

**AN EXAMINATION OF THE EFFECTIVENESS OF SANCTION BASED TAX
COMPLIANCE PERSUASIVE MESSAGES OVER REPEATED PERIODS**

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ABSTRACT

Prior tax compliance literature has examined the use of persuasive messages that emphasize audit and sanctions as a way to increase compliance. This work has been done in single period experiments using either survey or field study methodologies. Results from the prior studies are mixed.

The theory of reasoned action is a theory of social behavior that promotes emphasizing direct consequences of actions to motivate specific behavior. Persuasive messages based on this theory have been found to be effective in a number of different disciplines. The theory of reasoned action has been used in the field of tax compliance to explain compliance behavior and examine the behavioral beliefs related to compliant reporting, but has not been used to design persuasive messages aimed at increasing compliance.

In this dissertation, I conduct a laboratory experiment that examines the effects of two types of messages - a traditional message consisting of a simple reminder of audit risk and a message designed based on the theory of reasoned action. Consistent with prior research on tax compliance, I test the messages in an initial single period but I extend prior research by also examining the effects of the messages over repeated periods. Neither the traditional message nor the message based on the theory of reasoned action have a significant effect on initial period compliance. The interaction effect of the traditional message and time on tax compliance is positive and significant and the interaction effect of the theory of reasoned action message and time on tax compliance is positive and marginally significant. These results provide evidence that the messages may be effective in increasing an individual's tax compliance over time. In the

repeated period data, the theory of reasoned action message exhibits a positive and significant impact on the amount of income reported when an individual reports less than 100% of their earned income, providing evidence that messages designed based on the theory of reasoned action may be an effective tool in reducing the tax gap.

DEDICATION

I dedicate this dissertation to my husband, David, and our daughter, Mara. Thank you for your love, encouragement, and support. You two are amazing, and I would not have made it through this without you.

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TABLE OF CONTENTS

CHAPTER 1: INTRODUCTION	1
CHAPTER 2: LITERATURE REVIEW AND DEVELOPMENT OF HYPOTHESES	5
2.1 SANCTION BASED PERSUASIVE MESSAGES AND MESSAGE BELIEVABILITY	5
2.1.1 Sanction Based Persuasive Messages.....	5
2.1.2 Message Believability.....	7
2.2 THE THEORY OF REASONED ACTION, TAX COMPLIANCE, AND PERSUASIVE MESSAGES	9
2.2.1 The Theory of Reasoned Action.....	9
2.2.2 The Theory of Reasoned Action and Tax Compliance	11
2.2.3 The Theory of Reasoned Action and Persuasive Messages	14
CHAPTER 3: RESEARCH METHOD	19
3.1 PARTICIPANTS	19
3.2 EXPERIMENTAL DESIGN	21
3.3 LABORATORY PROCEDURES	22
CHAPTER 4: ANALYSIS AND HYPOTHESIS TESTING.....	30
4.1 INDEPENDENT, DEPENDENT, AND CONTROL VARIABLES	30
4.1.1 Independent and Dependent Variables	30
4.1.2 Control Variables.....	31
4.2 HYPOTHESIS TESTING.....	34
4.2.1 Tests of Hypotheses H1 and H3	34
4.2.2 Tests of Hypotheses H2 and H4	35
4.2 ADDITIONAL ANALYSIS.....	39
CHAPTER 5: CONCLUSION	41
5.1 DISCUSSION AND IMPLICATIONS.....	41
5.2 CONTRIBUTIONS AND LIMITATIONS.....	43
5.3 FUTURE RESEARCH	44
REFERENCES	46
APPENDIX A: FIGURES	52

APPENDIX B: TABLES	59
APPENDIX C: EXPERIMENTAL PROGRAM SCREENSHOTS.....	76
EXPERIMENTAL INSTRUCTIONS	76
EXAMPLE 1: ROUND 1 WITH A CONTROL GROUP PARTICIPANT	83
EXAMPLE 2: ROUND 2 WITH A TRADITIONAL MESSAGE GROUP PARTICIPANT	86
EXAMPLE 3: ROUND 3 WITH A TRA MESSAGE GROUP PARTICIPANT	89
DEMOGRAPHICS	92
APPENDIX D: RISK GAME AND INSTRUCTIONS	96
APPENDIX E: ATTITUDE AND PERSONALITY SCALES.....	98

LIST OF FIGURES

Figure 1: The Theory of Reasoned Action	52
Figure 2: Action-Outcome Matrix	53
Figure 3: Normal Probability Plot for Round 1 Compliance.....	54
Figure 4: Normal Probability Plot for Compliance with Post Audit Data Dropped.....	55
Figure 5: Mean Compliance over Time with Post Audit Data Dropped	56
Figure 6: Mean Compliance over Time with Post Audit Data Included	57
Figure 7: Normal Probability Plot for Compliance with Post Audit Data Included.....	58

LIST OF TABLES

Table 1: Summary of Participant Demographics.....	59
Table 2: Summary of Participant Taxpaying Experience.....	60
Table 3: Descriptive Statistics for Round 1 Compliance.....	61
Table 4: Test of the Effect of the Traditional Message Group on Compliance in the Initial Round (H1) - Right Censored Tobit Models.....	62
Table 5: Test of the Effect of the TRA Message Group on Compliance in the Initial Round (H3) - Right Censored Tobit Models with TRA Message Group as Intercept.....	64
Table 6: Compliance by Round with Post Audit Observations Dropped.....	66
Table 7: Tests of the Effect of Message Group on Compliance Across All Rounds (H2 and H4) - Random Effect Panel Data Two Limit Tobit Models.....	68
Table 8: Two Part Test of the Effect of Message Group on Compliance Across All Rounds (H2 and H4).....	70
Table 9: Compliance by Round with Post Audit Observations Included.....	72
Table 10: Additional Analysis: Test of Effect of Messages on Compliance Across all Rounds with Post Audit Observations Included - Random Effect Two Limit Tobit Model.....	74

CHAPTER 1

INTRODUCTION

Recent Internal Revenue Service (IRS) estimates place the annual United States tax gap at approximately \$300 billion (Blanthorne and Selvey 2006; *The Tax Gap* 2007). The IRS has estimated that individual underreporting behavior is responsible for the majority of the total tax gap (Blanthorne and Selvey 2006; Congress Looking to Close "Tax Gap" 2007; *The Tax Gap* 2007). Regulators have recently shown renewed interest in reducing or eliminating the US tax gap (Abrams 2007; Blanthorne and Selvey 2006; Congress Looking to Close "Tax Gap" 2007; Russell 2007). One potential way to reduce the tax gap is through greater enforcement efforts; however, enforcement efforts require large monetary and personnel resources and therefore may not be the most practical way of addressing the tax gap problem (Congress Looking to Close "Tax Gap" 2007). Another, potentially less costly, method of trying to close the tax gap is to motivate greater levels of tax compliance through the use of persuasive messages.

The use of persuasive messages to increase compliant tax reporting behavior has been examined in the prior literature since the late 1960's. The types of persuasive messages previously tested tend to fall into two broad categories: messages that remind individuals of the potential for audit and sanctions (Hasseldine et al. 2007; Schwartz and Orleans 1967; Slemrod et al. 2001; Violette 1989; Hasseldine and Kaplan 1992; Hite 1989; Jackson and Jaouen 1989) and messages that emphasize a moral or social responsibility to report compliantly (Blumenthal et al. 2001; Hasseldine et al. 2007; Hite 1997; McGraw and Scholz 1991; Roberts 1994; Schwartz and Orleans 1967; Kaplan et al. 1997). While much of the evidence in regards to the effectiveness of sanction based persuasive messages in increasing tax compliance is mixed, a recent field study (Hasseldine et al. 2007) found that messages that emphasized an increased risk of audit were

more effective in reducing aggressive tax behavior than messages that emphasized a moral appeal or provided information about tax assistance services. In this study and all of the prior studies the effectiveness of persuasive messages in increasing compliant tax reporting behavior was tested in a single period. In the United States tax system, however, individuals must file and pay income taxes on an annual basis throughout their lifetime. When individuals are in a situation in which they must choose one course of action over another, they evaluate the success of their chosen action based on the consequences of that action and will use their evaluations of their prior choices when making the same decision in the future (Einhorn and Hogarth 1978; Einhorn 1982). Thus, I believe it is important to examine the effectiveness of these messages over multiple, repeated periods.

The theory of reasoned action (Fishbein and Ajzen 1975; Ajzen and Fishbein 1980) and its successor, the theory of planned behavior (Ajzen 1991) are theories of social behavior that have been used successfully in the tax compliance literature to explain overall tax compliance in the United States (Hanno and Violette 1996; Bobek and Hatfield 2003). The theory of reasoned action has also been used to design persuasive messages to influence behavior in other contexts such as littering, condom usage, tobacco usage, and registering for career counseling (Evans 1977; Treise and Weigold 2001; Murphy 2002; Lee et al. 2006; Lepre 2007; White et al. 2008), but has not, to my knowledge, been used to design persuasive messages aimed at increasing compliant tax reporting behavior.

I conduct a study in which I examine the effectiveness of the traditionally tested sanction based persuasive message over repeated periods. I also design a persuasive message based on the theory of reasoned action and incorporating the findings of prior tax compliance research conducted within the framework of the theory of reasoned action. I examine the effectiveness of

these messages in both a single period and repeated periods. To investigate effects over repeated periods, I employ an experimental economics laboratory design where a micro economy is created in the laboratory and individuals earn income and are asked to voluntarily report their income for taxation. The results of this study fail to find evidence that either the traditionally tested message or the message of theory of reasoned action have a significant impact on compliance in the initial period. However, I find a significant positive message by time interaction for the traditional message and a marginally significant positive message by time interaction effect for the theory of reasoned action message, which provides some evidence that even though the messages do not affect compliance in the initial period, they have a positive impact on compliance over time. These results illustrate the importance of testing the messages over time rather than just in a single period. In addition, I find that even though the theory of reasoned action message does not have a significant effect on the probability that an individual would report 100% of his income, it does have a significant positive main effect on the amount of income that is reported when individuals report less than 100% of their income. Contrary to expectations, I do not find that the results change substantially based on whether or not the observations after an individual is audited are included in the sample.

This study contributes to the current literature in three ways. First, I test the effects of persuasive messages in increasing tax compliance over repeated periods. Previous research has examined only the initial period effect of persuasive messages; however, tax compliance is an activity that is repeated regularly over an individual's lifetime, making it important to consider the effects of the messages over repeated periods. Secondly, I incorporate prior research results into the theory of reasoned action framework and test a persuasive message based on the theory of reasoned action. This type of message has been found to be effective in other settings, but it

has not been tested in the tax compliance literature. Finally, I test the messages in a laboratory experiment rather than through a survey based methodology or a field study. The laboratory experiment allows me to provide incentives similar to those that exist in the real world context while maintaining a high level of control.

The remainder of this paper is organized as follows. In Chapter 2, I present the literature review and hypotheses development. In Chapter 3, I describe the experimental design and laboratory procedures employed. In Chapter 4, I present the statistical analysis and tests of hypotheses. I conclude the paper with Chapter 5, in which I discuss my findings and the implications of those findings, the contributions and limitations of the research, and possible avenues for future research.

CHAPTER 2

LITERATURE REVIEW AND DEVELOPMENT OF HYPOTHESES

2.1 Sanction Based Persuasive Messages and Message Believability

2.1.1 Sanction Based Persuasive Messages

Prior research has investigated the usefulness of reminding taxpayers about the possibility of audit, sanctions, and penalties in increasing tax compliance (Hasseldine et al. 2007; Hasseldine and Kaplan 1992; Hite 1989; Jackson and Jaouen 1989; Schwartz and Orleans 1967; Violette 1989). While most of the results have been mixed, some studies, including a recent field study conducted in Great Britain (Hasseldine et al. 2007), have found evidence that these messages can be used effectively to increase tax compliance. Except where noted, prior studies were conducted using a survey type instrument.

Schwartz and Orleans (1967) conducted a field study in which they found that threat of sanction did increase tax compliance over a control group, but not over a group who were exposed to an appeal to conscience. Jackson and Jaouen (1989) examined the effects of sanction messages or appeals to conscience on participants' propensity to evade taxes, measured using the tax resistance scale developed by Spicer and Lundstedt (1976), and found that neither message reduced the participants' propensity to evade. In different, but similar studies, Violette (1989) and Hasseldine and Kaplan (1992) compared messages that emphasized legal sanctions to messages that emphasized informal social sanctions. In both studies, participants responded to a hypothetical tax reporting scenario after reading a persuasive message. Violette (1989) found that participants who were exposed to either a legal sanctions only message or a message that combined both legal and informal social sanctions behaved more compliantly than a control

group. Hasseldine and Kaplan (1992) found that neither of the messages tested in their study was effective in increasing tax compliant behavior.

The most recent study that examined the use of sanction based persuasive messages in increasing tax compliance was a field study conducted in the United Kingdom by Hasseldine, Hite, James, and Toumi (2007). In this study, the authors tested the effectiveness of five different persuasive messages in increasing tax compliance. They found evidence that the three messages that emphasized sanctions were overall more effective at increasing tax compliance than the messages that emphasized citizenship or provided information in regards to available tax assistance services. Hasseldine et al (2007) began each of the three sanction based messages with the following statement:

The Inland Revenue is substantially increasing the number of enquiries into the tax returns of people who have previously reported a turnover of just below £15,000 in successive years.

Your 2001 return may be one of those chosen for enquiry. (Hasseldine et al. 2007)

While not explicitly stated, the objective of the sanction messages presented in the Hasseldine et al (2007) study appears to be to increase the recipients' perceptions of the likelihood of audit. In addition, examination of the studies conducted prior to Hasseldine et al (2007) leads the reader to conclude that, in those studies, exposure to the sanction based message was expected to either increase the recipients' perceptions of the probability that noncompliance would be detected or to increase the perceived severity of the penalties related to noncompliance. Violette (1989) found that the perceived severity of sanctions was greater for a group of participants presented only with factual information about audit and sanction rates than for a control group that did not receive the factual information. Thus, even messages that do no more than present factual information appear to have an influence on the perceptions of those who receive them.

2.1.2 Message Believability

For a persuasive communication to influence behavior, it is necessary that the individual receiving the communication believe the information that is presented in the persuasive communication (Fishbein and Ajzen 1975; Ajzen and Fishbein 1980). Message believability has been found to be an important factor affecting message persuasion in various other social advertising domains such as warning labels for dangerous products and messages promoting specific political choices (Andrews and Netemeyer 1991; Andrews and Netemeyer 1990; Beltramini 1988; Beltramini and Evans 1985; O'Cass 2002; O'Cass and Griffin 2006). Message believability refers to the extent to which the recipient of the message accepts, or believes, the content of the message (O'Cass and Griffin 2006).

An important factor affecting message believability is the credibility of the source delivering the message. Highly credible sources can increase overall message believability (Andrews and Netemeyer 1991; Andrews 1989; Andrews and Netemeyer 1990; Arora et al. 2006; Fishbein and Ajzen 1975; Pornpitakpan 2004). Messages such as those presented in Hasseldine et al (2007) that come from a highly credible source, the actual taxing authority, are most likely perceived as believable in the year that they are initially received and therefore increase the recipients' perceptions of their own audit probability. Carnes and Englebrecht (1995) found evidence that not just detection risk, but also taxpayers' perceptions of detection risk influence their tax compliance decisions. It would then follow that the increased perception of risk on the part of the message recipient would lead the recipient to behave in a more compliant manner.

H1: Individuals exposed to a persuasive message reminding them of the risk of audit will exhibit more compliant tax reporting behavior in the initial period that they receive the message than individuals who are not exposed to the persuasive message.

Prior work with messages designed to increase perception of audit probability has been conducted as a one period experiment; however, the act of paying one's taxes is repeated on a regular basis over one's lifetime. Thus, one could argue that the goal of any program designed to increase tax compliance should be to increase tax compliance over the long term and not just in the current period. In order for the message that increases a taxpayer's perception of audit risk to remain effective over repeated periods, the message must remain believable.

Because source credibility is one of the determinants of message believability, source credibility must remain high for continued messages from that source to be viewed as believable. While they do not test their assertion, Alm and McKee (2006) argue that sending letters to taxpayers that increase their perception of audit probability, but not backing the letters up with an actual audit, will cause the taxing authority to lose credibility. This loss in credibility should then cause a decrease in the believability of future messages received by the taxing authority.

Another factor that has been found to reduce message believability is personal experience with the target of the message that conflicts with the information contained in the message itself (Andrews and Netemeyer 1991). In cases in which direct observation or personal experience conflict with third party information, the direct observation and personal experience are greater determinants of the nature and strength of an individual's beliefs than the third party information (Cialdini et al. 1981; Fishbein and Ajzen 1975; Smith and Swinyard 1982). Evans (1977) found evidence of a strong attitude change in the opposite direction from the persuasive message when

public service announcements were deemed unbelievable based on the subject's personal experience with the target of the message.

A relatively small portion of the population in the United States is audited each year. If a mass communication designed to increase the perception of audit probability is used, but the majority of the population does not experience an audit in the years after the initial message is received, the loss in source credibility and the conflicting personal experience of the message recipients should lead to decreased message believability. If the message is repeated and is perceived as less believable by the portion of the population who has not experienced an audit, the effectiveness of the message in increasing tax compliant reporting behavior will be impaired.

H2: Compliance behavior of individuals not previously subject to audit will not differ significantly over time between those receiving a persuasive message reminding them of the risk of audit and those who are not exposed to the persuasive message.

2.2 The Theory of Reasoned Action, Tax Compliance, and Persuasive Messages

2.2.1 The Theory of Reasoned Action

The theory of reasoned action (Ajzen and Fishbein 1980; Fishbein and Ajzen 1975) is a theory of social behavior that states that the primary determinant of behavior is intention, and the determinants of intention are attitudes and subjective norms. Intention is whether an individual plans on performing the behavior or not, attitudes are the individual's evaluations, either positive or negative, of performing the behavior, and subjective norms are the individual's perceptions of the social pressures to either perform or not perform the behavior (Fishbein and Ajzen 1975; Ajzen and Fishbein 1980). For example, if we were examining the behavior of speeding, whether a specific individual drives over the speed limit or not will most immediately be

determined by whether he plans to drive above the speed limit. Whether he plans to drive over the speed limit will be determined both by how positively or negatively he evaluates the behavior of speeding and the social pressures that he feels to either speed or not speed. The relative weight of the attitudes and subjective norms on forming intention will vary based on the individual and the situation. Expressed mathematically, the theory of reasoned action states:

$$BI = (AB)(W_{AB}) + (SN)(W_{SN})$$

Where:

BI = Behavioral Intent

AB = Attitude toward the Behavior

SN = Subject Norms toward the Behavior

W = the relative weight of attitude and subjective norms

The determinants of attitudes towards behaviors are the salient behavioral beliefs, while the determinants of subjective norms are the salient normative beliefs. Behavioral beliefs are an individual's beliefs about the direct consequences of either performing or not performing the behavior (Fishbein and Ajzen 1975; Ajzen and Fishbein 1980). In the example of an examination of speeding behavior, two potential behavioral beliefs that an individual may hold would be that speeding would get him to his destination faster than not speeding and that speeding would lead to a traffic ticket. The linkage between behavioral beliefs and the attitudes toward the behavior are more specifically defined in the expectancy-value view of behavioral attitudes (Fishbein and Ajzen 1975). Normative beliefs are the individual's beliefs about whether the people important to him think he should perform the behavior or not (Fishbein and Ajzen 1975; Ajzen and Fishbein 1980). Individuals may hold many behavioral and normative beliefs about a behavior, but only those that are salient affect attitudes and norms (Fishbein and

Ajzen 1975). The behavioral linkages for the theory of reasoned action are presented in Figure 1.

Ajzen and Fishbein's (1975) expectancy-value view of behavioral attitudes states that an attitude is a function of an individual's evaluation of the probability that performing a behavior will lead to a specific outcome and the evaluation or relative favorableness of that outcome. The expectancy-value view of attitude formation can be expressed mathematically as:

$$AB \propto \sum_{i=1}^n b_i e_i$$

Where:

AB = Attitude toward behavior X

b_i = the subjective probability that performing behavior X will lead to outcome i

e_i = the evaluation of the outcome i

2.2.2 *The Theory of Reasoned Action and Tax Compliance*

The theory of reasoned action and its successor, the theory of planned behavior, have both been used to explain overall tax compliance behavior. Hanno and Violette (1996) used the theory of reasoned action framework to examine overall tax compliance behavior in the United States. They used prior research to compile a list of salient beliefs in regards to tax compliance behavior and then looked at differences in the beliefs held between individuals who were classified as either compliers or non-compliers based on survey answers. Belief strength (that the behavior would lead to the outcome) and the evaluation of the outcomes were also measured along with measurements of subjective norms and intentions. They found that both attitudes and subjective norms were correlated with intention and for the participants in their study the relative weight of attitudinal considerations was higher than normative considerations. They also found that intention was correlated with behavior. In terms of the beliefs held by compliers compared

to non-compliers, they found that compliers evaluated fulfilling their duty as a citizen and their personal moral and ethical obligations as a more desirable outcome to tax compliance than did non-compliers. Non-compliers evaluated receiving a larger tax refund as a more desirable outcome to noncompliance than did compliers. Non-compliers also evaluated avoiding an IRS audit as a more desirable outcome and paying a large percentage of their earnings for taxes as a more negative outcome to compliance than did compliers.

Bobek and Hatfield (2003) examined tax compliance in the context of the theory of planned behavior (Ajzen 1991), an extension of the theory of reasoned action that differs only in that it includes a third variable, perceived behavioral control, as a determinant of intention. In the tax context, the level of perceived behavioral control a taxpayer has is based on the type of income the taxpayer earns and the type of deductions the taxpayer claims. A taxpayer whose sole source of income is wages reported on a Form W-2 by their employer and who takes a standard deduction will not have an opportunity to underreport his taxable income. Taxpayers with large amounts of income not reported to the IRS by a third party will have greater perceived behavioral control (Blanthorne 2000; Bobek and Hatfield 2003). Examples of income that is not reported to the IRS by a third party are sole proprietorship income reported on a Schedule C, income from a flow-through entity such as an S-corporation or a partnership reported on the Schedule E for which the taxpayer has control over the amounts reported on the flow-through entity's tax return, and farm income reported on a Schedule F. In addition, itemized deductions on the Schedule A provide an opportunity for taxpayers to underreport their taxable income through the overstatement of deductions. The additional perceived behavioral control variable in the theory of planned behavior is helpful in explaining overall tax compliance behavior in the United States because individual taxpayers will have varying levels of perceived behavioral

control based on the types of income and deductions that they report on their tax return.

Campaigns directed at increasing compliant tax reporting behavior would be expected to have an effect only on the compliance of those taxpayers who report the types of income and deductions that would provide them with the opportunity to underreport their income and, therefore, have some level of perceived behavioral control. In the current research, I am investigating the effectiveness of a campaign directed at increasing tax compliance, which is inherently focused on taxpayers who should have some level of perceived behavioral control; therefore, I have designed the research around the framework of the theory of reasoned action rather than the theory of planned behavior. The tests of behavioral beliefs and subjective norms presented in the research done by Bobek and Hatfield (2003) can, however, provide useful information for the current research.

