Some effects of tax information services reliability and availability on tax reporting behavior

Vossler, Christian A. and McKee, Michael and Jones, Michael

Department of Economics and Howard H. Baker Jr. Center for Public Policy, University of Tennessee, Appalachian State University, Bridgewater State University

June 2011

June 2011

Some Effects of Tax Information Services Reliability and Availability on Tax Reporting Behavior[1]

Christian Vossler, University of Tennessee
Michael McKee, Appalachian State University
Michael Jones, Bridgewater State University

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. 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. Almost everyone agrees that the personal income tax system in the US is complex. It is counterproductive to make information resolving the complexity costly to obtain and/or unreliable.[2] Further, the "service" paradigm for tax administration fits squarely with the perspective that emphasizes the role of social norms play in tax compliance (Feld and Frey, 2002).

The effect of such service programs on tax reporting is an open empirical question but intuitively, more reliable and available service programs will be more likely to positively affect tax reporting behavior. Testing such a proposition requires an analysis of individual-level data under alternative information service settings. While some changes in the service programs have

[1]Funding provided by IRS under TRNO – 09Z – 00019. The views expressed are those of the authors and do not reflect the opinions of the IRS or of any researchers working within the IRS. An earlier version was presented at the New Perspectives on Tax Administration: An IRS-TPC Research Conference, June 22, 2011. We are grateful to the participants and to Marsha Blumenthal in particular for comments that improved the paper.

[2] The value of the taxpayer service derives from the costs imposed on the taxpayer for non-compliance. 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.

been undertaken in the past, there is not a full spectrum of such programs in existence and so field data are incomplete. Just as important, even for taxpayers that undergo a full audit, the determination of tax liability is vulnerable to audit error.

As an alternative to analyzing field data, our research utilizes controlled laboratory experiments with human decision makers and salient financial incentives. Within the laboratory, we determine (hence, know) the true tax liability, and then identify the effects of information services by systematically varying the setting across groups of players. In particular, our experimental design varies the degree of accessibility and accuracy of information services. 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.

2. Experimental Design and Treatments

Our experimental design includes many of the fundamental elements of the voluntary reporting system of the U.S. individual income tax. In each decision period of the experiment, participants earn income by performing a task and self-report their tax liability to a tax authority. Tax liability is (possibly) uncertain, and is a function of earned income, the tax rate, and tax credits applied. If an audit occurs, unreported taxes are discovered without error. If the participant 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. We discuss further the details of the experiment below.

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; the design implements a blind setting among the participants.

After earning income, participants are presented with a screen that informs them of the earnings information and the tax policy parameters (tax rate, audit probability, and penalty rate) which are fixed throughout the experiment.[3] The decisions the participants make in the period are whether to request an information service (if one exists) 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 under reporting 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 starts at 1000 for an income of zero and declines at a rate of 0.5 for each additional dollar of income. The amount of the credit is high relative to the initial tax liability so that the credit decision is financially salient.

We implement uncertainty in the credit by placing uniform distributions around the

[3] 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.

expected credit amounts, and then randomly drawing from the distributions to determine the true credit amounts. In particular, the true credit amount can be anything in the range of the expected credit plus or minus 100%. 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.[4]

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.[5] 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 uncertainty.[6] Table 1 reports 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

---

[4] 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.

[5] 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.

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

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.

Audits are determined 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. The audit applies only to the current period declarations, not to previous (or future) periods. The computer automatically deducts taxes paid and penalties (if any are owed) from participants' accounts.

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 their tax liability no over payments are returned to the individual.[7] Tax revenues and any penalties 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. This process is repeated for a total of 20 paid rounds, but to minimize potential end-

[7] 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.

of-game effects the number of rounds is not disclosed.

2.1 Participant pool and detailed 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).[8] Student and non-students participated at separate times, and the lone difference in student versus non-student sessions is that the latter utilized a higher 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 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.[9] 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 proceeds 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 (see Appendix for an example). Clarification questions are addressed after the participants have completed the

[8] An individual session included only students or non-student participants – they were not mixed in a session.
[9] Other experimental projects were ongoing at the time and participants may have participated in other types of experiments.

instructions and two practice rounds. 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 including information on tax reporting experience. Payments are made privately at the end of the session.

