Winter 12-2019

Three essays on Incentive Mechanisms

Kuangli Xie
Southern Methodist University, kxie@smu.edu

THREE ESSAYS ON INCENTIVE MECHANISMS

Approved by:

Dr. Timothy Salmon
Professor

Dr. Bo Chen
Associate Professor

Dr. Saltuk Ozerturk
Associate Professor

Dr. Danila Serra
Associate Professor

THREE ESSAYS ON INCENTIVE MECHANISMS

A Dissertation Presented to the Graduate Faculty of the

Dedman College

Southern Methodist University

in

Partial Fulfillment of the Requirements

for the degree of

Doctor of Philosophy

with a

Major in Economics

by

Kuangli Xie

B.S., Finance, East China Normal University
M.S., Finance, Xiamen University

December 21, 2019

Acknowledgments

I would like to express the deepest appreciation to my committee chair Professor Tim Salmon, who has the attitude and the substance of a genius: he continually and convincingly conveyed a spirit of adventure in regard to research and scholarship. Without his guidance and persistent help this dissertation would not have been possible.

I would like to thank my committee members, Professor Danila Serra and Professor Saltuk Ozerturk and Professor Bo Chen, whose work demonstrated to me that concern for global affairs supported by an "engagement" in comparative literature and modern technology, should always transcend academia and provide a quest for our times.

In addition, a thank you to Professor Jason Shachat, who introduced me to Experimental Economics, and whose enthusiasm had lasting effect.

Last but not least, I would like to express my deepest gratitude to my family and friends. This dissertation would not have been possible without their warm love, continued patience, and endless support.

Xie, Kuangli

B.S., Finance, East China Normal University
M.S., Finance, Xiamen University

<u>Three essays on Incentive Mechanisms</u>

Advisor: Dr. Timothy Salmon

Doctor of Philosophy degree conferred December 21, 2019

Dissertation completed November 1, 2019

In this thesis, I use both theoretical and experimental methods to investigate incentive mechanisms of the sort commonly used by firms and governments. I have finished three chapters, two of which focus on how firms choose the appropriate incentive mechanisms to motivate their employees and the other one investigates how governments design mechanisms to improve social welfare. Chapter two provides a novel explanations why stock options are widely used in executive incentive contracts. Chapter three titled "Ask Your Workers to Report Frequently, But Not Too Often" studies how having employees reporting their progress to supervisors affects the employees' work effort. Chapter four entitled "Arbitrage Opportunities: Anatomy and Remediation" (jointly with Peter Bossaerts and Jason Shachat) introduces a experiment design to investigate how to eliminate persistent arbitrage opportunities in asset markets by regulating or deregulating market restrains.

TABLE OF CONTENTS

LIST OF FIGURES

LIST OF TABLES

Table Page

Chapter 1

Introduction

In this thesis, I use both theoretical and experimental methods to compare alternative incentive mechanisms. I usually start with theory that predicts people's responses to the change of mechanisms. I derive the theory from the game theory models that capture the strategic interactions between individuals, or experimental or empirical evidence shown in the literature. Even though the theory provide good benchmarks, people's behavior might deviate from the theoretical predictions. Therefore, I use experiments to test people's reactions to alternative mechanism. The experiment design sometimes involves creating new tasks people perform in the time allocation game or innovating new dividend path for the asset in the market game. For the analysis of experimental data, I use many statistical tools.

In chapter two: *"Can Firm Competition Explain the Use of Executive Stock Options?"*, I investigate a popular incentive mechanism, firms granting stock options to their CEOs. These executive stock options are considered to be a significant contributor to the bankruptcy of many firms in the 2007 Financial crisis, but the literature fails to provide a satisfactory explanation on why they are so popular. I found an important reason that justifies the extensive use of executive stock options. When firms are engaging in the competition in the product market, it is their CEOs are making the decisions on the behalf of their firms. A firm granting stock options to its CEO affects its own and rival CEOs in a way such that the firm can gain advantage in the competition.

A firm granting stock options would make its CEO more aggressive in the competition since the CEO can enjoy the financial gain if the firm wins the competition while does not bear the loss if the firm loses. There is a secondary effect on the rival firm which are not considered in previous studies. The rival CEO might be less aggressive in the competition if they know each other's compensation packages before their competitive decisions. My theoretical results suggest that the competing firms have little incentive to grant executive stock options as they have limited impact on the rival CEO. My experimental results, however, show that the behavior of the rival CEO is impacted much more than theoretically anticipated leading to the use of stock options to be more profitable. Of particular interest is that while individually each firm benefits from their use, it turns out that if both firms use them they are worse off than if neither did.

In chapter three: *"Ask Your Workers to Report Frequently, But Not Too Often"*, I investigate another popular mechanism in many firms. Firms have employees report to supervisors on the status of current projects or tasks at specified intervals. For example, firms, including Adobe Systems, Accenture PLC, Deloitte and General Electric, encourage their managers to check in with their employees every week or every other week. Even though these reports are popular, there are some controversial aspects of this practice which include how frequently to request reports and what form the reporting should take. Chapter three investigates these questions. Regardless of many other possible uses, these reports play an important role in monitoring employees to push them to work on appropriate firm tasks and put effort towards company goals.

The frequent reporting might improve employee effort by frequently reminding the employees the productivity, showing the productivity to their bosses or letting their bosses comment on the productivity. This is supported by the previous research which has shown that many individuals possess intrinsic motivation to help others and that many individuals place substantial concern on how others see their behavior. On the other hand, there are also indications in prior work that the frequent reporting might be counterproductive. These reports can be interpreted as distrust which can diminish employees' self-esteem and reduce their work effort. The frequent reporting can cause the pressure to produce positive results in relatively narrow time windows essentially de-incentivizing working on long run projects which take longer to mature.

In chapter three, I use experiments to examine how the reporting frequency and the reporting regime affect employees' effort decisions, including how much effort they spend on work, as well as how they allocate their effort across different tasks available in the firm. I find that increasing the frequency of reporting can increase worker effort on firm tasks but when set too high, the reporting frequency can have a less beneficial effect. If workers are asked to report too often, they shift to performing less lucrative tasks which have more near term payoffs but lower payoffs over all. While there is substantial literature suggesting observability and feedback affect individuals' behavior, I find limited impact of them on employees' effort decisions. I plan to do further research to determine the reason why observaibility and feedback may have a weaker effect here than in other settings.

Chapter four: *"Arbitrage Opportunities: Anatomy and Remediation"* with Peter Bossaerts and Jason Shachat examines how the regulatory mechanisms affect the arbitrage opportunities in the asset markets. Arbitrage is a financial transaction that nets a certain increase in cash holdings while not degrading the portfolio's value in any potential state of the world. When an arbitrage opportunity arises in a market setting, participants will presumably compete for its execution until the value is fully dissipated. The no arbitrage condition is one of the defining characteristics of a complete and competitive market as well as an assumption of many fundamental theories of finance. Despite compelling arguments for the contrary, arbitrage opportunities occur with surprising frequency in developed markets.

Chapter four introduces an experimental design where arbitrage opportunities emerge reliably and repeatedly, and then investigate how mitigate them by removing the commonly identified restrains. We find relaxing margin requirements, shortsale restrictions, or both have neither statistically nor economically significant effects. Increasing competition (more participants, each with small stakes) and more impactful stakes (unchanged number of participants but each with large exposures), generate large reductions in arbitrage opportunities. Hence, we advocate increased competition for small markets, and allowance for large stakes in large markets, rather than relaxation of rules on margin purchases or shortsales.

In summary, this thesis focuses on the incentive mechanisms in two major domains, the incentives the firms use to motivate or influence their employees and the regulatory mechanisms the government use to improve the social welfare.

Chapter 2

Can Firm Competition Explain the Use of Executive Stock Options?

2.1. Introduction

The financial crisis of 2007-2008[1] is considered to have been the worst financial crisis since the Great Depression of the 1930s [1]. There are a variety of causes for the original crisis. The one of interest here is that the US-government sponsored Financial Crisis Inquiry Commission (FCIC) found that stock options granted to top managers were a significant contributor to the bankruptcy of many firms because they can encourage excessive risk taking. Executive stock options promote such risk taking because they lead to executive compensation that is asymmetric in gains and losses. If the executives of a company engage in behavior that improves the stock price of the firm, then they can execute their options to buy stock in the company at the price when they received the option and enjoy the financial rewards of their efforts. If the executives make poor decisions that leads to a lowering of their stock price, then they do not exercise those stock options and their loss is capped at zero regardless of how far they drive down the price. At this point, one would therefore expect that firms would be discontinuing their use. In 2012, however, more than 90% of the CEOs in S&P 500 firms[2] had stock options in their pay package [2]. The median value of stock options granted to these CEOs was $2.82 million, accounting for approximately one-quarter of their total pay [2]. Moreover, the banking industry, a major contributor to the financial crisis, is still enthusiastic about granting executive stock options. A study

[1] The crisis was followed by the failure of key businesses, the unemployment of millions of workers, the foreclosure of four million homes, the loss of a trillion dollars in household wealth, and a downturn in economic activity leading to the Great Recession of 2008-2012.

[2] S&P 500 essentially includes the largest 500 US firms ranked by market value

conducted by Blanchard Consulting Group found 39% of approximately 200 publicly traded banks granted stock options to their top executives in 2013.

Given the obvious drawbacks to the use of executive stock options, it is a puzzle that they are still in use. There are several commonly proposed reasons such as stock options making managers more aggressive or having accounting and tax benefits. What is often overlooked in the literature is the effect of stock options in a competitive environment. Compensation contracts are important to consider in a competitive setting because the incentive contract a firm signs with a manager will affect not only the behavior of that manager but also of the managers in rival firms. In fact, when a firm chooses to compensate its manager using stock options to make its manager more aggressive, it may push managers of rival firms to be less aggressive. As a result, stock options may become more effective than we would expect had the competitive effect not been considered. On the other hand, it is possible that granting stock options makes the rival firms' managers more aggressive and therefore decreases the firm's profit. The goal for this study is to examine this issue both theoretically and empirically to determine if the effects of stock options under competition might help explain their continued use by firms.

Consider the following example. As of December 31, 2015, the CEO of Pfizer held 6,006,135 stock options and 1,261,099 shares of common stocks. Suppose Pfizer engages in a contest against its major competitor, Merck, to develop the most effective drug to cure a certain disease. In the competition, it is usually firms' CEOs that make the decisions. Here, the CEO of Pfizer has a strong incentive to invest heavily in R&D to win the competition, because he can earn $7.26 million for each dollar increase in their stock price if Pfizer wins the competition while he would lose $1.26 million for each dollar decrease in their stock price if Pfizer loses. His investment decision would be more aggressive than if he had only stocks in the pay package where a $1 loss would lead to losing $7.26 million. Therefore, the asymmetry in gains and losses induces greater risk-taking in firm competition.

There is a secondary effect of stock options on the rival firm, which has not been considered in previous studies. Executive stock options might pull back the investment of the rival manager. In this example, the CEO of Merck might decrease their R&D investment since he would expect the Pfizer's CEO would invest aggressively to win the competition. This, however, depends on what incentive packages Merck's CEO has. This secondary effect might make stock options more useful than if the firm competition is not taken into account. An important precondition to achieve this secondary effect is that managers of the competing firms need to know each other's compensation packages before their competitive decisions. The condition is usually satisfied. After executive incentive contracts are signed, the SEC[3] requires public firms to reveal these pay packages in their annual report. Even for private firms which have no obligation to reveal this information, they are likely to reveal it because they likely expect that granting stock options would affect the behavior of their rival managers in the way that improves their profit.

A related issue that emerges when competition is considered is that firm owners may employ executive stock options to overcome a competitive disadvantage. For instance, it is possible that Pfizer has a higher marginal cost of R&D than Merck, perhaps because Pfizer has less experienced scientists, or Merck has superior research facilities. Then, Pfizer's CEO is likely to invest less in developing the new drug because of its cost disadvantage. In this situation, Pfizer's owner can make their CEO invest more aggressively to counter the disadvantage. On the other hand, the owner of the advantaged firm, Merck in the example, can also grant stock options to secure her advantage. Understanding how these incentives balance out requires a full equilibrium analysis.

We provide a theoretical examination of a two-stage game where firm owners write compensation contracts with the managers in the first stage, and then in the second stage managers decide how much their firms will invest in a competition given these incentive

[3] U.S. Securities and Exchange Commission

packages of their own and their opponents. We model the firm competition as a lottery contest [3]. This setting is particularly useful to examine the impact of stock options, and it is representative of important firm competitions, such as contests for a patent, a license or an indivisible market. With regard to available executive compensation contracts that can be chosen by firm owners, we constrain them to be linear combinations of base salary, restricted stock and restricted stock options where restriction means stock and stock options are not tradable or exercisable until the end of the competition. These three elements usually take up more than 60% of the total value of the compensation [2]. The reason for the linear assumptions is that linear contracts are common in practice [4,5], and our goal is to examine how and why stock options are used in practice.

Theoretically, we find that whether firm owners should grant stock options depends on the risk preferences of managers. If we assume managers are risk-neutral, firm owners have little incentive to grant stock option. The reason is that owners granting options cause their rival managers to decrease the investment by a little amount or increase it. If managers are loss averse, they usually invest less in the competition than if they were not. Granting stock options can be a profitable strategy for firm owners since loss aversion enhances the impact of stock options on their own and opponent managers. The impact of risk aversion is ambiguous and depends on the nature of risk aversion. When managers have CRRA[4] utility function, they usually invest less than if they were risk-neutral. Granting stock options can improve owners' profit as it enhances the investment of their own managers significantly to overcome an inherent bias towards risk avoiding behavior managers possess.

We use laboratory experiments to examine how firm owners granting stock options would affect the investment behavior of their own and opponent managers. The data observed in our experiments provide an empirical justification for the extensive use of stock options. When the marginal cost of investment is symmetric between the two competing firms, owners have

[4]constant relative risk aversion

the incentive to deviate from stock to stock options if the opponent grants stock. Even when the opponent grants stock options, firm owners still have the incentive to deviate from stock to stock options, since by doing so they can lower the opponent's profit significantly at almost no cost. We found the equilibrium empirically occur is that both firm owners grant stock options, even though they are expected to earn significantly less than if they both use stock. Therefore, contrary to the theoretical prediction, empirical data show firm owners face a prisoner's dilemma situation in which both firms acting rationally leads to a socially suboptimal outcome. When the marginal cost of investment is asymmetric between the two competing firms, empirical data show that owners are actually playing a Hawk-Dove game, where their optimal choice depends on what their opponent is doing. If the opponent grants stock, they should grant options. If the opponent grants options, they should grant stock. In both the symmetric-cost and asymmetric-cost cases, the effectiveness of stock options that is not predicted by the theory is attributable to opposing managers overreacting and becoming less aggressive. The overreaction is also not consistent with the predictions generated based on the assumption that managers are loss averse or risk averse.

These results provide a possible justification for the continued use of stock options. While there seems to be little theoretical reason to use them even based on the competitive effects, we found that behaviorally people respond to these option contracts stronger than what theory predicts. In particular, we found that the managers of rival firms over-respond to the use of stock options by their competitor. This leads to the finding that each firm would most prefer to use stock options while their rivals do not. Sometimes, firms would use stock options even if their rivals use stock options in order to keep down the expected profit of their opponent firms. This explains why firms might be still extensively using stock options despite the fact that it is well known at this point that they lead to overly aggressive behavior that could lead a company to bankruptcy. If a company pulls only its own options back, it puts itself at a competitive disadvantage relative to its competitors, and no firm wants to be the first to do that.

While the effect of competition provides a justification for using options, it is important to note that many other explanations have also been put forward. These alternatives seem deficient in providing a clear justification for current use of stock options. The most fundamental reason is that in some cases, firms might want to encourage such risk-taking behavior in hope of overcoming an inherent bias towards risk avoiding behavior executives might possess. While this is certainly a possibility, the financial crisis suggests that firms went too far in correcting for risk avoiding behavior and again, one would expect that they would now be pulling back. Another set of widely discussed reasons is related to the tax benefits and accounting advantages of using stock options to compensate executives over alternative approaches. For example, before 2004, the value of the option was only required to be disclosed in a footnote to the financial statements allowing companies to essentially underreport executive compensation and make their potential profits look greater. In December 2004, Financial Accounting Standards Board (FASB) announced FAS123R, which required all U.S. firms to recognize an accounting expense when granting stock options, which removed the ability to use stock options for this purpose. These studies do not take into account how the use of stock options might affect how a firm's manager will make choices in a competitive environment, and they certainly do not examine how the compensation of a rival manager might impact the behavior of a firm's manager.

There are some previous studies which have explored the strategic impact of precommitment contracts, such as financial contracts or incentive contracts, on competition between firms. Some literature suggests that by choosing the capital structure prior to engaging in product market competition firms can change the intensity of competition [6–8]. Another body of literature justifies firm owners granting managers with incentives different from maximizing firm profit [9–13]. Most contributions to this second set investigate the strategic effects of managerial incentives in two-stage models, where owners simultaneously choose their managers' incentives schemes before a one-shot market interaction between manager-led firms. Some research [10] found firm owners can precommit to a more aggressive market behavior from their managers by choosing parameters of a managerial contract linear in profits and sales revenue. Some research [13] found the lack of relative performance-based incentives can soften the product market competition. However, the competitive impact of executive stock options is not fully explored by the literature.

2.2. Theoretical Model

We use a two-stage game to model stock options in executive incentive contracts affect managers' behavior in the competitive environment and how owners should design the incentive schemes to maximize firm profit. Firm owners write compensation contracts with the managers in the first stage, and then in the second stage managers decide how much their firms invest in a competition given the incentive packages. We focus on the case of two competing firms, firm i and j, but most of our results can be extended to cases with more firms. There is one owner and one manager for each firm, so we call the owner of firm i as owner i and the manager of firm i as manager i, and define owner j and manager j in the same manner.

The competition between firms is modeled as a lottery contest [3], since such a contest is representative of important firm competitions. Firms i and j compete for an indivisible prize valued at R by simultaneously choosing their effective investment, b_i and b_j. The effective investment is what compared against each other in the competition to determine the winning probability. Firm i's probability of winning is equal to the ratio of its own effective investment to the total effective investment of both firms, or $P_i = b_i/(b_i + b_j)$. Regardless of whether a firm wins the contest or not, it forgos its total investment cost. Firm i's total investment cost would equal the amount of effective investment times the unit cost of its effective investment, $b_i c_i$, as we assume the marginal cost of the effective investment is constant. Therefore, the expected profit from the competition for firm i, $E(\pi_i)$, can be expressed as Eq. 2.1.

$$E\left[\pi_i\right] = \frac{b_i}{b_i + b_j} R - b_i c_i \tag{2.1}$$

Let us look at the information structure of this game. We assume that the value of the prize, R, is common knowledge. The marginal investment cost of both firms is known by the managers but not the owners. For example, manager i can observe both c_i and c_j before the investment decision. On the other hand, owner i can observe neither c_i nor c_j. The owner cannot observe the effective investment chosen by her manager, b_i. Even though the owner can back out the total investment cost, $b_i c_i$, from the competition outcome, they have little information on the effectiveness of the spending which is captured by the marginal cost, c_i. For example, an owner can see that her manager has spent $1 million in R&D but not how that cost translates to effective research which is compared in the patent competition. Such unobservability is natural from the owners' perspective. It also gives managers a role in discerning the effectiveness of their own firms as well as that of their rival firms. The unobservability eliminates the feasibility of contracting on the effective investment. Owners can only contract on observable variables, such as firm value and firm profit. This unobservability enables competing owners to strategically manipulate their executive incentive contracts and therefore affect the competition outcome.

We also place some assumptions on the relationship between firm value and competition profit as well as the relationship between executive compensation and competition profit. In order to concentrate on the impact of competition, we assume that firm value will be changed only by the net profit from the competition. Furthermore, we assume that the compensation paid to the manager is negligible compared to competition profit, as the compensation usually is relatively small compared to firm profit. For example, in 2011 the median CEO compensation of S&P 500 companies was $9.6 million while the median earnings and median market value of these companies were $2.12 billion and $31.7 billion respectively (Murphy, 2012). Thus the firm's value at the end of this game is equal to its value before the competition plus profit from the competition. It can be rewritten as Eq. 2.2, where V_{i0} is firm value before the competition, π_i is profit from competition and V_{i1} is the value after the competition.

$$V_{i1} = V_{i0} + \pi_i \tag{2.2}$$

We constrain the available incentive contracts to be linear combinations of base salary, shares of stock and stock options as shown by Eq. 2.3. The reason for the linear assumption is that linear contracts are common in practice [4, 5]. In the first stage of the game, by writing these contracts, the owners make promises [5] on base salary, the quantity of stock, and the quantity of stock options, which are denoted by e_i, x_i and y_i respectively. In the second stage, managers make competitive decisions on the basis of these incentive packages. When the competition result is revealed, managers obtain the base salary, stock and stock options according to their incentive contracts.

$$W_{i1} = e_i + x_i P_{i1}(V_{i1}) + y_i max\{(P_{i1}(V_{i1}) - S_i), 0\} \tag{2.3}$$

The stock is defined as a share of the firm. Empirical studies [14, 15] have shown that the stock price, P_{it}, positively correlates with the firm value, V_{it}, and the information on firm value is reflected in the stock price very quickly. We assume that P_{it} is a non-decreasing linear function of V_{it}[6]. The stock option is defined as the right to purchase a share of the firm

[5] Some firms might grant their managers stocks or stock options at the very beginning, but these equity-related incentives usually have a vesting period where they are not tradable.

[6] Even though the relation between stock price and firm value might take more complex form, our comparative analysis can still provide useful insight for the situations where the linearity assumption does not hold. Furthermore, we let V_{it} be the current firm value without considering any expected profit in the future, so initial firm value, V_{i0} , does not reflect the expected earnings from the coming game. We concede that the expected profit could be accounted into initial firm value since optimal contracts and the resulting expected competition profit can be calculated in advance given certain belief on the behavior of managers and owners. However, even if the firm value is defined in this way, it still changes at the end of the game because the competition outcome is probabilistically determined. And, the dynamics of the value change will be similar

at the strike price, S_i. Holding stock options will generate positive payoff for the manager if the stock price goes above the strike price, and zero payoff otherwise. In practice, the strike price is usually set to be the stock price at the granted date (Murphy, 2012), so here we make the strike price equal to the stock price before the competition, $P_{i0}(V_{i0})$.

Next, we normalize the quantity of stock in the contract using the total stock shares of the firm, which is equal to the ratio of firm value to stock price, $\beta_i = x_i P_{i1}(V_{i1})/V_{i1}$. Similarly, we also normalize the quantity of stock options, $\gamma_i = y_i max\{(P_{i1}(V_{i1}) - S_i), 0\}/V_{i1}$. The value of the compensation at the end of the game can be written as Eq. 2.4.

$$W_{i1} = e_i + \beta_i V_{i1} + \gamma_i max\{(V_{i1} - V_{i0}), 0\} \tag{2.4}$$

Since neither the base salary, e_i, nor the initial value of stock, $\beta_i V_{i0}$, is affected by the managers' investment decisions or the competition outcome, we can replace the sum of them using $\alpha_i = d_i + \beta_i V_{i0}$. If we plug Eq. 2.2 into Eq. 2.4, we get Eq. 2.5.

$$W_{i1} = \alpha_i + \beta_i \pi_i + \gamma_i max\{\pi_i, 0\} \tag{2.5}$$

We can write down the manager i's optimization problem as shown in Eq. 2.6, where $\underline{U_i}$ refers to the manager's reservation utility. Manager i chooses the effective investment for her firm, b_i, to maximize her utility from the compensation.

$$\begin{gathered} max_{b_i} E[U(\alpha_i + \beta_i \pi_i + \gamma_i max\{\pi_i, 0\})] \\ \text{s.t.} E[U(\alpha_i + \beta_i \pi_i + \gamma_i max\{\pi_i, 0\})] \geqslant \underline{U_i} \end{gathered} \tag{2.6}$$

regardless of which way the firm value is defined. Since managers' incentives are based on these changes, our model implication can still be proper if expectation is considered. Similarly, we assume the stock price, P_{it}, is only affected by current firm value not future earnings.

By plugging Eq. 2.1 into Eq. 2.6, we can rewrite the optimization as Eq. 2.7. The managers' equilibrium actions (b_i^*, b_j^*) are functions of $(\alpha_i, \beta_i, \gamma_i)$ and $(\alpha_j, \beta_j, \gamma_j)$, which are determined by their owners' incentive choices, (d_i, f_i, g_i) and (d_j, f_j, g_j), respectively.

$$\begin{aligned} max_{b_i} \frac{b_i}{b_i + b_j} U(\alpha_i + (\beta_i + \gamma_i)(R - b_i c_i)) + \frac{b_j}{b_i + b_j} U(\alpha_i - \beta_i b_i c_i)) \\ \text{s.t.} \frac{b_i}{b_i + b_j} U(\alpha_i + (\beta_i + \gamma_i)(R - b_i c_i)) + \frac{b_j}{b_i + b_j} U(\alpha_i - \beta_i b_i c_i)) \geqslant \underline{U_i} \end{aligned} \tag{2.7}$$

Based on the managers' optimal strategies in the second stage, firm owners choose base salary, the amount of stock and the amount of stock options to maximize the firm's net profit from the competition. We have assumed that the executive compensation is relatively small compared to the firm's competition profit, so owners would conduct their optimization following the steps. First, the firm owner would derive a set of incentive schemes that induce her manager to choose the effective investment level maximizing the firm's competition profit. Within this set, the owner can then select a subset that satisfies her manager's participation constraint. Finally, from this subset, the owner finds out the incentive contract that minimizes the expected cost of the executive compensation. For example, if the optimal effective investment turns out to be high, then the owner would grant the manager more stock options relative to shares of stock. So, in order to take care of the participation constraint and compensation cost, the owner optimally choose the base salary and the total amount of shares of stock and stock options.

Notice that the equilibrium actions of both managers and owners depend on their risk preferences. We assume that firm owners are risk-neutral, and they treat gains and losses equally. However, managers might have different preferences. Managers' preferences affect how they react to executive stock options, therefore determining whether granting stock options is a profitable strategy for firm owners. In the following subsections, we identify owners' optimal contracts given different assumptions on managers' preferences, including risk neutrality, loss aversion and risk aversion.

2.2.1. Risk Neutral Managers

We start from the simplest case where managers are risk-neutral. Manager i's maximization problem can be written as Eq. 2.8.

$$\begin{aligned} &max_{b_i}\alpha_i+(\beta_i+\gamma_i)(\frac{b_i}{b_i+b_j}(R-b_ic_i)-\frac{\beta_i}{\beta_i+\gamma_i}\frac{b_j}{b_i+b_j}(b_ic_i)) \\ &\text{s.t. } \alpha_i+(\beta_i+\gamma_i)(\frac{b_i}{b_i+b_j}(R-b_ic_i)-\frac{\beta_i}{\beta_i+\gamma_i}\frac{b_j}{b_i+b_j}(b_ic_i))\geqslant \underline{U_i} \end{aligned} \tag{2.8}$$

In Eq. 2.8, α_i refers to the sum of base salary and initial value of stock in the contract. We refer to the sum of normalized amount of stock and normalized amount of stock options, $\beta_i+\gamma_i$, as normalized amount of equity. Since the values of those parameters are decided by the owners rather than the managers, we can simplify manager i's objective function as shown in Eq. 2.9, where θ_i is defined as the ratio of the normalized amount of stock options to the normalized amount of equity, $\theta_i=\frac{\gamma_i}{\beta_i+\gamma_i}$.

$$\begin{aligned} &max_{b_i}\frac{b_i}{b_i+b_j}(R-b_ic_i)-(1-\theta_i)\frac{b_j}{b_i+b_j}(b_ic_i) \\ &\text{s.t. } \alpha_i+(\beta_i+\gamma_i)(\frac{b_i}{b_i+b_j}(R-b_ic_i)-(1-\theta_i)\frac{b_j}{b_i+b_j}(b_ic_i))\geqslant \underline{U_i} \end{aligned} \tag{2.9}$$

The simplified objective function indicates that owner i affects both managers' investment decisions through the choice of θ_i. As we have assumed that the executive compensation is negligible compared to the competition profit, firm owner i would first choose the value of θ_i to induce managers' behaviors that maximize the competition profit of firm i. Given the optimal choice of θ_i, the owner then determines α_i and $\beta_i + \gamma_i$ that satisfy manager i's participation constraint and minimizes the cost of manager i's compensation. Thus we focus on the owners' choices of the ratios of normalized amount of stock options to the normalized amount of equity, (θ_i, θ_j), and their managers' reactions to these ratios.

When the marginal investment cost is symmetric between the two firms, $c_i = c_j = 1$, the analytical solution is derived through backward induction. The only symmetric subgame perfect Nash equilibrium is that both owners grant no stock option, that is, $(\theta_i^*, \theta_j^*) = (0, 0)$. In the equilibrium, the managers choose the investment level, $\frac{1}{4}R$, which is equal to the Nash equilibrium of the lottery contest with no delegation.

When the marginal investment cost is asymmetric, a closed form solution is not available but it can be computationally approximated. We constrain the available strategies for firm owners to be discrete. In particular, owner i and owner j choose θ_i and θ_j respectively among the values: $0, 0.01, 0.02, \cdots 0.99, 1.00$. We first solve the managers' equilibrium choices of b_i and b_j numerically given any possible combination of θ_i and θ_j, and then we can calculate owners' expected competition profit for each combination. Then, we find the owners' Nash equilibrium in the 101×101 normal form game. When doing the numerical calculation, we need to specify the values of the competition prize and the normalized amount of equity in managers' pay packages. We let $R = 200,000$, and $\beta_i + \gamma_i = \beta_j + \gamma_j = 0.1\%$. The values of these parameters are simply scaling issues, so our results hold if any other value is chosen.

In the asymmetric-cost case, we found in equilibrium the cost-advantaged owner grants stock options while the cost-disadvantaged owner does not. We refer to the owners of high-cost firm and low-cost firm as cost-advantaged owner and cost-disadvantaged owner respectively, and we refer to managers of high-cost firm and low-cost firm as cost-advantaged manager and cost-disadvantaged manager respectively. When high cost is 20% above the low cost, the cost-advantaged firm owner grants only stock options as the performance pay.

But we found that the incentive for the cost-advantaged owner to grant stock options rather than stock is relatively small, less than 1% of the expected competition profit.

As shown above, when competing managers are risk-neutral, there is little incentive for owners to grant executive stock options in both symmetric-cost and asymmetric-cost cases. These results can be understood by looking at the reactions of managers. When firm owners grant stock options, they increase the aggressiveness of their own managers, because losing the competition would not drive down the value of their managers' incentive packages. The increase in aggressiveness of their own managers does not improve the owners' profits unless the rival manager decreases the investment significantly. In the equilibrium, however, the rival manager either decreases her investment a little or increases it.

2.2.2. Loss Aversion

We also look at the case where managers are loss-averse. To explore the effect of loss aversion, we adopt the utility function found in previous papers [16] as shown in Eq. 2.10, where y represents the income relative to the reference point. In our model, we set managers' reference points as the compensation they can obtain if their firms earns zero profit from the competition. So, manager i's reference compensation is equal to α_i, the sum of base salary and initial value of stock in her contract. We found that loss averse managers invest less in the competition compared to risk neutral managers.

$$U(x) = \begin{cases} y^{\delta} & \text{if } y \geqslant 0 \\ -\lambda(-y)^{\delta} & \text{if } y < 0 \end{cases} \tag{2.10}$$

We compute the Nash equilibria of the two-stage game for different levels of loss aversion ($\lambda = 1.20, 1.50, 2.00, 2.25, 2.50, 3.00$) and different levels of sensitivity ($\delta = 0.60, 0.70, 0.88, 1.00$

)[7] by employing the same computation method used in the risk-neutral case. When the marginal investment cost is symmetric, we found that there are usually two pure strategy Nash equilibria for each combination of loss aversion level and sensitivity level. In each of these equilibria one firm owner chooses only stock options for the performance pay while the other owner chooses the performance pay consisting mostly of stock. When the marginal investment cost is asymmetric, there are usually two equilibria like those in the symmetric-cost case[8]. Either the cost-advantaged owner or the cost-disadvantaged owner can be the one that grant stock options. Granting stock options can be a profitable strategy for firm owners since loss aversion enhances not only the encouragement effect of stock options on their own managers but also the discouragement effect on the opponent managers.

