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AUDIT INFORMATION DISSEMINATION, TAXPAYER COMMUNICATION, AND COMPLIANCE BEHAVIOR

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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. Audits are also believed to have an *indirect* deterrent effect on individuals not audited, and in fact there is some empirical evidence that audit rates affect compliance beyond the audited individuals themselves. However, empirical studies cannot measure or control for taxpayer awareness of audit risk, and they also cannot uncover the behavioral channels through which the direct and indirect effects operate; that is, the ways in which taxpayers learn about – and communicate among themselves – audit rates, and the subsequent effects on compliance, are not known and cannot be discovered by empirical studies. In this study, we use laboratory experiments to examine several types of information dissemination and taxpayer communication about audit frequency and audit results. These experiments allow us to test hypotheses about the effects of two types of communication of audit policies and results, in order to explore the direct and the indirect effects of audits: "official" information disseminated by 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 on compliance: messages that indicate that a subject was not audited or was able to cheat actually reduce compliance, while messages that a subject was audited or paid his or her taxes increase compliance. Also, "official" announcements of information may not always 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 in fact there is some empirical evidence 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. More recent work by Dubin (2004) estimates an even larger ripple effect, at 8 to 12. 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 but still significant impact, and one felt only by high-income wage and salary workers; for this group of taxpayers, they estimate an indirect effect 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 Tauchen, Witte, and Beron (1989), Dubin, Graetz, and Wilde (1990), and Dubin (2004) 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.

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, then the revenue cost of reduced audit rates is significantly greater than $7.2 billion, perhaps from 3 to 12 times as much.

However, the magnitude and, especially, the underlying cause of these indirect impacts are still largely speculative. Indirect effects of audits must ultimately arise through the information about audit probabilities and outcomes that is communicated among taxpayers. Such information may serve to refine individual estimates of audit probabilities. While overall audit rates are quite low, among certain income and occupation classes they are more frequent. To the extent that communication takes place within such cohorts, this may serve to increase audit awareness and thus compliance. Audits of others may also convey a reminder effect, reminding individuals that audits do occur even if they themselves have no personal experiences.

Despite the insights from empirical studies using field data, these studies cannot measure or control for taxpayer awareness of audit risk, and they also cannot uncover the channels through which information dissemination and taxpayer communication work. As a result, 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. Put differently, the effects on

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

compliance of the ways in which taxpayers learn about – and communicate among themselves – audit rates are not known, and simply cannot be addressed or discovered by empirical studies.

More generally, in fact, 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.

The purpose of this paper is to examine the roles of information dissemination and taxpayer communication on voluntary compliance. Since the questions pertaining to the indirect effects of audits are behavioral, we follow a lengthy tradition (Friedland, Maital, and Rutenberg, 1978; Spicer and Everett, 1982; Becker, Buchner, and Sleeking, 1987; Webley, Robben, Elffers, and Hessing1991; Alm, Jackson and McKee, 1992; Sour, 2001) by utilizing a laboratory market setting to investigate the underlying behavioral factors contributing to the indirect effects of

audits, especially the ways in which information and communication affect compliance. In particular, we examine several types of communication about audits and audit results using laboratory market experiments in which the audit setting and communication opportunities are controlled. 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 official audit rate for the period (as well as the actual number of audits and the results of the audits in some versions of this treatment). In a 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 experimental design therefore allows us to test hypotheses about the effects of two types of communication of audit policies and results, in order to explore the direct and the indirect effects of audits: "official" information disseminated by 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 generally result in higher compliance, although some forms of such communication actually reduce compliance. For example, communication about who experienced an audit and whether this individual complied with income reporting both lead to higher compliance, while communication that individuals were not audited or that they had not complied tends to lower overall compliance. Indirect effects of audits therefore exist, but are more complicated than simple demonstration effects. Official information dissemination also has complicated effects on compliance. Announcing the official audit *rate* on balance increases compliance, but reporting the actual *number* of audits

conducted in the previous round does not lead to higher compliance; that is, official information has a somewhat mixed impact on compliance. We are also able to estimate the ripple effect of audits. On average, we calculate this ripple effect at roughly 4, or a number somewhere between the estimates of Beron, Tauchen, and Witte (1989) and those of Dubin, Graetz, and Wilde (1990) and of Dubin (2004).

