

A Survey of Experiments on Tax Compliance

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Abstract

This report provides a survey of experiments on tax compliance. The experimental design and major results are described for experiments undertaken from 1978 to the present.

1 Introduction

The importance of experiments for research into tax compliance is enhanced by the illegality of the failure to correctly comply with tax legislation. The legal position makes it difficult to directly observe actual compliance behaviour and implies that surveys that question taxpayers about compliance will be unlikely to elicit truthful reports. There is also very little publicly available data on compliance behaviour. These facts support the use of the experimentation as a focussed method of investigation.

The importance of experimentation as a method of investigating economic behaviour has increased significantly in the past two decades. It was once possible to claim confidently that economics was a non-experimental science. This is no longer the case, and experiments have been successfully applied in many areas of economics. The basis of laboratory experimentation is the creation of an environment that allows the experimenter to isolate a particular aspect of individual decision-making while keeping other decision criteria constant. This approach has numerous advantages over the use of data from actual behaviour. First, it avoids the problem of distinguishing between individual preferences and the circumstances in which a decision is made. Second, it permits control of the circumstances in which decisions are made. Third, it allows the investigation of decisions in hypothetical situations or situations for which it is difficult to collect actual data. The most obvious drawback is that it can never be guaranteed that the behaviour observed in the laboratory is truly representative of actual behaviour. Whether it is, or not, is a consequence of good experimental design.

An experimental laboratory consists of a network of linked computers placed in cubicles that isolate participants and prevent communication except via the channels permitted by the experiment. The subjects respond to instructions on

the screen by making decisions or communicating via the network with other subjects. The decisions lead to payoffs, and the payoffs are converted into actual payments of cash at the completion of the experiment. The payment of cash gives an incentive to subjects to make decisions that maximise the payoff. The response of subjects to changes in economic variables can be found by repeating the experiment for different values of the variables. All data from the experiment, including communication between subjects, is collected by the experimenter and analysed after the experiment.

Numerous experiments have been conducted to explore tax compliance behaviour. The literature began with the seminal research of Friedland et al. (1978) and a steady flow of contributions have followed. The typical experiment takes a group of university student subjects who must choose how much of a given income to declare to the tax authority. This choice problem is repeated over a number of rounds. Many different treatments can be applied within this structure. The experimenter can vary the probability that an income declaration is audited, the rate of income tax, and the rate of fine on evaded tax. These variables may be public information or may be unknown to the subjects. In the latter case, the role of communication between subjects can be explored. Public goods, or some other benefit resulting from the payment of taxes, can be introduced to test reciprocity theories.

The basic experimental design has not changed a great deal in the 30 plus years since the literature was initiated. There have been refinements to the experiments and tests of alternative formulations, but the basic experimental design has remained robust. A great variety of treatments have been employed using the design, not all of which have had the expected outcome. There are some findings that are consistent across experiment - such as the effect of gender - but the experiments do not always agree on how some other variables affect compliance.

This survey discusses the key elements and central results of each paper that has contributed to the literature. The survey attempts to be comprehensive in covering the papers reporting laboratory experiments on compliance published in refereed journals of economics and also reviews papers in journals from related disciplines. Some unpublished working papers are also reviewed when they make a sufficient contribution. In addition, four papers reporting field experiments are also included. Field experiments have been rare for compliance but offer a new avenue for exploration. The experiments are presented in chronological order. This method of presentation was chosen to highlight how the experimental specification has evolved over time.

There are numerous findings that emerge from the experiments. Some of the treatment variables, such as the audit probability and the level of the fine, have the effect predicted by economic theory in most of the experiments. There are also personal characteristics that have a consistent effect in many of the experiments. Typically, females comply more than males, and the old comply more than the young. Other variables - and the tax rate is notable among them - have an effect that differs across experiments. The theoretical prediction of Yizhaki (1974) is that an increase in the tax rate will raise compliance since

this raises the effective punishment when a non-compliant taxpayer is discovered (the product of the rate of fine and the tax rate). The majority of experiments use students as experimental subjects. There is ample debate within the discipline as to whether students are representative of the wider population (Harrison and List, 2004). A major criticism of the standard economic model of tax compliance is that it predicts less compliance than is actually observed (see Hashimzade, Myles, and Tran-Nam, 2012). This has motivated alternative explanations of compliance based on psychological costs, psychological distortion of probabilities, social customs, and tax morale. All of these explanations have their roots in learning and adaptation to social processes that relate directly to the illegal nature of tax evasion. This can make choices framed in the context of evasion different to choices framed in a neutral context (see Baldry, 1987, below). Student subjects will not have experience of tax payment and will not have become immersed in the social process. As a consequence, their decisions may reflect only strategic considerations based solely on financial payoffs rather than capturing actual behaviour in a compliance decision.

The remainder of the survey is organised as follows. A glossary of experimental terminology is provided in Section 2. Section 3 provides the main body of the survey. Section 4 surveys non-laboratory research. A convenient summary table and some observations on the experiments that have been surveyed are provided in Section 5. Conclusions are given in Section 6. The results of econometric analysis of the experimental data are reported in tables collected in the Appendix.

2 Glossary

This section provides a glossary of the terms used in describing the experiments and their results.

Audit probability

The chance that an individual taxpayer will be subjected to a random audit. The value of the audit probability is between 0 (no chance of an audit) and 1 (an audit will definitely take place). A value of 0.5 represents a 50:50 chance of an audit.

Backward-looking audit

An audit that reviews tax returns and incomes from a fixed number of previous periods as well as from the current period.

Compliance rate

The proportion of income that is declared. This has a value between 0 (no income is declared) and 1 (all income is declared). The compliance rate is defined as

$$\text{Compliance rate} = \frac{\text{Declared income}}{\text{Actual income}}.$$

Endowment

The initial income allocated to experimental subjects at the start of the experiment.

Experimental currency units (ECUs)

The artificial currency used in experiments. Subjects will be informed of the exchange rate between ECUs and real currency at the outset of the experiment. At the conclusion of the experiment the ECUs that have been earned are converted to real currency using the pre-announced exchange rate.

Experimental subject

A participant in an experiment. Subjects in a laboratory experiments are volunteers. Subjects in a field experiment may be volunteers or may be participating without being aware of the fact.

Field experiment

An experiment conducted in the normal environment of the subjects involved. In some case the subjects may be informed that they are participating in an experiment. In other cases the experiment may be conducted without subjects being informed.

Forward-looking audit

An audit that reviews tax returns and incomes from a fixed number of future periods as well as from the current period.

Ghost

An individual that does not submit a tax return and consequently pays no tax unless investigated. A ghost effectively operates outside of the tax system.

Lab dollars

An alternative name for experimental currency units.

Laboratory experiment

An experiment conducted in a laboratory setting. Subjects attend the laboratory voluntarily to participate in the experiment.

Pre-announced

Information that is announced to subjects prior to a decision being made.

Pre-testing

A test administered prior to commencement of the main part of an experiment. For example, pre-testing can be used to determine the degree of risk aversion of a subject by testing choice in a risky situation.

Propensity to evade

The proportion of the subjects that declare less than the correct level of income. This has a value between 0 (no subject declares less than correct income) and 1 (all subjects declare less than the correct income). The propensity to evade is defined as

$$\text{Propensity to evade} = \frac{\text{Number of subjects declaring less than correct income}}{\text{Total number of subjects}}.$$

Public good

A good that benefits all experimental subjects simultaneously. In contrast, a private good benefits only a single subject.

Revenue service

The part of government that is responsible for collecting tax revenues and undertaking audits.

Round

A complete play of one cycle of the experiment. In a typical compliance experiment a round consists of earning income, making a declaration, possibly being audited, and notification of payoff for the round. Sometimes denoted as 'period'.

Session

The complete experimental period from initial introduction to the laboratory through to completion of the experiment. A session typically consists of initial activities (provision of information and pre-testing), several rounds of the experiments, and a debrief.

Statistically significant

A variable in a regression equation with an estimated coefficient that is different from zero with a high probability. A variable that is statistically significant at the 5% level is different from zero with a probability of 19/20, and if significant at the 1% level is different from zero with a probability of 99/100.

Tax withholding

A tax payment on account in advance of the final liability being realised.

Treatment

An implementation of an experiment with a given set of values for the treatment variables.

Treatment variable

A variable in an experiment whose value is systematically changed to determine the effect on behaviour. In a tax compliance experiment the tax rate is often a treatment variable. The experiment is repeated for different values of the tax rate to test if compliance increases or decreases as the tax rate is raised.

Underreporting

The act of reporting less than the correct level of income.

Underwithheld

A situation where the tax payments on account sum to less than the final tax liability.

3 Experiments

The experiments are reported in chronological order and identified by authors and date. Some of the experiments have been reported in two or more documents at different dates. Generally, the most recent document will be surveyed unless older versions contain especially relevant information. We will report sample size as number of participants. This may not always coincide with the number of independent observations, for instance when the same participant makes a series of repeated decisions.

3.1 Friedland, Maital, and Rutenberg (1978)

The experiment employed a tax evasion game in which participants were given a "monthly" income in and informed of the tax rate, the probability of being

audited, and the rate of the fine if caught evading. The monthly structure was used to instill a sense of familiarity and enhance the context. Given this information, they were requested to make an income declaration. The focus of the experiment was whether a high fine rate with a low probability of audit was more effective than a low fine with a higher probability of audit.

The experimental subjects were 15 Israeli undergraduate psychologists (7 males, 8 females). The monthly income that subjects were given was increased each month as the experiment progressed. The experiment had 4 rounds with each round being composed of 10 months. The audit probability, the tax rate, and the fine rate were varied across rounds and were pre-announced at the start of the round. When the fine rate was 15 times unreported income the audit probability was 1/15. When the fine rate was 3 times unreported income the audit probability was 5/15. Table 1 describes the alternative treatments in the experiment.

	Tax rate	
Fine magnitude	25%	50%
15 times	Round one	Round three
3 times	Round two	Round four

Table 1: Treatments

Table 2 summarises the average results for each round of the experiment. The proportion of subjects who evaded is denoted by p , x is the average proportion of income not declared when a subject evaded, and $q = 1 - px$ is the overall proportion of income declared. The table shows that an increase in the tax rate increased underreporting (counter to the theoretical prediction), whereas a higher fine reduced it (in line with the theoretical prediction).

		Tax rate	
Fine magnitude		25%	50%
15 times	Proportion evading (p)	47%	78%
	Proportion of income not declared when evading (x)	26.8%	43.1%
	Proportion of income declared (q)	87.4%	66.4%
3 times	Proportion evading (p)	57%	81%
	Proportion of income not declared when evading (x)	37.5%	53.7%
	Proportion of income declared (q)	79.6%	56.5%

Table 2: Summary of results

The regression analysis of the results related evasion behaviour to individual characteristics (Table A1). The results showed that women evaded more often

than men, but evaded a smaller fraction of income when they chose to evade. The purchasers of lottery tickets, who were presumed to be the less risk averse subjects, had the same probability of evasion as non-purchasers but evaded a larger fraction of income when they did evade. An increase in the tax rate caused a statistically significant increase in the proportion who chose to evade and the fraction of income not declared.

The paper concluded that large fines tended to be a more effective deterrent to evasion than a high probability of audit (despite the magnitude of the fine not proving to be statistically significant in the regressions). The validity of this claim is limited by the fact there was no independent treatment of the fine rate and probability of audit. There was also evidence that the decision to underreport and the extent of underreporting were influenced by different individual characteristics, and that individual factors were important for evasion behaviour.

3.2 Spicer and Becker (1980)

The purpose of the experiment was to examine the relationship between tax evasion and the perceived inequity of the tax system. This was achieved by providing experimental subjects with different information about the average tax rate. All subjects were informed of the rate of income tax (which was the same for all subjects). However, some were informed that the average tax rate was below the rate they faced whereas others were informed that the average tax rate was above their rate. This was designed to instill different views on equity.

The subjects were 57 students from Colorado (21 male, 36 female). The experiment was repeated for ten rounds. Audits were random and subjects were informed that there was a 1 in 15 chance of being audited. They were also informed that the fine for evasion was 15 times evaded tax. The tax rate was 40 percent. 19 subjects were informed that the average tax rate was 65 percent and 19 subjects were told it was 15 percent. The remainder were correctly informed. A questionnaire was administered at the completion of the experiment which elicited information that included a "tax resistance scale".

Table 3 shows that the percentage of required tax payments that were evaded conforms with the expected effect of the information given. The low-tax group are those who were told that their tax rate was lower than average and they had a correspondingly lower percentage of tax evaded than the other two groups. The high-tax group were told their tax rate was higher than average. This group evaded the most. The medium group were told the average tax rate was equal to the tax rate they faced.

	Percent of tax evaded
Low-tax group	12.26
Medium-tax group	24.50
High-tax group	32.63
All groups	23.13

Table 3: Evasion by group

The econometric analysis of the results confirmed the statistical significance of these averages (Table A2). The most important observation was that the perceived relative tax rate had a positive and significant effect on the proportion of tax evaded. Since the information given about the average tax rate did affect behaviour, this showed that perceptions of the equity of the tax system mattered for compliance behaviour. The econometric analysis also showed that males declared a lower proportion of income than females. The experiment supported the idea that perceived inequity and individual characteristics affect compliance behaviour.

