THREE ESSAYS ON DYNAMIC MECHANISM DESIGN

by

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Three Essays on Dynamic Mechanism Design

A dissertation submitted in partial fulfillment of the requirements for the degree of Doctor of Philosophy at George Mason University

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Dedication

I dedicate this dissertation to my family and my friends.

Acknowledgments

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Abstract

THREE ESSAYS ON DYNAMIC MECHANISM DESIGN

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The dissertation focuses on exploring theory and experiments in dynamic mechanism design, aiming to provide helpful guidance for dynamic mechanism design in natural environments.

The first chapter, co-authored with Daniel Houser, compares the experimental revenue of the "optimal non-clairvoyant dynamic mechanism" with its theoretical prediction, thereby emphasizing the practical value of this novel mechanism. Dynamic mechanisms provide a powerful means for optimizing the revenue and efficiency of repeated auctions. However, implementing them is complicated due to a number of conditions that are difficult to satisfy in practice. These include the fact that the auction designer must be clairvoyant, in the sense that they must have reliable forecasts of participants' valuation distributions in all future periods. Recently, Mirrokni et al. (2020) introduced a non-clairvoyant dynamic mechanism and showed that it is optimal within the class of dynamic mechanisms that do not rely on strong assumptions regarding knowledge about the future. We showed, however, that an optimal static mechanism (a Myerson auction) can under certain conditions outperform their dynamic mechanism. Here, we report data from an experiment designed to test the performance of the Mirrokni et al. (2020) mechanism in relation to the Myerson auction. Our results support the theory: the optimal non-clairvoyant dynamic mechanism

either outperforms or underperforms the repeat static Myerson according to theory predictions. Our results highlight the practical importance of non-clairvoyant mechanisms as implementable approaches to dynamic auction design.

The second chapter, co-authored with Daniel Houser, investigates the decision-making processes of human sellers when it comes to selecting dynamic mechanisms. We propose an experimental design to investigate how human sellers choose between two easily-conducted dynamic mechanisms: the optimal non-clairvoyant dynamic mechanism (NC) (Mirrokni et al., 2020) and the optimal repeated static mechanism (RS) (Myerson, 1981). Our results indicate that human sellers can harness their experience in an environment to choose the optimal mechanism later in the experiment. In addition, sellers tend to adjust their choice of mechanism based on past revenue. We further find that: (i) sellers generally overprice; and (ii) buyers participate less in NC due to the greater-than-suggested upfront fee, leading to the theoretical-experimental revenue gap. Our results shed light on how sellers choose dynamic mechanisms and can potentially help improve mechanism design.

The third chapter, co-authored with Jingnan Chen, Erte Xiao, and Daniel Houser, shifts the focus to gender inequality in leadership positions within the workplace. Employing an online experiment, we scrutinize efficacy of an easily implementable default (opt-out) approach in mitigating the gender disparity in willingness to contribute ideas. Our investigation encompasses two types of defaults: non-merit-based defaults, where individuals are randomly assigned to a leadership position for contributions (with the option to opt out), and merit-based defaults, where individuals are placed in their default position based on their skills and abilities. Our findings reveal that defaults wield significant effects on mitigating the gender gap in willingness to contribute ideas. We find little evidence, however, that defaults mitigate stereotype bias in the willingness to contribute. Notably, both the random default and the merit-based default yield equivalent results in narrowing this gap. This study highlights the practical significance of the default option in fostering a more equitable working environment for women.

Chapter 1: Non-clairvoyant Dynamic Mechanism Design: Experimental Evidence

1.1 Introduction

Dynamic auction environments are ubiquitous. They include the many types of repeated procurement activities of a firm within long-term principal-agent relationships. Another important example is online advertising markets where advertisers (the buyers) submit bids for the impressions (items) arriving one-by-one from the platform (the seller). While advertisers possess private information regarding the item valuations, the public information about the valuation distribution can change suddenly due to naturally occurring events. Consequently, both seller and buyer face Knightian uncertainty[1].

The theory literature on the dynamic mechanism design is rich and sophisticated; however, to obtain results, the literature has traditionally assumed "clairvoyance," in the sense that the distributions of buyers' valuations across all periods past, present and future are common knowledge. Under this assumption, the dynamic mechanism design can be shown to achieve more revenue for the seller (Courty and Li, 2000; Pavan et al., 2014) and to generate more efficient outcomes (Athey and Segal, 2013; Bergemann and Välimäki, 2010). Indeed, Papadimitriou et al. (2022) demonstrate that the revenue from clairvoyant dynamic mechanisms can be arbitrarily large compared to the Myerson (1981) repeated static optimal mechanism (henceforth RS).

We see few examples of natural environments implementing optimal dynamic mechanisms, despite decades of theory advances on dynamic mechanisms, and the practical interest in optimal auction design. A key reason is that such mechanisms require both sellers and

[1]As opposed to Bayesian uncertainty, Knightian uncertainty is a lack of any distributional knowledge.

buyers to hold identical and correct beliefs about the future. This condition is obviously difficult to satisfy. Moreover, optimal dynamic mechanisms are environment-dependent, complex, and lacking in general form (Mirrokni et al., 2020). The complexity of optimal clairvoyant dynamic auction mechanisms may also explain why they have not been studied in the experimental literature, which has otherwise devoted substantial attention to various single- and multi-good static or repeated-static auction environments[2].

Mirrokni et al. (2020) developed a non-clairvoyant dynamic mechanism (henceforth NC) that is optimal among non-clairvoyant mechanisms where designers are restricted from using future distributional information. It does not require information about future valuations, but is also robust to differences in beliefs across participants; simple in form; and scenario-independent. Here, our study focuses on the two-period single-buyer environment, where the optimal non-clairvoyant mechanism can theoretically guarantee revenues equal to at least 50% of those produced by the optimal clairvoyant mechanism.

At a high level, the two-period single-buyer non-clairvoyant mechanism proceeds as follows: In the first period, sellers provide half of the items at no cost. The other half of the items are sold according to Myerson's auction. In the second period, sellers ask for an upfront buyer-specific fee to enter the auction. They then conduct auctions with buyer-specific reserve prices. When the auction concludes, half the buyers are refunded their up-front entry fee. As detailed below, a key result in Mirrokni et al. (2020) is that it guarantees the inter-period revenue that comes from using past bid information: the second period entry fees and reserve prices can be set so that buyers have an incentive to enter, while the inter-period revenues of sellers (which comes from linking past information and current information together) are bounded below by one-half of the expected inter-period revenue from an optimal clairvoyant mechanism. Depending on realizations of second-period buyer values and corresponding inter-period revenues, total sellers' revenues in NC can be much

[2]For the single good dynamic mechanism, see Courty and Li (2000), Eső and Szentes (2007), Board (2007) Filiz-Ozbay and Ozbay (2007); for single good repeated selling, see Baron and Besanko (1984), Papadimitriou et al. (2022), Balseiro et al. (2018), Devanur et al. (2019), Chawla et al. (2022), etc; for multiple good static mechanism design, see McAfee and McMillan (1988), Armstrong (1996), Manelli et al. (2006). See more discussion in section 2, Literature Review.

greater than expected revenues using the repeat Myerson mechanism. The reason is that repeat static generates optimal intra-period revenues that comes from using only current information, but zero inter-period revenues.

The non-clairvoyant approach incorporates several mechanisms that are widely used in natural environments: auctions with reserve prices are common on eBay and online advertising markets; upfront fees have the same form as membership fees used in Amazon Fresh and Costco; and individualized reserve prices are closely related to customized coupons, like those used by CVS. As a practical matter, however, one would only choose to implement Mirrokni et al. (2020) when it is likely to outperform the repeated static Myerson's auction under the non-clairvoyant environment. Intuitively, in the two-period case, the non-clairvoyant dynamic mechanism outperforms the repeated static mechanism whenever expected revenues from the entry fee and associated reserve price in the second period more than offset the reduced revenue due to giving away items in the first period. This occurs whenever inter-period revenue is sufficiently higher than intra-period revenue. More specifically, this occurs whenever buyers' expected second-period valuation is sufficiently great in relation to the expected revenue from the Myerson auction[3]. When the intra-period revenue is sufficiently greater than inter-period revenues, then the repeated static is preferable to NC (e.g., if the buyer consistently values the item at zero in the second period, RS generates more revenue than NC). The reasoning is similar: the expected buyers' valuation is an upper bound for a sellers' expected revenue in every period. When expected revenue in the second period is not sufficient to offset the loss in the first period, the non-clairvoyant mechanism will underperform in relation to a Myerson auction.

Note that buyers' willingness to pay an upfront participation fee in the second period is crucial to the theoretical performance of the non-clairvoyant mechanism. Theory requires only that buyer behavior satisfy ex-post individual rationality, rather than single-period individual rationality. That is, the buyer need only have non-negative total utility given

[3]For example, if there are possible "target buyers" with high valuations but low probability, Myerson's auction sets low reserve price and generates low revenue, while NC can extract more revenue by setting upfront fee and higher reserve price in the second period.

the up-front fee. However, whether buyers actually behave as theory predicts remains an open and empirical question. Davis et al. (2014) observed that buyers do not enter 100% of auctions they should enter. One example in natural environments is Amazon, where customers can shift to monthly Prime fees by canceling their membership at the beginning of the month. Considering this, the non-clairvoyant mechanism might not achieve its theoretically promised revenues, as agents can opt out at any time during the dynamic game. Perhaps especially when buyers are risk-averse, prepaid upfront fees can perceived as a risky investment and may deter participation.

We conducted laboratory experiments to study behavior under the Mirrokni et al. (2020) non-clairvoyant mechanism and compare its performance to Myerson's auctions. To our knowledge, we are the first to investigate behavior under dynamic auction mechanisms in general, and optimal non-clairvoyant mechanisms in particular. We consider two scenarios, one where the repeated static Myerson's auction theoretically outperforms the optimal non-clairvoyant dynamic mechanism, and the other where theory predicts the reverse outcome.

We observe that revenue differences between auction formats are qualitatively consistent with theory predictions, despite the presence of systematic behavioral deviations from theory. In particular, we observe that: (1) in the non-clairvoyant dynamic mechanism, buyers enter less than predicted in the second period; and (2) in the repeated static mechanism, buyers substantially overbid. We provide evidence that both of these behavioral anomalies seem to be associated with risk aversion.

1.2 Literature Review

1.2.1 Optimal dynamic mechanism design

Our paper contributes to the optimal dynamic mechanism design literature[4], which studies how to determine price-discovery and allocation rules as agents (buyers) receive information over time. Since the seminal work of Baron and Besanko (1984), which analyzed monopoly

[4]For efficient dynamic mechanism design, see dynamic pivot mechanism (Bergemann and Välimäki, 2010), extended VCG in the interdependent-value case (Liu, 2018), and team mechanism (Athey and Segal, 2013).

regulations, it has been widely assumed that agents are "clairvoyant," in the sense that they know that everyone perfectly knows the stochastic process governing all future economic outcomes (information and states).

The optimal clairvoyant dynamic mechanism lacks general form and but rather must be tailored to specific environments. For example, when agents face asymmetric uncertainty, the optimal mechanism involves subsidizing agents with lower future uncertainty in order to reduce rent-seeking by consumers facing higher uncertainty (Courty and Li, 2000). Alternatively, in the case where agents obtain a private value signal from the seller, the optimal mechanism includes a premium paid for extra information, combined with a second price auction where the winner pays the second-highest bid plus the premium (Eső and Szentes, 2007).[5] The optimal mechanism should be randomized when private information over periods is a Markov process (Pavan et al., 2014). Jackson and Sonnenschein (2007), Balseiro et al. (2018), and others have also found environment-specific results. In general, the optimal clairvoyant mechanism is environment-dependent. The reason is that because different environments constrain dynamic incentive compatibility and individual rationality, both of which are crucial for optimality, and different environments constrain them in different ways.

While clairvoyant mechanisms require participants' valuation distributions to be known across all future periods, yet this is difficult to achieve as a practical matter. Empirical results show that agents tend to have biased beliefs about the future (DellaVigna and Malmendier, 2006). Of course, one can design optimal clairvoyant mechanisms that try to take this into account. For example, if one assumes agents will be overconfident regarding the precision of their demand forecasts, it can be shown that the optimal mechanism is a three-part tariff similar to what is used for the kind used for cell-phone contracts (Grubb (2009); see also Eliaz and Spiegler (2008)).

Our paper studies a non-clairvoyant environment where future distributional knowledge is not available. As a result, reliable future forecasts regarding future distributions and

[5]Extension of infinite time horizon see Board (2007).

outcomes are not needed. Mechanisms restricted from using future distributional knowledge can be shown to have a general form applicable to any non-clairvoyant environment (Mirrokni et al., 2020).

1.2.2 Non-clairvoyant dynamic mechanism design

There are two feasible mechanisms under the non-clairvoyant environment where sellers are restricted from using future information in designing current rules. The first option is to implement Myerson's auction (Myerson, 1981) in each period. We denote this as the repeated static mechanism (RS). In RS, rules in each period are independent of each other, in order to maximize the intra-period revenue that arises from using only current-period information. Given that there is no inter-period revenue (derived from linking past information to current information), the revenue from the RS as compared with optimal clairvoyant revenue can be arbitrarily small (Papadimitriou et al., 2022).

The second option we study is the optimal non-clairvoyant dynamic mechanism (NC) introduced by Mirrokni et al. (2020). NC uses past bid information to design future roles to maximize the revenue guarantee. We focus on the two-period single buyer case, where NC can guarantee revenues equal to at least 50% of those produced by the optimal clairvoyant mechanism under all scenarios, regardless of the size of the inter-period revenues.

In broad terms, to guarantee 50% of the optimal intra-period revenue, NC always allocates one-half of items in each period using Myerson's auction. In the two-period case we study here, to guarantee 50% of the optimal inter-period revenue, it allocates the other half of the items at a price of zero in the first period. Then, in the second period, it charges half the buyers a buyer-specific upfront entry fee to participate in the auction and sets buyer-specific reserve prices. Both the upfront entry fee and the reserve prices are set according to each buyer's first-period bid decisions.

NC cannot always outperform RS. The revenue comparison between the two mechanisms is determined by the relative size of intra- and inter-period revenues and is scenario dependent. To compare the performance of the two mechanisms, we design two different scenarios, one which theoretically favors the non-clairvoyant mechanism and one which favors the repeat-static approach.

Mechanism design under non-clairvoyance contributes to the recent interest in "simple" mechanisms that do not require players to make contingent plans across the entire future of a dynamic game (Li, 2017; Pycia and Troyan, 2023). Within this context, the optimal non-clairvoyant dynamic mechanism is the best response of sellers who perceive only their own current information set as simple. Both the optimal non-clairvoyant dynamic mechanism and the optimal repeated static mechanism with simple forms are implementable in the lab. We take advantage of laboratory experiments to provide comprehensive comparisons of the two mechanisms under different designated scenarios.

1.2.3 Experiments informing dynamic mechanism design

Experiments informing dynamic mechanism design have focused primarily on revenue or efficiency comparison among multi-unit sequential auctions. This literature assumes clairvoyance, and reports experiments where a Myersonian approach can be used. Manelli et al. (2006) compared efficiencies and found similar efficiency between the Vickrey auction and the ascending-price auction; Ledyard et al. (1997) observed that the simultaneous discrete auction outperforms the sequential auction in efficiency. Lucking-Reiley (1999) compared revenues and showed that revenue equivalence survives in all-pay and winner-pay auctions in the multi-unit setting; Brunner et al. (2010) observed revenues that vary among several non-deterministic combinatorial mechanisms; and Back and Zender (2015) showed lower experimental revenue than theoretically predicted in sealed bid auctions. Our paper differs from this line of experiments, as we study the feasibility of dynamic mechanisms in a non-clairvoyant environment, where agents' valuations evolve over time and future distributional knowledge is not available. This approach necessarily differs from environments with fixed agent's valuation but dynamic arrival or departure.

Whether buyers actually behave as theory predicts remains an important open and empirical question. Bidders' behavior has also been studied in the experimental literature to explain how experimental observations might differ from theoretical prediction: both overbidding (Neri, 2015) and underbidding (Chen and Takeuchi, 2010) have been observed; Kagel and Levin (2009) demonstrated the bounded rationality of bidders; and Bernard (2005) found affiliation of bids in repeated identical trials. Particularly in an environment where bidders incur a cost to learn their valuations, Davis et al. (2014) found fewer bidders entering an English auction where entry decisions were made simultaneously among bidders than the sequential entering and bidding mechanism. Results show that the sequential mechanism outperforms the English auction with regard to revenue, which overturns the theoretical prediction on revenue comparison.

Davis et al. (2014) also questioned whether auctions in experiments can meet the requirement of individual rationality. This is an important question for our study. The reason is that the possible upfront fee in the second period of the optional non-clairvoyant dynamic mechanism also serves as the entry cost for bidders. Risk-averse buyers in particular may perceive prepaid upfront fees as risky; as a result, such fees may deter their participation. In light of this, NC might fail to achieve its theoretically promised revenues. Therefore, in our experiments, we investigate whether buyers enter in the second period, as well as the extent to which partial entry of buyers impacts the revenue of NC.

1.3 Theoretical Framework

1.3.1 Non-clairvoyant dynamic environment

We study a two-period repeated selling environment with a single buyer. Each period introduces a new item the buyer can purchase. We use a single buyer over two periods to create a simple environment in which to apply the non-clairvoyant dynamic mechanism. We keep the setup simple to help participants understand the incentives of the environment. In both periods, it is common knowledge that there is no cost associated with producing an item; however, valuations are private information. The first-period valuation is independent of the second period's.[6]

The non-clairvoyant dynamic environment differs from the clairvoyant environment in that the latter assumes that prior distributions of the buyer's valuation in the two periods, (F_1 and F_2) are common knowledge for both the buyer and the seller at the beginning of the first period. By contrast, in the non-clairvoyant setting, the future distribution F_2 is not available until the second period. Consequently, allocation rules and prices in the first period cannot take into account the second period distributional information.

The sequence of events and actions in the selling game in each period $t \in \{1, 2\}$, are summarized as follows:

1. The seller and the buyer learn the distribution of buyer's valuation for this period, F_t.
2. The seller describes their allocation rule $x_t \in [0, 1]$ and price policy $p_t \in \mathbb{R}^+$.
3. The buyer who enters the trade learns their valuation of the item $v_t \sim F_t$, and makes a bid b_t.
4. The seller implements their allocation rule $x_t(b_t)$ and price policy $p_t(b_t)$.
5. The buyer accrues period utility $u_t = u_t(b_t, v_t) = v_t * x_t(b_t) - p_t(b_t)$.

Here, we follow the common setting in the literature (Bergemann and Välimäki, 2019) and assume quasilinear preferences for the buyer. The total payoff of the buyer is the sum of utility in both periods without discounting: $U = u_1 + u_2$.

We focus on direct mechanisms satisfying the buyer's participation constraint. That is, dynamic mechanisms with allocation rules (x_1, x_2) and price policy (p_1, p_2) are subject to Dynamic Incentive Compatibility (DIC) and Ex post Individual Rationality (EPIR) as detailed below.

[6]Devanur et al. (2019) studied the repeated selling of fresh copies to consumers who have fixed valuation through multi-period. They focus on the effect of commitment power of seller on revenue, while we assume fully commitment power.

$$
\begin{aligned}
DIC: & \begin{cases} u_2(v_2|v_2) \geq u_2(b_2|v_2), & \forall v_2 \in F_2 \\ u_1(v_1|v_1) + \mathbb{E}_{v_2} u_2(v_2|(v_1, v_2)) \geq u_1(b_1|v_1) + \mathbb{E}_{v_2} u_2(v_2|(b_1, v_2)), & \forall v_1 \in F_1 \end{cases} \\
EPIR: & \quad U = u_1 + u_2 \geq 0, \qquad \forall v_1 \in F_1, v_2 \in F_2
\end{aligned}
$$

Dynamic Incentive Compatibility implies that the buyer's best response to the seller's decisions is to report the buyer's true value in every period. DIC in the non-clairvoyant environment can be illustrated by backward induction: in the second period, the buyer maximizes their payoff by bidding the valuation; in the first period, the buyer also has an incentive to report the true value given that the buyer bids their value in the next period. Ex-Post Individual Rationality requires the total utility the agent obtains to exceed total payments after the realization of the value for all periods so that the buyer is incentivized to participate.

In a nutshell, under a non-clairvoyant environment, the mechanism uses no future distributional knowledge, but is required to satisfy DIC and EPIR. The optimal repeated static mechanism (Myerson, 1981) and the optimal non-clairvoyant dynamic mechanism (Mirrokni et al., 2020) are feasible under the non-clairvoyant environment described above. We introduce and compare the two mechanisms in Section 3.4.

1.3.2 Repeated Static Mechanism

While the non-clairvoyant environment restricts the seller from using future distributional knowledge, the seller can implement the optimal mechanism (Myerson, 1981) in each period independently. This is denoted as a repeated static mechanism. The reason is that the seller maximizes revenue in each of the two periods separately.

In the single-buyer case, the optimal revenue is obtained by conducting the auction with monopoly reserve price $r_t = arg \max r(1-F_t(r))$. Denoting the optimal reserve price in each period as r_1^*, r_2^*, respectively, optimal revenue in the two periods is $Rev_1^* = r_1^*(1 - F_1(r_1^*))$,

$Rev_2^* = r_2^*(1 - F_2(r_2^*))$. Thus, the revenue in repeated optimal static mechanism is the sum of optimal revenues in the two periods, $Rev^S = Rev_1^* + Rev_2^*$.

