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AUDIT INFORMATION DISSEMINATION, TAXPAYER COMMUNICATION, AND COMPLIANCE: AN EXPERIMENTAL APPROACH

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ABSTRACT

Taxpayer audits are a central feature of the voluntary compliance system in the United States federal individual income tax. Audits are thought to have a *direct* deterrent effect on the individuals actually audited. In addition, audits are believed to have an *indirect* deterrent effect on individuals not audited, and there is some empirical evidence that suggests that changes in audit rates affect compliance beyond the audited individuals themselves. However, empirical studies cannot measure or control for taxpayer awareness of audit risk. As a result, there is no evidence on the magnitude of the effects of audit risk awareness on taxpayer compliance; that is, the effects on compliance of the ways in which taxpayers learn about – and communicate among themselves – audit rates are not known, and cannot be addressed or discovered by empirical studies. In this study, we examine three types of communication about audit frequency and audit results using laboratory market experiments in which the audit setting and communication opportunities are controlled. In all experimental treatments, subjects are informed of the objective probability that their return will be audited and the success rate of the audit process. In the base case sessions, the subjects receive no information about audit results beyond their own audit experience. In a second treatment the same objective audit rates are in effect, and subjects are also told by the experimenter the actual number of audits conducted during a period. In the third treatment the subjects are offered the opportunity to send a "message" to the other participants about their audit experience; subjects may also choose to send no message; and subjects may choose to send a message that is truthful or not. The data allow us to test hypotheses about the effects of two types of communication of audit results, in order to explore the direct and the indirect effects of audits: "official" communications from the "government" (e.g., the experimenter) and "unofficial", or informal, communications among "taxpayers" (e.g., the subjects). Our results indicate that "unofficial" communications have a strong indirect effect that increases compliance, but that "official" communications may not encourage voluntary compliance.

1. INTRODUCTION

Taxpayer audits are a central feature of the voluntary compliance system in the United States federal individual income tax. Audits are thought to have a *direct* deterrent effect on the individuals actually audited. In addition, audits are believed to have an *indirect* deterrent effect on individuals not audited, and there is some empirical evidence that suggests that changes in audit rates affect compliance beyond the audited individuals themselves. For example, in an econometric study using U.S. state-level reporting data for the years 1977 to 1986, Dubin, Graetz and Wilde (1990) find that, for every dollar of revenue produced because of taxpayer audits, an additional six dollars of revenue were generated from the indirect or "ripple" effects. Tauchen, Witte, and Beron (1989) use taxpayer audit data from the 1969 Taxpayer Compliance Measurement Program (TCMP), and find that raising the audit rate had overall a smaller impact, and one mainly felt on high-income wage and salary workers; for this group of taxpayers, they estimate an indirect effect of audits that is almost three times the direct revenue effect.

Given the importance of audits in the voluntary compliance system of the U.S., it is significant that taxpayer audit rates have fallen dramatically since the 1960s, and have continued their decline in recent years. In the early 1960s the percentage of individual tax returns that were audited by the Internal Revenue Service (IRS) was about 6 percent, and this percentage fell to 2.5 percent by the mid-1970s. Over the next decade, the audit rate fell further to roughly 1 percent. According to the Inspector General for Tax Analysis report in 2002, taxpayer audit rates have fallen another 56 percent between 1997 and 2001. As a result, at present well less than 1 percent of all individual tax returns are audited. Seen in the context of the Dubin, Graetz, and Wilde (1990) and Tauchen, Witte, and Beron (1989) studies, the effect of declining audit rates is not confined to the direct effect due to fewer audited taxpayers. Rather, there is an indirect effect that extends to taxpayers in general, who respond to the reduced overall probability of audit by lowering their compliance.

On balance, it seems likely that the decline in audit rates since the 1960s has affected voluntary compliance. For example, it is estimated that government coffers have been shortchanged by $7.2 billion of "real money" as a direct result of lower audit frequency.[1] As significant as the dollar amount lost directly because of lower audit rates is, it may pale in comparison to the dollars lost indirectly through taxpayer responses as they become aware of lower audit risk; that is, if the *indirect* effect of audits is largely than the *direct* effect, as some empirical evidence suggests, then the revenue cost of reduced audit rates is significantly greater than $7.2 billion.