The next section gives a brief overview of the relevant theory of taxpayer compliance. Section 3 discusses our experimental design, and section 4 suggests several hypotheses. Section 5 presents our experimental results, and 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 (ignoring labor supply effects), an individual is assumed to receive an income I and to choose how much of this income to declare to the tax authorities. 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 underreported. 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 $I_N=I-tD$.

[2] Cummings, Martinez-Vazquez, and McKee (2001) 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.

An expected utility maximizing individual chooses declared income to maximize the expected utility *EU(I)* of the evasion gamble, or

$$EU(I) = pU(I_C) + (1-p)U(I_N), \quad (1)$$

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.[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, which depends upon the individual's attitude toward risk. If the individual exhibits decreasing absolute risk aversion, then higher incomes (and wealth) are associated with lower levels of compliance.

The standard model has been modified in a number of ways.[5] 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, or

$$EV = I - td - pf[t(I - D)]. \quad (2)$$

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

$$\partial EU(I)/\partial D = pt(f-1)U'(I_C) - (1-p)tU'(I_N) = 0$$

$$\partial^2 EU(I)/\partial D^2 = p[t(f-1)]^2 U''(I_C) + (1-p)t^2 U''(I_N) < 0.$$

[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

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

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] See Cowell (1990) and Andreoni, Erard, and Feinstein (1998) for a discussion of the standard evasion model and its variants.

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 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, then any fine rate less than 100 will lead a risk-neutral individual to report zero income.

These examples illustrate a standard result: given actual audit and fine rates (and standard levels of risk aversion), most people should rationally choose to cheat. The incorporation of risk-aversion certainly affects these calculations, but even so the basic expected utility model of tax evasion (and its many variants) is generally unable to explain observed compliance rates.

However, incorporating non-expected utility behavior into the analysis (e.g., individuals exhibiting loss aversion or more extreme forms of risk aversion, such as a rank dependent expected utility) can provide predictions of compliance more consistent with observed behavior (Bernasconi, 1998). For example, for individuals described by a rank dependent expected utility model, we can modify the basic maximization problem of equation (1) to one in which individuals maximize

$$EU(I) = gpU(I_C) + (1-g)(1-p)U(I_N), \qquad (3)$$

where g serves to overweight the probability of the "bad" outcome (or detection and punishment). This alternative approach also helps illuminate the roles of information dissemination and communication. Either official information provided by the tax authority or taxpayer-to-taxpayer communication that describes audits and penalties is likely to increase the weighted probability of an audit. In contrast, if the information and/or communication highlights

the absence of audits or their lack of effectiveness, we would expect that they would lower the weighted probability of an audit. Note that individuals who exhibit such rank dependent expected utility are likely to respond less to reports of the absence of audits, and thus the overall effect of communication should be to increase tax compliance beyond the level consistent with objective audit probabilities, a result consistent with the field data.

Even so, in all of these analyses it is still assumed that taxpayers *know* the actual audit probability and penalties. What is unavoidably and necessarily missing from the empirical work of Tauchen, Witte, and Beron (1989), Dubin, Graetz, and Wilde (1990), and Dubin (2004) is a model of the manner by which information concerning the true audit probability and the levied fines is communicated among and understood by the taxpayers. The IRS does not generally announce that it will be raising or lowering the audit rate, even though this information is available. 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: how do taxpayers learn that the audit rate is declining and thereby 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.[6] Human subjects in a controlled laboratory environment earn income through their performance in a task. The actual income earned is

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

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 a random audit, and the subject must then pay the owed taxes plus a fine based on the unpaid taxes.[7] 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 U.S. 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, 1987). As implemented, our experimental design follows the main elements of Alm, Jackson, and McKee (1992a, 1992b, 1993) and Alm, McClelland, and Schulze (1992), but incorporates some additional features to improve parallelism with taxpayers' decision making in the naturally occurring world. For example, in the current design, subjects earn income by performing a task (rather than receiving an endowment), they disclose income, and they face an audit process similar to that in the naturally occurring setting. Also, these experiments utilize tax language in the instructions and in the computer interface used to present information and elicit income reporting behavior. While the stakes are small, the decision setting is also simplified relative to that of the natural setting.

[7] It may be argued that current audit practice implements purely endogenous audits, since a taxpayer either elicits an audit or not depending on his or her "score" in an audit rule. However, whether a taxpayer is actually audited depends on the score and on the audit budget of the tax authority. Since the taxpayer cannot know this latter item with certainty, there remains a random component to the audit process.

