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An Experimental Study on the Effects of Democracy on Tax Compliance[†]

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† Go to http://www.econ.sinica.edu.tw/english/Appendix/483-A.pdf to visit the article page for online Appendix.

ABSTRACT

We experimentally investigate behavioral compliance and the effects of democracy on tax compliance in an environment in which subjects are allowed to vote on the tax, audit, or fine rate. We control for the selection effect by adding a randomization stage of computer decisions similar to that proposed by Dal Bó et al. (2010). Our experimental evidence shows that democracy has an effect on compliance when fine rates are to be voted on. We also find that subjects behave more compliantly when a higher audit rate or a higher fine rate is applied than when a lower counterpart is applied. However, subjects having voted for a high rate are not necessarily more compliant than those who voted for a low rate, especially when a high rate is applied.

1. INTRODUCTION

Because government revenues and therefore government spending are very much related to the amount of taxes that are reported honestly, improving tax compliance is an important job for tax authorities in many countries. To attain this goal, the central question concerns how taxpayers' behavioral compliance is affected by various aspects of the tax system. To explore this question, appealing to empirical works using observational data is a possibility. However, as pointed out by Andreoni et al. (1998), the difficulty of this approach is the lack of reliable information on taxpayers' reporting behavior. As a consequence, experimental methods are likely to be the most, if not the only, viable way.

Three aspects of the tax system are generally examined by experimental studies, namely the tax rate, the audit rate, and the penalty rate. In the experimental literature, some studies assume that these variables are fixed, and examine the impacts of changes in these variables on compliance (Spicer and Becker, 1980; Spicer and Thomas, 1982; Becker et al., 1987; Alm et al., 1990; Beck et al., 1991; Collins and Plumlee, 1991; Alm et al., 1992b; Alm et al., 1992; and Alm et al., 1995), while some other studies allow some or all of these variables to be non-fixed or endogenously determined (Alm et al., 1992a; Alm et al., 1993; Alm and McKee, 2004; Clark et al., 2004; Gërxhani and Schram, 2006).

All aspects of the tax system in the above studies are set up by the government. Individuals have no chance to participate in the establishment of these institutions, nor can they express their preferences for the various aspects of the tax system. Under this framework, individuals who report income honestly can only rely on the institutions set up by the government, or switch their honest behavior to cheating in order to punish tax dodgers. As a result, compliance may be kept at a low level. Although this outcome is undesirable, the above studies fail to incorporate the fact that individuals in a political economy often have direct or indirect influence on government policies.[1] In fact,

[1] An example of direct influence is the voting on tax increases to improve Atlanta's infrastructure. As is reported by The Economist (2011), Atlantans have the longest average rush-hour commute in America, and according to Georgia's government, the state spends less per head on transport than any other state with the exception of Tennessee. Since improving the infrastructure means raising taxes, in June 2010 Georgia's legislature decided to let citizens vote on whether to raise their own taxes. As for indirect influences, individuals may bring their influence to bear on or petition the legislative members to pass favorable laws or regulations.

some experimental studies have reached a conclusion that individual participation in the decision-making process can improve compliance or cooperation. In a public good experiment, for instance, Putterman et al. (2011) allow subjects to vote on whether "private account" or "public account" contributions are subject to penalties. They find that there is almost uniform support for penalizing non-contribution to the public account, and contributions to the public good are significantly higher when there are formal sanctions than when sanctions are absent.

In tax compliance experiments, Pommerehne et al. (1994) suggest that a democratic process tends to raise tax morale and therefore tax compliance. Alm et al. (1993) find that compliance is higher if subjects are allowed to select the public sector expenditure program themselves by majority voting. By designing an experiment in which the punishment is certain, that is, the audit probability is one, Feld and Tyran (2002) ask subjects to state their contributions for all possible voting outcomes. They find that the possibility of voting on fines significantly increases tax compliance, since subjects who vote for the punishment scheme feel obliged to consistently comply with their decisions by making larger contributions.

By contrast, Alm et al. (1999) obtain different results. They find that although the impact of voting on the tax rate is mixed, in all four fine sessions the majority votes for the low fine rate, and in the other four audit rate sessions the majority votes for the low audit rate. Furthermore, the average compliance rates in the voting stage are lower than the corresponding average compliance rates in the no-vote stage in all sessions. They appeal to the notion of the social norm that an individual will comply as long as he or she believes others will comply. The group decision regarding enforcement reveals the lack of a social norm of tax compliance, and thus compliance with voting is lower than that without voting.

Despite the inconsistent results shown above, as pointed out by Dal Bó et al. (2010), a central problem with the examination of the effects of democracy is that "one cannot rule out the possibility that there are unobserved factors that explain both responses to policies and either the degree of participation in policymaking or the particular policies selected." Briefly put, there is a selection problem. That is, the observed higher level of cooperation under voting may be attributable to individuals' inherent preferences for the chosen policy, and not simply because of their participation in the democratic process. To control for the selection effect, Dal Bó et al. (2010) add a stage of computer decisions after voting. In their prisoner's dilemma experiment, subjects vote on two alternatives: modifying the payoff matrix or not, and then the computer

decides whether to consider the outcome of majority voting or not. If the computer accepts the outcome of majority voting, then whether the payoff matrix is modified is consistent with the outcome of majority voting. If the computer rejects it, then the computer will decide whether to modify the payoff matrix or not. After the computer's decision, subjects play the prisoner's dilemma game for another ten rounds with the payoff matrix determined by the above procedure. The addition of computer decisions breaks the direct connection between the preference for the chosen alternative and the outcome of majority voting, and therefore the effects of democracy can be properly measured.

This paper's identification strategy is inspired by that of Dal Bó et al. (2010). We examine tax compliance in an environment that allows subjects to vote on the tax, audit, or fine rate. There are several major differences between Dal Bó et al. (2010) experimental design and ours. First, Dal Bó et al. (2010) use a prisoner's dilemma game, while ours is a tax compliance game. Thus, their focus is very different from ours. Second, in Dal Bó, Foster, and Putterman's experiments, subjects in groups of four first play the game without voting for ten rounds and then vote on modifying the payoff matrix before the start of the eleventh round. Since a procedure of computer decision is added to determine whether the treatment is endogenous (the computer considers the votes) or exogenous (the computer does not consider the votes), subjects can only vote once, and the outcome of this one-time majority voting applies to all of the next ten rounds. In our experiments, the voting procedure occurs at the beginning of each of the ten rounds involving majority voting, so that we can not only observe subjects' behavioral compliance, but also obtain a rich set of information associated with their compliant behavior conditional on their voting decisions. Third, in Dal Bó, Foster, and Putterman's experiments, subjects are informed of whether or not the computer considers the votes. We perceive that a reason for this design is that given the endogenous and exogenous treatments defined by Dal Bó, Foster, and Putterman, subjects need to know whether the computer considers the votes or not. By contrast, in our experiment subjects are only informed of the tax, audit or fine rate to be applied, but not of the way in which it is determined (i.e., majority voting or random assignment by the computer). This is the most important difference between our experimental design and that of Dal Bó et al. (2010).