However, the magnitude of these impacts is still largely speculative. Despite the insights from empirical studies on the direct versus the indirect effects of audit rates on compliance, these studies cannot measure or control for taxpayer awareness of audit risk. As a result, there is no evidence on the magnitude of the effects on voluntary compliance of audit risk awareness or of changes in audit rates. In particular, there is no evidence on the impact on compliance – if any – of the ways in which audit information is disseminated among taxpayers or communicated by taxpayers. As a result, there is no evidence on the magnitude of the effects of audit risk awareness on taxpayer compliance; that is, the effects on compliance of the ways in which taxpayers learn about – and communicate among themselves – audit rates are not known, and cannot be addressed or discovered by empirical studies.

Indeed, the ways in which audits deter taxpayers from evading, whether from their direct or indirect effects, is not well understood. According to Plumley (1996), "[i]t is generally believed … that many taxpayers would perceive increased auditing by IRS as an increase in their

[1] See the U.S. Department of the Treasury Inspector General for Tax Administration (TIGTA) (2002).

chances of being audited, and that they would improve their voluntary compliance as a result." From this description, it is clear that audit-based deterrence depends on taxpayer awareness of the level and year-to-year change in examination rates as a necessary, though not a sufficient, condition. Therefore, a valid test for the existence of indirect effects must ensure taxpayers are aware of the likelihood of audit. However, it is unlikely that such awareness can be gleaned from data based on random taxpayer audits. A greater degree of control is possible in field studies, but such data also may contain a broad array of exogenous influences, such as changes in tax law or economic conditions that may cause taxpayers to change their behavior during the period of study. Indeed, some recent research (Alm and McKee forthcoming) suggests that the presence of random audits is necessary if the systematic audits are to be effective; that is, random and systematic audits are complementary beyond the direct use of random audits to verify the efficacy of the systematic selection rules.

The purpose of this study is to examine the roles of information dissemination and taxpayer communication on voluntary compliance. In particular, we examine three types of communication about audit frequency and audit results using laboratory market experiments in which the audit setting and communication opportunities are controlled. In all experiment treatments subjects are informed of the objective probability that their return will be audited and the success rate of the audit process. In the base case sessions, the subjects receive no further information about audit results beyond their own audit experience. In a second treatment the same objective audit rates are in effect, and subjects are also told by the experimenter the actual number of audits conducted during a period. In the third treatment the subjects are offered the opportunity to send a "message" to the other participants about their audit experience; subjects may also choose to send no message; and subjects may choose to send a message that is truthful or not. The data therefore allow us to test hypotheses about the effects of two types of communication of audit results, in order to explore the direct and the indirect effects of audits: "official" communications from the "government" (e.g., the experimenter) and "unofficial", or informal, communications among "taxpayers" (e.g., the subjects).

Our results indicate that "unofficial" communications have a strong indirect effect that increases compliance. However, "official" communications may not encourage voluntary compliance.

The next section gives a brief overview of the relevant theory of taxpayer compliance. Section 3 discusses our experimental design, and section 4 presents our experimental results. In the final section we discuss our conclusions.

2. THEORY

The economic model of income tax evasion (Allingham and Sandmo 1972) is based on the economics-of-crime approach pioneered by Becker (1968). This model focuses on the income reporting behavior of taxpayers, and ignores other forms of evasion such as non-payment, excessive reporting of deductions, and non-filing.[2]

In its simplest form, an individual is assumed to receive a fixed amount of income I, and must choose how much of this income to declare to the tax authorities and how much to underreport. The individual pays taxes at rate t on every dollar D of income that is declared, while no taxes are paid on underreported income. However, the individual may be audited with a fixed, random probability p; if audited, then all underreported income is discovered, and the individual must pay a penalty at rate f on each dollar that he or she was supposed to pay in taxes

[2] Cummings, Martinez-Vazquez, and McKee (2001b) have investigated the effect of alternative forms of evasion, and find that individuals respond to relative enforcement by choosing the evasion mode with the lower expected penalty.

