Can Strategic Uncertainty Help Deter Tax Evasion? — An Experiment on Auditing Rules

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Abstract

This paper adds to the economic-psychological research on tax compliance by experimentally testing a simple auditing rule that induces strategic uncertainty among taxpayers. Under this rule, termed the *bounded* rule, taxpayers are informed of the maximum number of audits by a tax authority, so that the audit probability depends on the joint decisions among the taxpayers. We compare the bounded rule to the widely studied *flat-rate* rule, where taxpayers are informed that they will be audited with a constant probability. The experimental evidence shows that, as theoretically predicted, the bounded rule induces the same level of compliance as the flat-rate rule when strategic uncertainty is low, and a higher level of compliance when strategic uncertainty is high. The bounded rule also suppresses the "bomb crater" effect often observed in prior studies. The results suggest that strategic uncertainty due to interactions among taxpayers could be an effective device to deter tax evasion.

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1 Introduction

Tax evasion is as old as the income tax itself. However, it was not until 1972 that Allingham and Sandmo presented the first economic model of tax evasion behavior based on Gary Becker's economics-of-crime approach (see Allingham and Sandmo (1972)). Taxpayers choose between honestly stating their income or cheating on taxes, which results in either extra money if not detected, or financial losses otherwise. Prior research has often assumed that tax authorities audit taxpayers with a constant and exogenous probability. We refer to this as the *flat-rate* rule (see, e.g., Spicer and Thomas (1982), Alm et al. (1992), Alm et al. (2009), Kastlunger et al. (2011) and Kleven et al. (2011), as well as the literature reviews by Andreoni et al. (1998), Alm and McKee (1998), Slemrod and Yitzhaki (2002) and Kirchler et al. (2007)).

While simple and intuitive, the Allingham-Sandmo framework neglects the potential impact of social interactions among taxpayers. Recent tax evasion studies argue from an economic psychology perspective that compliance decisions are affected by personal, social and societal norms (Kirchler (2007)).[2] In other words, the compliance decisions of taxpayers do not merely depend on their isolated assessments of economic variables such as income, audit probability and fine, but also on their beliefs about what they should do and what others would do. Given the limited audit resources of a tax authority for a fixed period of time, a taxpayer's belief regarding the compliance decisions of others may affect his own compliance decision and consequently the ex-post probability of being audited. This could lead to distinctive tax evasion dynamics and equilibria across societies.

So far, most of the studies on tax compliance norms (especially social norms) elicit beliefs through surveys or experiments concerning the extent to which people think others would evade taxes or whether this kind of behavior could be justified (see, e.g., Torgler (2002), Wenzel (2005), Alm and Torgler (2006)).[3] This paper takes a different approach by observing evasion behavior directly in a laboratory environment that induces strategic uncertainty among taxpayers. We create the strategic uncertainty by informing the taxpayers of the maximum number of audits to be carried out, instead of telling them directly what the audit probability is. We refer to this as the *bounded rule* because the number of audits is bounded by the limited resources of the tax authority.

[2] A personal norm, which is defined as "a moral imperative that one should deliberately comply", is associated with factors such as moral reasoning, religious beliefs and political party preference. A social norm, according to Wenzel (2005), is "prevalence or acceptance of tax evasion among a reference group" (e.g. friends, colleagues or acquaintances). A societal (or culture) norm, which reflects the general attitude towards tax evasion in a large population, is often addressed as tax morale or civic duty.

[3] There are a few exceptions, such as Fortin et al. (2007) and Lefebvre et al. (2011), that inform the taxpayers of the evasion decisions of others in order to examine how the information would affect the taxpayers' compliance decisions.

Studying the bounded rule is interesting for two reasons. First, the bounded rule naturally incorporates the analysis of beliefs via game theory. Since the tax authority only conducts a fixed number of audits, the actual audit probability faced by a taxpayer is endogenously determined by the evasion decisions of other taxpayers. Consequently, a taxpayer has to infer the audit probability by forming expectations on the decisions of others. The second reason is that, the bounded rule, relative to the flat-rate rule, describes the actual auditing practice more realistically. Most organizations, public and private alike, plan their activities such as auditing according to the committed budget of a period. Once the budget is allocated for a certain purpose, it becomes difficult to be reshuffled during the course of a fiscal year. Given the fixed audit capacity of a tax authority in a certain period, it is difficult to commit to a pre-specified audit probability.

