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Abstract:

Tax authorities utilize the audit process, imposing penalties on tax evaders, as their primary means of enforcement. In recent years, a "service" paradigm, whereby tax authorities provide information about correct tax reporting to taxpayers, has shown the potential to further "encourage" correct tax reporting. This research utilizes laboratory experiments to investigate the behavioral dynamics pertaining to information acquisition and tax evasion. The results show that the overall effect of a helpful information service is to decrease tax evasion. Further, an audit has the behavioral effect of lowering information acquisition rates and increasing evasion immediately after experiencing a penalty. This effect persists (although diminishes) in subsequent tax reporting decisions.

Keywords: Tax evasion; Tax compliance; Behavioral Dynamics; Behavioral economics; Experimental economics

PsycINFO classification: 2300; 2900; 3000; 4200

JEL classification: H26; C91

1. Introduction

To "encourage" correct tax reporting it is likely that enforcement effort, audits and penalties, will continue to be a primary tool in the tax authority's arsenal. This approach is based on the basic model of tax evasion which views the taxpayer as engaging in an evasion "gamble" in which the bad state of nature involves the taxpayer being audited and paying a penalty on evaded taxes.[1] However, many tax agencies are exploring complementary instruments of which one is the provision of information and assistance services to taxpayers. This revised paradigm recognizes that tax administrators have a role as facilitators and a provider of services to taxpayer-citizens. And, it opens up the possibility that the audit and the service approaches to enhance tax reporting may be synergistic.[2] Further, the "service" paradigm for tax administration fits squarely with the perspective that emphasizes the role social norms play in tax compliance (Feld and Frey, 2002), and these link directly to the behavioral issues that arise in understanding the dynamic interaction between taxpayers and the tax authority.

Some basic effects of an information service program on tax reporting have been recently examined in Alm et. al, (2010). Using an experimental design that shares some common features with experiments reported here, they find that taxpayers respond positively to service programs. However, Alm et. al., do not report on the dynamic effects of prior audits. We continue in this research direction by implementing a richer design that that allows us to investigate dynamic behavioral effects of tax audits, as well as the effects of varying the quality and cost of the service. Lab data are especially useful here since the experiment allows for control of

[1] This approach derives from the classic "economics of crime" pioneered by Becker (1968) and applied to tax evasion by Allingham and Sandmo (1982).
[2] The value of the taxpayer service derives from the costs imposed on the taxpayer for noncompliance. For the payoff maximizing individual, absent enforcement effort, service that resolves tax liability uncertainty would have no value to the taxpayer. However, a taxpayer wishing to honestly report would value the information since it would enable such honesty.

institutional features and addresses the problem that field data encounter. The audit may not correctly reveal true tax liability but this is explicitly induced in the lab setting and so we know the exact amount of evasion. Finally, while some changes in the service programs have been undertaken in the past, there is not a full spectrum of such programs in existence and so such field data as may exist are incomplete.

Our research utilizes controlled laboratory experiments with human decision makers and salient financial incentives. Within the laboratory, we induce the true tax liability (which is not known with certainty to participants), and then identify the effects of information services (to resolve all or some of the uncertainty) by systematically varying the setting across groups of players. Since audits are random in our design, we are also able to investigate the effects of prior audits and information acquisition on tax reporting over time. This design then permits investigation of behavioral dynamics in two dimensions: tax reporting and information acquisition as well as examining the interaction effects of the service program and the audit program.[3]

Specifically, our design allows us to observe both the tax reporting behavior as well as the propensity of the taxpayer to obtain information by making information acquisition a (sometimes costly) choice. Since we observe tax reporting and information acquisition decisions over several decision periods our design allows the examination of the dynamic effects of prior audits on both the taxpayers' reporting behavior and on their subsequent utilization of the information services. Using these data we test for some previously described behavioral responses to prior audits as well as the propensity to obtain tax information contingent on past audit experience. In previous literature (e.g., Kastlunger et al, 2009; Erard, 1992; Alm, Jackson,

[3] Endogenous or systematic audit rules would make it difficult to undertake this investigation as the behavioral impacts of an audit outcome would be clouded by the institutional change.

and McKee, 1992a, b) the focus has been on the effects of past audits on tax reporting only. Typical findings are that individuals report less income after an audit and various motives have been suggested to explain this dynamic response. However, to our knowledge our experiments represent the first attempt at examining dynamic behavior when a taxpayer service program and an audit program are operating at the same time. Since we can observe the propensity to obtain information as well as the tax reporting decisions, we are able to disentangle some of the motives affecting tax reporting decisions in the periods after an individual has been audited.

We find that our subjects respond to prior audit experience by their tax reporting decisions and also by their decisions to obtain information in the presence of tax liability uncertainty. It is the inclusion of the information services that allow us to tease out the distinction between "loss repair" (Andreoni, Erard, and Feinstein, 1998; Maciejovsky, Kirchler, and Schwarzenberger, 2007) and the "bomb crater effect" (Mittone, 2006) as responses to prior audits. We find greater support for the loss repair hypothesis, as in the period following an audit only those found to be noncompliant are prone to increase (expected) tax evasion. Those audited and found compliant, if anything, actually report a higher tax liability in subsequent rounds. These effects are mirrored in the information acquisition as, for example, those found in violation in the previous period are less likely to acquire information.

2. Conceptual Framework

In order to cleanly identify important effects related to information services, and associated behavioral dynamics, we consider a stylized setting that captures some fundamental features of the personal income tax system while abstracting away from much of its complexities. The setting we consider is one where the taxpayer makes a tax reporting decision –

in particular chooses a tax credit to report – and then files a return to the tax authority. The true tax liability is uncertain, which makes an information service potentially valuable. To motivate compliance, the tax authority undertakes audits with probability p. Audits are completely random and independent of whether other persons are audited or the reported tax liability. If an audit occurs, it perfectly reveals any unpaid taxes. In addition to being liable for unpaid taxes, there is a constant per-dollar penalty $\beta > 0$ assessed on unpaid taxes. No refund is given if taxes are over-paid, and in this sense an audit is never beneficial. The audit process is static in that only the current period tax return is scrutinized and there is no possibility of penalties for (yet undiscovered) past non-compliance nor does a violation lead to a higher future audit probability.

2.1 Basic economic theory

A risk-neutral expected-utility maximizer simply weighs the expected marginal benefits and marginal costs of tax under-reporting. In the special case where tax liability is certain, given the above audit process, the marginal expected costs associated with every dollar of tax under-reporting is constant and equals $p(\beta + 1)$. As such, a corner solution of full compliance (i.e. truthful reporting) arises if $p(\beta + 1) \geq 1$, and otherwise the taxpayer fully evades (i.e. reports the lowest tax liability possible). With uncertain tax liability, interior or corner solutions are possible, and this depends upon the distribution of tax liability values perceived to be probable by the taxpayer.

Turning to information services, we consider a service "helpful" if possible tax liabilities that are viewed as being probable, but in actuality are untruthful, are perceived as having an equal or lower probability of occurring upon receipt of the service. Intuitively, and as shown formally in Vossler, McKee and Jones (2010), a helpful information service: (1) leads the taxpayer to optimally report more truthfully; and (2) has value to the taxpayer.[4] For example, consider the simple case where the taxpayer believes her liability is either $1000 or $2000 with equal probability. With expected costs of under-reporting sufficiently high, this will lead to an optimal report of $2000. However, if her true liability is $1000 and an information service allows her to calculate this, she will then optimally report her true liability and further avoids over-paying taxes. Not surprisingly, the value of the service increases with the "helpfulness" of the service. For example, an information service that reduces more uncertainty has more value. An ancillary implication relevant for our experiment is that taxpayers should be willing to pay more to acquire a more helpful information service.

