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Essays On Charitable Giving And Taxpayer Behavior

Seiyoun Kim

ABSTRACT

ESSAYS ON CHARITABLE GIVING AND TAXPAYER BEHAVIOR

By

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The central theme of my dissertation is understanding the relationship between different types of institutions and individual behavior. The types of institutions I specifically study are tax enforcement agencies, charities, and governments. The corresponding responses I examine are tax compliance, giving, and tax sheltering.

One of the Internal Revenue Services' audit methods is to select individuals engaged in transactions with other taxpayers whose tax returns were selected for audit. This joint liability in the audit mechanism introduces an externality where one's action affects the likelihood of others being audited. Therefore, it can induce different tax compliance behavior compared to the random selection audit mechanism. Chapter 1 studies the effectiveness of this group accountability audit mechanism against a purely random selection audit mechanism using a lab experiment. Data shows that tax compliance is higher in the group accountability audit mechanism than in the random selection audit mechanism.

Chapter 2 uses laboratory experiments to study individual decisions on donating to a charity in response to changes in the tax rate and income in the presence of matching and two types of rebate subsidies: deterministic and stochastic. Private consumption is taxed, and contributions are subsidized such that the relative price of giving across the fundraising mechanisms is

equivalent. Data shows that tax framing and rebate subsidy elicit less charitable contribution than neutral framing and matching subsidy; the negative effect on donation is smaller for stochastic than deterministic rebate subsidies.

Chapter 3 examines the effect of identification with the government on tax sheltering behavior using secondary data. Individuals' attitudes towards a new government depend on whether their political preference is aligned with the elected government leader. Those who are aligned are more likely to be positive and agree with how the government uses tax revenue. On the other hand, those who are not aligned are more likely to be negative and disagree with how the tax revenue is spent. Such emotions and attitudes towards the new government may be reflected through the tax sheltering behavior. I find that charitable contribution deduction increased for counties that were not politically aligned when the presidency changed.

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ACCEPTANCE

This dissertation was prepared under the direction of the candidate's Dissertation Committee. It has been approved and accepted by all members of that committee, and it has been accepted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in Economics in the Andrew Young School of Policy Studies of Georgia State University.

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DEDICATION

To my beloved parents who always had my education their priority.

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1.1 Introduction

Governments need tax revenue to provide certain goods and services such as national defense, health care, social security, education, and various welfare programs. Tax evasion limits the government from providing such public goods by causing a loss in government revenue. To recover revenue loss, the government needs to raise the tax rate or cut government spending. However, raising the tax rate could increase tax evasion (Crane and Nourzad, 1990; Fisman and Wei, 2004), and austerity measures are faced with strong opposition from the citizens (Rüdig and Karyotis, 2014). Moreover, high tax evasion decreases the morale of honest taxpayers (Torgler, 2005). One of the many ways to resolve this dilemma is by recovering evaded tax revenues and reducing tax evasion through cost-effective audit strategies. Since tax enforcing agencies have a limited government budget for tax enforcement, it is crucial to devise a cost-efficient audit mechanism that deters tax evasion.

The conventional method government tax agencies use to reduce tax evasion is through audits and charging a fine when evasion is caught. The selection process of tax returns for audit is usually not purely random, unlike what many researchers assume when conducting tax evasion-related experiments in the lab. For example, the Internal Revenue Service (IRS) uses different methods of audit strategy to detect tax evasion ("IRS Audits," 2017). One approach is selecting tax returns using a statistical formula that identifies income tax returns, which are different from the norm of similar tax returns. Another method the IRS uses for audit selection involves a similar procedure to joint liability. The IRS would select tax returns for an audit if they were

[1] This chapter is a collaborative work with a colleague Puneet Arora. The research has been supported by Grant # 1811-09066 from the Russell Sage Foundation. Any opinions expressed are those of the authors and should not be construed as representing the opinions of the Foundation.

engaged in transactions with other taxpayers whose tax returns were already chosen for audit. Since the cost of an audit is expensive regarding time and resources spent, purely randomly choosing tax returns for audit or merely increasing the audit probability may not be the most cost-efficient way to deter tax evasion (Nessa et al., 2016). Thus, the high cost of conducting audits calls for the identification of cost-effective deterrence strategies that can reduce tax evasion the most, given the number of audits.

Several types of deterrence strategies have been studied in tax compliance experiments, and many of these strategies are shown to reduce tax evasion. For example, strategic audit selection rules (Alm, Cronshaw, and McKee, 1992; Alm and McKee, 2004) and publically shaming the evaders (Alm et al., 2017; Coricelli et al., 2010) are effective in inducing higher tax compliance. However, the effect of the joint liability audit mechanism on tax evasion deterrence is unknown to the best of our knowledge. In this paper, we propose an audit selection rule that incorporates group liability. We argue that introducing group accountability in the audit selection process may be an efficient way to reduce tax evasion. In this mechanism, taxpayers are first exogenously combined into several groups. When one person in the group is randomly selected for audit and found to be misreporting actual income, all group members are also audited. We hypothesize this audit rule induces social pressure as one's action can potentially harm others. Our experiment compares efficiency between the group liability audit mechanism and the purely random selection audit mechanism. The experiment also varies anonymity and group size within the group liability mechanism.

We are interested in comparing the efficacy of the two audit rules. As a result, we keep the number of audits constant in all our treatments, which other research papers (Alm, Cronshaw, and McKee, 1993; Coricelli et al., 2010) do not do when comparing different types of treatment interventions. Moreover, although the IRS uses a joint liability mechanism for the audit selection process, research on the effect of joint liability on tax evasion is scarce to the best of our knowledge. Hence, this paper is one of the few pieces of evidence that studies and looks at how joint liability embedded in the audit selection process influences taxpaying behavior using a controlled laboratory setting.

We also intend to contribute to the design of an audit mechanism that could increase tax compliance and a corresponding rise in tax revenue. While this can potentially help governments fund welfare programs and provide public goods, it will also help reduce income inequality. According to Alstadsæter et al. (2017), tax evaders are more likely to be in the wealthier group since tax evasion increases with wealth. Therefore, studying taxpaying behavior and finding methods to improve tax compliance is essential for reducing income inequality.

Our data suggest that the group accountability (GA) audit mechanism induces higher tax compliance than the individual accountability audit mechanism (IA) at the extensive margin but not at the intensive margin. The result is robust in both cases when the group members are anonymous and not anonymous. We also find that the group size has no differential impact within the GA audit mechanism. Our finding suggests that having a joint liability embedded in the audit mechanism can deter certain taxpayers from evading taxes and become fully compliant taxpayers. However, it does not change the amount of evasion if a taxpayer already elects to evade taxes.

The paper is organized as follows. Section 2 provides a literature review of studies that focus on finding effective tax evasion deterrence strategies. Section 3 presents the theoretical model and hypotheses. Section 4 introduces the design of the experiment. Section 5 provides the result. Lastly, section 6 concludes.

1.2 Literature Review

A large part of the tax evasion literature is dedicated to finding efficient deterrence strategies. These include strategic audit selection rules (Alm, Cronshaw, and McKee, 1992; Alm and McKee, 2004: Greenberg 1984), positive reward system (Brockmann, Genschel, and Seelkopf, 2016; Kastlunger et al., 2011), and public shame (Alm et al., 2017; Coricelli et al., 2010). Among the studied deterrence strategies, strategic audit selection rules, and publically shaming the evaders improve tax compliance. The positive reward strategy has mixed results.

Apart from deterrence strategies, several works of literature study how information on other's behavior impact social norms, such as tax compliance (Alm, Bloomquist, and McKee, 2017), giving (Bicchieri et al., 2019), and risky behaviors (Eisenberg, Golberstein, and Whitlock, 2014).

1.2.1 Endogenous Audit Selection

Strategic audit selection rules are found to produce lower tax evasion than simple random audit selection rules in theory (Greenberg, 1984) and laboratory experiments (Alm, Cronshaw, and McKee, 1992; Alm and McKee, 2004). In such audit rules, government tax agency uses the information on taxpayers or tax returns to determine the audit probability instead of randomly selecting taxpayers for audit.

Greenberg (1984) introduces a strategic auditing scheme that depends on the behavior of taxpayers. The proposed auditing scheme is theoretically optimal, given the tax authority's penalty rate and budget constraint. According to this audit strategy, taxpayers are classified into three audit groups with different audit probabilities. Taxpayers in group 1 are audited with some probability, those in group 2 are audited with a lower probability, and individuals in group 3 are audited with certainty. The movement among the groups depends on whether or not the individual is caught cheating on taxes. Taxpayers who are found to be dishonest in group 2 are moved to group 3 and will always remain in group 3. Taxpayers who are audited and reported truthfully in group 2 are moved to group 1. If individuals are found to be cheating in group 1, then they are moved to group 2. However, if individuals are found to be honest, then they remain in group 1. The optimal choice for those in group 1 is to be dishonest, and for those in group 2 is to be honest. In group 3, the optimal choice is also to be honest as the audit probability is 100%, but no one will end up in this group. The paper theoretically shows that in equilibrium, the percentage of tax evaders can be kept arbitrarily small if individuals choose the optimal choice in groups 1 and 2.

Alm, Cronshaw, and McKee (1992) tests a similar audit strategy using a laboratory experiment. They compare three different endogenous audit strategies with the random audit selection rule. The three endogenous audit selection rules examined in their experiment are the cutoff rule, conditional future audit rule, and conditional back audit rule. The cutoff rule audits individuals whose reported incomes fall short from the cutoff income level. Both conditional future and back audit rules target individuals who are already caught cheating. The conditional future audit rule adopts a similar mechanism as in Greenberg (1984), in which individuals who are caught cheating are audited with certainty in the future. On the other hand, in the conditional back audit rule, individuals who are caught cheating at the current period before will be audited for past reported income. Their data shows that the cutoff rule generates the highest tax compliance rate but requires a higher number of audits. Therefore, the cost of the cutoff rule is high.

Another method of endogenous audit uses the information on average reported income (Alm and McKee, 2004). In this audit strategy, those with the highest deviation from the group's average reported income are selected for audit. Their result indicates that taxpayers cannot coordinate at the zero tax compliance equilibrium.

1.2.2 Positive Reward System

Some studies compare a positive rewards system to the punishment system (Brockmann, Genschel, and Seelkopf, 2016; Kastlunger et al., 2011). While the usual approach to deter tax evasion is through imposing a cost to tax evaders, a novel method is through rewarding tax compliance behaviors. The effectiveness of a positive rewards system seems to be mixed. Kastlunger et al. (2011) finds no effect on tax compliance with a positive rewards system. On the other hand, Brockmann, Genschel, and Seelkopf (2016) find that the reward system increased tax compliance for females, but it decreased tax compliance for males. Therefore, the effectiveness of positive rewards for tax compliance behavior is not robust across males and females.

1.2.3 Public Shaming

We induce public shame in our group accountability treatment by having individuals know who their group members are. The effect of public shaming and full disclosure of tax evaders on the compliance rate is explored in a few studies (Alm et al., 2017; Coricelli et al., 2010). In these papers, shame is induced by publically displaying the photo of the tax evader on the computer screen for everyone to see. Both studies find treatment effects. The number of tax evaders and the amount of evaded taxes are found to be reduced when tax evading behavior was made public to others (Coricelli et al., 2010). Similarly, data from Alm et al. (2017) suggests that public shame reduces tax evasion both at the intensive and extensive margin. The effect at the extensive margin is stronger, suggesting that public shaming is more effective in deterring people from electing to evade taxes than increasing compliance of evaders.

1.2.4 Information on Others and Social Norms

The group accountability audit strategy in our experiment is designed such that information on the tax compliance behavior of group members is disclosed when one of the group members is audited. If one group member is caught evading, and as a result, all group members are audited, this signals the individuals that there is a tax evader in the group. On the other hand, if one group member is audited and reported truthfully, then individuals in the group know that there is an honest taxpayer in the group. Hence, the result of the audit will provide some information on the tax compliance behavior of others. Information on other's behavior is shown to influence behavior such as tax compliance (Alm, Bloomquist, and McKee, 2017), giving (Bicchieri et al., 2019), and risky behaviors (Eisenberg, Golberstein, and Whitlock, 2014).

According to Alm et al. (2017), providing information on the tax compliance behavior of neighbors does not always result in improvement in tax compliance, but it does have a significant impact on filing and reporting decisions. Tax compliance is not the only behavior that is influenced by others' behavior. In Bicchieri et al. (2019), the authors find that exposure to information on others' behavior leads to a decline in the prosocial norm of giving in response to norm violators. However, when social proximity is reduced within the group, the reduction in norm compliance is lesser as individuals also respond to norm followers.

1.3. Model and Predictions

1.3.1 The Basic Game

The setup of the tax evasion game in our experiment is similar to the ones in previous tax compliance experiments. The tax rate, auditing scheme, and penalty rate are public information to the subjects. The individual receives a fixed endowment of 500 cents (5 US dollars) from the experimenter. The subjects decide how much of the income to report. Reported income is taxed at 30% and is fixed across treatments. Unreported income is not taxed. The minimum the individual can declare is 0, and the maximum the individual can report is the total endowment, which is 500 cents. After the individuals declare their income and pay their taxes, some individuals are selected for audit.

The auditing scheme depends on the individual's treatment, and a bingo cage is used to determine which individuals to audit. If an individual is audited and has undeclared income, the penalty is imposed on the total tax evaded, which is 100 percent of the unpaid tax. In other words, if an individual is caught in tax evasion, then the taxpayer needs to pay the penalty equal to the amount of unpaid tax in addition to the outstanding tax amount. This whole process is repeated for several rounds. We used z-Tree for our experiment (Fischbacher, 2007).

1.3.2 Theoretical Model

We study two audit strategies, the individual accountability (IA) and the group accountability (GA) audit mechanism and compare the efficiency of these two methods on deterring tax evasion. The IA audit mechanism is the random selection audit rule in which the probability of audit is constant and does not depend on others. On the other hand, an individual's likelihood of audit depends on the behavior of the others in the GA audit mechanism. In the GA audit mechanism, tax authorities audit taxpayers whose group members are caught cheating. We extend the conventional pure random audit selection to a GA audit procedure.

A taxpayer chooses to report income, X, that maximizes the following expected utility function (Yitzhaki, 1974).

$$E[U] = (1-\alpha)U(W-\theta X) + \alpha U\big(W-\theta X-\pi\theta(W-X)\big) \quad (1)$$

Where the probability of audit is α, and the taxpayer's payoff depends on whether they are audited or not. If a taxpayer is not audited, the taxpayer pays the tax rate, θ on reported income,

X (not on actual income, W). However, the taxpayer pays a fine, π on evaded tax amount if selected for audit. The probability of being audited is $\alpha = \frac{\rho}{n}$ in the IA audit mechanism, where ρ is the number of audits set by the tax authority, and n is the total population.

In the GA audit mechanism, the audit probability, α depends on the action of other taxpayers. We first divide the total taxpaying population, n into groups of equal sizes k. We then propose an audit selection mechanism, unlike the random audit rule, where an individual's audit probability depends on the tax compliance of group members as well. In the first part of the GA audit process, we randomly select ω tax returns for audit, where ω is a number less than ρ. The audit is chosen such that not more than one person in each group is audited in the first part. If one of the group members is selected for audit and evaded taxes, then everyone in the group that taxpayer belongs to is audited. However, if the audited group member reported truthfully, then the remaining number of audits, $\rho - \omega$, are selected randomly in the second part. The number of audits in the first part, ω is chosen such that $k\omega = \rho$. This condition ensures that the number of audits does not exceed the maximum audits feasible when all ω subjects who were audited first evaded taxes, and as a result, all members in the groups are audited.

Although the total number of audits is still ρ out of n taxpayers in both IA and GA settings, the probability of audit in the latter case depends on the proportion of cheaters in their own group and other groups. For the clarity of exposition, we focus on a special case where the proportion of cheaters in other groups is constant. Suppose probability of cheating of group members is p, and the probability of cheating of outside group members q. In the GA audit mechanism, one is audited under the following four possible scenarios and α is the summation of the probability of each four scenarios.

Scenario 1: A taxpayer is selected for audit from the ω random draws in the first part. The probability of scenario 1 is $\frac{\omega}{n}$.

Scenario 2: A taxpayer is audited because one of the group members randomly chosen for audit in the first part cheated on taxes. The probability of scenario 2 is $\frac{\omega(k-1)}{n}p$

Scenario 3: A taxpayer is audited because one of the group members randomly selected for audit first reported truthfully and the taxpayer is randomly chosen for audit in the second part of the audit for remaining number of audits. The probability of scenario 3 is $\frac{\omega(k-1)(1-p)}{n}\ \phi(q,\omega)$. The function, $\phi(q,\omega)$ depends on ω, the number of audits in part 1.

$$\phi(q,\omega) = \sum_{m=0}^{\omega-1} \binom{\omega-1}{m} (1-q)^m q^{\omega-1-m} \left(\frac{(m+1)(k-1)}{n-m-1-(\omega-m-1)k}\right) \tag{2}$$

Scenario 4: A taxpayer is selected for an audit because the taxpayer's group was not selected in the first part, but the taxpayer was selected for audit in the second part random draw for remaining audits. The probability of scenario 4 is $\frac{n-\rho}{n}\ \psi(q,\omega)$. The function, $\phi(q,\omega)$ depends on ω, the number of audits in part 1.

$$\psi(q,\omega) = \sum_{m=0}^{\omega} \binom{\omega}{m} (1-q)^m q^{\omega-m} \left(\frac{m(k-1)}{n-m-(\omega-m)k}\right) \tag{3}$$

To summarize, one's audit probability depends on the actions of group members' and other groups' members.

The audit probability in the GA is the same as the IA when the probability of cheating behavior in the group is same as the probability of cheating behavior in the other groups, $p = q$. If there is more evading behavior within the group than other groups, $p > q$ then the audit probability is higher in the GA than the IA. However, if the probability of cheating behavior in other groups is higher than own group, $p < q$ then the audit probability is lower in the IA case than the GA case.