The Bobek and Hatfield (2003) study differs from the Hanno and Violette (1996) study in that Bobek and Hatfield used a two step process in which they first elicited salient beliefs about tax compliance, rather than drawing them from prior research, and then they incorporated those beliefs into an instrument that examined multiple taxpaying scenarios and measured subjects' beliefs, attitudes, and behavioral intention in each separate scenario. They found five salient beliefs related to tax compliance in their subject pool. Subjects believed that failing to comply would result in their engaging in illegal behavior, feeling guilty, incurring a penalty, minimizing their taxes paid, and a failure to pay their fair share. Three of the beliefs – feeling guilty, incurring a penalty, and minimization of taxes paid – were included in the list of beliefs identified in the prior study by Hanno and Violette (1996). Beliefs about guilt, tax minimization, fairness, and illegality of behavior differed between compliers and non-compliers in all scenarios while beliefs about penalty incursion differed between compliers and non-compliers for some,

but not all scenarios. Both the theory of reasoned action and the theory of planned behavior were deemed to be appropriate models to examine overall tax compliance in the United States. To my knowledge, though, the theory of reasoned action and the results of these studies have not been used to design persuasive messages aimed at influencing tax compliance behavior.

2.2.3 The Theory of Reasoned Action and Persuasive Messages

The theory of reasoned action is not only useful to those who wish to explain a particular behavior, but also to those who wish to influence that behavior (Sheppard et al. 1988; Randall 1989). Persuasive messages designed based on the framework provided by the theory of reasoned action have been found to be effective in influencing a variety of behaviors including littering (Evans 1977), condom usage (Treise and Weigold 2001), tobacco usage (Murphy 2002), reactions to mobile phone advertisements (Lee et al. 2006), registering for career counseling (Lepre 2007), and reactions to affirmative action programs (White et al. 2008). This study examines the use of the theory of reasoned action to design a persuasive message aimed at increasing compliant tax reporting behavior.

Persuasive messages based on the theory of reasoned action can address either behavioral beliefs about the behavior of interest, normative beliefs about the behavior of interest, or a combination of both behavioral and normative beliefs about the behavior of interest. There is evidence in the prior tax compliance research that normative beliefs and subjective norms are related to tax compliance behavior (Bobek et al. 2007; Jackson and Milliron 1986; Wenzel 2004; Hanno and Violette 1996; Bobek and Hatfield 2003), which suggests that a multi-pronged approach to reducing noncompliant behavior might be the most effective approach. However, to keep the scope of this research manageable and to isolate the effects of changes in behavioral beliefs alone, this study focuses on messages that address behavioral beliefs only.

To influence a specific behavior using persuasive messages, the message must focus on beliefs about the direct consequences of performing or not performing the behavior of interest and not on beliefs about items tangentially related to the behavior (Fishbein and Ajzen 1975; Ajzen and Fishbein 1980). For example, if one wanted to influence individuals to sign up for an alcohol treatment program, the persuasive communication should not focus on the negative aspects of alcoholism, but rather the direct positive consequences of signing up for the treatment program or the direct negative consequences of not signing up for the treatment program (Ajzen and Fishbein 1980).

To influence tax compliance using persuasive messages, the persuasive messages should target the salient beliefs related to the direct consequences of paying one's taxes compliantly or failing to pay one's taxes compliantly rather than the overall beliefs about audit probability. The idea behind a deterrence model of compliance is that noncompliant behavior is detected and punished. However, with the low tax audit rates in the United States, it is not possible to detect and punish all noncompliant taxpayers. Therefore, a message focusing on the negative consequences of noncompliance would have the same believability issues as the traditionally tested message designed to influence overall perception of audit probability. Hanno and Violette (1996) found that non-compliers, the target population for the persuasive message, evaluated receiving a larger refund and avoiding an IRS audit as more positive consequences of failing to comply and complying, respectively, than compliers and paying a larger percentage of one's income in taxes as a more negative consequence of compliance than did compliers. In addition, Bobek and Hatfield (2003) found that the evaluation of penalty incursion differed between non-compliers and compliers in some tax scenarios.

Based on the prior research about the salient beliefs related to tax compliance and following the framework provided by the theory of reasoned action, an effective persuasive message would be one that links compliant tax reporting behavior with the positive consequence of avoiding an IRS audit and emphasizes the fact that incurring sanctions and penalties would increase the taxpayer's total tax bill to a level greater than it would have been if the taxpayer had paid honestly to begin with. The purpose of this message would be twofold. The first purpose would be to strengthen the belief that paying one's taxes compliantly will lead to the positive consequence of avoiding an audit. The second purpose is to try to influence the recipients' evaluations of the consequences of compliant and noncompliant behavior such that the payment of a larger percentage of one's income is evaluated less negatively and the incursion of penalties is evaluated more negatively. Given that the IRS does use some selection rules aimed at targeting audits more effectively and that the persuasive message would come from a highly credible source, the belief that paying one's taxes honestly would result in a reduction in audit probability is plausible and the theory of reasoned action message should have the same believability in the initial period as the message aimed at increasing perception of overall audit probability.

When presenting a persuasive message based on the theory of reasoned action, the concern is not with individuals' perceptions of audit rates, but with their beliefs about the consequences of compliant taxpaying behavior. Individuals who receive these messages may still believe that their overall chance of audit is low. Research in the area of law provides evidence that people will use more care than is efficient if they believe that they can reduce a remote risk to zero through careful behavior and if the perceived cost of the care taken is less than the perceived reduction in the potential negative consequences if care is not taken (Posner

2005). Thus, the expectation is that the persuasive message based on the theory of reasoned action would be effective even for individuals who perceive audit rates to be low.

H3: Individuals exposed to a persuasive message based on the theory of reasoned action will exhibit more compliant tax reporting behavior in the initial period that they receive the message than both individuals who are not exposed to any message and individuals who are exposed to a persuasive message reminding them of their audit risk.

For the persuasive message based on the theory of reasoned action to be effective over repeated periods, a strong belief that paying one's taxes honestly will lead to a reduced likelihood of audit must persist. In a situation such as the tax compliance decision, in which an individual must choose one course of action over another, outcomes are often the only source of information through which the individual can evaluate the effectiveness of their decision (Einhorn and Hogarth 1978). Tax audit rates in the United States are such that many individuals will not experience an audit during their lifetime. Thus, it is necessary to consider the expected effects of paying one's taxes and then observing the outcomes of that behavior on the individual's belief strength about the consequences of compliant or noncompliant tax reporting behavior.

When presented with a decision rule, individuals must first choose to accept the decision rule and act based upon it or to reject the decision rule and act contrary to it. Once an action has been taken, individuals observe the outcomes of their decision to evaluate the effectiveness of the chosen action (Einhorn and Hogarth 1978; Einhorn 1982). Individuals learn from experience by learning action-outcome linkages, which they often misinterpret as cause-effect linkages (Einhorn 1982), and they cognitively categorize outcomes with actions as either success or

failures (Estes 1976). This leads to a two by two action-outcome matrix as presented in Figure 2 (Einhorn and Hogarth 1978; Einhorn 1982). Applying this matrix to the current study, acceptance of the decision rule would be to accept the belief that compliant tax behavior leads to the positive outcome of avoidance of an IRS audit and to choose to comply, while rejection of the decision rule would be expected to lead to noncompliant behavior. A success would be a lack of IRS audit while a failure would be incurring an IRS audit.

When evaluating a decision rule based on outcomes, individuals focus on positive hit rates and ignore the other three cells even when the information about all four cells is readily available (Einhorn and Hogarth 1978; Einhorn 1982; Ward and Jenkins 1965). Estes (1976) found that participants would only consider the frequency of failing outcomes when specifically instructed to do so. Individuals who initially accept the decision rule that paying one's taxes compliantly will reduce the likelihood that one will experience an audit and report their taxes honestly and then do not experience a subsequent audit, will learn the action-outcome linkage that paying one's taxes honestly leads to absence of an audit. This learned action-outcome linkage should increase belief strength that compliant tax behavior reduces audit probability, which would lead to a persistence of the theory of reasoned action persuasive message's effectiveness over time.

H4: Individuals not previously subject to an audit who are exposed to a persuasive message based on the theory of reasoned action will exhibit more compliant tax reporting behavior over time than individuals who are not exposed to the persuasive message.

CHAPTER 3

RESEARCH METHOD

I test the hypotheses using data gathered through a laboratory experiment. A difficulty faced by researchers conducting tax compliance research is that subjects are often motivated by social forces to conceal their true tax compliance decisions (Alm and Jacobson 2007).

Individuals responding to tax compliance scenarios in a survey style instrument are subject to social incentives to present themselves as honest taxpayers, but are not subject to financial incentives similar to those present in the real world tax compliance decision. Creating a micro-economy inside the laboratory in which subjects receive monetary compensation based directly on the decisions they make during the course of the experiment can provide financial incentives similar to those present in the real world tax compliance context (Alm 1991; Alm and Jacobson 2007; Smith 1976; Davis and Swenson 1988). The experimental laboratory design also provides a setting in which the effects of the treatments can be examined over repeated periods as the subjects are required to choose a course of action, observe the outcomes of their choice, and then make the same choice again in the subsequent period. This pattern of choice followed by observation of outcome repeated over multiple periods mirrors the tax decision-making context faced by taxpayers over the course of their lifetime.

3.1 Participants

One hundred seven students recruited from principles level classes in the College of Business and the Department of Economics at a large university in the Southeastern United States participated in this study. One participant failed to complete the experiment, and one participant reported that he or she was under 18 years of age even though recruiting documents stated that individuals must be 18 years of age to be eligible to participate in the experiment.

Both participants were dropped from the data set. Participant demographics are presented in Table 1. The sample was relatively evenly split between male (55%) and female (45%). Participants' ages ranged from 18 to 25 with 62% of the sample falling in the 18 to 19 age range. Fifty-nine percent of students reported that they believed their family income to be about average when compared to other students at the university where the study was conducted, while 15% perceived their family income to be below average when compared to other students and 26% perceived their family income to be above average when compared to other students. A majority of participants were business majors (60%), and 93% of participants reported their GPA as 2.5 or greater.

While the use of student subjects is sometimes considered less than ideal due to students' lack of experience with real world contexts, there is evidence that the experimental responses of students in a properly designed laboratory setting are rarely different than the responses of other subject groups, which leaves no reason to believe that the cognitive processes of students are different from those of "real" people (Plott 1987; Alm and Jacobson 2007; Davis and Swenson 1988). In addition, the current study is based on the use of persuasive messages to influence salient behavioral beliefs related to tax compliance and how those salient beliefs are either reinforced or undermined by the outcomes of the taxpayers' compliance decisions. Direct experience with a behavior is a major factor influencing the direction and strength of an individual's behavioral beliefs (Cialdini et al. 1981; Fishbein and Ajzen 1975). The behavioral beliefs of individuals with little or no taxpaying experience should then be more malleable than individuals who are very experienced with the taxpaying context, making new taxpayers an ideal target population for a campaign designed to influence and reinforce specific behavioral beliefs. Undergraduate business and economics students should be familiar with the United States tax

system, but may have little or no direct taxpaying experience. These students then serve as an appropriate proxy for the population of new taxpayers.

3.2 Experimental Design

I designed the laboratory experiment for this study based on the guidelines for conducting tax compliance laboratory experiments as outlined by Davis and Swenson (1988), Alm (1991), and Alm and Jacobson (2007). In addition, I considered and examined the appropriate design elements of the tax compliance laboratory experiments conducted in the body of substantive research of Alm and his co-authors (Alm et al. 1992a; Alm et al. 1992b; Alm et al. 1993a; Alm et al. 1993b; Alm et al. 1993c; Alm et al. 1999; Alm and McKee 2004, 2006). I have relied most heavily on the procedures used by Alm and McKee (2006).

The study had three experimental groups: a control group and two treatment groups. The first treatment group received a persuasive message reminding them of the existence of an audit risk. The second treatment group received a persuasive message designed based on the theory of reasoned action. The control group received no message.

The experiment was conducted in the specific context of taxation. Embedding an experiment in the tax context can help improve parallelism with the real world tax compliance decision context (Alm and McKee 2006). Parallelism is one of the precepts of experimental economics design as outlined by Smith (1982) which is satisfied when the experimental setting replicates the main elements of the real world decision context. In addition, Kachelmeier (1995) argues that context free laboratory experiments simply provide evidence that individuals will act in a self-interested manner in a laboratory setting. By utilizing the tax context in the laboratory, the features of the real world tax compliance decision context that have the potential to counteract self-interested behavior are incorporated into the laboratory experiment.

3.3 Laboratory Procedures

The experiment was computerized¹ and was conducted in the experimental economics lab at the university. The lab could accommodate up to 12 subjects at one time. The design of the experiment did not require a specific number of subjects to be in the lab at the same time. Sessions were run with as few as one experimental subject and as many as eleven. When the participants entered the lab, they were asked to read and sign a consent form. This form instructed them that all of their decisions and responses would be anonymous, that they would be paid in private at the end of the experiment, and that the only documentation from the experiment that would contain their identity would be the receipt of payment, which would not contain their participant number. When a participant returned the signed consent form, he was given a participant number. Each participant number was generated by the experimental software prior to the start of the experimental session and was linked to either the control group or one of the two treatment groups. Participant numbers were handed out randomly to the participants as they returned their consent forms. As the participants used their participant numbers to log into the experimental software, the software automatically took them to the appropriate experimental condition for that participant number. In each experimental session, all participants started the experiment at the same time. Participants were not allowed to communicate or interact with each other at any point during the experiment.

Written instructions were provided to the participants through a series of instructional and tutorial screens via the computer terminal. I read the instructions to the participants after they logged into the experimental software, but prior to the start of the experiment. As the instructions were read, participants followed along on their computer screens. I read the

¹ An outside programmer was utilized to design and write software for the specific purpose of conducting this research.

instructions exactly as they were written on the computer screen. As I read the instructions to the participants, I allowed time for them to interact with the tutorial screens. The tutorial screens were the same as the screens that the participants interacted with during the experiment, and they were required to interact with the tutorial screens in the same manner that they interacted with the actual experimental screens so that they were familiar and comfortable with the experimental user interface. After I completed the reading of the instructions, participants were allowed to navigate forward and backwards through the instructional and tutorial screens until they felt comfortable with the rules of the experiment and the user interface. At this point, they were required to indicate that they understood the instructions and were ready to begin the experiment. The software did not start the experimental session for any participant until all participants had indicated that they understood the instructions and were ready to start the experiment. The use of written, computerized instructions ensures that participants across all experimental sessions receive the same information, which may not be true when instructions are given in a verbal format only (Davis and Swenson 1988). Once participants started the experiment, they were not allowed to return to the instructional screens, nor were they given a copy of the instructions. Participants were notified prior to the start of the experiment that they would not be allowed to return to the instructional screens. By controlling access to the experimental instructions, I was able to control the number of times and the manner in which participants were reminded that they faced a possibility of audit.

Participants were instructed that the experiment would continue for an unknown number of periods, although the total number of periods for all experimental sessions was predetermined at 20 periods. At the beginning of each experimental round, participants were asked to proofread a paragraph of text and identify and correct any grammatical or spelling errors in the passage.

Participants earned income at a rate of \$0.35 per error identified and properly corrected.

Participants did not earn income for errors identified, but corrected improperly, nor were they penalized for false errors identified. Participants were asked to proofread a new paragraph of text at the beginning of each period; however, all participants in all experimental conditions received the same paragraph of text in the same experimental period. Each paragraph contained five errors in total, so that the total potential earned income was \$1.75 per round or \$35.00 for the 20 experimental rounds. The number of errors in each paragraph was given in the experimental instructions. Subjects were given a maximum of two minutes to complete the proofreading task. They were able to submit the proofreading task before the two minutes had expired if they had completed the task. If they had not submitted the proofreading task at the end of the two minute period, they were automatically taken out of the proofreading task and directed to the next step in the experiment. Allowing subjects to earn income amounts that differ across subjects in each period and differ within subjects across periods may add some noise to the data, but it also increases parallelism in the experimental design (Alm and McKee 2006) and is a common design feature in tax compliance laboratory experiments (Alm 1991).

Once a subject submitted the proofreading task, the amount of income earned from the current period task was reported to the subject in an earnings report and added to the subject's electronic bank. The electronic bank was displayed in the top right hand corner of the subject's screen and reported both the cumulative earnings and the earnings for the current round.

Subjects then moved from the earnings report to the income tax return. In the control condition, subjects moved directly from the earnings task to the income tax return without any additional steps. In the traditional message treatment condition, after subjects left the earnings task, but before they were taken to the income tax return, subjects were taken to a screen that presented

the following message that reiterated the information provided to them in the instructions about audit probability and penalties:

REMINDER: The amount of income that you report may be audited for accuracy. If you are found to have reported less than your earned income, you will be required to pay the tax due on the unreported income plus a penalty equal to 1.5 times the unpaid taxes.

This message is similar to those used in prior research in that it simply provides the reader with a reminder that the risk of audit and penalties exists.

In the theory of reasoned action (TRA) message treatment condition, after subjects left the earnings task, but before they were taken to the income tax return, subjects were taken to a screen that displayed the following persuasive message designed based on the theory of reasoned action:

You can reduce your chances of incurring audit and penalties by honestly reporting 100% of your earned income. If you are audited and found to have reported less than your earned income, you will be required to pay the tax due on the unreported income plus a penalty equal to 1.5 times the unpaid taxes. If you have to pay penalties on underpaid taxes, you will be paying more out of pocket than if you had paid 100% of your true tax liability to begin with.

This message conforms to the theory of reasoned action by attempting to link the action of paying one's taxes honestly with the consequence of reducing one's audit risk and by emphasizing the negative aspects of incurring penalties. The goal of the message is to create a behavioral belief about the consequences of honest taxpaying, which would promote a more positive attitude towards honest tax reporting and to increase the negative evaluation of the outcome of incurring penalties, which would promote a more negative attitude toward dishonest

tax reporting. In both treatment conditions, the message appeared on the screen for 15 seconds before the navigation button that allowed the subjects to move to the next screen became available.

On the income tax return screen, subjects were asked to report an amount of income earned for the period and were given an estimate of the tax due on that income. Tax was calculated at a rate of 20% of reported income in all periods for all experimental conditions. The tax rate of 20% was chosen for its proximity to tax rates normally experienced in the United States tax system. After subjects finalized their income reporting, the tax due was calculated and deducted from the subjects' electronic banks. The instructions provided at the beginning of the experiment clearly stated that the amount of tax paid in any given period would be calculated based on the reported income rather than the earned income for that period and that subjects should feel free to try to earn as much money as possible during the course of the experiment.

The final step in each period of the experiment was the audit process. In the experimental instructions, subjects were informed that their reported earnings each period might be subject to an audit, that audits would be conducted privately through the computer interface so that only the individuals themselves would know whether they were audited and the results of any audits that occurred, that audit selection would be determined either randomly or based on a set of audit rules, and that if they were audited and had underreported their income, they would be required to pay the tax due on the unreported income plus a penalty equal to 1.5 times the amount of unpaid taxes. The actual selection procedures and true audit probabilities used in the experiment were intentionally left unknown to the subjects. This parallels the situation in the United States tax system in which taxpayers know that they may be subject to an IRS audit, but do not know the specific audit selection procedures or their true audit risk. Because the

hypotheses in this study are directed at individuals' reactions to persuasive messages assuming that they have not previously experienced a tax audit, observations that occurred in all periods after an individual experienced an audit were eliminated for the purposes of hypothesis testing. Subjects were notified that they had been selected for audit by a message screen that appeared after they finalized their income reporting, but before they were allowed to proceed to the next round. This message screen notified the subject that his reporting had been selected for audit and asked him to navigate to the next screen to see the results of the audit. The results of the audit were displayed on the next screen and any additional amounts due were deducted from the individual's electronic bank. The individual was then allowed to proceed to a wait screen for the next round of the experiment. Individuals who were not selected for audit were not explicitly told that they had not been selected for audit. They simply advanced to a wait screen for the next round after they finalized their income reporting in the current round. The audit procedure was designed to replicate the audit procedure in the United States tax system. In the United States tax system, taxpayers send their tax returns to the IRS and are notified by letter if they will be subject to an audit, but never receive official notification of the fact that their return has not been selected for audit.

The audit procedures are also designed to ensure that the participants remain blind to the audit probabilities and selection rules. The true audit probability in each period differed based on whether the individual reported the full amount of his earned income or underreported his earned income in that period. In a period in which he reported the full amount of his earned income, an individual was subject to a 1% chance of being audited. In a period in which he has underreported his earned income, an individual was subject to a 2% chance of audit. The audit probabilities for the periods were chosen to approximate the annual United States audit rates of 1

– 2% (Alm and McKee 2006). Once an individual submitted his final reported income, the experimental software compared the reported income to the recorded earned income to determine the appropriate audit probability and then randomly determined whether the individual would be subject to an audit or not.

The completion of the audit process marked the end of the experimental round. As the subjects completed each experimental round, they were taken to a screen asking them to wait until all participants had completed the round. The next experimental round began once all participants had completed the prior round. Each experimental round proceeded in the exact same manner as the prior periods. After the completion of all experimental rounds, subjects completed a risk game. The game was a version of the lottery choice game as published in Holt (2002). In this game, participants are presented with ten choices between pairs of gambles. The gambles were presented in a table of two columns and ten rows. For each row, participants chose whether they would rather play the gamble in the left column or the gamble in the right column. The potential payouts of the gambles in each column did not change on a row by row basis, but the likelihood of receiving each payout was different in each row, so that the expected utility of the gamble changed on a row by row basis. When participants checked out of the experiment, one of the rows was chosen at random and the participant played the gamble that he chose for that row. Participants were given a written copy of the instructions for the risk game and these instructions were read aloud to them prior to completing the game. A copy of the instructions and the gamble choices can be found in Appendix D. Participants also completed a series of attitude surveys and a demographics questionnaire. After the participants finished the demographics questionnaire, they were checked out of the experiment at which time they completed the risk game and were paid in cash for their participation. Participants were checked

out individually and payments were made in private. Payment was equal to the cumulative balance in their electronic bank at the end of the final experimental period plus the earnings from the risk game. The average payout was approximately \$26. Each experimental session lasted approximately one and a half hours from check-in to check-out. Experimental instructions and example experimental interface screens are included in Appendix C.

CHAPTER 4

ANALYSIS AND HYPOTHESIS TESTING

4.1 Independent, dependent, and control variables

4.1.1 *Independent and Dependent Variables*

This study examines the effect of a persuasive message treatment on tax reporting behavior both in a single time period and over multiple time periods. The independent variable of interest in this study is the experimental condition to which the subjects were assigned: control, traditional message group, or the TRA message group. The experimental round number is included to control for the effects of time, and the interaction of the messages with the round is included to examine the effects of the messages across time. All other factors in the experimental context, including tax rates, audit rates, and penalty rates, were held constant across all experimental conditions and all experimental periods.