but did not pay. The individual's income I_C if caught underreporting equals $I_C=I\text{-}tD\text{-}f[t(I\text{-}D)]$, while if underreporting is not caught income I_N is $I_N=I\text{-}tD$. The individual chooses declared income to maximize the expected utility $\tilde{o}\ U(I)$ of the evasion gamble, or $\tilde{o}\ U(I)=pU(I_C)+(1\text{-}p)U(I_N)$, where $\tilde{o}$ is the expectation operator and utility $U(I)$ is a function only of income. This optimization generates a standard first-order condition for an interior solution; given concavity of the utility function, the second-order condition will be satisfied.[3]

Comparative statics results are easily derived. It is straightforward to show that an increase in the probability of detection p and the penalty rate f unambiguously increase declared income.[4] An increase in income has an ambiguous effect on declared income, an effect that depends upon the individual's attitude toward risk.[5] Surprisingly, an increase in the tax rate t has an ambiguous effect on declared income. A higher tax rate increases the return to cheating, which reduces the amount of declared income. However, a higher tax rate also reduces income; if, as is usually assumed, the individual exhibits decreasing absolute risk aversion, then the lower income makes the evasion gamble less attractive and declared income increases accordingly. In fact, it is straightforward to show that a higher tax rate will increase declared income when the penalty is imposed at a proportional rate on evaded taxes.

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[3] The first- and second-order conditions are, respectively (where each prime denotes a derivative),

$$M\ \tilde{o}\ U(I)/MD = pt(f\text{-}1)U'(I_C) - (1\text{-}p)tU'(I_N) = 0$$

$$M^2\ \tilde{o}\ U(I)/MD^2 = p[t(f\text{-}1)]^2\ U''(I_C) + (1\text{-}p)t^2\ U''(I_N) < 0.$$

[4] For example, total differentiation of the first-order condition demonstrates that the impact of a change in the probability of audit on declared income is given by

$$MD/Mp = -[t(f\text{-}1)U'(I_C) + tU'(I_N)]/[pt^2(f\text{-}1)^2U''(I_C) + (1\text{-}p)t^2U''(I_N)].$$

Given the second-order conditions (and the obvious requirement that $f>1$), the sign of this expression is unambiguously positive. Other comparative statics results are similarly derived.

[5] There are two standard measures of risk aversion that are considered in expected utility theory. One is absolute risk aversion $A(I)$, equal to $-U''(I)/U'(I)$. The second is relative risk aversion $R(I)/\text{-}IU''(I)/U'(I)$. It is typically assumed that $A(I)$ decreases with income, while $R(I)$ increases with income.

The standard model has been modified in a number of ways.[6] A variation that illustrates quite simply the fiscal incentives for compliance is to assume that the individual is risk neutral. As shown by Alm, Jackson, and McKee (1992a) and Alm, McClelland, and Schulze (1992), a risk-neutral individual will determine the amount of income to declare to tax authorities (D) based on the following expected value (EV) relationship $EV = I - td - pf\,[\,t\,(I - D)\,]$. Maximizing EV with respect to D indicates that an individual will optimally report all income when $pf > 1$, and will report zero income if the inequality is reversed. Using this inequality, we can follow Alm, McClelland, and Schulze (1992) to determine the combination of audit rates and fine rates that will induce a risk neutral individual to report all income. For example, when f equals 2, then the audit rate must exceed 50 percent to induce taxpayers to report all of their income; if the fine rate equals 5, then the audit rate must exceed 20 percent. Similarly, if the audit rate equals 1 percent (as it does in the U.S.), then any fine rate less than 100 will lead a risk-neutral individual to report zero income.[7] The incorporation of risk-averse behavior, especially at low audit probabilities (Bernasconi 1998), will affect these calculations.

However, this analysis assumes that taxpayers know the audit rate. What is unavoidably and necessarily missing from the empirical work of Tauchen, Witte, and Beron (1989) and Dubin, Graetz, and Wilde (1990) is a model of the manner by which information concerning the true audit rate is communicated among and understood by the taxpayers. The IRS does not announce that it will be raising or lowering the audit rate. As emphasized by Plumley (1996), an open empirical question is how a taxpayer forms an assessment of the probability of audit and then responds to changes in this audit rate. Put differently, we do not know how information is disseminated and communicated; that is, how do taxpayers learn that the audit rate is declining

[6] See Alm (1999) and Andreoni, Erard, and Feinstein (1998) for reviews of extensions to the standard evasion model.
[7] An implied assumption is that auditors are 100 percent successful at finding unreported income.

and adjust their behavior to generate the reported result? We address this learning phenomenon in our experimental design, as discussed in the next section.

