Essays on Contest Design and Contract Design

by

Jiang Jiang

A dissertation submitted in partial fulfillment
of the requirements for the degree of
Doctor of Philosophy
(Economics)
in The University of Michigan
2016

Doctoral Committee:

Professor Yan Chen, Co-Chair
Associate Professor Yusufcan Masatlioglu, Co-Chair
Professor Stephan Lauermann
Assistant Professor Stephen Leider

This dissertation is dedicated to my parents Huaimu Jiang and Ping Tao.

ACKNOWLEDGEMENTS

I would like to thank Yan Chen, Stephan Lauermann, Stephen Leider, Yusufcan Masatlioglu, Roy Chen, Sherry Xin Li, Tracy Liu, Neslihan Uler, members of the BEE & ICD lab group, and seminar participants at the University of Michigan, the 2013 Annual Meetings of the Midwest Economics Association (Columbus, OH), and the 2015 North American Meetings of the Economic Science Association (Dallas, TX) for helpful discussions and comments, David Gill and Victoria Prowse for generously sharing their experiment program, and Shenzhe Wang and Tyler Fisher for excellent research assistance. The financial support from the National Science Foundation through grant no. SES 0720936, from Rackham Graduate School through the Rackham Graduate Student Research Grant, and from the Department of Economics through the MITRE Graduate Student Research Grant is gratefully acknowledged.

TABLE OF CONTENTS

LIST OF FIGURES

Figure

LIST OF TABLES

Table

LIST OF APPENDICES

Appendix

ABSTRACT

Essays on Contest Design and Contract Design

by

Jiang Jiang

Co-Chairs: Yan Chen, Yusufcan Masatlioglu

This dissertation is broadly on the topic of behavioral economics and mechanism design, using lab experiments to study two settings: contest design and contract design.

The first chapter studies how designers of two-stage elimination contests should allocate prizes and reveal interim performance information to achieve desired outcomes. To investigate the interactive effect of prize allocation and interim information revelation, I design a two-stage contest experiment with different combinations of these two instruments. While my theoretical model predicts a positive effect of awarding a single prize on effort regardless of information structure, I find this effect in the lab only when interim information is revealed. Moreover, revealing information motivates effort only under a single prize. These findings are consistent with a framework that ties together the intuitions of how these two instruments work on contestants: information on others' performance allows contestants to estimate their own probabilities of achieving different ranks, and hence increases their sensitivity to changes in the prize allocation that affects the ranks they are likely to reach. Given this interplay of prize and information, my findings suggest that contest designers should take into

account the state of one instrument when optimizing the other.

In the second chapter, we design a real-effort laboratory experiment to investigate how group identity influences decisions in a principal-agent framework with hidden action. Group identity is induced by random assignment to groups, and is further enhanced using a collective puzzle solving task. We find that the principals show ingroup favoritism towards ingroup agents by making more generous revenue-sharing offers. While ingroup agents are less tolerant of low offers from their principals, they exert greater effort in response to higher offers, relative to the control condition. The impact of group identity and incentives on agents' effort also depends on their perceptions of fair offers.

CHAPTER I

Prize Allocation and Information Revelation in Two-Stage Contests: Theoretical and Experimental Evidence

1.1 Introduction

Multi-stage contests are widely observed in real life and vary in terms of the dispersion of prizes across stages. For example, in the U.S. presidential campaign, each candidate first needs to win the nomination in his or her own party and then competes with the nominees from other parties for the presidency, which is the single prize in this contest. Sports tournaments also represent a large group of multiple-stage contests and the prize structure varies across games. For example, the World Cup has a single final prize, whereas tennis tournaments, such as U.S. Open, reward winners at each stage. Recently, we also observe many online contest platforms which solicit expertise around the world. Such online contest websites have become popular labor markets for companies and individuals to outsource tasks. One example is Topcoder, the largest software development website in the world, where both single- and multi-stage contests are conducted with various prize structures. Though the effect of prize allocation is well discussed under single-stage contest, few studies examine its effect in multi-stage contest. Therefore, it is important to understand the optimal prize allocation in multi-stage contests.

In addition to the prize structure, contestants may fully or partially observe each other's performance in real-life contests. For example, in the World Cup, the performance of each team in each stage is publicly observed. For workers climbing the job ladder in a firm, their performance at each stage is at least partially observed by their competitors. However, whether providing interim information has positive effects on individual effort has mixed answers in the literature. In particular, how workers with different ability levels respond to opponent's performance is an open question. Furthermore, most studies in the literature examine how information revelation affects individuals' future behavior, e.g., second-stage effort in multi-stage contest, while it may also have impact on individuals' first-stage effort.

Prize allocation and information revelation structures, however, may interact with each other's performance. On the one hand, varying the prize allocation rule changes the rewards for different ranks. This change in reward affects contestants' behaviors, and the effect may intensify when a contestant knows his probability of achieving each rank. For example, increasing the second-class award of a merit-based scholarship has more motivating effect on students who realize that they are excellent but yet to be the best, compared with their counterparts who know nothing of their positions. To examine how information may influence the prize effect, Freeman and Gelber (2010) engage subjects in a maze-solving task and vary the number of prizes as well as the information being revealed on contestants' pre-contest performance under a piece-rate incentive scheme. They find that awarding multiple prizes induces better performance by low-ability contestants than awarding a single prize. This prize effect, however, is only observed when contestant's pre-contest performance is publicly revealed so that low-ability contestants know that they have little chance of winning under a single prize.

On the other hand, revealing others' performance provides information on a contestant's relative ability and hence on his probability of achieving each rank. This information effect on the contestants behavior is enlarged when his winning probability is sufficiently large or small under certain prize allocation rule. For example, telling runners that they will encounter strong rivals in a race may causes more people to give up when only the fastest runner is awarded than the case when all top ten performers are awarded. In line with this intuition on how prize allocation rule may impact the information effect, Fershtman and Gneezy (2011) find that asking children to run side by side, similar to providing them instantaneous feedback on their opponents performance, induces more quitting than having them run separately under a single large prize. In comparison, they find no effect of information revelation under a single small prize.

While both Freeman and Gelber (2010) and Fershtman and Gneezy (2011) lend insight into the interplay between prize allocation and information revelation in one-stage contests, this study asks how the designer of a two-stage contest might simultaneously choose the prize and information structures to achieve desirable outcomes. Contest designers in the real life may have various objects, depending on the contexts of the contests, and this paper uses three of them to assess the optimal prize and information structures. The first object we look at is the average output in each stage. This is analogous to the revenue in auction models if we think of it in the total term. Corresponding real-life applications could be a sports tournament to motivate physical exercise by all participants, or an academic paper competition to encourage cutting-edge studies in a certain field. The second object we are interested in is the highest output in each stage. The intuition is that sometimes contest designers care about the best performance in addition to, or even instead of, the average performance by contestants. For example, the organizer of a worldwide sports tournament may care about breaking world records, and a user of the online labor market who posts a question may care about the quality of the best answer. The last object we use to measure the optimality is the aggregate output by a contestant in the two stages. In online labor markets such as TopCoder, it is common for contests designers who post crowdsourcing tasks to have all participants submit proposals in the first stage and promote only those with the best proposals into the second stage. Then in the second stage, participants develop their proposals into complete submissions to the task, based on which the final winner is chosen. In this real-life application, the quality of one's final submission is the quality of her proposal in the first stage plus the additional value she adds to the proposal in the second stage. So the contest designer cares about the aggregate output across stages.

Using a laboratory experiment that varies the prize dispersion and revelation of contestants' first-stage performance before the second stage in a two-stage elimination contest, this study allows a thorough comparison on performances of different combinations of the two structures. The contest experiment employs real effort slider task first introduced in Gill and Prowse (2012). Unlike my theoretical model which predicts a positive effect of awarding a single prize on effort regardless of information structure, I find this effect in the lab only when interim information is revealed. Moreover, revealing information motivates effort only under a single prize. These findings are consistent with a framework that ties together the intuitions of how these two instruments work on contestants: information on others' performance allows contestants to estimate their own probabilities of achieving different ranks, and hence increases their sensitivity to changes in the prize allocation that affects the ranks they are likely to reach. Given this interplay of prize and information, the findings suggest that contest designers should take into account the state of one instrument when optimizing the other.

The rest of the paper is organized as follows. Section 1.2 reviews the contest literature on prize allocation and information revelation. Section 1.3 presents a two-stage contest model to motivate the experiment. Section 1.4 describes the experimental design and procedures. Section 1.5 presents the analysis and results. Finally, Section 2.5 discusses the results and their implications in contest design.

1.2 Literature Review

This study fits into the contest literature on games in which players expend scarce resources to impact their probabilities of winning prizes while forfeiting the costs of expenditures regardless of winning. In this literature, three major frameworks have been used to model a wide variety of contests depending on how players' expenditures are translated into their outputs and winning probabilities: (1) the Tullock contest (Tullock, 1980), (2) the all-pay auction (Hillman and Riley, 1989; Baye et al., 1996), and (3) the rank-order tournament (Lazear and Rosen, 1981).

The three frameworks can be unified using a nested formulation (Dechenaux et al., 2015) as follows. Assume each player i expends effort e_i at a cost of $c(e_i)$. The output of player i is $y_i = e_i + \epsilon_i$, where ϵ_i is a noise term drawn independently for each player from a common distribution. Given the outputs by all players, player i's probability of winning as a function of all players' output profile $y = (y_1, y_2, ..., y_n) \geq 0$ is given by

$$p_i(y_i, y_{-i}) = \frac{y_i^\gamma}{\sum_{j=1}^n y_j^\gamma}$$

if $\sum_{j=1}^n y_j > 0$ and $\frac{1}{n}$ otherwise. The parameter γ measures the sensitivity of the contest rule in picking the winner. This nested formulation evolves into a simple Tullock contest when we assume a probabilistic contest outcome ($0 \leq \gamma < \infty$), no noise in the output ($\epsilon_i = 0$) and an identity cost function ($c(e_i) = e_i$). A special case of the Tullock contest occurs when $\gamma = 1$, which we refer to as the lottery contest. In comparison, if we require a deterministic outcome ($\gamma = \infty$) while keeping the other two assumptions as above, the nested formulation evolves into a simple all-pay auction. In a generalized case where we relax the common cost function requirement and allow $c(e_i)$ to be individual-specific, termed $c_i(e_i)$, this auction model is extended to an all-pay contest (Siegel, 2009). Lastly, the nested formulation evolves into a simple rank-order tournament when we require a deterministic contest outcome ($\gamma = \infty$) and a common cost function ($c_i(e_i) = c(e_i)$), but allow noise in the output ($y_i = e_i + \epsilon_i$).

The present paper considers contests that pick the player with the highest output as the winner and therefore requires a deterministic outcome ($\gamma = \infty$). For simplicity, our model assumes that players choose outputs directly instead of choosing effort; this avoids noise in the output ($\epsilon_i = 0$). This assumption of choosing outputs is reasonable when we notice that athletes in sports tournaments and students in academic competitions often set a goal for their grades or rankings and then strive for it. Additionally, the model also lets cost depend on effort in an individual-specific pattern as $c_i(e_i)$ to capture heterogeneous abilities of contestants. Given these constraints, the paper employs an all-pay contest model to motivate the experiment.

Since the seminal work of Rosen (1986), there has been a growing interest in multi-stage elimination contests. Theoretical research within this area has explored two types of elimination, which Fu and Lu (2012) refer to as matching protocol and pooling protocol. Matching protocol divides contestants in each stage into groups. A single winner from each group enters the next stage while losers are eliminated. Relevant to the present study, Moldovanu and Sela (2006) characterize the subgame perfect Nash equilibrium in a two-stage contest which is modeled as an all-pay contest with incomplete information. The second type of elimination, the pooling protocol, gathers contestants in the same stage such that each contestant confronts all others to compete for a number of prizes. No one may win more than one prize. So winners are eliminated from subsequent play and losers are promoted to the next stage. While most studies exploring the pooling protocol adopt the Tullock contest framework, Clark and Riis (1998a) extend a classical all-pay auction model with complete information from a single prize to multiple prizes and characterize the corresponding subgroup perfect Nash equilibrium. In a comparison between these two elimination protocols, Amegashie (2000) demonstrates in a two-stage Tullock contest that the pooling protocol collects more effort in the first stage. Nevertheless, the current work focuses on the matching protocol for two reasons. First, it is unclear to what extent the result of this comparison translates to the all-pay setup. Second, and more importantly, the matching protocol is a better approximation than the pooling protocol to many real-life contests where competitions in early stages are conducted within groups rather than in a large pool due to resource constraints.

A common theme among contest studies is to treat the contest structure as an endogenous choice of the designer and explore the effort-maximizing structure along various dimensions. The present paper is related to this theme with regard to two instruments, prize allocation and information revelation. Literature on prize allocation allows designers to allocate a fixed amount of prize money across ranks. Compared to the bulk body of theoretical work about one-stage contests (Glazer and Hassin, 1988; Barut and Kovenock, 1998; Clark and Riis, 1998b; Moldovanu and Sela, 2001; Moldovanu et al., 2007; Cohen and Sela, 2008; Sisak, 2009), only a handful of papers have analyzed the optimal prize allocation in multi-stage elimination contests. Rosen (1986) analytically addresses the importance of weighing the top prize heavily in order to maintain incentives along the hierarchical ladder. Fu and Lu (2012) also show that awarding the entire prize to the final winner generates the highest total effort over all contestants and stages, in their case in a multi-stage Tullock contest. However, a grand prize is not always optimal. In a two-stage all-pay contest with incomplete information, Moldovanu and Sela (2006) show that a grand prize elicits the highest total effort under linear cost-of-effort functions, whereas awarding multiple prizes among finalists may be optimal under convex costs. This conclusion is attributed to the role that cost convexity plays in the effect of secondary prizes on heterogeneous contestants. Awarding secondary prizes discourages contestants with high abilities while motivating low-ability contestants. This motivating effect gets amplified by convex costs since low-ability contestants face relatively low marginal costs, which may yield a higher total effort.

Experimental research on one-stage contests provides several interesting insights into optimal prize allocation in a contest setting. Evidence shows that optimality interacts with various features of a contest and its contestants. Sheremeta (2011) observes in a lottery contest that a single prize induces higher effort than multiple prizes. Allowing more flexibility in the form of the cost function, Müller and Schotter (2010) find support for the predictions of Moldovanu and Sela (2001) that a single-prize all-pay contest is optimal in case of a linear cost function while having two prizes is optimal given a convex cost function. While the aforementioned studies treat contestants as risk neutral, Lim et al. (2009) design a rank-order tournament with the prize spread endogenously determined using the elicited risk preference of subjects. They show that awarding multiple prizes collects more effort than awarding a single prize, consistent with the predictions of Kalra and Shi (2001). Furthermore, contestant heterogeneity can also be relevant. Chen et al. (2011) introduce two types of contestants with different levels of endowments, denoted "favorites" and "underdogs", in a rank-order tournament. In a treatment with two favorites and one underdog, they find that favorites exert more effort under two prizes than under a single prize, contrary to the theory prediction. They explain this inconsistency using a social comparison model that allows psychological losses from losing for favorites and psychological gains from winning for underdogs, since contestants' types and outcomes are publicly announced in their experiment. In summary, evidence across a wide range of specific contest models seems to support a single prize as being optimal for one-stage contests given basic assumptions, while multiple prizes may perform better when we take into account cost convexity, risk aversion or heterogeneity of contestants. However, it is important to emphasize that there are many ways to distribute prize values across ranks under a multiple-prize structure. So given that a multiple-prize structure is optimal for an one-stage contest, the prize spread must be theoretically optimal as well, which requires the designer to have *ex ante* knowledge on relevant parameters and choose the prize spread accordingly, as satisfied in Müller and Schotter (2010) and Lim et al. (2009). A multiple-prize design that fails to do so may yield under-performance compared with a single-prize structure, even if awarding multiple prizes is predicted to be optimal.

Two recent experiments on prize allocation rules in two-stage elimination contests find divergent results. Altmann et al. (2012) compare two multiple-prize structures in an all-pay contest with complete information and observe that, when the prize spread gets more convex (i.e., more weighted toward the top prize), the first-stage effort decreases by just 8% in spite of a striking decline by 43% in equilibrium. Since both schemes yield excess effort in the first stage, this observation implies that the overprovision of first-stage effort is boosted by a more convex prize spread. In contrast, Stracke et al. (2012) find that holding a single prize increases performance in the second stage at the expense of first-stage performance compared with holding multiple prizes in a lottery contest. A likely reason for the different findings of these two experiments is that their designs differ in several ways: contest framework (all-pay versus lottery), total prize money across competing prize structures (different versus identical) and number of rounds (1 versus 30). Besides, Stracke et al. (2012) also examines optimality in terms of total effort over all contestants and both stages. They find that, although the single-prize structure is predicted to be optimal, this optimality is attenuated by the attractiveness of the multiple-prize structure for risk-averse subjects.

Literature on the other instrument relevant to this study, information revelation in contests, endogenizes the provision of feedback on contestants' relative performance for the designer. Among the theoretical work along this dimension, Zhang and Wang (2009) examine the effect of revealing interim information in elimination contests. In a two-stage contest modeled as an all-pay auction, they show the non-existence of symmetric separating equilibrium strategies in the first stage when interim information is provided between stages, which decreases efficiency and total revenue in the first stage.

Experiments in the laboratory and the field about the effect of information revelation on contestants' behavior, although mostly concentrated on one-stage or multi-stage non-elimination contests, yield mixed findings. Eriksson et al. (2009) manipulate the degree of information revelation on rivals' performance in a two-person contest which asks subjects to add four two-digit numbers in 20 minutes. They find that continuous provision of feedback decreases the performance quality of underdogs, which they suspect to be driven by underdogs' anxiety and stress. However, Berger and Pope (2011) observe that being slightly behind at halftime increases success in professional basketball games where relative performance is instantaneously announced and attribute this to the role of self-efficacy. Some other papers have found the effect depends on contestants' relative status. Ludwig and Lünser (2012), for example, find in a two-stage rank-order tournament which promotes all contestants that those who lag in the first stage tend to increase their effort in the second stage and those who lead tend to reduce it. Besides the *ex post* effect, findings on the *ex ante* effect are also mixed. Kuhnen and Tymula (2012) find in a one-stage tournament that contestants work harder and expect to rank better when told that they will learn their rankings, relative to cases when they know that no feedback will be provided. Conversely, Sheremeta (2010) finds that disclosure of interim information decreases first-stage effort in a two-stage elimination lottery contest with carryover between stages. Although these studies are based on different theoretical models and thus are not directly comparable, the striking difference in findings highlights the importance of investigating both *ex ante* and *ex post* effects of information revelation in multi-stage elimination all-pay contests.

Different from Freeman and Gelber (2010) and Fershtman and Gneezy (2011) that focus on the interactive effect of prize allocation and information revelation in one-stage contests, this study is one of the first, to my knowledge, that examines this relationship in a two-stage contest environment. By allowing the designer to choose whether to reveal information on contestants' first-stage performance before the second stage, this experiment provides evidence on both *ex ante* and *ex post* effect of this interactive relationship.

1.3 Theoretical Framework

This section depicts the theoretical framework from which comparative statics predictions are derived as the basis of our experimental design and hypotheses. Following the model in Moldovanu and Sela (2006), we consider a two-stage elimination contest with n contestants, all assumed to be risk-neutral. The total prize in the contest is fixed at V to avoid the potential prize size effect.

Initially, the n contestants are evenly divided into t parallel groups, each with $k = \frac{n}{t}$ members. In the first stage, contestants compete within each group by choosing their first-stage performances, which can be considered as each choosing an output level. The one who achieves the highest output within each group is awarded a proportion α of the entire prize and promoted to the next stage. Then in the second stage, the t first-stage winners, one from each group, compete against each other for an single and additional award of the remaining prize $(1-t\alpha)V$. All other contestants are eliminated after the first stage. To make things well-defined, we assume $2 \leq t \leq n$ and $0 \leq \alpha \leq \frac{1}{t}$.

Each contestant i is characterized by an individual-specific parameter c_i that represents his ability. A high c_i implies a low ability for contestant i and vice versa. Abilities are assumed to affect the contestants through the costs they bear when producing in the contest. Specifically, each time contestant i produces output b, it yields a cost of $c_i b$. This cost function captures the idea that it is more costly for a low ability contestant to achieve a certain output level, compared with someone with high ability. Furthermore, we adopt the separability assumption in Moldovanu and Sela (2006) that first-stage outputs have no long-term effects on the second stage in terms of costs. Therefore, if contestant i produces $b_{1,i}$ and $b_{2,i}$ in the two stages, respectively, she bears a total cost of $c_i b_{1,i} + c_i b_{2,i}$.

The ability parameter c_i is private information to contestant i and drawn independently from the interval $[m, 1]$ according to the cumulative density function F, which is commonly known by all contestants. Without loss of generality, we assume F to be continuously differentiable with $f \equiv F' > 0$. The lower bound m is restricted to be strictly positive so as to refrain from infinite outputs due to zero costs. Later when we investigate the effect of splitting the prize among finalists under no information revelation, Proposition 1 further requires that $m \in [\frac{1}{2}, 1]$.

Given this contest model, two underlying features are worth paying attention to. First, we note that the output of a contestant is a noisy function of his effort and ability. Instead of choosing the invisible effort, contestants in our model choose the visible output, which is deterministically translated into the outcome of the contest. In this sense, our model fits into a perfectly discriminating contest. Second, the feature of contests determines that contestants pay for their outputs regardless of winning or not. In particular, the payoff of contestant i with ability parameter c_i is $\alpha V + (1 - t\alpha)V - c_i b_{1,i} - c_i b_{2,i}$ if she produces $b_{1,i}$ and $b_{2,i}$ in the two stages respectively and becomes the final winner, or $\alpha V - c_i b_{1,i} - c_i b_{2,i}$ if she wins in stage 1 but loses in stage 2, or $-c_i b_{1,i}$ if she loses in stage 1. For these two reasons, we model the contest described above using the all-pay auction framework.

The contest designer maximizes the expected average output among the participants in each stage by manipulating two characteristics of the contest: the number of prizes and the degree of information revelation. To control the former, the designer chooses α with $\alpha = 0$ corresponding to the case in which the final winner receives the entire prize and $\alpha > 0$ corresponding to the case in which the prize is split among all finalists in some certain way. To control the latter, the designer determines whether to reveal contestants' performance in the first stage before the second-stage competition. Taking the prize and information structures as exogenous, each contestant decides her outputs in the two stages (conditional on entering stage 2) to maximize her expected utility. In what follows, we characterize the comparative statics predictions by separately investigating the two-stage contest models with and without information revelation. We also restrict ourselves to the specific case relevant to our experimental design by assuming $n = 4$ and $t = 2$.

1.3.1 Two-Stage Elimination Contest without Information Revelation

When contestants' outputs in the first stage are concealed until the end of the contest, participants in the second stage know nothing about their opponents' past performance except that they won in the previous stage. Given this knowledge, each contestant in the second stage updates her beliefs on the distribution of her opponents' abilities if they are assumed to play separating equilibrium strategies in the first stage (which is verified by the equilibrium presented below). In particular, when each contestant i' first-stage output decreases in c_i, only the one with the lowest ability parameter in each group gets to enter the next stage. Since F represents the ability distribution of contestants in the first stage and k represents the size of each group, each second-stage contestant perceives the ability parameters of her opponents as being randomly drawn from the distribution of $G \equiv F_{(1,k)} = F_{(1,2)}$.[1] Here we denote

[1]We take $n = 4$ and $t = 2$ in the experiment, which implies $k = \frac{n}{t} = 2$.

by $F_{(1,k)}$ the cumulative density function of the first order statistics, which means the smallest value, of k independent random variables drawn from the distribution of F.

In this sense, the contest is equivalent to a two-stage nested all-pay auction with incomplete information, and hence is solvable using backward induction. From the analysis above, the two stages only differ in the distribution of contestants' abilities, the values of prizes (with the one in stage 2 being $(1-2\alpha)V$ and the one in stage 1 being αV plus the contestant's expected payoff in the second stage conditional on entering), and the numbers of participants. Moldovanu and Sela (2006) characterize the corresponding subgame perfect equilibrium, which is unique under the assumptions of strict monotonicity and symmetry on the equilibrium strategy. [2] Specifically, the equilibrium strategy in the first stage for a contestant with ability parameter c is given by

$$\beta_1(c) \;=\; \int_c^1 [\alpha V + (1-G(c))(1-2\alpha)V - c\beta_2(c)]\frac{1}{s}\,\mathrm{d}F(s) \tag{1.1}$$

And the equilibrium strategy in the second stage is

$$\beta_2(c) \;=\; \int_c^1 (1-2\alpha)V\frac{1}{s}\,\mathrm{d}G(s) \tag{1.2}$$

It is straightforward to find both $\beta_1(c)$ and $\beta_2(c)$ being strictly decreasing in c, indicating that the stronger contestants produce more outputs in both stages. This verifies the strict monotonicity assumption for the equilibrium.