Theoretical Result 1

Suppose individuals are assumed to be selfish agents. Tax evasion under individual and group accountability rules is the same conditional on the belief that $p = q$. Consequently, one's decision on how much to report in the GA setting depends on the distribution of cheaters inside and outside the group.

Suppose individuals are prosocial and care about other's utility. One might expect taxpayers who care about their group members are more likely to report truthfully under the GA audit mechanism than the IA audit rule, given the same audit probability. For tractability, we use an additive specification to model this prosocial behavior. Let, $\lambda_s(k)$ denote a disutility function, where s is a binary measure of prosociality, in which $s = 1$ indicates a higher level of prosociality and $s = 0$ indicates a lower level of prosociality. We assume that $\lambda_s(k)$ is an increasing function of k. The disutility function is additional cost to cheating that is not captured by lost in income from being audited. The expected utility of income, $E(U)$, is affected by how much one underreports but the disutility of cheating is not dependent on the amount of underreporting but on whether one gets caught cheating or not. Then the new utility function under the GA audit rule with prosociality is presented by

$$E[V] = E(U) - \frac{\omega}{n}\lambda_s(k) \tag{4}$$

The disutility function, $\lambda_s(k)$ is equal to 0 if one reports their income truthfully $X = W$, and is positive if one underreports their income, $X < W$, and triggers audits for all group members. The total disutility depends on the number of people in the group because an increase in the number of people in the group means that one's cheating behavior will affect more people if caught. The audit probability of group members from one's tax evasion is equal to the likelihood of the evader in the group being audited in the first part random draw, which is $\frac{\omega}{n}$. We assume that $\lambda_{s=0}(k) < \lambda_{s=1}(k)$, that is the more prosocial one is, the higher is the disutility of externalities on other group members. The model with prosociality predicts a higher compliance rate at the extensive margin under the GA audit mechanism than the IA audit mechanism, conditional that the audit probabilities being the same.

The optimal report income value, $X*$ is implicitly determined by

$$\theta(1-\alpha)U'(W-\theta X*) = \theta(\pi-1)\alpha U'\big(W-\theta X* - \pi\theta(W-X*)\big) \tag{5}$$

The first order condition is the same across the models with and without prosociality, which implies $X*$ stays the same. However, the condition to cheat or not is different across the two models. Suppose $X*$ is the optimal report income value. A taxpayer will evade taxes if $E(U(X*)) - \frac{\omega}{n}\lambda_s(k) > E(U(X=W))$, which has higher threshold to evade taxes compared to the model with no prosociality, $E(U(X*)) > E(U(X=W))$. The threshold becomes higher as the group size, k, increases. The theoretical result implies that there will be no change in the intensity of cheating in the prosociality model if the taxpayer elects to evade taxes, conditional that the audit probabilities are the same across the two models.

Theoretical Result 2

Tax evasion is lower under group accountability rule over individual accountability rule at the extensive margin when individuals are assumed to be prosocial, and audit probability is the same.

1.4. Experiment Design

1.4.1 Implementation Procedure

We recruited undergraduate students from Georgia State University as subjects for the experiment. Communication among subjects was not allowed during the experiment. At the start of each treatment, the experimenter provided a paper copy of the instruction, and the computer screen displayed the same instruction. After the participants completed reading the instruction, the experimenter provided a summary of the direction. The students participated in two practice rounds with predetermined audit results and hypothetical payoffs before starting the actual round. The treatments differed by the type of audit strategy, group size, and anonymity.

The experiment consisted of two parts. The first part was the tax compliance game, and the second part was an investment game. The tax compliance game had four stages: two stages of IA treatments and two stages of GA treatments. Subjects faced different group members between the stages of GA treatments, but the members remained fixed within each stage. We varied the order of the treatments across the sessions to test for order effects. In some sessions, the first two stages were IA treatments, and the last two stages were GA treatments. In other sessions, stages one and three were IA treatments and stages two and four were GA treatments. Each stage consisted of seven rounds of the same treatment, and subjects were aware of this information.

After completing the tax compliance game, the subjects took part in an individual investment game designed to capture risk attitudes. Lastly, subjects participated in a survey after completing all tasks. The survey included questions on demographic characteristics and general views on taxes. At the end of the experiment, subjects were paid according to their performance.

1.4.2 Payment Protocol

We randomly selected one of the four stages in Part 1 for subject payment. The subjects were paid total earnings from all seven rounds of the selected stage. At the end of Part 1, the experimenter randomly drew one bingo ball from a bag with bingo balls numbered one to four. The number on the bingo ball determined the stage subjects were paid. In Part 2, the subjects were given an opportunity to invest the money earned in Part 1. The final payment depended on the investment choice.

1.4.3 Treatments

Table 1. Treatments

Treatments	**Description**	**Anonymity**	**Group Size**	**Audit Rate**
IA 1	Random audit rule	Yes	0	20%
IA 2	Random audit rue	Yes	0	33%
GA	Audit depends on the behavior of group members and group members are anonymous	Yes	3	20%
GA Not Anonymous	Audit depends on the group members and subjects are provided with the seat number of each group member.	No	3	20%
GA Not Anonymous and Small Group Size	Audit depends on the group members and subjects are provided with the seat number of each group members.	No	3	33%
GA Not Anonymous and Big Group Size	Audit depends on the group members and subjects are provided with the seat number of each group members.	No	6	33%

This experiment is designed to provide information on the performance of two different types of audit strategies. Table 1 provides the summary of the treatments. In the individual accountability (IA) treatment, the audit selection is random. In the group accountability (GA) treatment, one's audit also depends on whether other members in the group are audited and evaded taxes. Hence, the audit probability depends on others' actions. Within the GA treatment, we vary group size and anonymity. The two different group sizes are three members per group and six members per group. In the anonymous GA treatment, the subjects do not know who their group members are. On the other hand, in the non-anonymous treatment, the subjects know the faces of their group members and where their group members are sitting. Each subject in our experiment participated in two of the same IA treatments and two of the same GA treatments. No subject participated in two different types (anonymity, group size) of GA treatments.

Both IA and GA treatments have two auditing procedures within each round. The purpose of having two steps in the auditing process is to keep the number of audits equal in all treatments and to keep the auditing process the same across treatments. This allows us to compare the effectiveness of audit strategies given the same budget constraint of tax authorities. In the real world, tax authorities have fixed budgets from the government and can only audit a limited number of tax returns.

Let the maximum number of audits be ρ (3 and 6 in our experiment), and ω (1 and 2 in our experiment) is a number less than ρ. As presented in Table 2, the first part of the auditing process involves randomly selecting the ω number of tax returns for audit in IA treatment and GA treatment. No more than one person from the same group is audited in the first part of the GA treatment. The second part of the auditing process is where GA treatment differs from IA treatment. For the IA case, the audit selection process is again random in the second part. Therefore, $\rho - \omega$ number of tax returns not audited are randomly selected for audit. The audit selection in the group accountability case depends on the result of the first part of the audit. If randomly selected individuals in the first part of the audit evaded taxes, then all members in the evaders' groups are audited. If at least one of the selected individuals in the first part of the audit reported truthfully, the remaining number of audits are chosen randomly from unaudited tax returns. Suppose everyone who was randomly selected for audit in the first part of the auditing process reported truthfully. In that case, ρ− ω number of tax returns are randomly selected for audit from tax returns that are not audited, just as in the IA case.

Table 2. Audit Strategy

<table>
<tr><td colspan="5">Number of subjects per session: 15
Total limited number of audits: 3
Group size: 3</td></tr>
<tr><td></td><td>Number of Audits</td><td>Individual Accountability</td><td colspan="2">Group Accountability</td></tr>
<tr><td>Part 1</td><td>1</td><td>Random selection</td><td colspan="2">Random selection</td></tr>
<tr><td rowspan="2">Part 2</td><td rowspan="2">2</td><td rowspan="2">Random selection</td><td>If caught evading</td><td>Everyone in the group is audited</td></tr>
<tr><td>If reported truthfully</td><td>Random selection</td></tr>
<tr><td colspan="5">Number of subjects per session: 18
Total limited number of audits: 6
Group size: 3</td></tr>
<tr><td></td><td>Number of Audits</td><td>Individual Accountability</td><td colspan="2">Group Accountability</td></tr>
<tr><td>Part 1</td><td>2</td><td>Random selection</td><td colspan="2">Random selection from two different groups</td></tr>
<tr><td rowspan="2">Part 2</td><td rowspan="2">4</td><td rowspan="2">Random selection</td><td>If caught evading</td><td>Select from groups that got caught evading in Part 1</td></tr>
<tr><td>If reported truthfully</td><td>Random selection</td></tr>
<tr><td colspan="5">Number of subjects per session: 18
Total limited number of audits: 6
Group size: 6</td></tr>
<tr><td></td><td>Number of Audits</td><td>Individual Accountability</td><td colspan="2">Group Accountability</td></tr>
<tr><td>Part 1</td><td>1</td><td>Random selection</td><td colspan="2">Random selection</td></tr>
<tr><td rowspan="2">Part 2</td><td rowspan="2">5</td><td rowspan="2">Random selection</td><td>If caught evading</td><td>Everyone in the group is audited</td></tr>
<tr><td>If reported truthfully</td><td>Random selection</td></tr>
</table>

1.4.4.1 Individual Accountability

The IA treatment is simply a random audit selection rule, as mentioned previously. In this treatment, subjects face a fixed number of purely random audits. A bingo cage determines the audit selection. If any of the numbers drawn from the bingo cage match the subject's assigned number, then they are audited. The audit process is divided into two parts, but the total audit probability is kept constant. For example, suppose the total number of subjects per session is 15 individuals and three tax returns are randomly selected for audit in each round. In the first part of the auditing process, ω number of tax returns are randomly selected for audit. In the second part of the auditing process, 3−ω are randomly audited as well. How ω is chosen depends on the group size of the associated group accountability treatment. The total number of audits is kept at three in both IA and compared GA treatment.

Table 2 shows that we test three variations of individual accountability treatment with audit probabilities of either 3 out of 15 subjects or 6 out of 18 subjects. The reason for this is to match the auditing process with the compared group accountability treatment. One variation is auditing one tax return in the first part and auditing two more in the second part of the auditing process from 15 subjects. The second variation is auditing one tax return in the first part and auditing five more in the latter part of the auditing process from 18 subjects. In the third variation, we audit two individuals in the first part and audit four more in the second part.

1.4.4.2 Group Accountability

Subjects in GA treatment are divided into groups. This treatment has two parts, as in the IA case. The first part is the random audit selection performed by the bingo cage. In the treatment with a 20% audit rate, we audit three subjects out of 15 in total. Same as the IA case, one tax return is randomly selected for audit first. The second stage is when the group liability comes in. In treatments with three individuals per group, if one of the selected group members for audit evaded taxes, everyone in the evader's group is audited. If the chosen group member reported truthfully, then the remaining two audits are randomly chosen from the remaining 14 individuals using the bingo cage. The total number of audits in one round is three in both IA and GA treatments, with an audit rate of 20%. Table 2 shows all configurations for different group sizes and audit rates.

1.4.4.3 Social Distance

The social distance varies within the GA treatment to examine how social distance influences tax-paying behavior when group liability is embedded in the audit selection process. We vary social distance through two methods, anonymity and group size. In the anonymity treatments, subjects stay anonymous during the experiment in GA treatments. Individuals do not know who their group members are. Whereas in the treatment without anonymity, subjects are provided with the seat number of their group members. The experimenter also asks the groups to stand up one after the other, so the students have the chance to see their group members and know where their group members are sitting. This process makes non-anonymity more salient.

We have two different group sizes in the GA treatment. The smaller group size treatment has three members, and the bigger group has six members. The audit rate is 33% in both treatments. For the treatment with a group size of three, two tax returns from 6 different groups are first randomly selected for audit. If both of the selected tax returns cheated on taxes, then all members are selected for audit. If both tax returns did not evade taxes, the remaining four audits are chosen randomly using the bingo cage. If only one of the two subjects selected for audit evaded taxes, then members of the evader's group are audited and the remaining two audits are randomly selected from other groups. As shown in Table 2, in the larger group size treatment, one tax return is randomly selected for audit first. If the selected person is evading taxes, everyone in the group is selected for audit. If the person chosen for the audit reported truthfully, then the remaining five audits are randomly selected. The group size treatments were conducted with non-anonymity.

We say that social distance is higher when anonymity is preserved, and social distance is lower when anonymity is not preserved. When the group size is larger, one's action influences the audit probability of more people, reflecting lower social distance.

1.4.4 Investment Task

At the end of the tax compliance task in Part 1, subjects are offered the opportunity to invest the money they earned in Part 1. The purpose of the investment task is to gain some insight into subjects' risk attitudes. Since tax compliance behavior is linked to risk preference, we try to elicit risk attitudes using a simple investment task. The subjects first choose the side of the coin they want to bet and then select the amount they want to invest. After everyone submits their coin choice and investment amount, the experimenter tosses a coin. If the coin toss matches the side an individual placed the bet on, then the individual receives 50% more of the investment. However, if the coin lands on the opposite side, the individual loses 50% of the investment. We expect risk averse subjects not to invest, risk neutral to be indifferent, and risk loving to invest.

1.5 Results

1.5.1 Aggregate Treatment Effect

A total of 231 Georgia State University students participated in the experiment. Each subject participated in two stages of IA treatment and two stages of GA treatment, with seven decision rounds in each stage. From each subject, we collected a total of 28 decisions. Table 3 provides the demographic characteristics of the subjects across sessions. Each session does not have an identical composition of subjects, but the differences are not incredibly substantial for most demographic variables. Table 4 provides information on the general view on taxes of the subjects and their decisions on the investment task.

Table 3. Subject Demographic Characteristics

Variable	Anonymous Low Audit	Not Anonymous Low Audit	Not Anonymous High Audit Group Size 6	Not Anonymous High Audit Group Size 3
Number of Subjects	60	45	72	54
Mean Age (years)	21.11	20.50	21.02	21.21
Female (percent of total)	70.00	75.56	62.50	59.26
Parent Income (mean income group)	$45001-$60000	$60001-$75000	$45001-$60000	$45001-$60000
Black/African American (percent of total)	55.00	66.67	75.00	64.81
Religion (percent of total)				
- Christianity	48.33	66.67	63.89	48.15
- Nonreligious	28.33	13.33	13.89	31.48
- Prefer not to answer	10.00	8.89	12.50	5.56
Not Working (percent of total)	33.33	24.44	23.61	27.78
Senior (percent of total)	43.33	37.78	44.44	37.04
GPA (percent of total)				
- Below 3.00	28.34	28.89	31.95	22.22
- 3.00 to 3.69	46.67	44.44	44.44	57.41
- Above 3.69	25.00	20.00	19.44	18.52
Major (percent of total)				
- Econ/Business	23.34	20.00	18.06	24.07
- STEM	38.33	42.22	45.84	46.30
- Other	38.34	37.78	36.11	29.62
Participated in an Experiment (percent of total)	78.33	80.00	59.72	77.78
Participated in an Election (percent of total)	46.67	44.44	52.78	50.00
Liberal (percent of total)	68.33	55.56	58.33	55.56
Filed Taxes (percent of total)	48.33	60.00	56.94	66.67

Table 4. Subject View on Tax and Investment Decision

Variable	Anonymous Low Audit	Not Anonymous Low Audit	Not Anonymous High Audit Group Size 6	Not Anonymous High Audit Group Size 3
Tax View				
“It is important to pay all the taxes you owe in order to be a good citizen.” (percent of total)				
- Agree Strongly	38.33	33.33	30.56	33.33
- Agree Slightly	41.67	53.33	47.22	44.44
- Disagree Slightly	16.67	8.89	22.22	14.81
- Disagree Strongly	3.33	4.44	0.00	7.41
“Not reporting all the income on your tax returns is morally acceptable.” (percent of total)				
- Agree Strongly	6.67	8.89	5.56	5.56
- Agree Slightly	26.67	15.56	34.72	25.93
- Disagree Slightly	46.67	46.67	36.11	42.59
- Disagree Strongly	20.00	28.89	23.61	25.93
Investment Decision				
Risk Averse Subjects (percent of total)	3.33	4.44	6.94	7.41

Table 5 summarizes statistics on the compliance rate organized by audit rate, anonymity, and group size. We first examine the general overview of average treatment effects. Later we provide regression analysis that takes account of demographic characteristics. Our dependent variables of interest are the compliance rate and the full compliance rate. The compliance rate is reported income divided by actual income for each subject in each round. The full compliance rate is an indicator variable that equals 1 when a subject fully reported income and 0 when a subject does not fully report income. The overall mean compliance rate was 45.36%, and the full compliance rate was 26.90% across all sessions, subjects, treatments, and rounds. These numbers were lower compared to other studies (Alm, Cox, and Sadiraj, 2019; Alm et al., 2017)

Table 5. Descriptive Statistics of Compliance

	Obs.	Reporting Compliance Rate (mean)	Full Compliance Rate (percent of total)
All Sessions	6,468	45.36%	26.80%
Low Audit IA	1,470	35.12%	17.35%
Low Audit GA	1,470	43.33%	29.32%
Low Audit GA Anonymous	840	41.81%	26.79%
Low Audit GA Not Anonymous	630	45.35%	32.70%
High Audit IA	1,764	46.28%	22.90%
High Audit GA	1,764	54.68%	36.51%
High Audit GA Big Group Size	1,008	59.75%	41.27%
High Audit GA Small Group Size	756	47.91%	30.16%

1.5.1.1 Audit Rate Effect

Subjects increased tax compliance when audit rates were increased under IA. An increase in audit rate from 20% to 33.33% increased the average compliance rate by 31.78% (from 35.12% to 46.28%, $p \approx 0.0049$)[2] and full compliance rate increased by 31.99% (from 17.35% to 22.90%, $p \approx 0.2834$)[3]. Similarly in the GA treatment, the mean compliance rate increased by 26.19% (from 43.33% to 54.68%, $p \approx 0.0096$). The full compliance rate increased by 24.52% (from 29.32% to 36.51%, $p \approx 0.0534$). This result was from pooling all GA treatments.