This paper studies two research questions. First, could the bounded rule induce the same level of compliance as the flat-rate rule widely studied in the literature? Second, how does the level of strategic uncertainty affect behavior? That is, how do taxpayers react when they are less certain about the actual audit probability as a result of peer interactions? In such circumstances, are they more likely to think that others will cheat on taxes?

We take an experimental approach to examine these questions. Compared to empirical data from the field, the laboratory offers tight controls on the tax-reporting institutions such as audit probability, tax rate, and income level. By carefully selecting the relevant parameters, we can directly compare the actual compliance behavior under the two auditing rules which are equally deterrent in theory. Moreover, we can measure tax evasion behavior repeatedly and inexpensively in the laboratory without the errors that may otherwise occur in field data (for more discussion on the methodology of experimental methods on tax evasion, see, e.g., Alm and McKee (1998) and Torgler (2002)).

Our laboratory setting follows the key features of a classical tax compliance game first developed by Graetz et al. (1986). Every taxpayer has a certain probability of receiving high or low income.[4] Knowing a certain auditing rule (flat-rate or bounded), they have to decide simultaneously and independently whether or not they will report their income truthfully to the tax authority. Then the tax authority implements the auditing rule, depending on the treatments. In the flat-rate rule treatment, every low-income report is audited with a constant probability. In contrast, the bounded rule audits a randomly selected sample of low-income reports whenever the number of

[4] Such a binary-income setting, or similar discrete-type extensions, are used in many studies (e.g., Mills and Sansing (2000), Alm and McKee (2004), and some others cited in footnote 4 of Yim (2009)).

these reports exceeds the maximum number of audits allowed by the budget. Otherwise, it audits all of the low-income reports.

To examine our first research question, we select parameters for the bounded rule such that 1) the theoretically predicted deterrence effect of the bounded rule in this treatment is statistically equivalent to that of the flat-rate rule; and 2) the level of strategic uncertainty is low, such that profit maximizers have a dominant strategy to cheat on taxes. To study the second question, we increase the level of strategic uncertainty faced by taxpayers. As a result, the equilibria depend on the independent beliefs of the taxpayers. If they are too optimistic (pessimistic) about other taxpayers' propensity to cheat, they will tacitly coordinate to cheat (to report truthfully) in equilibrium.

The main results of our experiment are as follows. Supporting our hypothesis, the compliance levels of the bounded rule and the flat-rate rule are statistically the same when strategic uncertainty is low. When the level of strategic uncertainty increases, the bounded rule becomes more effective in deterring tax evaders, even though the maximum number of audits of a tax authority does not change. The data also suggest that the bounded rule suppresses the "bomb crater" effect observed in previous studies, which refers to a pattern whereby evasion is high immediately after an audit. While the bomb crater effect still exists under the flat-rate rule, the opposite takes place under the bounded rule: Subjects become more compliant immediately after an audit.[5]

This paper makes several contributions to the economic psychology literature on tax compliance. Our experiment provides evidence suggesting that strategic uncertainty could help to deter tax evasion. Under the bounded rule, even though taxpayers may be aware of the limited audit capacity of a tax authority, the uncertainty about the decisions of others makes it difficult to assess the actual audit probability. This is particularly true when the degree of uncertainty is high.

In addition, this is the first study empirically examining the bounded rule, which explicitly models interactions among taxpayers, and therefore offers a way to study the effect of norms on tax evasion decisions game theoretically. By experimentally comparing the levels of compliance induced by the bounded rule and the widely studied flat-rate rule, we set the stage for using the bounded rule to examine the effect of norms on taxpayers' decisions in the future.

Last but not least, our analysis on the dynamics of compliance behavior suggests that the effect of past audits on future behavior is tightly linked with the auditing institutions. Compared to a flat-rate rule with uncertainty arising from the nature, a bounded rule highlights the influence of uncertainty arising from the compliance behavior of others on the audit probability, which leads

[5] Kastlunger et al. (2009) call this the "jump effect".

taxpayers to react differently to past audits. Thus, it would be interesting to examine further the dynamic behavioral patterns, such as the bomb crater effect, in various institutions.