This equilibrium allows us to make comparative statistics predictions on the effects of splitting the entire prize among finalists in both stage when no interim information

[2]By "strict monotoncity and symmetry" we mean an equilibrium in which all players adopt the same strategy that strictly decreases in their ability parameters. Later in this paper, we also mention this equilibrium as a symmetric separating equilibrium, for players with different types choose different equilibrium strategies. Besides, with the parameter values used in our experimental design, direct competitions in both stages engage only two contestants. In this specific situation, related literature (Amann and Leininger, 1996; Lizzeri and Persico, 2000) shows that the equilibrium characterized here remains unique even if asymmetric strategies are considered.

is revealed. For the first stage, Proposition 1 shows analytically that awarding only the final winner decreases contestants' expected average output when an additional assumption of $m \in [\frac{1}{2}, 1]$ is imposed.

Proposition 1. *When no interim information is revealed to contestant's before the second stage and under the assumption of* $m \in [\frac{1}{2}, 1]$*, the expected average output in the first stage is higher when the entire prize is shared by the finalists than the case when it is awarded only to the final winner.*

Proof. See Appendix A.1. □

Given this analytical result, we hypothesize that the effect is the similar when $m \in (0, \frac{1}{2})$. For the second stage, Proposition 2 indicates that awarding only the final winner increases contestants' expected average output in the second stage.

Proposition 2. *When no interim information is revealed to contestant's before the second stage, the expected average output in the second stage is lower when the entire prize is shared by the finalists than the case when it is awarded only to the final winner.*

Proof. See Appendix A.1. □

Based on Propositions 1 and 2, we expect that a multiple-prize contest with no information revelation should induce higher average output in the first stage but lower average output in the second stage than a single-prize contest, all else being equal. These predictions are formally stated in the following hypothesis:

Hypothesis 1 (Effect of Prize Split on Average Output, No Information Revelation)**.** *When no interim information is revealed to contestants before the second stage,*

a. *Splitting the entire prize among finalists increases the average output in stage 1, compared with awarding only the final winner.*

b. *Splitting the entire prize among finalists decreases the average output in stage 2, compared with awarding only the final winner.*

Hypothesis 1 states that the effect of splitting the prize works oppositely in the two stages. Intuitively, a prize split among the finalists is equivalent to an allocation of the entire prize toward the first stage, leaving a smaller additional award to the final winner. As a result, second-stage contestants with all abilities decrease their outputs, which yields a lower average output in the second stage. To understand the effect of such a prize split on contest in the first stage, we need to aggregate two conflicting influences: a larger monetary award to stage 1 winners and a lower expected payoff of entering the second stage due to the smaller stage 2 award. The weight on the second influence increases in contestants' abilities, since strong contestants enjoy larger winning probabilities and lower costs, which yields a larger expected payoff in the second stage compared with weak contestants. However, the joint effect of the first influence on all contestants exceeds that of the second influence, inducing a higher average output in the first stage.

This prediction allows us to determine the prize structure in the experiment through α under different objectives. The proof of Propositions 1 and 2 shows that α uniformly increases the expected average output in the first stage under certain constraint on m, and uniformly decreases it in the second stage. Therefore, it is optimal to choose $\alpha = \frac{1}{2}$ if the designer cares about stage 1 or $\alpha = 0$ if he cares about stage 2. In the experiment, we let $\alpha = 0$ for half of the treatments and $\alpha = \frac{1}{4}$ for the other half. The reason of having $\frac{1}{4}$ rather than $\frac{1}{2}$ is to make the contest close to reality, as $\alpha = \frac{1}{2}$ implies no additional prize for the last stage, which is rarely seen in real-life contests. By setting $\alpha = \frac{1}{4}$, we assume that the designer cares about outcomes in both stages and hence uses an equal split of the entire prize between the two stage.

1.3.2 Two-Stage Elimination Contest with Information Revelation

When contestants' outputs in the first stage are publicly revealed before the second-stage competition, each finalist observes the first-stage outputs of all other finalists before competing with them. The release of this interim information affects contestants' behaviors in both stages. In particular, contestants in the second stage will take the revealed information as a signal of their opponents' abilities and respond accordingly. In expectation of this response, each first-stage contestant may find it profitable to take advantage of the signaling effect in an attempt to manipulate the beliefs of her opponents in the next stage on her ability and thus their behaviors. For example, by producing a high output in the first stage, a contestant signals a strong ability to her potential opponents who will optimize by competing mildly. Knowing this and if the contestant enters the second stage, she will be better off by choosing fairly low outputs to minimize her cost while still enjoying a positive possibility of winning. Alternatively, a contestant biases others' beliefs of her ability downward by choosing a low output in the first stage. Knowing that she will be perceived as a weak type and hence encounter gentle competition in the second stage, the contestant will be better off by choosing some output level in stage 2 that barely surpasses her opponents to guarantee winning.

This intuition of how the information revelation mechanism works nullifies the existence of a symmetric separating equilibrium for the contest. Suppose such an equilibrium exists. It is required by the definition of a Nash equilibrium that no contestant has an incentive to deviate unilaterally from the equilibrium strategy when all other contestant are on the equilibrium path. The intuition discussed in the previous paragraph indicates that a contestant has different incentives in increasing and decreasing her output in the first stage. However, Zhang and Wang (2009) show in a model with identical two-stage hierarchical mechanism that no strictly monotone equilibrium function in the first stage satisfies the incentive compatibility constraints in both directions. While they establish this in the canonical all-pay auction model, we replicate their result in our contest environment, as presented by Proposition 3. Accordingly, if an equilibrium exists in the contest with information revelation, it must involve contestants playing either pooling or non-monotone or asymmetric or mixed strategies in the first stage.

Proposition 3. *In the two-stage elimination contest in which interim information is revealed before the second stage, no symmetric separating equilibrium exists in which all contestants adopt the same strictly monotone equilibrium strategy in the first stage.*

Proof. See Appendix A.2. □

The nonexistence of a symmetric separating equilibrium implies a loss of efficiency in the first stage. Simply put, efficiency in the first stage in our model means that only contestants with the higher abilities in their respective groups win in stage 1 and enter stage 2. To ensure efficiency given any realization of abilities, the equilibrium strategy in the first stage must be symmetric and strictly increasing in ability, or equivalently, strictly decreasing in ability parameter c. This requirement, however, is not satisfied when interim information is revealed according to Proposition 3.

To understand how this loss of efficiency affects the expected average output in the first stage, let us consider the competition within each group from the designer's perspective. Suppose that the contest designer's objective is to maximize the expected average output in each group. In doing so, he must select a group winner and award her an invisible prize (which is the monetary prize in the first stage, if any, plus entry into the next stage). This designer's problem is almost identical to the one studied in Myerson (1981), which investigates the optimal auction to achieve the highest expected revenue. We call it "almost" because our environment distinguishes from the Myerson environment in three ways: first of all, the contest designer can not keep the prize for himself; second, our problem is interpreted as a contest instead of an auction; and lastly, in our two-stage case, the prize of the first-stage contest is the monetary prize plus the expected payoff from the second stage. However, these differences do not deter us from using their conclusion as a benchmark. It is because, on the one hand, the Myerson environment nests our restriction on the designer by setting his personal valuation of the object at zero. And on the other hand, we show rigorously in Proposition 4 that comparable results hold in a grand contest environment. In particular, the expected average output of a group is maximized if the contestant with the higher ability wins.

Proposition 4. *All else being equal, the expected average output in a grand contest (i.e. one-stage contest with a single prize) is the highest when the contest achieves efficiency with the highest ability contestant being the winner.*

Proof. See Appendix A.3. □

Based on Propositions 3 and 4, we hypothesize that the average output over the whole group in the first stage is maximized when efficiency is achieved in both groups, which is the case only under no information revelation. Due to the loss of efficiency in the first stage under information revelation, contestants who enter the second stage do not necessarily have higher abilities than their first-stage opponents. This biases down the average abilities of contestants in the second stage, compared with the case with no information revelation. Therefore, it is reasonable to expect that revealing information also decreases the average output in the second stage. These conjectures are formally summarized as:

Hypothesis 2 (Effect of Information Revelation on Average Output, Single and Multiple Prizes)**.** *Revealing interim performance information decreases the average outputs in both stages, regardless of the prize structure.*

We next compare the average outputs under a single versus multiple prizes when interim information is revealed. As we have no analytical solutions on the existence of the equilibrium strategy or its specific form under information revelation, we hypothesize this to be the same as the case with no information revelation.

Hypothesis 3 (Effect of Prize Split on Average Output, With Information Revelation)**.** *When interim information is revealed to contestants before the second stage,*

a. Splitting the entire prize among finalists increases the average output in stage 1, compared with awarding only the final winner.

b. Splitting the entire prize among finalists decreases the average output in stage 2, compared with awarding only the final winner.

In addition to the average outputs, we are interested in the effects of prize allocation and information revelation on the highest outputs in both stages. To obtain hypotheses on the prize effect on highest outputs under no information revelation, we follow similar procedures in Propositions 1 and 2. Our prediction for the prize effect on average outputs under no information revelation is a derivative result of the individual-level conclusion summarized by equations (1.1) and (1.2). The two equations altogether indicate that, when no interim information is revealed, a player in the single-prize case with *any* given ability parameter produces less in stage 1 and more in stage 2 than in the multiple-prize case. By aggregating this individual-level result, Propositions 1 and 2 present average patterns that are similar to patterns on the individual level. Similarly, we can obtain the prize effect on the highest output in each stage under no revelation of information as another derivative result of equations (1.1) and (1.2). For the case with information revelation, we hypothesize that, just like the case of average outputs, the prize effects on the highest outputs are preserved when moving from no-information to with-information structure. Formally, we have the following:

Hypothesis 4 (Effect of Prize Split on Highest Output)**.** *Regardless of the information structure,*

a. Splitting the entire prize among finalists increases the highest output in stage 1, compared with awarding only the final winner.

b. *Splitting the entire prize among finalists decreases the highest output in stage 2, compared with awarding only the final winner.*

As for the effect of information revelation on the highest output, we obtain our hypothesis from the intuition of how the information revelation mechanism works on players with different abilities. When interim information is revealed before the second stage, Proposition 3 shows that no symmetric separating equilibrium exists in which players adopt the same and strictly monotone equilibrium output function in the first stage. An intuitive explanation to this nonexistence of monotone equilibrium strategies in the first stage is the manipulation incentive for players in the first stage. Specifically, when players in the second stage observe their opponents' first-stage outcomes, they form up a belief on their opponents' abilities and respond accordingly in the second stage. They know their opponents will do the same thing inversely. Going backward to the first stage, this gives them an incentive to manipulate the beliefs of their second-stage opponents by hiding their true abilities. In particular, those with high abilities have an incentive to restrict their outputs in stage 1 so as to fool their stage 2 opponents, while those with low abilities have an incentive to elevate their outputs in order to scare their stage 2 opponents. However, players in the latter group are not able to increase their outputs by too much given their low abilities and the resulting high costs. As a result, we expect the highest output in stage 1 to decrease, compared with the case when no information is revealed.

Hypothesis 5 (Effect of Information Revelation on Highest Output)**.** *Revealing information on performance in the first stage before the second-stage competition decreases the highest output in both stages, regardless of the prize structure.*

Lastly, it would be interesting to study the prize and information effects on the aggregate of a contestant's outputs in stage 1 and stage 2 given that she participates in both stages. As there is little theoretical or empirical evidence on how prize and information structures affect the sum of outputs in the two stages, we are agnostic to the directions of the impact. However, the analytical discussions in this section demonstrates that both splitting the entire prize among finalists and revealing interim information impact the theoretically predictions on contestants' behaviors in both stages. So it is reasonable to expect that the aggregate output is also affected.

Hypothesis 6 (Effect of Prize Split and Information Revelation on Aggregate Output Across Stages)**.** *The aggregate output of the two stages differs under different prize and information structures.*

1.4 Experimental Design

To test the theoretical predictions in Section 1.3, a laboratory experiment is conducted with alternative prize and information structures for a two-stage elimination contest. For the content of the contest, subjects undertake a computerized real effort task, which is derived from Gill and Prowse (2012) and referred to later as the "slider task". In each stage of competition, subjects work on a computer screen displaying 48 sliders for 120 seconds. To complete each slider, they need to use the computer mouse to position the cursor of the slider at exactly the midpoint. Their output is measured as the number of sliders completed at the end of the 120 seconds. Figures of the initial position for each slider, a completed slider, and a screen of the 48 sliders altogether, are shown in the instructions as provided in Appendix B. In addition to the desirable features of this slider task such as independence of preexisting expertise and identity across repetitions, as mentioned in Gill and Prowse (2012), it is chosen since it involves only routine physical activities with no practical value for the output. This attribute minimizes utility resulting from enjoyment and intrinsic motivation in undertaking real effort tasks (Brüggen and Strobel, 2007), which makes this slider task consistent with the model in Section 1.3 and therefore suitable for the experiment.

Built upon this two-stage elimination contest as illustrated in Figure 1.1, the experiment follows a 2×2 factorial design. Along the first dimension, the prize allocation rule is varied. In the "Single-Prize" treatments, the final winner of each cohort receives 100 tokens. In the "Multiple-Prize" treatments, the two finalists each receive a milestone prize of 25 tokens at the end of the first stage and the one who becomes the final winner receives an additional prize of 50 tokens at the end of the second stage. All other subjects earn 0 tokens. The total prize money is fixed across the two prize structures to avoid the size effect of the total prize.

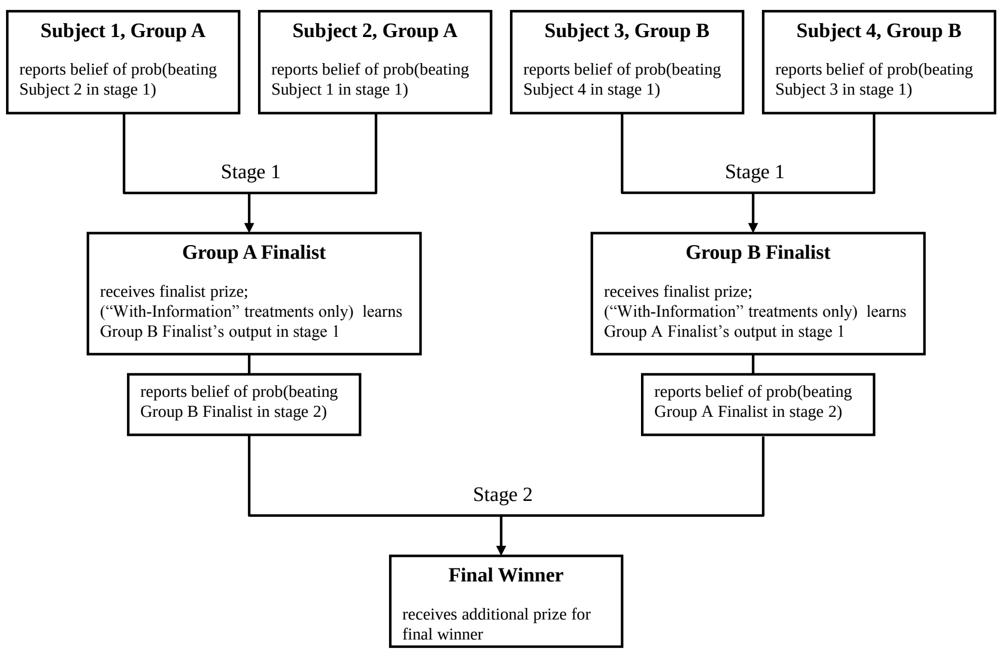


Figure 1.1: Flowchart of the Two-Stage Elimination Contest

Along the second dimension, the information revealed between the two stages is varied. In the "No-Information" treatments, subjects learn only the first-stage outputs for themselves and their opponents within the group. In the "With-Information" treatments, subjects also learn the first-stage outputs for the two subjects from the other group within their cohort. This difference in feedback about the first stage implies different information structures for competition in the second stage. Specifically, since paired finalists in the second stage are from different groups of the same cohort, they don't know each other's first-stage output in the "No-Information" treatments, but have the knowledge in the "With-Information" treatments. A summary of features for all four treatments is provided in Table 1.1 and instructions for the Multiple-Prize-With-Information (MP-WI) treatment are included in Appendix B.

Table 1.1: Experimental Design

Treatments	Stage 1 Prize	Stage 2 Prize	Information Revelation	Num. Subjects
Single-Prize-No-Information (SP-NI)	0	100	No	3×12
Multiple-Prize-No-Information (MP-NI)	25×2	50	No	3×12
Single-Prize-With-Information (SP-WI)	0	100	Yes	3×12
Multiple-Prize-With-Information (MP-WI)	25×2	50	Yes	3×12

Before starting the contests, subjects take a computerized quiz on the two-stage mechanism to enhance their understanding. Those who submit an incorrect entry to any quiz question are informed of the correct answer and provided an explanation on the screen. At the end of the experiment, subjects fill out a questionnaire on their demographic information, personality traits, risk and loss attitudes, competitiveness as well as their experience in the experiment. The questionnaire with response statistics is included in Appendix C.

As displayed in Table 1.1, three independent sessions were run for each treatment, with twelve subjects in each session. This yielded a total of twelve sessions conducted at the Behavioral and Experimental Economics Laboratory at the University of Michigan in July of 2012. Subjects were mostly students from the University of

Michigan, with each participating in only one session. People who had previous experience in either the slider task or similar contest-based economics experiments were excluded. Considering the feature of the slider task that asks for muscle movements of subjects, all sessions were held at the same time of each day to avoid potential confounds due to fluctuations in peoples physical statuses during the day. The experiment was programmed using zTree (Fischbacher, 2007).[3] Payoffs were denominated in experimental tokens and converted into U.S. dollars at the exchange rate of 1 token = \$0.05. The average per subject earning was \$19.17, including a \$5 show-up fee. Each session lasted approximately 60 minutes. Data are available from the author upon request.

1.5 Results

Before analyzing the results, one data issue due to an important feature of the experiment warrants some discussion. In the model described in Section 1.3, each player is characterized by an ability parameter that represents his ability and affects his cost in the contest. However, since the experiment requires subjects' real effort, each player's task-relevant ability is innate and thus invisible. So it is impossible to be measured directly. In dealing with this issue, we identify subjects' abilities using the number of sliders they complete in the second practice round.[4] Given this identification method, the only thing remaining before analyzing the data for the treatment effects is to check whether players' abilities in the slider task are randomly distributed across treatments. Pairwise Kolmogorov-Smirnov tests comparing the distribution of ability across treatments yield $p > 0.1$ for all comparisons, indicating

[3] I thank David Gill and Victoria Prowse for generously sharing their experiment program for the slider task.

[4] I use the second instead of the first practice round, as data collected from the four piece-rate sessions show that the average number of sliders completed increases by 18:429% (with $p < 0.05$) from the first round to the second round. This indicates a sizable learning process between the first two times that a player undertakes the slider task, which makes her ability in the second practice round a more precise approximation of her ability during the contests.

that the distributions of ability are comparable across different treatments. Results from this comparability test serve as the basis for the study of the treatment effects.

The following two subsections present the results from the experiment. We proceed by first reporting in Section 1.5.1 the results directly related to the theoretical predictions. Specifically, we investigate the effect of prize allocation and that of information revelation on three outcomes: (1) the average number of sliders completed in each stage; (2) the highest number of sliders completed in each stage; (3) the aggregate number of sliders completed across the two stages. Then, Section 2.5 presents data analysis results on the individual level. In the rest of the paper, output is defined as the number of sliders completed (which means correctly positioned) at the end of the two minutes if not particularly specified.

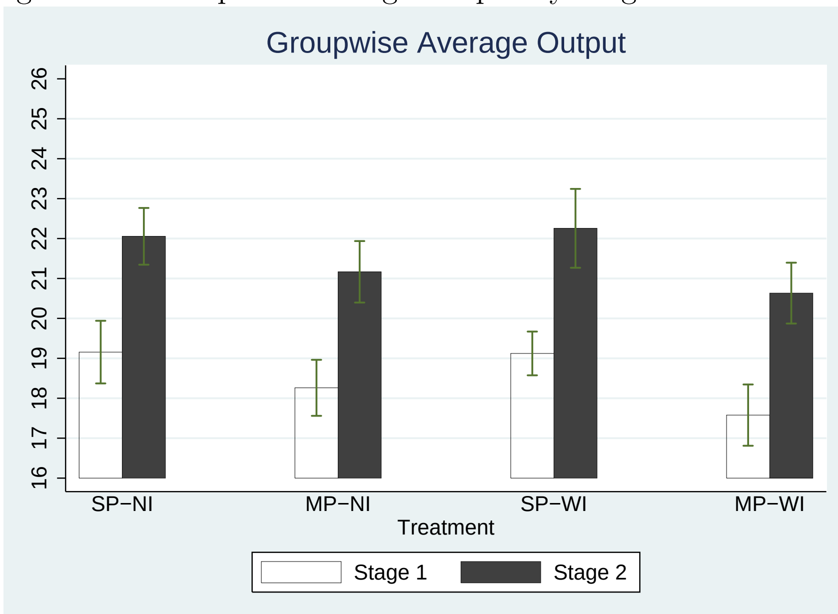
1.5.1 Treatment Effects

1.5.1.1 Average Output in Each Stage

We first examine the effect of prize allocation on the average output in each stage. Based on Hypothesis 1, we expect the single-prize structure to yield lower average output in stage 1 but higher average output in stage 2 than the multiple-prize structure in no-information treatments. Figure 1.2 presents the average number of completed sliders in both stages for all treatments. The horizontal axis presents the treatments: SP-NI (Single-Prize-No-Information), MP-NI (Multiple-Prize-No-Information), SP-WI (Single-Prize-With-Information), and MP-WI (Multiple-Prize-With-Information). For each treatment, the white bar denotes the mean (and standard deviation) of the groupwise average output in stage 1 while the black bar denotes the mean (and standard deviation) of the groupwise average output in stage 2. These observations lead to the following result.

Result 1 (Effect of Prize Split on Average Output, No Information Revelation)**.** *For both stages, the average outputs are the same in the Single-Prize No-Information*

Figure 1.2: Groupwise Average Output by Stage and Treatment



treatment and the Multiple-Prize No-Information treatment.

Support. *Table 1.2 presents the summary statistics and treatment effects for the groupwise average outputs in both stages. Specifically, we find the average outputs in both stages higher in the SP-NI treatment than in the MP-NI treatment (stage 1: 19.16 vs. 18.26; stage 2: 22.06 vs. 21.17). But neither of the differences is statistically significant ($p > 0.1$, two-sided permutation tests).*

Result 1 fails to reject the null of Hypothesis 1. Specifically, when no information is revealed, we find that splitting the entire prize had no significant impact on the average output in either stage.

We next analyze the effect of information revelation on the average output in each stage. Based on Hypothesis 2, we expect the revelation of interim information before the second-stage competition to yield lower average in both stages for single-prize and multiple-prize treatments. To check this hypothesis, we present in the upper panel of Table 1.3 the results from four Ordinary Least Squares (OLS) regressions to investigate the treatment effects on the average outputs. The dependent variables are

Table 1.2: Treatment Effects on the Average Output

Stage 1	No-Information	With-Information	*Information Effect*
Single-Prize	19.16	19.12	$p = 0.504$
Multiple-Prize	18.26	17.58	$p = 0.291$
Prize Effect	$p = 0.104$	$p = 0.049$	
Stage 2	No-Information	With-Information	*Information Effect*
Single-Prize	22.06	22.26	$p = 0.344$
Multiple-Prize	21.17	20.63	$p = 0.345$
Prize Effect	$p = 0.200$	$p = 0.097$	

the groupwise average outputs in the two stages, respectively. In specifications (1) and (3), the independent variables include the following (with omitted variables in parentheses): dummy variable for the multiple-prize structure (single-prize), dummy variable for the with-information structure (no-information) and the interaction term between the two dummies. These three independent variables allows us to test the effect of prize allocation and the effect of information revelation on the groupwise average output level. Specifications (2) and (4) further control for round to capture the possible learning effect in the slider task, and control for the groupwise average ability to take care of the ability variations across different groups. The two latter independent variables allows a robustness check on the results found in (1) and (3). Standard errors in parentheses are clustered at the session level. The middle and bottom panels of Table 1.3 present the results of the four pairwise comparisons across treatments to give us a more direct idea of the treatment effects on the average output in each stage.

In Table 1.3, we find the coefficient of the multiple-prize dummy negative but insignificant in all the four specifications. This verifies Result 1 that splitting the entire prize among finalists has no significant impact on the average output in either stage when no information is revealed. Furthermore, the coefficient of the information revelation dummy is significantly positive in (2) and weakly significant in (4), indicating a positive effect of revealing interim information under a single prize. Specifically, from no-information revelation to with-information revelation, the groupwise average output in either stage increases by 1. Lastly, the coefficient of the interaction term is significantly negative in (2) while insignificant but still negative in (4). This indicates that the increase in the average output driven by information revelation is larger for the single-prize treatments than for the multiple-prize treatments, especially in stage 1. This interaction factor contributes to the effect of awarding multiple prizes under information revelation and the effect of revealing interim information under multiple prizes by working jointly with the two dummy variables. We summarize the results below.