3.3 Friedland (1982)

The experimental design was based on that in Friedland et al. (1978). The subjects were 13 law students. The experiment had 16 rounds, and in each round the subjects received a monthly income and were informed about the tax rate (35 percent for all treatments), the rate of the fine for evasion, and the probability of being audited. The treatments differed in the way that the information on audit probability was presented. This was either given in precise or vague terms (how these differ is not explained in the paper).

The results of the experiment are summarized in Tables 4 and 5 which report the percentage of income that was declared for the alternative treatments. The tables show that vague information about the probability of an audit increased the deterrence effect of low fines and low probability audits. In contrast, vague information about the fine rate did not affect compliance.

		Magnitude of fines	
		Low	High
Precision of information about audit probability	Precise	79.31%	86.47%
	Vague	83.36%	85.83%

Table 4: Percentage of income declared: audit information

		Audit probability	
		Low	High
Precision of information about rate of fine	Precise	71.11%	94.67%
	Vague	74.46%	94.73%

Table 5: Percentage of income declared: fine information

The paper concluded that individuals are more responsive to information about the probability of a threat than they are to information about the size of the threat. It is this effect that causes compliance to be greater when information about the audit probability is vague.

3.4 Spicer and Thomas (1982)

This experiment was designed to determine the relationship between the probability of a tax audit and the tax evasion decision.

The experimental subjects were 54 Colorado students. The experiment involved 3 rounds with each round having 8 pay periods. In every pay period each subject had to decide how much of a given salary to report and then compute the tax due on that salary. Audits were undertaken randomly. Any subject caught evading was fined at a rate of 700% of the amount of tax evaded.

The information on audit probabilities given to the subjects varied across treatments. 18 subjects in the experiment were given precise information on audit probabilities. They were informed that the probability was 1 in 20 in the first round, 5 in 20 in the second round, and 3 in 20 in the third round. Another 18 subjects were informed imprecisely about the audit probability. Their information was limited to knowing that the probability was "low", "high", or "medium". The final 18 subjects were given no information. The percentage of taxes evaded and the likelihood of committing evasion were measured.

The results showed that the percentage of tax evaded was negatively and significantly correlated with the audit probability only for respondents receiving precise information (Table A3). This is not surprising since there was no opportunity for the no-information group to learn. There was also a significant negative association between the likelihood of evasion and the audit probability for the groups with precise and imprecise information. There was no significant relationship between either the percentage of tax evaded or the likelihood of evasion for the subjects with no information.

The paper concluded that the effect of changing the audit probability – provided the change was known to subjects – affected the decision to not comply fully more than it affected the amount that was evaded.

3.5 Spicer and Hero (1985)

The motivation for this experiment was to analyze the "rules of thumb" used in making a compliance decision. The experiment manipulated the information given to subjects in order to test the consequences.

The experimental subjects were students at Colorado. There were 10 rounds. In each round the subjects had to decide how much of a given income to declare and to compute the tax payment that was due on the declared income. Audits were random and the fine when caught evading was 1000% of the amount of unpaid tax.

The treatments involved subjects being told different information. 12 subjects were told that in a previous game the participants had paid only 10 percent of taxes due. 12 subjects were told that it was 50 percent, and the remaining 12 were told that it was 90 percent. There had been no prior game so all of these statements involved a deception.

The analysis of the experimental outcomes involved regressing the amount of taxes paid on the outcome reported from the previous game and on the gender

of subjects. The results showed that information from the previous game was not statistically significant. Gender was significant, with males tending to evade a greater amount than females.

A second analysis regressed the amount of taxes evaded in the final round on the amount evaded in the first round and the number of audits. Both variables were statistically significant. A high level of evasion in the first round went with a high level of evasion in the final round, and a higher number of audits reduced evasion.

The paper concluded that the experiment failed to demonstrate that evasion behaviour was significantly affected by the information that was provided on the behaviour of other taxpayers in the "previous" game.

3.6 Baldry (1986)

The motivation for the experiment was to investigate whether the compliance decision was treated by the experimental subjects as a simple gamble or whether it was viewed from a different perspective. The methodology was to contrast the findings of two alternative experiments. The first experiment was explicitly framed as a tax evasion decision. This second experiment was framed as a straightforward gamble. The two experiments were constructed to have the same structure of payoffs. If the evasion decision were treated as a gamble then average behaviour in the two experiments should be identical.

In the first experiment (tax evasion) there were 72 evasion attempts out of 104 observations. In contrast, in the second experiment (gambling) every participant made a positive bet in every round, and each made the maximum bet. The maximum level of tax evasion (a zero declaration) was witnessed in only 40 out of 104 observations in the evasion experiment.

The behaviour in the two experiments was significantly different which led to the claim that tax evasion was not viewed by the subjects in as being just a gamble. It was concluded that there were moral and social dimensions in the tax evasion decision which did not feature in the experiment framed as a gamble.

3.7 Becker, Büchner, and Sleeking (1987)

This was a similar experiment to Friedland, Maital, and Rutenberg (1978, section 3.1) but with two important differences. First, income was earned by the participants. Second, the additional feature of endogenous transfers of tax revenue back to the taxpayers was added. The endogenous transfers were intended to capture the idea that compliance may be higher when taxpayers feel they are receiving something from the system.

Two identical experiments were conducted at Bonn (with 85 students) and at Cologne (with 31 students). Income was earned by a test which involved the completion of a succession of numerical series. The tax system was progressive so the marginal tax rate increased with income. The tax rate was 33% for incomes below 4.50DM, 50% for incomes between 6.00DM and 10DM, and 60% for income between 10DM and 13DM. The format of the experiment restricted

income declarations to be within these ranges. The subjects were told that the transfers they received would correspond to the total value of tax payments but this value was not observed by subjects. They were also told their share of the total transfers. There were three different shares (−0.6 percent, 1.2 percent, 1.8 percent in Bonn, 1.7 percent, 3.4 percent, and 5.1 percent in Cologne) and these numbers were known to the subjects. Auditing was decided randomly by dice rolling (so the probability of audit was $1/6$) but this was not known to subjects. Tax evasion led to a fixed fine of $2DM$ and a charge of three times unpaid tax. Subjects were told if the fine exceeded income earned within the experiment they had to pay out of pocket.

The econometric analysis produced several important results (see Table A4). It was discovered that the propensity to evade was reduced by a high transfer payment and a high expected probability of audit, and by an increase in the perceived tax burden. An increase in income level also raised the propensity to evade. The paper also conducted a separate analysis of the behaviour of the tax evaders (36 of all cases). This showed that only the probability of an audit being undertaken had a significant effect on the proportion of due tax payment that was evaded. No other variable had a statistically significant effect.

The main result of this experiment was that tax compliance depended not only on the tax rate, audit and fine, but also on the benefits received from the system.

3.8 Alm, McKee, and Beck (1990)

The experiment explored the effects of tax amnesties on compliance behaviour. The setting was standard with subjects receiving an income, making a declaration, and facing the probability of an audit. Tax payments were collected in a group fund, multiplied by two, and then redistributed equally between subjects. This was intended to represent a public good. There were several treatments in which a tax amnesty was introduced in different ways. In one treatment it was offered in the middle of the experiment with no warning. In other treatments, the subjects were told before the experiment there would be an amnesty.

The treatments are summarized in Table 6. For all treatments the tax rate was 30%. The terminology of the experiments was chosen to be neutral so that it avoided terms (such as audit) that had a possible contextual effect. Subjects were from a Principles of Economics class at Boulder. The average payment per subject was between \$15 and \$25.

Treatment	Audit probability	Fine on unpaid tax	Amnesty
1	0.04	2	No
2	0.04	2	Unannounced amnesty after round 13
3	0.04, then 0.6 after round 13	2, then 4 after round 13	No
4	0.04, then 0.6 after round 13	2, then 4 after round 13	Unannounced amnesty after round 13
5	0.04	2	Announced at start but not given
6	0.04	2	Announced at start, given after round 13
7	0.04	2	Commitment to a single amnesty

Table 6: Treatments

The results on average compliance are given in Table 7. The results of the experiment showed that the average level of compliance fell after an amnesty. The expectation of an amnesty also reduced compliance prior to the amnesty. Compliance was higher if an amnesty was accompanied by tougher enforcement.

Treatment	Rounds 1-13	Rounds 14-25	All rounds
1	0.532	0.585	0.56
2	0.518	0.449	0.48
3	0.517	0.553	0.53
4	0.575	0.685	0.63
5	0.488	0.398	0.44
6	0.556	0.486	0.52
7	0.564	0.506	0.54

Table 7: Average compliance rates

3.9 Beck, Davis, and Jung (1991)

The experiment was designed to test the effects of uncertainty about taxable income on the compliance decision. It also tested the effect on reported income of changes in the level of uncertainty about income, the tax rate, the fine levied on evaded tax, and probability of being audited.

Each session had 7 subjects who were endowed at the start of the session with a pre-tax income of 1000 ECUs. There were 60 rounds for each subject. The subjects were 112 undergraduates and graduates from University of Illinois, Urbana-Champaign.

The experimental task was to report the amount of taxable income. The actual taxable income was not known by the subject when the report had to be made. After the report was made the level of taxable income for a subject

was determined by making draw from a uniform probability distribution.¹ The choice of a uniform distribution made every income level between the lowest level and the highest level equally likely as the outcome of the draw.

After making each report in the 60 rounds a subject paid a proportional tax (characterized as a "surcharge" in the experimental instructions) on reported income and faced the possibility of being audited. The two sources of uncertainty were the possibility of an audit taking place and the determination of the level of taxable income if there were an audit. Subjects were told that audits were random and occurred with some unknown probability. The level of income uncertainty was made observable by drawing balls from a cage. The number of balls was either 11 or 51. The number could be seen to represent low uncertainty (11 balls) or high uncertainty (51 balls). In the event of an audit a draw was made from the uniform distribution to determine taxable income. A proportional fine was then imposed on the unpaid tax if the randomly drawn level of taxable income exceeded the reported income level.

The results showed that an increase in the fine or in the probability of being audited resulted in significantly higher levels of taxable income being reported. Furthermore, changes in the uncertainty level interacted with the penalty rate and audit probability. The tax rate was not found to be significant in the compliance decision.

3.10 Collins and Plumlee (1991)

The paper reported an experiment that examined the effect of three alternative audit systems on the decisions of taxpayers concerning the amount of labour to supply and the level of income to report to the revenue service. The audit systems differed in the information that the tax authority used to determine which reports to audit. The three alternative audit systems were:

- (a) audits were random so no information was used;
- (b) auditing was determined by a cut-off rule so reports below a given level were audited;
- (c) auditing used a conditional system where an estimate of true income was used in addition to reported income.

The effect on compliance of changes in the tax rate and the fine levied on earned but underreported income was also analysed.

The subjects were 120 undergraduate business or economics students. Income was earned by the subjects performing an exercise that involved decoding letters. The performance at the decoding task was rewarded on a piece-rate basis. On average, subjects earned approximately \$10 net of taxes and penalties. There were twelve treatments (3 audit schemes, 2 tax rates (30%, 60%), and 2 fine rates (1.2 times underreported tax, 2.0 times underreported tax). The random audit scheme had a probability of audit of 0.2. The expected number of audits was held constant across the three audit schemes. So, for example,

¹The uniform probability distribution was defined by $f(x) = 1/[H - L]$, where $[L, H]$ was the range of possible values for taxable income.

the cut-off rule was chosen to ensure that the lowest 20 percent of reports were audited. The conditional scheme used subject performance in a practice earning exercise prior to the experiment as a noisy signal of true income.

The experimental results showed that the conditional audit process which used the signal sent by the taxpayers was more successful in reducing underreporting compared to random audits. Underreporting was also generally greater when the tax rate was high and the penalty rate was low. Underreporting and effort in the income exercise were positively related: subjects who chose to underreport also earned significantly more income.

3.11 Alm, Jackson, and McKee (1992a)

The experiment was designed to investigate the effect of uncertainty in enforcement and tax policies. This question was motivated by a discussion of whether this was a deliberate strategy of the Internal Revenue Service in the US. A case could be made for increasing uncertainty on the grounds that it increased the variance of income without raising the mean. A risk-averse taxpayer should respond to the increase in variance by increasing compliance.

The role of this form of uncertainty was studied by comparing the compliance behaviour of individuals facing known values for the tax rate, the probability of audit, and the level of fine with the compliance behaviour of taxpayers who knew the values were drawn randomly (and knew the distribution from which they were drawn). The impact of government expenditure on compliance was also examined by introducing a public good in some treatments.

The experimental subjects were volunteers from Principles of Economics classes with no prior experience with the experimental setting. Each session lasted approximately one hour, with the average payoff per subject varying from \$15 to \$25.