The policy question is the nature of the indirect effect of audits. The design specifically addresses this question by varying the information conveyed to subjects about audit probabilities, by providing various types of audit result information to the subjects, and by allowing taxpayer communication in some settings.

Subjects are recruited from undergraduate classes in economics and business.[8] 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 the presence of 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 is predetermined, and the sessions last 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. Subject earnings range from $19 to $37, depending upon subject 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

[8] 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.

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. (See Appendix A.) 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 (Appendix B). 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 the subjects are told the 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 in all sessions. There is no public good financed by the tax 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 90 lab dollars per 1 U.S. dollar.

There are several ways in which information regarding the audit activity of the tax authority can reach the taxpayers and, potentially, affect their compliance behavior. In our experimental design, we investigate two different information transmission mechanisms. In the first mechanism, the subjects are provided some "official" information from the tax authority. The simplest information here is a public announcement of the audit probability that subjects face in any given round. In some sessions (denoted "A" in Table 1), the audit rate is announced; in other sessions (denoted "B" in Table 1), the audit rate is not announced. A different form of "official" information is the actual number of audits that occurred in the previous period (and the results of these audits), and this information on audit results is announced in some sessions (Treatment 2, or T2 in Table 1) and not in others (T1 in Table 1). We also combine these official information treatments, to give T1A, T1B, T2A, and T2B.

In some other sessions (T3), we allow subjects to send one message in each round to all other persons in their group; each person may send at most one message in a round, and all subjects in the group receive the message. The possible messages are reported in Table 2. We refer to this as "unofficial" information, in which taxpayer-to-taxpayer communication may affect behavior, and we combine this unofficial treatment with announcement of the audit rate (T3A) and non-announcement of the audit rate (T3B). This process works as follows. 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[9], and indeed the experimental setting does not impose the requirement that the information be truthful.

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 tax preparation, given that our subjects are typically college sophomores or juniors.

The process of determining who is audited is given by a computerized draw. In sessions in which the audit probability is announced, the subjects are presented with a computerized representation of a bucket in which a draw is made. 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, 2001), but differs from Alm, Jackson, and McKee (1992a, 1992b, 1993) and 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; the subject simply receives a text message that reports whether she was audited or not. After the subject reports income, 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 new screen that provides the earnings and audit outcome summary for the round. Where taxpayer-to-taxpayer communication is allowed (T3A and T3B), the subjects then choose to send one of the messages reported in Table 2. After all subjects have sent a message, the subjects receive further feedback

[9] In the field, individuals may know the IRS audit results, and may 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 with whom they were communicating spoke the truth.

in the form of a table that reports the number of persons sending each of the messages. In the treatment (T2A and T2B) for which audit result 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 only.

A total of 326 subjects participated in the experiments. The number of subjects participating in each treatment is shown in Table 1, where some summary statistics for each of the treatments are also reported.

4. BEHAVIORAL HYPOTHESES

There are several basic behavioral hypotheses that are typically investigated in compliance studies. Our focus is on the indirect effect of audits. The experiments are designed to provide different forms of audit information and taxpayer communication, so that we can investigate the effects of information and communication on subsequent compliance behavior. In particular, our design allows us to investigate the attributes of the information that contribute to increased compliance and those that lead to reduced compliance. We also investigate the impacts of other variables on compliance. For example, as discussed above, expected utility maximizers (as well as those exhibiting rank dependent expected utility) are predicted to increase their declared income when the audit probability increases. The effects of income and "wealth" (or accumulated income) depend upon risk attitudes. The effect of past audits on individual compliance behavior is ambiguous. The taxpayer may use the audit experience to update his or her probability of being audited in the future. Such updating can lead to lower or higher future compliance. The "gambler's fallacy" – the notion that "If I was audited in the last round, then there is less chance I will be audited this round" – will lead to lower compliance after an audit.

However, if the individual feels he or she had underestimated the probability of an audit, then, assuming a Bayesian updating process, the response to an audit will be to increase the estimated probability and thereby to increase compliance in the future.

Again, our focus is on information and communication. In the real world, individuals have varying levels of information regarding the objective probability of an audit. The tax authority may formally announce an audit rate, there may be less precise information reported in the press, or the audit rate (and audit process) may be a complete secret. The available information allows individuals to form priors regarding the probability that they will be audited. Although the announced audit probability is predicted to influence behavior directly, it is likely that taxpayers make use of other types of information to refine their subjective estimates of individual audit probabilities. For example, the tax authority may also announce the results of any audits actually undertaken in the previous period, including the number of individuals audited, the total fines levied, and/or the average fine levied. The less certain (or more diffuse) the prior audit probability, the more such official information may be used to allow individuals to update their audit prospects.