2.2 Insights from behavioral economics

There are several behavioral responses to the audit process that have been documented in past experiments involving tax reporting decisions and simple random audit enforcement mechanisms. Mittone (2006) finds that, on average, tax compliance drops in the period immediately after an audit. Mittone labels this behavior as the "bomb crater" effect (BCE). Subjects behave *as if* the probability of being audited immediately following a period in which they were audited is significantly lower and therefore perceive the cost of evasion to be low. Mittone also finds that after several filing periods, compliance increases, which he argues is likely due to an increase in the perceived probability of an audit. Another behavioral response to the audit process is known as "loss repair" (Andreoni, Erard, and Feinstein, 1998; Maciejovsky, Kirchler, and Schwarzenberger, 2007). Loss repair is the notion that the penalties that are incurred during the audit process might induce subjects to "want to evade more in the future in

[4] In stating these conditions we are implicitly assuming an interior solution in the absence of information. This is consistent with our experimental design and rules out situations where the information service has a null effect on tax reporting and zero value to the taxpayer.

an attempt to 'get back' at the tax agency" (pp. 844). Therefore, subjects experiencing audits and penalties may try to recover their 'losses' by engaging in tax evasion in future filings.

Information acquisition is unique to our experimental design, but to the extent the above behavioral motivations exist one would expect related effects. In particular, if a taxpayer is motivated to under-report taxes in the period immediately following an audit, the value of the information service (and associated willingness to pay for it) should be significantly lower. Therefore, a result consistent with the BCE would find that information acquisition is lower in the period immediately following an audit. A result consistent with loss repair would find similar effects as the BCE for those who were penalized, but would find little effect on those that were audited and found compliant.

3. Experimental Design

3.1 Decision setting

Our experimental setting implements the fundamental elements of a voluntary reporting system such as applied in the U.S. individual income tax. Participants earn income by performing a task and self-report their tax liability to a tax authority. In the present setting final tax liability is a function of earned income, the tax rate, and tax credits claimed. If an audit occurs unreported taxes are discovered. The audit process performs without error; if the individual has evaded taxes both the unpaid taxes and a penalty are collected.

The participant's earnings for the decision period, which are denominated in "lab dollars", are her earned income less taxes paid (and penalties, if applicable). The overall earnings for the experiment are the sum of the lab dollars earned over all decision periods multiplied by a common (and known) lab to US dollar exchange rate. In each period of the experiment, participants earn income based upon their performance in a simple computerized task, in which they are required to sort numbers into the correct order. Those who finish the task the fastest earn the highest income of 1500 lab dollars for the period, those who finish in the middle of pack earn 1250 lab dollars, and the slowest earn 1000 lab dollars. Participants are presented information about the distribution of group earnings to ensure that they believe the relative nature of the earnings. The earnings task is the only source of interaction and payoff interdependence; this design implements a blind setting among the participants.

After earning income, participants are presented with a screen that informs them of their earnings and the tax policy parameters (tax rate, audit probability, and penalty rate).[5] In each period, the participants decide whether to request an information service (if one is available) and how much to claim in tax credits. Although other institutional details are embedded in the design (e.g. tax rate, taxable income, etc.), and in particular the tax form, the participant can only manipulate her tax liability through her credit reporting choice. As there are penalties for tax underreporting if audited, and foregone earnings associated with over-paying taxes, there is value to resolving any uncertainty regarding the tax credit. The expected tax credit is calculated according to the formula $1000 - 0.5 * (earned\ income)$, such that the expected credits equal 500, 375 and 250 for the three income categories. The amount of the credit is high relative to the initial tax liability so that the credit decision is financially salient.

[5] These are fixed throughout the experiment. Our experimental setting is very contextual and the presence of the income earning task provides, we argue, for the necessary degree of "parallelism" to the naturally occurring world that is crucial to the applicability of experimental results (Smith, 1982; Plott, 1987). The experimental setting need not – and should not – attempt to capture all of the variation in the naturally occurring environment, but it should include the fundamental elements of the naturally occurring world for the results to be relevant in policy debates. In this regard, our experimental design uses tax language (which is presented via the subject interface), requires that the participants earn income in each period, and also requires that the participants disclose tax liabilities in the same manner as in the typical tax form. As in the naturally occurring setting, there is a time limit on the filing of income. A clock at the bottom of the screen reminds the participants of the time remaining, and there is a penalty for failing to file on time set equal in all sessions to 10 percent of taxes owed; also, the individual is automatically audited if he or she fails to file on time, so that the participant pays the non-compliance fine as well.

We implement uncertainty in the credit by placing uniform distributions around the expected credit amounts. The “true” credit is then simply a random draw from this distribution (this draw is independent across decision periods and individuals), and is unknown absent the information service or an audit. In particular, the true credit amount can be anything in the range of the expected credit plus or minus 100%. Based on this implementation, uncertainty – and hence the value of resolving it – increases with the expected credit (or, analogously, decreases in income). With uncertainty, prior to making a credit choice or acquiring information (if possible), each participant sees the supports of the uniform distribution that coincides with her income. If an information service is available, participants can acquire the information with the click of a button.[6]

The participants are informed of the audit probability and the penalty rate, and know these values with certainty. In all sessions we fix the tax rate at 50% of earned income, the audit probability at 30%, and the penalty rate at 300% of unpaid taxes. Our audit rate is much higher than actual *full* audit rates in the United States. However, the IRS conducts a range of audits, and for many types of audits the actual rates are quite high.[7] The penalty rate is consistent with penalties imposed by the IRS for tax underreporting. Enforcement effort is held constant since the effects of enforcement efforts have been widely investigated and we only need this effort to be salient in the current setting to give value to the information that resolves tax liability

[6] Such information reduces the cognitive burden of computing tax liabilities. The issue of tax liability uncertainty differs from enforcement uncertainty. As Alm, Jackson, and McKee (1992b) demonstrate, the tax authority may use enforcement uncertainty to increase compliance. Theory predicts that uncertain penalties increase compliance by risk-averse agents and this is borne out in the data from a set of experiments. Alm and McKee (2006) extend this and report on the compliance effects of informing the taxpayer their return will be audited with certainty.

[7] While overall audit rates are quite low, among certain income and occupation classes they are more frequent. The oft-reported IRS audit rate (currently less than one percent) is somewhat of an understatement. This reported rate usually refers to full audits. In fact, the IRS conducts a wide range of audit-type activities, including line matching and requests for information, and these activities are much more frequent. For example, in 2005 only 1.2 million individual returns (or less than one percent of the 131 million individual returns filed) were actually audited. However, in that year the IRS sent 3.1 million “math error notices” and received from third parties nearly 1.5 billion “information returns”, which are used to verify items reported on individual income tax returns.

uncertainty.[8] Table 1 summarizes the key parameters of the experiment.

Participants are able to revise their credit decision prior to filing their return, and the tax form updates their tax liability as the claimed credit is revised. Thus, they can observe the potential changes in their reported tax liability for each potential reporting strategy they investigate. A timer at the bottom of the tax form counts down the remaining time. The participants are allowed 90 seconds to file and the counter begins to flash when there are fifteen seconds remaining. Thus, the process in the lab mimics that by which a taxpayer may well conduct different calculations in the time prior to actually filing her taxes (whether he or she uses one of the available tax software programs or simply does the tax return by hand). If an information service is available, this can be requested at any time.

The audit selection process is completely random and the participants face the same probability in each period independent of current and past reporting behavior and past audit outcomes. The random audit selection process is illustrated by the use of a "virtual" bingo cage that appears on the computer screen. A box with blue and white bingo balls appears on the screen following the tax filing. The ratio of blue to white balls determines the audit probability. The balls begin to bounce around in the box, and after a brief interval a door opens at the top of the box. If a blue ball exits, the participant is audited; a white ball signifies no audit.

When an audit occurs, the true value of the credit is used to determine taxes owed. The individual's declarations are examined. If the individual has under-reported her tax liability, she must make up for the difference as well as pay a penalty. If an individual has over-reported her tax liability no over payments are returned to the individual.[9] Tax revenues and any penalties

[8] See Alm, Jackson, and McKee, 1992a.

