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AUDIT INFORMATION DISSEMINATION, TAXPAYER COMMUNICATION AND TAX COMPLIANCE: AN EXPERIMENTAL INVESTIGATION OF INDIRECT AUDIT EFFECTS

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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 using field data 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.

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

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 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, 2004) 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.

Since the questions pertaining to the indirect effects of audits are behavioral it seems appropriate to follow a lengthy tradition (see Alm, McKee, and Beck, 1990; Alm, Jackson and McKee, 1992) and utilize a laboratory market setting to investigate the underlying *behavioral* factors contributing to spillover or indirect effects of audits. Thus, 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 (and the fines collected in some versions of this treatment). 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. Taken as whole, such unofficial (taxpayer to taxpayer) communications result in higher compliance but some forms of such communication actually reduce compliance. Briefly, the data show that communication regarding the incidence of audits and that the individual has complied with income reporting lead to higher compliance throughout while communication that individuals were not audited or that they had not complied tends to lower overall compliance. Indirect effects of audits exist but are more complicated than simple demonstration effects. Reporting fines collected does not lead to higher compliance when other forms of feedback information are taken into account.

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 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 $? U(I)$ of the evasion gamble, or $? U(I)=pU(I_C)+(1-p)U(I_N)$, where $?$ 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]

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[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.

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

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.

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*)

$$? \; ?U(I)/?D = pt(f\text{-}1)U'(I_C) - (1\text{-}p)tU'(I_N) = 0$$

$$?^2 \; ? \; U(I)/?D^2 = p[t(f\text{-}1)]^2 \, U''(I_C) + (1\text{-}p)t^2 \, U''(I_N) < 0.$$

[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

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

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 $R(I)/-IU''(I)/U'(I)$. It is typically assumed that $A(I)$ decreases with income, while $R(I)$ increases with income.

[6] See Alm (1999) and Andreoni, Erard, and Feinstein (1998) for reviews of extensions to the standard evasion model.

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

[7] An implied assumption is that auditors are 100 percent successful at finding unreported income.

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 their performance in a task. The actual income earned is determined by the relative performance in this 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 the owed taxes plus a fine based 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 earnings equal to the laboratory market earnings converted to US dollars.

Since these are experiments designed to inform policy makers 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 (Plott, 1989). In the current setting 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. The policy question is the nature of the indirect effect of audits. Thus the design specifically addresses this question by

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

varying the prior information concerning audit probabilities and by providing various types of audit result information to the subjects.

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, here subjects earn income rather than receiving an endowment and these experiments utilize tax language in the instructions 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 six to eight persons with multiple groups in each session. The subjects do not know who is in their group, only the number in their group and that there are at least two groups in the session. Basic instructions are provided via hardcopy while the main instructions are provided via a series of computer screens and practice rounds. Subjects are not allowed to communicate with one another during the session except when allowed via the computer interface. 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. After the practice rounds are completed any final procedural questions are answered. The full experiment then begins. Sessions last approximately 90 minutes, and subject earnings ranged from $19 to $37, depending upon his or her performance during

[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.

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 mouse at the numbers and "clicking" on the numbers in the correct sequence. On the computer screen a 3 by 3 matrix with the digits in random order appears on the right side of the screen and as the numbers are "clicked" they appear in a 3 by 3 matrix on the left side of the screen. A counter on the screen shows the elapsed time from when the first number is "clicked" and when all nine have been ordered, the subject clicks the "Continue" button to transmit this time to the server. 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 all subjects have completed the income task, they 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 tax policy information in effect for the session. In all treatments they are informed of the current tax rate and the penalty rate applied to non-disclosed income. In some treatments they know the current probability of an audit while in others they must infer this from their own experience and, depending on the treatment, on the post-audit information provided. As noted above, these experiments present the instructions and computer interface using tax 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 basic 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 provided some "official" information from the tax authority. The simplest information here is a reporting of the number of audits that actually occurred in the previous period. In the richer settings, the official information is expanded to include the average fines collected as well as the number of persons audited. 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. The possible messages are reported in Table 2 and each person may send only one message in a round. 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 in this setting, there is no means by which the subjects can verify whether this information is truthful.[10] At the end of the experiment, we also ask the subjects to report their age, gender and whether they prepare and file their own taxes. If they respond "No" to this last question, 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 a colored ball from a bucket on the subject's computer screen. In this bucket there are 20 balls with the number of blue ones determining the audit probability. A white ball signifies "no audit" and a blue one denotes an audit. 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. When the audit probabilities are not announced the bingo cage does not appear on the screen. After the subject files there is a delay while the server performs a random process that is identical to that used by the virtual bingo cage and announces to the subject whether they were audited on not.