3. EXPERIMENTAL DESIGN

The experimental design captures the essential features of the voluntary income reporting and tax assessment system used in many countries.[8] Human subjects in a controlled laboratory environment earn income through performance in a task, where the actual income earned is determined by the (relative) performance in the task. The subjects must decide how much of this income to report to a tax agency. Taxes are paid on reported income, and no taxes are paid on unreported income. However, unreported income may be discovered via an audit with some probability, and the subject must then pay a fine on the unpaid taxes. This reporting, audit, and penalty process is repeated for a given number of rounds that each represent a tax period, and is replicated with different sets of subjects. At the completion of the experiment, each subject is paid an amount that depends upon his or her performance during the experiment.

Since these are experiments designed to inform policy makers (Roth 1989), they must satisfy Smith's (1982) precept of *parallelism*. Parallelism is satisfied when the experimental setting captures the essential elements of the decision problem faced in the naturally occurring setting. It is not necessary (nor is it desirable) that the experiment setting implement all of the complexity of the naturally occurring setting. Here, subjects earn income, disclose income, and face an audit process similar to that in the naturally occurring setting. While the stakes are small, the decision setting is also simplified relative to that of the natural setting.

[8] The full set of experimental instructions is available upon request.

Thus, our basic experimental design follows the essential elements of Alm, Jackson, and McKee (1992a, 1992b, 1993) and Alm, McClelland, and Schulze (1992), but incorporates a number of additional features to improve parallelism with taxpayers' decision making in the naturally occurring world. For example, test subjects earn income instead of receiving an endowment, and these experiments utilize tax language in the instruction and the computer interface.

Subjects are recruited from undergraduate classes in economics and business.[9] Upon arrival at the lab, the subjects are organized into groups of eight persons with multiple groups in each session. Basic instructions are provided via hardcopy while the main instructions are provided via computer screens. Subjects are not allowed to communicate with one another during the session except via the computer interface in one of the treatments. They are told that the experiment will last an unknown number of periods; in actual practice the number of sessions was predetermined, and the sessions lasted for 30 real rounds. Three practice rounds are given, and procedural questions are answered. The full experiment then begins. Each session lasts 90 minutes, and subject earnings ranged from $19 to $37, depending upon his or her performance during the experiment. Subjects are told that payments will be made in private at the end of the session, that all responses are anonymous, and that the only record of participation that contains their name is the receipt signed when they receive their payments.

The earnings task requires the subjects to sort the digits 1 through 9 into the correct order from a randomized order presented in a 3 by 3 matrix. They do this by pointing the computer

[9] Recruiting was conducted through announcements in various classes and a sign up via a web page in which the subjects posted their contact information and the time blocks of their availability. Subjects were permitted to participate in only one tax experiment, although other experimental projects were ongoing at the time and many participated in other types of experiments. We actively discourage "snowball" sampling in which recruited subjects bring additional subjects to a session. When we recruit subjects, we do not reveal the exact nature of the experiment. All experiments were conducted at the University of Tennessee at Knoxville.

mouse at the numbers in the correct sequence and "clicking" on the numbers. Actual income is determined by the relative speed of performance, with the fastest performer receiving the highest income and the slowest performer receiving the lowest income. Once the income task is completed, the subjects are informed via the computer of their income for the round and presented with a screen that resembles a tax form in which they report their income. This screen informs the subjects of the current tax rate, the current probability of an audit, and the penalty rate applied to non-disclosed income. The language in the experiment uses tax lexicon, unlike some other experimental work (e.g., Alm, Jackson, and McKee 1992a, 1992b, 1993) where the experiments used neutral language.

In keeping with the central objective of this investigation, certain parameters (e.g., the tax rate and the penalty rate) are fixed throughout the experiments so that we may focus on the effect of information concerning audit results. All audits investigate only the current period disclosure.

