of his product's quality to rational consumers. Hauser (2020) also study how a firm manages its reputation by investing in the quality of its product and censoring bad news. However, He shows theoretically that without censorship, the threat of bad news provides strong incentives for investment. Meanwhile, disclosing the information later than it supposed to be is also one kind of censorship. For example, Gratton, Holden, and Kolotilin (2017) show that, in equilibrium, bad senders release information later than good senders, and they also find empirical support for this using data on the timing of U.S. presidential scandals and U.S. initial public offerings.

The literature above provides empirical evidence for the existence and the characteristics of censorship. Here we will also discuss previous experimental and theoretical studies of censorship. To the best of our knowledge, Chen and Yang (2019) conduct a first field experiment in China to measure the effects of providing citizens with access to an uncensored internet. They calibrate a simple model to show that the low demand for uncensored information and moderate social transmission means China's censorship apparatus may remain robust to a large number of citizens receiving access to an uncensored internet. Sun (2020) model censorship as a dynamic game between an agent and an evaluator when there are two types of public news, good and bad news, are informative about the agent's ability. However, the agent can hide bad news from the evaluator, at some cost, and will do so if and only if this secures her a significant increase in tenure. However, they do not fully focus on understanding the underlying mechanism of censorship methodology, and neither the coordination between citizens in their studies. In our study, we address this coordination problem between citizens in a coordination resistance game and study this censorship experimentally based on the game we modified.

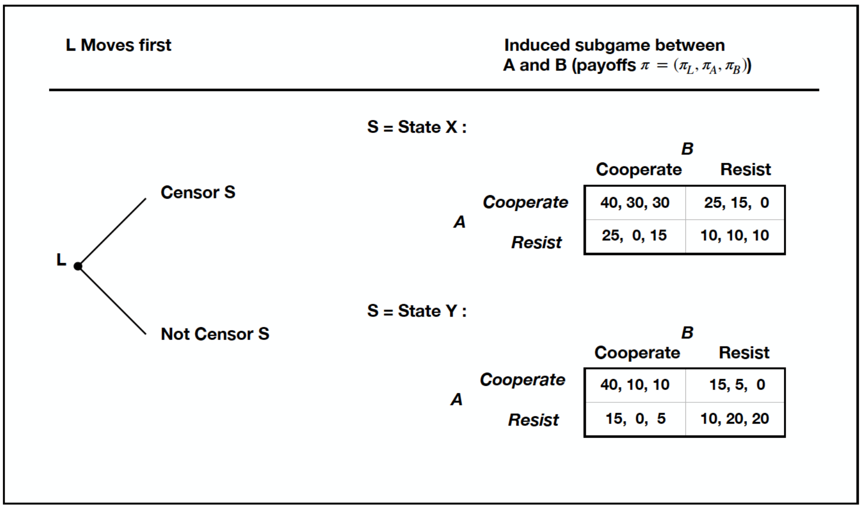
4.2 Modified coordinated resistance game

We modified a Coordinated Resistance (CR) game firstly introduced by Weingast (1997) and combines it with the features of a signaling game from Cho and Kreps (1987) to analyze coordination decisions under censorship.

The game involves three agents: a leader (player L) and two symmetric subordinates (player A and B). Before agents make their decisions, Nature determines the state of the world S from a state space $S = \{X, Y\}$ with a probability $p_x = p$. Upon observing the true state S, player L moves first to decide whether to censor this information S before it reaches player A and player B, denoted by an indicator variable I, which equals to 1 if player L censors the information and 0 otherwise. Censorship entails no cost. Following player L's action, Player A and B observe one of the messages from the set: $m \in \{X, Y, \}$. Next, player A and B simultaneously choose whether to cooperate(C) with player L or to resist(R) player L, denoted by a pair $a = (a_A, a_B) \in \{C, R\} \times \{C, R\}$.

The payoffs of three agents depend on the true state S and the actions a from subordinates players (player A and player B), as given in the Figure 4.1 below. Each cell in the table represents a payoff vector of $\pi = (\pi_L, \pi_A, \pi_B)$. Note that the leader is always better off than the subordinates, except in cases where player A and B coordinate to resist. Given their choices, all players are better off in state X than in state Y unless coordinated resistance occurs. In the "good" state X, players' interests are aligned, and everyone prefers full cooperation. However, the "bad" state Y creates a conflict of interest between the leader and the subordinates: player L prefers full cooperation from player A and player B, while the latter can earn higher payoffs by coordinated resistance, which produces the worst outcome for player L. In either state, the payoff structure induces a coordination problem between player A and player B and imposes a high risk on the decision to resist since a coordination failure gives nothing to the player who resists alone.

Figure 4.1: Payoffs for Leader-Subordinates Coordination Game



When player L can not censor the information, the game degenerates to a coordination game with complete information between player A and B. In state X, the coordination game has only one Nash equilibrium (C, C). In state Y, there are two Nash equilibria (C, C) and (R, R). (C, C) is the risk-dominant equilibrium, and (R, R) is the payoff-dominant equilibrium. To simplify the hypotheses, here we assume that player A and player B are payoff maximizer and strictly prefer (R, R) to (C, C) in state Y.

When player L has the ability to censor the information, the game is essentially a signaling game whose sub-game is a coordination game between player A and player B. Think of the state of the world as the "type" of the leader. The leader chooses whether to disclose information about her type to the subordinates. The subordinates choose whether to cooperate, depending on the leader's type. The assumption that they are only willing to cooperate with the good leaders provides an incentive for the bad leader to hide her type. The game is presented in the extensive form in Figure 4.1. In this discussion section, we take it as given that coordination between A and B is always successful and postpone our discussion about coordination failure to the next section.

This signaling game has multiple equilibria. The most interesting equilibria that contribute to our conjectured hypotheses are as follows. There is a separating equilibrium in which the leader censors the information only when $S = Y$ and the subordinates cooperate only if they see a message $m = X$. Moreover, a sequential equilibrium satisfying the D1 Criterion (See Cho and Kreps (1987)) exists in which the subordinates use the same strategy, and the leader never censors any information. The intuition is simple: the leader will not benefit from censoring the information when $S = X$; only when $S = Y$ does the leader want to hide such information because she will be better off given certain out-of-equilibrium beliefs of the subordinates. D1 criterion requires that, if whenever the good type either wish to deviate from her equilibrium strategy or indifferent given the subordinates' choices and the bad type strictly wish to deviate by sending a message m, then the subordinates should believe that the message m is infinitely more likely from the bad type.[4] Therefore, the bad type will not benefit from censoring her information since subordinates can infer her type from her censorship. This equilibrium is equivalent to the previous one in terms of outcome. Finally, there is one more pooling equilibrium in which the leaders always censor the state information, and the subordinates cooperate. Our hypotheses will mainly be based on these discussions and check which Nash equilibrium that our subjects will follow or deviate in what environments.

4.3 Experimental design and hypotheses

4.3.1 Experimental treatments

Table 4.1 summarizes all treatments in our experiment. We implement a 2 × 2 design varying whether the leader can censor the information or not and whether the subordinates can communicate or not. Communication is computerized free-form chat. Each participant participates in one treatment.

Three participants are ranomly matched to play the game. Each participant is assigned either as a leader (player L) or as a subordinate (player A or B).