[9] Certain errors on the part of the taxpayer may not be easily verified in the event of an audit. For example, failure to claim a deduction for a charitable contribution because the taxpayer was uncertain of the status (e.g., 501c(3) status) of the organization may not be observed by the tax agency even in the event of an audit.

paid are not distributed to the participants; tax collections are not used to provide a public good in order to ensure that the participants focus on the individual income disclosure decision and not on any public good provision decision. After the tax return is filed and an audit (if any) is determined, participants see one final screen that summarizes everything that happened during the period. After two practice periods to allow subjects to gain familiarity with the interface, the process just described is repeated for a total of 20 paid periods. To minimize potential end-of-game effects the number of periods is not disclosed prior to its realization.

3.2 Treatments

With the exception of the variation in earned income, which is again varies across subjects in a session as determined by a simple task at the beginning of each decision period, we employ a between-subjects design. The main treatment variables (varied across sessions only) are the presence/absence of an information service, the quality of the service if provided, and the cost of obtaining the information. These factors are held constant throughout a session. There are five basic treatments (see Table 2). The first (T1) is a treatment with certain tax liability, which we use as a baseline for comparison against uncertain information treatment. In this treatment, participants are automatically given information on their true credit and there is no notion of an information service. In the second treatment (T2), the individual's tax credit is uncertain and there is no information service available. This establishes a second baseline for comparison. In the remaining three treatments, there is an information service available. The status quo in the information service treatments, i.e. if the information service is not utilized, is identical to the uncertainty baseline.

The "perfect" information service reveals the true credit with certainty (T3). Under the other two information service types, the service is imperfect in the sense that up to two possible credit amounts can be provided and each amount has a 50% chance of being correct. Specifically, under the "simultaneous" information service treatment (T4) the authority simultaneously provides two credit amounts, one of which is the truth while the other is a decoy. With the "sequential" information service (T5), the participant can make up to two information requests and with each request is delivered one possible credit amount. If two requests are made, then the simultaneous and sequential services reveal the same information. However, the sequential information treatment leaves the possibility that only one credit amount is delivered, in which case it still has the same 50% chance of being the truth.

To assess the value of information services, we vary the cost to acquire information in the information service treatments (see Table 2). The three cost levels are 0, 50 and 100 lab dollars for the perfect and the simultaneous information settings. For the sequential setting, these costs are halved and assessed separately for the two sources.

3.3 Participants and procedures

The experiments were conducted at dedicated experimental laboratories at the University of Tennessee and Appalachian State University, which both utilized the same software and experimental protocol, and have similar computer networks. The participant pools included students and non-students (university staff, mostly).[10] Student and non-students participated at separate times, and the lone difference in student versus non-student sessions is that the latter utilized a lower lab dollar to US dollar exchange rate (375 to 1 versus 750 to 1) in order to reflect the higher opportunity cost of participation. Recruiting was conducted using the Online

[10] An individual session included only students or non-student participants – they were not mixed in a session.

Recruiting System for Experimental Economics (ORSEE) developed by Greiner (2004). Databases of potential participants were built using announcements sent via email to university students and staff. Registered individuals were contacted, via email, and were permitted to participate in only one tax experiment.[11] Only participants recruited specifically for a session were allowed to participate, and no participant had prior experience in this experimental setting. Methods adhere to all guidelines concerning the ethical treatment of human participants. Earnings averaged $25 for student participants and $45 for non-students. Sessions lasted between 60 and 90 minutes. A total of 730 participants took part in these sessions.

The experiment session proceeded in the following fashion. Each participant sits at a computer located in a cubicle, and is not allowed to communicate with other participants. The instructions are conveyed by a series of computer screens that the participants read at their own pace, with a printed summary sheet provided and read aloud by the experimenter. (Appendix A provides representative screenshots from the experiment and Appendix B provides instructions from one of the treatments.) Clarification questions are addressed after the participants have completed the instructions and two practice periods. The participants are informed that all decisions will be private; the experimenter is unable to observe the decisions, and the experimenter does not move about the room once the session starts to emphasize the fact that the experimenter is not observing the participants' compliance decisions. This reduces, to the extent possible, peer and experimenter effects that could affect the decisions of the participants. All actions that participants take are made on their computer. After the 20 paid decision periods, participants are asked to fill out a brief questionnaire, which collects basic demographics

[11] Other experimental projects were ongoing at the time and participants may have participated in other types of experiments.

including information on tax reporting experience. Payments are made privately at the end of the session.

4. Testable hypotheses

4.1 Basic economic hypotheses

With our chosen audit probability and penalty rate, when the true credit is known with certainty it follows that the expected cost of under-reporting by one lab dollar equals $0.3(3 + 1) = 1.2$ lab dollars such that it is optimal to report truthfully.[12] When the credit is uncertain, based on the assumed uniform distributions, the taxpayer will optimally evade through over-claiming the credit. The extent of the deviation from truthful reporting increases with the level of uncertainty. As those with the lowest income have the widest uncertainty range, theory suggests the highest relative amount of tax evasion for these individuals. Point predictions from the basic theory have that it is optimal to evade by 333 lab dollars for those with earned income of 1000, by 250 for those with an income of 1250, and by 167 for those with an income of 1500.

Tying in the imperfect information service, when the uncertainty is reduced to two possible credit outcomes, the optimal decision is to report one of the two possible amounts. With our chosen parameters it is optimal to choose the higher of the two amounts. Intuitively, it is not optimal to choose something in the middle of the two amounts as, in expectation, you forego an allowable credit and pay a larger penalty. Theoretically, although we forego the derivations here, compliance is actually *worse* with two information sources than in the case of no information (i.e. the base uncertainty situation). This is a general theoretical result that does not depend on the particular parameters chosen for the experiment. Intuitively this is driven by the fact that

[12] We note that in Alm, et al. (2010) the expected cost is much less than 1, and the optimal strategy in that experiment (all treatments) is a corner solution of maximal evasion.

under full uncertainty one's decision is driven by the expected value of the underlying credit distribution – which is the true credit – whereas the higher (lower) of the two draws is away from the truth on average.

The decision of whether to request the information service(s) to resolve uncertainty (at least partially) is driven by the value of information. Theoretically, and quite intuitively, the taxpayer's willingness-to-pay (WTP) is increasing in the initial level of uncertainty as well as the accuracy of the information. In the context of the experimental design, those with lower incomes face a larger range of uncertainty and, *ceteris paribus*, have a higher WTP for information. Further, knowing the true credit is more valuable than receiving two possible amounts only one of which is correct. In terms of point predictions, since information has value, in all situations information should be requested when it is free. At the other extreme, in all situations no information should be requested at our highest cost amount of $100 (or $50 for one imperfect information source). At the middle cost amount, those at the lowest income level should request the information (imperfect or perfect), at the middle income level it is beneficial to request perfect information, and it is not beneficial for those with high income to request information.

The basic economic (null) hypotheses that can be evaluated with our experiment are summarized as follows:

Hypothesis 1. The cost of the information service has no effect on the propensity to acquire information.

Hypothesis 2. Uncertainty has no effect on the propensity to acquire information (i.e. income does not affect the propensity to acquire information).

Hypothesis 3. Information services have no effect on tax evasion.

It is expected that all hypotheses will be rejected, and in particular directions as suggested by the above discussion.

4.2 Testable behavioral hypotheses

The instructions and information provided to the experiment subjects is explicit about the fixed audit probability, the purely random selection process, and independence over periods. Therefore, economic theory would predict that the amount of tax credit claimed by subjects will be independent of their audit history. Given our experimental design, we can test for the BCE and loss repair effects in the absence of other confounds that may exist in naturally occurring settings (e.g. forward or backward audit). These two effects can in particular be identified by comparing pre- and post-audit credit reporting decisions. A basis from which to distinguish between the two competing theories arises as there is predicted to be a difference between those who were audited and found to be compliant, and those who were audited and found to be in violation and were therefore penalized. If compliant taxpayers do not evade more in the period immediately following a period where they were audited, but penalized violators do, then those results would more favorable comport with loss repair than with BCE.