The reasoning for the third difference between Dal Bó, Foster, and Putterman's design and ours is the following. In Dal Bó, Foster, and Putterman's experiments, in addition to informing subjects of whether the computer considers the votes or not,

they also inform subjects of the outcomes of majority voting if the computer considers votes.[2] This is straightforward, since if the computer considers votes, then the outcome of voting is certainly revealed. However, they adopt an asymmetric design — the outcomes of majority voting are not revealed when the computer does not consider votes. Hence, there is a possible informational effect stemming from the effects of democracy. To test the information hypothesis, Dal Bó, Foster, and Putterman conduct additional sessions, in which, after voting subjects are told whether at most two or at least two subjects in the group voted for modification, regardless of whether the computer takes votes into account.

In the cases where the outcomes of majority voting are announced, a signaling effect arises from the fact that a subject would be able to decipher his or her peers' preferences from the outcome of voting and then respond to his or her expectation of these preferences. For example, in the tax compliance setting of the current study, if a low tax rate is determined by majority voting and this is conveyed to subjects, a subject may infer that most of his or her peers in the group may not comply because they prefer a low tax rate to a high tax rate. This signaling effect is found in Alm et al. (1999), who refer to this effect as social norms.

In sum, if we reveal the computer's decisions to subjects, and if the computer considers the outcome of voting, then there exists a possible signaling effect. As a consequence, subjects' behavior may be affected by both the signaling effect and the effect of democracy. To separate these two effects, more sessions are needed to be conducted, as is done by Dal Bó, Foster, and Putterman. Hence, to avoid the problem of asymmetric information and to use our budget for this research efficiently, subjects are only informed of the tax, audit or fine rate to eventually be applied, and neither the computer's decision nor the outcome of majority voting is announced. One may suspect that under this setting subjects may not perceive that they are involved in a democratic situation. To fix this problem, we inform subjects that the computer will consider the outcome of majority voting with a certain probability. We do not inform them of the exact magnitude of this probability, but since it is set at 0.7 in the experiments, in most cases the computer's decisions would coincide with the outcomes of majority voting. In addition, following Dal Bó et al. (2010), we set the size of groups to be four to maximize the chance of a tie. By allowing subjects to vote in each of the ten rounds, instead of only once as in Dal Bó et al. (2010), subjects are able to learn

[2] Subjects are not informed of the distribution of votes. If there is a tie, then the computer breaks the tie, but this remains unannounced to subjects.

and to adjust their voting and cooperative decisions from past experience.

Because a low tax, audit, or fine rate is used in the no-vote stage, compared with Dal Bó et al. (2010), the drawback of our experimental design is that we are not able to compare the levels of compliance between settings with and without democracy among individuals who voted for a particular tax, audit or fine rate. We are restricted to comparing the level of compliance in the no-vote stage with that in the voting stage where a low tax, audit or fine rate is applied.

We circumvent this deficiency in terms of controlling for unobserved heterogeneity by using a fixed effects regression strategy. With repeated observations based on subjects' behavioral compliance, we use a fixed effects regression specification to control for their time-invariant heterogeneity in tax compliance behavior. The unobserved factors that remain in determining a subject's behavioral compliance will be time-varying (i.e., will change over rounds) and these time-varying factors are unlikely to be correlated with his or her voting behavior. Furthermore, we examine the difference in a subject's compliant behavior when he or she voted for a high versus a low tax, audit, or fine rate. If our fixed effects specification is successful in controlling for the correlation between compliant behavior and voting behavior due to unobserved factors, we will not find a significant difference in compliance when he or she voted differently.

Our experimental evidence shows that democracy has an effect on compliance only when fine rates are to be voted on. Furthermore, our findings show that subjects behave more compliantly when a high audit rate or a high fine rate is applied than when a lower counterpart is applied, and that their voting decisions have no significant effects on their compliant behavior. These findings suggest that to improve compliance, democratic participation in determining the fine rate is a feasible way, or the government can just impose a high audit rate or a high fine rate instead.

The remainder of this paper is organized as follows. Section 2 describes the experimental design. Section 3 presents the theoretical predictions. Section 4 reports the results of the experiment, and Section 5 concludes.

2. EXPERIMENTAL DESIGN

The fundamental experimental design of this paper is similar to those of experiments on VCM (the voluntary contribution mechanism) and tax compliance. In the experi-

ment, each subject receives an exogenous amount of income and he or she pays the tax according to the income that he or she declares. The tax is used to provide the public good that benefits only members in the same group. After declaration, the subject's true income is audited based on some probability. The subject who is audited and caught cheating will pay the evaded tax and a fine. A subject's original income net of the tax he or she has paid and the evaded tax and fine, if any, is his or her private good consumption. His or her payoff is the sum of the public good consumption and his or her private good consumption. To prevent any emotional responses, neutral terms are used in the experimental instructions. Furthermore, because the tax authority simply collects taxes and fines without making any decisions in the experiment, the role of the tax authority is not particularly mentioned.

Six treatments are conducted in this research. They are denoted as Tax-o1 (tax rate-order one), Tax-o2 (tax rate-order two), Audit-o1 (audit rate-order one), Audit-o2 (audit rate-order two), Fine-o1 (fine rate-order one), and Fine-o2 (fine rate-order two). The framework of the experiment and the magnitudes of various aspects of the tax system are provided in Table 1. Four sessions are conducted for each treatment and 12 subjects are recruited for each session, for a total of 288 subjects used in this study. To increase anonymity, two independent sessions under the same treatment are run at the same time, but subjects are unaware of this. All subjects are undergraduate students at National Chengchi University in Taiwan and none of them has previously participated in tax compliance or public goods experiments.

Each treatment consists of two parts, and each part contains 10 rounds. Subjects are informed of the contents of the two parts at the beginning of the experiment. All the experimental settings in the first part (rounds 1–10) of the Tax-o1, Audit-o1, and Fine-o1 treatments are the same, while in the second part (rounds 11–20) a voting process is added and subjects in the three treatments vote on different variables of the tax system. The three variables to be voted on are the tax rate, audit rate, and fine rate. Since subjects' compliant behavior and their attitudes towards the three variables may be affected by the timing of voting, to control for the order effect, three corresponding treatments with the opposite order of the two parts are also conducted. They are indicated as the Tax-o2 treatment, Audit-o2 treatment, and Fine-o2 treatment. Except for the order of voting, the three treatments are exactly the same as their counterparts.

The experimental procedures of the Tax-o1, Audit-o1, and Fine-o1 treatments are as follows. Subjects make decisions in each of the 20 rounds. In each round, the 12 subjects in the same session are randomly and anonymously divided into three groups

Table 1 Framework of the Experiment and Parameters Used for Various Aspects of the Tax System

Treatment	Tax-o1	Tax-o2	Audit-o1	Audit-o2	Fine-o1	Fine-o2
Part I: Rounds 1–10						
Voting	no	yes: on t	no	yes: on p	no	yes: on f
Tax rate (t)	0.2	0.2 vs. 0.4	0.2	0.2	0.2	0.2
Audit probability (p)	0.1	0.1	0.1	0.1 vs. 0.4	0.1	0.1
Fine rate (f)	3	3	3	3	3	3 vs. 6
Part II: Rounds 11–20						
Voting	yes: on t	no	yes: on p	no	yes: on f	no
Tax rate (t)	0.2 vs. 0.4	0.2	0.2	0.2	0.2	0.2
Audit probability (p)	0.1	0.1	0.1 vs. 0.4	0.1	0.1	0.1
Fine rate (f)	3	3	3	3	3 vs. 6	3

of size $n = 4$. To minimize the repeated-game effect, they are re-matched when a new round started. At the beginning of each round, four income levels (70, 90, 110 and 130 points) and four codes (A, B, C and D) are randomly assigned to the four subjects in the same group. Call the income assigned to a subject his or her true income w_i. When a new round starts, the four levels of income and the four codes are randomly reassigned. A subject knows his or her own code and income and the distribution of income, but not the income for each of the other three group members.

