The Effects of Communication Among Taxpayers on Compliance

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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 $1 of revenue produced because of taxpayer audits, an additional $6 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 1960's and have continued their decline in recent years. In the early 1960's, 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-1970's. Over the next decade, the audit rate fell further to roughly 1 percent. According to the Inspector General for Tax Analysis (2002), taxpayer audit rates have fallen another 56 percent between 1997 and 2001. As a result, at present, 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.

It, therefore, seems likely that the decline in audit rates since the 1960's 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] However, 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 larger 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 direct versus 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, we do not know how compliance is affected by the ways in which audit information is disseminated among or communicated by taxpayers; that is, the effects on compliance of the ways in which taxpayers learn about audit rates and communicate among themselves are not known and cannot be addressed or discovered by empirical studies.

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 experimental treatments, subjects are informed of the objective probability that their returns will be audited and the success rate of the audit process. In our base case sessions, the subjects receive no further information about audit results beyond their own audit experiences. 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 experiences; 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 direct and indirect effects of audits: "official" communications from the "Government" (e.g., the experimenter) and "unofficial," or informal, communications among "taxpayers" (e.g., the subjects). At this point, our results are preliminary and indicate that "unofficial" communications have a strong indirect effect that increases compliance, while "official" communications may not encourage voluntary compliance.

The next section gives a brief overview of the relevant theory of taxpayer compliance. We then discuss our experimental design and present our experimental results. Finally, we discuss our conclusions.

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.[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 but did not pay. The individual's income I_C if caught underreporting equals $I_C=I-tD-f[t(I-D)]$, while, if underreporting is not caught, income I_N is $I_N=I-tD$. The individual chooses declared income to maximize the expected utility $E\ U(I)$ of the evasion gamble, or $E\ U(I)=pU(I_C)+(1-p)U(I_N)$, where E 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. Comparative statistics results are easily derived. For example, an increase in the probability of detection p unambiguously increases declared income.

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

Experimental Design

The experimental design captures the essential features of the voluntary income reporting and tax assessment system used in many countries.[3] 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 by the tax agency 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 on his or her performance during the experiment. This basic experimental design follows the essential elements of Alm, Jackson, and McKee (1992, 1993) and Alm, McClelland, and Schulze (1992), but incorporates a number of additional features to improve parallelism with taxpayers' decisionmaking 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. On 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 on 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 names is the receipt signed when they receive their payments. 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.

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 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 incomes for the round and presented with a screen that resembles a tax form

in which they report their incomes. 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.

At this point, our 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. All audits investigate only the current period disclosure. 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. The tax rate is set at 0.35 throughout the experiments, and the fine rate is set at 150 percent. In all experiments, subjects are told the objective probability that they may be audited. 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.

Table 1--Experimental Design [a]

Information	Communication	
	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). There are four different audit rates employed (0.05, 0.10, 0.30, and 0.40), and these are applied in each of the treatments.

There are several ways in which information regarding the audit activity of the IRS can reach the taxpayers and, potentially, affect their compliance behaviors. In our base case treatment (T1), as well as in all other treatments, each subject knows the result of his or her own audit (if any), and subjects receive no information about the audit results of other subjects. We also investigate two different information transmission mechanisms. In T2, 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 T3, 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.

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.

Several hypotheses concerning compliance behavior can be formulated based on the experimental structure and design. Of most interest are hypotheses about the different information treatments. Although the announced audit probability is predicted to influence behavior directly, individuals may 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 behaviors. If the updating yields an expectation that the actual audit probability is lower, then individuals will reduce their compliance rates relative to what they would have done knowing only the announced audit probability; conversely, individuals will increase their compliance rates if the subsequent information leads to a higher subjective probability of audit. We expect that the announcement effect will be to increase compliance, and we, thus, expect that "official" information will increase compliance:

H1: 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 also seems likely to be positive:

*H2: Unofficial communication between taxpayers will, ceteris paribus, increase compliance.*

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

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, and additional treatments also need to be done.

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 compliance rate across all subjects, where the compliance rate for each subject is calculated as declared income divided by (true) income. These aggregate data reveal that the average compliance rate largely increases as the audit probability increases. These data also indicate that, 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).

Figure 1
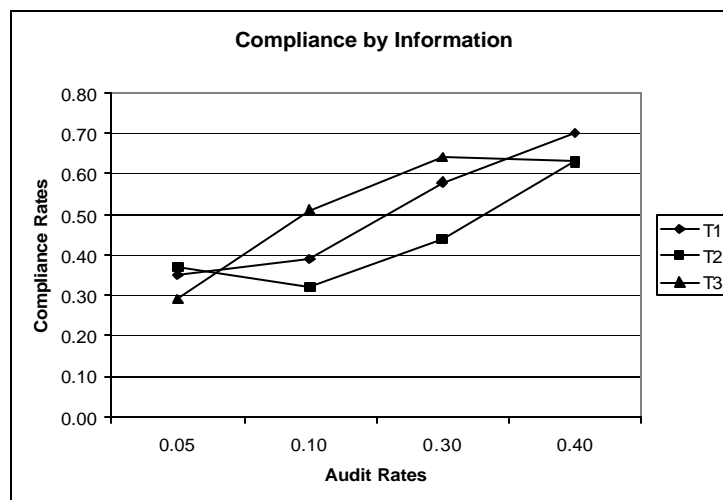
We can also examine the data at the individual level, by round and by treatment. The experimental design suggests that either the compliance rate of the individual or the amount of declared income by the individual in each round will depend on such things as (true) income, accumulated wealth, the probability of audit, and dummy variables that represent the different information treatments. Since either dependent variable (e.g., the compliance rate or the amount of declared income) is censored, we use Tobit maximum likelihood methods. In addition, 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 can estimate the determinants of individual compliance using panel estimation methods. Accordingly, we also employ a fixed effects model that allows us to exploit the panel structure of the data (Alm and McKee, 2004).

The Tobit estimation results indicate that, when the dependent variable is declared income, 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 the compliance rate 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 measures of the dependent variable, while "unofficial" communication between the subjects between rounds tends to increase both measures. We, therefore, find some initial results that support Hypothesis 2 but not Hypothesis 1. The panel estimation results are quite similar to the Tobit results. Again, "official" information reduces both measures of the dependent variable, while "unofficial" communication has the opposite results.

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.

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.

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Footnotes

[1]   See the U.S. Department of the Treasury Inspector General for Tax Administration (TIGTA) (2002). Also, see Internal Revenue Service (1996).

[2]   See Cowell (1990) and Andreoni, Erard, and Feinstein (1998) for surveys of the tax compliance literature.

[3]   The full set of experimental instructions is available on request.