Given the immediate response (if any) to being audited, the persistence of the effect is also of interest. Theoretically, there should be no immediate response to being audited, and therefore its effects would not persist. The main testable (null) hypothesis related to behavioral conjectures are summarized below:

Hypothesis 4. An audit has no immediate effects on the level of tax evasion or the propensity to acquire information.

Hypothesis 5. An audit has no lasting effects on the level of tax evasion or the propensity to acquire information.

5. Results

In the analysis that follows, we first estimate linear regressions to provide a snapshot of the basic treatment effects regarding uncertainty and information services on the tax reporting and information acquisition decisions. These models are presented in Table 4 and 5, respectively. Then, we add additional structure and variables to the models which allows us to focus on dynamic behavior. These models are presented in Table 6 and 7. To control for possible heteroskedasticity and autocorrelation of unknown form in the regressions, we use robust standard errors with clustering at the participant-level. Further, heteroskedasticity and autocorrelation robust *t* and *F* statistics are used when evaluating hypotheses. Table 3 provides a description and summary statistics for key variables used in these models.

For the tax reporting regressions we use as the dependent variable the level of expected tax evasion. As every dollar taken as a credit reduces taxes paid (pre-audit) by one dollar, evasion is calculated as the reported credit less the "true" credit. For cases of uncertainty where no information is acquired, the true credit is an expectation and simply calculated as the midpoint of the range of possible actual credit amounts shown to the participant. When two possible credit amounts are acquired through the information service, the average of the two is the expected true credit. Finally, in the simultaneous information treatment where only one piece of information is acquired, the expected credit is the average of the original uniform distribution and the single possible credit draw. For the information acquisition regressions, we use as the dependent variable a binary indicator variable where a value of 1 denotes acquisition of the service (i.e. we estimate linear probability models). [13]

[13] These formulations are consistent with the theory, which is also from the perspective of the taxpayer. However, since the information services are unbiased, and given a large number of random credit draws are accumulated over participants and rounds, if we instead use the (ex post) actual level of tax evasion as the dependent variable this leads to very trivial differences in results.

Finally, for the tax reporting models we estimate the treatment effects and behavioral dynamics separately by specific experiment "conditions" as defined by treatment and information interactions. The first two, "Certainty Baseline" and "Uncertainty Baseline", simply correspond with all observations from T1 and T2, respectively. The third, "No Information", includes observations from information service treatments where information was not acquired. The remaining three correspond to observations where information was acquired: "Perfect Information" is associated with T3; "Two Information Sources" is associated with T4, and those in T5 who sequentially requested information from both sources; and "One Information Source" is associated with T5 for those who obtained one of the two available services.

5.1 Basic analysis of treatment effects

Our analysis first investigates the basic treatment effects identifiable through the experimental design. Tax Reporting Model I (Table 4) estimates the mean levels of tax evasion by experiment condition and income level. Information Acquisition Model I (Table 5) estimates the mean information request probabilities for each unique income and information cost combination, separately by treatment.

Starting with evidence on tax evasion, one prominent effect is that the level of tax evasion is actually *increasing* in income. For the Certainty Baseline and Perfect Information conditions, this effect is not consistent with theory, which predicts zero tax evasion for all income levels. For the other conditions, subjects face uncertainty and theory predicts that evasion is decreasing in income as in our design the level of uncertainty is inversely related to income. Thus, overall, unobserved behavioral factors tied to income appear to be an important driver of the tax reporting decisions and, under uncertainty, are strong enough to counteract the economic net benefits of reporting truthfully. Our finding is not altogether unexpected, however, as it is consistent with earlier findings in a related experiment (Vossler, McKee and Jones, 2010).

A second prominent effect is that those receiving information services have the lowest levels of evasion. In fact, those receiving information evade roughly 80%, 70% and 60% less, across the respective income levels, as compared to those in the uncertainty baseline. The average difference in evasion is statistically significant beyond the 5% level between any of the three information service conditions and the certainty baseline, uncertainty baseline or no information condition. Also evident from this model is that participants in the information service treatments who do not acquire information (i.e. the No Information subgroup) tend to have reasonably high levels of evasion. However, evasion for this subgroup is statistically different from, and overall lower than, evasion in the certainty baseline ($F=4.88$; $p<0.01$) or the uncertainty baseline ($F=6.20$; $p<0.01$) for this subgroup. Overall, as those who do not acquire information evade at similar levels to those in the uncertainty baseline, and those who receive information evade less, this suggests that there is an on overall effect that the presence of the information service decreases evasion.[14] We summarize the results based on our analysis of the tax reporting decisions succinctly below:

Result 1. Tax evasion increases with income.

Result 2. Those who acquire information evade less than those who do not.

Turning to information acquisition, the model shows a clear pattern where information requests are increasing with income and decreasing with cost. In particular, pooling across treatments, when the information cost is 100, information acquisition rates are statistically

[14] We also estimated an alternative version of this model where effects are allowed to vary by treatment rather than by condition. The treatment-specific result posited here can be shown statistically based on this model.

different, and lower, than when the information cost is 50 (F=3.62, p<0.01), and when the information cost is 50, information acquisition rates are statistically different, and lower, from when the information cost is 0 (F=13.72, p<0.01). The difference in information acquisition rates between the 1250 and 1500 income levels is statistically different, and higher for the latter (F=2.92, p<0.01). The overall difference in acquisition rates is higher for the 1250 versus 1000 income groups, but this difference is not significant at conventional levels (F = 1.52, p =0.14).

The negative effect of cost on information acquisition is of course consistent with theory, whereas the positive income effect is not. Recall that the credit is a function of income, and the uncertainty range (+/- 100% of expected credit) is decreasing with income. Thus, those at the low income of 1000 theoretically have a higher WTP for information. This effect is simply not borne out in the data. One possibility may be that participants decided whether they can "afford" the information and based this entirely on how well they fared from the income earnings task. Another possibility is that participants were motivated by relative earnings, and as such low income participants may have been compelled to "keep up with the Joneses" by not paying for information. Overall, Hypothesis 1 and Hypotheses 2 are rejected, but theory only supports the directional results from the rejection of Hypothesis 1. We summarize our main findings on the information acquisition decision below:

> **Result 3**. The propensity to acquire information increases with income and decreases with information cost.

5.2 Behavioral Dynamics

We now investigate behavioral dynamics with Tax Evasion Model II (Table 6) and Information Acquisition Model II (Table 7). In particular, the variables used are constructed to test for post-audit behavioral dynamics, and the model also controls for subjective probabilities and the basic treatment effects identified previously. The variables "Violation Last Period" and "Compliant Last Period" are binary variables that indicate whether the subject was audited in the previous period and whether she was found to be in violation (and therefore paid a penalty) or compliant (and therefore did not pay a penalty). The "Persistence" variables measure the lasting effects of these two different scenarios and are the inverse of the number of rounds that have passed since the most recent "Compliant" or "Violation" audit. To parse between the immediate and lasting effects of the audits, the "Persistence" variables are equal to zero in the rounds immediately following an audit.[15] The "Subjective Probability" variable was constructed using the number of audits in prior rounds divided by the number of prior rounds.[16]

First, note that the basic treatment effects identified in the simpler evasion and information models continue to persist when controlling for dynamics: evasion increases with income and information acquisition increases with income and decreases with cost. Next, focusing on the tax evasion analysis, for all experimental conditions (less the One Information Source condition[17]) there is a significant and positive effect corresponding to being audited and penalized in the previous period. The persistence of this effect is also significant and positive under the same conditions. As an illustration, those who report taxes without information report on average 249.42 less in taxes after a penalizing audit. The persistence of this effect is rather strong, as in the second period following a penalizing audit tax evasion is 469.56/2 or 234.8. By the tenth period after a penalizing audit, its effect on evasion is 469.56/10 or 47.0 lab dollars.

