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Essays on Financial Fraud and Tax Evasion

Martin W. Tackie
University of Tennessee - Knoxville

To the Graduate Council:

I am submitting herewith a dissertation written by Martin W. Tackie entitled "Essays on Financial Fraud and Tax Evasion." I have examined the final electronic copy of this dissertation for form and content and recommend that it be accepted in partial fulfillment of the requirements for the degree of Doctor of Philosophy, with a major in Economics.

Rudy Santore, Major Professor

We have read this dissertation and recommend its acceptance:

William S. Neilson, Matthew N. Murray, Michael Price, Phillip Daves

Accepted for the Council:

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Essays on Financial Fraud and Tax Evasion

A Dissertation

Presented for the

Doctor of Philosophy

Degree

The University of Tennessee, Knoxville

Martin W. Tackie

August 2009

Dedication

I dedicate this dissertation to my wonderful family for their prayers, financial support, and countless words of encouragement. I love you all dearly.

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Abstract

This dissertation focuses on the problem of misreporting in the corporate setting, where managers may commit accounting fraud, and in the public sector, where taxpayers may not truthfully report their income. Both accounting fraud and income misreporting have contributed to unprecedented financial losses to shareholders and governments respectively. As a result, policy-makers and shareholders are focused on one goal, that is, to mitigate the occurrence of accounting fraud and income misreporting. The process of achieving this goal starts with understanding how compensation contracts and tax schemes influence an agent's willingness to misreport. This dissertation pursues these objectives using a blend of theory, experimental techniques, and exhaustive empirical analyses.

Chapter 1 has a theoretical focus; this chapter evaluates the incentive effects of various contracts within the class of stock option contracts. In this chapter, we develop a principal-agent model of managerial fraud to determine whether there exists a contract that 'dominates' another contract by generating relatively greater effort while minimizing fraud. While there exists an infinity of stock-option contracts that induce a given level of effort, we show that within the class of stock option contracts, any two contracts that induce the same effort must necessarily induce the same level of fraud. We also characterize the schedule of implementable effort-fraud pairs.

Chapters 2 and 3 have an experimental focus; in Chapter 2, we implement the theoretical model in Chapter 1 and test whether contracts that are predicted to induce the same level of effort and fraud are behaviorally equivalent. The experiment produced strong results in support of our hypothesis. The predicted equivalent class of stock option contracts induced the same level of effort and the same level of fraud. In a behavioral sense, stock option contracts are the same as simple equity contracts.

Chapter 3 focuses on tax compliance behavior under the progressive and the regressive tax systems in an experimental setting. This chapter contributes to the growing literature on tax

compliance by experimentally testing whether tax compliance behavior of taxpayers is sensitive to either the progressive or the regressive tax system. All else constant, experimental results showed no difference in average tax compliance between the progressive and the regressive tax systems. However, fairness, risk-aversion, inequality aversion, and gender played an important role in explaining variations in tax compliance behavior.

Preface

The economic theory of crime has become a mainstay approach that is used by most economists to rationalize criminal behavior problems, including agents' willingness to either misreport the true firm value or their true income. In these settings, a rational economic agent may engage in criminal behavior if the benefits outweigh the costs associated with such activities. Becker (1968) was the first to develop an economic theory of crime, in which people commit crime because the expected utility of the payoff exceeds the expected disutility of getting caught and punished.

Criminal behavior in most economic settings is prevalent when there is an asymmetric information problem. For example, across the corporate landscape, we can talk about shareholders being victims of fraudulent reporting by managers. In addition, we can also discuss the untruthful income reporting behavior of taxpayers to tax agencies. In both examples, one party has more information than the other party. *Ex-ante*, both the manager and the taxpayer believe that they would not get caught or avoid the punishment if they are caught. *Ex-post*, their actions disrupt the economic and financial health of other economic agents.

The problem of criminal behavior is addressed under the umbrella of misreporting in two ways. The first form of misreporting addressed in this dissertation is *managerial fraud*. Managerial fraud refers to any falsification or misrepresentation of a firm's true value. The second form of misreporting is the understatement of one's true income. When an agent under-reports his or her true income, it is termed *evasion*. Under these two forms of misreporting, agents face an expected penalty for providing misleading information. However, the economic gains from misreporting in these settings are sometimes worthwhile.

The problem of managerial fraud and tax evasion is not only confined to the US. These two

forms of misreporting are real and serious threats that can bring financial institutions of developed nations to their knees. There are several documented cases that have been attributed to mounting losses to either shareholders of public firms or governments. As an anecdote, the Enron scandal was estimated to cost $4,000$ employees their jobs. In 1998, Russia was at the brink of a financial crisis because the government was not able to collect sufficient tax revenue. For most governments, these revenue losses may result in reducing support for vital public projects such as hospitals, road construction, and building new schools.

This dissertation has been motivated by many factors, the most important of which is how to mitigate managerial fraud or tax evasion. Both forms of misreporting have increasingly received more attention in the wake of more corporate scandals and high mounting losses due to tax evasion. As a result, policy-makers are more oriented towards discovering ways to assuage the occurrence of managerial fraud. Firm owners today are also seeking better ways to incentivize managers without attracting fraud. Furthermore, tax authorities or governments are also interested in discovering mechanisms to discourage tax evasion in order to meet their financial obligations.

To incorporate the essential characteristics of criminal behavior, we model the agent's decision using the expected utility theory. Following Allingham and Sandmo (1972), and Andergassen (2008), we make the simple assumption that economic agents are risk neutral and face some uncertainty about being caught for misreporting either the firm's true value or income.

A manager's response to incentive contracts provided to him or her by a firm owner is complicated by the fact that managers can potentially manipulate the firm's financial report which reflect the firm's value. Goldman and Slezak (2006) and Andergassen (2008) are examples of research work that use the principal-agent model to examine the relationship between incentive contracts and managerial fraud. There are numerous research studies that have established that some incentive contracts induce unintended managerial fraud. While there are many ways of keeping fraud in check, this dissertation will focus on reducing managerial fraud through contract design.

Experiments are also employed in this dissertation to examine the behavioral response of agents to different incentive mechanisms and different tax regimes. Experiments are used in this dissertation because of the paucity of field data on managerial fraud and tax compliance. Nevertheless,

experiments provide a fertile ground to generate clean data and test the behavioral response of economic agents.

There have been made few initial attempts to examine the behavioral response of managers to incentive contracts. The only existing research work is Bruner et al. (2008) and this study looks at the behavioral response of managers as equity share and the probability of detection increases when compensated with equity-based contracts. The objective of this study is to also examine the behavioral response of managers when compensated with various forms of stock option incentive contracts using laboratory experiments.

This dissertation also focuses on tax evasion by using experiments to examine how taxpayers respond to different tax schemes. The existing literature indicates that tax compliance is affected by many factors including the magnitude of fines, detection rates, social norms, perception, and voting. Unfortunately, there is limited documentation on the tax evasion opportunities that may exist when taxpayers are given either a progressive versus a regressive tax regime. Again, an experimental approach is employed to compare the average tax compliance behavior of taxpayers under either a progressive or a regressive tax regime.

The dissertation is organized as follows: In Chapter 1 , we compare the incentive effects of various stock option contracts to examine whether there exists a contract that dominates another by inducing relatively greater effort while minimizing fraud. In Chapter 2, we implement the theory in Chapter 1 in an experimental setting. We explicitly test whether contracts that are predicted to induce the same level of effort and fraud are equivalent in a behavioral sense. The concluding chapter is an experimental analysis that focuses on the behavioral response of taxpayers under the progressive and the regressive tax regimes.

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Chapter 1

Behavioral Equivalence of Stock Option Incentive Contracts

1.1 Introduction

A common strategy for aligning the interests of managers with those of shareholders is to give the managers equity-based incentive contracts. Typically such contracts provide top management with either shares of equity or options, which allow the managers to purchase the firm's stock at a fixed price. Hall and Liebman (1998) and Hall and Murphy (2003) document that stock options were heavily used in the U.S. throughout the 1980s and 1990s. Anderson et al. (2000) also document that these incentive contracts are especially pronounced in so-called "new-economy" firms like Microsoft and Yahoo.[1] These contracts have traditionally been used to induce productive effort,[2] however, there is mounting evidence that these types of contracts may have the unintended consequence of inducing fraud. Beyond the anecdotal evidence provided by relatively recent corporate scandals (for example, Tyco International, Enron, and Worldcom), there is growing theoretical and empirical evidence linking managerial fraud to equity-based compensation.

In the words of Goldman and Slezak (2006), an equity-based compensation contract is a "double-edged sword," inducing productive effort from the manager while generating fraud as well. The unintended consequences associated with equity compensation naturally lead to the question of how to minimize a manager's incentive to commit fraud. In response to the frequent corporate scandals during the past decade, the US Congress took swift action to improve and tighten regulation laws by enacting the Sarbanes-Oxley Act of 2002 (SOX). SOX attempts to mitigate fraud through regulation by creating new and stricter standards to which public companies must adhere.[3]

Unfortunately, the capacity of firm owners to keep fraud in check is quite limited although they are not insulated from the realized financial damages after the discovery of fraud. The use of incentive contracts has only generated a dilemma for firm owners. The need for firm owners to motivate productive effort from managers while motivating effort with equity-based contracts has rather placed firm owners at a crossroad; whether to discontinue their use or find other means to mitigate the occurrence of fraud. The latter sounds reasonable and has thus far been well accepted.

[1] Both Murphy (1999) and Ittner et al. (2003) comment that this form of compensation has not been limited to top level management but has also been extended to the rank and file workers.

[2] Many theoretical studies like Jensen and Meckling (1976) favor the alignment view.

[3] See Cotten and Santore (2008) for a discussion of the whistle-blower protections contained in the Sarbanes-Oxley legislation.

For their part, owners have at least two means to control fraud. One is through monitoring of management and stronger accounting controls; the other is through contract design, the focus of this paper.

Our goal is to determine whether there exists one certain stock option contract that 'behaviorally dominates' others by inducing relatively greater effort while generating relatively less fraud. To this end, we provide a principal-agent model that allows us to compare the incentive effects of various stock option contracts. Our model is similar to Goldman and Slezak (2006) who focused on equity compensation and Andergassen (2008), who compared equity compensation to stock option. Since effort is unobservable, the manager's compensation is directly tied to the performance of the firm's stock by giving the manager a stock option contract. However, unlike standard agency models (Ross (1973) and Holmstrom (1979)), we assume the manager can also fraudulently misreport the firm's financial status, thereby artificially inflating the market value of the firm. There is an exogenous probability that the manager will be caught and sanctioned for inflating the firm's value. Thus, some fraud may go unpunished.

We show that, although there are infinitely many stock option contracts that induce a given level of effort, all such contracts necessarily induce the same level of fraud. The result implies that there is no way to mitigate fraud while maintaining a manager's incentive to provide effort. We also provide a simple characterization of equilibrium fraud as an increasing function of the induced equilibrium effort, effectively providing a schedule of effort-fraud pairs that can be induced with stock options.

A substantial stream of theoretical studies have examined the relationship between incentive contracts and managerial fraud. Goldman and Slezak (2006) consider how the optimal pay-for-performance sensitivity changes when managers can manipulate information regarding the firm's true value. Robison and Santore (2008) focus on owners' incentive to conduct *ex-ante* monitoring, which reduces the probability that fraud is possible, and *ex-post* monitoring, which increases the probability of detecting fraud that has occurred. Kadan and Yang (2006) focus on earnings management and not actual fraud. They find that an increase in the "moneyness" of options leads to an increase in earnings management.[4]

[4]There are other empirical and experimental works that focus on the relationship between various forms of com-

Andergassen (2008) focuses on the optimal compensation for the manager from the owner's perspective when the manager can commit fraud. To our knowledge, Andergassen (2008) is the only paper that compares the performance of different types of incentive contracts (for example, equity versus options) when managers can commit fraud. Andergassen finds conditions under which the owner prefers to compensate the manager with simple equity (stock option) contracts. Andergassen (2008) claims that when the long-term negative effects of fraud are sufficiently large, long-term stock price decreases as the incentives increase (that is as strike price increase); therefore stock-based compensation is optimal. However, when the long term effects are sufficiently low, then option-based compensation is optimal.

In contrast with Andergassen (2008), we focused on the optimal behavioral response of a manager by comparing the incentive effects of various stock option contracts regardless of the expected cost of the compensation contract. The result from our model suggests that if any two contracts induce the same level of effort, they will induce the same level of fraud.

We also provide a simple characterization of equilibrium fraud as an increasing function of the induced equilibrium effort, effectively providing a schedule of effort-fraud pairs that can be induced with stock options. Since the schedule of implementable effort-fraud pairs depends on the curvature of the penalty function, policy parameters then directly affect the shape of the effort-fraud pairs. Thus, improving monitoring and accounting standards would only help to induce less fraud for the same level of effort from the manager.

1.2 Model

We begin with a sketch of the model, followed by a more formal specification. There are three periods in the model. Periods 1, 2, and 3 are referred to as the *short-run* and period 4 is referred to as the *long-run*. A risk-neutral manager[5] of a publicly traded firm receives a compensation package

pensation and managerial fraud. Ke (2003), Johnson et al. (2008), Burns and Kedia (2006), and Bergstresser and Philippon (2006) empirically provide a positive link between managerial fraud and equity-based incentive contracts. However, there is a minority group that report otherwise. Erickson et al. (2006) for example, find no consistent evidence that equity-based incentives are actually associated with fraud. The study by Bruner et al. (2008) is the first to show the positive link between managerial fraud and incentive contracts in an experimental setting. They find that as the share of equity increased, fraud also increased.

[5]Of course, we assume that the risk-neutral manager is cash-constrained and hence cannot buy the firm and invest an appropriate level of effort. This assumption is consistent with the observations made within industries that

consisting of a salary and stock options, which are vested in the short-run. The manager provides unobservable effort which, along with an independent random productivity shock, determines the true value of a firm. However, risk-neutral investors in the market do not directly observe the *short-run* value of the firm, allowing the manager to fraudulently manipulate the firm's market value. In the event that the manager inflates the firm's value, the firm's *long-run* value is decreased, and with some probability, the manager is sanctioned.

Our focus is on the manager's behavior for various stock option contracts, regardless of the expected compensation. To this end, we start with a risk-neutral manager who is compensated with a stock option contract (α, K, ω), which allows the manager to buy share $\alpha(0 < \alpha \leq 1)$ of the firm at price $K \geq 0$ (the strike price), and also pays the manager a fixed salary ω. We assume the salary is large enough that the manager accepts the contract. Our result does not depend on whether the manager's participation constraint bind or if the manager earns economic rents at the given contract. Note that when $K = 0$, the stock option contract is equivalent to giving the manager a share of equity.

In period 1, a risk-neutral manager[6] chooses an effort level, e, and a fraud level, f. Throughout, random variables are indicated with tilde ($\sim$) and realized variables are indicated with no tilde. On the other hand, managerial fraud artificially inflates the value observed by investors (perhaps through misrepresentation of the firm's financial health).

In period 2, the firm's true *short-run* value [7] is a function of the manager's effort, $e \geq 0$, and random shock, $\tilde{\mu}$:

$$S_2^T(e, \tilde{\mu}) = v(e) + \mu \tag{1.1}$$

where $v' > 0$, $v'' < 0$, and $\tilde{\mu} \in [-\infty, \infty]$ has density function $g(\mu) > 0$ and cumulative density function $G(\mu)$ [8]. While the true value is as given in equation (1.1), the value of the firm observed[9]

motivate firm managers with option-based compensation contracts.

[6]The assumption of risk-neutrality allows us to solely focus on the tradeoff between a manager's effort and fraud choice. We also argue that due to liquidity constraint, the risk-neutral manager cannot buy the firm and simply invest the optimal amount of effort. This is consistent with what we observe in industries where managers are compensated with stock-based compensation contracts.

[7]Here, the value of the firm is equivalent to the firm's stock price since we normalize the number of share to one.

[8]The noise term can be interpreted as the uncertainty that exists between the period when the manager chooses effort and develops a fraud and the period when the manager exercises the stock option.

[9]The observed firm value can be interpreted as the reported earnings on the balance sheets provided to the market

by investors is the true short-run value plus the amount of managerial fraud[10]

$$S_2^O(e, f, \tilde{\mu}) = S_2^T(e, \tilde{\mu}) + f = v(e) + \tilde{\mu} + f \tag{1.2}$$

Although the market does not observe the true value of the firm, the market is rational and anticipates the manager's choice of fraud and the resulting damages to the long-run value. Let f^e denote the level of fraud expected by investors in the market. In period 3, investors incorporate their expectations, f^e, and the observed value of the firm, S_2^O, to arrive at the market price, S_3^M. Below we discuss the pricing rule used by investors. Finally, the manager exercises the options if $S_3^M > K$; that is, if the options are "in the money."

In period 4, we assume that the market learns the true long-run value of the firm. We also assume that managerial fraud (weakly) decreases the long-term value of the firm. Let $D(f)$ denote the decrease in the firm's long-term value that arises from the manager's fraudulent activity, where $D(0) = 0$ and $D' \geq 0$. Thus, regardless of whether or not the manager is sanctioned for fraud (discussed more below), the long-term value of the firm is

$$S_4^T(e, f, \mu) = S_2^T(e, \mu) - D(f). \tag{1.3}$$

Although the market does not observe the true value of the firm, rational investors recognize the potential for fraud and rationally anticipates the manager's choice of fraud and any resulting damages to the *long-term* value. Therefore, investors are willing to pay the observed value less the expected fraud, f^e, and less any expected *long-term* damages, $D(f^e)$:

$$S_4^T(e, f, \mu, f^e) = S_2^O(e, f, \mu) - f^e - D(f^e) = v(e) + \mu + f - f^e - D(f^e). \tag{1.4}$$

The above equation can be thought of as a pricing rule, which incorporates the observed value of the firms' and the investors' expectations regarding fraud. Note that if these expectations are correct,

by public firms.

[10]The market does not observe the true value of the firm. Rather the manager sends a report to the market, which may overstate the true value of the firm. Let f denote the amount by which the manager overstates the true value of the firm, also referred to as fraud.

so that $f = f^e$, the pricing rule in equation (1.4) implies $S_3^M(e,f,\mu,f^e) = S_2^T(e,\mu) - D(f) = S_4^T(e,f,\mu)$. In other words, as long as the expectations are correct, the market price equals the true long-term value.

The effort and fraud both entail a cost to the manager. The monetary cost of effort is certain and is given by the function $\phi(e)$, where $\phi(0) = 0$, $\phi' > 0$ and $\phi'' > 0$. Fraud also has an expected cost for the manager: If the manager commits fraud, then with probability $p(0 < p < 1)$[11], the fraud will be detected and the manager will pay sanction $x(f)$, where $x(0) = 0$, $x' > 0$and $x'' > 0$. Throughout, we ignore the uninteresting case when the manager chooses effort, $e = 0$.

The sequence of events for the outlined model is as follows: First, the manager simultaneously chooses unobservable levels of effort and fraud and the noise term is simultaneously realized. Second, the market observes the *short-run* value of the firm, S_2^O. Third, the manager exercises the option if $S_2^M > K$. Fourth, the firm's *long-term* value S_4^T decreases by $D(f)$ with probability p, the fraud is detected[12] and the manager pays sanction $x(f)$.

Prior to the realization of $\tilde{\mu}$, the risk-neutral manager's expected utility is

$$E\left[\tilde{U}_M\right] = E\left[Max\{0, \alpha(\tilde{S}_3^M - K)\}\right] + \omega - \phi(e) - p \cdot x(f) \tag{1.5}$$

Note that from (1.4) it follows that $\tilde{S}_3^M < K$ iff $\tilde{\mu} < K - v(e) - f - f^e + D(f^e)$. Therefore, using (1.4) the manager's expected utility can be written as

$$E\left[\tilde{U}_M\right] = \int_{\triangle(e,f)}^{\infty} \alpha\left(v(e) + f - f^e - D(f^e) + \tilde{\mu} - K\right) g(\mu) d\mu + \omega - \phi(e) - p \cdot x(f) \tag{1.6}$$

where $\triangle(e,f) = K - v(e) - f - f^e - D(f^e)$. The first term represents the share of the expected gain from the stock option when the market stock price is above the strike price. The second term represents the cost of the manager's choice of effort and the last term represents the expected sanction cost to the manager if he or she is caught committing fraud.

[11] For simplicity, and without loss of generality, we follow Goldman and Slezak (2006) and Andergassen (2008) by allowing the level of detection to be exogenously determined. The results continue to hold for a more general expected sanction function, such as $h(f) = p(f)x(f)$, as long as $h(f)$ is a convex function.

[12] Our assumption that the manager is able to exercise the option prior to fraud being detected is similar to Goldman and Slezak (2006). However, Andergassen (2008) assumes that the manager does not receive the proceeds from the illegal activity if the fraud is detected.

The first-order condition for effort is

$$\int_{\triangle(e,f)}^{\infty} \alpha[v'(e)]g(\mu)d\mu + \alpha\left[v(e) + f - f^e - D(f^e) - v(e) - f + f^e + D(f^e) + K\right] \\ \times g\left(K - v(e) - f + f^e + D(f^e)\right) \cdot v'(e) - \phi'(e) = 0 \quad (1.7)$$

The first-order condition for fraud is

$$\int_{\triangle(e,f)}^{\infty} \alpha g(\mu)d\mu + \alpha\left[v(e) + f - f^e - D(f^e) - v(e) - f + f^e + D(f^e) + K\right] \\ \times g\left(K - v(e) - f + f^e + D(f^e)\right) - px'(f) = 0 \quad (1.8)$$

In equilibrium, we expect the market's expectations regarding fraud to be correct. Define (e^*, f^*) as the manager's equilibrium choice of effort and fraud when the market's expectations are correct. Formally, if we substitute $f = f^e$ into (1.7) and (1.8), we get two equations that implicitly define (e^*, f^*):

$$\alpha\left[1 - G(K - v(e^*) + D(f^*))\right] - \frac{\phi'(e^*)}{v'(e^*)} = 0 \quad (1.9)$$

$$\alpha\left[1 - G(K - v(e^*) + D(f^*))\right] - px'(f^*) = 0 \quad (1.10)$$

For notational convenience, we write (e^*, f^*) rather than $(e^*(\alpha, K, p), f^*(\alpha, K, p))$ whenever there is no risk of confusion.

1.3 Result

There exist many stock option contracts that induce the same equilibrium behavior. To see this, let (e^*, f^*) denote the equilibrium choice when the contract is (α, K, ω). Now consider another contract $(\alpha^1, K^1, \omega^1)$ such that

$$\alpha^1\left[1 - G\left(K^1 - v(e^*) + D(f^*)\right)\right] = \alpha\left[1 - G\left(K - v(e^*) + D(f^*)\right)\right] \quad (1.11)$$

It is easily observed that (1.9), (1.10) and (1.11) imply

$$\alpha^1 \left[1 - G\left(K^1 - v(e^*) + D(f^*)\right)\right] - \frac{\phi'(e^*)}{v'(e^*)} = 0 \tag{1.12}$$

$$\alpha^1 \left[1 - G\left(K^1 - v(e^*) + D(f^*)\right)\right] - px'(f^*) = 0 \tag{1.13}$$

Since (1.12) and (1.13) are identical to (1.9) and (1.10), it follows that (e^*, f^*) are the manager's equilibrium choices when he is compensated with $(\alpha^1, K^1, \omega^1)$. The above discussion yields the following result.

Proposition 1:

Let (e^*, f^*) denote the manager's equilibrium choice of effort and fraud when the contract is $(\alpha^1, K^1, \omega^1)$, then (e^*, f^*) is also the manager's equilibrium choice of effort and fraud for any contract $(\alpha^2, K^2, \omega^2)$, satisfying (1.11).

According to Proposition 1, there exist infinitely many contracts capable of inducing the same behavior. It is also easy to see that in order to induce same equilibrium behavior, (e^*, f^*), and increase in the strike price must be accompanied by an increase in α. Rearranging (1.11) yields

$$\alpha^1 - \alpha = \left[1 - G\left(K - v(e^*) + D(f^*)\right)\right] - \left[1 - G\left(K^1 - v(e^*) + D(f^*)\right)\right] \tag{1.14}$$

We now ask whether it is possible for one contract to fraud dominate another. That is, we ask whether it is possible to design a stock option contract that induces (weakly) greater effort and strictly less fraud than some other contract. The answer is no.

Proposition 2:

Let e^* denote the manager's equilibrium choice of effort for some contract (α, K, ω), then the manager's equilibrium choice of fraud is $f^* = h(e^*)$, where $h(e^*) \equiv y(\frac{\phi'(e^*)}{pv'(e^*)})$ is an increasing function and y is the inverse function of $x'(f)$.

Proof: First note that the assumption $x'' > 0$ implies that x' has an increasing inverse function.

By definition, (1.9) and (1.10) define (e^*, f^*). Combining (1.9) and (1.10) yields

$$x'(f*) = \frac{\phi'(e^*)}{pv'(e^*)} \tag{1.15}$$

Since y is the inverse function of x', it follows that $y(x'(f^*)) = f^*$ which along with (1.15) implies

$$f^* = y(\frac{\phi'(e)}{pv'(e)}). \tag{1.16}$$

Finally, the assumptions that $\phi'' > 0$ and $v'' < 0$ imply that the function $b(e) = \frac{\phi'(e)}{pv'(e)}$ is an increasing function. Therefore, the composite function $h(e) = y(\frac{\phi'(e)}{pv'(e)})$ is an increasing function of effort. ■

The above proposition provides a schedule of effort-fraud pairs[13] that can be induced by stock option contracts. This schedule ultimately depends on the curvature of the cost function, $\phi(e)$, the production function, $v(e)$, the sanction function, $x(f)$, and the probability of detection, p. Changes in any of these factors will alter the effort-fraud schedule. For example, consider the impact of an increase in the probability of detection on the effort-fraud schedule. Differentiation of (1.16) with respect to f and p while holding e constant yields

$$\frac{df^*}{fp} = -\frac{1}{x''(\frac{\phi'(e^*)}{pv'(e^*)})} \cdot [\frac{p^2 v'(e^*)}{\phi'(e^*)}]. \tag{1.17}$$

Thus, an increase in the probability of detection shifts the effort-fraud schedule down, implying less fraud for a given level of effort.

1.4 Conclusion

Throughout the 1980s and 1990s, stock options have been used to align the interests of managers with those of owners. However, the recent wave of corporate scandals illustrates that managerial fraud can be an unintended consequence of incentive contracts tied to a firm's stock price. It thus behooves owners to look for ways to motivate managerial effort while minimizing a manager's

[13] See Figure 3 in the appendix for a graphical illustration of effort-fraud pairs. This figure shows that by increasing the probability of detection, the effort-fraud pair is shifted downward.

incentive to commit fraud.

The finding in this paper comes from a model of managerial fraud that focused on the optimal behavioral response by comparing the incentive effects of various stock option contracts regardless of the expected cost of the compensation. We demonstrated that within the class of stock option contracts (including simple equity), any two contracts that induce a given level of managerial effort will necessarily induce the same level of fraud. As a result, there is no way to reduce the severity of fraud without a simultaneous reduction in managerial effort through contract design. We also characterized a schedule of implementable effort-fraud pairs.

It follows that it is optimal for owners to offer the contract that induces a given level of effort at the least cost, since the induced fraud is uniquely determined by the level of effort. Finally, increasing the probability of detection by improving accounting controls and strengthening monitoring mechanisms can shift the schedule of implementable effort-fraud pairs down, so that less fraud accompanies a given level of effort.

Chapter 2

Behavioral Equivalence of Stock Option Incentive Contracts: Evidence from Experiments

2.1 Introduction

Chapter 2 provides an extension of Chapter 1 by implementing the theoretical model in a controlled experimental setting. As discussed in the previous chapter, incentive contracts are referred to as 'behaviorally equivalent' if they induce the same level of effort and the same level of fraud. The focus of this chapter is to provide an answer to the following questions: Do incentive contracts that are predicted to be equivalent induce similar behavior? Is there any contract that 'dominates' other contracts by inducing relatively greater effort while generating relatively less fraud?

In practice, different equity-based incentive contracts are used by firm owners to prod high productive effort from firm managers. From a behavioral perspective, one may argue that the various equity-based compensation contracts may induce different behavioral response from firm managers. The possibility that managerial effort induced by these incentive contracts may be accompanied by managerial fraud raises huge concern. The problem of managerial fraud, which may be excessive for certain contract types has consequently prompted both policy-makers and firm owners to re-examine the way to use them. Policy-makers have played an active role in mitigating fraud through regulation by passing the Sarbanes-Oxley Act of 2002. Perhaps firm owners can also play an effective role to keep fraud in check although they are equipped with limited set of tools. As previously discussed in Chapter 1, firm owners can mitigate fraud associated with equity or stock option incentive contracts in two ways. First, through monitoring of management and stronger accounting controls. The other is through contract design, the focus of this experimental test. The theory in Chapter 1 predicts that even though firm owners can reduce fraud through contract design, they cannot reduce fraud without reducing productive effort from managers.

The nature of the manager's response to different incentive contracts (in terms of how much effort they invest or fraud they commit) is ultimately an empirical question. An empirical approach crucially depends on the measure of effort and fraud associated with predicted equivalent incentive contracts. Given the difficulty of directly measuring managerial effort and fraud in reality, an experimental approach is used to generate a sample of effort and fraud decisions associated with equalized contracts.

There is an empirical literature that focuses on the relationship between various forms of com-

pensation[1] and managerial fraud. For example, Johnson et al. (2008) find strong evidence that the likelihood of fraud is positively related to unrestricted stocks used by firms as a means of compensating managers. However, they find no support for an increase in the likelihood of fraud when management received restricted stock, vested options or unvested options.[2] Bergstresser and Philippon (2006) and Burns and Kedia (2006) all find that some incentive contracts cause more fraud. Bergstresser and Philippon (2006) examine whether increase in accruals are related to increase in stock-based CEO compensation. They conclude that the use of discretionary accruals to manipulate reported earnings is more pronounced at firms where CEO compensation is more closely tied to the company's share prices. They also document that the period of high accruals coincide with the periods when CEOs sold unusually large amounts of their options. Burns and Kedia (2006) find that the sensitivity of the CEOs option portfolios to stock price is significant and positively related to the propensity to misreport. However, they find that other components of CEO compensation, like simple equity, do not have a significant impact on the propensity to misreport.

While the general focus of Johnson et al. (2008), Bergstresser and Philippon (2006), and Burns and Kedia (2006) so far has been on the propensity to misreport under different incentive contracts, these studies do not allow one to determine whether one incentive contract dominates another in a behavioral sense. As previously mentioned, these studies reported that some incentive contract types were causing more fraud. However, it is plausible that these contracts also induced more unobservable effort from managers relative to other contract types. Therefore, an empirical approach using field data may not be an entirely clean experiment. An experimental approach is well suited to to generate clean data for this empirical analysis.

Laboratory experiments can also be useful in many ways. The laboratory offers many advantages over field studies when comparing the behavior induced by various incentive contracts. Firstly, while the level of equity-based executive compensation is generally public knowledge, the effective enforcement effort is not public information. The controlled experimental setting allows us to control the probability of detection and guarantee that this probability is known by the manager. Secondly,

[1] Murphy (2003) cite that there has been increasing use of equity-based compensation for top-level executives.

[2] Ironically, according to Jeff Skilling's indictment, he received approximately $300 million from the sale of Enron stock options and restricted stock, netting over $89 million in profit [Kay (2004)].

in the laboratory we are able to implement contracts that are predicted to induce identical behavior, rather than trying to compare contracts that are observed in the field. Thirdly, in the laboratory we are able to observe all managerial fraud, including the fraud that goes undetected. Finally, the laboratory allows us to observe managerial effort precisely which would otherwise not be observable in the field. It is noteworthy that in a controlled environment, we can observe every decision subjects make. Thus, the experimental approach is well suited to test our model's predictions.

There has been only one experimental study on the behavioral response of managers to equity-based incentives. Bruner et al. (2008) examined the correlation between equity-based compensation contract and both managerial effort and fraud. They reported that as the share of equity increased, the observed effort and fraud increased. The observed fraud level was also reported to decrease as the probability of detection increased.[3] This latter result suggests that a legislation like SOX can be effective in mitigating fraud.

The experiment was designed to provide some "parallelism" to the naturally occurring environment faced by managers that are compensated with stock option incentive contracts. As pointed by Plott (1987), this should be an important feature of an experiment to make some generalization of our results beyond the lab. Consequently the subject-managers were compensated with either simple equity or conventional stock option compensation contract. Once the subject-manager was given a contract, he or she was given an opportunity to make two consequential decisions; to increase a project value by making a contribution from his initial salary and to artificially inflate the value of the project. The reported project value was checked according to some exogenous probability and sanctioned if caught reporting an artificial amount.

We find no statistical difference between both effort and fraud levels induced by simple equity and stock option incentive contracts. Thus, the evidence from the experiment provide strong support for behavioral equivalence between contracts that are predicted to induce the same level of effort and the same level of fraud. The broad implication of this result is that, there is no way to reduce fraud without reducing effort. Therefore, firm owners will be better off choosing the cheapest incentive contract within the class of stock option contracts to motivate productive effort from the

[3]Although effort was predicted to remain unchanged after an increase in the probability of detection, the observed effort level actually increased.

firm manager.

2.2 Theoretical Model of Managerial Fraud

The model in Chapter 1 provides the basis of comparing the amount of effort and fraud induced from various stock option contracts including simple equity. A brief sketch of the model that is implemented in the laboratory is provided below.

The model consists of three players: a *risk-neutral owner*, a *risk-neutral manager*, and a *risk-neutral investor*. The *owner* hires the *manager* who is compensated with either stock option or simple equity contract . The stock option contract is defined by (α, K, ω), where α is the share of the firm offered to the manager in the form of stock options, K represents the strike price for the option, and ω is the manager's fixed salary. The simple equity contract is defined by (α, ω). Given either the stock option or simple equity contract, the manager must simultaneously make two decisions; the level of effort to invest in the firm and the value of the firm to report to the market.

The manager's investment in effort, E directly adds value to the firm. The firm's true value is $S^T = v(E) + \mu$, where $v(E)$ is the deterministic firm value, and $\mu \in [-b, b]$[4] is the random productivity shock that is assumed to be uniformly distributed. The uniform distribution is primarily used because it is relatively easier to implement and explain to subjects in the experiment. When the manager invests an effort level of E, the manager also incurs a monetary cost denoted by $\phi(E)$.