The idea of the bounded rule (i.e., examining up to some fixed number of audits) was first studied in a theory paper by Yim (2009). In that paper, the tax authority interacts strategically with taxpayers by choosing an audit capacity without openly committing to it before taxpayers make their reporting choices. The main result is that the bounded rule can always induce the same level of compliance as the flat-rate rule of a certain given audit probability. To facilitate the design of an experiment, we modify Yim's model by allowing the tax authority to commit to a fixed audit capacity. The reason is that any off-equilibrium decisions by subjects taking the tax authority's role will have unpredictable impacts on others taking the taxpayer role, leading to unmanageable complications in comparing the treatment results. Therefore, our experiment is not a strict test of Yim (2009).

Our paper is related to the tax compliance literature on conditional auditing rules. Some studies argue that an efficient way to deter tax cheaters is to let audit probabilities depend on history. Some of these studies examine a forward-looking rule in which the audit probability and fine increase if taxpayers are caught cheating on taxes in the current period (see, e.g., Harrington (1988), Landsberger and Meilijson (1982) and Greenberg (1984)). In contrast, Friesen (2003) proposes an alternative rule where the fine and the audit probability decrease when taxpayers are compliant.

Another strand of literature lets the audit probability depend on reported income. Reinganum and Wilde (1985) analyze an "audit cut-off" policy in which an audit is triggered if the reported income is below a certain threshold, and otherwise no audit if the reported income is above the threshold. Follow-up papers conclude that if audit probability could depend on reported income, then the optimal strategy for the auditor is to randomly audit individuals who report below some threshold level of income. In equilibrium, only low-income taxpayers report honestly, while high-income taxpayers report exactly at the threshold level (Sanchez and Sobel (1993), Cremer and Gahvari (1996), Mookherjee and Png (1989), Scotchmer (1987), and Bayer and Cowell (2009)).

The above-mentioned rules are tested and compared in some experimental studies. For instance, Cason and Gangadharan (2006) test the Harrington (1988) rule and find qualitative support. Clark et al. (2004) find that the random auditing rule deters tax evaders more effectively than do the rules by Harrington (1988) and Friesen (2003), although at the expense of more audits. Alm et al. (1993) experimentally compare a purely random rule, a forward (backward) -looking rule and a cut-off rule. They find that the cut-off rule is the most effective in deterring tax evaders. Collins and Plumlee (1991) report similar results. Unlike the bounded rule, however, the cut-off rule requires a large number of random audits.

In all of the papers discussed above, the attention is focused on the interaction between the auditor and a taxpayer, without considering the interactions among taxpayers. A notable exception is Alm and McKee (2004), who experimentally study a "DIF" rule that represents the IRS's audit policy based on discriminant function (DIF) scores. The audit probability of their DIF rule depends on the deviation of an individual's reported income from the average income reported by all other players. This audit rule induces a coordination problem for taxpayers who want to cheat on taxes. They find that a DIF rule combined with some random audits are the most effective mechanism in deterring tax evasion. Our paper differs from theirs in that taxpayers within a group do not always receive the same level of income in a given period. Furthermore, the interaction induced by the bounded rule among taxpayers does not always need to be a coordination game.

The organization of this paper is as follows. Section 2 describes the experiment design, procedures and testing hypotheses. Section 3 reports the treatment effects of the experiment. Section 4 studies the individual-level behavioral dynamics under the two auditing rules. Section 5 contains concluding remarks. The technical details and the experiment instructions can be found in the appendix.

2 The Experiment

2.1 Design

In all treatments of our experiment, the tax compliance game has three stages: (i) income reporting and tax deduction, (ii) audit and fine deduction and (iii) feedback. Subjects receive either a high income $I_{\mathrm{H}} =$ €25 (H-type) or a low income $I_{\mathrm{L}} =$ €10 (L-type) with probability q or $1-q$, respectively. Subjects are informed of the group size N and the probability q. Based on the capacity constraint in the lab, the size of the taxpayer population is fixed to be $N = 8$. The parameter q is either 0.5 or 0.9, depending on the treatment.

During the income reporting stage, subjects have to decide simultaneously and independently which income they should report to a tax authority which is simulated by a computer. The computer automatically deducts taxes according to the reported income. The tax for subjects reporting a "high income" is $T_{\mathrm{H}} = €12.5$, whereas the tax for subjects reporting a "low income" is $T_{\mathrm{L}} = €2.5$.[6] Subjects are told that taxes are deducted based on their reported income instead of received income. For instance, if H-type players submit a "low-income" report, they receive €22.5, instead of €12.5. Similarly, L-type players receive −€2.5, instead of €7.5, if they submit a "high-income" report.[7] In the audit stage, depending on the treatment, the computer implements either a flat-rate rule or a bounded rule to audit "low-income" reports. In the experiment, "high-income" reports are not audited. This is consistent with auditing practices in reality (see, e.g., the Internal Revenue Manual (IRM) guidelines by the IRS (2010)).[8]

Described below is the design of the three treatments of the experiment. Key parameters of the treatments are summarized in Table 1.

