THREE EXPERIMENTAL STUDIES ON THE DESIGN OF CONTESTS AND AUCTIONS

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To my family

Abstract

Contests and auctions are two commonly used tools for principals to regulate agents towards desired objectives. Due to their successful application in many fields, extensive empirical and theoretical research examines how the design of such environments affect agents' decisions and principals' welfare. However, some of the major variables cannot be obtained from the observed field data. In contests, principals can only glean the agents' performance but not their investment (effort, time,etc.); in auctions, the auctioneer only observes bidders' bids but not their valuation of the asset under auction. This missing information is crucial in evaluating the theory and providing guidance for practice. This experimental study serves as an additional instrument in this case by using controlled laboratory experiments to examine how the design of contests and auctions affects individual decisions from a policymaker perspective. This thesis shows that: (1) in Tullock contests, when the entry is endogenous, disclosing (concealing) the number of contestants can elicit higher total effort when the cost of the effort function is concave(convex); (2) increasing the competition in a rank-order tournament will not only increase the dispersion but many also increase the skewness of contestants' strategy; and (3) simple indicative bidding can improve auctioneers' revenue by encouraging more entry when bidders face a relatively high entry cost. This thesis contributes to the literature on the design of contests and auctions and provides experimental evidence to inform policymaking and market design.

Keywords

Tullock contest, Information disclosure, Rank-order tournament, Two-stage auction, Experiment, Endogenous entry, Risk-taking

Statement of Original Authorship

The work contained in this thesis has not been previously submitted to meet requirements for an award at this or any other higher education institution. To the best of my knowledge and belief, the thesis contains no material previously published or written by another person except where due reference is made.

Signature: QUT Verified Signature

Date: 12/04/2021

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Chapter 1

Introduction

"One of the most important contributions of economics has been to the understanding how incentive works."

– Hugo F. Sonnenschein

In many economic, political, and even biological situations, competitive environments are used to incentivize agents to behave in such a way that certain outcomes prevail (Menezes and Monteiro, 2000). For example, promotion within company can elicit higher effort from employees (Kini and Williams, 2012), R& D competition encourages technology innovation (Erat and Gneezy, 2016), auctions are used to allocate resources to the most suitable bidder (Bulow and Klemperer, 2009), and competing for a mate is part of the natural selection process (Dekel and Scotchmer, 1999). These activities are usually held by different parties and serve different purposes. However, one question of common interest remains: how does the design of institutional arrangements in these activities affect agents' behavior and to what degree can the organizer utilise this design to achieve a certain goal?

There is a well-established literature on mechanism design and game theory that predicts how the structural factors shape individual decisions (Klemperer, 1999; Konrad, 2009). These studies provide valuable guidance for institutional design in practice. Examples of how theory inspires the design of economic activities includes but is not limited to the spectrum license auction in the U.S., the 3G auction in Europe, and the electricity auction in Australia (Milgrom, 2004).

The process of mechanism design is based on ubiquitous awareness and thoroughly rational calculations. Sophisticated strategic thinking and complicated computation make it questionable that the economic agents, even those experienced participants, behave in practice exactly as the model predicted. How to evaluate the effect of institutional design, and whether individual behavior deviates from theoretical prediction are questions worth examining prudently.

One way of testing these theories is via an empirical study using field data. Such a study serve as an excellent tool to estimate parameters in the model and test whether observed data follows predictions. However, as Dechenaux et al. (2015) note in their survey paper of the contest literature, measuring individual behavior (such as their effort or risk-strategy) using observed field data is nontrivial, let alone the capture of unobserved individual characteristics such as risk preferences and their valuations. Besides, in terms of assessing a new mechanism, examining field data can either be too expensive or too time-consuming. Therefore, a controlled experimental study emerges as a suitable alternative approach.

The primary purpose of this thesis is to use the experimental method as a wind tunnel to examine how the institutional design in a competitive environment incentivizes individuals' decisions and consequently affects the organizers' welfare. I explore the two most extensively used competitive environments, namely contests and auctions. The first part of this thesis contains two studies that focus on the design of contests. In the second part of the thesis (the last study), I investigate the design of auctions. To lay out the theoretical foundation, I set forth the basic model of contests and auctions in Section 1.1 and Section 1.2, respectively. Section 1.3 provides an overview of the thesis.

1.1 Contest models

Contests can be defined as interactions among a group of players who expend costly and irreversible efforts or resources to affect their probabilities of winning a prize (Dechenaux et al., 2015). Conventionally, according to the contest success function (CSF) that maps the effort to the probability of winning, contest models are categorized into Tullock contests (Tullock, 1980), rank-order tournaments (Lazear and Rosen, 1981), and all-pay auctions (Hirshleifer et al., 1978).

The major differences between the rank-order tournament and Tullock contest are in the assumption of performance and the winner selection.[1] In a Tullock contest, contestants' performance is deterministic and equal to the effort. The randomness is in the selection process wherein each contestant has a probability of winning the prize. However, in a rank-order tournament, contestants' performance is a distribution determined by both the effort and a random factor. Wining is deterministic given a realized performance. The consequence of this difference is that when considering how the design affects contestants, the organizers' major concern is contestants' effort level under the Tullock contest model, but both the effort and the riskiness of the effort (random factors in the performance)[2] in the rank-order tournament model.

Despite the differences in assumptions, both models are often applied but not limited to sports competitions, political campaigns, promotions, and R&D races to elicit higher performance from the contestant(s).[3] I leave the discussion of which type of contest model best mimics the practical situations to further empirical study. In this thesis, I only consider the perspective of policymakers (or contest organizers) regarding how the design of the contest affects contestants' behavior (effort and/or risk-taking) in the Tullock contest and rank-order tournament model, respectively.

1.1.1 Tullock contest with endogenous entry

In Tullock contest literature, it has been long proved that contestants' incentive to exert effort can be affected by environmental factors including but not limited to contest successful function, prize, cost structure, and the number of participants (Moldovanu and Sela, 2001; Sheremeta, 2010; Chowdhury et al., 2014). The majority of these studies are under the "fixed-N" paradigm in which the number of players are exogenously given

[1]For brevity, I do not discuss all-pay auctions, interested readers can refer to Konrad (2009) and Dechenaux et al. (2015).

[2]In the rank-order tournament literature, some studies assume the random factors are exogenous (out of the control of both contest organizer and the contestants) (Lazear and Rosen, 1981). Other studies assume the random factors is the riskiness of the effort, which can be controlled by the contestants (Hvide, 2002). This thesis takes the second approach in the discussion of the rank-order tournament.

[3]In some cases, the organizer's goal is to elicit a higher total level of effort in the contest; while in other cases, the organizer only cares about the highest performance in the contest.

and known to all the contestants. The recent emerging literature on contest models are built with population uncertainty or endogenous entry (Warneryd, 2006; Münster, 2006; Li and Zheng, 2009; Fu et al., 2015; Boosey et al., 2017). When the population is uncertain, contestants are not aware of the actual number of entrants in the contest, while contest organizers typically obtain this information after participants enter. This *ex post* information asymmetry is another instrument that contest organizers can use to improve the contest design (Fu et al., 2011). In real-life constest scenarios (such as company promotions, collage applications and R&D races), when should the contest organizer reveal the number of contestants? What is the factor that determines whether this information should be revealed or not? Chapter 2 explores these questions using both a theoretical and experimental approach.

1.1.2 Risk taking in rank-order tournaments

The second study (Chapter 3) in this thesis is based on the rank-order tournament model. There are two main streams of literature on the rank-order tournament. The first, led by Lazear and Rosen (1981), considers the random factor in the performance to be exogenous; following Hvide (2002), the second stream assumes the random factor in the performance is the riskiness of the strategy (which can be chosen by contestants). There is growing empirical evidence that participants in the rank-order reward system compet by taking risks (Diamond and Rajan, 2009; Grund et al., 2013; Faravelli et al., 2015). Despite extensive research on how the design of the rank-order tournament affects contestants' effort, there is a lack of research on contestants' risk-taking behavior, particularly the experimental evidence on risk-taking. Out of the limited studies which examine the risk-taking behavior in tournaments, the majority assumes that performances are restricted to symmetric distributions, reducing the choice of risk to the choice of variance. In contrast, Fang and Noe's 2016 theoretical model shows that in the equilibrium, performance distributions freely chosen by the participants need not be symmetric. Chapter 3 presents an experiment which examines how contestants shift their performance distribution with the change in tournament design when they can choose any shape of performance distribution within the budget constraint.

1.2 Second price auction with costly entry

The second part of this thesis (Chapter 4) discusses auction design. Auctions are a transaction that the seller uses to allocate resources to buyers through competitive bidding (Vickrey, 1961). Distinct from contests, the goal of auctions is to select the bidder who values the asset under auction the most, so that the auctioneer can achieve the highest possible revenue.[4] There are four basic auction mechanisms: first price and second-price sealed auctions, English auctions and Dutch auctions.[5]

The mechanism of interest in this thesis is the single-unit sealed second-price auction. In this type of auction, several bidders make competitive bids to obtain one single indivisible object. All the bidders bid simultaneously and the bidder with the highest price wins the prize and pays the second highest bid. One of the critical features is that the weakly dominant strategy for each bidder is to bid their value. Hence, the crucial problem for the auctioneer becomes how to attract and include those bidders with higher values to participate in the auction.

In many of the auctions where the underlying asset is complex and of great value, the bidders need to forgo a significant amount of time and resources to assess their valuation towards the asset once they decide to enter the auction. This high cost will decrease bidders' willingness to participate, which potentially causes auctions to fail and leads to lower expected revenue. To deal with this issue, theoretical studies have offered several ways to encourage participation in auctions. Bhattacharya et al. (2014) and Sweeting and Bhattacharya (2015) study the entry right auction (ERA) which can ensure the bidder with the highest value enters the auction. However, the ERA requires bidders to pay a significant amount in the pre-auction bid stage, which stops some of the bidders from participating (Quint and Hendricks, 2018). Ye (2007) studies indicative bidding mechanism which only require non-binding pre-auction bids, but finds that it cannot guarantee the highest bidders to be selected theoretically. This is supported by the experimental study by Kagel et al. (2008). Based on the latest theoretical paper of Quint and Hendricks (2018), Chapter 4 of this thesis provides an experiment

[4] For procurement, the object is the opposite; auctioneers' best interest is to find the bidder with the lowest bid (cost).

[5] See books by Menezes and Monteiro (2000) and Milgrom (2004) which introduce different types of auctions.

which investigates a simple indicative bidding mechanism, which theoretically should encourage more entry and improve the efficiency of selecting suitable bidders.

1.3 Research outline

This thesis comprises three studies that build upon previous theoretical and experimental studies.

Chapter 2 investigates whether a contest organizer who seeks to maximize the expected total effort of participants should disclose the actual number of contestants when entry in a contest is endogenous.[6] A Tullock contest with an entry-stage model is developed in this chapter. The theoretical model suggests that whether to disclose the number of participants depends on the convexity of the cost of effort function. Even though the equilibrium entry rate and rent dissipation are invariant to the disclosure policy, disclosing (concealing) the actual number of entrants can lead to a higher total effort when the cost function is concave (convex). To test these theoretical predictions, I conduct a 2×3 between-subjects laboratory experiment with lottery contests.[7] The experiment changes the disclosure policy (fully disclosed vs. fully concealed) in one dimension and the curvature of the cost of effort function (concave, linear, or convex) in the other dimension. The results are largely consistent with the theoretical predictions regarding the optimal disclosure policy, despite the presence of moderate over-entry and over-exertion behavior that is commonly observed in the literature on experimental contests.

Chapter 3 addresses how contestants' risk-taking behavior in a rank-order tournament is affected by the tournament design.[8] Building upon the theoretical model of Fang and Noe (2016), I design an experiment in which participants can use a visualized interactive distribution builder to choose any performance distribution. This unique design allows us to study how changes in the prize schedule, the number of contestants and the size of the contests affect participants' risk-taking behavior. Participants in the experiment react to changes in the tournament design in the direction predicted by

[6] Co-authored with Changxia Ke and Qian Jiao.

[7] A lottery contest is a special form of Tullock contest when $r1$ in the CSF equals 1. Although this parameter is chosen for the experiment, the results from the model apply to all Tullock contests.

[8] Co-authored with Changxia Ke, Gregory kubitz and Lionel Page.

comparative statics of the equilibrium play: (1) decreasing the proportion of winning prizes in a simple contest leads to more risk-taking behavior (reflected by choosing performance distributions that are both more dispersed and more negatively skewed); (2) convexifying the prize schedule encourages choices of performance distributions that are both more dispersed and more positively skewed (i.e. selecting riskier strategies); (3) adding more contestants into the contest induces participants to choose riskier performance distributions that are more dispersed. The results in this study have important implications for real-world tournaments that resemble the characteristics of rank-order tournaments in which the primary choice variable is the level of risks to be taken. The design of these tournaments may incentivize contestants to take too much risk, potentially at the cost of the principals. Principals could underestimate the probability of having low outcomes if they assume that the performance distributions are always symmetric.

Chapter 4 discuss a simple indicative bidding model that theoretically can improve the selection efficiency compared with the conventional indicative bidding mechanism.[9] Using a laboratory experiment, this study compares the simple indicative bidding with the unrestricted auction, and the restricted auction. The experiment provides compelling evidence that auctioneers who aim to optimize revenue might choose the simple indicative bidding model over the other two mechanisms. In addition, by observing the bidders' entry choice and their valuation (which are usually not observable in the empirical study), I disentangle the two channels that cause the differences in expected revenue under the three different mechanisms, namely the participation effect and the selection effect. The findings indicate that the revenue advantage in the simple indicative bidding model is primarily attributable to the participation effect, i.e., encouraging more potential bidders to enter the auction.

The three studies examine some of the most recently developed theoretical models in contests or auction theory. The findings enhance our understanding of how agents' entry choice, effort invested, and risk-taking decisions react to the design factors in contests and auctions. Although most real-life contests and auctions are more complex than the laboratory experiment environment, this thesis offers insightful results useful to policymakers when designing such activities. Finally, Chapter 5 discusses the general

[9]Co-authored with Changxia Ke and Gregory Kubitz.

conclusions of this thesis.

Chapter 2

When should we disclose the number of contestants?

2.1 Introduction

Contests, in which, a number of players exerting costly and irreversible efforts to compete for a limited number of prizes, are ubiquitous in real-world activities. In the literature on contest theories and experiments, the number of contestants is mostly fixed and common knowledge, assuming that contestants enter by default and that entry is free.[1] However, this assumption can be violated in real-life contests because entry is often costly and endogenous. For example, R&D firms must decide which patent race(s) to join from many possibilities, and job applicants must consider which position(s) to apply for from all job posts. Entry can be costly due to either the initial fixed investment required to start projects (or to prepare job-application materials) or the various opportunity costs associated with these decisions. Similar arguments can also be applied to sports or promotional contests. While there is *ex-ante* population uncertainty for both contest organizers and contestants, the organizers usually observe the actual number of contestants after entry decisions have been made. Therefore, there is *ex-post* information asymmetry between the organizers and the contestants, which naturally raises the question about whether a contest organizer should disclose or conceal the actual number of contestants before costly efforts have been exerted.

[1] See Konrad (2009) and Fu and Wu (2019) for reviews on contest theories and Dechenaux et al. (2015) for a survey of contest experiments.

In this paper, we aim to address this question both theoretically and empirically using a laboratory experiment. We examine contests in which efforts made by contestants are considered to be beneficial; therefore the objective of the organizers is to elicit the maximum aggregate effort from all participants.[2] In particular, we focus on the relationship between the optimal disclosure policy and the curvature of the cost of effort function. Based on the existing contest literature on information disclosure (Lim and Matros, 2009; Fu et al., 2016; Feng and Lu, 2016; Boosey et al., 2017), which mostly study contests with exogenous stochastic entry under linear cost of effort, our paper is the first to explore whether organizers should disclose the actual number of entrants in contests with endogenous entry and non-linear cost of efforts.

Following Fu et al. (2015), we model the contest as a two-stage game in which the participants first decide whether to incur a fixed cost to enter the contest in Stage 1, and then make their effort choices to compete for a prize in Stage 2. In addition, we add a preliminary stage (Stage 0), during which the contest organizer must pre-commit to either fully disclosing or fully concealing the actual number of contestants after entry decisions have been made in Stage 1. Adopting the well-studied Tullock (1980) contest, we predict that even though the equilibrium entry probabilities and rent-dissipation rates are invariant to the disclosure policy in all cost structures, fully disclosing (concealing) the number of entrants will lead to a higher (expected) total effort when the cost of effort function is concave (convex). Similar to Lim and Matros (2009), which predicts when entry is exogenous and stochastic, the expected total effort is invariant to the disclosure policy under the linear cost of effort function, we show that their finding can also be extended to endogenous entry.

The type of contests we examine in the model has wide applications. In contests like R&D races or sports contests, the marginal cost of investment/effort is usually increasing due to the difficulty of pushing the limit. Conversely, when learning and practice play a major role in contests (e.g., college admissions, professional qualification exams, and sales contests), the marginal cost of effort could be decreasing (thus the cost functions tend to be concave). Our analysis suggests that the contest organizer should carefully examine the nature (i.e., concavity/convexity) of the cost of effort in a contest

[2]Examples as such include promotion or sports contests that motivate better performance from all employees or athletes, or R&D races that encourage more firms to engage in R&D activities.

before committing to a disclosure policy.

Fully disclosing or concealing the actual number of entrants in contests (i.e., a *Disclosure* policy or a *Concealment* policy) involves different equilibrium concepts from the perspective of game theory. For a given entry probability, the disclosure policy only affects the expected total effort through the equilibrium effort choices. When the actual number of entrants is disclosed, the equilibrium individual effort decreases on this realized number. When the cost function is concave, the contestants behave as if they were risk loving: a small number of contestants (i.e., a more favorable contest) *motivates* contestants more than a large number of contestants (i.e., a less favorable contest) *demotivates* them. In contrast, the equilibrium effort of their counterparts under a *Concealment* policy is uniform. As a result, the expected total effort is higher when the actual number of entrants is disclosed than when it is concealed. Conversely, when the cost function is convex, the contestants behave as if they were risk averse, and the expected total effort is higher when the actual number of entrants is concealed. Regardless of the disclosure policy, the *ex ante* expected payoff (before each participant makes an entry decision) should be the same, because it should offset the cost of entry. Therefore, the (expected) total cost of effort and the equilibrium entry rate should be the same under different disclosure policies.

To test these theoretical predictions, we conducted a 2×3 between-subjects experiment at Wuhan University (China) at the end of 2017. We manipulate the disclosure policy (fully disclosed or fully concealed) or the curvature of the cost function (concave, linear, or convex), one at a time. Our experimental results provide reasonably good support for our model predictions. First, given a certain cost structure the participants enter the contests with similar probabilities, irrespective of whether the actual number of entrants is disclosed. Second, in line with theory, the total cost of effort (rent dissipation) is not significantly different across disclosure policies for a given cost structure, especially when the data from the second half of the experimental sessions are considered. Third, as predicted, the average total effort is insensitive to the disclosure policy when the cost function is linear, while it is significantly higher in the disclosed treatment when the cost function is concave. The only deviation from our theoretical predictions is when the cost function is convex: in this scenario, although the average total effort is higher in the concealed treatment (following the prediction), the difference is not statistically significant. Finally, the data at the individual level show moderate levels of over-entry (particularly when the cost function is concave) and over-exertion (particularly when the cost function is convex), which are commonly observed in previous contest experiments.

Our paper is broadly related to the theoretical and experimental literature on contests with *ex ante* population uncertainty. In the growing theoretical literature, population uncertainty is either modeled as exogenous stochastic entry, in which potential contestants enter with a given probability (Higgins et al., 1985; Myerson and Wärneryd, 2006; Münster, 2006; Lim and Matros, 2009; Fu et al., 2011; Kahana and Klunover, 2015, 2016; Ryvkin and Drugov, 2020), or driven by endogenous entry decisions made before a contest (Higgins et al., 1985; Fu and Lu, 2010; Kaplan and Sela, 2010; Fu et al., 2015).

While Higgins et al. (1985), Münster (2006), and Myerson and Wärneryd (2006) pioneered the theoretical models of contests under stochastic entry, Lim and Matros (2009) and Fu et al. (2011) are the first papers to examine whether contest organizers should reveal the actual number of entrants. Extending on Lim and Matros (2009) which finds that the expected total effort does not depend on the disclosure policy when the cost of effort is linear, Fu et al. (2011) adopt a more general ratio-form contest success function, with the standard Tullock success function as a special case. Similarly, they find that the optimal disclosure policy depends on the property of the characteristic function: fully disclosing (concealing) the actual number of entrants generates a higher expected total effort when the characteristic function is strictly concave (convex).[3] While our model with a non-linear cost of effort is isomorphic to a contest with linear cost and appropriately adjusted discriminatory power using the general ratio-form contest success function adopted in Fu et al. (2011), we study optimal disclosure policy under endogenous rather than exogenous stochastic entry.[4]

For a contest setting with endogenous entry, our theoretical model is closely related to Fu et al. (2015). They establish a symmetric entry-bidding equilibrium under a wide

[3]The ratio-form contest success function is $p_i = f(x_i)/\sum_{j=1}^{N} f(x_j)$ and $H(\cdot) = f(\cdot)/f'(\cdot)$ is defined as the characteristic function.

[4]Feng and Lu (2016) study a wider range of disclosure policies that allow for partial disclosure (concealment) using a Bayesian persuasion approach and find that partial disclosure is always sub-optimal, drawing similar conclusions to Fu et al. (2011). Ryvkin and Drugov (2020) further generalize the results of Fu et al. (2011) to arbitrary tournaments and arbitrary distributions of the number of players. They also show that the optimal disclosure policy depends on the shape of players' cost function in winner-take-all rank-order tournaments.

class of contest technologies, and investigate how bidding efficiency and optimal design are affected by relevant institutional elements (e.g., the discriminatory power of the success function and prize allocation scheme), in a nested Tullock contest. However, they assume the actual number of participants is always concealed to all participants.[5] To the best of our knowledge, our paper is the first to study the optimal disclosure policy with respect to the actual number of contestants in Tullock contests with endogenous entry. Taking a linear-cost function as a baseline, we examine how the concavity/convexity of players' cost of effort affects the optimal disclosure policy.

Compared with the theoretical literature on contests with *ex ante* population uncertainty, the experimental literature is sparse. Only a handful of studies examine contests with endogenous entry. Anderson and Stafford (2003) investigate how entry in rent-seeking contests and contest expenditure are affected by the available number of participants, cost heterogeneity, and the entry fee. Cason et al. (2010) compare entry in winner-take-all and proportional-prize contests. Morgan et al. (2012) and Morgan et al. (2016) allow participants to choose to enter sequentially in continuous time, and the number of entrants at each time-point is observable to all participants. Hammond et al. (2019) study all-pay contests in which bidders have private valuations to explore how to set a prize-augmenting entry fee to elicit more effort, when bids must be made without knowing the number of entrants.[6] None of the aforementioned studies investigate the optimal disclosure policy and the actual number of entrants is either fully disclosed or fully concealed throughout the contests.

The experimental studies closest to ours are those of Boosey et al. (2017, 2019).[7] Boosey et al. (2017) test the theoretical predictions of Lim and Matros (2009) in lottery contests with stochastic entry and linear cost of effort. Boosey et al. (2019) further study the optimal disclosure policy in lottery contests under endogenous entry and linear cost of effort. Our paper shares one dimension of the experimental variation with this paper,

[5]In contrast, both Fu and Lu (2010) and Kaplan and Sela (2010) assume endogenous entry, but the number of entrants is always known to all participants.

[6]In addition, Liu et al. (2014) use field experimental data collected from the online crowd-sourcing platform Taskcn to study how contest participation and submission quality depend on the size of the reward and the presence of a soft reserve or early high-quality submission.

[7]Aycinena and Rentschler (2018) also study information disclosure, but in first-price sealed-bid auctions and English auctions. They find that concealing the number of entrants generates higher revenue in first-price sealed-bid auctions, while disclosing the number of entrants is better for English auctions.

namely whether the actual number of entrants is fully disclosed or concealed. Regarding the other dimension, while Boosey et al. (2019) vary the outside option (low or high) so that the endogenous entry probability is high or low, we change the curvature of the cost function (keeping the entry cost constant across all treatments). The two treatments of our experiment with a linear cost (under the *Disclosure* or *Concealment* policy) are similar to the two treatments with the low outside option in Boosey et al. (2019). It is reassuring to note that all four treatments confirm that the expected total effort is independent of the prevailing disclosure policy when the cost of effort is linear. Our main contribution to the literature lies in the optimal disclosure policy when the cost of effort is *non-linear*. Both the theory and the experimental evidence suggest that disclosing the actual number of entrants or not is indeed a contest-design issue that the organizers should carefully consider if the cost of effort function might be non-linear.[8]

2.2 The Model and Predictions

We consider a Tullock contest in a three-stage framework. A fixed pool of $M(\geq 2)$ potential risk-neutral participants show their interest in joining the contest with a winner purse $V > 0$. In the preliminary stage, the contest organizer chooses and commits to her information-disclosure policy denoted by $d \in \{D, C\}$, D and C denoting the full *Disclosure* policy and the full *Concealment* policy, respectively. In Stage 1, after observing the rules of the contest, the participants simultaneously decide whether or not to enter the contest. Each participant pays a fixed cost $\Delta > 0$ for entry.[9] In Stage 2, N $(1 \leq N \leq M)$ participating contestants choose their level of effort $\mathbf{x}_N = (x_1, x_2, \ldots, x_N)$ to compete for V. A winner is selected and receives V according to the Tullock (1980) contest success function. Therefore, the winning probability of a participating contestant i is given by

$$p_{i,N}(x_i, \mathbf{x}_{-i}) = \frac{x_i^r}{\sum_{j=1}^N x_j^r}, \text{ if } N \geq 2 \text{ and } \sum_{j=1}^N x_j^r > 0. \tag{2.1}$$

By exerting an effort of x_i, contestant i incurs a cost of $c(x_i) = x_i^\alpha$, with $\alpha > 0$. The

[8]Note that our review is limited to participation in a stand-alone contest with a ratio-form contest success function. Studies have also been performed on theoretical and experimental auctions with entry or participation in multiple contests. For a recent summary, see Boosey et al. (2019).

[9]We make the regular assumption $\frac{V}{M} < \Delta < V$ following Fu et al. (2015) to guarantee that potential participants enter the contest randomly.

parameter $r \in (0, \overline{r}] \in (0, \alpha\frac{M}{M-1}]$ conventionally represents the discriminatory power of the selection mechanism. We simplify our analysis by imposing an upper limit, which guarantees the existence of a pure-strategy equilibrium effort. A higher r indicates that one's win depends more on his level of effort than on other noisy factors. If there is only one participant, he automatically receives the prize V regardless of his level of effort. If more than one contestant enters, but all of them make zero effort, the winner is randomly chosen from the pool of participating contestants. If nobody enters the contest, the organizer withdraws the prize.

Our model with a non-linear cost of effort is isomorphic to a contest with linear cost and appropriately adjusted discriminatory power in Fu et al. (2011). Let $y_i = x_i^\alpha$ be the *expenditure* of participant i and let $f(y_i) = y_i^{\frac{r}{\alpha}}$. Then the ($N$-player) contest can be framed in terms of expenditure, with contest success function

$$p_{i,N}(y_i, \mathbf{y}_{-i}) = \frac{y_i^{\frac{r}{\alpha}}}{\sum_{j=1}^{N} y_j^{\frac{r}{\alpha}}}, \text{ if } N \geq 2 \text{ and } \sum_{j=1}^{N} y_j^{\frac{r}{\alpha}} > 0.$$

The log-concavity of function $f(y_i)$, together with the discriminatory power $\frac{r}{\alpha} \in (0, \frac{M}{M-1}]$ guarantee the existence and uniqueness of symmetric pure strategy equilibrium in the contest.

When policy D is implemented, all contestants know N before deciding on their level of effort. The two-stage subgame boils down to a standard symmetric N-player contest (in Stage 2) with endogenous entry (in Stage 1). Whenever $N \geq 2$, each participant i chooses his level of effort x_i to maximize his payoff

$$\pi_i = p_{i,N}(x_i, \mathbf{x}_{-i})V - x_i^\alpha, \tag{2.2}$$

which is equivalent to chooses expenditure y_i to maximize

$$\pi_i = p_{i,N}(y_i, \mathbf{y}_{-i})V - y_i.$$

As shown by Fu et al. (2011), with characteristic function $H(y) \equiv \frac{f(y)}{f'(y)}$, the corresponding symmetric pure-strategy equilibrium expenditure is $y_N^* = \frac{N-1}{N^2}\frac{rV}{\alpha}$. Therefore, the symmetric pure-strategy Nash equilibrium effort can be written as $x_N^* = \left(\frac{N-1}{N^2}\frac{rV}{\alpha}\right)^{\frac{1}{\alpha}}$, which leads to an expected equilibrium payoff of $\pi_N^* = \frac{V}{N}\left(1 - \frac{N-1}{N}\frac{r}{\alpha}\right)$.[10]

Following the standard argument for games with endogenous entry (Levin and Smith, 1994), each contestant participates if and only if his expected payoff offsets the entry cost. Hence, the unique symmetric subgame perfect equilibrium entry probability $q_D^* \in (0,1)$ in Stage 1 should solve the following equation

$$\sum_{N=1}^{M} C_{M-1}^{N-1} q_D^{*N-1}(1-q_D^*)^{M-N}\pi_N^* = \Delta, \tag{2.3}$$[11]

where $C_{M-1}^{N-1} q_D^{*N-1}(1-q_D^*)^{M-N}\pi_N^*$ is the expected payoff of a representative contestant who enters the contest, while another $N-1$ contestants participate at the same time.

Given the equilibrium entry probability (q_D^*), the expected total effort (TE_D) is given by

$$TE_D^*(q_D^*) = Mq_D^* \sum_{N=1}^{M} C_{M-1}^{N-1} q_D^{*N-1}(1-q_D^*)^{M-N}\left(\frac{N-1}{N^2}\frac{rV}{\alpha}\right)^{\frac{1}{\alpha}}. \tag{2.4}$$

When policy C is implemented, N remains unknown to all participants in the two-stage subgame (after the preliminary stage). Therefore, no proper subgame exists after the entry stage and the subgame perfect equilibrium does not bite. Each participant chooses his level of effort after entry based on his (rational) belief about others' entry strategies, without knowing the actual number of entrants.

In this case, the strategy of each potential contestant is given by a pair $(q_{i,C}, x_{i,C})$, where $q_{i,C}$ is the entry probability of a potential contestant i and $x_{i,C}$ is his effort after entry. The symmetric equilibrium has been derived by Fu et al. (2015),[12] which can be summarized as follows: there exists a unique symmetric perfect Bayesian equilibrium, in which each potential contestant enters with a probability q_C^* that uniquely solves the following equation

$$\sum_{N=1}^{M} C_{M-1}^{N-1} q_C^{*N-1}(1-q_C^*)^{M-N}\frac{V}{N}\left(1 - \frac{N-1}{N}\frac{r}{\alpha}\right) = \Delta\,, \tag{2.5}$$

[10]This symmetric pure-strategy Nash equilibrium can also be obtained by using the standard technique, we relegate the details to the appendix.

[11]We recognize that there may exist asymmetric equilibrium entry strategies, i.e., some contestants may participate with a positive probability, while others may never enter. However, following the mainstream literature, we focus on the symmetric equilibrium.

[12]We again relegate the detailed proof of the unique symmetric equilibrium to the appendix.

and (upon entry) chooses a level of effort x_C^* given by $x_C^* = [\sum_{N=1}^{M} C_{M-1}^{N-1} q_C^{*N-1}(1-q_C^*)^{M-N} \frac{N-1}{N^2}\frac{rV}{\alpha}]^{\frac{1}{\alpha}}$. The expected total effort (TE_C) elicited by the contest organizer is

$$\begin{aligned} TE_C^*(q_C^*) &= Mq_C^* x_C^* \\ &= Mq_C^*[\sum_{N=1}^{M} C_{M-1}^{N-1} q_C^{*N-1}(1-q_C^*)^{M-N}\frac{N-1}{N^2}\frac{rV}{\alpha}]^{\frac{1}{\alpha}}. \end{aligned} \tag{2.6}$$

We are now able to compare the two equilibrium outcomes to explore the optimal disclosure policy. First, by directly comparing Equations (3) and (5), which are essentially the same, we can derive the following prediction.

Prediction 1 *In a Tullock contest with costly endogenous entry, the equilibrium entry probability of the potential contestants does not depend on the disclosure policy, i.e.,* $q_D^* = q_C^* = q^*$.