2.2 Treatments

We employ a between-subjects design, where the treatment variables across sessions are the presence/absence of an information service, the quality of the service if provided, and the cost of obtaining the information. These 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. 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 (between sessions) the cost to acquire information in the information service treatments (see Table 1). The three cost levels are $0, $50 and $100 for the perfect and the simultaneous information settings. For the sequential setting, these costs are halved and assessed separately for the two sources.

2.3 Testable hypotheses

To derive testable implications based on economic theory, we draw heavily from the theoretical model derived in Vossler, McKee and Jones (2010). In particular, the experimental game described above represents a special case of the theory, one in which the taxpayer makes a decision on a single "line item" and the taxpayer is required to file a return. With our experimental parameters, assuming risk neutrality, when the true credit is certain the taxpayer will optimally choose to report truthfully. When the credit is uncertain, for all levels of earned income, the taxpayer will over-claim 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 range of possible credits, theory suggests the highest relative amount of over-claiming for these individuals. Point predictions from the theory have that it is optimal to over-report the tax credit by 333 lab dollars for those with earned income of 1000, over-report by 250 for those with an income of 1250, and over-report 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 the information is nevertheless valuable to the player, tax underreporting is actually *higher* 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 our choice of parameters. Intuitively this is driven by the fact that 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. But, to be clear here, the information is valuable to the player.

The decision of whether to request the information service(s) to resolve (some) uncertainty is driven by the value of information. Theoretically, and quite intuitively, the taxpayer's willingness-to-pay (WTP) is increasing in the 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.[10] Further, knowing the true credit is more valuable than receiving two possible amounts only one of which is correct.[11] 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

[10] This may be partially offset by the income effect since information is expected to be a normal good.
[11] This stems from the adage that "if a person has one clock she always knows what time it is but if she has two clocks she is never quite sure."

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 main testable implications of the theory are summarized below as formal hypotheses:

Hypothesis 1. The level of tax underreporting is higher when tax liability is uncertain.

Hypothesis 2. Tax underreporting increases with the level of uncertainty (i.e. decreases with income)

Hypothesis 3. Tax underreporting decreases when information services are provided.

Hypothesis 4. Tax underreporting decreases when information service quality improves.

3. Results of the Data Analysis

In the analysis that follows, we largely let the data "speak" by specifying OLS regression models that simply allow the mean outcome to differ across unique experiment scenarios. As such, the purpose of the regression is largely to estimate appropriate standard errors for the means and to facilitate hypothesis testing. On this note, to control for possible heteroskedasticity and autocorrelation of unknown form, 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. To more parsimoniously illustrate some key patterns in the data, we also estimate models that implement some additional structure.

Tables 3 – 5 present three models using the credit decision data.[12] In all models we use as the dependent variable the difference between the credits claimed on the tax form and the expected amount of the credit. Formulating the dependent variable in this way allows the model parameters to be interpreted as the average amount of tax underreporting. The expected credit

[12] The analysis excludes the 6 rounds out of 14,600 (730 participants x 20 rounds) where the tax form "timed out".

depends upon treatment conditions and is measured from the perspective of the participant.[13] That is, in our baseline certainty treatment or when perfect information is obtained in Treatment 3, the expected credit is simply the true credit. In uncertainty treatments where no information is acquired, the expected credit is simply the midpoint of the uncertainty interval. When two sources of information are obtained in Treatments 4 and 5, the expected credit is the average of the two. Finally, with one (imperfect) information source, the expectation is simply the average between the information draw and the midpoint of the uncertainty interval.

Model I estimates the average level of tax underreporting separately by each income level and each treatment. Model II extends the analysis to allow the average level of underreporting to be based on whether an information service was acquired and, if so, the type of service. To accomplish this, we define four new experiment "conditions". The first includes observations from information service treatments where information was not acquired ("No Information"). The remaining three correspond to observations where information was acquired: "Perfect Information" is associated with Treatment 3; "Two Information Sources" is associated with Treatment 4, and those in Treatment 5 who sequentially requested information from both sources; and "One Information Source" is associated with Treatment 5 for those who obtained one of the two available services. Finally, the main effects of income and information cost (where relevant), are estimated by experiment condition in Model III.