Table 1: Experimental treatments

Treatment	**High-income probability** q	**Audit probability** a **or capacity** K	**Number of subjects**
Flat-rate	0.5	$a = 0.4$	64
Bounded	0.5	$K = 2$	64
Bounded-hi-q	0.9	$K = 2$	64

Flat-rate: In this treatment, subjects are told that "low-income" reports independently face an audit probability of $a = 0.4$. This audit probability induces the same compliance rate as the bounded rule does with an audit capacity $K = 2$.[9] If subjects report honestly, nothing will happen

[6] Experimental parameters concerning taxation are chosen to be in line with reality. For instance, the real-world tax rates for high-income and low-income taxpayers are usually dependent on the levels of their incomes. In particular, many countries (such as Britain, the Netherlands, Germany, Italy and the USA) use a progressive tax system instead of a proportional one. Hence, this experiment adopts a progressive tax system for the sake of facilitating subjects' understanding.

[7] Even when a subject with a low income makes a loss by submitting a "high-income" report and that decision is selected for payment, the potential loss is covered by a show-up fee of €3. During the experiment sessions, this situation never actually arose.

[8] Though stylized, the binary-income setting captures some salient features of audit selection in reality. For example, low-income taxpayers in the setting have no incentive to submit "high-income" reports. So these reports must have been submitted by high-income taxpayers. Because *auditing such reports cannot lead to higher tax revenue, these reports are not audited* under either of the audit rules considered in our experiment. Indeed, the IRM prescribes that "[c]lassifiers [who review computer-prescreened tax returns to determine which are to be put forth for examination (i.e., audit)] should compare the potential benefits to be derived from examining a return to the resources required to perform the examination. Although you may identify some potentially good issues on the return, *if they would not yield a significant adjustment, the return should be accepted as filed.*" (emphasis added) (see paragraph 1 of IRM 4.1.5.1.5.1.1 (10-24-2006) in Section 5 "Classification and Case Building" of the manual). In line with this, a recent study by Phillips (2010) shows that the IRS focuses on auditing taxpayers expected to have high unmatched income (i.e., income cannot be cross-checked with third-party reports such as Form W-2) and rarely examines taxpayers likely to have only matched income.

[9] As a flat-rate rule induces all-or-none behavior in compliance in our setting, such a rule with an audit proba-

to their final payoffs. If cheaters are caught by the tax authority, they need to pay their evaded taxes of €10 plus a fine of $F =$ €10.

Bounded: In this treatment, the fine for cheaters is exactly the same as in the *Flat-rate* treatment. The audit probability, however, depends on the total number of "low-income" reports received. The maximum number of audits to be conducted is $K = 2$. This value of the parameter guarantees a unique Nash equilibrium based on non-cooperative game theory (see Section 2.3 and the appendix for details). Setting $K = 2$ means that if the number of "low-income" reports does not exceed two, then all of those reports will be audited with probability 1. Otherwise, the audit probability decreases monotonically with the number of "low-income" reports, denoted by L. In particular, the probability is 0.67 for $L = 3$; 0.5 for $L = 4$; 0.4 for $L = 5$; 0.33 for $L = 6$; 0.29 for $L = 7$; 0.25 for $L = 8$.

Bounded-hi-*q:* Except for the ex-ante probability q of receiving a high income, this treatment is the same as the *Bounded* treatment.[10] Compared to the *Bounded* treatment, subjects cheating in this treatment face a higher degree of uncertainty given the low probability of being an L-type player. Consequently, there are fewer honest "low-income" reports to pool with cheated "low-income" reports, making it easier for the auditor to detect cheating. The theoretical analysis provided in the appendix shows that the game in this treatment has multiple equilibria. We are interested in knowing whether the behavior observed in the *Bounded* treatment is sensitive to the presence of multiple equilibria when q is high.