There are two stages in each round of the first part of the Tax-o1, Audit-o1, and Fine-o1 treatments. In stage one, the declaration stage, each subject is required to report a level of income R_i ($0 \leq R_i \leq w_i$), and the reported income is taxed at the rate $t = 0.2$. The tax is invested in the public account (the public good), and the rest of the income is maintained in the subject's private account (the private good). The marginal per capita return (MPCR) of the public good is set at $m = 0.5$. That is, each point invested in the public good yields every group member a return of 0.5 points. Note that m has to satisfy the condition $1/n < m < 1$ so that each individual has the incentive to cooperate and to cheat. After all subjects have reported their incomes, they proceed to the second stage, the auditing stage, in which each subject is audited by a probability $p = 0.1$. It is assumed that a subject's true income is revealed once he or she is audited. Any subject who is audited and caught cheating has to pay the evaded tax plus a penalty, which is twice the amount of the evaded tax. For simplicity,

in the experiment we inform subjects that the fine rate is 3. The fine rate is denote as f and here $f = 3$.

Given the above procedures, the expected monetary payoff for each subject i in each round of the first part of the Tax-o1, Audit-o1, and Fine-o1 sessions is given by

$$\pi_i = (1-p)(w_i - tR_i) + p[w_i - tR_i - ft(w_i - R_i)] + mt \sum_{j=1}^{n} R_j. \tag{1}$$

In equation (1), the sum of the first two terms is the subject's expected private good consumption and the third term is his or her public good consumption.

The tax rate $t = 0.2$, audit rate $p = 0.1$, and fine rate $f = 3$ serve as the benchmark. When subjects move to the second-part of the experiment, an additional voting process is added at the beginning of each round, and one of these benchmark values is to be voted against another higher value. Specifically, in the Tax-o1 treatment the four members in the same group vote between two alternative levels of tax rates, 0.2 and 0.4; in the Audit-o1 treatment the four members vote between two alternative levels of audit rates, 0.1 and 0.4; and in the Fine-o1 treatment the four members vote between two alternative levels of fine rates, 3 and 6. The other two variables that are not to be voted on remain at the same levels as in the first-part of the experiment. As a consequence, there are three stages in each round of the second part of the Tax-o1, Audit-o1, and Fine-o1 treatments: a voting stage, a declaration stage, and an auditing stage. Except for the variable to be voted on, Tax-o1, Audit-o1, and Fine-o1 are exactly the same in every other aspect.

Let us explain the second part in more detail by taking the Tax-o1 treatment as an example. At the beginning of each round of rounds 11 to 20, subjects are required to vote between two tax rates, 0.2 and 0.4, for their own groups via majority voting. Subjects are informed that after all group members have made their own voting decisions, the computer will randomly determine whether to accept the outcome of majority voting or not. If the computer accepts the outcome of majority voting, the tax rate for the group is determined accordingly. If the computer rejects the outcome of majority voting or if a tie occurs, the computer will randomly assign either tax rate to the group.

We set the probability that the computer randomly accepts the outcome of majority voting to be 0.7. Once the computer rejects the outcome of majority voting or when a tie occurs, the probability that the computer randomly assigns either tax rate to the group is 0.5. Subjects are only informed of the above procedure and the final

tax rate for their own group. They are unaware of the outcome of majority voting, the decision made by the computer, and the information regarding the probabilities for the computer's random choices. These settings aim to reduce speculation on the part of the subjects regarding other group members' voting decisions, and along with the setup where the size of each group was four persons, the selection problem can be kept to a minimum via the maximum possible intervention from the computer.

Following the voting stage, the second stage (the declaration stage) and the third stage (the auditing stage) of the second part of the Tax-o1, Audit-o1, and Fine-o1 treatments are exactly the same as the first and second stages in the first part of the three treatments. Given the above procedure, the expected monetary payoffs for the subject in the second part of the Tax-o1, Audit-o1, and Fine-o1 treatments are the same as in equation (1) except that the tax, audit, or fine rates are determined by majority voting and computer decisions.

At the end of each round of the experiment, each subject is informed of the result, which consists of the following information: the outcome of the voting stage (if there is one), the subject's declared income, his or her investment in the public account according to his or her declared income, the total income declared and the total investment in the public account excluding and including the subject's own investment, the code of the subject who is audited, the subject's payoff from his or her private account, the subject's payoff from the public account, the reduction in the subject's payoff if he or she is caught under-reporting, and the subject's payoff for this round.

In all sessions, subjects are given written instructions in Chinese. The experimenter reads the instructions aloud and answers any questions raised by the subjects. After reading the instructions, subjects answer four quiz questions in relation to the calculation of payoffs and the experimental procedures.[3] The experiment will not start until everyone has answered all questions correctly. Each session lasts about 90 minutes. The average payoff (including a participation fee of NT$100) for all participants is NT$529.98 (with a standard deviation of NT$24.85, a maximum of NT$595, and a minimum of NT$445.7).[4]

[3] An English translation of the Subjects' Instructions and quiz questions for the Tax-o1 treatment is provided in Appendix 1. Both Appendix 1 and Appendix 2 are available online.

[4] When these sessions were conducted, the exchange rate between the NT (New Taiwan) dollar and the US dollar was about 30:1. The part-time hourly wage rate for an undergraduate student in Taiwan is about NT$120.

3. THEORETICAL PREDICTIONS

To have a clear-cut theoretical prediction of subjects' behavior, it is assumed that all subjects are self-interested and maximize their own monetary payoffs, and that this feature is common knowledge to all subjects. Recall that when all the variables of the tax system are exogenously given in the no-vote part of the experiment, the subject's expected monetary payoff is characterized by equation (1). Differentiating equation (1) with respect to R_i yields

$$\frac{\partial \pi_i}{\partial R_i} = t(pf + m - 1). \tag{2}$$

Given the benchmark values of $p = 0.1$ and $f = 3$, and $m = 0.5$, equation (2) is certainly negative, implying that $R_i^* = 0$ for all i. That is, the dominant strategy for a self-interested and reward-maximizing subject is to report zero income, and hence he or she earns an expected payoff of $\pi_i = w_i(1 - pft) = 0.94w_i$. The corner solution of reporting zero income is the same as zero contributions in the VCM of public good provision.[5]

To find the equilibrium when a voting stage is involved, we can construct a two-stage game and solve the game by backward induction. The game proceeds as follows. In the first stage, all group members vote on two alternative levels of the tax, audit, or fine rate. Then, based on some probabilities, the computer randomly determines whether to accept the outcome of majority voting, and randomly assigns either level to the group if it rejects this outcome or if a tie occurs. To be consistent with our experimental design, it is assumed that subjects are only aware of the final tax, audit, or fine rates applied to their groups. In the second stage, given the final outcomes of the tax, audit, or fine rates, subjects declare income simultaneously and, after declaration, they are audited by some probability. When a subject makes his or her voting decision in the first stage, he or she assumes that all other group members have made their optimal voting decisions. When a subject makes his or her declaration decision in the second

[5] If there is no auditing, and hence no fines, taxes reported and paid are equivalent to voluntary contributions to public goods. The first-order condition will be reduced to $m - 1$, implying zero contributions given the assumption that $m < 1$. One may refer to Davis and Holt (1993) and John Ledyard's survey in Kagel and Roth (1995) for related studies.

stage, he or she assumes that other group members have chosen their optimal levels of declarations, and takes other group members' voting decisions and the computer's random assignment into consideration.