Result 2 (Effect of Information Revelation on Average Output, Single and Multiple Prizes)**.** *While information revelation has no treatment effect in summary statistics for either stage, it increases the average outputs in both stages (only) for the single-prize treatments, after controlling for learning and groupwise average ability.*

Support. *Table 1.2 reports the p-values of two-sided permutation tests for the effect of revealing interim information on the average output in each stage for both single-prize and multiple-prize treatments. None of the comparisons yields significant difference at the 10% level. In comparison, Table 1.3 reports the OLS regression results for average outputs. The coefficient of the information revelation dummy is positive and significant after controlling for learning and groupwise average ability (column (2): 0.882,* $p < 0.01$*; column (3): 1.006,* $p < 0.10$*), indicating a positive effect of information revelation for the single-prize treatments in both stages. The joint coefficient of the information revelation dummy and the interaction term is insignificant at the 10% level, indicating no effect of information revelation for the multiple-prize treatments in either stage.*

Result 2 suggests that revealing interim information before the second stage increases the average outputs in both stages under a single prize. This allows us to

Table 1.3: OLS: Determinants of Average Output

	Groupwise Average Output			
	Stage 1		Stage 2	
	(1)	(2)	(3)	(4)
Multiple Prizes (β_1)	-0.894	-0.166	-0.889	-0.248
	(0.765)	(0.539)	(0.962)	(0.913)
Info. Rev. (β_2)	-0.033	0.882***	0.200	1.006*
	(0.640)	(0.284)	(0.686)	(0.562)
Multiple Prizes $\times$ Info. Rev. (β_3)	-0.650	-1.584**	-0.733	-1.556
	(0.925)	(0.606)	(1.240)	(1.130)
Groupwise Average Ability		0.673***		0.592***
		(0.073)		(0.106)
Round		0.498***		0.531***
		(0.084)		(0.156)
Constant	19.156***	5.986***	22.056***	10.187***
	(0.616)	(1.192)	(0.401)	(2.075)
Observations	180	180	180	180
Pseudo R^2	0.074	0.420	0.057	0.286
Effect of Splitting a Prize: (*baseline treatment comes first*)				
SP-NI vs MP-NI: (β_1)	-0.894	-0.166	-0.889	-0.248
SP-WI vs MP-WI: ($\beta_1 + \beta_3$)	-1.544**	-1.750***	-1.622*	-1.803**
Effect of Revealing Info.: (*baseline treatment comes first*)				
SP-NI vs SP-WI: (β_2)	-0.033	0.882***	0.200	1.006*
MP-NI vs MP-WI: ($\beta_2 + \beta_3$)	-0.683	-0.702	-0.533	-0.550

Notes:

1. Standard errors in parentheses are clustered on the session level.
2. * $p < 10\%$; ** $p < 5\%$; *** $p < 1\%$.

reject the null in favor of Hypothesis 2 in the single-prize case. This observation on the first stage may be attributed to the incentive that first-stage contestants may have to impress their second-stage opponents and hence manipulate their opponents' behaviors in the second stage. The finding on the second stage may be caused by the possibility that people feel pressured to work hard when they learn how well they did compared with their opponents. We will explore this in Session 1.5.2. For the multiple-prize case, however, we are not able to reject the null hypothesis based on Result 2

Our next comparison result to check about the average output is the effect of splitting a single prize under information revelation. Based on Hypothesis 3, we expect lower first-stage average output but higher second-stage average output under a single prize than under multiple prizes, when interim information is revealed. Observations from Tables 1.2 and 1.3 are summarized below:

Result 3 (Effect of Prize Split on Average Output, With Information Revelation)**.** *For both stages, the average outputs are significantly higher in the Single-Prize With-Information treatment than in the Multiple-Prize With-Information treatment.*

Support. *The summary statistics in Table 1.2 shows that the average output in stage 1 is significantly higher in the SP-WI treatment than in the MP-WI treatment (19.12 vs. 17.58, $p = 0.049$, two-sided permutation test). Furthermore, this difference is weakly significant for the average output in stage 2 (22.26 vs. 20.63, $p = 0.097$, two-sided permutation tests). This result is verified by the parametric regressions in Table 1.3. As reported in the middle panel of Table 1.3, the joint coefficient of the multiple-prize dummy and the interaction term is significantly negatively after controlling for learning and groupwise average ability (column (2): -1.750, $p < 0.001$; column (3): -1.803, $p < 0.05$), indicating a negative effect of splitting the entire prize on the average outputs in both stages when interim information is revealed.*

Result 3 is inconsistent with Hypothesis 3a on the prize effect under information revelation in the first stage, but consistent with Hypothesis 3b in the second stage. Specifically, when interim performance information is revealed, splitting the entire prize increases the average outputs in both stages.

1.5.1.2 Highest Output in Each Stage

Now we move on to examine the prize and information effects on the second measure: the highest output in each stage. Similar to the definition of the average outputs, we define the highest output in stage 1 as the highest number of sliders completed in stage 1 among the four players in the same group, and that in stage 2 as the higher number of sliders completed by the two second-stage players in stage 2.

The means (and standard deviations) of the groupwise highest outputs in both stages are displayed in Table 1.4 and graphically presented in Figure 1.3. Table 1.4 also presents results from the significance tests for pairwise comparisons using two-sided permutation tests. Specifically, we find:

Result 4 (Effect of Prize Splite on Highest Output)**.** *For both stages, the highest outputs are significantly higher in the Single-Prize With-Information treatment than in the Multiple-Prize With-Information treatment. Comparisons between the Single-Prize No-Information and Multiple-Prize No-Information treatments yield no significant differences.*

Support. *In Table 1.4 that reports the summary statistics and treatment effects for the groupwise highest outputs in both stages, we find the average output to be (weakly) significantly higher in the SP-WI treatment than in the MP-WI treatment for both stages (stage 1: 23.47 vs. 21.82,* $p = 0.097$*; stage 2: 24.40 vs. 22.53,* $p = 0.097$*; two-sided permutation tests). In addition, the average outputs in both stages are also higher in the SP-NI treatment than that in the MP-NI treatment (stage 1: 23.87 vs. 22.27; stage 2: 24.16 vs. 23.27). But neither of the differences is statistically significant (*$p > 0.1$*, two-sided permutation tests).*

Figure 1.3: Groupwise Highest Output by Stage and Treatment

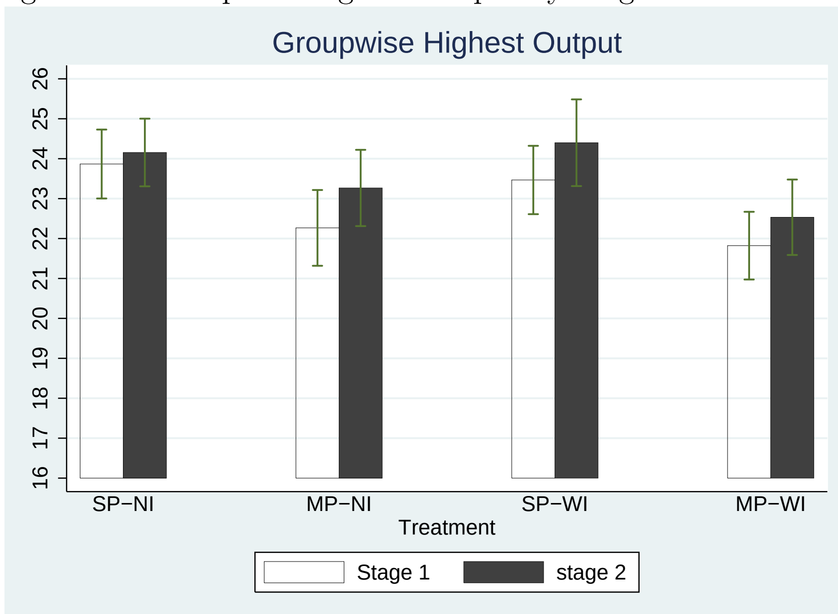


Table 1.4: Treatment Effects on the Groupwise Highest Output

Stage 1	No-Information	With-Information	*Information Effect*
Single-Prize	23.87	23.47	$p = 0.450$
Multiple-Prize	22.27	21.82	$p = 0.499$
Prize Effect	$p = 0.200$	$p = 0.097$	
Stage 2	No-Information	With-Information	*Information Effect*
Single-Prize	24.16	24.40	$p = 0.397$
Multiple-Prize	23.27	22.53	$p = 0.246$
Prize Effect	$p = 0.303$	$p = 0.097$	

For the no-information treatments, Result 4 fails to reject the null of Hypothesis 4; that is, splitting the entire prize has no imapct on the highest output in either stage under no information revelation. For the information treatments, Result 4 rejects Hypothesis 4a but fails to reject Hypothesis 4b; that is, splitting the entire prize decreases the highest outputs in both stages under information revelation. This is similar to our evaluations of Hypotheses 1 and 3. Since the prize effects on the average outputs and on the highest outputs shows common pattern, it is natural to wonder whether awarding a single prize increases the individual outputs in both stages for subjects regardless of their task-relevant abilities. We will explore this in Section 1.5.2.

Table 1.5: OLS: Determinants of Highest Output

	Groupwise Highest Output			
	Stage 1		Stage 2	
	(1)	(2)	(3)	(4)
Multiple Prizes (β_1)	-1.600	-0.982	-0.889	-0.323
	(1.151)	(1.154)	(1.122)	(1.167)
Info. Rev. (β_2)	-0.400	0.376	0.244	0.955
	(0.701)	(0.676)	(0.755)	(0.665)
Multiple Prizes × Info. Rev. (β_3)	-0.044	-0.836	-0.978	-1.703
	(1.373)	(1.377)	(1.447)	(1.419)
Ability of Highest-Output Achiever		0.570***		0.522***
		(0.075)		(0.103)
Round		0.681***		0.522**
		(0.100)		(0.181)
Constant	23.867***	11.925***	24.156***	13.525***
	(0.259)	(1.405)	(0.365)	(1.907)
Observations	180	180	180	180
Pseudo R^2	0.077	0.300	0.052	0.190
Effect of Splitting a Prize: (*baseline treatment comes first*)				
SP-NI vs MP-NI: (β_1)	-1.6	-0.982	-0.889	-0.323
SP-WI vs MP-WI: ($\beta_1 + \beta_3$)	-1.644*	-1.819**	-1.867*	-2.026**
Effect of Revealing Info.: (*baseline treatment comes first*)				
SP-NI vs SP-WI: (β_2)	-0.400	0.376	0.244	0.955
MP-NI vs MP-WI: ($\beta_2 + \beta_3$)	-0.444	-0.460	-0.733	-0.748

Notes:
1. Standard errors in parentheses are clustered on the session level.
2. * $p < 10\%$; ** $p < 5\%$; *** $p < 1\%$.

Table 1.5 displays the results from the OLS regressions on the highest output in each stage. The dependent variables are the groupwise highest outputs in the two stages, respectively. In specifications (1) and (3), we control for the multiple-prize dummy, the information-revelation dummy and their interaction. In (2) and (4), we further control for round and ability of the player who achieves the groupwise highest output in the corresponding stage. If several group members tie for the highest output in any stage, we take their average ability as the ability of the highest output player. Standard errors in parentheses are clustered at the session level. Similar to Table 1.3, we also report the results of direct pairwise comparisons across treatments in the middle and bottom panel of Table 1.5.

Three findings in Table 1.5 bring up to our attention. First, the coefficient of the multiple-prize dummy is negative but insignificant in all specifications. Second, the joint coefficient of the multiple-prize dummy and the interaction term is significantly negative, especially in specifications (2) and (4) where we control for round and the ability of the highest-output achiever. These two findings are in line with Result 4 that splitting the entire prize among finalists elevates the highest output in both stages only when a single prize is awarded. Finally, neither the coefficient of the information revelation dummy nor its joint coefficient with that of the interaction term shows any significant result, indicating no information effect in either stage on the highest output. This last finding is formally stated below:

Result 5 (Effect of Information Revelation on Highest Output)**.** *Information revelation has no treatment effect on the highest output in either stage, regardless of the prize structure.*

Support. *Table 1.4 reports no significant treatment effect of information revelation in summary statistics for either stage, regardless of the prize structure. This is further verified in Table 1.5. Specifically, neither the coefficient of the information revelation dummy and its joint coefficient with that of the interaction term is significant at the 10% level, indicating no effect of information revelation on the highest output in either stage.*

Result 5 allows us to reject Hypothesis 5. It indicates that, in both stages, the information effect is weaker on the highest outputs than on the average outputs. Noting that the highest outputs are more likely to be produced by subjects with high abilities, we suspect that subjects with high abilities are less responsive to the revelation of interim information while subjects with low abilities are more responsive.

1.5.1.3 Finalist's Aggregate of Output Across Stages

Finally, we would like to examine the treatment effects on the third measure: the aggregate output in the two stages. Since this outcome is defined as the sum of the numbers of sliders completed by a contestant in the two stages, it is available only for finalists.

Table 1.6 presents the results from two OLS specifications that investigate the factors affects the aggregate outputs in the two stages. The dependent variable is the sum of a finalist's outputs across stages while the independent variables of interest are the multiple-prize dummy, the information revelation dummy, and their interaction terms. Following similar reasons as in the previous regressions, we control for round and individual-level ability. In comparison to the contest literature, we also control for gender (with male being the omitted group) and the degree of risk aversion (on a scale of 1 to 10). Standard errors in parentheses are clustered at the session level. The middle and bottom panels of Table 1.6 display the results of direct pairwise comparisons across treatments.

Result 6 (Effect of Prize Split and Information Revelation on Aggregate Output Across Stages)**.** *The aggregate output of the two stages is significantly higher in the Single-Prize With-Information Treatment than in the Multiple-Prize With-Information Treatment. No treatment effect is found in other pairwise comparisons.*

Support. *In Table 1.6, the joint coefficient of the multiple-prize dummy and the interaction term is negative and significant at the 10% level in specification (1). This negative impact becomes more significant (i.e., at the 1% level) in specification (2) once controlling for individual ability, risk attitude, gender and round.*

Table 1.6: OLS: Determinants of Finalist's Aggregate Output in Two Stages

	Finalist's Aggregate Output	
	(1)	(2)
Multiple Prizes (β_1)	-2.256	-0.726
	(1.957)	(1.013)
Info. Rev. (β_2)	-0.378	0.778
	(1.263)	(0.590)
Multiple Prizes $\times$ Info. Rev. (β_3)	-0.433	-1.648
	(2.386)	(1.175)
Ability		0.993***
		(0.119)
Risk Aversion		0.325*
		(0.163)
Female		-2.482***
		(0.752)
Round		1.078***
		(0.149)
Constant	43.956***	21.495***
	(0.833)	(2.503)
Observations	360	360
Pseudo R^2	0.038	0.518
Effect of Splitting a Prize: (*baseline treatment comes first*)		
SP-NI vs MP-NI: (β_1)	-2.256	-0.726
SP-WI vs MP-WI: ($\beta_1 + \beta_3$)	-2.689*	-2.374***
Effect of Revealing Info.: (*baseline treatment comes first*)		
SP-NI vs SP-WI: (β_2)	-0.378	0.778
MP-NI vs MP-WI: ($\beta_2 + \beta_3$)	-0.811	-0.870

Notes:
1. Standard errors in parentheses are clustered on the session level.
2. * $p < 10\%$; ** $p < 5\%$; *** $p < 1\%$.

In summary, we find three discrepancies between the hypotheses and the data. The first is the interactive relationship between the prize and information effects. Based on the theoretical model, Hypotheses 1, 3 and 4 make uniform predictions on the prize effect regardless of the information structure, while the predictions in Hypotheses 2 and 5 on the information effect are also independent of the prize structure. However, the data indicates that awarding only the final winner boosts the average (and highest) outputs in both stage only when interim performance information is revealed. Besides, revealing interim information motivates the average outputs in both stages only when a single prize is awarded. Therefore, the effects of the prize and information structures depends on the status of each other.

The second discrepancy lies in the directions of the information effect in both stages. Based on Proposition 3 which demonstrates the nonexistence of a first-stage separating equilibrium under information revelation, Hypothesis 2 predicts a negative effect of revealing information on the average outputs in both stages when a single prize is awarded. In contrast, Result 2 shows that information revelation actually elevates the average outputs instead. Furthermore, Tables 1.5 and 1.6 provide more supporting evidences of this disconsistency in direction using the information effects on the highest output in each stage and the aggregate outputs across stages. Although neither of the latter two effects is statistically significant, their magnitudes aggree with the information effect on the average output in Result 2.

The third discrepancy lies in the prize effect in the first stage. Although Hypotheses 3 and 4 predict positive effects of splitting the entire prize among finalists on the average and highest outputs in stage 1, Results 3 and 4 show that both outcomes are lower in the case with multiple prizes than the case with a single prize. The findings in the prize effect in the second stage, however, is consistent with the corresponding hypotheses.

1.5.2 Individual Level Analysis

In this subsection, we investigate the extent to which the dicrepancies above are driven by contestants' different reactions to prize allocation and information revelation, which may depend on their different statuses in a two-stage contest.

For a robustness check, Table 1.7 reports results of four OLS specifications to investigate the determinants of the individual outputs, with standard errors clustered at the session level. The dependent variable is the individual outputs in either stage, while the independent variables are same as those in Table 1.6. The pairewise comparison results presented in Table 1.7 indicate that the treatment effects on the individual outputs are consistent with those at the aggregate level. Specifically, the joint coefficient of the multiple-prize dummy and the interaction term is negative in all four specifications, and signficiant at the 1% level in columns (2) and (4) once we control for individual features. This implies a negative effect of awarding multiple prizes on all individuals in both stages under information revelation. This treatment effect of prize allocation on individual outputs lends support to the common pattern in the prize effects that we find on the average output in each stage (Results 1 and 3), the highest output in each stage (Result 4), the aggregate output across the two stages (Result 6). In addition, the information revelation dummy is significantly positive at the 5% level, implying a positive effect of revealing interim information regardless of the stage under a single prize . Neither of the two other pairwise comparisons on the treatment effects (treatments SP-NI vs. MP-NI, and treatments MP-NI vs. MP-WI) shows any significant difference.

Besides the treatment effects, the significance on round indicates that learning the effects matter in the outputs. As subjects gain experience with the slider task, their skills improve and this lead to better performance. The significance on ability indicates that in addition to the learning process, subjects' outputs are also affected by their initial positions in undertaking the slider task, which is actually their inherent abilities. This result supports our approach of identifying subjects' relative abilities in the contest using their outputs under piece-rate pay schemes. The last interesting finding in the regression results is the change in the significance of the demographic variables. In particular, the significance of the risk aversion declines remarkably from being significant on the 1% level to being insignificant, while the opposite pattern is observed for the female dummy. This agrees with the intuition that the first-stage competition, being at an earlier stage of the contest, involves more uncertainty and hence is more relevant to subjects' risk attitudes. And the second-stage competition, being at a later stage of the contest, requires more persistence and thus relies more on subjects' demographic characteristics. A large experimental literature on gender differences in contests suggests that women are less competitive than men. Specifically Gneezy et al. (2003) compare the gender gap in competitive environments with that in noncompetitive environments. They find that men performed significantly better than women as the competitiveness of the environment increases. But it is only observed when the competition is mixed-gender. Niederle and Vesterlund (2007) find that, although no gender differences in performance are observed in their experiment, women are less likely to self select into tournaments than men. Our finding that women completes significantly less sliders than men in both stages makes us consistent with the general conclusions of the literature. In addition, when moving from stage 1 to stage 2, we find the gender difference to increase in terms of both the magnitude and the significance. This is in line with the results in Gneezy et al. (2003) if we consider the second-stage competition as a more competitive environment than the first stage, given the stronger participants and larger prizes in stage 2.

One potential explanation to the second discrepancy on the positive effect of information revelation in the second stage is how information revelation interacts with the relative status of a second-stage contestant in the pair in regard to their first-stage outputs. On one hand, second-stage contestants who did well in the first

Table 1.7: OLS: Determinants of Individual Output

	Individual Output			
	Stage 1		Stage 2	
	(1)	(2)	(3)	(4)
Multiple Prizes (β_1)	-0.894	-0.079	-0.889	-0.095
	(0.760)	(0.470)	(0.958)	(0.493)
Info. Rev. (β_2)	-0.033	0.882**	0.200	0.822**
	(0.636)	(0.354)	(0.683)	(0.317)
Multiple Prize × Info. Rev. (β_3)	-0.650	-1.463**	-0.733	-1.409**
	(0.919)	(0.593)	(1.234)	(0.643)
Ability		0.599***		0.527***
		(0.062)		(0.070)
Risk Aversion		0.361***		0.100
		(0.078)		(0.083)
Female		-1.041*		-1.129***
		(0.518)		(0.372)
Round		0.498***		0.491***
		(0.083)		(0.140)
Constant	19.156***	5.973***	22.056***	10.645***
	(0.612)	(1.287)	(0.399)	(0.414)
Observations	720	720	360	360
Pseudo R^2	0.020	0.400	0.030	0.399
Effect of Splitting a Prize: (*baseline treatment comes first*)				
SP-NI vs MP-NI: (β_1)	-0.894	-0.789	-0.889	-0.095
SP-WI vs MP-WI: ($\beta_1 + \beta_3$)	-1.544**	-1.542***	-1.622*	-1.504***
Effect of Revealing Info.: (*baseline treatment comes first*)				
SP-NI vs SP-WI: (β_2)	-0.033	0.882**	0.200	0.822**
MP-NI vs MP-WI: ($\beta_2 + \beta_3$)	-0.683	-0.581	-0.533	-0.586

Notes:

1. The dependent variable is agent's effort, i.e., the number of sliders completed.
2. Standard errors in parentheses are clustered on the session level.
3. * $p < 10\%$; ** $p < 5\%$; *** $p < 1\%$.

stage may feel encouraged when they learn how much they were ahead of their second-stage opponents in the first stage. On the other hand, those who did not do as well but still managed to win in the first stage may feel pressured to work hard when they learn how much they were lagged behind. Moreover, these interactions may depend on the size of the gap in the first-stage outputs within the pair. To explore this possibility, we divide second-stage contestants into three categories based on their relative statuses: "advantageous stage 2 players" who outperformed their second-stage opponents in the first stage, "disadvantageous stage 2 players" who underperformed their second-stage opponents in the first stage, and those who tie with their second-stage opponents in terms of first-stage performance. Table 1.7 investigates whether and how the presence and content of the information being revealed may influence the behaviors of second-stage contestants, with standard errors clustered at the session level. The dependent variable is the individual output in the second stage. The independent variables of interest are the size of first-stage gap between the contestant and her second-stage opponent, the information revelation dummy and the interaction term. The top panel displays the results of the OLS regressions while the bottom panel displays the results on how the first-stage gap size influences contestants in the second stage in regard to their relative statuses for no-information and with-information treatments, respectively.

Column (2) of Table 1.8 focuses on the case with only advantageous stage 2 players. The coefficient of the first-stage gap size is insignificant, implying that advantageous stage 2 players in no-information treatments show negligible reactions to the gap in the first-stage outputs. The joint coefficient of the gap size and the interaction term is significantly positive at the 1% level, indicating that their counterparts in the with-information treatments are motivated by learning how much they are ahead of their second-stage opponents in their first-stage performance. Similar patterns are shown in column (1) once we aggregate all stage 2 contestants.

Table 1.8: OLS: Advantageous vs Disadvantageous Stage 2 Players

Dependent Variable:	Individual Stage 2 Output		
	(1) All	(2) Advantageous	(3) Disadvantageous
| Diff. in Stage 1 Outputs| (γ_1)	0.004	0.092	-0.183*
	(0.043)	(0.107)	(0.085)
Info. Rev. (γ_2)	-0.377	-0.391	-1.321*
	(0.499)	(0.754)	(0.730)
|Diff. in Stage 1 Outputs| × Info. Rev. (γ_3)	0.129*	0.081	0.321**
	(0.065)	(0.104)	(0.119)
Ability	0.521***	0.481***	0.385***
	(0.072)	(0.104)	(0.092)
Female	-1.173***	-1.257*	-0.602
	(0.375)	(0.695)	(0.362)
Round	0.480***	0.522**	0.448**
	(0.141)	(0.187)	(0.201)
Observations	360	161	161
R^2	0.384	0.388	0.243
Direct Effect of | Diff. in Stage 1 Outputs|			
No-Info. Treatments: (γ_1)	0.004	0.092	-0.183*
With-Info. Treatments: ($\gamma_1 + \gamma_3$)	0.132**	0.173***	0.139

Notes:

1.Standard errors are adjusted for clustering at the session level.

2.Significant at: * 10%; ** 5%; *** 1%.