The dependent variable of interest in this study is the level of tax compliance exhibited in the reporting decision. Strictly defined, reporting 100% of one's taxable income would be considered to be compliant behavior while reporting anything less than 100% of one's taxable income would be viewed as noncompliant behavior. However, the practical motivation for this study is the need to find a way to increase the collection of tax revenues and reduce (rather than totally eliminate) the tax gap. Consider a case where a persuasive message induces a taxpayer who would normally only report 60% of his true taxable income to instead report 90% of his true taxable income. In this case, the message would lead to increased collection of tax revenues and would be considered a successful message even though the taxpayer in question would still be considered to be noncompliant under a strict definition of tax compliance. Measuring tax compliance based on the strict definition will only detect differences in the number of people

who report 100% of their earned income and will not capture cases in which people report less than 100% of their earned income but still report a larger percentage of their earned income than they would have otherwise. Therefore, for the purposes of this study, tax compliance is measured as the percent of earned income reported in each experimental period.

4.1.2 Control Variables

A number of control variables were gathered and used for analysis. The control variables can be placed into three separate categories: demographics, tax experience, and attitudes and preferences. Prior research has found that gender (Jackson and Milliron 1986) and income level (Andreoni et al. 1998; Jackson and Milliron 1986) have a significant effect on tax compliance; therefore, I have included both gender and income level as control variables in the current study. Gender was measured as a dichotomous variable with 0 equal to male and 1 equal to female. As the majority of the participants were undergraduate students, income level was measured as their perception of their family's income in comparison (below average, average, or above average) to other students at the university where the study was conducted.

The experiment was placed in the tax context to increase parallelism with the real world context. The theory of reasoned action tells us that our experiences are important in shaping our behavioral beliefs within a specific context (Fishbein and Ajzen 1975; Ajzen and Fishbein 1980). Participants coming into the experiment who have experience with the tax context might then have differing behavioral beliefs about taxation that could affect their behavior in this study. Personal tax paying experience of the participants is presented in Table 2. The mean years having filed a tax return for the 51% of the sample that reported having filed a return was 2.85 years with standard deviation of 2.07. Only 4% of those individuals had ever been contacted by the IRS about their tax return. As there is not much variability in the taxpaying experience of the

sample, personal tax experience is measured as a dichotomous variable with 0 equal to having never filed a tax return and 1 equal to having filed a tax return at least once. In addition to personal experience, knowledge of the experiences of those around us can affect behavioral beliefs (Fishbein and Ajzen 1975; Ajzen and Fishbein 1980). Participants in the study who have knowledge of friends or family members being subject to an IRS audit may have different behavioral beliefs about compliant tax reporting. Twenty percent of the participants reported that they knew someone who had been subject to an IRS audit. These responses were included as a dichotomous variable with 0 equal to not knowing anyone who had been audited and 1 equal to knowing someone who had been audited.

In addition to experience, personal attributes might have an effect on an individual's tax reporting behavior. Risk preference is a personal attribute that has been found to have an effect on tax compliance in prior research (Christensen and Hite 1997; Fischer et al. 1992) and is relevant in the current experiment where the focus is on the individuals' behavioral beliefs about tax compliance and audit risk. Risk preference was tested using a lottery choice game as published in Holt (2002) and included in Appendix D. In the lottery choice game, participants were asked to make ten choices between two gambles. The gambles were presented in a table format with ten rows and two columns. For each row, participants chose whether they would prefer to play the gamble in the left column of the table or the gamble in the right column of the table. The potential payout for the gamble in the left column of the table was always either \$2.00 or \$1.60. The potential payout for the gamble in right column of the table was always either \$3.85 or \$0.10. For both columns, in the first row, the probability of receiving the higher payout was 10% and the probability of receiving the lower payout was 90%. In each subsequent row, the probability of receiving the higher payout increased by 10% and the probability of receiving

the lower payout decreased by 10%. For the first four rows, the expected utility of the gamble in the left column was higher than the gamble in the right column, and for the last six rows, the expected utility of the gamble in the right column was higher than the gamble in the left column; therefore, a completely risk neutral individual would choose the gamble in the left column for the first four rows and the gamble in the right column for the last six rows. Even though the expected utility of the gamble in the right column eventually exceeded that of the gamble in the left column, when an individual chose the gamble in the left column, he was guaranteed a payout of at least \$1.60 where the gamble in the right column could have led to a payout of only \$0.10. Risk preference is measured as the number of times the participant chose the gamble in the left column of the gamble choice table where the more times they chose the left column gamble indicates greater risk aversion.

The second personal attribute that was controlled for was a measure of honesty and personal values. This variable was measured using a subset of the ten most relevant questions from the Honesty and Personal Values Scale (Robinson et al. 1991). Scores can range from 10 to 30 with higher responses indicating less honest personal attributes. The items used from the Honesty and Personal Values Scale can be found in Appendix E.

Prior research provides evidence that normative beliefs and subjective norms are related to tax compliance behavior (Bobek et al. 2007; Jackson and Milliron 1986). The current study focuses on the behavioral beliefs related to tax compliance, but two variables were included to control for perceptions of normative beliefs and subjective norms. The first variable is a measure of the perception of the morality of tax compliance. This is a dichotomous variable that is equal to 0 if the participant indicated that he did not believe tax evasion to be morally wrong and 1 if the participant indicated that he did believe tax evasion to be morally wrong. The second

variable is a measure of civic attitudes using a subset of the ten most relevant questions from the Social Attitude Scale (Harris 1957; Robinson et al. 1991). Language in the questions was updated to current usage. The scale ranges from 10 to 50 with higher scores indicating greater attitudes of civic responsibility. The items used from the Social Attitude Scale can be found in Appendix E.

4.2 Hypothesis Testing

4.2.1 Tests of Hypotheses H1 and H3

I test H1 and H3 using the compliance dependent variable from the initial round of the experiment. H1 states that individuals that receive a message reminding them of the fact that the risk of an audit exists will behave more compliantly in the initial period that the message is received than individuals that do not receive the message. H3 states that individuals that receive a persuasive message based on the theory of reasoned action will behave more compliantly in the initial round that the message is received than individuals who receive only a reminder that an audit risk exists and individuals who receive no message at all. Descriptive statistics for round 1 compliance are presented in Table 3. I conduct tests of normality by creating a normal probability plot which is presented in Figure 3 and conducting a Kolmogorov-Smirnov test for normality. The Kolmogorov-Smirnov test is significant ($D = .475$, $p < .01$) which leads to rejection of the null hypothesis of normality. In addition, the dependent variable is measured as a proportion which does not allow for observations below 0 or above 1 and potentially creates a sigmoidal curve rather than a linear relationship (Long 1997). Examination of the normal probability plot reveals that there are no observations at the potential lower limit of 0, but a

substantial number of observations at the upper limit of 1.0. I test H1 and H3 using a Tobit model with an upper limit of 1.0 to better model the non-linear relationship².

I test H1 using four separate Tobit models. Each model includes the independent variables of interest and builds on the prior models by including additional control variables. The fourth model includes the independent variables and all control variables used in this study and is the most complete of the four models. In order to simplify the discussion of the results, I limit my discussion to the fourth and most complete model. All four models are presented in Table 4. The traditional message does not have a significant effect ($t = .027, p = .791$), which fails to support H1. In addition, none of the control variables are significant with the exception of the honesty and personal values scale which has a marginally significant negative effect ($t = -1.76, p = .082$), indicating that lower honesty and personal values have a negative effect on tax reporting.

To test H3, I run the four models including the TRA message group as the intercept rather than the control group. This allows me to compare the TRA message group to both the control group and the traditional message group. I present all four models in Table 5, but limit my discussion to the fourth model. Neither the coefficient on the control group ($t = -.74, p = .463$) or the traditional message group ($t = -.49, p = .622$) are significant which fails to support H3. The significance of the control variables is the same as in the model with the control group as the intercept.

4.2.2 Tests of Hypotheses H2 and H4

I test H2 and H4 using all observations from the 20 experimental rounds prior to which the individual had not experienced an audit. H2 states that over time, compliance of individuals

²I verify the results of the Tobit regressions by redefining the dependent variable as a dichotomous measure where 1 = 100% earned income reported and 0 = anything less than 100% earned income reported and running a Probit model. The results of the Probit model do not differ from the results of the Tobit model.

who receive a message reminding them that an audit risk exists and do not incur an audit will not differ from individuals who do not receive the message. H4 states that over time, individuals who receive a persuasive message based on the theory of reasoned action and do not incur an audit will be more compliant than individuals who do not receive the persuasive message. Any observations after an individual experienced an audit are dropped for the purpose of this analysis. Mean compliance, standard deviation, and sample size for each round are presented in Table 6. A graph of mean compliance over time is presented in Figure 5. A Kolmogorov-Smirnov test for normality confirms that the data is not normally distributed ($D = 0.433$, $p < .01$) and the normal probability plot (Figure 4) presents a sigmoidal distribution with some censoring at the lower limit of 0 and substantial censoring at the upper limit of 1.0. In addition, the data is cross-sectional, longitudinal data. When multiple observations from the same subjects are included in the sample, the data set will include within subject correlation (Cameron and Trivedi 2009). I test the hypotheses by running random effects panel data Tobit models to control for the within subject correlation and to better model the non-linear relationship.

The results of the tests of H2 and H4 are presented in Table 7. The discussion of the results is limited to the fourth model. There is no main effect for either the traditional message ($z = .54$, $p = .590$) or the TRA message³ ($z = -.2$, $p = .843$), but there is a significant negative main effect for time ($z = -7.13$, $p = .000$) indicating that compliance decreases over time. The interaction between the traditional message and time has a significant positive effect ($z = 2.13$, $p = .033$) and the interaction between the TRA message and time is marginally significant and positive ($z = 1.70$, $p = .088$). These interaction effects indicate that the messages reduce the severity of the decrease in compliance over time. In addition, gender is marginally significant (z

³ I also run the models with the TRA message group as the intercept to examine differences between the TRA message group and the traditional message group. No significant differences are found.

= 1.83, $p = .067$) indicating greater tax compliance for females over males. Risk aversion has a significant positive effect ($z = 2.33$, $p = .02$), and lower honesty and personal values has a significant negative effect ($z = -3.84$, $p = .000$). The significant interaction effect for the traditional message and time leads to rejection of the hypothesis of no effect for the traditional message over time (H2) while the marginally significant interaction effect for the TRA message and time provides partial support for H4⁴.

As additional tests of H2 and H4, I relax the assumption of the Tobit model that both the probability that an individual will be 100% compliant and the proportion of income reported by non-compliant individuals is controlled by the same mechanism and test the hypotheses using both a Probit model and a generalized least squares regression (Cameron and Trivedi 2009). The random effects Probit model is run with a dichotomous dependent variable of 1 = 100% of earned income reported and 0 = anything less than 100% of earned income reported. The Probit model allows me to examine the effects of the variables on the probability that an individual would report 100% of their earned income. For the random effects generalized least squares model the compliance variable is measured as the proportion of earned income that was reported. Only those observations where the proportion was less than 1.0 are included in the model. The general least squares model allows me to examine the effects of the variables on the proportion of income reported for individuals who underreported. For the Probit model and the generalized least squares model, I run the analysis on only the most complete model with the independent

⁴ Per examination of average compliance rates over time in Table 6, it appears that compliance rates do not drop as sharply each round after the first twelve rounds of the experiment. As a sensitivity analysis, I run the same model with the data from only the last eight rounds of the experiment. There is no longer a significant time effect ($z = -0.75$, $p = .453$) or a significant interaction effect for time and the TRA message group ($z = -0.84$, $p = .403$). There is a marginally significant positive interaction effect for the time and the traditional message group ($z = 1.65$, $p = .099$). Income level is marginally significant and negative ($z = -1.95$, $p = .051$), risk preference is significant and positive ($z = 2.33$, $p = .02$), and lower honesty and personal values is significant and negative ($z = -3.69$, $p = .000$).

variables of interest and all control variables included. The results of these tests are presented in Table 8.

The results of the Probit model are similar to those of the Tobit model. There is no main effect for either the traditional message ($z = .42, p = .672$) or the TRA message ($z = -.6, p = .552$), but there is a negative main effect for time ($z = -4.47, p = .000$), indicating that the probability that an individual will report 100% of their earned income decreases over time. Both the interaction effect for the traditional message group and time ($z = 3.11, p = .002$) and the TRA message group and time ($z = 2.23, p = .026$) are positive and significant indicating that the messages have a positive impact on the severity of the over time reduction in the probability that an individual will report 100% of their income. Gender has a marginally significant positive effect ($z = 1.87, p = .061$), risk aversion has a significant positive effect ($z = 2.37, p = .018$), and lower honesty and personal values has a significant negative effect ($z = -3.85, p = .000$).

The results of the random effects generalized least squares model differ from both the Tobit model and the Probit model, suggesting that the messages have a different effect on the amount of income reported for individuals who reported less than 100% of their earned income than they do on the probability that an individual will report 100% of their earned income. There is no significant main effect for the traditional message ($z = .44, p = .658$) in this test, but there is a significant positive main effect for the TRA message ($z = 2.55, p = .011$), suggesting that receiving the TRA message increases the amount of earned income reported when an individual reports less than 100% of their income. The main effect for time is negative and significant ($z = -7.20, p = .000$) in this model as well, indicating that over time, individuals who are reporting less than 100% of their income report a smaller proportion of their earned income. Neither the interaction of the traditional message and time ($z = -.84, p = .403$) or the TRA message and time

($z = -.65$, $p = .514$) are significant suggesting that the messages do not have an effect on the magnitude of the reduction in proportion of income reported over time. Income is significant and negative ($z = -2.21$, $p = .027$) indicating the higher the perception of family income levels, the lower the proportion of income reported when an individual reports less than 100% of their income. Knowledge of someone who has experienced an IRS audit is positive and marginally significant ($z = 1.92$, $p = .054$) indicating that individuals who know someone who had been audited by the IRS report a greater proportion of their income if they are reporting less than 100%. Tax morality is positive and marginally significant ($z = 1.65$, $p = .099$) providing some evidence that individuals who believe tax evasion to be immoral report a greater proportion of their earned income when reporting less than 100%.

4.2 Additional Analysis

To examine whether including observations after an individual was audited changes the results, I run the random effects panel two limit Tobit⁵ models with all observations included. Mean compliance and standard deviation for each round with all observations included are presented in Table 9. A graph of mean compliance over time with post audit data included is presented in Figure 6. A Kolmogorov-Smirnov test confirms that the data is not normally distributed ($D = .422$, $p < .01$), and the normal probability plot (Figure 7) confirms that the distribution has some censoring at a lower limit of 0 and substantial censoring at an upper limit of 1. Results for all four models tested are presented in Table 10, but I limit my discussion to the fourth and most complete model.

⁵ In untabulated results, I examine the effect of experiencing an audit during the experiment directly by including an indicator variable for rounds after audit (1 if the participant has been audited in any previous round and 0 otherwise) in the full Tobit model with the post audit data included. I find that audit has a negative and significant ($z = -2.07$, $p = .039$) impact on compliance. This result is consistent with results in Alm and McKee (2004).

The results do not change substantially when the post audit data is included in the model⁶. There are no main effects for either the traditional message ($z = .00$, $p = .998$) or the TRA message ($z = -.54$, $p = .592$), but there is a significant negative effect for time ($z = -8.67$, $p = .000$). The interaction effect for the traditional message and time is positive and significant ($z = 2.59$, $p = .01$), and the interaction effect for the TRA message and time is positive and marginally significant ($z = 1.95$, $p = .051$). There is a marginally significant positive effect for gender ($z = 1.93$, $p = .054$) and a significant negative effect for lower honesty and personal values ($z = -3.02$, $p = .003$). The one difference from the model with the post audit data dropped is that risk aversion is no longer significant when the post audit data is included in the model ($z = 1.59$, $p = .112$)⁷.

⁶ I report the results of the random effects Tobit model with the post audit data included. I also run random effects Probit models for all observations and generalized least squares models for only observations less than 1.0 with the post audit data included. The results of these models do not differ substantially from those that did not include the post audit data.

⁷ Per examination of Table 9, it appears that compliance rates do not drop as sharply each round after round twelve of the experiment. As an additional sensitivity analysis, I run the Tobit model for all observations for just the data from the last eight rounds of the experiment. There is no longer a significant time effect ($z = -0.99$, $p = .324$) or a significant interaction effect for time and the TRA message group ($z = -0.18$, $p = .859$). There is still a significant positive interaction effect for the time and the traditional message group ($z = 2.10$, $p = .035$). The only control variable that is significant in this test is lower honesty and personal values which still has a significant negative effect ($z = -3.86$, $p = .000$).

CHAPTER 5

CONCLUSION

5.1 Discussion and Implications

Consistent with Jackson and Jaouen (1989) and Hasseldine and Kaplan (1992), I do not find that reminding an individual of their audit risk increases their tax compliance in the initial period that they received the message. I also do not find that exposing individuals to a message based on the theory of reasoned action to try to shape behavioral beliefs about the consequences of compliant tax behavior has an effect on compliance in the initial period that the message is received. I do find, however, that when the messages are repeated over consecutive periods, the traditional message has a significant and the TRA message has a marginally significant counteractive effect on the overall across time decrease in compliance. As tax reporting is an activity that is repeated over an individual's lifetime, a message that does not increase tax compliance in the first period that it is received, but that has a positive impact on tax compliance across time may still be an effective tool in reducing the tax gap. These results illustrate the importance of examining the effects of programs to increase tax compliance over repeated periods.

I am able to separate the effects that the messages have on the probability that an individual will report 100% of their income and on the amount of income that an individual reports in periods in which he reports less than 100% by running separate Probit models on the entire data set and general least squares models on only observations in which an individual reports less than 100% of his income. Neither the traditional message or the TRA message have an overall effect on the probability that an individual will report 100% of his income, but the traditional message reduces the severity of the decrease over time in the probability that an

individual will report less than 100% of this income. The TRA message has a marginally significant counteractive impact on the decrease in probability over time. In contrast, the TRA message has a significant overall impact on the amount of income that is reported when an individual reports less than 100% of this income. The traditional message does not have an effect on the amount of income that is reported when an individual reports less than 100% of his income. Neither of the message by time interactions are significant in this model. The results of the two part analysis suggest that the messages have differing impacts on the probability of an individual reporting 100% of his income and the amount of income that he reports when he reports less than 100%. Both an increase in the probability of reporting 100% and an increase in an amount of income reported, even if it is less 100%, can be useful in reducing the tax gap thus making it important to consider both impacts when evaluating the usefulness of these messages. This also suggests that in the effort to reduce the tax gap, a program which includes multiple types of messages might be the most effective.

Additional analyses that do include observations after an individual was audited do not yield substantially different results. Contrary to expectations, this suggests that the presence or absence of an audit to reinforce the content of the message does not have an impact on the effectiveness of the message. Because of the low audit rates in the current study, only approximately 14% of the total observations occurred after audit. It might be fruitful in future research to design a study that would provide a more even percentage of post audit observations across treatment groups to more closely examine the effects of both the messages and audit and the interactions of the messages and audit on tax compliance.

I find that several of the control variables utilized in the study are correlated with tax compliance behavior in a manner consistent with prior tax compliance research. Specifically, I

find that being female (Jackson and Milliron 1986), higher levels of risk aversion (Fischer et al. 1992), and greater levels of honesty and personal values (Hanno and Violette 1996; Bobek and Hatfield 2003) are all positively correlated with tax compliance. The consistency of these results with prior research helps to further validate the use of students in a laboratory experiment to examine tax compliance issues.

5.2 Contributions and Limitations

I conduct a study that examines the effectiveness of two types of sanction based persuasive messages on levels of tax compliance both in the initial period that the messages are received and over repeated periods. This study contributes to the current tax compliance literature in three important ways. First, the prior literature has examined the effectiveness of sanction based persuasive messages in the initial period that the messages are received, but to my knowledge, no prior study has examined the effectiveness of persuasive messages over repeated periods. Secondly, in this study, I incorporate findings from prior tax compliance research to test the effectiveness of a sanction based persuasive message designed based on the theory of reasoned action. Finally, much of the prior research on the effectiveness of persuasive messages in increasing tax compliance has been conducted using survey type instruments in which the participants have no financial incentive to admit that they would behave non-compliantly. This study contributes to the research by providing an additional test of the effectiveness of sanction based persuasive messages using a laboratory experiment in which participants are subject to direct financial incentives similar to those experienced when making real-world compliance decisions.

Like all research, this study is subject to limitations. The main limitation of this study is that, while measures were taken to increase parallelism between the laboratory and the real world

context, the laboratory is an artificial environment and is not able to incorporate all aspects of the real world context. One major instance in this study in which the laboratory environment differs from the real world context is in the timing of the tax payment, audit notification, and repetition of the taxpaying cycle. A process that is spread out over years in the real world is condensed into a series of periods that can be completed within an hour to an hour and a half in the laboratory.

In addition, all experimental research is subject to design flaws. One such flaw in the current study is that the last section of the instructions before the participants started the first experimental round contained the audit risk information, potentially making the audit risk extremely salient for all groups going into the first round. This may have contributed to the insignificant results for round 1. It is possible the lack of results for the initial round is an artifact of the proximity of the audit risk information in the instructions to the initial round messages.

5.3 Future Research

In the current study, I use the theory of reasoned action to design a persuasive message aimed at increasing tax compliance, but I have chosen to focus on only the behavioral beliefs and behavioral attitude determinants of behavioral intentions. Prior research has shown that normative beliefs affect tax compliance (Bobek and Hatfield 2003; Bobek et al. 2007; Jackson and Milliron 1986; Hanno and Violette 1996; Wenzel 2004). Future research could use this same framework to design messages targeted at the social normative determinant of behavioral intention or messages designed to address both the behavioral attitude and social normative factors simultaneously and to examine whether the different types of messages have different effects on the probability of engaging in 100% compliance versus the amount of non-compliance

an individual engages in. In addition, future research could examine whether individuals with different attitudes and preferences are impacted differently by different types of messages.

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Appendix A Figures

Figure 1: The Theory of Reasoned Action

Adapted from Fishbein, M., and I. Ajzen. 1975. *Belief, Attitude, Intention, and Behavior: An Introduction to Theory and Research*. Addison-Wesley Publishing Company, Inc.