The repeated static optimal mechanism satisfies DIC and EPIR, as it is incentive compatible in each period and satisfies single period individual rationality. In each period, it is a weakly dominant strategy for a buyer to bid their true value. The buyer obtains the item if their valuation exceeds the reserve price. Given that the seller is conducting Myerson's auctions in each period, we use the superscript M to denote the repeated static mechanism and summarize the allocation and price rules as follows:

$$\begin{cases} x_1^M = \mathbb{1}\{v_1 \geq r_1^*\}, & p_1^M = r_1^* \cdot \mathbb{1}\{v_1 \geq r_1^*\}, \\ x_2^M = \mathbb{1}\{v_2 \geq r_2^*\}, & p_2^M = r_2^* \cdot \mathbb{1}\{v_2 \geq r_2^*\}. \end{cases}$$

Thus, in the repeated static mechanism, there is maximized intra-period revenue in each period, but zero inter-period revenue. The reason is that the mechanism does not use first-period knowledge in determining rules of allocation and price in the second-period. We denote the optimal clairvoyant dynamic revenue with both maximized intra-period revenue and inter-period revenue as Rev^*; the ratio $\frac{Rev^S}{Rev^*}$ can be arbitrarily small (Papadimitriou et al., 2022).

1.3.3 Optimal Non-clairvoyant Dynamic Mechanism

The objective of the non-clairvoyant dynamic mechanism (Mirrokni et al., 2020) is to guarantee maximum revenue, i.e., to maximize the non-clairvoyant revenue to clairvoyant revenue ratio, $\frac{Rev^{NC}}{Rev^*}$, for all second-period distributions which are unknown at the beginning of the first period. Therefore, in contrast to the repeated static mechanism, where rules in the two periods are independent, the optimal non-clairvoyant dynamic mechanism must consider past information when designing rules in the second period to ensure the inter-period revenue is also guaranteed.

In the two-period case, the optimal non-clairvoyant dynamic mechanism with at least one-half optimal clairvoyant revenue guarantee has a simple and general form: it guarantees one-half maximized intra-period revenue by allocating one-half item in each period via the Myerson's auction (M). It further guarantees at least one-half of the maximized inter-period revenue by allocating the other-half item in the first period via the Give For Free auction (F) to accumulate the most consumer surplus and allocating the other half-item in the second period via the Posted Price auction (P) to recoup all of the accumulated consumer surplus.

The Give for Free auction (F) allocates the item to any buyer for free, regardless of bid: $x_t^F = 1$ and $p_t^F = 0$. The Posted Price auction (P) exploits all trading surplus by setting an upfront fee in advance for all buyers and then conducting an auction with a reserve price. The upfront fee in the second period is customized for each buyer based on the buyer's bids in the first period, $s_2 = \min\{b_1, \mathbb{E}_{v_2}\}$. It is bounded by the second period expected valuation to ensure participation of the buyer *ex ante*. The associated reserve price r_2^P is set to ensure that the buyer's payoff in this period is zero, to maximize the intra-period revenue. That is, $\mathbb{E}_{v_2 \sim F_2}[v_2 - r_2^P]^+ - s_2 = 0$.

The optimal non-clairvoyant dynamic mechanism is dynamic incentive compatible: in each period, it is a uniform combination of mechanisms incentivizing a buyer to report true values. In ensuring the upfront fee is bounded by the first-period valuation, it also satisfies the EPIR. Below, we summarize the rules of allocation and price rule of the non-clairvoyant dynamic mechanism design.

$$\begin{cases} x_1 = \frac{1}{2}[x_1^M + x_1^F] = & \frac{1}{2}(\mathbb{1}\{v_1 \geq r_1^*\} + 1), \\ p_1 = \frac{1}{2}[p_1^M + p_1^F] = & \frac{1}{2} r_1^* \cdot \mathbb{1}\{v_1 \geq r_1^*\}, \\ x_2 = \frac{1}{2}[x_2^M + x_2^P] = & \frac{1}{2}[\mathbb{1}\{v_2 \geq r_2^*\} + \mathbb{1}\{v_2 \geq r_2^P\}], \\ p_2 = \frac{1}{2}[p_2^M + p_2^P] = & \frac{1}{2}[r_2^* \cdot \mathbb{1}\{v_2 \geq r_2^*\} + s_2 + r_2^P \cdot \mathbb{1}\{v_2 \geq r_2^P\}]. \end{cases}$$

The optimal non-clairvoyant dynamic mechanism design can thus be applied to environments, as its design does not use future distributional knowledge.

1.3.4 Theoretical revenue comparison

In contrast to the repeated static mechanism maximizing intra-period revenue independently with zero inter-period revenue, the optimal non-clairvoyant dynamic mechanism guarantees one-half optimal inter-period revenue in all scenarios with the loss of one-half optimal intra-period revenue. As a result, the revenue comparison of the optimal non-clairvoyant dynamic mechanism and repeated static mechanism is determined by the relative size of intra- and inter-period revenues and is specific-scenario dependent.

In the scenario where there is a thin market in the second period, *i.e.*,the distribution of the buyer's valuation is long-tailed, the optimal static revenue produced by Myerson's auction is relatively small compared to the high expected value in that period. This means that the optimal non-clairvoyant dynamic mechanism outperforms the repeated static mechanism: it loses one-half less intra-period revenue, but guarantees one-half greater inter-period revenue through the upfront fee in the Posted Price auction in the second period. We describe a specific scenario using distributions of buyers' valuations in each period (F_1, F_2). We first define Scenario A as below.

Scenario A (S_A): under which the non-clairvoyant mechanism has more revenue than the repeated mechanism.

$$\begin{cases} F_1 = F_A = & \{(v, p(v))\} = \{(2, \frac{1}{2}), (4, \frac{1}{2})\} \\ F_2 = F_B = & \{(v, p(v))\} = \{(2, \frac{1}{2}), (4, \frac{1}{4}), (8, \frac{1}{8}), (16, \frac{1}{16}), (32, \frac{1}{16})\}, \end{cases}$$

where F_1 and F_2 are discrete distributions shown in pairs of values and associated probabilities $(v, p(v))$ in each period.

In Scenario A, the repeated static mechanism can achieve revenue of 4, as the reserve price of Myerson's auction (M) is the same in both periods, $r^M = 2$. The revenue in M is

2 in each period, which is relatively low compared to the high expected value of the long-tailed distribution F_2 in the second period, $\mathbb{E}_{v_2} = 6$. Thus, the non-clairvoyant dynamic mechanism can outperform the repeated static mechanism through high inter-period revenue produced by Posted Price auction in second-period. The upfront fee in the Posted Price auction (P) is $s_2 = \min\{b_1, 6\}$; the associated reserve price is $r_2^P = 0$ if $b_1 \geq 6$; $r_2^P = 2$ if $b_1 = 4$; $r_2^P = 8$ if $b_1 = 2$; and $r_2^P = 32$ if $b_1 = 0$. This leads to a revenue of 5 in Posted Price auction. By assuming the buyer bids their true value in Period 1 and fully participates in both periods, we derive the theoretical revenue comparisons of the two mechanisms in Table 1.1.

Table 1.1: Theoretical Revenues under Scenario A.

S1-Revenue	Non-clairvoyant Dynamic		Repeated Static	
Period 1	Give for Free (F)	0	Myerson's Auction (M)	2
	Myerson's Auction (M)	2		
Period 2	Post Price Auction (P)	**5**	Myerson's Auction (M)	2
	Myerson's Auction (M)	2		
Total		4.5		4
Intra-period revenue		2		4
Intra-period revenue		2.5		0

While switching the distribution of the two periods does not alter the revenue in the repeated static mechanism, it affects the performance of the non-clairvoyant dynamic mechanism design. If we switch the scenario described above, in the non-clairvoyant dynamic mechanism, the revenue loss in the first period in Give for Free auction (F) cannot be covered by the upfront fee bounded by the low expected value in the second period. This leads to an overturn of previous revenue comparison. We define Scenario B as below.

Scenario B (S_B): under which non-clairvoyant mechanism has less revenue than the repeated static mechanism.

$$F_1 = F_B, F_2 = F_A.$$

In scenario B, the expected value in the second is not sufficiently great, $\mathbb{E}_{v_2} = 3$. The upfront fee in Posted Price auction (P) is $s_2 = \min\{b_1, 3\}$, and the associated reserve price is $r_2^P = 0$ if $b_1 \geq 3$; $r_2^P = 1$ if $b_1 = 2$; and $r_2^P = 4$ if $b_1 = 0$. Although the Posted Price auction in the second period produces revenue of 3, extracting all the trading surplus, it still cannot make up the loss of a half-optimal intra-period revenue. Thus, the repeated static mechanism produces more revenue than the non-clairvoyant dynamic mechanism in S_B, as shown in Table 1.2.

Table 1.2: Theoretical Revenues under Scenario B.

S2-Revenue	Non-clairvoyant Dynamic		Repeated Static	
Period 1	Give for Free (F)	0	Myerson's Auction (M)	2
	Myerson's Auction (M)	2		
Period 2	Post Price Auction (P)	**3**	Myerson's Auction (M)	2
	Myerson's Auction (M)	2		
Total		3.5		4
Intra-period revenue		2		4
Intra-period revenue		1.5		0

1.4 Experimental Design and Hypotheses

We design our experiment to compare the performance of the non-clairvoyant dynamic mechanism and repeated static mechanism, as discussed in Section 3.4. To further investigate the extent to which incentive compatibility and individual rationality impact these mechanisms, we also examine truthful valuation revelation and participation behaviors.

In our experiments, participants act as buyers by trading with a robot seller individually for two items (one item per period). Participants know that there is zero cost associated with producing the items. Participants also know that the buyer may value the same item differently in different periods, and the robot seller will never know the buyer's value for the item.

At the beginning of each period, participants learn the possible valuations in that period via a pie chart. They know that the robot seller will set the reserve price in that period based on the distribution of possible buyer values. If there is an upfront fee for the period, participants learn their value of the item only after they pay the upfront fee. To win the item, a buyer must bid greater than or equal to the seller's reserve price. When this occurs, the buyer pays a price equal to the reserve price.

We create the non-clairvoyant environment by waiting until the second period to reveal possible values of the second item, so that neither the robot seller nor the participants have any knowledge in the first period regarding the future value distribution. Treatments vary both in the mechanism used to allocate the items, and in the buyer value distributions.

1.4.1 Experimental design

Our experiments have a two-mechanism-by-two-scenario design. Participants are randomly assigned to one of four treatments. Each treatment consists a two-period treading game (one round).[7] The two scenarios are S_A and S_B, as discussed in Section 3.4. In the two scenarios, we switch the distributions in the two periods, which derives reversed theoretical revenue comparison prediction.

For the two treatments conducting the repeated static mechanism, participants are told that there is no upfront fee[8] in each period and the robot seller sets reserve prices for each period based on that period's value distribution. Buyers purchase the item if they bid

[7]We used a one-shot game to establish a non-clairvoyant environment where both buyers and sellers are not aware of future distributional knowledge. Also, buyers' expectations about the future cannot play a role in a one-shot game.

[8]For subjects' ease of understanding, we used "membership fee" instead of "upfront fee," and "secret price" instead of reserve price in the experiment.

greater than or equal to the reserve price. They only need to pay the reserve price, even if they actually bid higher. The reserve price in each period is set as r_M, as described in Section 3.4.

For the two treatments conducting the non-clairvoyant mechanism treatment, the buyer knows there is no upfront fee in the first period. They also know that they have a 50% chance of purchasing the item for nothing; otherwise, they need to bid greater than or equal to the reserve price to earn the right to buy the item at the reserve price. In the second period, after learning the distributional knowledge F_2, they observe the upfront fee and decide whether to enter the auction. If they decide to pay the upfront fee, they can they learn their valuation for the item and make a bid accordingly. They are told that there is a 50% chance for the upfront fee to be refunded. They purchase the item if their bid is greater than or equal to reserve price, in which case the purchase price is equal to the reserve price. The reserve price in the first period is set as r_M. In the second period, half of the buyers are assigned to Myerson's auction, with reserve price r_M, and the remaining buyers are assigned to the Posted Price auction, with reserve price r_P.

The timeline for the NC treatments is as follows:

In Period 1,

- The seller sets a reserve price r_1 based on the distributional knowledge F_1.
- The buyer learns their value (v_1) and makes a bid, b_1.
- The buyer has a 50% chance to get the item for free, $p_1 = 0$;
  Otherwise, the buyer can buy the item only when $b_1 \geq r_1$ and pays $p_1 = r_1$.
- The buyer receives information about the reserve price and the buyer's payoff.

In Period 2,

- The seller sets an upfront fee $s_2 = min(b_1, \mathbb{E}_{v_2})$.
- The buyer decides to pay the upfront fee ($enter = 1$) or to leave ($enter = 0$). If the buyer leaves, the game is be over.

- If the buyer pays, ($enter = 1$):
  - The buyer learns their value, v_2, and makes a bid, b_2;
  - The buyer has a 50% chance to be refunded the upfront fee;
  - The seller sets two reserve prices r_2 based on the F_2, s_2 and whether the buyer receives the refund on the entry fee. The buyer can buy the item only when $b_2 \geq r_2$ and pays $p_2 = r_2$.

To mitigate potential losses, we restrict the bid to any positive integral less than twice the buyer's valuation. Each buyer starts with 50 points and can lose points during the experiment. After the two-period trading experiment we elicit risk attitudes using Holt and Laury (2002).

1.4.2 Hypotheses

We implement the environments detailed in Section 3.4 above, and our first hypotheses are based on the theoretical revenue comparison results derived in that section. We predict that the non-clairvoyant dynamic mechanism outperforms the repeated static mechanism in scenario S_A. When we flip the two distributions in the two periods in scenario S_B, the second period does not provide enough intra-period revenue to compensate for the loss from giving half of the items for free in the first period. We predict that the repeated static mechanism outperforms the optimal non-clairvoyant dynamic mechanism design. Our Hypothesis 1 is as follows.

Hypothesis 1. *In S_A, the non-clairvoyant mechanism generates greater revenue than the repeated static mechanism;*

In S_B, the non-clairvoyant mechanism generates less revenue than the repeated static mechanism.

We also investigate the bidding and participation behaviors of buyers. We first state a simple proposition:

Proposition 1. *Any non-clairvoyant mechanism achieving more revenue than repeated optimal Static mechanism violates single period Individual Rationality (IR).*

The proof is direct. Myerson's auction is the solution that satisfies single-period incentive compatibility and single-period individual rationality (IR). Likewise, the non-clairvoyant mechanism satisfies IC for each period. Thus, if the non-clairvoyant mechanism has more revenue under some market environment (e.g., Scenario A), it must violate IR in the second period.

It is worth noting that to ensure high revenue, the robot seller sets the upfront fee before the buyer decides whether to enter the market. Risk-averse buyers should take into consideration the risk[9] that the upfront fee will not be refunded. Thus, rational buyers facing high upfront fees have an incentive not to enter the second period. When upfront fees are not paid, the revenue of the non-clairvoyant mechanism declines. We have our Hypothesis 2:

Hypothesis 2. *Some buyers choose not to pay the upfront fee, such that the experimental revenue of the non-clairvoyant mechanism is less than its theoretical prediction.*

Additionally, whether participants in experiments bid their true value impacts the performance of dynamic mechanism design. Particularly in the non-clairvoyant dynamic mechanism, the upfront fee in the second period is based on the bid in the first period. Given that buyers, in general, overbid in experiments, we might observe less overbidding in the non-clairvoyant dynamic mechanism. There are two reasons for this: first, buyers will be less aggressive as they have one-half chance to get a free item in the first period; second, buyers are deterred by a possible nonrefundable upfront fee, so they bid less in case they lose more. This suggests our Hypothesis 3:

Hypothesis 3. *Participants' bids are closer to true value under NC than RS.*

[9]We view it as risk rather than loss as the non-clairvoyant dynamic mechanism is designed to satisfy the EPIR, which means the buyer is guaranteed of non-negative expected total payoff after the realization of valuations in two periods. In addition, the buyer is not informed whether the upfront fee is refunded or not before making a bid in Period 2.

1.4.3 Procedures

The study was pre-registered on OSF Registries (`https://osf.io/a2ber/`). We recruited our participants from George Mason University. We advertised our study on the recruiting system (experiments.gmu.edu). We pre-selected only subjects over 18-years-old. The advertisement specified that the experiment would last for 45 minutes. Subjects were informed that they could receive a participation bonus of \$10, as well as additional payments, depending on their decisions in the experiment.

The experiment was programmed in oTree (Chen et al., 2016) and conducted from September to October in 2021. We used a between-subject design where 256 Subjects were randomly assigned to one of the four treatments. For each treatment, we collected 64 independent observations.[10]

Subjects received instructions and then took a quiz. After answering all quiz questions correctly, they proceeded to a two-period practice session and a two-period bidding task, followed by a risk-aversion elicitation (Holt and Laury, 2002). They were paid in cash privately after completing a demographic questionnaire.

1.5 Results

Demographic summary statistics are reported in Table 1.3. We have balanced gender for each treatment. Among 256 subjects, 47% are male, with an average age of 22. The average risk attitude index[11] from is 4.63 (risk aversion),[12] and the average payoff is \$17.1 (including \$10 show-up bonus).

[10] We ran G^* power analysis:for $\alpha = 0.05$, balanced sample size of 64 in each treatment, one-tail t-test has power $= 1 - \beta = 0.85$ for mechanism comparison in Scenario A, power $= 1 - \beta = 0.99$ in scenario B. We assume 10% of buyers quit the second period in NC, and 20% buyers deviate from bidding true value in both NC and RS in determining the pairs (mean, standard deviation): (3.78, 0.80) for RS, (4.20, 0.96) , (3.10, 0.83) for NC in Scenario A, and in Scenario B, respectively.

[11] The risk attitude index is the point where the subject switches from choosing a risky lottery to a safe lottery. We observe 14.8% of subjects to switch at least twice in the experiment, and there is no significant difference between treatments in switching more than once.

[12] We do not observe significant differences on gender, age, or risk attitude among treatment.

Table 1.3: Summary Statistic

Treatment	Scenario A		Scenario B	
	NC	RS	NC	RS
Age	21.6	22.3	21.9	22.7
Gender (Male=1)	0.48	0.43	0.50	0.47
Risk aversion	4.45	4.91	4.51	4.63
Observation	64	64	64	64

1.5.1 Experimental observations match with theoretical prediction

We first test Hypothesis 1 by comparing revenues between the two mechanisms under two scenarios. Theory predicts that the non-clairvoyant dynamic mechanism should outperform the repeated static mechanism in Scenario A, but not in Scenario B. Our experimental results verify the theoretical prediction.

The revenue of the first period is shown in Figure 1.1[13]. In S_A, the first-period revenue in the non-clairvoyant dynamic mechanism (0.97) is only half of the revenue in the repeated static mechanism (1.93). The reason is that the non-clairvoyant dynamic mechanism gives half of the buyers a free item in the first period. The experimental revenue is consistent with the theoretical prediction. Given that the optimal reserve price is the same in both scenarios, the revenue comparison under Scenario B has exactly the same result as Scenario A.

[13] Standard errors are shown in those error bars

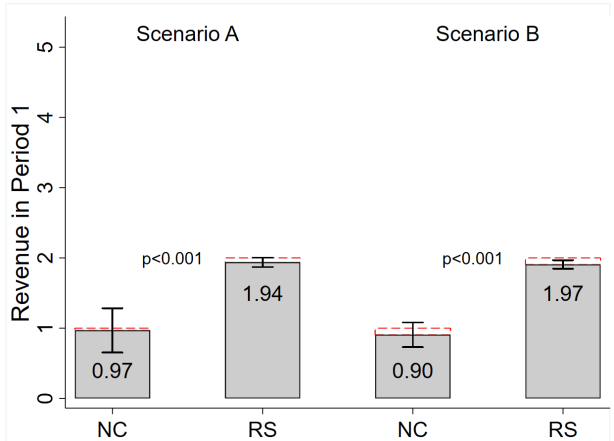


Figure 1.1: Revenue of Period 1 in each Treatment

In the second period, the non-clairvoyant dynamic mechanism sets up the upfront fee after both seller and buyer learn the distributional knowledge. Observing the customized upfront fee, buyers decide whether to enter the second period trade and bid on the item or to quit. When buyers are assigned to the Myerson's auction, the reserve price is the optimal reserve price (2), and the upfront fee is refunded; otherwise, they cannot redeem the upfront fee, and usually pay a lower reserve price. The upfront fee, which is bounded by the expected valuation of buyers, plays a critical role in the success of the non-clairvoyant dynamic mechanism. As a result, if the economy is in Scenario A, where the expected valuation of buyers is increasing in the second period, the seller can set a higher upfront fee. This leads to the outperformance of the non-clairvoyant dynamic mechanism compared to the repeated static mechanism. On the other hand, if the economy is in Scenario B, where the second-period valuation is lower on average, the non-clairvoyant dynamic mechanism is out-performed by the repeated static mechanism.

Total revenue is shown in Figure 1.2: bars with red dash outlines represent the theoretical revenue prediction; light gray bars represent the first-period revenue; dark grey bars represent the second-period revenue; and the whisker bars represent one standard error. The experimental revenue of the non-clairvoyant dynamic mechanism is significantly greater than the repeated static mechanism in Scenario A ($p = 0.057$, one-sided t-test[14]), but is significantly less in Scenario B ($p < 0.001$, one-sided t-test)[15].

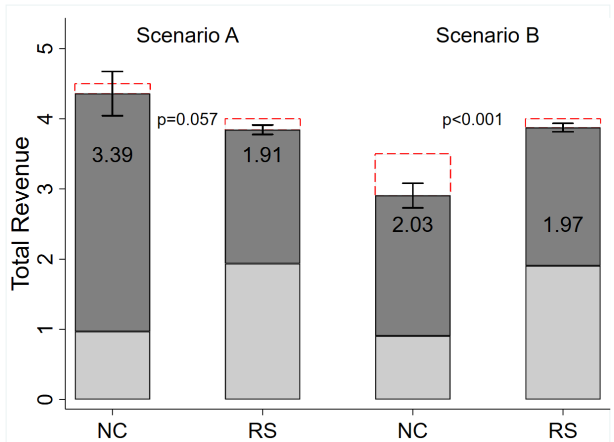


Figure 1.2: Total Revenues in each Treatment

The revenue decomposition of the two mechanisms in S_A is reported in Table 1.4. In the first period, the non-clairvoyant dynamic mechanism is the uniform combination of the Give For Free mechanism and Myerson's auction. In the second period, the non-clairvoyant dynamic mechanism combines Posted Price Auction with Myerson's auction in the same way. The theoretical revenue prediction and the experimental data on revenue are shown in the third and the fourth columns, respectively. The repeated static mechanism conducts Myerson's auction in each period. Theoretically, Myerson's auction achieves the same revenue in both periods. By comparing the theoretical revenue with the experimental revenue, we further verify that the experimental result supports the theory.