However, the regression results for disadvantageous stage 2 players, as presented in column (3), are different. First, the significantly negative coefficient of the first-stage gap size indicates that, when no interim information is revealed, the second-stage output by disadvantageous stage 2 players decreases with their first-stage gap. In addition, we lose the significance in the joint coefficient of the gap size and the interaction term, indicating that the first-stage gap has no effect on disadvantageous stage 2 players when interim information is revealed.

These different results in columns (2) and (3) jointly provides us a complete picture on how information on performance in earlier stages of a multiple-stage contest affects later-stage performance by contestants with different statuses. When interim information is revealed, contestants at relatively advantageous statuses are encouraged by learning how much they are ahead, and contestants at relatively disadvantageous statuses stop responding negatively to the gap in performance in earlier stages. These two impacts jointly result in the positive *ex post* effect of information revelation that we find in the data.

1.6 Conclusion

For contest designers, an important decision is to choose the rules of prize allocation and information revelation in order to achieve desirable outcomes. Despite the large volume of the theoretical and empirical research in this area, the interactive effect of these two instruments has been surprisingly understudied. This study investigates the interplay of prize allocation and information revelation in a two-stage contest using a real-effort laboratory experiment. The results show that awarding a single prize to the final winner significantly elevates contestants' average effort in both stages only when interim information is revealed. In addition, revealing information positively affects contestants' average effort in both stages only under a single prize. This dependency of the optimal prize allocation on the information structure is also observed if the contest designer cares about contestants' highest effort in each stage or each contestant's aggregate effort across stages. Based on these findings, this study suggests that a contest design who wants to optimize either the prize or the information structure should consider the two simultaneously. Within the framework of the two-stage elimination contests studied in this paper, the optimal choice is to awarding only the final winner while revealing interim information at the same time.

In addition to the result on the optimal combination of prize and information structures, we explain the unpredicted effect of information revelation in the second stage. To investigate this discrepancy between predictions and the data, we categorize the contestants in the second stage into two types depending on their relative statuses compared with their opponents, and consider the important role that the content of the information being revealed may play on these two types. We find that when information is revealed, the difference between the finalists' first-stage outputs positively affects greatly motivates contestants with relatively high achievements in the first stage. For contestants with relatively low achievements in the first stage, information revelation also diminishes their negative response to the first-stage output gap. These results suggests that the role of information revelation on performance in earlier stages should be emphasized in multiple-stage contests.

While this study focuses on the interplay between prize allocation and information revelation in two-stage elimination contests, our model and experimental protocol could be extended into further research on the optimal prize and information structures in elimination contests with more than two stages and non-elimination dynamic contests. More research could also be conducted on the unpredicted *ex ante* effect of information revelation on contestants' behaviors in the first stage.

CHAPTER II

Group Identity and Partnership

2.1 Introduction

The central topic of contract theory in economics is how to design incentive schemes to motivate agents to align their interest with that of the principals (Bolton and Dewatripont, 2005). Although a large volume of literature shows that economic agents respond to monetary incentives, it also provides compelling evidence on the drawbacks of incentive schemes (see the reviews by Gibbons (1998) and by Prendergast (1999)). In another stream of research, an increasing number of studies investigate non-pecuniary factors that may be useful supplements to monetary incentives in increasing organizational efficiency (e.g., workers' fairness concerns in Akerlof and Yellen (1990), morale in Bewley (1999), team spirit in Kandel and Lazear (1992), intrinsic motivation in Besley and Ghatak (2005) and Prendergast (2007)).[1] Among these studies one important contribution is Akerlof and Kranton (2005) that first introduces social identity as a missing source of motivation, *albeit* essential to psychology and sociology of the employee-organization relationship, into an economic model of incentives. They incorporate social identity as behavioral norms into principal-agent models, and show the important interactions between identity and incentives.

[1] Research also shows that extrinsic (i.e., monetary) incentives may crowd out intrinsic motivations for economic agents (see a review by Gneezy et al. (2011)).

Social identity theory in psychology was developed by Tajfel and Turner (1979). The important impact of group identity – perceived membership in a social group – on individual behavior has been documented in a large volume of psychology literature (see the reviews by Brewer (1991) and by Abdelal et al. (2009)). The concept of social identity is introduced into economics in the seminal work by Akerlof and Kranton (2000) to analyze gender discrimination, poverty, social exclusion, and the household division of labor. Since then economic research on identity has flourished, and new insights have been gained into economic phenomena that traditional economic analysis cannot explain. Specifically, social identity has been incorporated into an expanding volume of theoretical research (Akerlof and Kranton, 2005; Fang and Loury, 2005; Basu, 2010; Bénabou and Tirole, 2011). In addition, a growing number of experimental economics studies show that identity has important impact on individual social preferences (McLeish and Oxoby, 2007; Chen and Li, 2009), cooperation in prisoner's dilemma games (Goette et al., 2006) and in public goods provision (Solow and Kirkwood, 2002; Eckel and Grossman, 2005; Croson and Marks, 2008; Benjamin et al., 2013; Charness et al., 2014), punishment of norm violation (Bernhard et al., 2006), coordination in the battle of the sexes game (Charness et al., 2007; Charness and Rustichini, 2011) and in the minimum-effort game (Chen and Chen, 2011; Chen and Li, 2015), trust (Pan and Houser, 2013), discrimination and inequality (Hoff and Pandey, 2006, 2014; Afridi et al., 2015), risk and time preferences (Benjamin et al., 2010). Much of this literature is summarized and discussed in (Akerlof and Kranton, 2010).

In this fast burgeoning literature, however, there has been surprisingly little research that explores empirically how identity influences work incentives in a principal-agent environment since Akerlof and Kranton (2005). One exception to our best knowledge is Masella et al. (2014). They study how group membership induced in the lab influences the effectiveness of the use of incentives and control mechanism in a principal-agent environment. They find that the control mechanism imposed by the principal on the agent's choice can have detrimental effects due to different reasons specific to group membership. The detrimental effect occurs within groups because the agent does not expect to be controlled by an ingroup principal and hence reacts negatively when being controlled. It occurs between groups since the agent perceives the control by an outgroup principal more hostile.

Different from Masella et al. (2014) that focus on the control mechanism, we ask how group identity influences the principals' decisions on the offer of monetary incentive, and the impact of this incentive on the agents' choices of *real* effort. We investigate these questions by designing a principal-agent experiment in the laboratory in which principals and agents, with their group identities being randomly assigned and further enhanced via a collective puzzle solving task, interact in a real effort game with hidden action. The principals and agents are paired within groups or across groups. Their decisions are then compared and contrasted to the situations when group categorization is absent. We find that the principals show ingroup favoritism by making more generous revenue-sharing offers to ingroup agents. Ingroup agents, however, respond to low offers with significantly *lower* effort, relative to the control and the outgroup treatments. Nevertheless, their effort increases *more* substantially as the offers improve, compared to the other two treatments. In contrast, outgroup agents are more tolerant of low offers, although their response in effort to better offers is significantly weaker than do ingroup agents. Interestingly, the impact of group identity on agents' effort and its interaction effect with monetary incentives is dependent upon perceptions on fairness of incentives.

This study contributes to the literature by showing the essence of incorporating identity-based motivation into the agency theory. Our results show that even with a simple incentive scheme such as revenue sharing, group identity has *differential* impact on the choices of principals and agents who interact within or between groups. These results highlight the crucial roles that identity-based "motivational capital" (Akerlof and Kranton 2005, p. 29) may play in the contract design for the principal-agent problems.

The remainder of the paper is organized as follows. In Section 2.2 we introduce the conceptual framework of the principal-agent problem with hidden action. Section 2.3 describes the experimental design, and discusses the hypotheses. Section 2.4 presents the analysis and results. Section 2.5 concludes.

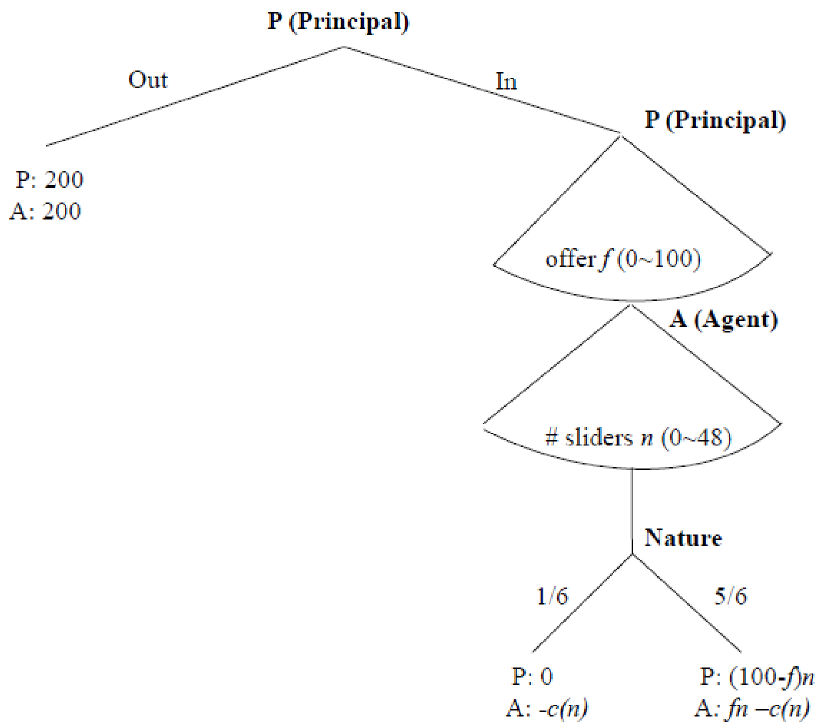
2.2 Principal-Agent Problem with Hidden Action

The principal-agent problem with hidden action that we consider is depicted in Figure 2.1. While the principal and agent are referred to as roles A and B in the experimental instructions, they are referred to as P (for principal) and A (for agent) in Figure 2.1 and the rest of the paper for the ease of explanation. The principal (he) could choose to form a business partnership with the agent (her) who will then work on a task on his behalf. However, the agent's intention and action is not perfectly observable to the principal. As shown in Figure 2.1, the principal first chooses between an outside option 'Out' which yields a payoff of 200 for each player and an option 'In' which leads to forming a partnership with the agent. If the principal chooses 'In', he will need to make the agent an offer f (an integer from 0 to 100) per slider.[2] Upon observing the offer, the agent will work on a real effort task, and her effort is measured by n – an integer from 0 to 48 (details on the task will be described in Section 2.3). Then nature moves to determine the final payoffs. With a $\frac{1}{6}$ chance, the agent's work will be destroyed, and both players will receive zero payoffs. With a $\frac{5}{6}$ chance, payoffs will be made based on the offer and the amount of work completed. Specifically, the principal receives a payoff of $(100 - f)n$, and the agent fn. The

[2]This design of outside option allows us to study if the principal's decision to engage in a partnership with the agent is influenced by their group membership.

agent's cost of effort, $c(n)$, enters as a negative term in her *net* payoffs in Figure 1 (assume $c'(\cdot) > 0$ and $c''(\cdot) > 0$.) To sum up, in this hidden action principal-agent game, the principal first decides how to split the revenue with the agent who then chooses her effort that ultimately determines the size of the revenue.

Figure 2.1: Hidden Action Game



This game is designed based on the hidden action trust game introduced by Charness and Dufwenberg (2006).[3] While the principal and the agent face binary choices throughout the game in Charness and Dufwenberg (2006), we extend their design by allowing both parties to choose from a wider range of incentive and effort, respectively. This framework is used to simulate the real-life scenarios in which the principal has less information than the agent on the production process (e.g., her intention and action), and hence tries to align the agent's interest through revenue sharing.

[3]Pan and Houser (2013) create cultural groups in the lab using a production process similar to Eckel and Grossman (2005), and then investigate participants' choices in the trust game with hidden game designed by Charness and Dufwenberg (2006). They find that cultural groups formed during a more cooperative production process display less parochialism than groups formed during a more independent production process.

We use a simple payment scheme – revenue sharing – in our study mainly for two reasons. First, revenue sharing is a popular incentive scheme and broadly applies to many forms of economics and financial transactions. For example, business owners may engage in partnerships and split profits among them. They may also pay partners or associates a percentage-based reward for referring new customers. Companies sometimes offer their employees bonuses based on the companies' profits. Some major professional sports leagues use revenue sharing with ticket proceeds and merchandising. Revenues generated through clicks on online advertisement links are sometimes shared among the companies that offer the services and the websites where the advertisements appear. In these examples, the parties involved may come from the same or different social groups. How their group identities affect the distribution of wealth and how it influences the effort of the parties involved are thus interesting research questions to study. Second, as one of the early empirical investigations on identity and incentives, our research aims to focus on the interaction between group identity and the *average level* of incentives. The use of the revenue sharing scheme helps us achieve this goal by isolating the research question of our interest from other dimensions of the incentive design problem (e.g., the variation of work incentives).[4]

In the experiment we will introduce group identity into the framework illustrated in Figure 2.1, and then study how group identity influences the principal-agent interactions. In the next section we will discuss our group manipulation approach and the testing hypotheses.

[4]One important implication of the identity-enhanced principal-agent models in Akerlof and Kranton (2005) is that agents who identify with the company as insiders are willing to put in high effort even with little wage variation. This is an interesting question to investigate empirically, but outside the scope of this paper.

2.3 Experimental Design and Hypotheses

A between-subject design is used in this experiment and summarized in Table 2.1. In the group treatments group identity is induced based on the enhanced-group approach designed by Chen and Li (2009) and Chen and Chen (2011), while the control treatment involves no group categorization. The experiment contains mainly two stages, group manipulation and enhancement in Stage 1, and a real-effort hidden action game in Stage 2. There are four independent sessions in each treatment, with twelve subjects in each session.

2.3.1 Group Manipulation

In the group treatments, subjects receive a white envelop that contains either a red or a green card, and are randomly assigned to two groups of six with the corresponding color. Each subject then receives a binder that contains 16 randomly ordered pictures that are taken from children's wordless picture books *Zoom* and *Re-Zoom* by Istvan Banyai.[5] Subjects are told that 14 of the pictures (from *Zoom*) can be used to form a sensible sequence while 2 pictures (from *Re-Zoom*) are irrelevant. Their task is to find the 14 pictures, identify the pattern (zoom-in or zoom-out), and enter the picture numbers – correctly ordered – on the computer screen within 10 minutes.

During the 10 minutes, they may chat virtually with their ingroup members about how to solve the puzzle. Chat messages are only shared within each group.[6] Subjects are also instructed (and all obeyed these rules) not to reveal any information that may identify themselves, and to refrain from using any obscene or offensive languages. Each participant is free to submit his or her own answer, and the correct answer

[5] The two wordless picture books present two different series of pictures in the effect of a camera lens zooming out. Subjects were neither told about the titles of the books nor given any hints on what the correct sequence might look like. Please refer to Banyai (1998b) and Banyai (1998a) for the two books, respectively.

[6] The problem solving task is similar to the one used in Chen and Li (2009), Chen and Chen (2011). The only difference is that this study uses the puzzle on the picture sequence whereas Chen and Li (2009) as well as Chen and Chen (2011) use paintings by Kandinsky and Klee.

Table 2.1: Experimental Design

Treatment	Stage 1: Group Manipulation		Stage 2:	Number of	Number of
	Group Categorization	Puzzle Solving	Hidden Action Game	Sessions	Subjects
Control	None	Individual	With another subject	4	48
Ingroup	Random	Chat within group	With ingroup only	4	48
Outgroup	Random	Chat within group	With outgroup only	4	48

is awarded with 200 tokens. Groups remain fixed throughout the experiment. To minimize potential endowment effect, the payoff information in this stage is withheld from subjects until the end of the experiment.

The control treatment parallels the group treatments. Subjects are asked to solve the picture puzzle individually with no communication with others. They submit their individual answers, and correct answers are awarded with 200 tokens, the same rate as in the group treatments.

2.3.2 Real-Effort Hidden Action Game

Stage 2 contains three paying rounds in which subjects are paired and play the real-effort hidden action game. In the group treatments, subjects are randomly assigned to be P (principal) or A (agent). Note we use neutral language in the experimental instructions and refer them as 'role A' and 'role B', respectively. It is public information that there are 3 Ps and 3 As in each group, and everyone's role is fixed for the remainder of the experiment. Then each subject is randomly paired with a different participant of the opposite role in each of the three rounds.

In each round, P (he) chooses between entering a contractual relationship with A (she) and an outside option. If P chooses the outside option, both P and A will each get 200 tokens. If P chooses to enter, he needs to make A a piece-rate offer f before A can perform the slider task for the pair. Since each correctly positioned slider brings to the pair 100 tokens, the piece-rate offer can be any integer from 0 to 100 tokens per slider.

After observing P's offer, A has 2 minutes to work on the sliders with the maximum possible of 48 sliders.[7] Principal P does not work on the sliders. While working, agent

[7]The slider task is adopted from Gill and Prowse (2012). This task suits the purpose of this study since it involves little learning and no guessing, is simple but tedious (to make the work costly for subjects), has no intrinsic value to experimenters (to minimize subjects' reciprocation to experimenters), needs minimal level of quality control (hence is logistically simple), and offers good degree of variation on the effort measure.

A could observe the number of sliders completed on the screen. After 2 minutes are up in a round, the computer randomly rolls a 6-side die, independently for each pair, to decide the outcome. If the roll comes up 1 through 5, agent A gets fn tokens where n is the number of sliders she has completed, and principal P gets $(100-f)n$ tokens. If the roll comes up 6, they each get zero tokens. Because of the die roll, principal cannot perfectly observe agent's actual effort. This design is used to simulate some real life principal-agent problems in which an agent usually has more information about her action and does not necessarily act in the best interest of the principal.[8]

At the end of each round, feedback information is given on each subject's own round payoff and the co-player's round payoff, as well as own cumulative payoff. The agent, but not the principal, is also informed of the die roll. Subjects are paid with the cumulative earnings through the three paying rounds.

We employ a between-subject design. Specifically, the co-players for each subject always come from ingroup in the Ingroup treatment, from outgroup in the Outgroup treatment, and from the rest of the subjects in the Control treatment. In addition, we adopt a rotation matching scheme from Cooper et al. (1996) to insure the matching between P and A in each round is truly one-shot matching so that any potential contagion effects could be minimized across different interactions. This matching scheme is explained to subjects so that they understand their actions would have no impact on whom they would be paired with in the future rounds.

2.3.3 Experimental Procedure

After Stage 1 in each session, the slider task was introduced. Then all subjects were allowed to practice the sliders on their own paces in two 2-minute rounds with no monetary award.[9] After the practice rounds, they were randomly paired and played

[8]As discussed in the results section, some agents indeed choose to complete zero number of sliders, most likely due to their dissatisfaction with the principals' offers.

[9]The average numbers of sliders completed were 15 and 16 in the two practice rounds.

the real-effort hidden action game for three rounds.

To assess individual heterogeneous capability in the slider task, a standalone piece-rate slider task was conducted right after Stage 2 ends (see Part 3 of the Experimental Instruction in Appendix D). In this task *all* subjects worked on sliders individually for 2 minutes with 20 tokens paid for each slider correctly positioned at 50. Since the task was incentivized using the piece-rate payment, the numbers of sliders completed in this additional round could be used in the empirical analysis as a proxy for subjects' capabilities in the slider task.

After the standalone piece-rate slider task, we elicited individual risk preferences using the gambling measure in Eckel and Grossman (2005). The feedback on the gambling outcome was provided immediately. Subjects were then asked to complete a questionnaire that included questions on their demographics, strategies used during the experiment, group attachment, and prior knowledge about the artwork used in the experiment. Experimental instructions are provided in Appendix D, and the post-experiment questionnaire in Appendix E.

The features of experimental sessions are summarized in Table 2.1. Overall, 12 independent computerized sessions were conducted at the University of Michigan School of Information Behavioral Laboratory in spring 2012, yielding a total of 144 subjects. In this lab the computer screens were set beneath the transparent glass on the desk surfaces. Subjects, mostly students from the University of Michigan, were recruited from an online subject pool for economic experiments. The experiment was programmed using zTree (Fischbacher, 2007). Each session lasted approximately 70 minutes. The exchange rate was 250 tokens for $1. Average earning was $17.31 including a $5 show-up fee.

2.3.4 Hypotheses

In the control treatment where the concerns of group identity are absent, the principal-agent problem can be solved using backward induction. In the equilibrium, the agent chooses to complete n^* sliders such that $c'(n^*) = \frac{5}{6}f$, yielding her equilibrium strategy as $n^* = l(f) \equiv c'^{-1}(\frac{5}{6}f)$. Given this equilibrium strategy of the agent, the principal offers f^* such that $(100 - f^*) \cdot l'(f^*) = l(f^*)$.

Our hypotheses regard how the principal's revenue-sharing offer f^* and the agent's effort n^* are related to the group identity induced in the experiment. Our general null hypothesis is that group identity does not affect the principal's and agent's choices, compared to in the control treatment. The alternative hypotheses are discussed and stated below.

One robust finding in the Social Identity Theory in psychology (reviewed in Brewer (1991)) is that individuals, when categorized into groups, show favoritism towards ingroup members and discriminate against outgroup members. Findings in an increasing number of economics studies also suggest that individuals tend to treat their ingroup members more favorably and less so towards the outgroup members (Goette et al., 2006; Bernhard et al., 2006; Chen and Li, 2009). These findings lead to Hypothesis 7.

Hypothesis 7 (Effect of Group Identity on the Principal's Offer)**.** *Compared to in the control treatment, a principal makes a higher offer to the ingroup agent, and a lower offer to the outgroup agent.*

Group identity may influence the agent's effort directly, or indirectly through the impact of monetary incentive. The economics literature of group identity has explored various mechanisms through which identity works on people's incentives. It would be interesting to derive hypothesis from each of these mechanisms and compare our results accordingly. In particular, we investigate four possible mechanisms for the effect group identity on agents' incentives: positive reciprocity, negative reciprocity, norm-based preference, and reference-based preference.

Positive reciprocity occurs when people are more forgiving towards ingroup members than towards output members. For example, Chen and Li (2009) find in a series of dictator games and response games that participants show more charity concerns and less envy concerns when they are matched with an ingroup member, as opposed to an outgroup member. In the contest of our hidden action game, if group identity works on agents' incentives through positive reciprocity, they will care less of a low offer from an ingroup principal and respond with higher effort in comparison to output pairings.

In contrast, negative reciprocity occurs when people punish more when they are not treated well by ingroup members, compared with the case with bad treatments from output matches. McLeish and Oxoby (2007) find that, in bargaining games with induced identity, responders engage in great punishment towards ingroup members than towards output members. In our context, this implies that agents with a low offer will feel more betrayed and thus respond with lower effort to an ingroup principal than to an output principal.

The third potential channel through which group identity may impact agents' incentives is the social norm. It assumes that players experience disutility caused by actions that deviate from their internalized social norm. Akerlof and Kranton (2005) propose a norm-based model of identity in the principal-agent relationship. In our context, the social norm theory implies that both principals and agents think offers should be higher within ingroup pairings than within outgroup pairings. As a result, agents will respond more passively to a low offer from an ingroup principal than to a low offer from an outgroup principal.

Finally, group identity could also affect an agent' behavior through her reference point of the outcome. According to Köszegi and Rabin (2006) who formalize a detailed model of reference-dependent preferences, a player has probabilistic beliefs about the choice sets she will face and the decision she will make for each choice set. In our context, being matched with an outgroup principal increases an agent's expectation of receiving a low offer and exerting low effort, and hence increases the loss she will feel if she chooses high effort. Driven by loss aversion, this feeling of loss will decrease the agent's incentive to choose high effort under low wages. In comparion, being matched with an ingroup principal increases the agent's expectation of receiving a high wage and working at high effort. Hence, choosing high effort does not feel like a loss. This means the agent has more tolerance of low wages and will be more likely to choose high effort when the wage is low. This effect is referred to as "attachment effect" in Köszegi and Rabin (2006).

Since we are interested in comparing our results with predictions from these different mechanisms, we set up two competing hypotheses in Hypotheses 8a and 8b on agent' responses to low offers across treatments. Furthermore, all the four mechanisms above provide the same implication on how an agent's effort is affected by her group identity as the principal's offer increases, which is formally summarized in Hypothesis 8c.

Hypothesis 8 (Effect of Group Identity on the Agent's Effort)**.**

a. *If group identity affects an agent's working incentive through its impact on her positive reciprocity behavior or her reference-based preference, she is more tolerant towards a low offer from an ingroup principal while less tolerant towards that from an outgroup principal, compared to in the control treatment.*

b. *If group identity affects an agent's working incentive through its impact on her negative reciprocity behavior or her norm-based preference, she is less tolerant towards a low offer from an ingroup principal while more tolerant towards that from an outgroup principal, compared to in the control treatment.*

c. *No matter through which of the four above mechanisms group identity impacts an agent's incentive, she responds more positively to an increase in the offer from an ingroup principal and less positively to that from an output principal, compared to in the control treatment.*

Last but not least, how group identity affects the agent's effort may be contingent on her perception on fairness since previous studies suggest that economic agents are not only motivated by incentives but also concerned about fairness (Akerlof and Yellen, 1990; Fehr and Schmidt, 1999). We state Hypothesis 9 with no predictions on the directions of the impact due to little theoretical or empirical evidence provided on this question in the principal-agent environment in the previous literature.