It is evident that once the tax, audit, or fine rate has been determined in the first stage, the subject's expected payoff will be characterized by equation (1), and as a result the equilibrium in the second stage is still solved by equation (2). By considering the equilibrium strategy adopted in the second stage, the subject makes his or her best voting decision in the first stage.

Let us start with the case in which the two tax rates are to be voted on. When in the first stage subjects vote between two tax rates, 0.2 and 0.4, they are aware of the fact that either tax rate will be selected eventually. They also understand that their votes will to some extent affect the outcome of majority voting and that this outcome will be accepted by the computer according to some probability. Hence, the subject will vote for a tax rate that yields him or her the higher expected payoff once the second stage arrives. In the second stage, since the sign of equation (2) is irrelevant to the tax rate and is negative given the benchmark values of p and f, the dominant strategy for the subject is still reporting zero income regardless of the outcome in the first stage. Given that zero income will be reported, $\pi_i = w_i(1 - pft) = 0.94w_i$ if the tax rate is 0.2 and $\pi_i = w_i(1 - pft) = 0.88w_i$ if the tax rate is 0.4. Hence, the subgame perfect equilibrium is that the subject votes for the low tax rate of 0.2 in the first stage and reports zero income in the second stage.

By applying similar analyses, the subgame perfect equilibrium for the case in which the two audit rates are to be voted on is that the subject votes for the high audit rate of 0.4 in the first stage and reports full income in the second stage. When the two fine rates are to be voted on, the subgame perfect equilibrium is that the subject votes for the high fine rate of 6 in the first stage and reports full income in the second stage.

Finally, how will democracy affect behavioral compliance? The hypothesis of the effect of democracy is that individuals will behave more cooperatively if they are provided with the opportunity to be involved in a political process than if they are not, and the more cooperative behavior is irrelevant to the outcome of the political process. We have employed a computer decision to control for the selection effect. To have an equal basis for comparison, we need to control further for the tax, audit, and fine rates, so that each of these variables has the same value with and without voting. Because the tax, audit, and fine rates are low without voting, the prediction of the effect of democracy is that those subjects for whom the low tax, audit, or fine rates are applied

in the voting rounds will behave more cooperatively than when they are in the no-vote rounds.

In the following section we will test the above equilibrium predictions, especially the effect of democracy. In addition, we will investigate subjects' behavioral compliance as well as compliance conditional on their voting decisions and the final magnitudes of the tax, audit, and fine rates applied to them.

4. EXPERIMENTAL RESULTS

We conducted twenty-four sessions in April and May of 2012 in the computer lab of the Department of Public Finance at National Chengchi University (NCCU) in Taiwan. We recruited subjects by posting advertisements on the homepage of NCCU and by emailing invitation letters to almost all undergraduate students at NCCU. Students who were willing to participate in our experiments were then selected as randomly as possible. Table A1 in Appendix 2, which is available online, reports that of the 288 subjects recruited, 74.31 percent of them were female, they had been studying at the university for an average of 2.11 years, their average age was 20.01 years, and 78.47 percent of them had taken one or more economics courses. The scale of the indicator "donation" ranged from one to six and the average was 2.09, meaning that, on average, subjects donated about NT$500 to NT$1,000 to charities during the year 2011. The scale of "risk-taking" ranged from 0 to 10, with 0 indicating not ready for taking any risks and 10 indicating fully prepared to take risks. The average level of risk-taking was 5.17, meaning that, on average, the subjects' attitude toward risks was modest.

4.1 A General Look at Compliant Behavior and Voting Decisions

We summarize the data resulting from the first 10 rounds and the last 10 rounds in each treatment in Tables A2 through A4 in Appendix 2. Round averages and standard errors of compliance rates are depicted in Figures A1 and A2 in Appendix 2.[6] In addition, Figure A1 provides the information regarding the average compliance rates conditional on subjects' voting decisions, and Figure A2 provides the average compliance rates conditional on the magnitudes of the tax, audit, or fine rate applied to the subjects in

[6] The standard error of the sample mean is calculated as $\sqrt{\sum_{i=1}^{n}(x_i-\overline{x})^2/(n-1)}/\sqrt{n}$, where x_i is the value of the observation, $\overline{x}$ is the sample mean, and n is the sample size.

the voting stage. The compliance rate for a subject is defined as his or her reported income divided by his or her true income. As we can see from Figures A1 and A2, except for the treatments of Audit-o1 and Audit-o2, subjects' compliance decisions are different between the two treatments with the same voting variable but in opposite voting stages. Hence, we do not combine the data, and instead report and discuss the evidence from each treatment separately.[7]

Several observations arise by looking at Tables A2 through A4. First, it is observed that average compliance rates for all six treatments lay between 0.54 and 0.64 in the first ten rounds, and declined to an average of 0.36 to 0.55 in last ten rounds. Second, in the no-vote rounds, although on average 26.04 percent of the subjects in the Audit-o2 treatment and 21.46 percent of the subjects in the Fine-o2 treatment reported zero income, these magnitudes are far below a hundred percent as is predicted by the theory. Even fewer than ten percent of the subjects reported zero income in each of the other four treatments.[8]

Third, in rounds with voting, on average, less than half of the subjects voted for the stricter values of the various variables of the tax system. Specifically, on average, 49.58 percent of the subjects in Tax-o1 and 43.33 percent of the subjects in Tax-o2 voted for the high tax rate. The percentages of subjects voting for the high fine rate were a little bit lower (42.5 percent in Fine-o1 and 35.83 percent in Fine-o2), but, on average only 31.04 percent of the subjects in the Audit-o1 treatment and 22.29 percent of the subjects in the Audit-o2 treatment voted for the high audit rate. These observations suggest that, on average, subjects preferred a less strict auditing environment. In addition, these observations are inconsistent with the theoretical predictions that subjects will vote for the low tax rate and high audit and fine rates when voting is allowed.

Fourth, in rounds with voting, there were only, on average, 6.04 percent to 17.5 percent of the subjects declaring zero income, and 7.5 percent to 19.79 percent of the subjects complying fully. These observations also fail to meet the theoretical predictions when voting is allowed.

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[7] Many studies regarding cooperation and punishment, for instance, Fehr and Gächter (2000, 2002) and Herrmann et al. (2008) also considered order effects, while for cost considerations or some other reasons Sefton et al. (2007) did not.