2.2.3. Risk Averse Managers

Risk aversion also serves as a potential reason for granting stock options, since there exists uncertainty in the competition outcomes. The issue of whether risk aversion can justify the use of stock options is not trivial. When mangers become more risk averse, no general conclusions on their equilibrium investment level can be made [19–22]. Higher level of risk aversion does not necessarily reduce or raise the investment, because increasing investment not only lowers wealth in all states of nature but also makes the better state more likely [21]. But if we use some specific forms of utility function, managers' equilibrium investment behavior can be predicted. [23] noted that if the third derivative of the utility function is positive, an increase in risk aversion will reduce equilibrium investment. They observed such reduction in their experimental results. Thus we use CRRA (constant relative risk aversion) utility function, $u(x) = x^{1-\gamma}/(1-\gamma)$, to model managers' risk preference, since its third derivative is positive.

[7] Experimental literature demonstrates that the estimate of δ is above 0.6 [16–18]

[8] When the level of cost asymmetry is high and the level of loss aversion is not high enough, there will be only one equilibrium where the cost-advantaged owner grants stock options while the cost-disadvantaged owner does not.

We found that managers invest less as they become more risk averse. By granting stock options, firm owners can enhance the investment of their own managers significantly to overcome an inherent bias towards risk avoiding behavior that managers possess. On the other hand, granting stock options decreases the opponent managers' investment by a small amount. We compute the Nash equilibria of the two-stage game given different levels of risk aversion ($\gamma = 0, 0.1, 0.2, ..., 0.7$) [9] by employing the same computation method used in the risk-neutral cases. We found there are usually two pure strategy Nash equilibria for each level of risk aversion.[10] This is true for both the symmetric-cost case and the asymmetric-cost case. In each of these equilibria one firm owner chooses only stock options for performance pay while the other owner chooses only stock for performance pay. For the asymmetric-cost case, either the cost-advantaged owner or the cost-disadvantaged owner can be the one granting stock options.

2.3. Experiment Design

Our theoretical investigation suggests that when both managers are following the risk-neutral equilibrium strategies, owners have little incentive to grant stock options. However, if managers' behavior deviates from the risk-neutral equilibrium, the use of stock options by firm owners may be justified. We use lab experiments to investigate managers' investment behavior when they are facing different combinations of their own and opponent incentive contracts. We only have the role of managers played in the experiment, and their contracts are assigned by the computer. The experiment is to determine whether managers' behavior can justify the use of stock options by owners, and identifying the effect of contracts on managers' behavior requires breaking endogeneity from owners choosing contracts.

2.3.1. The Game

[9] Experimental literature demonstrates that more than 80% of the population has the level of risk aversion between 0 and 0.7 [24–26].

[10] We do the calculations for different levels of managers' initial wealth. These results hold when we vary their initial wealth from the same as the value of compensation to as ten times as the value of compensation.

We have our subjects play a series of two-player lottery contests. In each round, subjects are randomly matched, and then they decide how much to invest in the contest. Their winning probability is determined by their own investment and the investment by their opponent. The parameterization for the experiment design is based on that used in the theoretical models.

In the theoretical models, when managers are making investment decisions for their firms in the competition, managers are actually playing a downsized version of the lottery contest because of the stock and stock options in their incentive contracts. The size of manager i's contest is determined by the normalized amount of equity in the contract, $\beta_i + \gamma_i$. When manager i is making the investment decision, $b_i * c_i$, for her firm, she is actually investing $x_i * c_i = b_i * c_i * (\beta_i + \gamma_i)$ in the downsized lottery contest. When her firm wins the prize, R, she actually earns $r_i = R * (\beta_i + \gamma_i)$. If we let the values of these parameters be the same as those in the theoretical section, $R = 200,000$, and $\beta_i + \gamma_i = 0.1\%$, then managers are competing for a prize for which their share would be 200. In the experiments, we have our subjects play a series of the downsized lottery contests. For each round of the contest, each subject is endowed with 100 ECUs, which corresponds to the sum of base salary and initial value of stock in the contract, α_i. They then compete for a 200 ECU prize inside the competition pair through choosing their effective investment, x_i. When we need to calculate the profit for the firm, we can derive the investment decision for firm i based on manager i's decision, $b_i = 1000 * x_i$.

We examine the impact of stock options on managers' investment behavior by varying the ratios of the normalized amount of stock options to the normalized amount of equity in their own and opponent's contracts, (θ_i, θ_j). Even though the incentive contracts are specified by the amount of base salary, the amount of stock and the amount of stock options, we have shown theoretically that (θ_i, θ_j) are crucial parameters affecting managers' competitive behavior. We test four contract configurations which only differ in these ratios. The contract configuration is defined from the perspective of one subject rather than the competition pair. In these four contract configurations manager i has either only stock or only stock options in her performance pay and so does her rival, $(\theta_i, \theta_j) = (0,0), (0,1), (1,0), (1,1)$. We call them Stock v Stock (S v S), Stock v Option (S v O), Option v Stock (O v S) and Option v Option (O v O). We focus on these four contract configurations because owners grant either stocks only or stock options only in a typical theoretical equilibrium. Another reason is to test subjects' reactions to substantial changes in stock options.

We inform managers how their payoff would be calculated under different incentive contracts without mentioning the above parameters. When a manager is assigned to be paid by Stock, we will give her a 100 ECU endowment and let her compete for a 200 ECU prize in the two-player lottery contest. She can spend as much as 300 ECUs to invest. Her payoff can be calculated as shown by Eq. 2.11, where x_i and x_j stand for the investment chosen by herself and her opponent. When a manager is assigned to be paid by Option, she does not pay the investment cost if she loses the competition. Her payoff is calculated as shown by Eq. 2.12.

$$\pi_i = \begin{cases} 100 + 200 - c_i x_i & \text{if subject } i \text{ wins} \\ 100 - c_i x_i & \text{if subject } i \text{ loses} \end{cases} \tag{2.11}$$

$$\pi_i = \begin{cases} 100 + 200 - c_i x_i & \text{if subject } i \text{ wins} \\ 100 & \text{if subject } i \text{ loses or invest more than 200} \end{cases} \tag{2.12}$$

The winning probability of a manager equals the ratio of her effective investment to the sum of the effective investment by herself and her opponent. In the case that neither of the paired managers invests, the prize will be granted to either of them with equal probability.

$$prob_i = \begin{cases} \frac{x_i}{x_i+x_j} & \text{if } x_i > 0 \text{ either } x_j > 0 \\ \frac{1}{2} & \text{if } x_i = 0 \text{ and } x_j = 0 \end{cases} \quad (2.13)$$

As you may have noticed, the expected compensation of contract Option would be higher than that of contract Stock if managers choose the same investment level under these two incentive schemes. There are two reasons why we design the contracts in this way rather than make them equivalent in terms of expected payoff. First, in order to make them payoff equivalent, we need to give managers different amounts of endowment when assigning them different contracts. Then, across treatments we vary not only the ratios of normalized amount of stock options to the normalized amount of equity but also the amount of endowment. This would make it hard to isolate managers' reactions to the change of these ratios. Secondly, even if we make these contracts payoff equivalent based on certain assumptions about managers' behavior, it is very likely that their actual payoffs in the experiment will be different from the prediction, because their actions may deviate from our assumptions. Thus, even though it is possible to make these contracts' payoff equivalent in theory, it is hard to make sure that managers with different contracts would earn the same expected payoff in the experiment.

Our theoretical model suggests that the asymmetry of marginal investment cost would affect managers' reactions to executive stock options. Thus, we examine their investment behavior under symmetric-cost and asymmetric-cost settings. In the symmetric-cost setting, rival subjects will have the same unit cost for the effective investment, $c_i = c_j = 1.00$ ECU. In the asymmetric-cost setting, one subject of each competition pair has a unit cost equal to 1.25 ECU while the other has 1.00 ECU. The reason for choosing 1.25 is that it is high enough to induce significant behavioral shift indicated by the theoretical models.

2.3.2. Procedure The experiment has a 2×4 design. We test their reactions to four different contract configurations within subjects. In each round of the experiment session, every subject faces any of the four contract configurations with equal probability. On the other hand, we test how cost asymmetry affects their reactions between subjects. We have half of our sessions where the cost is symmetric between the competing subjects while the half where cost is asymmetric. The cost-advantaged players is randomly selected for each round.

In each session of the experiment, the subjects play the lottery contest for 30 rounds. At the beginning of each round, subjects are paired randomly, and they compete against each other within the pair. After the pairing, every subject is assigned the contract either Stock or Option with equal probability, so she has a 25% chance of facing each of the four contract configurations. For example, one subject has a 50% chance to get Stock, and her opponent has a 50% chance to get Option, so that subject has a 25% chance to face the contract configuration Stock v Option. Then, the unit cost of effective investment for each subject is determined. In the symmetric-cost sessions, all subjects have the unit cost equal to 1.00 ECU. In the asymmetric-cost sessions, within each pair, we randomly choose one subject to have a unit cost of 1.25 ECU while the other have 1.00 ECU. The pairing, assignment of contracts and determination of unit cost are redone in every round.

The contract assigned to subjects determine how their payoff is going to be calculated as shown by Eq. 2.11 and Eq. 2.12. However, during the experiment, we do not mention these words, "stock" or "stock option". Instead, subjects are explained how their payoffs would be calculated under different circumstances corresponding to different contracts. Subjects are told how much endowment they have, how much they can earn if they win the prize, and how much the investment costs. In the experiment, we use the phrase "loss exemption" to help them understand the fundamental incentives of stock options. Subjects are asked to pay attention to how their potential earnings are different due to the loss exemption.

After we finish pairing subjects, assigning contracts and determining unit cost, subjects are asked to choose the effective investment in the lottery contest. On their decision screens, we show their own contract and their opponent's contract, as well as their own unit cost and that of their opponent. In order to help them understand the consequences of their investment decisions, we provide them a calculator. They can enter a potential number of their own effective investment and a guess about opponent's, and then we show them their probability of winning, earnings if they win and earnings if they lose. Once all subjects have made decisions, the computer determines the winner based on Eq. 2.13. At the end of each round, we inform subjects whether they win the competition or not, and how much they earned for the round.

Each session began with an introduction of the games, after which we demonstrated the computer interface and the rules through two sample rounds of competition against robot players. The interactive software system is programmed using z-Tree [27]. Subjects engaged in the lottery contest for thirty rounds, and their final payoff was equal to $10 show-up fee plus the sum of earnings in five rounds randomly draw from the thirty rounds. Each session lasted an hour and a half to two hours. Payments including the show-up fee ranged from a minimum of $10 to a maximum of around $ 45, with an average of $35.

We have run three symmetric-cost sessions with 60 subjects and three asymmetric-cost sessions with 60 subjects. We expected to have $450(= 60 \times 30 \times 0.25)$ observations of the investment decision for each contract configuration in either the cost-symmetric or the cost-asymmetric case. Table 3.2 shows how many observations we actually got in the experiment for each contract configuration. All of our experiment sessions were conducted at Southern Methodist University. Subjects were recruited from a university-wide subject pool comprising undergraduate and graduate students who had indicated a willingness to be paid volunteers in decision-making experiments.

Table 2.1: Experimental Design and Data Points

	Sessions	Subjects	S v S	S v O	O v S	O v O
Symmetric Cost	3	60	476	453	453	418
Asymmetric Cost	3	60	462	429	429	480

2.4. Hypotheses

Following the parameterization, Table 2.2 shows the predictions on managers' investment behavior and owners' expected profit based on the assumption that managers are risk neutral. The predicted investment is shown in ECUs. The expected profit of firm owners is shown in thousands of ECUs. We treat the configuration where both owners grant Stock as the baseline. If managers follow the risk neutral prediction, there is little incentive for the firm owners to deviate from Stock to Option. When the cost is symmetric, the owners are expected to earn 50.0 thousands of ECUs if they keep to Stock while expected to earn 49.4 thousands of ECUs or less if they deviate. When the cost is asymmetric, Stock is still the dominant strategy for the cost-disadvantaged owner, as 39.5 is bigger than 36.9 and 32.2 is bigger than 23.7. For the cost-advantaged owner, by deviating from Stock to Option she can increase her profit from 61.7 thousands of ECUs to 62.4 thousands of ECUs. We will use the experimental data to test whether managers choose the equilibrium strategies and whether the use of stock option by firm owners can be justified.

Table 2.2: Predictions on Managers' Investment Behavior and Owners' Expected Profit

	Contract Configuration	Symmetric_Cost	Asymmetric_Cost	
			Cost_Advantaged	Cost_Disadvantaged
Investment chosen by managers (ECUs)	Stock v Stock	50.0	49.4	49.4
	Option v Stock	61.6	57.3	65.6
	Stock v Option	49.4	50.0	48.1
	Option v Option	66.7	63.6	69.6
Expected Profit of firm owners (thousands of ECUs)	Stock v Stock	50.0	61.7	39.5
	Option v Stock	49.4	62.4	36.9
	Stock v Option	39.6	47.6	32.2
	Option v Option	33.3	43.1	23.7

Hypothesis 1 *The investment chosen by managers is equal to the risk neutral equilibrium prediction under every contract configuration.*

We will first test whether the investment chosen by managers is consistent with the prediction investment as shown in the upper section of Table 2.2. If we fail to reject Hypothesis 1, firm owners have little incentive to grant Option as we have shown in the theoretical section. However, we do not expect that managers would choose the investment equal to the risk-neutral equilibrium. For example, previous experimental studies [28–33] have shown that in the symmetric-cost case when both competing managers are paid by an incentive scheme corresponding to Stock, they invest significantly above the risk neutral prediction. Granting Option is likely to make managers more aggressive on the investment decision, so we believe they may also over-invest when paid by Option.

We will then examine whether a firm owner shifting from Stock to Option would affect the investment behavior of her own and opponent managers in a way that improves the owner's profit. In particular, we will investigate whether a firm owner shifting to Option makes her own manager choose the the owner's optimal investment, the investment that maximizes her expected profit given the empirical distribution of the opponent's investment. Based on the experiment data, we can calculate the owner's optimal investment as shown in the lower section of Table 2.5 and Table 2.6 in the result section. We also examine whether the change in her opponent manager's investment caused by the shift is significantly different from the prediction. For example, in the symmetric-cost case, if the owner deviates from the baseline to Option, her opponent manager decreases the investment by 0.6 ECU (=50.0-49.4). We are going to test whether the reaction of her opponent manager is consistent with this prediction. As you may have noticed, the effect of granting Option depends on what contract the opponent owner grants, so we investigate the case when her opponent grants Stock and the case when her opponent grants Option respectively.

Hypothesis 2a *When a firm owner grants Stock given her opponent granting Stock, her own manager chooses the investment maximizing the owner's expected profit.*

Hypothesis 2b *When a firm owner grants Option given her opponent granting Stock, her own manager chooses the investment maximizing the owner's expected profit.*

Hypothesis 2c *When a firm owner grants Stock given her opponent granting Option, her own manager chooses the investment maximizing the owner's expected profit.*

Hypothesis 2d *When a firm owner grants Option given her opponent granting Option, her own manager chooses the investment maximizing the owner's expected profit.*

Hypothesis 3a *When a firm owner shifts from Stock to Option given her opponent granting Stock, the change in her opponent manager's investment is equal to the prediction.*

Hypothesis 3b *When a firm owner shifts from Stock to Option given her opponent granting Option, the change in her opponent manager's investment is equal to the prediction.*

Even though these tests on the empirical investment tell us how stock options would affect manager's investment, we still need to know how investment behavior induced by stock options would affect profit of firm owners. We first test whether the expected profit based on the empirical investment behavior is equal to that based on risk-neutral predictions on managers' investment. If we fail to reject, Hypothesis 4 then firm owners have little incentive to grant Option as we have shown in the theoretical section.

Hypothesis 4 *The expected profit for firm owners given managers' empirical investment is equal to the expected profit given risk neutral equilibrium investment under every contract configuration.*

However, Hypothesis 4 is likely to be rejected because managers are unlikely to follow the risk neutral equilibrium, so it is possible that granting stock options is a profitable strategy for firm owners. Therefore, based on the empirical investment observed in the experiment, we look at whether a firm owner shifting from Stock to Option would increase her profit. We investigate the case when her opponent grants Stock and the case when her opponent grants Option, respectively. In addition, we look at the situation when both of the competing firm owners deviate from Stock to Option to see whether they will end up with a better or worse outcome.

Hypothesis 5a *When a firm owner shifts from Stock to Option given opponents granting Stock, her profit does not change.*

Hypothesis 5b *When a firm owner shifts from Stock to Option given opponents granting Option, her profit does not change.*

Hypothesis 5c *When both firm owners in the competition shift from Stock to Option, their profit does not change.*

As we have mentioned before, cost asymmetry might affect managers' reactions to stock options. Even though we did not mention the cost configuration when stating these hypotheses, we will test all of them for firms in the symmetric-cost case, and cost-advantaged and cost-disadvantaged firms in the asymmetric-cost case respectively.

2.5. Experiment Results

2.5.1. Summary Statistics

The summary statistics of experimental data for symmetric-cost and asymmetric-cost sessions are shown in Table 4.5 and Table 2.4 respectively[11]. All numbers in this section are in ECU. We first examine the experiment data by using the Wilcoxon tests. We consider these Wilcoxon tests as preliminary rather than final tests of our hypothesis. Based on the Wilcoxon tests, we found that investment levels are not significantly different from the prediction if they are paid by Stock (Stock v Stock or Stock v Option), while it is significantly above the prediction if managers are paid by Option (Option v Stock or Option v Option). The expected compensation and expected profit are usually significantly below the prediction except for when the manager is paid by Option while her opponent is paid by Stock (Option v Stock). Under Option v Stock, the compensation and profit are usually either significantly above or not significantly different from the prediction.

We can also look at how investment, expected compensation and expected profit differ across the four contract configurations. If we treat the configuration when both competing managers are paid by Stock as the baseline (Stock v Stock), a firm owner deviating to Option

[11]The expected compensation of a manager and expected profit of her firm are calculated based on the investment by that manager and by her opponent.

Table 2.3: Summary statistics for symmetric-cost sessions

	Investment (ECUs)		Expected Compensation (ECUs)		Expected Profit (thousands of ECUs)	
	Obs.	Theory	Obs.	Theory	Obs.	Thoery
Stock v Stock	56.0 (5.00)	50.0	41.8*** (3.28)	50.0	41.8*** (3.28)	50.0
Option v Stock	68.0* (3.54)	61.6	77.0 (3.39)	76.8	55.7* (3.54)	49.4
Stock v Option	48.3 (1.55)	49.4	26.5*** (2.67)	39.6	26.5*** (2.67)	39.6
Option v Option	74.2 (5.99)	66.7	55.6*** (2.60)	66.7	25.9*** (2.46)	33.3

Notes: the investment and compensation are in ECUs while the profit is in thousands of ECUs. Standard deviations in parentheses. *'s indicate p-value of Wilcoxon test compared to theoretical prediction. *** $p < 0.01$. ** $p < 0.05$. * $p < 0.1$.

makes her own manager, whose contract configuration becomes Option v Stock, increase the investment significantly. [12] On the other hand, her opponent manager, whose configuration becomes Stock v Option, would decrease the investment significantly. [13] In the case when both firm owners deviate to Option, the managers invest even more. [14] As for the expected profit for firm owners, we found deviating from Stock to Option would increase owners' profit if they are in the symmetric-cost case or if they are cost-advantaged in the asymmetric-cost case.

2.5.2. Investment Behavior

As we have mentioned in the hypothesis section, we want to test whether the investment chosen by managers under different contract configurations is consistent with the risk-neutral equilibrium prediction as well as how firm owners granting Option would affect the investment behavior of their own and opponent managers. We conduct regressions to facilitate the formal tests of our hypotheses on managers' investment behavior. All of our regressions are conducted using a fixed effects panel specification with standard errors clustered at the individual subject level.

We run regressions for the symmetric-cost sessions and asymmetric-cost sessions separately. In the basic regressions, the dependent variable is the investment chosen by a manager in a given period, and the independent variables include a constant and dummy variables indicating the manager's contract configuration in that period. For example, Stock v Option denotes the configuration where the manager is paid by Stock while her opponent paid by Option. For asymmetric-cost sessions, we also have a dummy to identify the cost-disadvantaged subjects as well as its interactions with the dummies for contract configurations. Additional

[12] Here, we only show the results of Wilcoxon test for the symmetric-cost case. As for the cost-asymmetric sessions, reactions of subjects are evidencing a roughly similar pattern. Wilcoxon test: $z = 3.07, Prob > |z| = 0.0022$.

[13] Wilcoxon test: $z = -2.78, Prob > |z| = 0.0054$.

[14] Wilcoxon test: $z = 4.08, Prob > |z| = 0.000$.

Table 2.4: Summary statistics for asymmetric-cost sessions

	Investment (ECUs)		Expected Compensation (ECUs)		Expected Profit (thousands of ECUs)	
	Obs.	Theory	Obs.	Theory	Obs.	Theory
Low cost						
Stock v Stock	52.0 (3.39)	49.4	55.0* (4.04)	61.7	55.0* (4.04)	61.7
Option v Stock	73.2*** (4.90)	57.3	82.1 (4.04)	85.4	66.2 (4.09)	62.4
Stock v Option	46.6 (3.98)	50.0	29.6*** (3.88)	47.6	29.6*** (3.88)	47.6
Option v Option	78.7*** (4.68)	63.6	55.5*** (2.93)	72.7	22.3*** (3.61)	43.1
High cost						
Stock v Stock	49.0 (0.70)	49.4	39.4 (0.45)	39.5	39.4 (0.45)	39.5
Option v Stock	83.1*** (5.07)	65.6	62.4* (3.05)	68.8	37.0 (6.67)	36.9
Stock v Option	44.2 (4.49)	48.1	20.2*** (3.06)	32.2	20.2*** (3.06)	32.2
Option v Option	91.5*** (5.94)	69.6	45.5*** (2.83)	60.9	6.5*** (3.39)	23.7

Notes: the investment and compensation are in ECUs while the profit is in thousands of ECUs. Standard deviations in parentheses. *'s indicate p-value of Wilcoxon test compared to theoretical prediction. *** $p < 0.01$. ** $p < 0.05$. * $p < 0.1$.

regression specifications include extra explanatory variables such as dummies indicating the subject's contract configurations in the prior period, a dummy for whether or not the subject won the prize in the prior period, and a dummy for whether or not the round is one of the last ten periods.

We have shown results of the regressions in the upper section of Table 2.5 and Table 2.6. The intercepts of these regressions can be interpreted as the average investment in the baseline treatments where both competing managers are paid by Stock. In the cost-asymmetric case, the baseline refers to the cost-advantaged manager under this contract configuration. The coefficients of these regressions indicate how the investment in the indicated treatment differs from that of the baseline. We test these constant terms and coefficients against the risk-neutral equilibrium predictions listed in the second column.

Based on the regression results, the average investment under different contract and cost configurations can be calculated as the linear combinations of intercept and coefficients, and the results are presented in the lower section of Table 2.5 and Table 2.6. We test them against the prediction listed in the second column in order to see whether managers

Table 2.5: Panel regressions for investment (ECUs) for symmetric sessions

			Regression		
	Prediction		Specification 1	Specification 2	Specification 3
Cons. (Stock v Stock)	50.00		56.82ˆ∗∗∗	55.82ˆ∗∗∗	56.49ˆ∗∗
			(1.777)	(2.138)	(2.688)
Option v Stock	11.60		11.84	11.19	10.87
			(3.567)	(3.676)	(3.629)
Stock v Option	-0.60		-7.109ˆ∗∗	-7.609ˆ∗∗∗	-7.548ˆ∗∗∗
			(2.692)	(2.581)	(2.604)
Option v Option	16.70		17.24	17.61	17.39
			(3.862)	(4.012)	(3.987)
L.Option v Stock					2.332
					(3.198)
L.Stock v Option					-3.463
					(2.275)
L.Option v Option					-2.145
					(1.929)
Last ten periods				5.357ˆ‡‡‡	5.423ˆ‡‡‡
				(2.295)	(2.241)
L.Win				-0.745	-0.337
				(1.706)	(1.686)
Observation			1800	1740	1740

			Investment under Configurations		
	Prediction	Owner's Optimal	Specification 1	Specification 2	Specification 3
Stock v Stock	50.00	37.7	56.82ˆ∗∗∗,†††	55.82ˆ∗∗∗,†††	56.49ˆ∗∗,†††
			(1.777)	(2.138)	(2.688)
Option v Stock	61.60	34.2	68.66ˆ∗∗∗,†††	67.01ˆ∗,†††	67.35ˆ ,†††
			(2.224)	(3.050)	(3.728)
Stock v Option	49.40	41.2	49.71ˆ ,†††	48.21ˆ ,†††	48.94ˆ ,†††
			(2.623)	(2.326)	(2.411)
Option v Option	66.70	38.7	74.06ˆ∗∗∗,†††	73.43ˆ∗∗,†††	73.87ˆ∗,†††
			(2.557)	(3.107)	(3.754)

Note 1: Robust standard errors in parentheses.

Note 2: ∗ means we test the estimators against the predictions listed in the second column. † means we test estimators against owner's optimal investment level listed in the third column. ‡ means we test the estimators against zero. In particular, $*p < 0.1, **p < 0.05, ***p < 0.01$. The same is true for † and ‡ .

Table 2.6: Panel regressions for investment (ECUs) for asymmetric sessions

			Regression	
	Prediction		Specification 1	Specification 2
Cons. (Stock v Stock)	49.38		52.35	52.38
			(2.577)	(3.343)
Option v Stock	7.92		21.07ˆ∗∗∗	20.69ˆ∗∗∗
			(4.592)	(4.867)
Stock v Option	0.588		-3.190	-3.321
			(3.225)	(3.467)
Option v Option	14.25		25.73ˆ∗∗∗	24.89ˆ∗∗
			(4.485)	(4.834)
Cost_dis	-0.173		-1.309	-1.445
			(3.360)	(3.439)
Cost_dis*Option v Stock	8.281		9.226	10.55
			(4.893)	(5.088)
Cost_dis*Stock v Option	-1.888		-4.537	-4.100
			(5.002)	(5.352)
Cost_dis*Option v Option	5.954		14.20ˆ∗∗	16.36ˆ∗∗
			(3.856)	(4.233)
L.Cost_dis				1.244
				(1.733)
L.Option v Stock				-1.099
				(2.208)
L.Stock v Option				0.449
				(2.109)
L.Option v Option				-2.248
				(2.472)
Last ten periods				-3.105
				(3.178)
L.Win				2.645
				(1.967)
Observation			1800	1740
			Investment under Configurations	
	Prediction	Owner's Optimal	Specification 1	Specification 2
Cost_adv + Stock v Stock	49.38	37.8	52.35ˆ , †††	52.38ˆ , †††
			(2.577)	(3.343)
Cost_adv + Option v Stock	57.30	32.1	73.41ˆ∗∗∗, †††	73.07ˆ∗∗∗, †††
			(2.807)	(3.752)
Cost_adv + Stock v Option	49.97	41.8	49.16ˆ ,††	49.05ˆ ,†
			(3.112)	(4.409)
Cost_adv + Option v Option	63.63	41.6	78.08ˆ∗∗∗, †††	77.26ˆ∗∗∗, †††
			(4.485)	(3.628)
Cost_dis + Stock v Stock	49.40	36.2	51.04ˆ , †††	50.93ˆ , †††
			(3.360)	(3.766)
Cost_dis + Option v Stock	65.60	34.4	81.33ˆ∗∗∗, †††	82.17ˆ∗∗∗, †††
			(2.507)	(3.252)
Cost_dis + Stock v Option	48.10	38.3	43.31ˆ ,	43.51ˆ ,
			(3.866)	(4.188)
Cost_dis + Option v Option	69.60	36.2	90.97ˆ∗∗∗, †††	92.17ˆ∗∗∗, †††
			(3.484)	(3.786)

Note 1: Robust standard errors in parentheses.

Note 2: ∗ means we test the estimators against the predictions listed in the second column. † means we test estimators against owner's optimal investment level listed in the third column. ‡ means we test the estimators against zero. In particular, $*p < 0.1, **p < 0.05, ***p < 0.01$. The same is true for † and ‡ .

choose the investment equal to the risk-neutral equilibrium strategy. Moreover, we test the observed investment against the owner's optimal investment listed in the third column to examine whether managers choose the investment that maximizes the profit of their owners given the empirical distribution of their opponents' investment. We also test the difference in investment between contract configurations to see how a firm owner granting Option changes the investment of her opponent manager. Therefore, we actually test Hypothesis 1, Hypothesis 2 and Hypothesis 3. The results of these tests are summarized in Table 2.7.

We found the empirical investment is not always equal to the risk-neutral equilibrium prediction. When both competing managers are paid by Stock, we found their investment is significantly above the prediction in the symmetric-cost case. This is consistent with previous experimental papers [28–33]. But, in the asymmetric-cost case, these managers' investment does not differ significantly from the prediction. When a manager is paid by Stock while her opponent is paid by Option, her investment is not significantly different from the prediction, in either symmetric-cost or asymmetric-cost sessions. When a manager is paid by Option, she invests significantly above the prediction in both symmetric-cost and asymmetric-cost cases, regardless of what contract her opponent has.

Since the investment chosen by managers does not always follow the risk neutral predictions, it is possible that granting Option is a profitable strategy for firm owners. We examined whether a firm owner granting Option makes her manager choose the investment maximizing owner's expected profit. We found managers usually invest significantly more than the owner's optimal regardless of what contracts they have and what contracts their opponents have. The only case where managers choose the investment not significantly different from the owner's optimal is where they are cost-disadvantaged and under contract configuration Stock v Option. In that case, firm owners granting Option actually push their own managers away from owners' optimal.

Table 2.7: Summary of Hypothesis Testing Results on Investment Behavior

Hypothesis	Contract Configuration	Symmetric_Cost	Asymmetric_Cost	
			Cost_Advantaged	Cost_Disadvantaged
H1: Empirical Investment = Risk Neutral Prediction ?	Stock v Stock	>	=	=
	Option v Stock	>	>	>
	Stock v Option	=	=	=
	Option v Option	>	>	>
H2: Empirical Investment = Owner's Optimal ?	Stock v Stock	>	>	>
	Option v Stock	>	>	>
	Stock v Option	>	>	=
	Option v Option	>	>	>
H3: Decreases Opponent's Empirical Investment More than Prediction ?	Stock v Stock → Option v Stock	*Yes*	*Yes*	*No*
	Stock v Option → Option v Option	*No*	*No*	*No*

Note 1: When we test Hypothesis 1, "=" means we cannot reject that empirical investment is equal to the risk neutral prediction in the given contract and cost configuration, and ">" means that the empirical investment is significantly above the risk neutral prediction.
Note 2: When we test Hypothesis 2, "=" means that we cannot reject that empirical investment is equal to the owner' optimal investment, and ">" means that the empirical investment is significantly above the owners' optimal investment.
Note 3: For Hypothesis 3, we actually increase
When we test Hypothesis 2, "=" means that we cannot reject that empirical investment is equal to the owner' optimal investment, and ">" means that the empirical investment is significantly above the owners' optimal investment.

Even though a firm owner granting Option fails to make her own manager choose the owner's optimal investment, it is possible that granting Option decreases the investment of her opponent manager and therefore increase the owner's profit. We found that the reaction of the opponent manager depends on the cost configuration and contract configuration. When the firm owner is in the symmetric-cost case or she is cost-advantaged in the asymmetric-cost case, the owner deviating to Option decreases her opponent's investment more than the prediction if her opponent grants Stock[15] . If the opponent manager is paid by Option, the firm owner granting Option usually increases the opponent's investment[16]. In addition, the overreaction of managers' investment is inconsistent with the prediction based on the assumption of loss averse managers.

[15] In the symmetric-cost case, firm owner granting Option would decrease the opponent's investment more than the prediction, $F(1,59) = 5.85, Prob > F = 0.0187$. In the asymmetric-cost case, cost-advantaged firm owner granting Option would decrease the opponent's investment more than the prediction, $F(1,59) = 3.48, Prob > F = 0.067$. In the asymmetric-cost case, cost-disadvantaged firm owner granting Option would decrease the opponent's investment not significantly different from the prediction, $F(1,59) = 1.37, Prob > F = 0.2462$.

[16] In the symmetric-cost case, firm owner granting Option would increase the opponent's investment not significantly different from the prediction,$F(1,59) = 0.01, Prob > F = 0.9182$. In the asymmetric-cost case, cost-advantaged firm owner granting Option would increase the opponent's investment not significantly different from the prediction, $F(1,59) = 0.19, Prob > F = 0.6604$. In the asymmetric-cost case, cost-disadvantaged firm owner granting Option would increase the opponent's investment not significantly different from the prediction, $F(1,59) = 2.38, Prob > F = 0.1286$.