Hypothesis 9 (Effect of Group Identity on the Agent's Perception on Fairness)**.** *The impact of group identity on the agents effort depends on her perception on fairness of the offer.*

2.4 Empirical Analysis and Results

In this section we analyze how group identity influences the principal's offer and the agent's effort. Note in our data two individual principals – one in the ingroup treatment and the other in the outgroup treatment – made the maximum possible offer of 100 tokens to their paired agents, very likely due to confusion. Therefore, our analysis excludes the six observations of these two principals and the corresponding six observations of the agents whom they were paired with.[10]

Descriptive statistics are presented in Table 2.2. The p-values of two-sided comparisons across treatments are included in the lower panel. For the principal, we find that the percentage of entering the hidden-action game varies between 95.7% and 97.2%, with no statistically significant differences across treatments. This suggests

[10]Results are robust if we include these observations.

Table 2.2: Descriptive Statistics

	Principal			Agent
	(1)	(2)	(3)	(4)
Treatment	Enter (%)	Offer	S.D. of Offer	Number of Sliders
Control	97.2	38.8	15.5	16.7
Ingroup	95.7	42.9	9.4	17.9
Outgroup	97.1	34.8	12.7	19.0
P-values of 2-Sided Comparisons				
Control vs. Ingroup	0.614	0.065	0.000	0.246
Control vs. Outgroup	0.966	0.101	0.112	0.041
Ingroup vs. Outgroup	0.649	0.000	0.016	0.320

Notes:
Test of proportions is used for column (1), test of equality of standard deviations for column (3), and test of means for all other columns.

that group identity does not affect the principal's decision to initiate a partnership with the agent. Conditional on entering the game, the principal's average offer is 38.8, 42.9 and 34.8 out of 100 in the control, ingroup, and outgroup treatments, respectively. The average offer for ingroup is closest to 50-50 split, and greater than that in the control ($p = 0.065$) and outgroup ($p < 0.001$) treatments. Interestingly, the principal's offer varies within a significantly smaller range in the ingroup treatment than in the other two treatments, as indicated by the comparisons of standard deviations (control v. ingroup $p < 0.001$; ingroup v. outgroup $p = 0.016$).

The agent's effort is measured by the number of sliders completed (column (4) in Table 2.2). Recall that the die roll at the end of each round would yield $\frac{1}{6}$ probability of zero number of slides regardless of the agent's actual effort. The purpose of this design is to minimize the agent's potential guilty feeling if she wants to produce zero sliders in response to a low offer. Thus, when a principal gets nothing in that round it is unclear whether the agent or the nature is responsible. Our data show that some agents indeed take advantage of this die-roll design, and choose to complete zero number of sliders, mainly due to their dissatisfaction of the principals' offers. Ten percent of agents in the control treatment choose to supply zero sliders in response to an average offer of 15.3 tokens (out of maximum possible 100 tokens). In the ingroup and outgroup treatments, 6.1 percent and 1.5 percent of agents choose to do so in response to average offers of 18.8 and 20 tokens, respectively. In addition, the agent's average effort is significantly higher in the outgroup treatment than in the control treatment (19.0 v. 16.7 sliders, $p = 0.041$). There is no significant difference in the average effort between the ingroup and the control treatment, or between the ingroup and the outgroup treatment.[11] Since the agent's effort depends on the offer received, we defer detailed discussions on how monetary incentives and group identity influence effort to the regression analysis.

We next use regression analysis to investigate the determinants of the principal's offer and the agent's effort. Table 2.3 presents the results of Tobit regressions with the principal's offer as the dependent variable. Standard errors are clustered at the session level. The independent variables of column (1) include only group treatment variables with the control treatment in the omitted category. This allows us to analyze the group effects on the principal's offer, with the control as the benchmark. Column (2) further controls for the principal's risk preference, gender, and round effect. Marginal effects are reported.

We find that on average the principal makes a significantly more generous offer to an ingroup agent, relative to in the control treatment where group categorization is absent (4.014, $p = 0.046$ in column (1); 4.453, $p = 0.034$ in column (2)). This finding supports Hypothesis 7 for the comparison in the principal's offer between the ingroup and the control treatments. In contrast, the offer made to an outgroup agent is lower, but statistically insignificant, than in the control treatment (-3.949, $p = 0.140$ in column (1); -2.557, $p = 0.294$ in column (2)). It is also in line with Hypothesis 7

[11]Since the subjects are randomized across treatments, there is no reason to believe that subjects from different treatments differ significantly in their capability of sliding. We analyze the number of sliders completed in the standalone piece-rate slider task which can be used as a proxy for subjects' capability of sliding. Two-sided Kolmogorov-Smirnov tests show that all the pairwise comparisons in the distributions of sliders across treatments, conditional or unconditional on subjects' roles as the principal or agent, are statistically insignificant ($p > 0.4$ for all cases).

Table 2.3: Principal's Offer (Tobit)

	(1)	(2)
Ingroup	4.014**	4.453**
	(2.015)	(2.094)
Outgroup	-3.949	-2.557
	(2.678)	(2.436)
Risk Aversion		-0.970
		(1.002)
Female		-1.974
		(3.828)
Round		-0.142
		(0.736)
Observations	203	203
Pseudo R^2	0.01	0.01
F test (p-value)		
Ingroup = Outgroup	0.001	0.001

Notes:
1. The dependent variable is principal's offer (0∼100 tokens).
2. Standard errors in parentheses are clustered on the session level.
3. * $p < 10\%$; ** $p < 5\%$; *** $p < 1\%$.

qualitatively on the comparison between the outgroup and the control treatments. We also find that the offer made to an ingroup agent is significantly higher than to an outgroup agent ($p = 0.001$ in columns (1) and (2)). Therefore, the principal shows favoritism towards the ingroup agent by making a more generous offer to her. Column (2) also shows that the offer does not differ significantly across round. In addition, more risk averse or female principal makes lower offer, but these coefficients are not statistically significant. These findings are summarized in Result 7.

Result 7 (Principal's offer)**.** *The principal makes a more generous offer to the ingroup agent, relative to in the control and outgroup treatments. No statistically significant difference is found in the principal's offer between the outgroup and the control treatments.*

Result 7 on the principal's greater generosity in his offer to the ingroup agent echoes ingroup favoritism documented in the psychology and economics literature discussed in Sections 2.1 and 2.3. This greater degree of generosity may reflect greater preferences towards ingroup (i.e., an ingroup agent enters the principal's utility function with a higher weight), and the principal's belief that an ingroup agent has higher expectation concerning the principal's offer, relative to the case with outgroup or when group categorization is absent. Ockenfels and Werner (2014) design a dictator game lab experiment to show that ingroup favoritism is partly belief-based. They find substantially less ingroup favoritism when participants' beliefs are accounted for.

We next investigate the determinants of the agent's effort as measured by the number of sliders completed. Tobit analysis is presented in Table 2.4. In column (1), we focus on the main explanatory variables including the principal's offer, group effects (with the control treatment being in the omitted category), and their interaction terms. Column (2) further includes additional control variables, i.e., the agent's performance in the standalone piece-rate round, risk aversion, gender, and round effect. We control for the agent's performance in the standalone piece-rate round in order to capture unobservable characteristics of the agent, e.g., motivation to work or innate skill of sliding. Marginal effects are reported. The direct effects of the principal's offer on the agents effort in the ingroup or outgroup treatments are reported in the lower panel of Table 2.4.

Results in column (1) show that the agent's effort increases significantly with the principal's offer in the control ($\alpha_1 = 0.214$, $p < 0.001$) and the ingroup treatments ($\alpha_1 + \alpha_4 = 0.400$, $p < 0.001$). This suggests that on average one token of increase in the principal's offer leads to an increase in the agent's effort by 0.214 sliders in the control treatment and 0.400 sliders in the ingroup treatment. Therefore, the impact of the principals offer on the agent's effort is significantly more positive in the ingroup than in the control treatment ($\alpha_4 = 0.186$, $p = 0.003$). We also find that, compared to their counterparts in the control treatment, the agents from ingroup are significantly *less* tolerant towards a low offer by the principal ($\alpha_2 =$ -7.633, $p = 0.027$). In sharp

Table 2.4: Determinants of the Agent's Effort (Tobit)

	(1)	(2)
Offer (α_1)	0.214***	0.208***
	(0.045)	(0.039)
Ingroup (α_2)	-7.633**	-7.851***
	(3.443)	(2.358)
Outgroup (α_3)	7.992**	4.823*
	(3.802)	(2.756)
Ingroup $\times$ Offer (α_4)	0.186***	0.172***
	(0.063)	(0.041)
Outgroup $\times$ Offer (α_5)	-0.137	-0.071
	(0.094)	(0.053)
Sliders in standalone piece rate round (α_6)		0.652***
		(0.097)
Risk Aversion (α_7)		0.895**
		(0.353)
Female (α_8)		-1.415
		(0.944)
Round (α_9)		0.980***
		(0.369)
Observations	203	203
Pseudo R^2	0.04	0.09
Direct effects of offer on effort:		
Ingroup: ($\alpha_1 + \alpha_4$)	0.400***	0.380***
	(0.041)	(0.024)
Outgroup: ($\alpha_1 + \alpha_5$)	0.077	0.137***
	(0.081)	(0.032)

Notes:

1. The dependent variable is agent's effort, i.e., the number of sliders completed.
2. Standard errors in parentheses are clustered on the session level.
3. * $p < 10\%$; ** $p < 5\%$; *** $p < 1\%$.

contrast, the agent from outgroup are *more* tolerant towards a low offer ($\alpha_3 = 7.992$, $p = 0.036$). Although the impact of offer on outgroup agent's effort is positive, it is not significantly different from zero ($\alpha_1 + \alpha_5 = 0.077$, $p = 0.341$).

Results in column (2) are consistent with those in column (1) except that after controlling for additional covariates, the impact of the principal's offer on outgroup agent's effort becomes statistically significant ($\alpha_1 + \alpha_5 = 0.137$, $p < 0.001$). In addition, we find that the agent's effort is positively correlated with her performance in sliding in the standalone piece-rate round. Moreover, the more risk averse the agent is, the more effort she exerts. This higher effort associated with risk aversion may be explained as means of the agent to make up for possible loss of earnings in the case of a bad outcome in the die roll. We also find that the agent's effort exhibits no gender difference. However, it tends to increase over time by a small amount (0.980, $p = 0.008$), indicating improvement in sliding performance due to learning. These findings are summarized in Result 8.

Result 8 (Agent's effort)**.** *Relative to in the control treatment, the agent from ingroup is less tolerant towards a low offer while the agent from outgroup is more tolerant towards a low offer by the principal. In addition, the agent from ingroup exerts even greater effort in response to an increase in the offer, compared to in the control treatment.*

Result 8 suggests that group identity and monetary incentives may serve as complements with each other. This is shown by the significant and positive coefficient of the interaction term between ingroup and offer ($\alpha_4 = 0.186$ in column (1) and 0.172 in column (2), $p < 0.01$ for both, in Table 2.4). It indicates that an ingroup agent responds to an increase in monetary incentives more positively than in the control treatment. An ingroup agentfs response to incentive is also greater than does an outgroup agent ($\alpha_4 > \alpha_5$, $p < 0.01$). Ingroup identity alone, however, does not guarantee a higher effort than in the control treatment. On the contrary, group identity may lead to a *lower* effort, relative to the control treatment, if the monetary incentive offered by the principal is not appealing enough (α_2 = -7.633 in column (1) and -7.851 in column (2), $p < 0.05$, in Table 2.4). Comparing these findings with the two competing hypotheses , we find the observations in line with Hypothesis 8b where group identity impacts an agent' working incentive through the agent's concerns of negative reciprocity or norm-based preference. Besides, Result 8 also supports Hypothesis 8c for the effect of group identity on how agents respond to an increase in the offer.

The outgroup agent's greater tolerance level towards a less generous offer, reported in Result 8, is actually foreshadowed in Table 2.2. Recall in Table 2.2 the highest average effort of the agent is found in the *outgroup* treatment (19.0 sliders) despite the unappealing offer made by the principal, 34.8 tokens, the lowest among all the three treatments. This observation, *albeit* interesting, should not be interpreted as that an organization would always benefit from keeping their employees as 'outsiders'. Since in our experiment the outcome of the production solely depends on the *quantity* of the product, our result on the higher productivity in the outgroup treatment may not be generalizable to the production process in which both quantity and quality matters.

The analysis in Table 2.4 is based on an implicit assumption that the agent's perception on a fair offer does not play any role in her response to incentives. As a matter of fact, the agent may react differently to an offer that she considers as unfair, relative to the situation in which the offer is considered as fair. For example, Masella et al. (2014) find that an ingroup agent may find it unfair and unexpected to be controlled by an ingroup principal, and hence reacts negatively. In Table 2.5 we investigate whether and how the agent's perception of fairness of an offer may influence her effort in response to the actual offer she receives. We are also interested in how this influence manifests itself differently across treatments. In the post-experimental questionnaire, we solicit participants' perception of fairness of an offer.[12] Depending on whether the actual offer they receive equals or exceeds their perceived fair offer, we then regroup the observations of agents into two subsamples and repeat the analysis conducted in Table 2.4.

Columns (1) and (2) of Table 2.5 present the results for the subsample of agents who receive an actual offer equal to or higher than their perceived fair offers. Columns (3) and (4) focus on the cases in which the actual offer is lower than the perceived fair offer. Marginal effects are reported. We find that when the actual offer equals or exceeds the perceived fair amount, the impact of ingroup identity and its interaction with the principal's offer is similar to what we find in Table 2.4 (β_2 = -3.937, p = 0.047; β_4 = 0.100, p = 0.042, column (1)).[13] It again suggests that ingroup identity itself does not necessarily lead to agent's high effort, rather it works complementarily to monetary incentives.

Several new observations emerge when the actual offer is lower than the perceived fair offer (columns (3) and (4)). First, the agent's response to a low offer shows no significant difference in the ingroup treatment, relative to the control treatment (β_2, $p > 0.10$). Second, the ingroup agent's response to an increase in the offer does not significantly differ from the response of her counterpart in the control treatment (β_4, $p > 0.10$). These two observations are in sharp contrast to those in Table 2.4 with the pooled sample and in column (1) of Table 2.5 in which the principal's offer is perceived fair. They indicate that the complementary relationship between group identity and monetary incentives vanishes when the principal's offer is regarded as unfair by the agent.

[12]In the post-experimental survey we asked agents what they thought a fair offer should be (see the exact wording in Question 27 in Appendix E). We find that the agents' self-reported fair offers are not correlated with their effort and the offers that they actually receive. Due to some technical errors, we were unable to record this measure in one control session and one ingroup session. So the analysis in Table 2.5 includes 3 control sessions, 3 ingroup sessions and 4 outgroup sessions.

[13]The effects of these two variables are not statistically significant in column (2) when other covariates are added maybe because of the small number of observations.

Table 2.5: Perception on Fair Offer and the Agent's Effort (Tobit)

	Actual offer *(weakly) higher* than perceived fair offer		Actual offer *lower* than perceived fair offer	
	(1)	(2)	(3)	(4)
Offer (β_1)	-0.026	-0.017	0.605***	0.506***
	(0.028)	(0.023)	(0.045)	(0.068)
Ingroup (β_2)	-3.937**	0.841	-2.689	-4.173
	(1.982)	(4.960)	(4.445)	(4.917)
Outgroup (β_3)	7.617	6.327	19.020***	10.676***
	(7.304)	(4.681)	(3.024)	(3.307)
Ingroup × Offer (β_4)	0.100**	-0.010	-0.003	-0.013
	(0.049)	(0.106)	(0.120)	(0.127)
Outgroup × Offer (β_5)	-0.120	-0.117	-0.522***	-0.329***
	(0.166)	(0.115)	(0.101)	(0.079)
Sliders in standalone piece rate round (β_6)		0.564***		0.689***
		(0.102)		(0.106)
Risk Aversion (β_7)		0.135		1.527***
		(0.331)		(0.380)
Female (β_8)		0.493		-2.272*
		(0.907)		(1.170)
Round (β_9)		0.559		1.049*
		(0.428)		(0.572)
Observations	58	58	110	110
Pseudo R^2	0.020	0.112	0.056	0.111
Direct effects of offer on effort:				
Ingroup: ($\beta_1 + \beta_4$)	0.074*	-0.027	0.643***	0.518***
	(0.040)	(0.090)	(0.122)	(0.123)
Outgroup: ($\beta_1 + \beta_5$)	-0.145	-0.134	0.088	0.186***
	(0.163)	(0.113)	(0.097)	(0.030)

Notes:
1. The dependent variable is agent's effort, i.e., the number of sliders completed.
2. Standard errors in parentheses are clustered on the session level.
3. * $p < 10\%$; ** $p < 5\%$; *** $p < 1\%$.

Results in columns (3) and (4) of Table 2.5 also confirm an early finding in Table 2.4 that an outgroup agent is more tolerant towards a low offer than in the control treatment ($\beta_3 > 0$, $p < 0.01$). However, when the actual offer is considered unfair, an outgroup agent is significantly *less* responsive to an increase in the incentives, compared with her counterparts in the other two treatments ($\beta_5 < 0$, $\beta_5 < \beta_4$, $p < 0.01$ for both tests in columns (3) and (4)).[14] Overall, new findings in Table 2.5 can be summarized in Result 9 .

Result 9 (Agent's perception on fair offer)**.** *When the offer is considered unfair, the agent's response in effort is similar in the ingroup and the control treatments. In addition, an outgroup agent is more tolerant towards a low offer, but her response in effort to incentives is significantly weaker, relative to the other two treatments.*

Result 9 reveals an important insight that agents' fairness concerns regarding principals' offers play a crucial role in how agents respond to incentives. When the offers of incentives are perceived unfair, group identity loses its positive influence on efforts by ingroup agents, and outgroup agents stop responding to the increase in incentives. In fact, *all* the cases in which agents chose to supply zero sliders (even at a cost to themselves) in our study took place when the principals' offers were deemed unfair.

2.5 Conclusion

In contract theory, incentive schemes are designed to motivate agents to work in the best interest of principals. Despite the large volume of the theoretical and empirical research in this area, the potential role of group identity has been surprisingly understudied. In this study we consider the important roles that group identity

[14] The impact of other covariates in column (4) is similar to that in Table 2.4 except that the effort by a female agent is marginally lower than that by her male counterpart (-2.272, $p = 0.055$). This implies that female agents are (marginally) more likely to be discouraged by unfair offers.

may play in the principal-agent environment with hidden action. We induce group identity in the laboratory and investigate how group identity influences the interactions between the principal and agent in a real effort game with hidden action. Our results show that the principals exhibit favoritism towards ingroup agents by making them more generous offers. In addition, group identity has *differential* impact on agents' tolerance level towards low offers as well as their response to increase in incentives. While the group identity literature has discussed over a number of mechanisms through which group identity may affect people's incentives, our result reconciles two competing hypotheses derived from four potential mechanisms and shows that the impact is dependent on agents' perceptions on fairness of the offers

As the workforce becomes increasingly diverse, building a common group identity has often been used by organizations as a tool to motivate employees from diverse background to work together effectively. How does the group identity building practice facilitate or interfere with the important roles that monetary incentives play on the employees' work performance? Should the organizations take into account their employees' organizational identity when designing optimal compensation schemes? Our research serves as a useful step towards understanding these questions.

Our results shed important light on the interplay of monetary incentives and group identity in tackling the principal-agent problems. Our findings show that even with a revenue sharing payment scheme, group identity influences differently the incentives offered by the principals and the effort exerted by the agents. These findings substantiate the importance of incorporating social identity into the model of work incentives discussed in the seminal work by Akerlof and Kranton (2005). While *separate* strands of economics literature provide supportive evidence on the positive roles that incentives or group identity plays in improving employees' work performance, our results suggest that incentives and identity of being an ingroup member with the organization may serve as complements under a revenue sharing payment scheme.

This complementary relationship only takes place if the principals offer incentives that are considered as fair by the agents. As a result, it is insufficient for institutions and companies to simply build group identities using training programs, orientations or team-building tasks. In addition, they need to complement group identity with reasonable wages that meet the expectations of workers in order to unlock the motivating effect of identity on workers' incentives. The subtleties of these results call for more research on the optimal design of work incentives that encompasses monetary incentives and "motivational capital" (Akerlof and Kranton (2005, p.29).

Although we use a revenue sharing compensation scheme in this study, our framework could be extended in fruitful future research to investigate how group identity influences the variation in the compensation scheme used to motivate the employees, as discussed in Akerlof and Kranton (2005, p.15). Moreover, in most existing economic research social identity has been treated as exogenous to monetary incentives, and its impact on individual decision making has been studied in static settings. More research effort could be dedicated to investigations on the interdependence of identity (or broadly, social incentives) and monetary incentives in a dynamic environment.

APPENDICES

APPENDIX A

Proofs

(Contest)

A.1 Proof of Propositions 1 and 2

Stage 1: Proof of Proposition 1

We first simplify the equilibrium strategy in the first stage for a contestant with ability parameter c by plugging equation (1.2) into (1.1). This yields

$$\begin{aligned}\beta_1(c) &= \int_c^1 [\alpha V + (1-G(c))(1-2\alpha)V - c\int_c^1 (1-2\alpha)V\frac{1}{s}\,\mathrm{d}G(s)]\frac{1}{s}\,\mathrm{d}F(s)\\ &= V[\alpha + (1-G(c))(1-2\alpha) - c\int_c^1 (1-2\alpha)\frac{1}{s}\,\mathrm{d}G(s)]\int_c^1 \frac{1}{s}\,\mathrm{d}F(s))\\ &= V[\alpha + (1-2\alpha)I_1(c)]I_2(c)\end{aligned}$$

where

$$\begin{aligned}I_1(c) &= 1 - G(c) - c\int_c^1 \frac{1}{s}\,\mathrm{d}G(s)\\ I_2(c) &= \int_c^1 \frac{1}{s}\,\mathrm{d}F(s)\end{aligned}$$

By definition, the expected average output of stage 1 is characterized as

$$\begin{aligned} A_1(\alpha) &= \int_m^1 \beta_1(c)\,\mathrm{d}F(c) \\ &= \int_m^1 V[\alpha + (1-2\alpha)I_1(c)]I_2(c)\,\mathrm{d}F(c) \end{aligned}$$

with $\alpha \in [0, \frac{1}{2}]$ being the proportion of the entire prize awarded to each first-stage winner.

Note that in our model, the single-prize case corresponds to $\alpha = 0$ and the multiple-prize case corresponds to $\alpha > 0$. So it is sufficient to show that $A_1'(\alpha) > 0$. Taking derivative with respect to α yields

$$A_1'(\alpha) = \int_m^1 V[1 - 2I_1(c)]I_2(c)\,\mathrm{d}F(c) \tag{A.1}$$

In the right hand side of equation (A.1), $I_2(c)$ is positive for any $c \in [m, 1]$. Hence, to obtain the sign of $A_1'(\alpha)$, we investigate the sign of $1 - 2I_1(c)$ over the interval of $[m, 1]$. From the definition of $I_1(c)$,

$$\begin{aligned} I_1'(c) &= -G'(c) - \int_c^1 \frac{1}{s}\,\mathrm{d}G(s) + c\frac{1}{c}G'(c)) \\ &= -\int_c^1 \frac{1}{s}\,\mathrm{d}G(s) \\ &< 0 \end{aligned}$$

So we obtain for any $c \in (m, 1]$ that

$$\begin{aligned} I_1(c) &< I_1(m) \\ &= 1 - G(m) - m \int_m^1 \frac{1}{s} \, \mathrm{d}G(s) \\ &= 1 - mE_G \frac{1}{s} \\ &< 1 - m \\ &\leq \frac{1}{2} \end{aligned}$$

The second inequality follows by noting that $s \in [m, 1]$ implies $\frac{1}{s} \in [1, \frac{1}{m}]$ and hence $E_G \frac{1}{s} \in (1, \frac{1}{m})$. The last inequality follows from the assumption that $m \in [\frac{1}{2}, 1]$. Given this result, $1 - 2I_1(c) > 0$ over $[m, 1]$, inducing the whole integrand of equation (A.1) to be strictly positive. Therefore, $A_1(\alpha)$ is strictly increasing in α, which leads to a higher expected average output in the first stage under multiple prizes than under a single prize. This completes the proof of Proposition 1.

Stage 2: Proof of Proposition 2

Note that in the second stage, contestants' abilities are randomly drawn from the distribution of G. So the expected average output in the second stage is given by

$$\begin{aligned} A_2(\alpha) &= \int_m^1 \beta_2(c) \, \mathrm{d}G(c) \\ &= \int_m^1 (V(1 - 2\alpha) \int_c^1 \frac{1}{s} \, \mathrm{d}G(s)) \, \mathrm{d}G(c) \end{aligned}$$

The last equality follows by plugging in equation (1.2).