[15] These "Persistence" variables are deliberately constructed so that the (absolute) effect of a past audit declines over time. This is consistent with more general regression specifications that estimate the separate period-by-period effects of past audit outcomes.
[16] To avoid having to omit period 1 observations from the analysis, we use information from the training rounds to construct these variables for period 1.
[17] The insignificance of these coefficients under the "One Information Source" condition may possibly be attributed to the low number of observations for this condition (n=397).

The effects of being audited and found compliant in the previous period are largely insignificant. The only significant coefficients for "Compliant Last Period" are under the No Information and Two Information Sources conditions; the same is true for "Compliant Persistence" with the addition of the Certainty Baseline condition. In all situations where the compliance related estimates are significant, there effects are *negative*, which is in opposition to the violation estimates. Thus, the overall effects lend support to the loss repair hypothesis over the BCE. Indeed, under the BCE we would have expected strong and positive effects of past audits on subsequent evasion (regardless of the outcome). In each condition, "Compliant Last Period" is statistically different from "Violation Last Period" and "Compliant Persistence" is statistically different from "Violation Persistence"[18]. We summarize the results based on our analysis of the reporting decision succinctly below:

> **Result 4.** Being audited and penalized increases tax evasion in the period immediately following the audit.
>
> **Result 5.** The increase in tax evasion is persistent for those that have been audited and penalized.

Turning to the information acquisition analysis, the coefficients on "Violation Last Period" and "Violation Persistence" indicate a statistically significant decrease in the propensity to acquire information for periods that immediately follow an audit where the subject was penalized, and that this effect is persistent. This is true for all three information Treatments, and is behaviorally consistent with the tax reporting findings given that, if one plans to evade anyway, then the information has less value. On a related note, some otherwise prone to request the information service may deliberately not do so as a means of justifying their evasion through a veil of ignorance (i.e. I do not know *ex ante* that I am under-reporting my true liability). The

[18] With the acceptation of the "One Source of Information" condition (F=1.39, p=0.25)

coefficients on "Compliant Last Period" in Information Acquisition Model II are not significant, which is also true for "Compliant Persistence", accept for T3. This means that in the period immediately following an audit where the subject was not penalized, there is not any significant effect on the propensity to acquire information. We summarize our behavioral findings on the information acquisition decision in our following results:

> **Result 6.** Being audited and penalized lowers the propensity to acquire information in the period immediately following the audit.

> **Result 7.** The lower propensity to acquire information is persistent for those that have been audited and penalized.

Jointly, Results 4 and 6 reject Hypothesis 4 and Results 5 and 7 are counter to Hypothesis 5. The results of both tax reporting and information decisions are complementary and provide evidence of loss repair and persistent behavioral effects after an audit where subjects are penalized.

6. Concluding Remarks

Our most basic finding is that, as predicted by economic theory, the provision of information – even when the quality is low – significantly increases tax compliance. Although we have not investigated subject pool effects for these treatments, other work using data from similar experimental settings suggests that observed behavior is broadly consistent across pools (Alm, Bloomquist, and McKee, 2011).[19]

Further research is being undertaken with the data reported in this paper to investigate the decision to acquire information as well as the factors affecting the propensity to take a second

[19] Further, as noted above, Alm, Bloomquist, and McKee (2011) demonstrate the external validity of the experimental setting through a series of comparisons with field data results. This effectively addresses the criticisms of some who have questioned the use of lab experiments in tax compliance research (see Gravelle, 2008 (commenting on Alm et.al., 2008); Cadsby, Maynes, and Trivedi, 2006). Recall, for the current experiments we have conducted sessions at 2 institutions and with 2 pools (students and non-students) at each. Thus we have several ways the pool effects could be analyzed.

"draw" in the sequential information setting. When the tax system is complex taxpayers are predicted to respond positively to the provision of information services that reduce the costs of computing true tax liabilities. The results reported here demonstrate that, first, when information services are provided the level of underreporting is lowered, and second, that the aggregate level of underreporting is lowered even when only a fraction (58%) of the participants avail themselves of the information service. The experimental setting does not incorporate a cost of the service to the tax agency but the improved underreporting behavior suggests a potential for a positive return from this service. As a final observation here, the participants respond to the costs of the service in a predictable fashion. While the "costs" in the experimental setting are monetary, we would expect a similar response to higher costs in the form of transaction costs, including waiting time. These are topics of our ongoing research.

We observe dynamic responses – current tax reporting is affected by prior experience with the audit process. Our participants follow up past audits with behavior consistent with loss repair. By including the opportunity to obtain tax liability information when these liabilities are uncertain, we are able to incorporate this behavior into the responses to prior audits and we find that taxpayers who are penalized by the audit are less likely to obtain information in subsequent periods, behavior that is consistent with the loss repair hypothesis. That is, these taxpayers wish to evade to recovery prior losses and have no desire to be informed of their true tax liabilities in the present period. This suggests an information program that directly informs those who were audited, rather than waiting for the information request from the taxpayer, may be a good policy option.

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Table 1. Experiment Parameters

Parameter / variable	Value(s)
Earned Income	1000, 1250 or 1500 lab dollars
Audit Probability (p)	30%
Penalty Rate (β)	300% on unpaid taxes
Tax rate	50% on Earned Income
Tax Credit	Expected value: 1000 – (0.5×Earned Income) Range: +/- 100% of expected value

Table 2. Experiment Treatments

Tax Liability Uncertain	Service Provided?			
	No	One Source (Complete and Correct)	Two Simultaneous Sources (One Correct)	Two Sequential Sources (One Correct)
No	T1	N/A	N/A	N/A
Yes	T2	T3 Price of Information: $0, $50, $100	T4 Price of Information: $0, $50, $100	T5 Price of Information: $0, $50, $100

Table 3. Data Description

Variable Name	Description	Sample Mean	(std. dev.)
Tax Evasion	Difference between credit claimed and (expected) actual credit	167.98	(319.90)
Information Acquisition	=1 if information service acquired; =0 otherwise	0.58	(0.49)
Income	Income from the income earnings task. Takes on values of 1000, 1250, or 1500	1271.78	(197.13)
Cost	Cost of information service, in lab dollars. Takes on values of 0, 50 or 100	46.02	(41.58)
Violation Last Period	=1 if subject was audited in the previous period and penalized; =0 otherwise	0.17	(0.37)
Compliant Last Period	=1 if subject was audited in the previous period and not penalized; =0 otherwise	0.13	(0.34)
Violation Persistence	The inverse of the number of rounds since the last audit where a penalty was incurred; equals zero in period immediately after audit	0.11	(0.18)
Compliant Persistence	The inverse of the number of periods since the last audit that did not result in a penalty; equals zero in period immediately after audit	0.09	(0.17)
Subjective Probability	The number of times the subject has been audited in past periods divided by the number of past periods.	0.26	(0.14)

Note: the descriptive statistics for Cost and Information Acquisition are computed for Treatments 3 – 5 only.

Table 4. Tax Reporting Model I

Dependent Variable: Tax Evasion						
	Experiment Condition					
	Certainty Baseline (N=1,620)	Uncertainty Baseline (N=1,520)	No Information (N=4,798)	Perfect Information (N=2,029)	Two Information Sources (N=4,096)	One Information Source (N=397)
Income=1000	160.36*** (31.38)	219.28*** (29.61)	76.67*** (24.83)	46.79** (22.45)	45.21*** (16.75)	22.82 (36.57)
Income=1250	186.97*** (30.52)	260.38*** (27.79)	218.84*** (19.07)	72.20*** (20.10)	98.21*** (12.59)	91.50*** (27.16)
Income=1500	257.32*** (32.32)	310.31*** (35.37)	363.36*** (21.01)	111.39*** (17.52)	143.04*** (14.43)	117.73*** (44.99)
N=14,454						
R^2=0.29						
F=34.98						

Notes: * , ** and *** denotes estimates that are statistically different from zero at the 10% , 5% and 1% significance levels, respectively. Cluster-robust standard errors are in parentheses.