[14]We do not use Wilcoxon–Mann-Whitney rank sum test as the variance of non-clairvoyant revenue is greater than the variance of repeated-static revenue. For one-sided Wilcoxon–Mann-Whitney rank sum test, $p = 0.495$ in Scenario A, $p < 0.001$ in Scenarios B.

[15]To be more specific, we use t-test with unequal variance here. And we apply one-side test as a response to the theoretical prediction

Table 1.4: Revenue Decomposition in Scenario A

Revenue in S_A	Non-clairvoyant Dynamic			Repeated Static		
	Theory		Experiment	Theory		Experiment
Period 1	Give it for free	0	0	Myerson's	2	1.94 (0.04)
	Myerson's auction	2	1.94 (0.06)			
Period 2	Post Price Auction	**5**	4.84 (0.47)	Myerson's	2	1.91 (0.05)
	Myerson's auction	2	1.94 (0.06)			
Total		5	4.35 (0.32)		4	3.84 (0.07)

Notes: Standard errors in parentheses.

It is worth noting that the Posted Price auction in the second period gives the non-clairvoyant dynamic mechanism the most revenue in Scenario A: the theoretical revenue of the Posted Price auction is 5 and the experimental revenue is 4.84. However, the Posted Price auction cannot always provide good revenue, as the upfront fee sellers can charge has an upper bound equal to the second-period expected valuation of the buyer. This is why in Scenario B, the Posted Price auction can only achieve a revenue of 3, as the second-period expected valuation of buyers is decreasing from 6 to 3. Accordingly, even though the Posted Price can compensate for part of the revenue loss from the Give For Free mechanism in the first period, the non-clairvoyant dynamic mechanism fails to outperform the repeated static mechanism, both theoretically and experimentally. The revenue decomposition of the two mechanisms in Scenario B is reported in Table 1.5.

Table 1.5: Revenue Decomposition in Scenario B

Revenue in S_B	**Non-clairvoyant Dynamic**			**Repeated Static**		
	Theory		**Experiment**	**Theory**		**Experiment**
Period 1	Give it for free	0	0	Myerson's	2	1.91 (0.05)
	Myerson's auction	2	1.81 (0.10)			
Period 2	Post Price Auction	**3**	2.31 (0.92)	Myerson's	2	1.97 (0.03)
	Myerson's auction	2	1.75 (0.12)			
Total		3.5	2.93 (0.18)		4	3.88 (0.06)

Notes: Standard errors in parentheses.

We conclude our first result as below.

Result 1. *Hypothesis 1 is supported. Experimental observations match theoretical predictions.*

In S_A, the non-clairvoyant dynamic mechanism gains more revenue than the repeated static mechanism.

In S_B, the non-clairvoyant dynamic mechanism gains less revenue than the repeated static mechanism.

1.5.2 Risk aversion and participation decisions

At the beginning of the second period in the non-clairvoyant dynamic mechanism, buyers can refuse to pay upfront fees and choose to quit. In Scenario A, four buyers quit the second period; this number doubles in Scenario B. Buyers choosing not to pay upfront fees negatively impacts the revenue of the non-clairvoyant dynamic mechanism.

As shown in Figure 1.3, if the four buyers paid the upfront fee, entered in the second period, and bid their true value, the revenue of the non-clairvoyant mechanism in S_A would increase by 78% of the gap between the theoretical prediction and the experimental observation[16]. The remaining 22% of the gap is attributable to underbidding during both

[16]The revenue of NC (4.47) if all buyers enter in Scenario A is less than its theoretical revenue (4), but the difference is not significant (p=0.46, one-sided t-test)

periods. Underbidding in the first period results in fewer paid upfront fees, and therefore less revenue in Period 2. Underbidding in the second period means buyers do not get to buy the item, as their bid is less than reserve price.

If the eight buyers paid the upfront fee, entered in the second period, and bid their true value, the revenue of the non-clairvoyant mechanism in S_B would increase by 45% of the gap between theoretical prediction and experimental observation[17]. The remaining 55% comes from their underbidding.

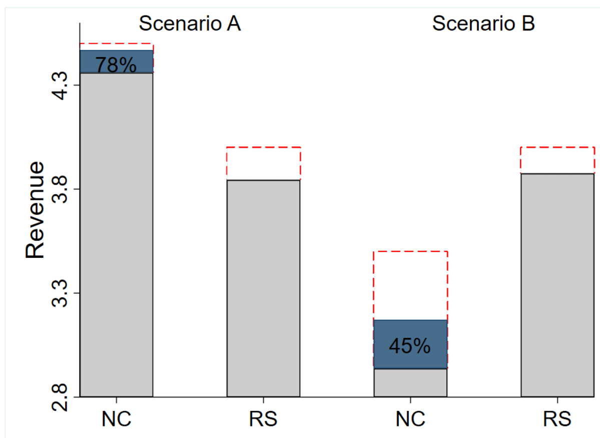


Figure 1.3: Revenues Increase if all Buyers Enter in Second Period.

We use OLS regressions[18] to further explore how buyers make participation decisions in Period 2. Based on comments of buyers who choose not to enter in Period 2 (see Appendix A), we include scenario (Scenario A =1), risk attitude index (i.e., number of safe choices, the higher the risk attitude index, the more risk averse is the buyer), whether the buyer receives the item for free in Period 1 ($not\ free_1 = 1$), the amount of upfront fee in Period 2 ($upfront_2$), the payoff in Period 1 ($payoff_1$), and the bid-value ratio in Period 1 ($bid_1/value_1$) as independent variables. We find buyers participate less in Scenario B even

[17]The revenue of NC (3.17) if all buyers enter in Scenario B is less than its theoretical revenue (3.5), the difference is still significant (p=0.01, one-sided t-test).

[18]We find similar results with Probit regressions.

after controlling for the payoff in Period 1. This suggests that buyers view participation in Period 2 with low expected value in Scenario B not worthy of a risky upfront fee. The risk attitude index does not directly affect the participation decision in Period 2. However, we find that, in Scenario A, buyers participate less in Period 2 when they do not receive the item for free in Period 1. A possible reason is that buyers who do not receive the item for free might think they were more likely to not receive the refund on the upfront fee, so that they view the participation in Period 2 an even more risky choice.

Table 1.6: Regression of Participation Decision

	DV: Enter in Period 2 (=1)	
	(1)	(2)
Scenario A (=1)	0.17^{**}	0.25^{*}
	(0.08)	(0.13)
$notfree_1 (= 1)$	0.07	0.08
	(0.07)	(0.11)
Scenario A * $notfree_1$	-0.18^{*}	-0.14
	(0.10)	(0.17)
$payoff_1$	0.00	0.00
	(0.01)	(0.01)
risk aversion	-0.01	-0.03
	(0.01)	(0.02)
$upfront_2$	-0.01	-0.03
	(0.03)	(0.05)
$bid/value_1$	-0.02	-0.09
	(0.07)	(0.11)
Constant	0.91^{***}	1.28^{***}
	(0.11)	(0.43)
Controls	No	Yes
R^2	0.05	0.14
Adj. R^2	-0.01	0.00
Num. obs.	128	128

Note: For Regressions (2), gender (male =1), age and graduate student (=1) are introduced as controls. We do not find significant controls.

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

We have our second result:

Result 2. *Hypothesis 2 is supported. Some buyers choose not to pay the upfront fee, leaving the revenue of the non-clairvoyant mechanism less than its theoretical prediction.*

1.5.3 Buyers overbid less under Non-clairvoyant Mechanism

In the experiment, the highest amount the buyer can bid is twice their valuation. We switch the distributions of buyers' valuation in two periods for our two scenarios. Scenario A has low-variance distribution (F_A) in the first period and high-variance distribution (F_B) in the second period. Scenario B has high-variance distribution (F_B) in the first period and low-variance distribution (F_A) in the second period. In general, buyers overbid in both distributions in both mechanisms. We report the bid-value ratio in Table 1.7. We find that buyers overbid in both mechanisms, as the ratio is greater than one for both distribution F_A and F_B. Under the repeated static mechanism, buyers overbid more ($p = 0.008^*$, two-sided t-test) in F_A (low variance distribution). The difference between the two distributions disappears under the non-clairvoyant dynamic mechanism. For the low-variance distribution, we find that buyers under the non-clairvoyant mechanism overbid less ($p = 0.06^*$, two-sided t-test) compared to buyers under the repeated static mechanism.

Table 1.7: Bid-Value Ratio Comparison

Bid/value	**Non-clairvoyant Dynamic**	**Repeated Static**	p-value
F_A (low variance)	1.264 (0.04)	1.379 (0.04)	0.060
F_B (High variance)	1.194 (0.05)	1.251 (0.04)	0.392
p-value	0.116	0.008	

Note: Standard errors in parentheses.

We use OLS regression to further investigate the different bidding behaviors under the two mechanisms. We consider bid-value ratio in each period separately. In Regressions (1) and (2), we regress bid-value ratio in Period 1 on mechanism (NC=1), scenario (Scenario A=1), valuations, and risk attitude index. We regress bid-value ratio in Period 2 also on valuation in Period 2 ($value_2$), the upfront fee ($upfront_2$), and whether the buyer gets the free item in Period 1 ($free_1$) in Regressions (3) and (4). From Regression (1), we find that buyers in the non-clairvoyant dynamic mechanism overbid less compared to those in the repeated static mechanism in Period 1 for both Scenario A and Scenario B. Perhaps buyers overbid less under NC in Period 2 due to the fact that a 50% chance of receiving a free item encourages less overbidding. This finding supports Hypothesis 3. When controls are included in Regression (2), risk aversion plays a role in mitigating the overbidding behavior. The overbidding lasts across periods, as shown in Regressions (3) and (4). The higher the bid-value ratio in Period 1, the higher the bid-value ratio in Period 2. Regression (4) further supports our Hypothesis 3 that buyers overbid less in NC, as the upfront fee in Period 2 mitigates the overbidding behavior, and the higher the upfront entry fee, the lower the bid-value ratio in Period 2. We report an analysis of whether a bid is greater than the valuation in Appendix A, and find similar results.

Table 1.8: Regression of bid-value ratio on mechanisms

	DV: Bid-Value Ratio			
	Period 1		Period 2	
	(1)	(2)	(3)	(4)
NC (=1)	-0.10	-0.09	0.03	0.35
	(0.06)	(0.08)	(0.14)	(0.20)
Scenario A (=1)	0.03	0.1	-0.10*	-0.1
	(0.06)	(0.08)	(0.06)	(0.09)
$value_1$	-0.02	-0.02		
	-0.01	-0.01		
Risk aversion	-0.03	-0.06	0.04	0.03
	(0.02)	(0.02)	(0.02)	(0.03)
$bid_1/value_1$			0.39	0.35
			(0.06)	(0.10)
$value_2$			-0.01	-0.01
			(0.01)	(0.01)
$upfront_2$			-0.02	-0.10
			(0.04)	(0.06)
$free_1$			0.04	0.01
			(0.08)	(0.12)
Constant	1.51	1.17	0.78	0.57
	(0.11)	(0.32)	(0.13)	(0.36)
Controls	No	Yes	No	Yes
Observations	256	256	244	244
R-squared	0.08	0.14	0.19	0.19

Note: For Regression (3) and (6), gender (male =1), age, and graduate student (=1) are introduced as controls. We do not find significant controls.

Standard errors in parentheses.

We then have our third result:

Result 3. *Hypothesis 3 is supported. Buyers overbid less under non-clairvoyant mechanism.*

1.6 Conclusion

We use laboratory experiments to test the mechanisms feasible under a non-clairvoyant dynamic environment (Mirrokni et al., 2020), where designers are restricted from using future distributional information. To our knowledge, our paper is the first to do so. In contrast to the traditional clairvoyant assumption that everyone perfectly knows the stochastic process governing all future economic outcomes, the non-clairvoyant environment is more practical. The reason is that mechanism design under this environment does not require a reliable future forecast regarding future distributions and outcomes; as a result, it follows a general, simple, implementable form. This environment is particularly related to online advertising markets, where advertisers repeatedly purchase impressions on webpages from advertising platforms like Google Ads, Microsoft Advertisement, Meta for Business, etc. In this market, both advertisers and advertising platforms are non-clairvoyant about possible values of future impressions, as internet traffic patterns can change suddenly due to naturally-occurring events. Other potential scenarios for deploying mechanism design under non-clairvoyant dynamic environments include designing airplane tickets, setting up rules for repeated selling, and constructing long-term contracts.

This paper tests two dynamic mechanisms feasible under a non-clairvoyant environment. Using past bids to design rules for later periods, the optimal non-clairvoyant dynamic mechanism (Mirrokni et al., 2020) links auctions in multiple periods to ensure inter-period revenue. By contrast, the repeated static mechanism (Myerson, 1981) optimizes each period individually and generates the most intra-period revenue. While the optimal non-clairvoyant dynamic mechanism provides the best revenue guarantee (at least 50% of that produced by the optimal clairvoyant mechanism in the two-period single-buyer case) its ability to outperform the repeated static mechanism depends on the relative size of intra- and inter-period revenues. We design two experimental scenarios with different inter-period revenues. Our findings support theoretical predictions: when the inter-period revenue is important, NC achieves more revenue than RS, as it guarantees 50% of the inter-period revenue; otherwise, NC has less revenue, as the loss of intra-period revenue from allocating half of the item for free cannot be recovered.

Our experiments illustrate that risk attitude matters in non-clairvoyant dynamic mechanism design. The customized upfront fee in the second period and the associated reserve price are critical in the inter-period revenue of the optimal non-clairvoyant dynamic mechanism. The upfront fee in the second period is well-designed to ensure non-negative expected total payoff of buyers. However, in our experiments, we observe many buyers choosing not to enter in the second period. The revenue loss from the buyer choosing not to participate comprises at least 45% of the revenue gap that we find between our experiments and theory. We find risk attitudes and first-period experience can help to explain second-period participation decisions. More generally, our data raises the question of how to promote entry in natural environments with risk-averse buyers.

Bidding behaviors also vary between mechanisms and valuation distributions. We find that buyers overbid in general, and more so when the valuation distribution has high variance in the repeated static mechanism. However, this gap disappears under the non-clairvoyant dynamic mechanism. We find that the non-clairvoyant dynamic mechanism mitigates overbidding behavior.

A limitation of our study is that it considers only a two-period single-buyer environment. Both non-clairvoyant dynamic mechanisms and repeat static mechanism can, of course. be applied to multi-period multi-buyer environments, and studying these might be interesting, particularly to those designing auctions for natural environments. Future studies can also investigate sellers' decisions regarding how to choose mechanisms, complementing our focus on buyers' behaviors.

Our main finding is that non-clairvoyant dynamic mechanisms appear to work as intended, and may represent an alternative to repeated static mechanisms in dynamic environments. They are generally applicable, simple in form, and come with a revenue guarantee. But NC mechanisms cannot provide value if they are not used. Knowing when and whether sellers in non-clairvoyant environments choose to implement NC mechanisms, especially as compared to the potentially sub-optimal RS alternative, is an important open question. Additionally, understanding how to promote buyer participation decisions under NC mechanisms promises to be a profitable avenue for future research.

Chapter 2: How Sellers Choose Mechanisms: Information Matters

2.1 Introduction

Understanding how sellers select dynamic mechanisms in the real world can aid in designing more accessible mechanisms. Since the seminal work of Baron and Besanko (1984), theorists have provided fruitful findings on the optimal price-discovery and allocation rules sellers should choose (see Bergemann and Välimäki (2019) for a recent survey). However, dynamic mechanisms are not widely used, due to their complexity and a lack of general form (Mirrokni et al., 2020). Thus, it is difficult to determine the mechanism human sellers would actually choose.

Designing an optimal dynamic mechanism[1] is complicated, in that its "clairvoyance" should allow sellers to use all information, including future demand. Therefore, the optimal dynamic mechanism must be tailored to a specific trading environment (Balseiro et al., 2018; Jackson and Sonnenschein, 2007; Pavan et al., 2014). For example, in Courty and Li (2000), where agents faced asymmetric uncertainty, the optimal mechanism involved subsidizing agents with less future uncertainty to reduce rent-seeking by consumers facing greater uncertainty. Similarly, in Eső and Szentes (2007), where agents obtained a private value signal from the seller, the optimal mechanism included a premium paid for extra information, combined with a second price auction where the winner paid the second-highest bid plus the premium.[2] The complexity of the clairvoyant dynamic mechanism means few experimental studies have focused on sellers' behaviors in a dynamic mechanism.

[1] Dynamic mechanism design studies how to determine the optimal price-discovery and allocation rules as buyers receive information over time.

[2] See Board (2007) for extension to infinite time horizon.

Recently, Mirrokni et al. (2020) introduced a new family of dynamic mechanisms: the "non-clairvoyant" dynamic mechanism, which restricts sellers from using future demand information. Non-clairvoyant dynamic mechanisms are scenario-independent, simple in form, and robust to differences in beliefs across participants. They can also be conducted in a lab. As a result, they offer a helpful resource for experimental studies on how human sellers choose dynamic mechanisms.

Here, we design experiments to study how sellers choose between two easily-conducted non-clairvoyant dynamic mechanisms: the optimal non-clairvoyant dynamic mechanism (henceforth NC, Mirrokni et al. (2020)) and the optimal repeated static mechanism (henceforth RS, Myerson (1981)). We focus on a two-period single-buyer environment. In this setting, RS conducts two Myerson's auctions to maximize each period's revenue independently, while NC works to guarantee revenues equal to at least 50% of those produced by the optimal clairvoyant mechanism. In Period 1, NC allocates half by give-for-free auction (buyer gets item for free) and the other half by Myerson's auction; in Period 2, NC first asks for a buyer-specific upfront entry fee, and then conducts auctions with buyer-specific reserve prices, after which half of the buyers can receive a refund on their upfront entry fee. (Myerson's auction is used if the upfront fee is refunded; otherwise, a posted-price auction is used.) Importantly, buyers who participate in the first period are not required to pay the upfront fee and participate in period 2. While it is theoretically optimal to participate, they may nevertheless choose not to participate in period 2 after participating in period 1.

Deciding on a dynamic mechanism involves a mapping from received information to the chosen mechanism and the price. Using NC and RS as the starting points, we attempt to determine the information human sellers use in choosing a dynamic mechanism, as well as whether that information changes over time. Specifically, we focus on three aspects of information: (i) the relative simplicity of a mechanism; (ii) the scenario-specific demand; and (iii) the feedback on revenue.

To investigate whether the relative simplicity of a mechanism affects sellers' choice of dynamic mechanism, we design two treatments with varying relative simplicity of NC. In treatment "Set 4", if NC is chosen, sellers must set four prices: two Myerson's reserve prices; the upfront fee; and the reserve price when the upfront fee is not refunded. In treatment "Set 2", sellers must only set two reserve prices for the Myerson's auction. The other two prices are set by the computer optimally. NC is relatively more challenging than RS to set up in treatment Set 4. Based on cognitive burden theory (Sweller, 1988), we expect sellers to choose NC more in treatment Set 2 due to the relative ease of NC in that treatment.

Learning the optimal mechanism for each scenario is challenging, as all prices in NC and RS depend on specific demand in each period, and the revenue comparison between NC and RS lacks a general form. Special examples in Papadimitriou et al. (2022) and Gui and Houser (2023) provide intuitions for the revenue comparison between NC and RS. In their examples, Myerson's revenue is the same in both periods, and the revenue comparison between NC and RS depends only on the expected demand in Period 2, which is the upper bound for the revenue of the posted-price auction (revenue from the upfront fee and the associated reserve price) to ensure the buyer's participation in Period 2. Intuitively, if the expected demand in Period 2 is low, the revenue lost by NC in Period 1 (giving the buyer the item for free) cannot be made up, so NC generates less revenue than RS. For a similar reason, NC can potentially outperform RS only when the expected demand is high in Period 2. This happens when Period 2 involves "target buyers" who value the item highly but exist in low probability. In this case, the posted-price auction captures more revenue than Myerson's auction, and NC thus generates more revenue than RS.

To study whether sellers choose a mechanism based on scenario-specific demand, we extend examples in Papadimitriou et al. (2022) and Gui and Houser (2023) and design 12 distinct scenarios (one for each round) with different theoretical comparisons between NC and RS. Those scenarios further simplify the decision-making problem and help sellers learn the intuition. As Harstad (2000) determined, subjects with experience in English auctions appear to transfer much of what they learned in that institution to later second-price auctions. In our experiment, we expect sellers to learn[3] the intuition of revenue comparison between NC and RS, particularly in the later stages after gaining experience in the trading environment. Feedback is also provided in each round to further study how sellers learn mechanisms according to past revenue.

Results show that sellers are indifferent to the relative simplicity in choosing a mechanism, as shown by no treatment difference in choosing NC. Strikingly, human sellers in our experiment can learn the intuition of revenue comparison between NC and RS in different scenarios and choose the correct mechanism with greater revenue for that scenario in the later stages of the experiment, after they have gained experience in the trading environment. We further find that sellers adjust mechanisms over time based on previously achieved revenue. Our findings shed light on how sellers choose mechanisms, which could provide helpful insight for designing more accessible dynamic mechanisms in the future.

In the experiment, sellers who make good decisions receive more revenue than sellers who choose the wrong mechanism. Consistent with Gui and Houser (2023), the theoretical revenue gap derives mostly from some buyers not participating in Period 2 in NC. Our results further indicate that buyers quit more in Period 2 in treatment Set 4, as sellers set greater-than-suggested upfront fees. We note the concern about the role of upfront fees in a dynamic mechanism design, as well as that sellers' behavioral considerations might play an important for future dynamic mechanism design.

The remainder of the paper is organized as follows. Section 2.2 illustrates the theoretical framework. Section 2.3 presents our experimental design based on the theoretical revenue comparison of NC and RS. Section 2.4 states corresponding hypotheses. Section 2.5 provides experimental results. Section 2.6 concludes.