The impact on compliance of this official information is uncertain. Individuals with rank dependent expected utility preferences will focus on the "bad" outcome (e.g., being audited), and so official information regarding audit rates and the results of audits of others will lead them to update their priors so as to increase their subjective audit risk. These individuals will therefore increase compliance relative to what they would have done based on their priors, so that we expect that the "official" announcement effect will be to increase compliance. However, the provision of official information may also convey to individuals that audits do not have as severe an impact as expected. In this case, individuals will adjust their priors to lower the negatives

associated with an audit, thereby reducing compliance. Overall, we expect that the "official" announcement effect will be to increase compliance, as reflected in the following hypotheses:

> *H1: The official announcement of the audit probability will, ceteris paribus, increase compliance.*
>
> *H2: The official announcement of the audit result (e.g., the number of individuals audited, the fines collected) in the previous period will, ceteris paribus, increase compliance.*

There is also "unofficial" information, or information communicated by taxpayers. Individual taxpayers may engage in communication with friends and acquaintances concerning their experiences at the hands of the tax auditors. Here the issue is whether taxpayer communication concerning audit selection and audit outcomes leads to higher or lower compliance. If we continue to assume that individuals underestimate the probability of audit and overestimate the success of the audit agency, then communication about audits and audit results will work in the same direction as the official release of information. Under expected utility theory, both types of information during communication will have the same positive effect on compliance; under non-expected utility models, individuals will overweight information that audits are successful, and so will again increase their compliance.[10]

However, matters are obviously more complicated. To the extent that paying taxes is viewed as a social contract (Alm, Jackson and McKee, 1993), communication that others comply will lead to higher compliance, but communication that others cheat will lower compliance; that is, communication regarding taxpayer behavior will establish – either increasing or decreasing – the "social norm" of compliance.

[10] Note that Alm and McKee (2004) investigate the effects of pre-filing communication on tax reporting behavior when the tax authority has the policy of selecting individuals for audit on the basis of relative reporting behavior. They find that, in such a coordination game, taxpayers are able to focus on the lower compliance Nash equilibrium when the taxpayers are able to communicate with one another.

The range of possible unofficial messages in Table 2 is quite large, but these messages may be usefully grouped for our analysis. For example, there are two messages (5 and 7) that the individual complies with the tax law, while two messages (4 and 6) report evasion. Three messages report that one was audited (3, 5, and 7), and three report that one was not audited (2, 4, and 5). These classifications allow us to investigate the relative effects of taxpayer communication that may have either a positive or a negative effect on compliance:

> *H3 (Social Norm): Reports that others comply with the tax rules will lead to higher compliance on the part of individuals receiving this information. Similarly, reports that others do not comply will lead to lower compliance.*
>
> *H4 (Indirect Effects): Reports that others have been audited will lead to higher compliance on the part of individuals receiving this information. Similarly, reports that others have not been audited will lead to lower compliance.*

It should be noted that we do not evaluate the veracity of the unofficial communication. The purpose of the present analysis is to investigate the effects of communication and the individuals in the experiment have no means to verify whether the information is truthful. Thus, we simply incorporate the received information into the estimated compliance model. 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 simplest analysis of our experimental results examines descriptive statistics on the effects of the different treatments on average *Compliance Rate*, or calculated as the ratio of income declared by the subject to the tax authority to true income. Aggregate statistics by treatment are shown in Table 1, where *Compliance Rate* is calculated as the simple average

across all subjects and all rounds of a given treatment. Allowing unofficial taxpayer communication always increases compliance, especially in the absence of official announcement of the audit probability (T3B versus T1B).

The impact of official information is more complicated. When the official audit probability is announced (or Set A treatments), official announcement of the audit results actually lowers the average compliance rate (T2A versus T1A); when the audit probability is not announced (Set B treatments), official announcement of the audit results increases the compliance rate (T2B versus T1B).

The effects of announcing the audit probability are also somewhat mixed in the aggregate. There are three different comparisons that we can make here. Comparing T1A to T1B (or treatments in which the audit results are not announced and there is no taxpayer communication), official information on the audit probability lowers the average compliance rate, from 59.4 percent in T1B to 51.5 percent in T1A. When the audit results are announced (and subjects are still not allowed to communicate with one another), the further announcement of audit probabilities again lowers compliance; compare T2B with T2A. Finally, the average compliance rate falls from 64.9 percent in T3B to 51.6 percent in T3A with the announcement of the audit probability (or treatments in which taxpayers are allowed to communicate and the audit results are not announced).