Table 5. Information Acquisition Model I

Dependent Variable: Information Acquisition (=1 if information acquired; =0 otherwise)			
	Experiment Treatment		
Parameter setting	Perfect Info Available (T3)	Simultaneous Info Available (T4)	Sequential Info Available (T5)
Income = 1000; Cost = 0	0.76*** (0.05)	0.84*** (0.05)	0.89*** (0.03)
Income = 1000; Cost = 50	0.33*** (0.07)	0.42*** (0.06)	0.44*** (0.08)
Income = 1000; Cost = 100	0.20*** (0.06)	0.20*** (0.05)	0.40*** (0.07)
Income = 1250; Cost = 0	0.77*** (0.05)	0.83*** (0.04)	0.90*** (0.03)
Income = 1250; Cost = 50	0.39*** (0.06)	0.56*** (0.05)	0.45*** (0.06)
Income = 1250; Cost = 100	0.24*** (0.05)	0.30*** (0.05)	0.44*** (0.05)
Income = 1500; Cost = 0	0.90*** (0.03)	0.86*** (0.04)	0.90*** (0.04)
Income = 1500; Cost = 50	0.56*** (0.06)	0.60*** (0.06)	0.60*** (0.06)
Income = 1500; Cost = 100	0.30*** (0.06)	0.30*** (0.06)	0.41*** (0.07)
	N=11,320 R^2=0.68 F=116.50		

Notes: * , ** and *** denotes estimates that are statistically different from zero at the 10% , 5% and 1% significance levels, respectively. Cluster-robust standard errors are in parentheses.

Table 6. Tax Reporting Model II

Dependent Variable: Tax Evasion

	Experiment Condition					
Explanatory variable	Certainty Baseline (N=1,620)	Uncertainty Baseline (N=1,520)	No Information (N=4,798)	Perfect Information (N=2,029)	Two Information Sources (N=4,096)	One Information Source (N=397)
Intercept	-109.34 (96.08)	83.91 (98.30)	-332.26*** (68.24)	-72.90 (64.56)	-87.03* (50.05)	-99.01 (134.93)
Income (in 1000s)	0.22*** (0.07)	0.20*** (0.07)	0.50*** (0.04)	0.15*** (0.04)	0.18*** (0.04)	0.21** (0.11)
Cost	N/A	N/A	-0.61* (0.36)	-1.11*** (0.34)	-0.77*** (0.22)	-1.09 (1.53)
Violation Last Period	177.55*** (41.07)	130.70*** (42.48)	249.42*** (26.64)	177.31*** (33.20)	68.12*** (25.88)	109.96 (67.78)
Compliant Last Period	-45.00 (46.46)	16.26 (52.42)	-134.98*** (32.63)	44.26 (28.71)	-58.56** (26.40)	13.63 (74.10)
Violation Persistence	296.30*** (85.25)	163.66* (89.51)	469.56*** (54.84)	365.60*** (73.05)	160.16*** (50.37)	241.70 (168.12)
Compliant Persistence	- 273.92*** (102.40)	-143.84 (107.49)	-363.81*** (70.29)	-3.59 (60.82)	-165.11*** (58.70)	69.52 (130.79)
Subjective Probability	-6.07 (166.97)	-406.51** (166.96)	-420.39*** (89.72)	-245.61*** (86.44)	-64.94 (79.09)	-362.45* (198.67)

N=14,454
R^2=0.38
F=33.19

Notes: * , ** and *** denotes estimates that are statistically different from zero at the 10% , 5% and 1% significance levels, respectively. Cluster-robust standard errors are in parentheses.

Table 7. Information Acquisition Model II

Dependent Variable: Information Acquisition (=1 if information acquired; =0 otherwise)			
	Experiment Treatment		
Explanatory variable	Perfect Info Available (T3)	Simultaneous Info Available (T4)	Sequential Info Available (T5)
Intercept	0.28***	0.61***	0.70***
	(0.11)	(0.11)	(0.11)
Income (in 1000s)	0.30***	0.21***	0.12
	(0.07)	(0.08)	(0.08)
Information Cost	-0.55***	-0.56***	-0.49***
	(0.06)	(0.05)	(0.05)
Violation Last Period	-0.17***	-0.15***	-0.17***
	(0.04)	(0.04)	(0.04)
Compliant Last Period	0.05	-0.02	-0.02
	(0.04)	(0.04)	(0.04)
Violation Persistence	-0.34***	-0.31***	-0.31***
	(0.09)	(0.09)	(0.09)
Compliant Persistence	0.26***	0.02	0.01
	(0.09)	(0.10)	(0.10)
Subjective Probability	0.51***	0.03	0.24
	(0.16)	(0.18)	(0.16)
N=11,320 R^2=0.68 F=143.18			

Notes: * , ** and *** denotes estimates that are statistically different from zero at the 10% , 5% and 1% significance levels, respectively. Cluster-robust standard errors are in parentheses.

Appendix A. Selected Experiment Screenshots (Treatment 3, Cost of $50)