[3]Learning of participants in multi-round experiments is intensively studied in the literature (see Bigoni and Suetens (2012) for the public goods game; see Matthey and Regner (2013) for the dictator game, the ultimatum game, and the trust game; see Benndorf et al. (2017) for a summary. The learning effect varies in context (Pritchett and Sandefur, 2015).

2.2 Theoretical Framework

2.2.1 Two-period Dynamic Trading

We study a two-period repeated selling environment. In each period, a newly-arrived item must be sold to a single buyer. It is common knowledge that producing an item in both periods exerts zero cost, and valuations are the private information of the single buyer.

A specific scenario is defined as the pair of distributions of the buyer's valuation (F_1, F_2), i.e., the demand for the item in each period. The two distributions are common knowledge from the beginning of Period 1. The first period valuation is independent of the second period valuation.[4]

The sequence of events and actions in the selling game in each period $t \in \{1, 2\}$ are summarized as follows:

1. The seller describes their allocation rule $x_t \in [0, 1]$ and price policy $p_t \in \mathbb{R}^+$.
2. The buyer who enters the trade learns their valuation of the item $v_t \sim F_t$, and makes a bid b_t.
3. The seller implements their allocation rule x_t and price policy p_t.
4. The buyer gains periodical utility $u_t = u_t(b_t, v_t) = v_t \cdot x_t - p_t$.

We focus on direct mechanisms satisfying the buyer's participation constraint. That is, the dynamic mechanisms with allocation rules (x_1, x_2) and price policy (p_1, p_2) are subject to Dynamic Incentive Compatibility (DIC) and Ex Post Individual Rationality (EPIR) as detailed below.

$$
\begin{aligned}
DIC: &\quad \begin{cases} u_2(v_2, v_2) \geq u_2(b_2, v_2), & \forall v_2 \in F_2 \\ u_1(v_1, v_1) + \mathbb{E}_{v_2} u_2(v_2, v_2) \geq u_1(b_1, v_1) + \mathbb{E}_{v_2} u_2(v_2, v_2), & \forall v_1 \in F_1 \end{cases} \\
EPIR: &\quad u_1 + u_2 \geq 0, \qquad\qquad\qquad\qquad \forall v_1 \in F_1, v_2 \in F_2
\end{aligned}
$$

[4] For similar settings, see Devanur et al. (2019). In Devanur et al. (2019), the buyer's valuation follows the same distribution in both periods. By contrast, in our setting, the distribution in each period is not necessarily the same.

Here, DIC is illustrated using backward induction: in Period 2, the buyer can achieve the greatest payoff by bidding their valuation; then, in Period 1, the buyer also has an incentive to report truthfully, given that true value is the optimal bid in Period 2. EPIR requires a non-negative total utility of the buyer $(u_1 + u_2)$ after the realization of the value for both periods.

Denote $\mathcal{M}$ as the set of mechanisms satisfying DIC and EPIR. For any dynamic mechanism $DM \in \mathcal{M}$, we define its revenue Rev^{DM} as a function of (F_1, F_2), the cumulative distribution functions of the demand in each period, in the natural way:

$$Rev^{DM} = Rev_1^{DM} + Rev_2^{DM} = \mathbb{E}_{v_1} p_1(v_1; F_1, F_2) + \mathbb{E}_{v_2} p_2(v_2; F_1, F_2).$$

Rev^{DM}, the revenue of a dynamic mechanism, can be further decomposed into intra-period revenue and inter-period revenue (Mirrokni et al., 2020). The intra-period revenue derives from rules that use only information within a period. The remainder of Rev^{DM} is the inter-period revenue derived from linking information across periods.

2.2.2 Two Simple Dynamic Mechanisms

Fixing the pair of distributions on demand in the two period (F_1, F_2), the optimal dynamic mechanism (denoted as $*$) generates revenue:

$$Rev^* = \max \left[\mathbb{E}_{v_1} p_1(v_1; F_1, F_2) + \mathbb{E}_{v_2} p_2(v_2; F_1, F_2)\right].$$

The optimal dynamic mechanism with the greatest revenue Rev^* requires the rules in Period 1 to use the distributional knowledge in Period 2. That is, $p_1^*(v_1; F_1, F_2)$, the optimal price rule in Period 1, is a function of the demand in Period 2. The optimal dynamic mechanism thus lacks general form and is complicated to calculate (Mirrokni et al., 2020; Papadimitriou et al., 2022).

In this section, we introduce two fixed-form mechanisms: the optimal repeated static mechanism (RS) and the optimal non-clairvoyant dynamic mechanism (NC). Both mechanisms belong to the family of "non-clairvoyant" dynamic mechanisms in that they do not use future distributional knowledge (F_2) in determining rules in Period 1. That is, their price rule in Period 1 $p_1(v_1; F_1)$ is not a function of the demand in Period 2. While RS and NC have different objectives, they both satisfy DIC and EPIR.

Optimal Repeated Static Mechanism

A dynamic mechanism can be viewed as a repeated static mechanism if x_t, the allocation rule, and p_t, the price policy at time t, depend only on information in period t (i.e., the distributional knowledge F_t and the bid b_1 in that period). A repeated static mechanism separates the dynamic two-period as two static periods. Likewise, the optimal repeated static mechanism (RS) implements Myerson's auction (Myerson (1981), denoted as M) in each period independently.

In the single-buyer case, Myerson's auction charges a reserve price and allocates the item to the buyer with the bid that is greater than or equal to the reserve price. The reserve price in Myerson's auction is the monopoly price in each period (denoted as r_t^*). The price rule in Myerson's auction is $x_t^M = r_t^* = arg\max r(1 - F_t(r))$, and the allocation rule in M is $p_1^M = r_1^* \cdot \mathbb{1}\{v_1 \geq r_1^*\}$ for $t = 1, 2$. Accordingly, x_t^{RS} , the allocation rule, and p_t^{RS} , the price policy in RS, are summarized as follows:

$$\begin{cases} x_1^{RS} = x_1^M = \mathbb{1}\{v_1 \geq r_1^*\}, \\ p_1^{RS} = p_1^M = r_1^* \cdot \mathbb{1}\{v_1 \geq r_1^*\}, \\ p_1^{RS} = x_2^M = \mathbb{1}\{v_2 \geq r_2^*\}, \\ p_2^{RS} = p_2^M = r_2^* \cdot \mathbb{1}\{v_2 \geq r_2^*\}. \end{cases}$$

Thus, the revenue in the optimal repeated static mechanism is the sum of revenues in Myerson's auction in the two periods, $Rev^{RS} = Rev_1^M + Rev_2^M$, where Myerson's revenue in the two periods is $Rev_1^M = r_1^*(1 - F_1(r_1^*))$ and $Rev_2^M = r_2^*(1 - F_2(r_2^*))$ respectively.

In RS, there is maximized intra-period revenue in each period, but zero inter-period revenue. The reason is that the mechanism does not use any information in Period 1 to determine rules in Period 2. Given that the optimal dynamic revenue Rev^* contains the maximized intra-period revenue and inter-period revenue, the ratio $\frac{Rev^{RS}}{Rev^*}$ can be arbitrarily small (Papadimitriou et al., 2022).

Optimal Non-clairvoyant Dynamic Mechanism

The objective of the non-clairvoyant dynamic mechanism (Mirrokni et al., 2020) is to have the greatest revenue guarantee, i.e., to maximize the non-clairvoyant revenue to clairvoyant revenue ratio, $\frac{Rev^{NC}}{Rev^*}$ for any demand in Period 2. The optimal non-clairvoyant dynamic mechanism (NC) can guarantee 50% of the optimal dynamic revenue in the two-period case. In Period 1, NC allocates half of the items by Myerson's auction and the other half by a give-for-free auction (denoted as F). In Period 2, NC allocates half of the items by Myerson's auction and the other half by a posted-price auction (denoted as P).

Specifically, in Period 1, the give-for-free auction allocates the item to the buyer regardless of bid and with no charge: the allocation rule is $x_1^F = 1$, and the price rule is $p_1^F = 0$, so that the revenue in F is $Rev_1^F = 0$. In Period 2, the posted-price auction sets an upfront fee in advance for the buyer and then conducts an auction with reserve price. The upfront fee is customized for each buyer based on the buyer's bid in Period 1, $s_2 = \min\{b_1, \mathbb{E}_2\}$, where $\mathbb{E}_2$ is the expected valuation in Period 2. To ensure the buyer's participation *ex ante*, it is bounded by the second period expected valuation. To satisfy the EXIP, it is bounded by the first period bid. The associated reserve price r_2^P is set to ensure the buyer's payoff in Period 2 is zero, so that all dealing surplus is exploited. That is, $\mathbb{E}_{v_2}[v_2 - r_2^P]^+ - s_2 = 0$. The allocation rule in the posted-price auction is $x_2^P = \mathbb{1}\{v_2 \geq r_2^P\}$; the price rule in P is

$p_2^P = s_2 + r_2^P \cdot \mathbb{1}\{v_2 \geq r_2^P\}$; and the revenue in P is $Rev_2^P = \mathbb{E}p_2^P$. Accordingly, we summarize the allocation rule (x_t^{NC}) and the price rule (p_t^{NC}) of NC below.

$$\begin{cases} x_1^{NC} = \frac{1}{2}[x_1^M + x_1^F] = & \frac{1}{2}(1 + \mathbb{1}\{v_1 \geq r_1^*\}), \\ p_1^{NC} = \frac{1}{2}[p_1^M + p_1^F] = & \frac{1}{2} r_1^* \cdot \mathbb{1}\{v_1 \geq r_1^*\}, \\ x_2^{NC} = \frac{1}{2}[x_2^M + x_2^P] = & \frac{1}{2}[\mathbb{1}\{v_2 \geq r_2^*\} + \mathbb{1}\{v_2 \geq r_2^P\}], \\ p_2^{NC} = \frac{1}{2}[p_2^M + p_2^P] = & \frac{1}{2}[r_2^* \cdot \mathbb{1}\{v_2 \geq r_2^*\} + s_2 + r_2^P \cdot \mathbb{1}\{v_2 \geq r_2^P\}]. \end{cases}$$

The revenue of NC is half of Myerson's revenues in the two periods and half of the revenue from the posted-price auction in Period 2, $Rev^{NC} = \frac{1}{2}(Rev_1^M + Rev_2^M + Rev_2^P)$. As shown in Mirrokni et al. (2020), the optimal inter-period revenue is bounded by $\mathbb{E}(s_2)$, which is exactly the expected upfront fee in the posted-price auction in Period 2. Hence, NC captures exactly half of the optimal intra-period revenue ($\frac{1}{2}Rev_1^M + \frac{1}{2}Rev_2^M$), and at least half of the optimal inter-period revenue $\frac{1}{2}Rev_2^P$.

2.2.3 Theoretical Revenue Comparison

The theoretical revenue difference between NC and RS is denoted below:

$$Rev^{NC} - Rev^{RS} = \frac{1}{2}[Rev_2^P - (Rev^{M_1} + Rev^{M_2})].$$

The relative size of inter-period revenue from a posted-price auction (Rev_2^P) with respect to the optimal intra-period revenue ($Rev^{M_1} + Rev^{M_2}$) determines the theoretical revenue comparison between RS and NC. Given that revenues from both the Myerson's auction and the posted-price auction depend on demand in the two periods (F_1, F_2), the theoretical revenue comparison is specific-scenario dependent. In a natural way, we characterize scenarios into three categories according to the theoretical revenue comparison between RS and NC:

scenarios with $Rev^{NC} > Rev^{RS}$ are "NC Better"; scenarios with $Rev^{NC} < Rev^{RS}$ are "RS Better"; and scenarios with $Rev^{NC} = Rev^{RS}$ are "Same".

The sufficient and necessary condition for those categories is challenging to quantify. The reason is that Rev_2^P, the revenue of the posted-price auction in Period 2, derives not only from the upfront fee (i.e., the optimal inter-period revenue), but also the associated reserve price. However, we know that to ensure the buyer's participation *ex ante.*, Rev_2^P is bounded by $\mathbb{E}_2$, the expected buyer's valuation in Period 2. As a result, $\mathbb{E}_2 < Rev^{RS}$ implies an RS Better scenario. This rule is helpful, particularly when the Myerson's auction generates the same revenue in both periods. In this case, when the expected value in Period 2 is less than twice the Myerson's revenue in Period 2 ($\mathbb{E}_2 < 2Rev_2^M$), RS generates more revenue than NC.

For a similar reason, an NC Better scenario requires the posted-price auction to generate sufficiently more revenue than Myerson's auction in Period 2. This happens when there are "target buyers" (high valuation but low probability) in Period 2, i.e., the demand in Period 2 is long-tailed. In this case, Myerson's auction sets a low reserve price and generates low revenue, while NC might extract more revenue by setting an upfront fee and greater reserve price in the second period.

We use the example below to further illustrate the intuition.

Example 1. *Consider scenarios with discrete distributions in both Periods. Let* $n_1 > 1$ *and* $n_2 > 1$ *be the number of possible valuations for the buyer in Period 1 and in Period 2. The valuation for the buyer in Period 1 takes* 2^i *with probability* 2^{-i} *for* $i = 1, ..., n_1 - 1$ *and value* 2^{n_1} *with probability* $2^{-(n_1-1)}$*. The valuation for the buyer in Period 2 takes* 2^i *with probability* 2^{-i} *for* $i = 1, ..., n_2 - 1$ *and value* 2^{n_2} *with probability* $2^{-(n_2-1)}$*. Note that* $\mathbb{E}_1$ *the expected valuation in Period 1 is* $n_1 + 1$*, and* $\mathbb{E}_2$ *the expected valuation in Period 2 is* $n_2 + 1$*. The distribution in each period can be specified by its expected value. We thus denote the scenario as* $S^E(\mathbb{E}_1, \mathbb{E}_2)$*.*

This example is a variation of Papadimitriou et al. (2022). In this example, revenue from Myerson's revenue in both periods is the same: consider setting any possible value in each period as the reserve price in Myerson's auction. That is, $Rev_1^M = Rev_2^M = 2$, so that Rev^{RS} the revenue in RS, is 4. Note that Rev^{RS} is fixed in both periods. This is very convenient, as it allows us to focus more on the revenue from posted-price auctions in NC. Given that $\mathbb{E}_2$, the expected valuation in Period 2, is the upper bound for Rev_2^P, it is straightforward that if $\mathbb{E}_2 < 4$, then $S^E(\mathbb{E}_1, \mathbb{E}_2)$ belongs to RS Better. An NC Better scenario thus requires $\mathbb{E}_2$, the expected valuation in Period 2, to exceed 4. This implies that the buyer's valuation in Period 2 should take more possible values (larger n_2) and have greater maximum value (larger 2^{n_2}). Hence, the thinner the market in Period 2, the more likely NC will generate more revenue than RS.

Rev_2^P can be quantified in our example. Consider a scenario $S^E(\mathbb{E}_1, 4)$ where $\mathbb{E}_2$, the expected buyer's valuation in Period 2, is 4. For any F_1, the distribution of the buyer's valuation in Period 1, the buyer's valuation takes value 2 with probability 50% and value greater than 2 with probability 50%. Recall that the upfront fee in the posted-price auction (P) is $s_2 = \min[v_1, \mathbb{E}_2]$, so the revenue of P from the upfront fee is 3. To extract the whole dealing surplus, the associated reserve price in P is 2 for the buyer with value of 2 in Period 1, and 0 for the buyer with value greater than 2. The reserve price contributes 1 of the revenue in P. As a result, Rev_2^P is 4, which is equal to the sum of Myerson's revenue in both periods; thus, NC generates the same revenue as RS. Further, if $\mathbb{E}_2$, the expected buyer's valuation in Period 2, is greater than 4, then either the expected upfront fee is greater than 3 or the expected associated reserve price is greater than 4. In both cases, we have $Rev^{NC} > Rev^{RS}$.

We summarize the theoretical comparison between NC and RS in the scenario $S^E(\mathbb{E}_1, \mathbb{E}_2)$ in Proposition 2.

Proposition 2. *For scenario $S^E(\mathbb{E}_1, \mathbb{E}_2)$ defined in Example 1, the revenue comparison between NC and RS depends only on $\mathbb{E}_2$:*

1. $Rev^{NC} > Rev^{RS}$ *if* $\mathbb{E}_2 > 4$ *;*

2. $Rev^{NC} = Rev^{RS}$ *if* $\mathbb{E}_2 = 4$ *;*

3. $Rev^{NC} < Rev^{RS}$ *otherwise.*

Those theoretical findings provide a foundation for our experimental design in Section 2.3.

2.3 Experimental Design

Our experiment is designed to answer two research questions: (i) what information do human sellers use in deciding on a dynamic mechanism?; and (ii) how does the information they use change over time? Thus, our experiment uses both within-subject design and between-subject design. Participants are randomly assigned to one of two treatments: treatment *Set 4*, and treatment *Set 2*. All participants attend all 12 rounds with distinct trading scenarios in each round. Section 2.3.1 illustrates the treatment differences in experimental tasks. Section 2.3.2 explains how we construct those 12 trading scenarios. Section 2.3.3 introduces the experimental procedure.

Participants in the experiment play the role of *seller* or *buyer*, with equal chance. The role is fixed for the entire experiment. In each round, sellers and buyers are randomly matched. There are two practice rounds, followed by 10 experimental rounds. Participants are paid for only one of the 10 experimental rounds.

Each round is a two-period trading game, in which the buyer can buy one item from the seller in each period. It is common knowledge that production cost is 0 for sellers. At the beginning of each round, both the buyer and the seller have access to pie charts showing the possible values of the item in both periods. This setting allows us to investigate whether scenario-specific demand affects a human seller's choice of dynamic mechanism. The seller must then choose between two mechanisms for this round: NC and RS. Afterward, the seller sets prices, and the buyer bids according to seller's chosen mechanism. At the end of each round, participants receive feedback on their earnings.

2.3.1 Experimental Task

In each round, if RS is chosen, the seller must set two prices. Treatments vary on the number of prices the seller must set in each round if NC is chosen. In treatment Set 4, a seller choosing NC must set four prices: one in Period 1, and three in Period 2. However, in treatment Set 2, a seller choosing NC must only set two prices (one in each period), with the other two prices in Period 2 set by the computer. Compared with treatment Set 4, Set 2 makes NC a relatively easier mechanism for the seller.

The experimental task in Period 1 is the same in both treatments. After the seller chooses between NC and RS, the buyer is informed of the structure of the chosen mechanism. Then the seller sets a private reserve price r_1^M, and the buyer learns the private value of the item and makes a bid. If RS is chosen, the buyer purchases the item if their bid is greater than or equal to the reserve price. The buyer then pays the seller's reserve price. If NC is chosen, the buyer has a 50% chance of purchasing the item by paying nothing; otherwise, the buyer purchases the item from the seller by paying the reserve price if their bid is greater than or equal to the reserve price.

Period 1

1. The seller chooses a dynamic mechanism, DM (=NC or RS), and the buyer is informed.

2. The seller sets a reserve price r_1^M for Period 1; buyer learns the value of the item v_1 and makes a bid b_1.

    - in RS: the buyer pays r_1^M if $b_1 \geq r_1^M$
    - in NC: the buyer has 50% chance to get free item;
      otherwise, the buyer pays r_1^M if $b_1 \geq r_1^M$.

The experimental task in Period 2 is the same in both treatments if RS is chosen in this round, with the same structure as the task in Period 1. The seller sets another private reserve price r_2^M. The buyer privately learns the new private value of the item in Period 2 and makes a bid. The buyer purchases the item if their bid is greater than or equal to the reserve price. The buyer then pays the reserve price to the seller.

The experimental tasks in Period 2 vary between treatments if NC is chosen by the seller for this round. In treatment Set 4, the seller must set three prices in Period 2: (i) the upfront fee s_2, which the buyer can pay to learn their private valuation on the item in Period 2 and make a bid; and (ii) two reserve prices: one for the case when the buyer receives the refund on the upfront fee (r_2^M, for Myerson's auction), and one for the case where the buyer has to pay the upfront fee (r_2^P, for the posted-price auction). The buyer has a 50% chance of receiving the refund on the upfront fee and a 50% chance of not receiving the refund. The same rules for the buyer receiving the item apply in either case: if the buyer's bid is greater than or equal to the secret price, they receive the item and pay the seller the secret price. The seller is informed of the suggested upfront fee, i.e., the theoretical optimal upfront fee that captures the whole trading surplus in the posted-price auction.

In treatment Set 2, s_2, the upfront fee, and r_2^P, the reserve price for the buyer (as shown inside dashed rectangles in Figure **??** below), are set by the computer optimally and automatically. The buyer cannot obtain a refund on the upfront fee. This setting makes NC an easier mechanism for the seller, allowing us to investigate whether the relative simplicity between mechanisms affects the seller's choice of mechanism. Meanwhile, we can test whether the buyer is more likely to accept and pay the upfront fee set by the computer than a fee set by a human seller. The timeline for Period 2 is summarized below. Figure **??** further illustrates the experimental task in Period 2.

Period 2

1. The seller sets reserve prices for Period 2.

    - In RS: the seller sets the reserve price r_2^M;
    - In NC in treatment Set 4: the seller sets r_2^M, s_2, and r_2^P;

- In NC in treatment Set 2: the seller sets r_2^M; the computer sets s_2 and r_2^P.

2. (NC only) The buyer chooses whether to pay the upfront fee s_2.

3. The buyer learns the value of the item v_2 and makes a bid b_2 in RS or in NC when entering the market.

4. Allocation and pricing implementation:

   - in RS: the buyer pays r_2^M if $b_2 \geq r_2^M$;

   - in NC: the buyer has a 50% chance of obtaining a refund on the upfront fee, and pays r_2^P if $b_2 \geq r_2^M$;

     otherwise, the buyer pays r_2^M if $b_2 \geq r_2^M$.

To reduce overbidding, we restrict the bid to any non-negative integer no greater than twice the buyer's valuation. We also restrict all reserve prices set by the seller such that they are bound by the highest value of the item in that period. For the upfront fee in treatment Set 4, the seller can set any non-negative integer no greater than twice the suggested optimal upfront fee. Participants begin with 60 points, as buyers may lose some points during the experiment. In the experiment, to improve participants' understanding, we use "entry fee" instead of "upfront fee," and "secret price" instead of reserve price.