These complex and interacting effects can be disentangled by looking at the individual-level information. The experimental data constitute a panel with 326 subjects and 30 decision rounds. Each subject makes one decision in a round (or the amount of income to declare). The variables that affect this decision are various economic factors (e.g., the income of the subject in the round), the experimental treatments (e.g., information and communication), and subject

characteristics. 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. (Because of the experimental nature of the data, there is perfect correlation between subjects and experimental treatment variables, so that we cannot use a subject fixed effects estimation method.) We also collect some subject characteristic data, and we find that these systematically affect estimated compliance behavior. However, we acknowledge that we may be missing some unobservable effects that could be addressed with a subject random effects estimation approach. The distributional assumptions required of a random effects estimation do not seem to be justified here. Hence, we opt for the use of the cross-section time series estimation utilizing a generalized least squares estimator incorporating panel-specific heteroskedastic error terms.[11]

The experimental design suggests that the *Compliance Rate* of a taxpayer in each round depends upon such factors as the individual's actual (or "true") earned *Income* in the round, the *Wealth* (or the accumulated earnings of the individual up to that round), individual characteristics, and variables that represent the experimental treatments on information and communication. The individual characteristics include a dummy variable for whether the individual has experience with preparing his or her own tax return (*Own Prepared Tax*), a dummy variable for gender (*Male*) to control for any systematic effects across subjects due to gender, and a variable (*Age*) to control for subject age.

The information and communication variables are constructed from the experimental design. The information and communication variables are of several types. "Official" information includes the announcement of the official audit probability (*Audit Probability Announced*) and of the audit results (*Audit Results Announced*). "Unofficial" communication

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

among the subjects includes the possibility of sending any type of message (*Unofficial Communication*), as well as the four different combinations of these messages. Definitions and summary statistics for all variables are reported in Table 3.

We report the results of various specifications in Table 4. The simplest specification has only subject factors; this is reported as Model 1. The results for this specification demonstrate that the subjects in these experiments exhibit behavior similar to that reported in much previous compliance research.[12] The compliance rate is decreasing in wealth and in income. We also find that compliance increases with age and is lower for males. Interestingly, the compliance rate is lower for individuals reporting that they prepare their own tax returns.

More interesting results emerge as we investigate the effects of various information and communication treatments to understand the indirect effects of audits. In Model 2 it is clear that the provision of official information on the audit rate (*Audit Probability Announced*, interacted with the *Audit Probability*, or *H1*) increases compliance, by 6.7 percentage points (relative to an overall average compliance rate of 55.3 percent). In contrast, official announcement of the results of audits reduces compliance (*H2*), as does *Lagged Audit* interacted with *Audit Probability Announced*. Given the magnitude of the coefficients on these three official information variables, however, official information on balance has a small positive impact on the compliance rate (by 1.4 percentage points).

In Model 3 we introduce taxpayer communication as a simple dummy variable (*Unofficial Communication*) for whether this was allowed. We find that this leads to higher compliance by 4.5 percentage points. However, we can organize the data to identify the effects of different types of communication (Model 4), and learn more about the transmission of such unofficial information. In Model 4 we see that messages that report evasion lead to lower

[12] For example, see Alm, Jackson and McKee (1992b).

compliance (*Unofficial Message – Subjects Evaded*), while those reporting compliance (*Unofficial Message – Subjects Complied*) lead to higher compliance (*H3*). The relative magnitude of the effects suggests that the compliance behavior messages increase compliance by marginally more than the evasion messages lower compliance. This result is consistent with the effect of a positive effect of social norms. Elsewhere, social norms have been shown to increase cooperative behavior, of which tax compliance is but one example (Cummings et al., 2004).

A similar asymmetric result occurs in Model 4 for the effect of messages reporting audit events (*H4*). Messages that convey past audits (*Unofficial Message – Subjects Audited*) increase overall compliance while those that convey the absence of audits (*Unofficial Message – Subjects Not Audited*) lower compliance. Again, the relative magnitude of these effects indicates that compliance on balance increases, by 2.0 percentage points. "Bad" news appears to have a greater impact, a result that is consistent with individuals adopting non-expected utility behavior. Decision models that emphasize such "bad" outcomes predict that individuals will overweight the likelihood of events that generate losses.