We have shown that risk neutral equilibrium prediction is not consistent with managers' investment behavior. We also examine whether alternative preference settings, including risk aversion and loss aversion, can explain the observed behavior. As shown in the theoretical section, when managers are risk averse or loss averse, they should invest less than if they are risk-neutral. The prevalent over-investment observed in the experiments indicates that neither risk aversion nor loss aversion can explain managers' investment levels under different contract configurations. We then investigate whether these alternative preference settings can justify managers' overreactions when they are paid by stock, and their opponent owners shift from stock to options. The reaction predicted by the risk aversion model is significantly smaller than what we observed in the data. For the loss aversion model, even though it suggests that managers in that scenario would drop their investment significantly, its predictions[17] still contradict the empirical results which are shown in Table 2.7.

2.5.3. Heterogeneous Investment Behavior

We have shown how the population as a whole behaves under various contract configurations, but remarkable heterogeneity exists in investment behavior as shown in Fig ??. Since managers of different types make investment decisions differently under these contract configurations, firm owners may need to design their executive incentive contracts accordingly. In this section, we will focus on separating these managers into different types and then examining their reactions to those four contract configurations.

To investigate the heterogeneity in managers' investment behavior, we apply a finite mixture model [34, 35], which can be used to analyze data where observations originate from various groups, and the group affiliations are not known [36, 37]. To separate subjects into groups, we adopt an expectation-maximization (EM) algorithm within a maximum

[17] The loss aversion model indicates that cost-disadvantaged firm owner granting options decreases her manager's investment by an amount significantly larger than then risk-neutral prediction, while cost-advantaged firm owner decreases her manager's investment not significantly different from risk-neutral prediction.

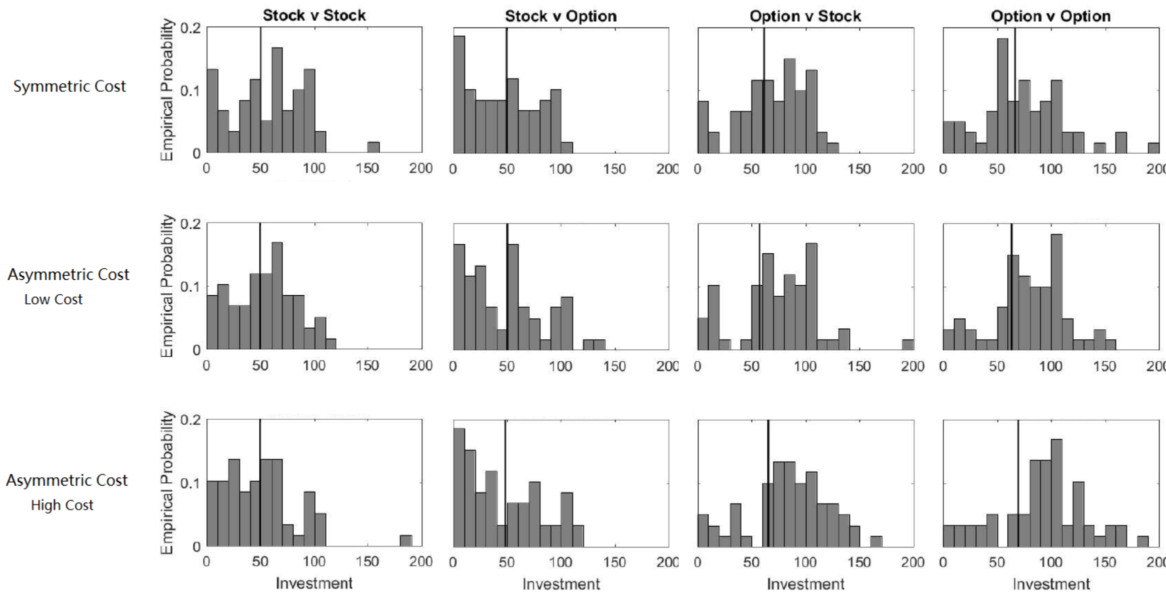

Figure 2.1: The Empirical Distribution of Investment (ECUs)

Notes:The vertical line in each subplot is the risk neutral equilibrium prediction under the indicated contract and cost configuration.

likelihood framework [37–39].[18] We then run regressions for each group using a fixed effect panel specification with standard errors clustered at the individual levels.

We report the results for the symmetric-cost sessions in Table 2.2. It contains a two-segment grouping and a three-segment grouping, which generate the highest AIC/BIC when we vary the number of segments. Each column of the table reports the proportion of the sample in the group, summary statistics, the estimated coefficients of the regressions, and estimated investment under different configurations. The two-segment grouping divides the population into under-investors (segment I) and over-investors (segment II). The investment

[18]The estimation-classification algorithm assumes that the choices of each person in the sample are described by a function F(theta), where theta is a vector of unknown model parameters. Heterogeneity is introduced by allowing that population to contains K segments, or types of person, with each type described by one of K different thetas. The thetas describe each type and what subjects are which type are estimated simultaneously. In estimation, person i's contribution to the likelihood function, given theta, is the maximum of the joint likelihood of all i's observations across the K types. Conventional maximization algorithms can be used to identify the theta which maximize the likelihood of the observed data, with care taken to ensure that the global maximum is identified in a likelihood function which often has many local maxima.

chosen by under-investors is significantly below the risk neutral equilibrium prediction given all contract configurations; that chosen by over-investors is significantly above. Since the over investors take up 70.0% of the sample, we observe pervasive overinvestment in the aggregate data. However, the over-investors decrease their investment by significantly more than the prediction if the contract configuration shifts from Stock v Stock to Stock v Option. In addition, adding another segment separates the extensively aggressive subjects out from the over-investors. There are only 4 out of 60 subjects placed in this aggressive group.

The grouping results for the asymmetric-cost sessions are presented in Table 2.3. The two-segment grouping also divides the population into under-investors (segment I) and over-investors (segment II). As for the three-segment grouping, there still exist a group for under-investors (segment I) and a group for over-investors (segment III). These subjects either overinvest or underinvest in the baseline treatment (cost-advantaged subjects under Stock v Stock), and their reaction to any shift of configuration is not significantly different from the prediction. In contrast, the investment by subjects in segment II is not significantly different from the prediction under the baseline treatment, but they overreact to the shift of contract configuration. The proportion of subjects in segment II is 56.67%.

2.5.4. Expected Profit of Firm Owners

In this section, we examine how granting Option would affect the profit of firm owners given managers' investment behavior. We calculate the empirically expected profit of firm owners based on the investment behavior observed in our experiments. For comparison, we also calculate the theoretically expected profit based on the risk-neutral equilibrium investment. When calculating the expected profit, we let the values of the parameters be the same as those used in the theoretical model and experiment design.

Table 2.8: Panel regressions on expected profit (thousands of ECUs) for symmetric sessions

	Prediction	Regression
Cons. (Stock v Stock)	50.00	41.98ˆ∗∗∗
		(0.918)
Option v Stock	-0.60	12.57ˆ∗∗∗
		(1.532)
Stock v Option	-10.39	-14.74ˆ∗∗∗
		(1.302)
Option v Option	-16.67	-15.50
		(2.000)
	Prediction	Expected Profit
Stock v Stock	50.00	41.86ˆ∗∗∗
		(0.906)
Option v Stock	49.40	54.55ˆ∗∗∗
		(1.036)
Stock v Option	39.61	27.24ˆ∗∗∗
		(1.020)
Option v Option	33.33	26.48ˆ∗∗∗
		(1.222)

Note 1: The profit in this table are in thousands of ECUs.
Note 2: Robust standard errors in parentheses.
Note 3: ∗ means we test the estimators against the predictions listed in the second column. † means we test estimators against owner's optimal investment level listed in the third column. ‡ means we test the estimators against zero. In particular, $*p < 0.1, **p < 0.05, ***p < 0.01$. The same is true for † and ‡.

Figure 2.2: Heterogeneous estimates of determinants on investment for symmetric-cost sessions

		Two-segment		Three-segment		
		I	II	I	II	III
Summary Statistics						
Proportion of subjects		30.0%	70.0%	30.0%	63.33%	6.67%
Average investment		26.04	77.43	26.04	73.85	111.45
Median investment		10.00	80.00	10.00	80.00	100
Overinvestment frequency		15.00%	72.70%	15.00%	72.72%	72.50%
Frequency of investment = 0		10.37%	1.59%	10.37%	1.75%	0%
Frequency of investment < 10		43.15%	3.89%	43.15%	3.42%	8.33%
Frequency of investment = 100		6.11%	22.86%	6.11%	24.47%	7.50%
Frequency of investment >= 100		6.66%	31.11%	6.66%	28.77%	53.33%
	Prediction	*Regression*				
Constant	50.00	17.83***	73.50***	17.83***	71.77***	89.96**
		(3.180)	(2.118)	(3.180)	(1.966)	(8.167)
Stock v Option	-0.60	-3.354	-8.702**	-3.354	-9.052**	-4.762
		(2.540)	(3.713)	(2.540)	(3.323)	(24.01)
Option v Stock	11.60	19.12	8.928	19.12	8.210	13.53
		(7.485)	(3.938)	(7.485)	(3.928)	(21.73)
Option v Option	16.70	19.10	16.50	19.10	9.560*	74.50*
		(5.495)	(5.055)	(5.495)	(3.784)	(19.15)
Log Likelihood		-9012.58		-8802.89		
AIC/BIC		18047.16/18107.62		17639.77/17733.19		
Observations		540	1260	540	1140	120
	Prediction	*Investment under Different Configurations*				
Stock v Stock	50.00	17.83***	73.50***	17.83***	71.77***	89.96**
		(3.180)	(2.118)	(3.180)	(1.966)	(8.167)
Stock v Option	49.40	14.47***	64.80***	14.47***	62.71***	85.19**
		(3.129)	(3.559)	(3.129)	(3.114)	(23.14)
Option v Stock	61.60	36.94***	82.42***	36.94***	79.98***	103.48
		(4.530)	(2.526)	(4.530)	(2.275)	(18.89)
Option v Option	66.70	36.93***	90.00***	36.93***	81.33***	164.45***
		(2.873)	(3.502)	(2.873)	(2.466)	(13.27)

Note 1: Robust standard errors in parentheses
Note 2: + means we test the estimators against the predictions in the first column.
means we test the estimators against zero.
Note 3: The stars on the left of these coefficients indicate whether they are significantly different from the predictions or zero.
In particular, *** p<0.01, **p<0.05, * p<0.1

We conduct panel regressions to facilitate the formal tests of our hypotheses on the expected profit for firm owners. The dependent variable is the empirically expected profit, and the independent variables include dummy variables indicating contract configurations and cost configurations. Based on the regression results, the average empirically expected profit under different contract and cost configurations can be calculated as linear combinations of intercept and coefficients. We test the empirically expected profit against the theoretical prediction (Hypothesis 4). We found that firm owners are usually expected to earn lower profit than the prediction due to the overinvestment by their own and opponent managers. But if the firm owner grants Option while her opponent grants Stock, then her expected profit would be either higher than or not different from the prediction. The results for the symmetric-cost sessions are shown in Table 2.8. For the asymmetric-cost sessions, we found similar results.

We then examine whether firm owners have an incentive to grant Option (Hypothesis 5). Given managers' investment behavior under different contract configurations, owners of

Figure 2.3: Heterogeneous estimates of determinants on investment for asymmetric-cost sessions

		Two-segment		Three-segment		
		I	II	I	II	III
Summary Statistics						
Proportion of subjects		43.33%	56.67%	28.33%	56.67%	28.33%
Average investment		42.37	83.07	33.61	65.49	97.16
Median investment		39.50	97.75	25.00	68.75	100.00
Overinvestment frequency		35.00%	77.25%	23.53%	62.43%	89.02%
Frequency of investment = 0		6.67%	4.31%	7.84%	6.79%	0.59%
Frequency of investment < 10		22.69%	7.55%	30.00%	11.28%	2.54%
Frequency of investment = 100		5.51%	28.82%	3.53%	19.10%	33.33%
Frequency of investment >= 100		8.33%	48.43%	4.12%	31.15%	57.84%
	Prediction	*Regression*				
Cons. (Stock v Stock)	49.38	35.42***	64.91***	32.98***	44.83	83.37***
		(3.882)	(3.202)	(4.148)	(3.283)	(4.607)
Stock v Option	0.588	-5.345	-1.773	-5.209	-10.46***	5.208
		(4.581)	(4.416)	(6.127)	(3.601)	(7.020)
Option v Stock	7.92	10.62	28.85***	1.208	34.18***	20.09
		(7.000)	(5.593)	(7.114)	(5.939)	(9.264)
Option v Option	14.25	14.32	34.86***	6.332	43.60***	16.30
		(4.902)	(6.593)	(5.158)	(6.385)	(7.922)
Cost_dis	0.0173	-0.912	-1.538	-4.797	-0.865	1.174
		(4.956)	(4.655)	(6.535)	(4.205)	(6.887)
Cost_dis*Stock v Option	-1.888	-0.963	-6.280	-2.111	-0.490	-5.006
		(5.783)	(7.309)	(8.341)	(6.077)	(12.62)
Cost_dis*Option v Stock	8.281	10.18	8.758	13.03	11.40	4.135
		(8.399)	(6.018)	(11.18)	(6.346)	(8.478)
Cost_dis*Option v Option	5.954	12.66	14.31	8.168	16.09**	15.09*
		(5.863)	(5.144)	(8.746)	(4.744)	(7.158)
Log Likelihood		-8864.18			-8725.76	
AIC/BIC		17766.35/17870.76			17509.52/17668.89	
Observations		780	1020	510	780	510
	Prediction	*Investment under Different Configurations*				
Cost_adv + Stock v Stock	49.38	35.42***	64.91***	32.98***	44.83	83.37***
		(3.882)	(3.202)	(4.148)	(3.283)	(4.607)
Cost_adv + Stock v Option	49.97	30.07***	63.13***	27.77***	34.37***	88.57***
		(3.999)	(4.638)	(4.088)	(4.514)	(5.776)
Cost_adv + Option v Stock	57.30	46.04***	93.76***	34.19***	79.01***	103.45***
		(3.741)	(3.778)	(3.859)	(4.072)	(6.077)
Cost_adv + Option v Option	63.63	49.74***	99.77***	39.31***	88.43***	99.67***
		(2.758)	(4.397)	(3.628)	(4.094)	(5.346)
Cost_dis + Stock v Stock	49.40	34.51***	63.37***	28.18***	43.97	84.54***
		(2.543)	(4.373)	(3.202)	(3.349)	(7.070)
Cost_dis + Stock v Option	48.1	28.19***	55.32	20.86***	33.02***	84.74***
		(3.340)	(5.983)	(4.159)	(5.233)	(8.944)
Cost_dis + Option v Stock	65.6	55.30***	100.98***	42.42***	89.54***	108.77***
		(3.375)	(3.534)	(4.358)	(4.264)	(3.741)
Cost_dis + Option v Option	69.6	61.49*	112.54***	42.68***	103.65***	115.93***
		(4.490)	(4.886)	(5.065)	(4.115)	(6.796)

Note 1: Robust standard errors in parentheses

Note 2: + means we test the estimators against the predictions in the first column.
means we test the estimators against zero.

Note 3: The stars on the left of these coefficients indicate whether they are significantly different from the predictions or zero.
In particular, *** p<0.01, **p<0.05, * p<0.1

Table 2.9: Payoff matrices for firm owners based on theoretical prediction and empirical data

Prediction for symmetric-cost sessions

$c_2 = 1.00$

$c_1 = 1.00$		Stock	Option
	Stock	**50.0**, **50.0**	**39.6**, 49.4
	Option	49.4, **39.6**	33.3, 33.3

Empirical data for symmetric-cost sessions

$c_2 = 1.00$

$c_1 = 1.00$		Stock	Option
	Stock	41.9, 41.9	**27.2**, **54.6** ***
	Option	**54.6** *** , **27.2**	**26.5**, **26.5**

Prediction for asymmetric-cost sessions

$c_2 = 1.25$

$c_1 = 1.00$		Stock	Option
	Stock	61.7, **39.5**	**47.6**, 32.3
	Option	**62.4**, **36.9**	43.1, 23.8

Empirical data for asymmetric-cost sessions

$c_2 = 1.25$

$c_1 = 1.00$		Stock	Option
	Stock	56.1, **39.4**	**29.5** **, **39.0**
	Option	**66.1** *** , **17.9** ˆ***	23.9, 6.6

Note 1: The profit in this table are in thousands of ECUs.
Note 2: We make the best response in bold. The best response refers the strategy(s) which produces the most favorable outcome for a firm owner, taking other owner's strategy as given.
Note 3: For matrices based on the empirical data, we test firm owner's empirical expected profit of one strategy against that of the other strategy while fixing opponent's strategy. If we found one strategy will generate significantly higher expected profit, we would mark it with $*$, and $*p < 0.1, **p < 0.05, ***p < 0.01$.

the two competing firms are actually playing a 2×2 normal form game where they choose either Stock or Option as shown in Table 2.9. On the left side, the payoff matrices indicate what firm owners are expected to earn if their managers choose the risk-neutral equilibrium investment level. On the right side of Table 2.9, the payoff matrices are based on managers' empirical investment behavior observed in our experiment.

We first look at the symmetric-cost case. Theoretically, we found that granting Stock is the dominant strategy. Therefore, if managers are following the risk-neutral equilibrium, the use of Option cannot be justified in the symmetric-cost case. Our experiment data, however, show that managers deviate from the equilibrium leading to a justification of Option. In the empirical payoff matrix, given the opponent grants Stock, the firm owner is expected to earn significantly higher profit granting Option instead of Stock. When the opponent grants Option, the expected profit for the firm owner is not significantly different between Stock and Option. We believe, however, the firm owner is more likely to grant Option, because doing so lowers the opponent's profit significantly at almost no cost. There are three Nash equilibria based on the empirical data: (Stock, Option), (Option, Stock), and (Option, Option). The equilibrium most likely to be chosen by owners is (Option, Option), where they are expected to earn significantly less than if both keep to Stock. Therefore, contrary to the theoretical prediction, empirical data suggest firm owners are faced with a prisoner's dilemma situation. Either owner would most prefer that she uses Option while her rival does not. The owner would also use Option even if her rival uses Option to keep down the opponent's expected profit. This creates a prisoner's dilemma setting in which both firms acting rationally leads to a socially suboptimal outcome.

For the asymmetric-cost case, we run a similar analysis. Theoretically, Stock is the dominant strategy for the cost-disadvantaged firm owner, while the best response for the cost-advantaged firm owner depends on her opponent's action. If the opponent grants Stock, the cost-advantaged owner may grant Option even though the incentive to grant Stock rather than Option is very small. On the other hand, if the opponent grants Option, the cost-advantaged owner is expected to earn significantly more by granting Stock. Thus if managers are following the risk-neutral equilibrium strategies, the Nash equilibrium is that the cost-advantaged firm owner grants Option while the cost-disadvantaged firm owner chooses Stock. Based on the empirical data, however, we found that the cost-disadvantaged firm owner has the incentive to grant Option if her opponent chooses Stock. The cost-advantaged firm owner still has the same best response, but she has a stronger incentive to grant Option if her opponent grants Stock. Therefore, the empirical data show that firm owners are playing a Hawk-Dove game, where their optimal choice depends on what their opponents are doing. If their opponents grant Stock, they should grant Option. If their opponents grant Option, they should grant Stock.

We also examine firm owners' optimal choices if managers are of different types. The results are shown in Table 2.10. When managers are under-investors, granting Option is the dominant strategy for firm owners even for the cost-disadvantaged owner in the cost-asymmetric cases. When managers are over-investors, firm owners in the cost-symmetric cases still have an incentive to deviate from Stock to Option, while both cost-advantaged and cost-disadvantaged firm owners in the asymmetric-cost case have no incentive to deviate from the Nash equilibrium, Stock v Stock.

2.6. Conclusion

Executive stock options are widely used in practice, but previous literature fails to provide a compelling justification for their extensive use. One important element left out of previous work is how stock options might affect the nature of competition between firms. The competitive setting is important because executive stock options granted to a manager would affect not only the behavior of that manager but also that of the managers in rival firms. If rival managers are pushed to be less aggressive, we would expect that stock options are more effective than had the competitive effect not been considered. This paper investigated this issue both theoretically and experimentally, and provided a justification for the use of stock options.

We theoretically examined a two-stage game where firm owners write compensation contracts with the managers in the first stage, and then in the second stage managers decide how much firms will invest in a competition given both their own and opponents' incentive packages. If managers are following risk neutral equilibrium, the theoretical model suggests that firm owners have little incentive to grant stock options. But if managers deviate from the risk neutral equilibrium, then the optimal contract for firm owners should be based on the empirical investment chosen by managers.

We use laboratory experiments to investigate how executive stock options would affect managers' investment decisions in the competition, and then based on managers' empirical investment behavior we examine whether granting stock options is a profitable strategy for firm owners. When the marginal cost of investment is symmetric between the competing firms, owners have the incentive to deviate from stock to stock options given that their opponents are granting stock. Even when the opponents grant stock options, they still have the

Table 2.10: Payoff matrices for firm owners given managers' types

Under investors & Symmetric-cost

$c_2 = 1.00$ / $c_1 = 1.00$	Stock	Option
Stock	(82.6, 82.6)	(41.8, 106.8***)
Option	(106.8***,41.8)	(63.1*** ,63.1ˆ***)

Over investors & Symmetric-cost

$c_2 = 1.00$ / $c_1 = 1.00$	Stock	Option
Stock	(26.5, 26.5)	(23.2***,29.6)
Option	(29.6, 23.2***)	(10.0, 10.0)

Under investors & Asymmetric-cost

$c_2 = 1.25$ / $c_1 = 1.00$	Stock	Option
Stock	(77.0, 53.1)	(50.1, 63.8***)
Option	(88.2***,37.6)	(50.8, 38.0)

Over investors & Asymmetric-cost

$c_2 = 1.25$ / $c_1 = 1.00$	Stock	Option
Stock	(47.4***,24.3ˆ***)	(24.1***,11.3)
Option	(42.1, 8.8***)	(5.4, −17.7)

Note 1: The profit in this table are in thousands of ECUs.
Note 2: For matrices based on the empirical data, we test firm owner's empirical expected profit of one strategy against that of the other strategy while fixing opponent's strategy. If we found one strategy will generate significantly higher expected profit, we would mark it with *, and $*p < 0.1, **p < 0.05, ***p < 0.01$.

incentive to deviate from stock to stock options, as by doing so they can lower opponents' profit significantly at almost no cost. When the marginal investment cost is asymmetric between firms, there are usually two equilbria, in either of which one owner grants options while the other grants stock. Either the cost-advantaged firm owner or the cost-disadvantaged firm owner can be the one granting options. Therefore, the empirical data actually provide strong support for the extensive use of stock options. The divergence between the empirical data and the theoretical model is attributable to managers' overreaction compared to the risk-neutral equilibrium prediction. The theory suggests that a firm owner granting stock options decreases the investment of the opponent manager by a very small amount or increases it. The empirical data, however, show that the owner deviating to options decreases her opponent's investment more than the prediction given that her opponent is granting stock.

These experimental results provide a possible justification for the continued use of stock options despite their problematic nature. This leads to the finding that each firm would most prefer to use stock options while their rivals do not. Sometimes, firms would use stock options even if their rivals use stock options in order to keep down the expected profit of their opponent firms. Of particular interest is that, while individually each firm benefits from their use, it turns out that if both firms use them they are worse off than if neither did.

The managers' overly aggressive investment behavior induced by the executive stock option has negative impact on society. When the investment of the contestants does not add any social value, as in the case of advertisement, firm owners granting stock options waste social resources. Even when the investment benefits the society, as in the case of R&D expenditure, the excessive investment might push the marginal cost of the investment above its marginal benefit, and therefore lead to a socially suboptimal outcome. Furthermore, managers' overly aggressive actions increase the bankruptcy risk for their firms. Sometimes, it is the whole society that bears the cost of their bankruptcy. For example, in the 2008 financial crisis, several major financial institutions[19] either failed, or were subject to government takeover. The ramifications of the banking collapse of 2008 will be felt for years if not decades to come[20].

In our results we see an indication that the use of stock options may lead to a decrease in total social welfare. One possible implication is that policy makers should consider this potential effect when deciding policy related to the use of stock options. It may be a good idea to construct policies aimed at discouraging their use but at a minimum, policy maker might consider not enacting policy reforms which further encourage them. Some government policies have removed the advantage of stock options in recent years. For example, before 2004 the value of the option was only required to be disclosed in a footnote to the financial

[19] These financial institutions included Lehman Brothers, Merrill Lynch, Fannie Mae, Freddie Mac, Washington Mutual, Wachovia, Citigroup, and AIG.

[20] Total home equity in the United States, which was valued at $13 trillion at its peak in 2006, had dropped to $8.8 trillion by mid-2008 and was still falling in late 2008. Total retirement assets, Americans' second-largest household asset, dropped by 22%, from $10.3 trillion in 2006 to $8 trillion in mid-2008. During the same period, savings and investment assets (apart from retirement savings) lost $1.2 trillion and pension assets lost $1.3 trillion. Taken together, these losses total a staggering $8.3 trillion. Since peaking in the second quarter of 2007, household wealth is down $14 trillion. Source: https://en.wikipedia.org/wiki/Financial_crisis_of_2007%E2%80%932008.

statements allowing companies to essentially underreport executive compensation and make their potential profits look greater. In December 2004, Financial Accounting Standards Board (FASB) announced FAS123R, which required all U.S. firms to recognize an accounting expense when granting stock options. Our results also show that it is hard to stop the use of stock options once they are widely used. If a company pulls its own options back, it puts itself at a competitive disadvantage relative to its competitors, and no firm wants to be the first to do that. By 2000, stock options accounted for more than half of the total compensation for a typical S&P 500 CEO. In the late 2000s, tax and accounting rule changes removed the advantage for stock options, but stock options still comprise around one-quarter of the total value of executive pay packages. Therefore, the government should be cautious when they issue policies that would affect the use of executive stock options. Once they issue the policies that increase the use of stock options, it is hard to wipe out its impact in the future even if they abandon those policies.

CHAPTER 3

Ask Your Workers to Report Frequently, But Not Too Often

3.1. Introduction

It is common practice in many firms to have employees report to their supervisors on the status of current projects at specified intervals. For example, firms, including Adobe Systems, Accenture PLC, Deloitte, and General Electric, encourage their managers to check in with their employees every week or every other week.[1,2] Companies such as Goldman Sachs and J.P. Morgan Chase are rolling out a new report and review system where managers can request their workers to report as often as the manager desires and can send ongoing feedback to workers.[3] Many high-tech firms such as Microsoft, Uber, and Walmartlabs encourage their teams to have daily stand-up meetings. The exact content and point of some of these types of meetings will vary from firm to firm but a common theme running through the design of most of them is for the employees to report on what they have accomplished since the prior meeting. In prior decades, high-frequency reporting as we see these days would have been very costly and in some cases impossible but firms, like General Electric, IBM and Amazon, have developed computer and mobile applications that enable frequent conversations between managers and workers to make daily reporting possible and maybe manageable.[4] With the advent of these sorts of capabilities, there is a reason to investigate them to determine what impacts this degree of monitoring might have on employees and what element of the monitoring system is responsible for any improvement in employee performance.

[1] https://www.wsj.com/articles/the-never-ending-performance-review-1494322200

[2] https://www.wsj.com/articles/ge-re-engineers-performance-reviews-pay-practices-1465358463

[3] https://www.wsj.com/articles/goldman-goes-beyond-annual-review-with-real-time-employee-ratings-1492786653

[4] https://hbr.org/2016/10/the-performance-management-revolution

There are any number of possible uses for these meetings such as a manager passing along instructions to his or her workers about priorities and goals, workers seeking feedback on how best to go about a project and so on [40–42] but one of the primary uses for monitoring employees like this is to verify that the employees are working on appropriate tasks and putting in effort towards company goals. That is because these monitoring schemes are of course an attempt to solve the standard principal-agent problem in which the firm wishes to extract high effort from employees who would otherwise prefer shirking, i.e., not working on firm-related projects.

In many of these monitoring schemes used in practice, financial incentives may be only weakly involved. That is, while eventual promotion and raise decisions may arise out of a year worth of weekly or daily meetings, it is rare for each meeting to involve explicit financial consequences. In fact, much of the point of these meetings is that they are thought to activate non-financial drivers of effort. This leads to an important question of determining if it is possible that frequent monitoring could potentially affect employee effort even absent any financial consequences. This will be the issue examined in this paper. It's not due to a contention that financial incentives do not matter, but rather an interest in determining if monitoring alone can have a significant impact on behavior and then if so, what is the channel through which this effect occurs.

There is a great deal of prior literature that one can look to for insight on this issue. There are a number of different possible channels through which monitoring might increase worker effort but also some evidence suggesting that monitoring could be problematic. One possible explanation for how monitoring could improve worker effort is that there is substantial prior evidence that individuals have a strong desire to conform to expected behavioral norms [43, 44]. Frequent monitoring could be a way for a firm to remind workers of a "norm" in which the worker should be working for the firm. Frequent reminders about progress could make this norm more salient or just make sure workers fully understand their current productivity. There is also prior evidence suggesting that individuals like doing things that benefit others due to the fact that these actions can provide an individual "warm-glow" utility [45]. It is certainly possible that workers could possess such preferences towards their employer and again frequent reporting could remind an individual of their progress towards achieving such a goal. Either of these types of motivations would represent purely intrinsic motivations in which the individual seeks to satisfy their inner preferences regarding putting in a high effort.

It is alternatively possible that intrinsic motivation is not the channel through which reporting affects behavior but extrinsic motivation through how an individual perceives others to think about their actions. Previous studies have shown people care about their social image or how others view them, so they behave more prosocially when observed by other individuals [46–49]. This holds purely on the basis of observation in some cases, without requiring any feedback from the observer. Therefore, if workers are concerned about how they are perceived by their employers, they might increase their work effort simply due to the knowledge that their employer is shown information on their productivity. And again, not because they think this will lead to a future raise or monetary benefit but rather because they are concerned about how the boss will judge them as a person. Of course, pure observation may not be enough to alter behavior, feedback may be required. There are multiple prior papers showing that individuals respond to a situation knowing that not only will their behavior be observed by others, but those others will have the chance to provide feedback [48, 50, 51]. Note the important issue is the knowledge that feedback will be provided and the behavior can be changed simply on the basis of that rather than on the basis of what the actual feedback is. If this is an operative motivation in this worker context, then an employer providing frequent feedback could certainly boost productivity.

There are also indications in prior work that requiring workers to report frequently may be counterproductive as it could drive them to work less. There are studies which show that monitoring individuals can cause the person being monitored to see the monitoring as a form of distrust and this could in turn possibly diminishing their self-esteem [52–55]. Workers who feel distrusted by an employer may work with less effort rather than more. Also, frequent reporting may have an impact on what type of tasks a worker performs for the firm. If a worker has short-term or easy projects which they expect will return positive results quickly, they may be more likely to shift towards these tasks and away from ones which require more of a time investment before observing a success. This would be due to perceived pressure to produce positive results in relatively narrow time windows essentially de-incentivizing working on long-run projects which take longer to mature. There is prior evidence of similar behavior in other settings which make it seem plausible that such behavior could occur in the workplace [56–61].

The empirical question is whether or not any of these behavioral motivations apply in a worker setting and if so, how they balance out. We use controlled experiments to investigate these issues. In the experiment, we simulate the relationship between a worker and an employer. The worker is endowed with a certain amount of time to divide between working for the firm and earning utility only for themselves, that is, we fully implement a standard labor supply model. The worker can choose between two different firm tasks which model the types of tasks described above that either payoff quickly or only after more time is invested. In an important methodological innovation, we also include a real leisure option as described in [62] so that we know that our subjects value their outside option of not working on the firms' behalf. The value of this outside option starts out generating more utility to the worker than the firm tasks but declines with time spent on it which mimics the standard assumption of a declining marginal utility of leisure. This gives us an interior equilibrium in terms of effort provision. We will then have the worker report to their manager their progress with varying frequency and styles of reporting regimes. By having a real leisure outside option, we can measure how much utility a worker is willing to give up to expend additional effort on the behalf of the employer under these different conditions.

The experiment is designed to allow us to identify which of the previously described behavioral motivations could be affecting worker effort. We have one regime in which the worker is reminded of their recent productivity knowing that the employer will not see it. A second treatment involves the employee seeing their productivity and knowing that it will be shown to the employer though with the employer having no opportunity to respond. Then we have a full reporting regime in which the employers observe and can send feedback regarding their level of satisfaction. We compare the behavior observed in these treatments to a baseline with no reporting. By examining how time spent on firm tasks changes between treatments, we can identify if intrinsic motivation, observation or feedback is sufficient to improve effort provision. In each of these treatments, we vary the frequency of reporting to determine if that can impact behavior. Finally, given the multiple tasks, the workers can choose, we can also observe whether the type of monitoring mechanism or the reporting frequency affects the types of tasks the worker chooses to work on.