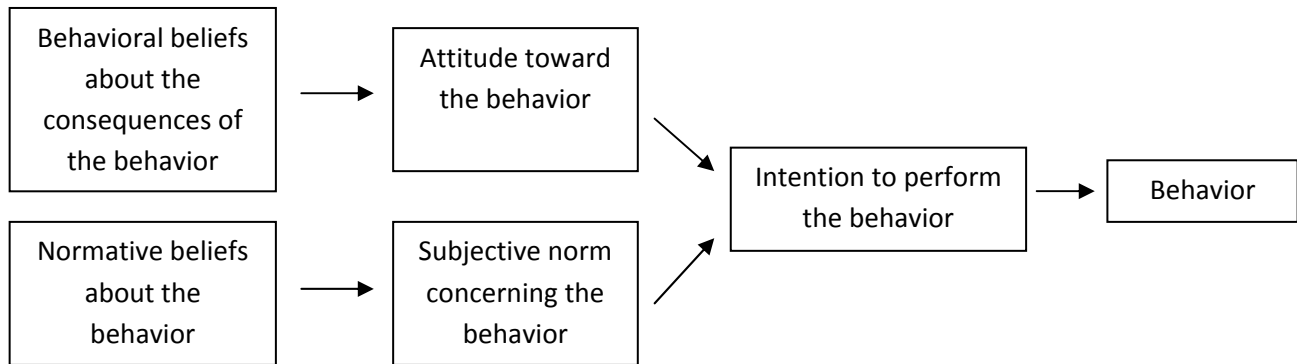
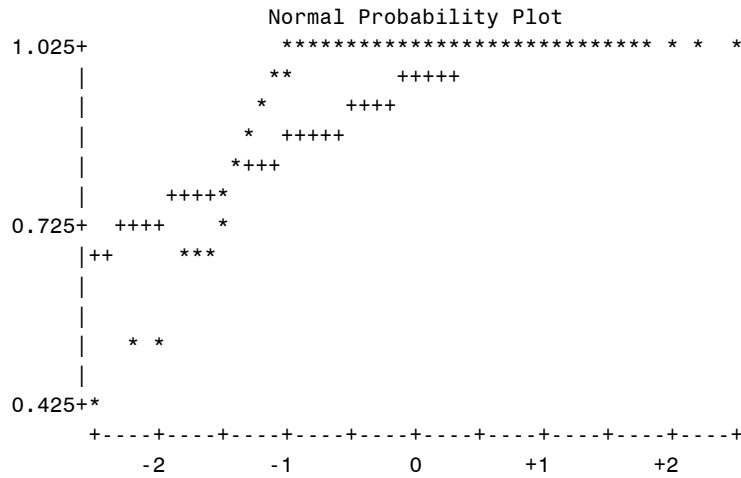


Figure 2: Action-Outcome Matrix

Adapted from Einhorn, H. J. 1982. Learning from Experience and Suboptimal Rules in Decision Making. In *Judgment under uncertainty: Heuristics and biases*, edited by D. Kahneman, P. Slovic and A. Tversky. Cambridge, MA: Cambridge University Press.

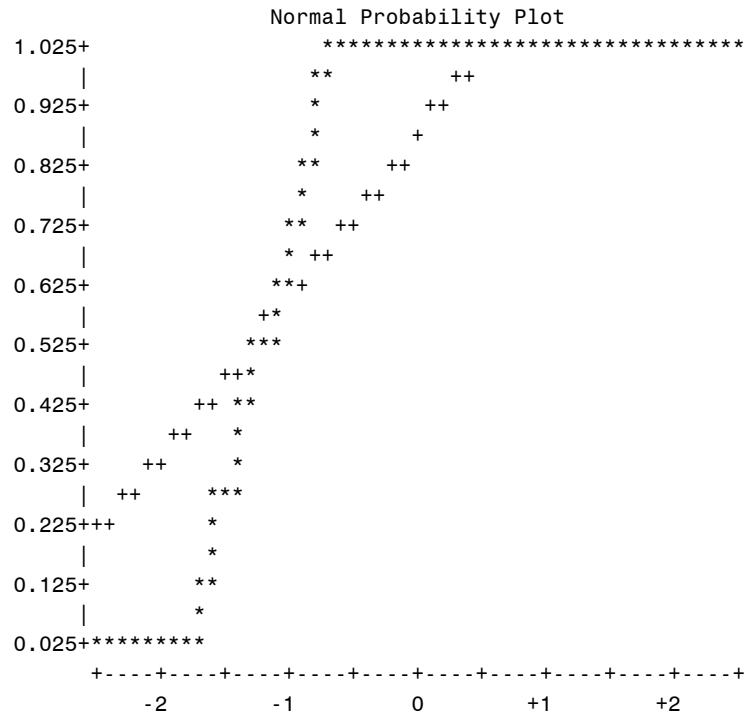
	<u>Acceptance of the Decision</u>	<u>Rejection of the Decision</u>
	<u>Rule</u>	<u>Rule</u>
Outcome “Success”	Positive Hits	False Negatives
Outcome “Failure”	False Positives	Negative Hits

Figure 3: Normal Probability Plot for Round 1 Compliance



The above graph plots the ordered values for the Compliance dependent variable (y-axis) against the quantiles of the normal distribution (x-axis). Compliance is measured as the proportion of earned income that is reported. The ++ line represents the expected shape of the distribution if it were normally distributed. The ** line represents the values of the variable. Each * can represent up to 29 data points.

Figure 4: Normal Probability Plot for Compliance with Post Audit Data Dropped



The above graph plots the ordered values for the Compliance dependent variable (y-axis) against the quantiles of the normal distribution (x-axis). Compliance is measured as the proportion of earned income that is reported. Data for all 20 rounds is included, but observations after an individual was audited are not included in the plot. The ++ line represents the expected shape of the distribution if it were normally distributed. The ** line represents the values of the variable. Each * can represent up to 29 data points.

Figure 5: Mean Compliance over Time with Post Audit Data Dropped

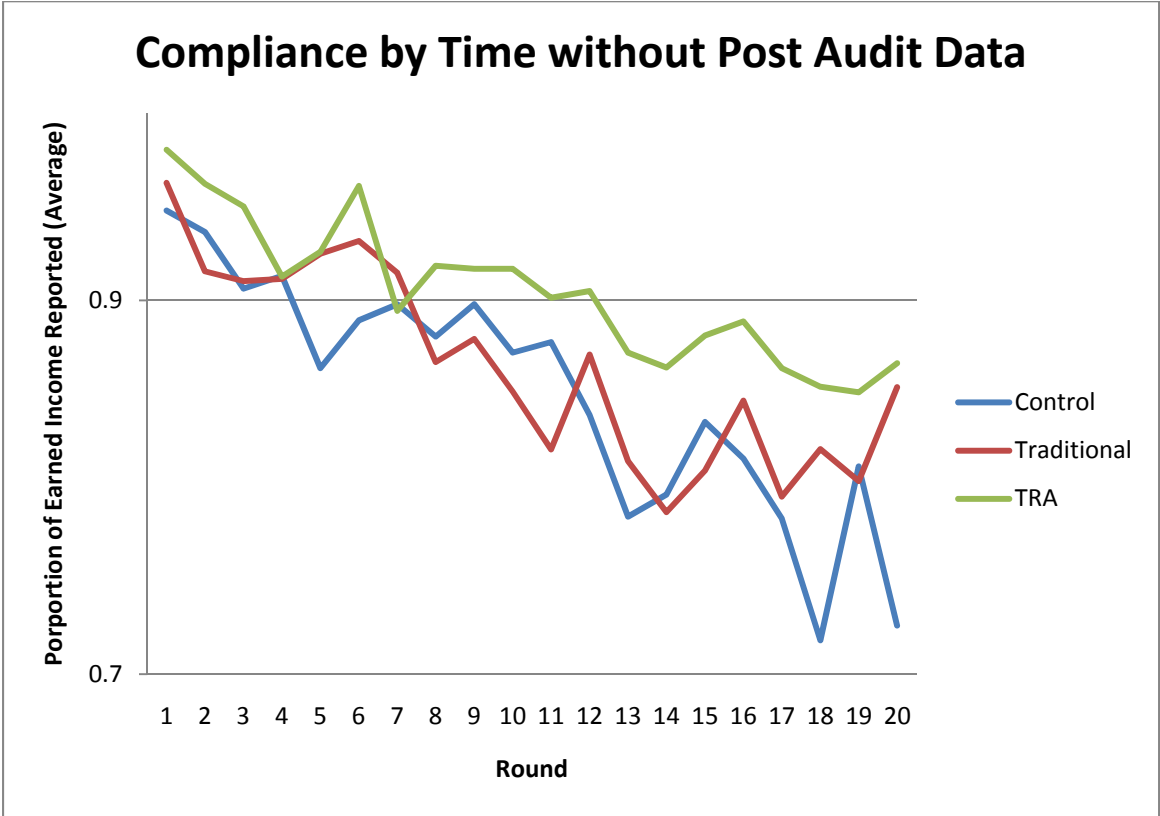


Figure 6: Mean Compliance over Time with Post Audit Data Included

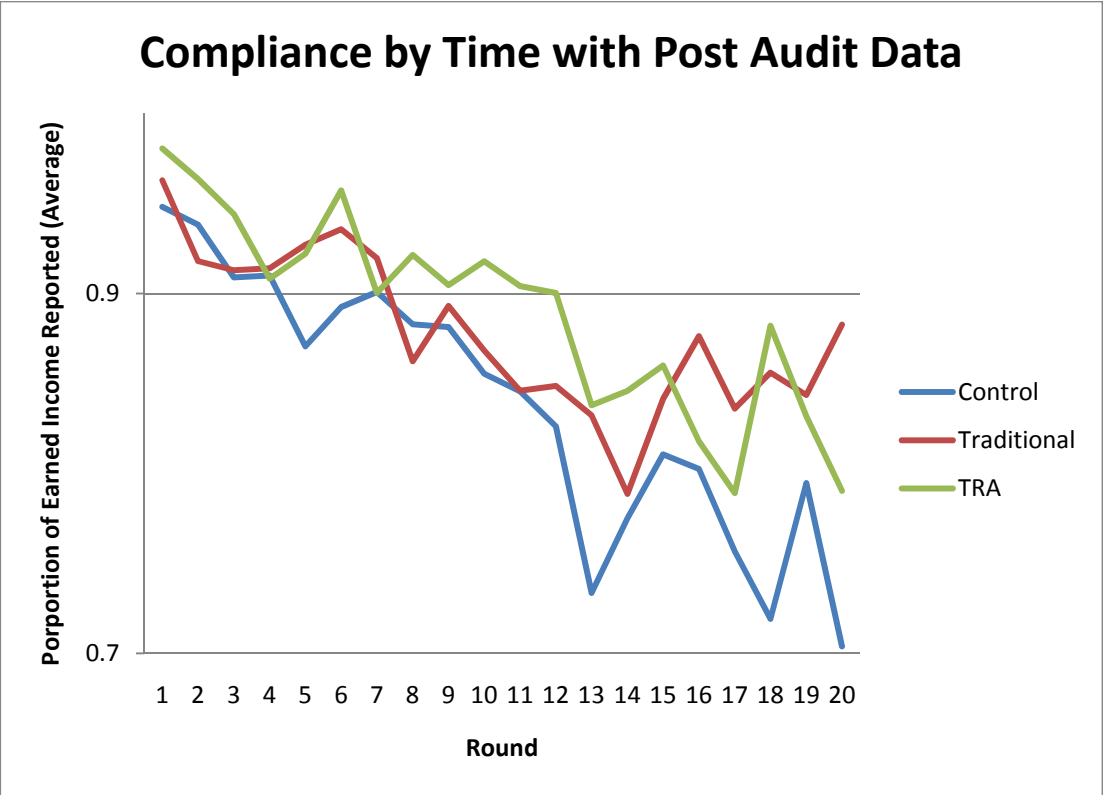
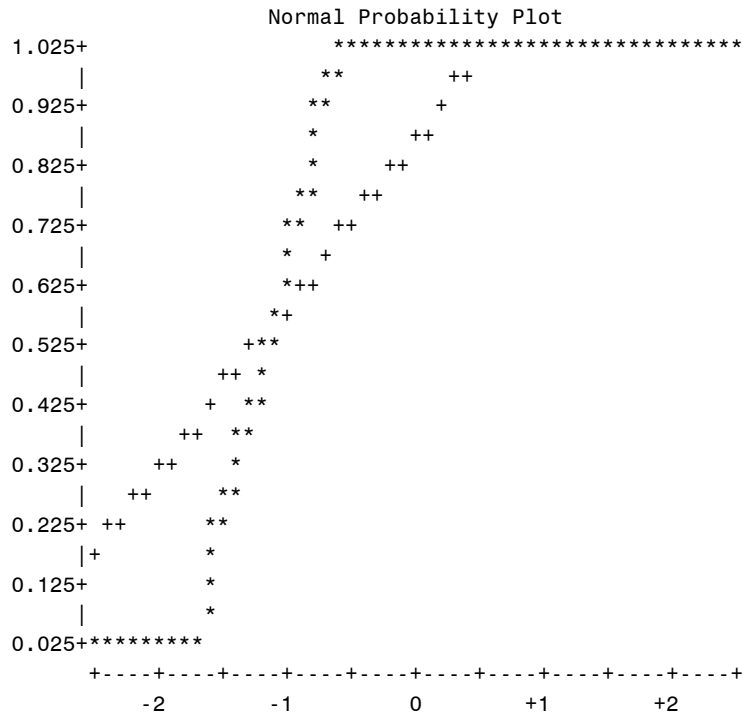


Figure 7: Normal Probability Plot for Compliance with Post Audit Data Included



The above graph plots the ordered values for the Compliance dependent variable (y-axis) against the quantiles of the normal distribution (x-axis). Compliance is measured as the proportion of earned income that is reported. Data from all 20 rounds and all observations after audit are included. The ++ line represents the expected shape of the distribution if it were normally distributed. The ** line represents the values of the variable. Each * can represent up to 29 data points.

Appendix B Tables

Table 1: Summary of Participant Demographics

Category	Response	Category	Response
Gender		Major	
Male	55%	Business	60%
Female	45%	Science/Engineering	15%
		Liberal Arts	5%
Age		Economics	3%
18-19	62%	Other	17%
20-21	28%		
22-25	10%	GPA	
		Below 2.0	1%
		2.0 - 2.4	6%
Family Income (relative to other students)		2.5 - 2.9	20%
Below		3.0 - 3.4	39%
Average	15%	3.4 - 3.9	32%
Average	59%	4.0	2%
Above			
Average	26%		

N = 105

Table 2: Summary of Participant Taxpaying Experience

Category	Response	Category	Response
Tax Return Experience (N=105)		Years Filed (N=54)	
Filed	51%	Median	2
Never Filed	49%	Mean	2.85
		Std Dev	2.07
Preparer (N=54)			
Paid Preparer	11%		
Self-Prepared	15%		
Friend/Family	74%		

Table 3: Descriptive Statistics for Round 1 Compliance

	N	Minimum	Maximum	Mean	Std Dev.
Control Group	36	0.4	1.0	0.948	0.146
Traditional Message Group	34	0.5	1.0	0.963	0.106
TRA Message Group	34	0.67	1.0	0.981	0.063
Total	104	0.4	1.0	0.964	0.11

Table presents observed minimum, maximum, and mean values for Compliance for the first round of the experiment. Compliance is the proportion of earned income that is reported. One participant in the control group earned \$0 income in round 1 and is treated as a missing data point.

Table 4: Test of the Effect of the Traditional Message Group on Compliance in the Initial Round (H1) - Right Censored Tobit Models

Variables	(1) Compliance	(2) Compliance	(3) Compliance	(4) Compliance
<u>Main Independent</u>				
Traditional Message Group (1 = Traditional Message Group)	-0.012 (.151)	-0.005 (.144)	0.015 (.142)	0.037 (.140)
TRA Message Group (1 = TRA Message Group)	0.062 (.156)	0.065 (.149)	0.064 (.143)	0.112 (.152)
<u>Demographics</u>				
Gender (1 = Female)		0.237* (.135)	0.209 (.133)	0.182 (.134)
Income Level		-0.154 (.100)	-0.161 (.100)	-0.151 (.104)
<u>Tax Experience</u>				
Tax Return (1 = has filed a tax return)			0.132 (.127)	0.143 (.131)
Audit Familiarity (1 = knows someone who has been audited)			0.039 (.153)	-0.041 (.159)
<u>Attitudes and Preferences</u>				
Risk				-0.004 (.045)
Morality of Tax Evasion (1 = believes that tax evasion is immoral)				-0.008 (.173)
Honesty and Personal Values				-0.052* (.030)
Civic Attitudes				-0.007 (.013)
Constant	1.43*** (.157)	1.48*** (.201)	1.41*** (.197)	2.57*** (.959)
Observations	104	104	104	104
Pseudo R-Squared	0.0035	0.0886	0.1077	0.1648

* Significant at $p < .1$, **Significant at $p < .05$, ***Significant at $p < .01$

Table presents the results (parameter estimates with standard errors in parentheses) of Tobit

regressions on the observations of the dependent variable in the first round of the experiment. The dependent variable of Compliance is the proportion of earned income that is reported. Traditional message group is 1 if the participant was in the traditional message group and 0 otherwise. TRA message group is 1 if the participant was in the TRA message group and 0 otherwise. Gender is 0 for male and 1 for female. Income level is measured on a scale of 1 to 3 whether the participant indicated their family income was below average, about average, or above average when compared to other students at the university at which the study was conducted. Tax return is 0 if the participant has never filed a US income tax return and 1 if the participant has filed a US income tax return. Audit familiarity is 0 if the participant reported that they did not know anyone who had been subject to an IRS audit and 1 if they reported that they did know someone who had been subject to an IRS audit. Risk is measured on a scale of 0 to 10 with higher values indicating greater risk aversion. Morality of Tax Evasion is 0 if the participant indicated that they did not believe that tax evasion was immoral and 1 if they indicated that they did believe tax evasion was immoral. Honesty and Personal Values are measured on a scale ranging from 10 to 30 where higher values indicate less honest attitudes. Civic attitudes are measured on a scale of 10 to 50 where higher values indicate greater attitudes of social responsibility. One participant earned \$0 income in round 1 and is treated as a missing data point for this test.

Table 5: Test of the Effect of the TRA Message Group on Compliance in the Initial Round (H3) - Right Censored Tobit Models with TRA Message Group as Intercept

Variables	(1) Compliance	(2) Compliance	(3) Compliance	(4) Compliance
<u>Main Independent</u>				
Control Group (1 = Control Group)	-0.062 (.156)	-0.065 (.149)	-0.064 (.145)	-0.112 (.153)
Traditional Message Group (1 = Traditional Message Group)	-0.074 (.156)	-0.07 (.149)	-0.048 (.146)	-0.075 (.152)
<u>Demographics</u>				
Gender (1 = female)		0.237* (.135)	0.209 (.133)	0.182 (.134)
Income Level		-0.154 (.100)	-0.161 (.100)	-0.151 (.104)
<u>Tax Experience</u>				
Tax Return (1 = has filed a tax return)			0.132 (.127)	0.143 (.131)
Audit Familiarity (1 = knows someone who has been audited)			0.039 (.153)	-0.041 (.159)
<u>Attitudes and Preferences</u>				
Risk				-0.004 (.045)
Morality of Tax Evasion (1 = believes that tax evasion is immoral)				-0.007 (.173)
Honesty and Personal Values				-0.052* (.030)
Civic Attitudes				-0.007 (.013)
constant	1.49*** (.161)	1.55*** (.206)	1.47*** (.202)	2.68*** (.982)
Observations	104	104	104	104
Pseudo R-Squared	0.0035	0.0886	0.1077	0.1648

* Significant at $p < .1$, **Significant at $p < .05$, ***Significant at $p < .01$

Table presents the results (parameter estimates with standard errors in parentheses) of Tobit regressions on the observations of the dependent variable in the first round of the

experiment. The dependent variable of Compliance is the proportion of earned income that is reported. Control group is 1 if the participant was in the control group and 0 otherwise. Traditional message group is 1 if the participant was in the Traditional message group and 0 otherwise. Gender is 0 for male and 1 for female. Income level is measured on a scale of 1 to 3 whether the participant indicated their family income was below average, about average, or above average when compared to other students at the university at which the study was conducted. Tax return is 0 if the participant has never filed a US income tax return and 1 if the participant has filed a US income tax return. Audit familiarity is 0 if the participant reported that they did not know anyone who had been subject to an IRS audit and 1 if they reported that they did know someone who had been subject to an IRS audit. Risk is measured on a scale of 0 to 10 with higher values indicating greater risk aversion. Morality of Tax Evasion is 0 if the participant indicated that they did not believe that tax evasion was immoral and 1 if they indicated that they did believe tax evasion was immoral. Honesty and Personal Values are measured on a scale ranging from 10 to 30 where higher values indicate less honest attitudes. Civic attitudes are measured on a scale of 10 to 50 where higher values indicate greater attitudes of social responsibility. One participant earned \$0 income in the first round and is treated as a missing data point for this test.

Table 6: Compliance by Round with Post Audit Observations Dropped

Round #		Message Group			
		Control	Traditional	TRA	All
1	Mean	0.9481	0.9629	0.9806	0.9636
	Std Dev	(.1457)	(.1055)	(.0623)	(.1105)
	N	36	34	34	104
2	Mean	0.9366	0.9156	0.9624	0.9379
	Std Dev	(.1430)	(.2053)	(.0922)	(.154)
	N	35	34	33	102
3	Mean	0.9063	0.9103	0.9503	0.9216
	Std Dev	(.1985)	(.2297)	(.1306)	(.1910)
	N	35	34	32	101
4	Mean	0.9132	0.9115	0.9127	0.9124
	Std Dev	(.2194)	(.2108)	(.2215)	(.2149)
	N	34	34	30	98
5	Mean	0.8638	0.925	0.926	0.9041
	Std Dev	(.2673)	(.1809)	(.1468)	(.2068)
	N	34	34	30	98
6	Mean	0.8894	0.9318	0.9613	0.9265
	Std Dev	(.2637)	(.1655)	(.1098)	(.1929)
	N	33	33	30	96
7	Mean	0.8979	0.9148	0.8944	0.9027
	Std Dev	(.2625)	(.2283)	(.1866)	(.2273)
	N	33	33	29	95
8	Mean	0.8806	0.867	0.9186	0.8876
	Std Dev	(.2586)	(.2819)	(.2083)	(.2512)
	N	31	33	29	93
9	Mean	0.898	0.8794	0.9169	0.8977
	Std Dev	(.2525)	(.2547)	(.1693)	(.2278)
	N	30	31	29	90
10	Mean	0.8721	0.8513	0.9169	0.8794
	Std Dev	(.2119)	(.3379)	(.1765)	(.2528)
	N	29	31	29	89
11	Mean	0.8777	0.8203	0.9014	0.8661
	Std Dev	(.2528)	(.3487)	(.2283)	(.2808)
	N	30	30	29	89
12	Mean	0.8387	0.871	0.905	0.8708
	Std Dev	(.2667)	(.2913)	(.2445)	(.27)

	N	30	30	28	88
13	Mean	0.7843	0.814	0.8721	0.8224
	Std Dev	(.3584)	(.3461)	(.2837)	(.3302)
	N	30	30	28	88
14	Mean	0.7961	0.7867	0.8641	0.8144
	Std Dev	(.3245)	(.3744)	(.3002)	(.3336)
	N	28	30	27	85
15	Mean	0.835	0.809	0.8812	0.8404
	Std Dev	(.309)	(.3604)	(.2566)	(.3110)
	N	28	29	26	83
16	Mean	0.8154	0.8464	0.8888	0.8493
	Std Dev	(.3111)	(.3231)	(.265)	(.2994)
	N	28	28	26	82
17	Mean	0.7833	0.795	0.8638	0.8132
	Std Dev	(.3634)	(.3793)	(.3211)	(.3535)
	N	27	28	26	81
18	Mean	0.7181	0.8204	0.8538	0.798
	Std Dev	(.402)	(.386)	(.2934)	(.3641)
	N	26	28	26	80
19	Mean	0.8112	0.8033	0.8508	0.8215
	Std Dev	(.3219)	(.3835)	(.317)	(.3389)
	N	26	27	26	79
20	Mean	0.726	0.8536	0.8664	0.8168
	Std Dev	(.4)	(.3323)	(.3047)	(.3485)
	N	25	28	25	78

Compliance is the proportion of earned income that is reported. Table presents mean compliance by round for each experimental group with all observations after a participant is audited dropped from the data set. In rounds 1, 10, and 19 a participant earned \$0 income and is treated as a missing data point.