Note

$$A_2'(\alpha) = -\int_m^1 (2V \int_c^1 \frac{1}{s} \, \mathrm{d}G(s)) \, \mathrm{d}G(c) < 0$$

This indicates that $A_2(\alpha)$ is strictly decreasing in α over $[0, \frac{1}{2}]$, yielding a lower expected average output in the second stage under multiple prizes than under a single prize. This completes the proof of Proposition 2.

A.2 Proof of Proposition 3

We follow the techniques proposed in Zhang and Wang (2009) who show similar results in the canonical all-pay auction model, and apply them to the contest environment.

We prove by negation. Assume a symmetric separating equilibrium does exist in the first stage. In such an equilibrium, the output levels that contestants choose in the first stage fully reveal their ability parameters and therefore make the second stage competition a complete-information contest. To investigate the existence of such an equilibrium, we need to exam whether there is any contestant that has incentive to deviate from her equilibrium strategy in the first stage. Without loss of generality, let's name this person contestant A and denote her type as c_A. When choosing her strategy in stage 1, contestant A actually decides her pretended type $\hat{c}_A$, which may or may not equal her actual type c_A. Note that she only has incentive to pretend to be another type if it allows her to enter the second stage as a first-stage winner. Besides, all other contestants are assumed to follow their equilibrium strategies in stage 1 and thus reveal their true types.

We implement backward induction and start from stage 2. Assuming the revealed ability parameters for the two contestants in stage 2 as $\{c_i\}_{i=1}^2$, we can divide the relationship among them into two situations: (1) $c_1 < c_2$; (2) $c_1 = c_2$. However, since $\{c_i\}_{i=1}^2$ as types of second-stage contestants are continuously distributed, set $\{(c_1, c_2)|c_1 = c_2, c_1 \in [m, 1], c_2 \in [m, 1]\}$ has measure zero. So given that our objectives are in terms of expected values, we only need to consider situation (1) in the rest of the proof.

To obtain the strategies of the two contestants in the second stage of the contest, we need the equilibrium strategies in a single-stage contest with complete information as a stepping stone. Lemma 1 depicts this equilibrium as a contest version result of Hillman and Riley (1989) and Baye et al. (1996):

Lemma 1. *In a single-stage all-pay contest with complete information, two contestants compete for a single prize v. Each contestant i has ability parameter c_i and bears a cost of $c_i x$ if he produces output x in the contest. Assume their ability parameters satisfy $c_1 < c_2$. In the unique Nash equilibrium, contestant 1 randomizes on $[0, \frac{v}{c_2}]$ according to c.d.f. $H_1(x_1) = \frac{c_2}{v} x_1$. Contestant 2 randomizes on $(0, \frac{v}{c_2}]$ according to c.d.f. $H_2(x_2) = (1 - \frac{c_1}{c_2}) + \frac{c_1}{v} x_2$ and places an atom at zero.*

Proof. We start our proof with the expected utilities of two contestants when they produce x_1 and x_2, respectively. Assume the cumulative density functions of x_i is H_i. Then contestant 1's expected utility is $U_1(x_1) = H_2(x_1)v - c_1 x_1$ and contestant 2's expected utility is $U_2(x_2) = H_1(x_2)v - c_2 x_2$.

We first show that contestant 1 as the stronger player has nonnegative utility at equilibrium. Since contestant 2 can always produce zero and get nothing, if he enters the contest, he will choose x_2 such that $U_2(x_2) \geq 0$. This implies

$$\begin{aligned} v - c_2 x_2 &\geq U_2(x_2) \equiv H_1(x_2)v - c_2 x_2 \geq 0 \\ x_2 &\leq \frac{v}{c_2} \end{aligned}$$

Knowing this, contestant 1 can guarantee a strictly positive payoff by choosing $x_1 = \frac{v}{c_2} + \epsilon$. So it follows that $U_1(x_1) > 0$ at equilibrium.

The next step is to show that contestant 2 as the weaker player has zero utility at equilibrium. We skip this step as it is similar to the proof in Hillman and Riley (1989).

Given $U_2(x) = H_1(x)v - c_2 x = 0$, we obtain the distribution of contestant 1's equilibrium strategy as $H_1(x) = \frac{c_2}{v}x$. Plugging in $\bar{x}_1$ as the upper bound of $H_1(\cdot)$ yields:

$$\begin{aligned} H_1(\bar{x}_1) &= \frac{c_2}{v}\bar{x}_1 = 1 \\ \bar{x}_1 &= \frac{v}{c_2} \end{aligned}$$

Since contestants 1 and 2 randomize on the same interval, the upper bound for $H_2(\cdot)$ is $\bar{x}_2 = \bar{x}_1 = \frac{v}{c_2}$. Therefore,

$$\begin{aligned} U_1(x) &= H_2(x)v - c_1 x \\ &= U_1(\bar{x}_2) \\ &= H_2(\bar{x}_2)v - c_1\bar{x}_2 \\ &= v - c_1\frac{v}{c_2} \\ H_2(x) &= (1 - \frac{c_1}{c_2}) + \frac{c_1}{v}x \end{aligned}$$

□

Now we are fully equipped to investigate the second stage of a two-stage elimination contest. Let us name the other second-stage player as contestant B, and his type c_B. According to the analysis and assumptions above, the revealed ability parameters of the two contestants in the second stage are $(\hat{c}_A, c_B)$. Lemma 2 describes their strategies as well as A's payoff in the second stage

Lemma 2. *In the second stage of the contest as described above, contestant B chooses his strategy β_B^2 according to the following distribution:*

$$H_B(\beta_B^2) = \begin{cases} (1 - \frac{\hat{c}_A}{c_B}) + \frac{\hat{c}_A}{(1-2\alpha)V}\beta_B^2 & \text{if } \hat{c}_A < c_B; \\ \frac{\hat{c}_A}{(1-2\alpha)V}\beta_B^2 & \text{if } \hat{c}_A \geq c_B. \end{cases}$$

Given B 's strategy, contestant A chooses her strategy as:

$$\beta_A^2(\hat{c}_A, c_A, c_B) = \begin{cases} 0 & \textit{if } \hat{c}_A < c_A \textit{ and } \hat{c}_A < c_B; \\ 0 & \textit{if } \hat{c}_A < c_A \textit{ and } \hat{c}_A \geq c_B; \\ \frac{(1-2\alpha)V}{c_B} & \textit{if } \hat{c}_A \geq c_A \textit{ and } \hat{c}_A < c_B; \\ \frac{(1-2\alpha)V}{\hat{c}_A} & \textit{if } \hat{c}_A \geq c_A \textit{ and } \hat{c}_A \geq c_B. \end{cases}$$

Lastly, contestant A 's payoff in the second stage is:

$$\pi_A^2(\hat{c}_A, c_A, c_B) = \begin{cases} (1-\frac{\hat{c}_A}{c_B})(1-2\alpha)V & \textit{if } \hat{c}_A < c_A \textit{ and } \hat{c}_A < c_B; \\ 0 & \textit{if } \hat{c}_A < c_A \textit{ and } \hat{c}_A \geq c_B; \\ (1-\frac{c_A}{c_B})(1-2\alpha)V & \textit{if } \hat{c}_A \geq c_A \textit{ and } \hat{c}_A < c_B; \\ (1-\frac{c_A}{\hat{c}_A})(1-2\alpha)V & \textit{if } \hat{c}_A \geq c_A \textit{ and } \hat{c}_A \geq c_B. \end{cases}$$

Proof. In the second stage, contestant B infers from contestant A's output in the first stage that her type is $\hat{c}_A$ and optimizes as if he were in a complete-information all-pay contest. Then $H_B(\beta_B^2)$ follows from Lemma 1.

Given the optimal strategy by B , contestant A faces the following optimization problem:

Case 1. $\hat{c}_A < c_B$

In this case, contestant A's pretended type is stronger than her opponent in stage 2. Contestant B, while taking himself as the weaker player, chooses his optimal strategy according to $H_B(\beta_B^2) = (1-\frac{\hat{c}_A}{c_B}) + \frac{\hat{c}_A}{(1-2\alpha)V}\beta_B^2$ over $[0, \frac{(1-2\alpha)V}{c_B}]$. Knowing this, contestant A solves:

$$\max_{\beta_A^2} \pi_A^2 = H_B(\beta_A^2)(1-2\alpha)V - c_A\beta_A^2$$
$$= (1 - \frac{\hat{c}_A}{c_B})(1-2\alpha)V + (\hat{c}_A - c_A)\beta_A^2$$

If $\hat{c}_A < c_A$, contestant A pretends to be stronger in stage 1 than her actual type. Under this constraint, $\frac{\partial \pi_A^2}{\partial \beta_A^2} = \hat{c}_A - c_A < 0$. So it is optimal for contestant A to choose ${\beta_A^2}^* = \underline{\beta_A^2} = 0$ and earn an expected payoff of $(1 - \frac{\hat{c}_A}{c_B})(1-2\alpha)V$.

If $\hat{c}_A > c_A$, contestant A pretends to be weaker in stage 1 than her actual type. Then it is optimal for contestant A to choose ${\beta_A^2}^* = \overline{\beta_A^2} = \frac{(1-2\alpha)V}{c_B}$ and earn an expected payoff of $(1 - \frac{c_A}{c_B})(1-2\alpha)V$, since the first order condition is positive.

Case 2. $\hat{c}_A \geq c_B$

In this case, contestant A's pretended type is weaker than her opponent in stage 2. Contestant B, while taking himself as the stronger player, chooses his optimal strategy from $H_B(\beta_B^2) = \frac{\hat{c}_A}{(1-2\alpha)V}\beta_B^2$ over $[0, \frac{(1-2\alpha)V}{\hat{c}_A}]$. Knowing this, contestant A solves:

$$\max_{\beta_A^2} \pi_A^2 = H_B(\beta_A^2)(1-2\alpha)V - c_A\beta_A^2$$
$$= (\hat{c}_A - c_A)\beta_A^2$$

If $\hat{c}_A < c_A$, contestant A pretends to be stronger in stage 1. Then $\frac{\partial \pi_A^2}{\partial \beta_A^2} = \hat{c}_A - c_A < 0$, which implies that contestant A's optimal strategy is ${\beta_A^2}^* = \underline{\beta_A^2} = 0$ and get an expected payoff of 0.

If $\hat{c}_A > c_A$, contestant A pretends to be weaker in stage 1. Then the first order condition is positive, suggesting contestant A's optimal strategy to be ${\beta_A^2}^* = \overline{\beta_A^2} = \frac{(1-2\alpha)V}{\hat{c}_A}$ with an expected payoff of $(1 - \frac{c_A}{\hat{c}_A})(1-2\alpha)V$. □

So far, we have solved for A's optimal strategy in stage 2 while assuming that all other players follow their separating equilibrium strategies in stage 1. Now we go backward to study whether A has any incentive to deviate from her equilibrium strategy in stage 1. Our discussion falls in the following two cases:

Case 1. If contestant A pretends to be a stronger type in stage 1, *i.e.*, $\hat{c}_A < c_A$

Her expected payoff in stage 1 is

$$\begin{aligned}
\pi_A^1(\hat{c}_A, c_A) &= -\text{cost} + P(\text{entering stage 2 as group winner } |\hat{c}_A < c_A) \\
&\quad \cdot [\text{stage 1 prize} + E(\text{payoff of entering stage 2 } |\hat{c}_A < c_A)] \\
&= -c_A\beta(\hat{c}_A) + [1 - F(\hat{c}_A)] \cdot [\alpha V + E(\pi_A^2 | \hat{c}_A < c_A)] \\
&= -c_A\beta(\hat{c}_A) + [1 - F(\hat{c}_A)] \cdot [\alpha V + \int_{\hat{c}_A}^1 (1 - \frac{\hat{c}_A}{c_B})(1 - 2\alpha)V \, \mathrm{d}F_{C_B}(c_B)] \\
&= -c_A\beta(\hat{c}_A) + [1 - F(\hat{c}_A)] \cdot [\alpha V + \int_{\hat{c}_A}^1 (1 - \frac{\hat{c}_A}{c_B})(1 - 2\alpha)V \, \mathrm{d}G(c_B)] \\
&= -c_A\beta(\hat{c}_A) + [1 - F(\hat{c}_A)]\alpha V + [1 - F(\hat{c}_A)](1 - 2\alpha)V[1 - G(\hat{c}_A)] \\
&\quad - (1 - F(\hat{c}_A))\hat{c}_A(1 - 2\alpha)V \int_{\hat{c}_A}^1 \frac{1}{c_B} \, \mathrm{d}G(c_B)
\end{aligned}$$

The third equality follows by plugging the corresponding value of π_A^2 from Lemma 2. The fourth equality follows by noting that the ability parameter of B as a normal contestant in the second stage is distributed according to G.

To ensure the existence of a separating equilibrium in stage 1 and thus truth-telling for contestant A, we need

$$\frac{\partial \pi_A^1(\hat{c}_A, c_A)}{\partial \hat{c}_A} |_{\hat{c}_A = c_A} \quad \geq \quad 0$$

which implies

$$\begin{aligned}
c_A\beta'(c_A) \quad \leq \quad & -f(c_A) \cdot \{\alpha V + (1 - 2\alpha)V[1 - G(c_A)]\} \\
& + f(c_A)c_A(1 - 2\alpha)V \int_{c_A}^1 \frac{1}{c_B} \, \mathrm{d}G(c) \\
& - [1 - F(c_A)](1 - 2\alpha)V \int_{c_A}^1 \frac{1}{c_B} \, \mathrm{d}G(c_B) \qquad \text{(A.2)}
\end{aligned}$$

Case 2. If contestant A pretends to be a weaker type in stage 1, *i.e.*, $\hat{c}_A > c_A$

Her expected payoff in stage 1 is

$$\begin{aligned}
\pi_A^1(\hat{c}_A, c_A) &= -\text{cost} + P(\text{entering stage 2 as group winner } |\hat{c}_A > c_A) \\
&\quad \cdot [\text{stage 1 prize} + E(\text{payoff of entering stage 2 } |\hat{c}_A > c_A)] \\
&= -c_A\beta(\hat{c}_A) + [1 - F(\hat{c}_A)] \cdot [\alpha V + E(\pi_A^2|\hat{c}_A > c_A]) \\
&= -c_A\beta(\hat{c}_A) + [1 - F(\hat{c}_A)] \cdot [\alpha V + \int_{\hat{c}_A}^{1} (1 - \frac{c_A}{c_B})(1-2\alpha)V \, \mathrm{d}F_{C_B}(c_B) \\
&\quad + \int_{m}^{\hat{c}_A} (1 - \frac{c_A}{\hat{c}_A})(1-2\alpha)V \, \mathrm{d}F_{C_B}(c_B)] \\
&= -c_A\beta(\hat{c}_A) + [1 - F(\hat{c}_A)]\alpha V + [1 - F(\hat{c}_A)](1-2\alpha)V[1 - G(\hat{c}_A)] \\
&\quad - [1 - F(\hat{c}_A)](1-2\alpha)Vc_A \int_{\hat{c}_A}^{1} \frac{1}{c_B} \, \mathrm{d}G(c_B) \\
&\quad + [1 - F(\hat{c}_A)](1 - \frac{c_A}{\hat{c}_A})(1-2\alpha)VG(\hat{c}_A)
\end{aligned}$$

To ensure truth-telling in stage 1, we need

$$\frac{\partial \pi_A^1(\hat{c}_A, c_A)}{\partial \hat{c}_A} \Big|_{\hat{c}_A = c_A} \quad \leq \quad 0$$

which implies

$$\begin{aligned}
c_A\beta'(c_A) \quad &\geq \quad -f(c_A) \cdot \{\alpha V + (1-2\alpha)V[1 - G(c_A)]\} \\
&\quad + f(c_A)c_A(1-2\alpha)V \int_{c_A}^{1} \frac{1}{c_B} \, \mathrm{d}G(c_B) \\
&\quad - [1 - F(c_A)]\frac{1-2\alpha}{c_A} vG(c_A) \qquad \text{(A.3)}
\end{aligned}$$

Obviously, inequalities (A.2) and (A.3) can not hold simultaneously for all $c_A \in [m, 1]$. Accordingly, no symmetric and separating equilibrium strategies exist in the first stage that prevent contestants of all types from deviating, which contradicts with our assumption at the very beginning. This completes the proof of Proposition 3.

A.3 Proof of Proposition 4

In this subsection of the appendix, we show that a grand contest yields the highest expected average output when the contest achieves efficiency with the highest ability contestant being the winner. While this conclusion seems close to the statements depicted by Myerson (1981) in the optimal auction design problem, our purpose is to rigorously replicate the Myerson results in the contest environment.

To fit into the theoretical framework outlined in Section 1.3, we confine the class of grand contests we consider to those in which contestants' costs depends proportionally on their types and outputs. In other words, producing output x_i yields contestant i a cost of $c_i x_i$.

Let us parameterize this optimal contest design problem by introducing the basic definitions and assumptions. Suppose that a contest designer faces k contestants and has an indivisible prize of value v to award. Each contestant i is uniquely identified by her ability parameter c_i, which is private information to herself and distributed independently over $[m_i, n_i]$ from a distribution with commonly known density f_i. We denote by C the space of all possible realizations of $c = (c_1, c_2)$ and by f_C the joint density function. Then $C = [m_1, n_1] \times [m_2, n_2]$ and $f_C(c) = f_1(c_1) f_2(c_2)$. For consistency with notations in other parts of the appendix, we present here only the situation relevant to the competition within each group in our contest model. In particular, we assume that $k = 2$, $[m_i, n_i] = [m, 1]$ and $f_i = f$ for $i \in \{1, 2\}$. But all conclusions and proofs can be extended to the general case.

To characterize the problems faced by the contestants and the designer, we only need to consider the so-called direct revelation mechanisms.[1] A direct revelation mechanism means that contestants simultaneously report their types to the designer,

[1] The revelation principle indicates that "to any Bayesian Nash equilibrium of a game of incomplete information, there exists a payoff-equivalent revelation mechanism that has an equilibrium where the players truthfully report their types" (Gibbons, 1992). So considering only direct revelation mechanisms yields no loss of generality.

who then determines the corresponding outcome for each contestant. In our context, the designer chooses a pair of outcomes $(p(c), x(c))$ upon receiving a vector of announced ability parameters c. Vector $p(c) = (p_1(c), p_2(c))$ represents the winning probabilities for the two contestants, and vector $x(c) = (x_1(c), x_2(c))$ represents their outputs in the contest.

Facing a contest mechanism (p, x), a contestant with type c_i earns an expected payoff of

$$U_i(p, x, c_i) \quad = \quad \int_m^1 (vp_i(c) - c_i x_i(c)) f(c_j) \, \mathrm{d}c_j$$

Given our interest, we assume that the designer cares about the expected average output of the contestants. So his expected payoff is

$$U_0(p, x) \quad = \quad \frac{1}{2} \sum_{i=1}^{2} \int_C x_i(c) f_C(c) \, \mathrm{d}c$$

The designer maximizes $U_0(p, x)$ by choosing the optimal (p, x) subject to three constraints: (1) $p_1(c) + p_2(c) = 1$ and $p_i(c) \geq 0$, $\forall i \in \{1, 2\}$, $\forall c \in C$; (2) $U_i(p, x, c_i) \geq 0$, $\forall i \in \{1, 2\}$, $\forall c_i \in [m, 1]$; (3) $U_i(p, x, c_i) \geq \int_m^1 (vp_i(s_i, c_j) - s_i x_i(s_i, c_j)) f(c_j) \, \mathrm{d}c_j$, $\forall i \in \{1, 2\}$, $\forall c_i, s_i \in [m, 1]$. When (p, x) satisfies all these three constraints, we call it a *feasible* contest mechanism.

Next, we want to show that in an optimal contest mechanism (p, x), p must satisfy

$$p_i(c) = \begin{cases} 1 & \text{if } c_1 \neq c_2 \text{ and } c_i = \min(c_1, c_2); \\ 0 & \text{if } c_1 \neq c_2 \text{ and } c_i \neq \min(c_1, c_2); \\ \text{any value} \in [m, 1] & \text{if } c_1 = c_2. \end{cases} \tag{A.4}$$

To investigate the optimality in p, we introduce

$$Q_i(p, c_i) = \int_m^1 x_i(c) f(c_j)\, \mathrm{d}c_j$$

for each constant i with an arbitrary type c_i. To understand it, note that $c_i Q_i(p, c_i)$ is the expected cost for contestant i with ability parameterc_i. Given this additional concept, our next lemma provides the equivalent conditions for the feasibility of (p, x). The proof is omitted as it is similar to the proof for Lemma 2 in Myerson (1981).

Lemma 1. *A contest mechanism (p, x) is feasible if and only if the following conditions holds:*

(1) If $s_i \leq c_i$, then $Q_i(p, s_i) \geq Q_i(p, c_i)$, $\forall i \in \{1, 2\}$, $\forall s_i, c_i \in [m, 1]$;

(2) $U_i(p, x, c_i) = U_i(p, x, 1) + \int_{c_i}^1 Q_i(p, s_i)\, \mathrm{d}s_i$, $\forall i \in \{1, 2\}$, $\forall c_i \in [m, 1]$;

(3) $U_i(p, x, 1) \geq 0$, $\forall i \in \{1, 2\}$;

(4) $p_1(c) + p_2(c) = 1$ and $p_i(c) \geq 0$, $\forall i \in \{1, 2\}$, $\forall c \in C$.

Lemma 1 enables us to re-write the designer's expected payoff as

$$\begin{aligned} U_0(p, x) &= \frac{1}{2} \sum_{i=1}^{2} \int_C x_i(c) f_C(c)\, \mathrm{d}c \\ &= \frac{1}{2} \sum_{i=1}^{2} \int_C (\frac{v p_i(c)}{c_i} - \frac{v p_i(c) - c_i x_i(c)}{c_i}) f_C(c)\, \mathrm{d}c \\ &= \frac{1}{2} (\sum_{i=1}^{2} \int_C \frac{v p_i(c)}{c_i} f_C(c)\, \mathrm{d}c - \sum_{i=1}^{2} \int_C \frac{v p_i(c) - c_i x_i(c)}{c_i} f_C(c)\, \mathrm{d}c) \\ &= \frac{1}{2} (\sum_{i=1}^{2} \int_C \frac{v p_i(c)}{c_i} f_C(c)\, \mathrm{d}c - \sum_{i=1}^{2} \int_m^1 \frac{U_i(p, x, c_i)}{c_i} f(c_i)\, \mathrm{d}c_i) \qquad \text{(A.5)} \end{aligned}$$

The last equality is implied by the definition of $U_i(p, x, c_i)$.

Note that Lemma 1 implies

$$\begin{aligned}
\int_m^1 \frac{U_i(p,x,c_i)}{c_i} f(c_i)\,\mathrm{d}c_i &= U_i(p,x,1)\int_m^1 \frac{1}{c_i} f(c_i)\,\mathrm{d}c_i + \int_m^1 \frac{1}{c_i} f(c_i)\,\mathrm{d}c_i \int_{c_i}^1 Q_i(p,s_i)\,\mathrm{d}s_i \\
&= U_i(p,x,1)\int_m^1 \frac{1}{c_i} f(c_i)\,\mathrm{d}c_i + \int_m^1 Q_i(p,s_i)\,\mathrm{d}s_i \int_m^{s_i} \frac{1}{c_i} f(c_i)\,\mathrm{d}c_i \\
&= U_i(p,x,1)\int_m^1 \frac{1}{c_i} f(c_i)\,\mathrm{d}c_i + \int_C (\int_m^{c_i} \frac{f(s_i)}{s_i}\,\mathrm{d}s_i) x_i(c) f(c_j)\,\mathrm{d}c_j
\end{aligned}$$

Plugging this into equation (A.5) yields

$$\begin{aligned}
U_0(p,x) &= \frac{1}{2}(\int_C (\sum_{i=1}^2 p_i(c)\frac{v}{c_i}) f_C(c)\,\mathrm{d}c - \int_C (\sum_{i=1}^2 x_i(c) \int_m^{c_i} \frac{f(s_i)}{s_i}\,\mathrm{d}s_i) f(c_j)\,\mathrm{d}c_j \\
&\quad - \sum_{i=1}^2 U_i(p,x,1)\int_m^1 \frac{f(c_i)}{c_i}\,\mathrm{d}c_i)
\end{aligned}$$

In the right hand side of the equation above, p only appears in the first term while $\frac{v}{c_i}$ is strictly decreasing in c_i. Therefore, to maximize $U_0(p,x)$, it is obvious that $p_i(c)$ should equal 1 when c_i is the smaller among the two contestants and 0 otherwise. When $c_1 = c_2$, it does not matter which contestant wins. Therefore, the optimal p for the designer, as depicted in equation (A.4), is to select the contestant with the lower ability parameter as the winner. This completes the proof of Proposition 4.