[8] Since the theoretical prediction is either zero compliance or complete compliance, evidence from experiments hardly coincides with these corner solutions. This phenomenon also occurs in many other experiments, e.g., experiments on the ultimatum game and the voluntary provision mechanism. One may refer to the surveys of Davis and Holt (1993), Kagel and Roth (1995), and Camerer (2003) for more details.

We summarize the above observations in Result 1 as follows:

Result 1 The theoretical prediction of zero compliance for the no-vote rounds in all six treatments and the vote rounds for the two tax treatments does not hold. The complete-compliance prediction for the vote rounds of the two audit treatments and the two fine treatments also fails. Furthermore, the experimental evidence does not support the theoretical predictions that subjects will vote for the low tax rate and report zero income, and that subjects will vote for the high audit rate and the high fine rate and comply fully.

4.2 Regression Analysis of the Effects of Democracy

We now examine whether the effect of democracy exists; that is, whether the level of compliance was higher if subjects were allowed to vote on various aspects of the tax system than when they were not. We estimate the following fixed effects regression model of tax compliance.

$$\text{compliance rate}_{it} = x_{it}\beta + \mu_i + \varepsilon_{it}, \tag{3}$$

where the subscripts i and t respectively represent subjects and rounds, x_{it} is a set of variables characterizing the experimental setting and subject behavior, β is a vector of coefficients to be estimated, μ_i is a fixed effects parameter, capturing subject i's time invariant heterogeneity in his or her compliant behavior, and ε_{it} is an error term, which is assumed to be normally distributed. We use two different sets of explanatory variables to explain the compliance rate. The first set, denoted as Specification I, consists of the variables "No-vote" (1 if voting was not allowed and 0 otherwise), "Round" (round number) and its square, "True income" (received income), the subjects' voting decision "Voted high" (1 if the subject voted for a high tax, audit, or fine rate, and 0 otherwise), "High rate" (1 if a high tax, audit, or fine rate was applied to the subject and 0 otherwise), and "Low rate" (1 if a low tax, audit, or fine rate was applied to the subject and 0 otherwise).

It is noted that we do not include a constant term in equation (3), allowing us to include mutually exclusive dummy variables "High rate," "Low rate," and "No-vote." The coefficients of these variables represent the average compliance rates associated with these mutually exclusive events, holding other things constant. This innocuous specification makes interpretations of the results more straightforward. The fixed ef-

fects specification, by controlling for time-invariant heterogeneity, allows us to obtain within-subject variations in tax compliance in response to changes in the experimental setting and outcomes (e.g., the prevailing tax, audit, or fine rate).

The definitions of variables used in the regression are listed in Table 2 and will be explained in detail later. The results in Tables 3–1 and 3–2 are obtained using a quadratic function of the round number to control for the unobserved effects over the whole of the sample periods (rounds 1–20). We rely on a change in the compliance rates surrounding the change in the tax, audit, or fine rate regime (whether or not voting is allowed) occurring in round 11 to identify the effects of democracy. Moreover, the quadratic form of the round number is to control for unobserved heterogeneity, which changes with the rounds of the experiment that a subject has played. These unobserved factors include learning by subjects and dynamics generated by interactions among subjects. We assume that the effects of these unobserved factors are a smooth function of the round number. The regression results are reported in Table 3–1.

To have a better understanding of compliant behavior, we also analyze the estimated fixed effects $\hat{\mu}_i$ by estimating a regression model as follows:

$$\hat{\mu}_i = \omega_i \gamma + e_i,$$

where ω_i is subject i's time invariant characteristics (e.g., gender, age, risk attitude, and previous donation behavior), which were collected after the experiments, γ is a vector of coefficients to be estimated, and e_i is a normally distributed error term.

In Table 3–1 the coefficients of "No-vote," "High rate," and "Low rate" denote the average compliance rates in the mutually exclusive events. The coefficient estimates show that the average compliance rates for the rounds when voting was not allowed ("No-vote" $= 1$), the tax, audit, or fine rate applied was high with voting allowed ("High rate" $= 1$), and the tax, audit, or fine rate applied was low with voting allowed ("Low rate" $= 1$) are all positive and significantly different from zero at conventional levels in all six treatments. Given that the tax, audit, and fine rates were always low in the rounds when voting was not allowed, to test the effect of democracy on compliance, we examine whether or not the compliance rate when voting was not allowed is equal to the compliance rate when the rate applied was low in the rounds with voting allowed. That is, we test the equality of the coefficients "No-vote" and "Low rate."

Table 2 Variable Definitions

	Definition
Compliance rate	Percentage of income reported (i.e., $100 \times \frac{\text{reported income}}{\text{true income}}$).
No-vote	Dummy variable indicating that the current round is a no-vote round.
Round	Round number.
Round^2	Round number squared.
True income	Income received by the subject.
Voted high	Dummy variable indicating that the subject voted for a high tax, fine, or audit rate in the round when voting was allowed.
High rate	Dummy variable indicating that a high tax, fine, or audit rate applied to a subject in the round when voting was allowed.
Low rate	Dummy variable indicating that a low tax, fine, or audit rate applied to a subject in the round when voting was allowed.
Voted high $\times$ High rate	Dummy variable indicating that the subject voted for a high tax, fine, or audit rate and that a high tax, fine, or audit rate was applied.
Voted low $\times$ High rate	Dummy variable indicating that the subject voted for a low tax, fine, or audit rate and that a high tax, fine, or audit rate was applied.
Voted high $\times$ Low rate	Dummy variable indicating that the subject voted for a high tax, fine, or audit rate and that a low tax, fine, or audit rate was applied.
Voted low $\times$ Low rate	Dummy variable indicating that the subject voted for a low tax, fine, or audit rate and that a low tax, fine, or audit rate was applied.

As suggested by the p-values reported in Table 3–1, it turns out that the null hypothesis of equality in compliance rates is accepted in all treatments except in the Audit-o2 and Fine-o1 treatments. However, in the Audit-o2 treatment, the average compliance rate was actually higher in the rounds with voting not allowed (86.58 percent vs. 72.64 percent). We also find higher compliance rates in the no-vote rounds for the Tax-o2, Audit-o1, and Fine-o2 treatments, even though the differences are statistically insignificant. For the Fine-o1 treatment, the compliance rate was higher when voting was allowed, as indicated by the p-value of the equality test of the "No-vote" and "Low rate" coefficient estimates. This is the only case indicating that democracy raises compliance. Notice that in the Fine-o1 treatment, voting was allowed in rounds 11–20 of the experiment, suggesting that a learning process may be needed for the existence of the democracy effect.