An inference that directly follows Prediction 1 is that the total cost of effort and thus the rent-dissipation rate are also insensitive to the disclosure policy. As each potential contestant receives zero net expected payoff in equilibrium, the total cost of effort of all participants should be exactly equal to the total prize value earned by all potential participants minus the total cost of entry. When all contestants enter with the same probability q^*, the expected total prize earned by all potential participants is $[1-(1-q^*)^M]V$, while the expected total cost of entry is $Mq^*\Delta$.[13] Therefore, the total cost of effort TCE should be the same under both policies

$$\begin{aligned} TCE_D &= TCE_C \\ &= [1-(1-q^*)^M]V - Mq^*\Delta. \end{aligned} \tag{2.7}$$

As the rent-dissipation rate is the ratio of the total cost of effort to the prize value, the two policies will lead to the same level of rent dissipation. Thus, we have the following prediction.

Prediction 2 *In a Tullock contest with costly endogenous entry, both the (expected) total cost of effort and the rent dissipation rate are the same under both disclosure policies.*

Given the same equilibrium entry probability, the choice of effort in the competition

[13]Note that $(1-q^*)^M$ is the probability that nobody enters the contest, thus the prize is kept by the organizer, and $1-(1-q^*)^M$ is the probability that the prize is taken by one participant.

stage should be the same as that with exogenous and stochastic entry. As shown by Fu et al. (2011) in Theorem 1(b), if $H(y)$ is linear, expected equilibrium *expenditure* y is invariant to the disclosure rule. In our model, $H(y) \equiv \frac{f(y)}{f'(y)} = \frac{\alpha}{r} y$ is indeed linear. Applying the inverse of effort-to-expenditure transformation $x^* = (y^*)^{\frac{1}{\alpha}}$, the expected total effort corresponding to the expenditure level must vary with the disclosure policy, in a direction that depends on whether $\alpha \gtrless 1$. Following the same intuition, by further comparing the solutions of Equations (4) and (6), we can summarize the following.

Prediction 3 *In a Tullock contest with costly endogenous entry: (a) concealing the actual number of contestants leads to an expected total effort that is strictly greater (lower) than disclosing the actual number of contestants if and only if the cost function is strictly convex (concave); (b) (Disclosure Irrelevance) the expected total effort is independent of the prevailing disclosure policy when the cost function is linear. That is,* $TE_C^* \gtreqless TE_D^*$ *if and only if* $\alpha \gtreqless 1$.[14]

2.3 Experimental Design and Procedure

We design a 2×3 between-subjects experiment that closely follows the theoretical framework described in Section 2.2. In one dimension, we vary the disclosure policy, i.e., whether the actual number of entrants (N) is disclosed after the entry stage. In the other dimension, we set the curvature of the cost of effort function to be concave (α=2/3), linear (α=1), or convex (α=4/3). We use a lottery contest success function (i.e., $r = 1$) to make the winning probabilities easier for the experimental participants to understand. We conducted two sessions for each treatment with 24 participants per session (except for the two sessions of the *Conceal 1* treatment involving 18 and 30 participants, respectively). Table 2.1 summarizes the 2×3 design with the treatment labels and the number of participants per session (in brackets).

Printed instructions were provided and read aloud by the experimenter before the start of each experimental session (See Appendix A for an example). To further ensure

[14]This can be easily proven: $\alpha \gtreqless 1$ implies that $TE_C^*(q) \gtreqless TE_D^*(q)$ as $[\sum_{N=1}^{M} C_{M-1}^{N-1} q^{N-1} (1-q)^{M-N} \frac{N-1}{N^2} \frac{rV}{\alpha}]^{\frac{1}{\alpha}} \gtreqless \sum_{N=1}^{M} C_{M-1}^{N-1} q^{N-1} (1-q)^{M-N} \left(\frac{N-1}{N^2} \frac{rV}{\alpha}\right)^{\frac{1}{\alpha}}$.

Table 2.1: Experimental design

	Concave	Linear	Convex
Disclosure	*Disclose 2/3* (24+24)	*Disclose 1* (24+24)	*Disclose 4/3* (24+24)
Concealment	*Conceal 2/3* (24+24)	*Conceal 1* (18+30)	*Conceal 4/3* (24+24)

that all participants understood the instructions correctly, at the beginning of each session they were asked to take a quiz based on the experimental instructions. The participants could only continue if they answered each question correctly. In each session, the participants were first randomly assigned to two (sub)session groups, which remained the same for the entire experiment. Therefore, the size of the (sub)session groups was 9, 12, or 15. Any subsequent random matching was conducted at this (sub)session level.[15]

Each session ran for 25 rounds. At the beginning of each round, the participants were randomly assigned to a group of three players, each receiving 80 experimental currency (EC) units as their initial endowment. In Stage 1, the participants simultaneously decided whether to enter. An entry fee of 40 EC was deducted from their initial endowment if they decided to enter. Those who did not enter kept their 80 EC as their payoff for the round, and were not allowed to participate in Stage 2. After all participants made their entry decisions, the actual number of entrants (N) was revealed (concealed) to all participants in the disclosed (concealed) treatments, including those who did not enter.[16] In Stage 2, all entrants chose their level of effort, x, to compete in the contest with a single prize worth 100 EC. Their level of effort was then converted into cost and deducted from their remaining endowment (i.e., 40 EC). The participants were given a table listing all available levels of effort (and their corresponding costs) and a graph showing the curvature of the cost function. Depending on the cost function used in the treatment, the range of effort levels varied and was bounded by the remaining endowment after entry fee is deducted (i.e., 40 EC).[17]

[15] This manipulation served the purpose of increasing the number of independent observations.

[16] By informing those who did not enter, we kept information and learning relatively symmetric between participating and non-participating players.

[17] This corresponded to a range of effort levels from 0 to 40 when the cost function is linear, 0 to 253 when it is concave, and 0 to 15.9 when it is convex. We allowed the participants to enter decimal points in their effort choices in all treatments so that the coarseness of the strategy space is more comparable across treatments.

After all of the entrants made their effort choices, a winner was randomly selected from each contest group according to the lottery contest success function. An animated lottery wheel was used to show the process of the random draw. Once a winner was determined, all group members received full feedback information, including all group members' entry and effort choices, their corresponding winning probabilities, the outcome of the random draw, and their own payoff for this round. One of the 25 rounds was then randomly chosen by the computer at the end of each session for payment calculation. The EC earned during this round were converted to RMB at a rate of 3.2 EC = RMB1. The participants earned RMB40 on average (including RMB15 as a show-up fee) and each session lasted approximately one hour.[18] The experiment was programmed and run by z-Tree (Fischbacher, 2007). All 288 participants were undergraduate or postgraduate students in Wuhan University in the winter of 2017. At the end of each experimental session, a standard questionnaire was used to collect demographic information (such as age, sex, study major, etc.).

2.4 Results

In this section, we first compare the average entry rate, the average total effort and total cost across disclosure policies to test the main predictions presented in Section 2.2. We then examine the individual effort choices and compare them with the equilibrium predictions. Unless otherwise specified, we always refer "treatment effect" to the comparison between the *Disclosed* treatment and the *Concealed* treatment under the same effort-cost function.

2.4.1 Entry Rate

Table 2.2 summarizes the average entry rates (with standard deviations in brackets) for each treatment, in contrast with their corresponding equilibrium predictions. The average entry rates are generally higher than the equilibrium predictions. When the cost function is concave, given a predicted probability of entry of 42%, over-entry is more prominent (69% and 62% under the *Disclosure* and *Concealment* policies, respectively).

[18]This average payment was equivalent to the hourly rate paid to a university research assistant in that region when the experiment was conducted.

When the cost of effort is linear, over-entry is moderate (60% and 59% against 50% in the predictions). When the cost function is convex, over-entry is the lowest (59% and 63% against 56% in the predictions).

Table 2.2: Average entry rates and equilibrium predictions

	Concave		Linear		Convex	
	Equ.	Rd.1-25	Equ.	Rd.1-25	Equ.	Rd.1-25
Disclosure	0.42	0.69	0.50	0.60	0.56	0.59
		(0.46)		(0.49)		(0.49)
Concealment	0.42	0.62	0.50	0.59	0.56	0.63
		(0.49)		(0.49)		(0.48)

Standard deviations are reported in brackets. The columns labeled "Equ." provide the equilibrium entry probabilities. Columns "Rd.1-25" show the summary statistics of observations from all 25 rounds.

We further use regression analysis with multi-level mixed-effects models to control for the random effects at the (sub)session and individual levels. Both linear and logistic regressions are presented in Table 2.3. The average entry probabilities are not significantly different across the *Disclosure* and the *Concealment* policies for all cost structures (see the coefficients of the treatment dummy "Concealed" in columns 2 to 4). Over-entry is slightly corrected from rounds 1-13 to rounds 14-25 in both treatments with a concave cost when over-entry is the greatest (see the coefficient for "2nd_half" in column 5). After controlling for the learning effect, the treatment effect remains insignificant under all cost structures. These results on entry behavior remain unchanged in Model 3 when multi-level mixed-effects logistic regressions are used.[19] Similar to the previous studies, our data also suggest that those who claimed to be more risk-loving also participated more frequently, and those who won in the previous round were more likely to participate in the next round.

[19] Adding other control variables to linear regressions does not change our main results either.

Table 2.3: Entry behavior: Regression analysis

	Mixed-effects Linear Regressions						Mixed-effects Logit Regressions		
	Model 1			Model 2			Model 3		
VARIABLES	Concave	Linear	Convex	Concave	Linear	Convex	Concave	Linear	Convex
Constant (Disclosed)	0.69***	0.60***	0.59***	0.72***	0.62***	0.59***	-1.41**	-1.80***	-1.59***
	(0.05)	(0.05)	(0.04)	(0.05)	(0.05)	(0.04)	(0.63)	(0.51)	(0.43)
Concealed	-0.07	-0.02	0.04	-0.07	-0.02	0.06	0.02	-0.32	0.26
	(0.06)	(0.07)	(0.06)	(0.07)	(0.07)	(0.06)	(0.46)	(0.44)	(0.39)
Second_half				-0.06***	-0.03	-0.01	-0.36**	-0.20	-0.06
				(0.02)	(0.02)	(0.02)	(0.16)	(0.16)	(0.15)
Conceal × Second_half				-0.00	0.00	-0.03	-0.08	0.12	-0.18
				(0.03)	(0.03)	(0.03)	(0.23)	(0.23)	(0.21)
Risk							0.49***	0.56***	0.44***
							(0.10)	(0.09)	(0.07)
Male							-0.05	-0.48	0.17
							(0.48)	(0.41)	(0.33)
Win_{t-1}							0.94***	0.93***	0.79***
							(0.15)	(0.13)	(0.13)
σ^2_{group}	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.07	0.07
	0.00	0.00	0.00	0.00	0.00	0.00	(0.00)	(0.19)	(0.14)
$\sigma^2_{individual}$	0.95	0.98	0.08	0.95	0.98	0.08	4.18	3.04	2.04
	(0.01)	(0.15)	(0.13)	(0.01)	(0.02)	(0.13)	(0.89)	(0.67)	(0.44)
Observations	2,400	2,400	2,400	2,400	2,400	2,400	2,304	2,304	2,304
Number of groups	8	8	8	8	8	8	8	8	8

We run multi-level mixed-effects regressions on binary entry choices as a function of the treatment dummy ("Concealed") in Model 1, controlling for the random effects at both the individual and the (sub)session levels. The *Disclosed* treatment under each cost structure is used as the baseline category. In Model 2, we add a time-specific dummy variable (2nd_half) to identify learning from rounds 1-13 to rounds 14-25. In Model 3, we run logistic regressions, adding other individual characteristics that may affect the participants' entry decisions. "Risk" is a self-reported measure of willingness to take risks in everyday life, which takes integers between 0 and 10, with 0 being "Not willing to take risks at all" and 10 being "Very willing to take risks." "Win_{t-1}" is a binary variable that is equal to 1 if the participant won in the previous round, and 0 otherwise. All standard errors are reported in brackets. Stars indicate the significance level (**$p < 0.05$, ***$p < 0.01$).

We then test the estimated average entry rates against their corresponding equilibrium predictions to establish the significance of the differences between the actual entry probabilities and the equilibrium predictions. Over-entry is significant in all treatments with the concave cost function at the 1% level even if we only examine the entry choices in rounds 14-25, but not significant at the conventional 5% level in other treatments with a linear or convex cost. [20]

Result 1 *The entry behavior of the participants is largely invariant to the disclosure policies, whether the cost of effort is concave, linear, or convex.* (*supporting Prediction 1*)

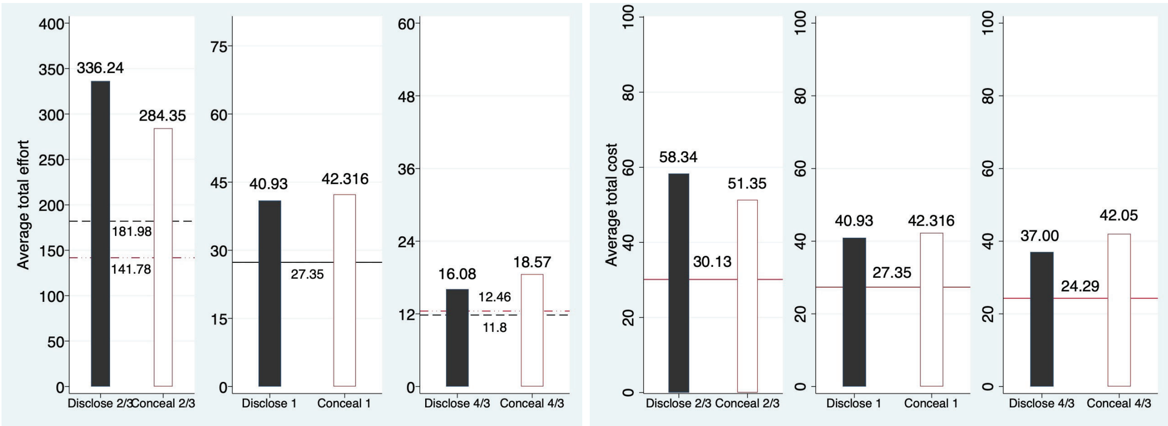
2.4.2 Total Effort and Total Cost

After examining the average entry rates across the disclosure polices, we compare the average total effort to identify the optimal disclosure policy that a contest organizer should adopt. The average total effort is taken as one unit of observation in this part of the analysis and it is calculated as the aggregate effort made by each three-player contest group averaged across all contest groups within a sub(session) in one round. Figure 2.1a presents the summary statistics of the average total effort. The corresponding equilibrium predictions are also represented by the dashed lines. Figure 2.1a suggests that the differences between the *Disclosure* and *Concealment* policies follow the direction of the theoretical predictions, although the average total effort is generally higher than the equilibrium predictions. When the cost function is concave, the average total effort is 18% higher under the *Disclosure* policy than under the *Concealment* policy (336.24 vs. 284.35). In contrast, when the cost of effort is convex, the average total effort in the disclosed treatment is 13% lower than that of the concealed treatment (16.08 vs. 18.57). When the cost function is linear, the difference between treatments is the smallest of all (40.93 vs. 42.32).

We again estimate the mixed-effects models to control for the random effects at the (sub)session level, and thereby to further test the significance of the treatment effects. The regression results are presented in Table 2.4. In the concave- and linear-cost cases, the regression results further confirm what we observe in Figure 2.1a: the average

[20] The p values are 0.00 and 0.00 for the concave case, 0.07 and 0.14 for the linear case, and 0.58 and 0.35 for the convex case, based on Wald tests.

Figure 2.1: A summary of the average total effort and the average total cost, in contrast to their equilibrium predictions.



(a) Average total effort

(b) Average total cost

total effort is significantly lower under the *Concealment* policy when the cost function is concave ($p = 0.036$), and is not significantly different across disclosure policies when the cost function is linear ($p = 0.70$). In the convex-cost case, although the direction of the effect is in line with the theoretical prediction, it is not statistically significant ($p = 0.21$). These results remain consistent even after we add a time dummy to control for the potential learning effect (see Model 2 in Table 2.4).

Result 2 *The average total effort is significantly higher under the* Disclosure *policy than under the* Concealment *policy when the cost of effort function is concave, and it is insensitive to the disclosure policy when the cost of effort is linear. The treatment effect is not significant when the cost function is convex, although the direction of the effect follows Prediction 3.* (*partially supporting Prediction 3*)

Note that *Prediction 2* also states that the total cost of effort should be invariant to the disclosure policies, and the same applies to the rent-dissipation rate. Similarly, We take the average total cost (of effort) that is calculated in the same way as the average total effort, as one unit of observation. Figure 2.1b summarizes the average total cost of effort for each treatment. The solid lines represent the corresponding equilibrium predictions. In general, moderate levels of over-dissipation occur across all treatments (in line with the higher than equilibrium levels of total effort), yet the treatment effects seem small for all cost structures.

Table 2.4: Total Effort: Mixed-effects Models

	Model 1			Model 2		
	Concave	Linear	Convex	Concave	Linear	Convex
Constant (Disclosed)	336.24***	40.93***	16.08***	357.09***	45.12***	16.22***
	(17.46)	(2.13)	(1.40)	(18.74)	(2.43)	(1.49)
Concealed	-51.89**	1.38	2.49	-53.61**	-0.18	3.09
	(24.70)	(3.01)	(1.98)	(26.51)	(3.44)	(2.11)
2nd_half				-43.44**	-8.73***	-0.31
				(14.17)	(2.46)	(1.06)
Concealed× 2nd_half				3.59	3.25	-1.27
				(20.05)	(3.48)	(1.50)
$\sigma^2_{(sub)session}$	1,001.42	11.49	6.73	1,019.49	12.04	6.74
	(610.39)	(9.07)	(3.93)	(610.39)	(9.07)	(3.93)
Obs	200	200	200	200	200	200
No. of groups	8	8	8	8	8	8

We compare the average total effort across disclosure policies by regressing the average total effort on the disclosure policy dummy ("Concealed") for different cost functions separately in Model 1. In Model 2, we add a time-specific dummy (2nd_half) that is equal to 1 for rounds 14-25. We use mixed-effects regressions to control for the random effects at the (sub)session level. Stars indicate the significance level (**$p < 0.05$, ***$p < 0.01$).

Following the same investigation logic as the average total effort, we run mixed-effects regressions, controlling for random effects at the (sub)session group level for the average total cost. The results are presented in Table 2.5. In line with the theoretical predictions, we find that the average total costs are not significantly different across disclosure policies when the cost function is linear and convex ($p = 0.46$ and $p = 0.28$, respectively). When the cost function is concave, the difference is marginally significant in Model 1 ($p = 0.065$). However, after controlling for the learning effect by adding a time-specific dummy in Model 2, we find that the difference becomes insignificant in rounds 14-25 at all conventional levels ($p = 0.107$).[21]

Result 3 *In line with Prediction 2, the average total cost is invariant to the disclosure policies under all cost structures after some learning from the early rounds (1-13).*

The total cost spent by the contestants is essentially borne by the contest organizer, as the total cost of effort spent by the participants must be offset by the prize value

[21] To test the difference in rounds 14-25, we conduct a post-estimation test by comparing the sum of the regression coefficients of the treatment dummy (Concealed) and the interaction term (Concealed × 2nd_half) with zero.

Table 2.5: Total Cost: Mixed-effects Models

	Model 1			Model 2		
VARIABLES	Concave	Linear	Convex	Concave	Linear	Convex
Constant (Disclosed)	58.34***	40.93***	37.01***	62.02***	45.12***	37.21***
	(2.68)	(2.13)	(3.30)	(2.90)	(2.43)	(3.52)
Concealed	-6.99	1.38	5.04	-7.26	-0.18	6.71
	(3.79)	(3.01)	(4.66)	(4.10)	(3.44)	(4.97)
2nd_half				-7.66***	-8.73***	-0.41
				(2.33)	(2.46)	(2.54)
Conceal × 2nd_half				0.57	3.25	-3.48
				(3.29)	(3.49)	(3.59)
$\sigma^2_{(sub)session}$	22.72	11.49	36.99	23.29	12.04	37.07
	(14.36)	(9.08)	(21.77)	(14.36)	(9.07)	(21.77)
Obs	200	200	200	200	200	200
No. of groups	8	8	8	8	8	8

We compare the average total cost across disclosure policies by regressing the average total cost on a disclosure policy dummy ("Concealed") for each cost function separately in Model 1. In Model 2, we add a time-specific dummy (2nd_half) equal to 1 for rounds 14-25. We use mixed-effects regressions to control for the random effects at the (sub)session level. Stars indicate the significance level (** $p < 0.05$, *** $p < 0.01$).

provided by the organizer. Integrating the results on the average total effort and the average total cost, our study suggests that if the objective of a contest organizer is to maximize the total effort while maintaining the cost of the contest, she should select the disclosure policy after carefully considering the nature of the effort-cost function. When the cost of effort is expected to be concave, fully disclosing the actual number of contestants is expected to be more effective in eliciting total effort.[22] When the cost of effort is expected to be linear, the choice is irrelevant, as the expected total effort and the expected total cost are both unresponsive to the disclosure policies. When the cost of effort is expected to be convex, our data may be lack of power in detecting the treatment effect, but ignoring the predicted differences across disclosure policies based on our theoretical model may be sub-optimal. [23]

[22]Naturally, if the objective of the contest organizer is to minimize the expected total effort, the optimal policy is the opposite.

[23]In the current paper, we focus on the case that contest organizer value the total effort exerted from the contest. There are other cases where minimizing the total cost of the effort is the only goal. Our result on total cost of effort implies that in the design of contests under such context, we don't need to consider the curvature of cost of effort or the disclosure policy.

2.4.3 Individual Effort

The participants in the *Disclosed* and *Concealed* treatments make their effort choices under different levels of information. In the *Disclosed* treatments, they know how many people they are competing against, while in the *Concealed* treatments, this information is not available, thus they must form a belief on the entry strategy of other people. In the former case, the equilibrium effort choices change with the actual number of entrants. We first compare the individual efforts exerted in different cases (i.e., $N = 1$, 2, or 3) in the *Disclosed* treatments to see if they follow the equilibrium predictions. In the latter case, not knowing the actual number of entrants, the contestants are predicted to make the same level of effort across different sizes of contests.[24] We then check if this is true and also compare the actual effort choices with the corresponding equilibrium predictions.

N is Disclosed

Table 2.6: Individual Effort in Disclosed Treatments

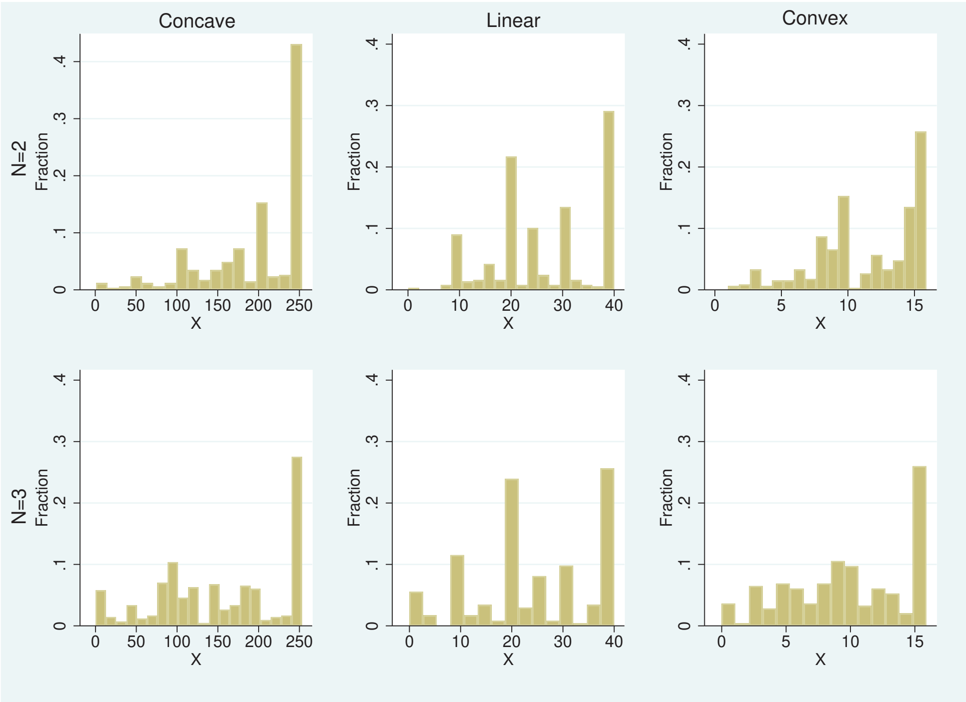
	Concave		Linear		Convex	
	Equ.	Rd.1-25	Equ.	Rd.1-25	Equ.	Rd.1-25
N=1	0	23.33	0	5.02	0	0.86
		(70.62)		(11.85)		(3.19)
N=2	229.27	197.48	25	26.76	9.01	11.64
		(63.33)		(10.39)		(3.95)
N=3	192.45	156.05	22.22	24.33	8.25	9.91
		(79.11)		(12.06)		(4.59)

Standard deviations are reported in brackets. The columns labeled "Equ." provide the equilibrium predictions. "Rd.1-25" show the summary statistics of observations from all 25 rounds.

Table 2.6 reports the average individual efforts in each of the three *Disclosed* treatments, conditional on the actual number of entrants, in contrast with the corresponding theoretical predictions of their behavior. When the participants enter the contest alone ($N = 1$), they should make 0 effort in all three treatments, as the prize is automatically awarded to them. The summary statistics show that some participants still make a small but positive effort, which may be driven by some confused participants not

[24]Formal proof of contestants' equilibrium effort level in both cases can be found in Appendix A.2

Figure 2.2: Histograms of individual effort in Disclosed treatments



fully understanding the rule of the contest at the beginning. Multi-level mixed-effects regressions using data from rounds 14-25 to study the choice of the participants after learning show that the average effort in this case is small and not significantly different from 0 (See Table 2.7, row "$N = 1$"). When more than one contestant enters the contest, their effort choices are in line with the equilibrium predictions when the cost function is linear (comparing columns 4 and 5 in Table 2.6). Similarly, mixed-effects regressions using data from the last 12 rounds provide estimates that are not significantly different from the equilibrium predictions (see Table 2.7 column 3).

When the cost function is non-linear, we observe that the individual effort is slightly lower than the equilibrium predictions when the cost function is concave (197.48 vs. 229.27 when $N = 2$, and 156.05 vs. 192.45 when $N = 3$). However, when the cost function is convex, the average effort is slightly above the equilibrium predictions (11.64 vs. 9.01 when $N = 2$, and 9.91 vs. 8.24 when $N = 3$). These results suggest that the effort choices respond to the curvature of the cost function in the right direction, although the reactions are slightly smaller than those suggested by the theoretical point predictions. Table 2.7 (columns 2 and 4) shows that these deviations from the equilibrium predictions are statistically significant when using mixed-effects regressions.

Table 2.7: Individual Effort in Disclosed Treatments: Rounds 14-25

	Mixed-effects model			Tobit model		
	Concave	Linear	Convex	Concave	Linear	Convex
N=1	12.73 (16.17)	1.42 (1.79)	0.73 (0.58)			
N=2	198.80** (13.10)	25.93 (1.54)	12.07*** (0.50)	242.60 (29.92)	28.49 (2.85)	13.22*** (0.80)
N=3	157.00*** (13.01)	22.63 (1.64)	9.57** (0.51)	183.90 (29.62)	24.56 (2.92)	10.34** (0.81)
$\sigma^2_{(sub)session}$	450.65 (452.61)	4.06 (6.28)	0.00 (0.00)	2,810 (2,483)	16.64 (22.61)	0.00 (0.00)
$\sigma^2_{individual}$	1,506.05 (448.04)	35.90 (11.48)	7.57 (1.94)	5,062 (1,626)	125.8 (38.92)	22.28 (6.01)
Censored				[10, 113]	[4, 60]	[3, 65]
Total Obs.	379	340	337	344	278	274

"Censored" represents the number of observations that are left-censored (first number in brackets) and right-censored (second number in brackets). We control for the random effects at the individual level ($\sigma^2_{individual}$) and the (sub)session level ($\sigma^2_{(sub)session}$) in the mixed-effects and Tobit models. Stars indicate the significance level of the estimated coefficients against their corresponding theoretical predictions (** $p < 0.05$, *** $p < 0.01$). Standard errors are reported in brackets.

The histograms of the individual effort choices presented in Figure 2.2 suggest that the effort choices are heterogeneous and often limited by the maximum effort available to the participants. For example, approximately 40% of the choices are distributed at the upper bound (253) when the cost function is concave, and the actual number of contestants is two. In other cases, between 25% and 30% of the choices are distributed at the upper bound. To take into account the effort choices bounded by the choice space, we also estimate the average individual effort in each case with multi-level mixed-effects Tobit models, using data from rounds 14-25. The results are presented in Table 2.7 (columns 5 to 7).[25] As Table 2.7 shows, the effort choices are not significantly different from the equilibrium predictions when the cost function is concave or linear, while the level of effort remains higher than the equilibrium predictions when the cost function is convex.

Result 4 *The participants' individual effort largely follows the equilibrium predictions when N is disclosed, although the participants slightly under-react to the curvature of the cost function*

[25] All of the regressions using Tobit models are right-censored at the maximum effort level available to the participants, and left-censored at the minimum level of 0. The case $N = 1$ is left out because most participants choose 0 when nobody competes with them.

when the cost of effort is non-linear, especially when it is convex.

N is Concealed

Table 2.8: Individual Effort in Concealed Treatments

Concave		Linear		Convex	
Equ.	Rd.1-25	Equ.	Rd.1-25	Equ.	Rd.1-25
117.97	154.12 (71.54)	18.12	23.78 (12.11)	7.42	9.88 (4.59)

Standard deviations are reported in brackets. The columns labeled "Equ." provide the equilibrium predictions. "Rd.1-25" show the summary statistics of observations from all 25 rounds.

Table 2.8 summarizes the average individual effort, with the standard deviations for each *Concealed* treatment given in brackets. The average individual effort is generally higher than the equilibrium predictions in all treatments when the actual number of contestants is concealed. To compare the individual effort with the equilibrium predictions, we estimate the average individual effort using multi-level mixed-effects models, controlling for the random effects at the individual and (sub)session levels. The estimates are significantly higher than the equilibrium predictions in all three treatments (p values are 0.00, 0.05, and 0.00, see Figure 2.3 for the estimated values.)[26]

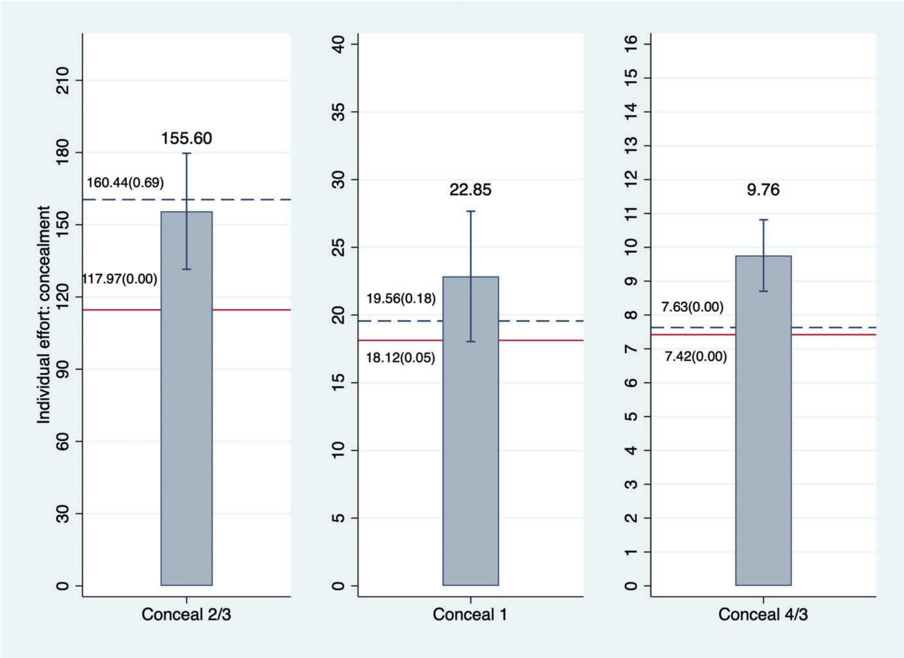
Under the *Concealment* policy, the participants make effort decisions without knowing the actual number of entrants. They should choose an effort level that maximizes their expected payoff, taking into account the probability distribution of the potential number of opponents they will face.[27] As the actual entry rates are slightly higher than the equilibrium entry rates, the probability distribution of the actual number of opponents they will face also deviates from the equilibrium predictions. Thus, the over-exertion observed under the *Concealment* policy may be a rational reaction to over-entry.