Despite the potential for multicollinearity in Model 4, on balance we see that the individual messages that a tax authority would regard as compliance-increasing (e.g., audited and complied messages) jointly contribute more to compliance than do messages that the authority would regard as compliance-decreasing.

Summary statistics for overall goodness-of-fit generally improve as we read across Table 4, from Model 1 through Model 4, and the coefficients on the common subject variables are quite stable across the various specifications. We find that *Unofficial Communication* improves compliance, even when the individual messages are included as separate variables in Model 4. However, the possibility exists for such communication to worsen compliance if the information

conveys the weakness of the audit process and the extent of noncompliance. This result is potentially worrisome for the tax authority.

6. CONCLUSIONS

Of perhaps most interest are our findings that the official provision of audit information by the tax authority has a small positive effect on subsequent compliance and that the provision of unofficial information via communication by the taxpayers themselves also generally increases compliance. However, informal communication works to increase subsequent compliance among the taxpayers when the communication is related to being audited. While not directly addressed in this paper, our data provide support for the presence of social norms in tax compliance decisions.

Our results show that the effect of audits is not limited to those actually audited. In fact, we can compute the magnitude of this indirect effect that we have been investigating, following the same general procedures as in Dubin (2004). The audits yield revenues equal to the tax owed plus penalties. Since individuals who are not actually audited do in fact pay taxes, the indirect effect of the audit mechanism can be computed as taxes collected from those not audited divided by the total audit yield. For all sessions taken together this ratio is 4.4; that is, the ripple effect of audits is 4.4, and total taxes collected are some 440 percent greater than the revenues collected via the audit process itself. This indirect effect is similar to, though somewhat lower than, estimates reported by Dubin, Graetz, and Wilde (1990) and by Dubin (2004), and somewhat larger than those of Beron, Tauchen, and Witte (1989), all of which are generated from field data.

Our focus here is on the response of taxpayers to the information provided by other taxpayers. Thus, we do not evaluate whether the transmitted information is truthful or not. The taxpayers in our experiment would have no means of evaluating the veracity of the communicated information. Our experiments address some of the behavioral questions concerning the mechanism by which the indirect audit effects are manifest. We find that information regarding audit experiences of others does have an effect. It is interesting to note that there is an asymmetry in taxpayer responses – messages that report audits and compliance have a larger absolute effect – and this finding 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 and Aggregate Results [a]

Set A – Audit Probability Announced	**Unofficial Taxpayer Communication?**	
Official Information?	No	Yes
Do Not Publicly Announce Audit Results	**T1A** Subjects=48 Compliance Rate=0.515 Tax Revenue=17.6% Audit Yield=$13.64	**T3A** Subjects=62 Compliance Rate=0.516 Tax Revenue=17.9% Audit Yield=$17.67
Publicly Announce Audit Results	**T2A** Subjects=72 Compliance Rate=0.445 Tax Revenue=15.5% Audit Yield=$19.50	
Set B – Audit Probability Not Announced	**Unofficial Taxpayer Communication?**	
Official Information?	No	Yes
Do Not Publicly Announce Audit Results	**T1B** Subjects=32 Compliance Rate=0.594 Tax Revenue= 0.7% Audit Yield=$16.36	**T3B** Subjects=40 Compliance Rate=0.649 Tax Revenue=22.5% Audit Yield=$15.04
Publicly Announce Audit Results	**T2B** Subjects = 72 Compliance Rate=0.646 Tax Revenue = 22.4% Audit Yield = $16.18	

[a] Treatments denoted A are those where the audit rate (probability) is announced, while 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, and subjects are organized into groups of six to eight persons. The income range is the same for all sessions (the maximum is 100 lab dollars and the minimum is 60 lab dollars (for eight person groups), in increments of 10 lab dollars with 2 persons in each income level). Audit rates vary from 0.05 to 0.40 in all treatments. "Compliance Rate" is calculated by dividing declared income by true income, for all subjects and all rounds of a given treatment. "Tax Revenue" is calculated by dividing taxes paid on declared income by taxes that would be paid if all income is reported, for all subjects and all rounds of a given treatment. "Audit Yield" is calculated by summing taxes and penalties paid on undeclared income, for all subjects and all rounds of a given treatment.