Admittedly, for each auditing probability in the flat-rate rule a, there exists more than one set of parameters N, K, and q that triggers the same level of compliance based on game theory. We select $N = 8$ based on the capacity of a conventional laboratory. Given $N = 8$, setting $K = 2$ gives us the possibility to examine the various properties of the bounded rule with different levels of strategic uncertainty (parameters qs). To maximize the salience while not to the extreme of $q = 1$, such that all taxpayers in the experiment are surely H-income taxpayers, we believe $q = 0.9$ strikes the best balance in this consideration.

bility $a < 0.5$ theoretically has the same deterrence effect as the bounded rule, assuming the standard setup with perfectly rational, risk-neutral players. We select a relatively high a to prevent readers from attributing the equivalent underreporting rates to a low parameter a (i.e. an easy benchmark).

[10] A real-world example of this treatment could be an area under the jurisdiction of an IRS District Office where taxpayers are more likely to have high income.

2.2 Procedures

The experiment was conducted at the laboratory of Tilburg University from October to December 2009. A total of 192 students (55.21% males and 44.79% females) participated as subjects in the experiment. Most of them majored in economics or business. The experiment instructions, provided in Appendix B.2, were modified from those in prior tax compliance studies, namely Alm et al. (2009), Kim et al. (2005), and Kim and Waller (2005). The experiment was conducted with z-Tree software (Fischbacher (2007)).

Every treatment consists of four sessions of 16 subjects each. The duration of a session is about 1 hour (including the initial instruction and final payment to subjects). Average earnings are €16.23 (including the €3 show-up fee). At the beginning of each session, subjects are randomly assigned to the computer terminals. Before the experiment starts, subjects have to complete some exercises making sure that they understand the rules of the tax compliance game.

The tax compliance game consists of 30 periods. At the beginning of each period, 16 subjects are randomly allocated into two groups of eight. At the end of each period, a summary screen is presented to subjects with feedback information including both the subject's true and reported income, and the final payoff for the period. Subjects are not informed about others' payoffs.

Upon finishing the tax compliance game, subjects are asked to complete a risk elicitation task based on Holt and Laury (2002). The instructions for the risk elicitation task are handed out only after the completion of the tax compliance game. Hence, the subjects are not aware of the existence of the task beforehand. The task measures subjects' risk aversion levels, which could be useful in explaining their behavior.

At the end of the experiment, subjects are asked to complete three sets of questionnaires. The first one explores their perception of the game, as well as the propensity for taking into consideration of the decisions of other participants in the experiment. The second one focuses on social background information such as gender, nationality, and years of studying economics. The third one elicits subjects' ethical orientation by the Machiavellian IV scale personality test (see Christie and Geis (1970)). This test measures a person's predisposition to act in accordance to one's own interests over ethical standards. A higher score indicates that a person is more individualistic and loosely bound to conventional moral standards.

During the payment stage, one period of the tax compliance game and the realization of one lottery of the risk elicitation task are randomly selected to determine the final payment to each subject. This random payment scheme mitigates the potential income effect that the subjects carry across different periods of the game and over to the risk elicitation task.

2.3 Hypotheses

To derive testable hypotheses, we start by assuming that players are self-interested profit maximizers. We then discuss how personal and social norms affect the robustness of predictions.

In this study, the deterrence effect is indicated by the underreporting rate in the population: namely, the proportion of high-income taxpayers filing "low-income" reports in a certain period. As discussed in Section 2.1, L-type players have a dominant strategy of reporting honestly, regardless of the audit rules.[11] Therefore, our analysis focuses on H-type players. In the following, let h denote the honest-reporting choice of an H-type player, and u the underreporting choice.

Flat-rate: In this treatment with $q = 0.5$, the audit probability a_{FR} is set at 0.4. Given this, an underreporting decision is equivalent to selecting a lottery of €22.5 with probability 0.6 and €2.5 with probability 0.4. The expected payoff therefore is: $E(\pi_u) = €22.5 \times 0.6 + €2.5 \times 0.4 = €14.5$, which is larger than the sure payoff €12.5 from honest reporting. Hence, H-type players are expected to submit "low income" reports. Note that q is an exogeneous variable which does not change the theoretical predictions even under the assumption that the subjects are risk averse.

Bounded: In this treatment (also with $q = 0.5$), H-type players again face the tax-evasion gamble of choosing between a sure payoff of €12.5 versus a risky lottery with a high payoff of €22.5, if not audited, but a low payoff of €2.5 otherwise. Unlike the flat-rate rule, however, the audit probability a_{BD} is not exogenously given. Instead, it depends on the audit capacity K set at 2 and the players' perceptions about others' choices. In particular, the audit probability perceived by player i is affected by his subjective belief about how likely a "low-income" report is submitted by another player.