In each experimental session the subjects were organized into groups of five. They were informed that the session would last for an unknown number of rounds, but the number was predetermined at 25. Each subject was given an initial endowment of 10 ECUs and was informed of the exchange rate for dollars. Each round began with a subject receiving a random income, drawn from the interval 2.00-3.00 in increments of 0.25. The individual then had to make an income declaration. After incomes were reported and taxes paid at most one person was randomly selected for an audit. An audit led to unpaid back taxes for the five previous rounds being collected and a fine equal to some multiple of unpaid taxes. In the treatments with a public good the taxes of the five individuals in the group were paid into a fund which was increased by a multiple of two and the resulting amount distributed in equal shares to the members of the group. This represented provision of a public good. Revenue collected from audits was not added to the fund.

Table 8 shows the treatments. In treatment 1 all parameters were known with certainty. Other treatments introduced uncertainty with each of the two possible values having probability one half of being used. The expected value

of the random variable equaled its certain value in treatment 1. All treatments were run twice: once with a public good and once without a public good.

Treatment	Fine rate	Tax rate	Probability
1: base case (perfect information)	2	0.3	0.04
2: fine uncertainty	either 1 or 3 mean= 2	0.3	0.04
3: tax uncertainty	2	either 0.1 or 0.5 mean= 0.3	0.04
4: detection uncertainty	2	0.3	either 0.02 or 0.06 mean= 0.04

Table 8: Experimental Treatments

Table 9 reports the mean and standard deviation of the compliance rate from the experiment. The results provided mixed support for the hypothesis that greater uncertainty increased tax compliance. The effect of uncertainty depended on the presence or absence of the public good. When there was no public good the existence of uncertainty increased compliance but with a public good uncertainty always lowered compliance. Therefore, the effect of uncertainty depended on how an individual's tax payments were linked with those of other group members in the provision of the public good. The stronger is the link, the more compliance would be reduced when uncertainty increased. In summary, the results showed that a revenue service policy of maintaining uncertainty to increase compliance may not have the desired effect.

Treatment	Mean compliance rate
No public good	
1: base case	0.262
2: fine uncertainty	0.374
3: tax uncertainty	0.370
4: detection uncertainty	0.481
Public good	
1: base case	0.557
2: fine uncertainty	0.501
3: tax uncertainty	0.398
4: detection uncertainty	0.519

Table 9: Mean Compliance Rate

3.12 Alm, Jackson, and McKee (1992b)

This experiment took an approach that was very similar to many of the earlier experiments. Student subjects were placed in groups of five, given an initial endowment, and then allocated additional random income at the start of each round. The subjects were not informed of the number of rounds at the outset of the experiment, but there were always 25 rounds. Audits were random but

subjects could see the mechanism (a bingo cage with numbered balls) used to make the random draw.

The initial endowment was 10 ECUs. The random income each round was either 2.00, 2.25, 2.50, 2.75, or 3.00 ECUs. Subjects earned between \$15 and \$25 from participation in the experiment. The treatments varied the tax rate, level of fine, and the probability of audit. One treatment included a public good: tax revenue was multiplied by a factor of two and redistributed equally to subjects. The treatments and the average compliance rate for each treatment are summarized in Table 10. Treatment 4 which had the highest tax rate had the lowest rate of compliance. Treatment 7 with the low audit probability also had less compliance. Compliance increased when the tax rate was low and when the fine was high.

Treatment	Tax rate	Fine	Audit probability	Public good	Average compliance rate
1	0.30	2.0	0.04	No	0.332
2	0.30	2.0	0.04	Yes	0.374
3	0.10	2.0	0.04	No	0.376
4	0.50	2.0	0.04	No	0.200
5	0.30	1.0	0.04	No	0.317
6	0.30	3.0	0.04	No	0.376
7	0.30	2.0	0.02	No	0.321
8	0.30	2.0	0.06	No	0.365

Table 10: Treatments and compliance

The econometric analysis of the experimental outcomes (Table A5) confirmed the observations made about the aggregate data. The only unusual result concerned the effect of the public good. The public good was entered as a dummy (1 when it was present, 0 when it was absent). The negative coefficient showed that the presence of a public good reduced the amount of income declared. The public good also entered the regression as an interaction with the payoff from the group fund in the previous round. The role of this variable was to capture the idea that past experience of a high payment from the public fund would increase compliance as taxpayers observed some benefit from making tax payments. The variable had a positive and significant coefficient so a larger payoff from the fund increased the amount of income declared.

3.13 Martinez-Vazquez, Harwood, and Larkins (1992)

The experiment was designed to investigate the effect of tax withholding on the level of tax evasion. The focus was placed on how the evasion decision was affected for individuals who found themselves unexpectedly underwithheld. Three different explanations were tested: the *framing effect* from prospect theory (taxpayers may be risk-lovers if they see additional payment from an underwithheld position as a loss), the *liquidity effect* (the taxpayer may not have enough funds

to pay the required taxes if underwithheld so is pushed into evasion) and *fiscal illusion* (paying taxes in a series of small amounts reduces resistance to the tax).

The experiment used MBA students as subjects. Each subject was presented with one tax scenario and asked to make choices. Two pairs of scenarios were used to test framing and liquidity. One pair of scenarios was used to test the illusion effect. There were no monetary payments involved in the experiment.

It was concluded that the withholding position (either under- or overwithholding) when combined with the element of surprise did not appear to affect compliance behaviour. The results failed to demonstrate that withholding encourages tax compliance via fiscal illusion and did not support the reflection effect from prospect theory. Some of the results provided a degree of support for the liquidity hypothesis since the proportion of individuals that chose to evade in an illiquid situation was significantly greater than the proportion that chose to evade when liquid.

3.14 Alm, McClelland, and Schulze (1992)

The purpose of the experiment was to investigate the role played by the overweighing of low probabilities in decision making (given the prominence of this feature in non-expected utility theories) and the effect that the provision of government services had on compliance.

		Rounds		
Treatment		1-15	16-30	31-45
		Probability of audit		
1	Multiplier on group fund = 2	0	0.02	0.10
2		0.02	0.10	0
3		0.10	0	0.02
		Multiplier on group fund		
4	Probability of audit = 0.02	$m = 2$	$m = 0$	$m = 6$
5		$m = 0$	$m = 6$	$m = 2$
6		$m = 6$	$m = 2$	$m = 0$

Table 11: Treatments

The subjects were undergraduates at Colorado. Incomes were randomly assigned to the subjects. The tax rate was 40% and the fine was 15 times the amount of unpaid tax. Tax revenue was placed into a group fund which was then multiplied by a scaling factor (three different multiplier values were used) and finally divided equally between subjects. For example, if the multiplier were 2 and \$20 were placed into the fund then \$40 would be paid out. Each subject was guaranteed a \$5.00 minimum payment from the experiment, and earnings were between \$15.00 and \$25.00. The six basic treatments are summarized in Table 11. The treatments differed in the value of the multiplier on the group fund and the probability of an audit. The first three treatments were run twice. One run used a neutral instruction set and one run used a "loaded" instruction set that placed a stress on audits and the consequences of evasion. The use of two

instruction sets was designed to test the consequences of induced preferences through the description of the experiment.

The mean value of the compliance rate for different treatments is given in Table 12a and 12b. The results show that the probability and the multiplier have an effect but that the loaded instructions did not matter. This demonstrated the absence of a framing effect in the compliance decision. The noteworthy result is that there was compliance even when audits would not take place ($p = 0$). The explanation for this finding could be the presence of the public good. The paper argued that this outcome cannot be explained by over-weighting but this seems wrong: any number of reasons could be advanced for the experimental subjects not finding the absence of audits to be credible.

	Treatment		
Probability of audit	0	0.02	0.10
Neutral instructions	0.200	0.503	0.675
Loaded instructions	0.189	0.522	0.672

Note: multiplier on group fund =2

Table 12a: Mean compliance rate

	Treatment		
Multiplier on group fund	0	2	6
Mean	0.435	0.537	0.592

Note: probability of audit = 0.02

Table 12b: Mean compliance rate

The paper concluded that the results provided evidence that some individuals overweight low probability events. It also observed that compliance behaviour did not stem from a belief that evasion was wrong since the two treatments with different terminology yielded identical results.

3.15 Alm, Conshaw, and McKee (1993)

The experiment was focussed on the effect of an audit probability that could, in some treatments, depend on the declaration made by a subject and the fact that audits can be linked intertemporally. Audits were linked backward in time when an audit in one period led to the returns from a given number of past periods being audited, and were linked forward in time when a given number of future returns were audited. The backward-linked audits reflect the practice of tax authorities auditing the returns of previous years whenever a taxpayer is found to be non-compliant.

Subjects in the experiment received income, paid taxes on the income that was voluntarily reported, and faced a probability that unreported income would be detected and a fine levied. The values of the tax rate and the fine were chosen to be approximately equal to actual values used in practice. The different treatments varied the rule for selecting tax returns for auditing. The treatments were:

- (1) Audits were random with a fixed probability;

- (2) The probability of audit was dependent upon a subject's actions.
 - (2.i) An audited subject found to be noncompliant in the current period was audited with certainty for a number of future periods.
 - (2.ii) An audited subject faced back audits if found to be noncompliant in the current period.
 - (2.iii) A subject who reported less than some cutoff level of income was audited with certainty.

Each session involved ten subjects who were told that the session would last an unknown number of rounds. The actual number of rounds was always 20. Each subject was given an initial endowment of 5 ECUs, with a fixed exchange rate of 4 ECUs per dollar at the end of the experiment. The income level of a subject was drawn randomly from the interval 2.00 to 4.00 ECUs in increments of 0.20. Tax was paid at the rate of 30% on declared income. The fine was 2 times unpaid tax and was the same in all sessions.

Subjects were volunteers from Principles of Economics classes, and had no prior experience with the experimental setting. Subjects were not allowed to communicate during a session. Each session lasted approximately one hour, and the subjects earned between \$10 and \$20.

The different treatments that were used are summarized in Table 13. The risk-based audit rules were all structured so that the particular selection rule was combined with a random audit rule with a probability of 0.05. Hence, the incremental impact of the each risk-based audit rule was the increase in compliance over that of the random probability.

Random Audit Rules (RA). The audit selection was purely random and made after all subjects reported their income and paid their taxes in a round. The process was to randomly draw one or more balls without replacement from a bingo cage in full view of the subjects. Audit probabilities of 0.05, 0.30, and 0.50 were used and the sessions denoted RA5, RA30, and RA50. In these sessions only current period declarations were examined.

CutoffRule (CO). The cutoff treatment combines a random audit with a probability of 0.05 and a cutoff rule set at 2.5 ECUs. An income report of less than 2.5 ECUs was audited for sure and a report in excess of 2.5 ECUs was audited with a probability of 0.05.

Conditional Future Audit Rule (CFA). This treatment applied a random probability of audit of 0.05 in each period. A subject who was audited and found to be noncompliant in the current period was then audited with certainty for the next two periods. After that the random audit probability of 0.05 applied again. A subject who was audited and found compliant was not audited in the next two periods.

Unconditional Future Audit (UFA). Any subject randomly selected for audit in one period was automatically audited in the following two periods regardless of the current report.

Conditional Back Audit Rule (CBA). The random audit with probability 0.05 was applied but the discovery of any unreported income in the current round led to an audit of the reports for the previous two rounds. All unreported income was discovered and unpaid taxes plus a penalty were paid for all three periods.

Unconditional Back Audit Rule (UBA) The random audit with probability 0.05 was applied but a subject selected for audit in the current round was also audited for the previous two rounds. All unreported income was discovered and unpaid taxes plus a penalty were paid for all three periods.

Information in audit	Periods audited		
	Current, back	Current	Current, future
None (random)	UBA	RA5,RA30,RA50	UFA
Current period	CBA	CO	CFA

Table 13: Experimental design

The experimental results for the different treatments are summarised in Table 14. The compliance rate for the CO, CFA, UFA, CFB, and UFB treatments always exceeded compliance for treatment RA. CO had the highest compliance rate of 0.808. The average compliance rate in CBA of 0.559 was also significantly higher than for RA50. Compliance in CFA (0.516) was greater than for RA50, but the difference was not statistically significant. The main finding is that the risk-based audit rules have higher compliance despite the number of audits being (on average) lower than for RA50.

Session	Information in audit	Periods audited	Average compliance rate (Standard deviation)	Audits per round	Taxes per round
RA5	None	Current period	0.277 (0.105)	0.31	2.48
RA30	None	Current period	0.343 (0.078)	3.31	3.05
RA50	None	Current period	0.492 (0.087)	4.31	4.44
CO	Current period	Current period	0.808 (0.061)	3.94	7.32
CFA	Current period	Current, future period	0.516 (0.119)	1.44	4.67
CBA	Current period	Current, back period	0.559 (0.083)	0.75	5.08
UFA	None	Current, future period	0.427 (0.069)	2.00	3.93
UBA	None	Current, back period	0.290 (0.074)	1.69	2.53

Table 14: Results

The results showed that risk-based audit rules led to a higher compliance level than a random audit rule even if the random probability of audit was high. The cutoff (CO) rule gave the highest compliance but the number of audits was high. Conditioning back audits (CBA) on current declarations also increased compliance significantly. The conditional future audit (CFA) rule was the least effective of the endogenous rules but still had higher compliance than the random audit rule. Some of the risk-based audit rules also reduced the variation in individual compliance rates so had the effect of smoothing tax collection over time.