The experimental design implements three treatments, as shown in Table 1. There are four different audit rates employed (0.05, 0.10, 0.30 and 0.40), and these are applied in each of the information treatments. The tax rate is set at 0.35 throughout the experiments, and the fine rate is set at 150 percent. There is no public good in these experiments.

The currency used in the experiment is called "lab dollars", and subjects are told that all lab dollars they earn during the experiment will be redeemed for cash at the end of the experiment at a fixed conversion rate of 100 lab dollars per 1 U.S. dollar.

There are several ways in which information regarding the audit activity of the IRS can reach the taxpayers and, potentially, affect their compliance behavior. We investigate two different information transmission mechanisms. In the first, the subjects are told the number of audits that actually occurred in the previous period but are not told the results of the audits. We refer to this as "official" information. In the second information treatment the subjects are given the opportunity in each round to send one message to the other persons in their group. Only one message may be sent by each person in each round; the possible messages are reported in Table 2. We refer to this as "unofficial" information. The experimental setting does not impose the requirement that the information transmitted be truthful. Before the next round begins the subjects receive a screen that reports the messages sent by the others in their group. The information is presented in a table showing the frequency of each message. Since the actual number of audits is not reported, there is no means by which the subjects can verify whether this information is truthful.

At the end of the experiment, we also ask the subjects several questions. One question is whether they prepare and file their own taxes. If they respond "No", we assume that their parents are responsible for this, given that our subjects are typically sophomores or juniors.

The process of determining who is audited is given by a computerized draw of numbered balls from a bucket on the subject's computer screen. This approach is similar to that used in some previous evasion studies (Sour 2001; Cummings, Martinez-Vazquez, and McKee 2001a, 2001b), but differs from Alm, Jackson, and McKee (1992a, 1992b, 1993) and from Alm, McClelland, and Schulze (1992) where a mechanical bingo cage was used.

Several hypotheses concerning compliance behavior can be formulated based on the experimental structure and design. Rational individuals will increase compliance when audit probability increases. Thus Hypothesis 1 is:

H1: Compliance will be higher with higher audit rates.

As for prior (or lagged audits), an individual audited in one round may in the immediately following round either increase or decrease compliance. However, two motives would suggest lower subsequent compliance. The first is the “gambler’s fallacy”, or the notion that “If I was audited in the last round, then there is less chance I will be audited this round”. The second is the prospect of “catching up”: since an audit may have resulted in a loss of income, an individual may attempt to redress this through subsequent evasion. Thus:

H2: Being audited in the previous period will reduce compliance.

Higher levels of income imply higher absolute tax burdens (since the tax rate is the same for all income levels in the experiment). Thus, individuals with higher incomes will earn higher payoffs from evasion, suggesting Hypothesis 2:

H3: Compliance will be lower for taxpayers with higher incomes.

It should be noted that the simple model of tax evasion predicts that the impact of income on declared income is uncertain, and depends upon taxpayer attitudes toward risk. This ambiguity is also reflected in the impact of accumulated earnings on compliance, in Hypothesis 3:

H4: The impact of wealth on compliance is uncertain.

Although the announced audit probability is predicted to influence behavior directly, individuals will use subsequent information to refine subjective estimates of audit probabilities.

If the tax authority announces the number of audits undertaken in the previous period, individuals are expected to update their prior beliefs, and thereby change behavior. If the updating yields an expectation that the actual audit probability is lower, then individuals will reduce their compliance rate relative to what they would have done knowing only the announced audit probability; conversely, individuals will increase their compliance rate if the subsequent information leads to a higher subjective probability of audit. We expect that the announcement effect will be to increase compliance, and thus the coefficient on *Official* sources of information will be positive:

> *H5: The official announcement of the number of audits in the previous period will, ceteris paribus, increase compliance*

Similarly, the effect of *Unofficial* communication on compliance seems likely to be positive:

> *H6: Unofficial communication between taxpayers will, ceteris paribus, increase compliance.*

The next section presents our experimental results and our tests of these hypotheses.