2.3.2 Three Categories of Scenarios

We construct 12 distinct scenarios based on Example 1 in Section 2.2.3. For a symmetric design, each category (NC Better, RS Better, and Same) contains four scenarios. That is, theoretically, four NC Better scenarios generate more revenue in NC than in RS; four RS Better scenarios generate more revenue in RS than in NC; and four Same scenarios generate the same revenue in NC and in RS.

In our experiment, the distribution of the value of items in a period follows one of four distributions: F_a, F_b, F_c, and F_d. Each distribution is discrete, as shown in pairs of values and associated probabilities $(v, p(v))$ below:

$$\begin{cases} F_a &= \{(v,p(v))\} = \{(2,\frac{1}{2}),(4,\frac{1}{4}),(8,\frac{1}{8}),(16,\frac{1}{16}),(32,\frac{1}{16})\}, & \mathbb{E}(F_a)=6 \\ F_b &= \{(v,p(v))\} = \{(2,\frac{1}{2}),(4,\frac{1}{2})\}, & \mathbb{E}(F_b)=3 \\ F_c &= \{(v,p(v))\} = \{(2,\frac{1}{2}),(4,\frac{1}{4}),(8,\frac{1}{4})\}, & \mathbb{E}(F_c)=4 \\ F_d &= \{(v,p(v))\} = \{(2,\frac{1}{2}),(4,\frac{1}{4}),(8,\frac{1}{8}),(16,\frac{1}{8})\}, & \mathbb{E}(F_d)=5 \end{cases}$$

For example, the distribution F_a, takes value 2 with probability $\frac{1}{2}$, value 4 with probability $\frac{1}{4}$..., and value 32 with probability $\frac{1}{16}$. The expected values for distributions F_a, F_b, F_c, and F_d are 6, 3, 4, and 5, respectively.

We capture a scenario as (F_1, F_2), the pair of distributions of buyer's value in Period 1 and Period 2. Myerson's revenue is 2 for both periods if the buyer's valuation draws from one of those four distributions. Thus, Rev^{RS}, the revenue of RS, equals 4 for all scenarios. From Proposition 2, the revenue comparison between NC and RS depends only on $\mathbb{E}(F_2)$: given that $\mathbb{E}(F_a) = 6 > 4$, if F_a is the distribution of buyer's valuation in Period 2, the scenario (F_1, F_a) belongs to NC Better regardless of F_1; the scenario (F_1, F_b) belong to RS Better, as the revenue in the posted-price auction in Period 2 is at most $\mathbb{E}(F_b) = 3$; and the scenario (F_1, F_c) belongs to Same, as the posted-price auction generates the same revenue as the sum of Myerson's revenue in Period 1 and in Period 2. We thus construct 12 scenarios, summarized below.

Twelve Scenarios in Three Categories:

NC Better: (F_a, F_a), (F_b, F_a), (F_c, F_a), and (F_d, F_a).

RS Better: (F_a, F_b), (F_b, F_b), (F_c, F_b), and (F_d, F_b).

Same: (F_a, F_c), (F_b, F_c), (F_c, F_c), and (F_d, F_c).

In these four NC Better scenarios, we have $REV^{RS} = 4$ and $REV^{NC} = 4.5$. The revenue of NC is 12.5% greater than that of RS. In all four RS Better scenarios, we have $REV^{RS} = 4$ and $REV^{NC} = 3.5$. The revenue of NC is 12.5% less than that of RS. And in all four Same scenarios, we have $REV^{RS} = REV^{NC} = 4$. The revenue of NC is the same as that of RS.

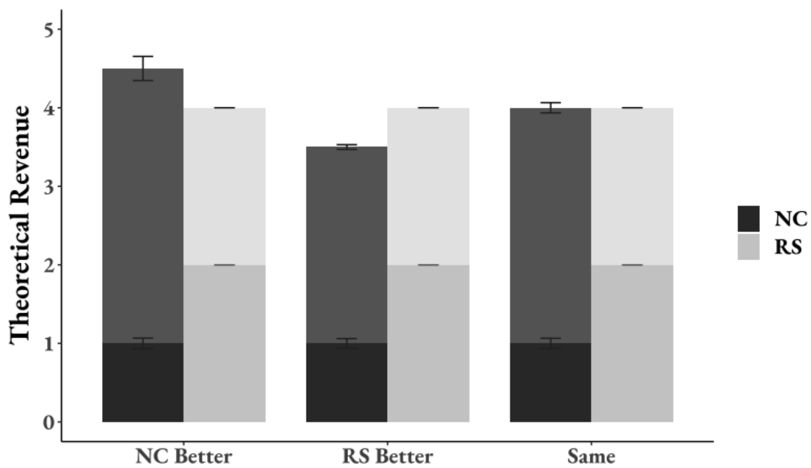


Figure 2.1: Theoretical Revenue (Total)

Figure 2.1 provides a summary of the theoretical total revenue: dark bars represent the revenue in Period 1, while light bars represent the revenue in Period 2. Theoretically, the revenue in Period 1 in NC is always half that of RS, as half of the buyers receive the item for free. Thus, the total revenue comparison depends on whether NC can make up the first-period revenue loss in the second period: NC will generate more (less) revenue than RS in NC (RS) Better scenarios, while in Same scenarios, the total revenue will be the same.

Given that buyers and sellers are re-matched for each round, we randomize the order of scenarios for each session rather than for each pair of buyers and sellers. Thus, for one session, participants (from 16 to 26) have the same order of scenarios. It is behaviorally interesting to examine how human sellers choose dynamic mechanisms when NC and RS generate the same revenue in Same scenarios; however, to minimize confusion for human sellers, we randomly select two out of four scenarios in Same scenarios (Practice) as the two practice rounds and assign the remaining two scenarios in Same scenarios as the final two experimental rounds (Tail). B.2.1 analyzes behaviors of participants in Same scenarios. For the remaining eight experimental rounds, we randomly select two NC Better scenarios and two RS Better scenarios for the early stages (first four experimental rounds), and the remaining four scenarios for the later stages (experimental rounds 5-8). This setting not only controls for sequential effect, but also maintains a symmetrical design between the early stages and the later stages.

2.3.3 Experiment Procedure

We recruited participants from George Mason University. We advertised our study on the recruiting system (`experiments.gmu.edu`), specifying that the experiment would last for 70 minutes. Subjects over 18-years-old were pre-selected. Subjects were informed that they could receive a participation bonus of $10 and additional payments depending on their decisions in the experiment.

The experiment was programmed in oTree (Chen et al., 2016). We conducted the experiment in October and November 2022. We had 256 participants (64 sellers and 64 buyers per treatment).[5]

Participants took a quiz after receiving instructions (see B.3). They then advanced to two practice rounds and 10 experimental rounds, followed by a risk-aversion elicitation (Holt and Laury, 2002) and an ambiguity-aversion elicitation in a random order. After completing a demographic questionnaire, participants learned their payments and were paid in cash privately.

[5]We ran G$*$ power analysis: for $\alpha = 0.5$, sample size of 44 has the power of 0.8 if we run Wilcoxon Signed-Rank Test on whether sellers can set optimal mechanisms more in the later stages than in the early stages. We assume that sellers start with randomizing between mechanisms but can choose the optimal mechanism more easily over rounds (the correct rate increases by 2% per round in the early stages and 3% per round in the later stages). Simulation results show that the average likelihood of choosing the correct mechanism for the early stages is 53% (std: 0.12). The likelihood increases by 12% with (std: 0.33). The effective size of 0.37 is moderate.

Participants took a quiz after receiving instructions. Then they proceeded to 2 practice rounds and 10 experimental rounds, followed by a risk-aversion elicitation (Holt and Laury, 2002) and a ambiguity-aversion elicitation in a random order. After completing a demographic questionnaire, participants learned their payments and were paid in cash privately.

2.4 Hypotheses

The first hypothesis concerns treatment differences on choosing a mechanism. Recall that in treatment Set 4, sellers need to set four prices in a round if NC is chosen, while in treatment Set 2, NC sellers need to set two prices per round. Setting proper prices is a challenging task, requiring intuition, calculation, and strategic adjustment based on feedback. Cognitive burden Sweller (1988) may make sellers less likely to choose NC due to the fact that they would need to set four prices for a round (rather than two prices in RS). As a result, it is reasonable to expect more sellers to choose NC if some of the prices in NC are set optimally and automatically by computer. We state our first Hypothesis as below:

Hypothesis 4 (Relative Simplicity)**.** *Sellers choose NC more in treatment Set 2 than in treatment Set 4.*

Our second hypothesis concerns whether sellers choose a dynamic mechanism based on scenario-specific demand information. In the experiment, sellers are informed of the specific scenarios by learning the distribution of the buyer's valuation in Period 1 and Period 2 at the beginning of each round. Sellers are not informed which mechanism is optimal in each scenario. However, our 12 designated simple scenarios provide intuition that helps sellers choose between NC and RS (whether NC is worthy of choosing depends only on the expected value of buyer's valuation in Period 2). In the early stages of the experiment, sellers might choose between NC and RS randomly to explore the trading environment. In the later stages, sellers might gain some experience with the environment. We thus expect that sellers will choose NC (RS) more than RS (NC) in NC (RS) Better scenarios, particularly in the later stages (experimental rounds 5-8). We state our second Hypothesis as below:

Hypothesis 5 (Scenario-specific Demand)**.** *In each treatment, sellers choose NC more (less) than 50% in NC (RS) Better scenarios in the later stages.*

The byproduct of Hypothesis 2 is that sellers will choose the correct mechanism more in the later stages. This should translate to revenue improvement from choosing the correct mechanism. We investigate this in the Results section.

The third hypothesis concerns how sellers choose a mechanism according to feedback. We expect sellers to adjust their choice of mechanism according to the feedback on realized revenue. Given that the variance in revenue from NC is greater than that from RS, sellers might react more to the revenue from NC in the preceding round. We expect sellers to choose NC more (less) when they received greater-(less-) than average revenue in the preceding round. We have our Hypothesis 3:

Hypothesis 6 (Feedback on Revenue)**.** *In each treatment, sellers choose NC more (less) when past revenue from NC is high (low).*

In the final hypothesis, we turn to buyers' behaviors. Based on Gui and Houser (2023)'s finding that some buyers refuse to pay the upfront fee in NC, we further hypothesize a treatment effect on buyers' participating decisions. Specifically, we expect buyers to be more likely to pay the upfront fee in NC when the upfront entry fee is determined by the computer in treatment Set 4, as compared to when the upfront fee is set by the human seller in treatment Set 2. Our expectation is an extension of previous literature finding that subjects are more likely to accept unfair offers proposed by computers versus humans (Blount, 1995).

Hypothesis 7 (Bidders' Behaviors)**.** *Buyers are more likely to pay the upfront fee in NC in treatment Set 2 than in treatment Set 4.*

2.5 Results

Demographic summary statistics are reported in Table 3.1. We have balanced gender for each treatment. Among 256 subjects, 56% are male, with an average age of 22.[6] The average payoff is $17.66.

In the Results section, we focus on behaviors in the eight experimental rounds on NC Better scenarios and RS Better scenarios. We divide those experimental rounds into the early stages (experimental rounds 1-4) and the later stages (experimental rounds 5-8). We provide analyses on behaviorally interesting Same scenarios in B.2.1.

Treatment	**Set 2**		**Set 4**	
Role	**Sellers**	**Buyers**	**Sellers**	**Buyers**
Age	22.6	22.2	21.2	22.5
Gender (Male=1)	0.59	0.62	0.52	0.50
Risk aversion	3.14	3.95	3.90	3.70
Ambiguity	3.30	3.02	3.67	3.32
Observation	64	64	64	64

Table 2.1: Summary Statistics

2.5.1 Relative Simplicity of NC in Choosing Mechanism

We first test Hypothesis 1 on relative simplicity by comparing the percentage of sellers choosing NC between the two treatments. We predict that sellers would choose NC more in treatment Set 2 as compared with treatment Set 4, as treatment Set 2 reduces the cognitive burden by automating half of the prices in NC. However, our experimental results appear to indicate that the relative simplicity of NC does not influence sellers' choice of mechanism.

[6]We do not observe significant differences on gender, age, risk attitude, or ambiguity attitude among treatments.

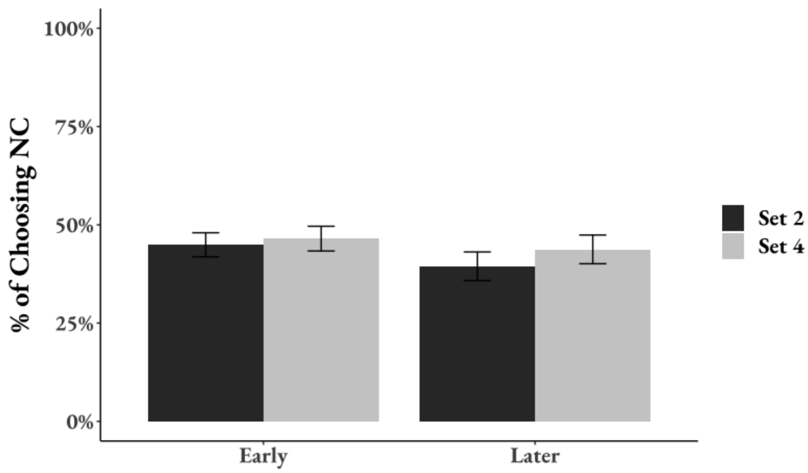


Figure 2.2: % of Choosing NC

The percentage of sellers choosing NC in each treatment for each stage is shown in Figure 2.2. There is no treatment difference in choosing NC either in the early stages (Set 2 vs. Set 4: 44.92% vs. 46.48%, $p = 0.64$, one-sided t-test) or the later stages (Set 2 vs. Set 4: 39.45% vs. 43.75%, $p = 0.80$, one-sided t-test). In the early stages, the percentage of choosing NC in both treatments does not significantly differ from 50% ($p = 0.10$ in Set 2, $p = 0.27$ in Set 4, two-sided t-test). This indicates that the behaviors of sellers in the early stages mimic randomizing. However, when sellers become familiar with the trading environment, their behavior differs significantly from randomly choosing ($p < 0.01$ in Set 2, $p = 0.09$ in Set 4, two-sided t-test). The Results section further discusses how sellers gain experience in the later stages.

We now have our first result:

Result 4. *Hypothesis 1 is not supported. The simplicity of NC in treatment Set 2 does not make sellers choose NC more, as compared with treatment Set 4.*

2.5.2 Discover the Optimal Mechanism

Recall that theoretically, NC will generate more (less) revenue than RS in NC (RS) Better scenarios in our experiment. We further expect that after gaining experience in the later stages, sellers in both treatments will choose NC (RS) more than RS (NC) in NC (RS) Better scenarios. That is, we expect that in the later stages, the percentage of choosing NC will be greater (lower) than 50% in NC (RS) Better scenarios in each treatment. Our results support this hypothesis: we find that sellers gain experience in the later stages, particularly in RS Better scenarios.

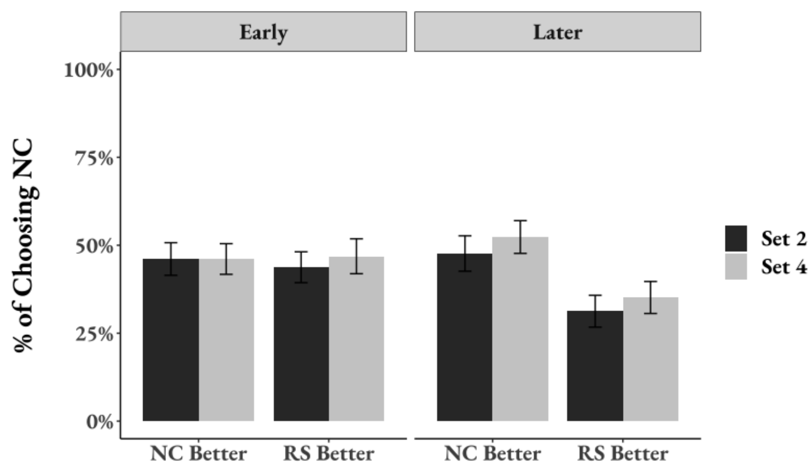


Figure 2.3: % of Choosing NC by Group of Scenario

Figure 2.3 shows the percentage of choosing NC in each treatment, in each stage, and in different scenarios. We first investigate sellers' decisions in RS Better scenarios. In the early stages, the percentage of choosing NC does not differ significantly from 50% (43.75% in Set 2, $p = 0.16$, and 46.88% in Set 4, p = 0.53, two-sided t-test). However, in the later stages, the percentage is significantly less than 50% (31.52% in Set 2, $p < 0.01$, and 35.16%

in Set 4, $p < 0.01$, two-sided t-test). This implies that sellers make better decisions in the later stages in RS Better scenarios.

However, sellers gain experience only in RS Better scenarios. In NC Better scenarios, sellers seem to randomize, even in the later stages. The percentage of choosing NC is not significantly different from 50% in the later stages (46.66% in Set 2, $p = 0.64$, and 52.34% in Set 4, $p = 0.62$, two-sided t-test) or the early stages (46.09% in Set 2, $p = 0.40$, and 46.09% in Set 4, $p = 0.37$, two-sided t-test).

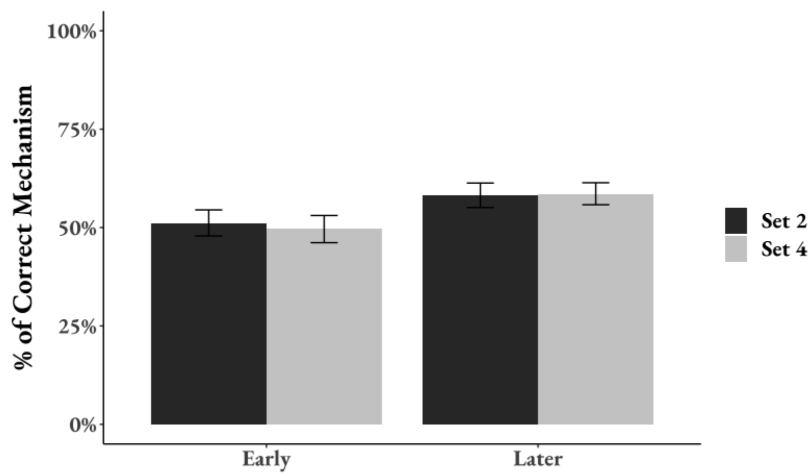


Figure 2.4: % of Choosing correct Mechanism

Further, we find that sellers choose the correct mechanism more in the later stages. The percentage of choosing the correct mechanism in each treatment and stage is shown in Figure 2.4. In the early stages, the percentage of choosing the correct mechanism in both treatments does not significantly differ from 50% (51.17% in Set 2, $p = 0.72$, and 49.61% in Set 4, $p = 0.91$, two-sided t-test), indicating that seller behavior in the early stages mimics randomizing. However, when sellers become familiar with the trading environment, they choose the correct mechanism significantly more than 50% of the time in the later stages

(58.20% in Set 2, $p = 0.01$, and 58.59% in Set 4, $p < 0.01$, two-sided t-test). Meanwhile, there is no treatment difference in choosing the correct mechanism either in the early stages ($p = 0.38$, one-sided t-test) or the later stages ($p = 0.54$, one-sided t-test), supporting the Result 1.

We now have our second result:

Result 5. *Hypothesis 2 is supported. Compared with the early stages, sellers choose NC less in RS Better scenarios in the later stages, and sellers choose the optimal mechanism more in the later stages.*

2.5.3 Reaction to Negative Feedback

The other byproduct of Hypothesis 2 is that sellers should earn more revenue when choosing the correct mechanism. Experimental results support this prediction. We find that sellers earn more revenue in the later stages in treatment Set 4.

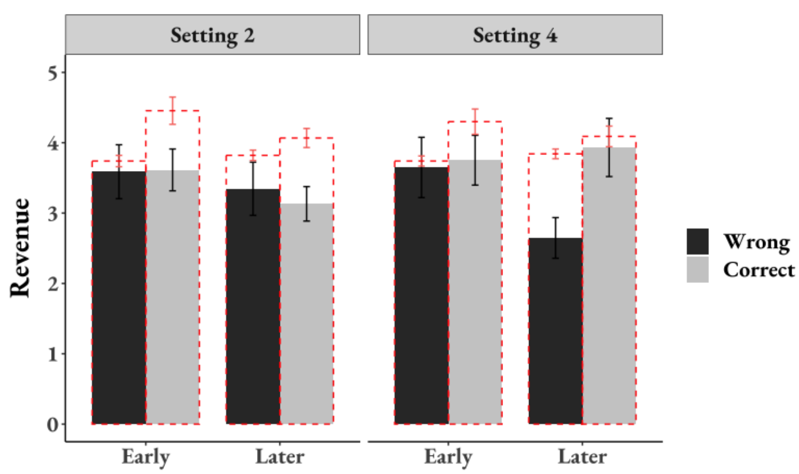


Figure 2.5: Experimental Revenue

Figure 2.5 reports experimental revenue in each treatment for each stage, noting whether the correct mechanism was chosen. We find that choosing the correct mechanism benefits sellers in treatment Set 4[7]: in the early stages, while the revenue from the correct mechanism (3.75) does not different significantly from that of the wrong mechanism (3.65, $p = 0.85$, one-sided t-test), in the later stages, the revenue from the correct mechanism (3.93) is significantly greater (2.65, $p = 0.01$, one-sided t-test).

Figure 2.5 also shows a gap between the theoretical revenue (in red dashed line) and the achieved experimental revenue. This gap mainly results from the revenue loss of NC due to buyers not entering Period 2.[8] This means that NC generates less than theoretically predicted, particularly in NC Better scenarios where NC should be the optimal mechanism. Meanwhile, NC has randomization in each period, and the variance of the revenue from NC is greater than that from RS, making NC generate lower revenue than RS, as a rule.

At the end of each round, the results of the trading (i.e., whether the buyer received the item, the price, and the revenue) are provided to sellers. In the next round, sellers might adjust their decisions based on the feedback, particularly revenue. We find that sellers adjust their mechanism decision based on negative feedback. Specifically, sellers are less likely to choose NC in the next round as a reaction to lower-than-average revenue of NC in the current round.