In addition to choosing an effort level, E, the manager can potentially inflate the value of the firm. The amount by which the manager inflates the true value of the firm is referred to as fraud, F.[5] The reported firm value, $S^R = v(E) + \mu + F$, is observed by the market.

The potential that the firm manager can manipulate the firm's financial report is enough to motivate firm owners and investors to form expectations about the fraud level, F^e, in the market. The market rationally incorporates this information in the determination of the firm value. The market firm value, $S^M = v(E) + \mu + F - F^e$ is also referred as the pricing rule for the firm's stock.

[4]See the Appendix for a graphical illustration of the potential distribution of "true values" (stock prices) for a given effort level. For example, when the effort level is E_1, the value of the stock ranges between S_1 and S_2 with a mean value of $v(E_1)$.

[5]Fraud, F simply represents any accounting and financial manipulation or misreport of the true value of the firm.

Undetected fraud is not costly to the manager; however the manager's report to the market may be audited with some exogenous probability, p. If fraud is detected, a sanction denoted by $x(F)$ is imposed on the manager. The fraud sanction increases with the amount of fraud.

Because we assume a uniform distribution, the location of the strike price can potentially generate three scenarios. The first scenario is when the equilibrium levels of effort and fraud are large enough to guarantee that the stock market price is always above the strike price: $\underline{S} > K$. The second scenario is when the market stock price is always below the strike price: $\bar{S} < K$. The third scenario is when the equilibrium effort and fraud levels guarantee that the market stock price is above the strike price with some probability. The second case is not very interesting because the manager will always choose an effort level, $E = 0$ and fraud level, $F = 0$. Hence, for a given location of the strike price, the value of the option to the manager is the $\max\{S - K, 0\}$.

The manager chooses effort and fraud to maximize his or her expected utility from the option. For the scenario in which the option is always *above-water*, $\underline{S} > K$, the manager's objective is given by:

$$\max_{F\geq 0, E\geq 0} \frac{\alpha}{2b} \cdot \int_{\underline{S}}^{\bar{S}} (v(E) + F - F^e + \mu - K)\, d\mu - \phi(E) - p \cdot x(F) \tag{2.1}$$

At the interior, the first-order condition with respect to effort, E is:

$$\alpha \cdot v'(E) - \phi'(E) = 0 \tag{2.2}$$

and the first-order condition with respect to fraud, F is:[6]

$$\alpha - p \cdot x'(F) = 0 \tag{2.3}$$

For the scenario in which the option may be either *above* or *under-water* ($\underline{S} < K < \bar{S}$), the

[6]For a meaningful solution, the second-order conditions must be satisfied. The second-order conditions with respect to effort and fraud are $\alpha \cdot v''(E) - \phi''(E) < 0$ and $-p \cdot x''(F) < 0$ respectively.

optimal effort and fraud choices are the solutions to[7]

$$\max_{F\geq 0, E\geq 0}\left\{\frac{\alpha}{2b}\int_K^{\bar{S}}\left(v(E)+F-F^e+\mu-K\right)d\mu-\phi(E)-p\cdot x(F)\right\} \tag{2.4}$$

At the interior solution, the simplified first-order condition with respect to E is

$$\alpha\cdot v'(E)\cdot\pi(E,F,F^e,K)-\phi'(E)=0 \tag{2.5}$$

Similarly, the simplified first-order condition with respect to F is[8]

$$\alpha\cdot\pi(E,F,F^e,K)-p\cdot x'(F)=0 \tag{2.6}$$

where $\pi(E,F,F^e,K)$ is the probability function of being *above-water*.

The results from the first-order conditions are standard. For example, the marginal benefit of effort (fraud) should equal the marginal cost effort (fraud). As previously shown in Chapter 1, the above analysis indicate that if two distinct incentive compensation contracts induce the same level of managerial effort, E^*, then they also induce the same level of fraud, F^*. This implies that the only difference between a simple equity contract (i.e., a stock option with $K=0$) that induces effort level E^* and a stock option contract that induces the same effort is the expected compensation to the manager.[9] We test this claim in the laboratory.

2.3 Experiment

We conduct an experiment designed to test whether one type of incentive contract 'dominates' another by inducing greater effort while minimizing fraud. This hypothesis is tested in the laboratory by comparing different types of incentive contracts (stock options and simple equity) that are

[7]We show in the Appendix 3.6 that the two objective functions represented by equations 2.1 and 2.4 converge as $K\rightarrow \underline{s}$, implying that the manager is objective is continuous.

[8]The optimal level of effort and fraud is guaranteed if we have a regular maximum. This local maximization conditions are $\alpha v''\pi+\alpha\pi_E v'-\phi''<0$ and $\alpha\pi_F-px''(F)<0$ for the effort and fraud equations respectively. The sufficient condition for a global max depends on the Hessian matrix, $H=\begin{pmatrix}\alpha v''\pi+\alpha v'\pi_E-\phi'' & \alpha v'\pi_F\\ \alpha\pi_E & \alpha\pi_F-px''\end{pmatrix}$. The Hessian matrix must be negative semi-definite.

[9]See Appendix 3.6 for the proof.

equivalent. The incentive contracts are equivalent in the sense that they are predicted to induce identical behavior.[10]

The experimental design to some limited extent replicates Bruner et al. (2008) by allowing subjects to choose effort and fraud for a given equity-based contract. The experiment by Bruner et al. (2008) tested the response of managerial effort and fraud to changes in the share of equity or the probability of detection. They found that effort and fraud increased as the share of equity increased. Fraud decreased as the probability of detection increased.[11] Our experimental design in contrast to their study differs in two ways. Firstly, our design incorporates stock option incentive contracts. Secondly, this experiment incorporates noise in a three-staged experiment.

Seventy-one inexperienced[12] undergraduate students participated in the experiments. The inexperienced human subjects were recruited from different disciplines from The University of Tennessee for each experimental session. Each session lasted for approximately 75 minutes and each subject typically earned between \$15 and \$24. The amount the subject earned depended on their own performance in the experiment. All entries by the subjects were made at their respective computer terminals via a mouse, and all calculations were performed by the computer.

At the beginning of the experiment, the subjects were randomly seated behind a computer terminal to input their decisions. The human subjects played the role of the manager while the shareholders and investors were computerized using the Ztree software. Subjects were not allowed to communicate with each other. They were allowed to talk to the experimenter only for the purposes of clarifying the instructions of the experiment. The first task that was performed by the subjects was the Holt and Laury (2002) risk elicitation gamble choice exercise. This is a typical exercise that requires the subjects to choose between a sure return and a gamble. This method enabled us to classify the subjects as being risk averse or not.

In the gamble choice exercise, subjects chose between ten individual gambles, paying either 0.50

[10]Experiments are important for many reasons. Experiments are especially useful in circumventing problems intrinsic to field data. Lab experiments are complementary to theory in that they provide controlled environments. Experiments also allow an experimenter to isolate parameter values that alter behavior. Finally, lab experiments allow experimentalist to artificially generate clean data for empirical analysis.

[11]In Bruner et al. (2008), effort was predicted to remain unchanged with changes in the probability of detection. However, they rejected the null hypothesis.

[12]They were inexperienced in that they had not participated in this experiment before.

Table 2.1: The Ten Paired Lottery-Choice Decisions

Option A	Option B	Expected Payoff Difference
1/10 of $5.00, 9/10 of $0.50	$3.00 guaranteed	-$2.05
2/10 of $5.00, 8/10 of $0.50	$3.00 guaranteed	-$1.6
3/10 of $5.00, 7/10 of $0.50	$3.00 guaranteed	-$1.15
4/10 of $5.00, 6/10 of $0.50	$3.00 guaranteed	-$0.70
5/10 of $5.00, 5/10 of $0.50	$3.00 guaranteed	-$0.25
6/10 of $5.00, 4/10 of $0.50	$3.00 guaranteed	$0.20
7/10 of $5.00, 3/10 of $0.50	$3.00 guaranteed	$0.65
8/10 of $5.00, 2/10 of $0.50	$3.00 guaranteed	$1.1
9/10 of $5.00, 1/10 of $0.50	$3.00 guaranteed	$1.55
10/10 of $5.00, 0/10 of $0.50	$3.00 guaranteed	$2.00

or 5 lab dollars, and a guaranteed amount of 3 lab dollars. A screen shot of the gamble game is shown in Figure 9. The list of paired lottery and guaranteed options that subjects chose from is provided in Table 2.1. As shown in Table 2.1, Option A is a riskier choice while Option B is a safer choice. The gambles vary in their respective probabilities of winning the large prize. When the probability of a high outcome increases, the subject should cross over from Option B to Option A. In this setup, risk-averse subjects should potentially switch over to Option A after the fifth choice when the expected payoff difference is $0.20. After they completed this task, subjects received instructions on their computer screens pertaining to the experiment. The instructions for the experiment[13] are provided in the Appendix 3.6. The subjects were also presented with examples of the relevant information screens, definitions, and descriptions of the information pertinent to the experiment. Since the University of Zurich's Ztree program does not allow subjects to review past instructions, we also provided a hard copy of the instructions of the experiment to the subjects.[14]

2.3.1 Experimental Design

Table 2.2 gives a summary of the parameter values used in the experiment. At the beginning of each decision round, each subject-manager received an incentive contract which was either a

[13]See Figures 9 to 26 for the instructions of the experiment.

[14]About 84% of the subject said that they understood the instructions of the experiment extremely well. The lowest rating for the clarity of the instruction was 3 from a range of 0 to 5.

Table 2.2: Parameter Values

Contracts	*Options*	*Simple Equity*
Share	$\alpha = 0.50$	$\alpha = 0.35$
Strike Price	$K = 850$	$K = 0$
Salary	$S = 150$	$S = 120$
Detection Probability	$p = 20\%$	$p = 20\%$

stock option or a simple equity. The components of the incentive contract (listed in Table 2.2) consisted of shares offered in terms of equity, the level of strike price, and a salary. For example, the stock option contract is defined by the set ($\alpha = 0.50, K = 850, S = 150$) whereas the simple equity contract is defined by the set ($\alpha = 0.35, S = 120$). Contract ($\alpha = 0.35, S = 120$) is affiliated with equations (2.2) and (2.3) because this contract guarantees that the option will always be in the money as long as the manager chooses the optimal effort and fraud levels. However, the contract ($\alpha = 0.50, K = 850, S = 150$) is associated with equations (2.5) and (2.6) because the strike price, $K = 850$ is large enough such that, the optimal level of effort and fraud only guarantees the option to be 'in the money' with some probability. The probability of being 'in the money', π, when the equilibrium effort and fraud levels are chosen for the stock option contract, ($\alpha = 0.50, K = 850, S = 150$) is 70%.[15] To illustrate how these contracts are equivalent, consider equations (2.3) and (2.6). Recall that equation (2.3) is associated with contract ($\alpha = 0.35, S = 120$). According to this equation, the expected benefit of fraud should equal 0.35 in equilibrium. Equation (2.6) which is associated with the stock option contract ($\alpha = 0.50, K = 850, S = 150$) also suggest that the expected benefit of fraud should equal 0.35 ($= 0.5 \times 0.7$). Holding all else constant, the expected benefit and cost associated with these contracts must therefore be equivalent. It is implied that the effort and fraud levels induced from these contracts are also the same.

For a particular treatment session, each subject-manager received the same type of contract. After the subject-managers received the contract, they had to make two consequential decisions. In

[15]The formula for calculating the probability of being 'in the money' is

$$\pi(E, F, F^e, K) = \frac{v(E^*) + b + F^* - F^e - K}{2 \times b} \tag{2.7}$$

this experiment, the subjects were given the opportunity to contribute, $C_i \in [0, 120]$, to a project that had an uncertain return. Each contribution amount was associated with a project value, $V(C_i)$. The project value, $V(C_i)$, was listed alongside each contribution level provided to the subject-manager. Subject-managers could increase the value of the project by simultaneously choosing to make a contribution and to artificially inflate, $A_i \in [0, 400]$, the true value of the project. The contribution amount by the subject-manager is analogous to effort. Effort has diminishing returns and was not costless. The cost of each level of effort contribution by the subject-manager was normalized to one. This reduced the salary of the manager by the amount of the effort contribution.

Similar to our model, the choice of effort directly affected the distribution of the project value. For example, high effort levels corresponded to the realization of high draws of project values and low effort levels corresponded to the realization of low draws. Following the theoretical model described in the previous section, the subject-manager faced some uncertainty. The uncertainty about the project value stemmed from the fact that the subject did not know *ex-ante* the value of the random component, μ. The random component was constrained to a uniform distribution that takes any value from -400 to 400. This means that any value within this range is equally likely. The sum of the project value, artificial component and the random component is referred to as reported project value.

The possibility of fraud occurring in the market is all that is required for investors and shareholders to form expectations of fraud in the market. However, subjects were not given information on the expected fraud level, F^e. Therefore, the final project value that the subjects see can be interpreted as the reported project less the expected fraud. The final realized project value is the sum of three components: the project value determined by the effort contribution, the artificial component (fraud), and the random component. The final realized value of the project is denoted by $FV_i = V(C_i) + A_i + \mu$, and is synonymous to the market stock price. Since F^e is set to zero, the market project value is equivalent to the reported project value in this experiment.

Similar to the theoretical model, choosing an artificial component amount (fraud) was not costless. In each treatment, the probability of detection was set to 20%. The manager faced a sanction when caught reporting in excess of the true project value. For simplicity, we adopted the possibility

of a detection mechanism that was imperfect. This meant that some artificial component went undetected. The sanction for choosing an artificial component (fraud) increased with increments in the amount of artificial component. The sanction levels denoted by $P(A_i)$ were displayed on the screen alongside with each artificial component level. Figure 24 shows the subject's screen image for this stage.

We conducted 4 sessions, each lasting 75 minutes with about 17 or 18 participants per session. To facilitate the search for the optimal effort and fraud bundle, the experiment was repeated for 20 rounds. After each round, the subject received feedback on their choices and earnings. The earnings of the subject-manager is defined according to

$$earnings = \begin{cases} \alpha(FV_i - K) + (S - C_i), & \text{if } K < FV_i \text{ \& not checked} \\ \alpha(FV - K) + (S - C_i) - P(A_i), & \text{if } K < FV_i \text{ \& checked} \\ (S - C_i), & \text{if } K > FV_i \text{ \& not checked} \\ (S - C_i) - P(A_i), & \text{if } K > FV_i \text{ \& checked} \end{cases} \tag{2.8}$$

In addition to the subject's salary, the earnings of the subject in part depended on whether the final value of the project was above a threshold amount denoted by K. The total earnings of the subject also depended on whether his or her final project value was checked and whether he or she chose an artificial component.

In summary, the strategic problem of any subject-manager is the tradeoff between how much to contribute to the project (effort) and how much to inflate the project value (fraud). By increasing his or her contribution level, the subject's salary was reduced. Also, by inflating the project value, the subject potentially faced a sanction. In the strategy space that we provided in the experiment, the following itemized strategies are not optimal: *(i)* zero effort or fraud; *(ii)* maximum effort or fraud. The dominant strategy in our calibrated experimental setting is to a choose a unique positive amount of contribution, $C_i > 0$, and a unique positive amount of artificial component, $A_i > 0$. Details of the dominant strategy for each incentive contract is discussed below.

Before proceeding further with the experimental design, let us introduce the following notation

Table 2.3: Experimental Design

	Treatments	
	Simple Equity	*Stock Option*
Contract	*ET* (36)	*ST* (35)

Simple Equity - These are contract types that are always in the money.
Option - These are contract types that are guaranteed to be in the money with some probability when the optimal effort and fraud levels are chosen.
The values in ( ) represent the number subjects in each cell.

- *ET* and *ST*. The simple equity type contract is denoted by *ET*, while the stock option type contract is denoted by *ST*. The experimental treatment structure yields a 1×2 factorial design resulting in two treatments.[16] The treatments vary by their prediction of effort and fraud level and their contract type (which is either simple equity or stock option).

Two equivalent contracts were constructed; a conventional stock option and a simple equity contract. Both incentive contracts were predicted to induce the same effort level and the same fraud level. For example, both the conventional stock option and simple equity contract predicts an effort level 60 and a fraud level of 100. These treatments were implemented in the laboratory in a between subject design. Table 2.3 provides a summary of treatments that were predicted to be equivalent.

The treatment-specifics are as follows: The *Simple Equity* treatment (i.e. *ET*) had little to no uncertainty regarding the earnings from the investment in the project. However, the *Stock Option* treatment (i.e. *ST*) had relatively higher uncertainty regarding the earning from the project. The subjects were told that their earnings depended on whether the final project value was above a certain threshold amount. If the final project value was greater than the threshold amount, then the subject-manager's earnings was the pre-defined share times the difference between the final project value and the threshold amount. In the case of the *Simple Equity* treatment, the subject's earnings from the project was the pre-defined share times the final value of the project.

[16] The word treatment or contract is used interchangeably to describe either equity type or stock option type contracts.

Hypothesis

The experimental design provides a means of testing the theoretical predictions. The testable hypothesis from the theory using experimental data is:

H1: *The difference between both effort levels and both fraud levels of any two predicted equivalent stock option contracts is zero.* According to Proposition 1 in Chapter 1, if any two distinct incentive compensation contracts induce the same level of managerial effort, E^*, then they also induce the same level of fraud, F^*. For this hypothesis to be true, both effort levels and fraud levels must remain unchanged when we compare treatment ET to ST.

2.4 Definition of Data

The experimental data was collected in June 2009, consisting of two incentive contract types. A total number of seventy-one subjects participated in the experiment. Approximately 55% (39) of the subjects were males while 45% (32) were females. Although our theory describes a one-shot game, we allow for learning and feedback by repeating the game for 20 periods. As a result, the data points collected constituted a panel of 1420 pooled observations.

In each session, we altered some parameters to create the environment of the desired treatment effect. The definition and description of variables used for our analysis are provided in Table 10 in the Appendix 3.6. Table 11 in the Appendix provides the summary statistics of the variables. The first two variables (effort and fraud) are the dependent variables. Properties[17] of the two variables and the estimation methodology will be addressed in a subsequent section.

The next two variables are the treatment (contract dummies) variables. They take the value of 0 or 1 depending on the treatment specification or definition. For example, treatment ST dummy takes the value of 1 when the option share is 50%, the strike price equals 850 lab dollars and salary is 150 lab dollars. On the other hand the ET dummy takes the value 1 when the option share is 35%, the strike price equals 0 lab dollars and salary is 120 lab dollars. Thirty-six subjects participated in the ET treatment while thirty-five subjects participated in the ST treatment.

[17]The distribution of the effort and fraud are provided in the Appendix. See Figures 7 and 8 in the Appendix.

Table 2.4: Predicted and Actual Values

Treatment	Predicted Effort	Actual Mean Effort	Predicted Fraud	Actual Mean Fraud
ET	60	81***	100	130***
		[1.091]		[4.911]
ST	60	83***	100	154***
		[1.149]		[5.021]

*** Statistically different from the predicted value at the 1% significance level. Standard errors are reported in [ ].

The next variable is a proxy for risk aversion. We constructed an indicator that takes the value 1 if the subject exhibited risk-averse preferences in the gamble game. When the subject chooses more than five safe choices, then the risk-averse parameter takes the value of 1 and 0 otherwise. The next variables are feedback variables. The first feedback variable is LAG PENALTY. LAG PENALTY is the amount of penalty the subject incurred in the previous period. The next feedback variable is LAG AUDIT and this variable is a dummy that takes the value of 1 when the subject was audited in the previous period. The last feedback variable is the LAG WEALTH. LAG WEALTH is the accumulated earnings from the previous rounds. Since our theoretical model is static, we are unable to make any predictions on the impact of these variables on both effort and fraud.

In addition to the feedback variables, we constructed a variable that proxy for the ability of the subjects. The ABILITY variable was constructed using the number of questions that the subject answered correctly. The questions were administered after the participants had completed the task of reading through the instructions.[18] Finally, we control for learning in our experiment using dummies for each period. However, the estimates of these variables are not reported.

2.5 Results

The results pertain to the experimental design shown in Table 2.3. Table 2.4 contains the point predictions of effort and fraud for a risk-neutral agent.[19] The subjects' actual mean effort and fraud levels are also reported with the point predictions in Table 2.4.

[18] Approximately 54% of the subjects had a score above 3 out of 5 points. Also 41% of the subjects had a score below 3 points. The overall average score was approximately 3 points.

[19] 20% of our subjects were considered to be risk-neutral. 20% were risk-loving and 60% were risk-averse.

Columns 2 and 4 contain the predicted effort and fraud values respectively. The corresponding actual mean effort and fraud values are also reported in columns 3 and 5. In general, the observed mean values of effort and fraud were higher than the predicted values. Bruner et al. (2008) also observed mean values that were above or different from their theoretical predictions. For example, the theoretical predictions of effort and fraud for both treatment *ET* and *ST* are 60 and 100 respectively. The observed mean values for treatment *ET* were 81 and 130 for effort and fraud respectively. The observed mean values in terms of the effort and fraud levels of treatment *ST* are also higher than the predicted values; the actual effort and fraud values are 83 and 154 respectively. The level of effort induce from the both the *ET* and the *ST* treatments appear to be close. The difference between mean level of fraud across the *ET* and the *ST* seems to be large. This preliminary result may indicative of the stock options potentially generating more than simple equity. Note that the difference between the actual mean and the predicted effort and fraud values are statistically different from zero at the 1% significance level. Also note that the standard errors are modest and the difference in the standard errors between treatment *ET* and *ST* are relatively small. Figure 2.1 provides a box plot of effort and fraud data points across the two treatments.[20] As Figure 2.1 suggests, the distribution of effort in both the *ET* and *ST* treatments appear to be similar, although the median effort of the *ST* treatment is relatively higher than the median effort level of the *ET* treatment. The distribution of fraud in both treatments also appear to be similar. From the box plot, it is shown that the median fraud levels of both the *ET* and *ST* treatment are the same. It is striking to note that the median fraud value of both treatments is 100 which is the same as the predicted fraud level. This evidence provides strong support for our theoretical prediction. Figures 2.2 and 2.3 also provide another dimension of effort and fraud decisions by subject-managers. As previously noted, our model is a one-shot game, however in the experiment the subject-managers were asked to repeat their effort and fraud decisions for 20 rounds. To summarize their effort and fraud decisions over time, we plot average effort and fraud decisions for each period. The plot of average effort and fraud over time in Figures 2.2 and 2.3 first indicate that average effort and

[20]The box plot provides a summary of the following statistical measures: median, upper and lower quartiles, and the minimum and the maximum data values. The box itself contains 50% of the data points. The upper edge of the box indicates the 75th percentile while the lower edge of the box indicates the 25th percentile of the data. The line the box indicates the median value of the data.

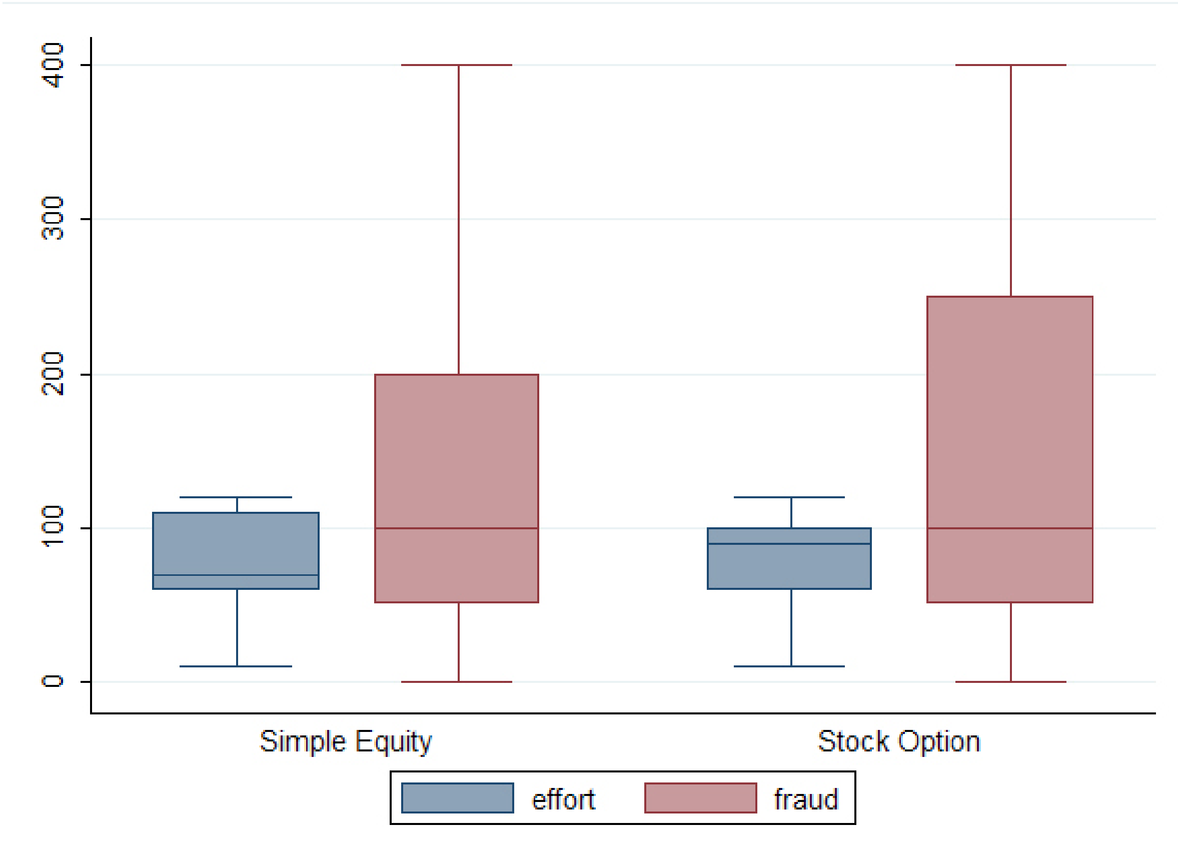

Figure 2.1: Note: Box plot graph for contracts predicted to induce effort level of 60 and fraud level of 100

fraud levels are higher than predicted values. Secondly, the average effort across the ET and ST treatments appear to be similar. Thirdly, average fraud levels appears to be slightly higher in the ST treatment after period 9.

2.5.1 Non-parametric Analysis

The non-parametric results are summarized in Table 2.5. The statistical analysis presented herein examines the difference in median of the average effort and fraud amounts of each subject across the two treatments.[21] The individual effort and fraud choice over 20 periods may not be independently distributed. However, independence can be achieved by taking the individual averages of effort and

[21] We impose some structure on our dataset, since we observe 20 data points for each subject. These data points are likely to be correlated.

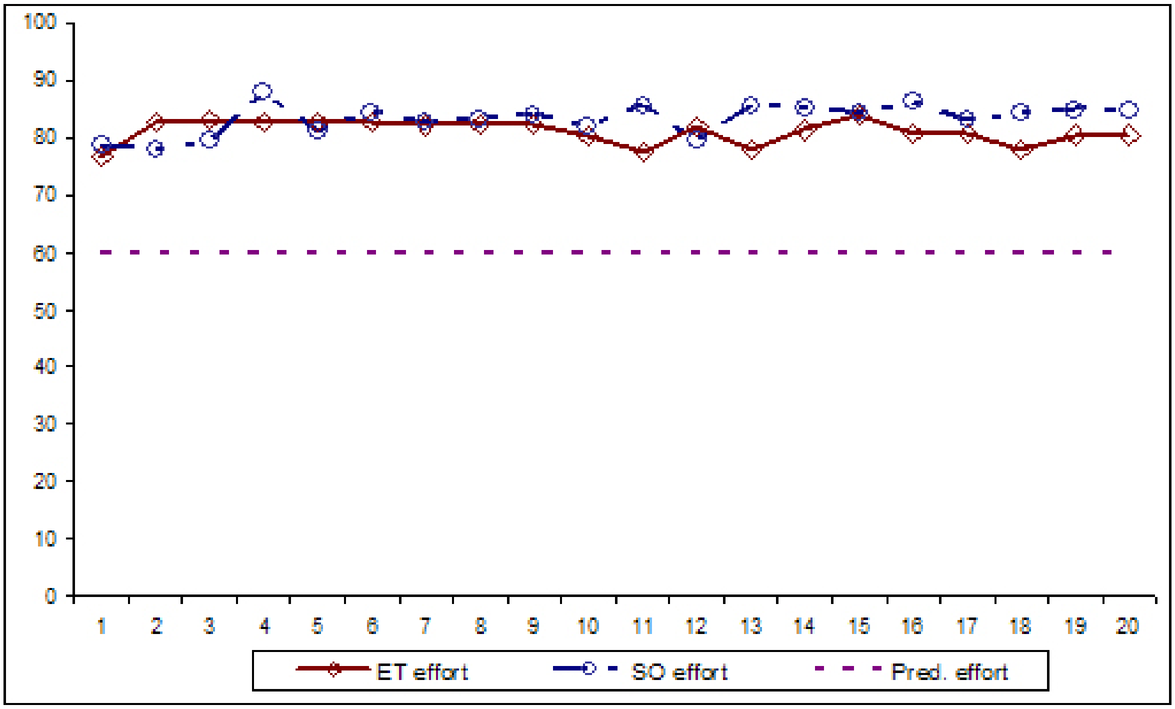

Figure 2.2: Average effort levels of ET and ST treatments across 20 paying rounds

fraud amounts for each treatment. This procedure generates 71 observations for all the treatments. The Kolmogorov-Smirnov and Mann-Whitney U tests were used to examine the validity of our research hypothesis.

The difference between effort levels for the *ET* and *ST* contracts were found to be statistically not different from zero. Table 2.5 reports *p-values* of 0.412 and 0.617 from the Kolmogorov-Smirnov and Mann-Whitney U tests respectively. The difference between the fraud levels for the *ET* and *ST* contracts were also found to be statistically not different from zero. The *p-values* of the Kolmogorov-Smirnov and Mann-Whitney U tests are 0.227 and 0.144 respectively. These values suggest that we cannot reject the null hypothesis of no difference in the distribution of the two independent samples (equity and stock option). The result obtained provides strong support for the our theoretical result in Chapter 1. The result obtained also confirm that the novel way of mitigating managerial fraud is through contract design. Although this result should be interpreted with caution, it still provides some guidance as to how the firm owner can use the design of contracts to mitigate fraud in a cost

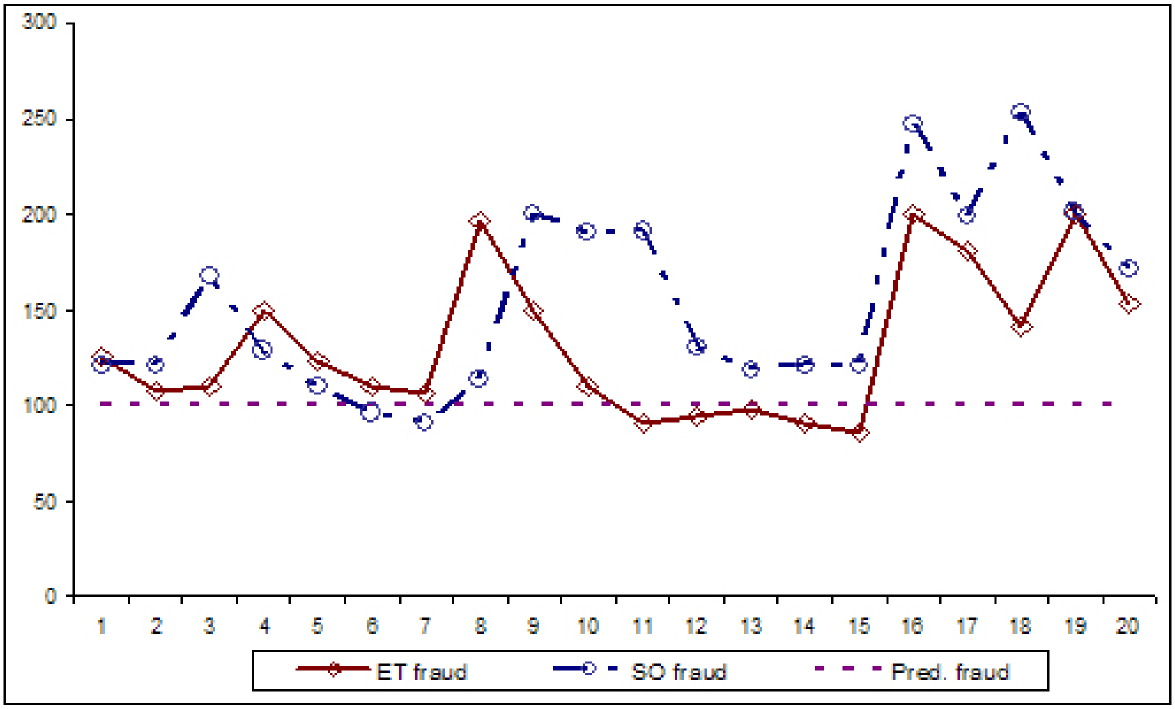

Figure 2.3: Average fraud levels of ET and ST treatments across 20 paying rounds

effective way.

Next, we perform a multivariate analysis to examine whether effort and fraud are the same across the predicted equivalent contracts while taking into account the effects of other variables such as risk postures of the subjects. The risk posture variable and feedback variables are introduced for exploratory purposes.

2.5.2 Multivariate Analysis

The property of our dataset enables us to utilize panel estimation method. The panel data allows us to control in a more natural way the effects of missing and unobserved variables. Unlike cross-sectional data, we can also allow for feedback and learning since our data is collected over a number of periods. The estimated reduced-form equations are:

$$\begin{aligned} effort_{it} &= \kappa_1 ET + \kappa_2 ST + \kappa_3 RiskAverse_{it} + \kappa_4 LagAudit_{it} \\ &\quad + \kappa_5 LagPenalty_{it} + \kappa_6 LagWealth_{it} + \eta_1 D_{it} + \varphi_i + \varepsilon_{it} \qquad (2.9) \end{aligned}$$

Table 2.5: Predicted and Actual Values

Null Hypothesis	Kolmogorov-Smirnov Test	Mann-Whitney U Test
Effort: $ET - ST = 0$	0.412	0.617
Fraud: $ET - ST = 0$	0.227	0.144
Result summary	Fail to Reject Ho!	Fail to Reject Ho!

$$\begin{aligned} fraud_{it} &= \beta_1 ET + \beta_2 ST + \beta_3 RiskAverse_{it} + \beta_4 LagAudit_{it} \\ &\quad + \beta_5 LagPenalty_{it} + \beta_6 LagWealth_{it} + \eta_2 D_{it} + \gamma_i + \epsilon_{it} \end{aligned} \tag{2.10}$$

where the subscript i denotes the subject i and the subscript t denotes the period t. κ and β are the coefficients of the independent variables of the effort and fraud equations respectively. η denotes the coefficient of the vector of period dummies. Finally ε_{it} and ϵ_{it} are the error terms of the effort and fraud equation respectively.