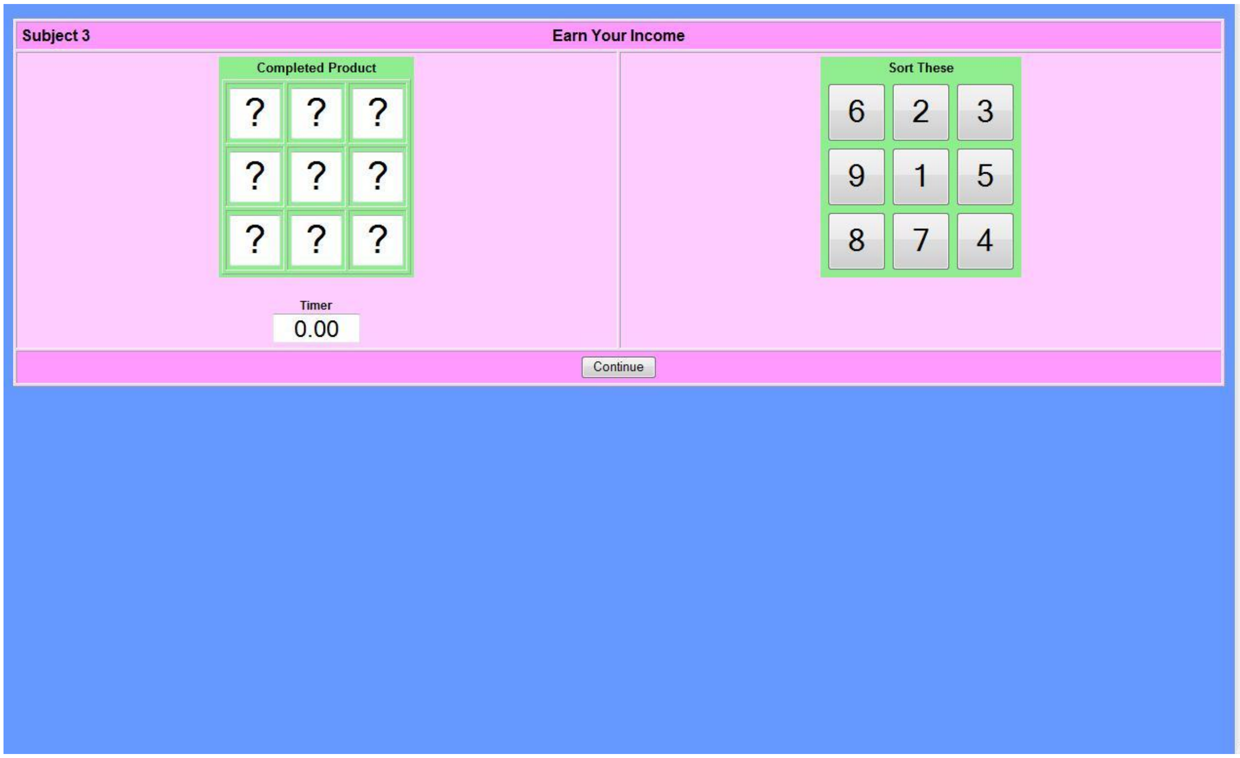

Figure A1. Income earnings task

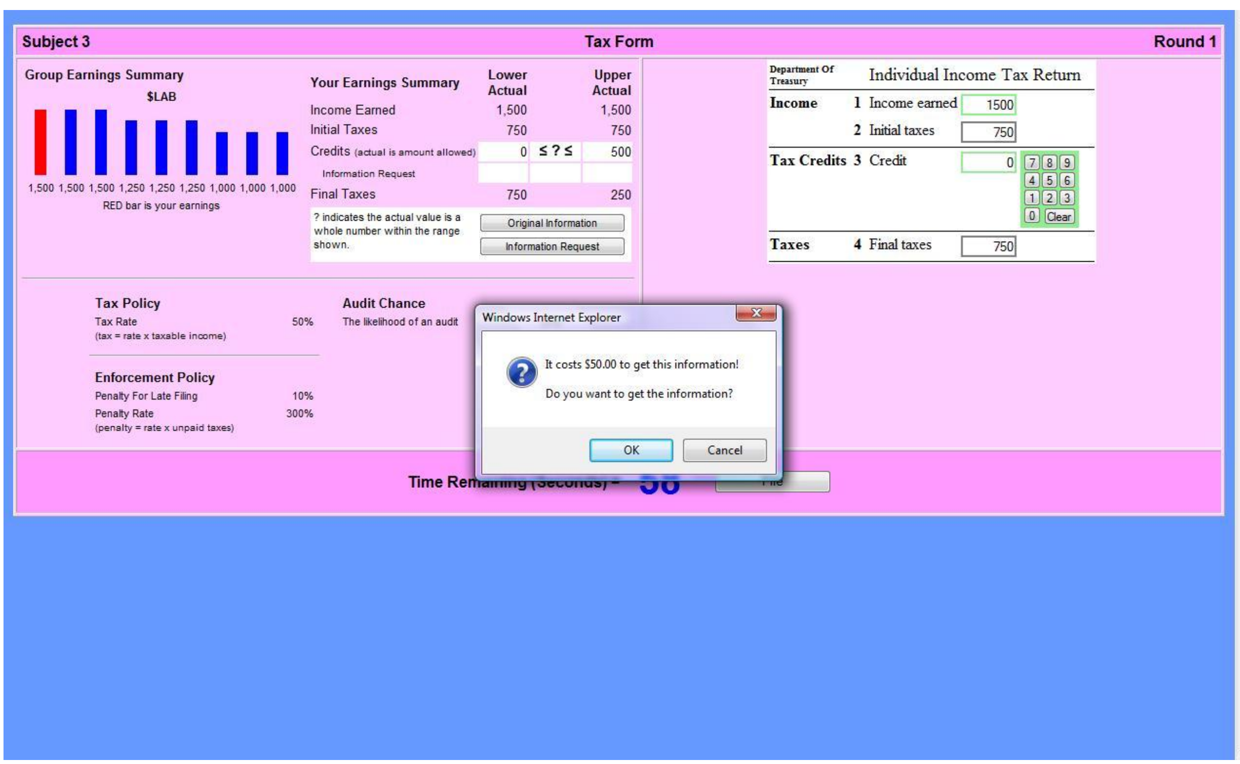

Figure A2. Treatment 3, Tax decision screen, information requested

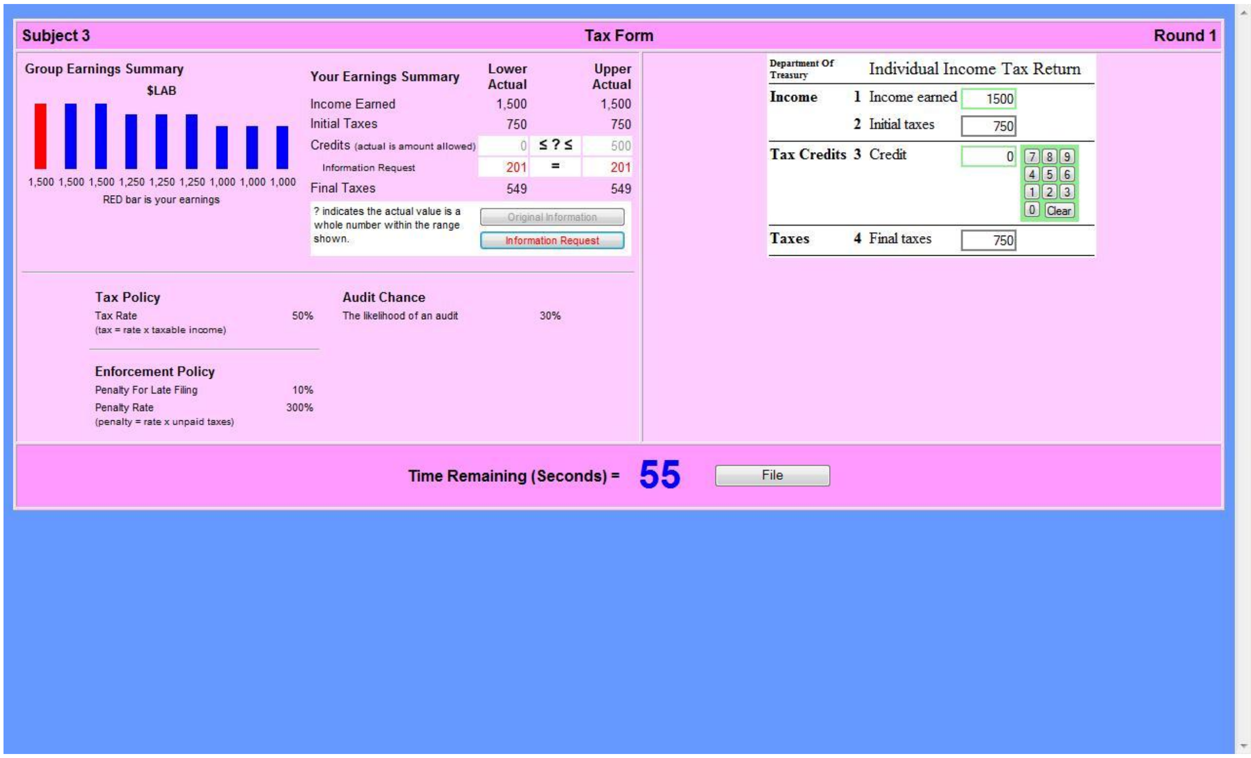

Figure A3. Treatment 3, Tax decision screen, after information acquired

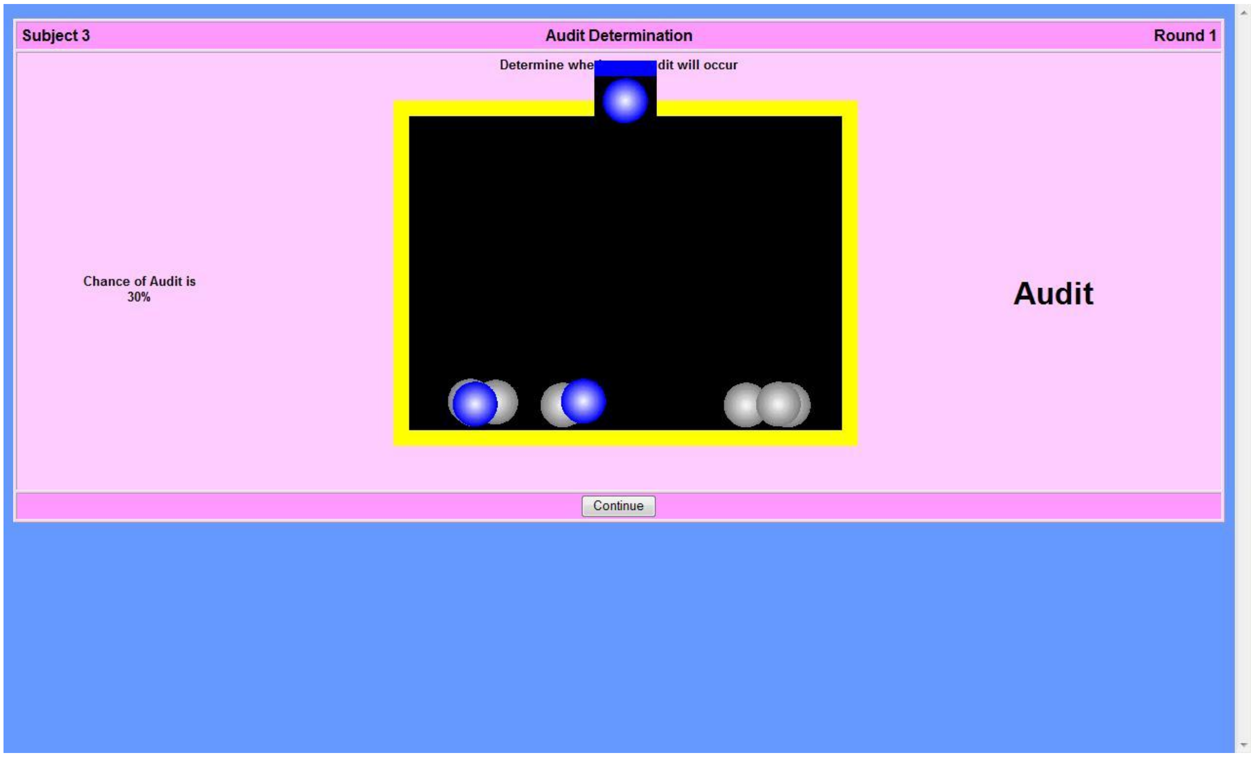

Figure A4. Audit selection process

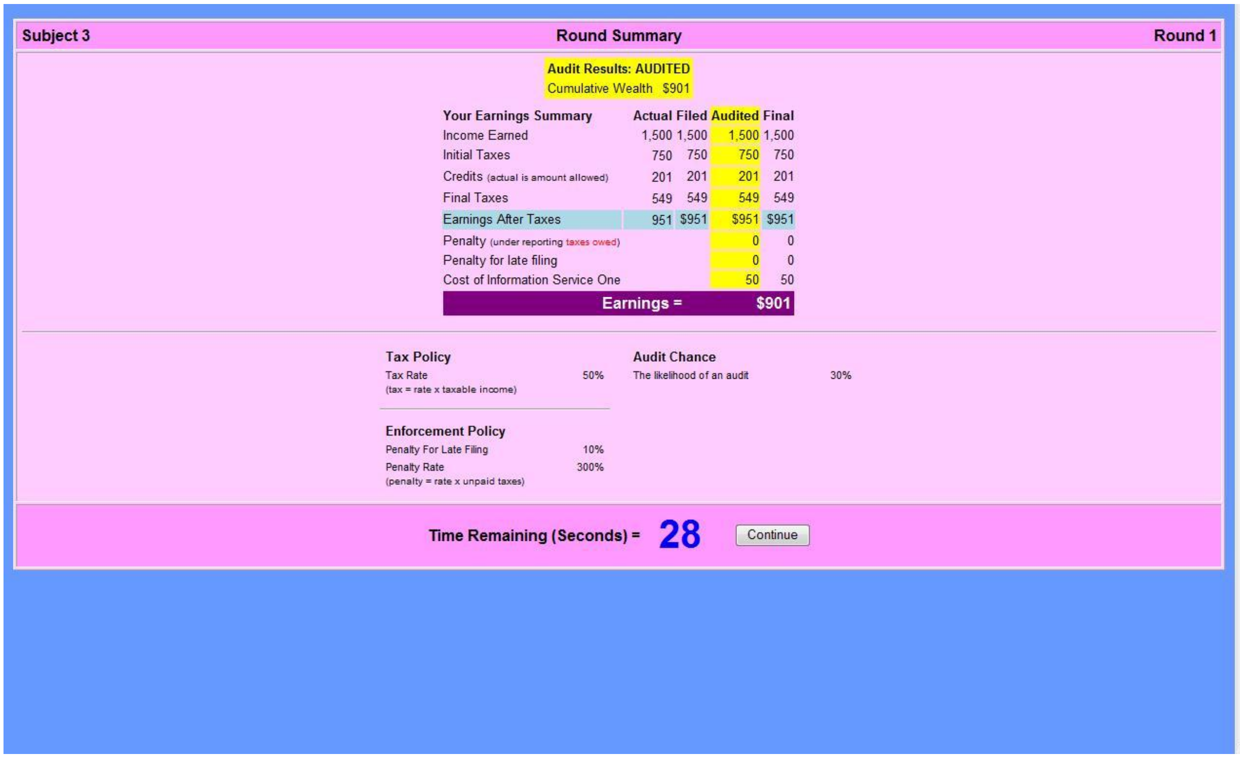

Figure A5. Results screen

Appendix B. Example Experiment Summary Sheet (Treatment 3, Cost of $50)

Experiment Overview

- You will be participating in a market simulation that lasts several decision "rounds". In each round, you first play an earnings game and then face a tax reporting decision.

- In the earnings game, you sort the numbers 1 through 9. Your Income earned is determined by how fast you sort the numbers relative to others. The participant in your group with the fastest time receives the highest Income earned.

- In the tax reporting stage, you fill out and file a tax form. How much you earn from the tax reporting decision depends on how much you claim in Tax Credit and whether or not you are audited. Note that the on-screen instructions do not specify the tax policy parameters (e.g. tax rate, penalty rate, etc.), but those specified below will be in effect for this experiment.

- Each round is completely independent from the others, which means your decisions in one round in no way affect the outcome of any other round.

How your earnings are determined each round

- On the tax form, your Initial Taxes will be calculated automatically. This amount is determined by multiplying your Income earned by a tax rate of 50%.