After the audit process has been completed, the subjects are presented a screen that provides the earnings and audit outcome summary for the round. Where communication is allowed, the subjects then choose to send one of the messages reported in Table 2 and they are told that this will be sent to all the persons in their group. After all subjects have sent a message the subjects in this treatment receive further feedback in

[10] This would be an interesting interaction to investigate. In the field, individuals may know the IRS audit results and also receive information from individuals that they know or know of. However, the numbers of taxpayers in the field are so large that it is unlikely that one could combine these data to know whether the person they were communicating with spoke the truth.

the form of a table that reports the number of persons sending each of the messages. In the treatment for which the information is provided by the tax authority, the subjects see a screen that reports the results of the audits: the number audited, the total fines collected and the average fine collected. All of these are the results for their group.

A total of 326 subjects participated in the experiments reported here. The number of subjects participating in each treatment is shown in Table 1.

4. BEHAVIORAL HYPOTHESES

We define "compliance" as declared income divided by actual income in the following discussions. There are several basic behavioral hypotheses that are typically investigated. For example, rational individuals are predicted to increase compliance when the audit probability increases. As for response to 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. 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, and so we predict that compliance will vary inversely with income.

The above hypotheses are investigated in the course of our empirical analysis reported below. However, the focus of the current research is the indirect effect of audits

and the experiments are designed to provide different forms of information feedback so that we can investigate the effects of the information on subsequent compliance behavior. In particular, the information feedback allows us to investigate the attributes of the information that contribute to increased compliance and those that lead to reduced compliance.

The tax authority often announces the potential for being audited during a given period. Although this announced audit probability is predicted to influence behavior directly, it is likely that taxpayers will make use of subsequent information to refine their subjective estimates of individual audit probabilities. The tax authority may announce the number of audits actually undertaken in the previous period, the total fines levied, and/or the average fine levied. The less certain the announced audit probability the more such subsequent information will be used to allow individuals to update their audit prospects. We hypothesize that individuals underestimate their priors regarding audits, thus updating increases the expected audit risk. If so, individuals will reduce their compliance rate relative to what they would have done knowing only the announced audit probability. We expect that the announcement effect will be to increase compliance, and thus the coefficient on *Official* sources of information will be positive and we have these specific hypotheses:

> *H1: The official announcement of the number of audits in the previous period will, ceteris paribus, increase compliance.*
>
> *H2: The official announcement of the result of audits (fines collected) in the previous period will, ceteris paribus, increase compliance.*

Individual taxpayers may engage in communication with friends and acquaintances concerning their experiences at the hands of the tax auditors. Alm and

McKee (2004) investigated the effects of pre-filing communication on tax reporting behavior when the tax authority selected individuals for audit on the basis of *relative* reporting behavior. They found that taxpayers were able to focus on the better (lower compliance) equilibrium with such communication. In this paper the issue is whether communication concerning audit selection and audit outcomes leads to higher or lower compliance. If we continue with the above maintained hypothesis that individuals under estimate the probability of audit and over estimate the success of the audit agency, then communication will work in the same direction as the official release of information. Under expected utility theory both types of information during communication would have the same effect on compliance. Under non-EU models (such as loss aversion) individuals will over weight information that audits are successful and will increase their compliance. To the extent that paying taxes is viewed as a social contract (see Alm, Jackson and McKee, 1993) communication that others comply will also lead to higher compliance. Thus, the effect of *Unofficial* communication on compliance seems likely to be positive:

> *H3: Unofficial communication among taxpayers will, ceteris paribus, increase compliance.*

The range of possible messages in Table 2 is quite large but it is possible to group these into some broader categories. For example, two messages (5 and 7) are that the individual does comply with the tax law. In another instance, three messages report that one was audited (3, 5, and 7) and three that one was not audited (2, 4, and 5). These classifications allow us to investigate the relative effects of information that the tax authority would view as positive or that it would view as negative. Thus:

H4: Reports that others comply with the tax rules will lead to higher compliance on the part of individuals receiving this information.

H5: Reports that others have been audited will lead to higher compliance on the part of individuals receiving this information.

H6: Positive information will have a greater impact than negative information.

At this stage in our analysis we do not evaluate the veracity of the unofficial communication. Taxpayers are able to reveal information that is truthful or untruthful. The experimental setting allows both just as would arise in the naturally occurring setting. The next section presents our experimental results and our tests of these hypotheses.

5. EXPERIMENTAL RESULTS

The experimental data constitute a panel with 326 subjects and 30 decision rounds. Each subject makes one decision in a round – the income to report. The potential explanatory variables are the experimental treatments, the results from previous rounds, and certain subject factors. Given this structure there are several options for analyzing the data. We have elected to utilize a panel estimation technique that allows us to address certain characteristics of the data at the expense of foregoing some other factors.