The estimation of the effort equation utilizes the random-effects model while the estimation of the fraud equation utilizes the random-effects two-tailed Tobit model.[22] To assess the validity of our theoretical prediction, a simple inference test was implemented to examine the difference between the coefficients of equivalent contracts. For our theoretical prediction to remain valid, the difference between coefficients of the equivalent contracts must not be statistically different from zero. The results are summarized in Tables 2.6 through 2.7 for the effort and fraud equations respectively. The main variable of interest in these regressions is the treatment dummy, which may either be ET or ST. Since there are only two treatments, one of the dummies is not included in the regression. For our research hypothesis to remain true, then our variable of interest must not be statistically different from zero in both regression equations. The results in columns (2) and (3) of Tables Tables 2.6 and 2.7 provided appealing results. The main distinction become columns (2) and (3) is that, the regression result in column (3) in both tables control for period effects. However, the results associated withe the period effects are not reported. Hereon, the discussion of our results is referenced from column (3) for both the effort and fraud estimated equations. The coefficients

[22]This approach was utilized because of the mass at zero fraud and the maximum amount of fraud. This OLS distributional assumptions are violated and thus applying least squares to the data leads to biased and inconsistent estimates. In our pooled data 252 observations were found at the left censor and 174 were found at the right censor.

Table 2.6: Reduced-Form Regression of Effort Equation

dependent variable: effort		
	Model 1	Model 2
Constant	81.07***	78.66***
	[4.082]	[5.282]
ET		-2.088
		[6.021]
ST	2.088	
	[6.021]	
Period Effects	N	Y
Number of Obs.	1420	1420
R^2	0.09	0.09

Notes: Standard errors are in [ ]. *** Significant at 1%; ** Significant at 5%; * Significant at 10%;

of the treatment dummy, ET in the effort and fraud equations are -2.09 and -31.91 respectively. The sign of these coefficients provide information about the direction of effort and fraud relative to the ST treatment. The negative sign suggests that stocks on average is inducing more effort and fraud than simple equity. Since the coefficient of the treatment variable in both the effort and fraud equations are not statistically significant, the cannot the null of our hypothesis that there is no difference in effort and fraud choice under both ET and ST treatments. Therefore, the punchline from the results in Tables 2.6 and 2.7 is that, simple equity and stock option contracts induce similar behavior. The above results generally confirm that contracts that are theoretically predicted to induce the same level of effort and the same level of fraud are also similar in a behavioral sense.

The distinct characteristics of the payoff tables for the stock option treatment and the simple equity treatment even make our result very profound. The main difference in the strategy space of the two treatments is that, the stock option treatment had a relatively steep payoff table while the simple equity payoff table was relatively flat. Yet, the differences in the average effort and fraud choices between treatment ET and ST were statistically not different from each other. In fact, the variance of both the effort and fraud levels were quite similar. Figure 2.1 provides evidence of this claim.

Table 2.7: Reduced-Form Regression of Fraud Equation

	dependent variable: fraud	
	Model 1	Model 2
Constant	117.74***	124.29***
	[22.247]	[26.791]
ET		-31.91
		[31.549]
ST	31.50	
	[31.622]	
Period Effects	N	Y
Number of Obs.	1420	1420
R^2	0.11	0.13

Notes: Standard errors are in [ ]. *** Significant at 1%; ** Significant at 5%; * Significant at 10%;

2.5.3 Robustness Check

This section provides robustness check in two ways. Firstly by examining the subject's choices of effort and fraud over sub-periods, and secondly by estimating a propensity model which examines whether subjects were more inclined to commit fraud under a specific incentive contract among other incentive contracts that were examined.

Now consider the first approach. It is typical to observe changes in the choices (effort and fraud) of subjects over repeated games. Since we repeat the one-shot game for 20 rounds, we also report the test for equivalence for different period blocks or sub-samples of our dataset. We report these results in Tables 2.11 and 2.12.

Results from Tables 2.11 and 2.12 are fairly consistent with full sample result of behavioral equivalence, especially in period block I, II, and IV. Four dummy variables were constructed by interacting period block dummies and the treatment dummy (i.e. stock option contract dummy).[23] The coefficient of these dummies have to be statistically not different from zero for our hypothesis to remain true. We found support of equivalence between contracts that were predicted to induce the same level of effort and fraud. We found the strongest evidence of equivalence in period blocks I, II,

[23]Four period block dummies were constructed. Period block I dummy ranges 1 to 5. Period block II dummy ranges from period 6 to 10, period block III dummy ranges from period 11 to 15,and period block IV dummy ranges from period 16 to 20.

Table 2.8: Robustness Check: Effort Regression

	dependent variable: effort
Constant	80.11***
	[4.019]
ST x Period 1-5	-0.75
	[6.735]
ST x Period 6-10	1.03
	[6.184]
ST x Period 11-15	3.61
	[5.798]
ST x Period 16-20	4.46
	[5.924]
Period 1-5	1.56
	[1.795]
Period 6-10	1.94
	[1.795]
Period 11-15	0.33
	[1.442]
Period 16-20	
Number of Obs.	1420
R^2	0.12

Notes: Standard errors are in [ ]. *** Significant at 1%; ** Significant at 5%; * Significant at 10%;

and IV. We rejected the null of behavioral equivalence in only one of the period blocks; the period block III induced a higher fraud level than the other period rounds although the effort level was statistically not different from zero. The results columns 2 and 3 of Table 2.12 capture the presence of end period effect. The results indicate the subjects chose higher amounts of fraud during the last rounds than all other rounds.[24]

Now consider the second approach. The propensity model is estimated using a panel Logit model. Although this analysis was not a direct test of our research hypothesis, it provided an opportunity to check whether fraud was more likely under a stock option contract versus a simple equity contract. The dependent variable in this model took a value of 1 if the subject chose a positive fraud amount. A regression analysis was performed by regressing the dependent variable on the same set of independent variables as in Table 2.7. The results of these analyses are reported

[24]This result also shows the importance of controlling for end period effects in our estimated model.

Table 2.9: Robustness Check: Fraud Regression

dependent variable: fraud		
	RE	2T-Tobit
Constant	180.23***	174.72***
	[23.738]	[16.977]
ST x Period 1-5	9.29	6.66
	[33.789]	[23.323]
ST x Period 6-10	1.75	3.83
	[33.751]	[23.929]
ST x Period 11-15	69. 95**	44.62**
	[33.85]	[22.340]
ST x Period 16-20	46.32	39.564
	[33.738]	[24.931]
Period 1-5	-73.68***	-51.67***
	[13.903]	[9.682]
Period 6-10	-53.05***	-40.556***
	[13.819]	[10.229]
Period 11-15	-123.82***	-83.06***
	[14.123]	[9.142]
Period 16-20		
Number of Obs.	1420	1420
R^2	0.14	0.15

Notes: Standard errors are in [ ]. *** Significant at 1%; ** Significant at 5%; * Significant at 10%;

in Table 2.10 in Appendix 3.6. The coefficients are the regular Logit coefficients, not marginal effects. The signs of the coefficients can be interpreted as directional responses.

Now consider the likelihood of fraud between the simple equity and stock option contracts. Holding predicted effort constant, the results suggested that there was no statistical difference in the likelihood of fraud between the simple equity and stock option contracts. This result re-confirms the strong evidence of behavioral equivalence between the *ET* and *ST* contracts.

2.5.4 Other Results

Next, results from the other co-variates are discussed. The results discussed are associated with Tables 2.11 and 2.12. All the exploratory variables were found not to account for any variation in the level of effort chosen by the subjects. The reduced fraud model indicates that some variation

Table 2.10: Logit Model Results: Propensity Model of Managerial Fraud

dependent variable: fraud = 0 or 1		
	Model 1	Model 2
Constant	3.36***	1.94
	(0.000)	(0.030)
ET	-0.66	
	(0.357)	
ST		1.07
		(0.150)
Risk Averse		0.46
		(0.344)
Lagged Audit		1.54***
		(0.000)
Lagged Wealth		0.002***
		(0.017)
Ability		-0.15
		(0.438)
Number of Obs.	1420	1420
R^2	0.13	0.18

Notes: p-values are in ( ). *** Significant at 1%; ** Significant at 5%; * Significant at 10%;

in fraud is attributable to risk aversion. Column 2 in Table 2.12 show that subjects that were classified as risk-averse in the conventional stock option treatment chose less fraud. This result is captured by the interactive term between ST and the risk-averse dummy variables. A rather odd result, is that subjects that were classified as risk-averse in the simple-equity treatment chose more fraud than less risk averse subjects. Our empirical results reported in column 3 of Table 2.12 also show evidence of feedback effects in the fraud regression. We found the odd but often observed "gambler's fallacy" behavior.[25] This outcome partially explains why the observed fraud amounts are lower than predicted in low detection regimes and higher than predicted fraud amounts in high detection regimes. The prevalence of the gambler's fallacy is often the reason why investors have the tendency to sell stock that have appreciated and hold on stocks that have plummeted in value. The coefficient of lagged audit variable was 76.21 and this result was significant at the 1% significance level. This implied that when the subjects were audited in the previous round, the subjects were

[25] Croson and Sundali (2005) also found existence of the gambler's fallacy using field data in a casino.

Table 2.11: Reduced-Form Regression of Effort Equation

	dependent variable: effort	
	Model 1	Model 2
Constant	78.02***	89.22***
	[6.005]	[6.271]
ET	-3.67	-1.407
	[7.380]	[5.840]
ST		
Risk Averse	3.673	2.287
	[3.964]	[2.664]
ET x Risk Averse		
ST x Risk Averse	-2.574	
	[5.984]	
Lagged Audit		-1.250
		[2.016]
Lagged Wealth		-0.001
		[0.004]
Ability		-1.526
		[1.405]
Period Effects	Y	Y
Number of Obs.	1420	1420
R-squared	0.10	0.15

Notes: Standard errors are in [ ]. *** Significant at 1%; ** Significant at 5%; * Significant at 10%;

more inclined to commit more fraud in the subsequent rounds. The lag of wealth was not found to be correlated with the choice of fraud level. Finally, the ability variable was also not found to be correlated with the amount of fraud chosen by the subject.

2.6 Conclusion

This chapter provided a robust test of the theoretical model in Chapter 1 by using an experimental technique. The testable research hypothesis that fall out from the theoretical model in Chapter 1 asserts that the difference in both the effort levels and both the fraud levels of any two contracts that are predicted to be equivalent is zero. The experimental test focused on whether there was evidence of behavioral equivalence.

Table 2.12: Reduced-Form Regression of Fraud Equation

dependent variable: fraud		
	Model 1	Model 2
Constant	152.16***	166.06***
	[31.99]	[44.529]
ET	-96.95***	
	[39.907]	
ST		33.47
		[32.511]
Risk Averse	60.66**	12.03
	[27.174]	[19.877]
ET x Risk Averse		
ST x Risk Averse	-108.59***	
	[40.779]	
Lagged Audit		76.21***
		[14.311]
Lagged Wealth		0.007
		[0.034]
Ability		-7.30
		[8.157]
Period Effects	Y	Y
Number of Obs.	1420	1420
R^2	0.14	0.21

Notes: Standard errors are in [ ]. *** Significant at 1%; ** Significant at 5%; * Significant at 10%;

Our experimental approach produced strong results in support of our research hypothesis. Both the non-parametric and parametric analyses yielded consistent results. The results from the parametric analyses were even robust after controlling for risk effects. When we contrasted the conventional stock option contract with simple equity contract, we found that both contract types induced the same level of fraud. Also, the difference in effort levels between the conventional stock option and simple equity contracts were generally consistent and statistically not different from zero. All in all, the experimental test showed evidence of behavioral equivalence.

The analyses presented in this paper provided an answer to the following question: Do incentive contracts that are predicted to be equivalent induce similar behavior? The answer to this question is yes. In support of this answer, we found no difference in both effort and fraud levels of the

contracts that were predicted to be equivalent. With respect to the contracts that were examined, we can conclude that there are no other contracts that can induce relatively greater effort and also generate relatively less fraud.

Based on the above results, one implication stands out. The evidence of behavioral equivalence in support of the theory re-confirm that, there is no way to design a stock option contract that reduces fraud without also reducing effort. Thus, the recent trend in the U.S. that shows firms switching from stock option to equity type contracts may not have any significant impact on managerial fraud.

The prescription for mitigating fraud that is presented in this experimental analysis is quite straightforward. Beyond enforcing strict accounting standards, firm owners cannot reduce fraud without reducing effort from the manager through contract design. Perhaps, firm owners will be better off choosing stock options because they are relatively cheaper to the firm owner than simple equity. This result is profound in the sense that although the characteristics of simple equity and stock option contracts are dissimilar, they are still capable of inducing similar behavior.

Chapter 3

Tax Evasion: Progressive and Regressive Tax Regimes

3.1 Introduction

Previous chapters examined the effect of various incentive contracts on the incidence of managerial fraud. Chapter 3 proceeds by investigating another form of fraud, tax evasion, under two tax schemes. This chapter goes on to investigate how misreporting of income differs across progressive and regressive tax regimes in an experimental setting.

Beginning with Friedland et al. (1978), there has been increased interest in the use of experimental methods to study tax compliance. These experimental studies (Spicer and Becker (1980), Spicer and Everett (1982), Alm et al. (1989), Beck et al. (1991), and Alm et al. (1992)) have provided a platform to examine various prescriptions for increasing tax compliance including the positive effects of perceptions, uncertainty, frequent audits, social norms, and public goods. Although the aforementioned papers differ significantly in detail, they all utilize tax systems that are primarily regressive (i.e. proportional tax rate) in nature. This paper experimentally attempts to test whether tax compliance behavior is different under either the progressive or the regressive tax systems assuming both tax systems are revenue equivalent.[1]

There are salient reasons to believe that tax compliance decisions may differ under the progressive and the regressive tax regimes. The major distinction between the two tax schemes is that, while the progressive tax scheme shifts a higher tax burden on the rich, the regressive on the other hand shifts a higher tax burden on the poor. Behaviorally, tax compliance may differ across the two tax schemes because of framing-effects. The framing-effects may increase the importance of one's consideration of fairness and inequality aversion on tax compliance decision. Finally, the risk preferences of taxpayers may also induce strategic behavior across the two tax schemes. The compounding effects of these factors on the tax evasion decision provide enough reasons to believe that there may be potential differences in tax compliance behavior across the two tax schemes. An experimental approach is well suited for this form of investigation.

The relevance of this experimental study stems from the fact that there are fundamental differ-

[1] Field studies have mainly focused on the efficiency cost of the progressive tax systems due to distortions of individual's labor supply decision and not evasion decision. To mention a few, Burtless (1987), Ballard (1988), Triest (1990), MaCurdy et al. (1990), Triest (1990), and Bosworth and Burtless (1992) focused primarily on the distortions of individual's labor supply. A bulk of these papers suggest that labor supply is less responsive to taxation (i.e. progressive tax systems) than had previously been thought.

ences in the tax systems across the world. Mitra et al. (1998) pointed out that income tax schedules in most of the industrial societies (including the *OECD* countries) are marginal rate progressive.[2] However, the tax schemes used in these countries sometimes differ in the degree of progressivity (or regressivity). The broad spectrum of tax schemes across the world is certainly not a new phenomenon. Even within the US, the tax systems across the states generally lack uniformity. For example, the southern region is relatively less progressive than the northeastern region. Chernick (2005) claims that overall state-local tax systems were regressive. He further acknowledges that the most progressive state-local tax system was more than three times as progressive as the least, in 1991.

Most often, the use of these tax systems have ignited vigorous debates on how tax burdens should be distributed, and less focused on how taxpayers respond in terms of tax compliance. The latter directly affects taxing authorities (or governments) in at least two ways; enforcement cost and revenue collected. Slemrod and Yitzhaki (2000) confirm that the government spends about 10% of its total tax revenue on tax enforcement alone. Moreover, the US Internal Revenue Service estimates that about 17% of due income taxes are not paid.

With the exception of the progressive and the regressive tax schemes, many factors have been considered to explain tax compliance behavior. If the fundamental tax scheme is important in determining tax compliance behavior, then tax authorities would carefully consider the type of tax regimes they operate. In doing so, taxing authorities may minimize the deadweight costs which may be in the form of administrative, compliance, or behavioral costs. The differences in tax schemes used across different taxing jurisdictions may be motivated by self-serving individual state incentives. Despite these differences, there seems to be little grasp on the direct implications on taxpayers within the state (in terms of tax compliance). To date, it is still uncertain whether a relatively progressive tax regime (hereon PTR) may drive higher non-compliance than a relatively regressive tax regime (hereon RTR).

To answer this question, the Allingham and Sandmo (1972) expected utility model is adopted to

[2] In the US, the state individual income tax in most states is progressive in nature. For example, 33 states have a progressive individual income state while 7 states utilize a flat rate tax (relatively regressive). On the contrary, about 32 states utilize a flat rate state corporate income tax. Approximately 12 states utilize a progressive state corporate income tax.

make predictions. The predictions are then tested in an experimental setting. This model which has widely been used in the tax compliance literature examines the behavior of taxpayers by comparing the benefit and the expected cost of under-reporting in a framework that utilizes a flat tax rate. Most studies that spurred off from this model have generally assessed the potential factors that inhibit truthful reporting using the same setup (a flat tax rate) with the exception of Koskela (1983). His theoretical note focuses on the sensitivity of the penalty scheme under a tax schedule which is progressive.

A bulk of the experimental studies on tax compliance behavior used a flat tax rate to examine different questions. Alm et al. (1992) for example investigate the impact of uncertainty about fine rate, tax rate, and the audit rate on tax compliance. Alm et al. (1999) also present experimental evidence of voting on tax, audit, and fine rates on tax compliance. Fortin et al. (2007), introduce peer effects or social interaction effects to examine tax reporting behavior. Feld and Tyran (2002) assessed tax compliance behavior by introducing voting to approve or renege the proposal of a fine. Cummings et al. (2006) investigated the compliance behavior of individuals when evasion can be accomplished via multiple items. Although the above papers provide reasonable game-simulation environment of the tax compliance process, the tax function or structure in these settings are a gross simplification of the tax structures often used in reality.

However, a handful of other studies like Guth and Mackscheidt (1985) and Becker et al. (1987) have introduced progressive tax schedules, but pay little attention to the impact of progressive tax schedules on tax compliance behavior. Becker et al. (1987) for example investigated the impact of public transfers on tax compliance behavior. They found strong positive correlation between the tax compliance and increase in an individual's transfer payment. This implies that if a taxpayer receives less transfer payment than others, then he or she would be more inclined to evade taxes. This is parallel to the result of Spicer and Becker (1980) who found that the perception of tax burden compared to that of others played a critical role in changing tax compliance behavior.[3]

The work done herein contributes to the literature by introducing the PTR and the RTR in an experimental setting to compare the predicted tax compliance behavior. By introducing the

[3]Both the PTR and RTR inject similar framing issue.

PTR and the RTR, this paper will address the following question: Is there any difference in tax compliance behavior between a progressive and a regressive tax regime assuming both are revenue equivalent?

Several implications arise if either the PTR or the RTR is confirmed to induce higher tax compliance distortions. Higher tax compliance distortions will directly affect revenue collections. When revenue collections decline as a result of lower tax compliance, the administrative cost of the tax system may potentially increase. This means that more resources may be dedicated to monitoring of income reporting. As cost of monitoring of tax reporting increases, tax revenue collections may fall since more resources may be used to collect the same amount of tax revenue. Furthermore, low tax compliance which may lead to low revenue collections by governments can result in dire financial crisis as in the case of Russia in 1998 and Argentina in 2002. The threat of financial crises is enough to foster the search for mechanisms that may encourage tax compliance. The mechanisms that can potentially achieve this goal include strict enforcement mechanisms and tax schemes, the focus of this paper.

As a benchmark, an income reporting model is provided to make inferences about tax compliance behavior. The difference in tax compliance behavior between the PTR and the RTR is simply modeled by allowing the marginal tax rate to change along with a tax subsidy (or lump-sum tax). To make the comparison between the PTR and the RTR concrete, the degree of progressivity (or regressivity) of the tax structure is adjusted such that under full tax compliance, the expected revenue to the government is held constant. Assuming risk neutrality, the model predicts that a utility maximizing taxpayer should comply less under the RTR versus the PTR. This result is predicated on the the assumption that the penalty rate is charged on the evaded taxes.[4] The theoretical prediction from this model is tested in an experimental setting.

The experimental setting which allows us to control for the enforcement, tax rates, income level, and decision environment are fairly similar to that of previous studies. The experiment consisted of

[4]Koskela (1983) showed that the nonequivalence of tax evasion depends on whether penalty is charged on undeclared income or evaded taxes. Goerke (2003) also showed that tax progressivity also influences tax evasion. In his setting, the nonequivalence in tax evasion does not only depend on the whether fine is on evaded tax or undeclared income. Goerke (2003) pointed out that tax evasion also depends on whether taxpayers have to declare their income or tax payments.

two main players, the tax authority and the taxpayer. The role of the tax authority was computerized in Ztree (Fishbacher (1999)). Subjects from the University of Tennessee were invited to play the role of the taxpayers. In each phase of the experiment, the subject-taxpayers were introduced to a tax regime which varied in the degree of progressivity (or regressivity). The subjects were then asked to report their income. The reported income was checked at some exogenous probability. In each of the experimental sessions conducted, the tax systems were anticipated to generate equivalent revenue if subjects reported truthfully. The decision stage was repeated for a number of rounds.

It is important to note that the theoretical model implemented in the laboratory allows for only one corner solution; no tax compliance. This is important, because approximately 51% of all observations are censored at the left tail of the compliance distribution. The main finding in this paper is related to the possibility of differences in tax compliance behavior between the PTR and the RTR. The experimental result confirms that there is no difference in the tax compliance behavior between the PTR and the RTR. The result remained robust across different model specifications. The result of no difference in tax compliance behavior is surprising. Perhaps the result of no difference in tax compliance behavior reaffirms how tax culture within a region potentially plays a far more superior role in determining tax compliance behavior. As observed in the experimental study by Cummings et al. (2004), individuals in Botswana and US were more compliant than those in South Africa; a result that is clearly beyond the conventional economics of crime. Cummings et al. (2004) proposed that if individual attitudes toward tax compliance are a function of social and cultural noms, then enhancing these norms may be a desirable policy.

The experimental results also indicated the tax compliance decision by taxpayers within the PTR and the RTR were motivated by other economic and non-economic factors. Aside from the main variable of interest, the gender of the subjects was found to influence overall tax compliance rate. The compliance rate of males were found to be significantly less than females. Tax compliance was also driven by factors such as fairness and inequality aversion. The experimental result provided strong support of fairness effects. Thus, subjects that considered taxes to be fair complied more relative to subjects that considered taxes to be unfair. Subjects with high inequality aversion complied less; a result that appears to be driven by subjects that participated in the RTR rather than

the PTR. Risk aversion also played an important role in determining tax compliance behavior. In general, tax compliance was found to be higher for relatively risk-averse subjects. Finally, feedback variables produced the gambler's fallacy result of less tax compliance whenever the subjects were audited in the previous round.

The next section of this paper presents the theoretical model, which was drawn from the Allingham and Sandmo (1972) model. It is followed by the experimental design and empirical results. The conclusions are presented in the last section.

3.2 A Model of Tax Compliance Behavior

Consider a simple income reporting model where the objective is for a taxpayer to maximize his or her expected income by choosing how much of his or her income to report. In this model, the only incentive for the economic agent to report their true income is the non-zero probability of not being caught and sanctioned. The focus here is to emphasize the potential differences in behaviors under different tax systems (in terms of the progressivity of the tax schedule). The change in progressivity of a tax schedule and tax compliance has been previously addressed by Koskela (1983) in a theoretical note. Koskela (1983) shows that the tax compliance is sensitive to the nature of the penalty schemes if the taxpayer is caught evading his or her taxes.[5] The sensitivity of tax compliance regarding the nature of penalty schemes forms the basis for the two benchmark models used in this experiment.

The sequence of events is as follows: At period 1, agents earn an income, w_i^k, where the superscript $k = 1$ or 2, such that $w_i^1 < w_i^2$. During period 2, agents are randomly given tax regime, t_j which differ by either the progressivity or the regressivity of the tax. The suffix, j distinguishes the tax regimes. The prevailing tax regime determines the tax schedule that is used to calculate the tax burden of each agent. Next, in period 3, agents are required to report their income for the purposes of calculating their tax burden. Agents can choose to under-report their true income in this period. By choosing to under-report their true income, they are able to reduce their tax burden. Finally

[5] This assertion holds true even when the expected tax revenues of government or the expected utility of taxpayer is held constant.

in period 4, the reported income is checked at some probability, p. The probability of detection is exogenously determined, which implies that it is independent of the reported income. The details of each period are discussed in the subsequent paragraphs.

3.2.1 Model Setup

The model consists of a tax agency and risk-neutral economic agents that play the role of taxpayers. The taxpayer i earns a taxable income of $w_i^k \in W^K$, where W^K is the set of K attainable income levels by the taxpayer. While the taxpayer knows his or her true income the tax authority does not. The taxpayer i must choose how much of his or her income to report. The reported amount is denoted by r_i such that $r_i \in [0, w_i]$. This means that there are no opportunities to report more than the taxpayer's true income.

The subject's tax burden is determined according to the prevailing tax system. The index $j = P$ represents the progressive tax regime while the index $j = R$ represents the regressive tax regime. Let $t_j(r_i)$ be the tax revenue function; for simplicity the tax revenue function is assumed to be linear. Mathematically, a linear progressive tax function is described by

$$t_j(r_i) = -\alpha_j + \beta_j \cdot r_i \tag{3.1}$$

where the parameter $-\alpha_j$ can be interpreted as the amount of tax subsidy. The parameter β_j can be interpreted as the marginal tax rate. For a progressive tax function, the average tax rate, denoted by ATR_j has to be increasing with increments in reported income, that is

$$\frac{dATR_j}{dr_i} = \frac{\alpha_j}{r_i^2} > 0 \tag{3.2}$$

For a regressive, i.e. $\frac{dATR_j}{dr_i} < 0$, the parameter α_j has to be a positive value and this parameter can be interpreted as the amount of lump-sum tax. To make the comparison between the PTR and the RTR concrete, we posit that the revenue generated from both tax regimes must be equivalent under truthful reporting. Since we assume that $t_P(w_i) = t_R(w_i)$, then it is implied that $\beta_P > \beta_R$ in order for revenue equivalence to hold. The condition $w_i > t_j(w_i)$ for all w_i is also imposed. This

assumption is necessary to ensure that there is no bankruptcy.

Finally, there is an exogenous probability, p, of being caught when a taxpayer under-reports and faces a fine denoted by $X(\cdot)$, which is assumed to be convex. The components that determine the penalty schedule are not consistent across the tax compliance literature. For example, in the Allingham and Sandmo (1972) model, the penalty when caught under-reporting income depends on the difference between the true income, w_i and the reported income, r_i. Other studies like Yitzhaki (1974) assume that economic agents are penalized on taxes owed in lieu of unreported income. To conform with penalty schemes in reality and studies like Yitzhaki (1974), a penalty scheme that is charged on the evaded taxes rather than the undeclared taxes is adopted.

Model

Assuming risk neutrality, the general objective of each taxpayer i is to maximize his or her expected utility by choosing r_i. When the penalty is charged on evaded taxes, the objective of the taxpayer i is

$$\max_{r_i} EU(I) = \max_{r_i} \left[w_i - t_j(r_i) - pX\left(t_j(w_i) - t_j(r_i)\right)\right] \tag{3.3}$$

The corresponding necessary condition for an interior solution is

$$-1 + pX'\left(t_j(w_i) - t_j(r_i)\right) = 0 \tag{3.4}$$

Substituting equation 3.1 into 3.4 yields

$$-1 + pX'\left((w_i - r_i)\beta_j\right) = 0 \tag{3.5}$$

The sufficient condition for an interior solution is

$$D = -pX''\beta_j < 0. \tag{3.6}$$

The optimal report, $r^* = r^*(\beta, p, w)$ is implicitly defined by equation (3.5). The following comparative statics are immediate

$$\frac{dr}{d\beta_j} = -\frac{p\theta X''(w_i - r_i)}{D} > 0 \tag{3.7}$$

$$\frac{dr}{d\alpha_j} = 0 \tag{3.8}$$

$$\frac{dr}{dp} = -\frac{X'}{D} > 0 \tag{3.9}$$

$$\frac{dr}{dw} = -\frac{pX''\beta_j}{D} > 0. \tag{3.10}$$

The comparative static result in equation (3.7) shows that tax compliance is increasing with respect to an increase in β_j. However, the level of income reporting will remain unchanged when the level of tax subsidy increases. According to equations (3.9)and (3.10), an increase in the level of detection probability and income is shown to increase tax compliance respectively.

The assumption of revenue neutrality under full tax compliance is key in assessing the effect of changes in the progressivity (or regressivity) of the tax schedule. This means that the change in the expected tax revenue of the tax authority must be equal to zero, i.e. $dE(T) = 0$ when parameters α_j and β_j are altered accordingly. The change in reported income due to a change the α_j and β_j can be decomposed as follows:

$$dr_i = r_{\alpha_j} d\alpha_j + r_{\beta_j} d\beta_j \tag{3.11}$$

where the subscripts are the partial derivatives. Equation (3.11) suggests that the change in income reporting behavior is explained by changes in tax subsidy (lump-sum tax) and the marginal tax rate. Since $r_\alpha = 0$, hence the direction of tax compliance to the changes in α_j and β_j that keeps expected revenue unchanged under full tax compliance is simply determined by

$$\frac{dr_i}{d\beta_j} = r_{\beta_j} = -\frac{pX''(w_i - r_i)}{D_0} > 0. \tag{3.12}$$

The result from equation (3.12) shows that only the marginal tax rate, β_j influences the tax compliance behavior and not the parameter α_j. Thus, the prediction of tax compliance decision is solely dependent on changes in the marginal tax rate.

3.3 Experimental Design

The motivation for the experiment is premised on the predicted differences in tax compliance behavior from the standard model of income reporting. Should taxpayers behave purely according to the compliance gamble, we anticipate that subjects would comply more under the PTR than the RTR. This prediction is directly obtained from equations (3.7) and (3.12). Hence, the experiment has some merits in uncovering the behavioral response of taxpayers under different tax incentives. In the subsequent section, the basic experimental design is introduced along with the parameter values of the experiment.

3.3.1 Basic Design

The experiment was performed to compare tax compliance behavior under the PTR and the RTR. According to the theoretical model, tax compliance decision solely depends on the marginal tax rate. In the spirit of this result, an experiment that imposes different marginal tax rates on different income groups is created. Different marginal tax rates are used on different income groups in order to appropriately simulate the PTR and the RTR. The experiment was designed and programmed in Ztree. The inexperienced subjects used in this experiment were volunteers from different academic backgrounds.[6] Approximately 93% of these volunteers have had some prior experience with experiments.

The experiment was conducted in the Experimental Economics Laboratory at The University of Tennessee. All entries in this experiment were recorded by the subjects via a mouse and keyboard at their respective computer terminals. The instructions of the experiment are included in Appendix 3.6.

The subjects that participated in this experiment appeared to understand the setting.[7] Since the University of Zurich's Ztree program does not allow subjects to review past instructions, a hard copy of the instructions of the experiment were provided to the subjects. Subjects were not allowed to communicate with each other during the experiment.

[6] They were inexperienced in that they had not participated in this experiment before.

[7] A majority of the participants chose a rating above 3 (between a range of 0 and 5) for the clarity of the instructions.

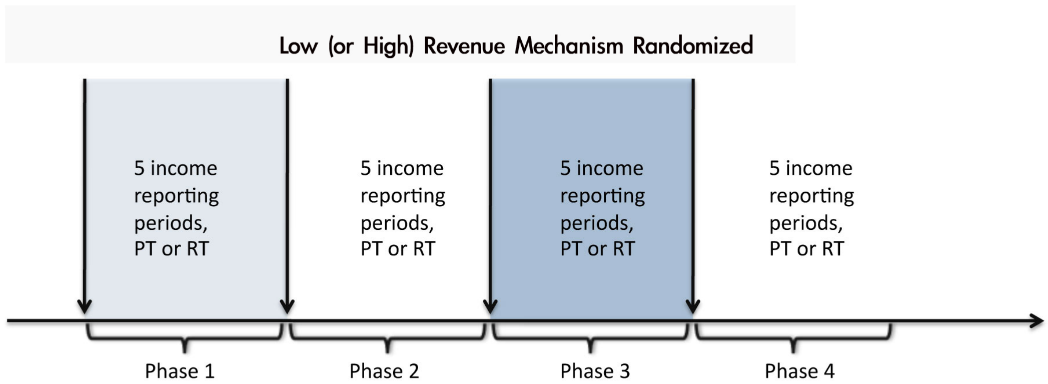

Figure 3.1: Sequence of Events in the Experiment

The experiment is not intended to replicate all the complexities with the income tax reporting process. The experiment consisted of several sessions and each of the experiments had the same basic structure. Figure 3.1 provides a graphical illustration of the sequence of events in this experiment. A typical session lasted about an hour. Each session consisted of four phases and a questionnaire. Each of the phases consisted of five paying rounds. The replication of these rounds was intended for the subjects to learn about their decisions and also obtain feedback on their previous decisions.

Subjects that participated in the experiment were told to play the role of taxpayers in order to frame the experiment. The role of the tax agency was computerized. At the beginning of each phase of the experiment, subjects were randomly assigned either a LOW-type or a HIGH-type. The LOW-type subject was given an income of 50 lab dollars while the HIGH-type was given an income of 100 lab dollars. The audit process was random. The subjects were well informed about the audit rate and the penalties. These parameters were fixed in each of the sessions. In this controlled environment, the participants were simply confronted with a simple task. Subjects were given the opportunity to decide how much of their income they wanted to declare.[8] Subjects paid no taxes on unreported income, however they were told that there was a 10% chance that their report will be

[8]An example of the screen shot of the income reporting decision stage is shown in Figure 45. At the top of the screen, the subjects were given the selected tax schedule. At the bottom of the screen, the subjects were provided a summary of the previous decisions. After the subjects have inputted their reported income, they must click on the declare button to file their taxes. Once the declare button is clicked, their decision cannot be reverted. Immediately after they clicked the declare button, their respective tax shares were calculated and automatically deducted from their earned income.