In the *NoCensorMute* treatment, the leader cannot censor any information and is, therefore, a passive player. Subordinates simultaneously decide whether to cooperate or resist without the possibility of communicating with each other. We use the strategic method[5] to elicit subordinates' complete profile of the strategy. Specifically, we ask them to make a decision in each of the three situations: (1) Situation X: when they see $S = X$ (SXS); (2) Situation Y: when they see $S = Y$ (SYS); (3) Situation

[4] Jin, Luca, and Martin (2015)

[5] Brandts and Charness (2011) present a survey of experimental studies that compare behaviors under the strategy method versus that under the direct-response method of eliciting decisions. They do find no difference from most of the studies.

Table 4.1: Overview of the treatments employed for a pilot

Treatment	Censorship	Communication
NoCensorMute	No	No
NoCensorChat	No	Yes
CensorMute	Yes	No
CensorChat	Yes	Yes

Note: Communication is a free-style talk for one minute between Player A and Player B.

Censorship implies that player L has a power to censor the information of state.

Unknown: when they do not see any information about the state $S =$ but $S = X$ with probability $p_x = 0.5$ ($S0S$). For all situations, they have maximal five minutes to reach their decisions. One of the three situation will be randomly selected, and payoffs are determined by subordinates' coordination outcomes in the selected situation.

In the *NCensorChat* treatment, the leaders remain passive. Subordinates can communicate with each other for five minutes intotal before they make contingent decisions for all situations.

In the *CensorMute* and *CensorChat* treatments, the leader player can censor the state information before it reaches the subordinates, and it is common knowledge to all players. Like above, we use a strategy method to elicit leaders' censorship strategy in each of the two states: (1)State X: when $S = X$ (SXL); (2)State Y: when $S = Y$ (SYL). Subordinates in these two treatments also make contingent decisions in three different situations: (1)Situation X: when they see a message $m = X$ (MXS); (2) Situation Y: when they see a message $m = Y$ (MYS); and (3)Situation Unknown: when they see a message $m =$ ($M0S$). The computer chooses a state with $p_x = 0.5$ first, and payoffs are determined by the leader's strategy together with the subordinates' coordination outcomes accordingly. The difference between *CensorMute* treatment and *CensorChat* treatment is whether the subordinates can communicate for five minutes for all situations before making contingent decisions.

4.3.2 Hypotheses

Here in this section, we mainly discuss our hypotheses to be tested in the experiment. Following the discussion of equilibrium (see section 4.2), we know that, if they can observe the state information, subordinates would prefer to coordinate on cooperation in state X and coordinate on resistance in state Y since these two strategies refer to the highest payoff in each state. When there is no possibility of censorship, what they see is what state is, so they should behave as the equilibrium that we discussed. While the censorship is possible, sine they can infer the leader's censorship behavior from not seeing any information following the refinement equilibrium,[6] so they will also follow the equilibrium of knowing the state information. When they could not see any information and the leader cannot censor any information, we assume here that they follow the utility maximization to coordinate on cooperation because it returns the highest expected payoff. So that We mainly construct 4 hypotheses between treatment *NoCensorMute* and *CensorMute* as below.

Hypothesis 4.1 *If subordinates can observe the information of State X, then they will coordinate on cooperation with the leader.*

Hypothesis 4.2 *If subordinates can observe the information of State Y, then they will coordinate on resistance with the leader.*

Hypothesis 4.3 *If subordinates can not observe any information about the states while leader cannot censor the information, then they will coordinate on cooperation.*

Hypothesis 4.4 *If subordinates can not observe any information about the states while leader can censor the information, then they are more likely to cooperate on resistance.*

As for the communication treatments, our hypotheses will based on the previous studies of communication in coordination game. For example, R. Cooper et al. (1992) present experimental evidence on nonbinding, pre-play communication in bilateral coordination games. In the game that one strategy is less "risky", two-way communication always leads to the Pareto-dominant Nash equilibrium. Also Brandts and D. J. Cooper (2007) study manager-employee interactions in experiments set in a weak-link game. They find that communication is a more effective tool than incentive changes for leading

[6] Jin, Luca, and Martin (2015) studied that when there is no news, people treated this as a bad news, which also suggested the prediction of their behavior.

organizations out of performance traps. They all suggest that the free-formed communication can help them to overcome the coordination failure. So that we construct our hypothesis 4.5. Meanwhile, when communication is allowed, it increases the possibility of an interaction between one smart subordinate and one naive subordinate. It turns out that the risk of coordination failure decreases. So they are expected to follow the equilibrium to coordinate on resistance when they could not see the information in treatment *CensorChat* and *CensorMute*.

Hypothesis 4.5 *If subordinates can observe the information of state, communication between subordinates will drive them more likely to cooperate on cooperation in state X and coordinate on resistance in state Y.*

Hypothesis 4.6 *When subordinates cannot observe any information about situation, communication between subordinates drives them more likely to cooperate on resistance in treatment CensorChat than treatment CensorMute*

As for the leader's censorship strategy in our game, we follow the refinement equilibrium that we discussed in section 4.2 to be simplified and keep consistent with other hypotheses. So the leader do not censor any state information, since subordinates can refer the state by observing censorship. we have one hypothesis that

Hypothesis 4.7 *Leaders won't censor any information no matter it is state X or state Y.*

4.3.3 Experimental procedure

We ran this online interactive experiment through the Lioness platform (see Giamattei et al. (2020)) and recruit participants through Prolific on the May of 2020. For this study, we have 128 participants with an average payment of 2.5 pounds. We have one session for all treatments. One-third of them will be randomly assigned as the role of leader players, called 'red player', and two-thirds of them are assigned as the role of subordinate players, called 'blue player' in the experiment. In each treatment, we have around 32 participants[7], and one leader player is randomly matched with two subordinate players. Our session lasts for only one round, which takes around

[7]Because of some unexplainable internet connection problem, some participants can not remain to be connected until the end of our experiment. We delete some inactive players, so we do not have exactly same number of players in each treatments. 32 is an average number.

15 minutes. To screen the participants, each of them has to do a warm-up exercise to learn how to read the payoff matrix successfully and then matches with others as a group to enter the instruction stage. This stage includes the instruction and comprehension test for players to understand the context (see details of instruction in section 4.6.1 and 4.6.2 in Appendix). They make their coordination decisions after they submitted correct answers in the comprehension tests. After they made their decisions, we run one post-questionnaire, which includes demographic questions and a belief-related questionnaire for us to understand their strategy. In the belief-related questionnaire, they are asked to guess the decisions of other members in their group (see screenshot in section 4.6.3 of Appendix). They can earn some bonus if they guess it correctly.

4.4 Experimental results

In this section, we present the experimental results of our study. We start by showing the coordination outcomes at group level across treatments and the total welfare per group. Then, we continue by exploring subjects' individual decisions for both roles and analyze the reason why they make such decisions. Meanwhile, we study the payoffs for both types of players to see who get a higher benefit. Then we compare the leader's belief versus the subordinate's belief in our post-questionnaire. To keep simplification, We sometimes refer to *NoCensorMute* treatment as NCM; *NoCensorChat* treatment as NCC; *CensorMute* treatment as CM and *CensorChat* as CC. Meanwhile, we also use Sit.X, Sit.Y and Sit.U to represent the situation X, in which they see the state information is X, situation Y in which they see the state information is Y, and situation Unknown, in which they see no state-related information, respectively. In our experiment, one group is independent with other group, since we only have one round without any re-matching. For testing the mean difference across treatments, we use the Mann–Whitney U test here. For testing the mean difference within treatments across situation, we use the Wilcoxon signed-rank test on individual data.