APPENDIX B

Experimental Instructions

(Contest)

Experimental Instructions

(for Multiple-Prize-With-Information Treatment)

General Instruction

Welcome. This is an experiment in decision-making. During this experiment you will participate in a series of tasks. The amount of money you make will depend partly on your actions in these tasks and partly on other participants' actions. Please turn off mobile phones and any other electronic devices. They must remain turned off for the duration of this experiment.

This experiment consists of two parts. Your total earnings will be the sum of your earnings in each part plus a \$5 participation fee. Your earnings are given in tokens. At the end of the experiment, you will be paid in private and IN CASH based on the following exchange rate:

\$1 = 20 tokens

You must not communicate with each other. If you have questions please raise your hand and the experimenter will assist you.

Slider Task

This experiment consists of two parts. In both parts, you will undertake an identical task multiple times. You will be paid differently in the two parts.

The task lasts 120 seconds and consists of a screen with 48 sliders. As shown below, each slider is initially positioned at 0. The slider can be moved as far as 100. The number to its right shows its current position. You can use the mouse in any way you like to move the slider, and readjust the position of the slider as many times as you wish. You may now practice by moving the slider below.

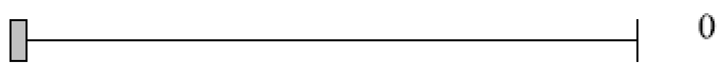


Figure B.1: Initial Position of Each Slider

To complete one piece of the slider task correctly, you will need to position the slider at exactly 50, as shown in the example below. Note the number to its right shows the correct position "50". Each time that you undertake the task, the "number of sliders completed" will be the number of sliders correctly positioned at exactly 50 at the end of the 120 seconds. Are there any questions?

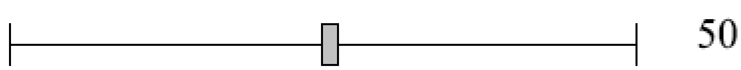


Figure B.2: Positioned at 50

Before we start part 1, please look at a sample screenshot below. The screen shot contains 48 sliders. The upper left corner shows it is the first of the two rounds in Part 1. The upper right corner shows the remaining time is 104 seconds. Three

Figure B.3: A Sample Screenshot of the Task Screen sliders are currently positioned at 50. So the box on the top of the screen shows that "currently, number of sliders complete is 3".

Part 1

Part 1 of the experiment is to help you get familiar with the slider task by letting you undertake the task for two rounds.

In each round, you will have 120 seconds to complete as many sliders as you can. You will be paid 1 token for each slider positioned at exactly 50. The task screen will show the remaining time and the number of sliders currently completed.

After each round, you will see a summary showing the number of sliders you completed, your payoff in that round, and your cumulative payoff in part 1.

When the two rounds are finished, you will have the chance to compare your performance to the performance of participants who previously undertook the same slider task. You will learn the distribution of the numbers of sliders completed by those participants, and you can compare your own performance against theirs to learn your relative skill in the slider task. Part 2 of the experiment will be explained after the completion of Part 1.

We will now start the first round.

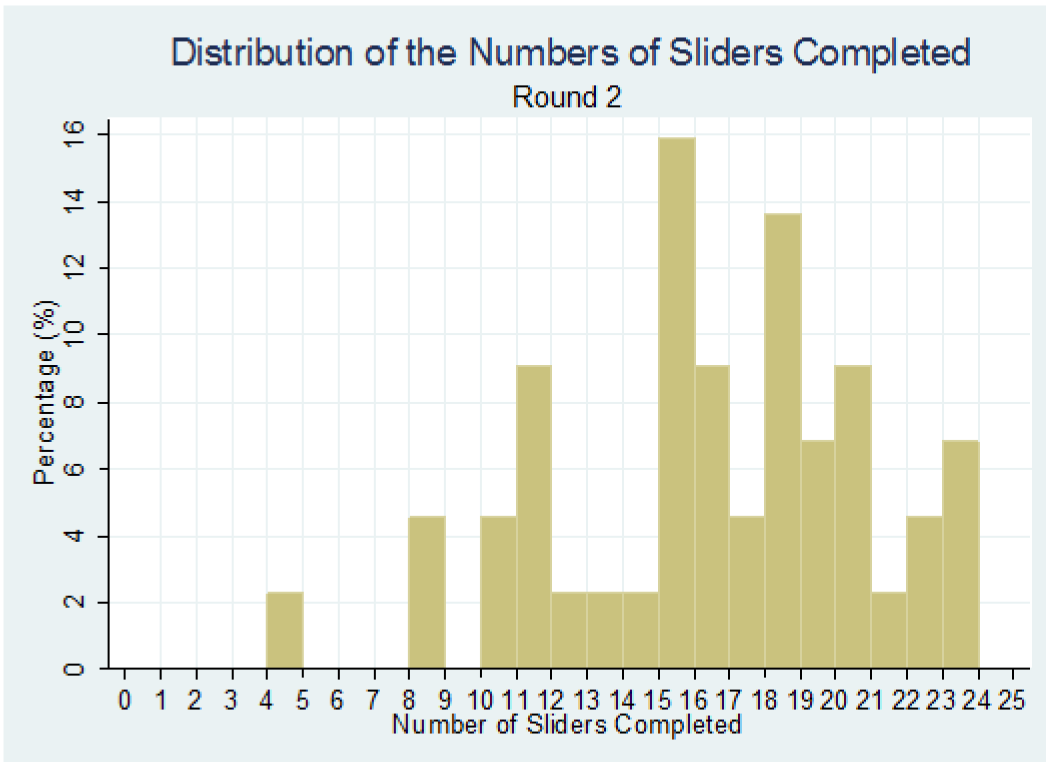
Evaluate Your Relative Skill in the Slider Task[1]

We previously ran sessions in which fifty participants undertook the same slider task for two rounds and earned 1 token for each completed slider the same as what you just did.

To help you evaluate your relative skill in the task compared with others, we provide you with the histogram on the left. It shows the distribution of the numbers

[1]This part is shown on the screen after the completion of the two rounds in part 1.

of sliders completed by those participants in round 2. The horizontal axis represents the number of sliders completed. The height of each column shows the percentage of participants who completed that particular number of sliders in round 2. The higher a column is, the more people who completed that number of sliders. For instance, 16% participants completed 15 sliders.



In addition, we provide you with the percentiles of their performance on the right to help you better understand the distribution:

Table B.1: Percentiles of the Number of Sliders by Previous Participants

	Number of Sliders Completed
Min.	4
25th Percentile	13.5
50th Percentile	16
75th Percentile	19
90th Percentile	22
Max.	24

Specifically, the minimum number of sliders completed is 4.

The 25th percentile is 13.5. This means a quarter of the participants completed less than 13 or 14 sliders.

The 50th percentile is 16. This means half of the participants completed less than 16 sliders. This is the median number of sliders completed.

The 75th percentile is 19. This means 75% of the participants completed less than 19 sliders.

The 90th percentile is 22. This means 90% of the participants completed less than 22 sliders.

The maximum number of sliders completed is 24.

Part 2

Rounds:

Part 2 of the experiment consists of 5 rounds within groups of four people.

Matching:

At the beginning of each round, you will be randomly matched with three other individuals to form a cohort of four people. You are equally likely to be matched with anyone in the room. Each cohort will be further divided into 2 groups, each with 2 people.

The composition of your cohort will change in each round. No one will ever learn with whom they were matched.

Stages and Rewards:

There are two stages in each round.

1. In stage 1, you and your match in the group undertake the slider task for 120 seconds. The one who completes more sliders receives a reward of 25 tokens and enters stage 2.

2. In stage 2, the two entering players – one from each group in stage 1 – undertake the slider task for 120 seconds again. The one who completes more sliders receives an additional reward of 50 tokens. People who do not enter stage 2 will wait until stage 2 players finish the task.

3. Tie-Breaking Rule: In any stage, if you and your match complete the same number of sliders, the computer will randomly choose one individual for the reward in that stage and for entry into the next stage (if it is stage 1). Each individual will have an equal chance of being chosen.

Slider Payoff:

In each round,

$$\text{Your slider payoff} = \text{slider payoff in stage 1} + \text{slider payoff in stage 2 (if entered)}$$

To be specific, in each round:

1. If you complete fewer sliders in stage 1 than your stage 1 match, you do not receive any reward and you do not enter stage 2:

$$\text{Your slider payoff} = 0$$

2. If you complete more sliders in stage 1 than your stage 1 match, you receive 25 tokens in stage 1 and enter stage 2. Then in stage 2, if you complete fewer sliders than your stage 2 match, you do not get any additional reward:

$$\text{Your slider payoff} = \text{Reward in stage 1} = 25$$

3. If you complete more sliders in stage 1 than your stage 1 match, you receive 25 tokens in stage 1 and enter stage 2. Then in stage 2, if you complete more sliders than your stage 2 match, you receive an additional reward of 50 tokens:

Your slider payoff = Reward in stage 1 + Reward in stage 2 = 25 + 50 = 75

Predicting the Outcome:

In both stage 1 and 2, before you undertake the slider task you will have a chance to earn additional tokens by predicting the likelihood that you will complete more sliders than your match.

To make a prediction, we will provide you with a form as follows:

Table B.2: Prediction Form in Stage 1 (&2)

Probability that **you** will complete **more** sliders in stage 1 (&2)	
Probability that your **stage 1 (&2) match** will complete **more** sliders	

For example, say you think there is a 90% chance that you will complete more sliders, and hence a 10% chance that your match will complete more sliders. This means that you believe that you are more likely than your match by a considerable margin to complete more sliders. If so, enter 90 in the upper space and 10 in the lower space. Note that the two numbers you enter should sum up to 100.

You are paid based on the accuracy of your prediction. If you believe that you will complete more sliders with 100% chance and you actually complete more sliders, you will get 10 tokens. On the other hand, if you believe that your match will complete more sliders with 100% chance and your match actually completes more sliders, you will also get 10 tokens.

Here is another example. Suppose you believe that you will complete more sliders with 90% chance and your match will complete more sliders with 10% chance.

1. If you actually complete more sliders, your prediction payoff is:

$$10 - 5 \cdot (1 - 0.\mathbf{90})^2 - 5 \cdot (0 - 0.\mathbf{10})^2 = 9.9$$

2. If your match actually complete more sliders, your prediction payoff is:

$$10 - 5 \cdot (0 - 0.\mathbf{90})^2 - 5 \cdot (1 - 0.\mathbf{10})^2 = 1.9$$

That is, the more accurate your belief is, the more you earn.

However, since your prediction is made before you know how many sliders you and your match actually complete, the best thing you can do to maximize the expected payoff of your prediction is to simply state your true beliefs about each outcome. Any other prediction will decrease your expected payoff.

Cumulative Payoff:

Your payoff in each round = your slider payoff + your prediction payoff

Your cumulative payoff in Part 2 = sum of your payoffs in the 5 rounds

Feedback:

1. Feedback after Stage 1

   At the end of stage 1, you will learn the numbers of sliders completed by you and your match in the group, as well as whether you enter stage 2 or not.

   You will also learn the numbers of sliders completed in stage 1 by the players from the other group within your cohort. In other words, everyone's performance in the first stage is revealed to all people in the same cohort before the second stage starts.

2. Feedback after Stage 2

   At the end of stage 2, if you entered stage 2, you will learn the numbers of sliders completed by you and your stage 2 match. If you did not enter stage 2, you will learn the numbers of sliders completed by the stage 2 players in your cohort.

   In addition, you will see a summary showing your payoffs in that round and your cumulative payoff in part 2.

APPENDIX C

Postexperimental Questionnaire (Contest)

Post-Experiment Questionnaire[1]

We are interested in whether there is a correlation between participants' decision behavior and some socio-psychological factors. The following information will be very helpful for our research. This information will be strictly confidential.

Questions about You in This Experiment:

1. What is your age? ________ (*Mean 22.60, Std Dev 6.02, Median 21, Min 18, Max 57*)

2. What is your gender?

   (a) Female (*52.08%*)

   (b) Male (*47.92%*)

3. Which of the following best describes your racial or ethnic background?

[1] Summary statistics in italics

(a) Asian (*28.47%*)

(b) Black (*9.03%*)

(c) Caucasian (*51.39%*)

(d) Hispanic (*2.08%*)

(e) Indian Subcontinent (*1.39%*)

(f) Middle Eastern (*2.08%*)

(g) Native American (*0.00%*)

(h) Multiracial (*3.47%*)

(i) Other (*2.08%*), please specify: __________

4. How many siblings do you have? (*Mean 1.63, Std Dev 1.92, Median 1, Min 0, Max 16*)

5. What is your grade?

   (a) Freshman (*1.39%*)

   (b) Sophomore (*9.03%*)

   (c) Junior (*31.25%*)

   (d) Senior (*29.86%*)

   (e) > 4 years of college (*7.64%*)

   (f) Graduate student (*20.83%*)

6. What is your academic program/major? __________

7. How would you describe the state of your own personal finances these days?

   (a) Excellent (*9.72%*)

   (b) Good (*46.53%*)

(c) Not so good (*34.03%*)

(d) Poor (*9.72%*)

8. Relative to other students on campus, would you say your FAMILY income is:

   (a) Much above average (*6.94%*)

   (b) Somewhat above average (*24.31%*)

   (c) About average (*34.03%*)

   (d) Somewhat below average (*24.31%*)

   (e) Much below average (*10.42%*)

9. From which countries did your family originate? __________

10. Would you describe yourself as: (Please choose one.)

    (a) Optimistic (*70.83%*)

    (b) Pessimistic (*11.81%*)

    (c) Neither (*17.36%*)

11. Which of the following emotions did you experience during the experiment? (Select all that apply.)

    (a) Anger (*18.06%*)

    (b) Anxiety (*60.42%*)

    (c) Confusion (*12.50%*)

    (d) Contentment (*34.03%*)

    (e) Fatigue (*42.36%*)

    (f) Happiness (*39.58%*)

    (g) Irritation (*41.67%*)

(h) Mood swings (*9.72%*)

(i) Withdrawal (*11.11%*)

12. In general, please rate on a scale from 1 to 7 how much you see yourself as someone who is willing, or even eager, to take risks? (*Mean 4.34, Std Dev 1.48, Median 4, Min 1, Max 7*)

13. Concerning just personal finance decisions, please rate on a scale from 1 to 7 how much you see yourself as someone who is willing, or even eager, to take risks? (*Mean 3.30, Std Dev 1.61, Median 3, Min 1, Max 7*)

14. In general, please rate on a scale from 1 to 7 how much you see yourself as someone who, when faced with an uncertain situation, worries a lot about possible losses? (*Mean 4.57, Std Dev 1.56, Median 5, Min 1, Max 7*)

15. Concerning just personal finance decisions, please rate on a scale from 1 to 7 how much you see yourself as someone who, when faced with an uncertain situation, worries a lot about possible losses? (*Mean 4.75, Std Dev 1.55, Median 5, Min 1, Max 7*)

16. In general, please rate on a scale from 1 to 7 how much you think you are competitive? (*Mean 5.30, Std Dev 1.53, Median 6, Min 1, Max 7*)

17. Concerning just sports and leisure activities, please rate on a scale from 1 to 7 how much you think you are competitive? (*Mean 4.97, Std Dev 1.84, Median 6, Min 1, Max 7*)

18. When you use computer in daily life, which of the statements is true?

    (a) I use mouse and touchpad equally often. (*11.81%*)

    (b) I use mouse more often than touch pad. (*22.22%*)

    (c) I use touchpad more often than mouse. (*65.97%*)

Questions about Your Experience in This Experiment:

19. After part 1, you learned your own performance and the performance of previous participants in the slider task. During part 2 of the experiment, how do you feel that information affected your PREDICTIONS on the chance that you would complete more sliders than your match? ______________

20. After part 1, you learned your own performance and the performance of previous participants in the slider task. During part 2 of the experiment, how do you feel that information affected your STRATEGIES in the slider tasks? ______________

21. Before you participated in stage 2, you were told how many sliders your stage 2 match just completed in the first stage. So did he/she know how many sliders you completed in stage 1. How do you feel that information affected your strategies in either stage? (If you did not get to enter stage 2, please take your best guess of the case if you had entered.)[2] ______________

22. If there was a round in which you completed MORE sliders in stage 2 than in stage 1, please explain why. (If you did not get to enter stage 2, please take your best guess of the case if you had entered.) ______________

23. If there was a round in which you completed FEWER sliders in stage 2 than in stage 1, please explain why. (If you did not get to enter stage 2, please take your best guess of the case if you had entered.) ______________

24. During part 2, how did you feel the outcomes in earlier rounds affected your strategies in later rounds? ______________

[2] Only for Treatments SP-WI and MP-WI.

APPENDIX D

Experimental Instructions

(Contract)

Experimental Instructions

This is the experimental instructions for the control treatment. Additional instructions for the ingroup treatment are inserted in the square brackets. Instructions for the outgroup treatment are similar to those in the ingroup treatment except that subjects are paired between groups.

Welcome. This is an experiment in decision-making. During this experiment you will participate in a series of tasks. The amount of money you make will depend partly on your actions in these tasks, partly on other participants' actions, and partly on chance. Please turn off mobile phones and any other electronic devices. They must remain turned off for the duration of this experiment.

This experiment consists of three parts, and will take about 1 hour. Your total earnings will be the sum of your earnings in each part plus a $5 participation fee. Your earnings are given in tokens. At the end of the experiment, you will be paid in private and IN CASH based on the following exchange rate: $1=250 tokens.

You must not communicate with each other. If you have questions please raise your hand.

There are 12 participants in this session. As you came in, you selected an index card [and a white envelope that contained a white card]. The number on the card is your ID number for this experiment. The ID number is used to insure the anonymity of your decisions. Note that the seating for each ID number has been randomized in this room.

[(For the group treatments only):

Now please open the white envelope. The envelope contains either a **Green** or a **Red** card. You have been assigned to the Green group if you received a Green card and to the Red group if you received a Red card. There are 12 participants in this session with 6 in each group. Your group assignment will remain the same throughout the experiment. If you drew a Green card, you will be in the Green group for the rest of the experiment. If you drew a Red card, you will be in the Red group for the rest of the experiment.]

Please now return your ID card to the envelope. Do not show it to others. Keep it safe as it will be required for payment at the end.

Part 1

In Part 1 everyone will receive a binder that contains 16 pictures. Among these 16 pictures, **14** pictures can be used to form a sequence while 2 other pictures are irrelevant. Your task is to find these 14 pictures and order them in their correct sequence within **10** minutes. When you have the correct sequence you will use the numbers on the pictures to write down their order at the bottom of this page.

[Meanwhile, **you can use a group chat program to get help from or offer help to other members in your own group. Messages will be shared *only***

among all the members from your own group. You will not be able to see the messages exchanged among the other group. People in the other group will not see the messages from your own group either.]

You are free to submit your answer when you're ready. A correct answer is worth 200 tokens. Raise your hand if you have any questions. You may remove the pictures from the binder. [Instructions for the group chat program are attached to the front of the binder you will receive.]

[If interested, other topics may be discussed using chat. There are, however, **two restrictions**:

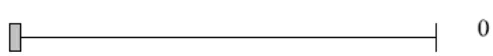
1. Please do not identify yourself or send any information that could be used to identify you (e.g. age, race, professional background, etc.).

2. Please refrain from using obscene or offensive language.]

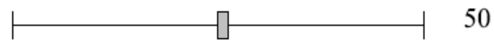
Part 2

Part 2 of the experiment consists of 2 practice rounds (for which you will not be paid) followed by 3 paying rounds. During the paying rounds, your earnings will depend on your actions, others' actions, and chance.

In each round you will have a chance to undertake an identical task that lasts 120 seconds. The task will consist of a screen with 48 sliders. As shown below, each slider is initially positioned at 0 The slider can be moved as far as 100. The number to its right shows its current position. You can use the mouse in any way you like to move the slider, and readjust the position of the slider as many times as you wish. You may now practice by moving the slider below.



To complete one piece of the slider task correctly, you will need to position the slider **at exactly** 50, as shown in the example below. Note the number to its right shows the correct position “50”. For each round, the “number of sliders completed” will be the number of sliders correctly positioned at exactly 50 at the end of the 120 seconds. Are there any questions?



Before we start the first practice round, please look at a sample screenshot below . The screen shot contains 48 sliders. Three sliders are currently positioned at 50. So the number of sliders completed is 3. [The information on your group and your pairing’s group as well as the] Remaining time is shown at the top of the screen.

We will now start the first practice round.

Paying Rounds: General Instructions

The practice rounds are finished. We will now move on to the 3 paying rounds. Your payoffs in each round will be included in your earnings for this experiment.

You will be randomly assigned to either role A or role B. There will be 6 role As and 6 role Bs. [Group treatments alternative: There will be 3 role As and 3 role Bs in each group.] Your role will be fixed throughout the 3 rounds. [In every round, each role A will be paired with a role B **from the same group**. That is, a role A from the **Green** group will be paired with a role B from the **Green** group; a role A from the **Red** group will be paired with a role B from the **Red** group.]

In each round, a role A will be paired with a role B. Role A will first choose whether to engage role B in a task. If role A chooses NOT to engage role B in the task, they will each receive 200 tokens and the round is over.

If role A chooses to engage role B, he/she will decide a **rate** (i.e., a certain number of tokens per slider) offered to role B to complete the slider task. Note each slider completed correctly by role B will bring 100 tokens to the pair of A and B, therefore the **rate** offered by A can be any integer from 0 to 100 tokens per slider. After seeing A's offer, B works on the sliders when A waits. Role A doesn't undertake the tasks. The task screen for role B will show the round number, [his/her group, his/her pairing's group,] the time remaining, the rate offered by his/her paired role A, and the number of sliders currently completed.

After B completes the tasks, a 6-sided die will be rolled **independently** for each pair by the computer to determine the payoffs. If the die comes up 1-5, role B's payoff is the **rate** times the number of sliders he/she completed, and A's payoff is (100 - **rate**) times the number of sliders completed by B. If the die comes up 6, both A and B receive 0 tokens. Roles A and B's payoffs are summarized below.

If A chooses NOT to engage B in the task,

A's payoff:	200 tokens
B's payoff:	200 tokens

If A chooses to engage B in the task,

A's payoff: $(100 - \textbf{rate}) \times$ number of sliders if die = 1, 2, 3, 4, or 5

0 tokens if die = 6

B's payoff: $\textbf{rate} \times$ number of sliders if die = 1, 2, 3, 4, or 5

0 tokens if die = 6

Note in the case of the zero roll, the number of sliders used to compute payoffs would be zero, instead of the actual number of sliders completed by B. In this case, role A would **not** be able to see the **actual** number of sliders completed by B.

At the end of each round, both A and B will see a summary showing whether A chooses to engage B in the task, each other's earnings in that round, as well as their own cumulative earnings. For the rounds in which A engages B in the task, both of them will also see the rate offered by A and the number of sliders for payoffs.

Are there any questions?

Is Everything Clear?

Please answer the following questions to test your understanding of the instructions. You will not be allowed to proceed until you have answered each question correctly. Please raise your hand if you need help.

(1) In each paying round, [a role A is paired with a role B from the same group and] role A first decides whether to engage role B in the slider task. If A chooses not to engage B, they will each receive 200 tokens for that round. (**True**/False)

(2) If A chooses to engage B in the task, she decides on a rate (from 0 to 100 tokens) offered to the paired B to complete the slider task. (**True**/False)

(3) After seeing A's offer, B will undertake the slider task while A does not undertake the task. (**True**/False)

(4) Suppose A engages B in the task and offers 40 tokens per slider. After seeing the offer, B completes 10 sliders. Please answer the following two questions.

a). If the 6-sided die comes up 1, A's payoff is 600 tokens and B's payoff is 400 tokens. (**True**/False)

b). If the die comes up 6, both A and B receive 0 tokens. (**True**/False)

(5) If role A chooses to engage B in the slider task, role A will always know the actual number of sliders completed by the paired role B. (True/**False**)

Who's Paired with Whom?

We will now go over the pairing rules.

For the 3 paying rounds, each participant will be paired with 3 **different** individuals of the opposite role, one in each round. [Group treatments alternative: For the 3 paying rounds, each participant will be paired with 3 **different** individuals from his/her group, one in each round.] **The pairings will be changed after each round, and no one will be paired with the same person twice.** [Note you will always be paired with someone from the same group. A **Green** role A will always be paired with a **Green** role B; a **Red** role A will always be paired with a **Red** role B.]

The pairings are done in such a way to guarantee the following:

1. who you will be paired with does **not** depend on your previous actions;
2. the actions you take in one round **cannot** affect, either directly or indirectly, the actions of the people you will be paired with in later rounds;
3. the actions of the person you are paired with in any given round cannot be affected by your actions in earlier rounds.

(If you are interested, this is because you will not be paired with a person who was paired with someone who had been paired with you, and you will not be paired with a person who was paired with someone who had been paired with someone who had been paired with you, and so on.)

Quiz: For the 3 paying rounds, everyone will be randomly paired with 3 different individuals [from one's own group]. No one's previous decisions will have any impact on whom he/she will be paired with in later rounds. (**True**/False)

Part 3

We will now start Part 3.