1.5.1.2 Group Accountability Effect

The effect of GA on tax compliance was positive regardless of group size and anonymity. This finding was robust across when audits are low and high. When the audit rate was low (20%), the mean compliance rate increased by 23.38% (from 35.12% in IA to 43.33% in GA, $p <$

[2] Audit rates vary across subjects, so we use a Mann-Whitney test for the comparison. We take the average compliance rate of each treatment to create one data per treatment and subject.

[3] We calculate the percentage of full compliance of each treatment to create one data per treatment and subject. For example, if a subject reported full income across all rounds in one treatment, the full compliance rate is 1. On the other hand, if a subject reported less than the full income across all rounds in one treatment, the full compliance rate is 0. If a subject sometimes reported full income and sometimes did not in a treatment full compliance rate takes a value between 0 and 1. The p-values are from the Mann-Whitney test.

$0.001)$[4], and the full compliance rate increased by 68.99% (from 17.35% in IA to 29.32% in GA, $p < 0.001$). For sessions in which the audit rate was higher (33.33%), the mean compliance rate increased by 18.15% (from 46.28% in IA to 54.68% in GA, $p < 0.001$), and the full compliance rate increased by 59.34% (from 22.90% in IA to 36.51% in GA, $p < 0.001$).

1.5.1.3 Anonymity and Group Size Effect

Within the GA treatment, we vary anonymity and group size. If subjects are perfectly randomly assigned to different sessions, we should not observe differences in the average compliance rate in the IA treatment across sessions. However, the average compliance rate during the IA treatment varied significantly by session. A more accurate measure of anonymity and group size effect is comparing within subjects and not between subjects. More specifically, we take the difference between the IA treatment and the GA treatment in the tax compliance rate and then compare the differences[5].

The anonymous GA treatment was only conducted with a low audit rate. Only data from sessions with low audit rates were used to compare the GA treatment effect between when group members are anonymous and when group members are not anonymous. The mean difference between the IA treatment and GA treatment is 6.19 percentage points for sessions with the anonymous case (35.62% in IA and 41.81% in GA Anonymous) and 10.89 percentage points for sessions with the non-anonymous case (34.46% in IA and 45.35% in GA Non-Anonymous). However, the distributions of the differences are not significantly different from each other, according to the Mann-Whitney test ($p \approx 0.1336$). The full compliance rate increased by 9.17

[4] Since subjects participated in both individual accountability and group accountability, we use the Wilcoxon test for the comparison.

[5] First, we calculate the average compliance rate of each treatment. We then take the difference in average compliance rate between the IA treatment and the GA treatment. This gives us one observation per subject. The difference between the IA treatment and the GA treatment is used to compare the treatment effects.

percentage points in the GA anonymous case (17.62% in IA and 26.79% in GA anonymous). In the GA non-anonymous case, the full compliance rate increased by 15.72 percentage points (16.98% in IA to 32.70% in GA non-anonymous). The difference in the differences is 6.55 percentage points (p≈ 0.0572). The finding from simple descriptive statistics suggests that the GA audit mechanism increases the compliance rate and full compliance rate. However, the answer to the research question of whether non-anonymity raises the tax compliance rate more than when group members are anonymous under the GA audit mechanism remains inconclusive.

We also vary the group size within the GA treatment to test whether group sizes result in different compliance rates under the GA audit mechanism. The mean difference between the IA treatment and GA treatment is 7.71 percentage points for GA treatments with the group size of six (52.04% in IA and 59.75% in GA big group) and 9.29 percentage points for GA treatments with a group size of three (38.62% in IA and 47.91% in GA small group). According to the Mann-Whitney test (p≈ 0.1844), the distributions of the differences are again not significantly different from each other. The full compliance rate increased by 14.78 percentage points in GA treatment with a group size of six (26.49% in IA and 41.27% in GA big group). In the GA treatment with a group size of three, the full compliance rate increased by 12.04 percentage points (18.12% in IA to 30.16% in GA non-anonymous). According to the Mann-Whitney test (p≈ 0.5712), the distributions of the differences are not significantly different from each other.

We conclude that an increase in audit rate and GA audit mechanism increases the compliance rate. Within the GA treatments, non-anonymity did result in a statistically significant higher treatment effect in the compliance rate than the anonymous case. Regarding variations in group sizes, GA treatment with a group size of three has a higher treatment effect in the compliance rate compared to GA treatment with a group size of six, when the number of audits is kept the same (6 audits out of 18) across the two group sizes. On the other hand, the treatment effect in the full compliance rate is higher in GA treatment with a group size of six than in GA treatment with a group size of three. However, the distributions are not statistically different from each other in both cases.

1.5.1.4 Treatment Effect at the Intensive Margin

Table 6 shows the effect of group accountability on tax compliance at an intensive margin. We excluded observations with a compliance rate of 100% from the descriptive statistics. When we only look at individuals who evaded taxes, we observe the opposite effect of GA treatment on tax compliance. When the audit rate was low (20%), the mean compliance rate decreased by 7.38% (from 21.50% in IA to 19.82% in GA, $p \approx 0.8202$). For sessions in which the audit rate was higher (33.33%), the mean compliance rate decreased by 5.67% (from 30.33% in IA to 28.61% in GA, $p \approx 0.5626$). This finding supports our theoretical result from the expected utility model with prosociality: GA treatment increases tax compliance at the extensive margin but not at the intensive margin.

Table 6. Descriptive Statistics of Compliance at the Intensive Margin

	Obs.	Reporting Compliance Rate (mean)
All Sessions	4,734	25.35%
Low Audit IA	1,215	21.50%
Low Audit GA	1,039	19.82%
Low Audit GA Anonymous	615	20.52%
Low Audit GA Not Anonymous	424	18.80%
High Audit IA	1,360	30.33%
High Audit GA	1,120	28.61%
High Audit GA Big Group Size	592	31.46%
High Audit GA Small Group Size	528	25.42%

1.5.2 Regression Analysis

We now examine the individual data by using the estimation of tobit model and probit model. Table 7 presents the regression results from 2 models. The first model is estimations from tobit regressions with an upper bound at one and a lower bound at 0 for the dependent variable, compliance rate. The compliance rate is defined by the ratio of reported income to actual income. The second model is estimations from probit regressions with binary variable 0 if the compliance rate is less than 100% and one if the compliance rate is 100%. According to our theoretical model with prosociality, an individual gains disutility if they are caught evading, and as a result, all group members are audited. This disutility changes non-compliers to full compliers. The estimation from the probit regression allows for detecting such changes in behavior.

Table 7. Effects of Treatment Variables on Choices – Censored and Binary Regression

All Observations		
Variable	Model (1) Panel tobit with random effects	Model (2) Panel probit with random effects
Group	0.179***	0.725***
	(0.021)	(0.092)
Anonymous	-0.097***	-0.182
	(0.036)	(0.169)
Low Audit	-0.159*	-0.327
	(0.093)	(0.203)
Small Group	-0.014	-0.101
	(0.036)	(0.153)
Constant	0.462***	-1.180***
	(0.063)	(0.135)
σ_α	0.688	
	(0.037)	
σ_ε	0.521	
	(0.007)	
$ln(\sigma_\alpha)$		0.687
		(0.161)
χ^2	114.28	99.39
(Prob > χ^2)	0.000	0.000
Log likelihood	-4888.675	-2497.935
Observations	6,468	6,468

Note: Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Model (2) presents estimation results from a panel tobit regression with random effects. The tobit model is represented as:

$$CR_{it}^{*} = X_{it}'\beta + \alpha_i + \varepsilon_{it} \tag{6}$$

The dependent variable CR_{it}^{*} is the latent compliance rate of subject i in round t, and the observed compliance rate is given by the equation below.

$$CR_{it} = \begin{cases} 1 \; if \; CR_{it}^{*} \geq 1 \\ CR_{it}^{*} \; if \; 0 < CR_{it}^{*} < 1 \\ 0 \; if \; CR_{it}^{*} \leq 0 \end{cases} \tag{7}$$

Model (4) shows estimation result from a panel probit regression with random effects. The probit model is similar to the expression above. The only difference is that the dependent variable is a binary variable that equals 1 when the compliance rate is 1 and 0 otherwise. The observed full compliance rate is given by the equation below.

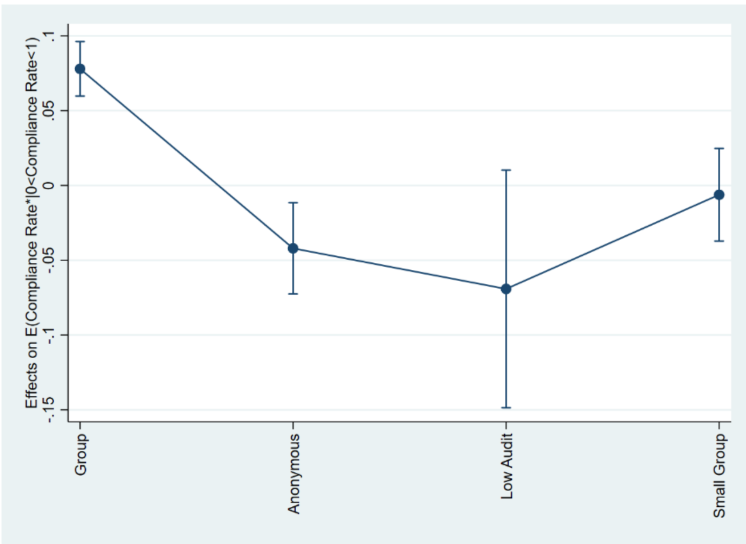
$$FR_{it} = \begin{cases} 1 \; if \; CR_{it}^{*} \geq 1 \\ 0 \; if \; 0 \leq CR_{it}^{*} < 1 \end{cases} \tag{8}$$

The regressor vector X_{it} contains four dummies for the treatment and an intercept. The first dummy variable, *Group,* equals 1 for treatments with GA audit mechanism and 0 for treatments with IA audit mechanism. The second dummy variable, *Anonymous,* is equal to 1 for the treatments with anonymous group members and 0 for others. The third dummy variable is *Low Audit,* which equals to 1 if the audit probability is 20% and 0 otherwise. The fourth dummy variable, *Small Group,* equals 1 if the group size is three and the audit rate is 33% as the comparison is treatments with group size of six and an audit rate of 33%. The variable is equal to

0 for others. The variable α_i represents random effects for the panel tobit model, which is assumed to be normally distributed with mean zero and variable of σ_α^2.

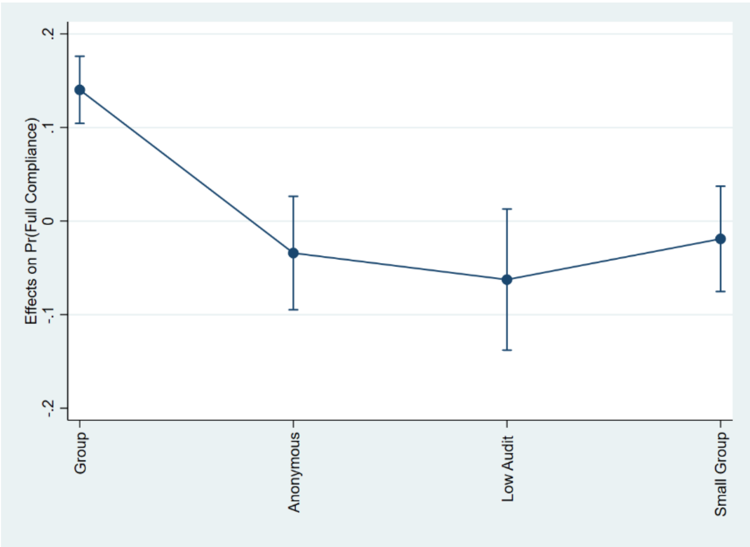
Model (1) estimates that compliance rate increases under group accountability audit mechanism, and compliance rate decreases when the audit probability is lower. It also finds a significant negative effect for *Anonymous* but a nonsignificant negative effect for *Small Group*. We compute the mean of the marginal effects on compliance behavior with respect to changes in the four dummy variables, *Group*, *Anonymous*, *Low Audit*, and *Small Group*. Figure 1 shows the marginal effects from the panel tobit model. In the panel tobit model, the marginal effect of *Group* is 7.80% ($p < 0.001$). For *Low Audit*, the marginal effect is -6.92% ($p \approx 0.088$) and -4.21% ($p \approx 0.007$) for *Anonymous*. The marginal effect for *Small Group* is not statistically significant.

Figure 1. Marginal Effects – Panel Tobit



The probit model allows us to examine how effective the GA audit mechanism is in turning non-compliers into full compliers. Model (2) estimates a significant positive effect on the full compliance rate for *Group*, which indicates that the group accountability mechanism effectively turns non-compliers to full compliers. This finding is consistent with our theoretical result from the model with prosociality. We do not find a significant effect for *Low Audit,* which suggests that high audit probability is less effective in turning non-compliers into full compliers. Figure 2 presents the marginal effects from the panel probit model. The marginal effects from the panel probit model indicates that the probability of full compliance increased by 14.02% ($p < 0.001$) for *Group.* The marginal effects for other variables were not statistically significant.

Figure 2. Marginal Effects – Panel Probit



According to our theoretical result from the expected utility model with prosociality, tax evasion is lower under group accountability rule over individual accountability rule at the extensive margin and not at the intensive margin. We excluded observations with a compliance rate of 100% from our regression analysis to test this theoretical result. The regression results are shown in Table 8. We observe a statistically significant negative effect for *Group* and a significant positive result for *Small Group,* which supports our theoretical result that the GA audit mechanism is effective at the extensive margin and not intensive margin. In fact, the GA mechanism reduced the compliance rate at the intensive margin.

Table 8. Effects of Treatment Variables on Compliance at the Intensive Margin

Variable	Model (3) Panel tobit with random effects
Group	-0.033**
	(0.013)
Anonymous	-0.024
	(0.022)
Low Audit	-0.080
	(0.053)
Small Group	0.050**
	(0.022)
Constant	0.206***
	(0.036)
σ_α	0.379
	(0.021)
σ_ε	0.274
	(0.004)
F	
(Prob > F)	
χ^2	21.00
(Prob > χ^2)	0.000
Log likelihood	-1423.439
Observations	4,734

Note: Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Figure 3. Marginal Effects at Intensive Margin – Panel Tobit

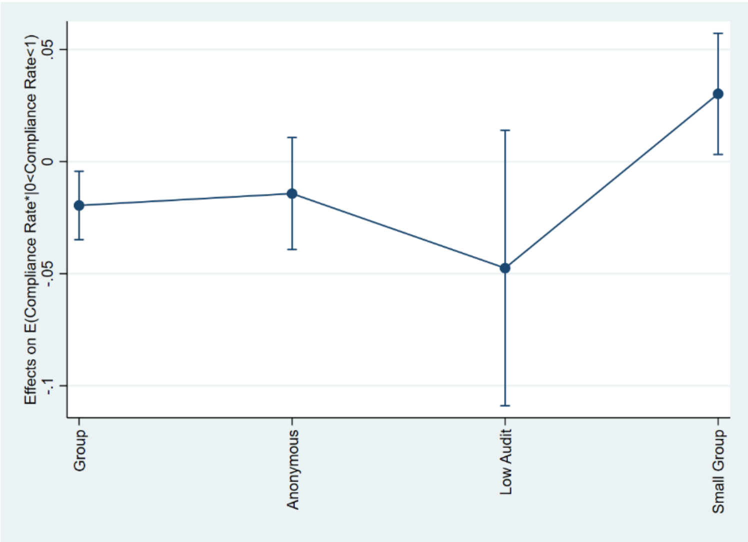


Figure 3 shows the marginal effects for model (3). The panel tobit model suggests that the marginal effects of *Group* is decrease in compliance rate by 1.96% (p≈ 0.012) and the marginal effect of *Small Group* is increase in compliance rate by 3.02% (p≈ 0.028). The marginal effects for other variables are not statistically significant.

We further test our results by including more variables in the regressor, and the regression estimates are presented in Table 9. The additional variables are *More Cheater in Group*, a dummy variable that equals 1 when in the previous round there were more cheaters in the group than other groups ($p_{t-1} > q_{t-1}$), *Female*, a dummy variable for female, an interaction term *Female*Group, Audited Last Round*, a dummy variable that equals if the subject was audited in the previous round, and *Accumulated Payoff,* which is the accumulated earnings.

The results in Table 9 are overall consistent with the results in Table 7 for tobit models. Female subjects are found to be more compliant than male subjects, which is consistent with the literature (Alm, Jackson, and McKee, 1992). The positive coefficient of the interaction term *Female*Group* is statistically significant only in the model (4), which suggests that females comply more under *Group* treatment compared to males. The *Audited Last Round* variable has a significant negative coefficient which indicates that subjects who were audited last round complied less in the following round. For the probit model, the results in Table 9 are consistent with results in Table 7. The statistically significant negative coefficients of *Accumulated Payoff* in models (4) and (5) show that we do have a wealth effect. In model (5), the negative coefficient of *More Cheater in Group* is statistically significant, which indicates that full compliance rate decreased under GA treatment when there were more cheaters in the group compared to other groups. This result hints that subjects retaliated cheating behavior of group members by cheating. However, this result is subject to further analysis in the future.