In our experiment, the participants received full feedback at the end of each round, irrespective of whether they entered the contest. As a result, the participants should have had a good perception of the actual average entry rates. We conjecture that the

[26]The full regression results using data from rounds 14-25 can be found in Appendix C.1. The estimated coefficients are similar if we use data from all rounds.

[27]We draw the histogram of individual effort in concealment treatments conditional on cost function and the number of entrants. Participants effort choice does not vary with the number of entrants. See Appendix C for the histogram.

Figure 2.3: Individual Effort in Concealed Treatments: Rounds 14-25.



Note: The red solid lines represent the equilibrium predictions and the blue dotted lines represent the predicted optimal individual effort, taking the actual entry rates as given. The figures in brackets next to the theoretical predictions are the p-values of the Wald tests, which compare the estimated coefficients using multi-level mixed-effects models for rounds 14-25 (see the number at the top of each bar) with the predictions adjusted by over-entry.

over-exertion observed may reflect the best responses of participants to their belief/perception of the actual entry behavior. To further test this conjecture, we calculate the payoff maximization effort based on the observed average entry rates using data from rounds 14-25. As shown in Figure 2.3, the estimated average individual effort in the concave and linear treatments (155.60 and 22.85) is not significantly different from the optimal effort choices predicted by the actual average entry rates indicated by the blue dashed lines (160.44 and 19.56, p =0.69 and p=0.18, respectively). However, when the cost of effort is convex, the estimated average effort is still significantly higher than the predicted adjusted optimal effort (9.76 vs. 7.63, $p \leq 0.01$).[28]

Result 5 *The participants' individual effort is generally higher than the equilibrium predictions in all treatments when the actual number of contestants is concealed. However, when considering over-entry, the effort choices are largely consistent with the predictions adjusted by*

[28]Note that over-entry is minimal when the cost of effort is convex. Therefore, controlling for over-entry hardly changes the optimal effort predicted compared with the Nash equilibrium prediction (7.63 vs. 7.42).

over-entry, except that there is still over-exertion when the cost function is convex.

In summary, our analysis on individual efforts suggests moderate levels of over-exertion when the cost of effort is convex, contrasting the majority of the experimental evidence on contest that shows much higher levels of over-exertion.[29] One could reasonably argue that the closer to equilibrium effort choices observed in our experiment might be driven the fact that the strategy space is bounded by the initial endowment assigned to the participants.[30] As is shown by the Tobit regressions, the strategy space is particularly restrictive under the *Disclosure* policy when the cost of effort is concave, however, it restricts the effort choices to a lesser extent under the *Concealment* policy. Therefore, we speculate that should the participants be allowed to exert higher effort, the treatment difference on the expected total effort would have been more prominent when the cost of effort is concave. When the cost function is convex, the difference in equilibrium-effort predictions across disclosure policies is very small, the bounded strategy space would be unlikely to affect the treatment effect significantly different.[31] All in all, in spite of the various deviations observed on the individual level of choices, our finding on the treatment effect with regard to the average total effort (summarized in Result 2) remains unchanged.

2.5 Conclusion

In this paper we theoretically and empirically examine whether a contest organizer should disclose the actual number of contestants when entry is endogenous. Although previous studies suggest that the expected total effort made in a Tullock contest does not depend on the disclosure policy when the cost of effort is linear, we theoretically show that the optimal disclosure policy essentially depends on the convexity of the cost function in Tullock contests when the cost of effort is non-linear, although the equilibrium entry probabilities and rent dissipation rates are invariant to the disclosure

[29]We have shown that the over-exertion in the concealed treatments (when the cost of effort is either linear or concave) may be driven by individuals best-responding to over-entries in these treatments.

[30]For example, Baik et al. (2020) provide experimental evidence on the impact of conflict budget on the intensity of conflict.

[31]The bounded strategy space is to ensure the entry cost is non-trivial and the endowment is comparable across cost structures. Obviously, restricted strategy space comes as its cost.

policy.

Our experimental results using a lottery contest function provide good qualitative support for our main theoretical predictions. We find that although the actual entry rates slightly deviate from the equilibrium predictions, the entry behavior of the participants is not significantly different under the *Disclosure* and *Concealment* policies, whether the cost of effort function is concave, linear, or convex. Over-entry may be driven by the usual disadvantages of a laboratory experiment, that is, the participants may get bored by not participating and doing nothing during the experiment. We also find that, consistent with our theoretical predictions, the total cost and thus the rent dissipation rates are invariant to the disclosure policy under all cost structures.

More importantly, we find that the *Disclosure* policy tends to elicit more total effort than the *Concealment* policy when the cost function is concave, and that the total effort is neutral to the disclosure policy when the cost function is linear, both of which are in line with our predictions. When the cost function is convex, although we observe that the *Concealment* policy leads to a higher average total effort, the difference is not statistically significant. The lack of significance may be attributed to two factors: first, the predicted treatment effect is rather small, thus it is more difficult to detect with our data, and second, relatively large standard errors in our sample prevent the results from being significant.

Our paper is the first to provide insights to contest organizers on the optimal information disclosure policy they should use when the cost of effort is non-linear and entry is endogenous. Our main results have important implications for the design of contests, as endogenous entry is widely observed in the field and the cost of effort is often non-linear. We argue that in contest environments, such as job applications and college entrance exams, the cost of effort tends to be concave, while in sports contests or R&D races, it tends to be convex. Further empirical research is warranted to identify the nature of the cost of effort function in practice when a specific real contest is concerned. Future studies are also needed to further explore the convex-cost case. In this paper, we have focused on full disclosure and full concealment policies. Although we expect that our main results can be extended to a more general information disclosure policy framework, new issues related to information disclosure may arise and create additional challenges for analysis. We leave these interesting questions for future work.

Chapter 3

Risk taking in rank-order tournaments

3.1 Introduction

In a rank-order tournament, a contestant can increase her probability of winning by either investing more effort or increasing the riskiness of her strategy. There are many examples in the real world where agents in tournaments only have a restricted level of effort: companies in a R&D race face limited time and resources; candidates in a promotion contest have ability boundaries in the short run; and fund managers whose compensation largely depends on their ranking among peers can only access a certain amount of funds. In these scenarios, even though their expected performances are limited by capacity constraints (resources, ability, money, etc.), contestants can flexibly choose the level of risks taken in their strategies to increase their expected payoff in the tournament.

In this paper, we investigate how the design of rank-order tournaments affects contestants' risk-taking behavior using a laboratory experiment. Following the seminal paper of Lazear and Rosen (1981), most theoretical and experimental work on rank-order tournaments examine how the design of the tournament affects contestants' effort choice (Ehrenberg and Bognanno, 1990; Orrison et al., 1997; Moldovanu and Sela, 2001; Harbring and Irlenbusch, 2008), while much less attention has been given to risk-taking behavior. However, extensive empirical literature, especially after the Global Financial Crisis (GFC), finds that the tournament incentive can drive excessive risk-taking and large volatility on the financial market (Diamond and Rajan, 2009; Kini and Williams, 2012; Coles et al., 2018; Kirchler et al., 2018). While empirical studies provide great insight, the types of tournament under examination are limited by data availability. More importantly, we can only observe agents' realized performance levels, but not their choice of the performance distribution. The pressing need to understand the effects of different tournament designs on contestants' risk-taking behavior demands more evidence from experimental studies.

There are a few studies that investigate risk-taking behavior experimentally. Gaba and Kalra (1999) compare rank-order tournaments and the quota-based compensation system using data from an experiment that mimics the sales force competition. Nieken and Sliwka (2010) study how the correlation among the contestants' realized performance affects contestants, risk-taking behavior. Eriksen and Kvaløy (2014) examine how myopia affects the risk-taking in the tournament while Eriksen and Kvaløy (2017) vary the competitiveness of the tournament to examine excessive (more than optimal) risk-taking. In these studies, contestants' risk choice is often regarded as a choice of variance. Consequently, the probability distribution of performance is treated as symmetric. Nevertheless, a recent study by Fang and Noe (2016) finds that the theoretical equilibrium performance distribution in rank-order tournaments is only symmetric when the prize schedule is symmetric.[1] When the prize schedule is convex (concave), contestants will choose a positively (negatively) skewed distribution.[2] This result indicates that in the most commonly studied tournaments, such as a *winner-takes-all* tournament or *elimination contest*, confining the risk choice to be symmetric cannot fully reveal contestants' risk-taking behavior.

Neglecting the skewness of the distribution can be hazardous from the risk governance perspective. The pay gap between fund managers with different rankings is usually significant and considered convex (Brown et al., 1996).[3] If fund companies believe the managers' risk strategy is symmetric, they would assume a small probability for

[1]We call a set of prizes ,$v_1 \leq v_2 \leq v_3 \leq ..., \leq v_n$, a prize schedule. For example, for a six-prize prize schedule, symmetric means $v_6 - v_5 = v_2 - v_1, v_5 - v_4 = v_3 - v_2$.

[2]The convexity of the prize schedule is the second difference of the prize schedule. For instance, $v_1 = 0, v_2 = 0, v_3 = 0, v_4 = 100$ is a convex prize schedule because the second differences of the prize schedule are 0 and 100, which are non-negative. $v_1 = 0, v_2 = 40, v_3 = 40, v_4 = 40$ is considered a concave prize schedule because its second differences are -40 and 0, which are non-positive.

[3]The best fund managers obtain not only the highest compensation but also improve client resources which is considered even more valuable. In contrast, the worst fund managers cannot lose more than they have no matter how badly they perform

both good and bad outcomes, but a substantial probability of observing mediocre performance. However, theory predicts that fund managers under a convex prize schedule will adopt a positively skewed distribution, which leads to either very good outcomes with a small probability or bad outcomes with a considerable probability. Therefore fund companies and even the financial market can face greater-than-expected overall volatility. In terms of social welfare, depending on the economic context of the contest, the welfare effect of skewness in performance can be either beneficial or disastrous.[4]

To investigate risk-taking behavior in the tournament without any assumption on the shape of the performance distribution,[5] we design an experiment where contestants can use a visualized interactive distribution builder to choose their risk strategies. Participants can build any distribution over non-negative potential performance levels as long as the capacity constraint is satisfied. Based on the model developed by Fang and Noe (2016), we vary the convexity of the prize schedules used in the tournament and the size of the tournament in the experiment to examine contestants' risk-taking behavior under different contest designs.

Our experimental results strongly support the convexity effect: contestants chose the distribution that is not only more dispersed but also more (positively) skewed with the increase in convexity of prize schedule. Intuitively, a more convex prize schedule indicates that moving forward one rank on the top of the prize schedule is more profitable than moving forward one rank at the bottom of the prize schedule. Hence, contestants have stronger incentives to stretch out the upper bound and fight for the highest prize. Consequently, they will choose a more positively skewed and more dispersed performance distribution.

We also find that the effect of tournament size on contestants' risk-taking behavior depends on how the tournament size is changed. When increasing the tournament size by adding more entrants (namely, entrant effect), contestants will increase their risk-taking by choosing a more dispersed distribution as predicted by the theory. However, when increasing the size of the tournament by multiplying the number of contestants

[4] Relevant literature includes but not limited to Dasgupta and Stiglitz (1980), Klette and De Meza (1986) and Brown et al. (2008).

[5] In this paper, the performance distribution needs to be non-negative. We do not limit the shape of the performance distribution as long as it is non-negative.

and the number of prizes by s (namely, scale-up), we observed that participants increase the riskiness of their strategy as the theory predicted; however, the effect is not statistically significant. These two methods of changing the size of the tournament affect the dispersion of the distribution in different ways. The entrant effect encourage contestants to stretch out the upper bound of the distribution in order to surpass more contestants, while the scale-up effect encourages contestants to put more probability mass on certain jumping points so that contestants can move up to a higher rank that actually increases the payoff.

Past literature that examines the changes in tournament design mostly focus on changing the proportion of winner prizes in a simple contest,[6] which is a special case of changing the convexity of prize schedule (Dekel and Scotchmer, 1999; Gaba and Kalra, 1999; Gaba et al., 2004; Kräkel and Sliwka, 2004; Schedlinsky et al., 2016). Our experiment can be seen as complementary to this branch of study. We also include this variation as a special case and extend previous findings in a simple contest to a broader range of prize schedules that are more complex. Gilpatric (2009) and Andersson et al. (2020) also study the risk-taking behavior under more complex prize schedules. However, unlike our paper, they assume risk-taking is costly and examine the case where participants can choose both the effort level and the riskiness of their strategy. Most importantly, we show that the convexity effect does not only increase the dispersion of the distribution, but also shifts the skewness of the distribution. To the best of our knowledge, our study is the first experiment that is able to show the change in the skewness of the distribution.

There are very few papers that study risk-taking under various group sizes.The few papers that do investigate this problem focus on the entrant effect. Eriksen and Kvaløy (2017) find in their experiment that adding more entrants to the tournament will induce even more excessive (non-rational) risk-taking. Hvide and Kristiansen (2003) study a model where contestants are asymmetric in ability and find that riskiness of contestants' strategy only increases when the number of contestants is small. In the most recent studies, List et al. (2020) and Drugov and Ryvkin (2020) discuss different risk distributions and the tournament size. However, instead of treating risk as a decision

[6] A simple contest is a type of contest that only has two distinct prize values, i.e., prizes with higher (lower) value are the winner (loser) prizes in a simple contest.

variable, they assume risk distribution is exogenous and study how the number of contestant affects the effort based on varying the risk distributions. Although whether to combine a few small tournaments into a grand tournament is also something the contest organizer might need to consider, surprisingly, there is no relevant experimental study on scaling-up the contest as far as we know. Fang and Noe (2016) and Fang et al. (2018) are the only two theoretical papers which discuss this effect.

Apart from the aforementioned literature, our paper is also related to the extensive empirical and experimental literature that studies risk-taking when contestants are asymmetric in status (Taylor, 2003; Grund and Gürtler, 2005; Kempf et al., 2009; Nieken and Sliwka, 2010; Genakos and Pagliero, 2012; Grund et al., 2013; Hopkins, 2018), the literature examining the how different tournament designs affect the stability of financial markets (Palomino and Prat, 2003; Fang et al., 2017), and the literature on tournaments and destructive activities (Harbring and Irlenbusch, 2008; Faravelli et al., 2015).

The remainder of the paper proceeds as follows, Section 3.2 briefly reviews the theory model and equilibrium, then moves to the experimental design, main predictions, and experiment implementation. Section 3.3 elaborates the findings of this study and Section 3.4 concludes.

3.2 Experimental framework

In tournaments, participants can typically choose both a level of average performance and a level of risk-taking. These two choices are usually not independent. Very risky strategies may for instance be associated with lower performance on average but also a greater chance of having an extremely good performance.[7] Since the link between average performance and performance distribution is unclear and likely specific to different tournament setting, it is useful to look at a simplified situation where we isolate risk-taking decisions from the concerns about average performance. We follow here Fang and Noe (2016)'s approach looking at a tournament where contestants do not

[7] It is the case when contestants attempt to achieve a very difficult level of performance with the final outcome being either a success or a failure. More ambitious targets can be associated with a lower expected outcome but also with a better outcome in case of success.

face a trade-off between risk-taking and average performance. Instead, players choose the distribution of their performance, taking the average performance as given.

3.2.1 Model

Our experiment design follows Fang and Noe (2016)'s formal framework. We present here this framework and its predictions about equilibrium play. N contestants compete for N prizes which ranked as: $0 \leq v_1 \leq v_2 \leq ... \leq v_{N-1} \leq v_N$ in an rank-order tournament. Participants compete by choosing a probability distribution, $F(\cdot)$, over non-negative performance levels subject to an upper bound on the expected performance level:

$$\int_0^\infty x dF(x) \leq \mu. \tag{3.1}$$

The parameter μ represents the average performance that the chosen distribution of outputs must not exceed.

The prize allocation is based on the rank of the realized performance levels with the highest performance receiving v_N, the second highest, v_{N-1}, and so on. Ranks are randomly decided in case of a tie.

The main result of Fang and Noe (2016) shows that this taking game has a unique equilibrium that is symmetric. This equilibrium distribution can be characterised by its quantile function[8]

$$Q_v(p) = \frac{\mu N}{V} \sum_{i=0}^{N-1} (v_{i+1} - v_1) C_{N-1}^i p^i (1-p)^{N-1-i}, \tag{3.2}$$

where $V = \sum_{i=1}^{N} (v_i - v_1)$ is the sum of real gains available in the contest.[9] From equation 3.2 it is clear that the equilibrium performance distribution is influenced both by the prize schedule and by the number of participants (N).

Using this framework, we investigate the effect of three types of variations in contest design which are predicted to affect the risk taking of participants: inequality in the

[8]See Theorem 1, Corollary 1, and preceding lemmas in Fang and Noe (2016). The quantile function is the inverse of equilibrium performance distribution $F(\cdot)$.

[9]The real gain of a prize is the amount in excess of v_1, the prize for having the lowest performance.

distribution of prizes, competitiveness, and scale.

Definition 1 (Inequality) *Let $v = (v_1, \dots, v_n)$ and $v' = (v'_1, \dots, v'_n)$ be two prize schedule, and $v^r = (0, v_2 - v_1 \dots, v_n - v_1)$ and $v^{r\prime} = (0, v'_2 - v'_1 \dots, v'_n - v'_1)$ be the vector of real gains from these two prize schedule. Let V and V' be the total real gains associated with v and v'. The prize schedule v' is said to be more unequal than v if the Lorenz curve of $v^{r\prime}$ lies below the Lorenz curve of v^r in the sense that:*

$$\sum_{i=1}^{k} \frac{v^{r\prime}}{V'} \leq \sum_{i=1}^{k} \frac{v^r}{V}, \quad \forall k \in \{1, \dots, n-1\}, \quad \text{and} \quad \sum_{i=1}^{n} v^{r\prime} = \sum_{i=1}^{n} v^r$$

A more unequal prize schedule is a distribution of prizes where the gains above the minimum prizes are more concentrated toward the higher rank of the tournament. One aspect which makes a prize schedule more or less unequal is its convexity.

Definition 2 (Convexity) *Let $\Delta v_i = v_{i+1} - v_i$. A prize schedule is convex if $\Delta v_j \geq \Delta v_i$ whenever $j \geq i$. A prize schedule is concave if $\Delta v_j \leq \Delta v_i$ whenever $j \geq i$. Furthermore, keeping the number of prizes in the schedule unchanged, a prize schedule v^* is more convex than prize schedule v, if $\Delta v^*_j / \Delta v^*_i \geq \Delta v_j / \Delta v_i$, whenever $i \leq j$.*

A prize schedule which is more convex than another one is also more unequal.

Proposition 1 (Inequality and risk taking) *Inequality influences the dispersion and skewness of the performance distribution.*

i. Tournaments with more unequal prize schedules induce more dispersion in performance (measured in Gini Mean Difference, GMD).

ii. Tournaments with more convex prize schedules also induce a greater skewness in performance.

This proposition comes from propositions 5, 7 and 10 in Fang and Noe (2016). Inequality in gains increases the dispersion of performance, measured with the Gini mean difference (GMD). The GMD is the weighted average absolute difference of any two values drawn from a probability distribution, where the weight is the probabilities of these two values to be drawn at the same time.[10]

When the prize schedule is convex, the prize differences among higher ranks is larger than the prize differences among the lower ranks. Contestants have stronger incentives to stretch out the upper bound of the distribution to fight for the the higher prize. Due to the limit of capacity, contestants have to choose a positively skewed distribution in order to reach a relatively high performance level on the upper bound. When the prize schedule is concave, the prize differences among lower ranks is larger than the prize differences among the higher ranks. Contestants have stronger incentives to put a large probability mass on intermediate performance levels to avoid ending up last. Consequently they should choose a negatively skewed distribution. When the prize schedule is linear, or more generally, as long as the prize differences are symmetric in the low-rank range and the high-rank range, contestants should distribute their probability mass symmetrically along the performance levels.

Another aspect which influences risk taking is the degree of competitiveness of the competition. We provide a minimal definition of competitiveness.

Definition 3 (Competitiveness) *Let $v = (v_1, \ldots, v_n)$ and $v' = (v'_1, \ldots, v'_m)$ be two prize schedules of tournaments with n and m participants, respectively. The prize schedule v' is said to be more competitive than v if the proportion of prizes equal to the top prize in v' is smaller than in v:*

$$\frac{\#\{v'_i = v'_m\}}{m} < \frac{\#\{v'_i = v'_n\}}{n}$$

Proposition 2 (Competitiveness and risk taking) *Competitiveness increases the dispersion of the performance distribution.*

i. If the number of top prizes is replaced for a bottom prize in a tournament, the equilibrium dispersion in performance increases.

ii. If the number of participants is increased in a tournament by simply adding new bottom prizes, the equilibrium dispersion in performance increases.

[10] GMD=$\Sigma_{i=1}^{n}\Sigma_{j=1}^{n} f(x_i)f(x_j)|x_i - x_j|$. While GMD and variance are both often used to measure the dispersion, studies have proved that when the distribution is not normal, GMD is a superior measurement of the dispersion of the distribution (Yitzhaki, 2003).

This proposition comes from Corollary 6.b and Proposition 8 in Fang and Noe (2016). The competitiveness of prize schedules increases as it gets harder to obtain the top prize. Intuitively, to aim for the same high prize as in the original tournament, a contestant needs to stretch out the upper bound of the distribution to surpass more opponents in the bigger tournament.

The entrant effect prediction can also be applied to situations where the contestants pool is growing while the number of prizes is not (e.g. college admission and job hunting). One can consider the lowest prizes in these scenarios as zero.

Definition 4 (Scaling) *Let $v = (v_1, \ldots, v_n)$ and $v^s = (v_1^s, \ldots, v_{sn}^s)$ be two prize schedules of tournaments with n and sn participants, respectively. The prize schedule v^s is said to be a scaled up version of v by factor s, if the v_s is generated by making s copies of each prize in v.*

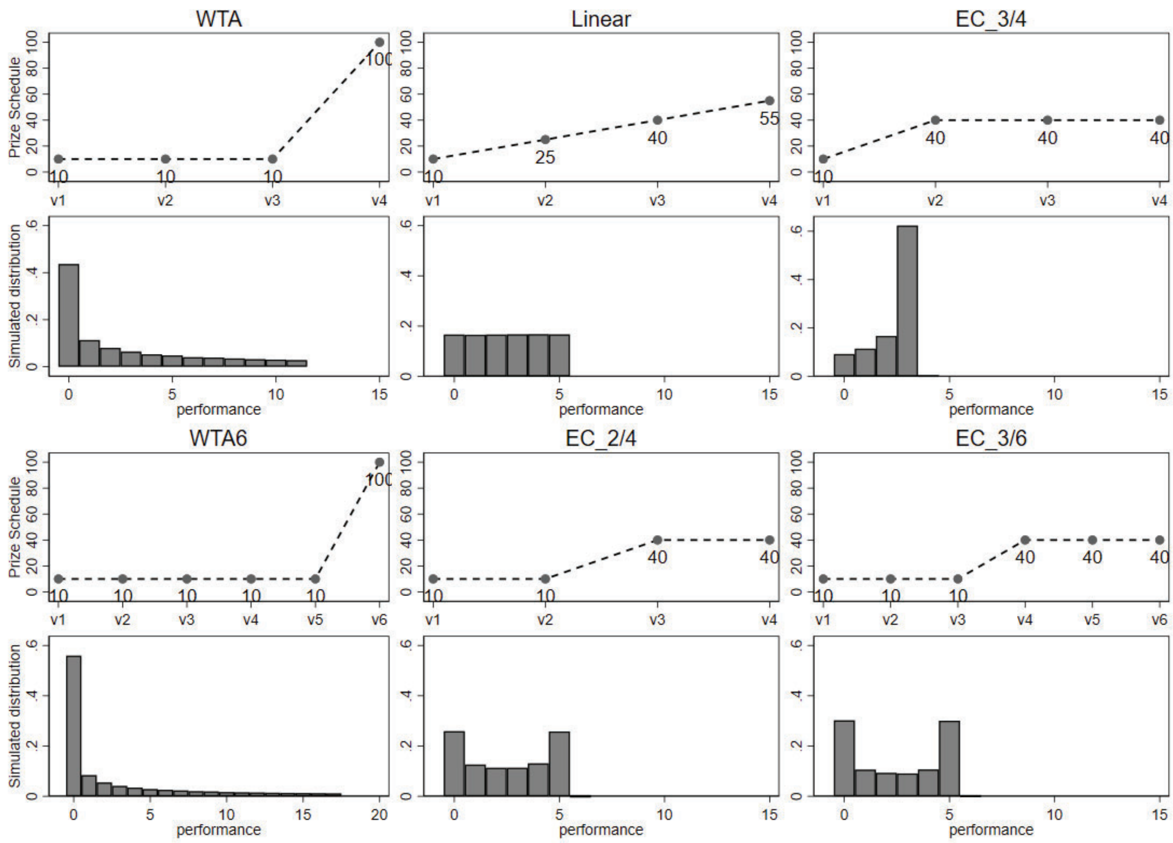
A scaled up tournament is a tournament with more players which retain the proportion of prizes from the original tournament. The scaled up tournament has the same level of inequality and competitiveness.

Proposition 3 *If a prize schedule v^s is a scaled up version of v, the equilibrium distribution of performance for v^s is more dispersed than for v.*

This proposition comes from Proposition 9 in Fang and Noe (2016). When there are s copies of each prize in the prize schedule, advancing in rank does not necessarily mean higher prize values. Contestants' payoff only increase when advancing to a higher rank with a higher value prize. Hence, contestants have the incentive to choose the performance distribution that clusters on the critical "jumping" points. Compared with the performance distribution in small tournaments, the clustered performance distribution is more dispersed.

3.2.2 Experimental design and predictions

We design an experiment which replicates this tournament design: participants compete in a rank-order tournament by making risk-taking choices (i.e., choosing a probability distribution over performance) under a capacity constraint, $\mu = 3$. The experiment includes six tournament structures which vary in prize schedules or the number of contestants. Figure 3.1 shows the six prize schedules we implemented in the experiment (see the first and the third row) along with the corresponding simulated equilibrium performance distributions[11] (see the second and the fourth row).



Note: The first and third row are six prize schedules used in the experiment, the corresponding equilibrium performance distribution is the bar graph below each prize schedule. To make the theoretical distribution comparable to the observed distribution, we take the floor of the simulated values using the quantile function and plot the discrete version of the theoretical distribution.

Figure 3.1: Prize schedules and equilibrium distributions

The first row presents three prize schedules with different levels of convexity, and, therefore, inequality. The *winner-take-all* (WTA) prize schedule corresponds to the situation where only one contestant gets the top prize valued at 100, while the other

[11]The simulated equilibrium distribution is obtained using Equation 3.2.

contestants get the low prize valued at 10. The *elimination contest* ($EC_{3/4}$) is a concave prize schedule which has 3 contestants getting a top prize of 40 and 1 contestant getting a prize of 10. In the *linear* prize schedule, contestants ranking from the lowest to the highest are rewarded 10, 25, 40 and 55, respectively. These three prize schedules only differ in terms of their convexity, but have the same number of prizes and total prize value of 130.

Prediction 1 (Inequality effect) *The dispersion and skewness of performances increases from the elimination contest* ($EC_{3/4}$) *to the linear contest and from the linear contest to the winner-take-all contest* (*WTA*).

This prediction comes from the fact that these three contests are ranked by inequality and convexity.

Prediction 1 implies that in our experiment, the performance distribution should increase in both dispersion and skewness when moving from treatments $EC_{3/4}$ to Linear, and from Linear to WTA. More specifically, the equilibrium distribution is positively skewed for WTA, symmetric for Linear, and negatively skewed for $EC_{3/4}$. This prediction stem from Proposition 1 and the fact that these three contests are ranked by inequality and convexity.

The treatments also features variations in the competitiveness of the tournament. In our design, the prize schedule WTA_6 and $EC_{3/6}$ (row three in Figure 3.1) corresponds respectively, to modifications of, respectively tournaments WTA and $EC_{3/4}$, to make them more competitive: Two new players and two bottom prizes have been added. We make the following prediction. The tournament $EC_{2/4}$ also represents a more competitive version of $EC_{3/4}$, the two tournaments are almost identical, with only one top prize being converted in a bottom prize.

From the Proposition 2, we make the following prediction:

Prediction 2 (Competitiveness effect) *The dispersion of the equilibrium distribution is greater in:*

i. WTA_6 *than WTA, due to the addition of participants and lower prizes.*

ii. $EC_{2/4}$ *than* $EC_{3/4}$ *due to the reduction in the number of top prizes*

Finally, our design also allows us to investigate the effect of the scale of the tournament. In our experiment, we can compare the performance distribution under $EC_{2/4}$ and $EC_{3/6}$ to investigate the scaling up effect.[12] From the Proposition 3, we make the following prediction:

Prediction 3 (Scaling up effect) *The dispersion of the equilibrium distribution is greater in:*

i. $EC_{3/6}$ *than* $EC_{2/4}$

3.2.3 Experiment implementation

We conducted the experiment at Wuhan University from February to April 2019. In order to get the same number of observation groups, we conducted 2 sessions for four 4-prize treatments and 3 sessions for two 6-prize treatments which results in 14 sessions in total.[13] Each session had 24 participant. In total, 336 students were recruited across the campus. The experiment lasted around 1.5 hours and the average payment is 70 CNY.

The experiment contains two parts. Part 1 is an introduction which has 6 rounds of one-shot games. Participants play each of the six prize schedules once in a random order. The order of the 6 prize schedules are independently drawn for each participants. Participants only make decisions. They do not observe the contest results.[14] We use Part 1 to familiarize the participants with the experimental interface and the prize schedules.

Part 2 is the main experiment. In Part 2, only one of the six prize schedules is used for all participants for all 12 rounds. We name the treatment after the prize schedule used in Part 2. Before the start of Part 2, participants are randomly assigned into groups of four in WTA, Linear and $EC_{3/4}$ treatments, or groups of six in WTA_6 and $EC_{3/6}$ treatments.

[12] Theoretically speaking, the only contest transformation that guarantees an increase in skewness for all contests is a convex transformation (Zwet, 1964) of the prize schedule. The effect of adding more entrants (Prediction 2) or increasing the scale of the contest (Prediction 3) on the change of skewness is indeterminate.

[13] The order of the treatment is determined by random draw before conducting the experiment.

[14] Participants are told they compete with other contestants in the tournament, but they are not matched when they make their decision. Computer randomly draws one prize schedule at the end of the experiment. Participants' decision under that prize schedule are then matched to generate the tournament result.

These groups stay the same from all 12 rounds. All the competitions are held within these groups.

In each of the 12 rounds, participants make their risk-taking decision by using a distribution builder (DB) to build their own performance distribution (Sharpe et al., 2000). Figure 3.2 presents a screenshot of the DB we use in the experiment. Each participant has 100 markers to allocate to build a probability distribution against columns numbered 0 to 25.[15] Each number represents a performance level. To place a marker on given number, the participant has to use incurs a cost equal to this number.[16] The cost is in "Experimental Currency Unit (ECU). For instance, the cost of 1 marker placed on number 5 is 5, the cost of 2 markers on the number 5 is 10, and so on. The capacity constraint takes the form of a budget of 300 ECU. Participants can choose a risk free distribution by placing all their 100 markers on the value 3. Or they can spread the markers around, but the sum of the values cannot exceed 300.[17] Participants can only submit the distribution when they use all 100 markers.

To help the participants make their decision, a budget box is placed next to the distribution builder. The budget box indicates how many markers have been used, how many markers are left and what is the total cost of the current distribution. Once the participant changes the allocation, the budget box changes accordingly.

Once all group members submit their distribution, the participants' performance is drawn according to the distribution they built. The probability of a certain performance level to be drawn is equal to the number of markers put on that number divided by 100. Participants are then ranked according to their drawn performance, and prizes are allocated to them according to their ranking. In case of ties, prizes are randomly allocated.

In order to facilitate learning in Part 2, we add three features in each round: First, feedback information including all group members' drawn number, ranking, prize won

[15]The highest theoretical upper bound of the equilibrium distribution in the six treatments is 18, we chose 25 as the maximum performance level to make sure the participants' choice of distribution is not bound by the design of the distribution builder.