4. EXPERIMENTAL RESULTS

We utilize the data generated by the experiments in a variety of ways. We stress at this point that the analysis is preliminary – much work remains to be done with these data. Since the data collected in the experiment report the behavior of a fixed set of subjects over a number of periods, we have a panel dataset, and we report the results of estimations of these panels.

It is useful to look first at the data in a qualitative manner. In Figure 1, we present for each treatment and for each audit probability the average level of compliance *Comprate* (defined as *Declared* income divided by true *Income*). These aggregate data reveal some interesting behavior. At very low audit rates, the compliance rate when we allow communication between subjects (T3) is lower than in the other treatments (significant at the 0.05 level). However, at the intermediate audit rates, the compliance rate in T3 is highest. When the actual number of audits is reported by the computer (our "official" information), the compliance rate is lowest overall (T2).

The experimental design suggests that the amount of income declared by a taxpayer in each round is of the general form:

(1) $$Declared \text{ (or } Comprate) = ß_0 + ß_1\, Income + ß_2\, Wealth + ß_3\, Praudit + ß_4\, Lagaudit + ß_5\, XXX$$

where *Declared* is the income declared for taxes and *Comprate* is the compliance rate (calculated as *Declared/Income*), *Income* is actual income, *Wealth* is accumulated earnings, *Praudit* is the audit rate, and *Lagaudit* is a dummy variable equal to 1 if the individual was audited in the previous period and 0 otherwise. Previous empirical studies with *Declared* as the dependent variable generally find positive signs on the variables *Income* and *Praudit*. The variable denoted *XXX* represents information treatments that are investigated to determine their effects on taxpayer compliance. Since either of the dependent variables is censored, we estimate equation (1) using Tobit maximum likelihood methods. However, in order to exploit the panel structure of the dataset, we also estimate a fixed effects model similar to that employed in Alm and McKee (forthcoming). These panel estimations allow us to investigate the effects of information regarding audit activity while controlling for other factors. Summary statistics for the variables used are reported in Table 3.

In Table 4 we report the results of estimates using the Tobit maximum likelihood estimation. When the dependent variable is *Declared*, individuals report higher taxable income when their income is higher, when their wealth is lower, when the probability of audit is higher, and when they do not prepare their own taxes; the results using *Comprate* are generally the same, except that *Income* now has a negative impact on the compliance rate. It is especially noteworthy that *Official* information reduces both *Declared* and *Comprate*, while *Unofficial* communication between the subjects between rounds tends to increase reported income.

In Table 5 we report the results from a set of panel estimations. All specifications employ subject fixed effects. In all cases, a panel specific AR1 process is implemented, and panels are homoskedastic. Since we utilize individual audit results from the previous round (*Lagaudit*), we drop the first round observations from the data.

The panel estimation results are quite similar to the Tobit results. In particular, individuals declare higher taxable income when *Income* is higher, *Wealth* is lower, the probability of audit (*Praudit*) is higher, and when they do not prepare their own taxes (*Preptax*); when *Comprate* is the dependent variable, the main difference is that *Income* has a negative impact on the compliance rate. Again, *Official* information reduces *Declared* and *Comprate*, and *Unofficial* communication between the subjects increases these variables.

Overall, then, the basic behavioral hypotheses H1 and H2 are not rejected by our data. In all specifications, the coefficient on audit probability is positive and statistically significant, and the coefficient on the lagged audit rate is negative and generally significant. As for H3 and H4, the coefficient on *Income* is positive and significant, while the coefficient on *Wealth* is negative and significant when *Declared* is the dependent variable; when *Comprate* is the dependent variable, both *Income* and *Wealth* have negative effects on compliance.

As for the information treatments, it appears that providing *Official* information reduces compliance (both the compliance rate and the level of declared income), counter to H5. However, H6 receives some support, since *Unofficial* information leads to higher levels of both dependent variables. Perhaps the official information is suspect in this setting.

To further explore the effects of information, we interact the number of audits with the official and unofficial provision of information. As shown in Table 5. Providing past audit numbers officially (*NauditXOfficial*) does not have a significant effect on compliance. If this information is unofficial (*NauditXUnofficial*), the effects on *Declared* and *Comprate* are positive and significant. Of course, in the "unofficial" information setting, there is no prohibition against sending false information. Thus, the subjects may not be informed of the true number of audits that occurred in the previous round. Analysis of the nature of the revealed information and correction for false information are part of the future planned research with these data.