Table 9. Censored and Binary Regression with Control Variables

All observations		
Variable	Model (4) Panel tobit with random effects	Model (5) Panel probit with random effects
Group	0.161***	0.735***
	(0.031)	(0.134)
Anonymous	-0.098***	-0.191
	(0.035)	(0.166)
Low Audit	-0.184**	-0.333
	(0.090)	(0.205)
Small Group	-0.004	-0.097
	(0.036)	(0.155)
More Cheater in Group	-0.055	-0.117*
	(0.023)	(0.070)
Female	0.216**	0.113
	(0.097)	(0.210)
Female*Group	0.053*	0.050
	(0.032)	(0.142)
Audited Last Round	-0.080***	-0.081
	(0.017)	(0.066)
Accumulated Payoff	-0.012***	-0.022***
	(0.001)	(0.003)
Constant	0.531***	-0.910***
	(0.086)	(0.179)
σ_α	0.667	
	(0.036)	
σ_ε	0.510	
	(0.007)	
$ln(\sigma_\alpha)$		0.681
		(0.161)
F		
(Prob > F)		
χ^2	317.30	165.27
(Prob > χ^2)	0.000	0.000
Log likelihood	-4786.720	-2462.501
Observations	6,468	6,468

Note: Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

We excluded observations with a compliance rate of 100% from our analysis and repeated the analysis of model (6). The results are shown in Table 10. Similary with Table 8, the negative coefficient for *Group* is statistically significant, which again supports our theoretical result that the GA audit mechanism effectively deters taxpayers from cheating. However, the GA audit mechanism is not effective in reducing the amount of underreporting for taxpayers who already elected to evade taxes. Unlike the result in Table 9, the coefficient for *More Cheater in Group* is positive. Although the estimation is statistically insignificant, it implies that individuals who already elected into cheating view more cheaters in the group as higher audit probability. Therefore, underreport their income at a lower intensity.

Table 10. Censored Regression with Control Variables at the Intensive Margin

Excluding observations with compliance rate of 100%	
	Model (6)
Variable	Panel tobit with random effects
Group	-0.089***
	(0.020)
Anonymous	-0.029
	(0.021)
Low Audit	-0.095*
	(0.052)
Small Group	0.061***
	(0.022)
More Cheater in Group	0.005
	(0.014)
Female	0.108*
	(0.055)
Female*Group	0.080***
	(0.020)
Audited Last Round	-0.060***
	(0.010)
Accumulated Payoff	-0.006***
	(0.001)
Constant	0.260***
	(0.049)
σ_α	0.369
	(0.020)
σ_ε	0.268
	(0.004)
F	
(Prob > F)	
χ^2	210.56
(Prob > χ^2)	0.000
Log likelihood	-1329.334
Observations	4,734

Note: Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

1.6 Conclusion

This paper varies audit mechanisms while keeping the total number of audits the same to compare efficiency using laboratory methods. We find evidence that having group accountable audit probability increases both compliance rate and full compliance rate than random selection audit mechanism, given the same number of audits.

However, we find conflicting results on whether knowing where the group members are sitting induces a higher compliance rate and a higher full compliance rate than not knowing the group members in the GA audit mechanism. It is not clear if this result is due to the lack of saliency in non-anonymity or from weakly induced closer social distance by providing seat numbers. Some subjects did not look around the room to see where their group members were sitting when asked to check their group members. Other subjects looked around to see who their group members are and showed their faces to their group members. This variation in the usage of seat numbers may have caused inconsistency in our results.

We also find inconsistent results on the effect of group size on compliance rate and full compliance rate in the GA audit strategy. The varying results indicate that having bigger group sizes in the GA audit strategy does not increase the compliance rate more than having a smaller group size in the same audit mechanism as long as the number of audits is the same. This finding implies that varying group size does not close the gap of social distance among the group members.

The IRS uses two main audit selection methods; one is a random selection or computer screening, in which the computer detects tax returns that are out of the "norm." The second method is related examinations, in which tax returns are audited if involve in issues or transactions with other taxpayers whose returns are selected for audit. Our results contribute to answering whether such audit method is effective in deterring tax evasion. The finding is that such a joint liability audit selection method is more effective in increasing the compliance rate and turning non-compliers to full compliers than the random audit selection method. Introducing GA in the audit selection process can be a powerful tool to deter tax evasion.

In this paper, the subjects are exogenously selected into groups. In the future, we would like to explore endogenous selection into groups in the GA audit mechanism. In this strategy, group members are determined by the compliance rate of the subjects. Subjects who evade taxes will be grouped with other subjects who are also evaders. On the other hand, subjects who are not evaders will be grouped with other subjects who are also evaders. We believe that studying variations of the GA audit mechanism can contribute to finding a cost-efficient audit strategy. Another variation of the GA audit mechanism that can be tested in the future is to select audits from new groups in the second part of the audit process when a taxpayer selected for audit first reported the truth. This approach will elicit more prosocial behavior because one's truthful behavior can help the group members.

Chapter 2 Tax Framing in Matching and Rebate Subsidy[6]

2.1 Introduction

The action of giving is prevalent in everyday life. It is common to see people giving food to the homeless in the streets, dropping off used objects to Goodwill, and donating money to areas hit by natural disasters or disease outbreaks. In the year 2019, Americans donated $449.64 billion (Giving USA, 2020). Such voluntary giving behavior benefits the economy. It relieves hardship (Agenor, Bayraktar, Aynaoui, 2008), acts as insurance to unanticipated events (Stromberg, 2007) and provides social service to areas the government might underinvest (Leslie and Ramey, 1988; Hughes and Lukssetich, 1999). The economic and social benefits of charitable giving call for the identification of policies that encourage giving. This paper studies payoff equivalent scenarios within a tax framework and compares the efficacy of rebate and matching subsidy on charitable giving.

We study behavioral responses to fundraising mechanisms through a controlled laboratory experiment. In our experiment, an individual faces several allocation choices of a certain amount of money into a private fund and a charity fund that benefits a third party under a different type of fundraising mechanism. We aim at getting some insights on the empirical performance of two types of fundraising mechanisms within a tax framework: matching and rebate subsidy.

The relative price of giving decreases when tax is imposed on the portion of income individuals keep for their own use but not on the charitable contribution. This is a current practice in the United States: taxpayers can deduct charitable contributions from their taxable income. We focus on two subsidy fundraising mechanisms that decrease the relative price of giving by subsidizing contributions to charity, either in matching subsidy or rebate subsidy. In

[6] This chapter is based on a collaborative research project with Vjollca Sadiraj, and Yongsheng Xu at the Department of Economics, Andrew Young School of Policy Studies, Georgia State University.

the matching subsidy treatment, the experimenter matches the subject's donation at a specific rate and adds to the contribution, while in the rebate subsidy treatment, the individual receives a certain percentage of the donation back from the experimenter after contributing to charity. The experimental design is such that the opportunity set in the tax and matching subsidy treatments is the same. The opportunity set in the rebate subsidy treatment is a subset of it unless subjects are allowed to donate their refund.

All of these policies reduce the effective price of giving but have different consequences, which incentivizes individuals to donate more. A tax deduction is different from matching and rebate subsidy because the price of giving is decreased in the form of reduced tax liability to the government. The government receives less tax revenue in exchange for more donations to charity. With matching subsidies, additional donations to charity come out of the pocket of lead donors. The difference between rebate and matching subsidy is when the discount in the price of giving takes effect. The reduction in the price of giving from rebate subsidy takes place sometime in the future, whereas the discount is instant with matching subsidy. As a result, even when the policies are equivalent with regard to reduction in the effective price of giving, behavioral responses to these policies may be different. Donors may care about other features such as who is funding the subsidy or whether the price reduction is instant.

Previous studies on matching and rebate subsidies (Eckel and Grossman, 2003; 2006a; 2006b; 2008) suggest that matching subsidy performs better than a payoff-equivalent rebate subsidy, but little is known about the performance of the two mechanisms when donations are integrated into income tax claims. According to Chatterjee et al. (2020), tax credits shift donations from non-qualifying charities to qualifying charities. We are interested in the efficacy of rebate and matching subsidy fundraising procedures when embedded within a tax framework and variations in the rebate subsidy and stochastic rebates to explore further forms of rebate subsidy.

The paper makes several contributions. First, we study how tax framing affects giving behavior. Second, we compare the performance of stochastic rebate subsidy and determining rebate subsidy on raising donations. Third, we provide time preference as an alternative explanation for why matching subsidy raises more donations than payoff equivalent rebate subsidy. Fourth, we find that donations are normal goods and are complements with private consumption.

The paper is organized as follows; section 2 provides a literature review of studies that compare rebate and matching subsidy fundraising mechanisms, as well as papers that examine the effect of taxes on charity donations. Section 3 introduces the design of the experiment. Section 4 presents the results from the experiment. Lastly, the paper ends with a conclusion.

2.2 Literature Review

Studies on the effect of different fundraising mechanisms have been conducted through laboratory experiments, field experiments, and observational data. Table 11 provides a summary of several experimental research papers that compared rebate and matching subsidies. Empirical studies focused on measuring the effect of taxes on charitable giving.

Table 11. Summary of Existing Studies on Matching and Rebate Subsidy

Author (Year)	Research Question	Findings
Eckel & Grossman (2003)	Matching subsidy vs (equivalent) Rebate subsidy.	Matching elicits larger net donation.
Davis et al. (2005)	Test of constant contribution rule.	Consistent with constant contribution rule.
Davis & Millner (2005)	Matching and rebate subsidy on consumer purchases (chocolate bars).	Matching elicits more purchases.
Eckel & Grossman (2006a)	Preferences over matching and rebate subsidy.	Equally preferred.
Eckel & Grossman (2006b)	Between vs within subject design.	Matching elicits larger net donation.

Davis (2006)	Test of isolation.	Fail to reject.
Eckel & Grossman (2008)	Matching vs (equivalent) Rebate subsidy in a field experiment.	Matching elicits larger net donation.

Eckel and Grossman (2003) was the first controlled study of two alternative donation subsidies: a matching subsidy and a rebate subsidy. The two types of subsidy are equivalent regarding the relative price of giving when the matching subsidy rate, s_m and the rebate subsidy rate, s_r satisfy $s_m = \frac{s_r}{(1-s_r)}$. Contrary to theoretical predictions (assuming conventional preferences), subjects allocated more money to the charity fund in matching subsidy than rebate subsidy overall. This result is replicated in Eckel and Grossman (2006a, 2006b, 2008). Authors argue that the matching subsidy may be more appealing than the rebate subsidy, for it may signal enhanced altruism at the population level. Results from Huck and Rasul (2011) and Huck et al. (2015) are consistent with the explanation. Both papers find that donation is larger when just the presence of a lead gift is announced without any matching offer.

Other researchers also conducted similar experiments to examine the reasoning behind the higher donation in matching subsidy than payoff equivalent rebate subsidy. One robust finding is that change in framing can produce different behavioral responses between a matching condition and equivalent rebate condition (Davis Millner, 2005; Davis, Millner, and Reilly, 2005). The second consistent result is that subjects tend to pass the same percentage of the endowment under matching subsidy and rebate subsidy (Davis, Millner and Reilly, 2005; Davis, 2006).

Davis and Millner (2005) changed the decision problem in the context of private consumption (buying a chocolate bar) instead of putting the decision problem in the context of charitable contributions. Rebate subsidy was framed as receiving a partial refund from the original price. The matching subsidy was framed as getting one more chocolate for free when purchasing one chocolate. Participants bought the most under the matching condition than under the equivalent rebate condition. This result seems to reject Eckel and Grossman's (2003) hypothesis on the "more cooperative" nature of matching driving higher contributions. Additionally, Davis, Millner, and Reilly (2005) used a neutral context by studying the allocation decision as an investment problem. The purpose was to investigate whether framing matters in explaining the pattern of allocations. The pattern of higher net contribution under matching subsidy was again observed in the data revealing that the pattern is robust to decision problems in the context of private consumption or investment.

Davis, Millner, and Reilly (2005) examined whether the "constant contribution" rule explains the data. They find that individuals tend to contribute a fixed percentage of their endowment on average, suggesting that "constant contribution" would be the reason why matching subsidy results in higher net contribution. The intuition behind this reasoning is that the decision problems look the same to individuals if they do not understand the difference among each decision problem, whether it is a matching condition, no subsidy, or rebate condition. Confused or distracted individuals see the different decision problems as identical. Since all decision problems appear the same, individuals give the same contribution to the problems instead of adjusting the amount of giving based on the total contribution receipt by the charity. The authors also looked at whether "constant contribution" behavior can be eliminated by providing subjects with detailed information on the net consequences of rebate and matching subsidies. The extra information did adjust the pass rate (percentage of endowment individuals choose to contribute) closer to the theoretical prediction, but total contribution under matching remained higher than rebate subsidy.

In a later study, Davis (2006) investigates whether higher charity receipts under matching subsidy are attributable to an isolation effect. The isolation effect proposes that individuals tend to focus on the salient aspect of a decision problem (McCaffery and Baron, 2006). Thus, when subjects are presented with matching or rebate subsidies, the focus is on the more salient part, which is the direct consequence of a decrease in income when donating to charity. They fail to account for the amount of money actually received by the charity. To test for the isolation effect, the author provided the subject with information on the maximum possible contribution. Individuals decided how much of the maximum possible contribution goes to charity. The treatment allowed the subject to donate part of the refund as well. The result supported that higher contribution under matching subsidy is attributable to isolation effect and not a preference for matching subsidy.

Alternative explanations for higher donation in matching were studied in Eckel and Grossman (2006a) and Eckel and Grossman (2006b). Eckel and Grossman (2006a) provided subjects with the opportunity to choose the type of subsidy game to play, to test whether subjects have a priory preference over the two equivalent subsidy games and how the selection affects play. Their finding was that voluntary giving remains greater under matching subsidy than the rebate subsidy. The rates of self-selection across the two games were similar, suggesting indifference between the two types of subsidy at the population level. The finding was replicated in a field experiment (Eckel and Grossman, 2008). In Eckel and Grossman (2006b), a between-subject experiment, an individual either gets a rebate subsidy or matching subsidy but not both. The purpose of the between-subject design was to reduce possible confusion created by having subjects decide under both types of subsidies. Although the relative price elasticity of giving was closer to the theoretical prediction in this between-subjects design (compared to the within-subjects study), differences in contributions between the two subsidies persisted.

Overall, four conclusions can be drawn from the above studies. First, the pattern of higher charity receipts under a matching subsidy is robust to framing and context. Second, the "cooperative nature of matching game" hypothesis suggested by Eckel and Grossman (2003) seems to be contradicted by Davis and Millner (2005). Third, the isolation effect hypothesis from Davis (2006) is better at explaining why charity receipts are higher under a matching subsidy compared to the hypothesis that individuals prefer a matching subsidy. This conclusion is consistent with Eckel and Grossman (2006a), who also find a similar rate of self-selection across matching subsidy and rebate subsidy. Lastly, the "constant contribution rule" from Davis et al. (2005) also seems to explain the higher total contribution in matching subsidy. The isolation effect and the "constant contribution rule" appear to be related. Individuals might contribute a constant percentage of their endowment across decision problems because of the isolation effect. If they focus only on the salient aspect of giving, then all decision problems may be perceived the same. While alternative explanations for why the total contribution is higher in matching subsidy exist, the answer is still unclear. More research is needed to understand giving behavior.

Several studies use observational data to study how taxes influence giving behavior. Taxes influence giving behavior through two channels: price effect and income effect. The general result is that demand for charitable contributions is price elastic and income inelastic in response to after-tax income (Feldstein, 1975; Feldstein and Clotfelter, 1976; Boskin and Feldstein, 1977; Feenberg; 1987). These traditional estimates are not robust to different data and empirical methods. Depending on the data and empirical model used, demand for charity giving is price inelastic and varies in some studies (Broman, 1989; Barrett et al., 1997; Randolph, 1995) across different income levels (Clotfelter and Steuerle, 1981; Feldstein and Taylor, 1976).

The possibility of omitted variable bias and confounding effects can drive these seemingly inconsistent findings. This paper controls for such limitations and cofounds and examines how giving is affected when a tax is imposed on income allocated to private funds but not allocations to a charity fund in a controlled laboratory environment.

2.3 Experiment Design

2.3.1 Decision Problem

Subjects are initially given an endowment and face nine decision problems. In each decision problem, subjects decide how to allocate the endowment between the private fund and the charity fund of their choice under a given condition. The portion of the endowment allocated to the private fund was for the participants to keep, and the portion allocated to the charity fund was donated to the designated organization. The subjects can participate in one unpaid practice round for each decision before they make the actual decision. The nine decision tasks differ by the tax rate, framing, type of subsidy, and whether or not the rebate rate is stochastic. After everyone completes all the allocation decision tasks, the experimenter draws one bingo ball from a bag with bingo balls numbered one to nine to decide the final earnings and contribution to charity. Each subject participates in three baseline treatments, three tax framing treatments, and three subsidy treatments (either matching, rebate, or stochastic rebate). Within the treatments, tax rate and endowment are varied.

2.3.2 Treatments

Table 12. Experimental Design

Treatments		Initial endowment	Value of one token in		Rebate	Price of giving	Value of all tokens in	
			Private	Charity			Private	Charity
Baseline Order of tasks A and B random. Task C after A and B.	A	18	$1	$2	No	1/2	$18	$36
	B	12	$1	$3	No	1/3	$12	$36
	C	e	$1	$3	No	1/3	e	e
Tax Frame Tax on allocations to the private fund but not to charity.	A	24	$1, t=0.25	$1.5	No	1/2	$18	$36
	B	24	$1, t=0.5	$1.5	No	1/3	$12	$36
	C	e	$1, t=0.5	$1.5	No	1/3	e	e
Matching Subsidy Donation to charity is matched.	A	24	$1, t=0.25	$1 s_m=0.5	No	1/2	$18	$36
	B	24	$1, t=0.5	$1, s_m=0.5	No	1/3	$12	$36
	C	e	$1, t=0.5	$1, s_m=0.5	No	1/3	e	e
Deterministic Rebate Part of donation to charity is refunded.	A	24	$1, t=0.25	$1	s_r=0.25	1/2	$18	$24
	B	24	$1, t=0.5	$1	s_r=0.17	1/3	$12	$24
	C	e	$1, t=0.5	$1	s_r=0.17	1/3	e	e
Stochastic Rebate Total donation to charity is refunded with some probability.	A	24	$1, t=0.25	$1	$E(s_r)$=0.25	1/2	$18	$24
	B	24	$1, t=0.5	$1	$E(s_r)$=0.17	1/3	$12	$24
	C	e	$1, t=0.5	$1	$E(s_r)$=0.17	1/3	e	e

Notes: The matching subsidy, deterministic rebate and stochastic rebate conditions are assigned between subjects. The baseline and tax frame conditions are assigned within subjects. In the case of task C, e is given in statement (1) for baseline, in statement (2) for tax framing, in statement (3) for matching, in statement (4) for rebate and stochastic. No tax is paid on rebate.