Table 3.1: The Ten Paired Lottery-Choice Decisions

Option A	Option B	Expected Payoff Difference
1/10 of \$5.00, 9/10 of \$0.50	\$3.00 guaranteed	-\$2.05
2/10 of \$5.00, 8/10 of \$0.50	\$3.00 guaranteed	-\$1.6
3/10 of \$5.00, 7/10 of \$0.50	\$3.00 guaranteed	-\$1.15
4/10 of \$5.00, 6/10 of \$0.50	\$3.00 guaranteed	-\$0.70
5/10 of \$5.00, 5/10 of \$0.50	\$3.00 guaranteed	-\$0.25
6/10 of \$5.00, 4/10 of \$0.50	\$3.00 guaranteed	\$0.20
7/10 of \$5.00, 3/10 of \$0.50	\$3.00 guaranteed	\$0.65
8/10 of \$5.00, 2/10 of \$0.50	\$3.00 guaranteed	\$1.1
9/10 of \$5.00, 1/10 of \$0.50	\$3.00 guaranteed	\$1.55
10/10 of \$5.00, 0/10 of \$0.50	\$3.00 guaranteed	\$2.00

checked. If the subject's report was checked, all unreported income was discovered and the subject paid a penalty equal to unpaid taxes times a penalty factor of 2. Before the beginning of the paying rounds, a number of practice rounds were administered to allow the subjects to familiarize themselves with the experiment. These practice rounds are important because they ensure that any kind of confusion in the experiment is minimized. The payoff of each subject in each earning round was determined according to

$$\pi_i = \begin{cases} e_i + r_i - t_j(r_i) & \text{if not caught} \\ e_i + r_i - t_j(r_i) - P(\cdot) & \text{if caught} \end{cases}$$

where e_i is the amount of unreported income (evasion), r_i is the reported income amount, $t_j(r_i)$ ($j = P$ or R) is the tax revenue function, and $P(\cdot)$ is the penalty function. The penalty function, $P(\cdot)$, is specified such that it is a function of evaded taxes. Hence, $P(\cdot)$ describes a penalty schedule that depends on evaded taxes. Before the start of the experiment, the subjects were first asked to participate in a gamble choice exercise to elicit their risk preferences or risk postures. This exercise was done since the subject's risk postures [Cummings et al. (2006)] may affect their decision to evade. The Holt and Laury (2002) methodology was adopted and used as a guide in classifying subjects as either being risk averse or not. The structure of the lottery incentive is provided in Table 3.1. The subjects were offered two options, namely Option A and Option B. This task

Table 3.2: Experimental Design

Tax System	Low Revenue (LRR)	High Revenue (HRR)
BASE	T1	T2
PTR	T3	T4
RTR	T5	T6

required that the subjects choose between ten individual gambles, that paid either 0.50 or 5 lab dollars in Option A, and a guaranteed amount of 3 lab dollars in Option B. The gambles vary in their respective probabilities of winning the large prize. Following Holt and Laury (2002), the degree of risk aversion is determined by the number of same choices that the subjects selected. After this task was completed, the main experiment was implemented.

The experimental design is shown in Table 3.2. Each session (treatment) varied according to the tax regime which may be either progressive (PTR), regressive (RTR) or proportional (BASE). The BASE treatment replicates the proportional tax experiments that have been previously done. In fact the BASE treatment is a replication of the experimental design of Alm et al. (1992). Within each of these sessions, two revenue targets were introduced, namely, low revenue requirement (hereon LRR) and high revenue requirement (hereon HRR). Across these sessions, the LRR (HRR) generates the same amount of revenue in the BASE, PTR and RTR treatments. The LRR generates 30 lab dollars and the HRR generates 45 lab dollars if the subjects report truthfully.

At the end of the tax experiment, subjects were asked to participate in a short survey. The questionnaire included questions on demographics and personal opinions. The questionnaire provided further control variables in the regression analysis.

There were eight sessions total, each having 16 to 18 subjects. Subjects that participated in the experiment earned about $16. The earnings depended solely on their performance in the experiment. The subjects were paid in cash privately at the end of the experiment.

Assuming all the subject participants report truthfully, revenue generated should be equivalent across each tax regime treatments for the LRR (30 lab dollars) and the HRR (45 lab dollars) respectively. In treatments 1 and 2 (or $T1$ and $T2$), subjects were asked to report their income in a proportional tax system (BASE). Subjects that received an income of 50 and 100 lab dollars both

Table 3.3: Experimental Parameter Values

Tax Systems	Income	ATR under LRR (30)	ATR under HRR (45)
PTR	50	0.18	0.25
	100	0.21	0.33
RTR	50	0.23	0.39
	100	0.19	0.26
BASE	50	0.2	0.3
	100	0.2	0.3

Notes: Table summarizes the average tax rate associated with the income of the subject-taxpayer. BASE represents the proportional tax treatment.

paid 2 cents on each reported lab dollar in the LRR and 3 cents in the HRR. The order of the LRR and HRR treatments within each session was randomized. Each of these treatments lasted for 5 rounds before it was altered to another treatment. There were 20 paying rounds in each session. The parameter values are listed in Table 3.3.

In treatments 3 and 4 (or $T3$ and $T4$), the subjects were asked to decide how much of their income to report under the PTR. Subjects that received an income of 50 lab dollars paid 18 cents and 25 cents in the LRR and HRR treatments respectively for each dollar of reported income. On the other hand, the subjects that received 100 lab dollars paid 21 cents and 33 cents in the LRR and HRR treatments respectively for each dollar of reported income. The information about the tax rate each income group received was public knowledge. The relative tax burden on high income subjects in this treatment was higher than the low income subjects. Thus, low income types knew the tax rate of high income types and vice versa. In treatments 5 and 6 (or $T5$ and $T6$), subjects faced the RTR where the relative tax burden of the low income subjects was higher than the high income subjects. For example, if the subject received an income of 50 lab dollars, the subject paid 23 cents and 39 cents in the LRR and HRR treatments respectively for each dollar of reported income. However, the high income subjects paid 19 cents and 25 cents in the LRR and HRR treatments respectively for each dollar of reported income.

As with the revenue mechanism treatments, the auditing rounds in each of these sessions were randomized. The auditing or detection rate was 10%. The income level of the subjects were also randomized in each phase of the experiment.

Table 3.4: Tax Compliance Predictions

	PTR	RTR	PTR vs RTR
	$\beta_j^L < \beta_j^H$	$\beta_j^L > \beta_j^H$	$\beta_j^P > \beta_j^R$
Model	full evasion	full evasion	full evasion
Inequality Averse	$CR_L \leq CR_H$	?	$CR_P \geq CR_R$
Risk Averse	$CR_L \leq CR_H$	$CR_L \geq CR_H$	$CR_P \geq CR_R$
Risk Loving	?	?	?

Notes: CR_L stands for compliance rate for a low income taxpayer and CR_H stands for compliance rate for a high income taxpayer.

3.3.2 Testable Hypothesis

Recall that the fundamental focus in this paper is the test on the behavioral differences between the PTR and the RTR. Based on the self-interested theoretical framework previously discussed, the following predictions can be tested. It is hypothesized that taxpayers should comply more under the PTR than the RTR. The configuration of the experimental parameters are similar to parameter values in the real world and previous studies. In theory, a risk-neutral expected utility maximizer would report zero income. However, other things being equal, the incentive to report truthfully is greater for taxpayers facing a higher marginal tax rate. To reconcile the theory and the experimental setting, a prediction of a weak inequality between the PTR and the RTR is established. In that sense, we should anticipate (weak) higher compliance rates in treatments $T3$ ($T4$), than in session $T5$ ($T6$). A summary of the predictions are summarized in Table 3.4.

Beyond the predictive power of the theoretical model discussed previously, the PTR and the RTR introduce other behavioral effects. It can be argued that a taxpayer's tax compliance decision is not only dependent on his or her tax burden alone but also on the tax burden of other taxpayers. As suggested by Fortin et al. (2007), taxpayers are not completely individualistic, thus some amount of social interactions may also influence tax compliance behavior. This is also in line with the study of Spicer and Becker (1980) who found strong correlation between tax compliance and perceptions. So it is expected that if taxpayers perceive a particular tax regime to be fair, then they would

comply more under that tax regime. However, tax compliance is expected to be higher under the PTR than the RTR if fairness (equality) plays a huge role in driving behavior.

Table 3.4 also provides a summary of other predictions if taxpayers weigh in their taste for equality (fairness) or risk preferences. The presence of high inequality aversion may drive higher non-compliance in the RTR than the PTR. However, presence of low inequality aversion will produce a counter result. Individuals in reality exhibit a significant amount of risk aversion [Binswanger (1980), Kachelmeier and Mohamed (1992), and Holt and Laury (2002)]. Therefore, taking the presence of risk into account, we can make some predictions based on our intuition. If an economic agent chooses to report an amount less than his or her true income, then he or she automatically faces a lottery. The lottery basically consists of *payoff* 1 if the agent is caught under-reporting, and *payoff* 2 if the agent is not caught. The variance between *payoff* 1 and *payoff* 2 under the PTR is larger than the alternate RTR. Differences in lottery under the PTR and the RTR directly change the costs and benefits of non-compliance. For example, the costs and benefits of non-compliance is higher under the PTR than the RTR.

Even without solving the model that imposes the assumption of risk-aversion, we can predict the following based on the differences in the marginal tax rates under the PTR and the RTR: *(i)* assuming the economic agent is risk-averse, then non-compliance in taxes will be greater under the RTR than the PTR. Note that the above predictions are specified with weak inequality due the experimental parameter values; *(ii)* if the agent is risk-seeking, then the direction of non-compliance may be ambiguous due to two offsetting effects. For example, a higher marginal tax rate under the PTR will increase tax compliance according the theoretical model. However, with a higher marginal tax rate the benefit from non-compliance if taxpayer is not caught is higher under the PTR than the RTR. As a result risk-seeking subjects under the PTR may comply less than those under the RTR. Hence the two compounding effects may produce an ambiguous tax compliance result.

3.4 Description of Data

The definition of the variables used for the analyses is provided in Table 12 in Appendix 3.6. Table 13 in the Appendix provides a statistical summary of the data collected from the experiment. The

average compliance rate across both the PTR and the RTR is about 25%. The means, standard deviations, minimum and maximum values are reported in columns 2 to 5.

One hundred and nineteen subjects participated in the experiment consisting of 20 paying rounds. Hence, the data-set constituted 2380 pooled observations. Out of 119 participants in the experiment, 81 of them were males and 38 were females. Their age ranged from 18 to 39 years. Approximately 38% of the observations were from the PTR treatment, 39% of the observations were from the RTR treatment, and 24% of the observations were from the BASE treatment. Overall, 48% of the subjects received an income of 50 lab dollars and 52% of the subjects received 100 lab dollars.

Throughout, a comparison between the behavioral response of subjects in terms of compliance rate instead of evasion rate will be made. Similar to Alm et al. (1992) and Alm et al. (1999), a compliance rate measure was constructed by dividing the reported income by the true income.

3.5 Results

Based on the parameter values that the subjects were provided, the optimal strategy for each subject is to report zero income amount regardless of the tax regime. Hence, the strategy space rules out any positive tax compliance rate. Each treatment was repeated a sufficient number of periods to insure convergence. At least 50% of the observations in the PTR and the RTR reported zero compliance rate. This result is important because it shows that subjects behaved according to the expected utility theory. A summary of the proportion zero and full tax compliance is reported in Table 3.5. Table 3.5 indicates that the decision to report income is influenced by different factors that may not necessarily be explained by expected utility theory. Schoemaker (1982) and Machina (1987) echo similar sentiment. Table 3.5 shows evidence of this claim in terms of the three staircases of tax compliance; full, some and zero compliance rate. First, it appears that a sizable portion of the subjects chose the optimal strategy, that is to report zero income. Second, a relatively large proportion of the subjects also chose to report some of their income. Third, a small fraction of subjects in all of the sessions did not even search for ways to cheat. Pyle (1991) points out that whilst the odds are heavily in favor of evaders getting away with cheating of taxes, some taxpayers

Table 3.5: Proportion of Compliance Groups

Tax System		Proportion of Zeros	Proportion of Ones
Progressive	LRR	0.52	0.06
	HRR	0.54	0.08
Regressive	LRR	0.54	0.09
	HRR	0.52	0.12
BASE	LRR	0.39	0.07
	HRR	0.43	0.12

are relatively more inclined to report honestly.

A comparison of tax compliance behavior between the PTR and the RTR is considered. The highest proportion of subjects that were honest were subjects that participated in RTR. Between 9% to 12% of the observations in the RTR were found at the right tail of the compliance rate distribution compared to 6% to 8% in the PTR. Both the PTR and the RTR had an average of about 53% of their observations massed at zero. The relatively large proportion of subjects that chose compliance rates between 0% and 100% justifies the exploration of potential differences in tax compliance behavior between the PTR and RTR.[9] Average compliance rates along with standard errors in the various treatments are reported in Table 3.6. On average, tax compliance rate for the baseline treatment was approximately 38% in the LRR and 34% in the HRR. Even with the small sample of data points for this treatment, the direction of tax compliance is consistent with Alm et al. (1992). The experimental result of Alm et al. (1992) claim that an increase in taxes leads to a decrease in tax compliance. However, tax compliance rate appears to increase with increments in the progressivity or regressivity of the tax. For example, the average compliance rate was approximately 24% in the LRR and 26% in the HRR under the RTR. In contrast to the PTR, participants on average had a tax compliance rate of approximately 23% in the LRR and 25% in the HRR. It is surprising that the average compliance rate is nearly identical across both the PTR and the RTR. Figure 3.2 also provides support for this claim. Figure 3.2 is simply a plot of average compliance rate over 20 paying rounds. Figures 29 and 30 also provide a summary of average compliance rates

[9]There were notable differences in tax compliance behavior between subjects that had participated in the experiment before. Overall, subjects that had participated in the experiment before complied less than new participants. See Figure 31 in the Appendix.

Table 3.6: Average Compliance Rates

Tax System		LI Comp. Rate [Std]	HI Comp. Rate [Std]	Overall Comp. Rate [Std]
Progressive	LRR	0.223	0.249	0.236
		[0.022]	[0.024]	[0.016]
	HRR	0.262	0.262	0.262
		[0.025]	[0.024]	[0.017]
Regressive	LRR	0.263	0.235	0.234
		[0.026]	[0.337]	[0.016]
	HRR	0.337	0.215	0.272
		[0.031]	[0.026]	[0.020]
BASE	LRR	0.394	0.369	0.381
		[0.035]	[0.029]	[0.023]
	HRR	0.399	0.302	0.347
		[0.036]	[0.030]	[0.023]

Notes: Average compliance rates are reported along with standard deviations. Standard errors are in [ ].

in the three tax systems. Figure 29 includes all the data points in each tax system while Figure 30 excludes observations at the extremities (or tails).

To fully assess tax compliance behavior across the tax regimes, a tax compliance measure is regressed on tax regime dummies and other controls. The parametric estimation results are summarized in Table 3.7. The property of the dependent variable provided an estimation challenge. Figure 27 in Appendix 3.6 provides the distribution of the compliance rate. A quick inspection of this figure indicates that some subjects are naturally honest and therefore comply fully even though it is not optimal. A bulk of the participants were completely dishonest. The large concentration of this extreme observed behavior in the dataset accounts for the mass at the two tails of the distribution. To appropriately account for these masses in the data, the two-tailed Tobit estimation method was employed. The two-tailed Tobit method provides consistent estimators.

The two-tailed Tobit model is reported alongside the random-effects model. The random-effects model is reported to provide some comparison. The reduced-form equation that explains the average compliance rate takes the form:

$$CR = a_0 + a_1 PROGRESSIVE + a_2 MTR + a_3 HIGHINCOME + \varepsilon \tag{3.13}$$

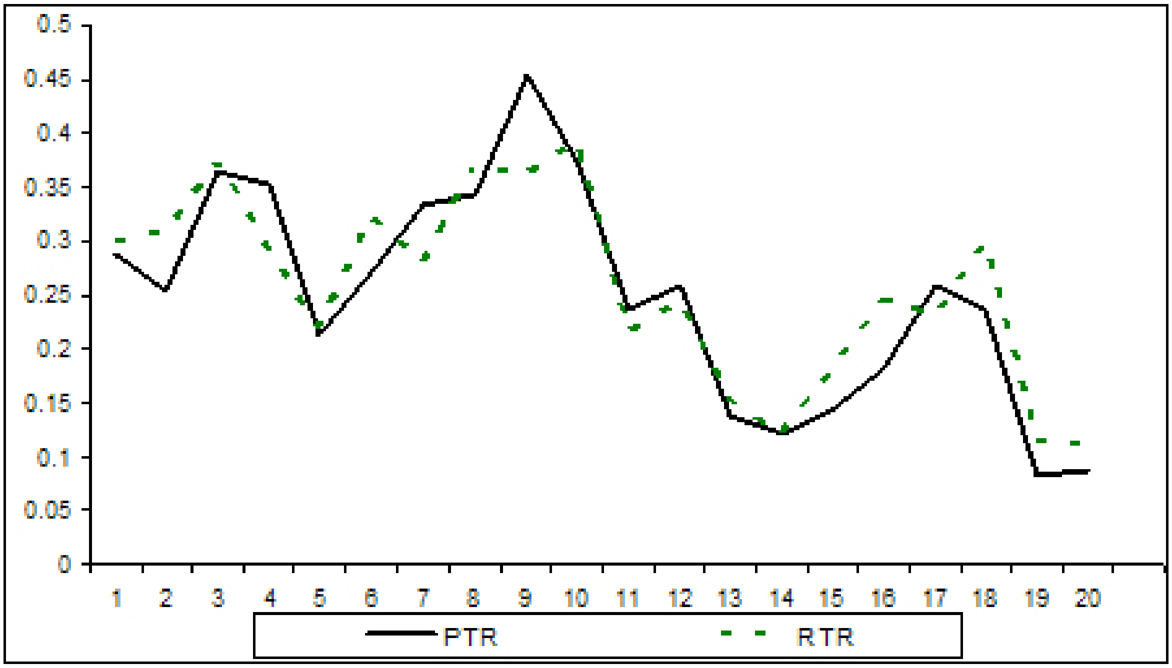

Figure 3.2: Average tax compliance rate across 20 paying rounds

where CR stands for compliance rate, and defined as the amount of reported income by the subject divided by the true income; PROGRESSIVE is a dummy variable that equals 1 if the tax system is progressive, MTR variable is the marginal tax rate on each dollar of lab dollar reported by the subject, and HIGH INCOME is a dummy variable that equals 1 if the salary of the subject is 100. Recall that the dependent variable CR is censored at 0 and 1, thus the regression equation is estimated using a two-tailed Tobit method. A thorough discussion of the estimation procedure is discussed in Appendix 3.6.[10]

The regression results reported in Table 3.7 are consistent across all model specifications. The econometric results focus mainly on the tax regime treatment. The important result from the econometric analysis is that, average compliance rate between the PTR and the RTR is statistically not different from zero, holding everything else constant.[11] While this result must be interpreted with caution, it was interesting to observe that subject-taxpayers were not influenced by the tax regime

[10]See Wooldridge (2002) and Cameron and Trivedi (2005).

[11]An evaluation of the subject decisions is done through several robustness check methods to ensure convincing findings. These results are discussed in the subsequent section.

Table 3.7: Estimation Results of Tax Compliance Equation

Variable	RE	2T-Tobit	RE	2T-Tobit
Constant	0.192***	-0.263***	0.189***	-0.269***
	[0.038]	[0.110]	[0.042]	[0.134]
MTR	0.237***	0.487**	0.198**	0.488**
	[0.101]	[0.233]	[0.092]	[0.234]
PROGRESSIVE			0.009	0.005
			[0.058]	[0.183]
HIGH INCOME			0.006	0.002
			[0.012]	[0.033]
Number of Obs.	1504	1504	1504	1504
R^2	0.09	0.11	0.10	0.11

Notes: Standard errors are in [ ]. *** Significant at 1%; ** Significant at 5%; * Significant at 10%;

imposed on them. The result remained robust across different model specifications.[12] Although this result may be uninteresting, it still conveys an important contribution about how redistribution of tax burden affects tax compliance. It also conveys the result of how social interaction effects (which is a derivative of the two tax schemes) affects tax compliance decision. The explanation of social interaction effects is that the fiscal parameters that both income groups faced in the two schemes was public knowledge. The most likely explanation for the result no social interaction effects is that, tax compliance behavior is an individualistic process. The public knowledge of the relative tax burden of each income group did not influence the income reporting decision of subjects. A broader interpretation of this result that is gaining traction is the effect of cultural norms on tax compliance behavior. Nerre (2008) points out that tax culture is far more important than the 'tax' component.[13] For the most part, differences in tax compliance were not present between the high income group and the low income group. Only one RTR session produced significant difference in tax compliance behavior. A rather odd result of higher non-compliance was found to be present in the high income group than the low income group. Apart from this result, there were no other significant differences in tax compliance behavior in either the PTR or the RTR. One should note

[12]There is evidence that a large proportion of subjects chose zero compliance rate in both the PTR and the RTR. Approximately 51% of the observations in the PTR and the RTR are massed at zero compliance rate.

[13]As an anecdote, the tax compliance rate is higher in the US than other developed and developing countries like Russia and Ghana.

that the observations from the experiment are relatively homogenous. Subjects that participated in the experiment were mainly from the southern region of the US.[14]

The MTR variable was statistically significant and positively related to compliance rate; a result also obtained by Fortin et al. (2007) and Alm et al. (1995). This finding is not surprising since most experimental results regarding the tax compliance response to changes in tax rates have not been clear-cut. Other studies like Friedland et al. (1978) and Alm et al. (1992) have found tax compliance to be decreasing with increase in tax rates. The level of income had no significant impact on tax compliance.[15]

For exploratory purposes, we report the responses of other variables to tax compliance. The results of the estimated equations are reported in Table 3.8. The control variables considered were AGE, GENDER, FEEDBACK, FAIRNESS, INEQUALITY AVERSION, and RISK AVERSION. The variables like age, gender, fairness and inequality aversion were ascertained by the post-experimental survey.

A MALE dummy variable was constructed and this variable equals 1 when the subject-taxpayer is a male. The coefficient of the MALE variable was negative and statistically significant at the 1% significance level. The coefficient suggests that average tax compliance was reduced by 26% when the subject-taxpayer was a male. Alm et al. (1989) also found similar results. Torgler (2003) does not find strong difference between males and females, but confirms that the tax compliance is a bit higher for females. Also, the average compliance rate was statistically and positively related to the AGE variable in the random-effects model (a result also obtained by Friedland et al. (1978)) and not the two-limit Tobit model. The implication from this result is that older individuals are more compliant than younger individuals. This is symmetrical to the view that young individuals like to take more risk than older individuals.

The feedback variables also produced interesting results. The lag variables were LAG PENALTY,

[14]Preferably, a pooled data-set that includes individuals from different regions in the US would provide enough heterogeneity and robustness test of the main research hypothesis in this paper.

[15]Similar to the relationship between tax compliance and taxes, the impact of income on tax compliance is also not clear-cut. The direction of income with respect to tax compliance is similar to the studies by Torgler (2003) and Friedland et al. (1978). They also found a negative relationship between tax compliance and income. Other studies like Spicer and Becker (1980) and Jackson and Jones (1985) found a positive relationship between tax compliance and income.

LAG AUDIT and LAG WEALTH. The result of gambler's fallacy was found across the two regression analysis. The lagged audit variable was found to be statistically and negatively related to tax compliance. This result is related to the notion that subjects put little weight on audit occurring in the subsequent rounds after they were audited in the previous round. As a result, subjects tend to evade more of their taxes after an audit round. Also the lagged wealth variable was found to be statistically and negatively related to the tax compliance rate.[16]

Following Fortin et al. (2007), an index that proxies for INEQUALITY AVERSION was constructed.[17] Evidence from the two-limit Tobit estimates, suggest that subjects who were classified as high inequality averse complied less.

A grade in terms of fairness of the tax regimes was obtained from the post-experimental survey. Subjects were asked to choose a value between 0 (least fair) and 5 (very fair) to indicate the degree of *fairness* of the the tax regime. According to regression results, fairness mattered in terms of tax compliance. Subjects complied more regardless of the tax regime as long as they thought it was a fair tax regime. Finally, a RISK AVERSE variable was constructed to classify subject-taxpayers as risk-averse. The RISK AVERSE variable was positively and significantly related to the average compliance rate. This result is consistent across all model specifications.

3.5.1 Robustness Check

The results discussed in this section pertain to a propensity model. The propensity model examines the tax compliance behavior by decomposing the observed compliance behavior of subjects including observations at the tail of the data distribution. The panel Logit model is estimated and reported in Table 3.9. The coefficients are the regular Logit coefficients, not marginal effects. The signs of the coefficients can be interpreted as directional responses. The results from the propensity model serve mainly as a robustness check. The results in Table 3.9 are associated with three different

[16]To further explore subjects decisions across different tax regimes, the level of tax evasion is regressed on covariates found in Table 3.8. The results from this model are consistent with previous result. For example, increase in the marginal tax rate was found to decrease the level of tax evasion. Risk aversion and fairness was also found to be negatively correlated to the level of tax evasion. Also increase in the level of income was found to increase tax evasion; a counter result of the theoretical model provided in the previous section.

[17]Subjects had to indicate their favorite share among to options: The alternative shares were (50 50) against (55, 65), (50 50) against (45, 70) and (50 50) against (35, 85). The index value is between 0 and 2. A high index value represents high aversion while a low index value represents low aversion.

equations. Column 2 of Table 3.9 reports the results associated with the equation that estimates the propensity to cheat fully. The dependent variable takes the value of 1 if the subject cheats fully and zero otherwise. The tax regime had no significant impact on the decision to cheat fully. The MALE variable is the only variable that was found to induce subjects to cheat fully. The result is significant at the 1% significance level.

Next, we consider the propensity model that estimates the factors that induce subjects to comply fully. The reported results in column 3 is associated with this estimated equation. Interestingly, the variable PTR had no impact. The MTR and RISK AVERSE variables were found to be statistically significant. These results indicate that high marginal tax rate and risk-aversion tend to influence the subject's decision to comply fully.

The last column in Table 3.9 report the results of the equation that estimates the propensity to partially cheat on taxes. All the variables were found to have no significant impact on the decision to partially cheat with the exception of the MALE dummy variable. The result indicates that males are more inclined to cheat than females.

3.6 Conclusion

This paper uses experimental data to simply examine whether taxpayers are sensitive to different tax schemes. The experiment captures the essential features of tax systems in the real world, namely the progressive and the regressive tax systems. Drawing on expected utility theory, this paper sheds light on the sensitivity of risk-neutral taxpayers when faced with a portfolio-choice problem that characterizes evasion as a risky asset and full disclosure as a riskless asset.

The testable hypothesis that falls out of the extended Allingham and Sandmo (1972) model of income reporting is that, tax compliance should be weakly greater in the progressive tax regime than the regressive tax regime. If this hypothesis is correct, then redistribution of income may lead to higher tax compliance in the progressive tax regime and lower tax compliance in the regressive tax regime.

The framing of the tax compliance decision introduces some social interaction effects. There were two income groups. Each income group knew the tax fiscal parameters they faced. The

fiscal parameters faced by the other group were also public knowledge. This explanation of social interaction effects is likely to drive tax compliance. This paper addresses whether these interaction effects are strong enough to drive different tax compliance behavior under the two tax schemes.

The results from the experiments indicate that the progressive and the regressive tax systems generate similar tax compliance behavior. This result was consistent across all model specifications. The result of equivalence in tax compliance between the progressive and the regressive tax schemes continue to prevail even after controlling for income level, tax rate, gender, age, fairness, inequality aversion, risk aversion, and feedback effects. There were at least three reasons that drove tax compliance behavior in both tax schemes. The most significant findings suggest that high tax revenue mechanisms lead to higher compliance rates. Fairness plays a significant role in determining individual tax compliance behavior. Similarly, risk aversion contributed to an individual's tax compliance behavior. Personal characteristics of the individual such as gender influenced tax compliance behavior.

From a broader perspective, the result of equivalence in tax compliance behavior between the progressive and the regressive tax schemes suggests that social interaction effects is not a strong factor that explains tax compliance. Rather, the results strengthen the position that tax compliance is an individualistic process that is more likely to be influenced by the tax-culture within a society. For the most part, subjects behaved according to the expected utility theory by choosing zero compliance in both the progressive and regressive tax regimes. Both tax schemes suggest that fiscal parameters and fairness-effects are more important in explaining individual tax compliance behavior.

Table 3.8: Estimation Results of Tax Compliance Equation

Variable	RE	2T-Tobit	RE	2T-Tobit
Constant	2.507***	6.134***	2.314***	5.131***
	[0.212]	[0.798]	[0.210]	[0.727]
PROGRESSIVE	-0.015	0.029		
	[0.028]	[0.162]		
REGRESSIVE			-0.002	-0.052
			[0.027]	[0.155]
MTR	0.077***	1.300***	0.76***	1.413***
	[0.014]	[0.237]	[0.014]	[0.238]
HIGH INCOME	-0.003	-0.010	-0.00001	-0.009
	[0.014]	[0.031]	[0.014]	[0.031]
AGE	0.012***	0.005	0.009*	0.004
	[0.005]	[0.021]	[0.005]	[0.021]
MALE	-0.319***	-0.835***	-0.299***	-0.729***
	[0.030]	[0.175]	[0.029]	[0.166]
RISK AVERSE	0.099***	0.381**	0.088***	0.295*
	[0.031]	[0.192]	[0.029]	[0.184]
INEQ. AVERSE	-0.052*	-0.321*		
	[0.028]	[0.183]		
FAIR			0.06***	0.376***
			[0.013]	[0.188]
LAG PENALTY	-0.116***	-0.007***	-0.119***	-0.007***
	[0.033]	[0.002]	[0.033]	[0.002]
LAG WEALTH	-0.0002***	-0.0005***	-0.0002***	-0.0005***
	[0.00002]	[0.00004]	[0.00002]	[0.00004]
EXPERIENCED	-0.220***	-0.638†	-0.241***	-0.669*
	[0.068]	[0.409]	[0.067]	[0.407]
Number of Obs.	1508	1508	1508	1508
R^2	0.273	0.194	0.229	0.197

Notes: Standard errors are in [ ]. *** Significant at 1%; ** Significant at 5%; * Significant at 10%;

Table 3.9: Tax Compliance Propensity Model

	Cheat Fully	Comply Fully	Cheat Some
PROG	-0.277	0.544	0.239
	[1.224]	[0.767]	[0.921]
HI	0.005	-0.114	-0.079
	[0.228]	[0.281]	[0.195]
MTR	-6.932***	7.589***	1.833
	[1.733]	[2.249]	[0.482]
Risk Averse	-2.191*	1.743*	0.294
	[1.323]	[0.960]	[1.026]
Age	-0.011	-0.010	-0.034
	[0.134]	[0.092]	[0.118]
Male	7.497***	-1.228†	-4.613***
	[1.345]	[0.783]	[1.009]
Lag Penalty	0.058***	-0.068†	-0.043***
	[0.014]	[0.043]	[0.012]
Lag Wealth	0.004***	-0.002***	-0.002
	[0.004]	[0.0004]	[0.0003]
Constant	-39.563***	17.16****	19.994***
	[5.510]	[5.004]	[4.229]
Number of Obs.	1501	1501	1501
R^2	0.246	0.151	0.146

Notes: Standard errors are in [ ]. *** Significant at 1%; ** Significant at 5%; * Significant at 10%;

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Appendices

Appendix A: Proofs

Proof of Proposition 1

Let the manager choose (E_1, F_1) when he or she is compensated with contract $(\alpha_1, K_1, \omega_1)$. There are three cases to consider: Case (i). If (E_1, F_1) is such that $\Pr(\tilde{S}^M > K) = 0$, then it is straight forward to show that $E_1 = 0$ and $F_1 = 0$. We ignore the uninteresting case when the stock option is "under-water" since the manager will choose zero effort and zero fraud. Case (ii). If (E_1, F_1) is such that $\Pr(\tilde{S}^M > K) = 1$, then (E_1, F_1) solves equations 2.2 and 2.3. Case(iii). If (E_1, F_1) is such that $0 < \Pr(\tilde{S}^M > K) < 1$, then (E_1, F_1) must be the simultaneous solution to equations 2.5 and 2.6. It is straightforward to verify that 2.2 and 2.3 together imply (A1) below, and that 2.5 and 2.6 together also imply (A1):

$$\frac{\phi'(E)}{pf'(E)} = x'(F) \tag{A1}$$

Therefore, for case (ii) and case (iii), the manager's optimal choice (E_1, F_1) must satisfy

$$\frac{\phi'(E_1)}{pf'(E_1)} = x'(F_1) \tag{A2}$$

Similar reasoning implies that if the manager chooses (E_2, F_2) when he or she is compensated with contract $(\alpha_2, K_2, \omega_2)$, then (E_2, F_2) must satisfy

$$\frac{\phi'(E_2)}{pf'(E_2)} = x'(F_2) \tag{A3}$$

Now suppose that $E_1 = E_2$, then (A2) and (A3) imply

$$x'(F_1) = x'(F_2) \tag{A4}$$

Since $x(F)$ is increasing and strictly convex, it follows that $x'(F)$ is monotonic. Finally, using the fact that $x'(F)$ is monotonic, (A4) implies $F_1 = F_2$.