We stress that these results are extremely preliminary at this point. We have considerable work to complete our investigations of these data. Some discussion of the directions of this work is presented in the next section.

5. CONCLUSIONS

At this stage, our results must be regarded as preliminary. While our sample sizes and replications are adequate, the depth of the data analysis is limited. Several elements of the data have not yet been explored. For example, the "unofficial" information treatment provides for a variety of messages. We have not incorporated the inherent richness of these data into the analysis. Further, the subjects are free to send false information, and we need to analyze the actual information sent and the effect on subsequent compliance.

With the above caveats in mind, we believe that our current results are interesting and provocative. Of perhaps most interest is the finding that the official provision of previous audit information by the tax authority has a negative effect on subsequent compliance, while the provision of unofficial information (and the allowance of communication) by the taxpayers themselves increases compliance. Future work will attempt to explore these linkages between information, communication, and compliance in more depth.

Table 1 – Experimental Design [a]

	Communication	
Information	No	Yes
Do Not Announce Audit Results	T1	T3
Announce Audit Results	T2	

[a] All treatments last 30 rounds. In all treatments, the tax rate is 0.35, the fine rate is 1.5, subjects are organized into groups of eight persons, and the income range is the same for all sessions (the maximum is 100 lab dollars and the minimum is 60 lab dollars, in increments of 5 lab dollars).

Table 2 – Possible Messages in Treatment 3 [a]

Message	**Message Content**
1	Do Not Send a Message
2	I Was Not Audited
3	I was Audited
4	I Was Not Audited and Did Not Report all my Taxes
5	I was Not Audited and Reported all my Taxes
6	I Was Audited and Did Not Report all my Taxes
7	I Was Audited and Reported all my Taxes

[a] Subjects are only permitted to send one message from this list in each round. They must send a message before they can proceed to the end of the current period.

Table 3 – Summary Statistics

Variable	**Definition**	**Mean**	**Standard Deviation**
Evaded	Income underreported for taxes, defined as (Income–Declared)	42.086	37.92
Declared	Income declared for tax purposes	38.13	36.77
Comprate	Compliance rate, defined as (Declared/Income)	0.48	0.45
Income	Income earned via the earning task for current round	80.22	12.13
Wealth	Accumulated earnings to date	996.77	558.52
Praudit	Probability of an audit	0.21	0.15
Official	Actual number of audits from previous round, reported via computer to subjects	0.39	0.49
Unofficial	Dummy variable equal to 1 if communication between subjects is allowed via computer and 0 otherwise	0.34	0.47
Naudit	Number of audits in previous round	1.57	1.58
NauditXOfficial	Number of audits interacted with whether this information is reported to the subjects (“Official”)	0.66	1.28
NauditXUnoffical	Number of audits interacted with whether subjects are permitted to communicate (“Unofficial”)	.208	0.59
Lagaudit	Dummy variable equal to 1 if the individual was audited in the previous period and 0 otherwise	0.20	0.40
Preptax	Dummy variable equal to 1 if the individual says he or she prepares and files their own taxes and 0 otherwise	0.31	0.46

Table 4 – Tobit Estimation [a]

	Dependent Variable	
Independent Variables	Declared	Comprate
Constant	6.701 (1.38)	1.232*** (10.72)
Income	0.411*** (7.28)	-0.006*** (4.54)
Wealth	-0.027*** (21.90)	-0.0006*** (19.52)
Praudit	90.99*** (18.79)	2.213*** (17.60)
Lagaudit	-2.598 (1.47)	-0.079* (1.87)
Official	-4.104** (2.41)	-0.132*** (3.27)
Unofficial	4.104** (2.08)	0.039 (0.93)
Preptax	-4.469*** (3.03)	-0.076** (2.18)
LR	933.73***	847.07***