Table 12 provides a summary of the treatments. The experiment has five treatments, each treatment consists of three tasks, including variation in tax rates and endowment amount. The treatments involve baseline treatments, tax framing treatments (taxes on allocations in the private fund but not on donations), matching treatments (same as the tax framing treatments but with matching on the donation), and two rebate treatments (same as the matching treatment but matching is replaced by either instantaneous or deterministic rebate).

2.3.2.1 Baseline

Tax is not imposed on money allocated to the private fund, and subsidies are not provided for the donation at the baseline. As shown in Table 12, each treatment has three tasks: A, B, C. In task A, each subject is endowed with 18 tokens. Every token allocated to the private fund is worth $1 to the subject. Every token allocated to a charity fund is worth $2 to the charity. If the subject allocates all 18 tokens in the private fund, then the subject receives $18. On the other hand, if the subject places all 18 tokens to charity, the charity receives $36.

In task B, we reduce the endowment amount to 12 tokens and increase the worth of each token in the charity fund to $3. The maximum amount the subject could take home was $12, but the maximum about the subject could donate was the same.

In task C, we change the number of tokens endowed to the subjects. The amount depends on the subject's choice in the first baseline treatment. We select the endowment such that the budget line from task B shifts parallel until the allocation selected by the subject in task A is on the budget line of task B. Hence, the endowment amount will be different from subject to subject since it depends on each participant's choice made in baseline 1. Suppose x_{b1} is the number of tokens the subject allocats to the charity fund in baseline 1, then the endowment amount, y_{b3}, in task C satisfies the following equation.

$$y_{b3} = 18 - \frac{1}{3}x_{b1} \tag{10}$$

2.3.2.2 Tax Framing

Tax is imposed on private fund in the tax framing treatment. In task A, each subject is endowed with 24 tokens and every token allocated to the private fund is taxed 25%. This makes each token in the private fund worth \$0.75. However, every token donated to the charity fund is not taxed and is worth \$1.50 to charity. Similarly, to task A, if the subject allocates all 24 tokens in the private fund, then the maximum money the subject can get is \$18. If the subject donates all 24 tokens to charity, then the maximum donation the charity receives is \$36. Hence, the budget line faced by the subjects is the same across baseline task A and tax framing task A.

We increase the tax rate to 50% in task B, which makes each token in the private fund worth \$0.50. The maximum amount the subject can take home is \$12 and the maximum amount the subject can donate is \$36. The budget line faced by the subjects is the same across baseline task B and tax framing task B.

In task C we again select the endowment such that budget line from task B shifts parallel until the allocation point selected by the subject in tax framing 1 is on the budget line of task C. Therefore, the endowment amount is dependent on the donation amount in task A. The following is the equation for the endowment amount in task C, y_{t3}, given the number of tokens the subject allocated to the charity fund in task A, x_{t1}.

$$y_{t3} = 36 - \frac{1}{2}x_{t1} \tag{11}$$

2.3.2.3 Matching Treatment

Each token donated to the charity fund is matched by the experimenter in the matching treatment. Same as tax framing task A, each token allocated to the private fund is taxed 25% and is worth \$0.75 in matching task A. Unlike tax framing task A, each token donated to charity fund is worth \$1 to charity and not \$1.50. However, each token donated to charity is matched with the rate of 50% by the experimenter in addition to the donation by the subjects. Consequently, the total amount the charity receives is \$1.50 for every token donated. Hence, the budget line faced by the subjects in task A is the same across baseline, tax framing, and matching.

In task B, each token allocated to the private fund is taxed 50% same as tax framing task B and the matching rate is kept the same at 50%. The budget line of task B is the same across task B in baseline, tax framing, and matching.

Similar to the task C in baseline task and tax framing, the amount of endowment in matching task C depends on the subject's donation. The following is the equation for the endowment in matching task C, y_{m3}, given the number of tokens the subject donated to the charity fund, x_{m1}.

$$y_{m3} = 36 - \frac{1}{2} x_{m1} \tag{12}$$

2.3.2.4 Rebate Treatment

The rebate treatment returns a portion of the donation to the individuals as a refund. We have two different rebate treatments: deterministic rebate and stochastic rebate. In the deterministic rebate treatment, the subject gets back 25% of the donation as a refund for sure in task A. On the other hand, in stochastic rebate task A, the subjects receive all donation back with 25% probability and nothing 75% probability. In stochastic rebate task B and C, the subjects receive all donation back with 17% probability and nothing with 83% probability. In both deterministic and stochastic cases, the expected rebate rate is 25% in task A and 17% in task B and C. We use bingo balls to determine the outcome of the refund in stochastic rebate treatments. In a black bag we place numbered bingo balls from 1 to 12. If a bingo ball numbered 1 to 9 is drawn, then no refund is given to the subjects. If a bingo ball numbered 10 to 12 is drawn, then the subjects get their donation fully refunded. The monitor draws the bingo balls.

In rebate and stochastic rebate task A, each subject is endowed with 24 tokens. Tokens allocated to the private fund is taxed 25% so each token allocated to the private fund is worth $0.75 after tax. Every token allocated to a charity fund is worth $1 to the charity. In rebate task A, the subject receive a refund of $0.25 for every token donated to charity. The maximum amount the subject can take home is $18 and the maximum amount the subject can donate is $24. In stochastic rebate task A, the refund amount is either zero or the full donation amount, depending on the outcome of the bingo ball draw. The maximum amount the subject can take home is $24 if the subject donates all the tokens and get fully refunded. The budget line in rebate task A is a subset of the budget line of task A in baseline, tax faming, and matching task. For stochastic rebate task A, the expected budget line is equivalent to the subset budget line of task A in baseline, tax framing, and matching.

Tax rate is increased to 50% in rebate and stochastic rebate task B, so each token allocated to the private fund is worth $0.50. The budget line of task B in rebate and the expected budget line of task B in stochastic rebate are subsets of the budget line of task B in baseline, tax framing, and matching.

In task C of rebate and stochastic rebate, we change the endowment amount in similar pattern of task C in baseline, tax framing, and matching. Given the token amount donated in rebate and stochastic rebate task A, x_r, the endowment amount in rebate and stochastic rebate task C, y_r , follows the equation below.

$$y_r = 36 - \frac{1}{2}x_r \tag{13}$$

2.3.3 Hypotheses

Suppose individuals only care about the relative price of giving, and the parameters, $\gamma, s_m, and\ s_r$ are chosen such that the relative price of giving is the same across all treatments. Since the relative price of giving is the same, the total charity receipt will be equal in all treatments. However, if price of giving is not the only factor that determines giving behavior the the total charity receipt will not be the same across the fundraising mechanisms.

The preferred allocation of a risk-neutral individual is predicted to be invariant between a subsidy with deterministic rebate rate and stochastic rebate rate that yields the same expected value. On the other hand, a risk-averse individual is predicted to donate more under a deterministic rebate rate than a stochastic rebate rate. A risk loving individual is predicted to donate more under a stochastic rebate rate than determinist rebate rate. Mathematical proof is provided in the Appendix.

2.3.4 Risk Elicitation Task

After the allocations tasks, the participants are given the opportunity to invest their earnings for more money. If they choose to invest, they have a 50% chance of receiving 50% more of their investment and a 50% chance of receiving 50% less of their investment. The process is determined with a coin flip, and the monitor flipped the coin. Earnings that are not invested is kept by the subjects. We expect risk averse subjects to not invest, risk neutral to be indifferent, and risk loving to invest.

2.3.5 Procedure

The experiment was conducted at the Experimental Economics Center (ExCEN) at Georgia State University. All six experimental sessions were run by z-Tree (Fischbacher, 2007). A total of 140 students attending Georgia State University voluntarily participated in the experiment as subjects, and among them, six students served as session monitors. Each subject was able to participate only in one session in this experiment. The experiment consisted of two parts. In the first part, subjects were given nine allocation decision tasks in which they need decide how much of the endowment to donate and take home. In the second part, the subjects participated in a risk preference elicitation task.

The experiment instructions were placed on each cubicle before the subjects entered the lab. The subjects were given time to read the instructions while waiting for the experiment to begin. After the subjects were done reading the instruction, the experimenter went over the instruction again for clarification. Talking between the participants was prohibited throughout the experiment. Before starting the allocation decision task, individuals were provided with a list of donation designations[7], each with a sentence description of how the donated money is used. The experimenter selected the donation designations such that the money was donated to organizations within Georgia State University. We wanted the distance between the recipient of the donation and the students to be close and relevant.

One monitor was randomly selected from the subjects before the experiment began to assist the experimenter throughout the experiment. The role of the monitor was to ensure that the donations received from the subjects were delivered to Georgia State University Foundation Office at One Park Place South. After the experiment, the monitor and the experimenter walked together to the Foundation Office to deliver the donation. Each monitor received a $20 flat payment after signing a statement verifying the donation amounts of the subjects.

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[7] The donation designations provided to subjects were: Panther's Pantry, Rialto Center for the Arts, Panther Retention Grant, Panther Athletic Club, Georgia State University Library, Keep Hope Alive Scholarship, University-wide Scholarships, Honors College, GSU Fund for Excellence, and Bio-Bus Program.

The experiment ended after the subjects filled out a survey, which included some variation of the Self-Report Altruism Scale (Rushton et al., 1981), and questions on demographic characteristics. Participants had the choice to be acknowledged or remain anonymous for the donation. Donations were calculated after the experiment and delivered to the Georgia State University Foundation Office by the student monitor and the experimenter.

2.4 Results

2.4.1 Summary Statistics

A total of 134 students voluntarily completed the decision tasks, and six students participated as session monitors. The summary of demographic characteristics of the subject pool is provided in Table 13. Overall, 61.7% of the subjects were females, 61.9% were Black or African Americans, 47.0% were Freshmen, and 24.63% were majoring in Business or Economics related majors.

Table 13. Demographic Summary Statistics

	Matching	Rebate	Stochastic Rebate	All
Number of Subjects	49	43	42	134
Age (mean)	20.76	19.58	20.26	20.2
Female (%)	73.47	58.14	50	61.65
Parent Income (mean group)	\$60K-\$75K	\$60K-\$75K	\$60K-\$75K	\$600K-\$75K
Black/African American (%)	55.1	74.42	57.14	61.94
Religion (%)				
Christianity	42.86	74.42	61.9	58.96
Nonreligious/Other	18.37	11.63	14.29	14.93
Prefer not to answer	10.2	6.98	7.14	8.21
GPA (%)				
Below 3.00	14.29	20.93	16.67	17.16
3.00 to 3.69	59.18	48.84	54.76	54.48
Above 3.69	18.37	30.23	26.19	24.63
Freshman (%)	44.9	48.84	47.62	47.01
Business/Economics Major (%)	18.37	34.88	21.43	24.63
Participated in an Election (%)	42.86	34.88	45.24	41.04
Liberal (%)	44.9	60.47	50	51.49

Table 14 provides a summary of the subjects' responses to the questionnaires on tax view and altruism. The subjects' answers to the questionnaires indicated that most of them donated money to a charity (80.60%), gave money to a stranger (91.04%), or volunteered before (94.03%). They disagreed that most people would stop and help a person whose car is disabled (64.18%) and agreed that it is important to pay all the taxes to be a good citizen (82.09%). The response had four categories which ranged from disagree strongly to agree strongly.

Table 14. Attitudes Towards Paying Taxes and Altruism

	Matching	Rebate	Stochastic Rebate	All
Tax View				
"It is important to pay all the taxes you owe in order to be a good citizen."				
Agree Strongly	26.53	18.6	28.57	24.63
Agree Slightly	59.18	62.79	50	57.46
Disagree Slightly	10.2	16.28	21.43	15.67
Disagree Strongly	4.08	2.33	0	2.24
Altruism				
"Most people would stop and help a person whose car is disabled."				
Agree Strongly	6.12	4.65	4.76	5.22
Agree Slightly	30.61	34.88	26.19	30.6
Disagree Slightly	51.02	41.86	45.24	46.27
Disagree Strongly	12.24	18.6	23.81	17.91
"I have given money to a stranger who needed it (or asked me for it)."				
Never	12.24	13.95	0	8.96
Once	12.24	13.95	21.43	15.67
More than Once	36.73	51.16	69.05	51.49
Often	28.57	18.6	4.76	17.91
Very Often	10.2	2.33	4.76	5.97
"I have done volunteer work for a charity."				
Never	6.12	6.98	4.76	5.97
Once	10.2	4.65	21.43	11.94
More than Once	42.86	60.47	30.95	44.78
Often	26.53	13.95	21.43	20.9
Very Often	14.29	13.95	21.43	16.42
"I have given money to a charity before this experiment."				

Never	24.49	25.58	7.14	19.4
Once	16.33	20.93	21.43	19.4
More than Once	38.78	34.88	50	41.04
Often	12.24	13.95	19.05	14.93
Very Often	8.16	4.65	2.38	5.22

Notes: Questionnaire on views on taxes and altruism.

The average gross contribution for each treatment is shown in Table 15. With disposable endowment of \$18 and price of giving of \$0.50, matching subsidy had the highest gross contribution of \$9.89 and rebate subsidy had the lowest gross contribution of \$4.29. With disposable income of \$12 and price of giving of \$0.33, the baseline had the highest gross contribution of \$7.60 and rebate subsidy had the lowest gross contribution of \$3.54. With conditional maximum take home earning and price of giving of \$0.33, the baseline had the highest gross contribution of \$9.96 and rebate subsidy had the lowest gross contribution of \$4.56. The common pattern we observe is that rebate subsidy raises the least amount of gross contribution to charity.

Table 15. Gross Contribution Across Treatments

Treatment	N	Task A E=18, p=1/2	Task B E=12, p=1/3	Task C E=e, p=1/3
Baseline	134	8.48 (6.01)	7.60 (5.77)	9.96 (8.46)
Tax Framing	134	7.83 (6.44)	6.29 (5.56)	7.94 (7.85)
Matching Subsidy	49	9.89 (6.34)	7.43 (5.02)	9.18 (5.69)
Rebate Subsidy	43	4.29 (3.74)	3.54 (2.72)	4.56 (4.29)
Stochastic Rebate	42	7.17 (6.27)	6.43 (5.87)	6.71 (6.54)
Differences				
Tax Framing[a]	134	-0.65**	-1.31***	-2.03***
Matching[b]	49	0.33	0.12	0.71
Rebate[b]	43	-1.17***	-0.81**	-2.25***
Stochastic Rebate[b]	42	-1.08**	-0.66**	-1.75***

Notes: [a] compared to the baseline, [b] compared to Tax framing. E denotes the initial endowment of tokens, p is the relative price of giving measured as cost of giving \$1 to a charity. The value of e varies across subjects and it is determined by choice in Task A. Standard deviations in parentheses. Statistical significance is based on Mann-Whitney U test. *** p<0.01, ** p<0.05, * p<0.1. Gross contribution is the total amount contribution received by the charity.

2.4.2 Tax Framing versus No Framing

Table 15 also shows the difference in gross contributions to charity between tax framing and the baseline. We first subtract gross contribution in tax framing from gross contribution in the baseline for each subject, then we calculate the mean of the differences for each task.

We compare gross contributions to charity between all treatments with tax framing and the baseline to examine whether giving behavior is consistent across payoff equivalent scenarios. In tax framing treatments, we impose taxes on endowments allocated to the private fund. On the other hand, endowments allocated to the charity fund are not taxed. The relative price of giving is the same across the baseline and tax framing. Overall, subjects did not make equivalent allocations. Charities received less money under tax framing compared to the baseline. For example, with disposable income of $18 and a price of giving of $0.50, charities receive $0.65 less on average in tax framing. The difference is starker with a lower endowment. With a disposable income of $12 and a price of giving of $0.33, charities receive $1.31 less on average in tax framing. With conditional endowment and a price of giving of $0.33, charities receive $2.03 less on average in tax framing.

2.4.3 Subsidy versus No Subsidy

We also estimate the difference in gross contributions to charity between tax framing without any subsidies and tax framing with subsidies. The estimations are shown in Table 15. We first subtract gross contribution in either matching, rebate, or stochastic rebate from gross contribution in tax framing for each subject. We then calculate the mean of the differences for each task.

Matching subsidies did not generate a statistically significant difference in gross contribution compared to tax framing without subsides. For example, Matching subsidies generated $0.33

more gross contribution to charity with an endowment of $18 and a price of giving of $0.50. However, it was not statistically significant. The result was robust to different endowments and the price of giving. On the other hand, we observed a decrease in gross contribution for rebate subsidies and stochastic rebate subsidies. With a disposable endowment of $18 and a price of giving of $0.50, we observed a decrease in the gross contribution by $1.17 in rebate subsidy and a decrease in the gross contribution by $1.08 in stochastic rebate subsidy compared to no subsidy. The outcome was robust to different endowments and the price of giving.