3.16 Alm and McKee (2004)

This experiment investigated the consequences of alternative audit strategies. The idea behind the experiment was that the selection of tax returns for audit was based on the deviation of each individual return from the average return. Returns that report particularly low incomes should be the focus of the revenue service. However, if this auditing strategy was implemented then the auditing process became a coordination game for the taxpayers. The feature of this game is that taxpayers would be collectively better-off if they succeeded in

coordinating on the declaration of low incomes. The experiment therefore tested two effects (i) the response of subjects to audits, (ii) the ability of the subjects to coordinate on low declarations.

The experimental subjects were drawn from undergraduate classes in business and economics. The subjects were organized into groups of 5. The low number in each group was chosen to increase the likelihood of coordination. The income levels were randomly assigned at the start of each round, and all individuals in a group were given the same income. This process was common knowledge. The tax rate was 30% and the fine rate was 2 times unreported income.

There were two different audit rules. In every case there was the *DIF rule*. This rule involved ranking the income declarations of the subjects on the basis of the deviation of the reported income from the average value of report income. The reports that were furthest below the average were then audited. For example, if reports of 50, 60, 40 were received then the average is 50, and the deviations are 0, +10, -10. The report of 40 (with a deviation of -10) would then be the first to be audited. This was combined in some treatments with a *cut-off rule*: only if the deviation from average exceeded the cut-off would an audit take place. In addition, two treatments added random audits to DIF if no-one exceeded the cutoff. These audit rules were combined with the possibility for individuals to discuss reporting strategies in "chat" during the session.

Table 15 summarizes the main results from the paper on the average compliance rate for each of the treatments. The value reported is the average compliance rate and the standard deviation across subjects is in brackets. The results showed that chat assisted coordination, and that the addition of a random audit raised compliance. The surprise result is that raising the cut-off level to a higher value increased compliance. A higher value of the cut-off permitted larger deviations to go unaudited so it would be expected for subjects to take advantage of this. If they did, then compliance would fall. The experiment revealed that the opposite was the case but did not provide any explanation. The statistical analysis of the data formalized these observations. It showed that compliance was higher under a cut-off rule than with no cut-off. Chat (or cheap talk) always reduced compliance, and that the addition of a random audit increased compliance. Compliance was negatively correlated with an audit in the previous round.

Treatment	Minimum differential cutoff		
	0	0.25	0.5
DIF without chat ²	0.550 (0.058)	0.631 (0.072)	0.748 (0.044)
DIF with chat	0.017 (0.025)		
DIF & random with chat	0.240 (0.082)		
DIF & random without chat	0.705 (0.113)		

Table 15: Average compliance rates

²This treatment is labelled "with chat" in the published paper. This appears to be an error so has been changed to "without chat".

The experiment showed that subjects were unable to coordinate on low declarations (and hence low compliance) when chat was not possible. Combining chat and the absence of a random audit did lead to very low compliance. The addition of a random audit component improved the outcome for the revenue service when chat was possible.

3.17 Alm, Deskins, and McKee (2004, 2007)

The purpose of the experiment was to determine the consequences of taxpayers receiving some income from employment that is reported by a third-party ("matched income") but also having additional self-employed income ("non-matched income") that is not reported by a third-party. The idea was to explore whether there is a higher compliance rate when there is third-party reporting.

The experimental subjects were students from an unidentified university. Income was earned by performing a task that involved moving numbers in the correct order from one location on a computer screen to another location. The subject who finished the task most quickly earned the highest income of 100 ECUs. The second and third place finishers earned 90 ECUs, the fourth and fifth place earned 80 ECUs, etc. Ties were broken randomly. Subjects were informed of the percentage of income that was matchable and how their earnings compared relative to the earnings of other subjects in their session. An income report was then made consisting of a percentage of their matchable and non-matchable incomes and taxes were paid. Audits were then randomly conducted with some known probability. The success of an audit was randomly determined with a known probability. If evasion was detected a fine was paid.

The audit probability for the first 15 rounds of a session was 0.1 and rose to 0.3 for the final 15 rounds. If an audit was conducted it was not necessarily the case that unreported income was discovered. The detection probability for non-matched income was 0.25, 0.50, or 0.75 in different treatments. The probability of detection was fixed at 1.0 for matched income. The fine levied on unreported income was 0.50. At the end of the experiment the ECUs were converted to US dollars at the rate of 90 to 1 and paid in cash. The treatments are described in Table 16.

Treatment	Non-matched income (%)	Tax rate (%)	Probability of detection
1	0	35	-
3	50	35	0.50
5	50	35	0.75
6	50	35	0.25
7	50	20	0.50
8	50	50	0.50
9	100	35	0.50

Table 16: Treatments

The outcome of the experiment was that most individuals had average compliance rates that were at the extremes of what was possible (either close to 1 or

close to 0). A larger portion of non-matched income was in the 0-0.20 average compliance rate range while a larger proportion of matched income fell in the 0.81-1 range. This provided weak evidence that non-matched income was less likely to be declared. It was found that compliance increased as the proportion of non-matched income rose from 0% to 50%. However, the compliance rate dropped sharply when 100% of income was non-matched. The results indicated that tax compliance decreased as the tax rate increased. The difference was much larger between the 20% and 35% rates than between the 35% and 50% rates. Compliance increased between 60 and 70 ECUs of income, but strictly decreased when income rose above 70 ECUs.

The data from the experiment did not provide evidence that tax compliance was significantly affected by the fraction of income that was non-matched. The regression results showed that the percentage of non-matched income was not a statistically significant variable (Table A6). The paper observed that this result left open the question as to why the self-employed have lower compliance than the employed. Two potential explanations were provided: the lack of income withholding for the self-employed, and the self-employed making more mistakes when completing tax returns.

The analysis of the results in the (2004) paper was extended in the (2007) paper by the addition of the probability of detection as a variable in the regression analysis (Table A7). The addition of this variable changed the outcome of the econometric estimation. The new results showed that the percentage of non-matched income became significant in one of the regression equations (Model 1), and was on the border of the significant region in two other equations (Models 2 and 4). This change in outcome is weak evidence that the proportion of unmatched income matters for the compliance decision. If the effect were strong then it would be more likely to be significant regardless of the precise choice of variables for the regression equation.

3.18 Cummings, Martinez-Vazquez, McKee, and Torgler (2005)

The purpose of the experiment was to compare tax paying behaviour across different cultures. It chose to look at the differences between behaviour in Botswana and in South Africa. The experimental part of the paper was very standard. The participants were told their income level (405 ECUs) and requested to make a declaration. The participants knew that audits were random and were told the base probability for the audit and the penalty rate.

The actual probability of audit was given by the formula

$$\text{Actual probability} = \text{Base probability} - 0.001(\text{Actual income} - \text{Disclosed income})$$

It is not clear from the paper whether the experimental subjects knew this formula when making compliance decisions. From what is written in the paper the best interpretation is that they did not. If this reading is correct, then the specification is difficult to understand because the experiment did not have

enough rounds for subjects to learn about the auditing rule. Once filing decisions were made the audits were conducted. There were several treatments (see Table 17) and each subject was involved in several different treatments. In the A series of treatments both the audit probability and the rate of fine were varied. In the B series only the audit probability was varied. The number of rounds and treatments were not made known to the participants. The payment rate was approximately 3 times the average wage. 6 sessions were run in South Africa (88 subjects, 33% non-students) and 6 sessions were run in Botswana (99 subjects, 17% non-students). The tax rate was 30% for all treatments.

Treatments Part A	Audit probability	Fine rate
A1	0.10	1.5
A2	0.30	3.0
A3	0.10	3.0
A4	0.30	1.5
Treatments Part B	Audit probability	Fine rate
B1	0.10	3.0
B2	0.20	3.0
B3	0.30	3.0
B4	0.40	3.0

Table 17: Treatments

The average compliance rates for the different treatments are shown in Table 18. The average rate of compliance was lower in South Africa than in Botswana for every treatment. This provided an indication that there were different cultural attitudes toward tax compliance in the two countries.

Treatments Part A	South Africa	Botswana
A1	0.494	0.617
A2	0.618	0.721
A3	0.485	0.622
A4	0.569	0.418
Treatments Part B		
B1	0.5128	0.5649
B2	0.5974	0.6598
B3	0.6366	0.7468
B4	0.6974	0.7496

Table 18: Average compliance rates

The econometric analysis of the results showed that individual compliance was increased when the audit probability or the level of the fine increased (Table

A8). The age variable had the expected relationship with compliance (compliance increased with age) while the occupation of the subject (either a student or a non-student) was not statistically significant in any specification. The effects of tax culture were investigated by introducing the subject pools as dummy variables and by interacting the pool dummy variables with the tax policy variables related to enforcement. The interaction of audit probability and/or the penalty with South African subject led to lower compliance.

The paper argued that the observed behavioural differences across the subject pools could not be due to differences in risk attitudes (meaning cultural differences about gambling) because the risk-taking behaviour of the subject pools was identical. It concluded that the differences must arise from cultural factors that could be attributed to differences in the fiscal environment of the two countries.

3.19 Alm, Jackson, and McKee (2006)

The aim of the experiment was to explore the idea that audits have both a direct effect on the individual audited and an indirect deterrent effect on other individuals via information dissemination. Several forms of information dissemination and taxpayer communication about audit frequency and audit results were considered. The key distinction was between “official”, or formal, information disseminated by the “government” and “unofficial”, or informal, communication among “taxpayers”.

The base case sessions had no information about audit rates. In treatments with formal communication the same objective audit rates were used but subjects were informed of the official audit rate for the period by the experimenter. Some treatments also reported the actual number of audits and the results of the audits. In a treatment with informal communication the subjects had the opportunity to send messages to the other participants.

Subjects earned income through sorting the digits 1 through 9 into the correct order from a randomized order presented in a 3 by 3 matrix. Actual income was determined by the relative speed of performance. The income range was the same for all sessions with a maximum of 100 ECUs and a minimum of 60 ECUs in increments of 10 ECUs. Once income was known each subject had to decide the amount of income to report to a tax agency. Taxes were paid on reported income, and no taxes were paid on unreported income. If unreported income was discovered through an audit then the subject had to pay the unpaid taxes plus a fine based on the unpaid taxes. The reporting, audit, and penalty process were repeated for a given number of rounds. At the completion of the experiment, each subject was paid earnings equal to the laboratory earnings converted to U.S. dollars.

The treatments involved official information on the audit probability or no official information. The actual number of audits, and the outcome of those audits, conducted in the previous round was announced in some treatments but not in other treatments. In treatments with unofficial communication subjects were allowed to send one message in each round to all other persons in their

group. There was no requirement that the information was truthful. For all treatments the tax rate was 35%, level of the fine was 1.5 times unpaid tax, and subjects were organized into groups of six to eight. The probability of audit varied from 0.05 to 0.40 in all treatments. The compliance rate was defined as the simple average across all subjects and all rounds for a given treatment.

The econometric analysis of the experimental data showed that official information on the outcome of audits had a small negative effect on compliance but that allowing unofficial communication among subjects increased compliance (Table A9). Looking more closely at the content of unofficial communication about compliance behaviour showed that content was also important. A message that other subjects had evaded or had not been audited caused the compliance rate to fall. Compliance rose when the message was that other subjects had complied or had been audited. These results reflect the social aspect of the evasion decision.

3.20 Gërxhani and Schram (2006)

The experiment had two central motivations. First, that the possibility of evasion is dependent upon income source. Second, that the level of evasion may differ across countries. This motivated the design of an experiment in which the subjects chose the source of income before making a declaration and running the experiment in Albania and the Netherlands.

Each session had 12 subjects in 3 groups of 4, and 8 rounds. In rounds 1-3 the only aspect was the determination of the income for each subject. In rounds 4-8 an income declaration had to be made as well. Tax revenue was pooled and returned equally to all group members. The experiment used five subject pools: (i) high school pupils; (ii) university students; (iii) high school teachers; (iv) university non-academic personnel; (v) university teachers. A value of probability of an audit of 0.5 was applied to subjects from all pools, but for some of the student pool a value of 0.166 was also used. The tax rate was 25%, and the level fine was 0.25 time evaded tax, over and above the correct payment being made. The fines were not added to the revenue pool that was shared among subjects.

The income for each subject was determined by a random draw. Each subject could choose to draw from either the "X-distribution" or the "Y-distribution". The X-distribution stochastically dominated the Y-distribution (meaning, that the probability of drawing a high income was larger for the X-distribution) but subjects were told that subjects choosing from the X-distribution would always be audited in rounds 4-8. The choice in rounds 1-3 indicated risk aversion. Income from the Y-distribution was audited with the announced probability. The X-distribution was designed to be interpreted as formal labour market income that was reported by a third party to the tax authority. The Y-distribution was meant to represent unregistered incomes with no third-party reporting.