One prominent effect, as evidenced by all models, is that the tax underreporting is increasing in income (i.e. decreasing with the degree of uncertainty). This is in the opposite direction predicted by theory (Hypothesis 2), but consistent with our earlier findings in a related

[13] This formulation is 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 actual level of tax under reporting as the dependent variable this should only lead to trivial differences in results.

experiment (Vossler, McKee and Jones, 2010). A second, basic implication of the theory is that the presence of uncertainty increases tax underreporting (Hypothesis 1). Comparing our certainty and uncertainty baselines, there is weak evidence of this effect. Underreporting is roughly 60 to 70 lab dollars higher for all income levels, but this difference is only marginally significant at the middle income level (p-value=0.08; based on Model I or II). Inconsistent with Hypothesis 3, levels of under reporting do not differ based on the quality of the information service. In particular, there are no statistical differences across the three information service conditions, either by income level (Model II: p=0.83 @1000; p=0.55 @1250; p=0.36 @1500) or, based on the main effects specification, we fail to reject the null hypothesis of equal intercepts, income effects and cost effects across the three conditions (Model III: $F_{6,729}$=1.39; p=0.22). Further, even with all participants pooled regardless of whether they acquired information, there are no differences across Treatments 3, 4 and 5 (Model I: p=0.78 @1000; p=0.77 @1250; p=0.41 @1500).

Our most important findings are that information services decrease tax underreporting for those that acquire the information and – even though services are acquired roughly 58% of the time – for the service treatments as a whole (i.e. Hypothesis 4 is supported). The joint finding is most important since information acquisition is a choice, and it could simply serve to sort the players into inherently compliant and non-compliant groups – with the overall effect of information being a wash. Model II illustrates that those who access the information service have the lowest levels of underreporting. In fact, those receiving information under report roughly 80%, 70% and 60% less, across the respective income levels, as compared to those in the uncertainty baseline. The differences in tax underreporting levels 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 (at any income level). Also evident from Model II, participants in the information service treatments who do not acquire information (i.e. the "No Information" subgroup) tend to have reasonably high levels of underreporting, albeit similar to the levels of underreporting in the uncertainty baseline.

Model I supports the finding that there is overall less tax underreporting in the information service treatments. Comparing Treatments 3, 4 and 5 with the uncertainty baseline suggests overall tax under reporting is cut in half. In eight of the nine possible cases, underreporting is significantly different – and lower – for the information service treatment relative to the uncertainty baseline. The lone exception is when comparing Treatment 4 and the uncertainty baseline at the high income level, where the effect is in the expected direction but marginal (t=1.62; p=0.11). We summarize the results based on our analysis of the credit decision succinctly below:

Result 1. Tax underreporting decreases with the level of uncertainty (i.e. increases with income).

Result 2. The quality, as measured by accuracy, of the information service has no effect on tax underreporting.

Result 3. Those who acquire information underreport significantly less than those who do not. Further, *unconditional* on whether the information service was acquired, the availability of information services has the *overall* effect of reducing tax underreporting.

4. Concluding remarks

Our most important finding is that, as predicted by economic theory, the provision of information – even when the quality is low – significantly increases tax compliance. Other hypotheses are not fully supported by the data and undoubtedly more about taxpayer behavior will be revealed through a more intricate data analysis. We have not yet investigated subject pool effects for these treatments but other work using data from similar experimental settings suggests that observed behavior is broadly consistent across pools (Alm, Bloomquist, and McKee, 2011).[14] 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 "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, with tax uncertainty the level of underreporting increases, second, when information services are provided the level of underreporting is lowered, and third, 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, 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. We are currently researching this topic.

---

[14] 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.