4.4.1 Coordination outcomes at group level

In this section, we present the coordination outcomes at the group level across treatments and situations in the Table 4.2. Since our study is online, some of the participants often encountered in a connection problem. We will count those subjects as active players if they remain in the experiment until the end and successfully submitted their decisions and answers. In Table 4.2, We have (A, A), which represents the strategy of coordination on cooperation, (B, B), which represents the strategy of coordination on resistance and (A, B) which means that they could not reach coordination. As we can see from this table, in situation X for all treatments, more than half of active groups choose to coordinate on the strategy (A, A). Specifically, almost 85.71% of the groups in treatment *NoCensorChat* coordinate on cooperation with the leader in this situation X. In censorship treatments of *CesnorMute* and *CensorChat*, only 30% and 24.14% of active leader subjects choose to censor the state information when the situation is X. It seems to support the Hypothesis 4.1 and Hypothesis 4.7. However, in situation Y, coordination on resistance is not a consistent strategy for all treatments, especially the treatment *NoCensorChat* and *CensorMute*. Meanwhile, 85% and 44.83% of active leader subjects choose to censor the state information of Y in treatment *CensorMute* (14.29%) and treatment *CesnorChat* (16.67%), respectively. Once the leader knows that subordinates can communicate with each other, they are less likely to censor the state information of Y.

Table 4.2: Coordination outcomes at group level in each situation

		NoCensorMute	NoCensorChat	CensorMute	CensorChat
Sit. X	(A, A)	**66.67%**	**85.71%**	**50%**	**75%**
	(B, B)	0%	0%	33.33%	0%
	(A, B)	33.33%	14.29%	16.67%	25%
	Censor	-	-	**30%**	24.14%
Sit. Y	(A, A)	0%	28.57%	16.67%	25%
	(B, B)	**50%**	**14.29%**	**16.67%**	**50%**
	(A, B)	50%	57.14%	66.67%	25%
	Cesnor	-	-	**85%**	44.83%
Sit. U	(A, A)	**83.33%**	**57.14%**	**33.33%**	**100%**
	(B, B)	0%	14.29%	33.33%	0%
	(A, B)	16.67%	28.57%	33.33%	0%
Other	(A/B)	40%	30%	40%	68.42%
P.Communication	-	-	50%	-	32.57%
Total observations		30	30	30	38

When subordinate players do not know the state information, they are more likely to coordinate on cooperation, except for those players in treatment *CensorMute*. It seems that some of them inferred that the leader censored the state information of Y, so switched to choose to coordinate on the resistance the leader compared to the subordinate subjects in treatment *NoCensorMute*. The term "Other" shows the percentage of inactive groups in each treatment. All of these groups have one subordinate player dropped out and did not remain in the experiment until the end. As you can see from our table, in treatment *CensorChat*, we only have less than 40% of active groups. It may suggest that the result in this treatment may be a little random.

As for the communication, we define that one group has effective communication only when both of the subordinates in that group successfully send decision-making related messages to each other. The P.Communication in the Table 4.2 shows the percentage of effective communication groups. In our online study, we have 50% of the groups had effective communication in treatment *NoCensorChat* and only 32.57% in treatment *CensorChat*. Probably because the internet connection is not very good, sometimes they could not send or receive messages.

Result 4.1 *(i) Subordinates are more likely to coordinate on cooperation in situation X for all treatments (Hypothesis 4.1); (ii) Subordinates can not coordinate on resistance in all treatments, and it goes against the Hypothesis 4.2; (iii) In situation U, subjects are more likely to coordinate on cooperation except the subjects in treatment CensorMute, in which they do not prefer to do so (Hypothesis 4.3 and 4.4).*

Then we applied OLS linear regression on all these strategies to check if there is any significant treatment difference based on our data. In the Table 4.3, we treat the treatment *NoCensorMute* as control and run OLS regression on subordinate coordination strategies in each situations across each treatments. In situation Y, when communication is allowed, and censorship is not allowed, we find that more subordinates are willing to coordinate on cooperation instead of resistance with a p-value level of 0.05.

Table 4.3: OLS regression of Treatment effect on coordination outcomes

		Sit.X			Sit.Y			Sit.U	
Strategy	(A,A)	(B,B)	(A,B)	(A,A)	(B,B)	(A,B)	(A,A)	(B,B)	(A,B)
NoCensorMute	(Base)	(Base)	(Base)	(Base)	(Base)	(Base)	(Base)	(Base)	(Base)
NoCensorChat	0.190	1.17e-16	-0.190	**0.286***	**-0.357***	0.0714	-0.262	0.143	0.119
	(1.31)	(0.00)	(-1.42)	(2.39)	(-2.53)	(0.45)	(-1.93)	(1.40)	(0.91)
CensorMute	-0.167	0.333***	-0.167	0.167	**-0.333***	0.167	**-0.500*****	**0.333****	0.167
	(-1.10)	(4.03)	(-1.20)	(1.34)	(-2.27)	(1.01)	(-3.54)	(3.15)	(1.23)
CensorChat	0.0833	1.20e-16	-0.0833	0.250	-3.64e-17	-0.250	0.167	3.64e-17	-0.167
	(0.49)	(0.00)	(-0.54)	(1.80)	(-0.00)	(-1.36)	(1.06)	(0.00)	(-1.10)
Constant	0.667***	-9.71e-17	0.333**	-1.11e-16	0.500***	0.500***	0.833***	0	0.167
	(6.25)	(-0.00)	(3.39)	(-0.00)	(4.82)	(4.29)	(8.35)	(0.00)	(1.74)
Observations	69	69	69	69	69	69	69	69	69

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

When we check the details of their communication, we find that two groups in treatment *NoCensorChat* showed their pro-social preference to be fair to the leading players when the leaders' payoff cannot do anything. While in the situation unknown of treatment *CensorMute*, their collective decisions go in the opposite direction. While the leader can censor the information and subordinates cannot observe any state information, it decreases their willingness to coordinate on cooperation and increases their willingness to coordinated on resistance with a p-value level of 0.001. The interesting thing is that when the leader has this ability to censor the state information, but the subordinate observed the state information of Y, they are less likely to coordinate on resistance with only a p-value of 0.05. Probably because that subordinates realized that the kindness of a leader's decision of no censorship, they also choose to be good/fair to them, as for the significant effect of censorship on subordinates' coordinated resistance in observing state X in treatment *CensorMute*. We could not find a reasonable explanation, and the one group of them may cause it to happen to make a random decision and happen to coordinate on resistance in online study. Alternatively, they notice the leader's power, and they do not want the leader to earn a higher payoff than them.