[16]Note that as we explained in the theory section, the performance levels do not have cost itself, the capacity constrain imposes a shadow price on each performance level, here the cost of each marker put on the number is to mimic the shadow price of the performances

[17]Saturating the budget constraint is trivial since moving a marker by one notch increases the budget used by one unit. In practice, most of the time participants (more than 90% of the decisions) do so in the experiment

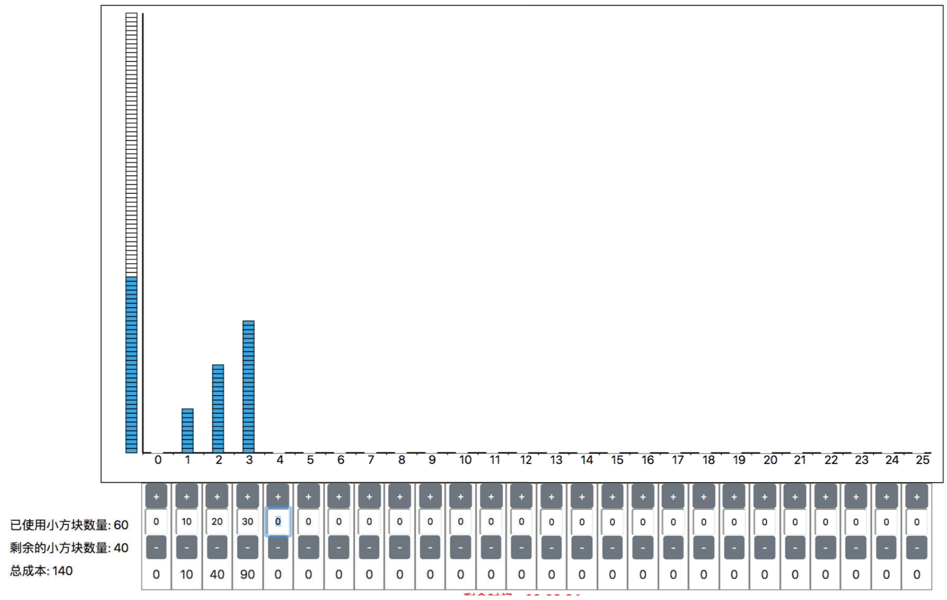


Figure 3.2: Distribution builder
Participants select markers (blue) and place them over number values on the x-axis.They can do so either using their mouse, or the + and - buttons under each number. Two numbers are written below the values on the x-axis in a budget box: the number of markers placed on this value and the total cost corresponding to these markers (e.g. 30 markers on "3" costs 90).

will be disclosed at the end of each round. Second, on the DB builder page, we add a "reload" button which participants can use to start from the distribution they built last round and make adjustments in the current round. Lastly, we provide a history box to remind participants of the numbers chosen by other group members and the distribution of prizes in all the previous rounds.

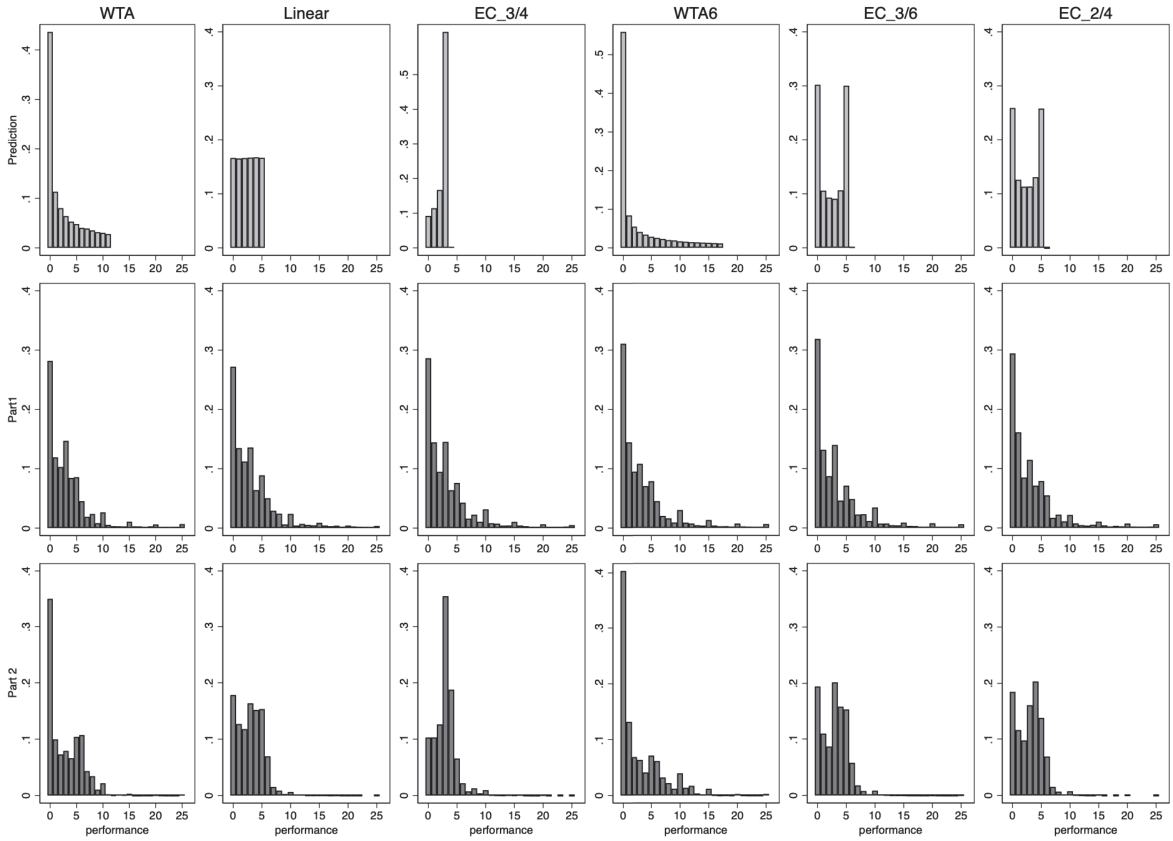
At the end of the experiment, the computer randomly draws one prize schedule in Part 1 and one round in Part 2 to decide participants' payoff. The prizes won under the prize schedule drawn in Part 1 and the prize won in the round drawn in Part 2 are converted in cash according to the exchange rate of 1 ECU = 0.5 RMB and 1 ECU = 1 RMB respectively, and paid to the participants together with their participation fee (20 RMB).[18] In addition to the two main parts, we also included a comprehension test and a practice round to give participants a good understanding of the experiment. At the end of the experiment, a questionnaire is also used to collect the demographic information and ask a few questions about how participants made their decisions.

[18]The exchange rate is around RMB1=USD0.15.

3.3 Results

3.3.1 Overview of the data

Figure 3.3 compares the performance distribution predicted by the equilibrium (first row),[19] and the actual distributions chosen by participants in Part 1 (second row), and Part 2 (third row).



Note: The first row is the simulated distribution from the equilibrium quantile function in each treatment. The second and third row show the aggregate performance distribution chosen by the participants in Part 1 and part 2, respectively.

Figure 3.3: Aggregate performance distribution in each treatment

Comparing the distributions of Part 1 across all treatments, although participants slightly shift their distribution with the change of the prize schedule, they always choose a positively skewed distribution no matter what prize schedule is implemented. Participants' chosen distribution get closer to equilibrium in Part 2.

[19]By design constraints the chosen distribution are de facto discrete in our experiment. So we rounded all the simulated performance levels from the quantile function to form a discrete version of the theoretical distribution and use this discrete theoretical distribution as the benchmark for the observed distributions.

The distribution in WTA_6 has the same shape as WTA but with a higher upper bound and a larger probability mass on the lower end. We can see that the performance distributions in treatment $\text{EC}_{2/4}$ and $\text{EC}_{3/6}$ do have relative heavy mass on the two ends of the distribution as predicted.

Table 3.1 compares the distributions obtained in the Part 2 of each treatment to the theoretical predictions. The second column presents a test of the overall difference in distributions. We build a bootstrap test, based on the *Kolmogorov-Smirnov* (KS) test statistic, D which measures the distance between two cumulative distribution at each point of x and takes the value of the largest distance[20]. The KS statistic indicates how different two distributions are. The standard Kolmogorov Smirnof test using this statistic requires for distributions to be generated by iid observations. It is not the case in the distributions we observe: each marker is part of a set of 100 markers allocated by a participant. A participant's choice to allocate one marker depends on the other choices for markers. Therefore, we cannot test for differences in distributions directly using the standard KS test.We therefore implement a bootstrap test whereby we generate a distribution of KS statistics by resampling participants in each treatment, pooling their distributions and calculating the KS statistics against the theoretical distribution. We then use this bootstrap distribution to calculate a p-value of the KS statistic actually observed with the whole sample. The results from these tests show that the empirical distributions we observe differ significantly from the theoretical predictions in all treatments.

Looking at the dispersion and skewness of these distributions give us an idea of how they differ. Table 3.1 presents the theoretical dispersion and skewness of the equilibrium distributions in all treatments. These values are compared to the observed dispersion and skewness. We test the difference between these values and the observed ones using a t-test (clustered at the participant level). We observe that the observed distributions tend to be less dispersed than the theoretical ones.[21] They also tend to have less skewness (positive or negative) than the theoretical one.[22] In short, it seems

[20] $D = \underset{x}{sup}|F_{1,n}(x) - F_{2,m}(x)|$, where $F_{1,n}$ and $F_{2,m}$ are the empirical distribution functions of sample 1 and sample 2

[21] The equilibrium GMD formula is provided by Fang and Noe (2016), $GMD = 2\mu\left(-1 + \frac{2}{n+1}\Sigma_{i=1}^{n}\left(\frac{v_i - v_1}{V}\right)i\right)$

[22] We measured the theoretical skewness using simulated equilibrium distributions in each

that the observed distributions are, in some sense, less extreme than the theoretical distribution. It may suggests an under-adaptation of participants' responses to the best response strategies predicted from equilibrium play.

Table 3.1: Summary statistics of each treatment

	Overall dist.	Dispersion (GMD)			Skewness			N
	p-value	NE	Obs.	p-value	NE	Obs.	p-value	
WTA	$p < 0.01$	3.60	1.46 (0.462)	$p < 0.01$	1.06	0.453 (0.803)	$p = 0.04$	564
Linear	$p < 0.01$	2.00	1.08 (0.455)	$p < 0.01$	0	0.112 (0.965)	$p = 0.27$	576
$EC_{3/4}$	$p < 0.01$	1.20	0.81 (1.107)	$p < 0.01$	-1.01	0.043 (0.193)	$p = 0.03$	564
$WTA_6 6$	$p < 0.01$	4.29	1.71 (0.506)	$p < 0.01$	1.65	0.879 (0.837)	$p = 0.87$	864
$EC_{3/6}$	$p < 0.01$	2.57	1.06 (0.489)	$p < 0.01$	0	0.082 (0.903)	$p = 0.13$	864
$EC_{2/4}$	$p < 0.01$	2.40	1.04 (0.422)	$p < 0.01$	0	0.015 (0.931)	$p = 0.67$	576

Note: The predicted skweness and GMD are calculated based on the simulated distributions and equlibrium GMD formula, respectively. p-value comparing the observed statistics and the predictions. p-value for skewness is based on two-sample t-test clustered on individual level, while p-value for GMD is based on one-sample t-test clustered on individual level. Column "N" indicates the number of observation.

3.3.2 Treatment effects

We now compare the observed distributions across treatment to investigate whether they display the comparative static predictions described in section 3.2.2.

We start with an overall test of difference in the shapes of the distributions, based on the KS statistic. Since the markers generating the distributions are not iid observations, we cannot use the KS test as such. We therefore design and implement a permutation test based on the KS statistic to calculate an accurate p-value. The principle of this test is simple. The KS statistic D^* observed between the distributions in two different treatments (or between a treatment and the theoretical prediction) is compared to the distribution F_D of the KS statistic D obtained, when the participants' decisions (their chosen distributions) are permuted randomly across treatment (i.e. participants are randomly relabelled as being from one or the other treatment). Under the null hypothesis, the participants' choices are then same across treatments. The statistics D^* should

measurements.

then also be drawn from the distribution F_D generated from the permutations. We can therefore place D^* in this distribution to get a p-value of the test that the distributions are indeed the same across treatment: $p = 1 - F(D^*)$.[23] The detailed process of conducting permutation test is described in Appendix B.

Table 3.3.2 displays the results of tests comparing each treatment with all the other treatments. The first test, assessing the significance of differences over the whole distribution is the permutation test discussed with the KS statistics. We observe that most treatments are significantly different from each others. This indicates that, while the observed distributions differ from the theoretical ones, the participants' behaviour reacted to the different incentives provided in the different treatments.

Table 3.2: Treatment comparison

	WTA		**Linear**		**$EC_{3/4}$**		**WTA_6**		**$EC_{3/6}$**	
Measure	Diff	p value	Diff	p value	Diff	p value	Diff	p value	Diff	p value
Linear										
Whole dist.	-	$p < 0.01$								
GMD	0.38	$p < 0.01$								
Var.	4.90	$p < 0.01$								
Skew.	0.34	$p = 0.02$								
$EC_{3/4}$										
Whole dist.	-	$p < 0.01$	-	$p < 0.01$						
GMD	0.64	$p < 0.01$	0.27	$p = 0.02$						
Var.	6.22	$p < 0.01$	1.33	$p = 0.28$						
Skew.	0.41	$p = 0.04$	0.07	$p = 0.70$						
WTA_6										
Whole dist.	-	$p < 0.01$	-	$p < 0.01$	-	$p < 0.01$				
GMD	-0.25	$p < 0.01$	-0.63	$p < 0.01$	-0.90	$p < 0.01$				
Var.	-5.22	$p < 0.01$	-10.11	$p < 0.01$	-11.44	$p < 0.01$				
Skew.	-0.43	$p < 0.01$	-0.77	$p < 0.01$	-0.84	$p < 0.01$				
$EC_{3/6}$										
Whole dist.	-	$p < 0.01$	-	$p < 0.01$	-	$p < 0.01$	-	$p < 0.01$		
GMD	0.40	$p < 0.01$	0.02	$p = 0.80$	-0.25	$p = 0.03$	0.65	$p < 0.01$		
Var.	4.84	$p < 0.01$	-0.06	$p = 0.95$	-1.39	$p = 0.21$	10.05	$p < 0.01$		
Skew.	0.37	$p < 0.01$	0.03	$p = 0.79$	0.03	$p = 0.81$	0.80	$p < 0.01$		
$EC_{2/4}$										
Whole dist.	-	$p < 0.01$	-	$p = 0.02$	-	$p < 0.01$	-	$p < 0.01$	-	$p < 0.01$
GMD	0.42	$p < 0.01$	0.04	$p = 0.61$	-0.23	$p = 0.03$	0.67	$p < 0.01$	0.23	$p = 0.03$
Var.	5.42	$p < 0.01$	0.52	$p = 0.51$	-0.80	$p = 0.44$	10.63	$p < 0.01$	-0.81	$p = 0.44$
Skew.	0.30	$p < 0.01$	0.10	$p = 0.45$	0.03	$p = 0.90$	0.86	$p < 0.01$	0.03	$p = 0.90$

Note: Tests of differences between the performance distributions observed in different treatments, overall and for specific distribution statistics.

[23] E.g. If $F_D(D^*) = 0.99, p = 0.01$: there is only 1% chance to observe a D greater or equal to D^* under the null.

In each comparison, the table also presents the tests for variations in dispersion (GMD and variance) and in skewness, using t-test with clustered standard errors at the level of the participants (each participant produces 12 distributions in Part 2).

Looking at the inequality affects risk taking, we find that the convex transformations of the prize schedule, from $EC_{3/4}$ to Linear and from Linear to WTA, increase both the dispersion and the skewness of the distribution. The values of the GMD are 1.2, 2, and 3.6 for the treatments $EC_{3/4}$, Linear and WTA, respectively. All these differences are significant from each other. Using variance gives a similar pattern, tough the difference between Linear and $EC_{3/4}$ is not statistically significant. Similarly, for the skewness, the observed distributions become more positively skewed as the convexity of the prize schedule increases, though the difference between Linear and $EC_{3/4}$ is only significant at 10%. These results are in support of Prediction 1:

Result 1 (Inequality) *In line with Prediction 1, for more convex prize schedules, participants opt for distributions of performance with a greater dispersion and skewness.*

Looking at the variations in competitiveness, in the form of the proportion of winners, the results are also supportive of our prediction. First, the competitiveness increases when a top prize in $EC_{3/4}$ is replaced for a bottom prize to make the prize schedule $EC_{2/4}$. We observe that the degree of dispersion, using the GMD, is significantly higher in $EC_{2/4}$, as predicted by the theory.

Second, the competitiveness also increases with the addition of new entrants (with the only the prizes at the bottom of the prize schedule being added). We observe that there is more dispersion (GMD and variance) and more skewness in WTA_6 relative to WTA. There is also more dispersion in $EC_{3/6}$ than in $EC_{3/4}$. The difference is statistically significant for GMD, not for variance. There is no effect on skewness. Over these different treatments, the variations in dispersion are all in line with the theoretical predictions with the differences always significant when using GMD as a measure of dispersion, as predicted by the theory.

Result 2 (Competitiveness) *An increase in competition in the form of a reduction of the proportion of winner or an an increase in the number of participants, without additional prizes above the lowest one, is associated with an increase in the dispersion of the distributions of* *performance chosen by the participants. This result is in line with Prediction 1*

Looking at the scaling up effect, we compare treatments $EC_{3/6}$ and $EC_{2/4}$ with the former being 1.5 times the size of the latter. Our KS permutation test shows that the distribution in $EC_{3/6}$ significantly different from the distribution in $EC_{3/4}$ ($p < 0.01$). The level of dispersion is greater in treatment $EC_{3/6}$, as predicted by Prediction 3, though it is not significant. Unlike the equilibrium performance distribution in other treatment which is uni-modal, the equilibrium performance distributions in $EC_{2/4}$ and $EC_{3/6}$ are bi-modal. The increase in dispersion of scaling up the tournament is not through the stretching out the upper bound of the distribution, but through putting more probability mass on the two modes. Using the former measurements to capture the scale-up effect might be deceptive. Recent study by Cavallo and Rigobon (2011) introduce the *proportional mass score* (*pm*-score) which use the ratio of the probability mass (per unit) within in the small interval around the center to the probability mass (per unit) within a larger interval around the center to measure the bi-modality. A bi-modal distribution should always have a *pm*-score which is smaller than 1, and a bi-modal distribution with more probability mass on the two ends should have a smaller *pm*-score. As we can see from column *pm*-score in Table **??**, in the second half of the experiment, the *pm*-score in $EC_{3/6}$ is 0.072 less than that in $EC_{2/4}$, though the difference is not significant ($p > 0.1$)[24]

By observing the aggregate distributions in Figure 3.3, we can see that the performance distributions in treatment $EC_{2/4}$ and $EC_{3/6}$ do have relative heavy mass on the two end of the distribution. However, instead of putting all the mass on the upper bound 6, participants put similar probability mass round the upper bound of the distribution. One possible limitation of our design is that we only multiply the size of the contest by 1.5, starting from a small contest, a larger scale or a larger contest might be helpful to increase the power of the test for the analysis of the scaling up effect.

Result 3 (Scale) *When we scale-up the tournament from $EC_{2/4}$ to $EC_{3/6}$, participants, we observe a non-significant increase in the dispersion of their performance distribution.*

[24]Another detail readers might notice is that unlike what the theory predict, the *pm*-score in both treatments are both larger than 1. The value of *pm*-score is sensitive to both the choice of the "center" and the intervals, however, the relative relationship of the two *pm*-scores under comparison is not affected as long as the same center and intervals are used in the calculation.

3.3.3 Learning

Since Part 2 takes place over 12 rounds, a natural question is whether participants get better at approximating the equilibrium predictions with experience.

Figure B.1 shows the aggregate distribution in selected rounds (1, 3, 7, 12), using local polynomial estimates. The estimates are contrasted with the predicted distributions represented by bar graphs. The observed distributions shift across the different rounds with participants' chosen distributions getting, overall, closer to the equilibrium predictions over time (with the exception of the WTA condition).

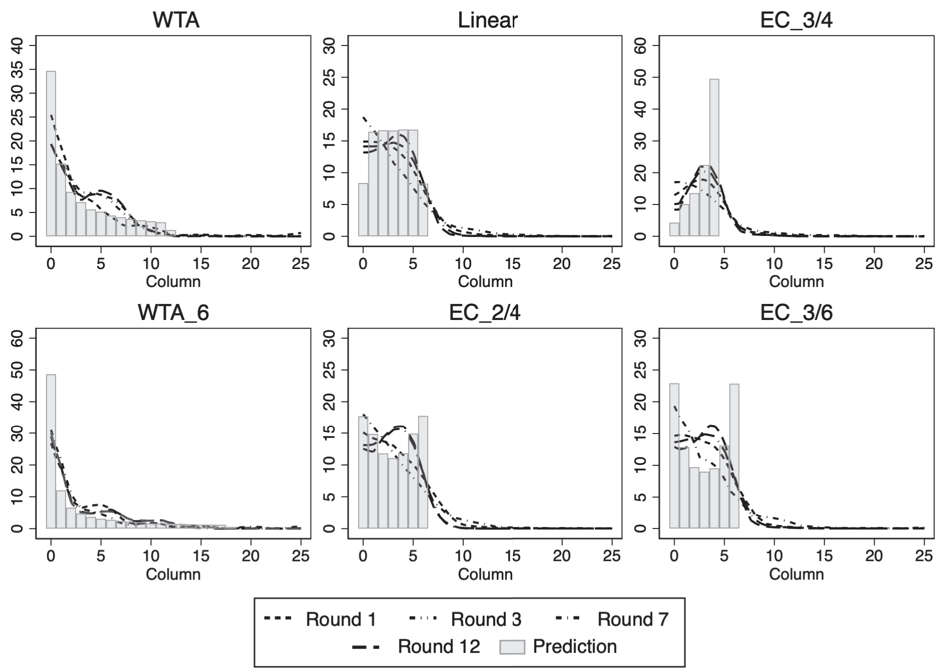


Figure 3.4: Learning

Figure B.3 illustrates the distribution of $\Delta D = D12 - D1$, which tests whether the distribution in round 12 is closer to equilibrium or not compared with the round 1 distribution. $D1$ ($D12$) is the KS statistic measure the distance between the equilibrium prediction and distribution of the $1st$ ($12th$) round in Part 2 in each treatment. If participants converge to equilibrium from round 1 to round 12 in part 2, the ΔD statistics should be negative. We use 10,000 bootstrap resamples, clustered by participants, to generate distributions of $D12$ and $D1$.[25] We then calculate $\Delta D = D12 - D1$ for each bootstrap sample. As we can see from the figure, only in treatment Linear are all the

[25]When bootstrapping, we re-sample the observation size equals to the original sample size.

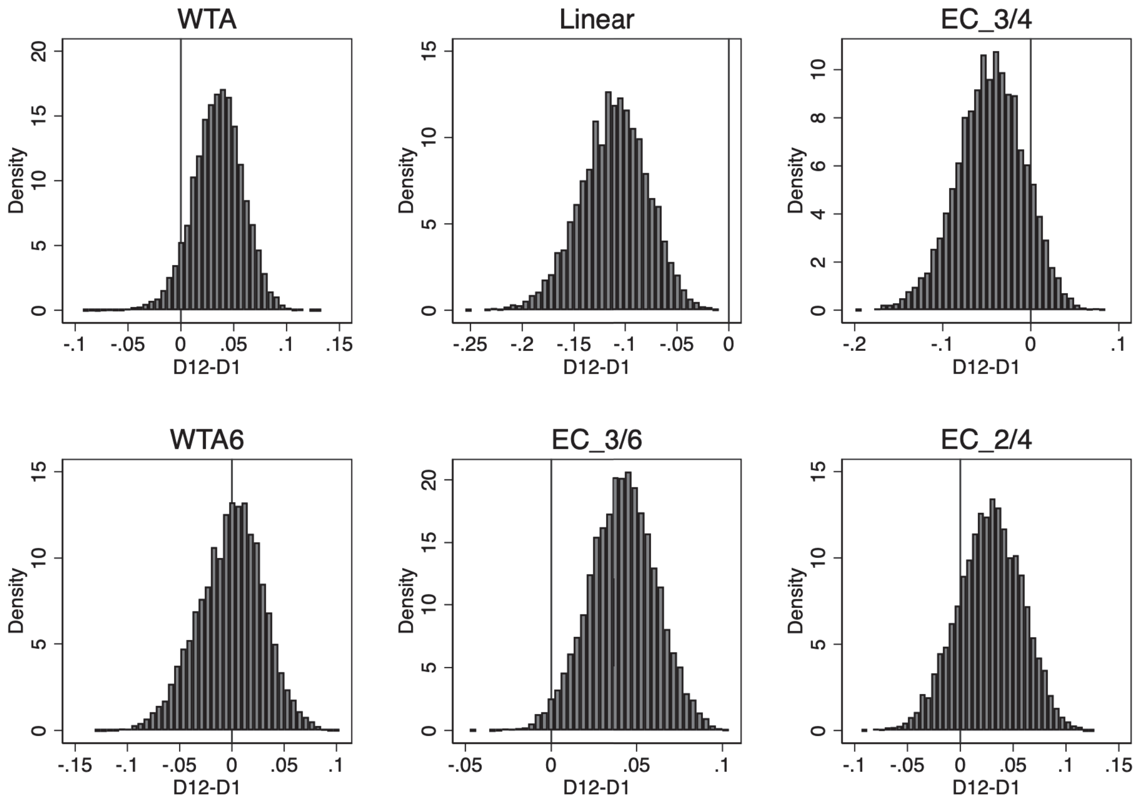


Figure 3.5: KS test for learning (round12-round1)

Note: $D1$ ($D12$) is the k-s statistics measure the distance between the equilibrium prediction and distribution of the $1st$ ($12th$) round in Part 2 in each treatment. The solid line is $\Delta D = 0$, which indicates the round 12 and round 1 are equally closed to the predicted distribution.

KS statistics below 0, which means participants converge to equilibrium distribution in the Linear treatment. In WTA_6 and $\text{EC}_{2/4}$ we see the distribution is mostly even around 0, meaning the difference is not significant. However, in treatment WTA and $\text{EC}_{3/6}$, we see most of the differences are positive, which indicates participants are even further away from the prediction in round 12 compared with round 1.

3.4 Conclusion

We investigate how the convexity of prize schedules and the number of participants in the tournament affect contestant's risk-taking behavior when they face a specific capacity constraint. Our experiment allows us to directly observe the performance

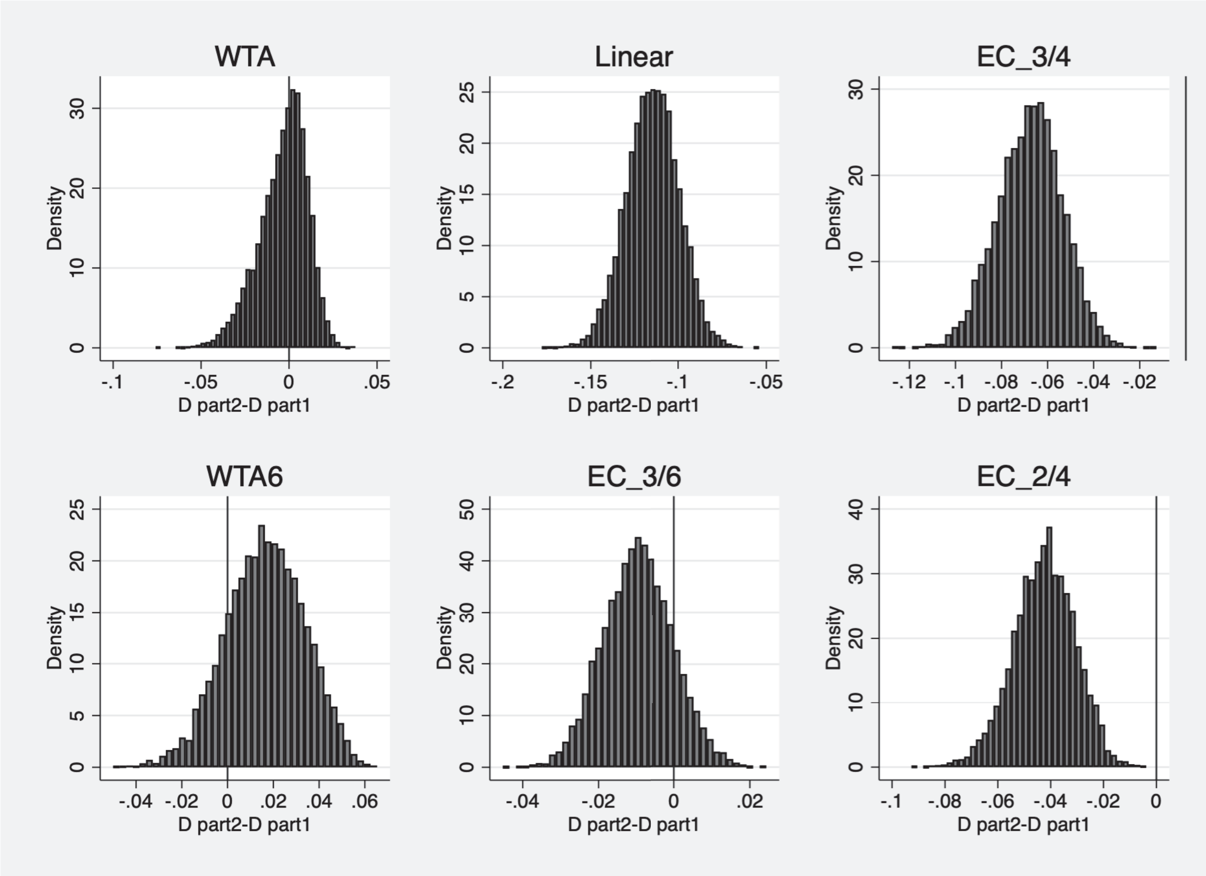


Figure 3.6: KS test for learning (part2-part1)

Note: $Dpart2$ ($Dpart1$) is the KS statistic of the difference between the equilibrium prediction and distribution of Part 1 (Part 2) in each treatment. The solid line is $\Delta D = DPart2 - DPart1 = 0$, which indicates the distribution of Part 2 and Part 1 are equally closed to the predicted distribution.

distribution chosen by participants without any assumption on the shape of the distribution.

Our experiment provides strong evidence that the performance distributions chosen by participants need not be symmetric. Participants in the experiment react to changes in the design of tournaments in the direction predicted by comparative statics of the equilibrium play: (1) increasing the convexity of the prize schedule not only increases participant's risk-taking behavior in terms of dispersion but also shifts the skewness of the distribution; and (2) adding more contestants into the contest induces participants to choose riskier performance distributions that are more dispersed. Although contestants choose a more dispersed distribution when the tournament is scaled up, the difference is not significant. One potential reason is that participants in the experiment under-react to the prize schedule when it is non-linear, which reduces the treatment effect.

In reality, the most prevalent prize schedules are either convex or concave. For example, in R&D contests, only the company with the highest performance gets the prize; in the annual evaluation within a company, only the lowest performing employee faces a career crisis. Our study has important implications for real-world tournaments that resemble the characteristics of a rank-order tournament in which the primary choice variable is the level of risks to be taken. The design of these tournaments may incentivize contestants to take too much risk, potentially at the cost of the principals. Principals could underestimate the probability of having low outcomes if they assume that the performance distributions are always symmetric. Moreover, the skewness of the distribution leads to a skewed wealth distribution, which is essential for the social planner to consider when measuring the social welfare.

Chapter 4

Indicative bidding in auctions with costly entry

4.1 Introduction

In company takeovers, government procurement and auctions of natural resources (oil, gas, timber, etc.), the exact value of the asset is complicated to estimate. Potential bidders only have imperfect information about their valuation before they enter the auction, but will learn their true values after they perform the due diligence process which is very costly and non-refundable (Li and Zheng, 2009, 2012; Athey et al., 2011; Roberts and Sweeting, 2013).

The considerable cost decreases bidders' willingness to participate, which potentially causes auction failure and hence lower expected revenue. This is especially a problem for the formerly mentioned auctions where the number of potential bidders is already scarce. To deal with this issue, auctioneers commonly use a non-binding bidding stage to select only the "right" bidders for the actual auction (Bhattacharya et al., 2014; Sweeting and Bhattacharya, 2015; Gentry et al., 2017). However, due to the lack of symmetric increasing equilibrium, the indicative bidding mechanism cannot ensure the selection is efficient (Ye, 2007).

In this paper, we investigate a "simple" indicative bidding model that can improve the selection efficiency compared with the conventional indicative bidding mechanism. By comparing the "simple" indicative bidding mechanism with an unrestricted auction, a restricted auction using the experimental method, we aim to achieve two goals: (1) to see whether the indicative bidding mechanism we consider can generate higher expected revenue than the alternative mechanisms; and (2) to disentangle the participation effect and the selection effect which cause the differences in expected revenue under the three different mechanisms. [1] [2]

The "simple" indicative bidding mechanism we consider was first studied by Quint and Hendricks (2018). It begins with potential bidders indicating a discrete and non-binding entry message (i.e., $m = 0, 1, 2, ...$) based on the imperfect signal they have regarding the asset.[3] Then the auctioneer shortlists two buyers with the highest entry messages as entrants to the auction. Only the bidders who are selected for the auction pay the cost of due diligence to investigate their true value and bid in the auction.