The first result of the paper was that unregistered income (choices from the Y-distribution) was chosen more frequently when tax evasion was possible. In every subject pool the proportion of unregistered income choices was higher in

rounds 4-8 than in rounds 1-3. The proportion of choices of unregistered income did not vary across treatments. The amount of underreported income was higher in the Netherlands than in Albania, and higher for pupils and students than for teachers. The audit probability did not affect evasion in Albania, but a higher probability reduced evasion in the Netherlands.

The econometric analysis modelled evasion as a discrete choice with value either 0 (no evasion) or 1 (evasion) and the choice of income distribution as a discrete variable with value 0 (X chosen) or 1 (Y chosen) in rounds 4-8.(Table A10). The variable "Job" captured people in employment (full or part time), so the variable "Not labour market" captures students with no part-time work. Choice 1-3 was the number of Y choices in rounds 1-3. Country was 0 (Albania) or 1 (Netherlands). The estimation results showed that the Dutch evaded more than the Albanians, while those not in the labour market evaded more. Females evaded less than males. The only statistically significant variable for choice of income distribution (whether to choose registered or unregistered income) was the choice made in earlier rounds.

3.21 Mittone (2006)

The central focus of the experiment was the consequence of having tax revenue returned as a payment to subjects. The return of revenue had first appeared in the experiment of Spicer and Becker (1980, 3.2) and several of the experiments that followed.

The subjects for the experiment were 30 undergraduate students of economics at Trento (15 men, 15 women). The experiment ran for 60 rounds. Income was allocated to subjects. In rounds 1 until 48 the income was 0.51 euro cents, then fell to 0.36 euro cents from round 49 to 60. The tax rate was 20% from round 1 until round 10, 30% from round 11 until round 30, and 40% from round 31 onwards. The probability of an audit probability was 0.06 from round 1 until round 21, then 0.10 from round 22 until round 40, and 0.15 from round 41 until the end. Any subject audited and found to have evaded paid the amount of the tax evaded, plus a fine equal to the tax evaded multiplied by 4.5. In the basic treatment an audit was backward-looking over the current round and the previous three rounds of the experiment.

The different treatments were defined as follows: Treatment 1 was the standard (base) experiment without redistribution of tax revenue. Treatment 1.0 had no backward-looking tax audits and in Treatment 1.10 the tax audit was extended over the previous 10 rounds. Treatment 1.W adopted a visual device (a red and white wheel) to show the subjects their probability of being audited. Treatment 2 included the redistribution of the tax revenue. Treatment 3 was the same as Treatment 2, except that the tax revenue was used to finance the provision of a public good (a scholarship fund that was really built up at the end of the experiment). Treatment 4 was a generic gamble with every reference to taxation eliminated (following the work of Baldry (1987)).

The subjects were divided into two groups. The auditing of the two groups was independent and undertaken in rounds randomly drawn at the beginning

of the experiment. The first group were audited in rounds 13, 31, 34, 48, 54, 58, and the second group in rounds 3, 24, 27, 40, 46, 50.

The aggregate results on evasion and tax revenue for the treatments are given in Table 19. The major result was that there was less evasion in Treatment 2 where the tax yield was redistributed. A similar outcome occurred for Treatment 3 in which the public good was provided. The removal of the backward-looking tax audits in Treatment 1.0 provided the highest number of instances of evasion. The gamble in Treatment 4 provided the third-highest number of instances of positive gambles being taken.

Treatment	Instances of evasion (max. 1800)	Total tax yield (euro)
1	951	0.2
1.0	1250	0.13
1.10	869	0.21
1.W	1037	0.19
2	499	0.25
3	715	0.24
4	1012	0.2

Table 19: Aggregate results

These results support the hypothesis that tax compliance is increased when taxpayers perceive a benefit from the tax system. In the case of this experiment the results suggest that the cash benefit was valued more highly than the public good so had a greater impact on compliance. Removing backward-looking audits significantly lessens the deterrent effect of auditing so the increase in instances of evasion is not surprising.

3.22 Alm, Jackson, and McKee (2009)

This paper was the published version of Alm, Jackson, and McKee (2006, 3.19). The results that are given differ slightly from the earlier version.

The experimental subjects were undergraduates students who earned income by undertaking a task. In the "Series A" treatments the probability of an audit was announced. In contrast, for the "Series B" treatments the probability of an audit was not announced. All experimental sessions lasted for 30 rounds.

Table 20 provides a summary of the treatments and the aggregate results. Unofficial communication always increased compliance, particularly when there was no official announcement of the audit probability (Treatment 3B versus Treatment 1B). When the official audit probability was announced (treatment of type "A") then official announcement of the audit results lowered the average compliance rate (Treatment 2A versus Treatment 1A) but when the audit probability was not announced (treatments of type "B") the official announcement of audit results increased the compliance rate (Treatment 2B versus Treatment 1B). The consequence of announcing the audit probability was not entirely clear at the aggregate level. Comparison of treatments in which the audit results were not announced and there was no unofficial communication showed that official

information on the audit probability lowered the average compliance rate. When the audit results were announced but still without unofficial communication, the announcement of the audit probability lowered compliance. With unofficial communication but no announcement of audit results the average compliance rate fell when the audit probability was announced.

Treatments A: audit probability announced	Allow unofficial taxpayer communication?	
Do not publicly announce audit results		
Official information provided?	No	Yes
Treatment	1A	3A
Number of subjects	48	62
Compliance rate	0.515	0.516
Audit yield	\$13.64	\$17.67
Publicly announce audit results		
Treatment	2A	
Number of subjects	72	
Compliance rate	0.44	
Audit yield	\$19.50	
Treatments B: audit probability not announced	Allow unofficial taxpayer communication?	
Do not publicly announce audit results		
Official information provided?	No	Yes
Treatment	1B	3B
Number of subjects	32	40
Compliance rate	0.594	0.649
Audit yield	\$16.36	\$15.04
Publicly announce audit results		
Treatment	2B	
Number of subjects	72	
Compliance rate	0.646	
Audit yield	\$16.18	

Table 20: Experimental design and aggregate results

The econometric analysis of the results included different explanatory variables compared to the (2007) paper.(Table A11). The results showed that public announcements led to higher compliance when the probability of audit was unknown but lower compliance when the probability of audit was known. The analysis also showed that communication among the subjects about being audited led to a rise in compliance when the probability of audit was unknown but no change in compliance when the probability of audit was known.

The experimental results were interpreted as suggesting that the revenue service should pre-announce the audit rate and reinforce this with reports of the frequency of actual audits undertaken. If pre-announcing the audit probability is not deemed credible then an alternative is to audit a sufficiently high fraction

of individuals that unofficial communication transmits the occurrence of audits sufficiently often. It is observed that these inferences were limited by the fact that communication would lower compliance unless the probability of audit was much higher than the actual level. An explanation for this observation is that the perceived value of the (unknown) probability of audit is higher than the actual value. If so, announcing the probability will lower compliance.

3.23 Bazart and Pickhardt (2009)

The paper used an experiment designed to explore the effects of positive rewards for correct compliance. It was observed that the standard policy tools for increasing compliance involve punishment (legal action, fines, imprisonment). The paper proposed that encouragement might be considered as an alternative to punishment.

The form of encouragement used in the experiment was entry into a lottery for honest declarations. Subjects who were audited but found to be fully compliant were entered into a lottery in which they could win further payments.

The experiment was conducted in Chemnitz and in Montpellier. The subjects were first year students from business administration and economics. There were seven subjects in each session. Five of the subjects acted as "taxpayers", one acted as an "institution", and 1 acted as a "public good calculator". The 5 subjects acting as taxpayers received an income of 100 ECUs per round and were told there was a "generally accepted rule" that 20 ECUs should be submitted to the institution in each round. However, they were also told that any number up to 20 could be given and that there was a random audit with penalty to check compliance. In the first 6 rounds all voluntary payments, plus fines paid following audits, were pooled and paid back as a public good. In the second set of 6 rounds the subjects chose between a higher or a lower probability of audit. If the higher probability of audit was chosen there was a positive incentive in the form of a lottery for honest taxpayers. The third block of rounds had a higher fine on unpaid tax. The typical earnings over the 18 rounds were between 20 and 30 euro.

In the first 6 rounds the probability of audit was 0.1, the tax rate on non-declared income was 30%, and the level of the fine was one half of unpaid tax. In the second 6 rounds the subjects who had chosen a high audit rate were placed in Group 1 and faced with a probability of audit of 0.2. When audited and found to be honest they were entered into a lottery with a prize of 72 ECUs. This prize was taken from the public good fund. The other subjects were those who had chosen the low probability of audit rate. They were placed in Group 2 and continued with a probability of 0.1. In the final 6 rounds the tax rate on non-declared income was increased to 40% and the fine was increased to exactly the value of unpaid tax.

It was claimed that results demonstrated that the positive reward offered by the lottery had a strong positive impact on compliance behaviour and that the impact was particularly strong for male subjects. In addition, it was suggested that the introduction of the lottery for honest taxpayers would be successful

in terms of an overall revenue increase for economies with a low rate of tax compliance and a high share of male taxpayers.

3.24 Alm, Cherry, McKee, and Jones (2010)

The issue of concern for this experiment was that much evasion occurs among people who are outside the tax system and do not file tax returns. This motivates the policy of using tax filing as an eligibility criterion for the receipt of welfare payments. This issue was studied in an experimental setting using a variety of inducements for filing, including social safety nets and tax credits.

The experimental subjects earned income through their performance at a number sorting task. The actual income was determined by the relative speed of performance, with the fastest performer receiving the highest income and the slowest performer receiving the lowest income. Subjects then decided how much of their income to report. Taxes were paid on reported income only, but unreported income could be discovered by random audits. If unreported income was discovered, the correct tax payment was made and a fine was levied proportional to the unpaid tax. The income earning, income reporting, auditing, and levying of fine were repeated over a number of rounds. The number of rounds was not revealed to the subjects in advance but was always 20. All subjects were paid in cash at the end of the experiment with ECUs converted to U.S. dollars. The subjects were undergraduate students at a major US public university. The groups were of size seven to ten people. No communication was allowed during the experiment. The subjects earned between \$14 and \$38. One hundred and six subjects participated in twelve sessions, with each session lasting between 18 and 20 rounds. The subjects averaged 20 years of age, and 51.9 percent were female.

After income was earned subjects were informed of the tax rate, the audit probability, and the penalty rate on discovered evasion. For the credit treatments, subjects were informed of the tax credit they were eligible to receive and that receipt of the credit was conditional on filing. For the income support treatments, subjects were informed of the probability of being unemployed, the duration of unemployment, and the income support for which they were eligible. The tax return was not provided at this point. Subjects chose whether to get a tax return or not, and there could be a cost for purchasing the return. If subjects chose not to obtain a tax return, then they did not file a return and were not subject to an audit in that round. If subjects chose to obtain a tax return, then the cost, if there was one, was deducted from income for the round. Even when subjects obtained the return, they could still choose not to file. A time limit was imposed on filing (75 seconds). If the time expired and a subject had not been filed a tax return they were automatically audited, and an additional fine of 0.1 of unpaid tax was imposed. The determination of who was audited was random, and subjects were audited independently. Each subject observed the process for their own random draw for audit or not. Sessions were conducted in which the inducement for filing a tax return was absent but the other features of the tax filing regime were incorporated. The treatment design

is sketched in Table 21.

Treatment	Sample variable		
Positive inducement	Cost of tax form and probability of audit		
Refundable tax credit	Conditional on low income	Available to low and median income	Available to all income levels
Employment risk	Support: moderate percentage of previous income	Support: high percentage of previous income	

Table 21: Treatment design

The treatment variables included the cost of obtaining the tax return (“Return Cost”), the audit probability, the opportunity to claim a credit (“Credit”), the availability of group audit information, the availability of unemployment benefits, and whether unemployment was possible. Observed outcomes included the subject’s earned income (“Income Earned”), whether the subject bought the tax form (“Form Bought”), and whether the subject filed the form (“Form Filed”). Table 22 presents the values used for the tax rate etc. in the treatments without inducements for filing a tax return. The probability of audit was set at 0.4 for the first 8 rounds, changed to 0.3 for the second 8 rounds, and reverted to 0.4 for the final 4 rounds. Subjects were instructed that the rate might change during the course of a session but were not told the pattern. In all cases the probability of audit was shown to each subject by the random draw. The tax return cost from zero to two ECUs, with the cost known.

Tax rate	Audit probability	Fine rate	Deduction	Income range	Return cost
35%	Rounds 1-8: 0.4 Rounds 9-16: 0.3 Rounds 17-20: 0.4	1.50	15%	10 to 100 1 per level	2,1,0

Table 22: Conditions for No inducement treatments

The first treatments with an inducement for filing a return involved tax credits targeted at lower-income taxpayers. This targeting aims to induce ghosts to submit a return. The Tax credit treatments are shown in Table 23. These differ in the equation linking income to credit.