A "low-income" report could come from two sources. The first source is from a truthful L-type player with probability $1 - q$. Alternatively, it could come from H-type players who dishonestly report that they have received a "low income." If a player thinks that the underreporting probability of H-type players i is b_i, this scenario will occur with probability qb_i. Hence, the overall probability B_i of receiving a "low-income" report from player i is the sum of the probabilities in these two situations: $B_i = 1 - q + qb_i$.

[11] The actual percentage of honest reports among L-type taxpayers are 99.68% and 99.28% across treatments, suggesting that they do play the dominant strategy.

The Nash equilibrium in the *Bounded* treatment can be solved by iterated elimination of dominated strategies. The intuition is as follows. Reporting high income is a dominated strategy for L-type players, since they have to pay a high tax and incur a lower payoff than they would otherwise. If the H-type players believe that the L-types obey dominance, then the strategy of reporting truthfully (h) is dominated. That is, even when an H-type player believes that no other players evade taxes, the expected payoff of underreporting is still higher than that of honest reporting. Such a high expected payoff is caused by a low audit probability strictly less than 0.5, which stems from the fact that all of the L-type players (about half of the population) report a "low income" truthfully. The calculation guarantees that evading taxes is always a best response for an H-type player when L-type players obey dominance. Proposition 1 stated below provides the theoretical foundation for our hypothesis for testing.

Proposition 1 *With $q = 0.5$, the game induced by the bounded rule with $K = 2$ is dominance-solvable. In equilibrium, both L-type and H-type taxpayers submit "low income" reports.*

The proof of Proposition 1 is in the appendix. The following hypothesis is built upon the previous analysis.

Hypothesis 1 *Given the current set of parameters, the underreporting rates in the Flat-rate treatment and the Bounded treatment are the same.*

The analysis so far assumes that taxpayers are all self-interested profit maximizers. However, field studies have categorized taxpayers as "typical taxpayers", "honest taxpayers" or "tax evaders" based on their attitudes towards tax evasion (see Kirchler (1998)). Even in controlled laboratory experiments with low stakes and punishment, many studies still find a considerable number of subjects who constantly behave honestly (e.g., James and Alley (2002)). Recent economic-psychology research on tax behavior has focused on the impact of norms on compliance. In particular, we consider two types of norms that may affect taxpayers' decisions. The first type is the personal norm, which is defined as "a moral imperative that one should deliberately comply" (Kirchler (2007), p59). The sources of personal norms, or tax ethics, could be moral reasoning (e.g., Trivedi et al. (2003), Kirchler (1998)), strong religious beliefs (e.g., Torgler (2003)) and political party preference (e.g., Wahlund (1992)). The second type of norm is the social norm, which according to Wenzel (2005), "prevalence or acceptance of tax evasion among a reference group". That means a taxpayer could be "conditionally honest": If he believes that non-compliance is widespread and socially accepted behavior, then he is less likely to comply.

We argue that the treatment effects remain unchanged if the taxpayer population consists of a mixture of self-interested and conditionally (intrinsically) honest players. For this conclusion we assume that: 1) distributions of types are the same for both treatments since the subjects are assigned randomly from a large common population; and 2) expectations are rational in equilibrium. Self-interested profit maximizers only care about their own payoffs, and then will choose to cheat in both treatments. Anticipating this, conditionally honest players will assess the proportion of self-interested profit maximizers in the population. If this proportion is large enough, they will choose to cheat; otherwise, they will report their income honestly. Intrinsically honest players could be considered as a special case of the conditionally honest players who (incorrectly) think that the proportion of self-interested profit maximizers is zero. For the formal analysis, see the appendix.

Bounded-hi-*q:* In this treatment with $q = 0.9$, the bounded rule with $K = 2$ changes the interaction among taxpayers into a coordination game.