The rationale behind the indicative bidding mechanism is that when bidders' information rent after entry is relatively small compared with the entry cost, potential bidders do not have incentives to lie about their values. A non-binding first stage bidding is efficient in selecting the bidders with the highest values. The small information rent condition is also satisfied when the information learnt after entry is highly correlated among bidders. This feature of indicative bidding makes it a perfect mechanism to consider for timber auctions, company takeovers and government procurement. Recently, a stream of empirical literature finds that in these auctions, the information learned after entry is highly correlated among bidders (Li and Zheng, 2009; Aradillas-López et al., 2013). Take company takeovers as an example. Upon entry, buyers conduct due diligence to estimate the value of the target firm in advance of the bidding. The primary purpose of due diligence is to examine the "skeletons in the closet" to eliminate potential uncertainties, which usually affect all bidders in the same manner. Conventional auction models and experiments do not capture the feature of correlated information among bidders. Our paper seeks to provide some insight by comparing under this correlated information structure.

Our central results provide answers to the intriguing question of how the deviation from the independent private value model will shift bidders' entry behavior, bidding

[1] In the unrestricted auction, participants first make their entry decision based on their initial signal. All the bidders who choose to enter pay the entry cost to learn their true values and then bid in the auction.

[2] In an auction with restricted entry, the auctioneer will randomly choose a certain number of bidders to enter the auction from those who chose to enter. In this sense, auctions with restricted entry can be viewed as having a certain number of seats which are filled based on a first-come-first-serve rule.

[3] In conventional indicative bidding, the first stage non-binding bids are continuous. For clarity, from now on, when we mention indicative bidding, we refer to "simple" indicative bidding.

behavior, and hence the auctioneer's choice among the three auction mechanisms under consideration. As we show in the theoretical analysis section (Section 4.3), indicative bidding always gives weakly higher expected revenue than either the unrestricted or restricted entry mechanisms through two effects. Firstly, the participation effect: restricting the number of participants encourages bidders to enter because it reduces the possibility of entering into a highly competitive auction. Secondly, indicative bidding improves expected revenue by selecting the bidders with higher valuations to enter. We also show that the relevant performance of the two alternative mechanisms depends on the magnitude of the entry cost. If the entry cost is low, simply restricting the number of entrants results in inefficiency in the selection which harms the auction revenue. On the contrary, when the entry cost is high, bidders with lower values automatically drop out, restricting the number of entrants and thus limit the competition size, which leads to higher revenue compared with the unrestricted auction.

Principally following the theoretical prediction, our experimental results show that indicative bidding always outperforms the restricted auction in terms of both selection efficiency and revenue generation. Although indicative bidding cannot ensure the same level of selection efficiency as the unrestricted auction, it generates higher revenue than the unrestricted auction when the entry cost is high. Even for relatively low entry cost, the indicative bidding mechanism generates at least as much revenue as the unrestricted auction. In terms of the point predictions, we observe significantly higher revenue in indicative bidding with high entry cost compared with the equilibrium. This is mainly driven by participants' over-entry in the entry stage, which results in a lower-than-equilibrium probability of auction failing. Interestingly, we find that in indicative bidding where they have two entry messages to choose from, participants tend to overuse the higher entry message, but under-use the lower entry message. Aside from our main findings, we also observe mild over-bidding in all treatments, which aligns with previous experimental literature.

The remainder of the paper proceeds as follows, Section 4.2 summarizes the previous literature, and Section 4.3 sets up the model and prediction. The experimental design is presented in Section 4.4 and the experimental results are discussed in Section 4.5. Section 4.6 concludes.

4.2 Literature review

The majority of previous studies on auctions with costly entry suggest that limiting the number of bidders in the auction can improve the expected revenue (Milgrom, 2004), regardless of the mechanism used to select the bidders. One branch of the literature assumes that bidders have no information about their valuation towards the asset under auction prior to entry: each bidder enters stochastically with certain probability (Levin and Smith, 1994; Pevnitskaya et al., 2004; Bulow and Klemperer, 2009). In this model, both buyer and seller do not posses pre-entry information on buyers' valuations, and selection can only be random. In contrast, another branch of the literature assumes that bidders have perfect information before they enter the auction and only bidders with private values that are higher than the entry cut-off will enter (Samuelson, 1985; Engelbrecht-Wiggans, 1987; McAfee and McMillan, 1987; Menezes and Monteiro, 2000). In this case, the selection is automatic since bidders with higher values will self-select into the auction.

A more recent stream of empirical research uses structural estimation or identification methods to examine auctions with the entry process. They find that the pre-entry information is neither perfect nor non-existent (Li and Zhang, 2010; Marmer et al., 2013; Gentry and Li, 2014; Gentry and Stroup, 2019). Correspondingly, the selection is neither random nor automatic. Gentry et al. (2017) theoretically characterize the arbitrary selection model (AS model) which allows the pre-entry private signal to be zero, imperfect or perfect. In the numerous imperfect pre-entry information cases, the efficiency of selection (entry) mechanism determines the expected auction revenue. Ye (2007) is the first to study selection mechanisms under the imperfect pre-entry information assumption. He finds that under the conventional indicative bidding mechanism[4] symmetric increasing equilibrium does not exist, which implies the selection is not efficient. However, with entry subsidy, entry right auction (ERA)[5] can induce efficient entry.

Several theoretical studies investigate other different entry (selection) mechanisms

[4]Under the conventional indicative bidding mechanism, the bidders submit non-binding continuous first-round bids.

[5]In the entry right auction (ERA) potential bidders need to place a binding bid for the right to enter the auction first, then bid for the asset in the actual auction

under the AS model. Bhattacharya et al. (2014); Sweeting and Bhattacharya (2015) examine how entry right auctions and sequential auctions[6] perform relative to the standard two-stage auction (unrestricted auction) in terms of revenue generation. They conclude that under the AS model when the pre-entry signal is informative, both entry right auctions and sequential auctions generate higher expected revenue than the unrestricted auction. Gentry and Stroup (2019) compare negotiations with the unrestricted auction. They find that less accurate pre-entry information weakens the link between bidders' valuation and pre-entry beliefs, which reduces the bargaining power of the seller in negotiation and encourages entry in the auction. Other papers investigate auctions with private value and additional common value, finding that lower the common values are, the more selection can affect the auction revenue(Goeree and Offerman, 2002; Boone et al., 2009; Aycinena et al., 2014). These studies imply that under the AS model, smaller post-entry information rent renders the selection (entry) process more crucial in terms of generating higher auction revenue.

In the auctions that the current paper investigates (company takeovers or timber auctions), bidders' information learned after entry during due diligence is often highly correlated (Li and Zheng, 2009; Aradillas-López et al., 2013). This suggests the bidders cannot gain much rent relative to others after-entry, or in other words, the post-entry information rent is small. Quint and Hendricks (2018) consider indicative bidding under the assumption that the post-entry information is trivial relative to the entry cost. They prove the existence of symmetric increasing entry equilibrium where participants partition into "types" according to their pre-entry signal. They also conclude that any mechanism with an efficient selection mechanism can improve the expected revenue compared with the unrestricted auction. We adopt their model as the theoretical benchmark for our experimental study. Our paper also relate to the literature on "cheap talk" game (Crawford and Sobel (1982), Farrell and Gibbons (1989)). However, what need to be mentioned is that in the model we examine, sellers commits to the auction rule and the monotone selection rule, which eliminates the multiplicity of equilibrium in the "cheap talk" game.

The amount of experimental literature on auctions with a costly entry is limited. Aycinena and Rentschler (2018) examine first price and ascending auctions and find

[6] In a sequential auction, bidders are approached in turns and bid while observing the bidding history.

the former generate a higher revenue. The differences in revenue are mainly driven by bidding after entry. Ivanova-Stenzel and Salmon (2008a,b, 2011) investigate the scenario where the ascending auction and the first price auction compete for a pool of bidders. They find bidders with low values choose the first-price auction more often but bid more aggressively. The combined effect results in no difference in terms of revenue under these two auction mechanisms. Palfrey and Pevnitskaya (2008) find that in the first-price auction, only risk-averse bidders enter the auction if bidders have no pre-entry information. Consequently, bidding and revenue are lower in the auction with entry compared with the one-stage auction. These studies extended the standard auction into a two-stage auction under either no or perfect pre-entry information assumption where the selection process is not considered.

Our paper is mostly related to the experiments that study how the selection mechanism can improve the auctioneer's revenue under the AS model. Kagel et al. (2008) compare indicative bidding with the entry right auction. They find that the variance of the first-round bid in the entry right auction is so large that it reduces the selection efficiency. Consequently, indicative bidding gives similar results in terms of selection efficiency while achieving higher revenue and lower bankruptcy. Boone et al. (2009) assume there is one fully informed "inside" bidder and use a non-binding first-stage which only excludes the bidder with the lowest bid. They observe arbitrary bidding in the non-binding stage, which lowers auction revenue compared with the standard one-stage second-price auction.

The current paper differs from these studies in several major ways. Firstly, our experiment is based on a generic model which mimics the information structure in auctions where the value of the asset is complicated to evaluate. To the best of our knowledge, our experiment is the first to incorporate the property of correlated private values in an auction with costly entry. Although in the broad literature on winner's curse (Kagel and Levin (1986), Camerer (1987),Camerer and Hogarth (1999)), bidders are also assumed to have incomplete information (private signals) of the asset, the valuation of the asset is assume to be a common value. Secondly, in addition to the revenue comparison, we distinguish the different effect of participation and selection in indicative bidding. Due to the lack of increasing equilibrium, previous studies do not provide evidence for mechanisms through which indicative bidding generates higher revenues. Our study provides a possible explanation for the extensive use of indicative bidding in practice. Thirdly, we share the dimension of revenue comparison under different mechanisms with previous literature, but also add the dimension of variation of entry cost, which enriches the experimental evidence for comparative analysis.

4.3 Theoretical background

4.3.1 The model

In a two-stage auction with costly entry, a set of $N \geq 3$ risk-neutral potential buyers bid for an indivisible asset. The value of the asset v_i to each buyer i ($i = 1, 2, ..., N$) is the sum of an initial signal s_i and an additional value t. The initial signal s_i is independently and privately drawn for each potential bidder from the uniform distribution $F(\cdot)$ on $[0, 1]$, whereas the additional value t is commonly drawn for all bidders from distribution $G(\cdot)$. Both $F(\cdot)$ and $G(\cdot)$ are commonly known by all participants.

The auction proceeds as the following: in stage 1, after observing their private signal s_i, potential bidders choose one entry message to be sent to the auctioneer. According to the pre-committed selection mechanism, the auctioneer chooses among those who are willing to enter and advance them to stage 2. All entrants pay an entry cost c to learn the additional value t and consequently their full valuation ($v_i = s_i + t$), and submit their bids simultaneously afterwards.[7] A second-price sealed-bid auction is used in stage 2.[8] We focus on three mechanisms, which differ in the entry messages available in stage 1 and/or the selection process used to advance interested bidders:

Unrestricted (Unr) In the unrestricted-entry mechanism there are only two entry messages ($m = \{0, 1\}$) available to the participants. Entry message $m = 0$ indicates a preference to stay out and message $m = 1$ indicates a preference to enter the auction. The selection process is unrestricted, namely, all bidders who expressed interests in entering are advanced to stage 2.

[7] We use "entrants" to refer to those who actually enter the auction and use it interchangeably with "bidders who enter the auction stage".

[8] We assume that bidders cannot observe others' entry decisions or the actual number of bidders while choosing their bids.

Restricted (Res) The restricted entry mechanism is otherwise the same as the unrestricted entry mechanism, except that the maximum number of entrants is restricted to 2. When there are more than 2 participants who choose message "1", the auctioneer will randomly select two bidders from among those choosing "1".

Indicative (Ind) In the indicative bidding mechanism, bidders have one additional message to use. Entry message $m = 0$ still indicates staying out while both $m = 1$ and $m = 2$ signal interest in entering the auction. Participants can choose message $m = 2$ to express their higher willingness to enter the auction compared to message $m = 1$. This third message gives them a higher priority of being selected when there are more than 2 participants choosing to enter the auction. The selection process works as follows: the auctioneer first randomly selects up to two entrants from those who chose the higher entry message $(m = 2)$. If the number of bidders selected is less than 2, the auctioneer then randomly chooses from those who sent the lower entry message $(m = 1)$.

4.3.2 Equilibrium Analysis

The setup of our theoretical framework is adopted from Quint and Hendricks (2018), who has proved that there is a unique symmetric equilibrium in all mechanisms under which the bidders' profit is monotonic in their initial signal s_i. When the additional value t is perfectly correlated among all bidders, all three mechanisms we have described above satisfy this condition.[9] Therefore, the potential bidders should select their entry message based on their initial signal s_i in equilibrium. In each mechanism, the equilibrium characterization involves identifying cut-off point(s) which separate(s) the participants into groups choosing different entry messages. The following analysis only presents the main results that are most relevant for our experimental design; interested readers can refer to the original paper for the formal proof and technical details.

Unrestricted (Unr) : There exists a marginal bidder i (with an initial signal s_i equal to

[9] The existence of a symmetric equilibrium in partition strategies (i.e. when intervals of types map to each message) requires that the information learned after entry is small relative to entry cost (i.e., the "small rent" condition in Quint and Hendricks (2018)), which is satisfied when the additional value (t) learned after entry is highly correlated. We allow t to be perfectly correlated which guarantees this equilibrium is unique.

the entry cut-off α_1) who is indifferent between entering or staying out and receiving 0 payoff. Above this cutoff, a potential bidder should always choose entry message "1". This equilibrium cutoff point α_1^{Unr} satisfies:

$$\pi(\alpha_1) = \alpha_1^{N-1}(\alpha_1 + E(t) - c) + \sum_{k=1}^{N-1} p_k \cdot (-c) = 0, \tag{4.1}$$

where $p_k = C_{N-1}^k (1-\alpha_1)^k \alpha_1^{N-1-k}$. In a standard second-price sealed-bid auction, the bidders have a (weakly) dominant strategy of bidding his own value v_i. Upon entry, a bidder is potentially faced with two scenarios: there is probability α_1^{N-1} that bidder i is advanced with no other competitor, and his payoff will be $\alpha_1 + E(t) - c$, which constitutes the first term in the equation; and there is probability p_k that bidder i enters with $k \geq 1$ other bidder(s), and his payoff will be $-c$ (since his opponent(s) will always outbid him as he is the marginal entrant), which is the second term in the equation.

Restricted (Res) : Under this mechanism, the maximum number of entrants is two. Similarly, the equilibrium entry cut-off, α_1^{Res}, can be obtained by solving:

$$\pi(\alpha_1) = \alpha_1^{N-1}(\alpha_1 + E(t) - c) + \sum_{k=1}^{N-1} p_k \cdot (-c) \cdot \frac{2}{k+1} = 0. \tag{4.2}$$

The first item in this equation is the same as the first item in Equation 4.1, because the marginal bidder will have the same expected payoff when he enters alone. However, when there are k other participants who choose $m = 1$, his payoff will still be $-c$ upon entering, but the probability of being selected into the auction is only $\frac{2}{k+1}$ and hence the second term in Equation 4.1 needs to be multiplied by it.

Indicative (Ind) : The equilibrium characterization under *ind* mechanism involves the identification of up to two cut-offs, α_1 and α_2, which divide the initial signal space $[0, 1]$ into three intervals ($[0, \alpha_1]$, $[\alpha_1, \alpha_2]$ and $[\alpha_2, 1]$). Corresponding to each interval, potential bidders will send message $m = 0$, $m = 1$ and $m = 2$, respectively. The equilibrium cut-offs α_1^{Ind} and α_2^{Ind} can be solved by considering the indifference conditions for the marginal bidder with signal $s_i = \alpha_1$ (who should be indifferent between choosing entry message "0" and "1") and with signal $s_i = \alpha_2$ (who should be indifferent between choosing entry message "1" and "2")[10]:

$$\pi(\alpha_1, m = 1) = \pi(\alpha_1, m = 0) = 0 \tag{4.3}$$

$$\pi(\alpha_2, m = 1) = \pi(\alpha_2, m = 2) \tag{4.4}$$

Note that when the entry cost c is high enough, the simultaneous equations 4.3 and 4.4 do not have a solution. Potential bidders only want to send the message "2" if they still want to enter the auction when they know they are facing competition from another entrant. When the entry cost is relatively high, the entry cut-off α_1 moves closer to the upper bound of the signal space (i.e., 1), such that all bidders who enter the auction have similar values. Under this mechanism, even the bidder with the highest signal $s_i = 1$ does not profit from sending message "2", because the valuation of another entrant is likely to be too close for the bidder's profit to cover the entry cost. The entry cost c must satisfy the following equation to ensure that $m = 2$ will be chosen:

$$\pi(s_i = 1, m = 2) > \pi(s_i = 1, m = 1) \tag{4.5}$$

When this condition is violated, the indicative-bidding mechanism will degenerate to the restricted-entry mechanism. With this result in mind, we design an experiment with both a *low-cost* and *high-cost* condition under each mechanism.

4.4 Experimental design and prediction

4.4.1 Design and implementation

Following the theoretical framework, our experiment has a 3×2 hybrid design. We vary the entry/selection process between subjects, which we denote as treatments $T \in \{Unr, Res, Ind\}$, and change the entry cost within subjects (i.e, *low-cost* and *high-cost* conditions). Every treatment contains 20 rounds of the two-stage auction with no carry-over value. The entry cost is set at $c = 5$ in rounds 1-10 and at $c = 25$ in rounds 11-20. The total number of potential bidders is fixed at five ($N = 5$). The participants are randomly assigned to groups of five in round 1 and stay with each other for ten rounds

[10] For the specific equations, see appendix C.3.

and are randomly re-matched again in round 11 (when the cost condition changes) and stay in the same group for the second 10 rounds.

In each round, the computer randomly and independently draws an integer from the uniform distribution of [0,100] for each participant as their initial signal s_i. Having observed their private signal, the participants choose an entry message, after which selected entrants are advanced to the second stage according the treatment's selection process. All the entrants then pay the entry cost and learn the additional value t which is randomly drawn for each group. This additional value follows a discrete distribution with probabilities of 25%, 50% and 25% of being 0, 100 and 200, respectively.[11] After learning the additional value, the second-price sealed-bid auction is conducted. At the bidding stage, entrants do not know the number of other entrants they are bidding against. When there is only one entrant, that entrant always wins the object and pays zero (i.e, no reserve price).

At the end of each round, all participants are provided with group-level feedback, which includes each participant's initial signal, entry decisions, (selected) entrants' bids, and each member's profits. These information is also recorded in a history table and displayed to them in every round after round 1. After participants finish all 20 rounds, the computer then randomly draws one round each from the *low-cost* and *high-cost* condition to pay them.[12]

The experiment is programmed in oTree (Chen et al., 2016) and was ran in November 2019. We recruited 360 participants campus-wide at Zhejiang Gongshang University, China and conducted four sessions per treatment, with thirty participants in each session.[13] Sessions lasted approximately 1.5 hours, and the average payment was 75 RMB, including a show-up fee of 30 RMB.

[11] See Appendix C.3 for details about how this was implemented in the experiment.

[12] To cover the entry cost, participants are given 30 experimental currency (i.e., EC) as their initial endowment. Hence, the final payment also includes the (remaining) endowment in those two rounds selected for payment. Their earnings from the payment rounds are converted to RMB using an exchange rate of 1 EC = 1.5 RMB.

[13] To help the participants understand the experimental procedure, at the beginning of each session, a video that summarizes the experimental instructions was played after they read the printed instructions. Please see Appendix C.3 for the experimental instructions and relevant screenshots. Participants were also given control questions wihch they have to answer all correctly after before they entered the main part of the experiment.

4.4.2 Predictions

For each treatment, the equilibrium predictions for the cut-offs, $\alpha^T = (\alpha_1^T, \alpha_2^T)$, and the resulting expected auction revenue, R^T, in each cost condition are given in Table 4.1.[14] Using the *Unr* treatment as a benchmark, we break down the revenues differences in the *Res* and *Ind* treatments into the differences attributable to the willingness to participate in the auction (*participation effect*) and those stemming from the mechanism's ability to select the participants with the highest value (*selection effect*). To do this, we consider the counterfactual scenarios where bidders send messages using cutoffs from treatment T, but are selected using the mechanism in treatment $\tilde{T}$. We denote the revenue of these counterfactual scenarios as $Rev(\alpha^T, \tilde{T})$.[15] We then define the *participation effect* of treatment T as $PE^T = Rev(\alpha^T, Unr) - Rev(\alpha^{Unr}, Unr)$ and the *selection effect* as $SE^T = Rev(\alpha^T, T) - Rev(\alpha^T, Unr)$.[16] The summation of these two effects is the expected revenue difference between treatment T and the *Unr* treatment.

Table 4.1: Theoretical Predictions

	Low-cost (c=5)			**High-cost(c=25)**		
	Unr	**Res**	**Ind**	**Unr**	**Res**	**Ind**
α_1	43.23	38.74	35.50	62.62	59.28	59.28
α_2			77.59			-
Revenue	151.39	146.79	155.77	109.89	116.73	116.73
$Rev(\alpha^T, Unr)$		156.536	159.39		119.38	119.38
participation effect		5.15	8.00		9.49	9.49
selection effect		-9.75	-3.62		-2.65	-2.65

As shown in Table 4.1, the expected revenue in *Ind* is higher than that of the *Unr* treatment in both the *low-cost* and *high-cost* conditions, while the relative revenue between the *Res* and *Unr* treatment depends on the entry cost.

[14]The expected revenue in equilibrium can be calculated once the entry threshold(s) are pinned down. Details can be found in the Appendix C.2.

[15]For example, $Rev(\alpha^{Ind}, Unr)$ is calculated by assuming bidders use the equilibrium cut-offs from *Ind*, but all bidders who choose $m = 1$ or $m = 2$ enter to the bidding stage and the revenue is equal to the second highest bid.

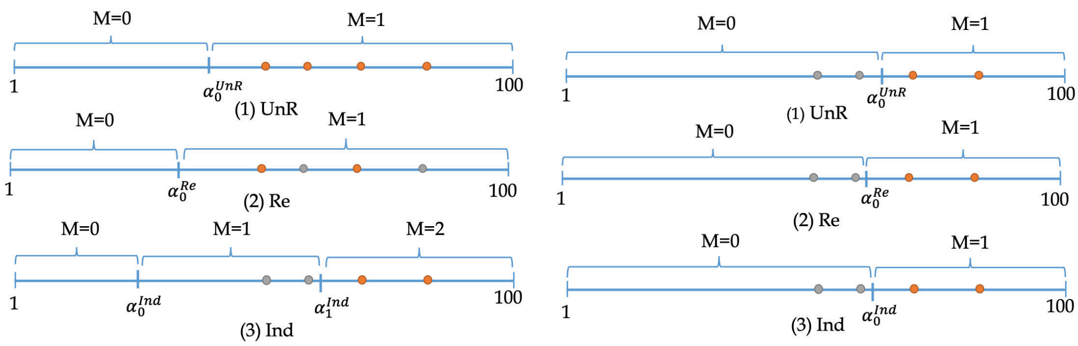
[16]Note that $Rev(\alpha^T, T) = R^T$.

Prediction 1 (***Revenue ranking***) *The relative revenue ranking of the three treatments depends on the entry-cost condition. Specifically: when entry cost is low,* $R^{Ind} > R^{Unr} > R^{Res}$*; when entry cost is high,* $R^{Ind} = R^{Res} > R^{Unr}$*.*

As is shown in Table 4.1, the relative advantage of the *Ind* treatment in terms of revenue is due to a larger positive *participation effect* relative to the negative *selection effect* (8 vs -3.62 in the *low-cost* condition and 9.46 vs -2.65 in the *high-cost* condition. On the contrary, the *Res* treatment performs the worst among all three mechanisms when the entry cost is low, which is driven by a more negative selection effect (-9.75 vs 5.15). When the entry message $m = 2$ is not required, the *Res* treatment as the same participation and selection effect as the *Ind* treatment.

To illustrate the *participation effect* intuitively, we can use the examples depicted by Figures 4.1a and 4.1b. The real line indicates the signal space $U[0, 100]$. The dots represent the signals of potential entrants.

Figure 4.1: Entry Thresholds and Selection



(a) Low entry cost (b) High entry cost

In the treatment with unrestricted entry, uncertainty about the competition in the auction stage lowers the willingness to pay the entry cost to enter the auction. The restricted-entry mechanism fixes the maximum number of entrants to two which ensures that an entrant faces at most one competitor in the auction stage. As a consequence, an entrant is more likely to be the highest-value bidder in the restricted-entry treatment than in the unrestricted treatment. This follows because interested bidders are less likely to be selected when the auction is more competitive (i.e. more participants are willing to enter). This encourages potential bidders to participate and lowers the entry cutoff from α_1^{UnR} to α_1^{Res}.

Indicative bidding (*Ind*) pushes the cutoff point further to the left because bidders with lower values (for instance, s_i slightly below α_1^{Res}) have the option to choose a message with lower entry priority. Using the message $m = 1$ decreases even further the chance the bidder is selected into an auction against a higher-value bidder who is more likely to use $m = 2$. Hence, there is a larger positive *participation effect* in the *Ind* treatment compared with the *Res* treatment when the bidders with the highest values are willing to choose $m = 2$. When the entry cost is too high, as in the *high-cost* treatment, all bidders prefer this safer message, and hence no bidders choose $m = 2$. Under this condition, the *Res* and *Ind* treatments are equivalent and therefore, result in the same positive *participation effect*.

Prediction 2 (*Participation effect*) *Under both cost conditions, both Res or Ind treatments have positive participation effects. When entry cost is low, $PE^{Ind} > PE^{Res} > 0$; when the entry cost is high, $PE^{Ind} = PE^{Res} > 0$.*

In the unrestricted-entry treatment, no bidder will be excluded once they choose to participate. In both the *Ind* and *Res* treatments, stochastic selection processes cannot ensure that the two highest value bidders that are willing to participate are actually selected into the auction (see Figures 4.1a and 4.1b). In the *Ind* treatment under the *low-cost* condition, bidders with high values will choose the higher entry message in the *low-cost* mechanism. This prioritized selection process will increase the chance of selecting the bidders with the highest value over the completely random process of *Res*. In the *high-cost* condition, all participating bidders will select $m = 1$ in both *Ind* and *Res* leading to a random selection process in both treatments.

Prediction 3 (*Selection effect*) *Under both cost conditions, both Res and Ind treatments have negative selection effects. When the entry cost is low, $SE^{Res} < SE^{Ind} < 0$; when the entry cost is high, $SE^{Res} = SE^{Ind} < 0$.*

4.5 Experimental Results

In this section, we first compare the auction revenues across treatments and examine how revenue rankings are affected by the *participation effect* and *selection effect*, respectively in Section 4.5.1. Then we report the individual bidding and entry choices in contrast to the equilibrium predictions, and how each has driven the discrepancies between the observed and predicted revenues in each treatment in Section 4.5.2. Lastly, we investigate the bidder's profit and social welfare in Section 4.5.3.

4.5.1 Auction revenue: participation and selection effects

Table 4.2 presents panel-regression results on auction revenue under each cost condition, controlling for the group-level random effect. In all regressions, the *Unr* treatment is set as the baseline group. In model 1, each of the *Res* and *Ind* treatments is compared to the *Unr* treatment, while the second-highest value of each group and the time trend (i.e., round number) are also added as control variables in model 2.[17] When the entry cost is low, the revenue generated in the *Res* treatment is around 13.90 units lower than that of the *Unr* treatment, whereas the revenue in the *Ind* treatment is also slightly lower than that of the *Unr* treatment, but the difference is not significant (see the second and third row in Table 4.2).[18] Under the *high-cost* condition, the average revenue in the *Ind* treatment is 25.37 units higher than that of the *Unr* treatment. However, the same (predicted) revenue advantage of the *Res* treatment in high-cost condition is not observed in our data. These results provide partial support for Hypothesis 1.

Result 1 *When the entry cost is low, the Ind and Unr treatments generate similar auction revenue and they both generate significantly higher revenue than the Res treatment; When entry cost is high, the average revenue in the Ind treatment is significantly higher than that of the other two treatments.*

[17] When estimates are reported, we will focus on model 2 only given that they are similar across the two models.

[18] The revenue in the *Res* treatment is also significantly lower than that of the *Ind* treatment ($p-value = 0.046$)

Table 4.2: Panel regressions on auction revenue

	Model 1		**Model 2**	
	low-cost	highcost	*low-cost*	highcost
Constant (Unr)	156.3***	107.1***	-10.28	-31.57***
	(5.74)	(7.44)	(7.21)	(10.53)
Res	-15.51*	-2.12	-13.90***	5.65
	(8.12)	(10.52)	(5.31)	(8.85)
Ind	-7.22	24.79**	-3.31	25.37***
	(8.12)	(10.52)	(5.31)	(8.84)
V_{2nd}			0.94***	0.84***
			(0.03)	(0.03)
Round			1.46**	0.24
			(0.71)	(1.02)
# of Obs.	720	720	720	720
# of Groups	72	72	72	72

Note: Both Model 1 and Model 2 use panel regressions, while model 2 further controls for the second-highest value in the group (V_{2nd}) and the time trend (i.e.Round). *** Significant at the 1% level, ** at the 5% level, and * at the 10% level.

Table 4.3: Participation effect and selection effect

	Low-cost (c=5)				**high-cost (c=25)**			
	Res		**Ind**		**Res**		**Ind**	
	Obs.	**Pred.**	**Obs.**	**Pred.**	**Obs.**	**Pred.**	**Obs.**	**Pred.**
Participation	2.85*	6.96	7.99	9.14	9.88	11.06	27.1***	6.28
	(43.67)	(34.99)	(57.80)	(40.42)	(80.38)	(43.49)	(91.43)	(32.14)
Selection	-17.39***	-10.28	-13.73***	-4.83	-7.64***	-2.04	-7.28***	-2.15
	(20.92)	(9.87)	(19.10)	(6.23)	(17.04)	(4.33)	(14.31)	(4.60)

Note: Columns labeled "Obs." present the observed *participation effect* and *selection effect*. Columns "Pred." present the predicted effects, using the actual values drawn in the experiment and assuming participants all follow the equilibrium entry and bidding behaviors. Numbers in the round parentheses are the standard deviations. Asterisks next to the observed effect stands for the significance level of the t-test (clustered on the group level) comparing the observed effect to the equilibrium prediction. *** Significant at the 1% level, ** at the 5% level, and * at the 10% level.

In theory, the cross-treatment revenue comparisons should be purely driven by the entry outcomes since entrants should simply bid their valuations in stage 2. The entry outcomes are co-products of the entry cutoffs shifted across treatments and the selection process. To study how entry outcomes affect the observed revenue differences across treatments and their deviations from the predicted rankings, we calculate both the predicted and observed participation and selection effects (as defined in Section 4.4), given the signals drawn for each treatment in the experiment.[19] Specifically, the predicted effects in 4.3 use equilibrium cutoffs to determine how drawn signals should map to entry choices while the observed effects use the actual entry messages chosen by the participants.[20] We use equilibrium bidding strategies in these calculations to segregate the impact of potential over-/under-bidding behavior on auction revenue.[21] Table 4.3 presents the summary statistics of these effects and p-values comparing them to corresponding equilibrium predictions. Panel regressions on across treatment comparisons are presented in Table 4.4

Table 4.4: Panel regressions: *participation effect* and *selection effect*

	Participation effect				Selection effect			
	Model 1		Model 2		Model 1		Model 2	
	low-cost	highcost	*low-cost*	highcost	lowhcost	highcost	*low-cost*	highcost
Res (Cons.)	2.85	9.88	-6.28	7.25	-17.39***	-7.64***	-12.34***	-5.09**
	(3.86)	(7.05)	(7.87)	(13.09)	(1.74)	(1.29)	(3.13)	(2.37)
Ind	5.13	17.22*	5.23	17.01*	3.65	0.36	3.55	0.58
	(5.46)	(9.97)	(5.47)	(10.05)	(2.46)	(1.82)	(2.48)	(1.81)
V_{2nd}			0.04	0.02			-0.04***	-0.03***
			(0.03)	(0.05)			(0.01)	(0.01)
Round			0.43	-0.21			0.37	0.26
			(0.80)	(1.33)			(0.30)	(0.24)
Obs.	480	480	480	480	480	480	480	480
# of Groups	48	48	48	48	48	48	48	48

Note: Model 1 and Model 2 are both panel regressions at the group level, while model 2 further controls for the second-highest value in the group (v_{2nd}) and the time trend variable "Round". Standard errors are in the parenthesis. *** Significant at the 1% level, ** at the 5% level, and * at the 10% level.

As shown in Table 4.3, the average *participation effect* is positive in all treatment-cost conditions. In the *Ind* treatment and high-cost condition the observed effect is much

[19]Predicted effects given in Table 4.3 differ from those in Table 4.1 which are calculated using the prior distribution of signals.