We find that increasing the frequency of reporting can increase worker effort on firm tasks but this can come at a cost. When set too high, workers shift to spending more time on the task which generates immediate payoffs despite the fact that this task yields lower expected earnings relative to the task which requires more time investment. While there is substantial literature suggesting that the level of observability of employee's actions should impact their behavior, we find a limited impact of differing levels of observation. In the conclusion, we discuss why this might be the case and discuss the possibility of future research to better identify the situations in which observability does and does not have strong impacts on behavior.

3.2. Experiment Design

The focus of this study is on determining what element of a worker reporting their progress to a supervisor can lead to increased work activity and whether the frequency with which these reports are made can impact worker effort. As described before, there are many indications from prior literature about how reporting mechanisms could lead to increased work activity but there are also some indications that the policy may be counter productive. Our goal is to design an experiment which will enable us to examine all of these issues.

In the experiment, we simulate the relationship between a worker and an employer. The worker will essentially be making labor supply decisions in which they can choose how to allocate their time between work and leisure so as to maximize their utility. Our experiment will allow the worker to choose between spending their time on multiple work related tasks and a real leisure option so that it is clear that the worker receives positive utility from that leisure choice. The choice environment is designed such that we expect the worker to prefer to devote most of their time to leisure and that when choosing to work for the firm, the worker should spend all of that time on a task that requires substantial time commitment to generate a positive outcome. We will then conduct multiple treatments in which we vary the manner in which a worker will report their earnings on firm tasks to their employer and we will vary how frequently these reports are made. This will allow us to identify any changes in worker behavior due to frequency of reporting or to the nature of the reports.

3.2.1. Tasks

As in a standard labor supply model, our workers are endowed with a fixed time budget which they can choose to allocate between multiple options. In our experiment the time endowment is 300 seconds. The workers will experience this as actual time so they will spend 300 seconds on trials of the different tasks with the ability to switch between tasks as often as they like. Each trial of a task that a worker engages in has the possibility to result in a successful outcome or in a failure. The probability of a worker experiencing a success depends on how much time allocated to that specific trial with the probability of success increasing in the amount of time spent on a trial. The specific functional form for this success function is $prob = 1 - e^{-\lambda t^2}$, where t refers to the amount of time spent on the trial and λ captures the curvature of the curve.[5] We use different values of λ for different tasks as shown in Table 3.1 and Figure 3.1. The worker will choose how long to spend on a trial, stop that trial and find out the outcome and then start a new trial of whatever task they like until their 300 seconds expires.

The worker has three available tasks; the Firm Challenging Task, the Firm Standard Task and the Own Standard Task. The first is designed to mimic a task which has high value to a firm but which can involve substantial time investment to generate a successful outcome. The Firm Standard task is a more mundane project with middling rewards but that can generate successes in relatively short amount of time. The Own Standard Task is the real leisure option. As seen in Table 3.1, the Firm Challenging tasks pays 30 ECUs per success to both the Employer and the worker while the Firm Standard Task only pays 10. By examining Figure 3.1, you can see that the probability of success in the Challenging task is lower for any time expenditure than the Standard task, or rather to achieve any particular probability of success, more time is required in the Challenging task than the Standard task. These two tasks have been specifically constructed such that both the Worker and the Employer should prefer the worker choosing to work on the Challenging task.

The success function for the Own Standard Task is the same as the Firm Standard Task, simply to make the decision problem easier for the subjects. The earnings per success of the Own task are, however, not constant as they are for the Firm tasks. In the Own task, the earnings per success are decreasing with each success achieved. This function is shown in Figure 3.1. This element is included to satisfy the standard assumption in a labor supply model that there are diminishing returns from leisure and to guarantee an interior solution for how much time the worker should allocate to his or her various tasks.

[5]The probability curve is convex when the amount of time is small and then concave as more time spent. This ensures the existence of an interior solution for the optimal amount of time on a trial.

Table 3.1: Success function and earnings per success for each task

	Success Function	Earnings per Success	
	$1 - e^{-\lambda_i t^2}$	Employer	Worker
Firm Challenging Task	$\lambda_{fc} = 1.395 * 10^{-3}$	30	30
Firm Standard Task	$\lambda_{fs} = 5.582 * 10^{-3}$	10	10
Own Standard Task	$\lambda_{fs} = 5.582 * 10^{-3}$	0	$w_{os}(n_{os})$

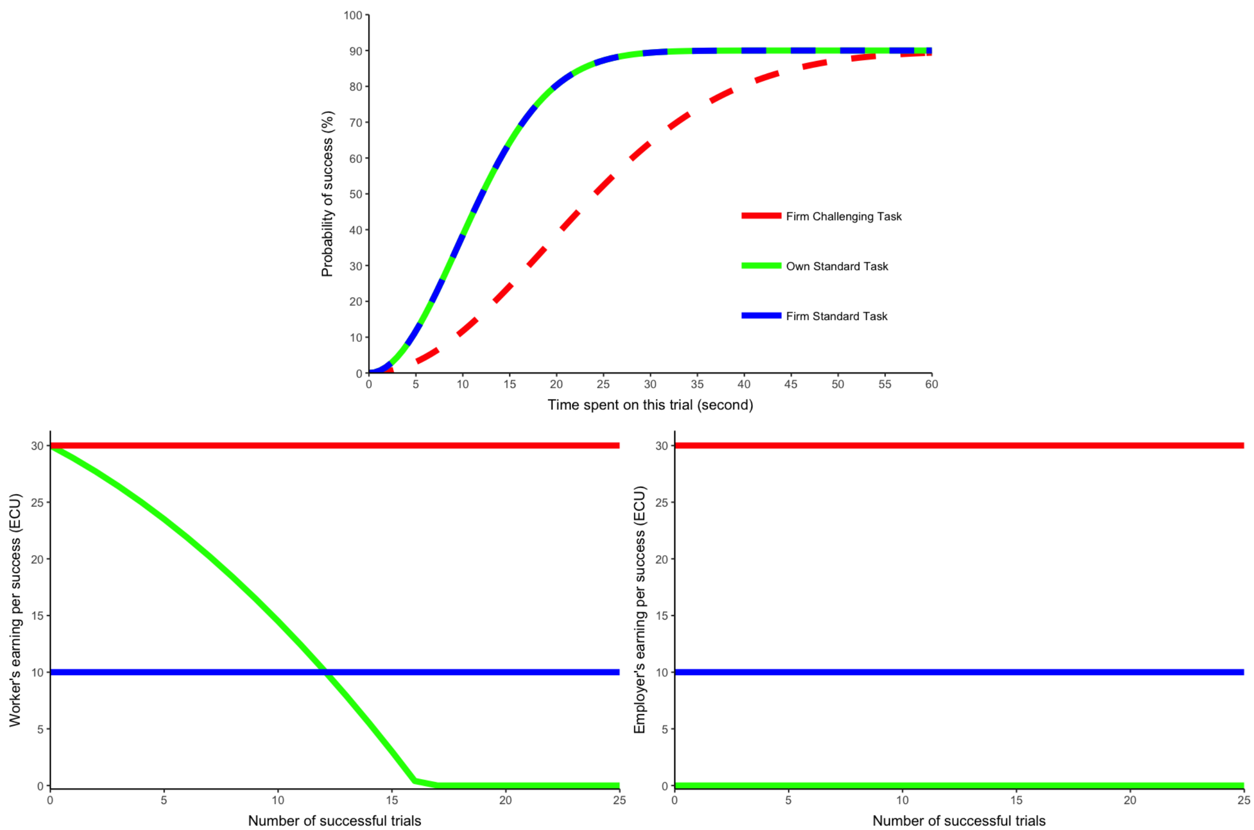

Figure 3.1: The Difference among Workers' Available Tasks

To see how a worker should allocate time between tasks, we can look at Figure 3.2. The first step in determining optimal allocation of time between tasks requires determining optimal time spent on each task per trial, since for any given amount of time on a task they need to maximize its expected earnings per second. For each trial of either the Own Standard Task or the Firm Standard task, the worker should spend 15 seconds. For each trial of the Firm Challenging task, the worker should spend 30 seconds, or double the amount of time as the Standard task. In all cases, this works out that the Worker will have a 64.37% chance of success for each trial regardless of which task it is from.

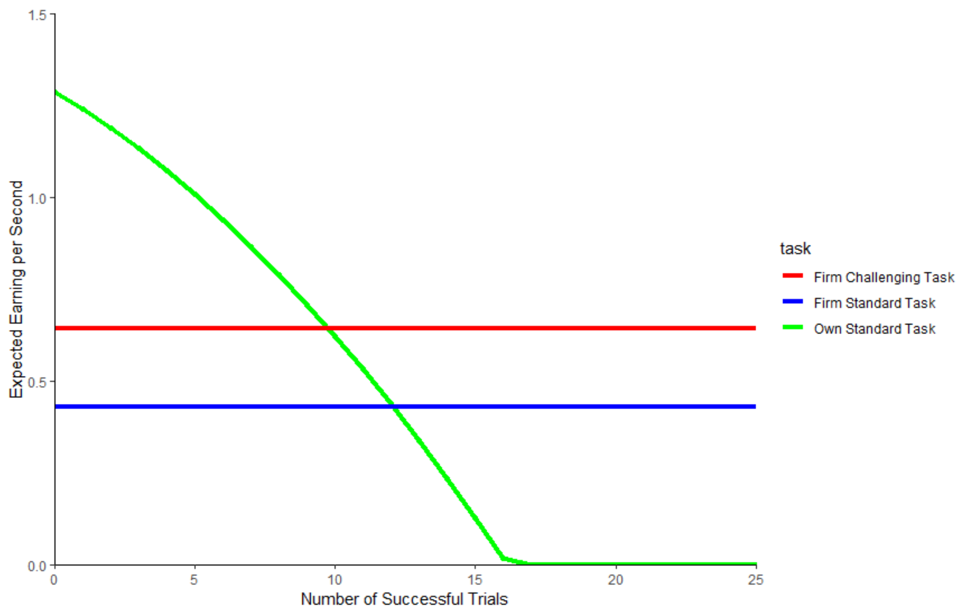

Figure 3.2: The worker's expected earnings per second for different tasks

Given optimal time spent per trial, it becomes easy to see why the Firm Challenging Task dominates the Firm Standard Task. The Firm Challenging Task pays off three times the rate of the Standard Task but only requires double the time investment. As Figure 3.2 shows, this leads to the expected earnings per successful trial of the Firm Challenging task always dominating the expected earnings from the Firm Standard Task. For the Own Standard task, the initial per success earnings are the same as the Firm Challenging task but require half of the time investment meaning that it is clearly optimal to begin on Own tasks. With cumulative successes, the expected earnings from the own task drops off until the expected earnings are equal for the Own Standard task and the Firm Challenging task at 10 successful trials of the Own Standard task. After a worker has accumulated 10 successful trials in the Own Standard task, they should clearly devote any remaining time to the Firm Challenging Task. Following this strategy, a worker is expected to spend 233 out of 300 seconds on the Own Standard Task with the remaining 77 seconds being spent on the Firm Challenging task. This should generate an expected 281.61 ECUs to the worker and 43.11 ECUs to his employer.

This baseline theoretical prediction suggests that a standard worker should spend most of their time on their Own task leaving little time for work on tasks for the employer. The preferences of the employer are of course that the Worker would spend more time on the Firm Challenging task. Thus we have the tension faced in a normal workplace and can investigate how an employer might go about increasing the time the worker spends on the Firm Challenging Task. As noted, we will specifically investigate using different treatments whether asking workers to report their earnings for the Firm can do that and if so what is the effect of different reporting frequencies and what behavioral channel is responsible for the affect.

3.2.2. Procedure

We have two phases in our experiment. The second phase is designed to investigate how the reports would affect workers' time allocation among these different tasks. However, all experiment sessions begin with an identical initial phase to get both workers and employers familiar with the tasks and to make sure that they understand the difference across the tasks. Prior to phase one, subjects are given complete instructions about it and are told that there would be a second phase and they would be given instructions for it after the first phase is complete. Their earnings from the experiment will be equal to the sum of the earnings from both phases.

3.2.2.1. Phase One

There is no role assignment in the first phase, and all the subjects are treated identically. They act independently, and their actions only affect their own earnings. There are two rounds in the first phase and in each phase, a subject will engage in trials for a single type of task without the ability to choose among multiple tasks. In one round, the subjects will work on trials of the Challenging Task and the other round they will work on the Standard task with the order of the two randomized for each subject. These two tasks are the same as the firm tasks described before except they will only generate earnings to the subject as there is no employer in this phase. When a subject works on the Challenging Task and gets a success, she receives 30 ECUs; when she works on the Standard Task and gets a success, she receives 10 ECUs. In each round, subjects are given 300 seconds to be spent on trials of the assigned task.

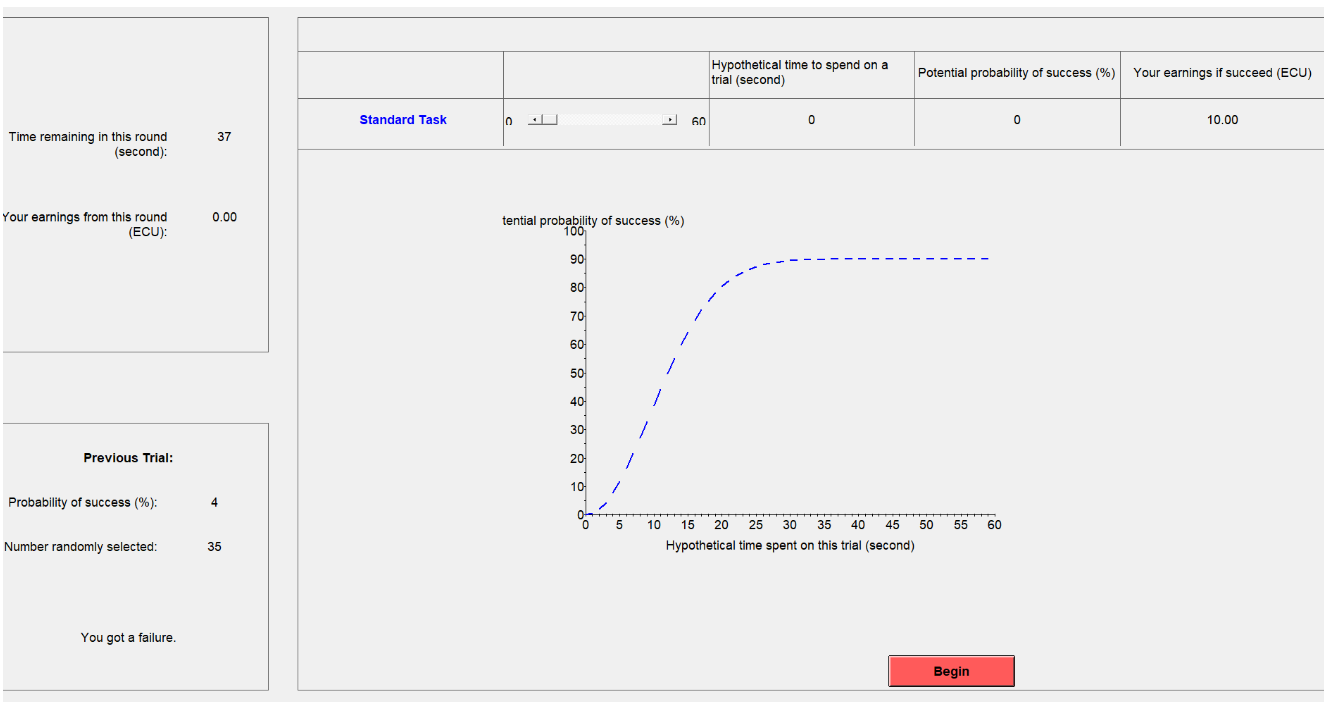

Figure 3.3: The screen worker a trial of the Standard Task

In Figure 3.3, we have shown the sample screen where the subject begins a trial. Before the subject starts a trial, she can explore its details on this screen. The success function for that task is shown in the middle. A slider in the table on the top right allows the subject to see how increasing or decreasing the hypothetical amount of time she might spend on the trial will affect her probability of success. As she moves the slider bar, there will be a dot tracking the curve. The subject can start a trial by clicking “Begin”. Then, she will be on another screen where she will spend actual time on the trial and her success probability is tracked along the success function as time progresses. She can submit the trial when she has achieved the success probability she desires. After the submission, the round timer will be paused and the outcome of the trial will be determined. She will be back on the screen in Figure 3.3 to start another trial. She will continue making these decisions until she uses up all 300 seconds for that round.

There are several purposes for this phase. Firstly, this phase gives subjects time to understand how much time they need to spend on a trial of a task to get certain chance of success and what earnings they would receive if they get a success. Secondly, the experience with the two tasks in this phase helps subjects understand the difference among the tasks that will be available to workers in the second phase. Thirdly, this phase would help employers in the second phase form reasonable expectations on the earnings their workers can generate to them. The reason that this is important will become clear as we explain the reporting treatments in phase 2.

3.2.2.2. Phase Two

In the second phase of the experiment, we assign half of the participants in an experiment session a role as an employer and the other half a role as a worker. Their roles are assigned at the beginning of the second phase and stay fixed throughout the second phase. There are four rounds in the second phase. At the beginning of each round, we randomly match every worker with an employer to establish the employment relationship. Every employer is required to pay a 10 ECU salary to her worker. We use this salary payment as a way of setting up the employment situation and to potentially activate a norm in which the worker should be working on behalf of their employer. Workers are re-matched to a new employer in every round. The only interaction between subjects in this phase is between each worker-employer pair.

Workers are endowed with 300 seconds (5 minutes) in each round to spend on the three available tasks, the Firm Challenging Task, the Firm Standard Task and the Own Standard Task. In the instructions for this phase, we explain the differences between these tasks to the subjects by showing Figure 3.1 on their computer screen and explaining the characteristics of each task. As they have just spent phase 1 performing trials of the two Firm tasks, the main elements to explain to them are the Own Standard task and how they will be able to choose among tasks.

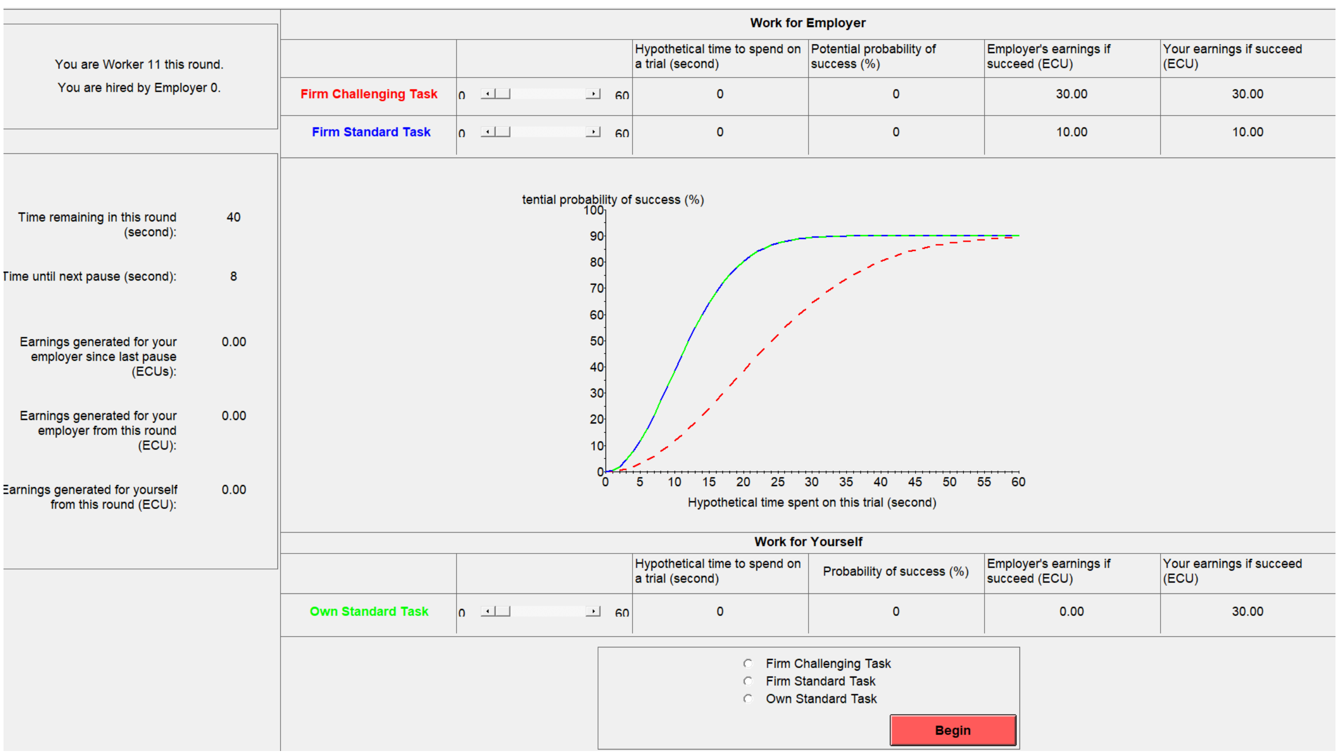

Figure 3.4: The screen worker chooses the task and starts a trial of the chosen task

In Figure 3.4, we have shown a sample of the screen where a worker will choose between the three available tasks and then start a trial of the chosen task. The graph and tables on the right of the screen can help the worker examine the potential earnings and probability of success of a trial for each of the three tasks. In those tables, the worker can see not only his earnings from a success but also his employer's earnings. The last column of the earnings table shows how much the worker can earn from the next success of the Own Standard task given how many successes he already achieved from that task so far in the current round. This is the key information the worker needs in making his choice among tasks. The worker can then choose a task and start a trial of the chosen task on the bottom of the screen. He will move to the screen where he spends time on the trial, which will be a screen almost identical to the one used in the first phase. As before, a worker can wait until the success probability hits the desired level and submit the trial. After the submission, he will be back on the screen in Figure 3.4 to observe the outcome and then choose a task for which to start another trial. He will continue making these decisions until he uses up all 300 seconds for that round.

Employers will not be sitting idly but instead they will also be working on a similar basis. We have them work on two tasks, known as Employer Challenging Task and Employer Standard Task. These two tasks have the same success functions as the tasks the worker will engage with but they generate much lower earnings. Employers only receive 3 ECUs per success from the Employer Challenging Task and 1 ECU from the Employer Standard Task. Workers receive no earnings from the success of their employer. The point of this activity is simply to give the employer some activity to engage in to prevent boredom and to prevent workers from identifying who in the room is an employer. Given that they will not generate much in earnings from their own work, this also makes it clear that the employers are highly dependent on the workers to generate their earnings. Employers are not allowed to choose between the tasks but rather which task they work on is randomly determined after each trial they submit.

3.2.3. Treatments

The first intervention we perform to determine its impact on worker effort is that we have the workers report on the earnings they have generated for the firm since their last report. To examine how the frequency of reporting affects behavior, we vary the frequency of reporting across rounds. Workers will be asked to report 1, 2, 5 or 10 times in a round. The ordering of these four reporting frequencies across the four production rounds is randomized across workers. A worker reporting once, implies that they only report at the end of the production round. If a worker is asked to report twice, then he will report after he has spent 150 seconds working and then report again after he has spent 300 seconds. The time spent on the reports is excluded from the 300 seconds. This removes the penalty to productivity that would come from extra time spent on the reporting itself. We do not believe such a reporting cost to be negligible in actual workplace settings, rather we wish to remove it here just to focus on the marginal effects of the reporting itself. The only information employers observe about worker productivity comes through this reporting mechanism.

In the introduction we explained that the expected impact of reporting frequency on employee effort is ambiguous based on prior research. Firms obviously use this reporting mechanism as an attempt to increase productivity but we noted several prior studies which cast doubt on whether that would occur. In particular, there is evidence that frequent monitoring could be seen as distrustful behavior by an employer which could de-motivate a worker. Further, if asked to report too frequently, this could shift behavior towards the Firm Standard task. If you consider the case of reporting 10 times, this means reporting every 30 seconds. That allows for only one trial of the challenging task but two from the standard task. This means that the probability of achieving a success between reports is quite low for the challenging task. A worker concerned about reporting a lack of results may well choose to spend time on the standard task as reporting windows narrow to increase the probability of achieving at least some positive results between reports. These claims form the basis for our first two hypotheses.

Hypothesis 6 ***(Frequency - Total Effect)*** *Increased frequency of reports will lead to an increase in the amount of time spent on the firm tasks.*

Hypothesis 7 ***(Frequency - Substitution Effect)*** *Increased frequency of reports will lead to a worker substituting trials of the standard task in place of the challenging task.*

The next issue we wish to investigate is what aspect of the reporting mechanism drives any increase in effort. While one might expect that having workers report and receive feedback would achieve an increase in worker effort, it isn't clear what aspect of the process is responsible for such a reaction. It is possible that the worker possesses intrinsic motivation for effort and simply reminding them of their production so far would spur them on to greater work effort. On the other hand it is possible that an individual only has such concerns when they know the affected party can observe what they have done and so perhaps simply knowing that the employer will view the results will lead to a productivity increase. Or, perhaps the only way to generate a response is for the worker to know that the employer will not only observe but also be able to provide feedback. We will construct four different reporting mechanisms that will allow us to differentiate between these channels. Each session of the experiment will use only one of these reporting treatments and it will hold for all of phase 2.

1. *Baseline:* Workers do not report. This is the baseline of the experiment. As a control for the other treatments, workers and employers are asked to pause for several seconds 1, 2, 5 or 10 times per round. The number of seconds of the pause is determined by the average time taken in the reporting stage in the Feedback treatment which is the longest among the three treatment. These pauses are included to help identify that any effort change observed between this treatment and the others is due to the reporting element and not the interruption in work activity caused by the reporting.

2. *Intrinsic Motivation:* Workers are required to acknowledge the earnings they have generated for their employers, but their employers do not see any information about

worker productivity. Workers are told this explicitly that while they will see reports on their productivity, their employer will not. Consequently, all the reporting element of this treatment consists of is to remind a worker how much money they have generated for their employer over the previous reporting period.

3. *Observability:* Workers are required to report the earnings they have generated for their employers, and these reports will be sent to and seen by the employers. Again, it is clearly explained to workers that when they send in an earnings report, their employer will see it but the employer has no way of communicating with the worker or responding to the report. This reporting mechanism achieves common knowledge between worker and employer regarding the employer earnings generated by the worker meaning the worker can expect that their employer may be judging them, but that judgment cannot be communicated to the worker. This element indicates why it was necessary for employers to participate in phase 1 as doing so should allow them to get some idea of how much earnings it might be reasonable or possible for the worker to produce for the employer.

4. *Feedback:* This is the full reporting regime in which workers send in their earnings to the employer and the employer can respond with feedback. For feedback, the employer can choose from a scale of 1 to 7 to express their level of satisfaction with the achieved earnings. On this scale, 1 represents strongly dissatisfied while 7 means strongly satisfied. In this treatment, workers might care about not only their employers' judgment but also the feedback sent by employers.

Each of these treatments was designed to test why having employees report might affect their productivity. We have previously discussed the possibility that individuals could possess intrinsic motivation, could be concerned about judgment or that they might be concerned about potentially negative feedback. Of course it is also possible that individuals are concerned about all three and each of these effects could add on to each other. By conducting the baseline and then these three other treatments, we can separate between these issues. This leads to our next three hypotheses.

Hypothesis 8 ***(Intrinsic Motivation)*** *Having workers acknowledge the earnings generated to their employers will increase the time spent on firm tasks compared to when they are not asked to acknowledge those earnings.*

Hypothesis 9 ***(Observability)*** *Having workers report the earnings generated to their employers and know that the report will be seen by their employers will increase time spent on the firm tasks compared to the non-reporting case.*

Hypothesis 10 ***(Feedback)****: Having workers report the earnings generated to their employers and know that the report will be seen and commented on by their employers will increase time spent on the firm tasks compared to the non-reporting case.*

3.2.4. Implementation

All of our experiment sessions were conducted at Southern Methodist University. Subjects were recruited from a university-wide subject pool using a computerized recruitment system based on h-root [63]. The pool consists of a mix undergraduate and graduate students who had indicated a willingness to be paid volunteers in decision-making experiments. The interactive software system is programmed using z-Tree [27]. Subjects' final payoff is equal to a $10 show-up fee plus the sum of earnings from both phases. For each phase, we randomly select one round for payment. We translate ECUs into dollars at the rate of 20.00 ECUs = $1.00. Payments including the show-up fee ranged from a minimum of $15.59 to a maximum of around $39.2, with an average of $26.0. Each session lasted an hour and a half to two hours.

Table 3.2: Experimental Design and Data Points

	Sessions	Subjects
Baseline	2	22
Intrinsic Motivation	3	36
Observability	3	32
Feedback	3	32

3.3. Experiment Results

We will begin presenting the results of the experiments by providing a set of summary statistics to provide an overview of the data. We will not conduct tests on these simple summary statistics as these tests are mis-specified given the nature of the data. Formal tests of the hypotheses will be conducted using properly specified regressions in the next section but having an understanding of these summary statistics can be helpful in properly interpreting the regressions.

An initial question to examine in the data is the degree to which subjects could solve the basic problem of figuring out how much time to spend on a trial of each task. Table 3.3 shows the average time the subjects spend on a trial by type of task in both phases of the experiment and indeed on average the subjects spent almost the exact optimal amount of time per trial or 15 seconds per trial on the standard task and 30 seconds per trial on the challenging task. Of course, while on average they chose correctly, this doesn't mean all choices were exactly at the optimal. Figure 3.5 and Figure 3.6 show the density plots of time spent on trials to reveal the full distribution. These again show that the mean is very close to the prediction with roughly normal distributions around that mean. Establishing that subjects on average figured out the correct amount of time to spend per trial is useful to make it clear that they understood the relevant incentives and the differences between the tasks. One point to note is that the largest errors as indicated by the plots are for those individuals choosing the standard task in Phase 2. For this group, the average is a little under the optimal choice and there is a fair amount of spread to the choices. Since these subjects are already making a mistake by choosing this task, it stands to reason that they would also make the most mistakes in regard to time spent per trial.

Table 3.3: Average time spent on a trial by task in both phases.

	Phase One	Phase Two	
		Employer	Worker
Challenging Task	26.92	25.21	29.82
Standard Task	15.95	14.56	15.94
Own Standard Task	-	-	16.67

Notes: standard errors clustered at the individual subject level. p-value in parentheses.
*** $p < 0.01$. ** $p < 0.05$. * $p < 0.1$.

One of the main research questions of the paper is the degree to which different reporting frequencies might impact behavior and the summary statistics breaking down work time allocation by frequency is shown in Table 3.4. Table 3.5 then shows similar summary statistics by reporting treatment. As a reminder, the worker is endowed with 300 seconds to be divided among three available tasks. If the worker only cares about his own earnings, he is expected to spend only 67 seconds on the Firm Challenging Task with the rest of the time, 233 seconds, spent on the Own Standard Task. A worker should spend no time on the Firm Standard Task. From these tables, we found the workers on average spend more than 150 seconds on the two firm tasks combined, which is a bit more than twice prediction. This could indicate a willingness on the part of the workers to sacrifice their own welfare to generate earnings for the employer. We also find that while they should spend no time on the Firm Standard Task, workers actually spend around 10% of their time on this task despite it being dominated by the Challenging Task with respect to earnings to the worker and the employer. We will examine possible reasons for this behavior in the next section.