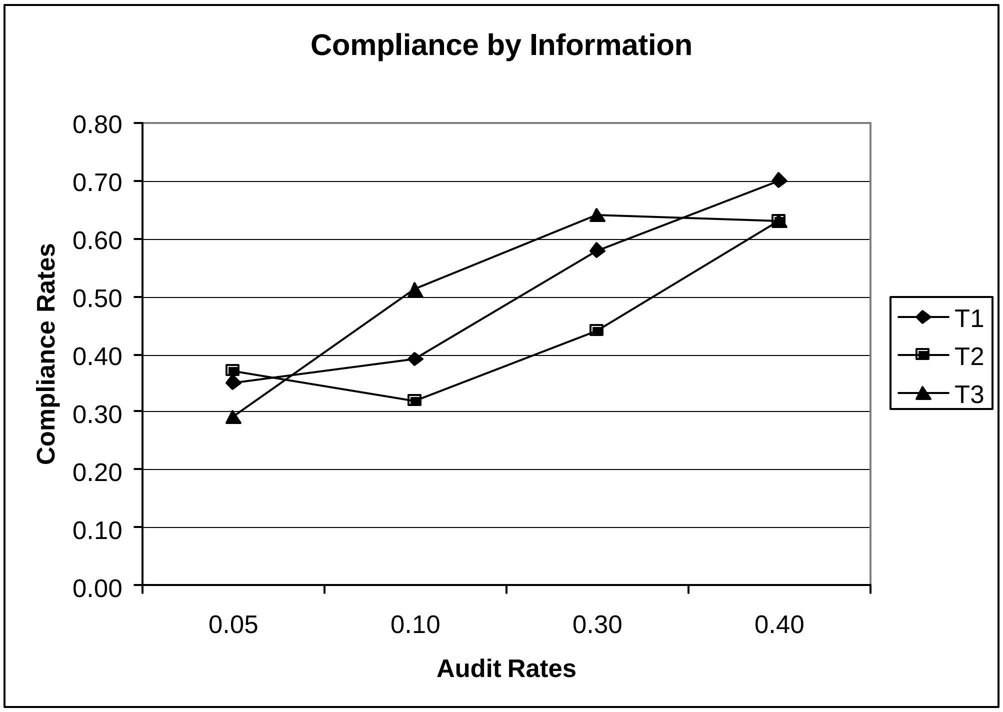
[a] In both estimations, the number of observations is 5278, the number of subjects is 182, and the number of time periods is 29. Numbers in parentheses are t-statistics. Significance levels are denoted as:
* 0.10, ** 0.05, *** 0.01.

Table 5 – Panel Estimation [a]

	Dependent Variable			
Independent Variables	Comprate	Comprate	Declared	Declared
Constant	0.499*** (15.94)	0.495*** (17.12)	3.793 (1.46)	3.887* (1.61)
Income	-0.0012*** (4.77)	-0.0013*** (5.07)	0.355*** (16.19)	0.348*** (15.82)
Wealth	-0.0002*** (13.05)	-0.0002*** (13.44)	-0.015*** (13.18)	-0.015*** (13.57)
Praudit	0.830*** (19.17)	0.839*** (19.07)	66.771*** (18.31)	66.831*** (18.22)
Lagaudit	-0.019*** (2.74)	-0.019*** (2.59)	-1.516** (2.49)	-1.497** (2.38)
NauditXOfficial		0.0001 (0.24)		0.097 (0.32)
NauditXUnofficial		0.013** (1.89)		0.929* (1.66)
Official	-0.067*** (3.51)		-5.266*** (3.34)	
Unofficial	0.059*** (2.89)		5.018*** (3.00)	
Preptax	-0.033** (1.93)	-0.027* (1.63)	-2.358* (1.67)	-2.245* (1.60)
Wald	725.90***	700.42***	673.07***	872.66***
Log-likelihood	-77.645	-95.591	-92.591	-23461.8

[a] These estimations are subject fixed effects estimations. In all estimations, the number of observations is 5278, the number of subjects (panels) is 182, and the number of time periods is 29. The numbers in parentheses are z-statistics. Significance levels are denoted as:
* 0.10, ** 0.05, *** 0.01.

Figure 1


Compliance by Information
Compliance Rates
0.80
0.70
0.60
0.50
0.40
0.30
0.20
0.10
0.00
0.05
0.10
0.30
0.40
Audit Rates
T1
T2
T3

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