Proof of Convergence

Consider two contracts; O and E. Contract is O is the option contract and contract E is the simple equity contract. Let $\underline{s}$ and $\bar{s}$ denote the lower and upper bound respectively. Let $\bar{s} = \underline{s} + 2b$. Now consider the contract E that is in the money with certainty. Under contract E, the manager's objective is

$$\frac{\alpha}{2b}\int_{\underline{s}}^{\underline{s}+2b}(S-K)dS - \phi(e) - px(f) \tag{14}$$

By further manipulation, the above objective reduces to

$$\alpha b + \alpha(\underline{s} - K) - \phi(e) - px(f) \tag{15}$$

In order to test for convergence, let's suppose that $K \to \underline{s}$, then

$$\lim_{K\to\underline{s}}[\alpha b + \alpha(\underline{s} - K) - \phi(e) - px(f)] \tag{16}$$

$$\alpha b + \alpha \cdot \lim_{K\to\underline{s}}[\underline{s} - K] - \phi(e) - px(f) \tag{17}$$

$$\alpha b + \alpha K \cdot \lim_{K\to\underline{s}}[\frac{\underline{s}}{K} - 1] - \phi(e) - px(f) \tag{18}$$

$$\alpha b + \alpha K \cdot \{\lim_{K\to\underline{s}}[\frac{\underline{s}}{K}] - \lim_{K\to\underline{s}}[1]\} - \phi(e) - px(f) \tag{19}$$

$$\alpha b + \alpha K \cdot \{(1) - (1)\} - \phi(e) - px(f) \tag{20}$$

$$\alpha b - \phi(e) - px(f) \tag{21}$$

Now consider the contract O. Recall that the manager's objective when the option is in the

money with some probability is

$$\alpha b\pi^2 - \phi(e) - px(f) \tag{22}$$

which can be re-written as

$$\alpha b\{\frac{[\bar{s} - K]}{2b}\frac{[\bar{s} - K]}{2b}\} - \phi(e) - px(f) \tag{23}$$

$$\alpha b\{\frac{[\underline{s} + 2b - K]}{2b}\frac{[\underline{s} + 2b - K]}{2b}\} - \phi(e) - px(f) \tag{24}$$

Again as $\underline{s} \to K$, then

$$\alpha b \lim_{K\to\underline{s}}\{(\frac{[\underline{s} - K]}{2b} + 1)(\frac{[\underline{s} - K]}{2b} + 1)\} - \phi(e) - px(f) \tag{25}$$

$$\alpha b \lim_{K\to\underline{s}}\{(\frac{[\underline{s} - K]}{2b} + 1)\} \cdot \{\lim_{K\to\underline{s}}(\frac{[\underline{s} - K]}{2b} + 1)\} - \phi(e) - px(f) \tag{26}$$

$$\alpha b\{\frac{K}{2b}[1 - 1] + 1)\}(\frac{K}{2b}[1 - 1] + 1)\} - \phi(e) - px(f) \tag{27}$$

$$\alpha b - \phi(e) - px(f) \tag{28}$$

This implies that the two objectives converge as $K \to \underline{s}$.

Appendix B: Estimation Notes

Two-Tailed Tobit Model

Consider the following linear model with random effects

$$y_{it} = X_{it}\beta + \delta_i + \epsilon_{it}$$

where $i = 1, ..., N$ represents each subject over $t = 1, ..., T$ periods. The dependent variable y_{it} represents compliance rate and X_{it} represents a set of covariates. The random effects, δ_i are i.i.d., $N(0, \sigma_\delta^2)$ and ϵ_{it} are i.i.d $N(0, \sigma_\epsilon^2)$ independently of δ_i. The percent contribution to the total variance of the panel-level variance is capture by

$$\rho = \frac{\sigma_\delta^2}{\sigma_\epsilon^2 + \sigma_\delta^2}$$

The tobit panel estimator is no different from the tobit pooled estimator if $\rho = 0$.

Let y_{it}^O denoted the observed data which also includes the censored version of y_{it}. y_{it}^O is censored at both tails of the distributions. At the left-censored, $y_{it} \leq y_{it}^O = 0$ while at the right-censored $y_{it} \geq y_{it}^O = 1$. If y_{it} is uncensored, $y_{it} = y_{it}^O$.

The joint (unconditional of δ_i) density of the observed data from the i^{th} panel

$$f(y_{i1}^O, ..., y_{in_i}^O | X_{i1}, ..., X_{in_i}) = \int_{-\infty}^{\infty} \frac{e^{-\delta_i^2/2\sigma_\delta^2}}{\sqrt{2\pi}\sigma_\delta} \left\{ \prod_{t=1}^{n_i} F\left(y_{it}^O, X_{it}\beta + \delta_i\right) \right\} d\delta_i$$

where

$$F\left(y_{it}^O, \triangle_{it}\right) = \begin{cases} (\sqrt{2\pi}\sigma_\epsilon)^{-1} e^{-(y_{it}^O - \triangle_{it})^2/2\sigma_\epsilon^2} & \text{if } y_{it}^O \in NC \\ \Phi\left(\frac{y_{it}^O - \triangle_{it}}{\sigma_\epsilon}\right) & \text{if } y_{it}^O \in L \\ 1 - \Phi\left(\frac{y_{it}^O - \triangle_{it}}{\sigma_\epsilon}\right) & \text{if } y_{it}^O \in R \end{cases}$$

where NC represent the noncensored observations, L is the set of left-censored observation, R is the set of right-censored observations, and $\Phi()$ is the cumulative normal distribution.

The panel level likelihood l_i is given by

$$l_i = \int_{-\infty}^{\infty} \frac{e^{-\delta_i^2/2\sigma_\delta^2}}{\sqrt{2\pi}\sigma_\delta} \left\{ \prod_{t=1}^{n_i} F\left(y_{it}^O, X_{it}\beta + \delta_i\right) \right\} d\delta_i \equiv \int_{-\infty}^{\infty} g\left(y_{it}^O, X_{it}, \sqrt{2}\hat{\sigma} a_m^* + \hat{\mu}_i\right) d\delta_i$$

The default approximation of the log likelihood is by adaptive Gauss-Hermite quadrature, which approximates the panel level likelihood with

$$l_i \approx \sqrt{2}\hat{\sigma} \sum_{m=1}^{M} w_m^* \exp\left\{(a_m^*)^2\right\} g\left(y_{it}^O, X_{it}, \sqrt{2}\hat{\sigma} a_m^* + \hat{\mu}_i\right)$$

The log likelihood is approximated by

$$\begin{aligned} L \quad \approx \quad & \sum_{i=1}^{n} w_i \log\{\sqrt{2}\hat{\sigma} \sum_{m=1}^{M} w_m^* \exp\left\{(a_m^*)^2\right\} \frac{\exp\left\{-\left(\sqrt{2}\hat{\sigma}_i a_m^* + \hat{\mu}_i\right)^2 / 2\sigma_\delta^2\right\}}{\sqrt{2\pi}\sigma_\delta} \\ & \prod_{t=1}^{n_i} F\left(y_{it}^O, X_{it}\beta + \sqrt{2}\hat{\sigma}_i a_m^* + \hat{\mu}_i\right)\} \end{aligned}$$

where $w_i = 1$ is a weight parameter for subject i.

Panel Logit Model

Consider an underlying latent variable model:

$$C_{it}^* = x_{it}\beta + v_{it}$$

where $i = 1, ..., N$ represents each individual over $t = 1, ..., T$ periods. The dependent variable C_{it}^* is a latent variable, v_{it} is a continuously distributed variable independent of x_{it}. Since C_{it}^* is a latent variable, we only observe the following

$$C_{it} = \begin{cases} 1 & C_{it}^* > 0 \\ 0 & \text{otherwise} \end{cases}$$

The Logit model is specified as follows: Assuming a normal distribution, $N(0, \sigma_v^2)$, for the random effects v_i,

$$Pr(C_{i1}, ..., C_{in_i}|x_{i1}, ..., x_{in_i}) = \int_{-\infty}^{\infty} \frac{e^{-v_i^2/2\sigma_v^2}}{\sqrt{2\pi\sigma_v}} \left\{ \prod_{t=1}^{n_i} F(C_{it}, x_{it}\beta + v_i \right\} dv_i$$

where

$$F(C, z) = \begin{cases} \frac{1}{1+exp(-z)} & \text{if } C \neq 0 \\ \frac{1}{1+exp(z)} & \text{otherwise} \end{cases}$$

The log likelihood, L is calculated using

$$L \approx \sum_{i=1}^{n} w_i \log \left[\frac{1}{\sqrt{\pi}} \sum_{m=1}^{M} w_m^* \prod_{t=1}^{n_i} F \left\{ C_{it}, x_{it}\beta + a_m^* \left(\frac{2\rho}{1-\rho} \right)^{1/2} \right\} \right]$$

where w_m^* denote the quadrature weights, a_m^* denote the quadrature abscissas, and $\rho = \sigma_v^2/(\sigma_v^2 + 1)$ denote the proportion of the total variance contributed by the panel-level variance component.

Appendix C: Tables

Table 10: Data Description

Variable Name	Variable Definition
Effort	Amount of contribution to the project
Fraud	False contribution to the project
ET_i	= 1 if treatment provides a simple equity contract
ST_i	= 1 if treatment provides a stock option contract
Lag Penalty	Penalty amount the subject received in the previous round
Lag Audit	= 1 if the subject was audited in the previous round
Lag Wealth	Cumulative amount of earnings from previous rounds
Risk Averse	= 1 if the subject shows risk aversion in the gamble exercise
Ability	Measures the number of questions answered correctly

Table 11: Description & Summary Statistics

Variable	Mean	Std. Dev.	Min	Max
Effort	82.09	29.84	10	120
Fraud	142.57	132.78	0	400
ET	0.51	0.50	0	1
ST	0.49	0.50	0	1
Risk Averse	0.60	0.49	0	1
Lag Penalty	45.50	154.91	0	920
Lag Audit	0.21	0.41	0	1
Lag Wealth	272.36	186.26	-540.5	655.15
Ability	3.17	1.94	0	5

Table 12: Data Description

Variable Name	Variable Definition
Compliance rate	Ratio of reported income to true income
PTR	= 1 if tax regime is progressive
RTR	= 1 if tax regime is regressive
BASE	= 1 if tax regime is proportional
MTR	Marginal tax rate
High Income	= 1 if income equals 100 lab dollars
Lag Penalty	Penalty amount the subject received in the previous round
Lag Audit	= 1 if the subject was audited in the previous round
Lag Wealth	Cumulative amount of earnings from previous rounds
Risk Averse	= 1 if the subject shows risk aversion in the gamble exercise
Age	Age of the subject
Male	= 1 of the subject is a male

Table 13: Summary Statistics of Tax Compliance Variables

Variable	Mean	SD	Min	Max
Compliance Rate	0.6769	0.351	0	1
MTR	0.253	0.063	0.18	0.39
PTR	0.451	0.494	0	1
RTR	0.318	0.46	0	1
BASE	0.262	0.439	0	1
Income (50)	0.476	0.499	0	1
Income (100)	0.523	0.499	0	1
Lag Penalty	3.900	7.748	0	65.6
Lag Audit	0.158	0.492	0	1
Lag Wealth	11003.1	393.79	0	1911.133
Age	22	3.619	18	39
Male	0.626	0.484	0	1

Appendix D: Figures

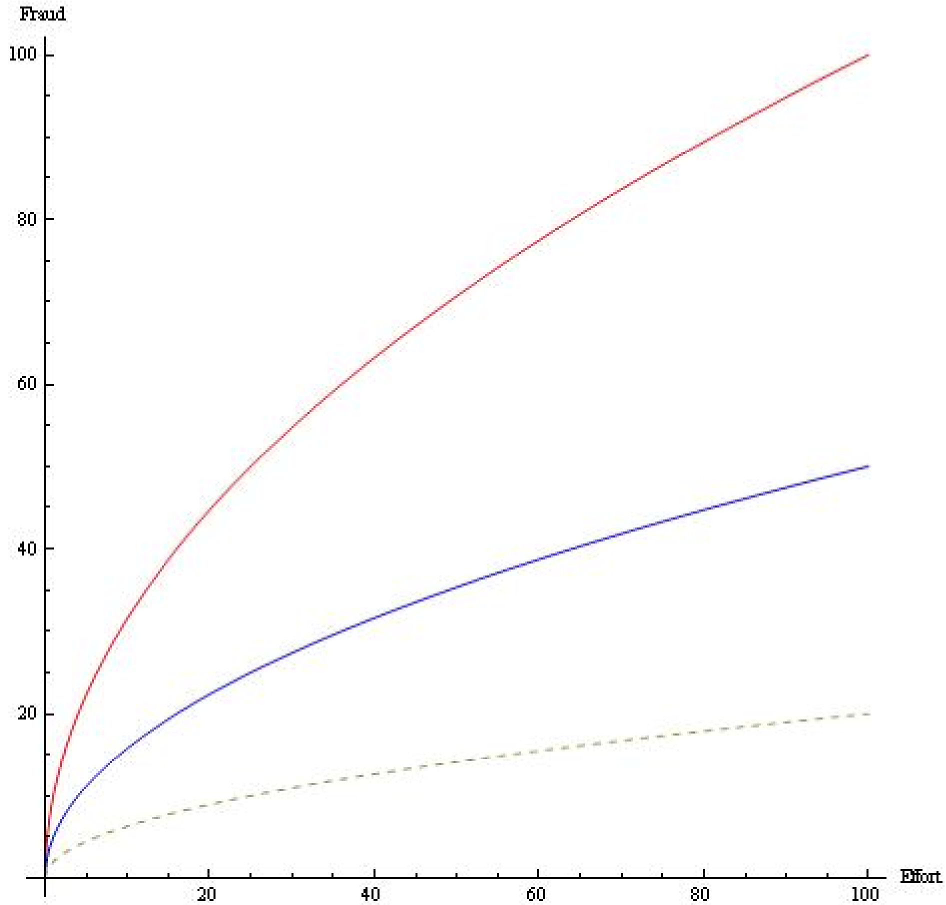


Figure 3: Note: Assuming the $v(e) = \sqrt{e}$, $\phi(e) = e$, $p = \{0.1, 0.2.0.5\}$, and $x(f) = f^2$, we obtain the following effort-fraud schedules.

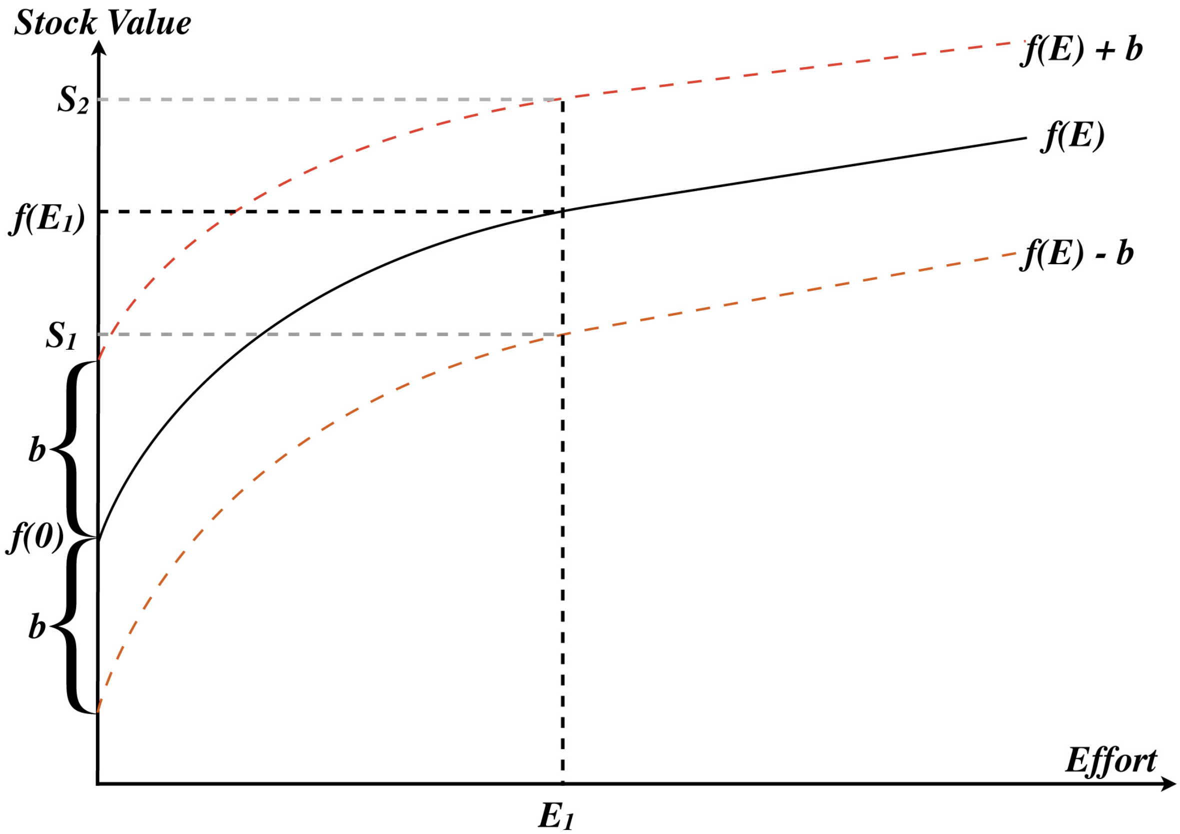


Figure 4: Note: As effort increases, the probability of good draws of true stock value increases. The restriction that $f(0) - b > 0$ ensures a realization of a positive value of the firm's stock in the event that the manager invests in no effort. This restriction also rules out the possibility that the manager can cash in from the stock compensation package by investing an effort level of zero

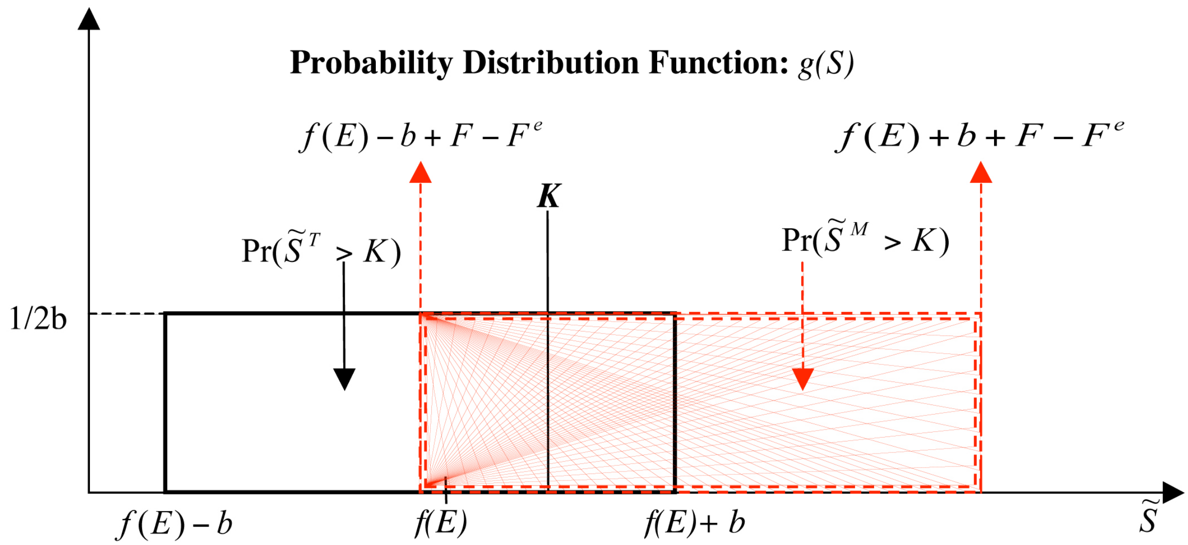


Figure 5: Note: As effort and fraud increases, the distribution shifts to the right, thus the propensity of the option to be above water increases. As expected fraud and strike price increases, the distribution shifts to the left, thus propensity of the option to be above water decreases

E/F	0	50	100	150	200	250	300	350	400
0	0%	0%	6%	13%	19%	25%	31%	38%	44%
10	11%	17%	24%	30%	36%	42%	49%	55%	61%
20	26%	32%	39%	45%	51%	57%	64%	70%	76%
30	40%	46%	52%	58%	65%	71%	77%	83%	90%
40	49%	55%	62%	68%	74%	80%	87%	93%	99%
50	54%	60%	67%	73%	79%	85%	92%	98%	100%
60	59%	65%	71%	77%	84%	90%	96%	100%	100%
70	62%	68%	74%	81%	87%	93%	99%	100%	100%
80	65%	71%	77%	84%	90%	96%	100%	100%	100%
90	68%	74%	80%	86%	93%	99%	100%	100%	100%
100	69%	76%	82%	88%	94%	100%	100%	100%	100%
110	71%	77%	83%	89%	96%	100%	100%	100%	100%
120	71%	77%	84%	90%	96%	100%	100%	100%	100%

TABLE 2 - OPTION (in the money with prob pi)

Payoff = alpha * b *pi^2 - E - Prob*x(Fraud)

		60		215	330				
Expected Sanction	0	8	23	42	65	91	120	151	184
Sanction	0	41	115	211	326	455	598	753	920
E/F	0	50	100	150	200	250	300	350	400
0	0.000	-8.144	-22.246	-39.170	-58.076	-78.480	-100.054	-122.559	-145.806
10	-7.479	-12.034	-21.766	-34.321	-48.858	-64.892	-82.097	-100.232	-119.110
20	-6.261	-7.072	-13.059	-21.868	-32.659	-44.949	-58.408	-72.797	-87.930
30	1.510	4.070	1.454	-3.984	-11.405	-20.323	-30.412	-41.431	-53.193
40	8.178	13.086	12.817	9.726	4.652	-1.920	-9.661	-18.333	-27.748
50	8.589	14.757	15.749	13.919	10.106	4.796	-1.685	-9.095	-17.590
60	8.723	16.015	18.131	17.424	14.735	10.548	5.191	-1.208	-9.612
70	6.545	14.648	17.575	17.680	15.802	12.427	7.881	1.775	-6.629
80	4.139	12.991	16.668	17.521	16.393	13.766	9.858	3.760	-4.644
90	1.416	10.955	15.318	16.858	16.416	14.476	10.845	4.747	-3.657
100	-3.797	6.179	10.979	12.957	12.951	11.441	7.836	1.738	-6.666
110	-10.302	-0.014	5.098	7.388	7.694	6.435	2.830	-3.268	-11.672
120	-18.602	-8.165	-2.903	-0.464	-0.007	-1.168	-4.773	-10.871	-19.275