If we look at how worker behavior varies with the reporting frequency, on average there seems to be an increase in time spent on firm tasks as frequency rises up to a peak at the 5

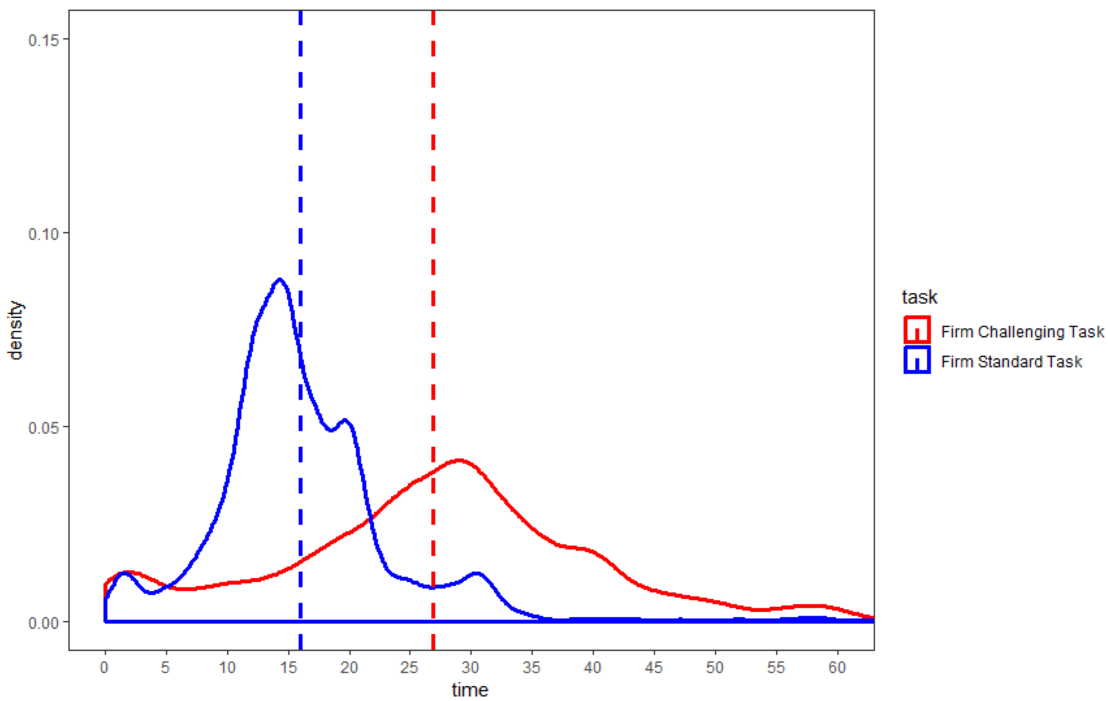

Figure 3.5: Average time spent on a trial for the two tasks in phase one

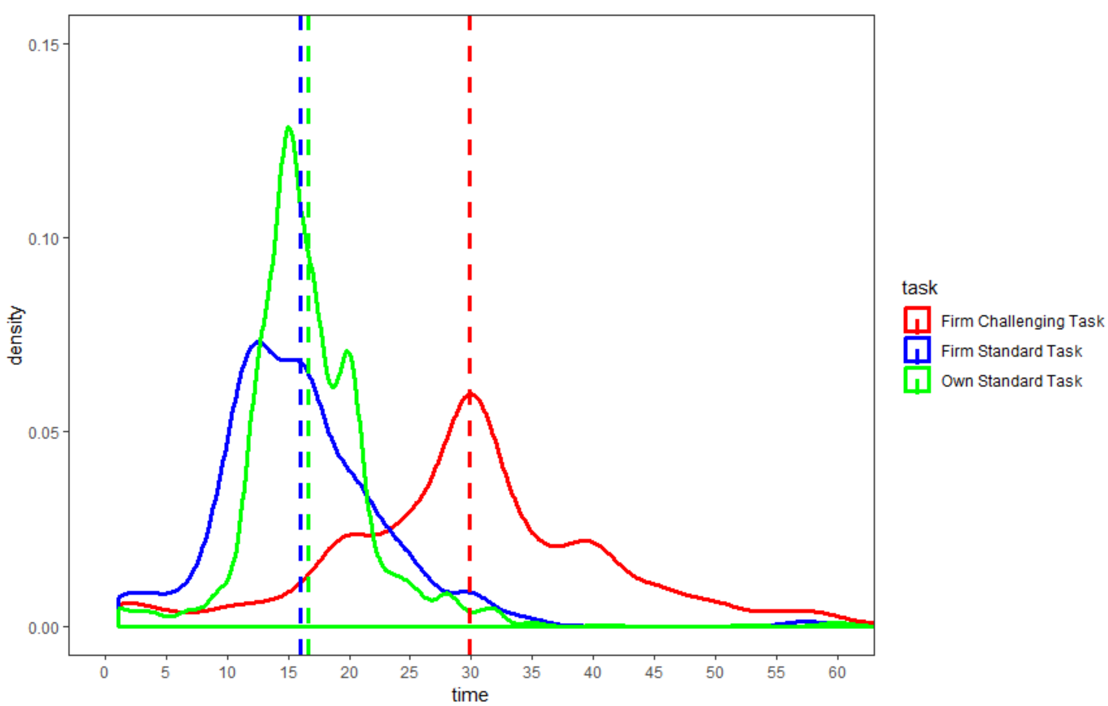

Figure 3.6: Average time spent on a trial for the workers' three tasks in phase two

Table 3.4: Average time allocation of workers by frequency.

	Firm	Firm Chal	Firm Std
Freq = 1	171.52	154.56	16.97
Freq = 2	182.38	159.92	22.36
Freq = 5	196.61	175.38	21.23
Freq = 10	184.41	148.07	36.34

Notes: Firm refers the total time spent on the two firm tasks. Firm Chal refers the Firm Challenging Task. Firm Std refers to the Firm Standard Task. Own Std refers to the Own Standard Task.

report frequency. Going all the way up to 10 reports seems to pull the total back down while also leading to a substantial increase in time spent on the Firm Standard task. Looking next at the data broekn down by reporting treatment shows that there could be an effect from the Feedback treatment of increasing total time spent on firm tasks relative to the no reporting treatment but neither the Intrinsic Motivation or Observability treatments seems to have improved worker effort by much. This is quite surprising given all the prior work demonstrating that in other contexts, interventions like this seem to have been enough to shift behavior.

Figure 3.7 shows another look at this data as it presents the time allocation split out by both reporting treatment and by reporting frequency. Again, it looks like the most substantial productivity improvement was for the 5 report regime in the Feedback treatment and

Table 3.5: Average time allocation of workers by treatment.

	Firm	Firm Chal	Firm Std
Baseline	175.31	148.40	26.92
Intrinsic Motivation	188.71	166.89	21.82
Observability	168.74	151.31	17.33
Feedback	197.33	158.25	39.08

Notes: Firm refers the total time spent on the two firm tasks. Firm Chal refers the Firm Challenging Task. Firm Std refers to the Firm Standard Task. Own Std refers to the Own Standard Task.

there is a general trend in all reporting treatments of moderate productivity improvements up to the 5 report condition followed by a decline in the 10 report condition.

3.3.1. Effect of Reporting Frequency on Time Allocation

We will now present a series of regressions to examine each of our hypotheses in order beginning with the hypotheses concerning the effect of changing the frequency of reporting. For an initial test, we will examine how reporting frequency might affect behavior differently between the baseline treatment without reporting and all of the reporting treatments pooled together. Table 3.6 contains a set of random effects panel regressions with standard errors clustered at the subject level to examine how worker behavior might vary with the frequency of reports. We examine in one specification their choice of total time on firm tasks and then we break that out into two other specifications for the time spent on the challenging task and time spent on the standard task. These regressions provide support for our first result.

Result 1 ***(Frequency Total Effect)*** *Increasing the frequency of reports generates a statistically significant increase in the amount of time spent on firm tasks with a maximum effect at the reporting frequency of 5.*

As Table 3.6 shows, for the non-baseline treatments or the treatments where subjects are actually reporting to one degree or another, we find that all of the indicator variables for each frequency of reporting to be at least marginally significant in the regression examining total time spent on the firm tasks. The coefficient on the 5 report condition is the largest. The χ^2 tests show that this coefficient is significantly different than the coefficients on the 2 report condition($Prob > \chi^2 = 0.0562$) while not significantly different from the 10 report condition($Prob > \chi^2 = 0.3722$). Thus productivity does increase as hypothesized though there does not seem to a be positive effect at very high levels of reporting

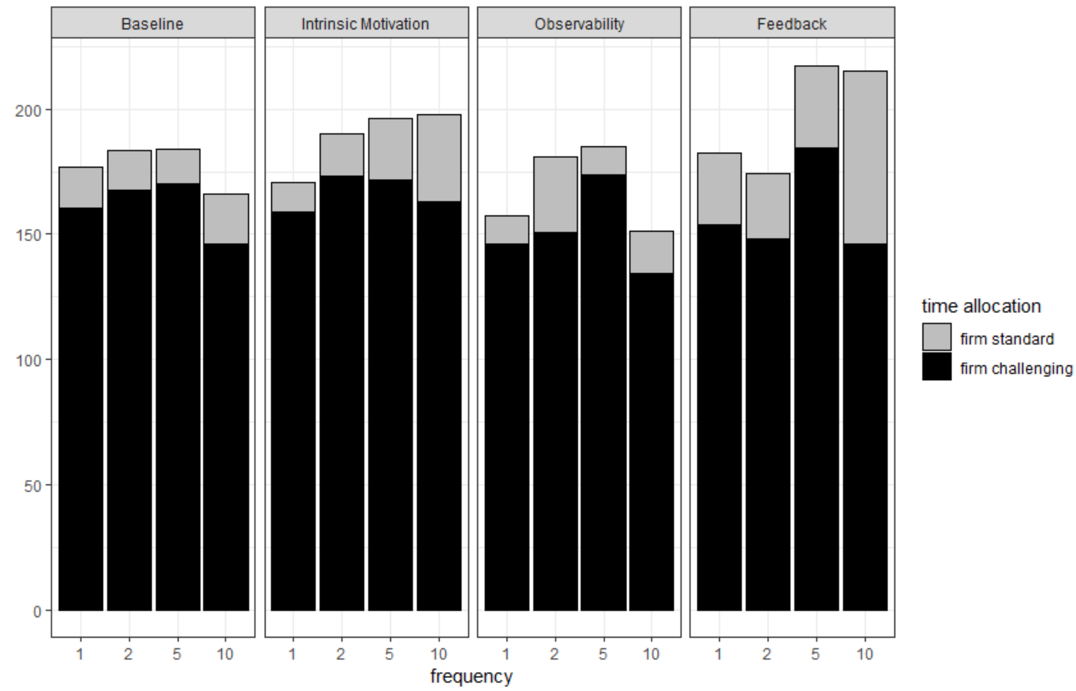

Figure 3.7: Time allocation for different settings and different frequencies

Table 3.6: Test how the frequency of reports/pauses affect workers' time allocation in the **Baseline** and **Non-Baseline Treatments**.

	Baseline			Non-Baseline		
	Firm	Firm Chal	Firm Std	Firm	Firm Chal	Firm Std
Cons.(Freq = 1)	227.5***	182.9***	44.5***	200.6***	161.7***	38.8***
	(0.000)	(0.000)	(0.009)	(0.000)	(0.000)	(0.000)
Freq = 2	9.90	8.96	0.95	12.2*	5.04	7.14
	(0.302)	(0.437)	(0.908)	(0.072)	(0.541)	(0.176)
Freq = 5	-5.02	4.38	-9.40	29.6***	23.4*	6.20
	(0.791)	(0.818)	(0.104)	(0.009)	(0.051)	(0.212)
Freq = 10	-19.6	-18.2	-1.36	20.2*	-4.19	24.3**
	(0.377)	(0.409)	(0.892)	(0.071)	(0.721)	(0.016)
Round	-18.5*	-8.27	-10.2**	-12.4***	-3.46	-8.94***
	(0.058)	(0.323)	(0.013)	(0.000)	(0.421)	(0.002)
N	44	44	44	200	200	200

Notes: standard errors clustered at the individual subject level. p-value in parentheses.
*** $p < 0.01$. ** $p < 0.05$. * $p < 0.1$.

Of course it is possible that this effect is simply due to subjects being asked to pause during their production phase. This possibility can be eliminated by examining the results of this same regression on the Baseline data. In that regression we find the indicator variables for frequency of pauses to be insignificant. Thus it is clear that it is some aspect of the monitoring combined with the increased reporting frequency that leads to the performance improvement. What is not clear though is which aspect of reporting is required to generate the effect.

Our next hypothesis deals with a possible negative aspect of frequent reporting and that is the possibility that the worker might substitute away from the more lucrative challenging task for the standard task which is more likely to give them positive results that they can include in their reports over short time intervals. Our next result finds in favor of this hypothesis.

Result 2 ***(Frequency - Substitution Effect)*** *Increasing reporting frequency past 5 reports leads to workers decreasing their time spent on the Firm Challenging Task while increasing the time spent on the Firm Standard Task indicating that at high frequencies they are substituting time spent on the Challenging Task for time spent on the Standard Task.*

To examine this we can look at the fifth and sixth columns of Table 3.6 showing how the time spent on the two types of tasks changes as reporting frequency increases. The key is that at a reporting frequency of 5, the coefficient on the challenging task is large and significant while the coefficient on the 10 report period is actually negative though insignificant. For the standard task regression, we find that this relationship is essentially reversed with the coefficient on the 5 report condition being insignificant but the coefficient on the 10 report condition is large and significant. It is in fact essentially the same as the coefficient as the one for the 5 report condition on the challenging task regression. This shows clear evidence that between the 5 and 10 report conditions, the workers are substituting their effort towards the standard task and away from the challenging task as hypothesized.

Result 1 establishes that increased frequency of reporting can increase effort on firm tasks but it does not tell us which reporting regime or regimes are necessary to deliver that effect. Table 3.7, provides similar regressions on worker time allocation but this time all of the reporting treatments are considered separately to identify which of them deliver the effect. What we find is that each treatment is delivering behavior shifts in the manner we identified in the pooled data but due to substantial variance in the data, few of these effects manage to obtain significance. The effects are closest to significance in the Observability and Feedback treatments, but even there p-values are in the 10-20 percent range. The indication here is that we are finding relatively weak effects in each treatment which only pass the significance test in the pooled data. This is an interesting finding to which we will return later.

An important consequence of the workers choosing to spend more time on firm tasks as reporting frequency rises is that they should expect to make more money for the employer but less for themselves. We can examine these effects as a way of determining how consequential are the shifts in time allocation that we observe. Table 3.8 presents random effects panel regressions with standard errors clustered at the subject level of both employer and worker earnings in the baseline and non-baseline data subsets. While the shift in worker behavior is enough to harm their own expected earnings at the 5 and 10 reporting frequency levels, the effect on the employer is not quite significant for any of the reporting levels. Of course it is important to note that what we were investigating here is whether reporting leads to a worker being willing to sacrifice their own welfare to work more for their employer. We find that the workers are actually sacrificing up to 10 percent of their earnings for their employer. Given the variability in the actual task, this doesn't necessarily translate into substantial earnings increase for the employer but that is simply due to the parameterization of this task. The key to the finding is that requesting more frequent reports can indeed induce

Table 3.7: Test how the frequency of reports affect workers' time allocation in **Each of the Non-Baseline Treatments**.

	Intrinsic Motivation			Observability			Feedback		
	Firm	Firm Chal	Firm Std	Firm	Firm Chal	Firm Std	Firm	Firm Chal	Firm Std
Constant	229.7***	181.5***	48.2***	167.4***	137.3***	30.0**	190.7***	156.2***	34.6
	(0.000)	(0.000)	(0.008)	(0.000)	(0.001)	(0.024)	(0.000)	(0.000)	(0.144)
Freq = 2	16.7	13.5	3.27	23.1*	4.82	18.3***	-7.48	-5.14	-2.34
	(0.135)	(0.162)	(0.683)	(0.060)	(0.689)	(0.008)	(0.472)	(0.795)	(0.853)
Freq = 5	27.7	13.8	13.9	28.4	27.1	1.30	34.4	30.6	3.83
	(0.125)	(0.523)	(0.248)	(0.190)	(0.205)	(0.691)	(0.118)	(0.185)	(0.622)
Freq = 10	12.0	-1.51	13.5	0.87	-18.0	18.8**	32.1	-7.68	39.8
	(0.380)	(0.948)	(0.426)	(0.954)	(0.166)	(0.012)	(0.232)	(0.714)	(0.120)
Round	-22.0***	-8.43	-13.6**	-4.73	4.20	-8.93***	-3.27	-0.95	-2.32
	(0.008)	(0.417)	(0.027)	(0.326)	(0.401)	(0.001)	(0.498)	(0.890)	(0.671)
N	72	72	72	64	64	64	64	64	64

Notes: standard errors clustered at the individual subject level. p-value in parentheses.
*** $p < 0.01$. ** $p < 0.05$. * $p < 0.1$.

workers to give up their own welfare to work more for the firm Of course in the 10 report case, the workers are giving up both their own and their employer's potential earnings due to the fact that they are shifting to time spent on the wrong task.

3.3.2. Effect of Reporting Treatments on Time Allocation

We now turn to examining the effect of the different reporting regimes on worker behavior to try to identify what aspect of the reporting task can lead to increases in worker effort. We again conduct a series of random effects panel regressions with standard errors clustered at the subject level with the dependent variables being time spent on firm tasks in total and then split out to the two types of tasks but this time with independent variables being the reporting regime. In these regressions, we are comparing the time allocation in each of these reporting treatments to the baseline without reporting. These regressions are shown in Table 3.9 and provide the basis for our next three results.

Table 3.8: Test how the frequency of reports/pauses affect the earnings (ECUs) workers generate to their employers and to themselves in the **Baseline** and **Non-Baseline Treatments**.

	Baseline		Non-Baseline	
	Employer	Worker	Employer	Worker
Constant	146.4***	241.7***	109.5***	209.2***
	(0.000)	(0.000)	(0.000)	(0.000)
Freq = 2	4.49	-13.4	11.5	0.95
	(0.816)	(0.540)	(0.117)	(0.905)
Freq = 5	-6.61	-2.40	11.3	-17.3*
	(0.789)	(0.882)	(0.132)	(0.058)
Freq = 10	-24.9	-23.9	0.80	-20.1**
	(0.230)	(0.198)	(0.922)	(0.033)
Round	-14.7*	-1.48	-3.72	8.56***
	(0.095)	(0.810)	(0.166)	(0.007)
N	44	44	200	200

Notes: standard errors clustered at the individual subject level. p-value in parentheses.
*** $p < 0.01$. ** $p < 0.05$. * $p < 0.1$.

Table 3.9: Test how the nature of the reports affects workers' time allocation.

	Firm	Firm Chal	Firm Std
Constant	209.9***	171.7***	38.2***
	(0.000)	(0.000)	(0.000)
Intrinsic	11.1	5.87	5.23
	(0.734)	(0.867)	(0.552)
Observability	-8.97	-9.71	0.74
	(0.798)	(0.796)	(0.944)
Feedback	19.7	-2.77	22.5*
	(0.543)	(0.937)	(0.071)
Round	-12.9***	-4.27	-8.64***
	(0.000)	(0.258)	(0.000)
N	244	244	244

Notes: standard errors clustered at the individual subject level. p-value in parentheses.
*** $p < 0.01$. ** $p < 0.05$. * $p < 0.1$.

Result 3 ***(Intrinsic Motivation)*** *Having workers acknowledge the earnings generated to their employers does not lead to a statistically significant increase in the time spent on firm tasks compared to when they are not asked to acknowledge those earnings.*

Result 4 ***(Observability)*** *Having workers report the earnings generated to their employers and know that the report will be seen by their employers does not lead to a statisticaly significant increase in time spent on the firm tasks compared to the non-reporting case.*

Result 5 ***(Feedback)*** *Having workers report the earnings generated to their employers and know that the report will be seen and commented by their employers does not lead to a statistically significant increase in time spent on the firm tasks compared to the non-reporting case.*

What we find is that all of these treatment coefficients are not significantly different from 0. This might be considered puzzling given the prior results showing that reporting frequency can improve worker effort. What we are essentially observing here is that the reporting regime on it's own is not sufficient to drive effort but interacting it with frequent reporting can do so. In these regressions we are pooling all of the reporting frequencies and given that we found offsetting results for the 5 and 10 report frequencies and small effect at the 2 report frequencies, this is perhaps less surprising than it more otherwise seem. What these regressions show is that there is not on overall level effect of the reporting regime that occurs at any and all reporting frequencies. One should not conclude that this means that the different types of reporting are ineffective at generating increased worker effort. Rather the indication is that one has to be careful in pairing the reporting regime with the right frequency to get the desired effect.

3.3.3. Effect on Timing and Trials

In addition to the main questions regarding how increased reporting frequency might impact overall effort provision, there are also important questions regarding other aspects of worker effort that might be impacted by reporting. One of thes important questions has to do with the timing with which firm effort is provided. Theoretically, a worker should worker on the Own Task for most of the time and then towards the end switch to the Firm Challenging Task. In addition to increasing the amount of time spent on the Firm Challenging Task, a firm might also want to have the worker start on that task earlier in the production period. This might be one important impact of asking for interim reports. While given the production function in this experiment, the timing of the effort isn't important, the timing might be important in many field situations and so it is worth looking at whether these reports can get workers to start on their firm tasks earlier.

In order to see the effect of reports on the timing of tasks, we divide each 300 second round into ten 30-second. We then conduct a series of regressions examining how the amount of time a worker spends on firm tasks is affected by the frequency of monitoring in each of these

10 intervals separately. What these regressions will show is whether any of our frequencies of monitoring increased the time spent on the firm tasks in each of these 10 intervals. Table 3.10 contains a set of these random effects panel regressions with standard errors clustered at the subject level to examine how the workers' time investment in firm for each interval affected by the changing frequency of reports. What we find is that for the 5 and 10 report conditions, there is a significant effect in several of the first 4 intervals while there is no systematic impact in later intervals. This indicates that the overall time increase on firm tasks we observed previously is primarily coming in the first half of the production period. Thus there is some indication that increased reporting frequency can induce workers to begin their work on firm tasks than without reporting or with lower frequency of reporting.

We can engage in the same exercise to determine if there is a systematic effect of the type of reporting mechanism in use on the timing of effort provision. It isn't clear that this should have an impact separate from the frequency of the reports but we can still examine the issue. Table 3.11 contains a set of random effects panel regressions with standard errors clustered at the subject level to investigate this. The dependent variables of these regressions are the same as those in Table 3.10, but the explanatory variables here are dummy variables for reporting treatments. What we find is that all of these treatment coefficients are not significantly different from 0. Thus the reporting regime alone does not lead to any time shifting absent the effect from the monitoring frequency.

A final issue to examine is another possible negative effect of frequent monitoring and this is the possibility that frequent monitoring could cause workers to become impatient or sloppy on their individual trials by submitting them too soon. Given that we've shown that overall subjects did an amazing job of spending the correct amount of time per trial, it is worth investigating whether frequent monitoring led to a decrease in the time per trial as workers may have been rushing to get some positive results. As a simple check on this we can examine the summary statistics of the time spent per trial on the different tasks

Table 3.10: Test how the frequency of reports affect workers' time on firm tasks in each 30-second interval

	1	2	3	4	5	6	7	8	9	10
Cons(Freq=1)	20.0***	19.1***	19.9***	15.8***	17.9***	21.3***	23.8***	21.7***	21.1***	20.0***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Freq=2	-1.38	0.80	0.84	0.88	0.83	-0.47	0.61	1.96	4.68**	3.43
	(0.411)	(0.579)	(0.513)	(0.501)	(0.592)	(0.759)	(0.735)	(0.340)	(0.020)	(0.112)
Freq=5	2.00	4.54***	3.52*	5.54***	2.45	2.01	-0.17	1.84	3.64**	4.25**
	(0.277)	(0.010)	(0.054)	(0.004)	(0.209)	(0.251)	(0.927)	(0.291)	(0.026)	(0.029)
Freq=10	3.25**	4.97***	2.48	5.01***	2.26	-0.86	-1.45	0.50	2.66	1.36
	(0.024)	(0.002)	(0.143)	(0.005)	(0.205)	(0.644)	(0.485)	(0.781)	(0.246)	(0.517)
Round	-1.95***	-1.43**	-1.48***	-0.92	-0.75	-1.24**	-1.81***	-0.98	-1.10	-0.73
	(0.000)	(0.013)	(0.008)	(0.107)	(0.177)	(0.032)	(0.002)	(0.117)	(0.150)	(0.255)
N	200	200	200	200	200	200	200	200	200	200

Notes: standard errors clustered at the individual subject level. p-value in parentheses.
*** $p < 0.01$. ** $p < 0.05$. * $p < 0.1$.

Table 3.11: Test how the report regimes affect workers' time on firm tasks in each 30-second interval.

	1	2	3	4	5	6	7	8	9	10
Cons(Baseline)	16.8***	19.3***	21.2***	21.2***	19.6***	21.4***	24.2***	22.6***	22.7***	20.8***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Intrinsic	5.57	2.78	1.80	-0.66	-0.35	1.19	-1.81	-0.53	1.05	2.05
	(0.208)	(0.526)	(0.694)	(0.875)	(0.924)	(0.755)	(0.645)	(0.885)	(0.787)	(0.627)
Observability	-0.21	0.18	0.095	-3.85	-0.90	-0.35	-2.88	-1.49	-0.26	0.70
	(0.965)	(0.971)	(0.984)	(0.386)	(0.815)	(0.928)	(0.455)	(0.689)	(0.944)	(0.866)
Feedback	5.02	4.36	1.20	-2.52	1.90	-0.11	1.35	1.67	2.17	4.66
	(0.270)	(0.336)	(0.790)	(0.540)	(0.611)	(0.975)	(0.721)	(0.643)	(0.556)	(0.224)
Round	-1.71***	-1.48***	-1.75***	-1.03*	-0.97*	-1.33**	-1.62***	-0.88	-1.05	-1.10*
	(0.001)	(0.007)	(0.001)	(0.073)	(0.076)	(0.023)	(0.001)	(0.112)	(0.138)	(0.064)
N	244	244	244	244	244	244	244	244	244	244

Notes: standard errors clustered at the individual subject level. p-value in parentheses.
*** $p < 0.01$. ** $p < 0.05$. * $p < 0.1$.

broken down by the reporting frequency. This is shown in Table 3.12 and then Table 3.13 shows similar summary statistics by reporting treatment. All of these summary statistics are practically identical making it clear that even asking subjects to report 10 times in a 300 second production period did not lead to the workers decreasing the time spent per trial.

3.4. Conclusion

Many firms have employees report to their supervisors on the status of current projects at specified intervals. An important purpose of these reports is to ensure their employees are working on appropriate tasks and putting sufficient effort in them, since the employees usually prefer shirking instead of working on firm projects as indicated by the standard principal agent problem. We use controlled experiments to examine how the frequency and style of these reports affect workers' work effort. We find that increasing the frequency of reporting improves workers' total effort on firm tasks but when set too high, the reporting frequency can have a less beneficial effect. The frequency also affects their choices on what firm tasks to put effort in. If workers are asked to report too often, they shift to performing less lucrative tasks which have more near term payoffs but lower payoffs over all. We also examine what elements of the reporting system, like the observability or feedback, are responsible for any improvement in employees' effort. While there is substantial literature suggesting that the observability of workers' actions or the employers' comments on workers' actions should

Table 3.12: Average time on a trial of workers' tasks by frequency.

	Firm Chal	Firm Std	Own Std
Freq = 1	30.91	16.69	16.78
Freq = 2	30.02	14.83	16.55
Freq = 5	29.23	14.55	16.73
Freq = 10	29.23	15.95	16.25

Notes: Firm Chal refers the Firm Challenging Task. Firm Std refers to the Firm Standard Task. Own Std refers to the Own Standard Task.

Table 3.13: Average time on a trial of workers' tasks by treatment.

	Firm Chal	Firm Std	Own Std
Baseline	30.80	15.21	15.17
Intrinsic Motivation	27.43	14.55	17.93
Observability	30.84	16.07	15.95
Feedback	31.36	15.93	17.16

Notes: Firm Chal refers the Firm Challenging Task. Firm Std refers to the Firm Standard Task. Own Std refers to the Own Standard Task.

impact their behavior, we find at best limited impact of these elements unless they interact with the appropriate frequencies.

Of course one still might be surprised that there was not a stronger overall effect from these different reporting regimes as there is much prior work showing that behavior shifts substantially between cases without observability and with [46–51]. There are many possible reasons why the effect of observability may be weaker here than in some of these prior papers. The issue with observability in these prior papers is that when the amount given or taken from another party is observed versus not, an individual can not escape moral blame for their actions. They know they have chosen the "wrong" action and they know that someone else has clearly observed their choice of "incorrect" behavior. In the environment for this experiment and importantly for many workplaces, individual actions are not what is observable; only outcomes are observable. Given that a particular outcome could occur based on a variety of different actions, there is no longer a clear implication about what actions a worker has taken when a bad outcome is observed. This fact may be enough for individuals to not feel quite the same pressure from observability on their outcomes as they would on their actions. This is consistent with [44,64,65]as in these studies, the authors find generally that individuals are not concerned with doing the right thing but rather with not being seen to do the wrong thing. Further, if they can find a way to essentially "blame" the bad outcome on another actor, then this essentially indemnifies them to be able to engage in more selfish behavior. That may be why in the current environment, the effect of these different reporting regimes is relatively weak. Given that this issue of observability being only possible on outcomes rather than actions is so important to many workplaces, this is an issue that warrants future research to determine whether or not there are other ways might want to design reporting regimes and frequencies to deliver even stronger results.

On the other hand, we do still observe an effect on behavior at certain reporting frequencies indicating that the effect is not actually zero. There are two possible reasons for this. Firstly, When workers are asked to report frequently, the time window and the number of possible trial attempts between two reports becomes smaller, so workers have less wiggle room in terms of the outcomes or earnings generated to their employers. Secondly, increasing the reporting frequency increases number of times of being observed or commented. It is possible that these elements have impact only if the intensity of these elements achieve certain level.

CHAPTER 4

Arbitrage Opportunities: Anatomy and Remediation

4.1. Introduction

Arbitrage is a financial transaction that nets a certain increase in cash holdings while not degrading the portfolio's value in any potential state of the world. When an arbitrage opportunity arises in a market setting, participants will presumably compete for its execution until the value is fully dissipated. On the other side of these transactions, the traders incurring certain losses can't do so indefinitely and their market participation is eventually extinguished. The inevitably of this 'invisible hand' effect is often called a no arbitrage condition and is one of the defining characteristics of a complete and competitive market. This no arbitrage condition is an assumption of many fundamental theories of finance: for example, the Modigliani-Miller capital structure propositions [66], the Black-Scholes option pricing formula [67] and the arbitrage asset pricing theory [68, 69].

Despite compelling arguments for the contrary, arbitrage opportunities occur with surprising frequency in developed markets. When empirical researchers examine instances of persistent arbitrage, typically through the lens that astute and unconstrained traders should compete it away, they conclude that traders' capacities to fully compete are somehow shackled. We enumerate a set of these commonly identified restraints. Then we describe a laboratory experiment that controls or eliminates each.

First, limitations on short sales prevent traders from profitably supplying sufficient amounts of an asset to extinguish an arbitrage opportunity. Persistent arbitrages often emerge in equity carve-outs in which a corporation spins off a division into a new corporation. A well known example, and detailed by [70], was the spin off Palm by 3Com. On March 2, 2000, 3Com sold a fraction of its stake in Palm to the general public via an initial public offering (IPO) for Palm, and retained ownership of 95 percent of the shares for the purpose of spinning off its remaining shares of Palm to its shareholders before the end of the year. 3Com shareholders would receive about 1.5 shares of Palm for every share of 3Com that they owned, so the price of 3Com must be at least 1.5 times the price of Palm. This lower bound occurring if 3com's value after the spin off was zero. After the first day of trading, Palm closed at $95.06 a share, implying that the price of 3Com should have have been at least $145. Instead its price was $81.81. lamont2003can analyze a large number of such carve out arbitrages and find that short sales limitations, arising from high costs of execution[1] and institutional restraints, are the predominant reason for such persistence arbitrages. In the case of 3Com, brokerage firms and institutional investors who controlled much of Palm's stock generally agreed not to lend the stock to short sellers prior to the IPO date.

Second, limitations on leveraged purchases prevent traders from buying a sufficient amount of an asset to extinguish an arbitrage opportunity. Some researchers have treated the 2007 financial crisis as an exogenous shock to the lending and standard practices offered on liquidity for arbitrage opportunities. For example, [72] examine arbitrages that entail borrowing in one currency and lending in another to take advantage of interest rate differentials while avoiding exchange rate risk. They show that arbitrage profits were large after the 2007 Financial Crisis, persisted for months and involved borrowing in dollars. Empirical analysis suggests that insufficient funding of liquidity in dollars kept traders from arbitraging away excess profits.

Third, noise trader risks [73, 74] and the limited horizons of arbitragers is also a common explanation for persistent arbitrage. Noise traders, who either have an incorrect model of

[1] The high cost of short sales, because of limited supply of loaned shares, have also been identified as a primary reason for persistent arbitrages associated with closed-end funds [71].

fundamental value or trade on the basis of ancillary motivations, generate mis-pricing and arbitrage opportunities. However, the unpredictability of noise traders' beliefs creates a risk in the price of the asset that deters rational arbitrageurs from aggressively betting against them. The arbitrageurs are usually highly specialized investors who are risk averse and have relatively short investment horizons. As a result, their willingness to take positions against noise traders is limited. An example of persistent arbitrage associated with noise trader risk is provided by [75] who examines the return for two pairs of "Siamese twin" stocks: Royal Dutch/Shell and Unilever NV/PLC. These unusual pairs of fundamentally identical stocks provide a unique opportunity to investigate two facets of noise trader risk: the fraction of total return variation unrelated to fundamentals, i.e., noise, and the short-run risk borne by arbitrageurs engaged in long-short pairs trading. She finds that about 15% of weekly return variation is attributable to noise.