Thus, we employ a panel structure that accounts for panel-specific heteroskedasticity. However, this precludes the use of fixed effects estimation and so does not allow us to address unobservable subject effects. We do collect some subject characteristic data and find that one particular variable does systematically affect estimated compliance behavior. But, clearly, we may be missing some unobservable

effects. Further, our dependent variable data are potentially censored in that the compliance rate is bounded by zero and one. Thus a tobit estimation may be justified. However, the only consistent estimator currently available for use with a panel dataset is the random effects (RE) estimation and the distributional assumptions required of the RE estimation do not seem to be justified here. Thus, we opt for the use of the cross-section time series (panel) estimation that incorporates panel-specific heteroskedastic error terms.[11]

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

$$Comprate = ß_0 + ß_1\, Income + ß_2\, Wealth + ß_3\, Praudit + ß_4\, Lagaudit + ß_5\, XXX + \varepsilon$$

where *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. Summary statistics for the variables used are reported in Table 3.

We report the results of our estimations for various specifications in Table 4. The simplest specification has only subject factors and is reported as Model 1 in the table. The results for this specification demonstrate that the subjects in these experiments exhibit behavior similar to that reported in previous research investigating compliance behavior. Compliance increases with higher probability of audit but decreases with

[11] All estimations are undertaken using the xtgls estimation in STATA release 8.

higher income and wealth as well as for individuals indicating that they prepare their own tax returns.

The more interesting results emerge as we investigate the effects of various information feedback treatments to understand the indirect effects of audits. In Model 2 it is clear that *Unofficial* information (communication by subjects) has the effect of increasing compliance even as we introduce a treatment for whether the actual audit rate was announced (*PrAudAnn*). It appears that announcing the audit probability lowers compliance, which would suggest that individuals overestimate the audit probability absent specific information.

In Models 3 and 4 we report the results when we decompose the *Unofficial* information to allow us to focus on common elements in the messages. We see in Model 3 that messages that report evasion lead to lower compliance while those reporting compliance lead to higher compliance. The relative magnitude of the effects suggests that the compliance messages increase compliance than the evasion messages lower compliance. The overall indirect effect is to increase compliance. It also appears that individuals attempt to play "catch up" following an audit. Given the structure of the experiment it is important not to attach too much significance to this effect. There are no conditional future audits in the experimental setting. Previous work (see Alm, Cronshaw, and McKee, 19xx) has shown that conditioning future audits on past, detected, evasion leads to substantial *increases* in future compliance.

In Model 4 the effect of messages reporting audit events is similarly shown to be asymmetric. Messages that convey past audits (*LagSumYes*) increase overall compliance while those that convey absence of audits (*LagSumNot*) do not have a statistically

significant effect. Bad news has greater impact and this is consistent with individuals adopting non-expected utility behavior. Decision models that emphasize bad outcomes (such as loss aversion) predict that individual will overweight (probability or outcome) bad events.

Model 5 reports results when all of the *Unofficial* communication aggregates are included. Despite the potential for multi-collinearity, we see that the individual messages that a tax authority would regard as good (audited and complied messages) jointly contribute more to increased compliance than do messages the authority would regard as bad. What is less satisfying is that the message that the tax authority could send regarding total fines collected in the previous period has a negative effect on compliance.

Overall, the coefficients on the included variables are quite stable across specifications. Summary statistics for overall goodness of fit improve as we read across Table 4 from left to right. *Unofficial* communication generally improves compliance when taken as a whole. However, the possibility exists for such communication to worsen compliance if there is a bias toward providing information conveying the weakness of the audit process and the extent of non-compliance. This is potentially worrisome for the tax authority and will be taken up in the next section. Of the *Official* information that can be transmitted, it would appear that the mention of fines imposed (collected) will worsen compliance as it has a negative indirect effect. This is also worrisome for the tax authority. Further, this result contradicts the broad class of decision models that weight bad outcomes (loss aversion and rank dependent expected utility for example).

6. CONCLUSIONS

We have a large and somewhat complex dataset and, undoubtedly, there are additional tests and specifications that will be investigated as we continue to analyze our data. At this stage, however, we have drawn several conclusions from our analysis as described in the previous section.

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.

We cannot use our results to answer the question how large is the indirect effect of audits? Such questions require the use of field data and gave been addressed by the work of Dubin cited above. However, the experiments can address some of the behavioral questions concerning the mechanism by which the indirect effects are manifest. We find that there is an asymmetry – messages that report audits and compliance have a larger absolute effect – and this supports the overall result that unofficial communication among taxpayers has the potential to increase overall compliance beyond the levels that individual audits alone provide.