Figure 6: Strategy Space of the Experiment

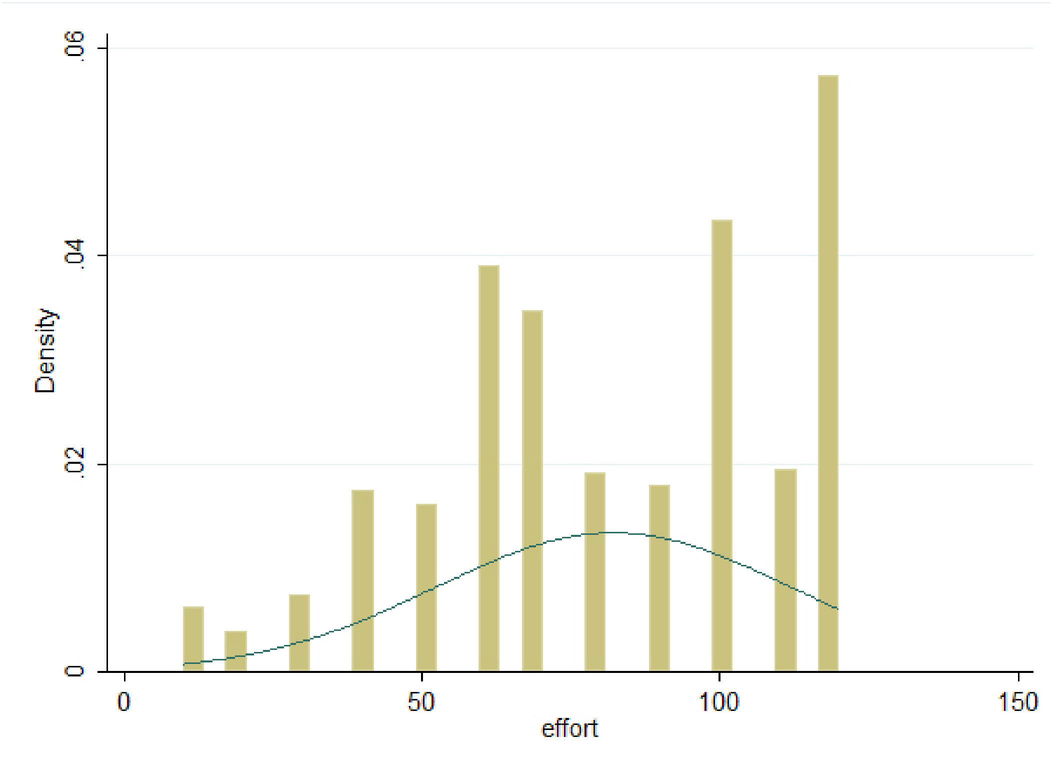


Figure 7: Distribution of Effort

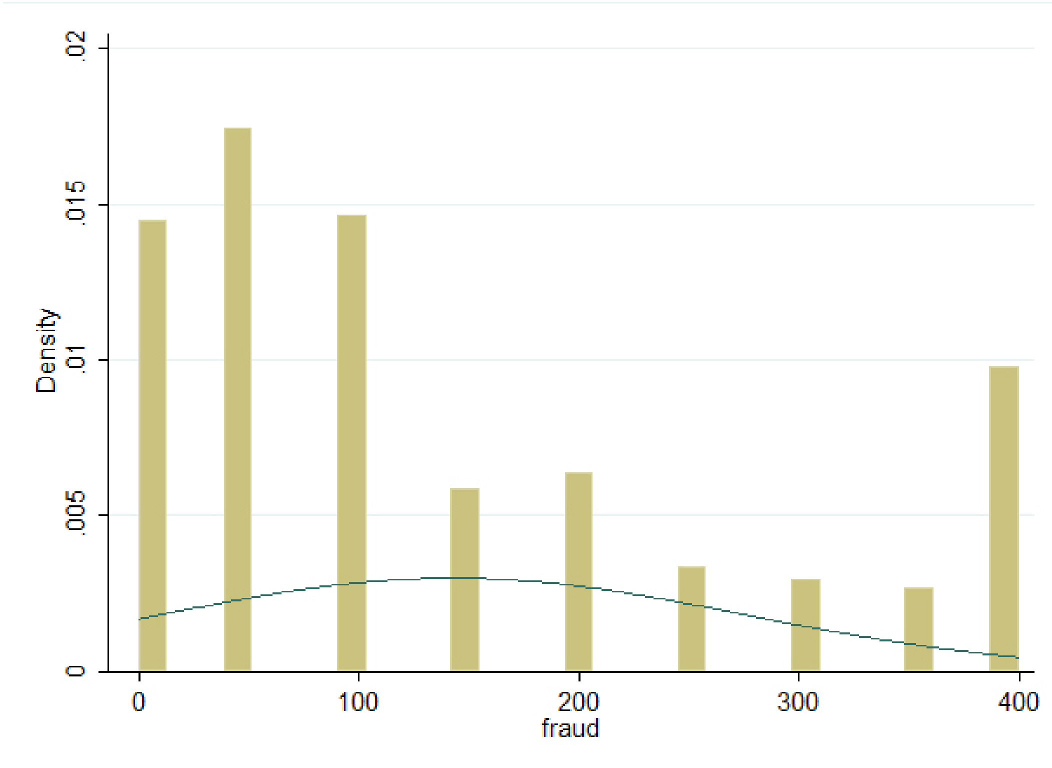


Figure 8: Distribution of Fraud

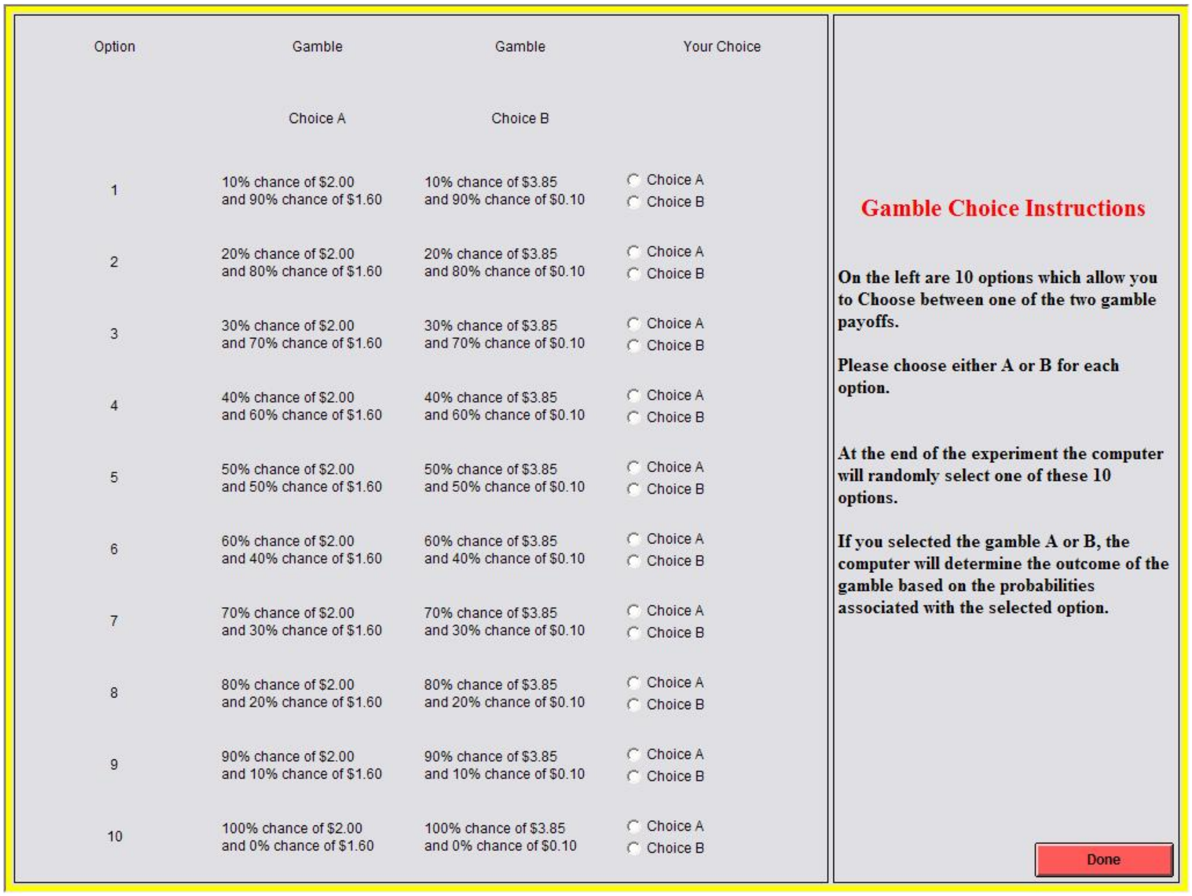


Figure 9: Screen Shot of Gamble-Guaranteed Game

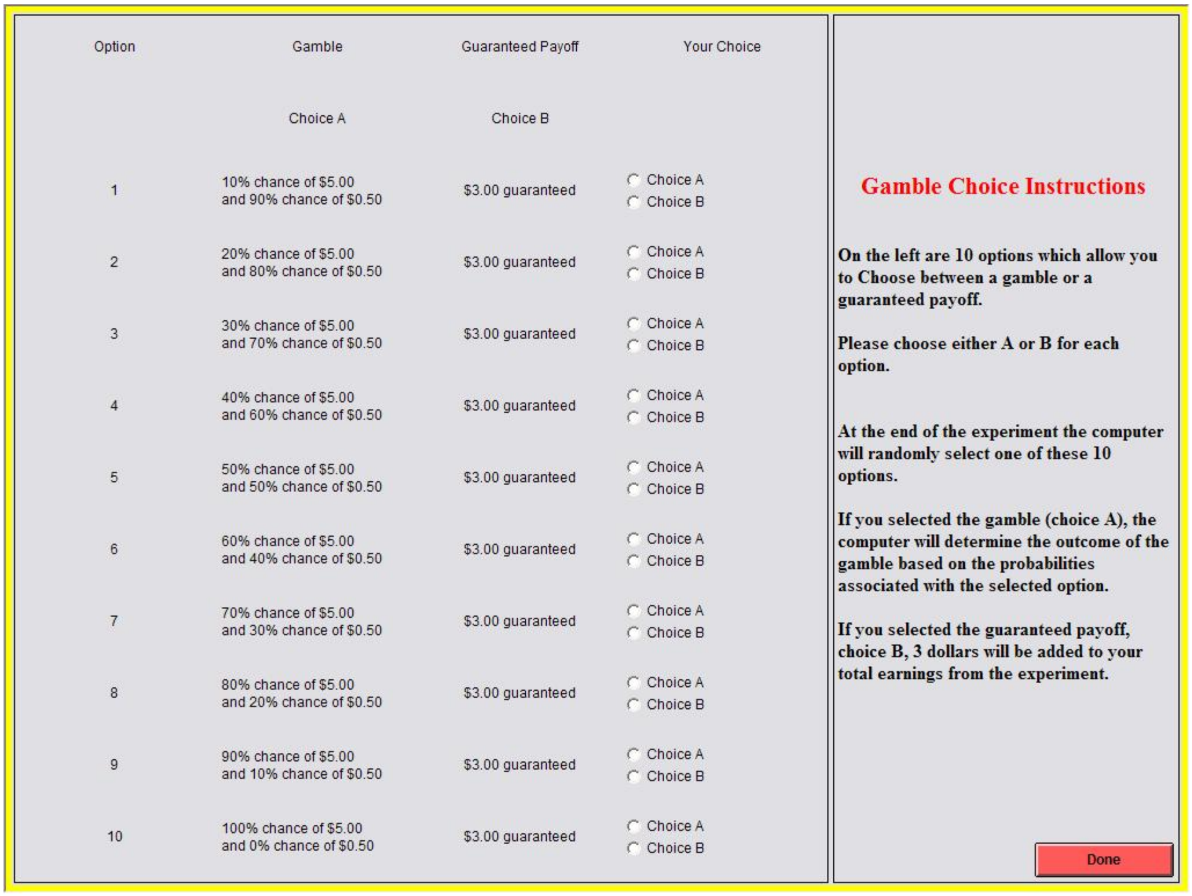


Figure 10: Screen Shot of Gamble Game

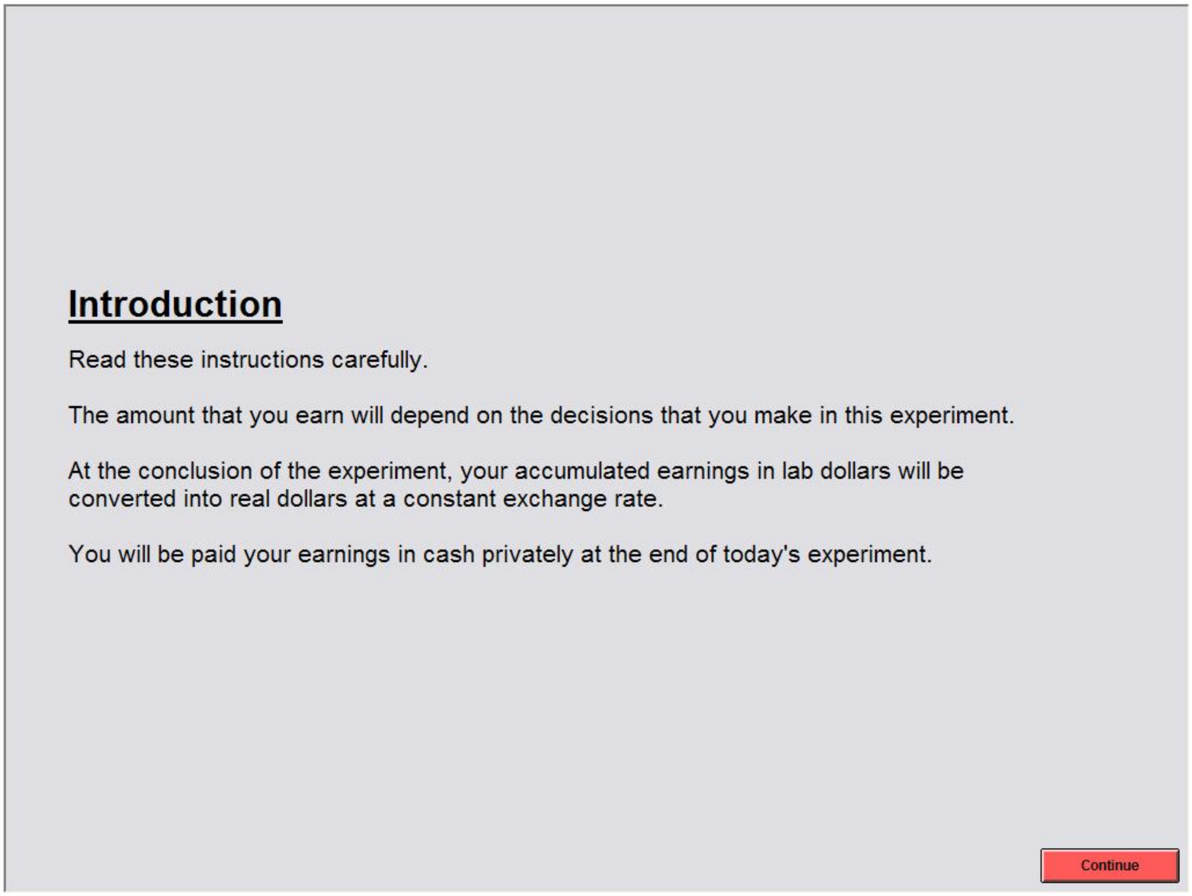


Figure 11: Introduction Screen Shot

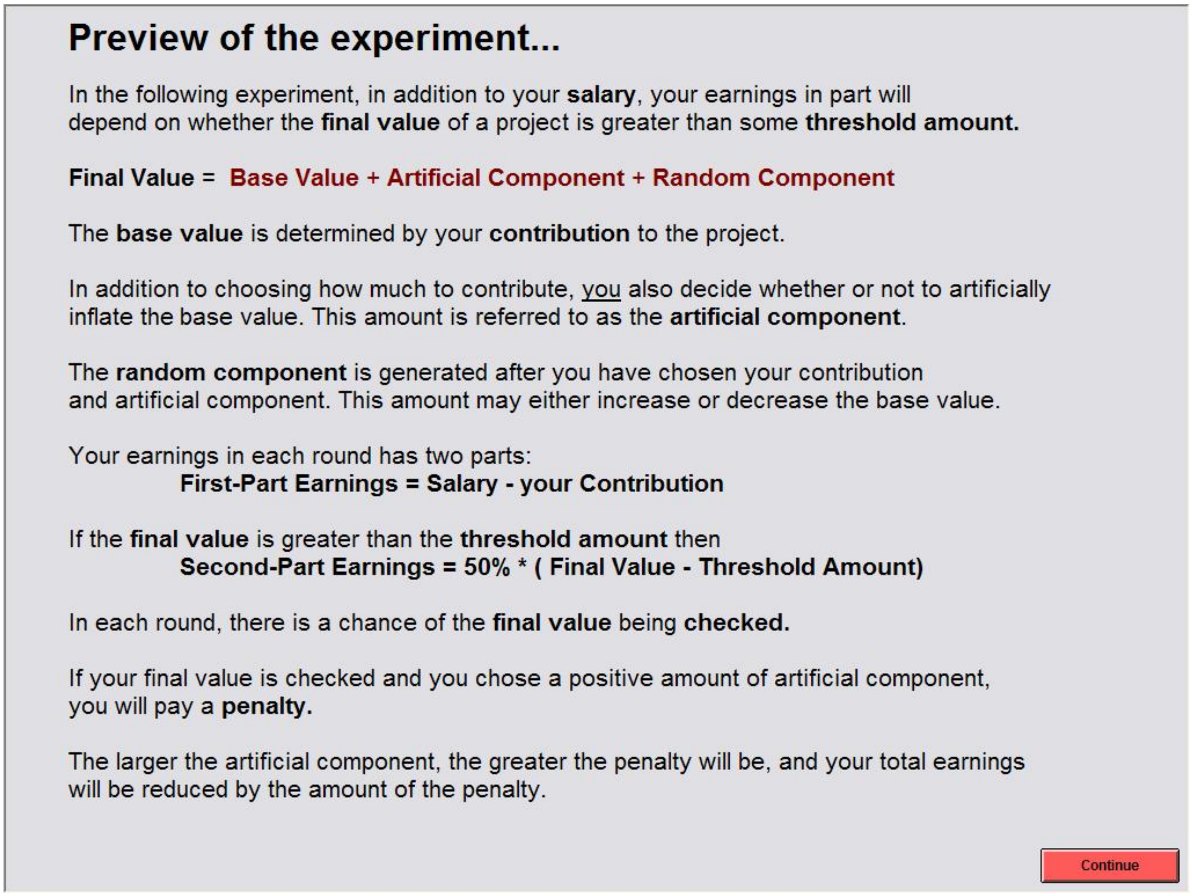


Figure 12: Preview of the Game Screen Shot

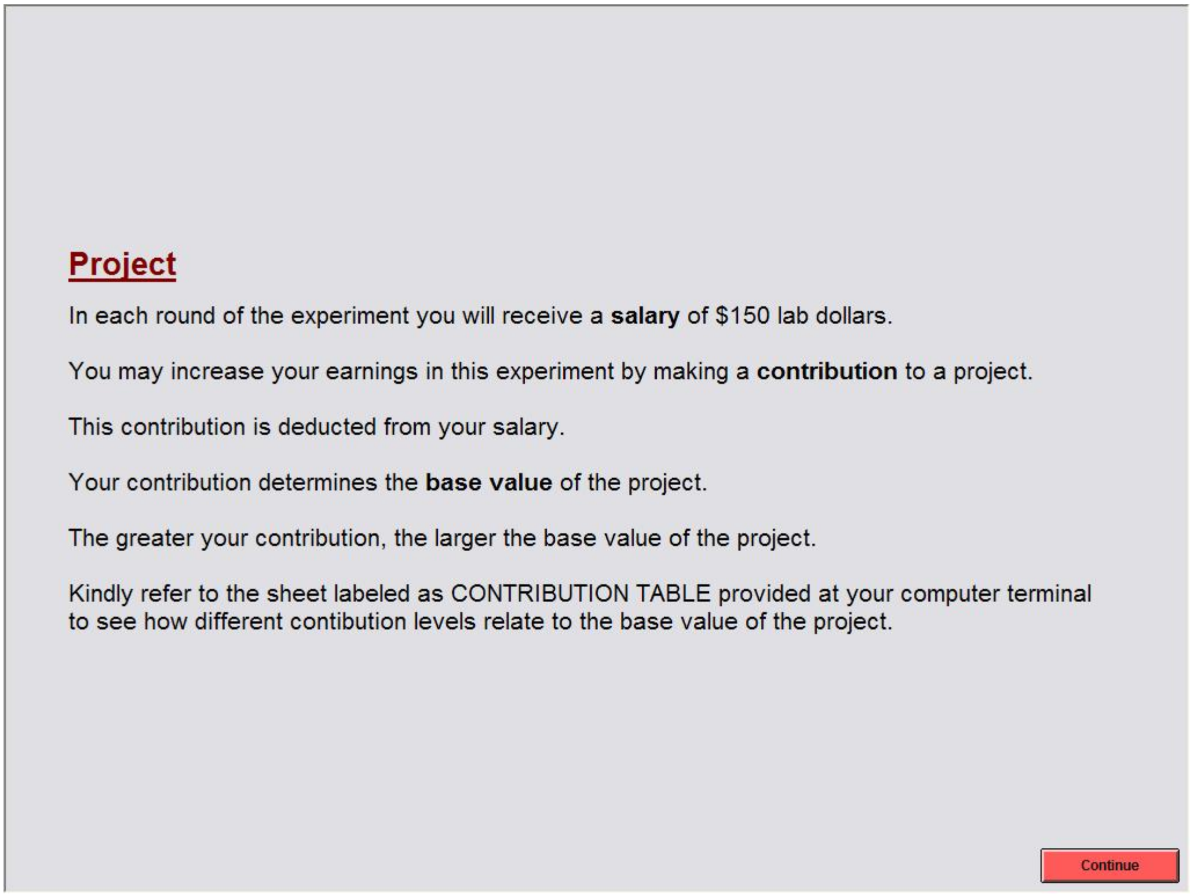


Figure 13: Project Description Screen Shot

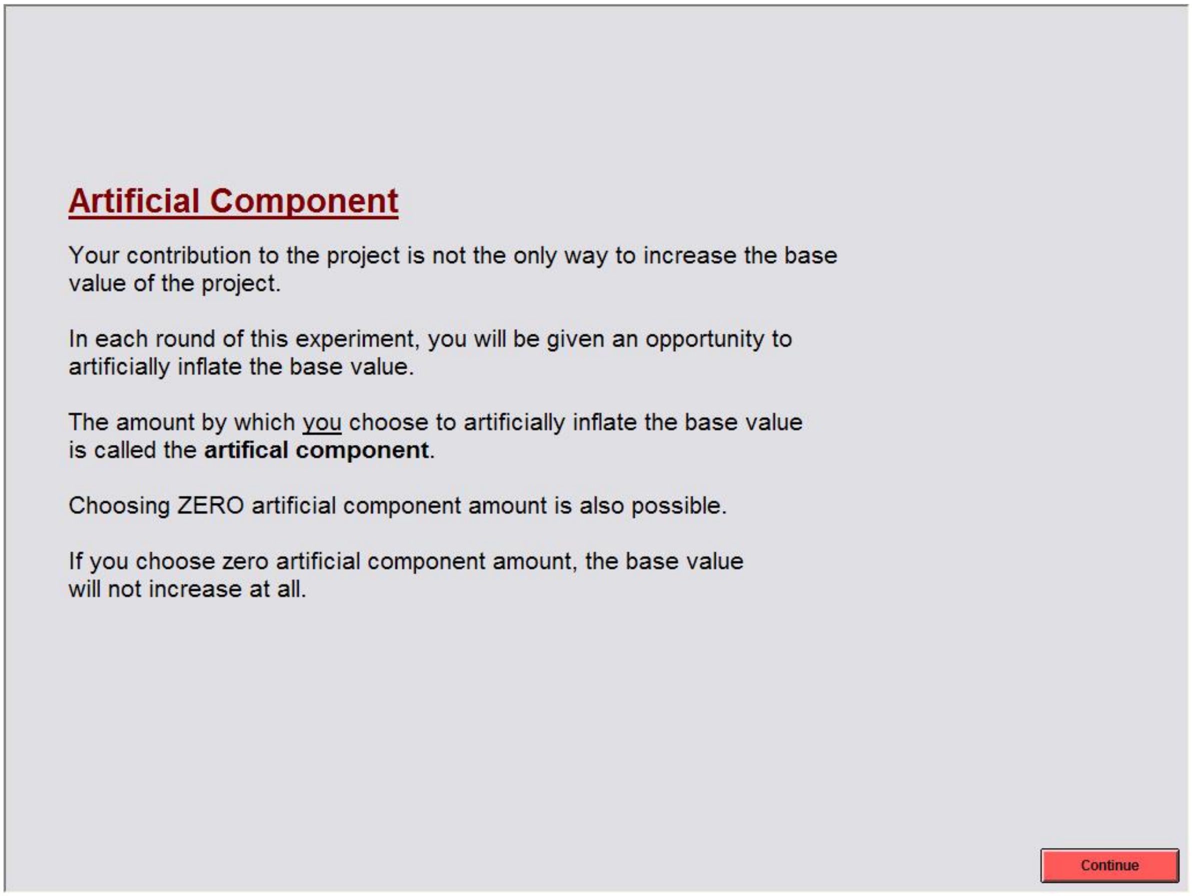


Figure 14: Artificial Component Description Screen Shot

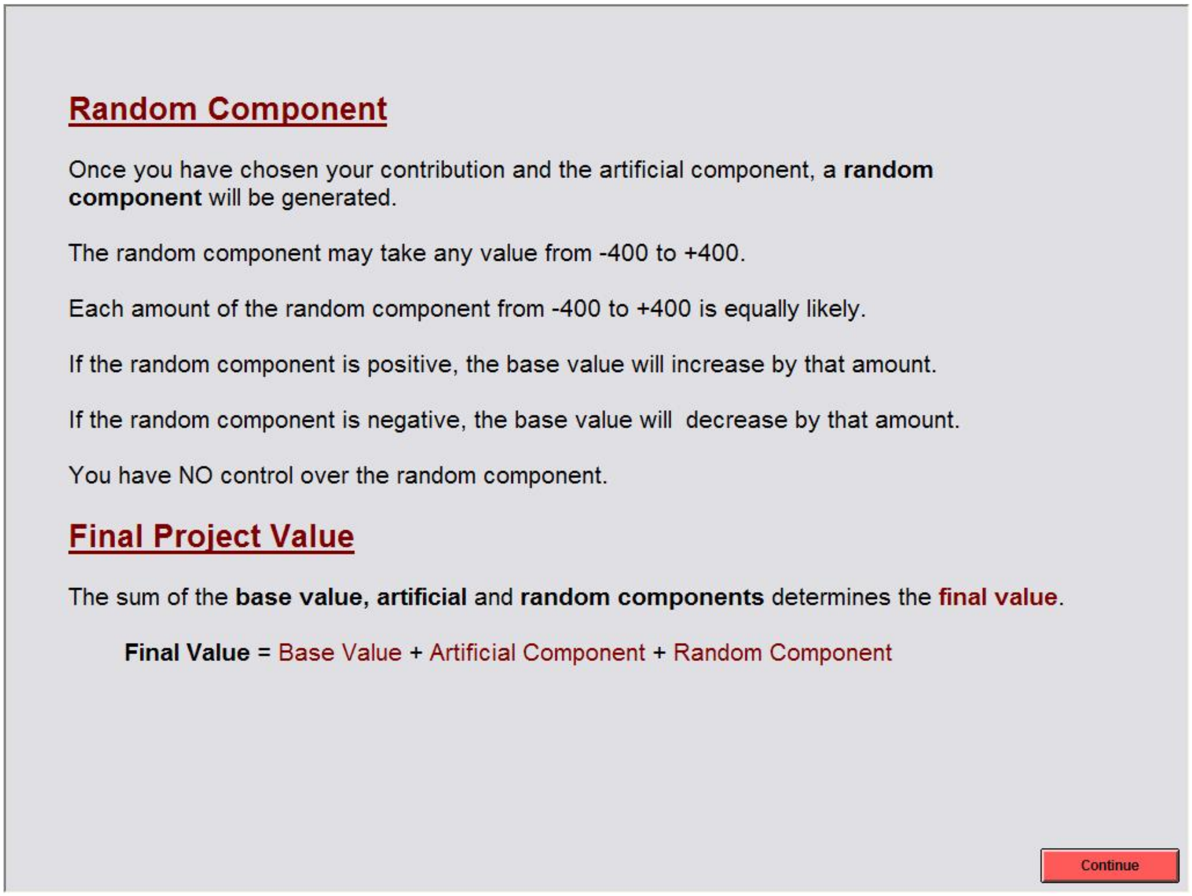


Figure 15: Random Amount Description Screen Shot

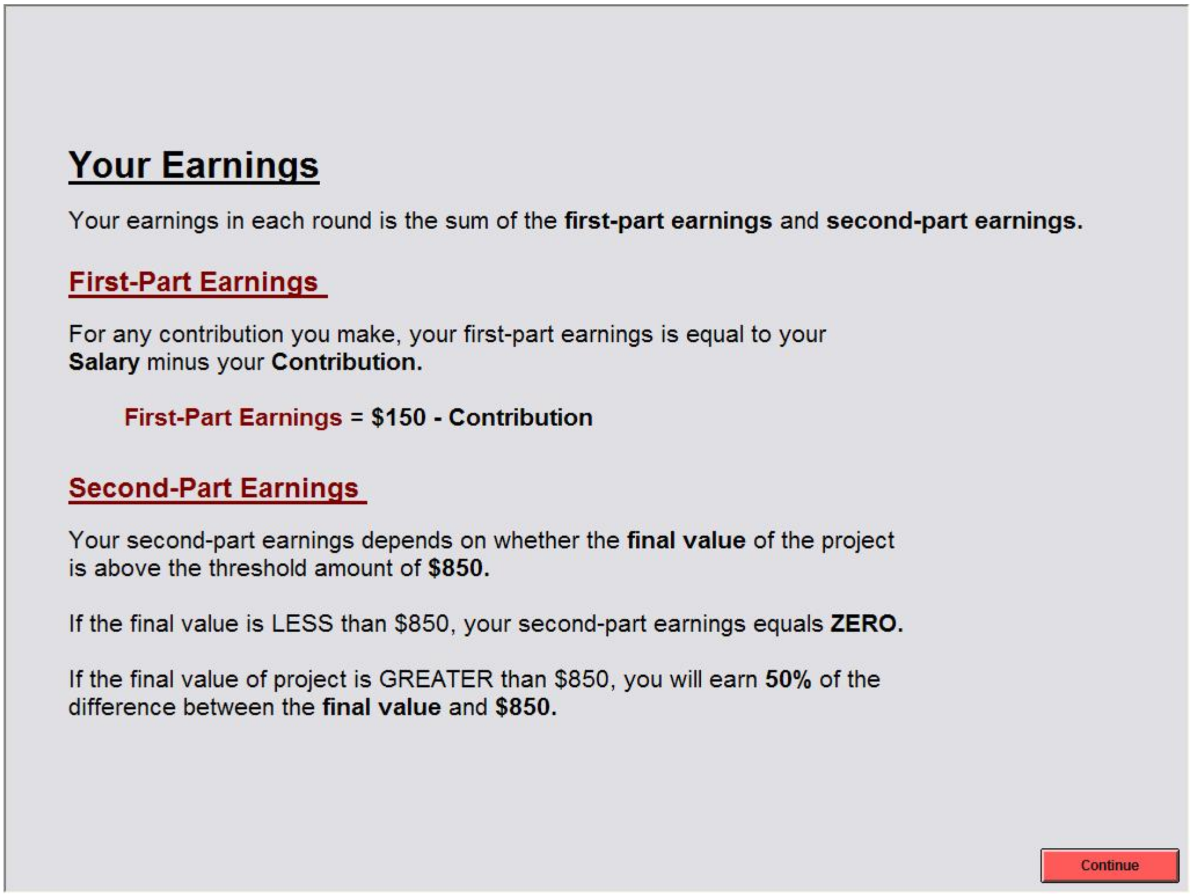


Figure 16: Earnings Description Screen Shot

Figure 17: Second-Part Earnings Description Screen Shot

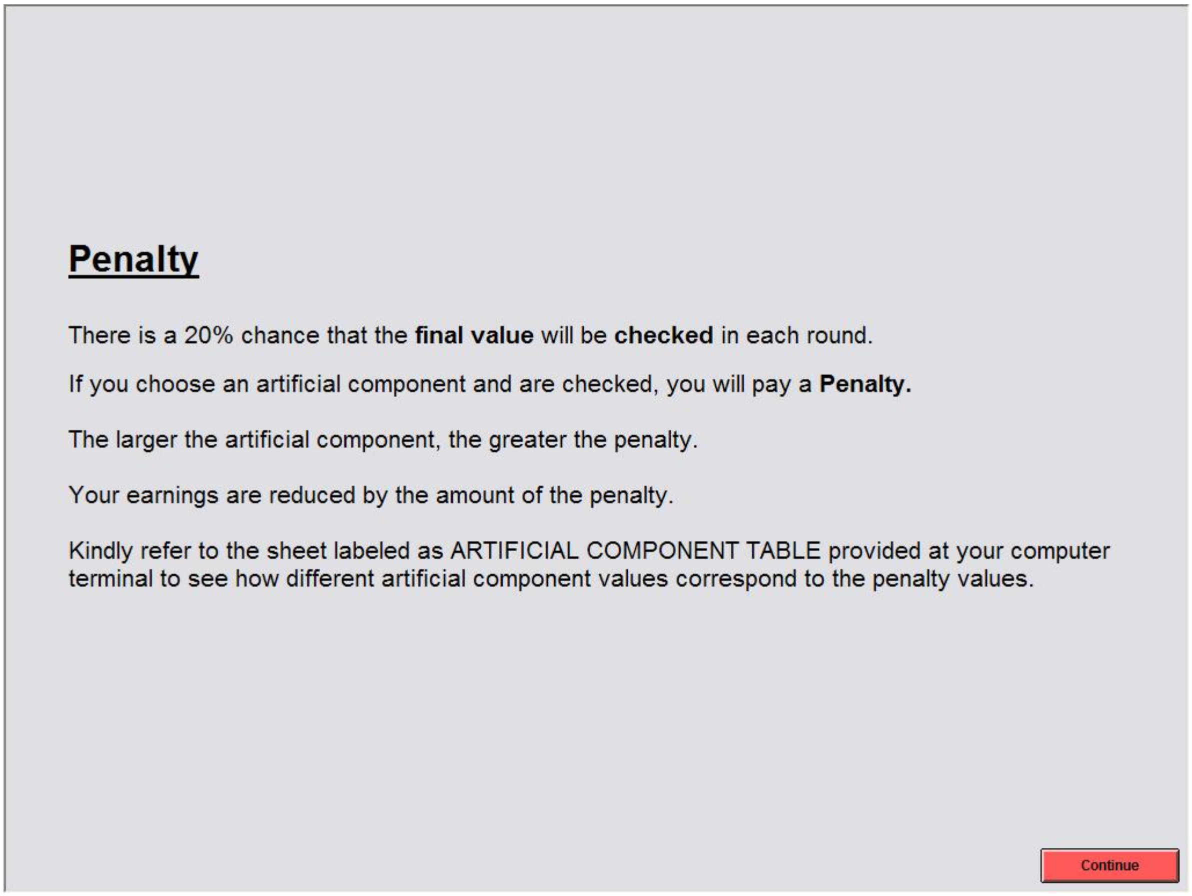


Figure 18: Penalty Description Screen Shot

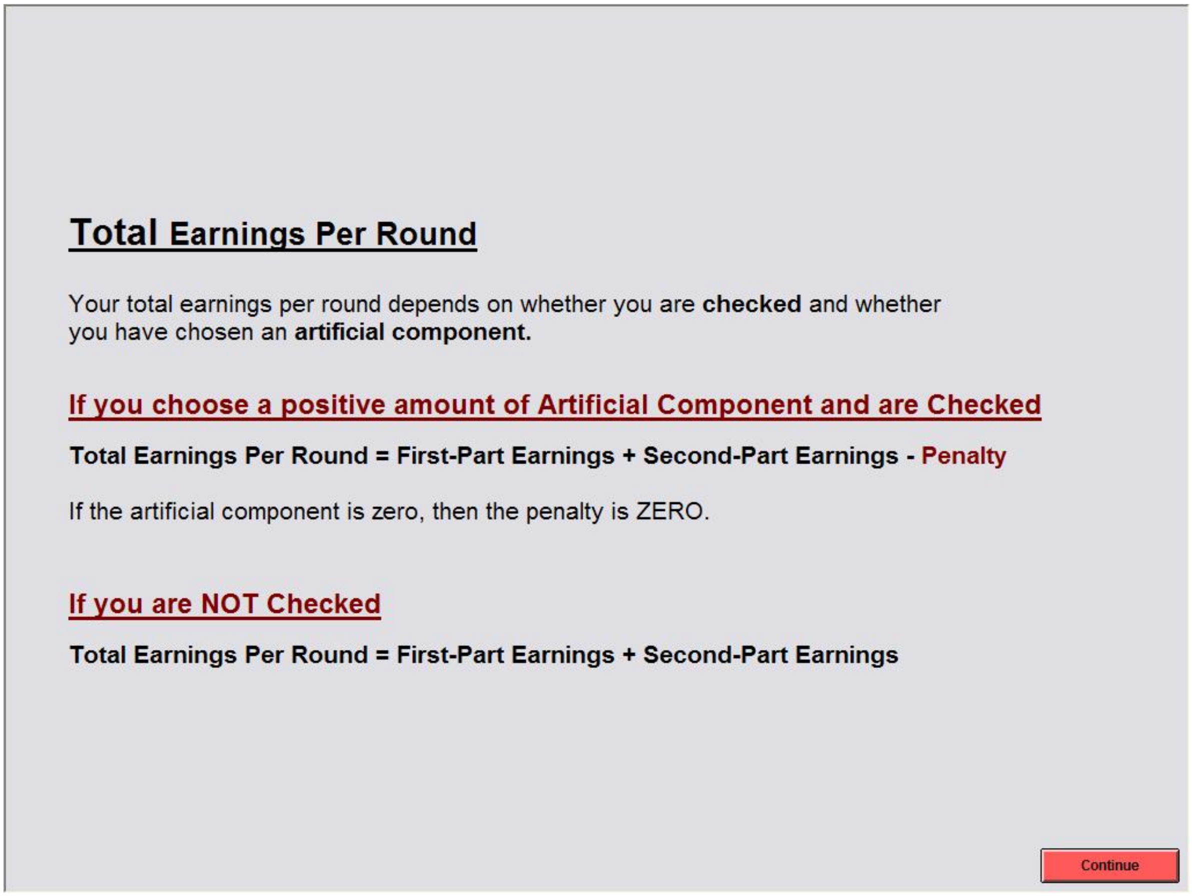


Figure 19: Total Earnings Description Screen Shot

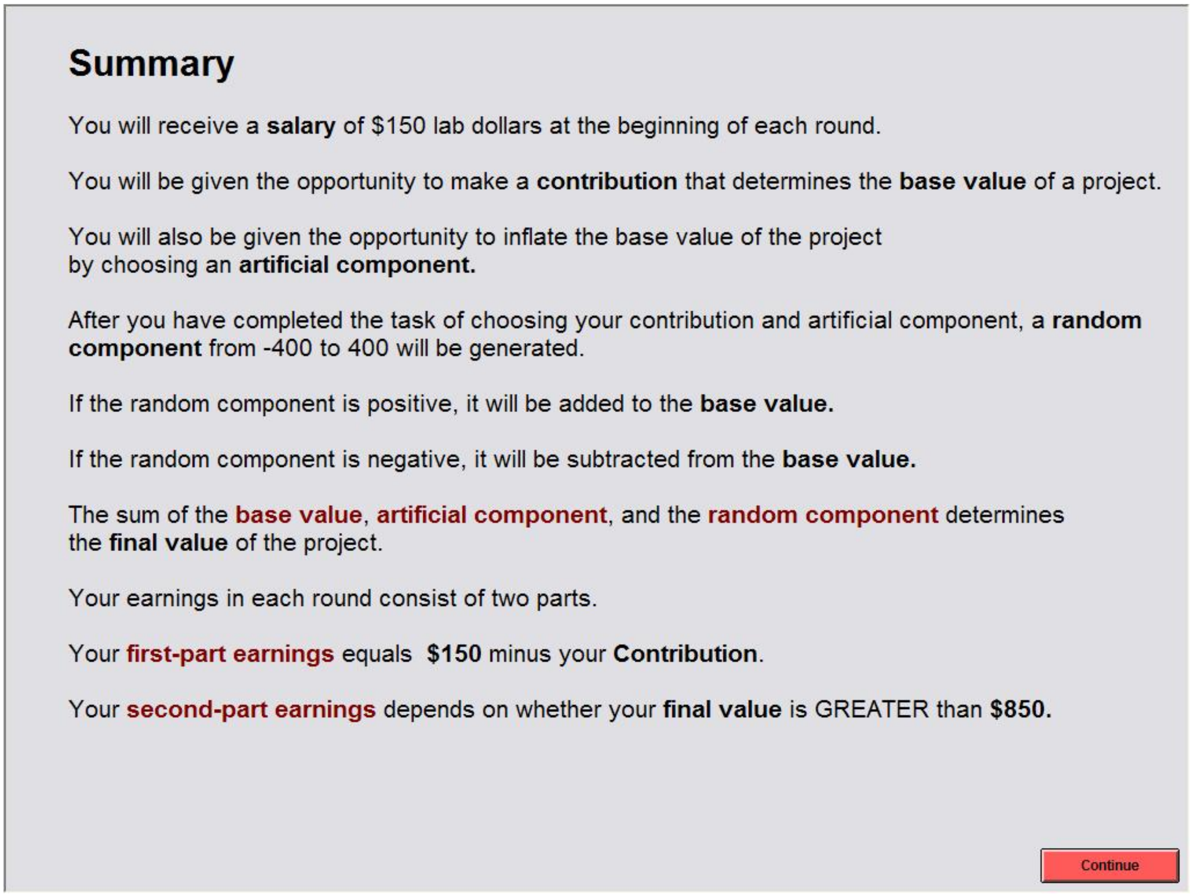


Figure 20: Summary A Description Screen Shot

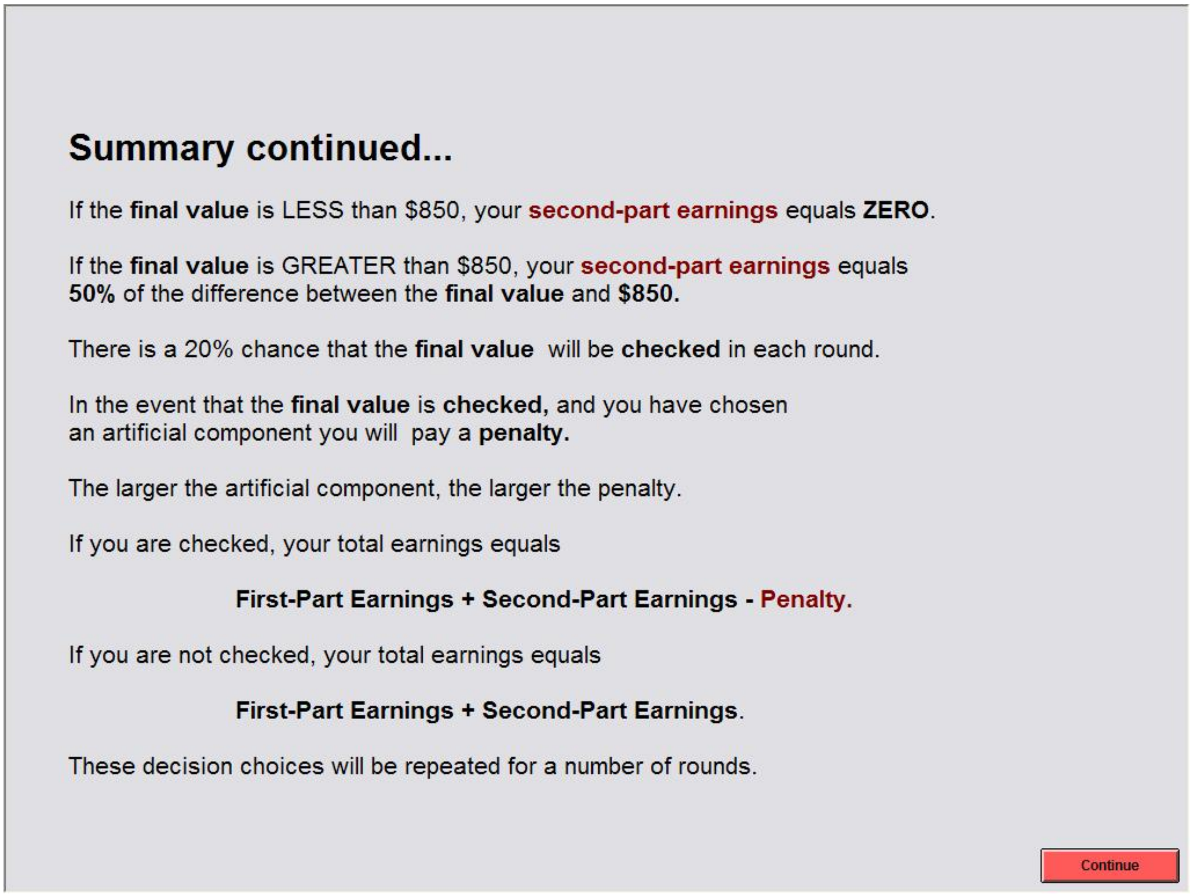


Figure 21: Summary B Description Screen Shot

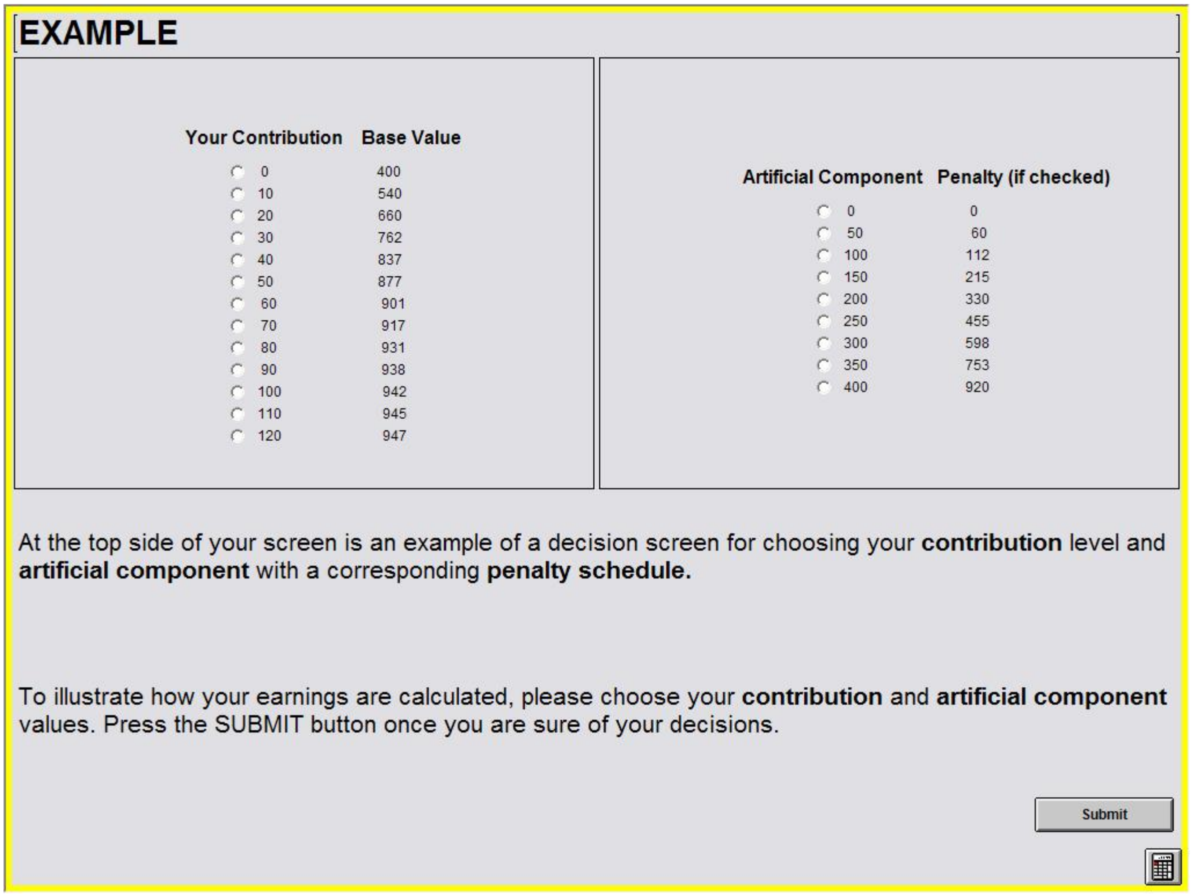


Figure 22: Calculation Example Screen Shot

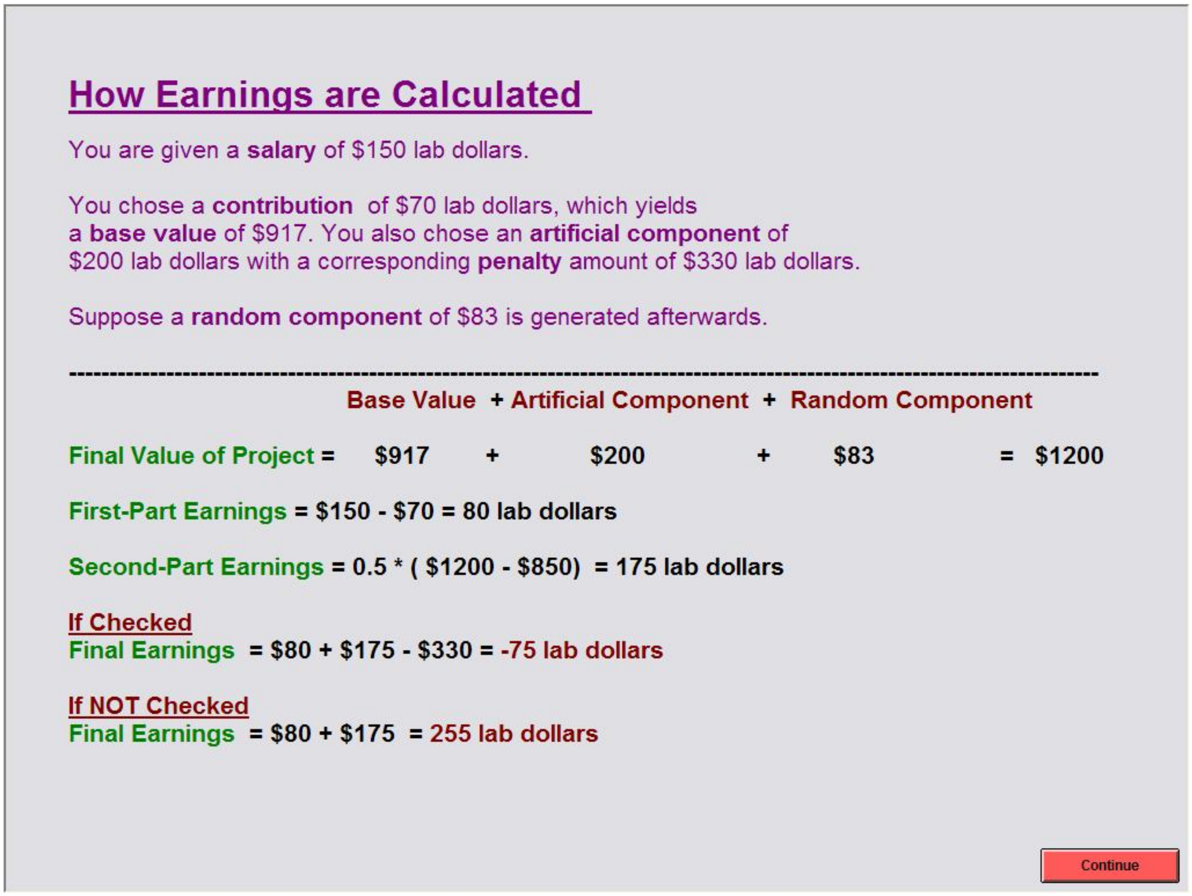


Figure 23: Calculation Example Results Screen Shot

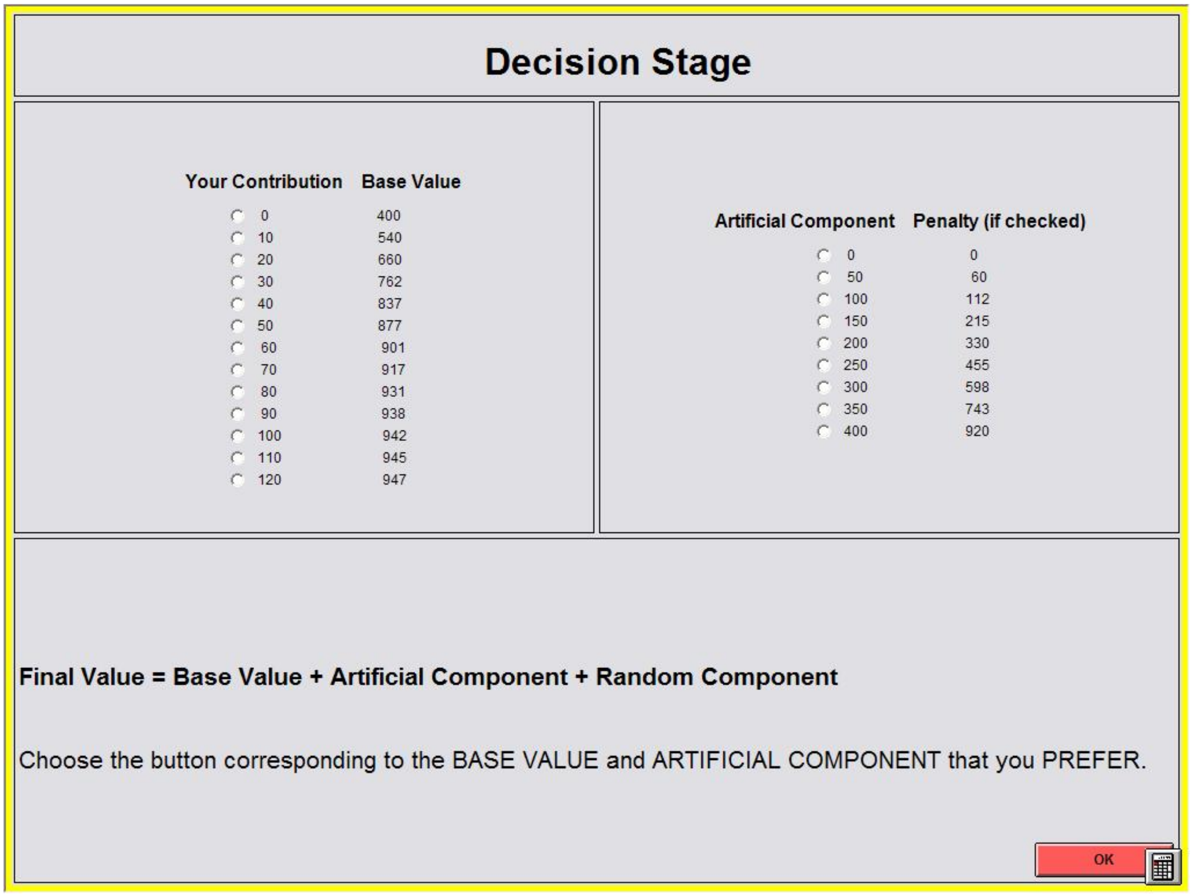


Figure 24: Decision Screen Shot

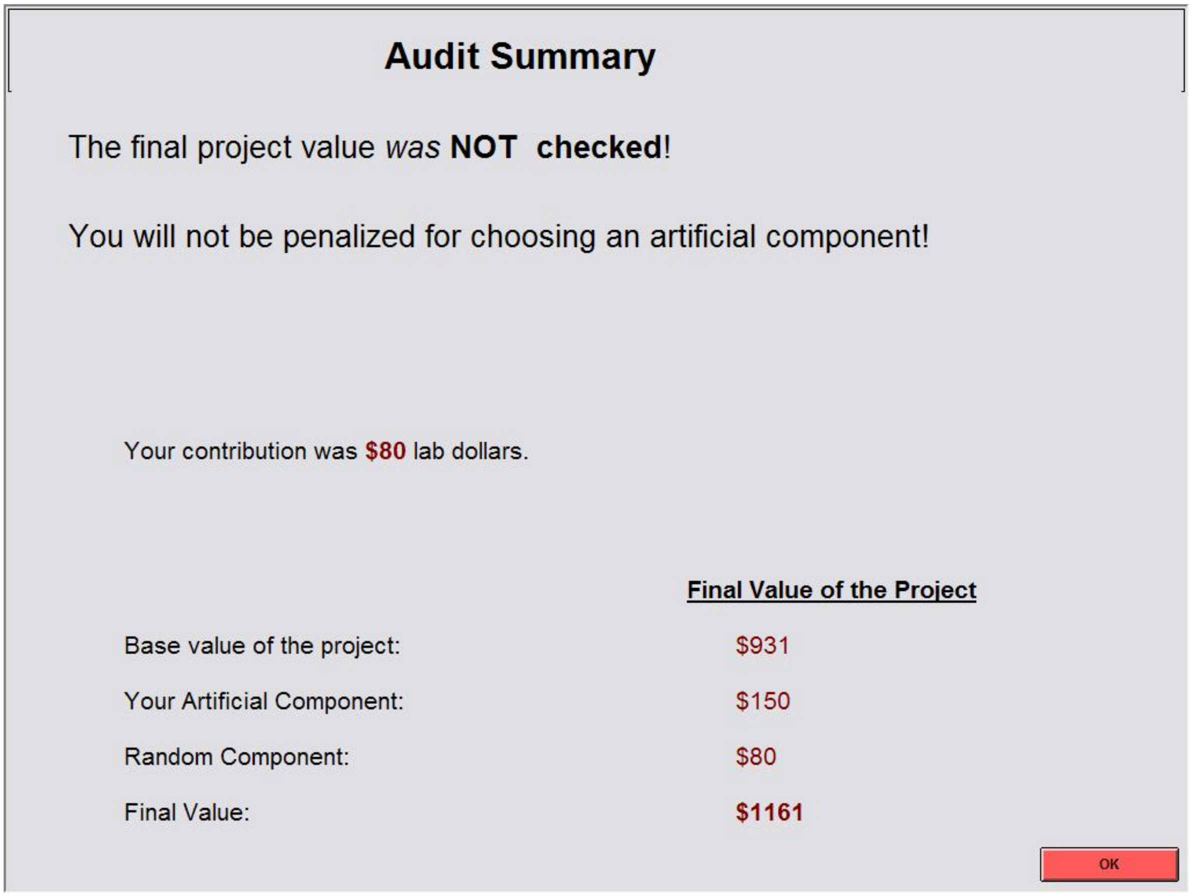


Figure 25: Audit Summary Screen Shot

Figure 26: Earnings Summary Screen Shot

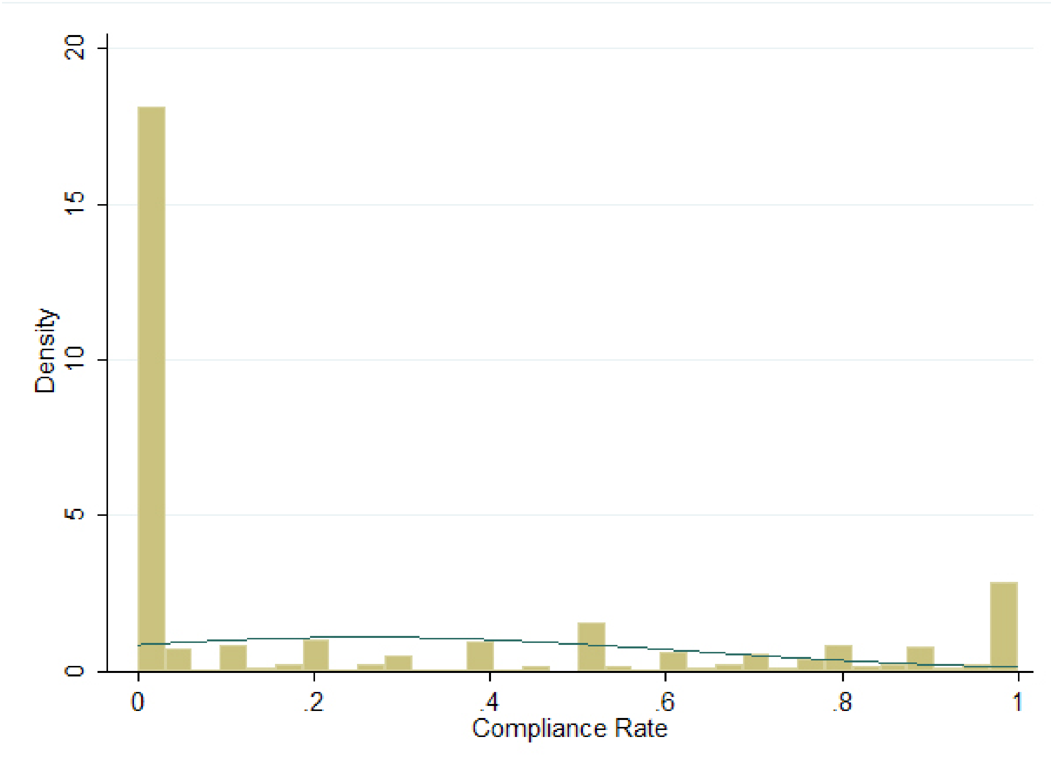


Figure 27: Full Sample - Distribution of Tax Compliance Rate

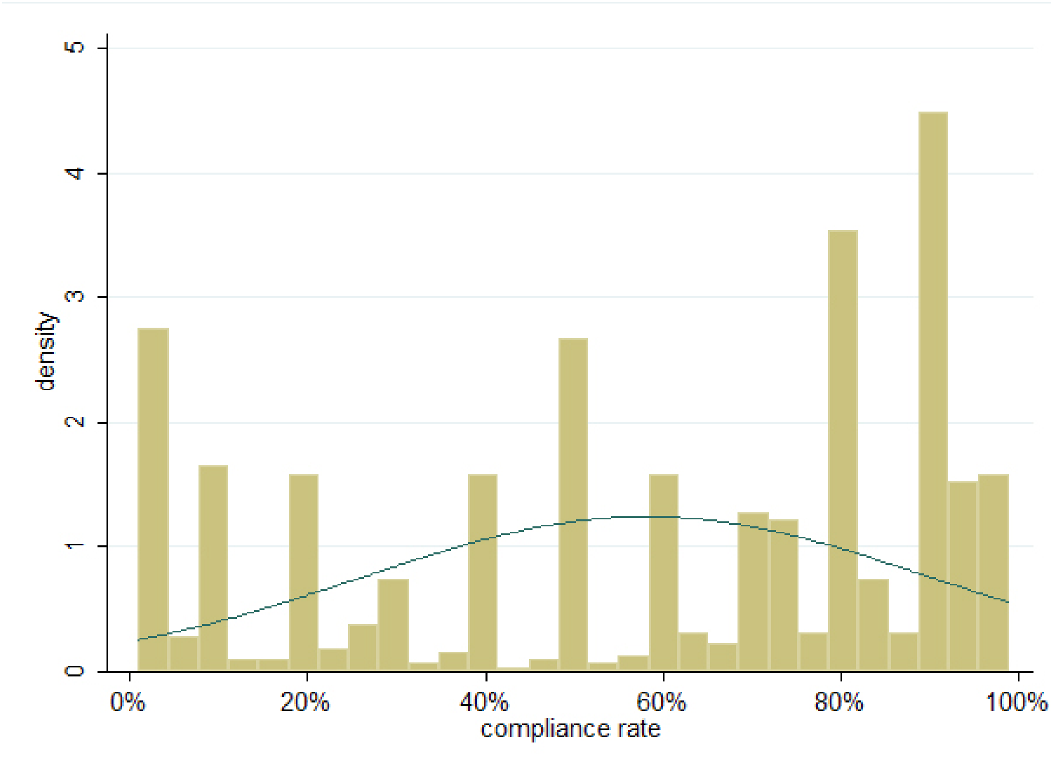


Figure 28: Noncensored Sample Distribution of Tax compliance Rate

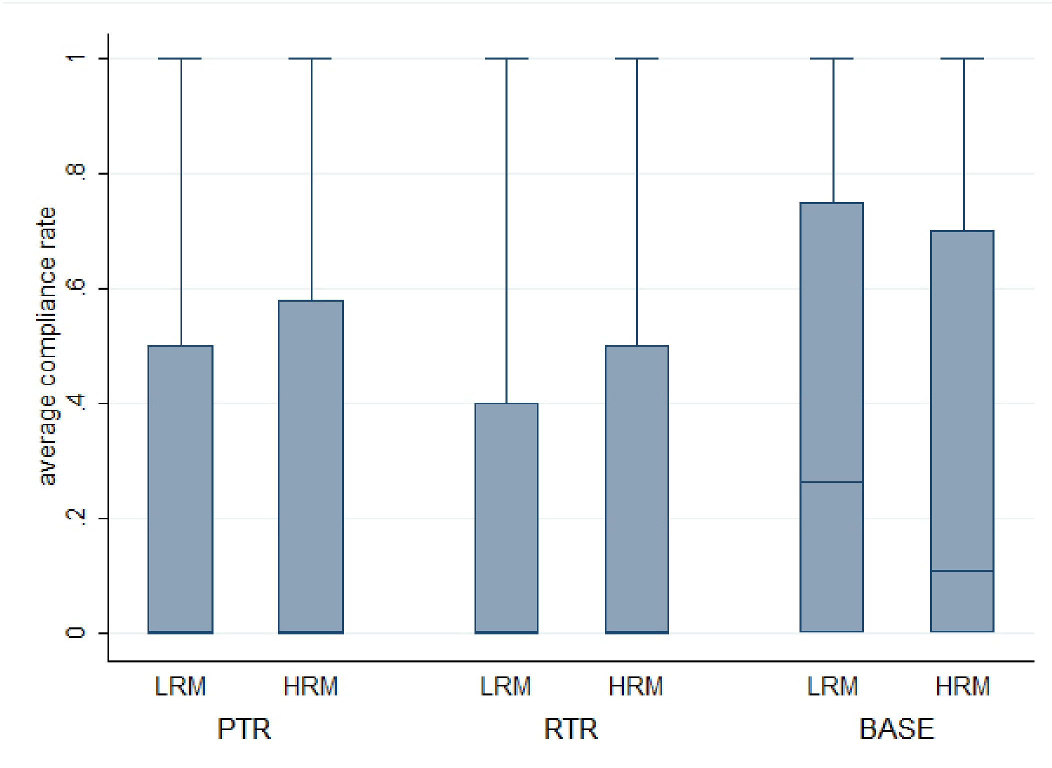


Figure 29: Full Sample - Average Tax Compliance Rate

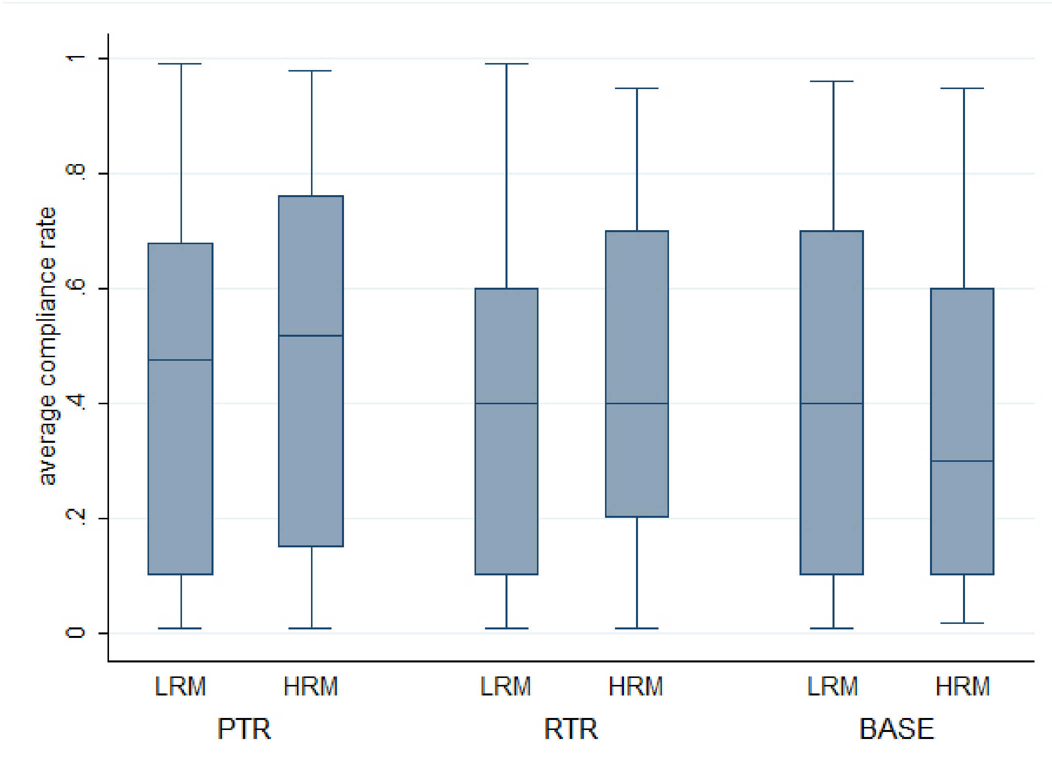


Figure 30: Sub-Sample - Average Tax Compliance Rate

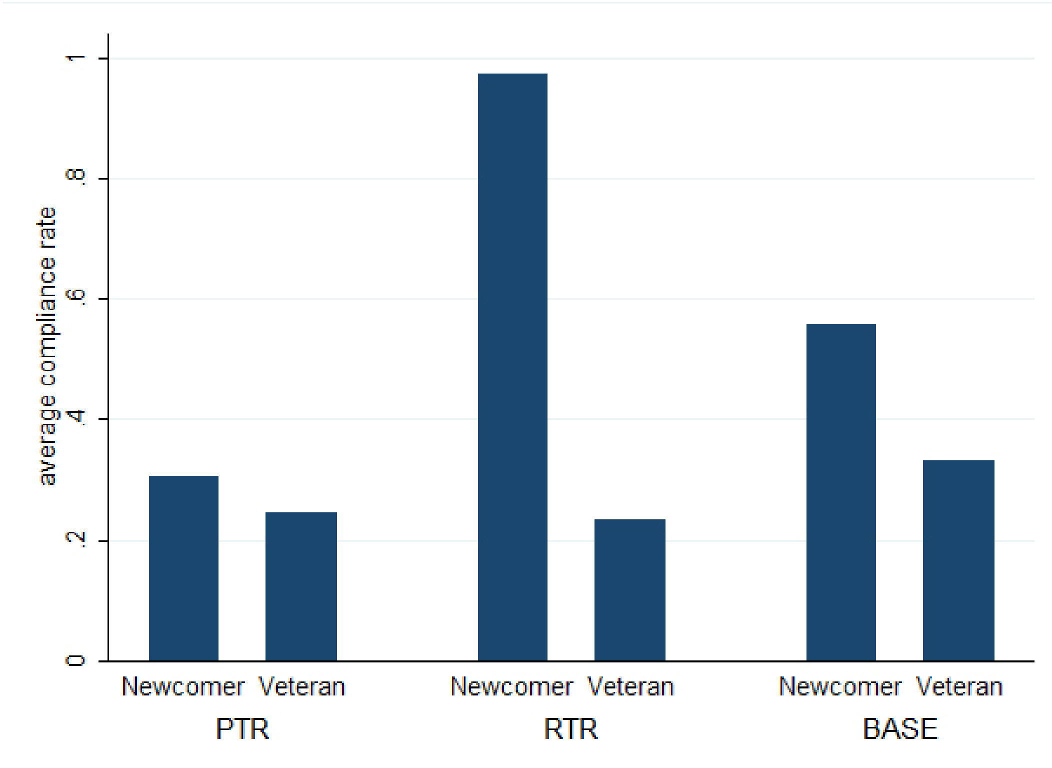