Fourth, insufficient market capitalization can also lead to persistent arbitrage. When there is an increasing number of potential arbitrageurs, they can collectively eliminate an arbitrage opportunity with individually smaller and inherently less risky positions. Alternatively if a fixed number of arbitrageurs individually have higher levels of initial wealth and decreasing risk aversion with respect to wealth, they will have a propensity to take riskier positions to exploit, and subsequently eliminate, arbitrage opportunities.

We design a series of controlled laboratory experiments that allow us to eliminate the noise trader risk limitation and to test the veracity of the other three limitations. In each experiment, there are two commodities in the market; a non-interest bearing and non-dividend paying commodity called "pesos" and an asset that pays a peso denominated dividend at the end of each period and a certain terminal redemption value. The dividend sequence of the asset is determined by randomly selecting *without* replacement from a set of values whose cardinality is the number of market periods. Consequently, the sum of future dividends and the terminal value is certain at every point of time. Therefore a riskless arbitrage can be executed with a single transaction. In fact, any transaction whose price differs from the fundamental value is a realized arbitrage.[2] Note that all traders - including potential arbitragers - have the same finite consumption/decision horizon. Thus, when there is unlimited access to short sales and liquidity borrowing participants can exercise arbitrage opportunities without concern of future prices. In other words, there is no noise trader risk.

In the baseline scenario, we mimic market conditions under which persistent arbitrage can arise by imposing the first two limitations: hard constraints on short selling and leveraged purchasing. At the beginning of the market each trader receives the same portfolio endowment of pesos and units of the asset. When a trader's available asset holding reaches zero, she can no longer sell units. Further, we do not provide her a facility to borrow pesos which can be used to purchase units of the asset. From this baseline we develop two experimental designs: one that examines the impact of market frictions and one that examines the impacts of market capitalization.

In our first experimental design we vary the presence of short sale and leveraged purchase constraints. In the "Short sales" environment, we allow any trader to hold a negative quantity of the asset up to a limit that is sufficient to absorb the aggregate endowment of pesos at the minimum possible fundamental value of the asset. In the "Liquidity" environment, we provide a facility from which any trader can borrow at a zero interest rate. The leverage limit allows a single trader to purchase the entire aggregate endowment of the asset at its maximum possible fundamental value. In the "Liquidity + Short sales" environment, we remove all market frictions, i.e. allow both short sale and leveraged purchases.

In our second experimental design we vary the capitalization in the market while maintaining leveraged purchase and short sale constraints. In the "Competition" treatment, we

[2]The concept that riskless arbitrage involves at least two simultaneous transactions is likely entrenched in many readers' minds. However this is based in the almost universal presence of fundamental value risk [68, 69] in which a riskless arbitrage is only obtained by buying an asset at a price lower than at which it is simultaneously sold.

increase the number of traders by 150% with traders retaining baseline endowments. In the "Big endowment" treatment, we increase the baseline endowments by 150% but maintain the same number of traders.

In our experimental sessions with the baseline setting, we observe persistent arbitrage opportunities with significantly more and larger sell-side than buy-side ones. Elimination of market frictions does not diminish arbitrage. When we allow generous short sales, the frequency of arbitrage does not diminish but asset prices decrease in general. Allowing generous leverage purchasing does not diminish arbitrage and sell-side opportunities grow even more dominant. When we add both leveraged purchases and short sales, we find arbitrages of similar magnitudes but greater frequency to those in the Baseline. These results are remarkable not because there are traders who make errors, but rather that astute traders do not use the sufficiently ample resources at their disposal nor compete vigorously enough to drive errant traders from the market. This is supported by the dynamics of the arbitrage opportunity dynamics within and across markets. Within markets arbitrage opportunities persistently arise in the fourth and fifth trading periods; and across markets even after having experienced the market four times with the same cohort of traders persistent arbitrage arises in almost all market periods.

Increasing market capitalization diminishes arbitrage. We find that increasing the number of traders reduces the magnitude of arbitrages but increases their occurrence. When we hold the number of participants constant but increase the size of their portfolio endowments both the average size and frequency of arbitrage are reduced.

We also examine the terminal portfolios of our subjects. We find the market frictions are binding for some subjects. In the baseline, there is a noticeable clustering of corner portfolios, either all peso or all asset. Introducing short sales or leveraged purchases results in a spread of terminal portfolios beyond these corners. Removing all market frictions make the spreads more extreme. Terminal portfolios do not vary only in composition but also value, or in other words wealth inequality emerges. Introducing short sales or leveraged purchases results in an increasing spread of the distribution of terminal wealth. Even though increasing the number of traders reduces arbitrage and improves market efficiency, it also drives greater wealth inequality. In contrast, increasing the initial portfolio endowment for each trader not only reduces arbitrage but also diminishes wealth inequality.

There is scant experimental research explicitly examining arbitrage. Some notable exceptions are some research [76] who studies arbitrage in a contingent claims political stock market; Charness(2014) [77] who evaluate the Modigliani and Miller capital structure proposition; Asparouhova (2016) [78] who evaluate the Modigliani and Miller's theorem of firm value invariance with respect to its dividend policy and Obrien(1991) [79] who study information aggregation in markets for multiple asset and find persistent arbitrage fails traditional statistical tests of informational efficiency. These studies all find persistent arbitrage but none examines whether the elimination of market frictions or market deregulation reduces it.

In contrast, a large experimental literature has examined price efficiency in asset markets and found that prices usually deviate significantly from the fundamental value, with a bias for over-pricing [80, 81]. These price deviations are not arbitrages because the independently drawn dividends create risks in the total sum of dividends one receives from purchasing and then holding a unit of the asset until termination. To our knowledge there is one exception to this fundamental value risk. Porter(1994) [81] demonstrate that when the asset dividend stream is certain, overpricing is not significantly reduced relative to when there is dividend uncertainty. Most of these studies only consider monotonically decreasing dividend paths[3] and some researchers, e.g. [84], have argued this decreasing fundamental path is counter intuitive and a source of mis-pricing. Our design generates non-monotonic fundamental value paths. A limited number of these experimental studies investigated how market frictions

[3]Some exceptions are Noussair(2010) [82] and Noussair(2011) [83].

affect price deviations. Relaxing short-selling constraints lowers prices in experimental asset markets, but does not induce prices to track fundamentals [85, 86]. In contrast, allowing borrowing increases overpricing in the market [86]. None of these studies allow sufficiently generous short sales to permit any trader from absorbing the aggregate endowment of currency, nor sufficiently generous leveraged purchases to permit any trader from absorbing the aggregate endowment of the asset. Thus, there is no sufficient precondition for a Bertrand competition that should eliminate asset mispricing.

We proceed by presenting the details of our two experimental experimental designs in the next section. After which we present our results section. The results are organized around how our various treatments impact three key factors: how arbitrage emerges in terms of buy-side versus sell-side opportunities, as well as the relative incidence of arbitrage arising from limit versus market order; market price efficiency; and degree of terminal wealth inequality. We conclude with discussions of how our results speak to effective market mediation and regulation.

4.2. Experimental Design

4.2.1. Assets, dividends and arbitrage   Consider a world with two commodities. One is a non-interest bearing and non-dividend paying commodity called "pesos," whose units we express in . The second is an asset that lives for five periods, pays a publicly observed peso dividend at the conclusion of each period and provides a commonly known terminal redemption of 21. The sequence of the asset's dividends is generated by randomly selecting without replacement from the following set of values: $\{-6, -6, -6, 6, 6\}$. After each period, we publicly inform the participants the realized dividend amount and how many of each "6" and "-6" dividends remain. At any point in time a trader knows with certainty the value of the sum of the remaining dividends and the terminal redemption value. Consequently, as long as she only values the closing balance of pesos after terminal redemptions[4], the asset always has a known and certain peso equivalent. This peso equivalent is the fundamental value of the asset.

The potential time paths of the fundamental value exhibit a variety of patterns, distinguishing this environment from other experimental studies of multi-period lived assets. Figure 4.1 presents the set of all potential fundamental value paths. All paths start at the value of fifteen in period one. Then the path either increases by six when a -6 dividend is drawn or decreases by six when a 6 dividend is drawn. The maximum potential fundamental value of thirty-three is realized when the first three dividend draws are -6. The minimum potential fundamental value of three is realized when the first two dividend draws are 6. In period five all dividend paths either reach the value of fifteen or twenty-seven.

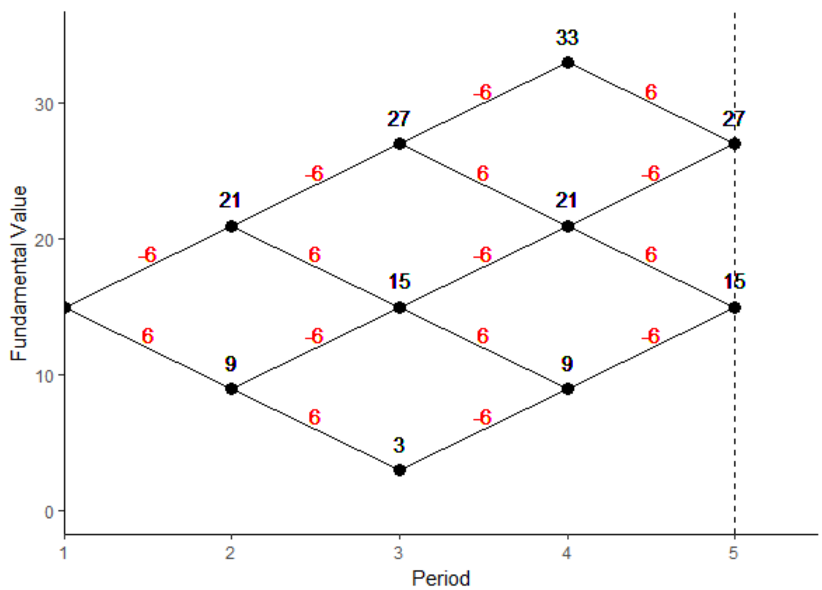

Figure 4.1: The set of all possible fundamental value paths across the five periods; the y-axis, and numbers above nodes are fundamental values, and the numbers above the branches are the realized dividend values

[4]Or alternatively she doesn't discount the stream of dividends, are indifferent over the sequence by which the future dividends are realized or have non-Bayesian subjective beliefs about when a remaining dividend value will be drawn.

What is arbitrage in this world? When there is an exchange of a unit of the asset for an amount of pesos which differs from the fundamental value, an arbitrage has occurred. If the amount of pesos is below the fundamental value, we call it a buy arbitrage; the buyer has ensured herself a certain gain in her final pesos holdings. Consider an example. Suppose it is period two and the period one dividend was 6. The fundamental value of the asset is now 9. If a trader purchases a unit of the asset at a price of 4, her final pesos holdings will assuredly increase by 5 assuming she holds the asset until the redemption.

When there is an exchange of a unit of the asset for an amount of pesos above the fundamental value, we call it a sell arbitrage. Consider another example. Suppose it is period four and the previous three dividends were 6, -6, and 6. The fundamental value of the asset is now 9. If a trader sells a unit of the asset at a price of 14, she assuredly increases her final pesos holdings by 5.

4.2.2. Market microstructure

All trades take place in a continuous double auction. Each period, prior to the dividend realization, there is a fixed length of time in which traders may generate publicly observable messages which can lead to bilateral trades. There are four types of messages traders can submit. The first two are limit orders. A limit bid is an amount of pesos at which the trader is willing to purchase a unit of the asset. A limit ask is an amount of pesos a trader is willing to accept to provide a unit of the asset. These limit bids and asks are publicly displayed in the "order book." Limit bids are listed from highest to lowest, while limit asks are listed from the lowest to highest.

We impose rules restricting the submission and removal of limit orders. Any new limit bid must exceed any limit bid in the order book, and any new limit ask must be lower than any other limit ask in the order book. A trader can freely withdraw a limit order from the order book as long as it is not the highest bid or lowest ask. We defer discussion of other restrictions that are conditional upon a trader's portfolio. Whenever a trader submits a limit bid above the current lowest limit ask a trade is triggered at the limit ask price. Likewise, when a trader submits a limit ask below the current highest limit bid, a trade occurs at the limit bid price. We evacuate the order book when a trading period concludes.

A trader can send two other types of messages: market buys and market sells. When a trader sends a market buy, and there is at least one limit ask in the order book, she purchases a unit of the asset at the lowest current ask from its submitter. Similarly, when a trader sends a market sell, and there is at least one limit bid on the order book, she sells a unit of the asset at the current highest limit bid to its submitter. Note that whenever a transaction occurs the involved limits order(s) are removed from the order book. We forbid traders from submitting market and limit orders that transact with their own limit orders. There are other restrictions on limit and market orders which are conditional upon a trader's portfolio, but we momentarily defer discussing these.

These rules define a continuous double auction, and allow for three types of arbitrage opportunities: explicit, implicit and unrealized. Each of these can manifest as either a buy or sell arbitrage. In an explicit arbitrage either a limit ask is submitted lower than the fundamental value and is accepted by a market buy or matched with a subsequent limit bid (explicit sell arbitrage), or a limit bid is submitted exceeding the fundamental value and is accepted by a market sell or matched with a subsequent limit ask (explicit buy arbitrage). When a limit ask is submitted which exceeds the fundamental value and is subsequently accepted, or when a limit bid is submitted below the fundamental value and is subsequently accepted, this is called implicit arbitrage. The former is an implicit sell arbitrage and the latter is an implicit buy arbitrage. Finally, when a limit ask is submitted below the fundamental value, or a limit bid is submitted above the fundamental value, but the trading period expires with the limit order still in the order book this is called an (buy or sell accordingly) unrealized arbitrage.

4.2.3. Endowments, feasible portfolios and market frictions

We complete the specification of the microeconomy by noting there are n traders each with a common portfolio endowment of pesos and units of the asset, $(\circ, \circ A) = (100, 3)$.[5] The specification of additional rules on limit and market orders define the sets of feasible commodities (i.e. portfolios) as well as market frictions.

We restrict limit asks and market sells conditional upon a trader's current holding of assets and her limits orders in the order book. These are short sale constraints. We define the short sale limit K as a lower bound on the number of assets held in a trader's portfolio less the number of limit asks she owns in the order book. When this difference reaches the lower bound K she can no longer submit any limit asks or market sell orders. When $K = 0$ there are no short sales permitted in the market. When we allow for short sales, we set $K = -235$. When the minimum possible fundamental value of the asset of three is realized, $K = -235$ is sufficient for any one trader to absorb the aggregate endowment of pesos in the market. When a trader holds a negative quantity of the asset at the conclusion of a trading period they "pay" rather than receive the dividend for each negative unit. If they hold a negative quantity of the asset at the end of period 5, they must pay the terminal redemption for each short sold unit of the asset.

We also restrict limit bids and market buys conditional upon a trader's current peso holdings and her limit bids in the order book. These are leverage constraints. We define the leverage limit L as a lower bound on a trader's peso holdings less the total value of pesos she has committed to limit bids in the order book. When $L = 0$ there is no facility to borrow pesos from in order to purchase units of the asset. At times we provide a facility from which

[5]The number of traders and the common endowment is public knowledge.

any trader can borrow pesos without interest. In this case the alternative leverage limit is $L = -600$. At this limit any trader can purchase the entire aggregate endowment of the asset at its maximum possible fundamental value of 33. If a trader holds a negative quantity of pesos after period 5, then she must pay this balance from her terminal redemption values of her final asset holdings.

4.2.4. Experimental treatments

We create the first of two experimental treatment designs by turning on and off the short sale and leverage constraints. When we impose short sale and leverage constraints, i.e. maximal market frictions, a trader's portfolio is approximately[6] constrained to the positive orthant of the Cartesian plane. This is depicted as region 1 in Figure 4.2, which includes the individual traders' common endowment $(\circ, \circ A) = (100, 3)$. We call this our "Baseline" environment.

When we allow for short sales the set of feasible portfolios approximately extends to include both regions 1 and 2, where the short sale limit $K = -235$ is indicated by the horizontal dashed line. We call this our "Short sale" treatment. When we allow for leveraged purchases, but no short sales, the feasible set of portfolios consists of regions 1 and 3, where the vertical dashed line indicates the leverage limit $L = -600$. We call this our "Liquidity" treatment. When we remove all market frictions, i.e. allow for both short sales and leveraged purchases, the set of feasible portfolios consist of regions 1 through 4. We call this our "Liquidity + short sales" treatment.

Our second experimental design varies the aggregate wealth of the two-good economy while maintaining our baseline levels of market frictions. We do this through the manip-

[6]When a trader takes a position with a large asset-to-peso ratio it is possible for her peso holding to be negative through the realization of negative valued dividends. In such cases, we don't force her to sell assets to comply with the non-negativity of pesos constraint, but do forbid her from submitting limit bids and making market buys.

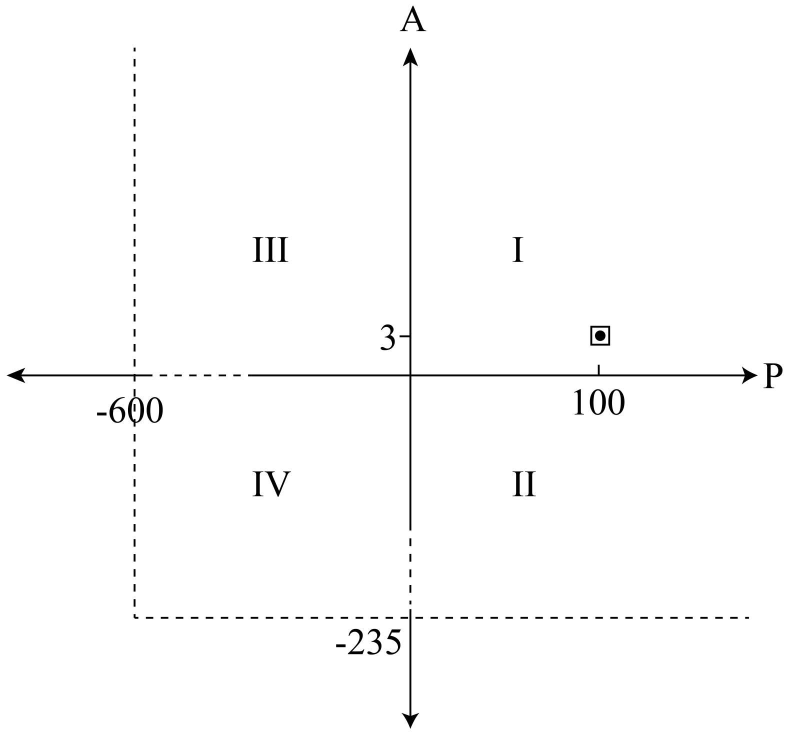

Figure 4.2: Feasible commodity spaces: the alternative sets of feasible portfolios as determined by alternative combinations of short sale and leverage constraints.

ulation of the number of traders, n, or the size of the traders' portfolio endowments. We utilize the same baseline as our first experimental design, an economy with eight traders, $n = 8$, each with a portfolio endowment of $(\circ, \circ A) = (100, 3)$. Next, we consider a 2.5 fold-replication of this baseline economy. In other words we enlarge the economy by including 2.5×8, or 20, traders each with the same portfolio endowment of $(100, 3)$. This leads to a 150% increase in the aggregate wealth, from 1160 in the Baseline treatment to 2900, while maintaining a per capita initial wealth of 145. We call this our "Competition" treatment. Our other capitalization manipulation is to maintain $n = 8$ while increasing initial portfolio endowments so that aggregate wealth is 2900. This is achieved by giving four traders the portfolio endowment $(250, 7)$ and the other four traders $(250, 8)$. We call this our "Big endowment" treatment. Table 4.1 summarizes our experimental designs.

4.2.5. Experimental procedures

Table 4.1: The two experimental treatment designs

(a) Treatment design 1: 2x2 factorial treatment design on short sales and leverage constraints

		Leveraged purchase	
		No	Yes
Short sales	No	Baseline	Liquidity
	Yes	Short sale	Liquidity + Short sale

(b) Treatment design 2: three capitalization variations

	Baseline	Competition	Big Endowment
Number of traders	8	20	8
Portfolio endowment	$(100, 3)$	$(100, 3)$	$(250, 7)/(250, 8)$

Note: For treatment design 1, we have 8 traders in each experimental session, and each trader has a portfolio endowment $(100, 3)$. Each treatment cell in both designs is applied to five experimental sessions.

Our two experimental designs incorporated a total of six treatments: Baseline, Short sales, Liquidity, Liquidity + Short sales, Competition and Big endowment. We used a between subject design; each experimental session experienced exactly one of the six treatments. For each treatment we conducted five sessions.

We started each experimental session by providing each participant a hard copy of the instructions[7] which we asked them to read along silently as a monitor read them aloud. This established public mutual knowledge regarding all aspects of the experimental session. After reading the instructions, we required traders to privately and correctly answer at least nine out of ten questions to demonstrate their adequate understanding of the dividend structure, how experimental earnings were determined and the trading rules. At this point we initiated a sequence of five independent markets, each lasting five periods. We paid the traders for only one of the five markets. At the conclusion final market, the monitor rotated a bingo

[7]We provide a translated set of these instruction in the Appendix. Original versions in Mandarin are available upon request.

cage and selected randomly from the five balls to determine which market we would use to determine the traders' earnings. Traders were paid their earnings privately and the session concluded.

The five markets were independent in the following sense. We reset the traders' initial portfolio endowments prior to each market. We also used a new independent realization of the dividend sequence.[8] An extensive literature examining experimental markets for a finite but multi-period asset with symmetric information on the dividend process, initiated by the seminal work of [80] and surveyed by [87], has established that mispricing is greatly dissipated after a cohort of traders has twice experienced the same market but with different dividend realizations. In this study we use five market repetitions because, unlike most previous studies, the fundamental value path varies in each iteration and perhaps eliminating price anchors that facilitate convergence of price to fundamental value.

We next provide details on our computerized implementation of the continuous double auction.[9] Each of the five trading periods in a market lasts for two minutes. Figure 4.3 presents an annotated screen capture of the trading screen used in the experiment. In the top portion of the screen a trader can find information about the realized and yet unrealized dividends, and her closing portfolios in each of the previous trading periods of the current market. In the middle portion of the screen she can find her current portfolio, and the amount of available pesos and asset units which she can use to make limit and market orders. We provide, in the middle of the screen, the fields by which she can make limit orders and the buttons she can use to make market orders. Below this, she can find the order book. In the lower right portion, she can find a list and a plot of the current period transaction prices.

[8] Prior to the experimental session, the monitor used a bingo cage to determine the dividend sequence for each of the five markets. The monitor inserted a written record of each dividend sequences into an envelope. The monitor taped these envelopes to a platform that all traders could see during the experiment. After each market, the monitor opened the just concluded market's envelope and projected its contents. This was done to publicly confirm the dividend sequence and verify procedural integrity.

[9] We programmed the continuous double auction experiment using z-Tree 3.2.8 [27] by modifying code generously provided by Michael Kirchler.

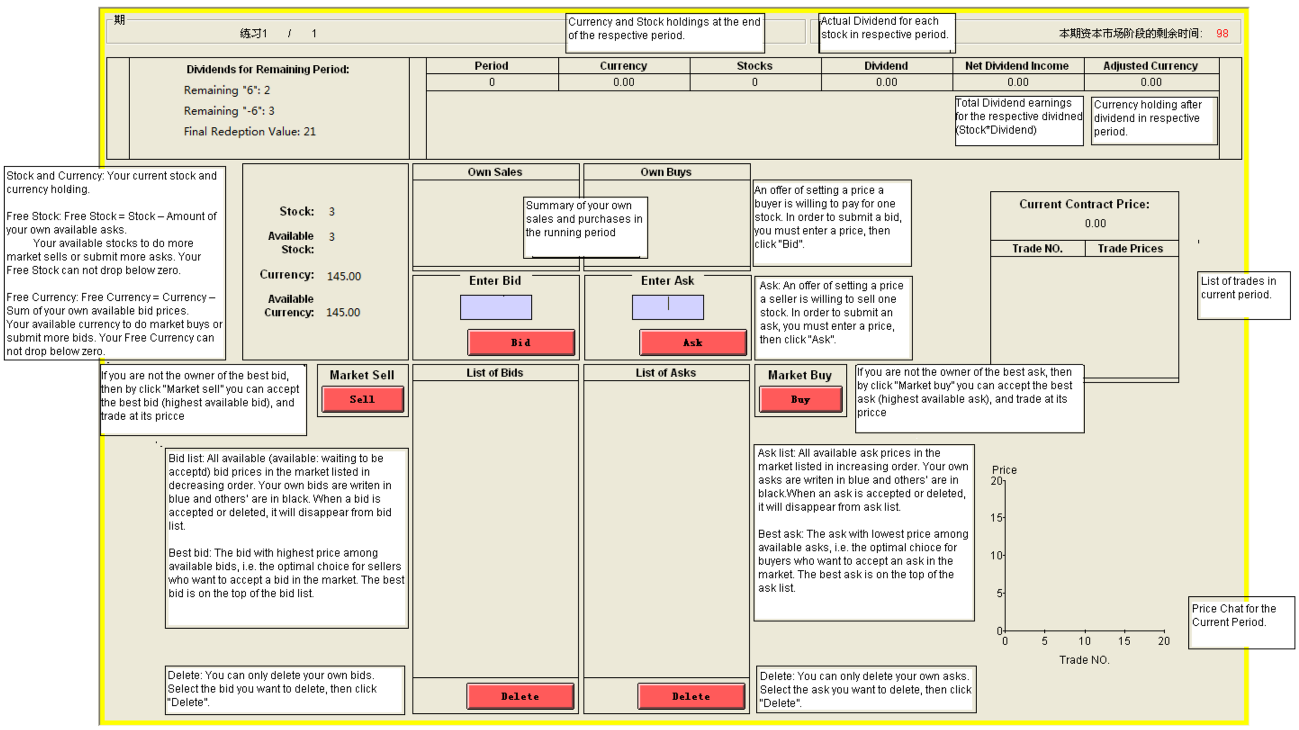

Figure 4.3: The continuous double auction trading screen

We conducted all sessions at the Finance and Economics Experimental Laboratory (FEEL) at Xiamen University. All three hundred traders were either undergraduate or master students attending Xiamen University. They came from various schools, such as law, computer science, chemistry and biology. But the most represented schools, with around 40% of the traders, were economics - which houses finance majors - and management. Most participants had previous experience in other studies at FEEL, but none in an asset market experiment. We only allowed traders to participate in a single session. We recruited participants using the ORSEE subject recruitment system [88]. There were approximately 1600 students in the subject pool database from which we randomly selected members to send e-mail invitations. The e-mail invitations conveyed that the experiment would last no longer than two and one-half hours and they would receive a show-up fee of 10. We added a trader's earnings from the selected market to her show-up fee. These market earnings were converted from pesos to Chinese Yuan at an exchange rate of 3 to 1. There was limited liability, and if a trader had a negative pesos balance she only received her show-up fee. This affected only one out of the three hundred traders.

4.3. Results

4.3.1. Arbitrage

We begin by presenting the times series of nominal arbitrages in each experimental market. Figures 4.4-4.9 display for each treatment a stack of five plots. Each layer of a stack corresponds to one of the five experimental sessions. The vertical-axis measures the peso amount of an arbitrage: the horizontal-axis measures continuous time.[10] For each trading period we provide two pairs of numbers. The top pair reports the number of realized and unrealized sell arbitrages while the bottom pair reports the number of realized and unreal-

[10]We break the layer into five segments, one for each five market iterations. These are demarcated by the thick vertical lines. Each of these market segments is further divided into five sub-segments, one for each trading period, which we demarcate by the thin vertical lines.

ized buy arbitrages. A realized arbitrage refers to a transaction whose price differs from the fundamental value. Unrealized sell arbitrage refers to an outstanding bid at the end of a period above the fundamental value while an unrealized buy arbitrage refers to an outstanding ask at the end of the period below the fundamental value.

The midpoint of the vertical-axis is zero, and the magnitude of plotted values above this reference line are the nominal peso amounts of sell arbitrages, and the magnitude of those below are the nominal peso amounts of buy arbitrages. Let's first consider sell arbitrages. We mark explicit and implicit sell arbitrage transactions with upward and downward pointing triangles respectively. We use a similar practice to mark realized buy arbitrages. Explicit arbitrage refers to an arbitrage realized by accepting limit orders while implicit arbitrage refers to an arbitrage realized by submitting limit orders. We mark unrealized arbitrage opportunities by black triangles plotted at the closing time of a trading period.

These time series plots convey our study's key findings. In the Baseline treatment, Figure 4.4 exhibits consistent arbitrages across markets with more Sell than Buy arbitrage.[11] When we allow generous leverage purchasing, see Figure 4.5, arbitrage does not diminish and becomes even more Sell arbitrage dominated. Adding Short sales, see Figure 4.6, does not diminish the frequency of arbitrage but does suppress prices in general; Buy arbitrage is now more frequent than Sell arbitrage. When we add both leveraged purchases and short sales, see Figure 4.7, we observe arbitrage of similar magnitude to the Baseline levels but with greater frequency. Returning to a world with market frictions but a larger number of traders, see Figure 4.8, seemingly reduces the magnitude of arbitrages but increases volume tremendously. Holding the number of participants constant but increasing the size of their portfolio endowments, see Figure 4.9, reduces the average size of arbitrage and its frequency. We found the arbitrage opportunities do not diminish as the time progresses in a market.

[11]This is consistent with the large body of literature on experimental asset markets, but our findings provide an important extension of these results to a non-monotonic and certain fundamental value paths.

We can see there are still a lot arbitrage opportunities even in the fourth or fifth period of markets.

We quantify these visually suggested effects of market frictions and capitalization by reporting summary statistics for All, Sell and Buy arbitrage by treatment in Table 4.3. Within each of these arbitrage types we consider implicit, explicit and either kind of arbitrage. For each category we report two statistics. The first statistic is the mean of the arbitrage magnitude conditional upon a transaction being the considered arbitrage type. The second statistic is the proportion of all trades which are of the considered arbitrage type.