[7]In treatment Set 2, choosing the correct mechanism generates similar revenue as choosing the wrong mechanism in both stages (in the early stages, correct vs, wrong: 3.61 vs 3.59, $p = 0.96$; in the later stages, correct vs, wrong: 3.35 vs 3.13, $p = 0.64$, one-sided t-test).

[8]On average, the percentage of buyers choosing not to enter Period 2 in NC is 23.94%. If those buyers paid the upfront fee, entered in the second period, and bid their true value, the revenue of sellers would increase by 79.94% of the gap between the theoretical prediction and the experimental observation.

	DV: Choosing NC	
	(1)	(2)
β_1: Last (Revenue of NC < Average)	-0.21^{***}	-0.21^{***}
	(0.07)	(0.07)
β_2: Later * Set 4	0.01	-0.00
	(0.04)	(0.04)
β_3: Later * RS Better	-0.18^{***}	-0.18^{***}
	(0.06)	(0.06)
β_4: Later * Last (NC)	0.16^{*}	0.17^{*}
	(0.09)	(0.09)
β_5: Later * Last (NC is Correct)	0.02	0.01
	(0.10)	(0.10)
Constant	0.46^{***}	0.31^{**}
	(0.04)	(0.13)
Controls	No	Yes
R^2	0.05	0.05
Adj. R^2	0.03	0.03
Num. obs.	1024	1024

Note: The unit of observation is round i answered by participant j. OLS regressions are reported, and we find similar results using Probit Models. The regression consists of four rounds in the early stages and four rounds in the later stages. Cluster-robust standard errors at individual level were used in the regressions. Controls include index of risk attitude, index of ambiguity attitude, age, gender, and whether the participant is a graduate student.
$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Table 2.2: Regression of Choosing NC

Table 2.2 reports regressions to investigate the determinants of choosing NC in the current round. We consider whether revenue of NC from the preceding round was less than average (β_1). The remaining four independent variables are: interaction between whether in the later stages and whether in RS Better scenarios (β_2); interaction between whether in the later stages and whether choosing NC in the preceding round (β_3); interaction between whether in the later stages and whether NC is the optimal mechanism in the preceding round (β_4); and interaction between whether in the later stages and whether in treatment Set 4 (β_5).

The significant negative β_1 implies that when revenue in the preceding round was low (less than average) and from NC, sellers are less likely to choose NC again in the next round. This supports Hypothesis 3, i.e., sellers react to negative feedback. We find no treatment difference in choosing NC in the later stages (the insignificant β_2). This confirms our Result 1. We further confirm our Result 2, i.e., that sellers are able to determine that NC is not the optimal mechanism in RS Better scenarios in the later stages (the significant negative β_3). Meanwhile, sellers' mechanism choice tends to be persistent in the later stages[9] (the significant negative β_4), while the optimal mechanism (theoretically) in the previous round does not affect decisions about mechanism in the current round (the insignificant β_5). Those findings further support the idea that sellers adjust mechanism decisions based on both current condition and past revenue.

We now have our third result:

Result 6. *Hypothesis 3 is supported. Sellers are less likely to choose NC if past revenue from NC is low (less than average).*

2.5.4 Entry Fee and Participation

The revenue achieved in our experiment depends on both mechanism choice and price setting. In this section, we focus on the entry fee in Period 2. The reason is that this

[9] In the later stages, six (13) sellers chose NC (RS) in all four rounds in treatment Set 2, and four (10) sellers chose NC (RS) in all four rounds in treatment Set 4. In the early stages, three (five) sellers chose NC (RS) in all four rounds in treatment Set 2, and two (five) sellers chose NC (RS) in all four rounds in treatment Set 4.

fee directly affects the participation behavior of buyers. We leave the discussion of reserve prices and bids for B.2.2.

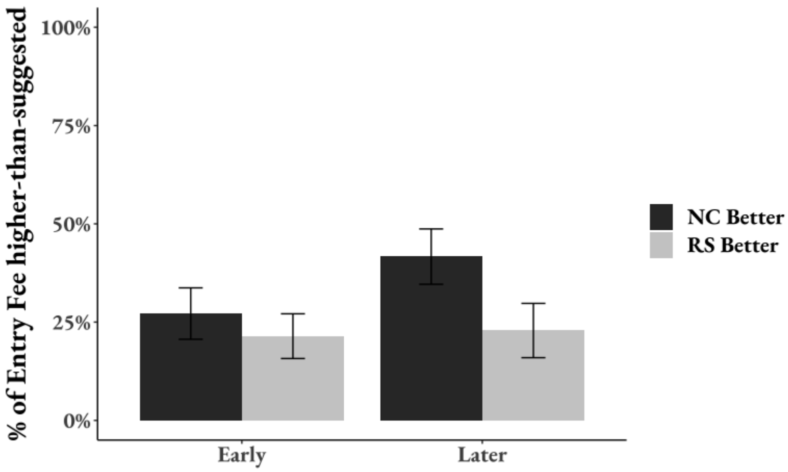


Figure 2.6: % of Setting Entry Fee Higher than Suggested

Recall that in treatment Set 2, the entry fee in NC is set by the computer automatically and optimally. In treatment Set 4, sellers are informed of the optimal entry fee (based on the bid in Period 1 and the valuation distribution in Period 2) and are free to set any entry fee lower than the suggested fee. The percentage of higher-than suggested entry fee by scenarios and by stage in treatment Set 4 is shown in Figure 2.7. In the early stages, 27.17% (21.43%) of the entry fees set by sellers in NC (RS) Better were higher than the suggested optimal fee, and the fraction is significantly more than 0% ($p < 0.01$ in NC better; $p < 0.01$ in RS better; one-sided t-test). In the later stages, 41.67% (22.86%) of the entry fees were higher than suggested by sellers, and the fraction is significantly more than 0% ($p < 0.01$ in NC better; $p < 0.01$ in RS better; one-sided t-test). Between scenarios, in the early stages, there is no significant difference in the percentage of setting the entry fee higher than suggested ($p = 0.51$, two-sided t-test). However, in the later stages, the entry fee is significantly more frequently set higher than suggested in NC Better than in RS better ($p = 0.06$, two-sided t-test).

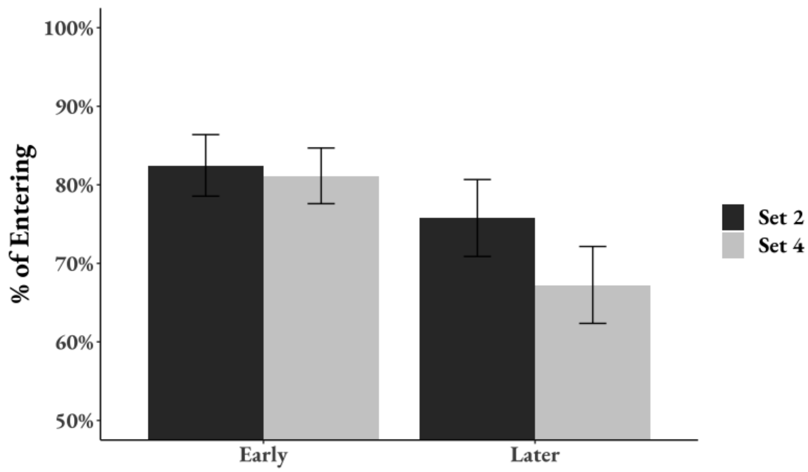


Figure 2.7: % of Entering Period 2

Accordingly, buyers participated less in the second period in NC. The percentage of entering Period 2 in NC is shown in Figure 2.7. In the early stages, the percentage of entering Period 2 in both treatments is significantly less than the theory prediction of 100% (82.47% in treatment Set 2, $p < 0.01$, 81.14% in treatment Set 4, $p < 0.01$, one-sided t-test). In treatment Set 2, the participation rate in the later stages (75.78%) is not significantly different from that in the early stages ($p = 0.14$, two-sided t-test). However, in treatment Set 4, the participation rate in the later stages in treatment Set 4 is 67.25%. The decline in participation is significant compared with the early stages ($p = 0.01$, two-sided t-test).[10]

[10]The direct treatment difference in participation rate is not significant in either the early stages ($p = 0.81$, one-sided t-test) or later stages ($p = 0.11$, one-sided t-test).

	DV: Enter in Period 2	
	(1)	(2)
β_1 : Set 4	-0.03	-0.06
	(0.05)	(0.06)
β_2 : Later * Set 4	-0.03	-0.05
	(0.08)	(0.08)
β_3 : Entry Fee	-0.07^{***}	-0.06^{***}
	(0.01)	(0.01)
β_4 : RS Better	-0.15^{***}	-0.13^{***}
	(0.06)	(0.04)
β_5 : Later	-0.10^{*}	-0.10^{*}
	(0.06)	(0.06)
β_6 : Risk Seeking		0.02^{*}
		(0.01)
Constant	1.10^{***}	1.15^{***}
	(0.07)	(0.17)
Controls	No	Yes
Num. obs.	447	447

Note: The unit of observation is round i answered by participant j. OLS regressions are reported. We find similar results using Probit models. The regression consists of four rounds in the early stages and four rounds in the later stages. Cluster-robust standard errors at individual levels were used in the regressions. Controls include a dummy for value in Period 1, whether the buyer received the item in Period 1, index of risk attitude, index of ambiguity attitude, age, gender, and whether the participant is a graduate student.

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Table 2.3: Regression of Enter in Period 2

To disentangle whether the treatment difference of participating in Period 2 in NC is due to pure treatment effect (i.e., buyers feel the price set by sellers is more unfair than the price set by computer), or due to price effect, or both, we regress whether the buyer is entering Period 2 on treatment; stage; interaction between treatment and stage; scenario; entry fee amount; and risk attitude. Results are reported in Table 2.3. The significant β_3 supports the price effect, implying that the higher the entry fee, the less likely buyers are to choose to participate in Period 2. After controlling for the entry fee amount, the insignificant β_1 and β_2 reject the pure treatment effect. In other words, whether the entry fee is set by computer or a human does not affect buyers' participation behaviors. The significant negative β_4 shows that buyers participate less in RS Better scenarios. The reason may be that buyers think RS Better scenarios with low expected value in Period 2 are not worthy of their participation. This is consistent with the finding of Gui and Houser (2023). The significant negative β_5 shows that in the experiment, buyers tend to participate less in the later stages as the experiment goes on. It is worth noting that β_6 shows that risk attitude might play a role in participation decisions: the more risk averse the buyer, the less likely they will participate in Period 2.

We now present our fourth result:

Result 7. *Hypothesis 4 is supported. Buyers participate significantly less in the later stages only in treatment Set 4. This is explained by the higher entry fee set by sellers in treatment Set 4.*

2.6 Conclusion

To our knowledge, our paper is the first to investigate how human sellers choose dynamic mechanisms and adjust their choices over time. Our study could provide guidance for, e.g., designing airplane tickets, implementing rules for repeated selling, designing online advertising markets, and constructing long-term contracts.

We use laboratory experiments to investigate how human sellers choose between the optimal non-clairvoyant dynamic mechanism (NC) (Mirrokni et al., 2020) and the optimal repeated static mechanism (RS) (Myerson, 1981) for scenarios with different revenue comparisons. Experimental results indicate that sellers choose a dynamic mechanism based on distributional knowledge. While sellers tend to randomize between NC and RS in the early stages of the experiment, they learn from their experiment gained in the trading environment and implement that knowledge in the later stages. They are less likely to choose NC for scenarios where the revenue gain from an upfront fee in Period 2 would not sufficiently compensate for the loss suffered in Period 1 due to providing some of the items for free. Meanwhile, sellers react to negative feedback by choosing NC less when the revenue from NC in the preceding round was extremely low.

Combining Myerson's auction with two other mechanisms, NC is a more complex mechanism than RS. In the experiment, we have one treatment (Set 4) where sellers must set all four prices in a round if they choose NC. We then have another treatment (Set 2) where sellers set the same two reserve prices in NC as in RS, with the automated optimal upfront fee and associated reserve prices. We find no treatment differences in mechanism choice. This implies that relative simplicity does not affect sellers' choice of mechanism. Nevertheless, buyers participate less in treatment Set 4, likely due to the higher upfront fee set by sellers.

We note that NC is more sensitive to prices, particularly the upfront fee, which directly affects future participation behaviors of buyers. Similarly, in the natural environment, a high subscription fee might lead buyers to cancel a subscription immediately after a free trial expires (e.g., one-month Amazon Prime, three-month Spotify, etc.). Future dynamic mechanism design with upfront fees should consider the possibly of less-than-full participation of buyers due to upfront fees.

A limitation of our study is that it considers only a two-period single-buyer environment. Both non-clairvoyant dynamic mechanisms and repeat static mechanisms can, of course, be applied to multi-period multi-buyer environments. Indeed, studying these environments would be interesting, particularly to those designing auctions for natural environments. Future research will benefit from focusing on how sellers choose mechanisms in a non-clairvoyant environment.

Chapter 3: Using (merit-based) default to reduce gender gaps in contribution of ideas: Evidence from an online experiment

3.1 Introduction

Women are found to be less active than men to share their ideas or advice members in group settings, especially in male-type tasks (Babcock and Laschever 2007; Cooper and Kagel 2012; Coffman 2014; Chen and Houser 2019). The under-contribution can be detrimental as it not only lowers the group performance but also hinder women career development and result in the gender gap in labor market. Positive feedback on expertise has only limited effect on encouraging high-ability women to contribute. It remains to be challenging how to promote more women to contribute ideas. In this paper, we investigate the power of default that has recently been shown successfully nudge more women to compete and participate in leadership selections (He et al., 2021; Erkal et al., 2022).

To date many institutions and programs have been carried out to promote gender diversity. Trainings are designed to help women improve skills and build confidence. Women are given lots of advice how to act as men (e.g. lean in) to catch up. The success of these programs, however, seems to be limited and sometimes even backfire (Bohnet, 2016). Organizations are also under the pressure of introducing more intrusive interventions such as affirmative action or quotas to force gender equity. Such mechanisms however are facing criticisms and concerns of the counterproductive consequences in practice (Matsa and Miller, 2013; Gangadharan et al., 2016; Afridi et al., 2017). In contrast to the effort of debiasing individuals or reforming institutions, changing the default aims at debiasing institutions via nudging. The idea is that the default underlying our current institutions often implicitly discourages women to participate. For example, the process of selecting leaders regularly requires potential candidates to actively apply for the position, which means the default is that one is not in the candidate pool. Erkal et al. (2022) show that such an opt-in type of selection mechanism contributes to the underrepresentation of women in leadership. They design an opt-out mechanism where candidates are automatically in the selection pool unless they opt-out and find that such a simple twist significantly reduces the gender gap.

We extend this literature by testing whether an opt-out mechanism can promote more women to contribute ideas. In addition to applying the idea to another important context where gender gap persists, our novel contribution is to examine the role of merit when setting up the default. Meritocracy can be particularly important in the context of contributing ideas. One important consideration in the effort of promoting the willingness to contribute ideas is that the interventions may lower the quality of the idea if they promote the wrong person (e.g., low performers) to contribute. This concern may call for a merit-based default where only high-qualified individuals are defaulted to contribute. However, merit-based default can be challenging in practices. For example, it requires the organization to identify what is "merit" and who has the "merit" at the first place which can be costly. In this paper, instead of making a normative argument whether default mechanism should be merit-based, we aim at providing descriptive evidence for the impact of merit by comparing the merit-based default with a random default mechanism where individuals are randomly defaulted to contribute the idea. There are two potential benefits of merit-based default. Mechanically, by promoting only the high-ability member to contribute, groups under the merit-default should achieve a better performance than those under random default. Another potential behavioral effect of the merit default is that people might be more likely to stay with the default when the defaulted position is legitimate, such as merit based. If this is the case, it will add another argument for the merit-based default.

To examine the behavioral consequences of the default on the willingness to contribute ideas, it is important to note that gender stereotype plays an essential role in the contribution decision. Coffman (2014) argues that the contribution decision is the outcome of the interaction of gender and the gender stereotyping associated with the decision-making domain. She finds that conditional on knowing the right answer both men and women are less willing to contribute ideas in the gender incongruent areas, with women being even more subject to such gender stereotyping. To explore the underlying mechanisms, she further shows that women are much less confidence in the male-type tasks. The differences in the beliefs, however, only partially explain women's reluctance to contribute in stereotypically male domains. She suggests that additional factors such as norms might at work. These findings provide important insights for studying the power of default in promoting contribution of ideas.

Previous research on default suggests that default can influence behavior by changing the perception of the norms (McKenzie et al. 2006; Davidai et al., 2012; Everett et al., 2015. We may consider there are two potential norms in the decisions to contribute ideas that leads to the gender gap. One is the general gender norm of whether one should contribute ideas which is not associated with gender stereotypical tasks. For example, in a context without any gender stereotype associated with the task per se, women might be less willing to contribute than men if there is a gender norm that women should not play an active role in contributing ideas in group. The other is a gender-stereotyping related norm of whether one should contribute to gender incongruent domains. For example, there might be a norm that women should not contribute ideas in male-type domains and men should not contribute ideas in female-type domains. When individuals are defaulted to contribute, the default may change both types of norms and sending an implicit message that contributing ideas is encouraged. The difference, however, is that changing the general gender norm promotes more women to contribute without necessarily reducing the differences in the contribution between male-type and female type tasks. In contrast, changing gender-stereotyping related norm should mitigate the gap between women's willingness to contribute in male-type and that in female-type domains, i.e. reduce the stereotype bias in the decision to contribute ideas. We will explore these two mechanisms.

Our experiment is built on Coffman (2014). The control treatment consists of three parts. In part A, participants answered 30 multiple-choice questions from six categories. The categories differ in their perceived gender congruence. In part B, two participants paired up and were given new 30 multiple choice questions but in the same six categories as part A. Each participant makes two decisions: their answer to the question; and the extent to which they want to put their answers as the group answers. The payment in this part was determined by how many group answers were correct. In part C, we measured participants' confidence in their own answers in part B, as well as in the answers of their group member.

The other three treatments differ from the control only in the way participants were asked to indicate their willingness to answer the question for the group in part B. In the MeritDefault (MD) treatment, before the start of part B questions, subjects received part A performance feedback information as compared to their paired group member. They were also informed that for the categories that they outperform their group member, they would be preselected to submit their answer for the group in part B. For the categories that they were worse than their group member, they would be preselected not to submit their answer. The preselected positions are not binding, and each participant can always freely choose whether they want to answer the question for the group.

To learn the role of merit in utilizing the default, we conduct a RandomDefault_NoFeedback(RD_NF) and a RandomDefault_Feedback(RD_F) treatment. In both treatments, one of the two group members were randomly assigned to submit their answer for the group and the other was assigned to no submit. In the RD_NF we do not provide any performance feedback at the start of part B. In the RD_F, feedback was provided at the beginning of part B as in the MD treatment.

We find gender gap in willingness to contribute in the control which replicates previous findings. However, regardless how the defaults are set, all three treatments with default option can mitigate the gender gap. In particular, being defaulted to the leading position to contribute promote more women to contribute in all areas, but still relatively less in male-than female-stereotyped environments. On the other hand, random default can backfire the gender stereotype effect when the defaults are mismatched with feedback. We do not find merit increases the propensity to stay with the defaulted leading position in that individuals are equal likely to stay with the defaulted position regardless of how the default is set up, compared with the random default. The outcome of better group performance in the MD treatment is mainly driven by the selection of higher performers to the default leading positions. This finding suggests that if the primary goal is to promote the willingness to contribute, a random default can be as effective as a merit-based default.

Interestingly, women also become more confident after being defaulted to the leading position in the MD treatment. Our data from RD_F suggest the positive effect on the confidence cannot be attributed to the information about their previous performance implied by the merit default. We discuss at the end of the paper one possible explanation based on cognitive-dissonance theory.

Our study contributes to three lines of literature. The first one is the recent studies on gender stereotypes on beliefs and contribution of ideas (Coffman, 2014: Chen and Houser, 2019; Bordale et al., 2019) that observes women (man) are underconfident about their performance in male-typed (female-typed) domains and consequently less willing to contribute their ideas to the group in the gender incongruent domains. These findings are also consistent with the prediction from Bordalo et al. (2019) on women's shyness away from men-type domains, and the findings from Benjamin et al. (2010) that argue norms related to social identity affect economic decisions. Little success has been documented in the attempt to mitigate the gender gap in the willingness to contribute ideas. We contribute to this literature by showing default can potentially alleviate the gender gap in the willingness to contribute the ideas although it does not change the gender stereotype effect.

By examining the default mechanism in the context of group decision making, we also extend the behavioral research on default to a new and important context. Previous studies have investigated the power of default in many domains, such as organ donation (Johnson & Goldstein, 2003), retirement saving program (Madrain & Shea, 2001), donation (Urminsky, 2016), reduce gender gap in voluntary leadership and willingness to compete (Erkal et al., 2022; He et al., 2021). Jachimowicz et al. (2018) conduct a meta-analysis summarizing 55 studies on default effects from 35 articles and show that on average the default option leads to a 27.24% more staying with the pre-selected. They highlight, however, that how effective defaults are and why defaults can take effects vary in environments. Dinner et al. (2011) proposes three psychological channels underlying the default effect. One is called the endorsement channel. That is, individuals are more likely to stay with defaults if they believe the default option is with good intentions (Tannenbaum et al., 2017). Another channel through endowment effect (Kahneman & Tversky, 1979). The third channel is through costly deviation from default options (Johnson et al., 2012) which, however, does not influence the effectiveness of the default. By investigating the relevance of merit in setting up the default, our study shed light on whether the legitimacy of the default option matters.

The comparison between merit-based and random-based mechanisms is also related to the profound discussion on the attitudes to the merit and luck. People are more likely to cheat when the payoffs are luck-based than performance-based (Kajackaite, 2018; Gravert, 2013). The willingness to redistribute is stronger when inequalities are not merit-based (Konow, 2002; Cappelen et al., 2007). When bargaining over legitimized wealth (effort-based), dictators act more selfish compared with unearned wealth (Cherry et al., 2002). Different reactions toward merit-based mechanism and luck-based mechanism even root from culture: Almas et al (2020) find that Americans are more meritocratic while egalitarianism is a more prominent fairness view in the Scandinavians. In contrast to this line of study, our paper proposes that individuals also react differently to default positions that are gained by luck than to those that are legitimated as their merit foundation. We also investigate the gender difference in reacting to different default mechanisms.