Table 2 – Possible Messages in Treatment 3[a]

Message	Message Content	Number of Messages Sent
1	Do not send a message	544
2	I was not audited	681
3	I was audited	281
4	I was not audited and did not report all my taxes	549
5	I was not audited and reported all my taxes	558
6	I was audited and did not report all my taxes	253
7	I was audited and reported all my taxes	194

[a] Subjects are only permitted to send one message from this list in each period, and the must send a message before they can proceed to the end of the current period. There were 3060 messages sent in total during Treatment 3 (A and B).

Table 3 – Summary Statistics

Variable	Definition	Mean (Standard Deviation)
Declared Income	Income reported to tax authority	44.436 (36.78)
Compliance Rate	Declared Income / (Earned) Income	0.553 (0.45)
Income	Income earned via the earning task	80.860 (11.95)
Wealth	Accumulated earnings to date	944.600 (560.18)
Audit Probability	Probability of an audit	0.214 (0.15)
Audit Probability Announced	Dummy variable equal to 1 if subjects are informed of the audit rate prior to reporting income and 0 otherwise	0.558 (0.50)
Audit Probability Not Announced	Dummy variable equal to 1 if subjects are not informed of the audit rate prior to reporting income and 0 otherwise	0.442 (0.50)
Audit Results Announced	Actual number of audits from previous round, reported via computer to subjects	0.442 (0.49)
Unofficial Communication	Dummy variable equal to 1 if any communication between subjects is allowed via computer and 0 otherwise	0.313 (0.47)
Unofficial Message – Do Not Send a Message[a]	Number of subjects who choose the "Do not send a message" option – Table 2	544 (---)
Unofficial Message – Subjects Not Audited[a]	Number of subjects in group sending a message that included not being audited (Messages 2, 4 and 5 – Table 2)	1788 (---)
Unofficial Message – Subjects Audited[a]	Number of subjects in group sending a message that included being audited (Messages 3, 6 and 7 – Table 2)	728 (---)
Unofficial Message – Subjects Evaded[a]	Number of subjects in group sending a message that included tax evasion (Messages 4 and 6 – Table 2)	802 (---)
Unofficial Message – Subjects Complied[a]	Number of subjects in group sending a message that included tax compliance (Messages 5 and 7 – Table 2)	752 (---)
Lagged Audit	Dummy variable equal to 1 if the individual was audited in the previous period and 0 otherwise	0.216 (0.41)
Own Prepared Tax	Dummy variable equal to 1 if the subject says in post-experiment survey that he or she prepares their own taxes and 0 otherwise	0.341 (0.47)
Age	Subject age as indicated by the subject in post-experiment survey	20.13 (3.02)
Male	Dummy variable equal to 1 if subject indicated in post-experiment survey gender as male and 0 otherwise	0.531 (0.49)

[a] There were 3060 messages sent in total during Treatment 3 (A and B). The numbers here do not add to 3060 due to repeated reporting of messages in the categories (e.g., Message 4 appears in two of the above classifications).

Table 4 – Estimation Results

	Specifications			
Independent Variables	Model 1 Individual Factors Only	Model 2 Official Information	Model 3 Unofficial Communication	Model 4 All Unofficial Communication
Constant	0.745*** (0.033)	0.737*** (0.034)	0.721*** (0.033)	0.712*** (0.033)
Income	-0.067** (0.032)	-0.068** (0.033)	-0.070** (0.033)	-0.063*** (0.033)
Wealth	-0.235*** (0.007)	-0.237*** (0.007)	-0.236*** (0.007)	-0.229*** (0.007)
Own Prepared Tax	-0.013* (0.008)	-0.015* (0.008)	-0.019** (0.008)	-0.016** (0.008)
Age	0.011** (0.001)	0.012*** (0.001)	0.011*** (0.001)	0.011*** (0.001)
Male	-0.203*** (0.008)	-0.202*** (0.008)	-0.206*** (0.008)	-0.199*** (0.008)
Audit Results Announced		-0.031*** (0.008)		
Audit Probability Announced X Audit Probability		0.067** (0.028)		
Audit Probability Announced X Lagged Audit		-0.022* (0.014)		
Audit Probability Not Announced X Lagged Audit			0.115*** (0.012)	0.110*** (0.012)
Unofficial Communication			0.045*** (0.008)	
Unofficial Message – Subjects Evaded				-0.036*** (0.005)
Unofficial Message – Subjects Complied				0.042*** (0.007)
Unofficial Message – Subjects Not Audited				-0.009*** (0.004)
Unofficial Message – Subjects Audited				0.029*** (0.005)
Wald statistic	2589.62***	2504.06***	2712.60***	2926.41***
Log-likelihood	-4816.65	-4700.10	-4652.72	-4589.87

[a] The dependent variable is *Compliance Rate*. The data constitute a panel, and estimation is conducted 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 * 0.10, ** 0.05, and *** 0.01.