2.4.4 Matching versus Rebate

Between matching subsidy and rebate subsidy, we find rebate subsidy generated less gross contribution to charity. Across all matching treatments, the matching subsidy generated on average $0.39 more gross contribution than tax framing without subsidies. However, rebate subsidy generated on average $1.41 less gross contribution than tax framing without subsidies. The difference in the two estimates was $1.80 ($p<0.001$). This result aligns with previous studies (Davis and Millner, 2005; Davis et al., 2005; Davis, 2006; Eckel and Grossman, 2003; Eckel and Grossman, 2006a; Eckel and Grossman, 2006b; Eckel and Grossman, 2008).

2.4.5 Rebate versus Stochastic Rebate

Stochastic rebate subsidy generated more gross contribution to charity than rebate subsidy, but the difference was not statistically significant. Rebate subsidy decreased gross contribution by $1.41 on average and stochastic subsidy decreased gross contribution by $1.17 on average compared to no subsidies. According to these two estimates, stochastic rebate subsidy performed better than rebate subsidy in raising money for donation by $0.24 ($p=0.103$).

2.4.6 Tax Rate and Income

Table 16 presents the effect of different tax rates and income on gross contribution for each treatment. Column 3 shows the impact of the higher tax rate on gross contribution, and column 4 shows the effect of higher income on gross contribution. Overall, gross contribution decreased when the tax rate increased from 25% to 50%. This increase in tax rate is equivalent to a decrease in the price of giving from $0.50 to $0.33 and a decrease in disposable endowment from $18 to $12. The decrease in gross contribution was statistically significant for the baseline, tax framing, and matching ($p<0.05$). The change in the gross contribution from the increase in the tax rate was the highest for matching subsidy. Gross contribution decreased by $2.46 on average when the tax rate increased in matching subsidy. Subjects were least sensitive to increases in the tax rate in stochastic rebate subsidy and rebate subsidy. In rebate and stochastic rebate subsidy, gross contribution decreased by $0.75 and $0.74, respectively. Both estimates were not statistically significant.

In general, gross contribution to charity increased with income. The largest change in gross contribution was observed in the baseline. In the baseline, gross contribution increased by $2.37 on average when subjects received higher income. Similar patterns can be observed in other treatments, and the estimates were statistically significant except for stochastic rebates. In tax framing, without subsidies, gross contribution increased by $1.65 with income. In matching subsidy, gross contribution increased by $1.75, and in rebate subsidy, gross contribution increased by $1.02. Subjects were least sensitive to an increase in income in stochastic rebate subsidy. In stochastic rebate subsidy, gross contribution increased by $0.28, and the value was not statistically significant.

Table 16. Change in Gross Contribution from Higher Tax Rate or Higher Income

Treatment	N	Task B−Task A (mean)	Task C−Task B (mean)
Baseline	134	-0.88**	2.37***
Tax Framing	134	-1.54***	1.65***
Matching	49	-2.46***	1.75***
Rebate	43	-0.75	1.02**
Stochastic Rebate	42	-0.74	0.28

Notes: Statistical significance is based on Mann-Whitney U test. *** p<0.01, ** p<0.05, * p<0.1

2.4.7 Regression Analysis

Table 17 estimates the marginal effects on contributions received by the charity, using maximum likelihood tobit regression with random effects. The results from the regression analysis support the findings from our descriptive statistics. The following is the equation estimated,

$$\mathrm{CONTRIBUTIONS}_{ij} = \beta_0 + \beta_1 \mathrm{TAXFRAMING}_{ij} + \beta_2 \mathrm{HIGHERTAX}_{ij} + \beta_3 \mathrm{HIGHERINCOME}_{ij} + \beta_2 X_i + \alpha_i + \epsilon_{ij} \quad (14)$$

where $i = 1, \dots, 134$ (index of subjects) and $j = 1, \dots, 9$ (index of allocation problems). $\mathrm{CONTRIBUTIONS}$ is the dollar value of contribution received by the charity, $\mathrm{TAXFRAMING}$ is a vector of indicators for treatments with tax framing, including: Tax framing without subsidy, tax framing with matching, tax framing with rebate, and tax framing with stochastic rebate. $\mathrm{HIGHERTAX}$ is an indicator for treatments with tax rate of 50%. The value of $\mathrm{HIGHERTAX}$ is 1 if the treatment belongs to task B. The value of $\mathrm{HIGHERTAX}$ is 0 for the rest of the treatments. $\mathrm{HIGHERINCOME}$ is an indicator for treatments with income shift. If the treatment belongs to task C, the value of $\mathrm{HIGHERINCOME}$ is 1 and is 0 for the rest of the treatments. X is a vector of individual characteristics which includes gender ($1 =$ female), religious ($1 =$ if have at least one religion), major ($1 =$ if the major is related to either business or economics), and past donation experience ($1 =$ if donated to a charity at least once before).

Table 17. Average Marginal Effects from Tobit Maximum Likelihood with Random Effects

Dependent variable=Contribution received by the charity in dollars		
Variable	(1)	(2)
Tax Framing - No Subsidy	-1.142***	
	(0.257)	
Tax Framing - Matching	-0.649*	0.501
	(0.381)	(0.359)
Tax Framing - Rebate	-2.727***	-1.508***
	(0.419)	(0.395)
Tax Framing - Stochastic Rebate	-1.902***	-0.868**
	(0.420)	(0.397)
Higher Tax	-1.105***	-1.262***
	(0.258)	(0.277)
Higher Income	1.408***	1.108***
	(0.259)	(0.276)
Female	0.261	-0.125
	(0.901)	(0.883)
Religious	-0.425	-0.820
	(1.012)	(0.990)
Business/Economics	0.479	0.641
	(1.018)	(0.997)
Gave Money to Charity Before	0.763	0.489
	(1.116)	(1.092)
N	1,206	804
Left Censored Ob.	131	83
Un Censored Ob.	1,064	713
Right Censored Ob.	11	8

Notes: Lower bound is 0, upper bound is either \$36 or \$54 depending on the treatment. In column 1, control is baseline. In column 2, control is tax framing with no subsidy. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

We first estimate the average marginal effects of all types of tax framing on charitable giving compared to the baseline, which is reported in column (1). The donation amount under all types of tax framing was lower than the donation amount under the baseline. However, the effect was smaller and statistically less significant when tax framing was combined with matching subsidy. When tax framing was combined with matching subsidy, the average marginal effect was a \$0.65 decrease in charitable contribution compared to the baseline. The biggest effect was when tax framing was combined with rebate subsidy. The average marginal effect was a $2.73 decrease in charitable contribution compared to the baseline.

In column (2), we estimate the average marginal effects of tax framing with subsidies on charitable giving compared to tax framing without subsidies. We exclude observations from the baseline treatment in our analysis. When comparing among treatments with tax framing, we did not observe statistically significant effects for matching subsidy. We found that rebate subsidy and stochastic rebate subsidy have negative marginal effects on donation compared to tax framing with no subsidy. The average marginal effects rebate subsidy and the stochastic subsidy were $1.51 and $0.87 reduction in donation, respectively, compared to tax framing with no subsidy. Our result also supports that stochastic rebate subsidy performs better than rebate subsidy in raising money for charity.

Both estimations from columns (1) and (2) find that contribution to charity decreased when tax was raised, and contribution to charity increased when income was increased. This result is expected. Although an increase in tax on private consumption decreases the price of giving, the higher tax also means a decrease in disposable endowment. None of the individual characteristics had a statistically significant effect on contribution to charity.

2.5 Conclusion

This paper studies how different institutions affect giving behavior and evaluate the efficiency of these institutions. The fundraising mechanisms evaluated in this paper are private consumption tax, matching subsidy, rebate subsidy, and stochastic rebate subsidy. The net cost of giving is kept equivalent across the mechanisms but, subjects respond differently to each mechanism. Consistent with previous literature, our experimental results show that the net cost of giving is not the only factor that influences giving behavior. Different theories predict different outcomes for fundraising mechanisms that seem equivalent on the surface.

We find that tax framing and rebate subsidy elicit less charitable contribution than neutral framing and matching subsidy. The negative effect on donation was smaller for stochastic than deterministic rebate subsidies. This result is not consistent with our hypothesis that if individuals are risk adverse individuals, then the donation amount would be less if rebate rates are stochastic than when rebate rates are deterministic. One explanation could be that individuals are risk loving towards money donated to charity. Our data also suggests that charitable giving is a normal good and that donations and private consumption are complements.

In the experiment, the donor that is matching the donations from the subjects is the experimenter. The experimenter is also the one that gives the subjects the refund in the rebate subsidy. Our findings may change depending on the reputation of the donor that is doing the matching. If a more reputable donor is doing the matching, then more donations will be raised. The deterministic rebate rate subsidy tested in our experiment is similar to a tax deduction on charitable giving. The taxpayer who claims tax deduction on charitable donations receives a refund paid by other taxpayers. Further research should explore how the nature of the matching and rebate subsidies contributor impacts giving behavior.

Chapter 3 Identification with the Government Leader and Tax Sheltering

3.1 Introduction

The recent 2016 United States presidential election result triggered mixed emotions among voters. Substantial number of people who voted for the non-elected party were disappointed when the result was announced. Their attitudes toward the new government were to a great degree negative and skeptical. On the other hand, those who voted for the winning party were feeling more hopeful and optimistic towards the new government. Overall, election results generate mixed emotions and attitudes among people, which mainly depends on whether the result matched his or her political preference. Such emotions and attitudes towards the new government are reflected through individual's behavior. One channel is through tax compliance. Evading or avoiding taxes is one method to express dissatisfaction with the government. If one did not vote for the elected president, then he or she is more likely to disagree with the new policies and how tax revenue is spent. Thus, the intensity of tax avoidance may differ between those who voted for the elected party and those who did not vote for the elected party. The purpose of this paper is to look at whether political identification with the government leader affects people's willingness to pay tax.

Several researchers study relationship between political affiliation of CEOs and corporate tax avoidance (Christensen et al, 2015; Francis et al, 2016; Peyer & Vermaelen, 2016). The findings are mixed. Christensen et al (2015) finds that firms that lean toward the Republic Party engage in less tax avoidance than firms that lean toward the Democracy Party. On the contrary, Peyer and Vermaelen (2016) argue that Republican CEOs are more likely to engage in tax avoidance behaviors and help their investors to save money on personal income taxes than Democratic CEOs. Francis et al (2016) finds that firms run by politically partisan CEOs participate more in corporate tax sheltering compare to firms led by nonpartisan CEOs. There is no stylized fact on the relationship between political affiliation of CEO's and tax avoidance or tax sheltering, suggesting that political affiliation alone is not a cause for tax avoidance or tax sheltering.

To the best of my knowledge, the only empirical study that examines relationship between political alignment and tax evasion is Cullen et al. (2018). The authors use vote shares of the president's party to measure approval of current government and use tax gap approach to estimate tax evasion. In their approach, reported taxable income is the dependent variable and the list of regressors include all possible economic controls to proxy for generated taxable income. The assumption is that any changes in reported income indicates changes in evasion when generated taxable income is kept fixed. The paper does find decrease in tax evasion at the county level when counties are more political aligned at both intensive margin and extensive margin.

This paper contributes to the tax compliance literature through examining the effect of political identification with the government on willingness to pay tax in the United States. It offers insights on the puzzle why people pay taxes when the risk of being audited is very low. Although Cullen et al. (2018) examines the same question, their method of measuring tax evasion relies on the assumption that generated taxable income is controlled perfectly by economic control variables. Therefore, possibility of measurement error cannot be ruled out from their estimates. If tax evasion is not measured accurately then their findings does not reflect the relationship between political alignment and tax evasion. The goal of this paper is to look at broader tax noncompliance behavior which includes tax avoidance and tax sheltering. We use claimed charitable contribution deductions to measure willingness to pay tax. The empirical methodology this paper differs from Cullen et al. (2018). We use difference in difference design for the analysis in which the first difference comes from pre-election and post-election. The second difference is between counties that experienced change in identification with the elected government and counties that did not. We compare counties that did not experience a change in match between their vote and the election outcome to counties that did experience a change in match between their vote and the outcome, after the election.

The paper is organized as follows, section 2 provides a literature review of studies on political affiliation and tax compliance, as well as papers that examine relationship between government approval and tax compliance. Section 3 presents the conceptual framework and hypotheses. Section 4 describes the identification strategy and data. Section 5 presents the findings and Section 6 is the conclusion.

3.2 Literature Review

3.2.1 Governance and Tax Compliance

Several papers already examined how individual’s perception of government spending affects tax compliance using survey data, laboratory experiments, and observational field data. Four stylized facts have emerged. One is that if government provides public good in exchange for taxes then tax compliance rate increases, which is supported by numerous experimental data (Alm, Cox, and Sadiraj, 2020; Alm, Jackson, and McKee, 1992; Alm, McClelland and Schulze, 1992; Becker, Buchner, & Sleeking, 1987). Second, tax compliance is higher when taxpayers have direct influence over government budgets (Alm, Jackson and McKee, 1993; Pommerehne and Weck-Hannemann, 1996). Third, trust in government increases voluntary tax compliance (Jimenez and Iyer, 2016; Kaplanoglou and Rapanos, 2015; Kogler et al., 2013; Scholz and Lubell, 1998). Fourth, perceived equity and fairness increase tax compliance (Cummings et al, 2009; Spicer and Becker, 1980).

3.2.1.1 Public Goods and Tax Compliance

In tax compliance experiments with public good provision, individuals propensity to pay taxes increases if they gain benefits from them (Alm, Jackson, and McKee, 1992; Alm, McClelland and Schulze, 1992; Becker, Buchner, & Sleeking, 1987). Tax compliance is found to be higher when individuals can vote on the type of public good they want, compare to the case when the type of public good is imposed (Alm, Jackson and McKee, 1993).

Pommerehne and Weck-Hannemann (1996) presents a finding from observational data that Swiss cantons that are classified as pure direct democracies have less discrepancy between income from tax return data and income from national income accounts, than cantons classified as pure representative democracies.

3.2.1.2 Trust, Fairness, and Tax Compliance

Tax morale has been embraced as an alternative explanation for the puzzle why individuals comply more than what the expected utility tax evasion model from Allingham and Sandmo's (1972) predicts. The general definition of tax morale is intrinsic motivation to pay tax, which operates through several mechanisms. Among them are trust in government and perceived fairness of public expenditure or fiscal policy. The two may be positively correlated to each other.

Jimenez and Iyer, 2016 uses survey data to examine social factors that influence tax compliance. They find that trust in government has positive impact on both observed fairness of the tax system and tax compliance. Kaplanoglou and Rapanos (2015) and Kogler et al (2013) test the slippery slope framework (Kirchler, Hoelzl, and Wahl, 2008) using experimental survey data. According to the slippery slope framework, two types of tax compliance exist. One is voluntary tax compliance resulting from trust in government and the other is enforced tax compliance resulting from power of government. The slippery slope framework suggests both trust and power influence tax compliance and the dynamic interaction between the two factors should be considered. Both Kaplanoglou and Rapanos (2015) and Kroger et al (2013) find trust increases voluntary compliance and power increases enforced compliance, with tax compliance being the highest when trust is high, and power is high and the lowest when trust is low and power is low.

Cummings et al. (2009) and Spicer and Becker (1980) both support that perception of fairness influences tax compliance. Higher perceptions of fairness and efficacy boost tax compliance in the artefactual field experiment from Cumming et al. (2009). The paper compares tax compliance between individuals from Botswana and South Africa. Tax compliance is found to be higher in individuals from Botswana than South Africa and different indicators on governance supports higher perceptions of good governance in Botswana. Laboratory experiment data from Spicer and Becker (1980) supports that fiscal inequity increases tax evasion. Fiscal inequity was simulated by providing false tax rates on others.

All the conclusions drawn from existing studies indicate that how individuals perceive the government impacts their willingness to pay tax. Hence, my hypothesis is political alignment with the current government will influence individual's tax paying behavior.

3.2.2 Political Affiliation and Tax Compliance

Only few papers have looked at the effect of political affiliation and tax paying behavior, focusing on tax compliance behavior at the firm level and not at the individual level (Christensen et al, 2015; Francis et al, 2016; Peyer & Vermaelen, 2016). The results are mixed which indicate that there are no stylized facts on political affiliation and tax compliance at least at the firm level.

All the papers above use information on contributions made to senate, house, and presidential candidates to identify political affiliation. They measure political orientation by taking the difference between contributions to the Republican Party and contributions to the Democratic Party, then divide by total contributions to both parties. For measuring tax avoidance or tax sheltering, each paper uses different methods. Christensen et al. (2015) uses two measures to identify tax avoidance: the firm's financial accounting effective tax rate and cash effective tax rate. In Peyer and Vermaelen (2016), the authors use the 2013 fiscal cliff as natural experiment to examine tax avoidance of CEOs. When Barack Obama was re-elected as president in 2012, people anticipated dividend tax cuts would expire in 2013 and marginal tax rate on dividend would increase. This motivated some firms to accelerate payment of their 2013 planned dividends to avoid anticipated high tax rate for their shareholders. The measure of tax avoidance in this case is the quantity of accelerated dividend payments before 2013. Francis et al. (2016) measures tax sheltering using book-tax differences.

Overall, results on political affiliation as a determinant of tax avoidance are mixed. This suggests that other factors such as political alignment between oneself and the government may be more relevant in predicting tax avoidance behavior. This is what this paper tries to explore at the individual level.

3.2.3 Political Alignment and Tax Evasion.

The closest study to this paper is an NBER working paper by Cullen et al. (2018) who also investigate whether taxpayer's level of government approval affects willingness to pay tax. The level of government approval is measured by vote shares at county level of the president's party. Cullen et al. (2018) uses tax gap approach to measure tax evasion, which relies on presumption that generated taxable income is controlled by economic variables. Our paper looks at tax sheltering behavior instead of tax evasion. It also tries to answer whether individuals will donate to charity that provides the public goods they prefer if they think the government is not funding the public good, they want. If taxpayers disagree with how the government is spending the tax revenue, then they can donate to a charity that aligns more with their preference in terms of public good provision and deduct the amount from their taxable income to reduce tax liability.