3.2 Experimental Design and Hypotheses

3.2.1 Experimental Task

Our experiment was built upon previous research on willingness to contribute ideas where subjects face tasks with varying gender stereotypes and the effect of default on gender gap (Bordale, et al., 2016; Chen & Houser, 2019; Coffman, 2014; Erkal et al. 2022). Subjects were randomly assigned to one of the four treatments: MeritDefault, RandomDefault (NoFeedback), RandomDefault(Feedback) and Control (i.e. no default and no feedback). All treatments followed a similar structure as detailed below.

The experiment was comprised of three incentivized parts (Part A, B, and C) and a post-experimental survey. All participants received general instructions informing them that one part of the experiment would be selected for payment and announced at the conclusion of the experiment. Participants faced multiple-choice questions (MCQ) from six categories: arts and literature (Art), cars (Car), disney movies (Disney), the Kardasians (Kard), and sports and games (Sport), Video Game (Video). Each question included five possible answers and was labeled with its corresponding category. Those six categories varied in their perceived gender stereotypeness as revealed in the answers to the post-experimental survey questions . With the exception of MeritDefault and RandomDefault (Feedback) , no feedback was during the experiment.

Part A: individual task

Participants answered 30 MCQ (5 from each category) on their own (see Appendix A, Fig A1 for an example question). The data from this part provided us with a baseline measurement of individual ability for each category. Subjects received 1 point for each correct answer.

Part B: elicitation of willingness to contribute answers to the group

As the subjects proceeded to part B, they were informed that they would be working with one randomly selected participant in the study as a group for this part. Each group is given new 30 MCQ. Each one's payment in this part depended on the submitted group answers. Each group member received 1 point for each correct answer and lost a quarter point for each incorrect answer.

To decide the group answers, the participants made two decisions for each of the new 30 MCQ: 1) their answer to the question (see Appendix A, Figure A2a); and 2) their position in line to submit their answer as the group answer. There were four positions in line (1, 2, 3 and 4). In each pair, the participant who selected the lowest number would have his/her answer submitted as the group's answer. If both members selected the same position in line, the computer randomly selected one member's answer as the group answer. Thus, the lower the position in line, the more willing the subject was to contribute their answers to the group. Our four treatments vary on whether any positions in line were pre-selected. In appendix, Figure A2b provide an example of the decisions screen in the control treatment.

Following Coffman (2014), in order to investigate gender stereotype effects in beliefs, before subjects started to make the two above decisions, they were asked to make incentivized guesses about their own rank within the newly formed group for each of the categories from Part A. They receive an additional quarter point for each correct guess. We used the guess as a measure of belief.

Part C: belief elicitation

We measured participants' confidence in their own answers, as well as in the answers of their group member. Participants were given the same questions and the respective answers from Part B again and were asked in an incentive-compatible way (a simplified Becker-DeGroot-Marschak method) to estimate the probability that their own answer was correct and the probability that their group member' answers were correct. Specifically, subjects made two decisions for each question: 1) indicated the probability of their own answer being correct with a number between 1 and 100 – a measure of confidence in one's own answer for question i; and 2) provided an estimate of the probability of the other group members' answer being correct—a measure of confidence in other groups members' answer for question i. A correct answer earned half a point, and incorrect answers earned nothing.

Post-experimental questionnaire

We asked our subjects for demographic and attitudinal information, including, for example, gender, age, where they attended high school, and which question categories they liked or disliked. As the last question in the questionnaire, we asked the subjects to evaluate the male- or female-typeness of each category. For each of the categories, the subjects were asked to indicate their answers by selecting a number from -5 to 5, where -5 was labelled as "women know more" 5 was labelled "men know more".

3.2.2 Treatments

Treatment varies on feedback and default form.

Control (no default + no feedback)

There were no pre-selected positions for part B, nor was there any part A performance feedback provided to the subjects in the control condition.

MeritDefault (MD)

Before the start of part B questions, subjects received part A performance feedback information as compared to their paired group member (see Appendix A, Fig. A3). They were also informed that position "1" was pre-selected for all the questions in the categories that they were at least as good as their group member and "4" was preselected for all questions in the categories that they were worse than their group member. However, the subjects were free to choose other positions in line (See Appendix, Fig. A4). Although our focus is the case when subjects are defaulted to position 1, we include in the design default to position 4. We think this feature can help to reduce experimenter demand effect which is probably more likely when we only allow default to position 1. Our design also all us to learn the effect of opt-out when one is defaulted to not to contribute.

RandomDefault_NoFeedback(RD_NF)

Contrary to the MD treatment, participants in were advised that position "1" was pre-selected for all the questions in some randomly selected categories and position "4" was pre-selected for all questions in the remaining categories. Moreover, the subjects did NOT receive any part A performance feedback. Thus, this treatment informs the pure default effect.

RandomDefault_Feedback(RD_F)

The only difference between this treatment and RD_NF is that subjects received part A performance feedback before they started on the questions in part B.

3.2.3 Procedures

Our study was pre-registered on OSF registries (`https://osf.io/a2ber/`). The experiment was programmed in oTree (Chen, et al., 2016) and conducted online using the research subjects platform Prolific (https://www.prolific.co) between July and August 2023. Only those who reside in the United States were eligible to participate, given the familiarity with the question categories (e.g., questions about the Kardashians).

To minimize cheating in the experiment, a 15 sec time limit was imposed on each of the MCQ questions in Part A and Part B. It is worth noting that no time limit was imposed on the position in line decisions in Part B, which allow use to rule out the possibility that the default treatment effect is due to lack of decision-making time. Moreover, attention check questions were included in both Part A and Part B to ensure the quality of the data collected.

Our participants' self-reported ages ranged from 18 to 70 (Mean = 34.56, Median = 28.00); 47% of female; 61% White, 9% Hispanic or Latino and 9% Black or African American; their highest attained degree ranged from Some high school to Doctorate degree (Median = Bachelor's degree); 54% reported working either full-time or part-time job. Each subject participated in exactly one treatment. A typical session lasted about 20 mins. The average payment was $9.30 .

Due to the nature of the experiment and the possibility of significant attrition in data collection, we chose NOT to live match subjects into groups. Instead, we sequentially conducted two waves of data collection per treatment condition. We matched randomly the subjects in wave two with those (or their decisions) in wave one. As explained next, this arrangement is particularly helpful in the MD and RD_F treatments, where performance comparison between group members was needed so as to provide feedback. The same instructions were used for subjects in both waves for the same treatment condition to ensure common information.

In Control, the subjects in both waves made the exact same decisions. In the MD, RD_NF and RD_F treatments, subjects in both waves were told that one person will be selected randomly and will have a pre-selected position when answering questions in part B. The explanations how the pre-selected position is determined vary among the three default treatments as described above. Subjects in wave 1 were informed that no position in line was pre-selected for them in Part B and that position in line was pre-selected for their matched partner according to different treatment conditions. After data collection in wave 1, we recruit participants in wave 2. In each treatment, each participant is randomly matched with a participant in wave 1. In the MD and RD_F treatments, the Part A performance of each participant in wave 2 will be compared with that of the matched participant in wave 1 and the feedback will be provided to the wave 2 participant at the beginning of Part B. In the MD treatment, the wave 2 participant's pre-selected position (1 or 4) will be determined according to their relatively performance in part A as compared to the matched wave 1 participant. The participants in wave 2 in RD_NF receive no feedback but are randomly defaulted to either postion 1 or 4. After wave 2 participants finish the experiments, both waves get their payments. Figure 1 illustrates the procedures of the experiment.

3.2.4 Hypotheses

We expect to replicate the previous findings on the gender differences and the stereotype bias in the decisions to contribute ideas in the control treatment. Thus, our first hypothesis is:

Hypothesis 8. *(Gender gap and stereotype bias) In the control treatment, women (men) are more likely to choose the leading position in female-type (male-type) questions. The stereotype bias is stronger among women than men. As a result, overall women are less willing to contribute than men.*

Our main interest is the effect of default. We first discuss the pure default effect by comparing the RD_NF and the control treatment where the only difference between the two treatments is the default. Then we analyze the merit effect by comparing the MD and the RD_NF and the RD_F treatment. As the goal is to promote the willingness to contribute, our discussion here focused on the condition where subjects are defaulted to position 1. Assuming the same default and merit effect applies to the case when subjects are defaulted to position 4, the analysis discussed below can be extended to default position 4 cases. The corresponding hypothesis regarding the default position 4 cases (together with the results) are provided in Appendix E.

Based on the previous studies of the default effect on gender inequality (He et al., 2021; Erkal et al., 2022), we expect participants tend to stay with the pre-selected position and the default effect is greater for women than men. When the pre-selected position is 1, we expect more people, especially more women, will choose 1 and the increase in the proportion of the choice of position 1 is greater among women than men. The previous literature, however, cannot inform whether the default can also mitigate the stereotype effect - the negative correlation between maleness of the questions and women's (men's) willingness to choose the leading position. To differentiate the two types of default effects, we call the first default effect an intercept effect and the second one a slope effect. While we expect defaulting the position to 1 can lower the intercept (i.e. the average chosen position number becomes smaller), the default may or may not impact the slope.

We use Figure 2 to illustrate the possible slope effects and their implications on the gender gaps. If the slope does not change under the default, we still expect a smaller gender gap due to the intercept effect. On the other hand, if the slope effect is significant, we expect that under the default position 1 condition, the correlation between the maleness and the choice becomes smaller, which further reduce the gender gap on the top of the intercept effect. Below we state the hypotheses regarding what we expect to observe if the default has an intercept or a slope effect.

Hypothesis 9. *a. Default-Intercept effect: For the questions in a given gender-type category, compared with the control treatments, participants in the RD_NF are more likely to select position 1 when this position is pre-selected for them. The increase of the proportion of position 1 is greater for women than men.*

b. Default-Slope effect: The negative effect of the maleness (femaleness) of the questions on women's (man's) willingness to choose leading position is smaller when subjects in the RD_NF treatment are defaulted to position 1 as compared to those in the control.

Next, we consider the effect of merit-based default. Mechanically, by assigning only the high-ability member to the default position 1, groups under the merit-default should achieve a better performance than those under random default. Our next hypotheses are regarding the potential behavioral effect of the merit default: people might be more likely to stay with the default when the defaulted position is merit-based than when it is randomly assigned. There are potential two reasons this might be the case. First, the default of position 1 under the MD treatment implies that the subject performs better than the other member. We call this information effect. Coffman (2014) find that feedback about one's performance alone does not have a significant impact on the willingness to contribute. Yet, it is an open question whether the positive feedback that one performs better than the other member leads them to be more likely to stay with the defaulted leading positions. If so, we should expect high performers to be more likely to contribute in both the MD and the RD_F treatments than in the RD_NF treatment.

Second, the default of position 1 is more legitimate when the participants receive the position because they have previously performed better than the partner in the corresponding category of the question. We call this legitimacy effect. Previous research has shown that people are more likely to comply with request when it is legitimate (CITE). If the legitimacy matters in the set up of the default, we expect high performers should be more willing to stay with the pre-selected position 1 in the MD than the RD_F treatment. The reason is that in both treatments, high performers know that they are better than the other member. The only difference between the two is that in the MD, it is common knowledge that one receives the position 1 because of their competence while in the RD_F, they receive the position by chance. Thus, we have the following hypotheses.

Hypothesis 10. *a. MeritDefault-Information effect: For high performers in Part A, when being defaulted to position 1 in Part B, they are more likely to stay with the pre-selected position in the MD and RD_F treatments than in the RD_NF treatment.*

b. MeritDefault-Legitimacy effect: For high performers in Part A, when being defaulted to position 1 in Part B, they are more likely to stay with the pre-selected position in the MD than in the RD_F treatment.

The hypotheses above are built on previous literature on willingness to contribute ideas and default effect. In our experiment, following the previous literature, we also elicit beliefs of performance in Part C as it plays an important role in the willingness to contribute although it only partially explains women's reluctance to contribute in male-typed domains. In principle, the default per se, without any feedback, should not affect one's beliefs of their performance. That is, subjects who are defaulted to position 1 in the RD_NF treatment should not be more confident than those in the control. Likewise, high performers who are defaulted to position 1 should not be more confident in the MD than in the RD_F treatment. Nevertheless, studies in psychology and economics suggest that people may shape their belief and attitudes based on their previous actions (Festinger and Carlsmith, 1959; DeJong, 1979; Benabou and Tirole, 2011; Ariely and Norton, 2007; Weber and Johnson, 2006). In our case, it is likely that after being nudged to choose position 1 under the default, participants form the belief that their performance is better than the partner to keep the beliefs coincide with the choices. Such a reasoning is probably even more likely to occur in the MeritDefault treatment when the connection between the decision to contribute and the qualification is made salient. As such a default effect on confidence is speculative, we do not write hypotheses here but simply to explore this possibility by comparing the belief data in different conditions.

3.3 Results

We ran eight online sessions in total (two sessions/waves for each treatment) and obtained observations from a total of 804 subjects. The summary statistics for the task performance are reported in Table 3.1. On average, people answer more questions correctly in Part B than Part A (two-sided t-test, $p < 0.01$); women have same correct answers as men (two-sided t-test, Part A: p=0.08, Part B: p=0.06).

Table 3.1: Part A and B performance summary Statistics

	No. of questions correct				
Treatment	Part A		Part B		**No. observations**
	Man	**Women**	**Man**	**Women**	
Control	11.74	12.28	15.23	15.95	200
MD	11.96	12.65	16.49	17.14	200
RD_F	11.92	13.04	16.30	17.65	200
RD_NF	12.21	11.62	16.11	15.69	204

Since all subjects in wave 1 do not have any pre-selected positions whether they are in the control or in one of the three default treatments. We also find subjects in wave one in the default treatments were statistically indistinguishable from those in Control. Consequently, we pooled the wave one data of all the four treatments when analyzing the data.

We first report the gender difference on the willingness to contribute ideas in Control (Hypotheses 1). We then report the differences between the RD_NF and the control treatments to test the pure default effect (Hypothesis 2). Next, we examine the merit effect (Hypothesis 3). As we are interested in reducing the reluctance to contribute ideas, for all the three default treatments, we focus on the cases where subjects are assigned to position 1. In Appendix E, we discuss the effect of defaulting to position 4. At the end, we analyze whether default influence beliefs on performance of oneself and the other group members, which has been argued to be critical in determining the willingness to contribute ideas.

3.3.1 Gender gap and Stereotype Effect in willingness to contribute ideas (Control)

Supporting Hypothesis 1, data from the control treatment shows that we replicate the previous findings that women are less willing to contribute and stereotyping bias contribute to the gender gap. First, the position in line numbers in Control suggest that women are 8% less willing to lead than men (2.29 vs. 2.49). Note that the lower the position in line, the greater the willingness to lead. This gender gap is statistically significant (p¡0.01, t-test).

Next, to examine the stereotyping bias, we calculate the standardized maleness scores of each category of questions based on the answers to the survey questions in the post experimental questionnaire as explained in section II.A . The maleness score of each category, by gender, are reported in Appendix B (Figure B1). Men and women, by and large, agree on the femaleness/maleness of the categories. Kardashians, Disney and Art are female stereotyped categories. Video games, Sports and Cars are considered as male stereotyped. Thus, gender stereotypes are present in the question categories in our study. Consistent with the previous findings, in general, women performed better than men in female stereotype categories and worse than men in male stereotyped categories. The task performance by gender in each category is reported in Appendix B (Figure B2). We find the gender stereotypes effect on women's but not men's willingness to contribute ideas in the Control treatment: the more male-typed the category, less willing are women to contribute ideas to the group. We will provide more statistical analysis when reporting the data from the default treatments.

We conclude our first result as below.

Result 8. *Hypothesis 1 is supported. Both gender gap and stereotype bias exist in control.*

3.3.2 Pure default effect on the gender gap in willingness to contribute ideas (RD_NF vs Control)

In contrast to the gender differences observed in the control, we find the gender gap in the willingness to lead disappears in RD_NF when subjects are defaulted to position 1 (the average chosen position in line is 2.05 for women and 1.98 for men, p=0.63, two-sided t-test). This provides the first evidence for the positive effect of the default. Next, we explore the intercept effect and the slope effect.

The intercept effect can also be shown by the "extensive margin", the increasing likelihood of choosing position 1 of RD_NF compared with Control in each category (For similar results of "intensive margin" : the increase in the average of the chosen position number compared with Control, see Appendix C). We find both man and women are more likely to stay with the default option by choosing position 1 more often when they are defaulted to position 1: on overall, men increase the likelihood of choosing position 1 from 35.86% to 55.54% (p ¡ 0.01, two-sided t-test), and women increase the likelihood of choosing position 1 from 28.82% to 52.99% (p ¡ 0.01, two-sided t-test). This results in the gender gap mitigating in most categories in RD_NF (except p =0.06 in category "Video Games"). There is no overall gender gap in choosing position 1 (p = 0.67, two-sided t-test). In contrast, in Control, there is significant gender gap in choosing leading position on overall (p ¡ 0.01, two-sided t-test).

To test the hypothesis on the slope effect, we check whether the stereotyping bias observed in the control treatment is mitigated in the RD_NF treatment. Recall that we only observe stereotyping bias among women. Thus, here we focus on this group. We find that in the RD_NF treatment, women remain to be less likely to choose position 1 in male-typed categories than in female-typed categories (44.02% vs. 62.24%, t-test, p ¡ 0.01), while decisions of men do not vary much in type of categories (54.93% vs. 58.55%, t-test, p = 0.61). This result suggests that the default does not help mitigate the stereotyping effect.

To visualize both the intercept effect and the slope effect, we plot in Figure 2 the likelihood of choosing leading positions by women and men in Part B by category and treatment. As revealed by the fitted lines positioning higher in RD_NF, both men and women are more willing to choose the leading position when defaulted to 1, which is consistent with the default intercept effect. However, the gender stereotypes effect for women exists in both Control and RD_NF: the more male-typed the category, women are less likely to choose position 1 (as revealed by the negative slope of the fitted line). Though the fitted line for men is upward sloped, the line is relatively flat. We observe default does not impact the slope: the negative effect of the maleness (femaleness) of the questions on women's (man's) willingness to choose leading position is not mitigated in RD_NF.

We further provide statistical tests for both the intercept and the slope effects by conducting regression analysis of the position choices. Table 3 reports results from regressions with choosing the leading position as the dependent variable, and maleness score, RD_NF dummy, and interactions of maleness score and RD_NF dummy as the main regressors. Control variables including demographic variables and task performances are introduced in regressions. Consistent with the results reported above, the significant negative β_0 reveals a gender gap in willingness to choosing a leading position in Control. However, the gender gap is mitigated in RD_NF, which indicates the pure default intercept effect (H_0: $\beta_0 + \beta_4 = 0$, p=0.66 in regression (1), p=0.48 in regression (2)). The mitigation of gender

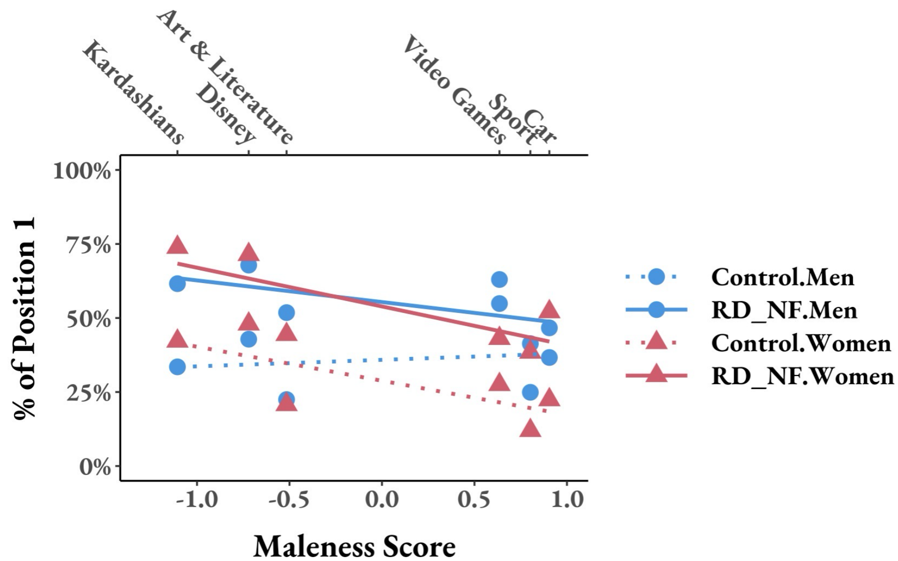


Figure 3.1: Intercept Effect and Stereotype Effect on Likelihood of Choosing Position 1

gap in RD_NF might come from the default-intercept effect is greater for women than for men, as revealed by the positive β_4, though this coefficient is not significant in regressions. The insignificant positive β_1 and significant negative β_2 in regression (2) imply that only women and not men are subject to stereotype effect in the Control treatment. Significant positive β_3 is consistent with our finding on the intercept effect of the default: both men and women are more likely to choose leading positions to contribute ideas when they default to 1. We did not observe any significant slope effect of the default as shown by insignificant β_5, and women's gender stereotype effect persists in RD_NF (H_0:$\beta_0 + \beta_1 + \beta_2 + \beta_5 = 0$ =0, p¡0.01 in regression (1), p¡0.01 in regression (2)).

Result 9. *Hypothesis 2a is supported and Hypothesis 2b is not supported. Random Default mitigates the gender gap in willingness to contribute ideas. However, the stereotype bias still exists in RD_NF.*

3.3.3 Merit effect

In addition to the mechanical benefit of selecting the best performers, we propose in the Hypotheses section, two behavioral hypotheses (H3a and H3b) regarding the effect of merit on the tendency to stay with the default when it is merit-based as compared to when it is randomly assigned. We now test each hypothesis.