Table 7: Tests of the Effect of Message Group on Compliance Across All Rounds (H2 and H4) - Random Effect Panel Data Two Limit Tobit Models

Variables	(1) Compliance	(2) Compliance	(3) Compliance	(4) Compliance
<u>Main Independent</u>				
Traditional Message Group (1 = Traditional Message Group)	0.014 (.249)	0.035 (.250)	0.065 (.251)	0.135 (.250)
TRA Message Group (1 = TRA Message Group)	-0.051 (.256)	-0.028 (.256)	-0.005 (.255)	-0.05 (.251)
Time	-0.039*** (.005)	-0.039*** (.005)	-0.039*** (.005)	-0.039*** (.005)
Traditional * Time	0.016** (.008)	0.016** (.008)	0.016** (.008)	0.016** (.008)
TRA * Time	0.013* (.008)	0.013* (.008)	0.013* (.008)	0.013* (.008)
<u>Demographics</u>				
Gender (1 = female)		0.448** (.195)	0.453** (.198)	0.362* (.198)
Income Level		-0.438*** (.157)	-0.461*** (.157)	-0.246 (.157)
<u>Tax Experience</u>				
Tax Return (1 = has filed a tax return)			0.131 (.205)	0.031 (.201)
Audit Familiarity (1 = knows someone who has been audited)			0.271 (.258)	0.078 (.258)
<u>Attitudes and Preferences</u>				
Risk				0.179** (.077)
Morality of Tax Evasion (1 = believes that tax evasion is immoral)				-0.231 (.290)
Honesty and Personal Values				-0.176*** (.046)

Civic Attitudes -0.009
(.022)

constant 2.25*** (.189) 2.51*** (.272) 2.38*** (.290) 4.64*** (1.39)

Participants 105 105 105 105

Observations 1799 1799 1799 1799

Pseudo R-Squared 0.0636 0.0713 0.0725 0.0849

* Significant at $p < .1$, **Significant at $p < .05$, ***Significant at $p < .01$

Table presents the results (parameter estimates with standard errors in parentheses) of Tobit regressions on the observations of the dependent variable for all rounds of the experiment. Any observation after a participant is audited is dropped from the data set. The dependent variable of Compliance is the proportion of earned income that is reported. Traditional message group is 1 if the participant was in the Traditional message group and 0 otherwise. TRA message group is 1 if the participant was in the TRA message group and 0 otherwise. Time is measured as the round number and ranges from 1 to 20. Gender is 0 for male and 1 for female. Income level is measured on a scale of 1 to 3 whether the participant indicated their family income was below average, about average, or above average when compared to other students at the university at which the study was conducted. Tax return is 0 if the participant has never filed a US income tax return and 1 if the participant has filed a US income tax return. Audit familiarity is 0 if the participant reported that they did not know anyone who had been subject to an IRS audit and 1 if they reported that they did know someone who had been subject to an IRS audit. Risk is measured on a scale of 0 to 10 with higher values indicating greater risk aversion. Morality of Tax Evasion is measured as 0 if the participant indicated that they did not believe that tax evasion was immoral and 1 if they indicated that they did believe tax evasion was immoral. Honesty and Personal Values are measured on a scale ranging from 10 to 30 where higher values indicate less honest attitudes. Civic attitudes are measured on a scale of 10 to 50 where higher values indicate greater attitudes of social responsibility.

Table 8: Two Part Test of the Effect of Message Group on Compliance Across All Rounds (H2 and H4)

Variables	Probit Model	GLS Model
<u>Independent</u>		
Traditional Message Group (1 = Traditional Message Group)	0.257 (.608)	0.04 (.091)
TRA Message Group (1 = TRA Message Group)	-0.366 (.615)	0.231** (.091)
Time	-0.077*** (.017)	-0.02*** (.003)
Traditional * Time	0.070*** (.022)	-0.003 (.004)
TRA * Time	0.052** (.023)	-0.003 (.004)
<u>Demographics</u>		
Gender (1 = female)	0.881* (.471)	-0.032 (.077)
Income Level	-0.473 (.379)	-0.118** (.053)
<u>Tax Experience</u>		
Tax Return (1 = has filed a tax return)	0.251 (.480)	-0.116 (.079)
Audit Familiarity (1 = knows someone who has been audited)	-0.222 (.618)	0.178* (.092)
<u>Attitudes and Preferences</u>		
Risk	0.432** (.183)	0.02 (.027)
Morality of Tax Evasion (1 = believes tax evasion is immoral)	-0.687 (.701)	0.168* (.102)
Honesty and Personal Values	-0.422*** (.110)	-0.014 (.017)
Civic Attitudes	-0.023 (.052)	-0.002 (.008)

constant	8.48** (3.29)	0.894* (.516)
Participants	105	46
Observations	1799	439
Pseudo R-Squared	0.0493	0.3530

*significant at $p < .1$, ** significant at $p < .05$, ***significant at $p < .01$

Table presents the results (parameter estimates with standard errors in parentheses) of a Probit regression on the observations of the dependent variable for all rounds of the experiment where Compliance is 1 if the participant reported 100% of their earned income and 0 if the participant reported anything less than 100% of their earned income and a generalized least squares model that includes only observations where the participant reported less than 100% of their earned income and Compliance is the proportion of earned income that is reported. Any observation after a participant is audited is dropped from the data set. Traditional message group is 1 if the participant was in the Traditional message group and 0 otherwise. TRA message group is 1 if the participant was in the TRA message group and 0 otherwise. Time is measured as the round number and ranges from 1 to 20. Gender is 0 for male and 1 for female. Income level is measured on a scale of 1 to 3 whether the participant indicated their family income was below average, about average, or above average when compared to other students at the university at which the study was conducted. Tax return is 0 if the participant has never filed a US income tax return and 1 if the participant has filed a US income tax return. Audit familiarity is 0 if the participant reported that they did not know anyone who had been subject to an IRS audit and 1 if they reported that they did know someone who had been subject to an IRS audit. Risk is measured on a scale of 0 to 10 with higher values indicating greater risk aversion. Morality of Tax Evasion is measured as 0 if the participant indicated that they did not believe that tax evasion was immoral and 1 if they indicated that they did believe tax evasion was immoral. Honesty and Personal Values are measured on a scale ranging from 10 to 30 where higher values indicate less honest attitudes. Civic attitudes are measured on a scale of 10 to 50 where higher values indicate greater attitudes of social responsibility.

Table 9: Compliance by Round with Post Audit Observations Included

Round #	Message Group			
	Control (N=37)	Traditional (N=34)	TRA (N=34)	All (N=105)
1	0.9481 (.1456)	0.9629 (.1055)	0.9806 (.0623)	0.9636 (.1104)
2	0.9383 (.1413)	0.918 (.2027)	0.9636 (.0911)	0.9397 (.1521)
3	0.9089 (.1963)	0.9129 (.2268)	0.9441 (.1346)	0.9216 (.1891)
4	0.91 (.2163)	0.914 (.2082)	0.9082 (.2127)	0.9108 (.2104)
5	0.8706 (.2611)	0.9271 (.1786)	0.9221 (.1525)	0.9061 (.2035)
6	0.8925 (.2537)	0.9357 (.1613)	0.9573 (.1124)	0.9279 (.1869)
7	0.9008 (.2527)	0.9197 (.2224)	0.9003 (.18)	0.907 (.2192)
8	0.8829 (.2501)	0.8623 (.2791)	0.9215 (.2)	0.8886 (.244)
9	0.8814 (.2774)	0.8931 (.2424)	0.9047 (.1793)	0.8929 (.2354)
10	0.8554 (.2491)	0.8683 (.3210)	0.9179 (.1701)	0.8802 (.254)
11	0.8456 (.2873)	0.846 (.3283)	0.9041 (.2162)	0.8647 (.2803)
12	0.8261 (.2886)	0.8486 (.3135)	0.9003 (.2356)	0.8576 (.2806)
13	0.7336	0.8323	0.8379	0.8003

	(.3865)	(.3259)	(.2993)	(.3403)
14	0.775 (.3499)	0.7886 (.3783)	0.8459 (.3005)	0.8025 (.3429)
15	0.8106 (.3283)	0.8414 (.3349)	0.86 (.2587)	0.8369 (.3076)
16	0.8025 (.3207)	0.8763 (.2943)	0.8179 (.3302)	0.8321 (.3139)
17	0.7567 (.3698)	0.836 (.3481)	0.7891 (.3734)	0.7936 (.3619)
18	0.7192 (.3993)	0.856 (.3515)	0.8821 (.2627)	0.8175 (.3481)
19	0.7947 (.3461)	0.8436 (.3497)	0.8318 (.3293)	0.8228 (.3392)
20	0.7039 (.4019)	0.8827 (.3019)	0.7903 (.3502)	0.7914 (.3585)

Compliance is the proportion of earned income that is reported.
Table presents mean compliance (with standard deviation in parentheses) by round for each experimental group with all observations after a participant is audited included in the data set.

Table 10: Additional Analysis: Test of Effect of Messages on Compliance Across all Rounds with Post Audit Observations Included - Random Effect Two Limit Tobit Model

Variables	(1) Compliance	(2) Compliance	(3) Compliance	(4) Compliance
<u>Independent</u>				
Traditional Message Group (1 = Traditional Message Group)	-0.09 (.245)	-0.075 (.244)	-0.035 (.245)	0.001 (.236)
TRA Message Group (1 = TRA Message Group)	-0.141 (.251)	-0.137 (.246)	-0.115 (.005)	-0.128 (.239)
Time	-0.041*** (.005)	-0.041*** (.005)	-0.041*** (.005)	-0.041*** (.005)
Traditional * Time	0.018*** (.007)	0.018*** (.007)	0.018*** (.007)	0.018** (.007)
TRA * Time	0.013** (.007)	0.013** (.007)	0.013* (.007)	0.013* (.007)
<u>Demographics</u>				
Gender (1 = female)		0.430** (.189)	0.432** (.193)	0.358* (.186)
Income Level		-0.320** (.151)	-0.348** (.152)	-0.203 (.151)
<u>Tax Experience</u>				
Tax Return (1 = has filed a tax return)			0.215 (.198)	0.157 (.189)
Audited Familiarity (1 = knows someone who has been audited)			0.243 (.249)	0.121 (.243)
<u>Attitudes and Preferences</u>				
Risk				0.107 (.067)
Morality of Tax Evasion (1 = believes that tax evasion is immoral)				-0.152 (.278)
Honesty and Personal Values				-0.125*** (.041)

Civic Attitudes 0.012
(.021)

constant 2.25*** (.184) 2.40*** (.262) 2.25*** (.280) 3.25** (1.27)

Participants 105 105 105 105

Observations 2096 2096 2096 2096

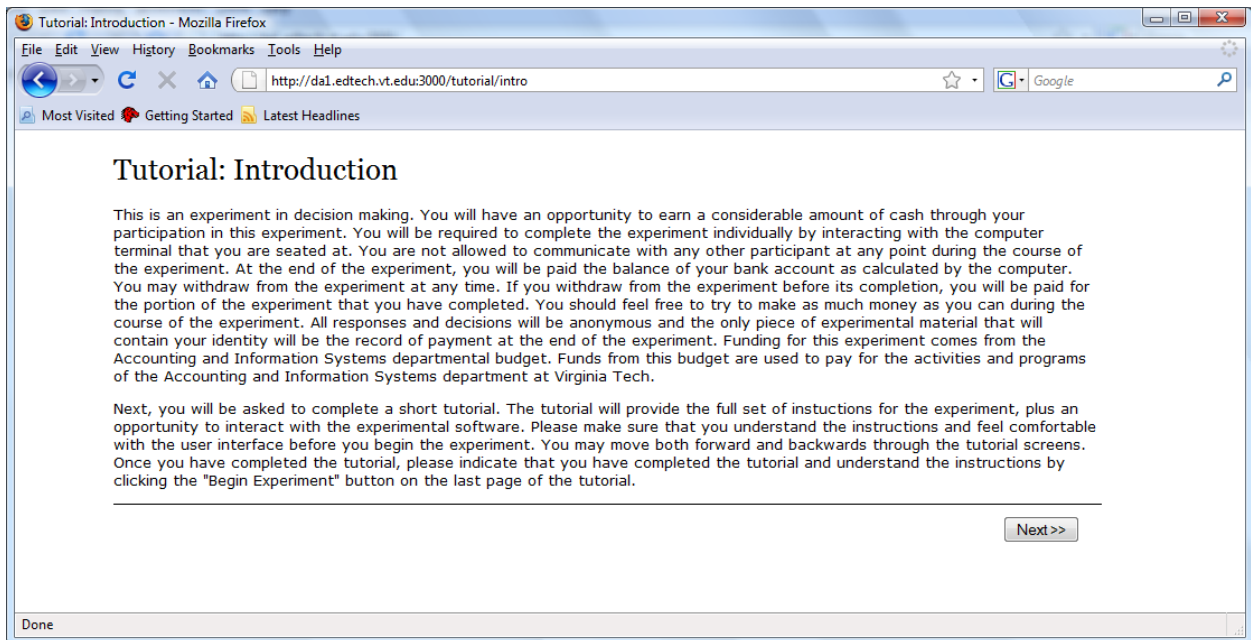
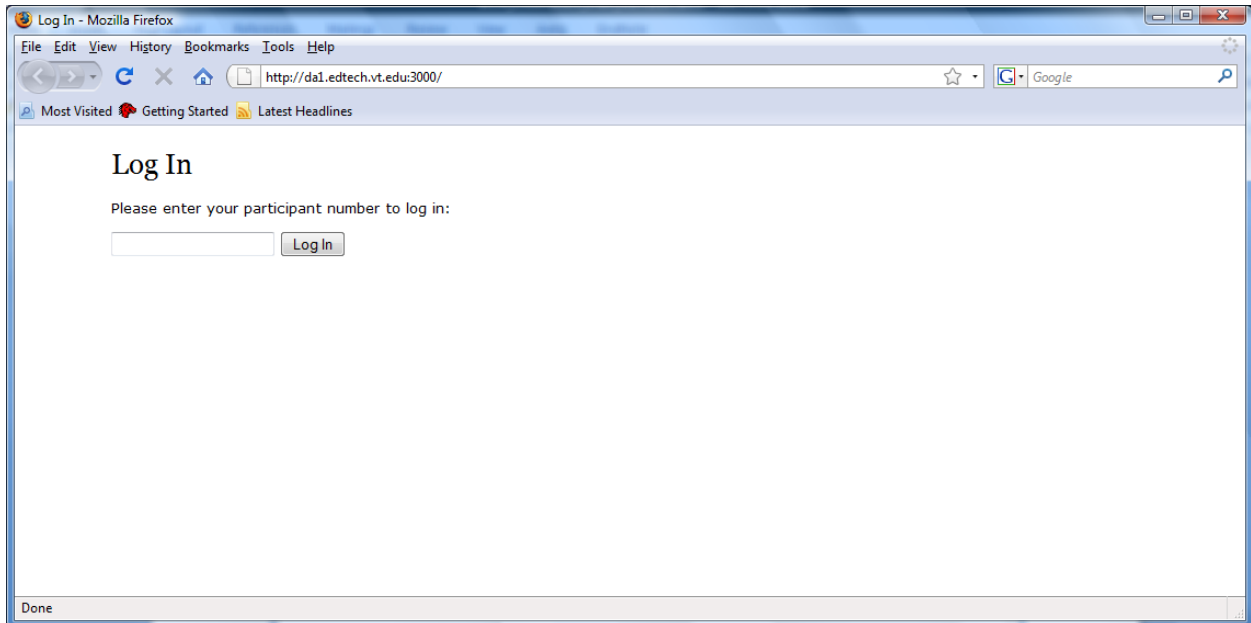
Pseudo R-Squared 0.0778 0.0826 0.0839 0.0902

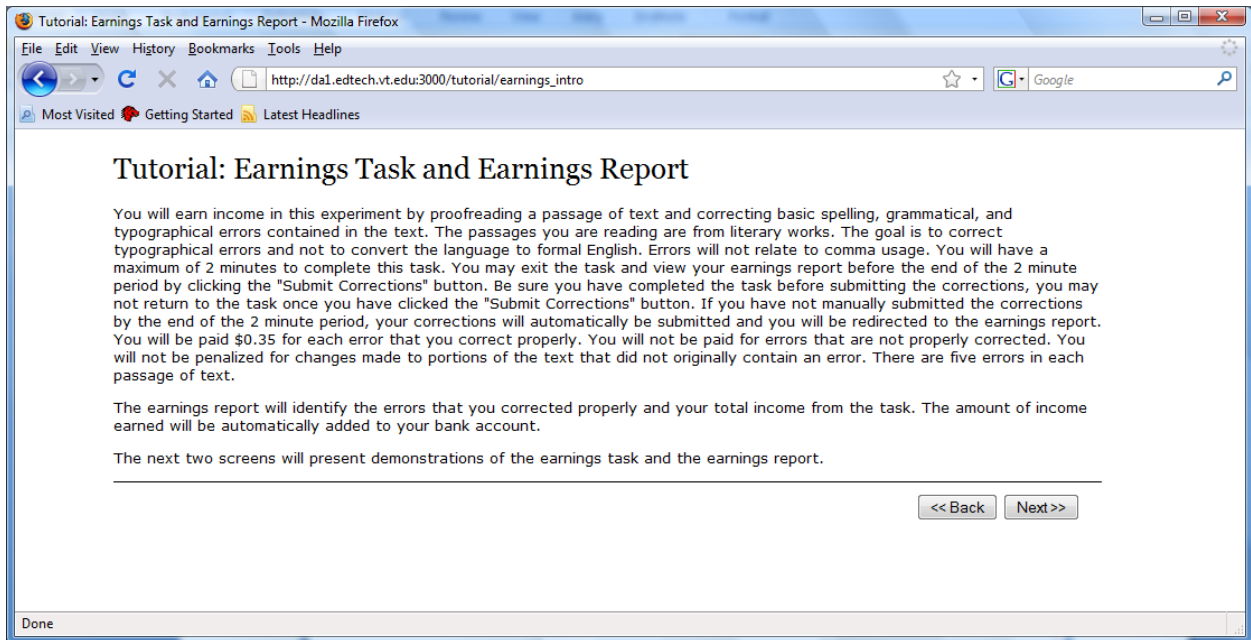
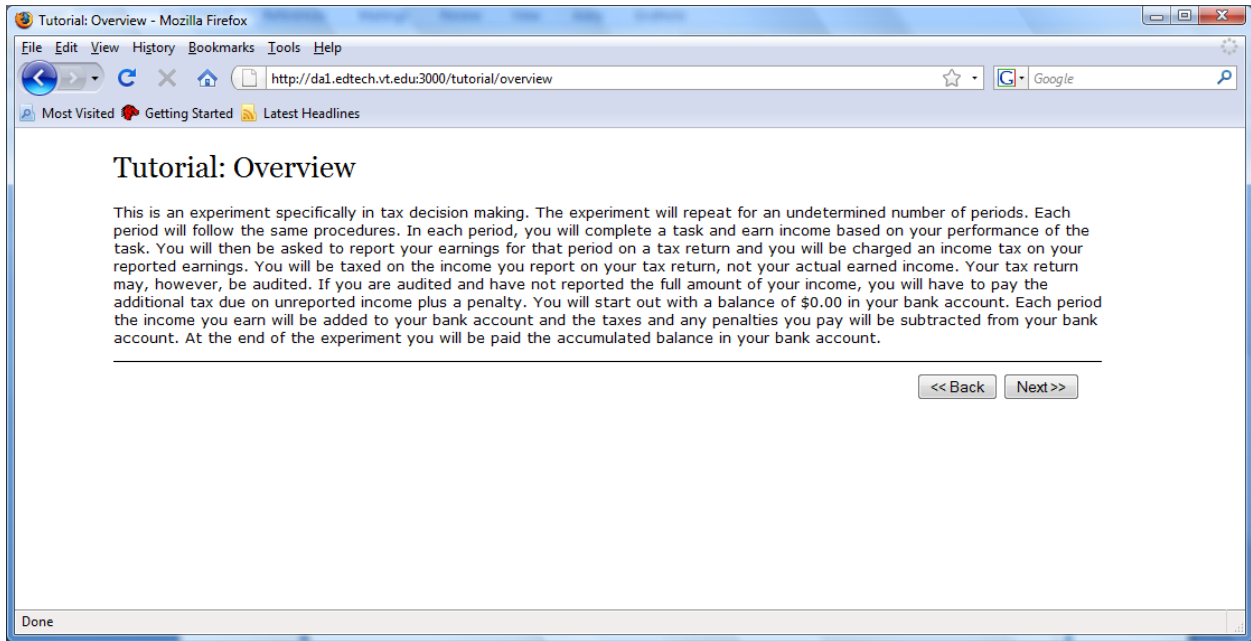
* Significant at $p < .1$, **Significant at $p < .05$, ***Significant at $p < .01$

Table presents the results (parameter estimates with standard errors in parentheses) of Tobit regressions on the observations of the dependent variable for all rounds of the experiment. All observations after a participant is audited are included in the data set. The dependent variable of Compliance is the proportion of earned income that is reported. Traditional message group is 1 if the participant was in the Traditional message group and 0 otherwise. TRA message group is 1 if the participant was in the TRA message group and 0 otherwise. Time is measured as the round number and ranges from 1 to 20. Gender is 0 for male and 1 for female. Income level is measured on a scale of 1 to 3 whether the participant indicated their family income was below average, about average, or above average when compared to other students at the university at which the study was conducted. Tax return is 0 if the participant has never filed a US income tax return and 1 if the participant has filed a US income tax return. Audit familiarity is 0 if the participant reported that they did not know anyone who had been subject to an IRS audit and 1 if they reported that they did know someone who had been subject to an IRS audit. Risk is measured on a scale of 0 to 10 with higher values indicating greater risk aversion. Morality of Tax Evasion is measured as 0 if the participant indicated that they did not believe that tax evasion was immoral and 1 if they indicated that they did believe tax evasion was immoral. Honesty and Personal Values are measured on a scale ranging from 10 to 30 where higher values indicate less honest attitudes. Civic attitudes are measured on a scale of 10 to 50 where higher values indicate greater attitudes of social responsibility.