Table 1. Experiment Parameters

Parameter / variable	Value(s)
Earned Income	1000, 1250 or 1500 lab dollars
Audit Probability	30%
Penalty Rate	300% on unpaid taxes
Tax rate	50% on taxable income
Tax Deduction	50% × Earned Income (pre filled on tax form)
Tax Credit	Expected value: 1000 – (0.5×Earned Income) Range: +/- 100% of expected value
Information Cost (if service is available)	0, 50 or 100 lab dollars

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.Credit Decision Model I

Dependent Variable: Credit claimed – (Expected) credit					
	Treatment 1 (Certainty Baseline)	**Treatment 2 (Uncertainty Baseline)**	**Treatment 3 (Perfect Info Available)**	**Treatment 4 (Simultan. Info Available)**	**Treatment 5 (Sequential Info Available)**
Income=1000	160.36** (31.38)	219.28** (29.61)	72.61** (26.13)	46.18* (26.76)	59.82** (20.68)
Income=1250	186.97** (30.52)	260.38** (27.79)	148.82** (20.04)	135.95** (18.16)	154.43** (18.57)
Income=1500	257.32** (32.32)	310.31** (35.37)	208.37** (20.81)	242.83** (21.89)	206.37** (21.59)
N=14,594 R^2=0.25 F=30.89**					

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

Table 4.Credit Decision Model II

Dependent Variable: Credit claimed – (Expected) credit						
	Experiment Condition					
	Certainty Baseline	Uncertainty Baseline	No Information	Perfect Information	Two Information Sources	One Information Source
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=14594 R^2=0.29 F=28.79**						

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

Table 5. Credit Decision Model III

Dependent Variable: Credit claimed – (Expected) credit						
	Experiment Condition					
	Certainty Baseline	Uncertainty Baseline	No Information	Perfect Information	Two Information Sources	One Information Source
Intercept	-47.38 (102.03)	-64.83 (94.26)	-428.11** (87.21)	-81.34 (62.03)	-135.73** (52.09)	153.81 (141.60)
Income	0.20** (0.08)	0.26** (0.08)	0.59** (0.06)	0.16** (0.05)	0.20** (0.04)	0.17** (0.11)
Information Cost			-1.02** (0.47)	-1.31** (0.39)	-0.73** (0.24)	-0.85 (0.80)
N=14,594 R^2=0.29 F=40.08*						

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

References

Alm, James, Kim Bloomquist, and Michael McKee, 2011, "On the External Validity of Tax Compliance Experiments," prepared for presentation at 2011 IRS Research Conference, Washington, DC.

Alm, James, Todd Cherry, Michael Jones, and Michael McKee, 2008. "Encouraging Filing via Tax Credits and Social Safety Nets," *The IRS Research Bulletin: Proceedings of the2008 IRS Research Conference*, pp 43 – 57.

____, 2010."Taxpayer Information Assistance Services and Tax Reporting Behavior."*Journal of Economic Psychology* 31 (4): 577-586.

Alm, James, Betty R. Jackson, and Michael McKee. 1992a. "Estimating the Determinants of Taxpayer Compliance with Experimental Data." *National Tax Journal* 65 (1): 107-114.

___________________________________. 1992b. "Institutional Uncertainty and Taxpayer Compliance," *American Economic Review*, 82 (4): 1018-1026.

Alm, James and Michael McKee. 2006. "Audit Certainty and Taxpayer Compliance." *National Tax Journal* 59 (4): 801-816.

Cadsby, C. Bram, Elizabeth Maynes, and Viswanath Umashanker Trivedi. 2006. "Tax Compliance and Obedience to Authority at Home and in the Lab: A New Experimental Approach," *Experimental Economics*, 9 (4): 343-359.

Feld, Lars and Bruno Frey. 2002. "Trust Breeds trust: How Taxpayers are Treated," *Economics of Governance*, 3: 87-99.

Gravell, Jane, 2008, "Comments," *The IRS Research Bulletin: Proceedings of the2008 IRS Research Conference*, pp 59-60.

Greiner, Ben, 2004. "The Online Recruitment System ORSEE 2.0 - A Guide for the Organization of Experiments in Economics," Working Paper Series in Economics 10, University of Cologne, Department of Economics.

Plott, Charles R. 1987. "Dimensions of Parallelism: Some Policy Applications of Experimental Methods." In Alvin E. Roth (ed.), *Laboratory Experimentation in Economics: Six Points of View*. New York, NY: Cambridge University Press.

Smith, Vernon. 1982. "Microeconomic Systems as an Experimental Science." *The American Economic Review* 72 (5): 923-955.

Vossler, Christian, Michael McKee, Michael Jones, 2010, "The Impact of Taxpayer Information Services on Tax Reporting and Tax Filing," presented at the 2010 meetings of the Southern Economics Association, Atlanta, GA.

Appendix. 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.