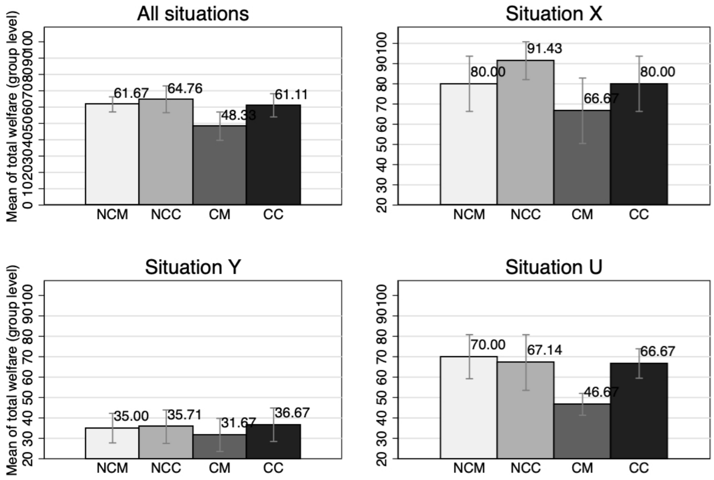
Result 4.2 *(i) The effect of communication on their coordination decision is significant in situation Y, the bad state. It encourages subjects to cooperate on cooperation with the leader while the leader can not do anything to mislead them (Not for Hypothesis 4.5); (ii) The effect of censorship is also significant and it drives them to coordinate on resistance more, when they could not observe any state information (Hypothesis 4.4).*

4.4.2 Total welfare at group level

Then we discuss the total welfare per group for each situation across treatments in this section. Figure 4.2 shows the average accumulated payoff within groups across treatments. The number in Figure 4.2 is displayed as the experimental points, and in total, the highest payoff for each group is 100 points. The sub-figure with a subtitle of "All situations" presents the average total welfare considering all situations. Our results suggest that subjects in treatment *CensorMute* seem to have relatively lower average total welfare than other treatments in all situations.

It seems that in state X, which is a good state, no censorship and communication can contribute to a higher total welfare per group. However, when we run the Mann–Whitney U test to check their mean difference, we can only find that the average total welfare per group in treatment *CensorChat* is significantly higher than that in treatment *CensorMute* at p-value level of 0.05 (P-value = 0.019). If we consider the p-value level of 0.5, then except situation Y, average total welfare in treatment *CensorMute* is significantly lower than all other treatments. Probably because we only have few active groups for comparing the mean difference at the group level across treatments. If we increase the data size, we may observe a more significant result. Again we also checked the mean difference between situations and within a treatment. Our Wilcoxon signed-rank test shows that except for the treatment *NoCensorMute*, in all other treatments,

Figure 4.2: Total welfare per group for all treatments



Note: Data in Experimental points

the total welfare in situation X is significantly higher than that in situation Y with a p-value of 0.05.

Result 4.3 *When the leader endogenously use the censorship strategy and subordinates cannot communicate with each other, the whole group will get relatively lower welfare.*

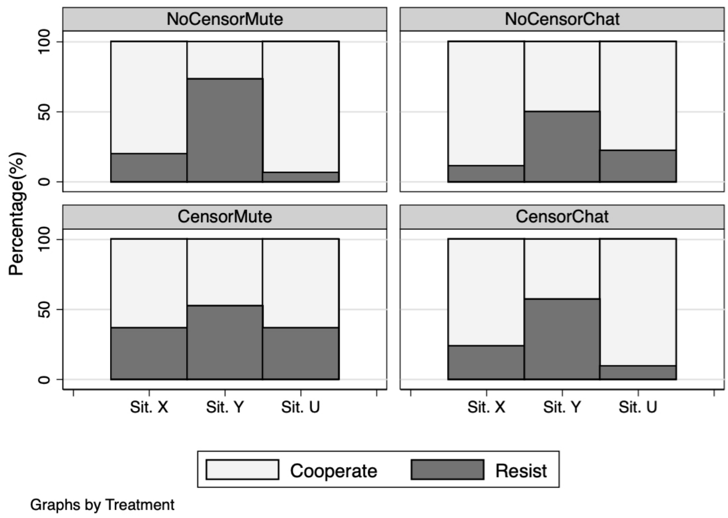
4.4.3 Individual decisions

In this section, we present individual decisions for both subordinate subjects and leader subjects in all situations and across treatments. Firstly, we show the results of subordinates' individual decisions to see how they make decisions to cooperate or resist. Then we also run the OLS linear regression to test the treatment effect on their individual decisions. Then we analyze individual decisions between censor or not censor the state information for leaders.

4.4.3.1 Individual decisions of subordinates

In Figure 4.3, we show the percentage of subordinate subjects choose to cooperate or resist in our experiment including also the data from these inactive groups. Each bar compares the percentage of subordinates' decisions between cooperate with a light grey color and resist with a deep grey color in one situation. Each sub-figure shows all subordinates' decisions in all situations for one specific treatment. All the sub-figures have the same y axis, so we observe the comparison through their heights.

Figure 4.3: Individual decisions for subordinates



We can see for their individual decisions, the highest cooperation rate in seeing state X information is in treatment *NoCensorChat* while the highest resistance rate in seeing state Y information is in *NoCensorMute*. When they could not observe any state information, the highest resistance rate is in treatment *CensorMute*. Specifically, when we compare it with the baseline treatment *NoCensorMute*, it seems that the actual censorship when subordinates are knowledgeable about this power of leader drives subordinates more likely to resist individually. However, our OLS regression did not find a significant censorship effect. Table 4.4 presents the OLS regression result of the individual decisions (1: Cooperate, 0: Resist) on treatment dummy variables and control variables, including their gender, risk level and also their beliefs of the other subordinate's decision in their group. Interestingly, we find a significant result that when the leader has this power but did not choose to censor the state information of Y, subordinates are more likely to cooperate individually in state Y.

Based on our OLS regression, we find that their decisions to cooperate and resist depending mostly on their belief of their partners' decision[8]. So these results suggest that if the leader does not censor the state information of Y, then subordinates are willing to cooperate with the leader.

Result 4.4 *Subordinates' willingness to cooperate with the leader is significantly higher when the leader didn't censor the state information of Y even they have this power to do so.*

[8]We did check the collinearity between all the explanatory variables. We did not find any severe collinearity among them.

Table 4.4: OLS on subordinates' individual decisions

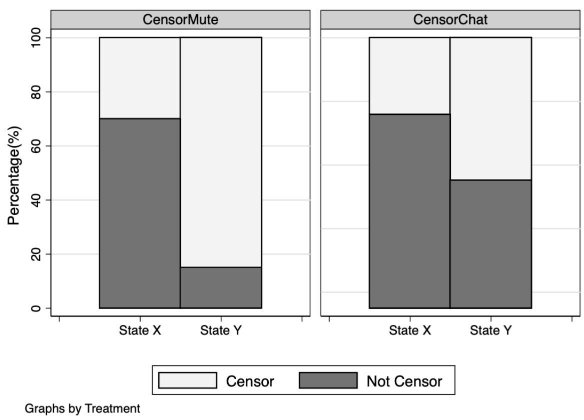
	(1) Sit.X	(2) Sit.Y	(3) Sit.U
NoCensorMute	(Base)	(Base)	(Base)
NoCensorChat	0.0594	0.170	-0.109
	(0.45)	(1.22)	(-0.79)
CensorMute	-0.0694	0.274*	-0.191
	(-0.53)	(2.02)	(-1.43)
CensorChat	-0.0181	0.185	0.0780
	(-0.14)	(1.39)	(0.58)
Gender	-0.0684	-0.0316	-0.233*
(1:Female, 0: Male)	(-0.78)	(-0.35)	(-2.57)
Risk	-0.0420*	-0.0118	-0.0163
	(-2.08)	(-0.58)	(-0.83)
Belief of others in Sit.x	0.421***		
	(3.78)		
Belief of others in Sit.Y		0.677***	
		(7.21)	
Belief of others in Sit.U			0.230*
			(2.34)
Constant	0.744***	0.0706	0.916***
	(3.96)	(0.46)	(5.40)
Observations	70	71	70