3.3 Conceptual Framework and Hypotheses

For simplicity, we assume that there are two role players in the economy, one is the taxpayer, and the other is the government leader. The government leader provides public goods using the tax revenue collected by taxpayers. The taxpayer decides on how much to donate. The total donation amount to charities is tax deductible. We assume that an individual gains higher utility from government provision of public goods when individual's political preference is aligned with government leader. The reasoning behind the assumption is that individuals who have closer political views with the government leader are more likely to agree with how the government is using the tax revenue. In addition to how the government distributes the money among different public sectors that favors the individual. As a result, they will value the benefits public good provided by the government is higher for individuals than those who have dissimilar political views with the government leader.

3.3.1 Optimization

Let an individual derive utility from disposable income, public goods provided by the government, and gift to the public good.

A taxpayer's overall utility is represented by

$$U(y, g) = (y - g_i)(1 - \tau) + \theta g_i + \gamma_i H(Y_{-i} + \tau(y - g_i)) \tag{15}$$

Where U is the taxpayer's total utility, y is income, τ is income tax rate, g is the amount of donation to charity. The first part of the equation represents disposable income. The second part of the equation represents utility from donating to charity, where $\theta \in [0,1)$. The last part of the equation $\gamma_i H(\cdot)$, represent utility from public good provided by the government, which is a function of tax revenue, where $Y_{-i} = \sum_{j \neq i}^{n} \tau\left(y - g_j\right)$ and $H(\cdot)$ is concave. We assume that the only way to reduce contributions to public good is through charitable giving and claiming deductions. Suppose $\gamma_1 H(Y_{-i} + \tau(y - g_i))$ represents benefit from public goods for individuals who are politically aligned with the government and $\gamma_0 H(Y_{-i} + \tau(y - g_i))$ represents those who are not politically aligned with the government. An individual who is more politically aligned with the government gains higher utility from public goods provided by the government, so we can assume that $\gamma_1 > \gamma_0$.

Let us denote g_1 as donation to charity when there is a political alignment with the government and g_0 as a donation to charity when there is a no political alignment with the government. For exogenous income and tax rate, by concavity of *H(.)*, it suffices to look at the first derivative of $U(\cdot)$ with respect to g for a utility maximizer taxpayer

$$U_g = \theta - (1 - \tau) - \tau\gamma_i H'\left(Y_{-i} + \tau(y - g_i)\right) \tag{16}$$

If $\theta \leq (1 - \tau)$ then the optimal g is 0 as $U_g < 0$. That is, such individuals never donate.

If $\theta \in (1 - \tau, 1)$, optimal g (at interior solution) is determined by the first order condition, $U_g = 0$.

If aligned,

$$H'\left(Y_{-i} + \tau(y - g_1)\right) = \frac{\theta + \tau - 1}{\tau\gamma_1} \tag{17}$$

If not aligned,

$$H'\big(Y_{-i} + \tau(y - g_0)\big) = \frac{\theta + \tau - 1}{\tau\gamma_0} \tag{18}$$

Since $\gamma_1 > \gamma_0$ and $\theta + \tau - 1 > 0$, the following inequality is derived from the first order conditions

$$H'\big(Y_{-i} + \tau(y - g_1)\big) < H'\big(Y_{-i} + \tau(y - g_0)\big) \tag{19}$$

The concavity of $H(\cdot)$ implies that $Y_{-i} + \tau(y - g_1) > Y_{-i} + \tau(y - g_0)$, which holds when $g_0 > g_1$.

Hypothesis

Ceteris paribus, an individual will donate more to a charity of choice if the political views with the government leader are more aligned.

Our model is a very simplified model and further extension is needed for more realistic representation of the world. Although the model requires further improvements, it clearly implies that donation to charity depends on political alignment with the government.

3.4 Methodology and Data

We apply difference in difference design for the empirical analysis and to test the theory. The first difference is in time, before and after political election. The second difference is in political alignment. The comparison is between counties that did not experience change in political alignment with the government leader and counties that did experience change in political alignment after the election. We will use the 2008 presidential election to define the post and pretreatment period. Our control group is counties where more than 50% of the voters voted for Bush in previous presidential election and more than 50% of the voters voted for Obama in

2008 (See Table 18). These are counties that did not experience a change in their political alignment. Our treatment group is counties in which the majority voted for Bush in previous presidential election and in the 2008 election the majority did not vote for Obama. Hence, the difference between the control group and treatment group is that the treatment group were not political aligned with the president in the new election.

Table 18. Control and Treatment Group

	Before 2008 Election	After 2008 Election
Control	Majority voted for Bush	Majority voted for Obama
Treatment	Majority voted for Bush	Majority did not vote for Obama

Let $TREAT_{\mathrm{ct}}$ be the indicator for whether a county c is in the treatment group and $POST_{\mathrm{ct}}$ is the indicator for whether the year t is in the post-election year. Given the treatment assignment, the regression equation we evaluate is the following

$$y_{ct} = \beta_0 + \beta_1 POST_t + \beta_2 TREAT_{ct} + \beta_3 (TREAT_{ct} \cdot POST) + \beta_4 X_{ct} + \varepsilon_{ct}, \qquad (22)$$

where y_{it} is amount of charitable contribution deduction claimed in county c in year t, β_0 is constant term, $\boldsymbol{X_{ct}}$ is vector of control variables, and $\boldsymbol{\varepsilon_{ct}}$ is error term. We are mainly interested in estimating the treatment effect, β_3. We predict that the control group will have no change in the amount of donation and the treatment group will increase their donation holding all other things constant. Hence, our prediction for β_3 would be positive.

3.4.1 Dependent Variable

Data on charitable contribution deduction claimed by taxpayers is available at Statistics of Income from the Internal Revenue Service. The Internal Revenue Service's (IRS) Statistics of Income program provides Individual Income Tax Data by state, county and ZIP code. We use ZIP code level data instead of county level data for the empirical analysis because the county level data does not have data on the amount of charitable contribution deduction claimed for years 2004 to 2010. We will map ZIP code data to counties using year-specific 5 digit ZIP code to county crosswalks. The data is also classified by adjusted gross income level which makes it possible for a subgroup analysis based on adjusted gross income level.

3.4.2 Independent Variables

Our measurement for political alignment and identification with the government is vote shares of county residents for the winning president. We use this data to assign counties into control groups or treatment groups. The data on share of votes is available at Voting and Elections Collection from CQ Press only at the county level and not ZIP code level.

3.4.3 Control Variables

We control for economic activity and demographic characteristics at the county level in our analysis. To control for economic activity, we use data on unemployment rate and income. Unemployment rate by county is available at the Bureau of Labor Statistics, which contains annual averages of labor force population, employed population, and unemployed population in addition to unemployment rate. Adjusted gross income data is available at Statistics of Income from the Internal Revenue Service. We also control for demographic characteristics for each county by using population data provided by the Census Bureau. The Census Bureau has Population and Housing Unit Estimates Datasets. The datasets include intercensal estimates of population by age, sex, race, and ethnic origin.

Table 19. Sample Characteristics

	Transition from Bush to Obama (2005 - 2012)		
Variables	Control	Treatment	p-value
Income Tax Return Variables			
Charitable Deduction Amount, $	96,355 (246,887)	24,683 (87,671)	<0.001
Charitable Deduction Amount per return, $	832 (428)	773 (457)	<0.001

Charitable Deduction Claim Rate	20.28	15.56	<0.001
	(11.92)	(9.89)	
Adjusted Gross Income, $	4,846,702	1,188,631	<0.001
	(11,800,000)	(3,945,462)	
Adjusted Gross Income per return, $	48,865	44,092	<0.001
	(13,178)	(11,224)	
	59.26	64.07	
Paid Preparer Filing Rate	(8.86)	(9.17)	<0.001
Demographic Variables			
Total Population	184,073	52,103	<0.001
	(427,392)	(137,199)	
Share of Female Population	0.50	0.50	0.277
	(0.179)	(0.219)	
Share of White Population	0.86	0.89	<0.001
	(0.142)	(0.111)	
Economic Variables			
Share of Employed Population	0.47	0.45	<0.001
	(0.0631)	(0.0675)	
Unemployment Rate	7.15	6.74	<0.001
	(3.07)	(2.98)	
Number of observations (county x year)	2,048	16,956	19,004

Note: Charitable deduction amount and adjusted gross income amount in thousands, Standard deviation in parentheses.

3.5 Results

3.5.1 Sample Characteristics

Table 19 presents the summary statistics of the control group and treatment group separately. Some notable differences were that taxpayers in control group claimed more charitable deduction amount than taxpayers in the treatment group on average. The amount of charitable deduction per return was $824 for the control group and $774 for the treatment group. Counties in the control group also have higher adjusted gross income per return ($48,865 in control group vs $44,092 in treatment group). The paid preparer filing rate was higher in the treatment group than the control group (59.26% in control group vs 64.07% in treatment group). In both groups, the percentage of White population was higher than other racial groups. All the differences were statistically significant at 1%.

3.5.2 Difference-in-Difference Analysis

Table 20 report the results from the difference in difference estimation. The sample period for Table 20 is from 2005 to 2012 so it covers the presidential election when there was a turnover from Republican to Democrat. The first and second columns report results when the dependent variable is the total amount of charitable contribution deduction claimed at the county level. The third and fourth column report results when the dependent variable is average charitable contribution deduction claimed at the county level.

Table 20. Generalized Linear Model Results (Difference-in-Difference Analysis)

	Dependent Variable			
	Total Charitable Deduction		Charitable Deduction per Return	
Variables	(1)	(2)	(3)	(4)
Treatment	-0.176**		-0.064*	
	(0.076)		(0.034)	
Treatment X Post Election	0.040***	0.052***	0.048***	0.043***
	(0.015)	(0.014)	(0.012)	(0.010)
Adjusted Gross Income	-0.000*	0.000***	0.000***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Unemployment Rate	-0.118***	-0.041***	-0.087***	-0.037***
	(0.013)	(0.002)	(0.005)	(0.002)
Total Population	0.000***	-0.000	-0.000**	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Black Population	-0.000***	-0.000*	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Constant	10.002***	9.388***	0.497***	-0.133***
	(0.190)	(0.020)	(0.060)	(0.017)
County Fixed Effects	No	Yes	No	Yes
State Fixed Effects	No	No	No	No
Year Fixed Effects	Yes	Yes	Yes	Yes
State X Year Fixed Effects	Yes	No	Yes	No

Note: Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1, Pre-election corresponds to year 2005 to 2008. Post-election corresponds to year 2009 to year 2012. Control group are countries that voted for Bush in 2004 and then Obama in 2008. Treatment group are counties that voted for Bush in 2004 and did not vote Obama in 2008. Coefficients presented here are from GLM estimation with gamma distribution and log link

Regression models (1) and (3) include year fixed effects and state by year fixed effects. The coefficient in the second row in columns (1) and (3) indicates that the total amount of charitable contribution deduction is 4.06% higher and the average charitable contribution deduction is 4.89% higher in the treatment group post 2008 election. Models (2) and (4) include county fixed effects and year fixed effects. According to the coefficients in the second row of models (2) and (4), the total amount of charitable contribution deduction is 5.32% higher in the treatment group post 2008 election and the average charitable contribution deduction is 4.39% higher in the treatment group post 2008 election. This result aligns with the hypothesis, which suggests taxpayers are more likely to donate to charity organization and claim tax deduction when their political interests are not aligned with the government.

3.5.3 Event Study

We test the parallel trends assumption behind the difference-in-differences model by examining whether there are differences in trends for charitable contribution deduction in the pre-election period. Figures 4 and 5 present the observed means of the total charitable deduction and charitable deduction per return, respectively, for the control and treatment counties for each year. We conduct an event study analysis to test the parallel trend assumption in more detail.

Figure 4. Total Charitable Deduction Parallel Trend Graph

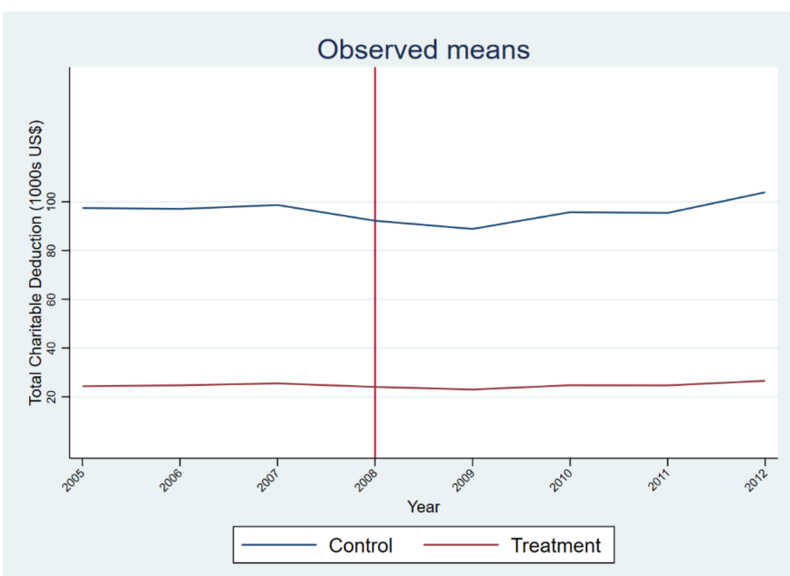
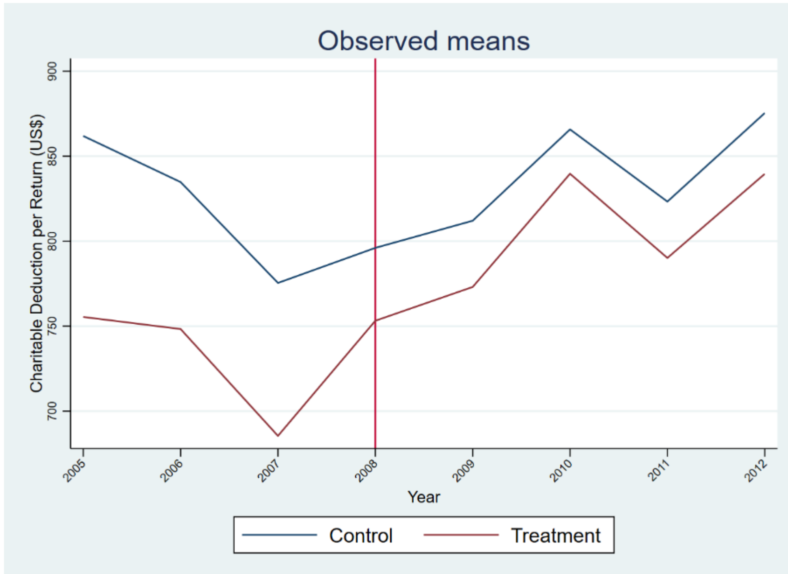


Figure 5. Charitable Deduction per Return Parallel Trend Graph



The estimation model we use is generalized linear model. Table 21 presents the results from the event study analysis. Models (1) and (2) report estimates for total county charitable contribution deduction amount. Models (3) and (4) present the event study results for average charitable contribution deduction. Event study analysis from models (1) and (3) include control variables and state by year fixed effects. In models (2) and (4), county fixed effects and year fixed effects are included in the model instead of state by year fixed effects.

The estimate from model (1) suggests that differences in pre-election trends for total charitable contribution deduction amount are not statistically significant between control counties and treatment counties. Figure 7 displays the event study from model (1). However, estimates from models 2, 3, and 4 shows that differences in pre-election trends for total charitable contribution deduction amount and average charitable contribution deduction are statistically significant between control and treatment counties. Figure 8, 9, and 10 graphs the estimates from event study in models (2), (3), and (4). Hence, we cannot confidently claim that higher charitable contribution deduction for treatment counties compared to control counties are due to change in political alignment alone.

Table 21. Event Study

	Dependent Variable			
	Total Charitable Deduction		Charitable Deduction per Return	
	(1)	(2)	(3)	(4)
Pre-Election				
2005*Treatment	-0.018	-0.042***	-0.066***	-0.068***
	(0.014)	(0.009)	(0.010)	(0.009)
2006*Treatment	-0.004	-0.019**	-0.039***	-0.046***
	(0.011)	(0.008)	(0.009)	(0.008)
2007*Treatment	-0.011	-0.011	-0.034***	-0.068***
	(0.011)	(0.007)	(0.010)	(0.009)
Post-Election				
2009*Treatment	0.043**	0.024	0.009	-0.008
	(0.019)	(0.016)	(0.014)	(0.010)
2010*Treatment	0.040*	0.040**	0.020	0.003
	(0.021)	(0.018)	(0.016)	(0.013)
2011*Treatment	0.026*	0.036**	0.008	-0.001
	(0.013)	(0.015)	(0.011)	(0.009)
2012*Treatment	0.017	0.036**	0.014	-0.003
	(0.016)	(0.017)	(0.012)	(0.011)
Control Variables	Yes	Yes	Yes	Yes
State by Year Fixed Effects	Yes	No	Yes	No
Year Fixed Effects	Yes	Yes	Yes	Yes
County Fixed Effects	No	Yes	No	Yes

Note: Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

3.6 Conclusion

The paper offers a study of the effect of identification with the government on taxpayers' charitable contribution deduction claim behavior. Contributing to a charity organization of choice and claiming tax deduction allows taxpayers to direct payments to activities they value more by reducing their taxes that go to government spending they do not support. We use the change in administration, the transition from the Bush administration to the Obama administration, to evaluate the causal impact change in political alignment on charitable contribution deduction amount at the county level. Our observations were counties where the majority voted for Bush in the 2004 presidential election. Among those counties, the control group was counties where the majority voted for Obama during the 2008 election. The treatment group was counties where the majority did not vote for Obama.

We find that the total charitable contribution deduction amount is 5.32% higher for counties in the treatment group after the 2008 election. However, the parallel trend assumption is not supported by the event study. Hence, further study is needed to establish causality. Moreover, it would be interesting to see whether the same result is observed when the transition in the administration was from President Obama to President Trump, as this transition accentuated the polarization of voters in the United States. Change in political alignment may impact other behaviors. Cullen, Turner, and Washington (2018) find an increase in tax evasion for counties with less political alignment.