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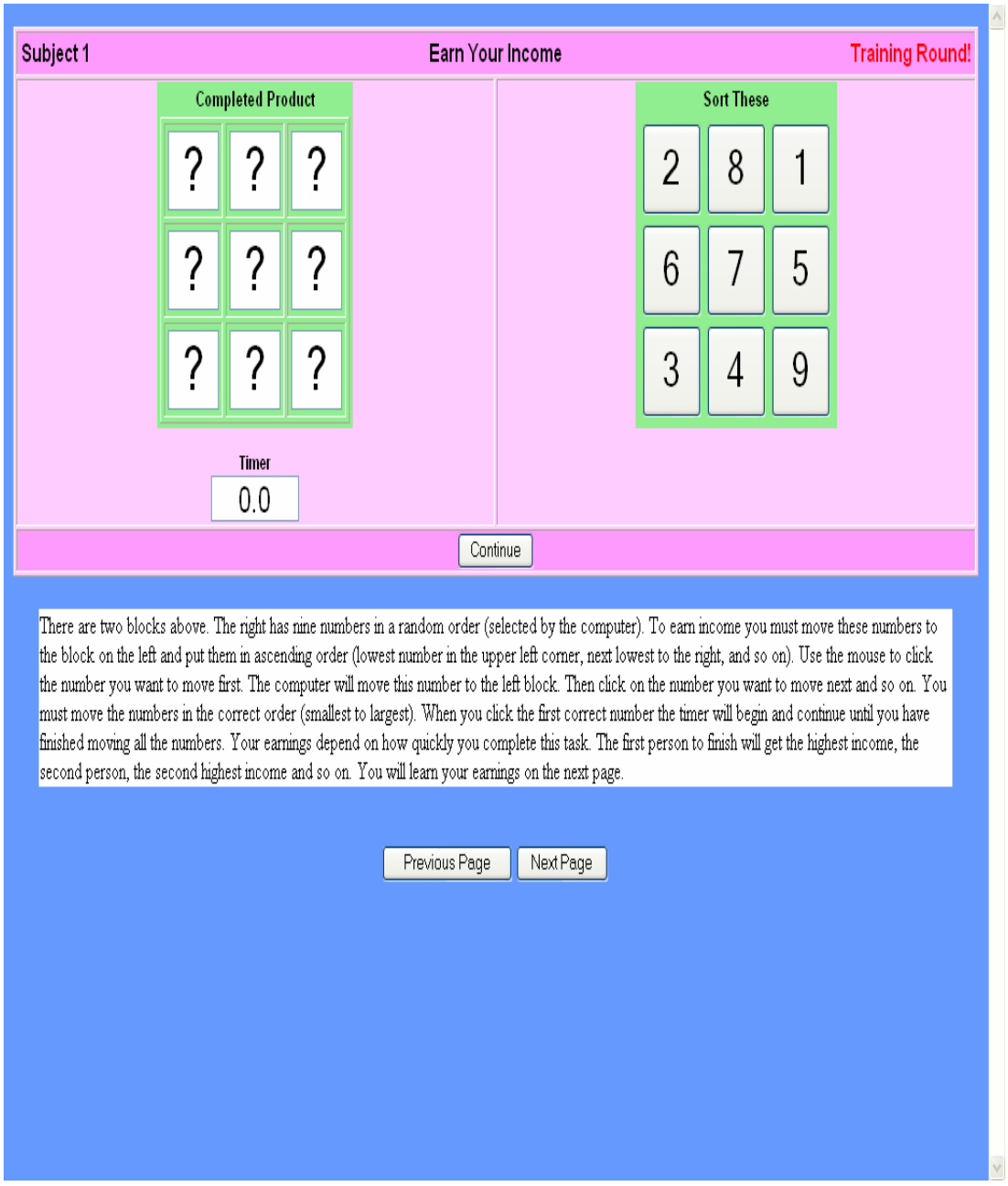
Appendix A: Income Earning Task Image



Appendix B: Tax Form Image