Table 1 – Experimental Design [a]

	Communication	
Information	No	Yes
Do Not Publicly Announce Audit Results	**T1** T1a – 48 T1b – 32	**T3** T3a – 62 T3b – 40
Publicly Announce Audit Results	**T2** T2a – 72 T2b – 72	

[a] The number of subjects participating in each treatment is reported. Treatments denoted a are those where the audit rate (probability) is announced. Those denoted b are where the audit probability is not announced. 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 10 lab dollars with 2 persons in each income level).

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
Comprate	Compliance rate, defined as (Declared/Income)	0.553	0.448
Income	Income earned via the earning task for current round	80.22	12.13
Wealth	Accumulated earnings to date	944.60	560.18
Praudit	Probability of an audit	0.214	0.145
Official	Actual number of audits from previous round, reported via computer to subjects	0.442	0.49
Unofficial	Dummy variable equal to 1 if communication between subjects is allowed via computer and 0 otherwise	0.313	0.47
Lagaudit	Dummy variable equal to 1 if the individual was audited in the previous period and 0 otherwise	0.169	0.40
Lagpenalty	The value of the penalty assessed in the previous period: equals zero if not audited		
Preptax	Dummy variable equal to 1 if the individual says he or she prepares and files their own taxes and 0 otherwise	0.341	0.460
Praudann	= 1 if subjects were informed of the audit rate prior to reporting income	0.558	0.496
Lagsumnot	Number of subjects in group sending a message that included not being audited (2, 4 and 5 – Table 2)	1.352	2.169
Lagsumyes	Number of subjects in group sending a message that included being audited (3, 6 and 7 – Table 2)	0.553	1.109
Lagsumevade	Number of subjects in group sending a message that included tax evasion (4 and 6 – Table 2)	0.593	1.133
Lagsumcomp	Number of subjects in group sending a message that included tax compliance (5 and 7 – Table 2)	0.573	1.087
Lagtotfine	The total fine paid by those in the subject's group audited in the previous period (if this information was reported to the subject in the treatment)	26.3	6.87

Table 4 – Estimation Results

	Specifications				
Independent Variables	Model 1	Model 2	Model 3	Model 4	Model 5
Constant	0.788*** (28.16)	0.949*** (34.39)	0.952*** (35.33)	0.960*** (35.56)	0.939*** (35.41)
Income	-0.0014*** (4.35)	-0.0018*** (6.03)	-0.0018*** (5.99)	-0.0019*** (6.13)	-0.0018*** (6.00)
Wealth	-0.0003*** (33.96)	-0.0002*** (33.16)	-0.0002*** (32.28)	-0.0002*** (33.18)	-0.0002*** (31.45)
Praudit	0.708*** (27.32)	0.702*** (28.55)	0.684*** (27.56)	0.679*** (26.56)	0.568*** (22.19)
Lagaudit					0.2123*** (15.93)
Lagpenalty	-0.0305*** (26.45)	-0.0294*** (26.16)	-0.0294*** (25.85)	-0.0295*** (26.14)	-0.0458*** (30.13)
Round	0.007*** (7.19)	0.006*** (6.52)	0.006*** (6.59)	0.006*** (6.50)	0.0055*** (5.99)
Preptax	-0.0268*** (3.38)	-0.0376*** (4.97)	-0.0348*** (4.64)	-0.0375*** (5.00)	-0.0318*** (4.32)
PrAudAnn		-0.262*** (36.58)	-0.274*** (37.14)	-0.271*** (36.73)	-0.271*** (37.89)
Official		-0.0127 (1.39)			
Unofficial		0.0435*** (4.45)			
LagSumEvade			-0.0172*** (4.87)		-0.0250*** (5.06)
LagSumComp			0.0367*** (10.05)		0.0221*** (3.72)
LagTotFine			-0.0039*** (5.02)	-0.0037*** (4.71)	
LagSumNot				-0.0004 (0.19)	0.005 (1.77)
LagSumYes				0.0236*** (6.35)	0.0159*** (3.42)
Wald	2855.19***	4485.57***	4717.64***	4625.19***	4993.32***
Log-likelihood	-4461.05	-3964.511	-3913.104	-3934.132	-3819.12

[a] The dependent variable is the compliance rate (COMPRATE). These estimations are panel models using feasible generalized least squares estimators. In all estimations, the number of observations is 9454, the number of subjects (panels) is 326, and the number of time periods is 29 (omitting period one for the lag operator). The numbers in parentheses are z-statistics. Significance levels are denoted as: * 0.10, ** 0.05, *** 0.01.

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