Appendices

Appendix A

General Instruction

Welcome and thank you for participating in this experiment. It is important that you do not talk or try to communicate with other participants during the session. If you have any questions, please raise your hand, and the experimenter will approach you and answer your questions in private.

Summary

The experiment has two parts. Part 1 contains four stages. The instruction for each stage will be provided as you begin each stage. After you complete the fourth stage in Part 1, you will proceed to Part 2. In Part 2, you will be given the option to invest your earnings from Part 1.

Payment Protocol

One of the four stages in Part 1 will be randomly selected for payment by drawing a numbered ball from a bag with 4 bingo balls that are numbered 1 to 4. For example, if a drawn bingo ball number is 2, Stage 2 will be chosen for payment. Since you don't know which stage will be chosen for payment, please decide carefully in every stage.

Individual Accountability Audit Instruction

Please read the instructions carefully to help you understand the task and earn more money.

Description

There will be 7 rounds in this stage. At the start of each round, you will receive an income of 500 cents. Your task is to decide how much of your income to report and pay taxes on reported income. The tax rate is 30% and is the same for all participants. After you submit your taxes, there is a chance that you will be audited. If you are not audited, then you pay tax on the amount you reported. If you are audited and you have reported less than your full income, then you pay tax on your income plus a fine. The fine is the amount of unpaid taxes.

An Example of Your Round Payoff

Suppose you were given 100 cents, but you declared 50 cents as your income. If you are not audited, then you pay 15 cents (50×0.3) in taxes and your round payoff is 85 cents $(100 - 15)$. If you are audited, then you pay 30 cents (100×0.3) in taxes as well as a fine of 15 cents $(30 - 15)$ and your round payoff is 55 cents $(100 - 30 - 15)$.

Audit Rule in Stage 1

In each round, 3 out of 15 people will be randomly selected for audit. Everyone in the room is assigned a unique number between 1 to 15. There are 15 bingo balls that are numbered from 1 to 15. The experimenter will first draw a ball from the bingo cage, after that he or she will draw 2 more balls. If any of the numbers drawn from the bingo cage matches the number given to you, you will be audited.

Practice Rounds

Before you participate in Real Rounds, you will first participate in two Practice Rounds. In the Practice Rounds, the audit results are not randomly selected. You will not be audited in the first Practice Round but will be audited in the second one. No money can be made or lost in the Practice Rounds. They are only for practice.

Group Accountability Audit Instruction

Instruction for Stage 3

Please read the instructions carefully to help you understand the task and earn more money.

Everything is same as Stage 1 and Stage 2 except the audit rule.

Audit Rule in Stage 3

You will be randomly matched with two other people in the room to form groups of three. The groups will remain fixed for 7 rounds.

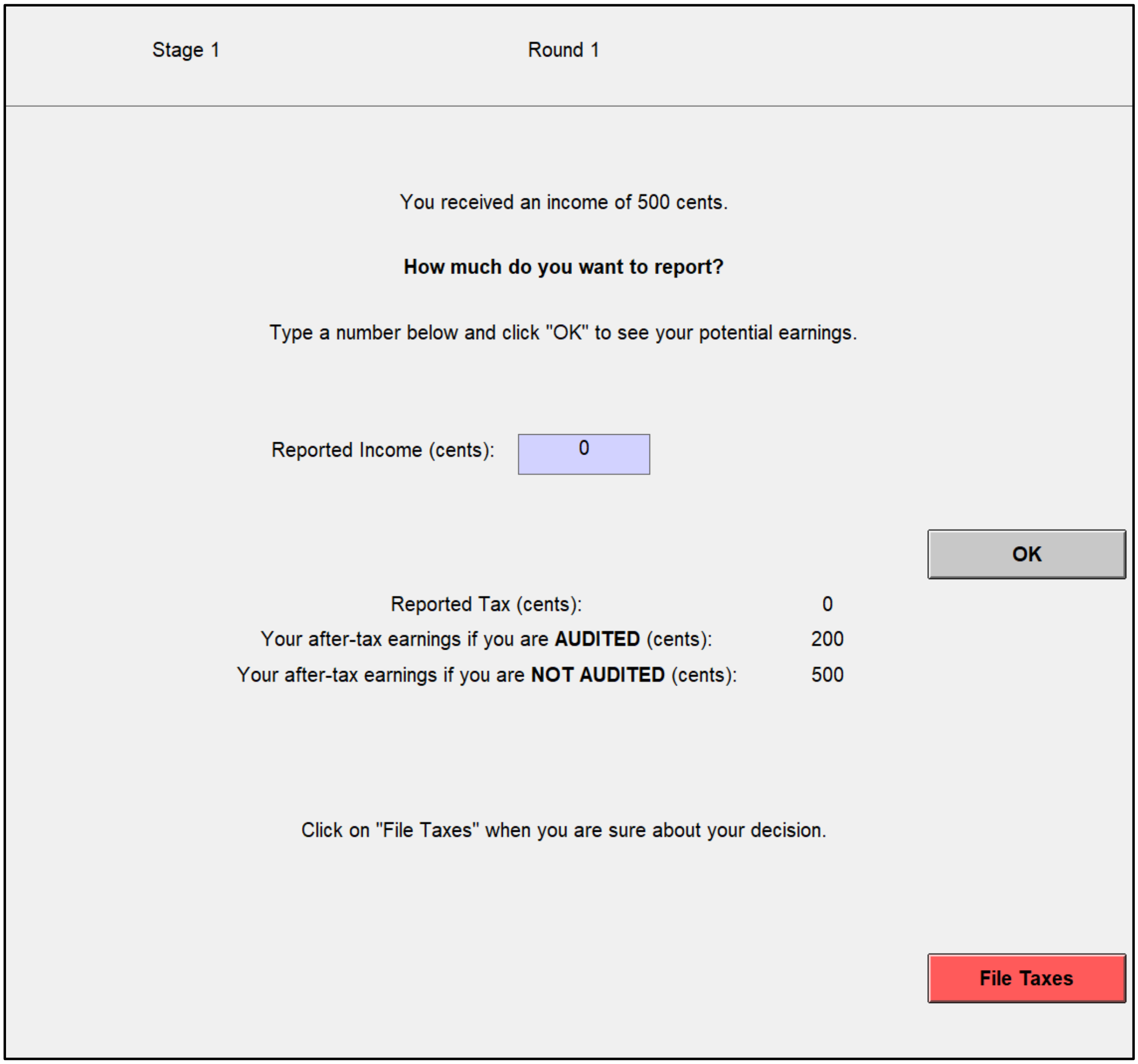
The experimenter will first draw a ball from the cage. The person with matching numbers will be audited first. The remaining 2 audits will be selected in the following way.

1. If the person who was audited first did <u>not</u> fully report his or her income,
   - then the rest of two group members in which that person belongs to will be audited.
2. If the person who was audited first did fully report his or her income,
   - then the remaining 2 audits will be randomly selected using the bingo cage.

Practice Rounds

Before you participate in Real Rounds you will first participate in two Practice Rounds. In the Practice Rounds, the audit results are not randomly selected. In the first Practice Round one of the members in your group is selected for audit and did not fully report income. In the second Practice Round one of the members from other group is selected for audit and did not fully report income. No money can be made or lost in the Practice Rounds. They are only for practice.

Figure A. Subject Screen



Appendix B

Subject Instructions

Talking is NOT allowed in this experiment.

If you have a question, please raise your hand, and the experimenter will approach you and answer your question in private.

Introduction

This is a study of how people make decisions. You will make multiple decisions given the information available.

A Monitor

One of the persons in the room is randomly chosen to be the monitor for today's study. The monitor is in charge of verifying that the experimenters follow the instructions explained below.

Decision Task

You will make multiple decisions. Each decision consists of allocating an endowment between your personal account and your charity of choice (selected from a list of 10 charities). The decision tasks differ from each other in terms of endowments you receive, and the relative value of each dollar allocated to the charity and your personal account.

Anonymity

This experiment is constructed such that other subjects will not know your personal decision. The experimenter will ask you whether or not you want to be recognized for your donations when you get paid privately. If you want to receive recognition from your designated charity for your contribution, then the experimenter will transfer your donation in your name. On the other hand, if you want to be anonymous, the experimenter will transfer your donation anonymously to the charity.

Payment Protocol

One of the decisions will be randomly selected to determine your payment and donation. Since you do not know which decision will be chosen for payment, please decide carefully in every allocation decision. There is no additional show up fee in this experiment. You will be paid in private at the end of the experiment. After everyone is paid, the monitor and an experimenter will go to the GSU Foundation Office (Suite 533, One Park Place South) to deposit your donation along with the gift deposit form. After signing the form that verifies that the study was conducted according to instructions, the monitor is free to leave.

Baseline Instructions

Task A

You have 18 tokens. You decide how many tokens to donate.

Number of Tokens: 18

The value of each token in

Charity Account: $2

Your Private Account: $1

How much do you want to donate?

Task B

You have 12 tokens. You decide how many tokens to donate.

Number of Tokens: 12

The value of each token in

Charity Account: $3

Your Private Account: $1

How much do you want to donate?

Task C

You have e tokens. You decide how many tokens to donate.

Number of Tokens: e

The value of each token in

Charity Account: $3

Your Private Account: $1

How much do you want to donate?

Tax Framing Instructions

Task A

You have 24 tokens. You decide how many tokens to donate.

You need to pay 25% tax on the amount you keep.

Number of Tokens: 24

The value of each token in

Charity Account: $1.50

Your Private Account: $1

How much do you want to donate?

Task B

You have 12 tokens. You decide how many tokens to donate.

You need to pay 50% tax on the amount you keep.

Number of Tokens: 12

The value of each token in

Charity Account: $1.50

Your Private Account: $1

How much do you want to donate?

Task C

You have e tokens. You decide how many tokens to donate.

You need to pay 50% tax on the amount you keep.

Number of Tokens: e

The value of each token in

Charity Account: $1.50

Your Private Account: $1

How much do you want to donate?

Matching Instructions

Task A

You have 24 tokens. You decide how many tokens to donate.

You need to pay 25% tax on the amount you keep.

For each token you donate, your charity of choice will receive an additional $0.50 from the experimenter.

Number of Tokens: 24

The value of each token in

Charity Account: $1

Your Private Account: $1

How much do you want to donate?

Task B

You have 24 tokens. You decide how many tokens to donate.

You need to pay 50% tax on the amount you keep.

For each token you donate, your charity of choice will receive an additional $0.50 from the experimenter.

Number of Tokens: 24

The value of each token in

Charity Account: $1

Your Private Account: $1

How much do you want to donate?

Task C

You have e tokens. You decide how many tokens to donate.

You need to pay 50% tax on the amount you keep.

For each token you donate, your charity of choice will receive an additional $0.50 from the experimenter.

Number of Tokens: e

The value of each token in

Charity Account: $1

Your Private Account: $1

How much do you want to donate?

Rebate Instructions

Task A

You have 24 tokens. You decide how many tokens to donate.

You need to pay 25% tax on the amount you keep.

Experimenter refunds you 1/4 of your donation.

Number of Tokens: 24

The value of each token in

Charity Account: $1

Your Private Account: $1

How much do you want to donate?

Task B

You have 24 tokens. You decide how many tokens to donate.

You need to pay 50% tax on the amount you keep.

Experimenter refunds you 1/6 of your donation.

Number of Tokens: 24

The value of each token in

Charity Account: $1

Your Private Account: $1

How much do you want to donate?

Task C

You have e tokens. You decide how many tokens to donate.

You need to pay 50% tax on the amount you keep.

Experimenter refunds you 1/6 of your donation.

Number of Tokens: e

The value of each token in

Charity Account: $1

Your Private Account: $1

How much do you want to donate?

Stochastic Rebate Instructions

Task A

You have 24 tokens. You decide how many tokens to donate.

You need to pay 25% tax on the amount you keep.

You have 1/4 chance of getting your donation fully refunded and 3/4 chance of getting nothing refunded from the experimenter.

Number of Tokens: 24

The value of each token in

Charity Account: $1

Your Private Account: $1

How much do you want to donate?

Task B

You have 24 tokens. You decide how many tokens to donate.

You need to pay 50% tax on the amount you keep.

You have 1/6 chance of getting your donation fully refunded and 5/6 chance of getting nothing refunded from the experimenter.

Number of Tokens: 24

The value of each token in

Charity Account: $1

Your Private Account: $1

How much do you want to donate?

Task C

You have 2 tokens. You decide how many tokens to donate.

You need to pay 50% tax on the amount you keep.

You have 1/6 chance of getting your donation fully refunded and 5/6 chance of getting nothing refunded from the experimenter.

Number of Tokens: e

The value of each token in

Charity Account: $1

Your Private Account: $1

How much do you want to donate?

Appendix C

Equivalent Condition for Matching Subsidy and Rebate Subsidy

In Eckel and Grossman (2003), subjects received subsidies in form of matching or rebate for their charitable contributions. The following is the budget constraint of an individual faced with matching subsidy in Eckel and Grossman (2003).

$$c + x = y \tag{23}$$

The parameter c is consumption, x is money donated to the charity, and y is endowment. The amount of money charity receives is $d = (1 + s_m)x$, where s_m is the matching rate. Using this information, the budget constraint can be rewritten as $c + \frac{d}{1+s_m} = y$.

The budget constraint of an individual faced with rebate subsidy is

$$c + x = y + xs_r \tag{24}$$

Parameter s_r represents the rebate rate. In this case, the charity receives, $d = x$ so the budget constraint can be rewritten as $c + d(1 - s_r) = y$. The two above budget constraints are equivalent when $s_m = \frac{s_r}{1-s_r}$.

Stochastic Rebate Subsidy Embedded in Tax Framework

Stochastic rebate subsidy is a variation of rebate subsidy. The difference is that contributor receives all donation back with ρ probability and nothing with $1 - \rho$ probability. Hence, the expected budget constraint of a donor provided with rebate subsidy is

$$c(1 + t) + x(1 - \rho) = y, \qquad x = d \tag{25}$$

To keep the expected rebate rate equivalent in deterministic rebate subsidy and stochastic rebate subsidy, the following condition needs to be satisfied.

$$\rho = s_r \tag{26}$$

Risk Averse Individuals and Stochastic Rebate

Suppose we assume that subjects who are risk averse have the following preference over own income and donation received by the charity.

$$u(c,x) = f(c) + v(x) \tag{27}$$

for some concave and increasing $f(.)$ and $v(.)$.

1. The decision problem in case of the deterministic rebate scenario is

$$\max_x [f\big((1-t)(y-x) + s_r x\big) + v(x)] \tag{28}$$

The optimal charity contribution, x then is implicitly determined by

$$f'\big((1-t)y - (1-t-s_r)x\big) = \frac{v'(x)}{1-t-s_r} \tag{29}$$

Using notation $d_r = x,$ the function can be rewritten as

$$f'\big((1-t)y - (1-t-s_r)d_r\big) = \frac{v'(d_r)}{1-t-s_r} \tag{30}$$

2. The decision problem in case of the stochastic rebate scenario is

$$\max\,[s_r f\big((1-t)(y-x) + x\big) + (1-s_r)f\big((1-t)(y-x)\big) + v(x) \tag{31}$$

The optimal total charity contribution, d_{sr} is implicitly determined by

$$-ts_r f'\big((1-t)y + td_{sr}\big) + (1-t)(1-s_r)f'\big((1-t)(y-d_{sr})\big) = v'(d_{sr}) \tag{32}$$

Suppose $d_r = d_s = d$

Then

$$(1-t-s_r)f'\big((1-t)y - (1-t-s_r)d\big)$$
$$= (1-t-s_r)f'\big((1-t)(y-d)\big) + ts_r\big(f'\big((1-t)(y-d)\big) - f'\big((1-t)y + td\big)\big) \tag{33}$$

By concavity of $f(.)$,

$$f'\big((1-t)(y-d)\big) > \ f'\big((1-t)y-(1-t-s_r)d\big) > f'\big((1-t)y+td\big) \qquad (34)$$

$$\begin{aligned}(1-t-s_r)(f'\big((1-t)y-(1-t-s_r)d\big)-f'\big((1-t)(y-d)\big))\\ = ts_r(f'\big((1-t)(y-d)\big)-f'\big((1-t)y+td\big))\end{aligned} \qquad (35)$$

The left-hand side is negative, and the right-hand side is positive. The equation does not hold. Therefore, $d_{\,r} \neq d_{sr}$

Suppose $d_{\,r} < d_{sr}$

Then

$$\begin{aligned}(1-t-s_r)f'\big((1-t)y-(1-t-s_r)d_{\,r}\big)\\ +ts_r f'\big((1-t)(y+td_{sr})\big)-(1-t)(1-s_r)f'\big((1-t)(y-d_{sr})\big) > 0\end{aligned} \qquad (36)$$

$$\begin{aligned}(1-t-s_r)(f'\big((1-t)y-(1-t-s_r)d_{\,r}\big)-f'\big((1-t)(y-d_{sr})\big))\\ +ts_r\Big(f'\big((1-t)y+td_{sr}\big)-f'\big((1-t)(y-d_{sr})\big)\Big) > 0\end{aligned} \qquad (37)$$

By concavity of $f(.)$,

$$f'\big((1-t)(y-d_{sr})\big) > f'\big((1-t)y-(1-t-s_r)d_{\,rebate}\big) \qquad (38)$$

and

$$f'\big((1-t)(y-d_{sr})\big) > f'\big((1-t)y+td_{s.\ rebate}\big) \qquad (39)$$

Equation (33) cannot be positive. Therefore, $d_{\,r} < d_{sr}$ is not true.

As a result, $d_{\,r} > d_{sr}$, if individuals are risk averse towards private consumption.

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