First, we don't observe the evidence for MeritDefault – Information effect (H3a). Figure 4 reports the frequency of choosing position 1 in each treatment, separately for high and low performers (except in the MD treatment where only high performers are defaulted to the position 1). Again, in the three default treatments, we only include those who were defaulted to position 1. As shown in Figure 4, high performers are more likely to stay with the pre-selected position (i.e. choose position 1) in the MD and RD_F treatments than in the RD_NF treatment (MD vs. RD_F vs. RD_NF: 64.07% vs. 59.68% vs. 54.41%, p=0.08, ANOVA).

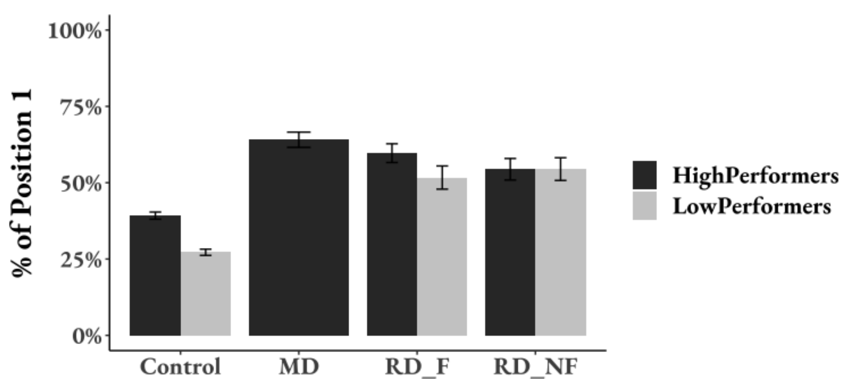


Figure 3.2: Likelihood of Choosing Leading position of High (Low) Performer

Second, Figure 4 shows that high performers in MD are not more willing to stay with leading position compared with RD_F (64.07% vs 59.68%, t-test, p = 0.27). This result does not support the MeritDefault-Legitimacy effect (H3b).

We further use regression framework to test the robustness of the above findings. We use Probit regression and separate our analysis of the default effects on the likelihood of choosing leading position by whether the participants are the high performers in a category in Table 4. Control variables including demographic variables and task performances are introduced in both regressions. In regression (1) for high performers, significant positive β_1, β_2, and β_3 show the default effect: high performers are more likely to choose the leading position when they are default to 1. Again, we don't find MeritDefault- information effect, as there is no magnitude difference among β_1, β_2, and β_3. MeritDefault – legitimacy effect can be shown by greater β_1 than β_2. However, the legitimacy effect is weakly significant in regression (1). The significant negative β_4 and β_6 in both regressions further confirm the presence of gender gap and gender stereotype effect of women among high performers. We omitted coefficients of the intersection of treatment condition, Maleness Score, and Female in both regressions as they are all insignificant, which implies defaults cannot mitigate the gender stereotype. This result is consistent with our finding in III.A.

Result 10. *Neither Hypothesis 3a Hypothesis 3b is supported. High performers in MD are not more willing to contribute ideas, compared with RD_NF and RD_F.*

3.4 Conclusion

Under-representation of qualified women in contributing ideas to the group is pervasive in many workplaces, especially in male-stereotyped fields. We develop and test using laboratory experiments an easily-implementable default (opt-out) approach to reducing this gender gap in environments with varying gender stereotypes. We consider both non-merit based defaults where people are randomly defaulted to the leading position to contribute (from which they can opt-out), as well as merit-based defaults where people's default position is determined by their skill and ability. We find that defaults have large and significant effects on people's willingness to contribute ideas, regardless of whether they are merit-based. The effect is especially large for women, who also demonstrate significant gains in confidence when the default is based on merit. We find little evidence, however, that defaults mitigate stereotype bias in the willingness to contribute. In particular, defaults lead women to be more likely to contribute in all areas, but still relatively less in male- than female-stereotyped environments.

Appendix A: Appendix for Chapter 1

A.1 Subjects' Comments

After the two-period trading experiment and before the exit survey, subjects are invited to answer questions on how they made decision in each period. The two questions are not required. Subjects did not gain payment from answering them. We report some comments of buyers who choose not to pay the upfront fee in the second period.

- "Since I got a profit the first time I didn't want to go again with my luck"
- "Risk vs Reward...... I got lucky and did not have to pay."
- "Based on the membership fee. "
- "didn't want to take any big risks so I just lowballed my offers and refused to take the membership"
- "i read the instructions carefully. i think the second period isn't worth losing the points - i had to pay membership fee and could only get the item by bidding higher than the price set by the seller...... honestly, i haven't been feeling lucky so i'd rather not take my chances. so i tried not to lose money in the first period and just left it as is."

A.2 Additional Analysis

In this section we use a greater-than-valuation bid as the measure of overbidding. We report the percentage of overbid in each mechanism and in each period in Table A.1 We find that significant less buyers overbid in NC than in RS (43.75% vs. 55.46%, $p = 0.061$, two-sided t-test) in Period 1. We don't find significant difference of overbidding behaviors in Period 2 (55.46% vs. 55.48%, $p = 0.900$, two-sided t-test)[1].

[1]For two-sided Proportion tests, p=0.061 for Period 1, p=0.095 for Period 2.

Table A.1: Percentage of Overbid Comparison

Bid/value	**Non-clairvoyant Dynamic**	**Repeated Static**	p-value
F_A (low variance)	43.75 (4.38)	55.46 (4.39)	0.061
F_B (High variance)	55.46 (4.39)	54.68 (4.40)	0.900

Note: Standard errors in parentheses.

We use OLS regressions[2] to further investigate the different bidding behaviors under the two mechanisms. We consider whether a bid is greater than the valuation (=1) in each period separately. In Regressions (1) and (2), we regress overbid in Period 1 on mechanism (NC=1), scenario (Scenario A =1), valuation in Period 1 ($value_1$), and risk attitude index. We regress bid-value ratio in Period 2 also on valuation in Period 2 ($value_2$), the amount of upfront fee ($upfront_2$), and whether the buyer gets the free item in Period 1 ($free_1$) in Regressions (3) and (4). From Regression (1), we find that buyers in the non-clairvoyant dynamic mechanism are less likely to overbid compared to those in the repeated static mechanism in Period 1 for both Scenario A and Scenario B. Perhaps buyers overbid less under NC in Period 1 due to the fact that a 50% chance of receiving a free item encourages less overbidding. This finding supports Hypothesis 3. When controls are included in Regression (2), risk aversion plays a role in mitigating the overbidding behavior. The overbidding lasts across periods, as shown in Regressions (3) and (4): if buyers overbid in Period 1, they are more likely to overbid in Period 2.

[2] We find similar results with Probit regressions.

Table A.2: Regression of Overbid on mechanisms

	DV: Overbid =1)			
	Period 1		**Period 2**	
	(1)	(2)	(3)	(4)
NC (=1)	-0.12	-0.12	-0.09	0.21
	(0.06)	(0.08)	(0.15)	(0.20)
Scenario A (=1)	0.00	0.05	-0.09	-0.05
	(0.06)	(0.09)	(0.07)	(0.10)
$value_1$	-0.01	-0.01		
	(0.01)	(0.01)		
Risk attitude	-0.04	-0.06	0.02	0.01
	(0.02)	(0.02)	(0.02)	(0.02)
$Overbid_1$			0.33	0.26
			(0.06)	(0.10)
$value_2$			-0.01	-0.01
			(0.01)	(0.01)
$upfront_2$			0.03	-0.05
			(0.04)	(0.06)
$free_1$			0.03	-0.04
			(0.09)	(0.13)
Constant	0.76	0.51	0.39	0.36
	(0.08)	(0.34)	(0.09)	(0.36)
Controls	No	Yes	No	Yes
Observations	256	256	244	244

Notes: Coefficients of Probit regressions are reported.

For Regression (2) and (4), gender (male =1), age, and graduate student (=1) are introduced as controls. We do not find significant controls.

Standard errors in parentheses.

A.3 Instructions

Welcome

This is an experiment in the economics of decision making. Your earnings will depend on your decisions. If you follow the instruction carefully and make thoughtful decisions, you may earn a considerable amount of money.

Your payoff will be determined by the experimental points that you earn during the experiment. The points will be converted into dollars at the end of the experiment at the following rate:

$$10 \text{ Points} = 1 \text{ Dollar.}$$

You will start with 50 points.

Your decision in the one practice session will not influence your earning. You will be paid according to your decision in the one experimental session. The experiment will be conducted only once.

Your experimental task

In this experiment, you will trade with a robot seller. You can gain points when you buy the item from the Seller. The amount of points you receive is equal to your given value of the item minus the amount you pay the Seller.

There are two periods. You can buy one item in each period from the robot seller by making a bid. The cost of producing the item for the robot seller is zero.

The value of the item for you in each period might be different. **The value of the item is only known to you**. The robot seller is not told the true value of the item, but they are told what the possible values are, and how likely that value is to be selected.

RS only Instructions

In each Period:

The robot seller will set a secret price based on the possible values for each period. The secret price in period 1 might be different from the secret price in period 2.

In each period you only get the item if your bid is greater than (or equal to) the secret price. However, you only have to pay the secret price, even if you bid more.

NC only Instructions

In the First Period:

There is no membership fee in this period.

Chance of getting the item for free:

You have an equal chance to get the item for free or not.

If you cannot get the free item, you win the item only if your bid is greater than (or equal to) the secret price that the seller chooses.

However, you only have to pay the secret price, even if you bid more.

In the Second Period:

The robot seller will set a membership fee and another secret price based on your bid in the first period.

If you don't pay the membership fee, the game ends.

If you pay the membership fee, you get to learn your value in the second period.

Chance of getting the item for free:

You have an equal chance to waive the membership fee or not.

If you have to pay the membership fee, the higher the membership fee you pay, the lower the secret price in this case.

Whether you can waive the membership fee or not, you only get the item if your bid is greater than (or equal to) the secret price. However, you only have to pay the secret price, even if you bid more.

Appendix B: Appendix for Chapter 1

B.1 Selected comments

In Period 1

- "Go big or go home".
- Aimed high, looking for a heavy bid
- You'd be surprised when I say - I based it off the charts.
- Random.

In Period 2

- Again, attempted high roll, but failed greedily.
- Higher price didn't work so I went lower.
- buyer bid for 1?? which makes no sense so I wanted to get some out of him and set the price to 6 as possible values could have been pretty high. Then set price to 4 as I would get it 50% of the time
- Set a low price, however, buyer decided not to purchase.

B.2 Additional Analysis

B.2.1 "Same" Scenarios

The percentage of choosing NC in each treatment in "Same" scenarios is shown in Figure B.1. There is no treatment difference in choosing NC either in the Practice stages (Set 2 vs. Set 4: 50.00% vs. 45.31%, $p = 0.53$, one-sided t-test) or in the Tail stages (Set 2 vs. Set 4: 42.19% vs. 39.84%, $p = 0.80$, one-sided t-test). In the Practice stages, the percentage of choosing NC in both treatments does not significantly differ from 50% (p = 1 in Set 2, p = 0.26 in Set 4, two-sided t-test). This finding further indicates that the behavior of sellers at the beginning of the experiment mimics randomizing. However, at the end of the experiment, sellers tend to choose NC significantly less than 50% (p = 0.13 in Set 2, $p = 0.02$ in Set 4, two-sided t-test).

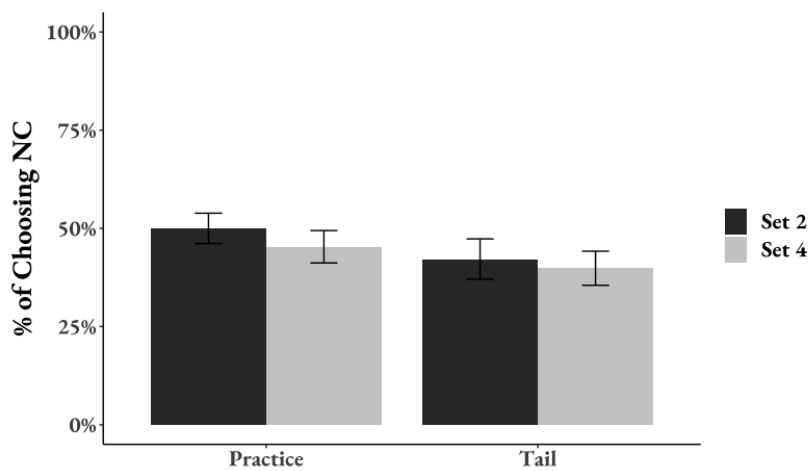


Figure B.1: % of Choosing NC in Same Scenarios

B.2.2 Reserve Prices and Bids

Figure B.2 shows the associated reserve price (r_2^P) set by sellers in the case that the upfront fee is not refunded. We find that, in the early stages, r_2^P in treatment Set 4 is significantly greater than that in treatment Set 2 (Set 2 vs. Set 4: 2.61 vs. 6.46, $p < 0.01$, two-sided t-test). While, there is no treatment difference in the later stages (Set 2 vs. Set 4: 5.87 vs 7.67, $p = 0.28$, two-sided t-test).

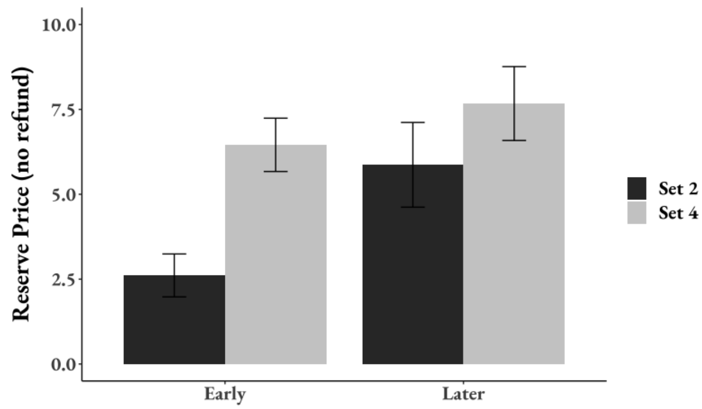
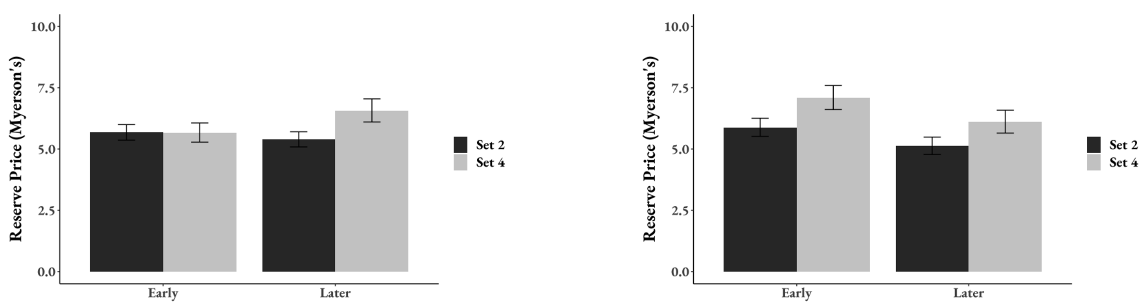


Figure B.2: Reserve Price in Period 2 (No Refund on Upfront Fee)

Figure B.3 shows that sellers in treatment Set 4 set higher Myerson's reserve prices than those in treatment Set 2. (Set 2 vs. Set 4: 5.39 vs. 6.57 in in the later stages of Period 1, $p = 0.04$; 5.88 vs. 7.10 in the early stages of Period 2, $p = 0.05$. Two-sided t-test.)

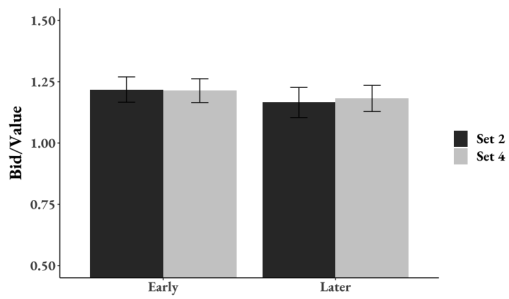


(a) In Period 1

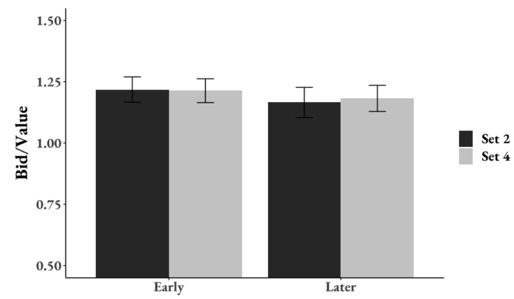
(b) In Period 2

Figure B.3: Myerson's Reserve Price

We find that buyers overbid in general. The overall bid-value ratio is significantly greater than 1 in the two periods (1.19 in Period 1, p¡0.01 in Period 1; 1.20 in Period 2, $p < 0.01$). Figure 1 shows the bid-value ratio in each treatment and in each stage. We do not observe any treatment difference in overbidding (1.22 vs. 1.21 in the early stages in Period 1, $p = 0.92$; 1.17 vs. 1.18 in the later stages in Period 1, $p = 0.77$; 1.20 vs. 1.21 in the early stages in Period 2, $p = 0.91$; 1.19 vs. 1.20 in the early stages in Period 2, $p = 0.89$. Two-sided t-test).



(a) In Period 1



(b) In Period 2

Figure B.4: Bid/Value Ratio

B.3 Instructions

Welcome

This is an experiment in the economics of decision making. Your earnings will depend on your decisions. If you follow the instruction carefully and make thoughtful decisions, you may earn a considerable amount of money.

Your payoff will be determined by the experimental points that you earn during the experiment. The points will be converted into dollars at the end of the experiment at the following rate:

$$10 \text{ Points} = 1 \text{ Dollar.}$$

Roles in the two-period experiment

There are two roles in this experiment, the Seller and the Buyer. Half of you will be selected at random to have the role the Seller, the other half of you will have the role of Buyer. Your role will NOT change during the whole experiment.

You will learn whether you are the Seller or the Buyer prior to making any decision.

There are 2 practice rounds and 10 experimental rounds. In each round, one Buyer and one Seller will be randomly paired. The Buyer will be paired with a different Seller in different rounds. Each round has 2 periods.

Your experimental task in each round

The Buyer can buy one item in each period from the Seller by making a bid. The cost of producing the item for the Seller is zero.

The value of the item for the Buyer in each period might be different. The value of the item is **only known to the Buyer**. The Seller is not told the true value of the item but they are told what the possible values are, and how likely that value is to be selected.

If you are the Buyer:

You can gain points when you buy the item from the Seller. The amount of points you receive is equal to your given value of the item minus the amount you pay the Seller.

In another round you may have to pay an entry fee. If you pay this, you lose some points but have a chance to buy the item. If you don't pay, you have no chance to buy the item.

If you are the Seller:

You can gain points when you sell the item to the Buyer. The amount of points you receive is equal to the amount the Buyer pays you.

You will start with **60 points** in each round.

Trading Procedure

To start:

The Seller first decides on the trading structure for the two periods. The Seller will choose between structure A and structure B, which will be explained in detail on the following page.

After the structure is chosen, in each period:

The Buyer and Seller are shown the possible values of the item. The Seller chooses a secret price for the item and this price is kept hidden from the Buyer.

The Buyer is told the value of the item, and the possible values that the Seller is given. The Buyer then has the opportunity to make a bid on the item.

After the Seller has set the price and the Buyer has made a bid (if they chose to make a bid), results from this are shown and the Buyer's bid amount is revealed to the Seller. Whether the Buyer wins the item and how much they must pay is partially determined by the trading structure that the Seller chose.

The next few pages go over the two trading structures that the Seller will choose from.

Trading Structure A

In Period 1

The Buyer has a 50% chance of getting the item **for free**. If the Buyer does get the item for free, the Buyer receives the item without paying the Seller any point.

If the Buyer does not get the item for free, the Buyer can buy the item if their bid is greater or equal to the secret price set by the Seller. Even if the Buyer's bid is higher than the Seller's price, the Buyer only has to pay the Seller's price to receive the item.

In Period 2

Entry fee is required for the Buyer.

In treatment Set 2 it reads:

The Buyer must pay an entry fee to be able to bid on the item. At this point, they know the potential values of the item but not their true value.

Instead of the Seller choosing the amount of the entry fee, The Computer will set an entry fee that is beneficial to the Seller, based on the Buyer's bid in Period 1 and the possible values for the item in this period.

In treatment Set 4 it reads:

The Buyer must pay an entry fee set by the Seller to be able to bid on the item. At this point, they know the potential values of the item but not their true value.

If the Buyer doesn't pay the entry fee, the game ends.

If the Buyer pays the entry fee, the Buyer gets to learn their true value of the item for Period 2. Once they pay this fee, the Buyer has a chance to earn a refund on the entry fee, meaning they receive all the points they paid for the entry fee back.

The Buyer has a 50% chance to receive the refund on the entry fee and a 50% chance of not receiving the refund.

In treatment Set 2 it reads:

If the Buyer does not receive the refund, the secret price for the item will be chosen by the computer. Instead of the Seller choosing the price, the Computer will set a price that is optimal for the Seller.

If the Buyer does receive the refund, the secret price for the item will be chosen by the Seller.

In treatment Set 4 it reads:

The Seller might set different secret price for the two cases.

The same rules for the Buyer receiving the item apply in either case, if their bid is higher than the secret price, then they receive the item and pay the Seller the secret price.

Summary of Structure A:

In Period 1, the Buyer has a 50% chance of receiving the item for free.

In Period 2, the Buyer has a 50% chance of receiving a refund on the entry fee if they choose to pay it.

Trading Structure B

In period 1

There is no opportunity for a free item.

Both the Buyer and the Seller receive the possible values for the item.

The Seller sets a secret price for the item that is hidden from the Buyer.

The Buyer receives their true value of the item and makes a bid on the item. The Buyer can buy the item if their bid is greater or equal to the secret price set by the Seller. Even if the Buyer's bid is higher than the Seller's price, the Buyer only has to pay the Seller's price to receive the item.

In Period 2

There are no entry fees required.

Repeats the process of Period 1. The Seller makes a new secret price and the Buyer receives a new value for the item and makes a bid.

Summary of Structure B:

In Period 1, there is no opportunity for a free item.

In Period 2, there is no entry fee required.

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Biography

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