Table 3–1 Specification I: Fixed Effects Regression Results with Observations from All Rounds

Independent variables	Tax-o1	Tax-o2	Audit-o1	Audit-o2	Fine-o1	Fine-o2
No-vote	48.4900** (7.0403)	25.3432** (6.2502)	73.8207** (5.7704)	86.5757** (7.0381)	95.3144** (7.3447)	66.1644** (7.3358)
Round	−1.9049** (0.2795)	−1.5860** (0.2593)	−1.5829** (0.2334)	−2.4772** (0.2782)	−2.7390** (0.2987)	−1.4463** (0.3016)
Round^2	0.0739** (0.0271)	0.0258 (0.0250)	0.0725** (0.0226)	0.0779** (0.0269)	0.0237 (0.0288)	0.0844** (0.0291)
True income	−0.0726** (0.0365)	−0.0214 (0.0337)	−0.0578* (0.0307)	−0.0272 (0.0361)	−0.0124 (0.0389)	−0.0537 (0.0394)
Voted high	−3.4909 (2.8147)	−1.1126 (2.5574)	−1.8947 (2.5636)	3.8298 (3.2268)	−4.0614 (3.0671)	−1.0789 (3.1408)
High rate	61.0472** (7.4753)	26.8079** (6.6498)	104.0498** (6.2119)	105.2370** (7.2696)	126.6290** (7.8697)	80.1713** (7.8517)
Low rate	49.3858** (7.4847)	25.1043** (6.4776)	71.2627** (6.0567)	72.6438** (7.0026)	102.8341** (7.6517)	63.8291** (7.5605)
Within R^2	0.1431	0.1437	0.2656	0.2805	0.1920	0.1468
Observations	960	960	960	960	960	960
p-value of Wald test for equality of coefficients						
"Low rate" = "No vote"	0.8052	0.9431	0.3675	0.0000	0.0426	0.5310
"Low rate" = "High rate"	0.0000	0.4437	0.0000	0.0000	0.0000	0.0000

Note: Results from the estimation of the fixed effects model: compliance rate$_{it} = x_{it}\beta + \mu_i + \varepsilon_{it}$. Standard errors are in parentheses. The notation ** denotes the 5% significance level and * denotes the 10% significance level.

Table 3–2 Specification I: Explaining the Fixed Effects

Independent variables	Tax-o1	Tax-o2	Audit-o1	Audit-o2	Fine-o1	Fine-o2
Female	8.3942 (6.3310)	9.4742 (6.9558)	8.9371 (7.0516)	1.1266 (8.9839)	−2.5519 (6.5239)	6.9051 (7.7868)
Risk-taking	−3.6683** (1.3219)	−2.7024 (1.7376)	−5.1434** (1.3608)	−3.1788* (1.6272)	−1.6439 (1.3254)	−3.6339** (1.3823)
Age	1.4886 (2.2507)	−3.8828 (2.3144)	1.5966 (3.0489)	−6.5444** (2.8539)	−3.0448 (2.6943)	−6.0408 (3.9014)
Econ	−12.0047 (7.9900)	−7.0448 (8.2979)	−18.2287** (8.7328)	−6.9888 (7.2228)	−10.6680* (5.9731)	−0.3621 (8.9131)
Donation	4.1651 (3.3243)	−1.0367 (4.5235)	5.0151 (3.5367)	5.3237 (3.7964)	12.5602** (2.5280)	−0.3986 (3.1091)
Constant	−14.9685 (44.5650)	92.7054* (49.2715)	−7.4921 (59.3486)	144.1756** (58.2793)	53.7814 (54.4691)	134.4339* (77.6586)
R^2	0.2100	0.1701	0.3733	0.2226	0.3080	0.1763
Observations	48	48	48	48	48	48

Note: Results from the regression model $\hat{\mu}_i = \omega_i\gamma + e_i$. Standard errors are in parentheses. The notation ** denotes the 5% significance level and * denotes the 10% significance level.

One may be concerned that the coefficient estimates are confounded by the effect of social norms (see Alm et al., 1999), as the actual tax, audit, or fine rates in the voting stage may reveal the preferences of a subject's peers in the same group, and the subject may react to expectations about his or her peers' preferences. However, given that we have a randomization mechanism, where the computer decides the tax, audit, or fine rate, subjects are not able to infer their peers' voting behavior or preferences.

A comparison of the coefficients for "High rate" and "Low rate" in Table 3–1 suggests that when a high tax, audit, or fine rate prevailed, a subject complied more, as the coefficients for "High rate" are significantly larger than those for "Low rate" in all treatments except Tax-o2. It is reasonable for subjects to comply more when faced with a higher audit or fine rate, but it is somewhat counter-intuitive to see that the compliance rate is also higher when the tax rate is higher. Since some randomization mechanism has been added to the determination of the tax, audit, and fine rates, this effect is unlikely to arise from a peer effect. This is because the tax, audit or fine rate that would prevail is not totally determined by majority voting, such that a subject may not perceive the tax, audit, or fine rate as a signal of other subjects' preferences.

In the literature, the effects of tax rates on compliance are divergent. Alm et al. (1995) and Kamdar (1995) show that high tax rates lead to higher levels of compliance. The experimental evidence from the Tax-o1 treatment demonstrates a similar result. In contrast, Alm et al. (1992b), Clotfelter (1983), Collins and Plumlee (1991), Crane and Nourzad (1986), Dubin et al. (1990), Friedland et al. (1978), and Pommerehne and Weck-Hannemann (1996) have found a negative relationship between tax rates and compliance. The finding in the Tax-o2 treatment is similar to that in Beck et al. (1991), who found that for risk-neutral subjects, tax rates have no effects on compliance.

The effects of audit rates on compliance in the literature are more consistent. Alm et al. (1992b) find that a high audit probability leads to higher compliance, but the effect is small and insignificant. Spicer and Thomas (1982) find that when the audit probability is precise, a high audit probability leads to high compliance. Alm et al. (1990) find that combined with an increase in the fine rate, a higher audit probability raises compliance. Alm et al. (1992), Alm et al. (1995), Beck et al. (1991), Cullis et al. (2006), Crane and Nourzad (1986), Dubin et al. (1990), Dubin and Wilde (1988), Pommerehne and Weck-Hannemann (1996), and Witte and Woodbury (1985) all find similar results that compliance increases with audit probabilities. Gërxhani and Schram (2006) find a positive relationship between compliance and audit probabilities

for Dutch subjects, while for Albanian subjects, audit probabilities have no effects on compliance.

Lastly, the experimental literature also demonstrates consistent results regarding the impacts of fine rates on compliance. For instance, Alm et al. (1992b), Beck et al. (1991), and Crane and Nourzad (1986) find high fine rates lead to higher compliance. Alm et al. (1990) find that combined with an increase in audit probabilities, higher fine rates raise compliance. Alm et al. (1995) also find a similar result when audit probabilities are higher. By using the 1994 and 1996 tax return data in Taiwan, Chen (2006) also finds that penalties have a significant countering effect on enterprises' tax evasion behavior in the future.

It is also interesting to see that a subject's own voting decision did not affect his or her compliant behavior as suggested by the coefficient estimates for "Voted high." As shown by Table 3–1, the coefficient estimates for "Voted high" in all treatments are statistically insignificant. This implies that after controlling for subjects' unobserved heterogeneity on compliant behavior, their voting behavior related to the tax, audit, or fine rate does not affect their compliant behavior.