t statistics in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

4.4.3.2 Individual decisions of leaders

In treatment *CensorMute* and *CensorChat*, leaders have the power to censor the state information, which is supposed to be public to all subjects. The decision of censor or not censor will decide all subjects' payoffs in the experiment. Figure 4.4 shows the individual decisions of leaders in these two censorship treatments and two states. We have 18 independent active leaders in these two censorship treatments, including seven from the treatment *CensorMute* and 11 from treatment *CensorChat*.

Figure 4.4: Individual decision of leaders



The two sub-figures imply that in state Y, leaders are more likely to censor the state information. Comparing the censorship rate in state Y between these two treatments, leaders censored more if subordinates could not communicate with each other. Then we also run OLS regression of the leader's censorship decision to check the existence of treatment effect on a leader's censorship decision.

Table 4.5: OLS regression on leader's individual decision

	(1) Decision	(2) Decision	(3) Decision
Communication	-0.230*	-0.402**	-0.567*
(1: Chat 0: Mute)	(-2.45)	(-3.06)	(-2.18)
State	-0.347***	-0.550***	-0.571*
(1:X 0:Y)	(-3.75)	(-3.85)	(-2.42)
Communication × State		0.343	0.349
		(1.85)	(1.11)
Gender			-0.239
(1:Female 0: Male)			(-1.14)
Risk			0.0464
			(0.84)
Belief of subordinates in Sit.Y			-0.135
			(-1.04)
Belief of subordinates in Sit.U			-0.00481
			(-0.05)
Constant	0.748***	0.850***	1.119*
	(8.71)	(8.40)	(2.31)
Observations	98	98	32

t statistics in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

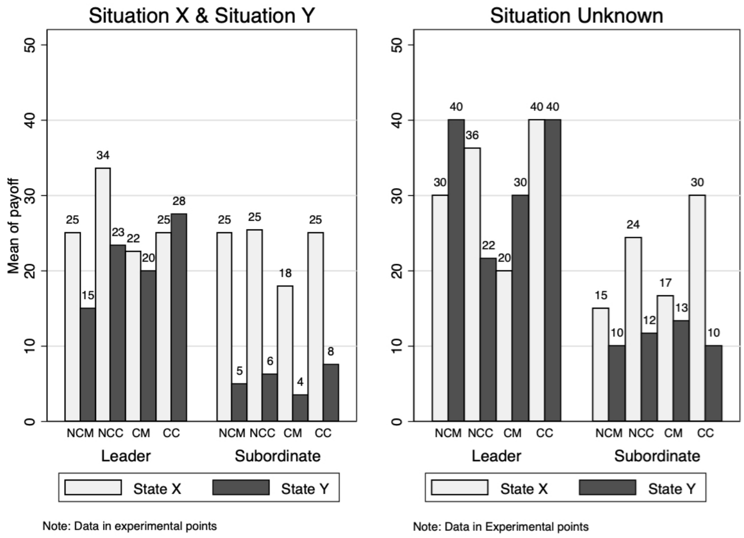
Table 4.5 shows the OLS regression results of leader's decisions only for treatment *CensorMute* and *CensorChat*. The variable "communication" is a dummy variable for indicating the treatment *CensorMute* with a value of 0 and the treatment *CesnorChat* with a value of 1. Table 4.5 implies that the possibility of communication between subordinates significantly decreases a leader's willingness to censor the state information on average. Furthermore, if the state is X, the leader prefers less to censor this information than if the state is Y. These two results are consistent even when we add the interaction term of "Communication × State" and other control variables. However, we did not find a significant effect on the interaction term.

Result 4.5 *(i) Leaders are more likely to censor the state information of Y, which is against the Hypothesis 4.7; (ii) When communication between subordinates is allowed, leaders are less likely to censor any state information.*

4.4.3.3 Individual payoffs of leaders and subordinates

In Figure 4.2, we present the total welfare across treatments and situations. Here in this section, we show the average individual payoffs in Figure 4.5 for leaders and subordinates, respectively. The sub-figure on the left of Figure 4.5 displays the leader and two subordinates' average payoff per person in situation X and situation Y. We can see, in State Y, a leader earns much more than the subordinates. The payoffs for both roles are more similar in situation X. When the state information is unknown (see the sub-figure on the right in Figure 4.5), leaders earn more if it turns out to be state Y across all treatment comparing to the case that the state information is known, except the treatment *NoCensorChat* where the two subordinates can communicate.

Figure 4.5: Average payoff of leaders and subordinates



It suggests that the optimal strategy for leaders is to censor the information of state Y when the subordinates can not talk. At the same time, we observed that subordinates earn more if the state is Y, and this information is unknown to them than if the state is Y, and they know this information. It seems that when they could not see any state information, they have more coordination, which contributes to their higher final payoff. Overall, we do find that leaders' payoffs are significantly higher subordinates on average overall and in situation Y also. We probably do not have so much data, so we could not find a lot of significant results.

Result 4.6 *Censorship benefits leaders in state Y and subordinates earn less if the state is Y and this is known to them.*

4.4.3.4 Leaders' belief vs subordinates' belief

Since we already get some ideas about whether or not subjects will make their decisions across treatments. In this section, we discuss the beliefs of leaders and subordinates.

Table 4.6 shows all subjects' beliefs, which they answered in our post-questionnaire, and they will be rewarded with a bonus if they get the correct answer. Since we care more about the difference between leaders' decisions in situation Y and situation U, so we only asked and provided these two types of belief in our table. To keep the richness of our data, we also asked leaders in non-censorship treatments the same questions. Compare this belief with the previous Table 4.2 of coordination outcomes, we find that in the non-censorship treatments of *NoCensorMute* and *NoCensorChat*, leader's guess of the coordination outcome is not the same but quite similar.

Table 4.6: Leaders' belief vs subordinates' belief

	Leaders' belief of subordinates' decisions			
	NoCensorMute	NoCensorChat	CensorMute	CensorChat
	Both choose A			
Situation Y	33.33%	25%	**0%**	33.33%
Situation unknown	66.67%	50%	**42.86%**	44.44%
	Both choose B			
Situation Y	44.45%	62.5%	**71.42%**	44.45%
Situation unknown	11.11%	37.5%	**28.57%**	33.33%
	One choose A and one choose B			
Situation Y	22.22%	12.5%	28.58%	22.22%
Situation unknown	22.22%	12.5%	28.57%	22.22%
	Subordinates' Guess of leaders' decisions (C: Censor NC : Not Censor)			
	CersorMute		CensorChat	
State X	C: 36.84% NC: 63.16%		C: 46.67% NC: 53.33%	
State Y	C: 26.32% NC: 73.68%		C: 38.10% NC: 61.90%	

Note: Data from 31 leader subjects and 86 subordinate subjects.