Appendix C Experimental Program Screenshots

Experimental Instructions





Tutorial: Earnings Task Example - Mozilla Firefox

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http://da1.edtech.vt.edu:3000/tutorial/earnings_task

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Tutorial: Earnings Task Example

Take a moment to correct errors in the passage and then click the "Submit" button to advance to the Earnings Report Example. Please do not advance beyond the Earnings Report until instructed to do so.

Earnings Task

1:48

Please correct the errors in the text below. Click "Submit Corrections" when you are done. You have two minutes. If you have not submitted your corrections when your time is up, you will be automatically redirected to the earnings report page.

By this time she had found her way into a tidy little room with a table in the window, and on it (as she had hoped) a fan and two or three pairs of tiny white kid-gloves. she took up the fan and a pair of the goves, and was just going ot leave the room, when her eye fell upon a little bottle that stood near the looking-glass. There was no label this time with the words "Drink me," but nevertheless she uncorked it and put it to her lips. "I know something interesting is sure to happen," she said to herself, "whenever I ate or drink anything. So I'll just see what this bottle does. I do hope it'll make me grow large again, for really Im quite tired of being such a tiny little thing!"

Done

Tutorial: Earnings Report Example - Mozilla Firefox

File Edit View History Bookmarks Tools Help

http://da1.edtech.vt.edu:3000/tutorial/earnings_report

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Tutorial: Earnings Report Example

Take a few seconds to review the earnings report and then advance to the next tutorial screen by clicking either the "Proceed" or the "Next" button. Please do not advance beyond the next tutorial screen until instructed to do so.

Earnings Report

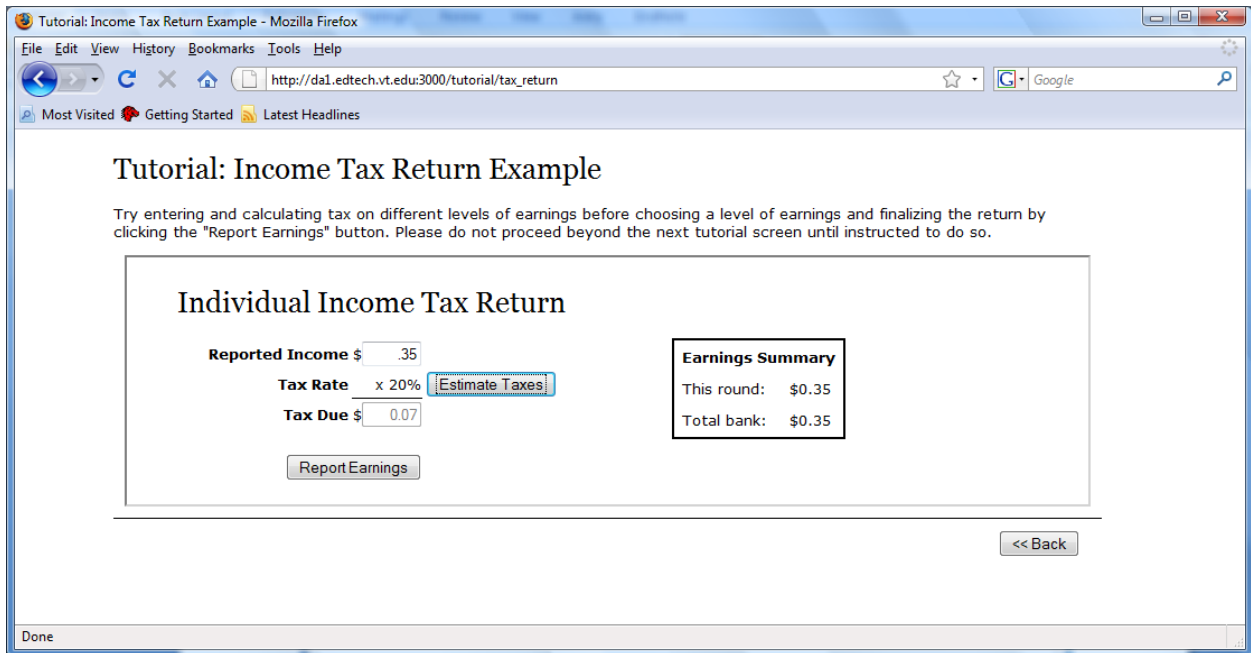
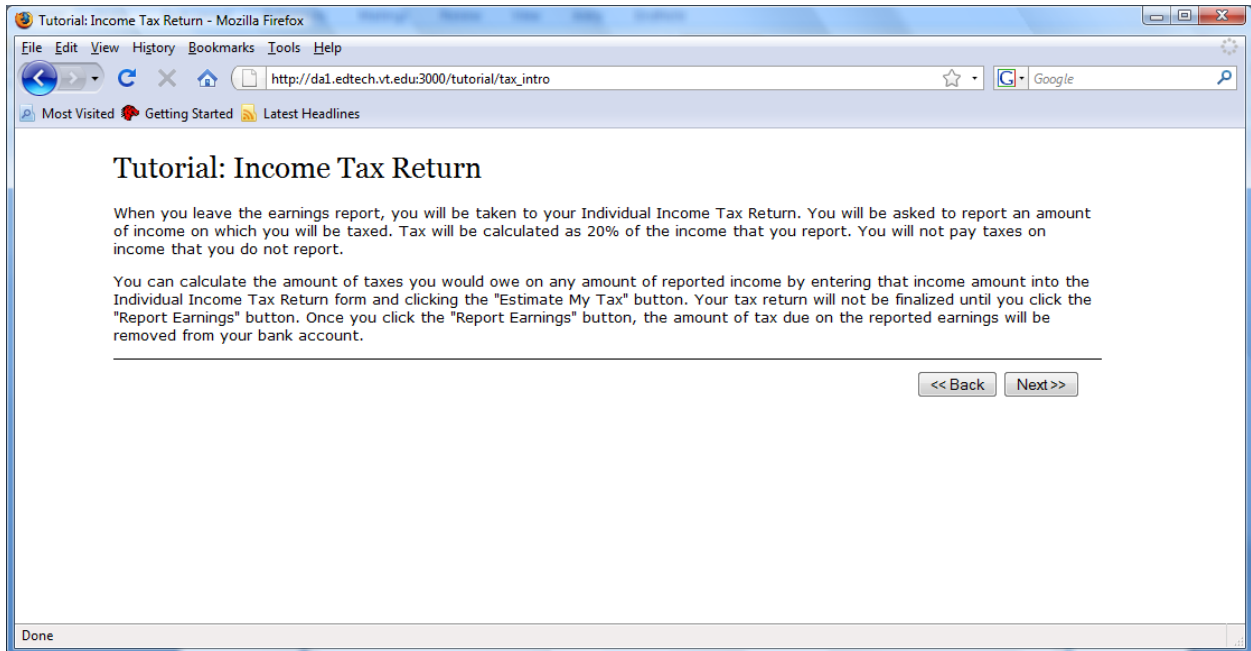
\$0.35

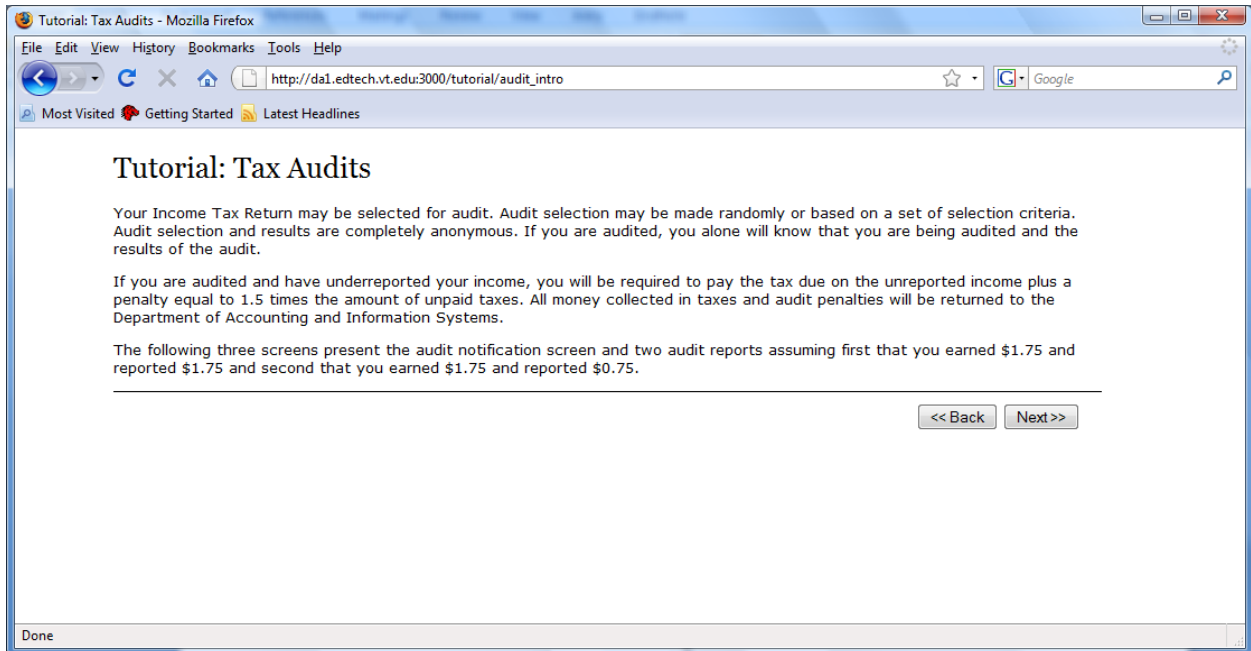
Errors Corrected

- ate was changed to eat

Errors Corrected	1
Income Per Correction x \$0.35	
Income Earned	\$0.35

Done





Tutorial: First Audit Report Example - Mozilla Firefox

File Edit View History Bookmarks Tools Help

http://da1.edtech.vt.edu:3000/tutorial/audit_ok

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Tutorial: First Audit Report Example

Please take a few seconds to review the audit report and then advance to the next screen by clicking either the "Proceed" or the "Next" button. Please do not advance beyond the next tutorial screen until instructed to do so.

Audit Report

Earned Income \$1.75	\$1.40
Reported Income <u>\$1.75</u>	
Difference \$0.00	

You do not owe any additional amounts.

Done

Tutorial: Second Audit Report Example - Mozilla Firefox

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http://da1.edtech.vt.edu:3000/tutorial/audit_error

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Tutorial: Second Audit Report Example

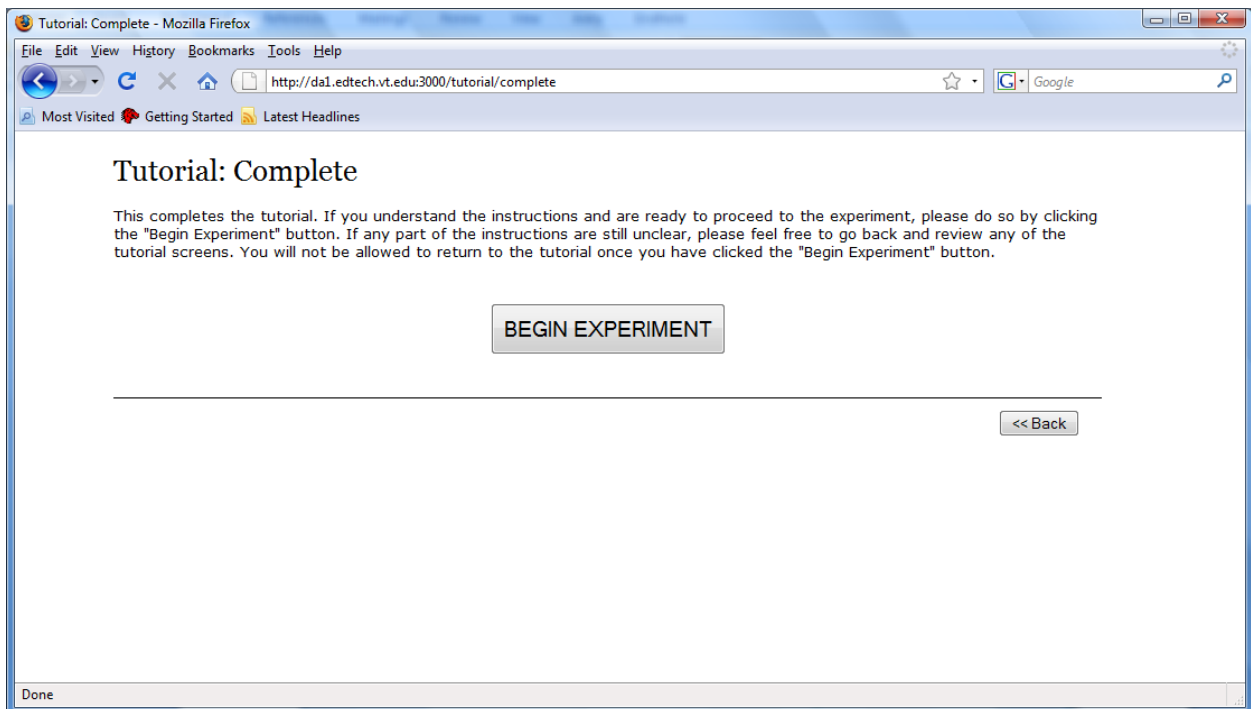
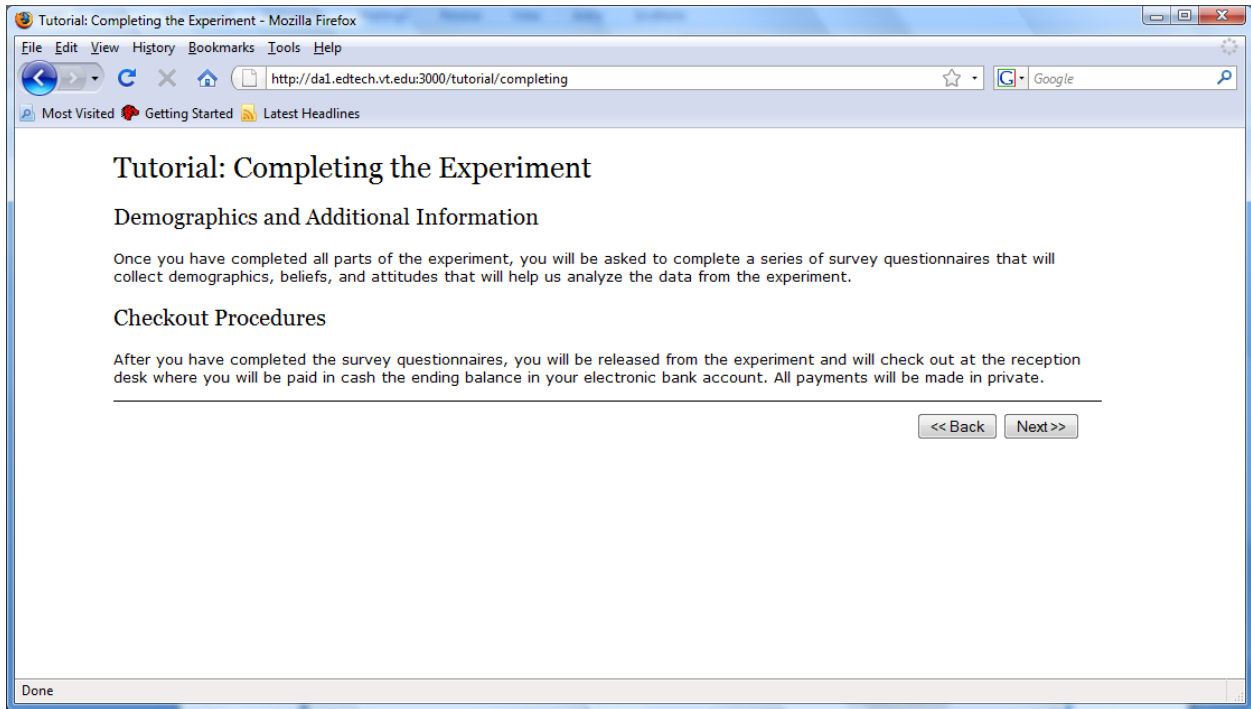
Please take a few seconds to review the audit report and then advance to the next screen by clicking either the "Proceed" or the "Next" button. Please do not advance beyond the next tutorial screen until instructed to do so.

Audit Report

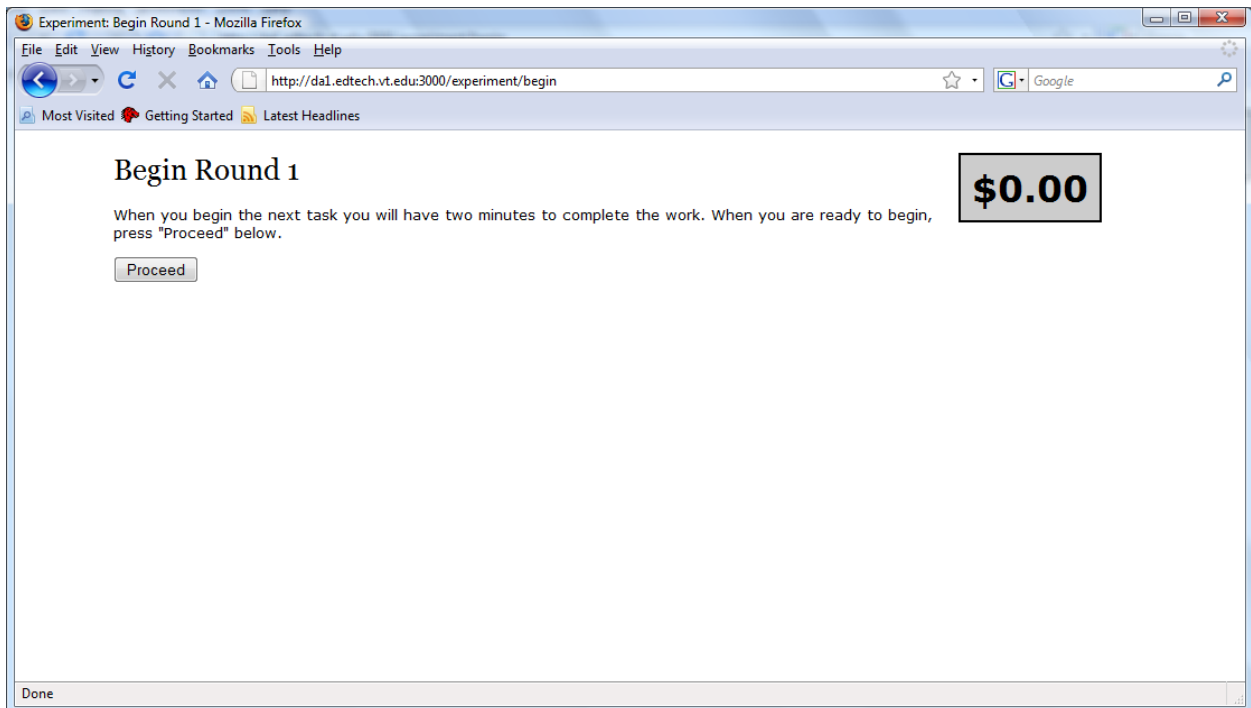
Earned Income \$1.75	\$1.10
Reported Income <u>\$0.75</u>	
Difference \$1.00	
Tax Rate x 20%	
Additional Tax Due \$0.20	
Penalties \$0.30	
Total Amount Due \$0.50	

You owe \$0.50 in penalties and unpaid taxes. This amount has been subtracted from your bank.

Done



Example 1: Round 1 with a Control Group Participant



Experiment: Earnings Task - Mozilla Firefox

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Earnings Task

Please correct the errors in the text below. Click "Submit Corrections" when you are done. You have two minutes. If you have not submitted your corrections when your time is up, you will be automatically redirected to the earnings report page.

1:46

Don't you know what that is? Its spring fever. That is what the name of it is. And when you've got it, you don' quite know what it is you do want, but it just fairly makes your heart ache, you want it so! It seems to you that mainly what you want is to get away. To get away from the same old tedious things you're so used to seeing and so tired of, and set something new. That is the idea. You want togo and be a wanderer. You want to go wandering far away to strange country's where everything is mysterious and wonderful and romantic. And if you can't do that, you'll put up with considerable less. You'll go anywhere you can go, just so as to get away, and be thankful of the chance, to.