Income range	Fine rate	Audit probabilities	Credit equation
10 to 100 1 per level	1.50	Rounds 1-8: 0.4 Rounds 9-16: 0.3 Rounds 17-20: 0.4	CR=20-0.2I (Moderate income credit)
10 to 100 1 per level	1.50	Rounds 1-8: 0.4 Rounds 9-16: 0.3 Rounds 17-20: 0.4	CR=30-0.6I (Low income credit)

Table 23: Conditions for treatments with Tax credits

The second inducement modelled in the experiment was Income Support provided during periods of unemployment. Subjects were informed of their unemployment when it occurred and the duration of the unemployment spell. Benefit payments were computed as a stated percentage of average income filed in previous periods. The value used for the variables in the sessions with unemployment are shown in Table 24. The treatments were the replacement rate and the number of filing periods necessary to qualify. Unemployment benefits were taxable so subjects were offered the opportunity of obtaining and submitting a tax return.

Probability of unemployment	Percentage benefits	Filing periods required	Audit probabilities	Return cost
0.4 and 0.2	0.5	2	Rounds 1-8: 0.4 Rounds 9-16: 0.3 Rounds 17-20: 0.4	2,1,0
0.4	0.6	2	Rounds 1-8: 0.4 Rounds 9-16: 0.3 Rounds 17-20: 0.4	2,1,0

Table 24: Income support settings

The paper provided the results of an econometric analysis of a “Return Bought” model and a “Return Filed” model (Table A12). For each model two econometric specifications were used. One specification (“1W” in Table A12) controlled for unobserved subject heterogeneity while the other specification (“2W” in Table A12) controlled for both subject heterogeneity and time period effects. The Hausman test suggested that the time effects were insignificant so should not be included within the econometric model.³ The results indicated that both forms of positive inducement encouraged submitting a return. The provision of a tax credit significantly increased the buying and filing of tax returns. The provision of unemployment benefit also significantly increased submission of returns but had no significant effect on “Form Bought”. The likelihood of purchasing a tax return was negatively related to the cost of the return. In the tax credit treatments compliance was negatively correlated with income.

The results are interpreted as suggesting that inducements can increase tax filing and that the most effective inducement is the access to tax credits.

3.25 Alm, Cherry, Jones, and McKee (2010)

The experiment was directed toward explaining the changing relationship between taxpayers and the revenue service. The paper observed that the “enforcement” role had moved toward being a “service” role. In the latter role the tax

³The Hausman test is used to check whether the specification of an econometric model is correct.

administration becomes a facilitator for compliance and a provider of services. The question of interest for the experiment was the effectiveness of taxpayer service programmes for increasing tax compliance. The idea underlying the experiment was to complicate the compliance decision and then provide services from the "tax administration" to assist experimental subjects with compliance.

The experiment was standard in many respects. The subjects were recruited from undergraduate students and staff at a US public university. Subjects earned income by performing a task and submitted a report of tax liability. Audits occurred with an announced probability, contingent on a return being submitted, and any unreported taxes were discovered. Unpaid taxes and a penalty were then collected. Subjects chose whether to obtain a tax return or not, and some treatments had a cost for the return. If a subject chose not to obtain a return they did not file and were not subject to an audit in that round. A subject could choose not submit a return, in which case the audit probability was reduced to zero. After-tax earnings were converted from ECUs to US dollars at the rate of 80 ECUs to 1 US dollar at the end of the experiment. Subjects were informed of the audit probability, the penalty rate, and the tax rate. The tax rate was 35% for all sessions and the fine was one half of unpaid tax.

The process of submitting a tax return was complicated by the introduction of the possibility of claiming an allowed deduction (or a reduction in taxable income) as well as a tax credit (comparable to the US Earned Income Tax Credit). The tax deduction was set at 15% of income, and both a Low Income Tax Credit and a more general Income Tax Credit were considered. Both forms of credit started at a given income and declined at a stated rate as income increased, and receipt was conditional upon filing. In some treatments the exact levels of the deduction and the credit were uncertain to the subjects at the time of submitting a return. Subjects were informed about the mean values and ranges of credits and deductions but mean-preserving spreads created uncertainty. This resulted in potential uncertainty about tax liability which permitted the provision of information services that resolved the uncertainty. The audit probability was varied within sessions, and the subjects were informed of the zero probability of audit if no tax form was filed. The tax credit created an incentive to file a tax return. In the treatment with information services the information provided was complete, accurate, and costless. The treatments are summarized in Table 25.

Tax liability uncertain?	Information service	
	No	Yes
No	1	-
Yes	2	3

Table 25: Treatments

A time limit was imposed on submitting a tax return. If a subject obtained a return but the time expired without the return being submitted then the subject was automatically audited and an additional 10 percent penalty was imposed.

If no information was provided on tax liability then subjects only learned their true liability if they were audited. Overpaid taxes were not returned to the taxpayer. The audit probabilities were set at values of 0.3 or 0.4, and all subjects experienced both rates during a session. The probability that an individual was detected evading taxes was the same for all lines on the tax return, or income, credits, and deductions. Information on the variables is summarised in Table 26.

Variable	Values
Income	Mean = 50, high = 100, low = 10, increment = 10
Audit probability	0.3 and 0.4
Fine rate	150% fixed across all sessions
Tax rate	35% fixed across all session
Deduction	20% with uncertainty via a uniform distribution
Income tax credit	Credit = $30 - 0.1 \cdot \text{income}$, with uncertainty via a uniform distribution
Low income tax credit	Credit = $30 - 0.6 \cdot \text{income}$, with uncertainty via a uniform distribution

Table 26: Variables

Table 27 reports the aggregate results for filing and reporting for the three treatments. The aggregate numbers show that uncertainty about tax liability increased the rate of submission of tax returns (the submission rates are higher for treatments 2 and 3 than for treatment 1). Uncertainty and no information reduced the reporting compliance rate (treatment 2 compared to treatment 1) but providing information that resolved the uncertainty increased the rate of reporting to a level above that with no uncertainty (treatment 3 compared to treatment 1).

Treatment	Tax return submission rate	Reporting compliance rate
	mean	mean
1 (no uncertainty)	0.6948	0.6731
2 (uncertainty, no information)	0.7029	0.6205
3 (uncertainty, information)	0.7282	0.7044

Table 27: Aggregate results

The paper reported the estimated coefficients for a linear probability model of the tax return submission decision (Table A13). The dependent variable was the probability that an individual submitted a tax return in a given period. The estimated equation showed that submission behavior was slightly affected by uncertainty and that the effect was offset almost exactly by the provision of information services. Estimates were also given for the tax reporting decision from a multivariate model⁴ with the dependent variable being the reporting compliance rate of an individual in a period. The estimated coefficients indicated that

⁴A Tobit specification was used to model the data.

uncertainty reduced the reporting compliance rate but that the provision of an information service that resolved the uncertainty increased tax reporting to an extent that more than offset the effect of uncertainty. Taken together these results showed that taxpayers underreported liability when there was uncertainty but when this uncertainty was resolved they responded by increasing reporting but not by increasing the probability of submitting. The remaining coefficients were generally consistent with previous studies. Submitting and reporting were higher for women than men, and were negatively correlated with both income and wealth. Submitting was also negatively correlated with the cost of obtaining the tax return. The audit probability was not a statistically significant determinant of submitting or reporting. Those who prepared their own tax return appeared to be more prone to submitting and to be more truthful in reporting tax liability.

The paper offered the conclusions that uncertainty reduced both the submitting and the reporting compliance of taxpayers and that information provided by the revenue service had a positive impact on filing and on reporting for taxpayers who submitted a return

3.26 Wahl, Kastlunger, and Kirchler (2010)

The paper explores the slippery slope framework which is the idea that tax payments are increased by *trust* in authority leading to voluntary compliance or by the *power* of authority which leads to enforced compliance. If either of these factors diminish then evasion will rise. Two experiments were run to test these effects.

The first experiment was conducted in the laboratory with 120 subjects. The subjects were informed of their income in each period (3500 ECUs), tax liability in each period (1400 ECUs, which was a 40% tax rate), the audit probability (0.10) and the fine if caught (the amount evaded). Each subject had to make a decision on how much tax to pay. This situation was repeated for 20 periods. Subjects were expected to believe they were located in a fictitious country and had to read a description of that country. There were four experimental conditions (low/high trust, low/high power) which were suggested by descriptions in the text. The effect of the descriptions upon the subjects' perception of trust in the tax authority and the power of the tax authority in the fictitious country was tested during and after the experiment. The different descriptions proved to have a significant effect on perception. After filing taxes the motivational posture was assessed to classify tax payment into voluntary or enforced.

An econometric analysis showed that after controlling for gender, age, and income the subjects complied more when the authority was described as trustworthy rather than untrustworthy (Table 28). Compliance was also high when the authority was described as powerful rather than weak. Tax payments were highest when trust and power were high and lowest when trust and power were low. Women complied more than men.

Dependent variable	Low trust		High trust	
	Low power <i>n</i> = 30	High power <i>n</i> = 31	Low power <i>n</i> = 30	High power <i>n</i> = 29
Mean tax payments	655.83 (80.87)	795.97 (78.22)	718.74 (81.20)	1042.58 (80.60)
Voluntary tax compliance	2.81 (0.22)	2.59 (0.22)	4.49 (0.22)	5.21 (0.22)
Enforced tax compliance	3.78 (0.16)	4.81 (0.16)	2.93 (0.16)	3.37 (0.16)

Table 28: Estimated Means and Standard Errors

The second experiment involved 186 self-employed taxpayers on the basis that they had more experience of paying taxes. This was not a laboratory experiment but is worthy of inclusion for its use of a non-student subject pool. The experiment was conducted with an online questionnaire. This was sent to self-employed people known to the researchers and these people were asked to forward it to further self-employed respondents. At the start of the questionnaire employment status was requested. Those in employment were then dismissed from the sample. The self-employed were randomly assigned to one of the four descriptions of the imaginary country. After reading the description they were then asked to answer three questions on general intention to pay taxes in the imaginary country, then questions to assess voluntary or enforced compliance, then questions on strategic taxpaying behaviour. The data from 127 of the subjects was suitable for analysis.

The results of the analysis (Table 29) showed that the subjects who were instructed that the authority was untrustworthy also reported less intention to pay taxes than subjects told that authority was high-trust. Subjects who read about a low-power authority indicated less tax compliance than participants who read about a high-power authority. The highest tax compliance was observed for a high-trust and high-power authority. Subjects were more voluntarily compliant when authorities are high-trust compared to low-trust. There was more voluntary tax compliance for subjects with a high-power authority rather than a low-power one. It should be observed that these are responses to hypothetical questions so do not involve the financial rewards that feature in previous experiments. This reduces the validity of the findings.

Dependent variable	Low trust		High trust	
	Low power $n = 32$	High power $n = 31$	Low power $n = 36$	High power $n = 28$
Intended tax payments	4.16 (0.28)	5.02 (0.29)	4.96 (0.27)	5.84 (0.30)
Voluntary tax compliance	4.29 (0.20)	3.60 (0.20)	5.45 (0.19)	5.43 (0.21)
Enforced tax compliance	4.39 (0.14)	5.26 (0.14)	3.64 (0.13)	3.94 (0.15)
Strategic taxpaying behaviour	4.12 (0.30)	4.70 (0.30)	4.31 (0.28)	3.58 (0.32)

Table 29: Estimated Means and Standard Errors

The paper concluded that both experiments demonstrated that when trust and power increased tax payments also increased, and that these effects seem to be quite robust.

3.27 Kastlunger, Muehlbacher, Kirchler, and Mittone, (2011)

This was an experiment into the effect of monetary rewards on tax compliance. The paper observed that providing a reward for honest tax compliance increased the expected value of the honest option in the compliance decision. This was modelled by introducing a reward that was paid when a taxpayer was audited but found to be compliant. Since audits were random this ensured that the receipt of a reward for a compliant taxpayer occurred with the same probability (the probability of being audited) as a fine for non-compliant taxpayer.

The experimental subjects were eighty-six undergraduate from the University of Trento, in Italy (39.5% women, 60.5% men). Three experimental treatments were used (control condition without rewards; reward of 200 ECUs; and reward of 400 ECUs). For each treatment, two sessions were conducted in groups of fifteen participants. The income in each period was 1,000 ECUs. Subjects were informed about the tax rate (20%), the audit probability (0.15), and that if audited they would pay the tax due plus a fine. The size of the reward was fixed in each treatment but varied across treatments. The three values of reward were 0 ECUs (the control case), 200 ECUs, and 400 ECUs. The two positive rewards were large relative to the level of income. The fine rate also varied across treatments (values of 1.0, 2.0, and 3.0 were used). The earnings for the experiment were linked to relative performance. The lowest performer earned 5 euro and the highest 15 euro. Each experimental session lasted for approximately one hour.

The outcome of the experiment was that there was no significant difference between the outcomes in the three treatments. Raising the reward increased the level of tax payments but did not affect the number of zero declarations. The paper reported that gender did make a difference but did not provide the definition of the gender variable. It is therefore not possible to infer the direction

of the effect. The paper purposes that the experiment shows the reward of honest taxpayers does not generally increase compliance.

4 Non-laboratory Research

The four papers that are described in this section are outside the series of laboratory-based experiments that have been reported above since they used different methodologies. However, they are merit inclusion on the basis that they illustrate alternative methodologies or make noteworthy observations on compliance.