The bold data in the table is what we are most interested in. We can see from the table that 42.86% of leaders believe that if subordinates can not observe any state information, they are more likely to coordinate on cooperation, while almost 71.42% of leaders guess that subordinates will coordinate on resistance if they see the state information is Y. This suggests that leaders do have an incentive to censor the state information of Y. Nevertheless, when it turns to the treatment *CensorChat*, this big difference disappeared, which means that leaders realize that once subordinates can communicate, they may not coordinate on cooperation when they could not see the state information. As for the belief of subordinates, we also ask them in two censorship treatments about guessing their leader's censorship behavior. It seems that no matter which state it is, they prefer to believe that their leader will not prefer to censor the state information. While in previous Table 4.2, we showed that the censorship rate is 85% in state Y and 30% in state X in treatment *CensorMute*. Only in treatment *CensorChat*, their guess is closer to the actual censorship rate with a 24.14% in State Y and 44.83% in state X. It implies that subordinates underestimate the use of censorship when they could not communicate with each other. So this may be the reason why a leader's use of censorship works.

Result 4.7 *(i) Based on leaders' belief of subordinates' behavior, leaders have an incentive to use censorship when the state information is Y; (ii) Subordinates underestimate the use of censorship when they could not communicate with each other.*

4.5 Conclusion

In this project, we conducted an online experiment to test the effect of the leader's power of censor and also investigate how it interacts with the possibility of communication between subordinates. To study the issue of censorship between leaders and their subordinates, we are the first to modify the Coordination Resistance game by Weingast (1997) and combine it with a signaling game by Cho and Kreps (1987) to design the experiment. Our modified CR game almost captured the main characteristics of the censorship environment in our society, including two different states: one bad and one good, the different economic status for two types of players and so on. We discussed several possible equilibria of our game and used the refinement sequential equilibrium prediction as our hypotheses to study this censorship effect. In this equilibrium, subordinates prefer to coordinate on cooperation with the leader if they see the state information is X, but they prefer to coordinate on resistance with the leader if they see the state information is Y. While they do not know the state information, they prefer to coordinate on cooperation in non-censorship treatments for a higher expected payoff and prefer to coordinate on resistance in censorship treatments because they can infer the use of censorship is an optimal strategy for leaders only in state Y. As for leaders, they prefer to censor the state information of Y when they have this power to do so.

Our experimental results is kind of complicated, which includes data from four treatments, three situations, and two roles. Our main results show that without censorship, subordinates‘ behaviors are more or less consistent with the equilibrium. When subordinates can communicate with each other, and the leader can not censor the state information, some of them showed their pro-social or fairness concern and switched from their equilibrium of coordinating on resistance in state Y to coordinate on cooperation in this case. Just like the prediction in refinement equilibrium, we observed that if subordinates cannot see any state information and if leader has the ability to censor subordinates coordinate more on resistance in state Y. What interesting is that in treatment *CensorMute*, when the leader can censor the state information of Y, but they did not, subordinates are willing to choose to cooperate even in state Y. This implies that if the ruling party or leader shares the bad news with the public, the public may not choose a selfish option but a cooperative option.

Moreover, since we also asked about their belief behind their decisions, we find that subordinates underestimate the censorship rate of leaders even when they know the leader's ability to censor. This suggests that censorship works if they can not communicate with others. Meanwhile, leaders have an incentive to use censorship since they believe that more subordinates coordinate on cooperation when they do not see the state information of Y. Finally, we find that when there is no censorship and subordinates can communicate with each other, we can get the highest total welfare.

Further study can try to work on the coordination of censorship to let leaders endogenously censor the communication channel among subordinates. It would be good to compare the results with ours to see which censorship works more efficiently in the leader's purpose. The other direction would move further to check the underlying mechanism of why censorship works to change subordinates' coordination decisions. Because they are naive, and then they are lack of the ability of k-level thinking. Alternatively, because they are uncertain about the existence of censorship and so on, once we understand the underlying mechanism of how censorship works, we could have a better suggestion for leaders or managers to use this method effectively.

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4.6 Appendix

4.6.1 Instructions

Welcome!

This is an interactive study conducted by a group of researchers. In this study, you will be playing a game in a group together with two real people who also enter this game at the same time.It is therefore important that you complete this game without interruptions. The study will take around 20 minutes. You will receive a participation fee of 1.5 pounds for completing the study. Additionally, you may earn points in the game that will be converted into real money (20 Points = 1 Pound). The points you earn depend on your answers, your decisions, and other participants' decisions. More information will be provided in the instructions. Participation is completely voluntary. You can stop participating at any time. However, we will not be able to pay you if you close this window or leave the game before reaching the end of the study. If you run out of time in a decision stage that requires you to make decisions within a time limit, we will not be able to pay you either. Please be aware of the timer.

All decisions are made anonymously and stored confidentially. If you consent to participate, please fill in your Prolific ID and click "Continue". Otherwise, you may close the window.

In this game, you earn points. The points you earn depend on your choice, other players' choices, and random choices of the computer. We will use payoff matrices to show how all these relevant choices determine the points each player will earn.

To familiarize yourself with payoff matrices, we provide a warm-up exercise. Please read the payoff matrix below carefully and answer a few test questions. You can only proceed to the game if you answer these test questions correctly.

Warm-up Exercise

This warm-up exercise involves two players: player 1 and player 2. Each of them chooses from two possible actions: A and B. The points they earn are determined by their choices, as shown in the matrix.

How to read the payoff matrix: - If both players choose A, both of them earn 30 points.

- If player 1 chooses A and player 2 chooses B, then player 1 earns 15 points and player

		Player 2	
		A	B
Player 1	A	Player 1 earns: 30 Player 2 earns: 30	Player 1 earns: 15 Player 2 earns: 0
	B	Player 1 earns: 0 Player 2 earns: 15	Player 1 earns: 10 Player 2 earns: 10

2 earns 0 points.

- If player 1 chooses B and player 2 chooses A, then player 1 earns 0 points and player 2 earns 15 points.

- If both players choose B, both of them earn 10 points.

On this page, we first present to you the rules of the game. Then, we explain what decisions you will need to make and how your payoff is computed. Please read the instructions carefully and we will ask you a few questions after this to make sure that you understand.

Game Rules

Roles

You will be matched with two other players. One of you will be assigned the role of "red" player and the other two of you are "blue" players.

Stage 1

There are two possible situations: X and Y. The computer randomly selects situation X or situation Y with equal probability.

NoCensorMute **Treatment**

Stage 2

The red player observes which situation the computer selects.

With a 50 % probability, the computer does not reveal its choice to blue players. In such cases, blue players do not know which situation the computer selects.

Stage 3

Blue players choose action A or action B at the same time.

Payoffs

The payoffs are determined by (1) the situation X/Y and (2) blue players' actions A/B, as shown in the payoff matrices below.

***NoCensorChat* Treatment**

Stage 2

The red player observes which situation the computer selects.

With a 50% probability, the computer does not reveal its choice to blue players. In such cases, blue players do not know which situation the computer selects.