Submit Corrections

Done

Experiment: Earnings Report - Mozilla Firefox

File Edit View History Bookmarks Tools Help

http://da1.edtech.vt.edu:3000/experiment/earnings

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Earnings Report

Errors Corrected

1. *Its* was changed to *It's*
2. *to* was changed to *too*
3. *don'* was changed to *don't*
4. *togo* was changed to *to go*

Errors Corrected 4

Income Per Correction x \$0.35

Income Earned \$1.40

Proceed

Done

\$1.40

Experiment: Individual Income Tax Return - Mozilla Firefox

File Edit View History Bookmarks Tools Help

http://da1.edtech.vt.edu:3000/experiment/report

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Individual Income Tax Return

Reported Income \$ 1.00

Tax Rate x 20% [Estimate Taxes](#)

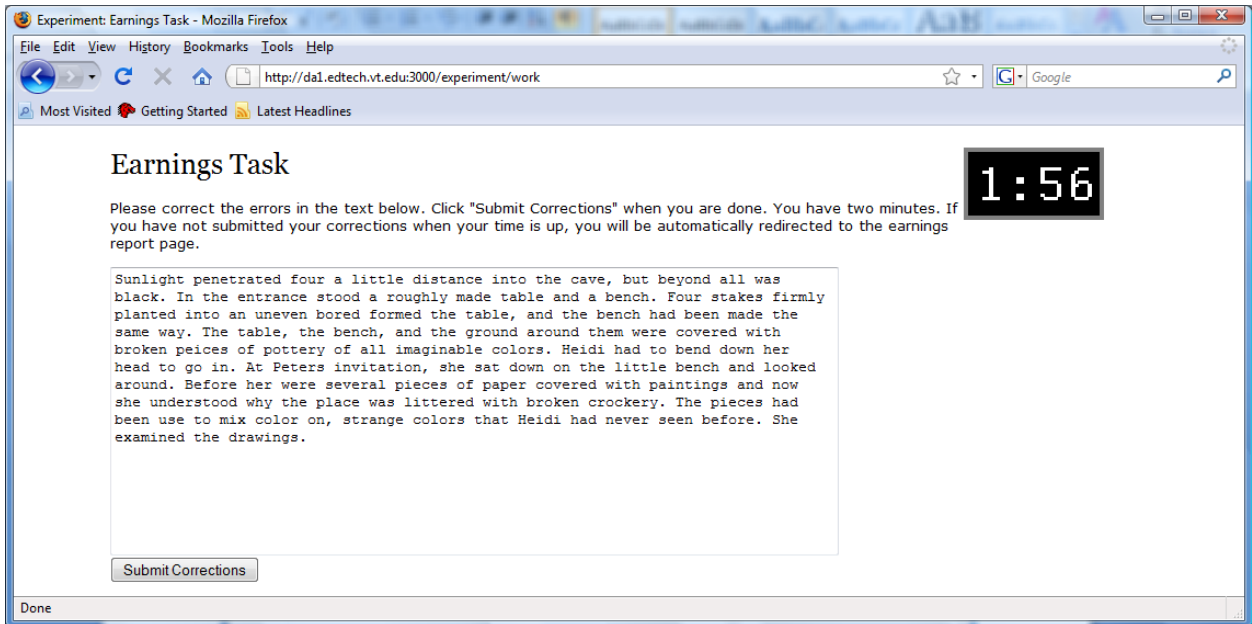
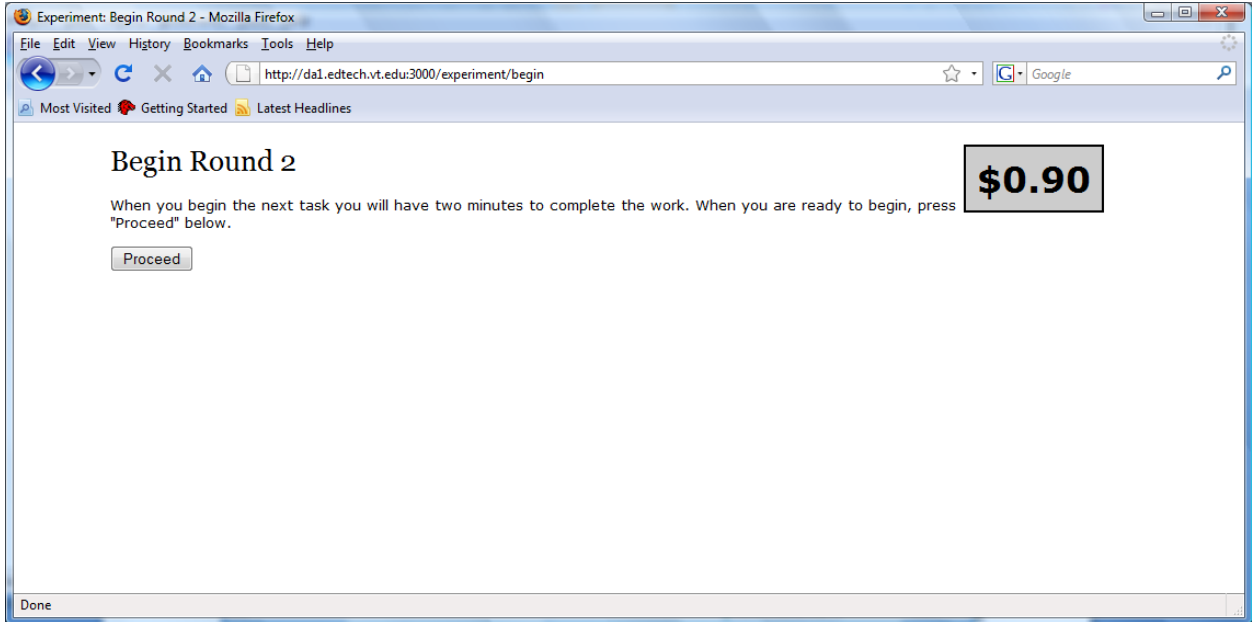
Tax Due \$ 0.20

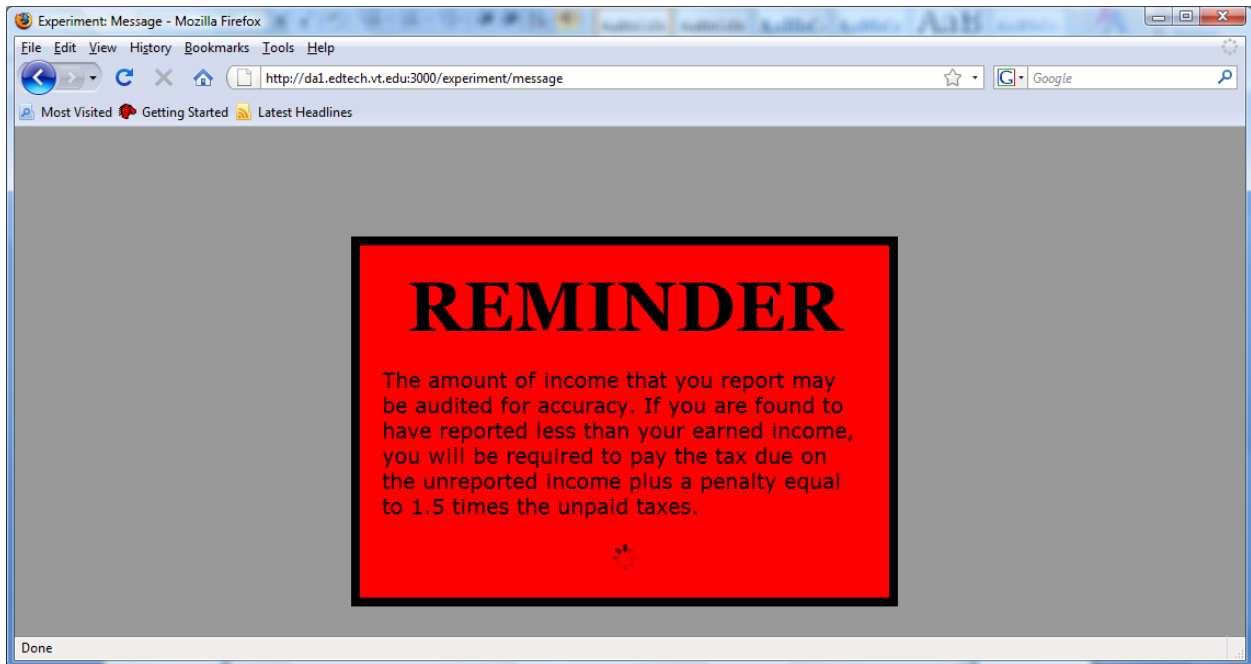
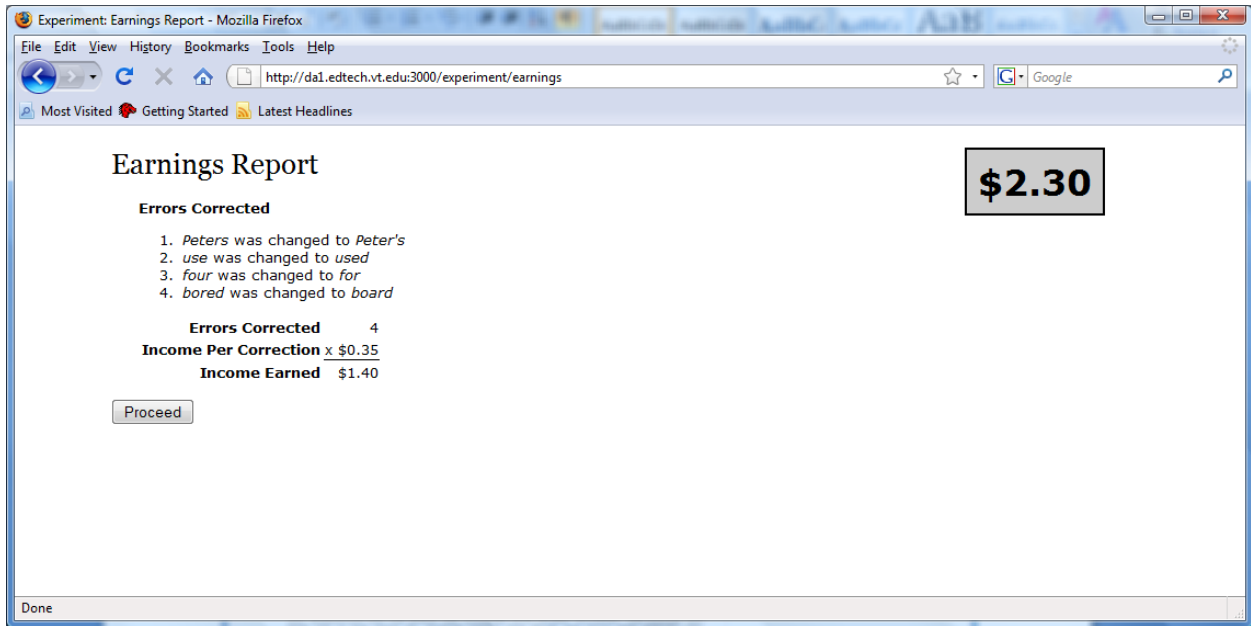
[Report Earnings](#)

Earnings Summary	
This round:	\$1.40
Total bank:	\$1.40

Done

Example 2: Round 2 with a Traditional Message Group participant





Experiment: Individual Income Tax Return - Mozilla Firefox

File Edit View History Bookmarks Tools Help

http://da1.edtech.vt.edu:3000/experiment/report

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Individual Income Tax Return

Reported Income \$

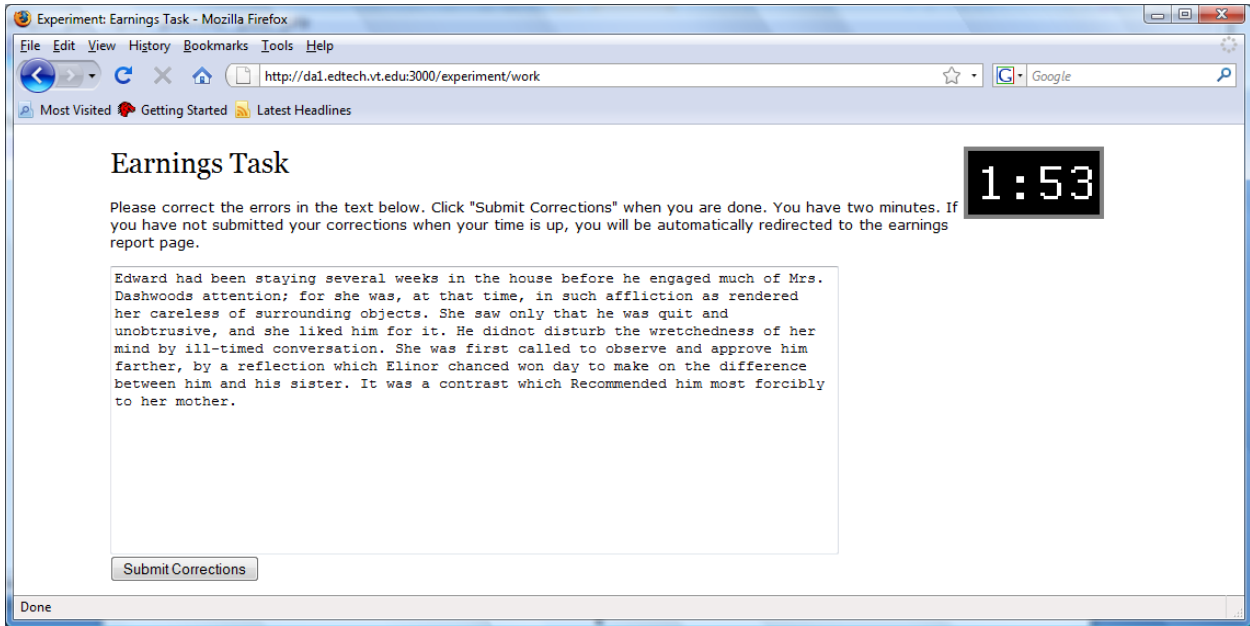
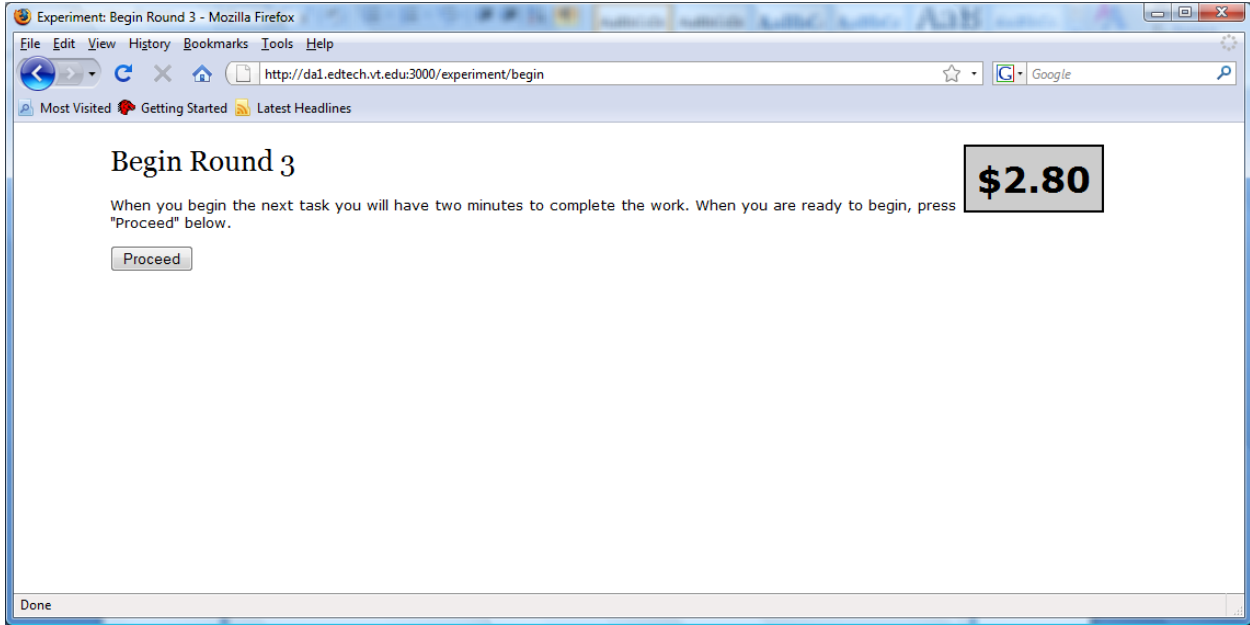
Tax Rate x 20%

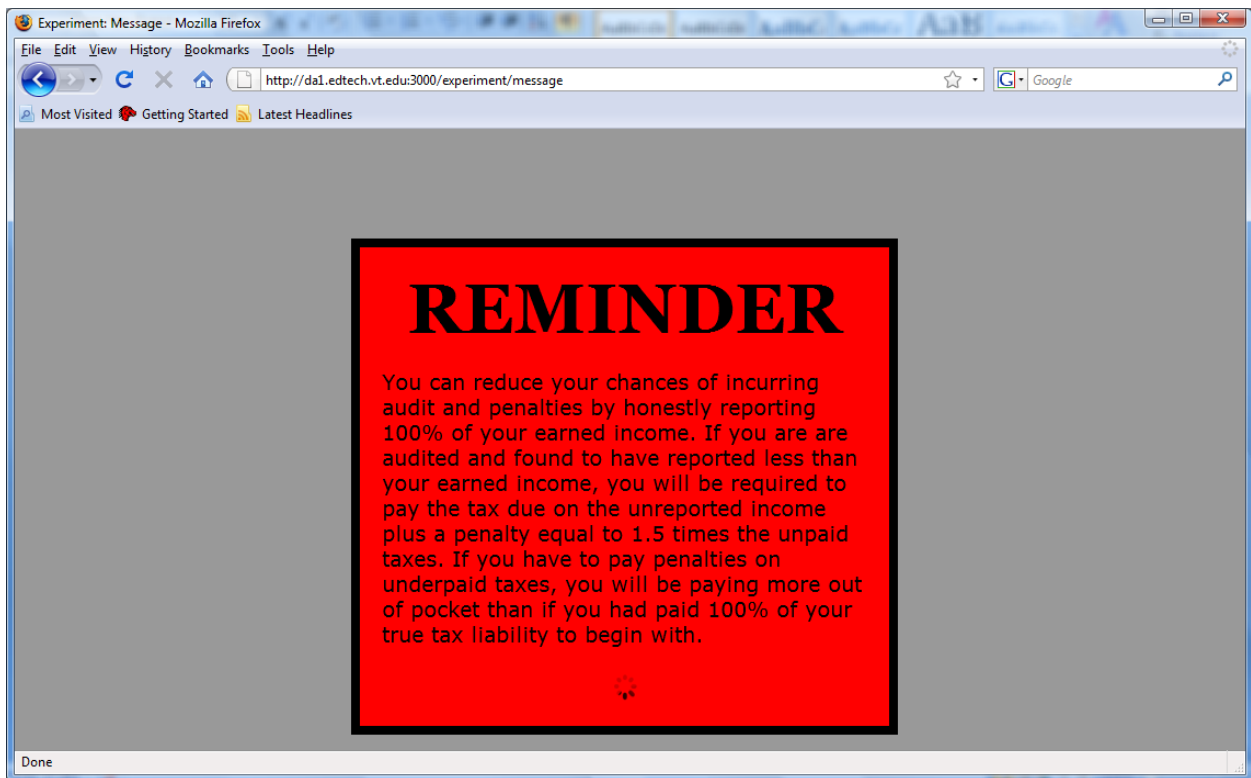
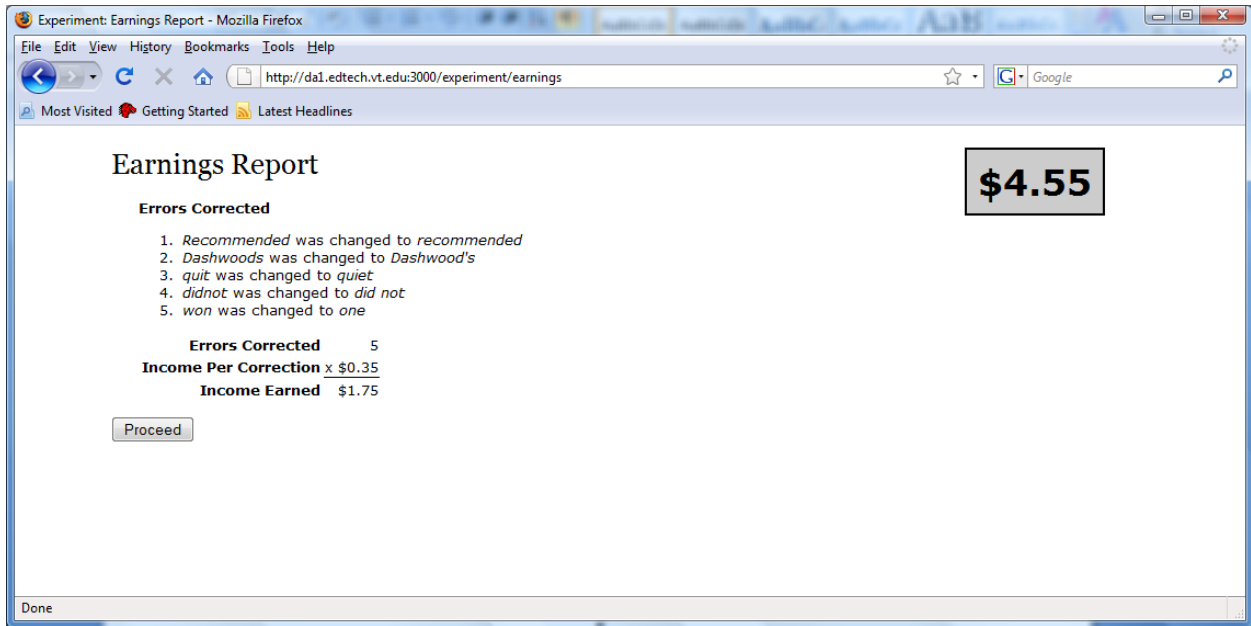
Tax Due \$

Earnings Summary	
This round:	\$1.40
Total bank:	\$2.30

Done

Example 3: Round 3 with a TRA Message Group Participant





Experiment: Individual Income Tax Return - Mozilla Firefox

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http://da1.edtech.vt.edu:3000/experiment/report

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Individual Income Tax Return

Reported Income \$

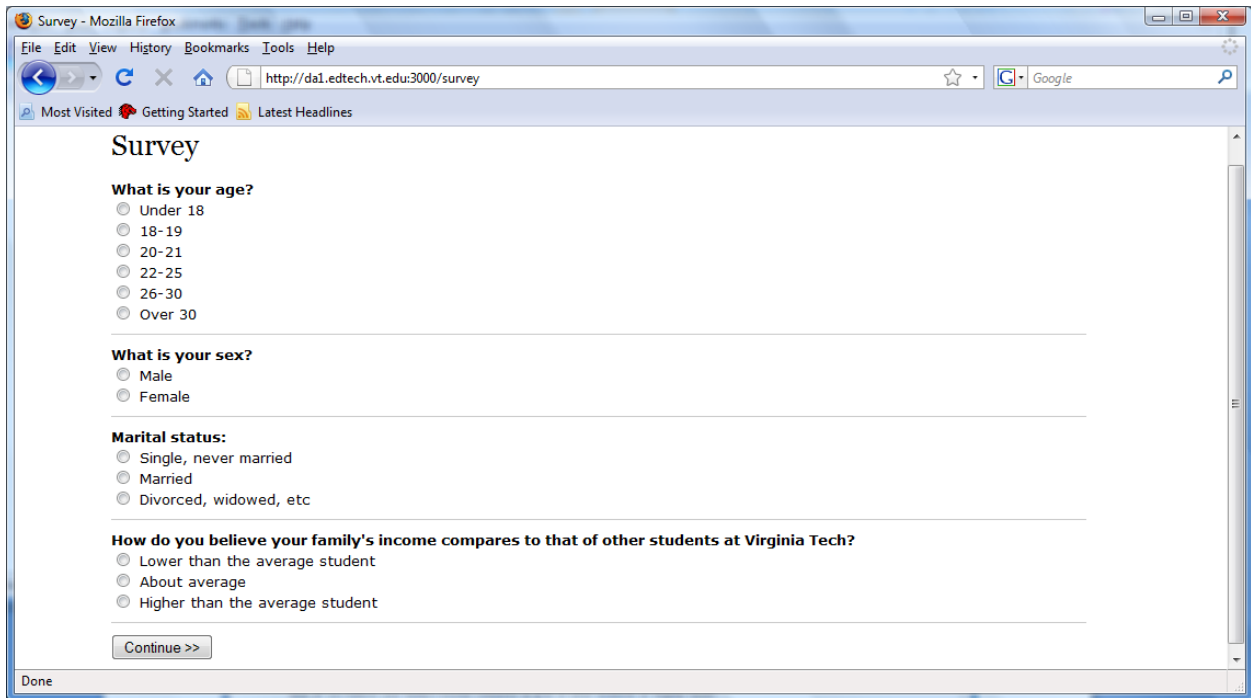
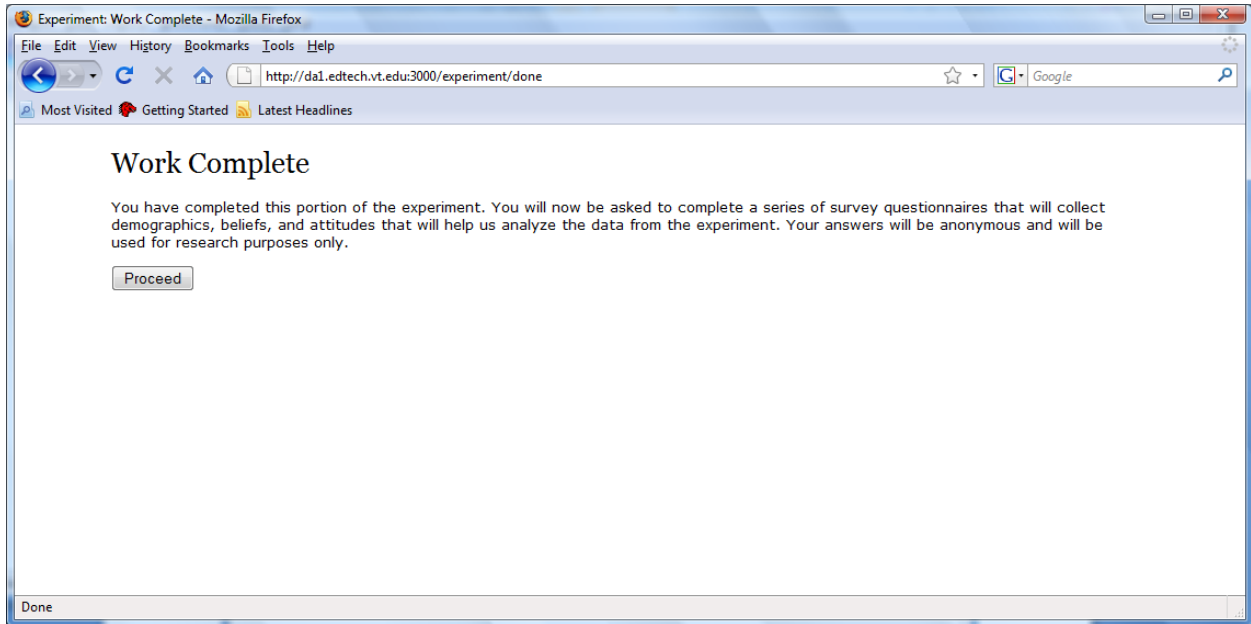
Tax Rate x 20%