Moreover, subjects receiving higher income had lower compliance rates, as indicated by the negative coefficients of "True income" in Table 3–1. This effect is significant in Tax-o1 and Audit-o1 and insignificant in all other treatments with fixed effects estimations. The effects of income on compliance are diverse in the literature. For instance, Collins and Plumlee (1991), Crane and Nourzad (1986), and Kamdar (1995) have found compliance decreases with income, while Alm et al. (1992b), Alm et al. (1990), Witte and Woodbury (1985) found that higher income leads to higher compliance. Alm et al. (1993) also found a positive effect of income on compliance, but the effect is insignificant. By looking at the proportion of income underreported, Crane and Nourzad (1986) discovered that compliance increases with income, while Pommerehne and Weck-Hannemann (1996) found income has no effects on compliance.

The above results are summarized in Result 2.

Result 2 The effect of democracy appears in the treatment of Fine-o1. Subjects behaved more compliantly when a high audit rate or a high fine rate was applied to them than when a lower counterpart was applied. Subjects' voting decisions had no significant effects on compliance. Income has negative impacts on compliance, and these impacts are significant in the tax-o1 and audit-o1 treatments.

Table 3–2 reports the effects of subject characteristics on the fixed effects estimate, denoted by $\hat{\mu}_i$, which represents the subject's specific average compliance rate after controlling for experimental settings and outcomes. The results in Table 3–2 show that gender has no significant effects on tax compliance. Subjects who were more risk tolerant had lower compliance rates in all treatments, as suggested by the negative coefficient estimates of "Risk-taking." However, the coefficient estimates are not statistically significant for the Tax-o2 and Fine-o1 treatments. The age of the subjects does not have a consistent effect across treatments. Its coefficient estimates are positive in some treatments (Tax-o1 and Audit-o1) and negative in others, and statistically significant only in the Audit-o2 treatment.

Having taken at least one economics course (i.e., "econ" $= 1$) has a negative effect on tax compliance, but only the estimates for the Audit-o1 and Fine-o1 treatments are statistically significant at conventional levels. Except for the Tax-o2 and Fine-o2 treatments, the coefficient estimates for "Donation" are all positive, but only the estimate for the Fine-o1 treatment is statistically significant. This seems to indicate that more charitable individuals are also more tax compliant. We summarize these results in Result 3.

Result 3 Gender had no significant impacts on compliance. The attitude toward risks had a significant and negative impact on compliance, especially when it was the audit rate to be voted on. Age and having taken at least one economics course had significant and negative impacts on compliance in some treatments. The amount of money donated to charities had a significant and positive effect on compliance in only the Fine-o1 treatment.

It is informative to compare these fixed effects estimates with the OLS (ordinary least squares) estimates (i.e., unobserved heterogeneity not controlled for). In Table 4, the OLS results indicate that "Voted high" is statistically significant for the Audit-o1, Audit-o2, Fine-o1, and Fine-o2 treatments. This implies that there is time-invariant unobserved heterogeneity affecting subjects' compliant behavior and this unobserved heterogeneity also affects their preferences for the audit and fine rates.

One may be concerned that our fixed effects specification is not able to eliminate unobserved factors affecting both compliant behavior and voting behavior, i.e., there may be some round-specific shocks affecting both kinds of behavior. Even though the coefficient estimate of "Voted high" is statistically insignificant in Table 3–1, the effect of voting behavior may be non-linear. To examine such a possibility we run estimate

Table 4 Specification I: OLS Results with Observations from All Rounds

Independent variables	Tax-o1	Tax-o2	Audit-o1	Audit-o2	Fine-o1	Fine-o2
No-vote	46.2170**	48.6928**	56.6799**	46.7052**	54.8843**	51.9108**
	(8.2417)	(8.1522)	(6.8585)	(8.5355)	(5.9348)	(7.5367)
Round	46.2170**	−1.6683**	−2.7895**	−1.4870**	−1.5493**	−2.5254**
	(8.2417)	(0.3295)	(0.3518)	(0.3431)	(0.3046)	(0.3688)
Round^2	0.0723**	0.0258	0.0198	0.0884**	0.0776**	0.0819**
	(0.0322)	(0.0250)	(0.0355)	(0.0294)	(0.0283)	(0.0269)
True income	−0.0911	−0.0214	−0.0150	−0.0094	−0.0327	−0.0103
	(0.0649)	(0.0337)	(0.0547)	(0.0640)	(0.0493)	(0.0448)
Voted high	−1.3401	6.3796	9.8700*	11.6060*	20.0481**	14.1401**
	(5.6237)	(4.6751)	(5.6965)	(6.0590)	(5.8897)	(6.0096)
High rate	56.7847**	46.2458**	83.8310**	56.4558**	76.5302**	68.0295**
	(9.1419)	(8.9092)	(6.7666)	(9.1109)	(6.8849)	(7.2324)
Low rate	46.9842**	44.3851**	57.9843**	39.1290**	45.7452**	35.1752**
	(8.1786)	(9.0150)	(6.8215)	(8.8622)	(6.3596)	(7.3079)
R^2	0.7113	0.7613	0.8061	0.6691	0.7397	0.6673
Observations	960	960	960	960	960	960
p-value of Wald test for equality of coefficients						
"Low rate" = "No-vote"	0.8584	0.3329	0.7181	0.0984	0.0079	0.0002
"Low rate" = "High rate"	0.0118	0.6167	0.0000	0.0002	0.0000	0.0000

Note: Refer to Table 3–1.

(3) again with a richer set of explanatory variables, which is denoted as Specification II and consists of interactions of "Voted high" and "Voted low" with "High rate" and "Low rate" (denoted as "Voted high × High rate," "Voted high × Low rate," "Voted low × High rate" and "Voted low × Low rate," respectively). Tables 5–1 and 5–2 report the results.

The results in Table 5–1 suggest that, across all treatments, there are not many differences in a subject's compliance rate when he or she voted for a high or a low rate for tax, audit or fine, given that a low rate applied. The only exception is the difference for treatment Audit-o2, for which the compliance rate is higher by 7.70 percentage points (p-value = 0.04) when a subject voted for a high audit rate than when a subject voted for a low audit rate. The rest of the differences are statistically insignificant as indicated by the Wald test's p-values.

When a high rate applied, a subject's compliance rate was lower when he or she voted for a high rate than when he or she voted for a low rate in all six treatments, and the differences are statistically significant in the Tax-o1, Audit-o1, and Fine-o1 treatments. We have previously found from Table 3–1 that on average subjects behave

Table 5–1 Specification II: Fixed Effects Regression Results with Observations from All Rounds