- You decide how much to claim in Tax Credit on the tax form. Each dollar you claim in credits reduces your Final taxes by one dollar. This amount is subtracted from the Initial Taxes to determine your Final Taxes. If Final Taxes is a negative number, this reflects a tax refund.

- You will be shown a range of tax credits (this range is highlighted in white on the left side of the decision screen), which depends on your Income earned. Each amount within the range has an equal chance of being your actual tax credit, which is the highest amount you can claim without possible penalty. You can choose to claim any amount between 0 and 1000.

- You have an information service available to you at a cost of **$50**. By clicking on the "Request Information" button you will know the *exact* amount of your actual tax credit.

- You have a 30% chance of being audited. Audits are determined completely at random and do not depend on how much you or anyone else claims in tax credits.

- If you are not audited, your earnings for the round are your Income earned minus Final taxes.

- If you are audited, but claimed *less* than or *equal* to the actual tax credit, your earnings for the round are your Income earned minus Final taxes. Know that if you under-reported the credit you will not receive additional money through the audit process.

- If you are audited, and claimed *more* than the actual tax credit, you pay back the extra tax credit you claimed and also pay a penalty.
    - The penalty is equal to 300% multiplied by the amount of *extra* tax credit you claimed. Thus, if you claimed an extra $100 your penalty is $100*300% or $300.
    - Your earnings for the round are then Income earned *minus* Final taxes *minus* the extra tax credit you claimed *minus* the penalty.