[20]Note that in calculating the observed participation effect, we do not observe the counterfactual entry choices under the unrestricted treatment for the signals drawn in the other treatments. Therefore, in order to calculate $R(\alpha^{Unr}, Unr)$ we use the equilibrium cutoffs in *Unr* and the drawn values from the relevant treatment.

[21]We will examine individual bidding behavior and it impact on revenue in the next subsection.

higher than its prediction (27.1 vs 6.28, $p = 0.004$). All other observed effects are all slightly smaller than the predicted values, though the differences are not statistically significant. Using panel regressions to assess the treatment difference (see Table 4.4), we find that the participation effect is generally higher in *Ind* (5.23 in low-cost condition and 17.01 in high-cost condition), though the difference compared to *Res* is only marginally significant under the high-cost condition. When we compare the observed effects to zero, they are generally not significant in the *Res* treatment ($p = 0.460$ in *low-cost* and $p = 0.161$ in *high-cost*), whereas we can reject that the participation effects are equal to zero in the *Ind* treatment ($p = 0.03$ in *low-cost* and $p < 0.01$ in *high-cost*).[22]

Result 2 *Although the average participation effects are positive in both the Res and Ind treatments under both conditions, the effects are only significantly different from zero in the Ind treatment. The effect in Ind is marginally larger than the effect in Res in the high-cost condition, but not in the low-cost condition.*

Beside the *participation effect*s, the random selection occurring in the *Res* and *Ind* treatments (when more than two interested bidders choosing to enter) are predicted to have negative *selection effect*s on auction revenue irrespective of whether the entry cost is low or high. As shown in Table 4.4, the selection effects are all negative and significantly different from zero. Furthermore, the *negative* selection effects are all significantly bigger than predicted under all treatment-cost conditions (see Table 4.3).

While the theory predicts that the *Ind* treatment should have a less negative *selection effect* when the cost is low, we do not observe significant treatment difference in terms of the selection effect under either cost condition (see the right panel of Table 4.4). When entry cost is high, message "2" is predicted to be not useful and hence the two treatments should have the same level of selection inefficiency. While this is in line with observed differences in the selection effect, there is still widespread use of the message "2" in the *Ind* treatment. These results indicates that participants used the entry messages differently than predicted, which we investigate in Section 4.5.2.

Result 3 *Following the theory, selection effects are always negative but the size of effect is always bigger than what is predicted by the equilibrium. Selection inefficiency impacts the Res and Ind*

[22]These two p-values are obtained from the Wald tests (based on the panel regression results in model 1), comparing the sum of the two estimates for *Res* and *Ind* against zero.

treatments similarly under both the low-cost and high-cost conditions.

In summary, we find that in the *high-cost* condition the revenue in the Ind treatment is much higher than the other treatments which is largely driven by the surprisingly high *participation effect*; in the *low-cost* condition the advantage of the *Ind* treatment compared to the *Unr* treatment is dampened by both the smaller (positive) *participation effect* and the larger (negative) *selection effect*, such that the predicted treatment difference between *Ind* and *Unr* is not observed. Unfortunately the *Res* treatment, which could be a simpler mechanism used under the *high-cost* condition to maximize auction revenue, lowers revenue in both cost conditions compared to the *Ind* treatment.

4.5.2 Comparisons to equilibrium predictions

After comparing auction revenue across treatments and identifying the impact of the *participation* and *selection effects*, we now investigate individual bidding and entry choices. We compare these choices to equilibrium predictions and use them to further understand the observed cross-treatment revenue comparisons.

Bidding Behavior

Theory predicts that in a second-price auction, participants' bids should equal to their value for the asset. Figure 4.2 shows participants' bids (on the vertical axis) against their values (on the horizontal axis). The majority of the bids are very close to the drawn values (i.e. near the 45 degree line). Among the bids which deviate from this, there appears to be more overbidding than underbidding in all treatments, a phenomenon which has been identified previously in the experimental literature (see Kagel et al. (1987)). Comparing the *high-cost* to *low-cost* conditions in the same treatment, Figure 4.2 shows that while both the number of overbids and underbids decreases, the number of underbids decreases more obviously, further suggesting that over-bidding is a rather persistent phenomenon in these second-price auctions.[23]

We calculate the bid-value ratio (*B-V ratio*), which is the observed bid divided by the valuation v, for each bid. A *B-V ratio* greater than 1 indicates overbidding and

[23]Because the *high-cost* rounds follow the *low-cost* rounds, subjects have had more time to learn about bidding in the second price auction in those treatments.

a *B-V ratio* lower than 1 indicates underbidding. To compare the bidding behavior across treatments, we regress *B-V ratio* on treatment dummy for *low-cost* and *high-cost* conditions separately using the multi-level mixed effect model. Table 4.5 presents the regression results. When entry cost is low, participants bid 15%, 22% and 35% more than their valuation in *Unr*, *Res*, and *Ind* treatments, respectively ($p = 0.042$, $p = 0.012$ and $p < 0.01$), whereas these numbers become 17%, 29% and 29% under the high cost condition. Comparing the overbidding across treatment for a given cost condition, the differences are not statistically significant at 5% of significance level.

Result 4 *In line with previous experimental evidence on second-price auctions, we also find significant and consistent overbidding in all treatments. However, the over-bidding behavior is not significantly different across treatments.*

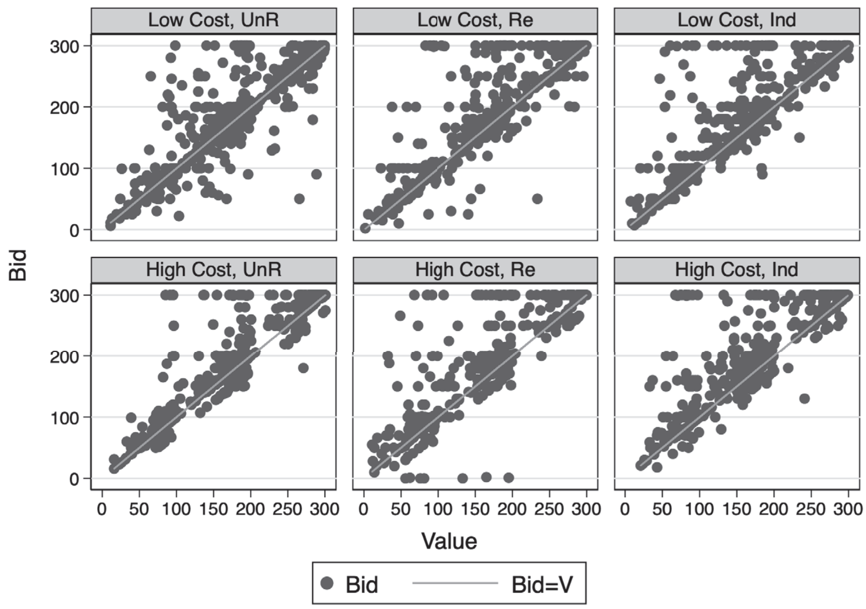


Figure 4.2: Bidding behavior

Entry behavior

To compare the observed entry choices to their equilibrium predictions, we break down participants' entry choices into Nash entry, over-entry and under-entry categories. Nash entry refers to the entry choices that are consistent with the equilibrium predictions given the signals. Over (under) entry refers to the entry choices wherein a higher

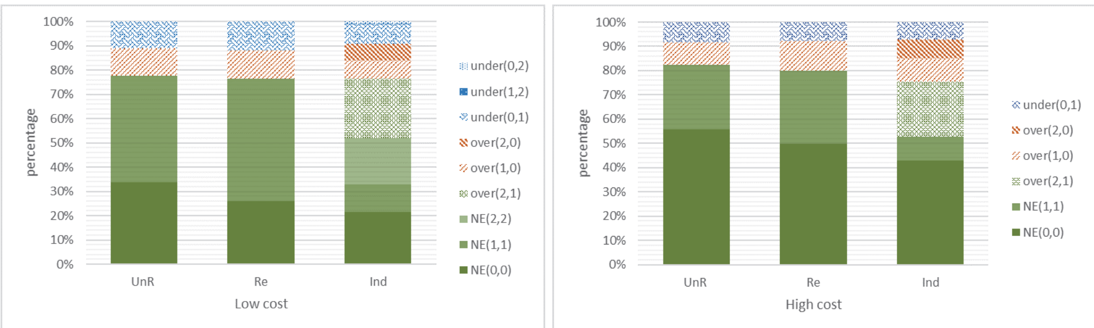
Table 4.5: Mixed-effect regression of *bid-value ratio*

	low-cost		Highcost	
	Coef	*Wald* test	Coef	*Wald* test
Unr (Cons.)	1.15***	$p = 0.042$	1.17***	$p < 0.01$
	(0.08)		(0.06)	
Res	0.07	$p = 0.012$	0.12	$p < 0.01$
	(0.11)		(0.09)	
Ind	0.20*	$p < 0.01$	0.12	$p < 0.01$
	(0.11)		(0.09)	
σ^2_{group}	0.08		0.02	
	(0.03)		(0.02)	
$\sigma^2_{individual}$	0		0.07	
	(0)		(0.03)	
# of Obs.	1,581		1,252	
# of Groups	72		72	

Note: Mixed-effect regressions control for both group and individual levels of random effects. *Wald* test are used to compare the observed *B-V ratio* with 1).[24]

(lower) entry message was chosen compared to the equilibrium prediction for that signal.

Figure 4.3: Entry choices compared to equilibrium predictions



(a) Low cost (b) High cost

Note: "NE" represents the Nash entry choice, "over" represents over entry and "under" represents under entry. The first number in the parenthesis is the actual entry message chosen by the participants, while the second number in the parenthesis is the entry message predicted by equilibrium.

As the stacked-bar graphs show in Figure 4.3a, the Nash entry, represented by the dark-green solid shares on each bar, accounts for 77.58% and 76.42% of all entry choices in the *Unr* and *Res* treatments respectively, under *low-cost* condition. In contrast, in *Ind*, only 52.08% of the entry choices are in line with the predictions. For the entry choices that deviate from the equilibrium, the percentage of under-entry choice and over-entry choice is balanced within each treatment in both the *Unr* and *Res* treatments(11% versus 11.42% in *Unr* and 11.75% versus 11.83% in *Res*).[25] This leads to the remarkably close-to-equilibrium distributions of the number of participants who chose to enter in these treatments (see Figure 4.4). On the contrary, in the *Ind* treatment, there is only 9.25% under entry, but 38.67% over entry, which shifts the distribution of the number of participants who chose to enter to the right in Figure 4.4. The majority of the over-entry choices in the *Ind* treatment is driven by the misuse of $m = 2$ when $m = 1$ should be chosen (accounting for 24.58% of the total entry choices). There are around 15% of entries (including instances when either $m = 2$ or $m = 1$ was chosen) that are not predicted by the theory.

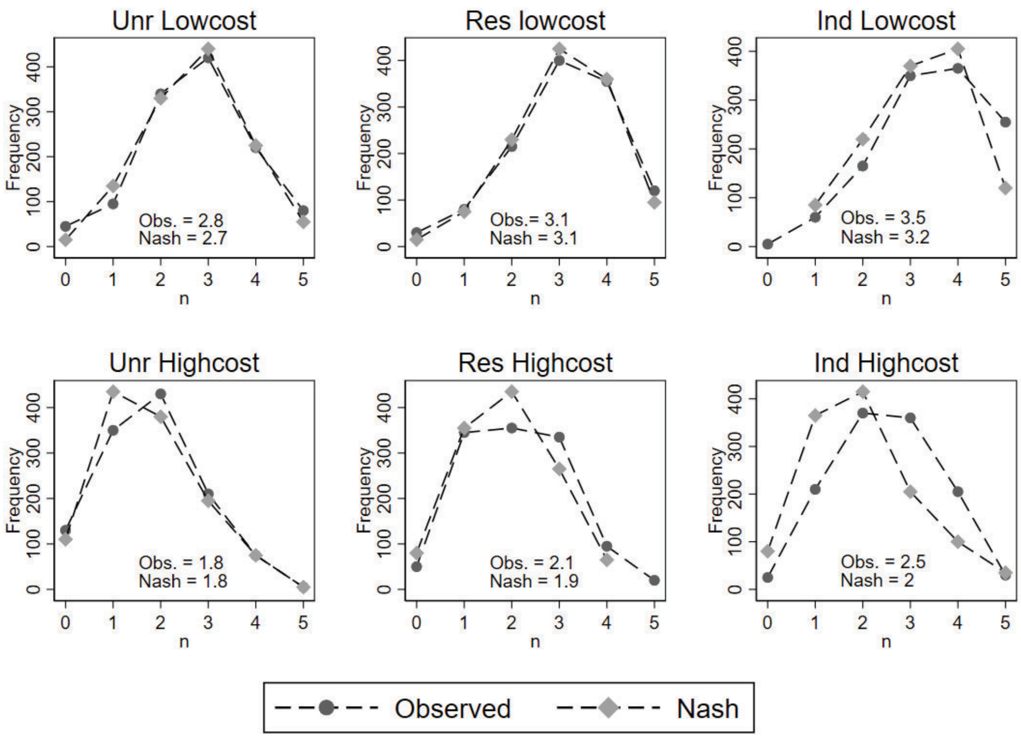


Figure 4.4: Frequency of the average number of participants chose to enter

For the *high-cost* condition, similar results as the *low-cost* condition hold for the *Unr* and *Res* treatments (see Figures 4.3b and 4.4). From Figure 4.4 we can observe that the over-entry allows the *Ind* treatment to have much fewer incidences of 0 or

[25]See the detailed summary statistics in Appendix C. We define over-entry (under-entry) as participants' entry choices that are higher (lower) than what they should have chosen in the equilibrium. Over-entry includes participants choosing $m = 1$ or $m = 2$ when they should have chosen $m = 0$ or $m = 1$, respectively. Under-entry includes participants choosing $m = 0$ or $m = 1$ when they should have chosen $m = 1$ or $m = 2$, respectively.

1 interested bidders in the auction compared with equilibrium (19.58% vs 37.09%) in *high-cost* condition, whereas the chance of 0 or 1 interested bidders in the auction in the *low-cost* condition is similar with the prediction (5.42% vs 7.08%). Whenever the auction has less than two bidders who chose to enter, the auction revenue is zero (given a second-price auction with no reserve price). This helps explains the larger than predicted *participation effect* observed in *Ind* treatment under the *high-cost* condition.

To further compare the entry behavior across treatments, we also estimate the probability of each message being chosen given the initial signal s_i (see Table C.2 in Appendix **??**). Similar to the observations in Figure 4.4, regardless of whether the entry cost is high or low, participants in the *Ind* treatment always have a significantly higher probability of choosing to enter (including both $m = 1$ and $m = 2$) than those in the Res treatment ($p < 0.01$, *t*-test) given a s_i, while participants are more likely to enter in *Res* compared with their counterpart in *Unr* ($p < 0.01$, *t*-test). Table C.2 further shows how often message "2" is overused in the *Ind* treatment, which together with over-entry, explain the higher than predicted negative *selection effect* observed in the experiment.

Result 5 *Except a small portion of mistakes with a balanced account of both over-entry and under-entry, almost 80% of the entry choices followed equilibrium predictions in both the Res and Unr treatments. However, only around 50% of the entry choices followed equilibrium prediction in the Ind treatment. The deviations from the equilibrium behavior are largely driven by over-entry and the misuse of message "2", which lead to both the higher than expected participation effect and more selection inefficiency observed in the Ind treatment under high-cost condition.*

Auction revenue compared to equilibrium

Having observed deviations of both the participants' bidding and entry behaviour, we put these together to consider the impact on deviations from predicted auction revenue. Figure 4.5 compares the average revenue in each treatment to its equilibrium prediction using the realised values from the experiment. In almost all treatments except for *Ind* with high entry cost, the observed revenue is very close to predictions.[26] When the

[26]Note that although the observed average revenue is remarkably close to the prediction based on the realized valuations, the revenue ranking only partially follows the equilibrium prediction. The main

entry cost is low, the observed average revenue compared with the prediction is 156.25 versus 153.9 in the *Unr* treatment, 140.75 versus 145.69 in the *Res* treatment and 149.04 versus 150.02 in the *Ind* treatment. None of them are statistically different from the prediction ($p > 0.1$ for all three treatments, *Wald*-test). When entry cost is high, the discrepancies between the observed and predicted revenue in *Unr* and *Res* treatments are still not significant ($p > 0.1$ for both treatments, *Wald*-test), while the observed average revenue is significantly higher than the prediction in the *Ind* treatment (131.89 versus 106.84, $p < 0.01$, *Wald*-test).

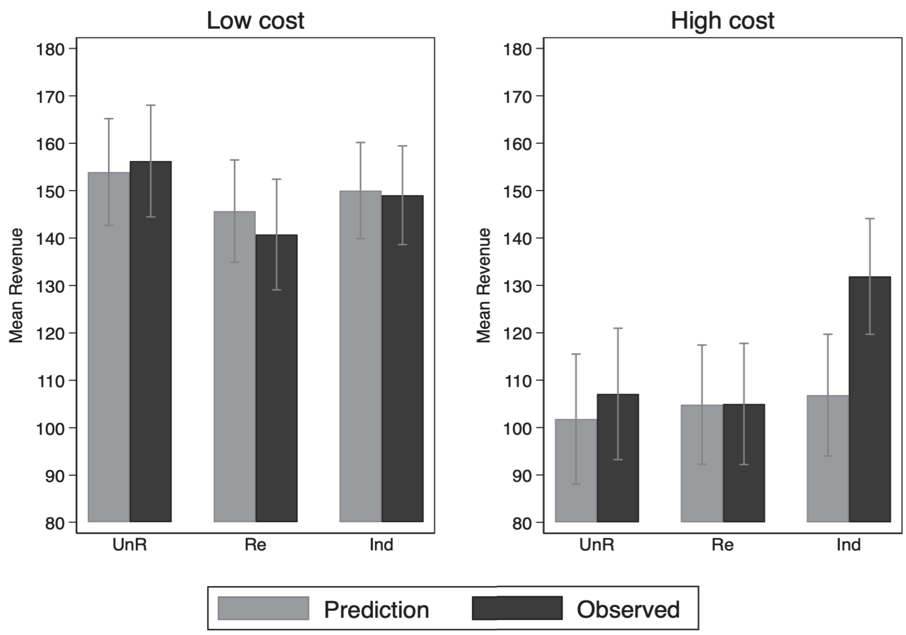


Figure 4.5: Average revenue

Connecting the dots we lined up in the former analysis, the (higher than predicted) revenue advantage in *Ind* treatment with high entry cost is mainly driven by participants' entry outcomes, more specifically, the prominent *participation effect*. By having significantly fewer auctions with 0 or 1 bidders, the indicative bidding avoid ending up with auction failure and zero auction revenue. Consequently, it generated higher auction revenue compared with other mechanisms and compared with the equilibrium.

reason is that the random draws of values are slightly different across treatment. In the previous analysis on revenue ranking, we account for this problem by controlling for the second highest value in each group.

4.5.3 Bidders' profit and social welfare

To fully assess the effectiveness of these three mechanisms, we also compare the bidders' profit and social welfare (total profit of buyers and sellers) across treatments. We measure the bidders' profit using the sum of the profit of all five potential bidders in each group for each decision round, and further calculate the social welfare as the sum of bidders' profit and the auction revenue.

Table 4.6: Bidders' profit and social welfare: Panel regression

	total bidders' profit				social welfare			
	Model 1		Model 2		Model 1		Model 2	
	lowcost	highcost	lowcost	highcost	lowcost	highcost	lowcost	highcost
Constant (Unr)	0.96	7.03	-13.36***	-26.90***	157.2***	114.1***	-27.12***	-59.02***
	(3.76)	(7.07)	(4.37)	(8.21)	(5.43)	(5.51)	(3.16)	(4.18)
Res	15.81***	4.275	15.84***	6.38	0.30	2.15	2.04	12.05***
	(5.32)	(10.00)	(5.19)	(9.81)	(7.68)	(7.79)	(2.62)	(3.79)
Ind	10.30*	-15.31	8.623*	-12.92	3.08	9.48	5.40**	11.39***
	(5.33)	(10.00)	(5.20)	(9.81)	(7.68)	(7.79)	(2.62)	(3.78)
$v_1 - v_2$			0.91***	1.81***				
			(0.15)	(0.23)				
v_1							1.00***	0.95***
							(0.01)	(0.02)
Obs.	720	720	720	720	720	720	720	720
# of groups	72	72	72	72	72	72	72	72

Note: Model 1 and Model 2 are both panel regressions, while model 2 further controls for the difference of the highest value and the second-highest value in the group $(v_1 - v_2)$in bidders' profit and controls for the highest value in the group (v_1) in social welfare. Standard errors are in the parenthesis. *** Significant at the 1% level, ** at the 5% level, and * at the 10% level.

Table 4.6 presents the panel regressions of bidders' profit and social welfare on a treatment dummy for each cost condition. Bidders in *Ind* and *Res* earn respectively 10.42 units and 15.86 units more profit compared to their counterparts in *Unr* treatment when the entry cost is low ($p = 0.05$ and $p = 0.003$, respectively, *Wald* test). There is no significant difference between *Ind* and *Res* treatments ($p = 0.31$, *Wald* test).[27] When entry cost is high, the bidders' profit is not significantly different in *Res* and *Unr* treatments ($p = 0.63$, *Wald* test), whereas the bidders' profit is significantly lower in *Ind*

[27]We discus the results based on Model 2, where the variation of random draw is controlled.

treatment than that in *Res* treatment ($p = 0.044$, *Wald* test). Due to the overbidding and mistakes participants made in entry choices in the experiment, the observed bidders' profit in *Unr* and *Ind* treatments are significantly lower than predicted (See the predictions in the Appendix Table **??**) regardless of the entry cost.[28] Although a similar level of overbidding and entry mistakes is observed in both *Res* and *Unr*, the bidders' profit follows the prediction well in *Res* ($p = 0.22$ and $p = 0.43$, respectively, t-test). This is because there is no restriction on the number of entrants in *Unr*. Any bidder who chooses to enter will pay the entry cost, whereas the maximum number of bidders that can pay the entry cost is limited to two in the *Res* treatment.

In terms of the social welfare, the *Ind* treatment generates significantly higher social welfare (compared to *Unr* treatment) regardless of the entry cost (see the right panel of Table 4.6). The advantage in auction revenue in the *Ind* is fully offset by the drop in the bidders' profit in both cost conditions such that it is on a par with the *Res* treatment on social welfare (p-values are 0.199 and 0.862 respectively)

Result 6 *The social welfare is higher in Ind treatment than in the Unr treatment, and there is no significant difference between the Ind and Res treatments, under both cost conditions.*

4.6 Conclusion

In this paper, we use the experimental method to investigate a "simple" indicative bidding mechanism which theory predicts should generate more revenue than a standard two-stage auction when the information rent after entry is small. In the selling of complex assets, the bidders possess only a part of the information required for valuation. They have to pay the significant entry cost to complete the rest of their valuation. Most information obtained after entry has a similar impact on the bidders, which means the post-information rent is small. The seller's and buyers' interests are aligned under this condition. Sellers only want the bidders with the highest value to enter, and only those buyers who have high enough pre-entry value can profit from entering.

In theory, under the assumption that the post-entry information is highly correlated

[28] $p = 0.026$ and $p = 0.003$ in *Unr*, $p = 0.017$ and $p = 0.026$ in *Ind* under *low-cost* and *high-cost* condition, respectively, t-test

among bidders, indicative bidding should perform better (weakly better) than unrestricted (restricted) auctions in terms of revenue generation. We are interested to see whether this can be proven by experimental evidence. More importantly, we seek to examine how participants behave in terms of entry, bidding, and how their behavior affects the auction revenue.

The primary result of our experiment is on auction revenue. We find that indicative bidding does have an advantage among the three mechanisms we considered. Indicative bidding performs significantly better than the restricted auction and as good as the unrestricted auction when the entry cost is low. In both indicative bidding and the restricted auction, the participation effect has a slightly positive impact on revenue as predicted. However, due to the non-equilibrium entry choices participants made, the negative effect of inefficient in selection is much higher than it should be in theory. Consequently, the indicative bidding treatment fails to outperform the unrestricted auction. When the entry cost is high, the participation effect still has a marginal impact on revenue in the restricted auction. In contrast, in indicative bidding, due to the significantly fewer auctions with 0 or 1 bidders, the participation effect has a strong positive impact on revenue. At the same time, higher entry cost increases the entry threshold. As bidders with lower values drop out, the negative selection effect is smaller. The overall impact of these two factors is that indicative bidding outperforms both the other two mechanisms when the entry cost is high.

In addition to the revenue results, we also find that participants' entry behaviour mostly follows the prediction. We only see very mild over-entry in the indicative bidding treatment with both high and low entry cost and in the restricted auction treatment with the high entry cost. As for selection efficiency, the unrestricted treatments is the most efficient, while indicative bidding and the restricted auction treatment have no significant difference in selection. In line with the previous experiment on second-price auction, we find a similar degree of overbidding across all treatments. Last but not least, we observe the misuse of entry message 1 and 2 observed in the indicative bidding treatment. This phenomenon is not abnormal since the entry choice in the indicative bidding treatment is more demanding compared with the other two treatments. However, it raises questions for future research regarding factors that cause these mistakes and how the auctioneers can improve the mechanism or nudge the bidders to make the right choices.

The current study contributes on the experimental literature of auctions with costly entry. By assuming correlated values among bidders, our experiment takes the literature a step closer mimicking the auction of complex assets (which usually involves millions of dollar). Even the slightest difference shown in the experiment could mean massive profit changes for the auctioneer. Our experiment provides some supportive evidence for the use of indicative bidding. In reality, the process of information learning after entry usually reveals important and non-public information about the asset. Auctioneers may wish to limit the number of entrants, even at the cost of selection efficiency. The results in this paper show that indicative bidding is one potential mechanism they can consider.

Chapter 5

General Conclusions

Contests and auctions play an essential role in modern economic activities. This thesis is part of the growing literature that studies how the design of contests and auctions affect agents' behavior in these activities. From the perspective of the contest or auction organizer, this thesis conducted three laboratory experiments to investigate: (1) how the information disclosure policy can affect contestants' effort elicited in a Tullock contest; (2) how contestants' risk-taking behavior is affected by the prize allocation and the size of the tournament; and (3) which two-stage auction mechanism can select and achieve the highest revenue for the auctioneer.

Chapter 2 investigated the design of a Tullock contest with endogenous entry. The model developed in this chapter indicates that whether to conceal or reveal the number of actual contestants in the contest depends on the curvature of the cost function. A 2 by 3 laboratory experiment which varies the disclosure policy (fully disclosed or fully concealed) on one dimension and the cost function (concave, linear or convex) on another dimension was used to test the predictions of the model. The main findings in the experiment are aligned with the theory. When the cost of effort function is concave (convex), the total effort invested by the contestants is higher when the number of actual entrants is disclosed (concealed) to the contestants. The experimental results also support the prediction that the disclosure policy does not affect contestants' entry probability. In this experiment, we also also find that overbidding, which is commonly observed in the contest literature, can be largely explained by over-entry in the first stage. This study contributes to the literature on Tullock contests with costly entry both theoretically and experimentally. Contests with an entry-stage extend contestants' decision to two dimensions. There is only a handful of studies examining the relationship between contest design and contestants' behavior under this framework. Chapter 2 provides another structural factor within the contest, namely the disclosure policy, that the organizer can utilize to regulate contestants' behavior.

Chapter 3 examined another aspect of contestants' behavior in contests: risk-taking behavior. In the experiment, contestants only decide the level of risks they are willing to take by building a performance distribution using a visualized distribution builder. Their realized performance and hence their rankings and the prize they get in the tournament depends on the distribution they build. By varying the prize allocation and the size of the tournament, we inspect how contestants' risk-taking behavior (the distribution they build) change in reaction to the change in tournament design. The results from the experiment suggest that when more entrants are added to a contest, contestants will choose a more dispersed distribution. In contrast, when the convexity of the prize schedule increased, contestants will choose not only a more dispersed but also a more skewed distribution. The results in Chapter 3 are enlightening from risk governance perspective. Previous studies on risk-taking in tournaments commonly measure risk using the variance. The experimental results of this chapter prove that for the most of prevalent tournaments (e.g. winner-takes-all tournament), using the variance as the only risk measurement might lead to underestimation of the overall volatility.

Chapter 4 investigated the revenue generation and selection efficiency of the indicative bidding mechanism. The experiment has three treatments, each with one auction mechanism. In the unrestricted entry treatment, all bidders who choose to enter can enter the auction stage; in the restricted entry treatment, at most two bidders who choose to enter are randomly selected to enter the auction stage; and in the indicative bidding treatment, at most two bidders who send the highest entry message are selected to enter the auction. Principally following the prediction, the indicative bidding treatment performs significantly better than the restricted auction, and as well as the unrestricted auction when the entry cost is low. When the entry cost is high, indicative bidding generates the highest revenue among the three mechanisms. This study has two key contributions. Firstly, we find supportive evidence for theoretical predictions and the wide application of indicative bidding in practice; secondly, unlike the field data where the entry decision and bidders' private value are usually not observable, the experimental method provides the opportunity to examine the channels that contribute to the auction revenue. Using the experimental data in this study, we disentangle two key effects that affect the revenue generation in two-stage auctions: the participation effect and the selection effect. Furthermore, we find that the main reason that indicative bidding has an advantage over the other two mechanisms is that it increases bidders' entry probability. Potentially, the findings in this study can shed light on the design of auctions with complex assets and very limited bidders.

Appendix A

Appendix of Chapter 2

A.1 Additional results

Table A.1: Individual effort in concealed treatments: mixed-effects regressions (Rounds 14-25)

VARIABLES	Concave	Linear	Convex
Effort	155.60***	22.85***	9.76***
	(12.30)	(2.46)	(0.54)
$\sigma^2_{(sub)session}$	258.59	14.44	0.00
	(435.39)	(17.63)	(0.00)
$\sigma^2_{individual}$	3,007.12	82.35	11.02
	(793.56)	(21.46)	(2.69)
Equ.	117.97	18.12	7.42
p-value	*0.00*	*0.05*	*0.00*
Adjusted Equ.	160.44	19.56	7.63
p-value	*0.69*	*0.18*	*0.00*
No. of Groups	4	4	4

We estimate the average individual effort for different cost functions separately with mixed-effects models to control for the random effects at the individual and (sub)session levels, using data from rounds 14-25. The p-values under "Equ." and "Adjusted Equ. " are from Wald tests, and compare the estimated average individual effort with the corresponding predictions. Stars indicate the significance level of each coefficient (** $p < 0.05$, *** $p < 0.01$).

A.2 Proof

Equilibrium Characterization when N is Disclosed

Whenever $N \geq 2$, each participant i chooses his level of effort x_i to maximize his expected payoff

$$\pi_i = \frac{x_i^r}{\sum_{j=1}^{N} x_j^r} V - x_i^\alpha,$$

The unique equilibrium effort x_N^* is determined by the first order condition

$$r\frac{N-1}{N^2 x_N} v = \alpha x_N^{\alpha-1}.$$

Note that payoff π_i of a representative contestant i is globally concave in x_i given all others taking the effort of x_N^*, therefore $x_N^* = \left(\frac{N-1}{N^2}\frac{rV}{\alpha}\right)^{\frac{1}{\alpha}}$ is a unique symmetric equilibrium. And the equilibrium payoff is $\pi_N^* = \frac{1}{N}V - (x_N^*)^\alpha = \frac{V}{N}\left(1 - \frac{N-1}{N}\frac{r}{\alpha}\right)$.

Equilibrium Characterization when N is Concealed

Consider an arbitrary potential bidder i who has entered the contest. Suppose that all other potential bidders play a strategy (q_C, x_C) with $x_C > 0$.[1] He chooses his bid $x_{i,C}$ to maximize his expected payoff

$$\pi_i(x_{i,C}|\, q_C, x_C) =_{N=1}^{M} C_{M-1}^{N-1} q_C^{N-1}(1-q_C)^{M-N}[\frac{x_{i,C}^r}{x_{i,C}^r + (N-1)x_C^r}V - x_{i,C}^\alpha].$$

Differentiating $\pi_i(x_{i,C}|\, q_C, x_C)$ with respect to $x_{i,C}$ yields

$$\frac{d\pi_i(x_{i,C}|\, q_C, x_C)}{dx_{i,C}} =_{N=1}^{M} C_{M-1}^{N-1} q_C^{N-1}(1-q_C)^{M-N}\frac{(N-1)r x_{i,C}^{r-1} x_C^r V}{[x_{i,C}^r + (N-1)x_C^r]^2} - \alpha x_{i,C}^{\alpha-1}.$$

Suppose that a symmetric equilibrium with pure-strategy bidding exists. The (pure) bidding strategy in the equilibrium can be solved by the first order condition $\frac{d\pi_i}{dx_{i,C}}|_{x_i=x} =$

[1] It is impossible to have all participating bidders bid zero deterministically in an equilibrium. When all others bid zero, a participating bidder would prefer to place an infinitely small positive bid, which allows him to win the prize with probability one.