Figure 31: Average Compliance Rate Across Experienced and Inexperienced Participants

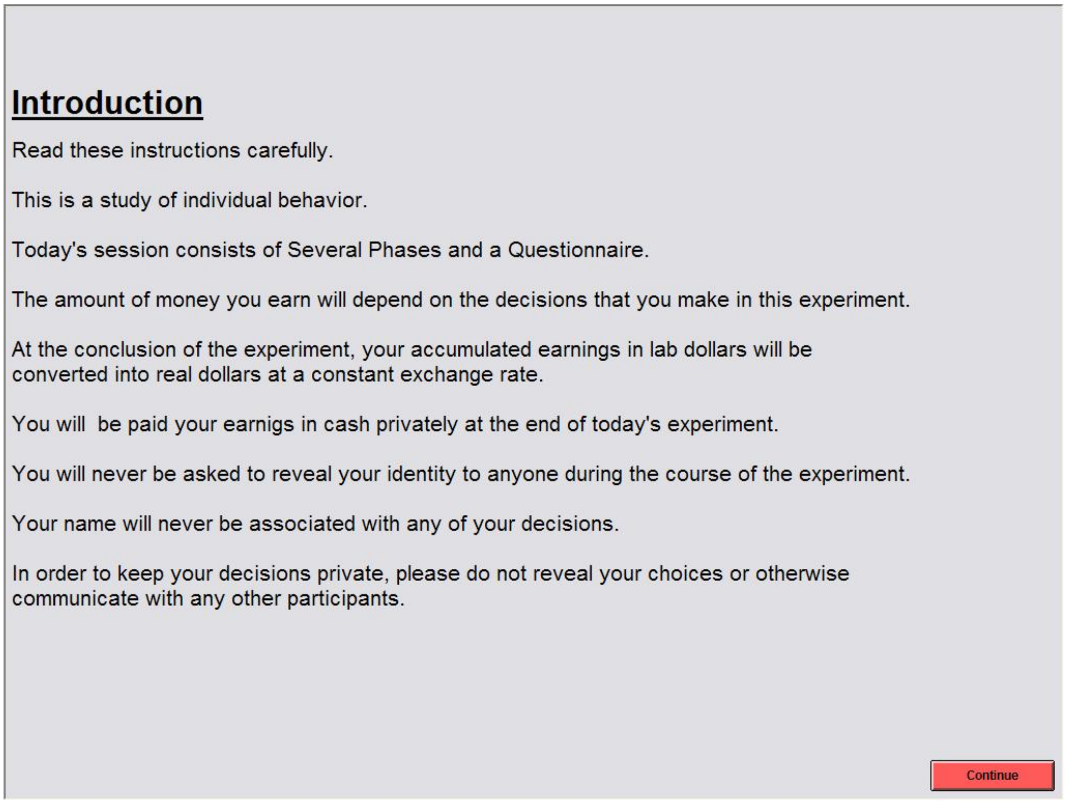


Figure 32: Introduction Screen Shot

Preview of the Experiment

Your earnings in this experiment depends on how much of your income you want to report.

There are several phases in this experiment.

In each phase, you will be randomly assigned a **TYPE** and a **TAX REGIME**.

If you are assigned a **LOW-TYPE**, you will receive an income of 50 lab dollars.

If you are assigned a **HIGH-TYPE**, you will receive an income of 100 lab dollars.

In each round of this experiment, you will be asked to report your income.

The amount that you choose to report will be used to calculate your **tax payment.**

The tax on each dollar reported for the LOW-TYPE is lower than the tax on
each dollar reported for the HIGH-TYPE.

Your payoff in each round equals **Unreported Income + After Tax Income.**

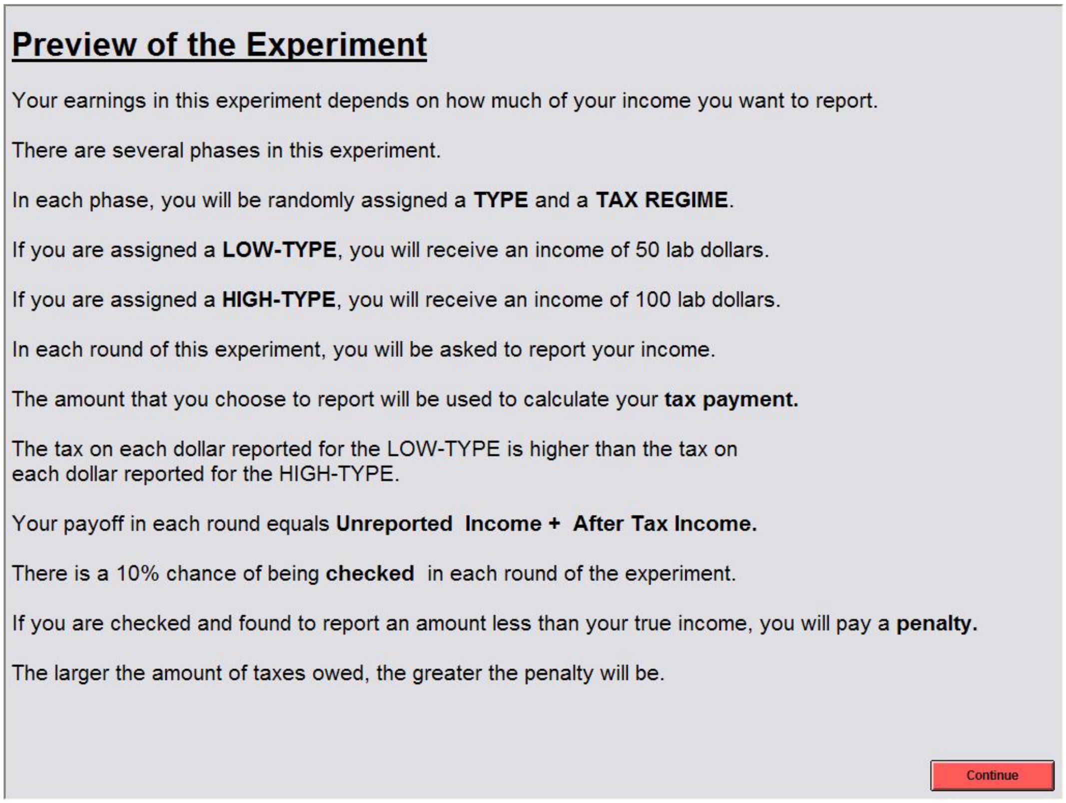
There is a 10% chance of being **checked** in each round of the experiment.

If you are checked and found to report an amount less than your true income, you will pay a **penalty.**

The larger the amount of taxes owed, the greater the penalty will be.

Continue

Figure 33: Preview of the PTR Experiment Instruction Screen Shot


Preview of the Experiment

Your earnings in this experiment depends on how much of your income you want to report.

There are several phases in this experiment.

In each phase, you will be randomly assigned a **TYPE** and a **TAX REGIME**.

If you are assigned a **LOW-TYPE**, you will receive an income of 50 lab dollars.

If you are assigned a **HIGH-TYPE**, you will receive an income of 100 lab dollars.

In each round of this experiment, you will be asked to report your income.

The amount that you choose to report will be used to calculate your **tax payment.**

The tax on each dollar reported for the LOW-TYPE is higher than the tax on each dollar reported for the HIGH-TYPE.

Your payoff in each round equals **Unreported Income + After Tax Income.**

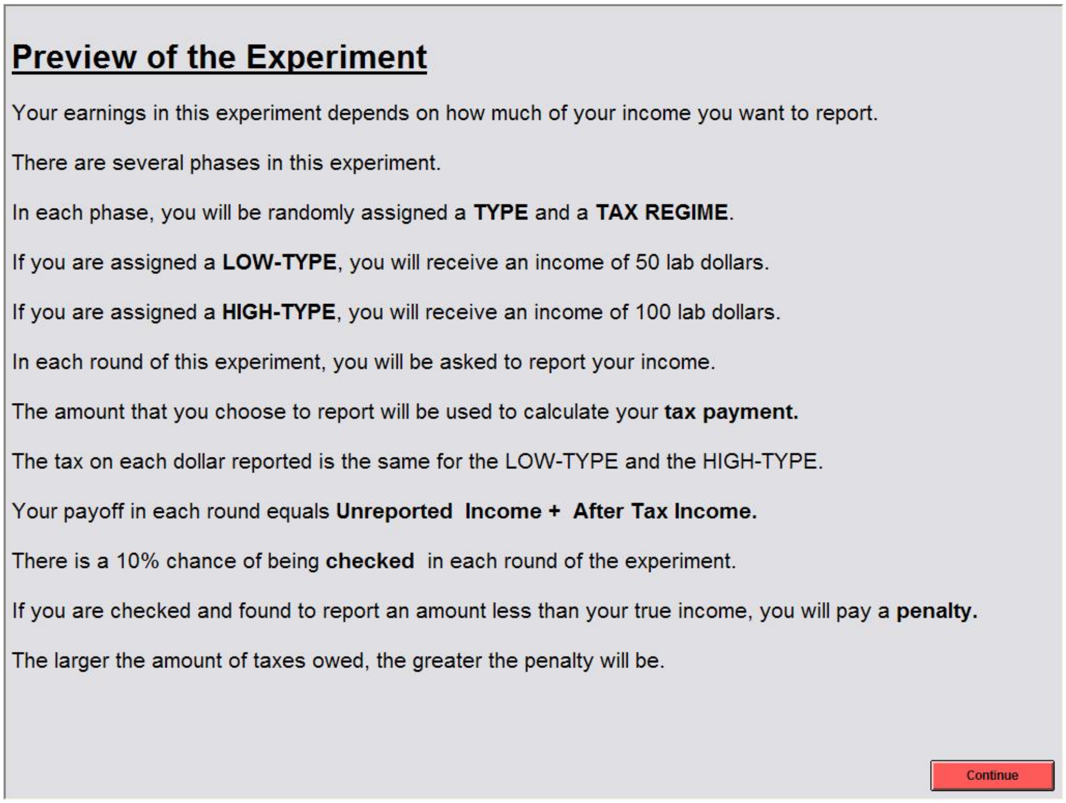
There is a 10% chance of being **checked** in each round of the experiment.