In this part, **everyone** will undertake the slider task independently for one paying round. You will have 120 seconds to complete as many sliders as you want. You will be paid **20 tokens** for each slider positioned at exactly 50. The task screen will show the remaining time and the number of sliders currently completed.

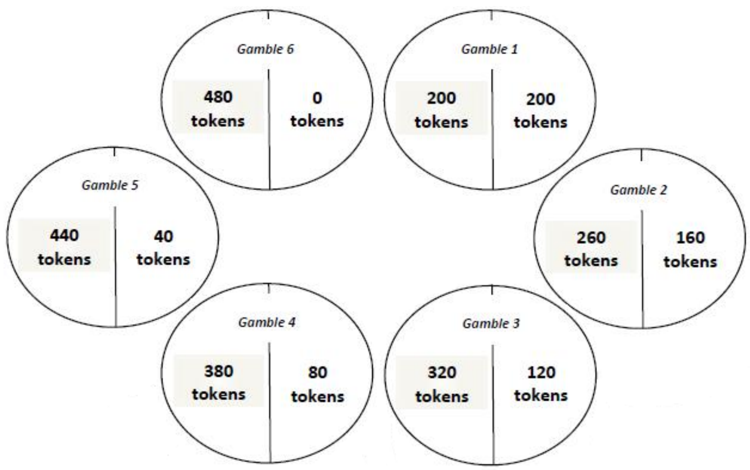
Press the button when you are ready to start.

Gamble Task

You may earn **additional** tokens in this task.

Each gamble (oval) has a 50-50 chance of a HIGH outcome and a LOW outcome. Choose a gamble to play, and the computer will draw a HIGH 'chip' or a LOW 'chip' (with 50-50 chance) **independently** for each participant. If HIGH comes up, you will get the HIGH amount on the **left** of the oval you choose. If LOW comes up you will get the LOW amount on the **right** of the oval you choose. Gamble 1 guarantees 200 tokens regardless.

Please choose the ONE gamble you prefer most.


Gamble 6
480 tokens
0 tokens
Gamble 1
200 tokens
200 tokens
Gamble 5
440 tokens
40 tokens
Gamble 2
260 tokens
160 tokens
Gamble 4
380 tokens
80 tokens
Gamble 3
320 tokens
120 tokens

APPENDIX E

Postexperimental Questionnaire (Contract)

Post-Experiment Questionnaire[1]

Please answer the following survey questions. Your answers will be used for this study only. Individual data will not be released.

Section 1: Questions about You in This Experiment

1. What is your age? __________ (*Mean 20.72, Std Dev 2.29, Median 21, Min 18, Max 33*)

2. What is your gender?

   (a) Female (*52.08%*)

   (b) Male (*47.92%*)

3. Which of the following best describes your racial or ethnic background?

   (a) Asian (*22.22%*)

[1] Summary statistics in italics

(b) Black (*4.17%*)
(c) Caucasian (*58.33%*)
(d) Hispanic (*2.78%*)
(e) Indian Subcontinent (*5.56%*)
(f) Middle Eastern (*1.39%*)
(g) Native American (*0.00%*)
(h) Multiracial (*5.56%*)
(i) Other (*0.00%*)

4. What is your education level?
   (a) Some college (*72.92%*)
   (b) College degree (*20.83%*)
   (c) Post graduate degree (*6.25%*)

5. What is your academic program/major? __________

6. What is your marital status?
   (a) Never married (*97.92%*)
   (b) Currently married (*2.08%*)
   (c) Previously married (*0.00%*)

7. How would you best describe your employment status?
   (a) Employed, full time (*5.56%*)
   (b) Employed, part time (*36.11%*)
   (c) Not employed (*58.33%*)

8. How many siblings do you have? _________ (*Mean 1.51, Std Dev 1.12, Median 1, Min 0, Max 7*)

9. Who in your household is primarily responsible for expenses and budget decisions? Please select all that apply.

   (a) Self (*26.39%*)

   (b) Spouse (*0.00%*)

   (c) Shared responsibility with spouse (*2.08%*)

   (d) Parent(s) (*69.44%*)

   (e) Other (*2.08%*)

10. How would you describe the state of your own personal finances these days?

    (a) Excellent (*9.72%*)

    (b) Good (*47.22%*)

    (c) Not so good (*38.89%*)

    (d) Poor (*4.17%*)

11. Relative to other students on campus, would you say your FAMILY income is:

    (a) Much above average (*4.86%*)

    (b) Somewhat above average (*40.28%*)

    (c) About average (*20.83%*)

    (d) Somewhat below average (*25.00%*)

    (e) Much below average (*9.03%*)

12. In politics today, do you consider yourself a Republican, a Democrat, an Independent, or something else?

(a) Democrat (*44.44%*)

(b) Republican (*21.53%*)

(c) Independent (*23.61%*)

(d) Other (*10.42%*), please specify: __________

13. Have you ever voted in a state or federal government election (in any country)?

(a) Yes (*52.78%*)

(b) No (*47.22%*)

14. What is your primary religious affiliation?

(a) Agnostic (*13.19%*)

(b) Atheist (*11.81%*)

(c) Hindu (*6.25%*)

(d) Jewish (*4.86%*)

(e) Muslim (*1.39%*)

(f) Protestant, denominational (*7.64%*)

(g) Protestant, non-denominational (*5.56%*)

(h) Roman Catholic (*19.44%*)

(i) I don't have one. (*20.83%*)

(j) I prefer not to answer. (*3.47%*)

(k) Other (*5.56%*), please specify: __________

15. How often do you attend religious services?

(a) At least once a month (*9.72%*)

(b) At least once a week (*11.81%*)

(c) Less than once a week (*32.64%*)

(d) Never (*45.83%*)

16. In the past twelve months, have you donated money to or done volunteer work for charities or other nonprofit organizations?

    (a) Yes (*81.25%*)

    (b) No (*18.75%*)

17. What is your residency status in the U.S.?

    (a) U.S. citizen (*90.28%*)

    (b) Permanent resident (U.S. green card holder) (*2.78%*)

    (c) Nonimmigrant alien (*6.94%*)

18. In what country or region were you primarily raised as a child?

    (a) Africa (*1.39%*)

    (b) Asia (*6.25%*)

    (c) Australia/New Zealand (*0.00%*)

    (d) Europe (*1.39%*)

    (e) Latin America (*0.00%*)

    (f) Middle East (*1.39%*)

    (g) US/Canada (*89.58%*)

19. What language do you speak at home?

    (a) English (*85.42%*)

    (b) Spanish (*0.69%*)

    (c) Other (*13.89%*), please specify: __________

People have different views about themselves and how they relate to the world. On a scale from 1 to 10, please rate to what extent you agree with each of the following statements about you see yourself? (1 means“not much at all)

20. I see myself as an American. (*Mean 8.28, Std Dev 2.47, Median 9.5, Min 1, Max 10*)

21. I see myself as a citizen of my birth country which is outside the U.S. (*Mean 2.64, Std Dev 3.16, Median 1, Min 1, Max 10*)

22. I see myself as a member of my ethnic group. (*Mean 6.41, Std Dev 2.84, Median 7, Min 1, Max 10*)

23. When you use computer in daily life, which of the statements is true?

    (a) I use mouse and touchpad equally often. (*12.50%*)

    (b) I use mouse more often than touch pad. (*18.06%*)

    (c) I use touchpad more often than mouse. (*69.44%*)

Section 2: Questions about Your Experience in This Experiment

24. On a scale from 1 to 10, please rate, prior to this study, how familiar you were with those artworks used in this experiment, with 1 meaning “not familiar at all and 10 “very familiar”). (*Mean 2.04, Std Dev 2.30, Median 1, Min 1, Max 10*)

25. On a scale from 1 to 10, please rate how much you think chat with your group members helped solve the painting puzzle (1 meaning “not helpful at all and 10 “very helpful). (*Mean 5.57, Std Dev 3.26, Median 6, Min 1, Max 10*)

26. On a scale from 1 to 10, please rate how closely attached you felt to your own group throughout the experiment (1 meaning “not at all and 10 “very closely attached). (*Mean 4.32, Std Dev 2.75, Median 3, Min 1, Max 10*)

Answer the next four questions if you were role A. Skip to Q31∼ Q33 if you were role B.

27. Please tell us how role Bs group membership affected your decision on whether to engage him/her in the slider task. If I had been matched with a role B from the other group [my own group],

    (a) I would be more likely to engage him/her in the slider task. (*4.17%*)

    (b) I would be less likely to engage him/her in the slider task. (*8.33%*)

    (c) I would be equally likely to engage him/her in the slider task. (*87.50%*)

28. In the slider tasks, without knowing the number of sliders completed by B, what did you think a fair offer should be? _________(out of 100 tokens) (*Mean 43.48, Std Dev 10.95, Median 50, Min 10, Max 70*)

29. In the slider tasks when you decided on how much to offer to role B, how would you describe the strategies you used most of the times? Select all that apply.

    (a) I made offers that gave equal payoffs to myself and role B in order to be fair. (*33.33%*)

    (b) I made offers that gave more payoffs to role B than to myself in order to make him/her work hard. (*15.28%*)

    (c) I made offers that gave less payoffs to role B than to myself since I expected B would work hard anyway to earn money for himself/herself. (*66.67%*)

    (d) Other (*5.56%*), please specify: _________

30. Please tell us how role Bs group membership affected your decisions on offers. If I had been matched with a role B from the other group [my own group],

    (a) I would have made higher offers. (*4.17%*)

(b) I would have made lower offers. (*20.83%*)

(c) I would not have changed the offers I made. (*75.00%*)

(d) Other (*0.00%*), please specify: _________

Answer the next three questions if you were role B.

31. In the slider tasks, what did you think a fair offer should be when role A didn't know how many sliders you may complete? _________(out of 100 tokens) (*Mean 47.97, Std Dev 8.79, Median 50, Min 20, Max 75*)

32. In the slider tasks when you decided on how many sliders to complete, how would you describe the strategies you used most of the times? Select all that apply.

    (a) I tried to complete as many sliders I could to earn as much money as possible for myself regardless of role As offer. (*76.39%*)

    (b) I tried to complete more sliders if As offer was high, and fewer sliders if As offer was low. (*20.83%*)

    (c) I tried to complete fewer sliders if As offer was high, and more sliders if As offer was low. (*0.00%*)

    (d) Other (*8.33%*) , please specify below: _________

33. Please tell us how your matched role As group membership affected your decisions on how many sliders to complete. If I had been matched with a role A from the other group [my own group],

    (a) I would have tried to complete more sliders. (*0.00%*)

    (b) I would have tried to complete fewer sliders. (*0.00%*)

    (c) I would not have changed my behavior. (*100.00%*)

    (d) Other (*0.00%*), please specify: _________

BIBLIOGRAPHY

BIBLIOGRAPHY

Abdelal, Rawi, Yoshiko M. Herrera, Alastair Iain Johnston, and Rose McDermott, *Measuring Identity: A Guide for Social Scientists*, Cambridge: Cambridge University Press, 2009.

Afridi, Farzana, Sherry Xin Li, and Yufei Ren, "Social Identity and Inequality: The Impact of China's *Hukou* System," *Journal of Public Economics*, 2015, *123.*

Akerlof, George A. and Janet L. Yellen, "The Fair Wage-Effort Hypothesis and Unemployment," *The Quarterly Journal of Economics*, 1990, *105* (2), 255–283.

_ **and Rachel E. Kranton**, "Economics and Identity," *The Quarterly Journal of Economics*, August 2000, *115* (3), 715–753.

_ **and** _ , "Identity and the Economics of Organizations," *Journal of Economic Perspectives*, Winter 2005, *19* (1), 9–32.

_ **and** _ , *Identity Economics: How Our Identities Shape Our Work, Wages, and Well-Being*, 1 ed., Princeton, NJ: Princeton University Press, 2010.

Altmann, Steffen, Armin Falk, and Matthias Wibral, "Promotions and Incentives: The Case of Multi-Stage Elimination Tournaments," *Journal of Labor Economics*, 2012, *30* (1), 149–174.

Amann, Erwin and Wolfgang Leininger, "Asymmetric All-Pay Auctions with Incomplete Information: The Two-Player Case," *Games and Economic Behavior*, 1996, *14* (1), 1–18.

Amegashie, J. Atsu, "Some Results on Rent-Seeking Contests with Shortlisting," *Public Choice*, 2000, *105* (3-4), 245–253.

Banyai, Istvan, *Re-Zoom*, New York, NY: Penguin Group (USA), 1998.

_ , *Zoom*, New York, NY: Penguin Group (USA), 1998.

Barut, Yasar and Dan Kovenock, "The Symmetric Multiple Prize All-Pay Auction with Complete Information," *European Journal of Political Economy*, 1998, *14* (4), 627–644.

Basu, Kaushik, "The Moral Basis of Prosperity and Oppression: Altruism, Other-Regarding Behaviour and Identity," *Economics and Philosophy*, 2010, *26* (2), 189–216.

Baye, Michael R., Dan Kovenock, and Casper G. de Vries, "The All-Pay Auction with Complete Information," *Economic Theory*, 1996, *8* (2), 291–305.

Bénabou, Roland and Jean Tirole, "Identity, Morals, and Taboos: Beliefs as Assets," *The Quarterly Journal of Economics*, 2011, *126* (2), 805–855.

Benjamin, Daniel J., James J. Choi, and A. Joshua Strickland, "Social Identity and Preferences," *American Economic Review*, 2010, *100* (4), 1913–1928.

_ , _ , and Geoffrey W. Fisher, "Religious Identity and Economic Behavior," 2013. NBER Working Paper 15925.

Berger, Jonah and Devin Pope, "Can Losing Lead to Winning?," *Management Science*, 2011, *57* (5), 817–827.

Bernhard, Helen, Ernst Fehr, and Urs Fischbacher, "Group Affiliation and Altruistic Norm Enforcement," *American Economic Review*, 2006, *96* (2), 217–221.

Besley, Timothy and Maitreesh Ghatak, "Competition and Incentives with Motivated Agents," *American Economic Review*, 2005, *95* (3), 616–636.

Bewley, Truman F., *Why Wages Don't Fall During a Recession*, Cambridge, MA: Harvard University Press, 1999.

Bolton, Patrick and Mathias Dewatripont, *Contract Theory*, Cambridge, MA: MIT Press, 2005.

Brewer, Marilynn B., "The Social Self: On Being the Same and Different at the Same Time," *Personality and Social Psychology Bulletin*, 1991, *17* (5), 475–482.

Brüggen, Alexander and Martin Strobel, "Real Effort versus Chosen Effort in Experiments," *Economics Letters*, 2007, *96* (2), 232–236.

Charness, Gary and Aldo Rustichini, "Gender Differences in Cooperation with Group Membership," *Games and Economic Behavior*, 2011, *72* (1), 77–85.

_ and Martin Dufwenberg, "Promises and Partnership," *Econometrica*, 2006, *74* (6), 1579–1601.

_ , Luca Rigotti, and Aldo Rustichini, "Individual Behavior and Group Membership," *American Economic Review*, September 2007, *97* (4), 1340–1352.

_ , Ramón Cobo-Reyes, and Natalia Jiménez, "Identities, Selection, and Contributions in a Public-Goods Game," *Games and Economic Behavior*, 2014, *87* (5), 322–338.

Chen, Hua, Sung H. Ham, and Noah Lim, "Designing Multiperson Tournaments with Asymmetric Contestants: An Experimental Study," *Management Science*, 2011, *57* (5), 864–883.

Chen, Roy and Sherry Xin Li, "Gender and Group Identity in Coordination Games," 2015. University of Texas at Dallas manuscript.

_ **and Yan Chen**, "The Potential of Social Identity for Equilibrium Selection," *American Economic Review*, October 2011, *101* (6), 2562–2589.

Chen, Yan and Sherry Xin Li, "Group Identity and Social Preferences," *American Economic Review*, March 2009, *99* (1), 431–457.

Clark, Derek J. and Christian Riis, "Competition over More than One Prize," *American Economic Review*, 1998, *88* (1), 276–289.

_ **and** _ , "Influence and the Discretionary Allocation of Several Prizes," *European Journal of Political Economy*, 1998, *14* (4), 605–625.

Cohen, Chen and Aner Sela, "Allocation of Prizes in Asymmetric All-Pay Auctions," *European Journal of Political Economy*, 2008, *24* (1), 123–132.

Cooper, Russell, Douglas V. DeJong, Robert Forsythe, and Thomas W. Ross, "Cooperation without Reputation: Experimental Evidence from Prisoner's Dilemma Games," *Games and Economic Behavior*, 1996, *12* (2), 187–218.

Croson, Rachel and Melanie Marks, "Groups Work for Women: Gender and Group Identity in the Provision of Public Goods," *The Negotiation Journal*, 2008, *24* (4), 411–427.

Dechenaux, Emmanuel, Dan Kovenock, and Roman M. Sheremeta, "A Survey of Experimental Research on Contests, All-Pay Auctions and Tournaments," *Experimental Economics*, 2015, *18* (4), 609–669.

Eckel, Catherine C. and Philip J. Grossman, "Managing Diversity by Creating Team Identity," *Journal of Economic Behavior & Organization*, November 2005, *58* (3), 371–392.

Eriksson, Tor, Anders Poulsen, and Marie Claire Villeval, "Feedback and Incentives: Experimental Evidence," *Labour Economics*, 2009, *16* (6), 679–688.

Fang, Hanming and Glenn C. Loury, ""Dysfunctional Identities" Can Be Rational," *American Economic Review*, 2005, *95* (2), 104–111.

Fehr, Ernst and Klaus M. Schmidt, "The Theory of Fairness, Competition, and Cooperation," *Quarterly Journal of Economics*, August 1999, *114* (3), 817–868.

Fershtman, Chaim and Uri Gneezy, "The Tradeoff between Performance and Quitting in High Power Tournaments," *Journal of the European Economic Association*, 2011, *9* (2), 318–336.

Fischbacher, Urs, "z-Tree: Zurich Toolbox for Ready-made Economic Experiments," *Experimental Economics*, June 2007, *10* (2), 171–178.

Freeman, Richard B. and Alexander M. Gelber, "Prize Structure and Information in Tournaments: Experimental Evidence," *American Economic Journal: Applied Economics*, 2010, *2* (1), 149–164.

Fu, Qiang and Jingfeng Lu, "The Optimal Multi-Stage Contest," *Economic Theory*, 2012, *51* (2), 351–382.

Gibbons, Robert, *Game Theory for Applied Economists*, Princeton University Press, 1992.

_ , "Incentives in Organizations," *Journal of Economic Perspectives*, 1998, *12* (4), 115–132.

Gill, David and Victoria Prowse, "A Structural Analysis of Disappointment Aversion in a Real Effort Competition," *American Economic Review*, 2012, *102* (1), 469–503.

Glazer, Amihai and Refael Hassin, "Optimal Contests," *Economic Inquiry*, 1988, *26* (1), 133–143.

Gneezy, Uri, Muriel Niederle, and Aldo Rustichini, "Performance in Competitive Environments: Gender Differences," *Quarterly Journal of Economics*, 2003, *118* (3), 1049–1074.

_ **, Stephan Meier, and Pedro Rey-Biel**, "When and Why Incentives (Don't) Work to Modify Behavior," *Journal of Economic Perspectives*, 2011, *25* (4), 191–209.

Goette, Lorenz, David Huffman, and Stephan Meier, "The Impact of Group Membership on Cooperation and Norm Enforcement: Evidence Using Random Assignment to Real Social Groups," *American Economic Review*, May 2006, *96* (2), 212–216.

Hillman, Arye L. and John G. Riley, "Politically Contestable Rents and Transfers," *Economics & Politics*, 1989, *1* (1), 17–39.

Hoff, Karla and Priyanka Pandey, "Discrimination, Social Identity and Durable Inequalities," *American Economic Review*, 2006, *96* (2), 206–211.

_ **and** _ , "Making up People – The Effect of Identity on Performance in a Modernizing Society," *Journal of Development Economics*, 2014, *106*, 118–131.

Kalra, Ajay and Mengze Shi, "Designing Optimal Sales Contests: A Theoretical Perspective," *Marketing Science*, 2001, *20* (2), 170–193.

Kandel, Eugene and Edward P. Lazear, "Peer Pressure and Partnerships," *Journal of Political Economy*, 1992, *100* (4), 801–817.

Köszegi, Botond and Matthew Rabin, "A Model of Reference-Dependent Preferences," *The Quarterly Journal of Economics*, November 2006, *121* (4), 1133–1165.

Kuhnen, Camelia M. and Agnieszka Tymula, "Feedback, Self-Esteem, and Performance in Organizations," *Management Science*, 2012, *58* (1), 94–113.

Lazear, Edward P. and Sherwin Rosen, "Rank-Order Tournaments as Optimum Labor Contracts," *Journal of Political Economy*, 1981, *89* (5), 841–864.

Lim, Noah, Michael J. Ahearne, and Sung H. Ham, "Designing Sales Contests: Does the Prize Structure Matter?," *Journal of Marketing Research*, 2009, *46* (3), 356–371.

Lizzeri, Alessandro and Nicola Persico, "Uniqueness and Existence of Equilibrium in Auctions with a Reserve Price," *Games and Economic Behavior*, 2000, *30* (1), 83–114.

Ludwig, Sandra and Gabriele K. Lünser, "Observing Your Competitor The Role of Effort Information in Two-Stage Tournaments," *Journal of Economic Psychology*, 2012, *33* (1), 166–182.

Masella, Paolo, Stephan Meier, and Philipp Zahn, "Incentives and Group Identity," *Games and Economic Behavior*, 2014, *86.*

McLeish, Kendra N. and Robert J. Oxoby, "Identity, Cooperation, and Punishment," 2007. Institute for the Study of Labor Discussion Paper 2572.

Moldovanu, Benny and Aner Sela, "The Optimal Allocation of Prizes in Contests," *American Economic Review*, 2001, *91* (3), 542–558.

_ and _ , "Contest Architecture," *Journal of Economic Theory*, 2006, *126* (1), 70–96.

_ , _ , and Xianwen Shi, "Contests for Status," *Journal of Political Economy*, 2007, *115* (2), 338–363.

Müller, Wieland and Andrew Schotter, "Workaholics and Dropouts in Organizations," *Journal of the European Economics Association*, 2010, *8* (4), 717–743.

Myerson, Roger B., "Optimal Auction Design," *Mathematics of Operations Research*, 1981, *6* (1), 58–73.

Niederle, Muriel and Lise Vesterlund, "Do Women Shy Away From Competition? Do Men Compete Too Much?," *Quarterly Journal of Economics*, 2007, *122* (3), 1067–1101.

Ockenfels, Axel and Peter Werner, "Beliefs and Ingroup Favoritism," *Journal of Economic Behavior & Organization*, 2014, *108.*

Pan, Xiaofei Sophia and Daniel Houser, "Cooperation During Cultural Group Formation Promotes Trust Towards Members of Out-groups," *Proceedings of the Royal Society of London B: Biological Sciences*, 2013, *280* (1762), 20130606.

Prendergast, Canice, "The Provision of Incentives in Firms," *Journal of Economic Literature*, 1999, *37* (1), 7–63.

— , "The Motivation and Bias of Bureaucrats," *American Economic Review*, 2007, *97* (1), 180–196.

Rosen, Sherwin, "Prizes and Incentives in Elimination Tournaments," *American Economic Review*, 1986, *76* (4), 701–715.

Sheremeta, Roman M., "Expenditures and Information Disclosure in Two-Stage Political Contests," *Journal of Conflict Resolution*, 2010, *54* (5), 771–798.

— , "Contest Design: An Experimental Investigation," *Economic Inquiry*, 2011, *49* (2), 573–590.

Siegel, Ron, "All-Pay Contests," *Econometrica*, 2009, *77* (1), 71–92.

Sisak, Dana, "Multiple-Prize Contests - The Optimal Allocation of Prizes," *Journal of Economic Surveys*, 2009, *23* (1), 82–114.

Solow, John L. and Nicole Kirkwood, "Group Identity and Gender in Public Goods Experiments," *Journal of Economic Behavior and Organization*, 2002, *48* (4), 403–412.

Stracke, Rudi, Wolfgang Höchtl, Rudolf Kerschbamer, and Uwe Sunde, "Optimal Prizes in Dynamic Elimination Contests: An Experimental Analysis," 2012.

Tajfel, Henri and John C. Turner, "An Integrative Theory of Intergroup Conflict," in Stephen Worchel and William G. Austin, eds., *The Social Psychology of Intergroup Relations*, Monterey, CA: Brooks/Cole, 1979.

Tullock, Gordonl, "Efficient Rent Seeking," in James M. Buchanan, Robert D. Tollison, and Gordon Tullock, eds., *Toward a Theory of the Rent-Seeking Society*, College Station, TX: Texas A&M University Press, 1980, pp. 97–112.

Zhang, Jun and Ruqu Wang, "The Role of Information Revelation in Elimination Contests," *The Economic Journal*, 2009, *119* (536), 613–641.