0 given the equilibrium entry probability q_C^*, while q_C^* is characterized by the zero-payoff condition.[2]

According to the first order condition $\frac{d\pi_i(x_{i,C})}{dx_{i,C}} = 0$ and the symmetry condition $x_{i,C} = x_C,\ x_C^*$ must solve

$$ {}_{N=1}^{M} C_{M-1}^{N-1} q_C^{N-1}(1-q_C)^{M-N}\frac{(N-1)rV}{N^2 x_C^*} - \alpha x_C^{*\alpha-1} = 0, $$

which yields

$$ x^*(q_C) = [{}_{N=1}^{M} C_{M-1}^{N-1} q_C^{N-1}(1-q_C)^{M-N}\frac{N-1}{N^2}\frac{rV}{\alpha}]^{\frac{1}{\alpha}}. $$

The equilibrium expected payoff is

$$ \begin{aligned} \pi^*(x^*(q_C), q_C) &= {}_{N=1}^{M} C_{M-1}^{N-1} q_C^{N-1}(1-q_C)^{M-N}\frac{V}{N} - [{}_{N=1}^{M} C_{M-1}^{N-1} q_C^{N-1}(1-q_C)^{M-N}\frac{N-1}{N^2}\frac{rV}{\alpha}] \\ &= {}_{N=1}^{M} C_{M-1}^{N-1} q_C^{N-1}(1-q_C)^{M-N}\frac{V}{N}(1-\frac{N-1}{N}\frac{r}{\alpha}). \end{aligned} $$

By entering the contest and submitting the bid $x^*(q_C)$, every potential contestant i ends up with an expected payoff

$$ \pi^*(x^*(q_C), q_C) - \Delta. $$

The equilibrium payoff cannot be negative. When $q_C^* \in (0,1)$, the equilibrium payoffs of players must be zero, otherwise there is no equilibrium (as players would enter with probability 1 and earn a positive payoff). Therefore, each potential bidder receives a zero expected payoff for the equilibrium entry q_C^*, i.e., $\pi^*(x^*(q_C^*), q_C^*) = \Delta$.

The expected overall effort of the contest $(TE_C^*(q_C^*))$ is as follows

$$ TE_C^*(q_C^*) = Mq_C^* x^*(q_C^*) = Mq_C^*[{}_{N=1}^{M} C_{M-1}^{N-1} q_C^{*N-1}(1-q_C^*)^{M-N}\frac{N-1}{N^2}\frac{rV}{\alpha}]^{\frac{1}{\alpha}}. $$

Therefore, q_C^* satisfies $F(q_C^*, r) ={}_{N=1}^{M} C_{M-1}^{N-1} q_C^{*N-1}(1-q_C^*)^{M-N}\frac{V}{N}(1-\frac{N-1}{N}\frac{r}{\alpha}) - \Delta = 0$. Apparently, $F(q_C^*, r)$ is continuous in and differentiable with both arguments. We first claim that $F(q_C^*, r)$ strictly decreases with q_C^*. Define $\pi_N = \frac{V}{N}(1-\frac{N-1}{N}\frac{r}{\alpha})$. Taking its first

[2] $r \leq \alpha\frac{M}{M-1}$ guarantees the pure-strategy bidding in the equilibrium.

order derivative yields

$$\begin{aligned}
\frac{F(q_C^*, r)}{dq_C^*} &= {}_{N=1}^{M} C_{M-1}^{N-1}[(N-1)q_C^{*N-2}(1-q_C^*)^{M-N} - (M-N)q_C^{*N-1}(1-q_C^*)^{M-N-1}]\pi_N \\
&= {}_{N=1}^{M} C_{M-1}^{N-1}(N-1)q_C^{*N-2}(1-q_C^*)^{M-N}\pi_N - {}_{N=1}^{M} C_{M-1}^{N-1}(M-N)q_C^{*N-1}(1-q_C^*)^{M-N-1}\pi_N \\
&= (M-1)\{{}_{N=2}^{M} C_{M-2}^{N-2}q_C^{*N-2}(1-q_C^*)^{M-N}\pi_N - {}_{N=1}^{M-1} C_{M-2}^{N-1}q_C^{*N-1}(1-q_C^*)^{M-N-1}\pi_N\} \\
&= (M-1){}_{N=1}^{M-1} C_{M-2}^{N-1}q_C^{*N-1}(1-q_C^*)^{M-N-1}\left(\pi_{N+1} - \pi_N\right),
\end{aligned}$$

which is obviously negative because $\pi_N = \frac{1}{N}\left[1 - \left(1 - \frac{1}{N}\right)\frac{r}{\alpha}\right]V \geq 0$ and it monotonically decreases with N.

When all other potential contestants play $q_C = 0$, a potential contestant receives a payoff $V - \Delta > 0$, and he must enter with probability one. When all others play $q_C = 1$, a participating contestant receives a negative expected payoff since $\frac{V}{M} < \Delta$, which cannot constitute an equilibrium either. Hence, a unique $q_C^* \in (0,1)$ must exist that solves $\pi^*(x^*, q_C) = \Delta$. Each potential contestant is indifferent between entering and staying inactive when all others play the strategy. This constitutes an equilibrium.

A.3 Experimental instructions

Welcome to our experiment! You will receive RMB15 for having shown up on time. Please read all of the instructions carefully. Properly understanding the instructions will help you to make better decisions and therefore earn you more money. The experiment will last approximately one hour. Your earnings in this experiment will be measured in the experimental currency (i.e., EC) unit. At the end of the experiment, we will convert your earnings in EC to RMB, and pay you your earnings in private. The exchange rate is 3.2 EC= RMB1.

Your total payment in this experiment will be the sum of

(1) Your show-up fee: RMB15;

(2) Your earnings in this experiment;

To make sure you understand the experiment, the experimenter will first read the instructions out loud before the start of the experiment, and support will also be available at any time during the experiment. Please remember that you are not allowed to communicate with other participants during the experiment. If you do not obey this rule, you will be asked to leave the laboratory and will not be paid. Whenever you have a question, please raise your hand and an experimenter will come to help you.

The game

In this experiment, there are two decision-making stages in each period. At the beginning of each period, you will be randomly assigned to a group of 3 players. Each of you will be randomly labeled A, B, or C and will receive 80 EC as your initial endowment.

Stage 1: Entry decision

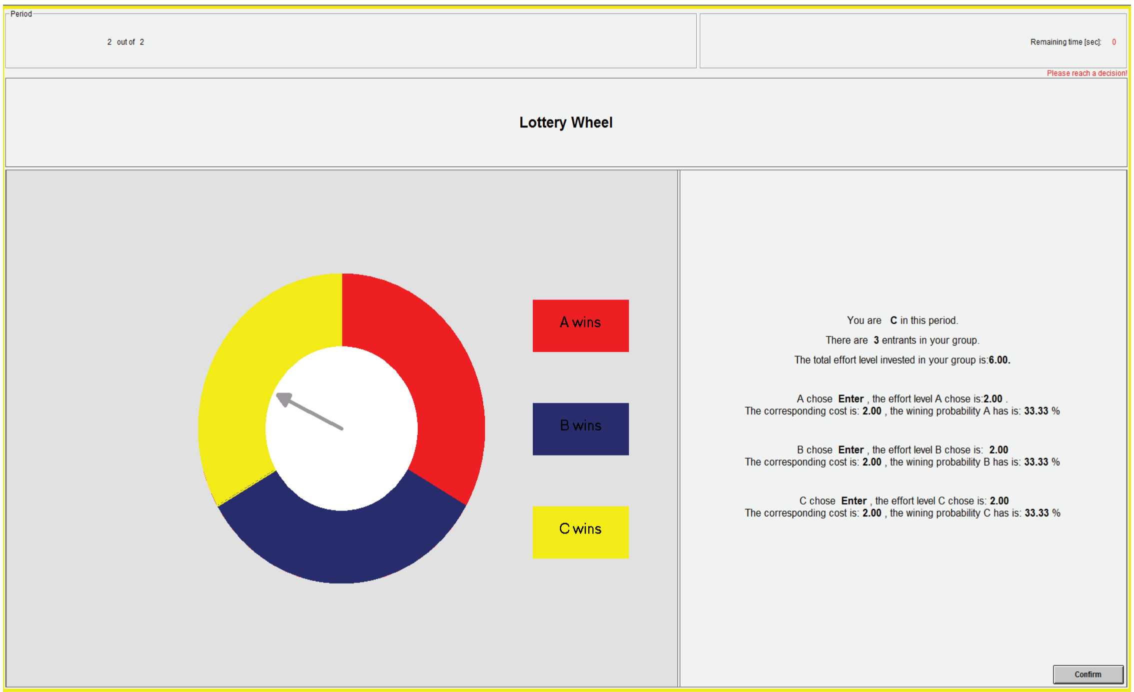
In this stage, you will have to choose whether to enter the competition stage (Stage 2).

- If you choose to enter the competition, an entry fee of 40 EC will automatically be deducted from your initial endowment. In exchange, you will have the opportunity to compete against your group members and receive a prize of 100 EC with a certain probability in Stage 2. Your winning probability will depend on both your decision and those of your group members in Stage 2, and on how many of you have chosen to enter Stage 2.
- If you choose not to enter Stage 2, no entry fee will be charged. However, you will not have a chance to win the prize.
- Once all players have made their entry decisions, the total number of participants in the competition in Stage 2 will be revealed to all members (participants and non-participants) in your group. Those who have chosen not to enter Stage 2 will no longer need to make decisions in this period, but will have to wait quietly for their group members to complete Stage 2. If no-one in your group enters Stage 2, the prize will be kept by the experimenter.

Stage 2: Competition

In this stage, all entrants compete for a prize of 100 EC. After learning the actual number of entrants in his/her group, each entrant must choose the level of effort he/she is willing to invest. The cost of effort x is calculated by a cost function, $C(x) = x^{\alpha}(\alpha = 2/3)$, and will be deducted from your initial endowment for this period (therefore, you can choose an effort level that costs less than the balance of your endowment, i.e., 40 EC.). After all entrants in your group have made their decisions, the computer will select one winner in your group:

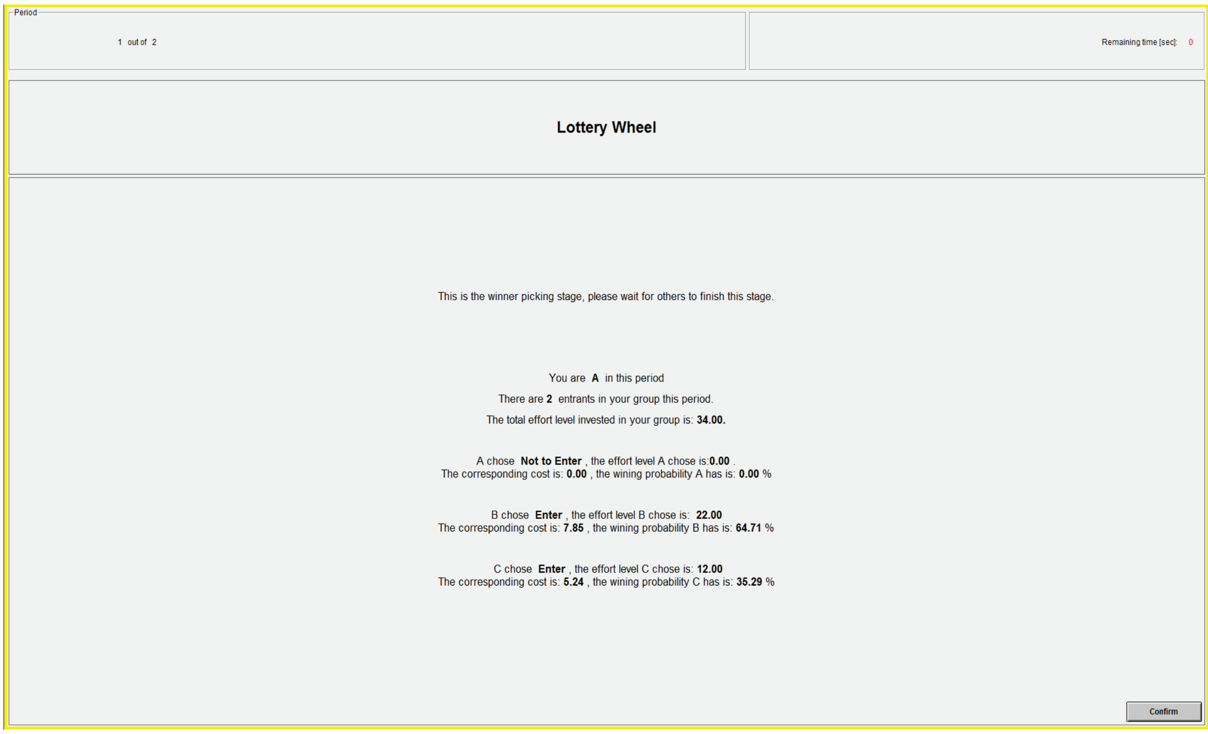
Figure A.1: Lottery Wheel Screenshot–Entrants



- If only one player has chosen to enter Stage 2, this player will receive the prize with a probability of 100%, no matter how much he/she has invested in the competition.

- If more than one player has chosen to enter Stage 2, your probability of winning the prize will depend on your choice of effort relative to that of all entrants in your group. Specifically, your probability of winning will be equal to your effort divided by the total effort of all entrants in your group, namely $P_i = (x_i)/(x_i + x_j)$, where x_j is the total effort of all other entrants in your group). Note that in this case you may have one or two other competitors in your group. After choosing your effort level, a lottery wheel will appear on your computer screen. The probability of all entrants winning and the random draw process will be displayed in

Figure A.2: Lottery Wheel Screenshot–Non-entrants



a dynamic lottery wheel. The wheel will be divided into three colored areas: red, blue, and yellow. The red area represents the winning area of participant A, the blue area, the winning area of participant B, and the yellow area, the winning area of participant C. The relative size of the colored areas will correspond to the probability of each participant winning (note that if there are only two entrants in your group, the wheel will only have two colors). In the center of the lottery wheel an arrow will initially point vertically upwards. When the random draw begins, the arrow will start spinning and after a while will stop randomly. If the arrow stops in the red area, participant A will win the prize. If the arrow stops in the blue area, participant B will win the prize. If the arrow stops in the yellow area, participant C will win the prize. Obviously, the higher the level of effort you choose relative to that of your competitor(s), the larger your winning area on the lottery wheel, and the more likely you will be the winner of this competition. At the same time, the higher the level of effort, the higher the cost.

(To help you to better understand the relationship between your choice of effort and the cost of your effort, we provide a table on the last page of this document that describes the levels of effort you can choose and their corresponding costs. You can also use the calculator button on your screen to help you with your decision.)

Your earnings

Your earnings for each period will be calculated at the end of each period, as follows (and displayed to you):

- If you choose not to enter Stage 2

$$your\ \ earnings = Endowment = 80EC$$

  (Please note that although you can keep your initial endowment for this period, it cannot be carried over to the next period(s) to help your decisions in other periods.)

- If you choose to enter Stage 2

  a If you lose,

$$your\ \ earnings = Endowment(80EC) - Entry\ \ Fee(40EC) - effort\ \ cost(x^{\alpha}EC)$$

  b If you win,

$$\begin{aligned} your\ \ earnings = Endowment(80EC) - Entry\ \ Fee(40EC) \\ + Prize(100EC) - effort\ \ cost(x^{\alpha}EC) \end{aligned}$$

Procedure

You will play 25 periods of this two-stage game. However, you will always be randomly matched with two participants and labeled A, B, or C at the beginning of each period. On the lottery screen, your group members' entry decision, effort level and corresponding cost, probability of winning, and the number of entrants in your group will be displayed on your screen, irrespective of whether you choose to enter Stage 2. (see the sample screenshots above) At the end of each period, your earnings will be calculated by the computer and displayed on your screen.

After completing all 25 periods, the computer will randomly draw one period out of these 25 periods. Your total earnings from this period will be converted to RMB (at the rate of 3.2 EC= RMB1) and paid to you, together with your show-up fee (RMB15).

To further ensure that all participants in this experiment understand the game correctly, you will need to answer several control questions designed based on the information provided in these instructions. The experiment will start after all participants have answered these questions correctly. Please do not hesitate to ask for help if you have any questions regarding the information provided in our instructions or the control questions.

At the end of today's experiment, you will also need to complete a short post-experiment questionnaire, including your demographic information (e.g., sex, age, study major, etc.) and your decisions in the experiment. All information provided will remain anonymous and will be kept strictly confidential. This information is collected only for academic research purposes.

Thank you again for your participation and your patience! The experiment will start soon.

Cost schedule

Cost Function of Your Effort Level $C(X) = X^{\alpha}$ ($\alpha = \frac{2}{3}$)

Effort	Cost	Effort	Cost	Effort	Cost	Effort	Cost	Effort	Cost	Effort	Cost
0	0.00										
1	1.00	**51**	13.75	**101**	21.69	**151**	28.36	**201**	34.31	**251**	39.79
2	1.59	**52**	13.93	**102**	21.83	**152**	28.48	**202**	34.43	**252**	39.90
3	2.08	**53**	14.11	**103**	21.97	**153**	28.61	**203**	34.54	**253**	40.00
4	2.52	**54**	14.29	**104**	22.12	**154**	28.73	**204**	34.65		
5	2.92	**55**	14.46	**105**	22.26	**155**	28.86	**205**	34.77		
6	3.30	**56**	14.64	**106**	22.40	**156**	28.98	**206**	34.88		
7	3.66	**57**	14.81	**107**	22.54	**157**	29.10	**207**	34.99		
8	4.00	**58**	14.98	**108**	22.68	**158**	29.23	**208**	35.11		
9	4.33	**59**	15.16	**109**	22.82	**159**	29.35	**209**	35.22		
10	4.64	**60**	15.33	**110**	22.96	**160**	29.47	**210**	35.33		
11	4.95	**61**	15.50	**111**	23.10	**161**	29.59	**211**	35.44		
12	5.24	**62**	15.66	**112**	23.24	**162**	29.72	**212**	35.55		
13	5.53	**63**	15.83	**113**	23.37	**163**	29.84	**213**	35.67		
14	5.81	**64**	16.00	**114**	23.51	**164**	29.96	**214**	35.78		
15	6.08	**65**	16.17	**115**	23.65	**165**	30.08	**215**	35.89		
16	6.35	**66**	16.33	**116**	23.79	**166**	30.20	**216**	36.00		
17	6.61	**67**	16.50	**117**	23.92	**167**	30.33	**217**	36.11		
18	6.87	**68**	16.66	**118**	24.06	**168**	30.45	**218**	36.22		
19	7.12	**69**	16.82	**119**	24.19	**169**	30.57	**219**	36.33		
20	7.37	**70**	16.98	**120**	24.33	**170**	30.69	**220**	36.44		
21	7.61	**71**	17.15	**121**	24.46	**171**	30.81	**221**	36.55		
22	7.85	**72**	17.31	**122**	24.60	**172**	30.93	**222**	36.66		
23	8.09	**73**	17.47	**123**	24.73	**173**	31.05	**223**	36.77		
24	8.32	**74**	17.63	**124**	24.87	**174**	31.17	**224**	36.88		
25	8.55	**75**	17.78	**125**	25.00	**175**	31.29	**225**	36.99		
26	8.78	**76**	17.94	**126**	25.13	**176**	31.41	**226**	37.10		
27	9.00	**77**	18.10	**127**	25.27	**177**	31.52	**227**	37.21		
28	9.22	**78**	18.26	**128**	25.40	**178**	31.64	**228**	37.32		
29	9.44	**79**	18.41	**129**	25.53	**179**	31.76	**229**	37.43		
30	9.65	**80**	18.57	**130**	25.66	**180**	31.88	**230**	37.54		
31	9.87	**81**	18.72	**131**	25.79	**181**	32.00	**231**	37.65		
32	10.08	**82**	18.87	**132**	25.92	**182**	32.12	**232**	37.76		
33	10.29	**83**	19.03	**133**	26.06	**183**	32.23	**233**	37.86		
34	10.50	**84**	19.18	**134**	26.19	**184**	32.35	**234**	37.97		
35	10.70	**85**	19.33	**135**	26.32	**185**	32.47	**235**	38.08		
36	10.90	**86**	19.48	**136**	26.45	**186**	32.58	**236**	38.19		
37	11.10	**87**	19.63	**137**	26.58	**187**	32.70	**237**	38.30		
38	11.30	**88**	19.78	**138**	26.70	**188**	32.82	**238**	38.40		
39	11.50	**89**	19.93	**139**	26.83	**189**	32.93	**239**	38.51		
40	11.70	**90**	20.08	**140**	26.96	**190**	33.05	**240**	38.62		
41	11.89	**91**	20.23	**141**	27.09	**191**	33.17	**241**	38.73		
42	12.08	**92**	20.38	**142**	27.22	**192**	33.28	**242**	38.83		
43	12.27	**93**	20.53	**143**	27.35	**193**	33.40	**243**	38.94		
44	12.46	**94**	20.67	**144**	27.47	**194**	33.51	**244**	39.05		
45	12.65	**95**	20.82	**145**	27.60	**195**	33.63	**245**	39.15		
46	12.84	**96**	20.97	**146**	27.73	**196**	33.74	**246**	39.26		
47	13.02	**97**	21.11	**147**	27.85	**197**	33.86	**247**	39.37		
48	13.21	**98**	21.26	**148**	27.98	**198**	33.97	**248**	39.47		
49	13.39	**99**	21.40	**149**	28.11	**199**	34.09	**249**	39.58		
50	13.57	**100**	21.54	**150**	28.23	**200**	34.20	**250**	39.69		

Appendix B

Appendix of Chapter 3

B.1 Additional results

B.1.1 Learning

We use the local polynomial estimation to show the aggregate distribution in selective rounds. The estimated graphs are contrasted with the predicted distribution shown by the bar graph in each treatment. The trajectory of the shift of observed distribution indicates participants adjust their risk-taking choice with learning.

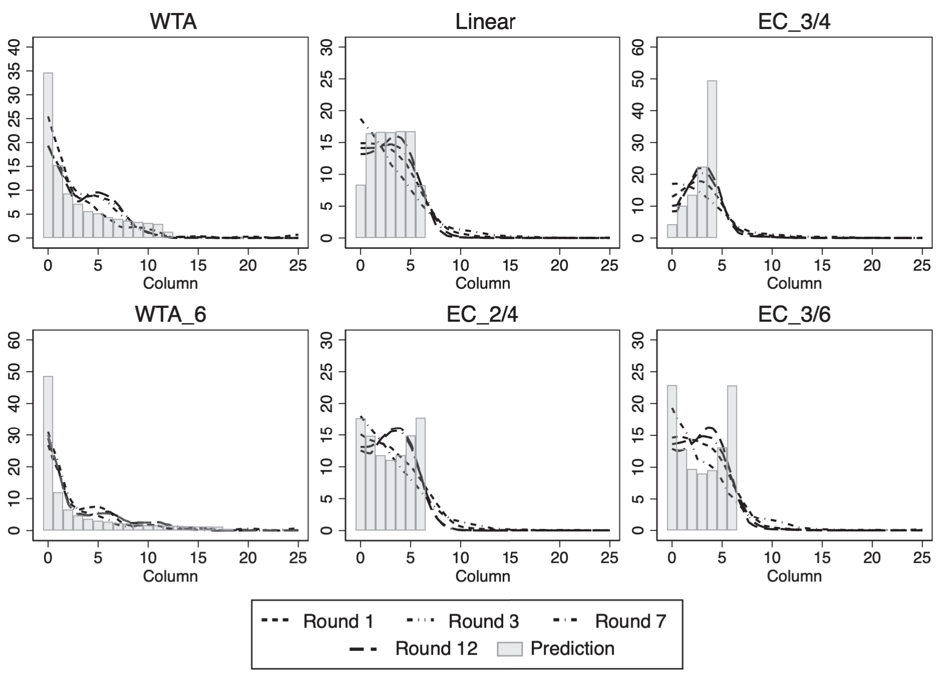


Figure B.1: Learning

B.1.2 Robustness check

To test the robustness of the treatment effect, we add individual characteristic variables into the mixed regressions. The results are similar to the main results observed in the paper.

Table B.1: Mixed regression on convexity effect with other controlled variables

	Skew		GMD	
VARIABLES	All rounds	Rnd 7-12	All rounds	Rnd 7-12
Linear (cons)	0.339	0.0119	1.049***	1.042***
	(0.303)	(0.318)	(0.162)	(0.167)
WTA	0.351*	0.490**	0.376***	0.462***
	(0.184)	(0.199)	(0.111)	(0.122)
EC34	-0.0864	-0.0660	-0.271**	-0.307**
	(0.185)	(0.200)	(0.111)	(0.122)
Gender	0.273**	0.240**	0.0335	0.0252
	(0.111)	(0.116)	(0.0575)	(0.0576)
Degree	-0.257	-0.180	-0.169*	-0.196**
	(0.191)	(0.199)	(0.0993)	(0.0997)
Major	-0.0939	-0.0679	0.00165	0.0157
	(0.0692)	(0.0721)	(0.0360)	(0.0361)
Risk	0.0160	0.00420	0.0408***	0.0231
	(0.0289)	(0.0302)	(0.0151)	(0.0153)
σ_{group}	0.115	0.143	0.051	0.066
	(0.049)	(0.057)	(0.018)	(0.021)
$\sigma_i ndividual$	0.29	0.312	0.077	0.078
	(0.046)	(0.049)	(0.012)	(0.011)
Observations	1,704	852	1,704	852
Number of groups	36	36	36	36

In the tables, gender, degree and major are category variables. Gender: ″1″ represents male while ″0″ represents female. Degree takes the value ″1″, ″2″, and ″3″ when the participant is an undergraduate, master, or doctoral student, respectively. Major is equal to ″1″, ″2″, ″3″, and ″4″ when the major is science and engineering, business and economics, social science other than business or economics, or others, respectively. Risk takes values from 0 to 10; the higher the number, the more risk-loving the participant is.

Table B.2: Proportion of winners: mixed regression with controlled variables

	Skewness		GMD	
VARIABLES	All rounds	Rnd. 7-12	All rounds	Rnd. 7-12
EC_3/4 (Cons.)	0.164	-0.0688	0.686***	0.801***
	(0.388)	(0.445)	(0.217)	(0.214)
EC_2/4	-0.0305	-0.00422	0.199*	0.242*
	(0.193)	(0.218)	(0.108)	(0.124)
Gender	0.418***	0.413**	0.0320	0.0212
	(0.140)	(0.160)	(0.0781)	(0.0747)
Degree	-0.248	-0.251	-0.162	-0.298*
	(0.287)	(0.329)	(0.160)	(0.154)
Major	-0.0584	-0.0553	0.0442	0.0332
	(0.0838)	(0.0962)	(0.0468)	(0.0448)
Risk	0.00563	0.00105	0.0442**	0.0265
	(0.0337)	(0.0386)	(0.0188)	(0.0184)
σ^2_{group}	0.136	0.17	0.043	0.068
	(0.067)	(0.085)	(0.021)	(0.027)
$\sigma^2_{individual}$	0.28	0.372	0.093	0.086
	(0.056)	(0.074)	(0.017)	(0.016)
Observations	1,140	570	1,140	570
Number of groups	24	24	24	24

Table B.3: Entrant effect: mixed regression with controlled variables

	Winner-takes-all				Elimination contest			
	Skew		GMD		Skew		GMD	
	All	R. 7-12	All	R. 7-12	All	R. 7-12	All	R. 7-12
Baseline (Cons.)	0.275	0.186	1.583***	1.604***	0.0270	-0.0582	0.668***	0.820***
	(0.298)	(0.261)	(0.176)	(0.181)	(0.375)	(0.377)	(0.208)	(0.214)
Entrant effect	0.412**	0.483***	0.226**	0.289***	0.0205	0.0505	0.242**	0.293***
	(0.160)	(0.172)	(0.101)	(0.111)	(0.163)	(0.180)	(0.0978)	(0.114)
Gender	0.136	0.164**	-0.00882	-0.0146	0.157	0.0819	0.0390	-0.0130
	(0.0967)	(0.0806)	(0.0563)	(0.0569)	(0.130)	(0.129)	(0.0714)	(0.0715)
Degree	-0.0476	0.0287	-0.109	-0.128	-0.0184	-0.0225	-0.131	-0.289*
	(0.167)	(0.140)	(0.0976)	(0.0988)	(0.279)	(0.277)	(0.153)	(0.155)
Major	-0.0619	-0.0423	-0.0506	-0.0291	0.0225	0.0153	0.0924**	0.0988**
	(0.0687)	(0.0576)	(0.0401)	(0.0406)	(0.0817)	(0.0809)	(0.0448)	(0.0449)
Risk	0.0538**	0.0181	0.0201	0.00882	-0.0165	-0.0411	0.0244	0.00241
	(0.0261)	(0.0219)	(0.0152)	(0.0154)	(0.0328)	(0.0328)	(0.0181)	(0.0183)
σ^2_{group}	0.0538**	0.0181	0.0201	0.056	0.082	0.118	0.033	0.054
	(0.0261)	(0.0219)	(0.0152)	(0.021)	(0.053)	(0.063)	(0.018)	(0.024)
$\sigma^2_{individual}$	0.213	0.152	0.07	0.073	0.341	0.328	0.103	0.103
	(0.035)	(0.025)	(0.012)	(0.012)	(0.057)	(0.055)	(0.017)	(0.017)
Observations	1,428	714	1,428	714	1,428	714	1,428	714
Number of groups	24	24	24	24	24	24	24	24

Note: Baseline under WTA and EC column correspond to WTA and EC_3/4.

Table B.4: Scale-up effect: mixed regression with controlled variables

	Skew		GMD	
	All rounds	Round 7-12	All rounds	Round 7-12
EC_2/4 (cons)	-0.0370	-0.146	0.998***	1.034***
	(0.365)	(0.390)	(0.199)	(0.214)
EC_3/6	0.0698	0.0566	0.0498	0.0606
	(0.110)	(0.128)	(0.0618)	(0.0716)
Gender	0.284**	0.299**	-0.0325	-0.0519
	(0.114)	(0.120)	(0.0620)	(0.0658)
Degree	-0.266	-0.245	-0.240*	-0.263*
	(0.256)	(0.272)	(0.139)	(0.149)
Major	-0.00953	-0.0600	0.133***	0.100**
	(0.0729)	(0.0761)	(0.0395)	(0.0417)
Risk	0.0383	0.0269	0.0132	0.00509
	(0.0305)	(0.0326)	(0.0167)	(0.0179)
σ^2_{group}	0.008	0.03	0.004	0.01
	(0.022)	(0.031)	(0.006)	(0.009)
$\sigma^2_{individual}$	0.245	0.266	0.074	0.085
	(0.043)	(0.046)	(0.012)	(0.013)
Observations	1,440	720	1,440	720
Number of groups	24	24	24	24

B.2 Permutation test

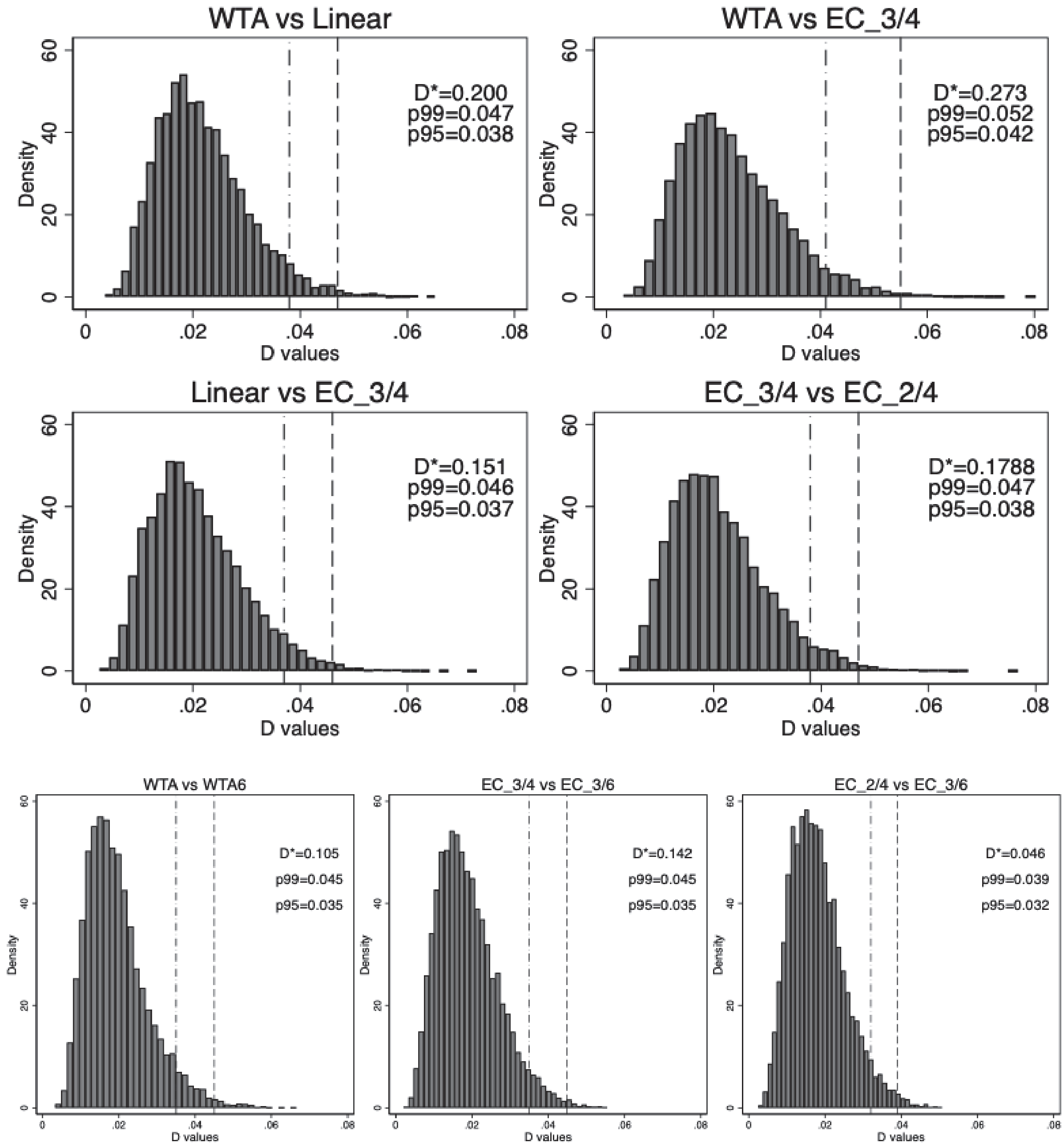
Consider two treatments with n observations each. Let F_1 and F_2 be the observations of two different treatments and $\overline{F}$ be the pooled observations over the two treatments. Each observation unit is one performance distribution chosen by one participant in one round.