4.1 Slemrod, Blumentahl, and Christian (2001)

This was a field experiment conducted on taxpayers in Minnesota. The experimental procedure was to select a sample group of 22,368 taxpayers that had filed a Minnesota tax return during 1994 that had been processed by the end of December 1995, or for whom a federal tax return had been filed during 1995. A letter was then sent to 1724 randomly selected taxpayers from this group. The remaining 20,831 taxpayers were not contacted as part of the experiment and were used as the control group. The letter informed the taxpayers that the return they were about to file would be "closely examined". The data on the tax returns (state and federal) of the individuals receiving the letter were made available for the year of the intervention (the 1994 return) and the year preceding the intervention (the 1993 return).

The results showed that the effect of the treatment variables depended upon the level of income. The low-income and middle-income taxpayers who received the letter increased the amount of income reported relative to the control group. The increase in income was also dependent on the source of income, which indicated the effect of opportunity to evade. The surprise result was that the reported tax liability of the high income treatment group fell sharply relative to the control group. It was proposed that this could be explained by the incentive to reduce the probability of an audit when the probability was less than one, as opposed to the belief that not all income would be discovered if audited for sure.

4.2 Gërxhani (2007)

The paper used a field survey of households in Tirana, Albania, in 2000. The idea of the paper was to test for a gender difference in evasion behaviour. This has been a feature of many of the laboratory experiments. The paper also wanted to test the relative importance of education income, age, number of children and other factors.

The data were obtained from a self-administered questionnaire. The survey sample was 1500 households with respondents selected randomly over a geographical area. The main income earner in each household was requested to

complete the questionnaire. The response rate was very high with 1340 questionnaires being returned.

The questionnaire used various indirect questions that were tax-related to explore compliance behaviour. From the responses three indicators measuring the occurrence, but not the amount of evasion, were constructed. These were binary $\{0, 1\}$ variables according to whether the responses indicated the presence of evasion, or not, for personal income tax, small-business income tax, and insurance tax. According to the constructed variables women evaded much less than men. Attitudes also varied according to gender: women had less agreement than men with propositions about the correctness of the Albanian tax system but agreed more strongly that they should pay the taxes they were supposed to and that evasion was high in Albania.

The paper also looked at personal characteristics and their effect on evasion behaviour. The strongest effect was from higher education which decreased the probability of evasion significantly. After taking these other characteristics into account, there was still evidence that women were less likely to evade than men.

4.3 Kleven, Knudsen, Kreiner, Pedersen, and Saez (2011)

The paper reports on the outcome of a field experiment in Denmark. The sample was 42,800 individuals in Denmark who were chosen to be representative of the population. In the initial year (2007) one half of the same were randomly selected for rigorous audit treatment while the remainder were not audited. In the next year (2008) letters containing the threat of an audit was randomly sent to individuals in both groups. The individuals were not informed that they were part of an experiment. One group received a letter stating that an audit would certainly take place, a second group received a letter stating that half the group would be audited, and a third group received no letter. These different letters provided an exogenous variation in the probability of being audited.

The effect of audits on future reported income was studied by comparing the audit and no-audit groups. This showed that audits had a strong positive impact on reported income in the following year. The effect of the probability of audit on reported income was analyzed using the threat-of-audit letter and no-letter groups. It was found that audit threats had a positive impact on self-reported income and that the effects were stronger for the threat of an audit for certain than for the threat that half the group would be audited.

4.4 Barile (2012)

This paper used an experimental methodology but is not directly related to the tax compliance literature. The aim of the paper was to analyse tax evasion as a factor that potentially affects internal control of firms. The question was the effect the possibility of evasion had upon the contract between the owner of the firm and the manager of the firm.

The motivation for exploring this question was based on the following reasoning. If the owner is responsible for making the compliance decision and the

manager suffers no consequences if evasion is discovered then there are no implications for the contract. Conversely, if the manager does bear a cost when evasion is discovered the contract will need to include a risk premium to compensate for this outcome. However, the contract cannot be made contingent on the outcome of an illegal activity (evasion) so it must be incomplete. This reduces risk-sharing and distorts the effort decision of the manager.

The experiment simulated a labour market. The experiment consisted of the owner offering an incentive contract to the manager. The manager responded with a choice of effort. The owner could either accept the effort level and implement the contract, or offer a revised contract. If offered a second contract, the manager had to make an all-or-nothing decision to accept (and provide effort already offered) or reject and receive nothing. There were three treatments. The baseline treatment had no possibility of evasion. In the other two treatments the manager was indirectly liable if the owner decided the firm would evade tax. These treatments differed according to whether the manager was risk neutral or risk averse. The degree of risk aversion was introduced through the structure of payoffs.

The results showed a positive relationship between wages offered by principal and effort provided by agent. In general, higher wages lead to more effort provision. However, when evasion and risk aversion were introduced into the analysis, individuals showed opportunistic behaviour and seemed to be less willing to cooperate for the benefit of the firm.

5 Summary and Observations

The previous section has reviewed a significant number of papers that differ in specification and conclusions. The purpose of this section is to summarise the key elements of the review in an accessible form and to offer some observations on elements of experimental design.

There are several results that are common to almost all of the experiments.

- The fine rate and the probability of audit have the expected effect on compliance: an increase in these variables will raise compliance if it is known to subjects.
- Gender matters for the compliance decision. Men evade more often than women and evade more.
- Age also matters: older subjects evade less. It should be noted that in many of the experiments the age range of the subjects was very narrow.
- The effect of the tax rate on compliance is not constant across the experiments. This is in line with the theory: the most basic model predicts compliance should increase as the tax rate increases (due to fine being proportional to the tax rate) but more complex models looking at interaction with expenditure can predict the converse. The experimental results are supportive of these observations.

- There is weak evidence of lower compliance for income derived from sources without third-party reporting.

Most of the treatments that have been employed deliver the expected outcome. Are there any surprise results in the experiments? What is possibly surprising is the number of treatments that have no effect on compliance behaviour despite a clear argument and expectation of an effect. Two examples of this are the terminology used to describe the experiment (though there is conflicting evidence on this) and the limited effect of communication between subjects. A possible explanation for the lack of a treatment effect is that behaviour is not determined by the optimisation process envisaged by the experimenter. For example, compliance behaviour driven by a social custom will often not respond to small variations in treatments.

Table 28 provides a summary of the experiments. The information is not comprehensive but is intended to convey the focus of the treatment in the experiment and the key results. The table also shows how income has changed from being allocated to being earned and the almost universal use of students as experimental subjects.

Experiment	Subjects	Sample size	Income	Treatments	Key results
Friedland et al. (1978)	Students	15	Allocated	Fine rate, tax rate	Higher tax rate increases evasion
Spicer and Becker (1980)	Students	57	Allocated	Information on average tax rate	Relative tax rate affects evasion
Friedland (1982)	Students	13	Allocated	Vague or precise information	Vague information increases deterrence
Spicer and Thomas (1982)	Students	54	Allocated	Information on audit probability	Decision to evade is affected
Spicer and Hero (1985)	Students	36	Allocated	Level of compliance in prior game	Evasion decision not affect
Baldry (1986)	Unreported	Unknown	Allocated	Evasion or gambling	Evasion is not the same as gambling
Becker et al. (1987)	Students	116	Earned	Transfer of tax revenue	Benefits determine evasion
Alm et al. (1990)	Students	60	Allocated	Probability, fine, amnesty	Amnesties reduce compliance
Beck et al. (1991)	Students	112	Allocated	Uncertainty about tax liability	Tax rate not significant
Collins and Plumlee (1991)	Students	120	Earned	Information used in audit rule	Conditional audits reduce evasion
Alm et al. (1992a)	Students	Unknown	Random	Uncertainty on fine, tax, probability	Effect linked to public good provision
Alm et al. (1992b)	Students	At most 120	Random	Tax, fine, probability, public good	Public good effect complex
Martinez-Vazquez et al. (1992)	Students	157	Described	Alternative withholding scenarios	Withholding position not important
Alm et al. (1992)	Students	72	Random	Probability, multiplier, terminology	No framing effect
Alm et al. (1993)	Students	80	Random	Rule for audit selection	Endogenous rule better
Alm and McKee (2004)	Students	40	Random	Audit rule and chat	Chat could coordinate low compliance
Alm et al. (2004)	Students	326	Earned	Percentage of matched income	Non-matched on edge of significant
Cummings et al. (2005)	Mixed	187	Allocated	Country, fine, probability	Compliance varies across countries

Table 28: Summary of results

Experiment	Subjects	Sample size	Income	Treatments	Key results
Alm et al. (2006)	Students	326	Earned	Official and unofficial information	Unofficial information raises compliance
Gérxhani and Schram (2006)	Students, Staff	144 92	Random	Registered or unregistered income	More evasion from unregistered
Mirtone (2006)	Students	240	Allocated	Audit backdating, transfers	Transfers reduce evasion
Alm et al. (2009)	Students	326	Earned	Official and unofficial information	Unofficial information raises compliance
Bazart and Pickhardt (2009)	Students	63	Allocated	Lottery as reward for honesty	Lottery increases compliance
Alm et al. (2010)	Students	338	Earned	Filing and eligibility for welfare	Inducements raise filing
Alm et al. (2010)	Students, Staff	131	Earned	Uncertainty and service	Uncertainty reduces compliance
Wahl et al. (2010)	Students Self-employed	124 186	Allocated Questionnaire	Trust and power	Responses as expected
Kastlunger et al. (2011)	Students	86	Allocated	Reward for honesty if audited	No significant effects

Table 28 (continued): Summary of results

Several observations can be made about the elements of the design of the experiments that have been surveyed.

1. The use of neutral terminology. The experiments provide conflicting results on the effect of framing the experiments as tax compliance. Some experiments have proposed the use of neutral terminology to not bias the experiment by labelling it as compliance. If the purpose of the experiment is to capture actual tax compliance behaviour it seems important that this is made clear in the terminology.
2. The choice of subject population. Almost all of the experiments have used students for the subject population. However, there is no evidence that students behave in the same way as experienced taxpayers. More recent experiments have begun to move away from a student subject base, but have used only a small sample of non-students. Only one of the laboratory experiments (Cummings et al., 2005) included in this review used people from outside an educational background as subjects. This is seen as a key point since the student population will in general have little experience of paying taxes. They are therefore more likely to act in a “strategic” way in the experiment rather than act in a “natural” way.
3. The process of income determination. In most of the experiments income is either directly allocated to subjects or is randomly allocated. The more recent experiments have moved toward using earned income generated from a real-effort task. The process of income determination should matter due to the ownership of income that is felt if it has to be earned. It is hard to see any argument against having income that is earned. The other issue about income is that of whether it is observed by the tax authority or not. Some experiments have had explicit differences between registered and non-registered income. In others it seems to be the case that the source of income was left to the interpretation of subjects.
4. Information about audit probabilities and strategy. When the values of the audit parameters have been varied in treatments they have had the expected effect. But this is a consequence of the values of the parameters being announced in the experiment. When they are not announced then variation does not have an effect. The question is whether they should be announced or not. If an experiment is trying to capture “natural” behaviour then they should not be announced but should reflect existing beliefs that are carried in to the experimental setting. There is a good case that people behave on the basis of their personal belief about these parameters and are not informed about the true values.

6 Conclusions

The Introduction stressed the potential value of conducting experiments on tax compliance. Amongst these was the ability of the experimenter to vary the

decision-making environment in a controlled manner to draw out the implications for behaviour. This has been amply demonstrated in this survey. A wide variety of treatments have been reported, ranging from the basic variables describing the tax and enforcement system through to concepts of customer service from the tax authority. It is difficult to see how such a range of variables could have been explored using more traditional empirical techniques.

However, there are always a number of caveats to consider when drawing conclusions for policy from experimental results. There is never any guarantee that an experiment draws out actual behaviour and this may be particularly acute for an issue as complex as compliance. The fact that some characteristics, such as gender and age, have implications that are consistent across experiments is reassuring. Variation of the basic auditing probabilities and strategy also leads consistently to the expected change in behaviour. Against this must be set some of the inconsistencies. The effect of varying the tax rate is unclear, and the framing effect caused by the choice of terminology remains to be confirmed or refuted. The fact that communication between subjects does not have the expected effect is not necessarily a failing. Instead, it could indicate mechanisms at work, such as social customs brought into the experiment from outside experience, that strongly govern compliance behaviour.

The major limitation with the typical experiment is the choice of subject population. The use of students in the context of compliance is very questionable. Students do not have experience or engagement with the taxation process. Good arguments have been advanced to support the contention that compliance is determined by broader social factors than monetary rewards. It is unlikely that students will have internalized these social factors which, if correct, implies that there experimental behaviour will not match that of the typical taxpayer. None of the experiments surveyed provide evidence to substantiate these claims. A thorough comparison of the behaviour of student and non-student subject pools in compliance experiments would be a valuable contribution to the literature.