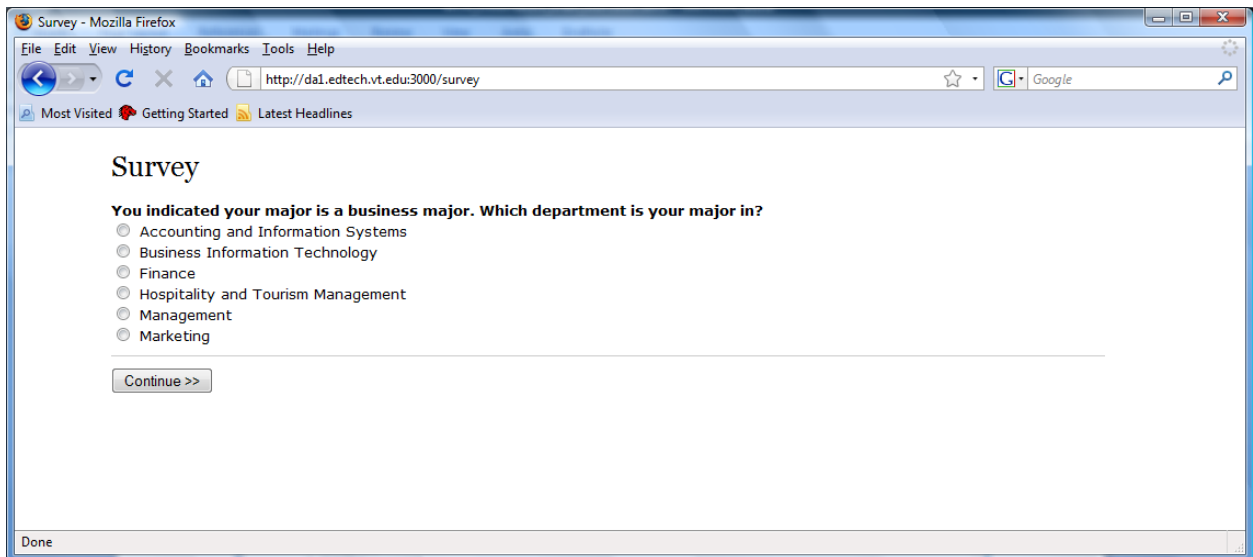
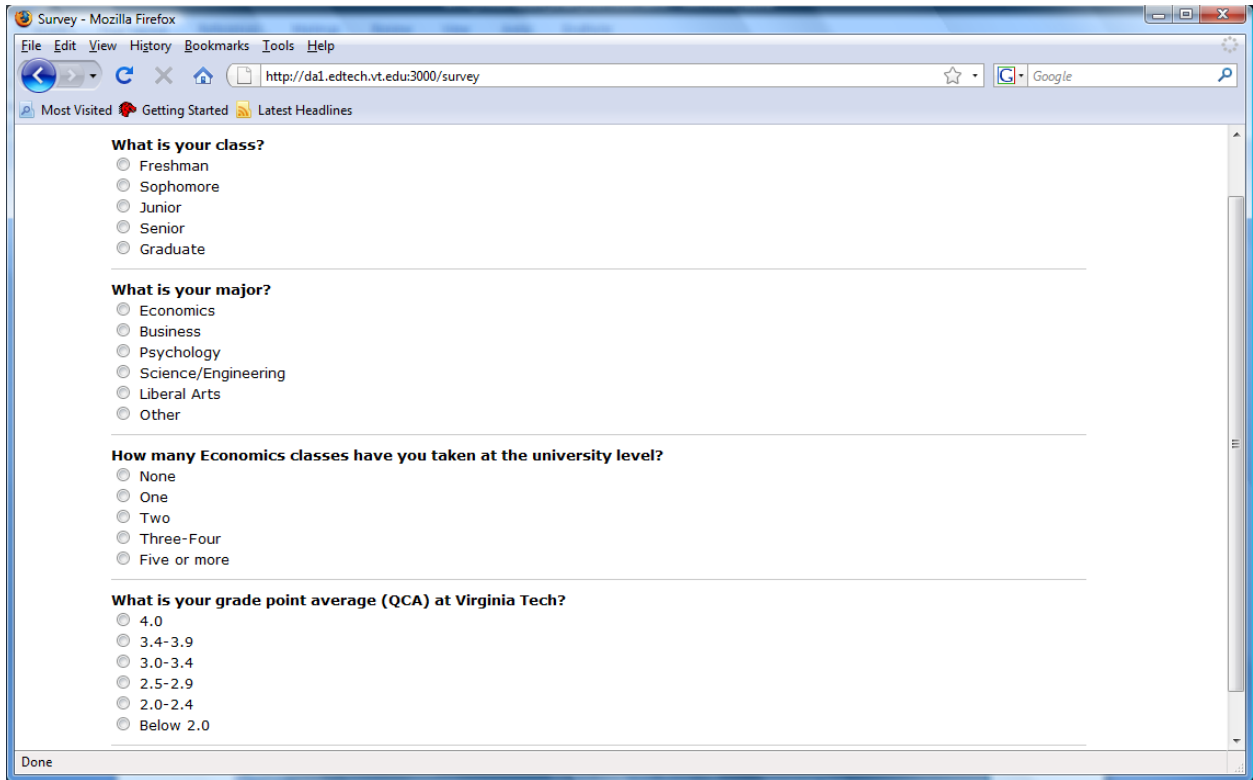
Tax Due \$

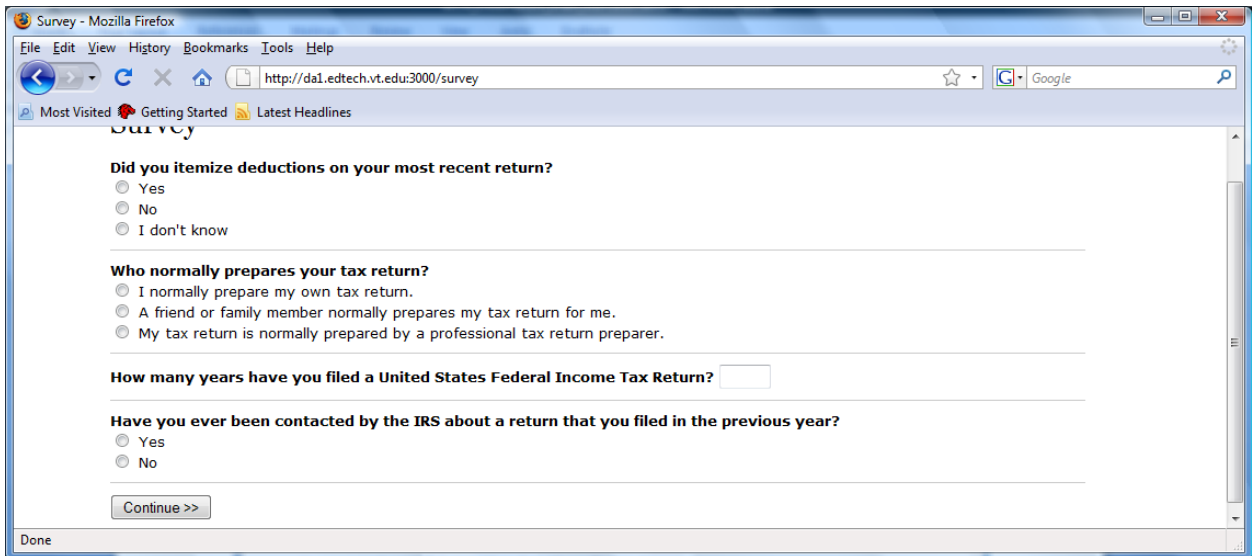
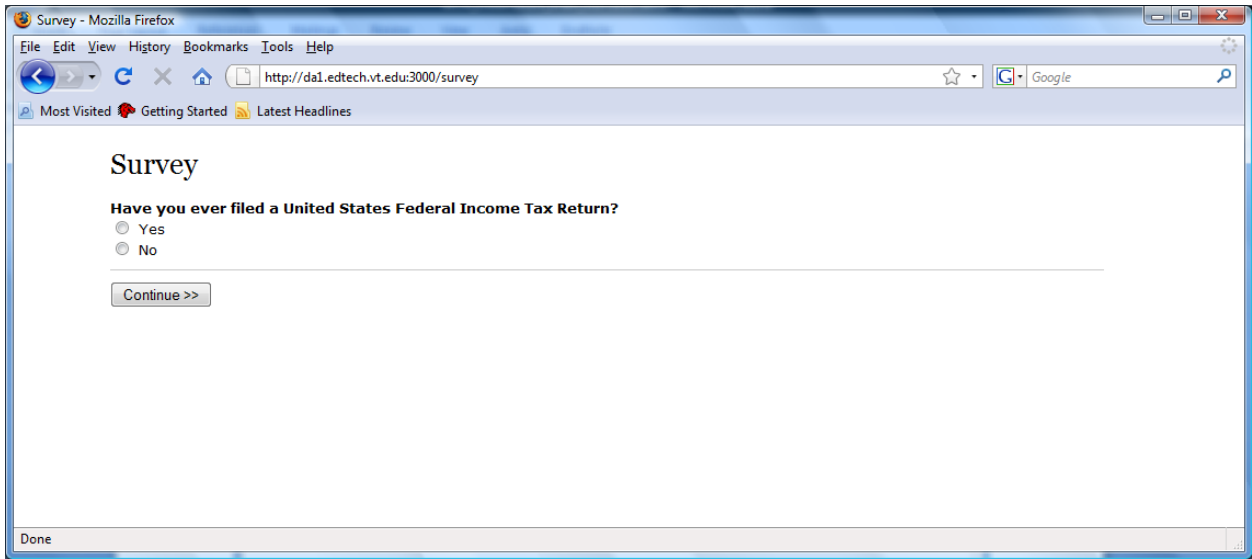
Earnings Summary	
This round:	\$1.75
Total bank:	\$4.55

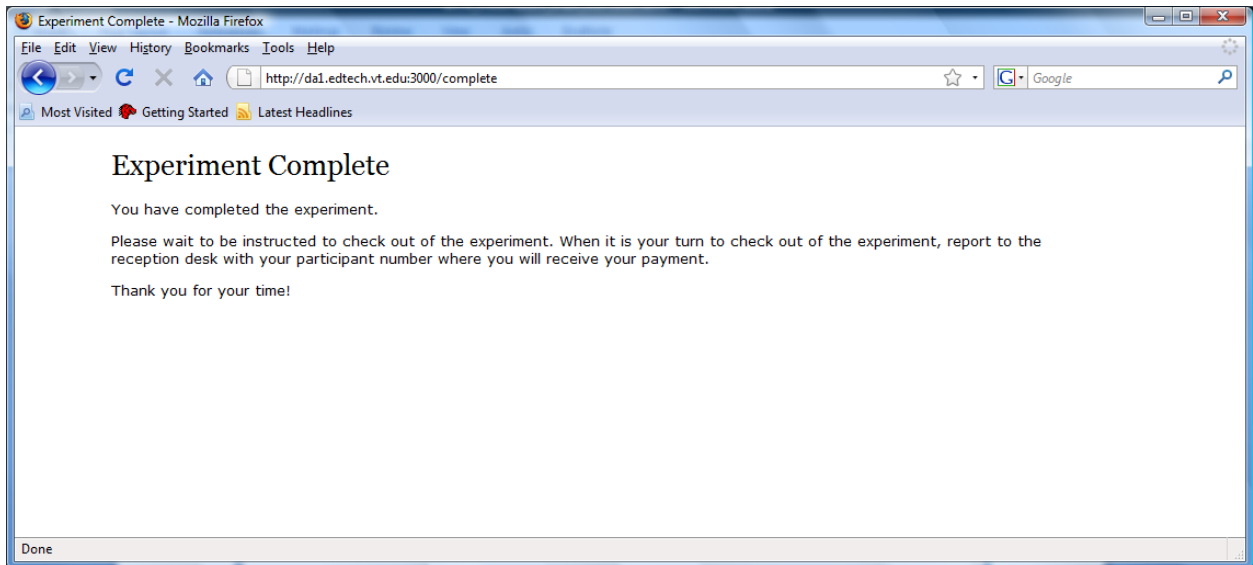
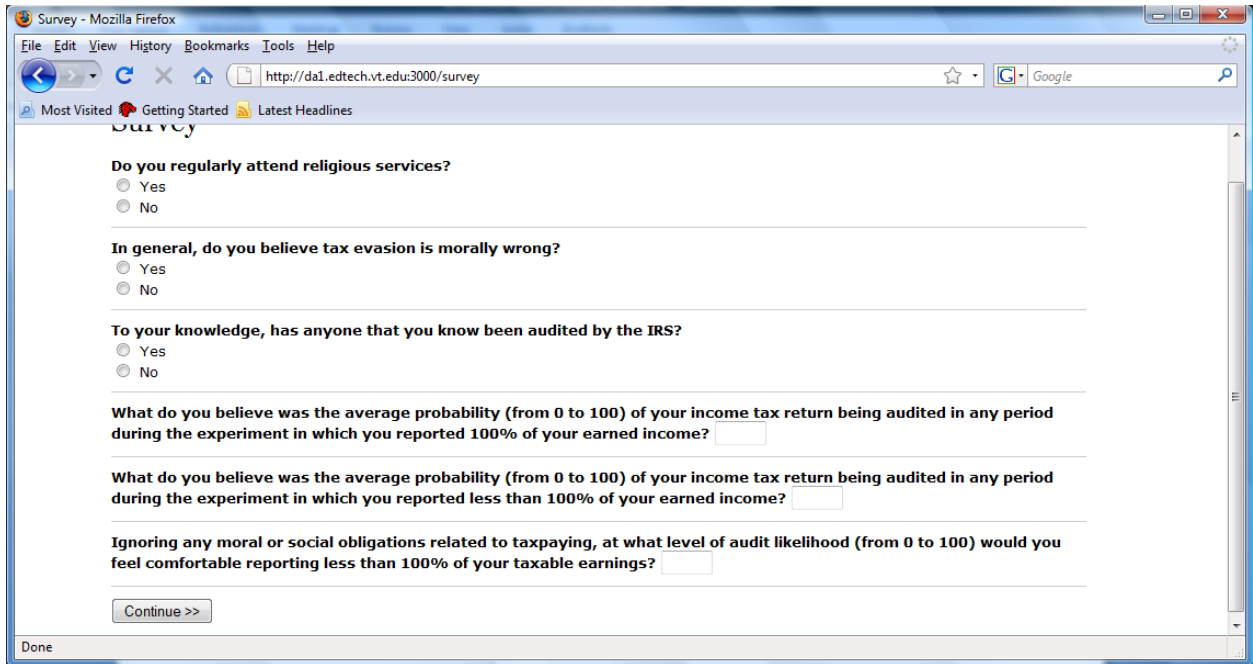
Done

Demographics









Appendix D

Risk Game and Instructions

Please proceed to the next screen, but do not make any decisions until I have completed reading the instructions.

Your decision screen shows ten decisions listed on the left, which are labeled 0, 1, 2 ... 9. Each decision is a paired choice between a randomly determined payoff described on the left and another one described on the right. You will make ten choices and record these in the final column, but only one of them will be used in the end to determine your earnings. Before you start making your ten choices, please let me explain how these choices will affect your earnings. You will be paid for making this decision as described below.

Here is a ten-sided die that will be used to determine payoffs; the faces are numbered from 0 to 9. When you check out after you've made your decisions, we will roll this die twice, once to select one of the ten decisions to be used, and then once more to determine what your payoff is for the option you chose (the left side or the right side) for the particular decision selected. Even though you will make ten decisions, only one of these will end up affecting your earnings, but you will not know in advance which decision will be used. Each decision has an equal chance of being used in the end.

For example, suppose that the first roll of the die comes up 0. This would mean that the gamble choice we end up using is Decision 0 at the top of the table. The second roll of the die determines the outcome of the gamble that you chose. The option on the left side pays \$2.00 if the result of the throw of the ten-sided die is 0 and it pays \$1.60 if the result of the throw of the ten-sided die is 1 - 9. The option on the right side pays \$3.85 if the result of the throw of the ten-sided die is 0 and it pays \$0.10 if the result of the throw of the ten-sided die is 1 - 9. Thus the option on the left side of Decision 0 provides 1 chance out of 10 (or a 10% chance) of getting \$2.00 and 9 chances out of 10 (or a 90% chance) of getting \$1.60 while the option on the right side of Decision 0 provides 1 chance out of 10 (or a 10% chance) of getting \$3.85 and 9 chances out of 10 (or a 90% chance) of getting \$0.10. The left and right options for the other decision rows are similar, but with differing chances of getting each payoff.

In summary, you will make 10 choices: for each decision row, you will have to choose between the option on the left and the option on the right. Make your choices by clicking either the L or R button to the right of each decision line. You may change your decisions and make them in any order. When you are finished click the submit button at the bottom of the screen. When you check out of the experiment, we will throw the 10 sided die to determine which decision number will be used and the payment that you will receive for the option that you chose for that decision number. This payment will be added to the balance in your electronic bank from the previous part of the experiment to determine your total payment from the experiment.

Please think carefully about each decision and please do not talk with others in the room. Are there any questions?

	<u>Left Side</u>	<u>Right Side</u>	Your Choice L or R
Decision 0	\$2.00 if throw of die is 0 \$1.60 if throw of die is 1 - 9	\$3.85 if throw of die is 0 \$0.10 if throw of die is 1 - 9	
Decision 1	\$2.00 if throw of die is 0 - 1 \$1.60 if throw of die is 2 - 9	\$3.85 if throw of die is 0 - 1 \$0.10 if throw of die is 2 - 9	
Decision 2	\$2.00 if throw of die is 0 - 2 \$1.60 if throw of die is 3 - 9	\$3.85 if throw of die is 0 - 2 \$0.10 if throw of die is 3 - 9	
Decision 3	\$2.00 if throw of die is 0 - 3 \$1.60 if throw of die is 4 - 9	\$3.85 if throw of die is 0 - 3 \$0.10 if throw of die is 4 - 9	
Decision 4	\$2.00 if throw of die is 0 - 4 \$1.60 if throw of die is 5 - 9	\$3.85 if throw of die is 0 - 4 \$0.10 if throw of die is 5 - 9	
Decision 5	\$2.00 if throw of die is 0 - 5 \$1.60 if throw of die is 6 - 9	\$3.85 if throw of die is 0 - 5 \$0.10 if throw of die is 6 - 9	
Decision 6	\$2.00 if throw of die is 0 - 6 \$1.60 if throw of die is 7 - 9	\$3.85 if throw of die is 0 - 6 \$0.10 if throw of die is 7 - 9	
Decision 7	\$2.00 if throw of die is 0 - 7 \$1.60 if throw of die is 8 - 9	\$3.85 if throw of die is 0 - 7 \$0.10 if throw of die is 8 - 9	
Decision 8	\$2.00 if throw of die is 0 - 8 \$1.60 if throw of die is 9 - 9	\$3.85 if throw of die is 0 - 8 \$0.10 if throw of die is 9 - 9	
Decision 9	\$2.00 if throw of die is 0 - 9	\$3.85 if throw of die is 0 - 9	

Appendix E

Attitude and Personality Scales

Honesty and Personal Values Scale

For each characteristic indicate how you view these characteristics in other people: Always Admire, Depends on the Situation, Always Dislike

1. Never cheating or having anything to do with cheating situations, even for a friend.
2. Always telling the truth, even though it may hurt oneself or others.
3. Helping a friend during an exam.
4. Speaking one's mind truthfully, without regard for the consequences.
5. Being dishonest in harmless ways.
6. Sticking up for the truth under all circumstances.
7. Going out of one's way to bring dishonest people to justice.
8. Volunteering information concerning wrongdoing, even if friends are involved.
9. Using a false ID card to get into restricted places
10. Presenting oneself completely and honestly, even if it is unnecessary to do so.

Item numbers 3, 5, and 9 were reverse coded.

Social Attitude Scale

The next set of questions describes a set of beliefs that some people may have. After each statement please indicate how strongly you agree with the statement. (1 = strongly disagree, 5 = strongly agree)

1. It is no use worrying about current events or public affairs; I cannot do anything about them anyhow.
2. We ought to worry about our own country and let the rest of the world take care of itself.
3. When a person does not tell all of his income in order to get out of paying some of his taxes, it is just as bad as stealing money from the government.
4. A person who does not vote when he can is not a good citizen.
5. Every citizen should take the time to find out about current events even if it means giving up some spare time.
6. Why bother to vote when you can do so little with just your one vote.
7. Our country would be a lot better if we didn't have elections and people didn't have to vote so often.
8. It's a good thing the Atlantic Ocean separates us from Europe because then we don't have to worry about them.
9. Every person should give some of his time for the good of his town or city.
10. It's more important to work for the good of the team than to work for your own good.

Item numbers 1, 2, 6, 7, and 8 were reverse coded.