Independent variables	Tax-o1	Tax-o2	Audit-o1	Audit-o2	Fine-o1	Fine-o2
No-vote	48.7943**	25.2179**	74.4603**	87.0896**	78.6984**	66.1462**
	(7.0349)	(6.2625)	(5.7591)	(7.0307)	(7.4092)	(7.3389)
Round	−1.9091**	−1.5862**	−1.5677**	−2.4774**	−2.7511**	−1.4411**
	(0.2792)	(0.2594)	(0.2328)	(0.2778)	(0.2987)	(0.3019)
Round^2	0.0730**	0.0257	0.0743**	0.0780**	0.0227	0.0840**
	(0.0270)	(0.0250)	(0.0225)	(0.0269)	(0.0288)	(0.0291)
True income	−0.0714*	−0.0210	−0.0581*	−0.0304	−0.0108	−0.0544
	(0.0365)	(0.0338)	(0.0306)	(0.0360)	(0.0389)	(0.0395)
Voted high × High rate	56.2613**	25.2134**	98.5984**	104.4199**	104.3767**	78.2969**
	(7.1228)	(6.4092)	(6.0166)	(8.0389)	(8.0205)	(7.5658)
Voted low × High rate	64.1375**	27.1639**	107.5370**	108.6910**	112.4620**	81.1167**
	(7.6806)	(6.7233)	(6.3472)	(7.4548)	(7.6626)	(8.0693)
Voted high × Low rate	48.5925**	24.4185**	72.9005**	80.1643**	84.4265**	63.5668**
	(7.4526)	(6.4255)	(5.9725)	(7.8192)	(8.1158)	(7.5077)
Voted low × Low rate	48.2400**	24.7237**	70.6964**	72.4612**	85.3423**	63.5590**
	(7.5063)	(6.5631)	(6.0432)	(6.9912)	(7.6126)	(7.5820)
Within R^2	0.1458	0.1438	0.2707	0.2838	0.1937	0.1470
Observations	960	960	960	960	960	960
p-value of Wald test for equality of coefficients						
"Voted high × High rate" = "Voted low × High rate"	0.0383	0.5696	0.0187	0.4058	0.0563	0.5427
"Voted high × Low rate" = "Voted low × Low rate"	0.9218	0.9279	0.4675	0.0400	0.8106	0.9984
"Voted low × Low rate" = "No-vote"	0.8817	0.8852	0.1899	0.0000	0.0773	0.4915
"Voted high × Low rate" = "No-vote"	0.9602	0.8343	0.6525	0.1165	0.1988	0.5644

Note: Refer to Table 3–1.

more compliantly when a high rate applied than when its lower counterpart applied, while the results of Table 5–1 suggest that subjects voting for high rates are not more compliant than subjects voting for low rates given when high rates are applied.

We next focus on the democracy effect, i.e., the comparisons of tax compliance in the no-vote rounds versus the rounds when voting was allowed and a low rate applied that was conditional on subjects' voting decisions. As reported in Table 5–1, for subjects voting for a low tax, audit or fine rate, this difference is statistically significant for the Audit-o2 treatment (p-value $= 0.00$) and marginally significant for the Fine-o1 treatment (p-value $= 0.08$). However, in Audit-o2, the average compliance

Table 5–2 Specification II: Explaining the Fixed Effects

Independent variables	Tax-o1	Tax-o2	Audit-o1	Audit-o2	Fine-o1	Fine-o2
Female	8.3639 (6.3056)	9.4822 (6.9573)	9.0035 (7.0568)	1.0146 (9.0424)	−2.5492 (6.5287)	6.8734 (7.7846)
Risk-taking	−3.6490** (1.3097)	−2.6982 (1.7391)	−5.0947** (1.3646)	−3.1759* (1.6326)	−1.6045 (1.3287)	−3.6317** (1.3824)
Age	1.4779 (2.2420)	−3.8845 (2.3125)	1.5865 (3.0427)	−6.4532** (2.8647)	−3.0534 (2.6938)	−6.0367 (3.9025)
Econ	−12.0665 (7.9450)	−7.0663 (8.3026)	−18.4542** (8.7210)	−6.9603 (7.2656)	−10.7037* (5.9716)	−0.3103 (8.9036)
Donation	4.1571 (3.3175)	−1.0278 (4.5263)	5.0191 (3.5286)	5.2785 (3.8099)	12.5102** (2.5313)	−0.4064 (3.1086)
Constant	−14.7684 (44.3925)	92.7122* (49.2523)	−7.4335 (59.3146)	142.4662** (58.4443)	53.8909 (54.4632)	134.3394* (77.6668)
R^2	0.2105	0.1701	0.3719	0.2181	0.3057	0.1761
Observations	48	48	48	48	48	48

Note: Refer to Table 3–2.

rate is actually lower in rounds with voting than when voting is not allowed. Hence, the democracy effect exists only in the Fine-o1 treatment, and this result is consistent with that of Table 3–1. For subjects voting for a high tax, audit or fine rate, there are no significant differences in the compliance rate during the no-vote rounds and the low-rate rounds in the voting stage as indicated by the p-values of the Wald test.

We summarize these results in Result 4 below.

Result 4 Decomposing subjects in the Fine-o1 treatment, the democracy effect exists specifically in the group of subjects voting for the low fine rate. In the group of subjects voting for the high fine rate, the democracy effect is insignificant. Subjects voting for a high tax, audit, or fine rate are not necessarily more compliant than subjects voting for a lower counterpart, especially when a high rate is applied.

5. CONCLUSION

This paper experimentally investigates individuals' compliance behavior when they are allowed to vote for the tax, audit, or fine rate. The democracy effect is also examined by this paper. Since individuals may behave more compliantly if the outcome of voting happens to be consistent with their inherent preferences, we apply Dal Bó et al. (2010) approach by adding a randomization stage of computer decisions after voting to control

for this selection problem. In addition, we also control for the order effect by switching the order of the rounds with voting and the rounds without voting.

The main findings of our paper are the following. First, subjects generally preferred a less severe auditing environment. On average more than half of the subjects voted for the low tax rate and the low fine rate, and even more than seventy percent of the subjects voted for the low audit rate. Second, income generally had negative impacts on compliance, while gender generally had no significant impact on compliance. The attitude toward risks had a significant and negative impact on compliance, especially when the audit rate was to be voted on. Third, subjects for whom the high audit rate or high fine rate was applied behaved significantly more compliantly than subjects for whom the lower counterpart was applied. However, subjects voting for a high tax, audit, or fine rate were not necessarily more compliant than subjects voting for a corresponding low rate, especially when a high rate was applied. Fourth, democracy has a positive effect on compliance in the Fine-o1 treatment, especially in the group of subjects voting for the low fine rate.

The results from our experiment bear some policy implications. First, whether democratic participation can improve compliance or not depends on which fiscal variables are to be voted on. Our experimental evidence suggests that allowing voting on the fine rate can improve compliance, while voting on the tax rate and audit rate does not. Second, because more than half of the subjects voted for a low tax rate and a low fine rate, and more than seventy percent of the subjects voted for a low audit rate, democratic participation may deteriorate compliance and government revenues. To improve compliance, enforcing directly a high audit rate and a high fine rate may be more effective.

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民主參與對租稅順從效果的實驗研究 †

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† 相關文章補充資料請參閱「線上附錄」（http://www.econ.sinica.edu.tw/Appendix/483-A.pdf）。

摘　要

我們以經濟學實驗方法探討實驗參與者在可以對稅率、稽核率、或懲罰率表決的環境中,他們的租稅順從行為與民主效果。我們藉由在實驗中加入由 Dal Bó et al.(2010)所提出的電腦隨機決策階段來控制選擇效果。我們的實驗證據顯示當投票的變數是懲罰率時,民主效果存在。我們也發現當實驗參與者面對高稽核率或高懲罰率時,會比面對低稽核率或低懲罰率來得較順從。不過投高稅率、稽核率、或懲罰率的人未必比投低稅率、稽核率、或懲罰率的人來得順從,尤其當最後適用的是較高者。