Table 4.2: Summary statistics by arbitrage type and treatment: mean arbitrage magnitude and the percentage of trades that are of a given arbitrage category

	All Arbitrage			Sell Arbitrage			Buy Arbitrage		
	Either	Explicit	Implicit	Either	Explicit	Implicit	Either	Explicit	Implicit
Competition	3.20	2.93	3.39	3.36	3.05	3.55	2.99	2.80	3.14
	86%	36%	51%	50%	19%	30%	36%	16%	20%
Big Endowment	2.75	2.71	2.76	3.13	3.10	3.14	2.13	2.36	1.97
	81%	25%	56%	50%	12%	38%	32%	13%	18%
Baseline	4.70	4.55	4.79	5.41	5.00	5.65	3.16	3.53	2.95
	88%	33%	55%	60%	23%	38%	28%	10%	18%
Liquidity	3.69	3.05	4.10	4.20	3.50	4.58	2.36	2.17	2.53
	91%	35%	56%	66%	23%	43%	25%	12%	13%
Short sale	3.86	3.74	3.98	3.77	3.65	3.81	3.93	3.77	4.15
	89%	42%	47%	35%	10%	25%	54%	31%	22%
Liquidity + Short sale	4.19	3.55	4.62	4.93	4.45	5.19	2.98	2.49	3.44
	89%	35%	54%	55%	19%	36%	34%	16%	18%

Table 4.3: Tests of the difference between the magnitude of sell and buy arbitrages

	Sell Arbitrage	Buy Arbitrage	t-statistic	p-value
Competition	3.36	2.99	4.09	0.000
Big Endowment	3.13	2.13	7.17	0.000
Baseline	5.41	3.16	8.98	0.000
Liquidity	4.20	2.36	12.10	0.000
Short sale	3.77	3.93	0.94	0.347
Liquidity + Short sale	4.93	2.98	9.36	0.000

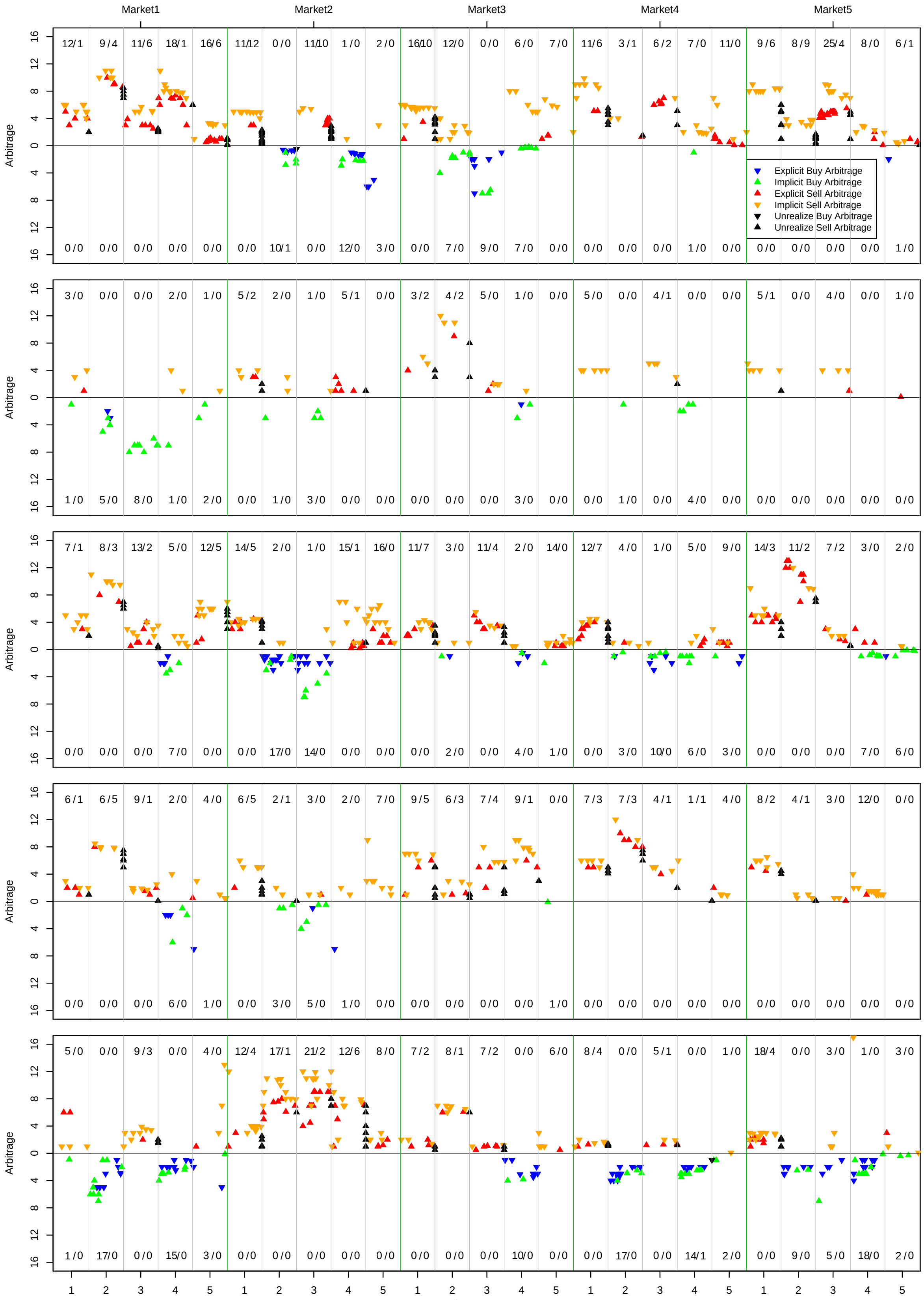

Figure 4.4: Arbitrage time series plots for all sessions: Baseline treatment.

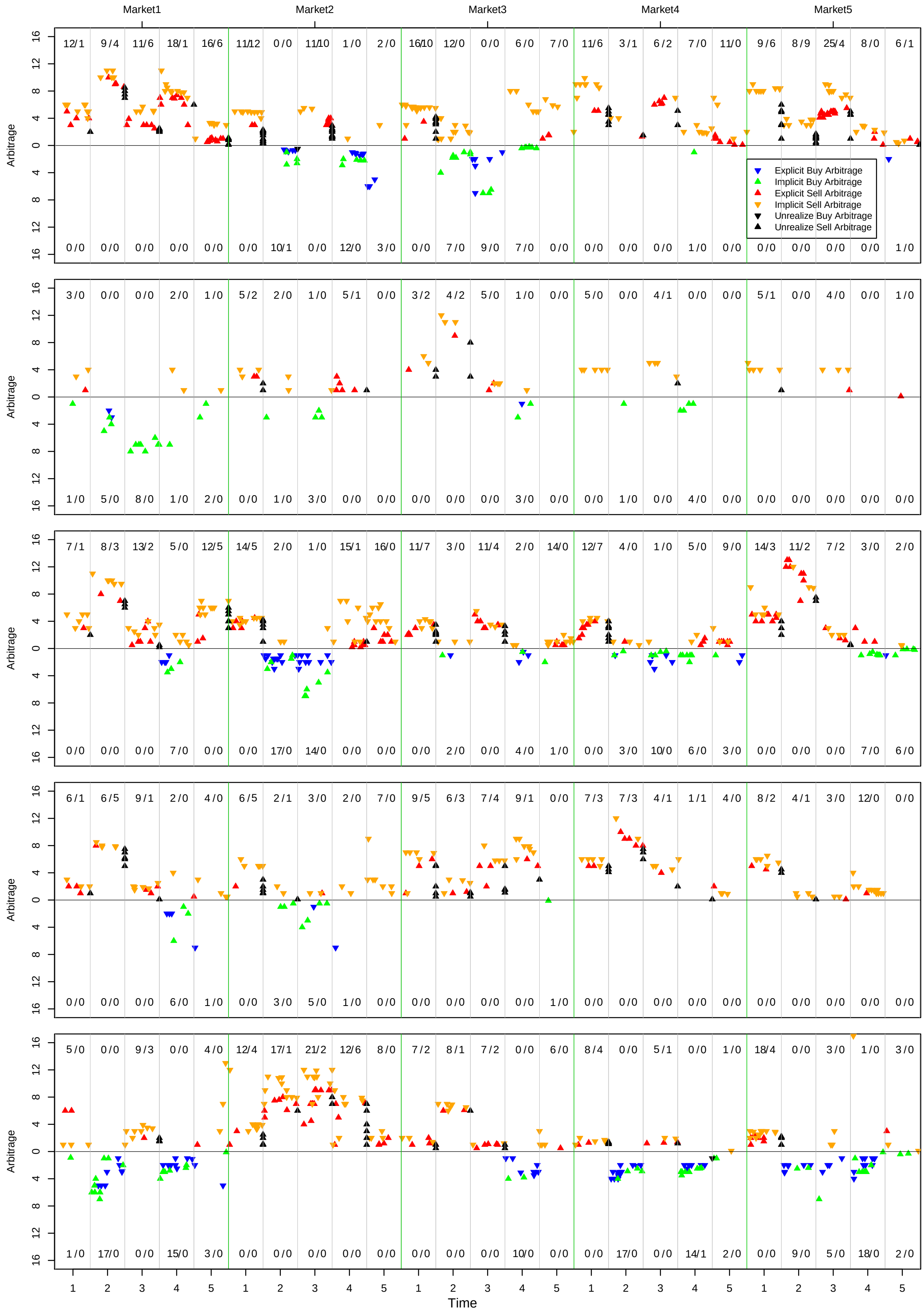

Figure 4.5: Arbitrage time series plots for all sessions: Liquidity treatment.

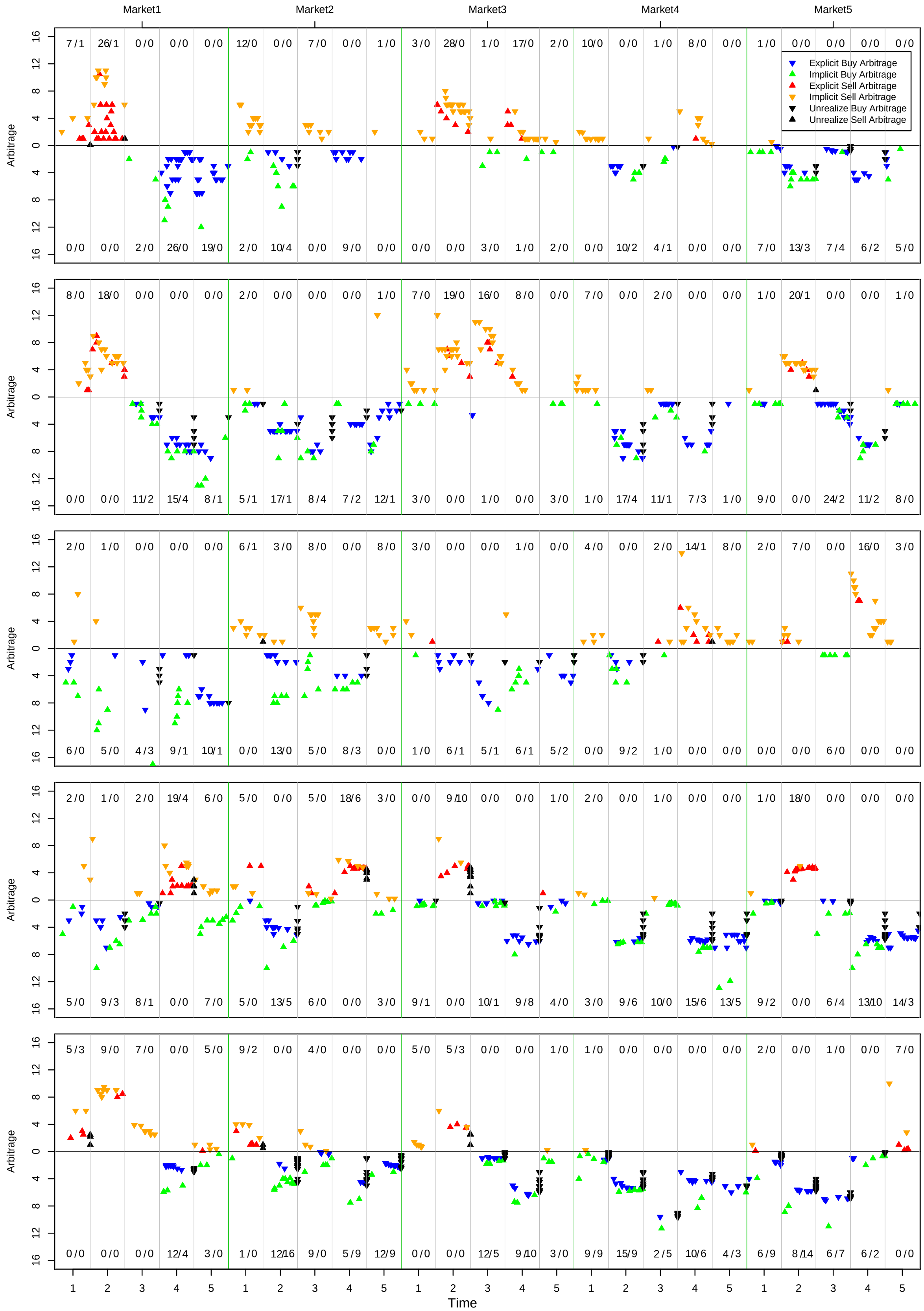

Figure 4.6: Arbitrage time series plots for all sessions: Short sale treatment.

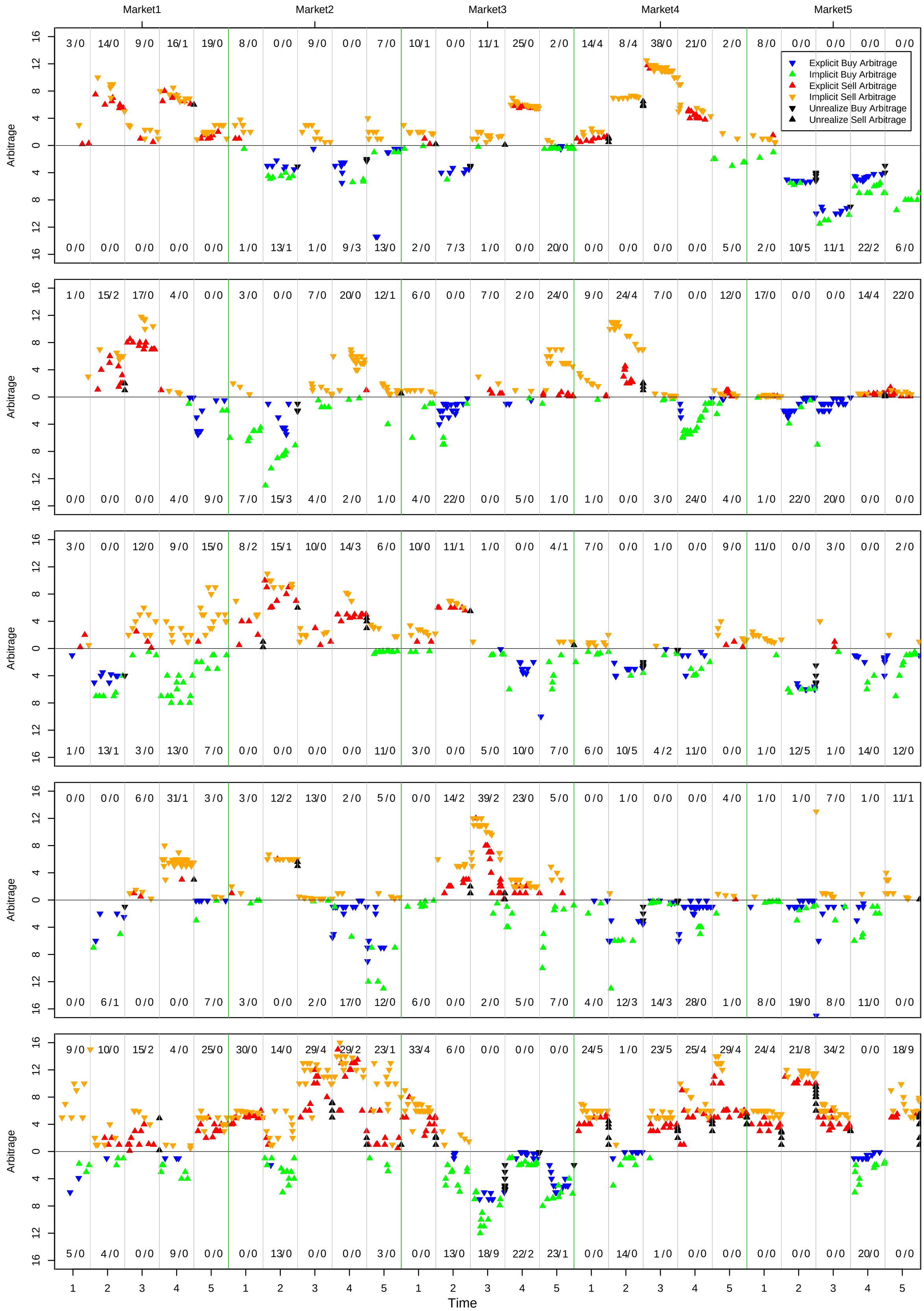

Figure 4.7: Arbitrage time series plots for all sessions: Liquidity + Short sale treatment.

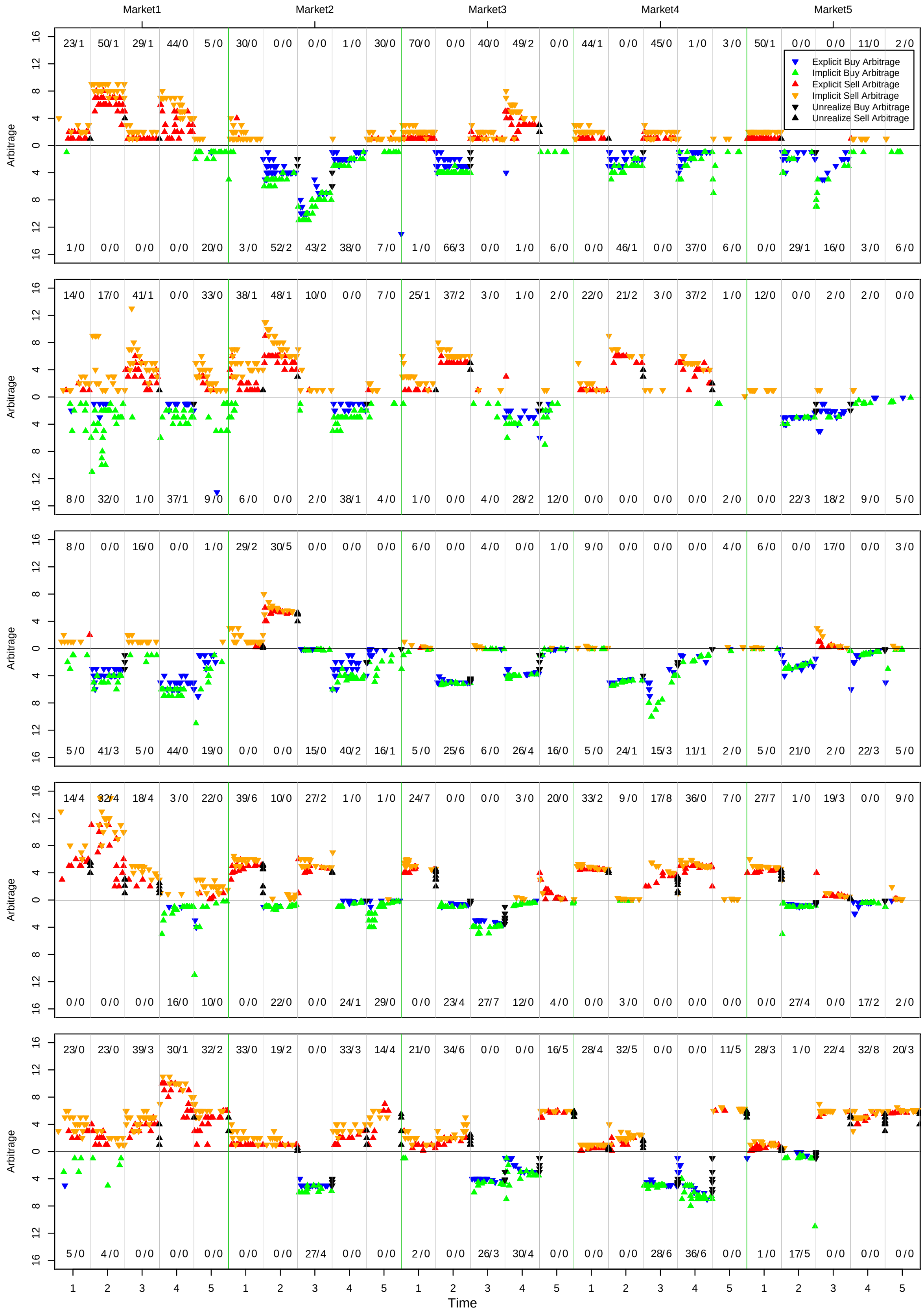

Figure 4.8: Arbitrage time series plots for all sessions: Competition treatment.

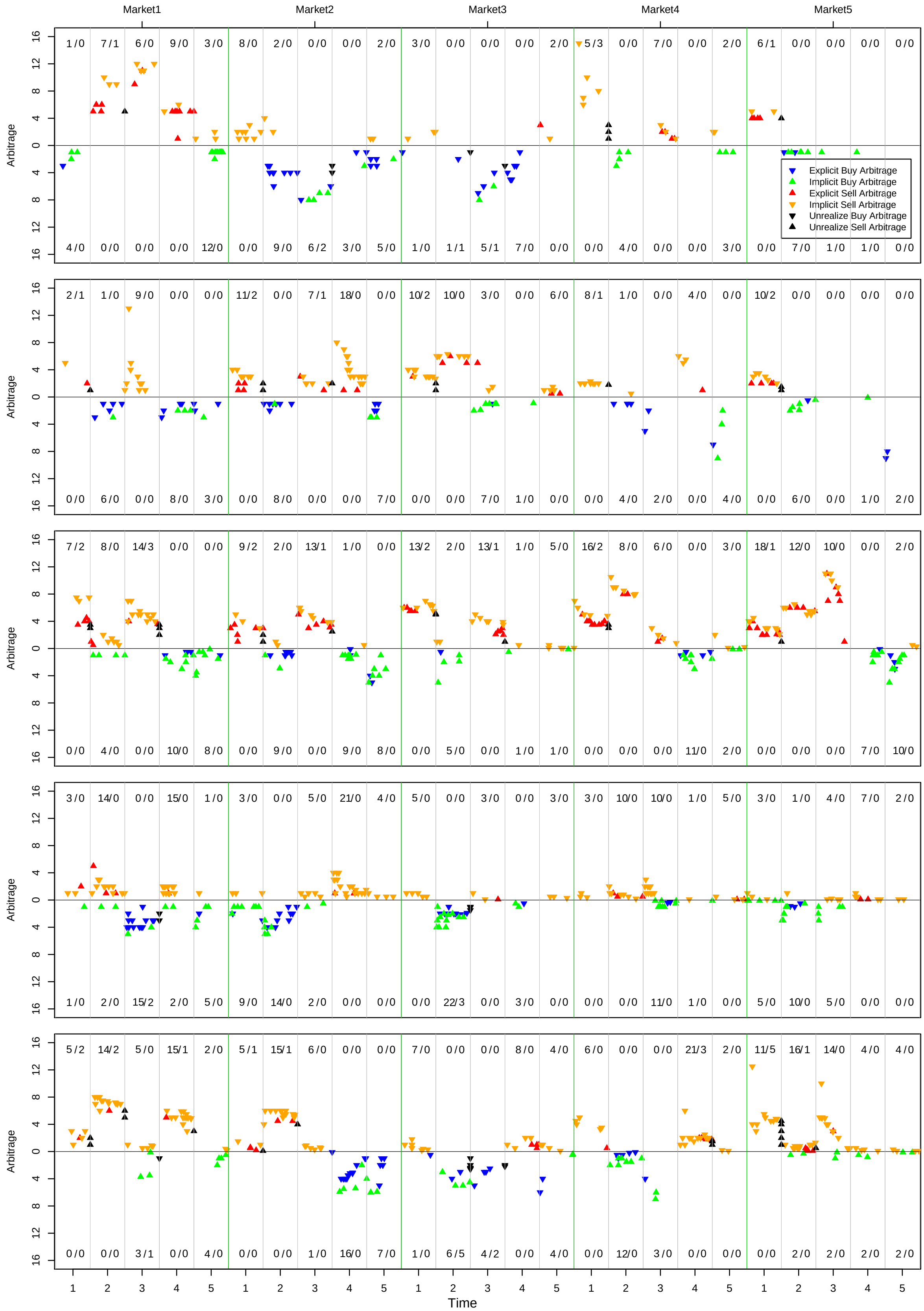

Figure 4.9: Arbitrage time series plots for all sessions: Big Endowment treatment.

The Baseline treatment generates the largest magnitude of arbitrage with an average of 4.70. Increasing capitalization by Competition or Big Endowment reduces the magnitude of the arbitrage to 3.20 and 2.75 respectively. These reductions are larger than we observe with leveraged purchases, 3.69, or short sales, 3.86. More over, simultaneously relaxing both types of frictions increases the average arbitrage amount to 4.19: counter to what we expect to happen when liberating the invisible hand.

Also, Sell arbitrage is more prevalent than Buy arbitrage in terms of magnitude and proportions. In fact, the majority of trades are Sell arbitrages in all treatments. The Short sale treatment is the exception. The magnitude of Sell arbitrage in this treatment is the lowest of the non-capitalization treatments. Further, Sell arbitrage only makes up 35% of the total transactions, while Buy arbitrage makes up the majority, 54%. This is consistent with [85] who find that short sales tend to dampen prices while failing to establish rational expectation ones.

Table 4.3 also provides insights into the microstructure of how arbitrage occurs. In all treatments, Implicit arbitrage occurs more frequently than Explicit arbitrage. Further in all cases, except for Buy arbitrage in the Baseline and Big endowment treatments, the average magnitude of Implicit exceeds Explicit ones. To summarize, the strategy to generate the most frequent and largest arbitrages is submitting limit asks above the fundamental value.

We provide further statistical evidence of our results and investigate the dynamic evolution of arbitrage in our markets through linear regression analysis. The average arbitrage amount in a trading period is our unit of observation, and we filter out periods where there are no arbitrages. Average arbitrage amount is the most economically sensible dependent variable; theoretically there are no comparative static predictions on the percentage of trades involving arbitrage as non-arbitrage trades lead to no change in portfolio valuations and thus can vary from zero to as many market ticks as possible in any treatment. However, we do observe varying numbers of arbitrages across periods and treatments and this does introduce a structural form of heteroskedasticity. Accordingly, we use weighted least squares (WLS) regression models.[12] For concern out of other forms of heteroskedasticity we use robust standard errors clustered at the session level when making statistical inferences.

We report the results of these WLS regressions in Table 4.4 for three dependent variables: All, Sell and Buy arbitrage. For each of these dependent variables we first estimate a simple treatment dummy-variable model, recreating the values given in Table 4.3 but allowing for more appropriate statistical evaluation. In these dummy-variable models, the t-statistics confirm that the capitalization treatments reduce the magnitude of All and Sell arbitrage, but only the Big endowment treatment significantly reduces Buy arbitrage magnitude. Removing market frictions does not reduce the magnitude of any type of arbitrage. In the second version of the WLS models we control for dynamic effects by introducing the variables Market iteration, to capture learning across market iterations, and Trading period, to control for the constriction of dividend paths within a market.[13] The trading period is not significant in the regression. This indicates that arbitrage opportunities do not diminish as time progresses in market. Here we find there is a statistically significant, but low-valued, learning trend across Market iterations; but the treatment effects are robust to adding this control. Our final models include the period's Cash-Asset ratio, a commonly identified factor that drives the formation of asset bubbles in experimental asset markets [84, 90]. We find this factor significant, positively for Sell arbitrage and negatively for Buy arbitrage.

[12]If we assume that the unobserved error of each arbitrage is independently and identically distributed, then the variance of the period average is inversely proportional to the number of arbitrages. To correct for this we use the efficient weighted least square regression technique, [89], by which we multiply the values of the dependent and independent variables by the square root of the respective period's arbitrages.

[13]Note, we have zero indexed these two variables, so that constant term reflects the average magnitude of arbitrage of trading period 1 in the first Market iteration.

Table 4.4: Weighted least square regression results for All, Sell, and Buy Arbitrage. t-statistics reported in parentheses. We use robust standard errors clustered at the session level in our statistical analyses.

	All Arbitrage			Sell Arbitrage			Buy Arbitrage		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
Competition	-1.50*	-1.50*	-1.34*	-2.05*	-2.11*	-1.94*	-0.17	-0.075	-0.097
	(-2.22)	(-2.21)	(-2.34)	(-2.33)	(-2.40)	(-2.19)	(-0.25)	(-0.11)	(-0.14)
Big endowment	-1.96**	-1.94**	-2.00**	-2.27**	-2.28**	-2.29**	-1.03*	-0.96*	-0.63
	(-2.89)	(-2.88)	(-2.94)	(-2.81)	(-2.87)	(-2.81)	(-2.32)	(-2.27)	(-1.56)
Constant	4.70***	5.40***	4.18***	5.41***	6.07***	4.41**	3.16***	3.51***	4.21***
(Baseline level)	(7.16)	(6.54)	(5.19)	(6.26)	(6.17)	(4.51)	(6.13)	(5.75)	(4.83)
Liquidity	-1.01	-0.97	-0.77	-1.21	-1.19	-0.85	-0.80	-0.71	-0.86
	(-1.10)	(-1.07)	(-0.95)	(-1.06)	(-1.07)	(-0.78)	(-1.25)	(-1.11)	(-1.20)
Short sale	-0.84	-0.78	-0.70	-1.64	-1.67	-1.95*	0.77	0.90	0.97
	(-1.44)	(-1.39)	(-1.47)	(-1.83)	(-2.07)	(-2.43)	(1.10)	(1.31)	(1.32)
Liquidity	-0.51	-0.38	-0.50	-0.48	-0.41	-0.64	-0.18	0.044	0.083
+ Short sale	(-0.46)	(-0.36)	(-0.49)	(-0.34)	(-0.30)	(-0.45)	(-0.38)	(0.10)	(0.18)
Market iteration		-0.25**	-0.20*		-0.22*	-0.16		-0.25*	-0.25*
		(-3.65)	(-2.25)		(-2.64)	(-1.44)		(-2.60)	(-2.61)
Trading period		-0.15	-0.12		-0.15	-0.20		0.011	-0.018
		(-1.34)	(-1.27)		(-0.80)	(-1.29)		(0.10)	(-0.15)
Cash-Asset Ratio			0.41***			0.51***			-0.45*
			(6.52)			(5.93)			(-2.30)
R^2	0.05	0.07	0.21	0.07	0.08	0.29	0.06	0.09	0.15
Observations	731	731	731	543	543	543	430	430	430

4.3.2. Market efficiency

Efficient market theories often rely upon a no-arbitrage assumption. This relationship is abundantly clear in our set-up as the dividend process renders every mis-priced transaction an arbitrage. In this subsection we investigate how our various treatments impact market efficiency. Here we focus on price deviations from the fundamental value, as symmetric information and homogeneous traders' preferences make price efficiency a sufficient condition for market efficiency. Here we find that differential degrees of arbitrage leads to similar differences in market efficiency.

The summary statistics in Table 4.5 suggest that increases in capitalization improve market efficiency, while the relaxations of market frictions fail to do so. The first column, FV, reports the average realized fundamental value across trading periods and the second column, Price, reports the average of the average price within periods. We report the results of t-tests that Price = FV, by using dagger indicators for rejections. We reject no price bias for the Baseline and all of the market friction treatments, but we fail to reject no price bias in the capitalization treatments. In the last column we report the average volume of transactions in a period. Volume is statistically greater in all treatments relative to the Baseline. Further, but not surprising, both Competition and Liquidity + Short Sale treatments have very high volumes.

The next four columns of Table 4.5 report various commonly used price efficiency measures [91] and compare them to the Baseline levels. The third column, PD, is the average difference of the average transaction price of a period and the fundamental value; the second column value less the first column value. In this case we evaluate whether this deviation is statistically differs from the Baseline treatment. Here we find our two capitalization treatments have smaller price biases. The Short sale treatment has a lower bias, but is in fact negative and of a similar magnitude as the Baseline.

Table 4.5: Summary statistics for Market Efficiency

	FV	Price	PD	APD	RPD	RAPD	Volume
Big endowment	17.50	18.15	0.66ˆ***	2.22ˆ***	0.12	0.19	9.87ˆ***
	(6.29)	(4.92)	(3.17)	(2.35)	(0.46)	(0.43)	(5.00)
Competition	17.21	17.81	0.61ˆ***	2.56ˆ**	0.11ˆ*	0.20ˆ*	30.41ˆ**
	(5.20)	(3.77)	(3.38)	(2.28)	(0.36)	(0.32)	(11.87)
Baseline	17.21	19.08ˆ†††	1.95	3.39	0.22	0.29	6.48
	(5.73)	(4.59)	(4.27)	(3.24)	(0.56)	(0.53)	(3.55)
Liquidity	18.74	20.86ˆ†††	2.17	3.01	0.20	0.24	9.23ˆ***
	(5.94)	(4.16)	(3.36)	(2.63)	(0.38)	(0.36)	(5.18)
Short sale	16.92	15.70ˆ††	-1.22ˆ***	3.11	0.00ˆ***	0.20	10.85ˆ***
	(5.20)	(2.57)	(3.88)	(2.62)	(0.33)	(0.26)	(5.05)
Liquidity + Short sale	16.44	17.50ˆ††	1.06ˆ*	3.10	0.19	0.28	17.40ˆ***
	(5.56)	(3.77)	(4.08)	(2.85)	(0.54)	(0.49)	(7.98)

Note 1: PD refers to Prices Deviation. $PD = Price - FV$, where FV refers to the fundamental value of the asset. APD refers to Absolute Price Deviation. $APD = |Price - FV|$. RPD refers to Relative Price Deviation. RPD = $(Price - FV)/FV$. RAPD refers to Relative Absolute Deviation= $|Price - FV|/FV$.
Note 2: In the table, we reported the mean values of these measurements. The standard deviations are in parentheses.
Note 3: We report t-tests evaluating the difference between the baseline and other treatments. If we observe the measurement in a treatment is significantly below that in the baseline, then we use $*$'s to indicate p-values of $^{***}p < 0.01$,$^{**}p < 0.05$ and $^{*}p < 0.1$. If we observe the measurement in a treatment is significantly above that in the baseline, then we use †'s to indicate p-values $^{\dagger\dagger\dagger}p < 0.01$,$^{\dagger\dagger}p < 0.05$ and $^{\dagger}p < 0.1$.

Under PD a positive and negative price deviation will tend to cancel each other out. To counter this, we examine the average absolute price deviation, APD. Under this measure we find only the capitalization treatments lead to a significant increase in market efficiency.

Some may argue that proportional price deviations are more meaningful than nominal deviations. In our environment the fundamental value can range from 3 to 33, which could lead to meaningful proportional differences. Columns 5 and 6 of Table 4.5 report the average relative price deviations, RPD, and the relative absolute price deviations, RAPD. We find only marginal evidence of market efficiency for the Competition treatment in terms of these two measures.