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Appendix: Econometric results

Explanatory variables	Dependent variable		
	Proportion evading (p)	Fraction of income not declared (x)	Overall fraction of income reported (q)
Tax rate	0.43 (4.0)	0.27 (2.5)	-0.36 (3.3)
Owens car?	0.30 (2.4)	-0.05 (0.4)	-0.16 (1.3)
Age (years)	-0.16 (1.3)	-0.33 (2.6)	0.41 (3.2)
Employed	0.13 (1.0)	0.11 (0.8)	-0.07 (0.6)
Married?	-0.35 (2.5)	0.44 (3.2)	-0.27 (1.9)
Male (0) or female (1)	0.28 (1.5)	-0.38 (2.1)	0.24 (1.3)
Magnitude of fine	-0.10 (1.0)	-0.18 (1.7)	0.11 (1.0)
Buys lottery tickets?	-0.02 (0.12)	0.25 (1.5)	-0.24 (1.5)
Constant	0.38 (1.2)	0.75 (2.6)	0.44 (2.0)
R^2	0.40	0.43	0.39

Table A1: Regression analysis (t values in brackets)
Friedland, Maital, and Rutenberg, 1978, section 3.1

Variable	Estimated coefficient
Perceived relative tax rate	0.260** (2.18)
Gender (female = 1, male = 2)	0.354*** (2.81)
Mean tax resistance scores	0.189 (1.49)
Age	-0.045 (0.31)
Income	0.139 (0.95)

Table A2: Regression results (Dependent variable = percentage of taxes evaded, t -values in brackets)

Spicer and Becker, 1980, section 3.2

Note: The tax resistance scale was derived from responses to a 15-item questionnaire. The items were statements regarding tax evasion. The subjects indicated how well each statement fitted their own feelings, experiences, and behaviour.

	Group A (precise)	Group B (imprecise)	Group C (no information)
Percentage of taxes evaded	-0.253*	-0.176	0.010
Likelihood of evasion	-0.315**	-0.376***	-0.141

* Significant at 10%, ** Significant at 5%, *** Significant at 1%

Table A3: Correlations with audit probabilities
Spicer and Thomas, 1982, section 3.4

	Estimate	Standard error
Constant	0.761	0.944
Income	0.092	0.044
Expected auditing probability	-0.028	0.015
Transfer payment	-0.367	0.194
Perceived tax burden (too low/fair = 0, too high=1)	-1.266	0.557

Table A4: Dependent variable is propensity to evade
Becker, Büchner, and Slesking, 1987, section 3.7

Variable	Model 1	Model 2
	Estimated coefficient	Estimated coefficient
Constant	-0.1458 (0.49)	-0.0559 (0.19)
Income	0.3943 (4.31)	0.3579 (4.16)
Tax rate	-2.3630 (7.75)	-2.3710 (7.73)
Fine rate	0.0252 (1.43)	0.0251 (1.42)
Audit rate	5.7502 (1.89)	5.7691 (1.89)
Public good		-0.4068 (1.599)
Public good x group fund		0.9153 (2.06)

Table A5: Regression on declared income
t-statistics in brackets
Alm, Jackson, and McKee, 1992b, section 3.12

	Dependent variable: % of total income reported			
Variable	Model 1	Model 2	Model 3	Model 4
Gross income	-0.454 (0.072)	-0.507 (0.071)	-0.286 (0.070)	-0.471 (0.074)
Percent non-matched	-0.046 (0.030)	-0.042 (0.029)	0.037 (0.029)	-0.036 (0.031)
Tax rate	-0.776 (0.098)	-0.994 (0.099)	-0.881 (0.095)	-0.781 (0.100)
Audit probability	0.323 (0.323)	0.323 (0.077)	2.145 (0.142)	0.361 (0.083)
Prepare taxes	-	-18.357 (1.776)	-	-
Wealth (total earnings)	-	-	-0.034 (0.002)	-
Audit last round	-	-	-	-0.246 (2.101)
Constant	109.5 (7.09)	126.4 (7.16)	97.10 (6.88)	109.42 (7.24)

Table A6: Generalized least squares regression results
Standard errors in parentheses
Alm, Deskins, and McKee, 2004, section 3.17

	Dependent variable: % of total income reported			
Variable	Model 1	Model 2	Model 3	Model 4
Percent non-matched	-0.062 (0.027)	-0.054 (0.026)	0.021 (0.026)	-0.053 (0.027)
Audit success rate	0.150 (0.057)	0.104 (0.056)	0.106 (0.055)	0.163 (0.058)
Gross income	-0.660 (0.065)	-0.693 (0.064)	-0.482 (0.064)	-0.677 (0.067)
Tax rate	-0.776 (0.098)	-1.023 (0.098)	-0.882 (0.095)	-0.782 (0.100)
Audit probability	0.245 (0.070)	0.245 (0.068)	2.087 (0.126)	0.289 (0.074)
Prepare taxes	-	-20.744 (1.534)	-	-
Wealth (total earnings)	-	-	-0.034 (0.002)	-
Audit last round	-	-	-	-1.645 (1.865)
Constant	119.88 (7.35)	139.43 (7.16)	108.93 (7.09)	119.48 (7.49)

Table A7: Generalized least squares regression results
Standard errors in parentheses
Alm, Deskins, and McKee, 2007, section 3.17

Independent variable	Model 1	Model 2	Model 3
Constant	0.1734 (1.64)	0.2412 (2.29)	0.3524 (3.64)
Age	0.0100 (3.91)	0.0114 (4.48)	0.0111 (4.36)
Occupation ($S = 1$)	0.0537 (1.22)	0.0355 (0.81)	0.0389 (0.89)
Audit probability	0.0249 (1.82)	0.0245 (1.36)	
Penalty rate	0.0411 (2.28)	0.0245 (1.36)	
South Africa		-0.1574 (6.32)	
S. Africa*penalty			-0.0332 (3.10)
S. Africa*audit probability			-0.1663 (4.24)
Log-likelihood	-1320.51	-1300.61	-1306.13
LR chi sq.	38.31	78.12	67.07

Table A8: Tobit estimation, Dependent variable = compliance rate (Declared income/Actual income)

Cummings, Martinez-Vazquez, McKee, and Torgler, 2005, section 3.18

	Specifications			
	Model 1 Individual factors only	Model 2 Official information	Model 3 Unofficial communication	Model 4 All unofficial communication
Constant	0.745*** (0.033)	0.737*** (0.034)	0.721*** (0.033)	0.712*** (0.033)
Income	-0.067** (0.032)	-0.068** (0.033)	-0.070** (0.033)	-0.063*** (0.033)
Wealth	-0.235*** (0.007)	-0.237*** (0.007)	-0.236*** (0.007)	-0.229*** (0.007)
Own prepared tax	-0.013* (0.008)	-0.015* (0.008)	-0.019** (0.008)	-0.016** (0.008)
Age	0.011** (0.001)	0.012*** (0.001)	0.011*** (0.001)	0.011*** (0.001)
Male	-0.203*** (0.008)	-0.202*** (0.008)	-0.206*** (0.008)	-0.199*** (0.008)
Audit results announced		-0.031*** (0.008)		
Audit probability announced x audit probability		0.067** (0.028)		
Audit probability announced x lagged audit		-0.022* (0.014)		
Audit probability not announced x lagged audit			0.115*** (0.012)	0.110*** (0.012)
Unofficial communication			0.045*** (0.008)	
Unofficial message- subjects evaded				-0.036*** (0.005)
Unofficial message- subjects complied				0.042*** (0.007)
Unofficial message- subjects not audited				-0.009*** (0.004)
Unofficial message- subjects audited				0.029*** (0.005)
Wald statistic	2589.62***	2504.06***	2712.60***	2926.41***
Log-likelihood	-4816.65	-4700.10	-4652.72	-4589.87

Table A9: Econometric analysis, dependent variable is compliance rate
z-statistics in parentheses, significance levels * 0.10, ** 0.05, *** 0.01

Alm, Jackson, and McKee, 2006, section 3.19

	Evade	Choice
Constant	-2.359**	0.137
Country	0.867**	-0.172
Not labour market	1.295**	-0.453
Audit prob. 0.06	0.912**	-0.320
Choice 1-3	-0.709	2.089**
Earn 4-8	2.615	-2.247
Age	0.720	-1.024
Job	0.134	-0.170
Gender	-0.753**	-0.045
ρ (random effects estimator)	0.312**	0.287

Table A10: Regression analysis.
Gërzhani and Schram, 2006, section 3.22

	Model 1 Individual factors only	Model 2 Official information	Model 3 Unofficial communication	Model 4 All unofficial communication
Constant	0.558 (8.94)	0.549 (9.24)	0.551 (9.520)	0.544 (10.03)
Income	-0.177 (3.80)	-0.168 (3.59)	-0.076 (1.66)	-0.086 (1.88)
Wealth	-0.219 (19.34)	-0.218 (19.46)	-0.181 (16.12)	-0.187 (16.77)
Prepared own tax return	-0.031 (2.51)	-0.031 (2.49)	-0.060 (5.43)	-0.060 (5.40)
Age	0.009 (4.89)	0.0096 (4.89)	0.021 (11.71)	0.022 (12.50)
Male	-0.174 (15.47)	-0.174 (15.47)	-0.171 (15.62)	-0.174 (16.06)
Audit probability	0.931 (11.03)	0.974 (11.59)	-0.061 (0.78)	-0.047 (0.60)
Audit probability x round	-0.017 (1.93)	-0.019 (2.15)	0.016 (1.95)	0.018 (2.10)
Lag own audit	-0.009 (0.47)	-0.029 (1.82)	-0.005 (0.27)	0.003 (0.27)
Lag own audit x official information provided	-0.043 (1.48)		0.011 (0.46)	

Table A11: Regression results
Alm, Jackson, and McKee, 2009, 3.22

	(continued)			
	Model 1	Model 2	Model 3	Model 4
Unofficial message allowed	0.103 (1.21)	-0.011 (0.72)	0.280 (4.40)	0.113 (7.81)
Official information provided	-0.082 (2.51)	-0.074 (5.23)	0.128 (4.15)	0.088 (6.52)
(Lag) Official total number audited reported	0.012 (2.04)		-0.026 (4.83)	
(Lag) Unofficial message: subject not audited	-0.013 (1.00)		-0.037 (3.53)	
(Lag) Unofficial message: subject audited	0.017 (1.29)		0.026 (2.27)	
(Lag) Unofficial message: subject complied	0.027 (3.16)		0.023 (1.89)	
Round	0.008 (2.51)	0.008 (3.34)	0.00 (0.08)	-0.001 (0.27)
Round x official audit information	-0.001 (0.07)		0.001 (0.16)	
Round x unofficial audit information	-0.002 (0.63)		0.00 (0.06)	
Wald statistic	1390.80	1340.36	856.68	809.52

Table A11 (continued): Regression results
Alm, Jackson, and McKee, 2009, 3.22

Independent variable	Form filed 1W	Form filed 2W	Form bought 1W	Form bought 2W
Constant	-0.779 (0.416)	-0.541 (0.452)	-0.295 (0.539)	-0.235 (0.532)
Form cost	–	–	-0.309 (0.182)	-0.303 (0.160)
Income earned	0.004 (0.002)	0.004 (0.002)	0.006 (0.002)	0.005 (0.002)
Audit probability	0.589 (0.722)	-0.036 (0.813)	0.895 (0.725)	0.540 (0.821)
Credit	1.406 (0.401)	1.414 (0.406)	1.111 (0.443)	1.123 (0.447)
Income earned x credit	-0.012 (0.003)	-0.012 (0.003)	-0.011 (0.003)	-0.011 (0.003)
Unemployment benefit	0.788 (0.397)	0.735 (0.403)	0.419 (0.463)	0.395 (0.467)
Unemployment (lagged 2 periods)	0.269 (0.139)	0.338 (0.142)	0.273 (0.140)	0.324 (0.143)
Wald Chi-square	31.14	50.73	35.21	46.44
Log likelihood	-921.61	-911.10	-913.11	-907.03

Table A12: Econometric results
Alm, Cherry, McKee, and Jones, 2010, section 3.24

Independent variables	Dependent variable	
	Tax form filing	Reporting compliance rate
Constant	0.8081 (0.000)	0.6719 (0.000)
Income	-0.0003 (0.020)	-0.0006 (0.012)
Wealth	-0.0003 (0.000)	-0.0002 (0.000)
Audit probability	0.0107 (0.907)	-0.0339 (0.813)
Lag audit	-	-0.0099 (0.330)
Tax form cost	-0.1323 (0.000)	-
Tax liability uncertainty	-0.0426 (0.021)	-0.1424 (0.000)
Tax agency information	0.0475 (0.028)	0.1972 (0.000)
Age	0.0022 (0.007)	0.0074 (0.000)
Male	-0.1607 (0.000)	-0.3622 (0.000)
Prepare own tax	0.0293 (0.036)	0.0557 (0.003)
Dependent	0.1314 (0.000)	0.3187 (0.000)
χ^2	221.15 (0.000)	676.40 (0.000)
N	2620	2489

Table A13: Estimated coefficients
Alm, Cherry, Jones, and McKee, 2010, section 3.25