Stage 3

Blue players choose action A or action B at the same time. They can discuss for 5 minutes before making decisions.

Payoffs

The payoffs are determined by (1) the situation X/Y and (2) blue players' actions A/B, as shown in the payoff matrices below.

***CensorMute* Treatment**

Stage 2 The red player observes which situation the computer selects.

The red player decides whether to withhold this information from blue players.

If the red player chooses not to withhold the information, blue players will receive a message informing them which situation the computer selects. If the red player chooses to withhold the information, blue players do not receive any message. Be aware that the red player cannot fake the message. A message can only tell the truth.

Stage 3 Blue players choose action A or action B.

Payoffs

The payoffs are determined by (1) the situation X/Y and (2) blue players' actions A/B, as shown in the payoff matrices below.

***CensorChat* Treatment**

Stage 2 The red player observes which situation the computer selects.

The red player decides whether to withhold this information from blue players.

If the red player chooses not to withhold the information, blue players will receive a message informing them which situation the computer selects. If the red player chooses to withhold the information, blue players do not receive any message. Be aware that the red player cannot fake the message. A message can only tell the truth.

Stage 3 Blue players choose action A or action B at the same time. They can discuss for 5 minutes before making decisions.

Payoffs

The payoffs are determined by (1) the situation X/Y and (2) blue players' actions A/B, as shown in the payoff matrices below.

In Situation X		Blue player 2	
		A	B
Blue player 1	A	Red player earns **40** Blue player 1 earns **30** Blue player 2 earns **30**	Red player earns **25** Blue player 1 earns **15** Blue player 2 earns **0**
	B	Red player earns **25** Blue player 1 earns **0** Blue player 2 earns **15**	Red player earns **10** Blue player 1 earns **10** Blue player 2 earns **10**

In Situation Y		Blue player 2	
		A	B
Blue player 1	A	Red player earns **40** Blue player 1 earns **10** Blue player 2 earns **10**	Red player earns **15** Blue player 1 earns **5** Blue player 2 earns **0**
	B	Red player earns **15** Blue player 1 earns **0** Blue player 2 earns **5**	Red player earns **10** Blue player 1 earns **20** Blue player 2 earns **20**

NoCensorMute Treatment

Decisions

You will submit a plan, in which you pre-specify the action you want to take in every possible scenario.

As a red player: You don't make any decision.

As a blue player: You face three scenarios. You will choose action A or B when: (1) you know that the computer selects situation X; (2) you know that the computer selects situation Y; and (3) you don't know which situation the computer selects.

Blue players have 5 minutes to make decisions. If they fail to submit decisions before time is up, they will receive only the participation fee of 1.5 pounds.

Payoff calculation

Once everyone has submitted their plan, the game runs automatically following your pre-specified plans. First, the computer selects a situation randomly. Next, it chooses whether to reveal its choice to blue players. Finally, it implements blue players' decisions according to their choices in the corresponding scenario. You will be informed of your earning and how it is calculated at the very end of the study.

***NoCensorChat* Treatment**

Decisions

You will submit a plan, in which you pre-specify the action you want to take in every possible scenario.

As a red player: You don't make a decision.

As a blue player: You face three scenarios. You will choose action A or B when: (1) you know that the computer selects situation X; (2) you know that the computer selects situation Y; and (3) you don't know which situation the computer selects.

Blue players have in total 5 minutes to communicate via a chatbox and make decisions. Although they can communicate, their decisions are privately submitted. If they fail to submit decisions before time is up, they will receive only the participation fee of 1.5 pounds.

Payoff calculation

Once everyone has submitted their plan, the game runs automatically following these pre-specified plans. First, the computer selects a situation randomly. Next, it chooses whether to reveal its choice to blue players. Finally, it implements blue players' decisions according to their choices in the corresponding scenario. You will be informed of your earning and how it is calculated at the very end of the study.

***CensorMute* Treatment**

Decisions

You will submit a plan, in which you pre-specify the action you want to take in every possible scenario.

As a red player: You face two scenarios. You will choose whether to withhold the information about the situation from the blue players when: (1) the computer selects situation X; (2) the computer selects situation Y.

As a blue player: You face three scenarios. You will choose action A or B when: (1) you receive a message "the computer selects situation X"; (2) you receive a message "the computer selects situation Y"; and (3) you don't receive any message. Every player has 5 minutes to make decisions. If you fail to submit decisions before time is up, you will receive only the participation fee of 1.5 pounds.

Payoff calculation

Once everyone has submitted their plan, the game runs automatically following these pre-specified plans. First, the computer selects a situation randomly. Next, it sends a message or no message to blue players according to the red player's choice in the corresponding scenario. Finally, it implements blue players' decisions according to their choices in the corresponding scenario. You will be informed of your earning and how it is calculated at the very end of the study.

CensorChat **Treatment**

Decisions

You will submit a plan, in which you pre-specify the action you want to take in every possible scenario.

As a red player: You face two scenarios. You will choose whether to withhold the information about the situation from the blue players when: (1) the computer selects situation X; (2) the computer selects situation Y.

Red player has 5 minutes to make decisions.

As a blue player: You face three scenarios. You will choose action A or B when: (1) you receive a message "the computer selects situation X"; (2) you receive a message "the computer selects situation Y"; and (3) you don't receive any message.

Blue players have in total 5 minutes to communicate via a chat box and make decisions. Although they can communicate, their decisions are privately submitted. If they fail to submit decisions before time is up, they will receive only the participation fee of 1.5 pounds.

Payoff calculation

Once everyone has submitted their plan, the game runs automatically following these pre-specified plans. First, the computer selects a situation randomly. Next, it sends a message or no message to blue players according to the red player's choice in the corresponding scenario. Finally, it implements blue players' decisions according to their choices in the corresponding scenario. You will be informed of your earning and how it is calculated at the very end of the study.

4.6.2 Comprehension test and post-questionnaire

Comprehension tests

Comprehension Test 1 after Warm-up: Suppose you are player 1. If you choose action B and player 2 chooses action A.

Q1: How many points do you earn? Q2: How many points does player 2 earn?

Comprehension test 2 after Game rules:

To make sure you understand the rules, we ask you a few questions. Q1: A red player's payoff is determined by her own decision. Choose between "True" and "False"

Q2: A red player's payoff is determined by two blue players' decisions in the same group. Choose between "True" or "False"

Q3: What happened if blue players didn't know which situation the computer selected?(T1 & T2) Choose between 1, The computer decided not to reveal this information. 2, The other blue player withheld this information.

Q4: What happened if blue players didn't receive any message about the situation the computer selected? (T3 & T4) Choose between 1, The computer decided not to reveal this information. 2, The red player withheld the information.

Q5: You will make decisions for multiple scenarios, but only one scenario will be played out to determine your payoff. Choose between "True" and "False"

Post-questionnaire

We ask a few questions about yourself or your opinions about other players' actions. It is most helpful to us if you answer these questions honestly. Notice that some questions may be marked as bonus questions. You will receive 2 points for each correct answer to these bonus questions.