4.3.3. Terminal portfolios and wealth distributions

Arbitrage, in our setting, generates wealth redistribution. A clear welfare concern is how do market frictions and capitalization impact wealth inequality. Figure 4.10 depicts wealth inequality and heterogeneity of terminal portfolios through an array of density plots with iso-wealth lines for zero wealth and the valuation of the initial endowment plotted for reference. Note we use the 15 terminal valuation to calculate the value of asset holdings, giving the iso-wealth lines a slope of -1.

In the Baseline plot, there is a noticeable clustering of corner portfolios, either all pesos or all asset[14], suggesting market frictions are binding. When we allow just short sales there is a predictable spread of terminal portfolios holding negative asset quantities. Moreover, traders appears to have heterogeneous capabilities in managing this market feature. A number of traders' portfolios lie near the zero wealth line, including some who have lost all of their peso and asset endowments. Introducing just liquidity results in a spread of leveraged terminal portfolios, but not as many near zero wealth portfolios as in the Short sale treatment. In the Liquidity + Short sale treatment the diversity of terminal portfolios and wealth distributions is more extreme than one would get from simply summing the "spreads" of the Liquidity and Short sale treatments.

Our two forms of increased capitalization both effectively reduced arbitrage and market inefficiencies, but have differential impact on terminal portfolios and wealth distributions. In the Big endowment treatment, we divide the terminal values by two and one-half to place them on the same scale as the other treatments. Here we see density massed on interior portfolios and little dispersion in wealth. The Competition treatment has more profound impact. We see mass is more concentrated on the corner portfolios. Further there is an increasing variance in wealth as the final asset holdings go to zero. There are also a number of traders who seem to "lose it all."

[14]Recall that portfolios with large asset-to-peso ratios, such these all asset portfolios, can generate negative peso holdings through negative dividends.

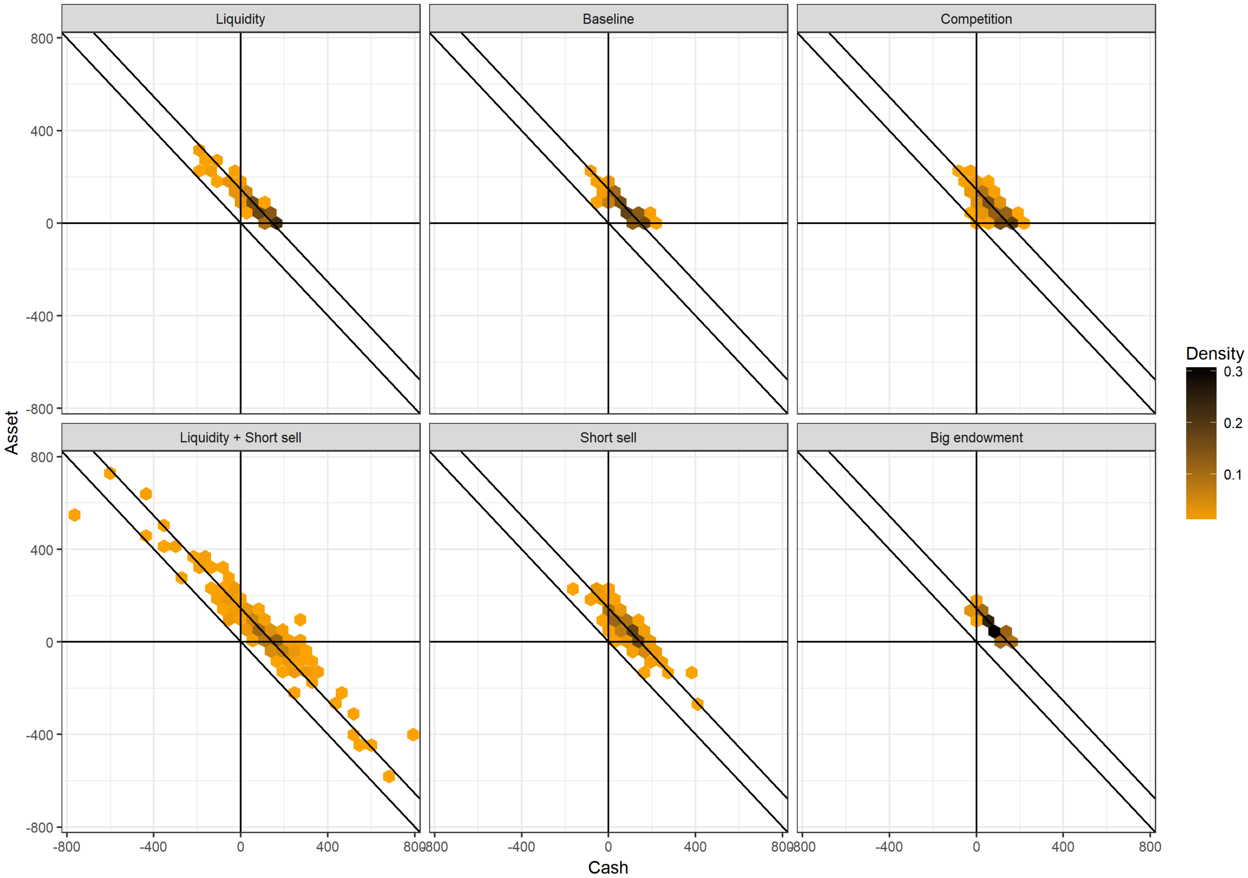

Figure 4.10: The empirical distributions of terminal portfolios plotted by hexagonal binning. Each asset unit held is evaluated by its terminal 21 redemption. The two reference lines with slope of -1 represent equi-wealth portfolios of the initial endowment and zero.

We quantify the relative inequality of wealth distributions by calculating the Gini coefficients[15] of the terminal wealth levels given in Figure 4.10 and report them in Table 4.6. These Gini coefficients confirm our observations that one finds the lowest wealth inequality in our Big endowment treatment, and the greatest inequality in the Competition and Liquidity + Short sale treatments. We find this result regarding the Competition treatment surprising. On one hand, the tremendous increase in liquidity, both in terms of total values of limit orders and the number of orders in the books, reduces arbitrage - and in turn in-

[15]The Gini coefficient is defined a $G = \sum_{i=1}^{M} \sum_{j=1}^{M} \sum_{s=1}^{5} \sum_{t=1}^{5} |w_{i,s} - w_{j,t}| 2M \sum_{i=1}^{M} \sum_{s=1}^{5} w_{i,s}$, where M is the total number of traders in a treatment, 40 in all except for 100 in the Competition treatment, and $w_{i,s}$ is trader i's earnings in Market s.

ducing greater market efficiency. But on the other hand, this form of increased competition drives greater wealth inequality.

4.4. Conclusion

The absence of arbitrage is an important indicator of well functioning and efficient asset markets. Consequently, the frequent occurrence of persistent arbitrage in financial markets prompts regulators to seek out reforms to extinguish these occurrences. While regulators have a myriad of reform levers at there disposal, there is a dearth of causal evidence on their respective effectiveness. Our study generates surprising causal evidence on the effectiveness of alternative policies. Namely the general relaxation of short sales and leveraged purchases does not reduce arbitrage opportunities while increasing income inequality. In contrast, increased market capitalization reduces arbitrage opportunities. This comes with the caveat that simply increasing the number of similarly wealthy traders can lead to increases in wealth inequality.

U.S. and Chinese financial markets, and their respective regulatory bodies, illustrate alternative challenges and approaches to financial market regulation. Chinese regulators have typically imposed stricter regulations on short sales and leveraged purchases. For instance, Chinese rules require traders to have at least 500,000 (around $80,000) in their margin account while traders in US usually need only $2000 in their accounts. Chinese

Table 4.6: Gini Coefficient of terminal wealth for each treatment

Treatment	Gini Coefficient
Competition	0.098
Big endowment	0.032
Baseline	0.078
Liquidity	0.071
Short sale	0.118
Liquidity + Short sale	0.192

regulators have considered allowing more liberal limitations on short sales and leveraged purchases. Our experiments suggests more liberal limitations will not diminish arbitrage, but will increase wealth inequality. Such increases could have widespread consequences as retail investors dominate Chinese stock markets.

U.S. financial markets are mature and offer access to investors from most parts of the world. In contrast Chinese markets are still developing and have been closed to foreign investors and until recently were only were accessed by a small proportion of Chinese households. However, the past decade has seen a dramatic increase the number of domestic households investing in Chinese stock markets and a gradual granting of access to foreign traders. To elaborate, retail investors account for around 85 percent of transactions in Chinese stock markets. This is unlike other major stock markets, which are dominated by professional money managers. Our experimental results suggests this trend increases the risk of greater wealth inequality, and suggest one way to garner the benefits of more limited arbitrage while mitigating this risk. Increase trader endowments not their number. In practical terms this could be achieved through the development of larger institutional investors in which individual households take vested interests. But such recommendations of consolidation in markets must also caution that this could lead to monopolistic power and excess management fees charged to the public if left unchecked.

Finally our results suggests that persistent arbitrage may arise in markets as an unintended policy consequence. Several substantial pieces of regulatory reform since the financial crisis of 2008 are motivated by limiting the size of trading entities in financial markets. For example, provisions in the 2010 Dodd-Frank legislation mandated that banks with over fifty billion dollars in assets enter come under strict oversight of the U.S. Federal Reserve Bank. Our results suggests this could lead to the unintended consequences of more incidences of persistent arbitrage in markets and, the perhaps more negative consequent of greater wealth inequality.

BIBLIOGRAPHY

[1] P. Temin, *The great recession & the great depression*, *Daedalus* **139** (2010) 115–124. 5

[2] K. J. Murphy, *Executive compensation: Where we are, and how we got there*, *Handbook of the Economics of Finance. Elsevier Science North Holland (Forthcoming)* (2012) . 5, 8

[3] G. Tullock, *Rent seeking as a negative-sum game*, *Toward a theory of the rent-seeking society* **16** (1980) 36. 8, 11

[4] M. Meyer, P. Milgrom and J. Roberts, *Organizational prospects, influence costs, and ownership changes*, *Journal of Economics & Management Strategy* **1** (1992) 9–35. 8, 13

[5] S. Bhattacharyya and F. Lafontaine, *Double-sided moral hazard and the nature of share contracts*, *The RAND Journal of Economics* (1995) 761–781. 8, 13

[6] P. Bolton and D. S. Scharfstein, *A theory of predation based on agency problems in financial contracting*, *The American economic review* (1990) 93–106. 10

[7] J. J. Rotemberg and D. S. Scharfstein, *Shareholder-value maximization and product-market competition*, *Review of Financial Studies* **3** (1990) 367–391. 10

[8] D. Kovenock and G. M. Phillips, *Capital structure and product market behavior: An examination of plant exit and investment decisions*, *Review of Financial Studies* **10** (1997) 767–803. 10

[9] J. Vickers, *Delegation and the theory of the firm*, *The Economic Journal* **95** (1985) 138–147. 10

[10] C. Fershtman and K. L. Judd, *Equilibrium incentives in oligopoly*, *The American Economic Review* (1987) 927–940. 10

[11] S. D. Sklivas, *The strategic choice of managerial incentives*, *The RAND Journal of Economics* (1987) 452–458. 10

[12] D. Reitman, *Stock options and the strategic use of managerial incentives*, *The American Economic Review* (1993) 513–524. 10

[13] R. K. Aggarwal and A. A. Samwick, *Executive compensation, strategic competition, and relative performance evaluation: Theory and evidence*, *The Journal of Finance* **54** (1999) . 10, 11

[14] C. P. Jones and R. H. Litzenberger, *Quarterly earnings reports and intermediate stock price trends*, *The Journal of Finance* **25** (1970) 143–148. 13

[15] J. M. Patell and M. A. Wolfson, *The intraday speed of adjustment of stock prices to earnings and dividend announcements*, *Journal of Financial Economics* **13** (1984) 223–252. 13

[16] A. Tversky and D. Kahneman, *Advances in prospect theory: Cumulative representation of uncertainty*, *Journal of Risk and uncertainty* **5** (1992) 297–323. 18, 19

[17] M. Abdellaoui, H. Bleichrodt and O. l'Haridon, *A tractable method to measure utility and loss aversion under prospect theory*, *Journal of Risk and uncertainty* **36** (2008) 245–266. 19

[18] G. W. Harrison and E. E. Rutström, *Expected utility theory and prospect theory: One wedding and a decent funeral*, *Experimental Economics* **12** (2009) 133–158. 19

[19] E. Briys and H. Schlesinger, *Risk aversion and the propensities for self-insurance and self-protection*, *Southern Economic Journal* (1990) 458–467. 19

[20] S. Skaperdas and L. Gan, *Risk aversion in contests*, *The Economic Journal* (1995) 951–962. 19

[21] K. A. Konrad and H. Schlesinger, *Risk aversion in rent-seeking and rent-augmenting games*, *The Economic Journal* **107** (1997) 1671–1683. 19

[22] M. McGuire, J. Pratt and R. Zeckhauser, *Paying to improve your chances: Gambling or insurance?*, *Journal of Risk and Uncertainty* **4** (1991) 329–338. 19

[23] E. L. Millner and M. D. Pratt, *Risk aversion and rent-seeking: An extension and some experimental evidence*, *Public Choice* **69** (1991) 81–92. 19

[24] C. A. Holt and S. Laury, *Risk aversion and incentive effects*, . 20

[25] J. C. Cox and R. L. Oaxaca, *Is bidding behavior consistent with bidding theory for private value auctions?*, *Research in experimental economics* **6** (1996) 131–148. 20

[26] J. K. Goeree, C. A. Holt and T. R. Palfrey, *Risk averse behavior in asymmetric matching pennies games*, tech. rep., Discussion Paper, University of Virginia, 2000. 20

[27] U. Fischbacher, *z-tree: Zurich toolbox for ready-made economic experiments*, *Experimental economics* **10** (2007) 171–178. 25, 66, 98

[28] E. Baharad and S. Nitzan, *Contest efforts in light of behavioural considerations*, *The Economic Journal* **118** (2008) 2047–2059. 27, 35

[29] T. Ahn, R. M. Isaac and T. C. Salmon, *Rent seeking in groups*, *International Journal of Industrial Organization* **29** (2011) 116–125. 27, 35

[30] R. M. Sheremeta, *Overbidding and heterogeneous behavior in contest experiments*, *Journal of Economic Surveys* **27** (2013) 491–514. 27, 35

[31] A. C. Savikhin and R. M. Sheremeta, *Simultaneous decision-making in competitive and cooperative environments*, *Economic Inquiry* **51** (2013) 1311–1323. 27, 35

[32] S. M. Chowdhury, R. M. Sheremeta and T. L. Turocy, *Overbidding and overspreading in rent-seeking experiments: Cost structure and prize allocation rules*, *Games and Economic Behavior* **87** (2014) 224–238. 27, 35

[33] E. Dechenaux, D. Kovenock and R. M. Sheremeta, *A survey of experimental research on contests, all-pay auctions and tournaments*, *Experimental Economics* **18** (2015) 609–669. 27, 35

[34] M. A. El-Gamal and D. M. Grether, *Are people bayesian? uncovering behavioral strategies*, *Journal of the American statistical Association* **90** (1995) 1137–1145. 37

[35] C. M. Anderson and L. Putterman, *Do non-strategic sanctions obey the law of demand? the demand for punishment in the voluntary contribution mechanism*, *Games and Economic Behavior* **54** (2006) 1–24. 37

[36] D. M. Titterington, A. F. Smith and U. E. Makov, *Statistical analysis of finite mixture distributions*. Wiley,, 1985. 37

[37] G. McLachlan and D. Peel, *Finite mixture models*. John Wiley & Sons, 2004. 37, 38

[38] A. P. Dempster, N. M. Laird and D. B. Rubin, *Maximum likelihood from incomplete data via the em algorithm*, *Journal of the royal statistical society. Series B (methodological)* (1977) 1–38. 38

[39] F. Leisch, *FlexMix: A general framework for finite mixture models and latent class regression in R*, *Journal of Statistical Software* **11** (2004) 1–18. 38

[40] J. S. Chhokar and J. A. Wallin, *A field study of the effect of feedback frequency on performance.*, *Journal of Applied Psychology* **69** (1984) 524. 50

[41] N. Lurie and J. Swaminathan, *Is timely information always better? the effect of feedback frequency on performance and knowledge acquistion*, tech. rep., Working Paper, Georgia Institute of Technology, 2007. 50

[42] F. Balcazar, B. L. Hopkins and Y. Suarez, *A critical, objective review of performance feedback*, *Journal of Organizational Behavior Management* **7** (1985) 65–89. 50

[43] R. Benabou and J. Tirole, *Laws and norms. iza discussion papers 6290*, *Institute for the Study of Labor (IZA)* (2012) . 50

[44] J. Andreoni and B. D. Bernheim, *Social image and the 50–50 norm: A theoretical and experimental analysis of audience effects*, *Econometrica* **77** (2009) 1607–1636. 50, 82

[45] J. Andreoni, *Impure altruism and donations to public goods: A theory of warm-glow giving*, *The economic journal* **100** (1990) 464–477. 51

[46] J. Andreoni and R. Petrie, *Public goods experiments without confidentiality: a glimpse into fund-raising*, *Journal of public Economics* **88** (2004) 1605–1623. 51, 82

[47] D. Karlan and M. A. McConnell, *Hey look at me: The effect of giving circles on giving*, *Journal of Economic Behavior & Organization* **106** (2014) 402–412. 51, 82

[48] T. C. Salmon and D. Serra, *Corruption, social judgment and culture: An experiment*, *Journal of Economic Behavior & Organization* **142** (2017) 64–78. 51, 82

[49] L. Bursztyn and R. Jensen, *Social image and economic behavior in the field: Identifying, understanding, and shaping social pressure*, Annual Review of Economics **9** (2017) 131–153. 51, 82

[50] D. Masclet, C. Noussair, S. Tucker and M.-C. Villeval, *Monetary and nonmonetary punishment in the voluntary contributions mechanism*, American Economic Review **93** (2003) 366–380. 51, 82

[51] E. Xiao and D. Houser, *Punish in public*, Journal of Public Economics **95** (2011) 1006–1017. 51, 82

[52] B. S. Frey, *Does monitoring increase work effort? the rivalry with trust and loyalty*, Economic Inquiry **31** (1993) 663–670. 52

[53] D. Dickinson and M. C. Villeval, *Does monitoring decrease work effort?*, Games and Economic Behavior/Games and Economic Behaviour **63** (2008) pp–56. 52

[54] A. Falk and M. Kosfeld, *The hidden costs of control*, American Economic Review **96** (2006) 1611–1630. 52

[55] M. Belot and M. Schröder, *The spillover effects of monitoring: A field experiment*, Management science **62** (2015) 37–45. 52

[56] A. Merlo and A. Schotter, *A surprise-quiz view of learning in economic experiments*, Games and Economic Behavior **28** (1999) 25–54. 52

[57] U. Gneezy, A. Kapteyn and J. Potters, *Evaluation periods and asset prices in a market experiment*, The Journal of Finance **58** (2003) 821–837. 52

[58] G. Manso, *Motivating innovation*, The Journal of Finance **66** (2011) 1823–1860. 52

[59] F. Ederer and G. Manso, *Is pay for performance detrimental to innovation?*, Management Science **59** (2013) 1496–1513. 52

[60] X. Tian and T. Y. Wang, *Tolerance for failure and corporate innovation*, The Review of Financial Studies **27** (2011) 211–255. 52

[61] P. Azoulay, J. S. Graff Zivin and G. Manso, *Incentives and creativity: evidence from the academic life sciences*, The RAND Journal of Economics **42** (2011) 527–554. 52

[62] G. Dutcher, T. Salmon and K. J. Saral, *Is' real'effort more real?*, . 52

[63] O. Bock, I. Baetge and A. Nicklisch, *hroot: Hamburg registration and organization online tool*, European Economic Review **71** (2014) 117–120. 66

[64] J. Dana, D. M. Cain and R. M. Dawes, *What you don't know won't hurt me: Costly (but quiet) exit in dictator games*, Organizational Behavior and human decision Processes **100** (2006) 193–201. 82

[65] J. Dana, R. A. Weber and J. X. Kuang, *Exploiting moral wiggle room: experiments demonstrating an illusory preference for fairness*, Economic Theory **33** (2007) 67–80. 82

[66] F. Modigliani and M. H. Miller, *The cost of capital, corporation finance and the theory of investment*, American Economic Review **48** (1958) 261–297. 84

[67] F. Black and M. Scholes, *The pricing of options and corporate liabilities*, *Journal of Political Economy* **81** (1973) 637–654. 84

[68] S. A. Ross et al., *Return, risk and arbitrage*. Rodney L. White Center for Financial Research, The Wharton School, University of Pennyslvania, 1973. 84, 87

[69] S. A. Ross, *The arbitrage theory of capital asset pricing*, . 84, 87

[70] O. A. Lamont and R. H. Thaler, *Can the market add and subtract? mispricing in tech stock carve-outs*, *Journal of Political Economy* **111** (2003) 227–268. 85

[71] J. Pontiff, *Costly arbitrage: Evidence from closed-end funds*, *Quarterly Journal of Economics* **111** (1996) 1135–1151. 85

[72] T. Mancini-Griffoli and A. Ranaldo, *Limits to arbitrage during the crisis: funding liquidity constraints and covered interest parity*, . 85

[73] J. B. De Long, A. Shleifer, L. H. Summers and R. J. Waldmann, *Noise trader risk in financial markets*, *Journal of Political Economy* **98** (1990) 703–738. 85

[74] A. Shleifer and R. W. Vishny, *The limits of arbitrage*, . 85

[75] J. T. Scruggs, *Noise trader risk: Evidence from the Siamese twins*, *Journal of Financial Markets* **10** (2007) 76–105. 86

[76] T. A. Rietz, *Behavioral mis-pricing and arbitrage in experimental asset markets*, working paper, University of Iowa, 2005. 89

[77] G. Charness and T. Neugebauer, *A test of the Modigliani-Miller invariance theorem and arbitrage in experimental asset markets*, *The Journal of Finance* (2017) . 89

[78] E. Asparouhova, C. Besliu, M. Lemmon and B. Rock, *Payout policy, investor rationality, and market efficiency: Evidence from laboratory experiments*, working paper, University of Utah, 2016. 89

[79] J. O'Brien and S. Srivastava, *Dynamic stock markets with multiple assets: An experimental analysis*, *The Journal of Finance* **46** (1991) 1811–1838. 89

[80] V. L. Smith, G. L. Suchanek and A. W. Williams, *Bubbles, crashes, and endogenous expectations in experimental spot asset markets*, *Econometrica* **56** (September, 1988) 1119–51. 89, 98

[81] D. P. Porter and V. L. Smith, *Stock market bubbles in the laboratory*, *Applied Mathematical Finance* **1** (1994) 111–128. 89

[82] C. N. Noussair and O. Powell, *Peaks and valleys: Price discovery in experimental asset markets with non-monotonic fundamentals*, *Journal of Economic Studies* **37** (2010) 152–180. 89

[83] C. Noussair, S. Robin and B. Ruffieux, *Price bubbles in laboratory asset markets with constant fundamental values*, *Experimental Economics* **4** (2001) 87–105. 89

[84] M. Kirchler, J. Huber and T. Stöckl, *Thar she bursts: Reducing confusion reduces bubbles*, *American Economic Review* **102** (April, 2012) 865–83. 89, 110

[85] E. Haruvy and C. N. Noussair, *The effect of short selling on bubbles and crashes in experimental spot asset markets*, *The Journal of Finance* **61** (2006) 1119–1157. 90, 109

[86] L. F. Ackert, N. Charupat, B. K. Church and R. Deaves, *An experimental examination of the house money effect in a multi-period setting*, *Experimental Economics* **9** (2006) 5–16. 90

[87] S. Palan, *A review of bubbles and crashes in experimental asset markets*, *Journal of Economic Surveys* **27** (2013) 570–588. 98

[88] B. Greiner, *An online recruitment system for economic experiments*, in *Forschung und wissenschaftliches Rechnen* (K. Kremer and V. Macho, eds.), vol. 63 of *Ges. fur Wiss. Datenverarbeitung*, pp. 79–93. GWDG Bericht, 2004. 100

[89] H. S. Houthakker, *Some calculations on electricity consumption in great britain*, *Journal of the Royal Statistical Society. Series A (General)* **114** (1951) 359–371. 110

[90] G. Caginalp, D. Porter and V. Smith, *Initial cash/asset ratio and asset prices: An experimental study*, *Proceedings of the National Academy of Sciences* **95** (1998) 756–761. 110

[91] T. Stöckl, J. Huber and M. Kirchler, *Bubble measures in experimental asset markets*, *Experimental Economics* **13** (2010) 284–298. 112

[92] F. C. I. Commission et al., *Report on the causes of the financial crisis*, *US Government Printing Office* (2011) .

[93] C. W. Smith and R. M. Stulz, *The determinants of firms' hedging policies*, *Journal of financial and quantitative analysis* **20** (1985) 391–405.

[94] D. O. May, *Do managerial motives influence firm risk reduction strategies?*, *The Journal of Finance* **50** (1995) 1291–1308.

[95] R. A. Lambert, *Executive effort and selection of risky projects*, *The Rand Journal of Economics* (1986) 77–88.

[96] D. Hirshleifer and Y. Suh, *Risk, managerial effort, and project choice*, *Journal of Financial Intermediation* **2** (1992) 308–345.

[97] W. R. Guay, *The sensitivity of ceo wealth to equity risk: an analysis of the magnitude and determinants*, *Journal of Financial Economics* **53** (1999) 43–71.

[98] J. L. Coles, N. D. Daniel and L. Naveen, *Managerial incentives and risk-taking*, *Journal of financial Economics* **79** (2006) 431–468.

[99] S. Rajgopal and T. Shevlin, *Empirical evidence on the relation between stock option compensation and risk taking*, *Journal of Accounting and Economics* **33** (2002) 145–171.

[100] M. C. Jensen and W. H. Meckling, *Theory of the firm: Managerial behavior, agency costs and ownership structure*, *Journal of financial economics* **3** (1976) 305–360.

[101] Z. Dong, C. Wang and F. Xie, *Do executive stock options induce excessive risk taking?*, *Journal of Banking & Finance* **34** (2010) 2518–2529.

[102] W. G. Lewellen et al., *Executive compensation in large industrial corporations*, *NBER Books* (1968) .

[103] E. L. Millner and M. D. Pratt, *An experimental investigation of efficient rent-seeking*, *Public Choice* **62** (1989) 139–151.

[104] D. D. Davis and R. J. Reilly, *Do too many cooks always spoil the stew? an experimental analysis of rent-seeking and the role of a strategic buyer*, *Public Choice* **95** (1998) 89–115.

[105] K. Abbink, J. Brandts, B. Herrmann and H. Orzen, *Intergroup conflict and intra-group punishment in an experimental contest game*, *The American Economic Review* **100** (2010) 420–447.

[106] G. W. Harrison, *Theory and misbehavior of first-price auctions*, *The American Economic Review* (1989) 749–762.

[107] ATLAS collaboration, M. Aaboud et al., *Measurement of the Inelastic Proton-Proton Cross Section at $\sqrt{s} = 13$ TeV with the ATLAS Detector at the LHC*, *Phys. Rev. Lett.* **117** (2016) 182002, [1606.02625].

[108] O. S. Brüning, P. Collier, P. Lebrun, S. Myers, R. Ostojic, J. Poole et al., *LHC Design Report*. CERN Yellow Reports: Monographs. CERN, Geneva, 2004.

[109] P. W. Higgs, *Broken symmetries, massless particles and gauge fields*, *Phys. Lett.* **12** (1964) 132–133.

[110] J. Abu-Doleh and D. Weir, *Dimensions of performance appraisal systems in jordanian private and public organizations*, *The International Journal of Human Resource Management* **18** (2007) 75–84.

[111] A. S. DeNisi and R. D. Pritchard, *Performance appraisal, performance management and improving individual performance: A motivational framework*, *management and Organization Review* **2** (2006) 253–277.

[112] K. Manasa and N. Reddy, *Role of training in improving performance*, .

[113] S. Selden and J. E. Sowa, *Performance management and appraisal in human service organizations: Management and staff perspectives*, *Public Personnel Management* **40** (2011) 251–264.

[114] D. Cederblom, *The performance appraisal interview: A review, implications, and suggestions*, *Academy of Management Review* **7** (1982) 219–227.

[115] A. S. Gerber, D. P. Green and C. W. Larimer, *Social pressure and voter turnout: Evidence from a large-scale field experiment*, *American political Science review* **102** (2008) 33–48.

[116] M. Rege and K. Telle, *The impact of social approval and framing on cooperation in public good situations*, *Journal of public Economics* **88** (2004) 1625–1644.

[117] J. Carpenter and E. Seki, *Do social preferences increase productivity? field experimental evidence from fishermen in toyama bay*, *Economic Inquiry* **49** (2011) 612–630.

[118] M. Schraeder, J. B. Becton and R. Portis, *A critical examination of performance appraisals: An organization's friend or foe?*, *The journal for quality and participation* **30** (2007) 20.

[119] A. Sudarsan, *Performance appraisal systems: A survey of organizational views*, *The IUP Journal of Organizational Behaviour* (2009) .

[120] A. N. Kluger and A. DeNisi, *The effects of feedback interventions on performance: A historical review, a meta-analysis, and a preliminary feedback intervention theory.*, *Psychological bulletin* **119** (1996) 254.

[121] A. M. Alvero, B. R. Bucklin and J. Austin, *An objective review of the effectiveness and essential characteristics of performance feedback in organizational settings (1985-1998)*, *Journal of Organizational Behavior Management* **21** (2001) 3–29.

[122] K. Kang, S. Oah and A. M. Dickinson, *The relative effects of different frequencies of feedback on work performance: A simulation*, *Journal of Organizational Behavior Management* **23** (2005) 21–53.

[123] G. B. Northcraft, A. M. Schmidt and S. J. Ashford, *Feedback and the rationing of time and effort among competing tasks.*, *Journal of Applied Psychology* **96** (2011) 1076.

[124] C. F. Lam, D. S. DeRue, E. P. Karam and J. R. Hollenbeck, *The impact of feedback frequency on learning and task performance: Challenging the "more is better" assumption*, *Organizational Behavior and Human Decision Processes* **116** (2011) 217–228.

[125] P. CASAS-ARCE, S. M. Lourenço and F. A. MARTÍNEZ-JEREZ, *The performance effect of feedback frequency and detail: Evidence from a field experiment in customer satisfaction*, *Journal of Accounting Research* **55** (2017) 1051–1088.

[126] G. E. Bolton and E. Katok, *Learning by doing in the newsvendor problem: A laboratory investigation of the role of experience and feedback*, *Manufacturing & Service Operations Management* **10** (2008) 519–538.

[127] R. Radner, *Rational expectations equilibrium: Generic existence and the information revealed by prices*, .

[128] W. F. M. De Bondt and R. H. Thaler, *Does the stock market overreact?*, *The Journal of Finance* **40** (1985) 793–805.

[129] O. R. Phillips, D. J. Menkhaus and J. L. Krogmeier, *Laboratory behavior in spot and forward auction markets*, *Experimental Economics* **4** (Dec., 2001) 243–256.

[130] E. Asparouhova, P. Bossaerts and J. O. Ledyard, *Price formation in continuous double auctions; with implications for finance*, working paper, University of Utah, 2011.

[131] J. Mei, J. Scheinkman and W. Xiong, *Speculative trading and stock prices: Evidence from Chinese A/B share premia*, tech. rep., National Bureau of Economic Research, 2005.

[132] B. G. Malkiel and E. F. Fama, *Efficient capital markets: A review of theory and empirical work*, *The Journal of Finance* **25** (1970) 383–417.

[133] F. Modigliani and M. H. Miller, *The cost of capital, corporation finance and the theory of investment*, *American Economic Review* **48** (1958) 261–297.

[134] J. C. Cox and S. A. Ross, *The valuation of options for alternative stochastic processes*, *Journal of Financial Economics* **3** (1976) 145–166.

[135] G. A. Karolyi, L. Li and R. Liao, *A (partial) resolution of the Chinese discount puzzle: the 2001 deregulation of the B-share market*, *Journal of Financial Economic Policy* **1** (2009) 80–106.

[136] B. G. Malkiel and E. F. Fama, *Efficient capital markets: A review of theory and empirical work*, *The Journal of Finance* **25** (1970) 383–417.

[137] E. F. Fama, *The behavior of stock-market prices*, *The Journal of Business* **38** (1965) 34–105.

[138] G. Caginalp, D. Porter and V. Smith, *Initial cash/asset ratio and asset prices: An experimental study*, *Proceedings of the National Academy of Sciences* **95** (1998) 756–761.