To construct the permutation test, we first establish the H_0 assumption, which is: $H_0 : F_1 = F_2 = \overline{F}$. The null hypothesis implies that if there is no difference in the two distributions from these two treatments, they can be seen as independent draws from the same pooled distribution. Then we calculate the observed two-sample K-S statistic D* comparing F_1 and F_2.

We then pool all the observations to get $\overline{F}$ and randomly reassign the treatment labels to the pooled observations to form new treatment group F_1^p and F_2^p. Next, we compute the K-S statistic D between F_1^p and F_2^p. This process is repeated 10,000 times and then we have a distribution of D_j $(j \in \{1, \cdots, 10,000\})$ draw from $\overline{F}$.

We can reject the H_0 hypothesis if D^* is above the 95_{th} percentile of the distribution of D_j. The implication of this test is that only if the D^* is significantly larger than D_j (which comes from the pooled data of two treatments), can we reject the null hypothesis that F_1 and F_2 come from the same distribution.

Figure B.2 illustrates the distribution of D_j, 55_{th} percentile, 99_{th} percentile and D^* values between any of the two treatments in our experiment. As we can see from the figure, all D^*s are significantly larger than the 99_{th} percentile of the distribution. The treatment difference is significant.



Note: Each figure is the histogram of all the permuted two sample K-S statistics. Each comparison (histogram) contains 10,000 repetitions. The short dashed line and the long dashed line represent the 95_{th} percentile and 99_{th} percentile of the all observed K-S statistics from the permutation, respectively. D* is observed K-S statistics.

Figure B.2: Permutation test of KS statistics

Figure B.3 illustrates the distribution of $\Delta D = D12 - D1$, which tests whether the distribution in round 12 is closer to equilibrium or not compared with the round 1 distribution. As we can see from the figure, only in treatment Linear are all the KS statistics below 0, which means participants converge to equilibrium distribution in the Linear treatment. In WTA6 and EC_2/4 we see the distribution is mostly even around 0,

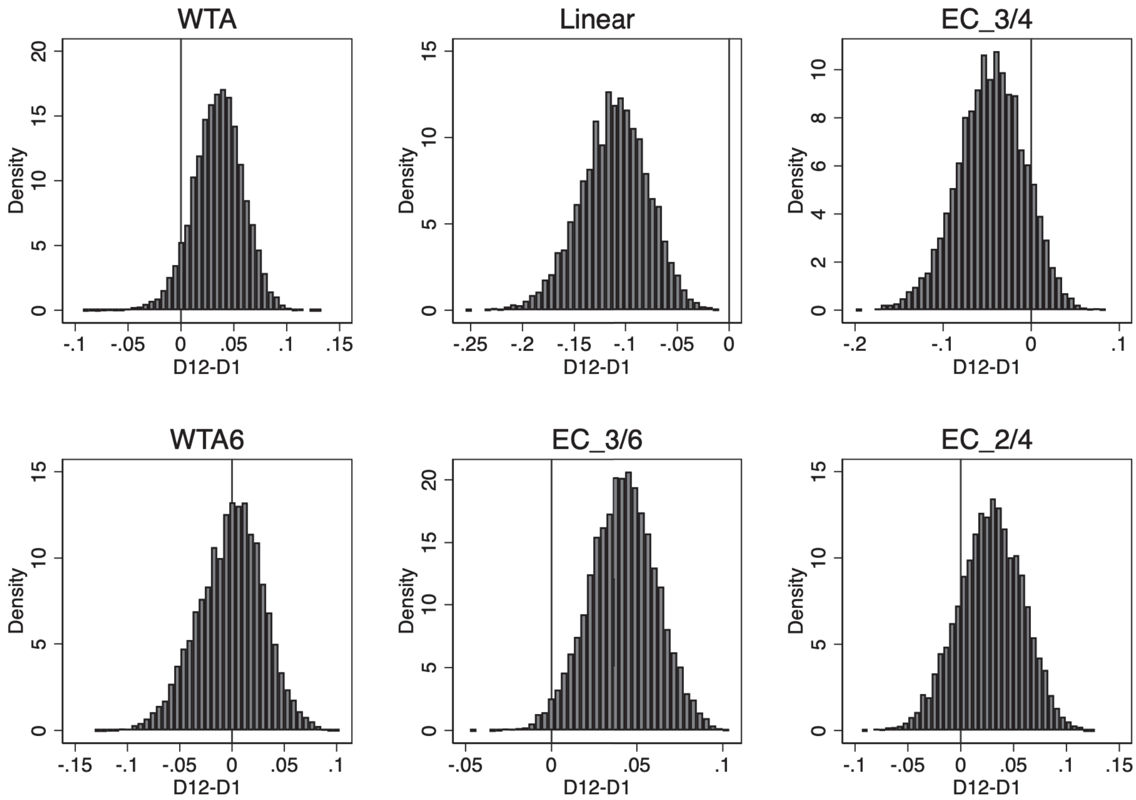


Figure B.3: KS test for learning

meaning the difference is not significant. However, in treatment WTA and EC_3/6, we see most of the differences are positive, which indicates participants are even further away from the prediction in round 12 compared with round 1.

B.3 Experimental instructions

Welcome to our experiment! You will receive 20 RMB for showing up on time. Please read these instructions carefully and completely. Properly understanding the instructions will help you make better decisions and hence earn more money. The experiment will last about 1.5 hour. Your earnings in this experiment will be measured in experimental currency (i.e., EC). At the end of the experiment, we will convert your earnings in EC to RMB, and pay you in private.

This experiment is composed of two parts. Instructions for Part 2 will be given out once all participants finished Part 1. Please do not start Part 2 until the experimenter finish giving out and reading the instructions for Part 2.

Your total payment from this experiment will be the sum of:

(1)Your show-up fee: 20 RMB;

(2)Your earnings in Part 1;

(3)Your earnings in Part 2.

To make sure you understand the experiment, the experimenter will first read the instructions aloud before the experiment starts. Support is available at any time during the experiment. Please keep in mind that you are not allowed to communicate with other participants during the experiment. If you do not obey this rule you will be asked to leave the laboratory and will not be paid. Whenever you have a question, please raise your hand; an experimenter will come to assist you.

Part 1

Part 1 is composed of 6 rounds. In each round, you will need to compete within a group of 4 or 6 participants to win one of the prizes in a set. At the beginning of each round, computer will randomly assign the group and decide which set of prizes to be used. You will see the number of people and prize set used in your group for this round on the screen. The set of prizes will look like the following example:

Figure B.4: Prize schedule Example

In this example, there are 4 participants in the group. The first row is the ranks (1 to 4) you can get within your group. The second row shows the corresponding prizes for each rank. The value of the prizes is measured by the number of EC. Which prize you get depends on your ranking in your group. The higher you rank in your group, the higher prize you will get and hence the higher chance that you can get a better payoff. The total number of prizes in the set should always be equal to the total number of group members. In other words, everyone will get a positive prize, but the size of prize increases in rank.

Your task

In each round, your task is to build a distribution against 0 to 25 (26 integers in total) using 100 markers. Computer will draw 1 of the 26 integers according to the distribution you build. The distribution you build will determine the chances of each number will be drawn. Each marker you put on a certain number represents 1% of the chance that this particular number will be drawn by the computer. That is to say, the more markers you put on a certain number, the more likely this number will be drawn. Computer will draw one number for each of your group member simultaneously. Similar with how the computer draw the number for you, which number will be drawn for other group members depend on the distribution they build. From the biggest number to the smallest, all group members' drawn numbers will be ranked and then prizes will be given according to the rank. For instance, if your draw is 7 and the others' draws are 5, 10 and 0 respectively, then your ranking is 2nd in your group, and you will receive the 2nd prize on the prize set for this round (According to the sample set of prizes above, you will get 15 EC).

You will use the following distribution builder in this experiment:

While building the distribution, you face the following budget constraint: the total cost of the 100 markers has to be less or equal to 300. Each marker you put

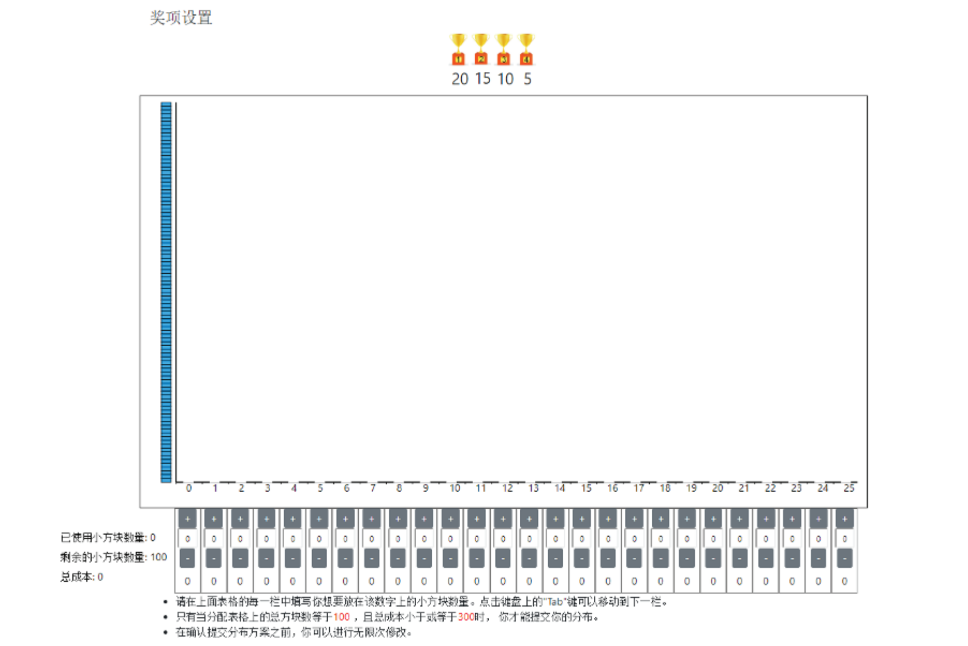


Figure B.5: Distribution builder

on different number will incur different cost. Although one more marker put on a bigger number would increase your chance to win the best prize in the set, it is also proportionally more costly. To be more specific:

The cost of each marker on certain number=this number

*The total cost of all markers put at each number = the number of markers on this number * the cost of each marker on this number (i.e., this number)*

For example, if you decide to put 10 markers on number 10, it will cost you 100 in total and at the same time give you 10% that 10 is drawn by the computer.

Note that while you are building the distribution, you cannot keep any unused budget, nor can you exceed your budget at the time you submit you allocation (Note: the cost of all markers placed on number 0 is 0.). Once you change the distribution, the cost will be calculated automatically by the computer. Only when the sum of the total cost is less or equal to 300 and the total amount of markers equals to 100, can you submit the distribution. However, you can change as many times as you like before you confirm that you would like to submit the distribution.

After you submit your allocation in each round, the computer will randomly draw one number for each group member according to their submitted distribution respectively, and rank the drawn numbers. In the case of tie, the computer will break the tie randomly. The random group assignment and number draw will be automatically done by computer in the background. You will NOT observe them nor receive any information about your ranking at the end of each round.

Your payoff

One out of the six rounds will be randomly selected for payment. The prize you received in the selected round will be converted into RMB according to the exchange rate of **1 EC =0.5 RMB** as your payoff in Part 1. All the information about the selected round, including the round number, the set of prizes used, the number of participants in your group, and the information about all group members' (including your own) drawn numbers, rankings, prizes allocated will be disclosed to you at the end of today's experiment.

Pre-experimental questionnaire and practice round

Before starting the experiment, you will answer several questions regarding the instructions. Once all of you have answered these questions correctly, you will proceed to the practice round. The purpose of practice round is to give you some idea how to operate the distribution builder and how computer use the lottery wheel to draw a number for you. The practice round will NOT be included in your payment. Please do not hesitate to ask for help if you have any questions regarding the information provided in the instructions or the questions we ask you to answer.

Thank again for your participation and patience! The experiment will start soon...

Part 2

Part 2 is composed of 12 rounds. You will be using 100 markers to build distributions against 0-25 like you did in part 1, but with the following changes:

1) Before Part 2 starts, you will be randomly assigned into a four-people group. Group members in your group will be randomly named as player 1, player 2, player 3 and player 4. Your name in the group and your group members and will stay the same for all 12 rounds.

2) You will compete for the same set of prizes for 12 rounds. Specifically, one of the five four-prize sets you faced in Part 1 will be randomly selected by the computer, and will be used throughout Part 2.

3) Computer will randomly draw one round for payment. The prize you won in the drawn round will be converted to RMB according to the exchange rate of **1 EUC=1 RMB** as your total earnings from Part 2.

4) After the distribution is submitted, you will observe computer using lottery wheel (which you have seen in the practice round) to draw a number for you. You will also receive information about your own as well as your group members' drawn number, rankings, and prize allocations at the end of each round (Except from round 1).

5) To help you build and adjust your distribution, you are also provided a "reload previous distribution" button in this part. You can (but are not obliged to) use this button (from round 2) to build your distribution in the new round based on what you have submitted before.

Post-experimental questionnaire

At the end of today's experiment, you will also need to fill out a small questionnaire, including questions about you (e.g., your gender, age, major...) and your decisions in the experiment. All the information you provide will be kept anonymous and in strictly confidential. The only purpose of collecting this information from you is for academic research analysis.

Appendix C

Appendix of Chapter 4

C.1 Additional results

Table C.1: Entry choice breakdown

	c=5						c=25					
	UnR		**Re**		**Ind**		**UnR**		**Re**		**Ind**	
	No.	Perct.	No.	Perct.	No.	Perct.	No.	Perct.	No.	Perct.	No.	Perct.
Nash Entry	**931**	**78%**	**917**	**76%**	**625**	**52%**	**990**	**83%**	**958**	**80%**	**632**	**53%**
0	405	34%	313	26%	260	22%	668	56%	597	50%	514	43%
1	526	44%	604	50%	134	11%	322	27%	361	30%	118	10%
2					231	19%						
Under Entry	**132**	**11%**	**141**	**12%**	**111**	**9%**	**99**	**8%**	**95**	**8%**	**86**	**7%**
(0,1)	132	11%	141	12%	97	8%	99	8%	95	8%	86	7%
(0,2)					8	0.6%						
(1,2)					6	0.4%						
Over Entry	**137**	**11%**	**142**	**12%**	**464**	**39%**	**111**	**9%**	**147**	**12%**	**482**	**40%**
(1,0)	137	11%	142	12%	89	8%	111	9%	147	12%	116	10%
(2,0)					80	7%					93	8%
(2,1)					295	25%					273	22%
N	1200		1200		1200		1200		1200		1200	

Table C.2: Average probability of the entry message being chosen

	UnR			Re			Ind		
	NE	**Mean**	p	**NE**	**Mean**	p	**NE**	**Mean**	p
Low cost									
m=1	0.55	0.55	0.73	0.62	0.64	0.09	0.44	0.22	0.00
		(0.372)			(0.352)			(0.160)	
m=2							0.20	0.51	0.00
								(0.375)	
High cost									
m=1	0.35	0.36	0.46	0.38	0.42	0.00	0.40	0.22	0.00
		(0.352)			(0.369)			(0.157)	
m=2							0	0.29	0.00
								(0.324)	

Note: The probability of each message been chosen given s for each observation is estimated by multi-level ordered logistic models controlled for both individual and group level random effects. We leave out the estimation of probability of $m = 0$ being chosen, because it can be calculated from $1 - p(enter)$. P values are from the *Wald* test clustered at group level comparing the observed probability with corresponding predicted entry threshold. Standard deviations are in parentheses.

Table C.3: Selection efficiency: regression

	Low cost			High cost		
	Logistic		Linear	Logistic		Linear
	$\text{Eff}_{1\|2}$	$\text{Eff}_{1\&2}$	RE	$\text{Eff}_{1\|2}$	$\text{Eff}_{1\&2}$	RE
Constant (Ind)	1.836***	-0.761***	0.624***	1.909***	-0.568**	0.886***
	(0.250)	(0.127)	(0.014)	(0.203)	(0.248)	(0.036)
UnR	0.724***	1.924***	0.328***	-0.182	1.436***	0.111***
	(0.218)	(0.171)	(0.019)	(0.227)	(0.350)	(0.042)
Re	0.021	-0.349*	-0.016	-0.039	-0.247	-0.067
	(0.302)	(0.189)	(0.023)	(0.182)	(0.279)	(0.044)
# of Obs	714	655	700	666	435	639
# of group	72	72	72	72	72	72

Note: Indicator Efficiency$_{1or2}$ (Eff$_{1|2}$) and Efficiency$_{1\&2}$ (Eff$_{1\&2}$) are binary variables which take the value 1 if they are efficient, 0 if otherwise. Ratio Efficiency (RE) is a continues variable which takes values from 0 to 1 (inclusive). The entry decision of each group in each round is one observation unit. We run panel logistic regressions for indicator Efficiency$_{1or2}$ and Efficiency$_{1\&2}$, and panel linear regressions for indicator Ratio Efficiency; all models are clustered at session level.

Table C.4: Participation and selection effect

	c=5			c=25		
	UnR	**Re**	**Ind**	**UnR**	**Re**	**Ind**
$R_{predicted}$	153.93	145.69	150.02	101.80	104.83	106.84
NE_{Runr}	151.39	155.97	154.85	101.80	106.86	109.00
Participation		4.58	3.46		5.06	7.20
Selection		-12.82	-7.37		-2.04	-2.15

Note: $R_{predicted}$ is the predicted expected revenue calculated using the drawn values in the experiment. NE_{Runr} is the expected revenue calculated using the drawn values in the experiment, assuming that all the bidders who choose to enter the auction by equilibrium can enter the auction.

C.2 Characterization of equilibrium

C.2.1 Expected payoff functions in indicative bidding

For entrant i's expected payoff conditional on advancing with the opponent whose signal is drawn from interval $[a, b]$ is:

$$\pi(s_i|[a,b]) = \int_a^b max\{0, s_i - s\}\frac{1}{b-a}ds - c$$

Assume there is an marginal entrant i, who received a signal α_0 in stage 1. His expected payoff is:

$$\begin{aligned}\pi(\alpha_0, 1) =& \alpha_0^{N-1}(\alpha_0 + E(t) - c) + \sum_{j=1}^{N-1} P_j \cdot \frac{2}{j+1} \cdot (-c) \\ &+ C_{N-1}^1(1-\alpha_1) \cdot \sum_{h=0}^{N-2} P_h \cdot \frac{1}{1+h} \cdot (-c)\end{aligned}$$

In which $P_j = C_{N-1}^j(\alpha_1 - \alpha_0)^j(\alpha_0)^{N-1-j}$, $P_h = C_{N-2}^h(\alpha_1 - \alpha_0)^h(\alpha_0)^{N-2-h}$. The second term is the expected payoff when j opponent(s) chose $m = 1$ and no one choose $m = 2$, while the third term is the expected payoff when h opponent(s) chose $m = 1$ and 1 opponent choose $m = 2$.

We then assume marginal entrant i received α_1, his expected payoff from entering the auction is:

$$\begin{aligned}\pi(\alpha_1, 1) =& (\alpha_0)^{N-1}(\alpha_0 + E(t) - c) + \sum_{j=1}^{N-1} P_j \cdot \frac{2}{j+1} \cdot \pi(\alpha_1|[\alpha_0, \alpha_1]) \\ &+ C_{N-1}^1(1-\alpha_1) \cdot \sum_{h=0}^{N-2} P_h \cdot \frac{1}{1+h} \cdot (-c)\end{aligned}$$

However, if he choose entry message $m = 2$ with the signal of α_1, his expected payoff is:

$$\pi(\alpha_1, 2) = (\alpha_0)^{N-1}(\alpha_0 + E(t) - c) + \sum_{j=1}^{N-1} P_j \cdot \pi(\alpha_1|[\alpha_0, \alpha_1]) + \sum_{l=1}^{N-1} P_l \cdot \frac{2}{1+l} \cdot (-c)$$

In which $P_j = C_{N-1}^j(\alpha_1 - \alpha_0)^j\alpha_0^{N-1-j}$, $P_l = C_{N-1}^l(1-\alpha_1)^l\alpha_1^{N-1-l}$. The second term is the expected payoff when j opponent(s) chose $m = 1$,no one choose $m = 2$, while the third term is the expected payoff when l opponent(s) choose $m = 2$.

C.2.2 Expected revenue under three mechanisms

The expected revenue of unrestricted auction equals to:

$$R^{UnR}(\alpha_0) = \sum_{n=2}^{N} P_n \cdot \left[\alpha_0 + \frac{n-1}{n+1} \cdot (100 - \alpha_0) + 100\right] \tag{C.1}$$

Where $P_n = C_N^n(\frac{100-\alpha_0}{100})^n(\frac{\alpha_0}{100})^{N-n}$. The expected revenue of unrestricted auction can be seen as the weighted average of the revenue from the second price auction with n fixed number of bidders, where the weight is the probability of auction has n entrants.

The expected revenue of restricted auction is:

$$R^{Re} = \left(\sum_{n=2}^{N} P_n\right) \cdot \left[\alpha_0 + \frac{1}{3} \cdot (100 - \alpha_0) + 100\right] \tag{C.2}$$

Where $P_n = C_N^n(\frac{100-\alpha_0}{100})^n(\frac{\alpha_0}{100})^{N-n}$. The expected revenue of restricted auction equals to the expected revenue from the second price auction with two bidders conditional on entry as long as the number of bidder who choose to enter is larger than 2.

The expected revenue of indicative bidding is:

$$\begin{aligned} R^{Ind} =& \sum_{n=2}^{N} P_n \cdot \left\{ (\frac{\alpha_1 - \alpha_0}{100 - \alpha_0})^n \cdot [\alpha_0 + \frac{1}{3} \cdot (\alpha_1 - \alpha_0)] \right. \\ &+ C_n^1 \cdot (\frac{\alpha_1 - \alpha_0}{100 - \alpha_0})^{n-1} \cdot (\frac{100 - \alpha_1}{100 - \alpha_0}) \cdot [\alpha_0 + \frac{1}{2} \cdot (\alpha_1 - \alpha_0)] \\ &\left. + \sum_{w=2}^{n} C_n^w \cdot (\frac{100 - \alpha_1}{100 - \alpha_0})^w \cdot [\alpha_1 + \frac{1}{3} \cdot (100 - \alpha_1)] + E(t) \right\} \end{aligned} \tag{C.3}$$

Where $P_n = C_N^n(\frac{100-\alpha_0}{100})^n(\frac{\alpha_0}{100})^{N-n}$, w is the number of bidders who choose $m = 2$. The first, second and their term in the cursive bracket is the expected revenue when 0, 1 or at least 2 bidder(s) choose $m = 2$ conditional on $n \geq 2$ bidders choose to enter $(m \geq 1)$, respectively. [1]

[1]When all bidder who choose to enter send $m = 1$ or at least 2 bidder who send $m =$, the expected revenue is the expected second highest signal in interval $[\alpha_0, \alpha_1]$ or $[\alpha_1, 100]$, plus the expected additional

C.3 Experimental instructions

Welcome to our experiment! You will receive 30 RMB for showing up on time. Please read these instructions carefully and completely. Properly understanding the instructions will help you make better decisions and hence earn more money. The experiment will last about 1.5 hours. Your payoff in this experiment will be measured in experimental currency (i.e., EC). At the end of the experiment, we will convert your payoff in EC to cash and pay you in private. The exchange rate is 1 EC = 1.5 RMB.

Your total payment from this experiment will be the sum of:

(1) Your show-up fee: 30 RMB;

(2) Your payoff in this experiment;

Please keep in mind that you are not allowed to communicate with other participants during the experiment. If you do not obey this rule you will be asked to leave the laboratory. Whenever you have a question, please raise your hand; an experimenter will come to assist you.

Your task

At the beginning of the experiment, you will be given 30 EC as your initial endowment. In this experiment, there are 20 rounds in total. Before round 1, you will be randomly assigned to a group of 5 players and will stay in this group for the first 10 rounds. In round 11, you will be randomly reassigned to a new group of 5 players and stay in that group for the rest of the experiment. The computer will randomly assign letters from A to E to each of you as your player label and this assignment changes in every round. You will only be competing against the participants in your group.

In each round, you will participate in an auction. The auction takes place in two stages: in stage 1, you decide if you would like to enter stage 2 or not; in stage 2, (if you enter successfully,) you choose how much to bid in the auction. Your total valuation

value t. When 1 bidder choose $m = 2$ and $n - 1$ choose $m = 2$, the expected revenue is the expected highest signal in interval $[\alpha_0, \alpha_1]$, plus the expected additional value t.

towards the asset that you are bidding for is composed of two parts: You will get the first part of your valuation in stage 1 before you make the entry decision; then you will get the second part of your valuation in stage 2 before you enter your bid if you enter the auction.

Stage 1: Entry decision

The computer first randomly draws an integer from 1 to 100 (including 1 and 100) for each participant independently. Each integer has 1% chance to be drawn. You only observe your own draw. This number reveals the first part of your valuation towards the asset that you are bidding for in stage 2. After seeing this information, you need to make a decision on whether or not to enter the auction stage based on this partial information you have about your total valuation for the asset. You can choose 1 of the 3 options to indicate your willingness to enter: "0" (Do Not Enter), "1" and "2" (both represent enter with 2 giving you a higher entry priority than 1).

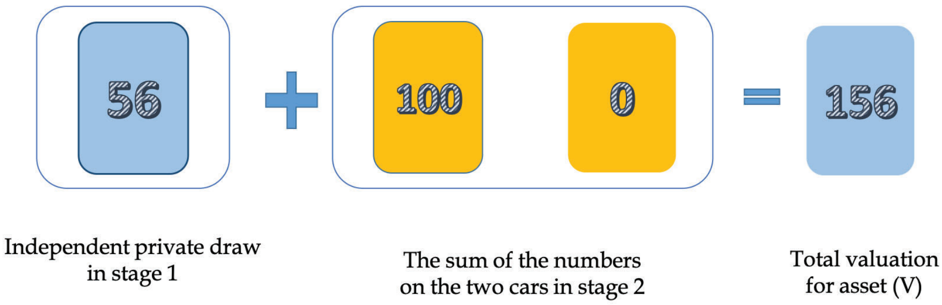
The number of entrants in each group in the auction stage is restricted to at most 2. If the number of players that choose either "1" or "2" is equal or less than 2, then all participants who choose "1" or "2" are selected to enter automatically. However, if there are more than 2 participants in your group choosing to enter, a selection process will apply. First we randomly select up to two players from those who chose "2". Then, if less than 2 players have been selected, we randomly select the remaining entrants from those who chose "1".

The selected entrant(s) will proceed to stage 2 and an entry fee will be charged. **Notice that the entry fee is 5 EC in Round 1-10, and then increases to 25 EC in Round 11-20.** If you are not selected, no entry fee will apply, and you do not need to make any more decisions in this round.

Stage 2: Auction stage

In this stage, all the entrants first see two cards on the screen. Each of the cards has two possible values, 0 and 100. The computer will randomly assign one of the values (0, or 100 with equal chance) independently to each card. The sum of the two numbers assigned to the two cards is the second part of your the asset valuation, which is the same for all entrants. There are four possible scenarios: both cards have 0; the first card is 0 and the second card is 100; the first card has 100 and the second card has 0; both cards have 100. Hence, the second part of the value shared by all entrants could be 0 EC with 25% chance, 100 EC with 50% chance, and 200 EC with 25% chance.

Each entrant's full valuation for the asset (V) is calculated as the sum of the first part of the valuation revealed in stage 1 (i.e., the independent private draw between 1-100) and the second part of the valuation (i.e., the sum of the numbers on the two cards which are drawn for each group) shared by all entrants revealed in stage 2. In the example below, if an entrant's draw in the first stage is 56, and then the computer assigned 100 and 0 to two cards in stage 2 respectively, his/her total valuation for the asset is 56+(0+100)=156.



After learning your total valuation, all the entrants need to bid for the asset. The entrant with the highest bid in your group will win the asset. The winner will pay an amount equal to the second highest bid. If there is a tie for the highest bid, a winner will be randomly selected among these bidders. In this case, the second highest bid is the same as the highest bid. When there is only one entrant, that player wins the asset and pays zero (i.e. the second highest bid).

Your payoff

Your payoff for each round will be calculated at the end of auction as the following:

- If you do not enter stage 2: your payoff = 0
- If you choose to enter and are selected:
  - If you lose, your payoff= - entry fee

– If you win, your payoff= your total valuation - the second highest bid - entry fee

Summary

You will play 20 rounds of this 2-stage auction game. You will be randomly assigned to a group of 5 participants twice during this experiment (first before round 1 and again before round 11). In each round, you decide whether to enter the auction or not and how much to bid. Your valuation for the asset in each round is determined by the sum of your independent draw (revealed in stage 1) and the common draw given to all entrants in stage 2. At the end of each round, all group members' entry decisions, two parts of the valuation information, entrants' bids and payoffs will be displayed to you, regardless of whether you chose to enter stage 2 or not. See a screenshot of this page below:

	Player A	Player B	Player C	Player D	Player E (You)
First signal	58 EC	73 EC	22 EC	93 EC	57 EC
Entry choice	1	1	2	2	0
Second value	-	-	0 EC	0 EC	-
Total valuation (V)	-	-	22 EC	93 EC	-
Bid	-	-	25 EC	20 EC	-
Win/Lose	-	-	Win	Lose	-
payoff	0 EC	0 EC	22 EC- 20 EC-5 EC= - 3 EC	0 - 5 EC= - 5 EC	0 EC

In addition, a history table which gives information about entry decisions and winner's payoffs from previous rounds is provided.

Your total payment

After you complete all 20 rounds, the computer will randomly draw 1 round to pay you. Your total payoff from the experiment will be your endowment (30 EC) plus your payoff in the drawn round. Your total payoff will then be converted into cash and paid to you together with your show-up fee (30 RMB) at the end of today's session.

Other information

To further ensure that everyone in this lab understands the game properly, you will need to answer several control questions that are constructed based on the information given out in these instructions. The experiment will start once all of you have answered these questions correctly. Please do not hesitate to ask for help if you have any questions regarding the information provided in our instructions or the control questions we ask you to answer.

At the end of today's experiment, you will also need to fill out a small post-experimental questionnaire, including some demographic information (e.g., your gender, age, major...) and your decisions in the experiment. All the information you provide will be kept anonymous and is strictly confidential. The only purpose of collecting this information from you is for academic research analysis.

Thank again for your participation and patience! The experiment will start soon…

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