If you are checked and found to report an amount less than your true income, you will pay a **penalty.**

The larger the amount of taxes owed, the greater the penalty will be.

Continue

Figure 34: Preview of the RTR Experiment Instruction Screen Shot



Preview of the Experiment

Your earnings in this experiment depends on how much of your income you want to report.

There are several phases in this experiment.

In each phase, you will be randomly assigned a **TYPE** and a **TAX REGIME**.

If you are assigned a **LOW-TYPE**, you will receive an income of 50 lab dollars.

If you are assigned a **HIGH-TYPE**, you will receive an income of 100 lab dollars.

In each round of this experiment, you will be asked to report your income.

The amount that you choose to report will be used to calculate your **tax payment.**

The tax on each dollar reported is the same for the LOW-TYPE and the HIGH-TYPE.

Your payoff in each round equals **Unreported Income + After Tax Income.**

There is a 10% chance of being **checked** in each round of the experiment.

If you are checked and found to report an amount less than your true income, you will pay a **penalty.**

The larger the amount of taxes owed, the greater the penalty will be.

Continue

Figure 35: Preview of the BASE Experiment Instruction Screen Shot

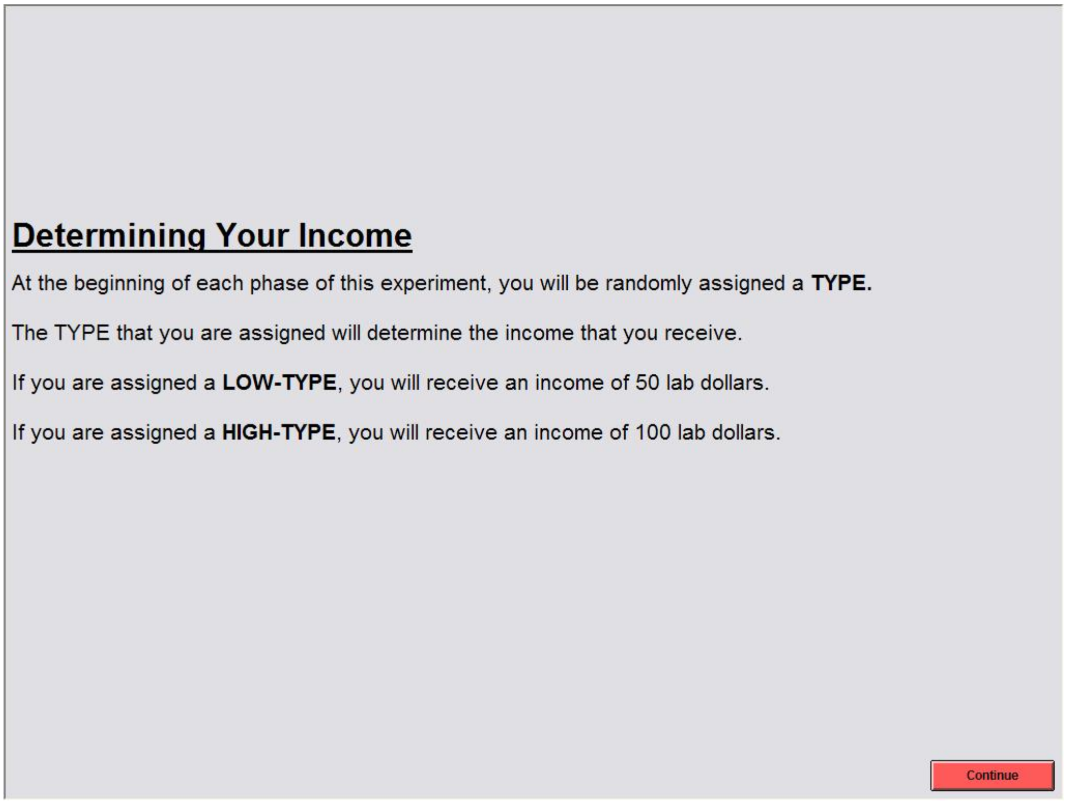


Figure 36: Determining Income Screen Shot

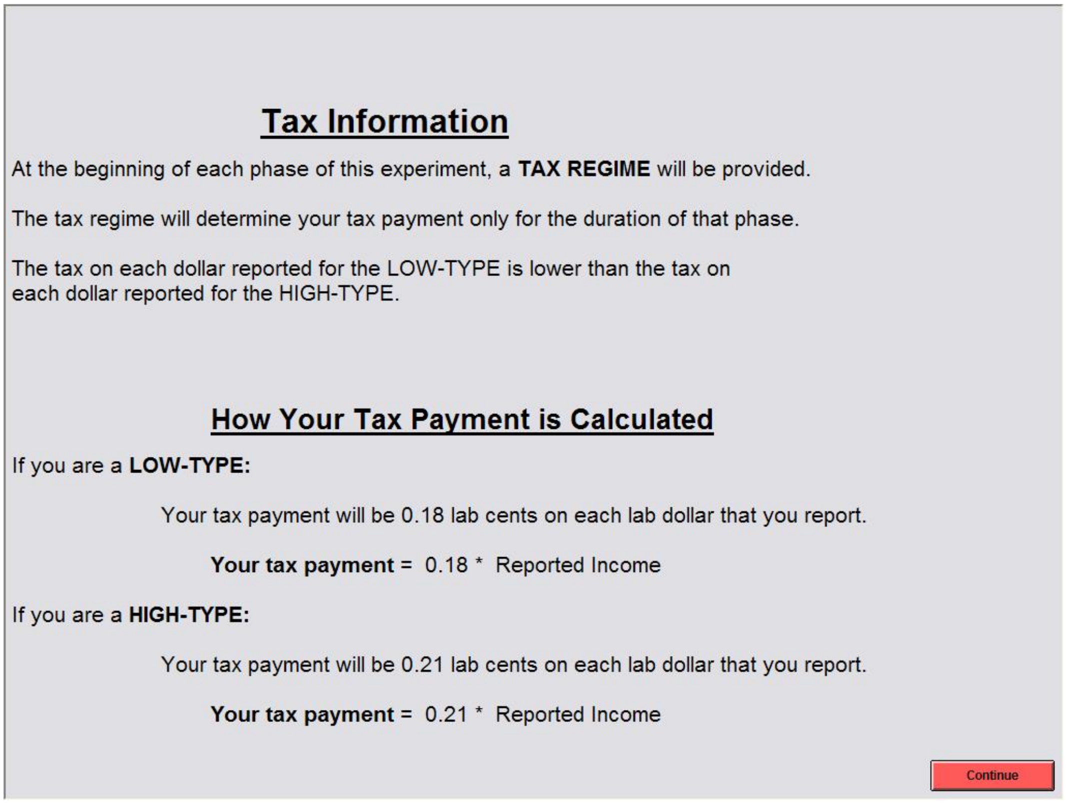


Figure 37: Tax Information Screen Shot

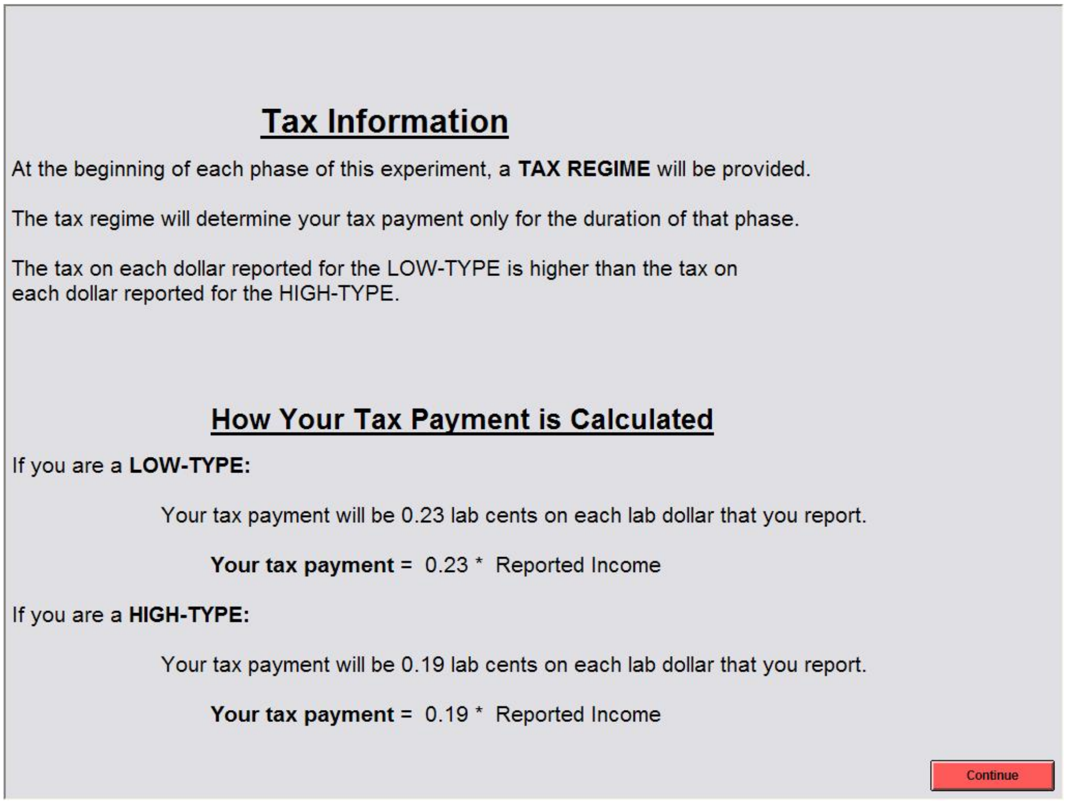


Figure 38: Tax Information Screen Shot

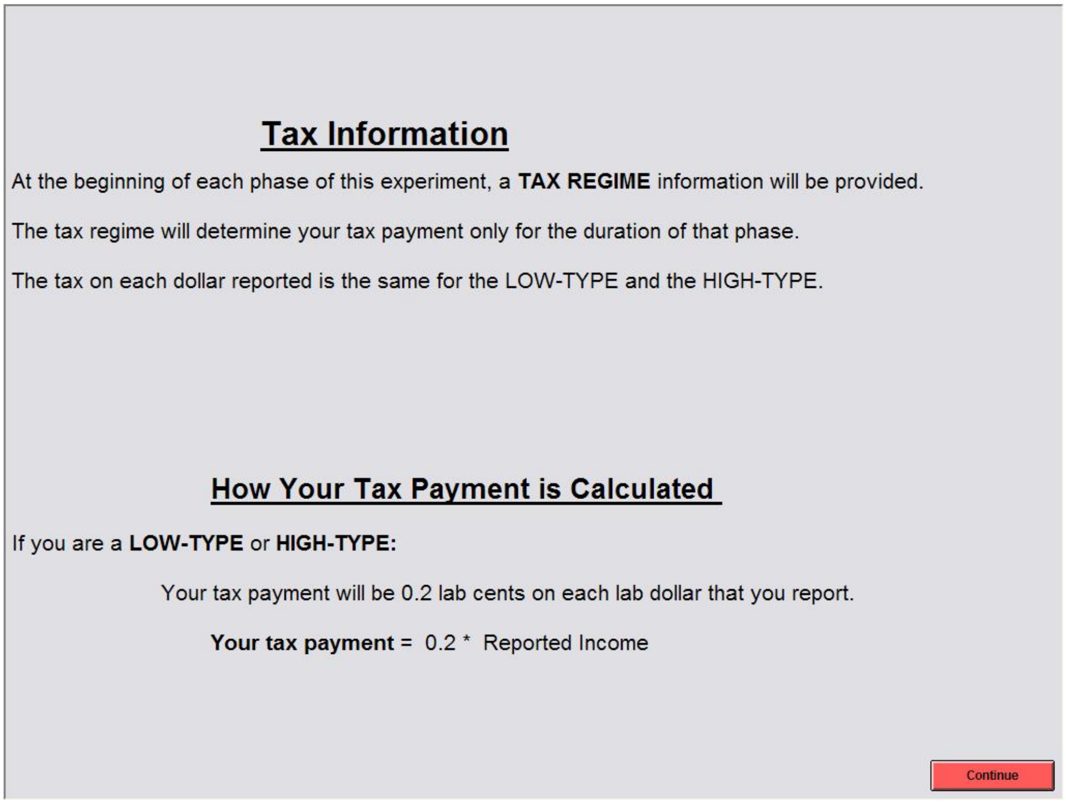


Figure 39: Tax Information Screen Shot

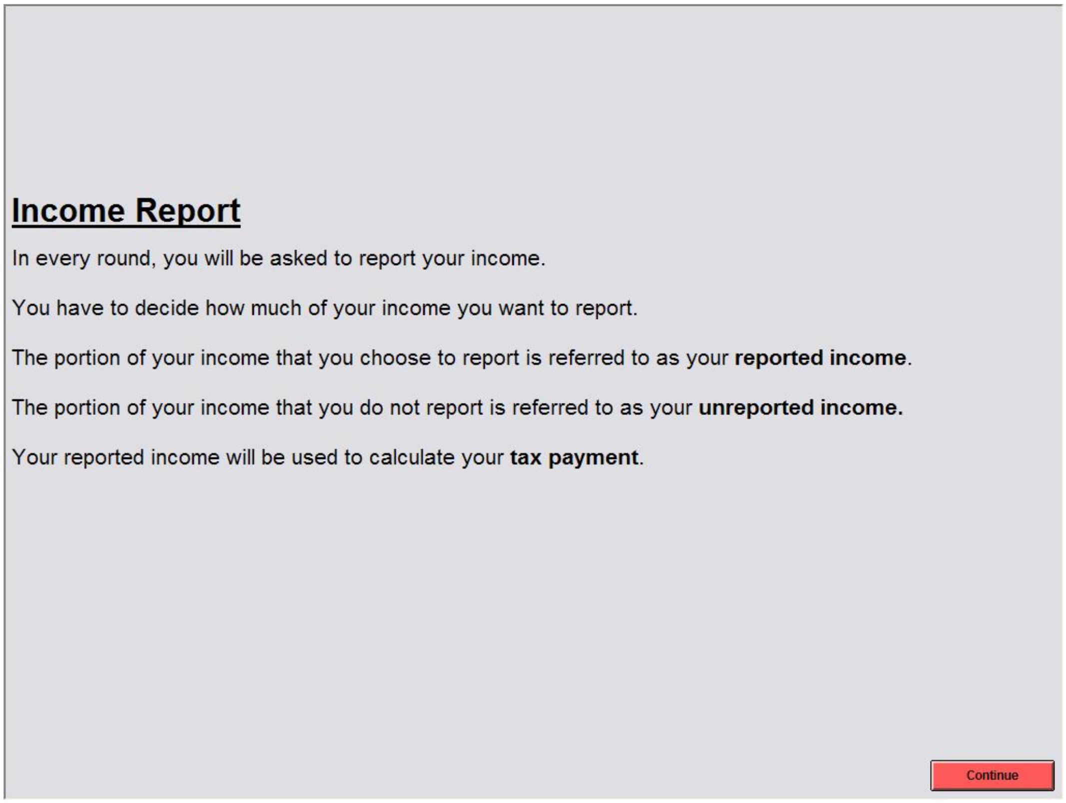


Figure 40: Income Report Screen Shot

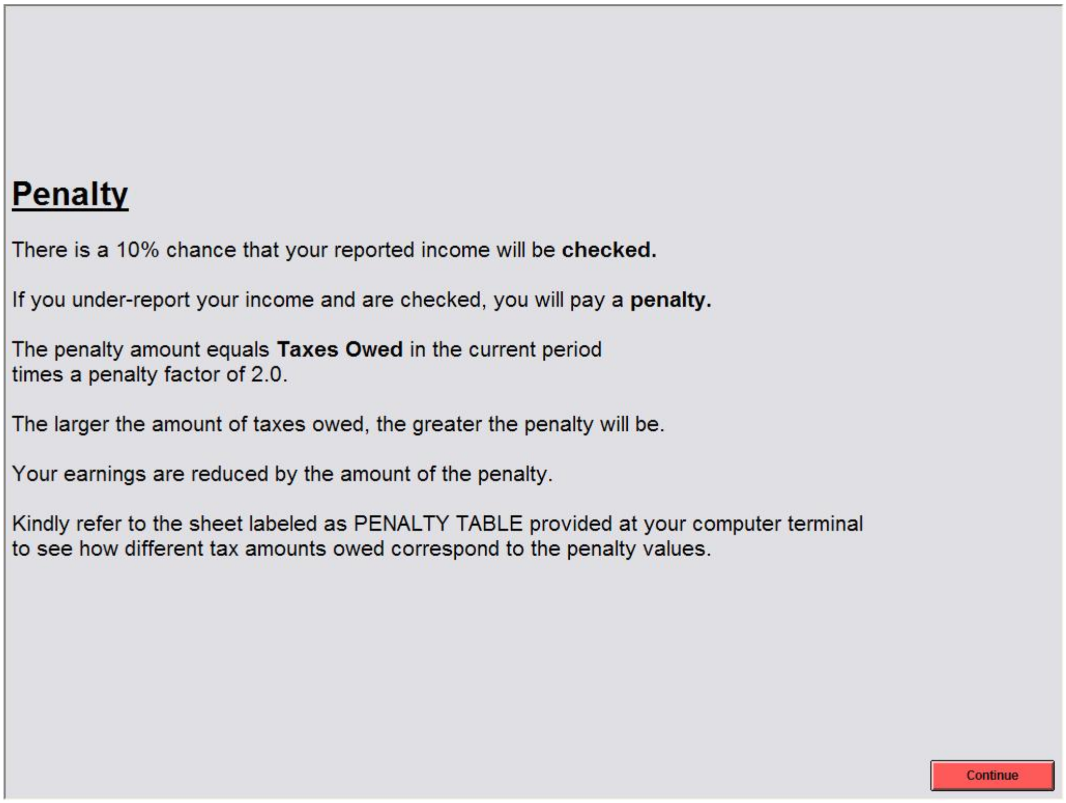
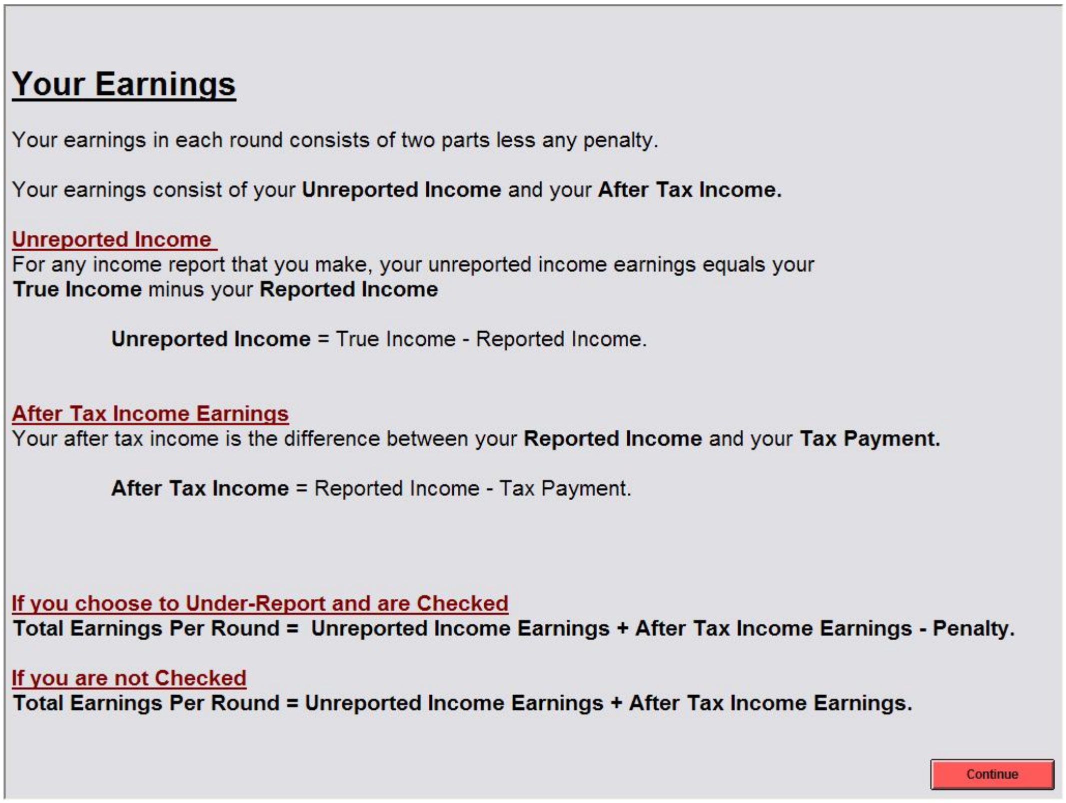


Figure 41: Penalty Structure Screen Shot



Your Earnings

Your earnings in each round consists of two parts less any penalty.

Your earnings consist of your **Unreported Income** and your **After Tax Income.**

Unreported Income
For any income report that you make, your unreported income earnings equals your **True Income** minus your **Reported Income**

Unreported Income = True Income - Reported Income.

After Tax Income Earnings
Your after tax income is the difference between your **Reported Income** and your **Tax Payment.**

After Tax Income = Reported Income - Tax Payment.

If you choose to Under-Report and are Checked
Total Earnings Per Round = Unreported Income Earnings + After Tax Income Earnings - Penalty.

If you are not Checked
Total Earnings Per Round = Unreported Income Earnings + After Tax Income Earnings.

Continue

Figure 42: Earnings Report Screen Shot

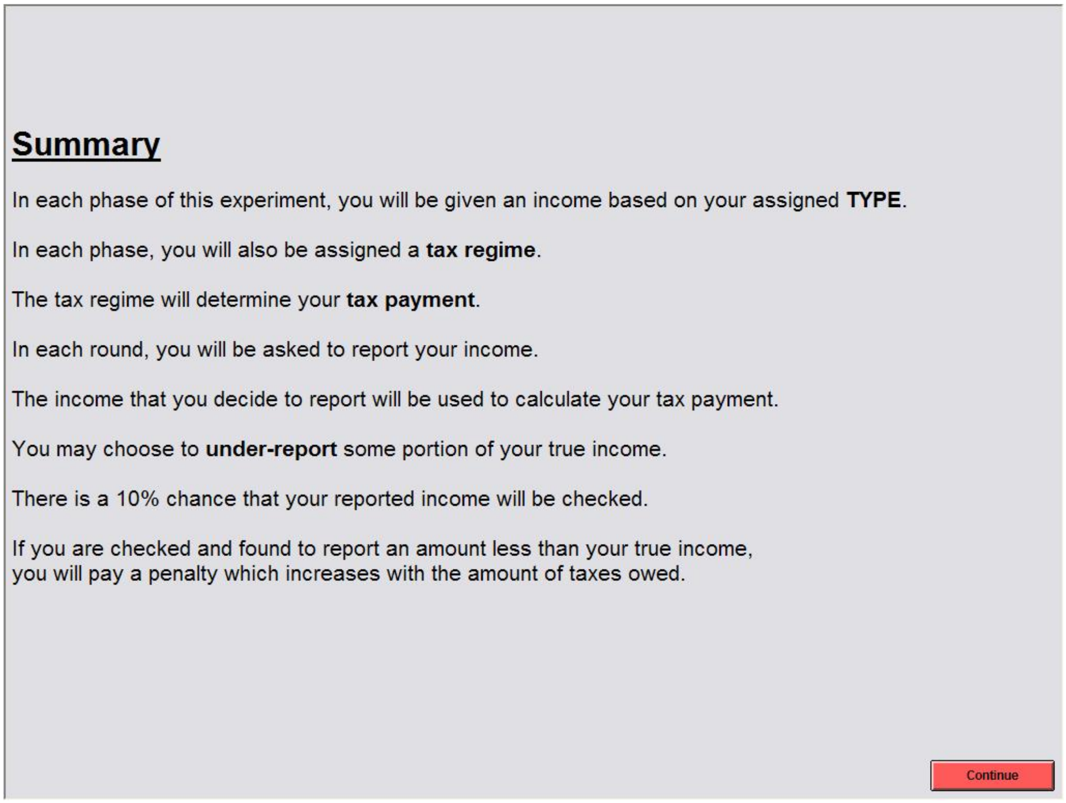


Figure 43: Summary 1 Screen Shot

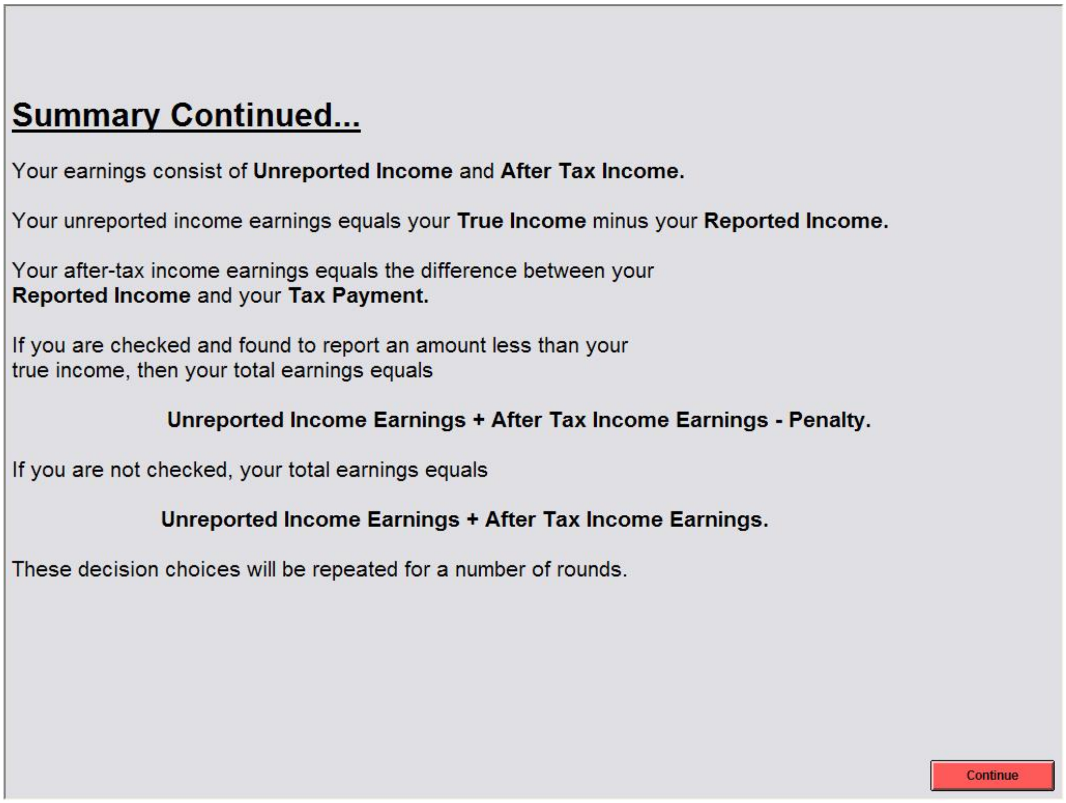


Figure 44: Summary 2 Screen Shot

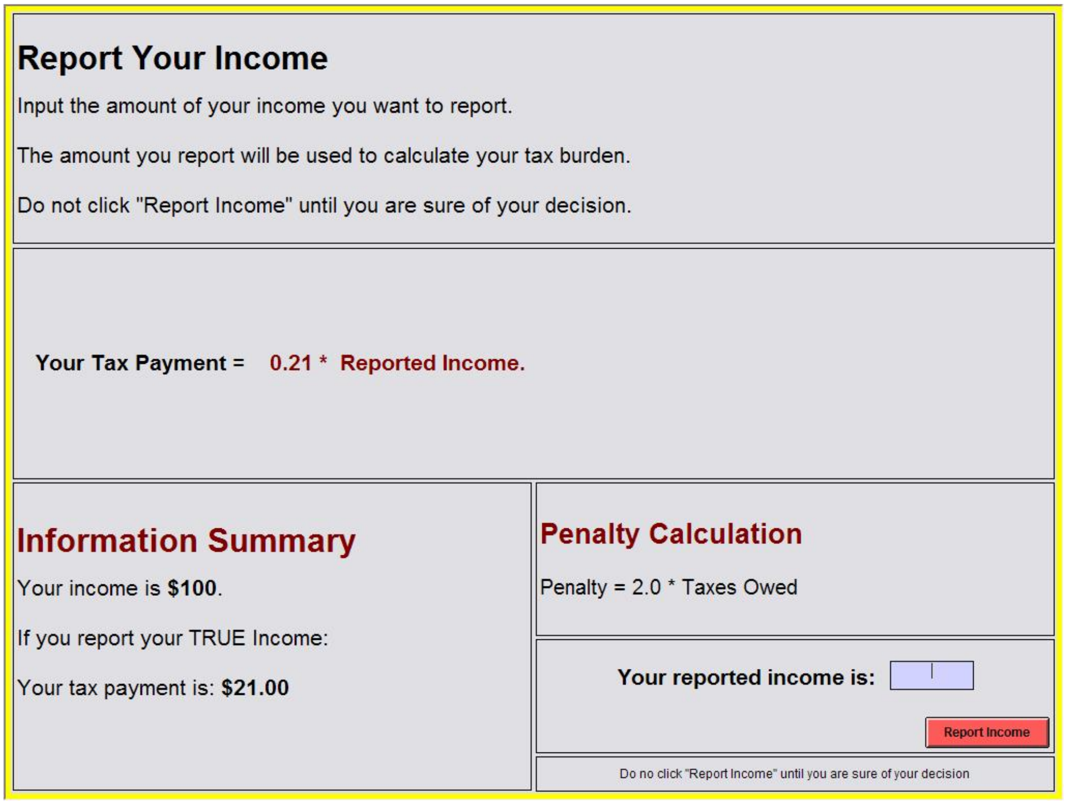


Figure 45: Income Reporting Screen Shot

THE UNIVERSITY of TENNESSEE
KNOXVILLE

Institutional Review Board
Office of Research
1534 White Avenue
Knoxville, TN 37996-1529
Phone: 865.974.3466
865.974.7400

April 30, 2009

IRB#: 7920 B

TITLE: Essays on Fraud and Tax Evasion: Evidence from Experiments

Tackie, Martin W.
Economics
Stokely Management Center 7th Floor
Campus - 4334

Santore, Rudy
Economics
513 Stokely Management Center
Campus - 0550

Your project listed above was reviewed and has been granted IRB approval under expedited review.

This approval is for a period ending one year from the date of this letter. Please make timely submission of renewal or prompt notification of project termination (see item #3 below).

Responsibilities of the investigator during the conduct of this project include the following:

1. To obtain prior approval from the Committee before instituting any changes in the project.
2. If signed consent forms are being obtained from subjects, they must be stored for at least three years following completion of the project
3. To submit a Form D to report changes in the project or to report termination at 12-month or less intervals.

The Committee wishes you every success in your research endeavor. This office will send you a renewal notice (Form R) on the anniversary of your approval date.

Sincerely,

Brenda Lawson

Brenda Lawson
Compliances

Enclosure

Figure 46: IRB Approval Letter

INFORMATION SHEET

The purpose of this study is to examine how individuals make decisions in the context of incentive mechanisms (or tax systems). There is no substantial benefit to you from the research. We hope that the research will benefit society by showing how economic institutions directly influence the decision making of its members.

You will receive all the instructions describing your task and your payoffs from this task. All information is correct and true. Our research protocols specifically forbid us from providing incorrect or misleading information.

All your decisions will remain confidential and your choices will not be identified. The only record of your participation will be the receipt form you sign at the end of the session when you are paid your earnings from the session.

You and the other participants will have the opportunity to earn cash through your decisions in an experimental market. Your participation is strictly voluntary. You may refuse to participate before the study begins or discontinue at any time. If you withdraw before the experiment begins, you will be given a "show up fee" of $3. If you withdraw during the experiment, you will be paid the cash you earned before withdrawing. Your decision to withdraw will not affect your academic standing or record.

In this experiment there is minimal risk. You have no greater physical, financial, or psychological risk from the experiment than you would from doing a similar amount of routine paperwork or computer-based activity in any similar University of Tennessee classroom or computer laboratory.

Please ask questions you have about the study before agreeing to participate. After the experiment, Graduate Research Assistant Martin Tackie (email: mtackie@utk.edu; phone: 974-3303) will be glad to answer any additional questions that you may have. Please feel free to take this sheet with you when you leave the experimental laboratory.

The paragraph below is only applicable to individuals participating in the **tax experiment**. Kindly ignore the paragraph below if you are participating in the incentive mechanism experiment.

Your participation in this study is voluntary; you may decline to participate without penalty. If you decide to participate, you may withdraw from the study at anytime without penalty and without loss of benefits to which you are otherwise entitled. If you withdraw from the study before data collection is completed you data will be returned to you or destroyed. Return of the completed survey (questionnaire) constitutes your consent to participate.

EXPEDITED APPROVED

DATE 4-30-2009

Brenda Lawson

Compliance Officer & I R B Administrator

Figure 47: Consent Information Sheet

Vita

Martin W. Tackie was born in Accra, Ghana on July 2, 1980. He received his Bachelor of Arts degree in Computer Science and Economics from Concordia College, Moorhead, Minnesota in May 2004. He matriculated at The University of Tennessee in August 2004, where he received his Master of Arts degree in Economics in August 2007. His doctoral degree would be received in August 2009. The author is a member of the American Economics Association and the Western Economic Association International.