[BONUS] What do you think the red player chose when the computer selected situation X? (T3 & T4 Blue players) Withhold this information Not withhold this information

[BONUS] What do you think the red player chose when the computer selected situation Y?(T3 & T4 Blue players) Withhold this information Not withhold this information

[BONUS] What do you think the other blue player chose when you receive a message ”the computer selects situation X”?(T3 & T4 Blue players) A B

[BONUS] What do you think the other blue player chose when you receive a message ”the computer selects situation Y”? (T3 & T4 Blue players) A B

[BONUS] What do you think the other blue player chose when you know that the computer selects situation X?(T1 & T2 Blue players) A B

[BONUS] What do you think the other blue player chose when you receive no message? (T3 & T4 Blue players) A B

[BONUS] What do you think the other blue player chose when you know that the computer selects situation Y?(T1 & T2 Blue players) A B

[BONUS] What do you think the other blue player chose when you don't know which situation the computer chooses? (T1 & T2 Blue players) A B

[BONUS] What do you think blue players chose when they don't know which situation the computer selects? (T1 & T2 Red Players) Both chose A Both chose B One chose A and the other chose B

[BONUS] What do you think blue players chose when they don't receive any message?(T3 & T4 Red Players) Both chose A Both chose B One chose A and the other chose B

[BONUS] What do you think blue players chose when they know that the computer selects situation Y? (T1 & T2 Red Players) Both chose A Both chose B One chose A and the other chose B

[BONUS] What do you think blue players chose when they receive a message ”the computer selects situation Y”? (T3 & T4 Red Players) Both chose A Both chose B One chose A and the other chose B

What is your biological gender? (To all players) Female Male

How do you see yourself? Are you generally a person who is fully prepared to take risks or do you try to avoid taking risks? Please check a box on the below scale, where the value 0 means ‘not at all willing to take risks’, and the value 10 means ‘very willing to take risks (To all players)

Not at all take risks . . . . Very willing to take risks

4.6.3 Screenshot for online experiment

Figure 4.6: Quiz 2

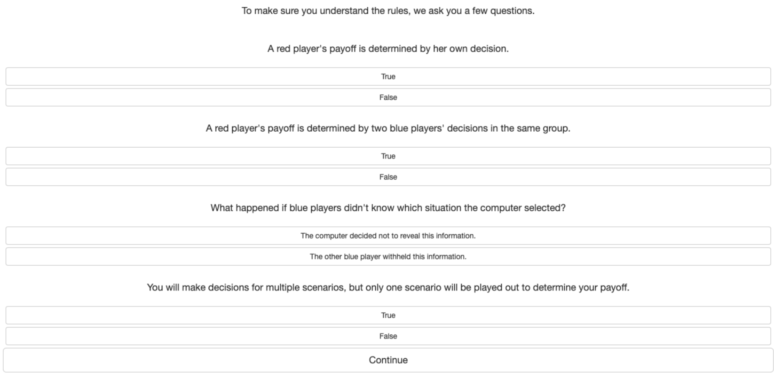

Figure 4.7: Decision screen for Blue players NoCensorMute

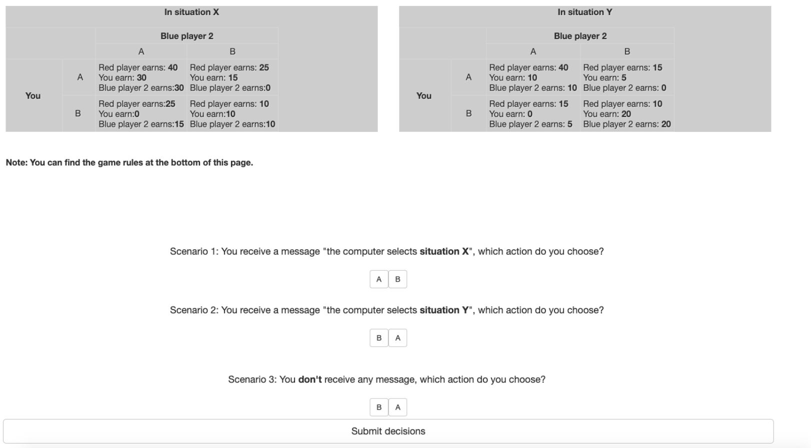

Figure 4.8: Decision screen for Red players non-censorship treatments

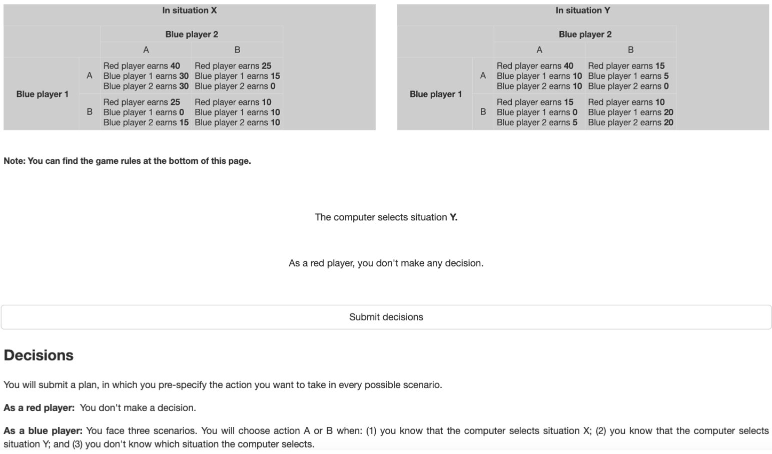

Figure 4.9: Decision screen for Blue players with Chat

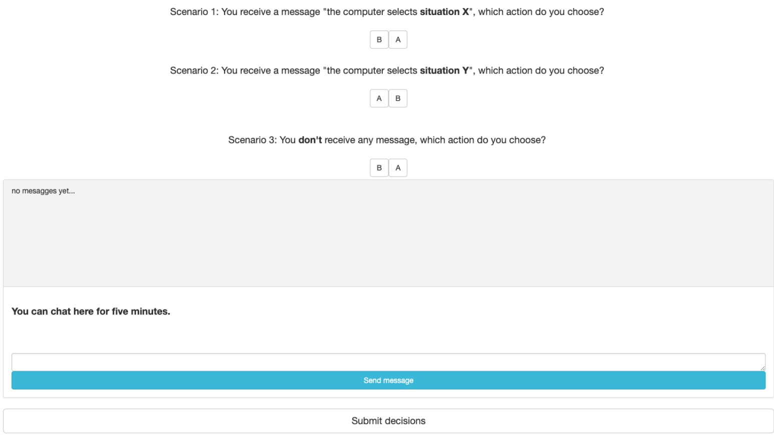

Figure 4.10: Decision screen for Red player in censorship treatments

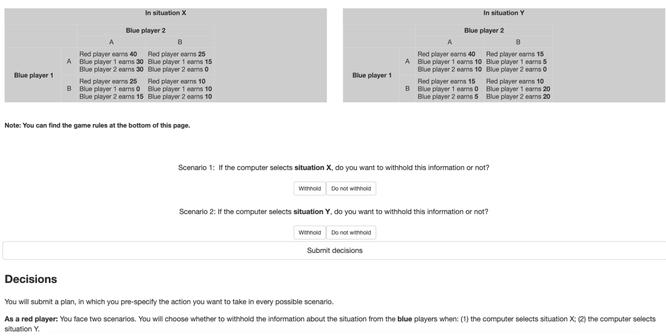

Figure 4.11: Payoff Screen example