Proposition 2 *With* $q = 0.9$*, the game induced by the bounded rule with* $K = 2$ *has two pure-strategy Nash equilibria and one mixed-strategy Nash equilibrium. In the pure-strategy equilibria, L-type taxpayers play their dominant strategy of honest reporting. Moreover, all H-type taxpayers opt for underreporting if they believe other H-type taxpayers cheat with a probability higher than* 0.432*; otherwise, they all opt for honest reporting. A symmetric mixed-strategy Nash equilibrium also exists, with H-type taxpayers each underreporting with probability* 0.432 *and honestly reporting with the complementary probability.*

We focus on the symmetric equilibria because asymmetric equilibria, although exist in this setting, require unrealistic coordination among the ex ante homogenous players. Theory does not predict which equilibrium will be selected. Nonetheless, previous laboratory studies on order-statistic coordination games (e.g., Huyck et al. (1990), Huyck et al. (1991), Blume and Ortmann (2007), Chaudhuri et al. (2005)) and stag-hunt games (e.g., Cooper et al. (1990), Cooper et al. (1992)) have found that coordination is difficult among multiple players. Due to the attractiveness of the secure strategy, players fail to coordinate on the payoff dominant equilibrium. The robustness of this result depends on a series of factors in game structures such as group size and the relative payoff attractiveness of the equilibria, as well as on behavioral determinants such as initial choices and pre-play communication.[12] Since the game structure and the design features in our *Bounded*-hi-q treatment are similar to those of the coordination games tested in the previous experiments, we also expect a stronger attraction for the risk-dominant equilibrium. That is, we hypothesize a higher tendency for subjects to honestly report their income in the *Bounded*-hi-q treatment than in the *Bounded* treatment.

Hypothesis 2 *Given the current set of parameters and results from the previous experiments on coordination games, the underreporting rate in the Bounded-hi-q treatment is lower than those in the Bounded treatment.*

3 Treatment Effects

This section focuses on the aggregate level of the compliance behavior. Table 2 summarizes the underreporting rates across treatments. The first three columns contain averages over 30 periods. The next three columns are averages over the last 10 periods, where subjects' behavior is expected to be more stable after becoming familiar with the environment. The variable "high-income frequency" is the actual frequency of the subjects being assigned as high-income taxpayers in a treatment. "Percentage of 'low-income' reports" is the total number of "low-income" reports received divided by 8, regardless of whether the reports are submitted by genuine low-income taxpayers or dishonest high-income taxpayers. "Underreporting rate" is the percentage of times where subjects when assigned as a high-income taxpayer submit a "low-income" report.

[12] For a comprehensive review on the conditions of coordination failure, see Devetag and Ortmann (2007).

Table 2: Summary statistics of treatments

	All 30 Periods			Last 10 Periods		
	Flat-rate	*Bounded*	*Bounded*-hi-q	*Flat-rate*	*Bounded*	*Bounded*-hi-q
			All subjects			
High-income frequency	0.514	0.491	0.898	0.527	0.519	0.908
	(0.007)	(0.039)	(0.024)	(0.042)	(0.038)	(0.013)
Percentage of "low-income" reports	79.74%	78.85%	40.31%	77.97%	75.94%	32.97%
	(0.074)	(0.015)	(0.053)	(0.066)	(0.018)	(0.055)
			H-type subjects			
Underreporting rate	60.83%	57.11%	33.95%	58.16%	53.32%	26.16%
	(0.144)	(0.049)	(0.038)	(0.143)	(0.052)	(0.046)
Bounded v. Flat-rate	$p = 0.386$			$p = 0.564$		
Bounded-hi-q *v. Bounded*		$p < 0.05$			$p < 0.05$	

Note: We use each session as an independent observation. Standard errors are in the parentheses. Statistical tests on the treatment effects are the two-sided Mann-Whitney rank-sum tests.

We first focus on the *Flat-rate* and *Bounded* treatments. The top panel of the table reports statistics concerning all subjects. The first row of the panel indicates that the actual frequency of being an H-type in the two treatments is very close to their ex-ante probabilities qs. The second row displays the percentage of "low-income" reports out of all reports received (i.e., the total number of reports from L-type players or dishonest H-type players, divided by 8). The ratio is around 80% for both treatments.

The bottom panels of the table provide data for testing our hypotheses. Our findings are summarized as follows:

Result 1 *Hypothesis 1 is supported. The difference between the underreporting rates observed in the Flat-rate and Bounded treatments is statistically insignificant.*

Support: The average underreporting rate is 60.83% in the *Flat-rate* treatment and 57.11% in the *Bounded* treatment. A two-sided Mann-Whitney rank-sum test cannot reject the null hypothesis that the underreporting rates of the two treatments are the same ($p = 0.386$). In the last 10 periods, the magnitude of the difference in underreporting rate becomes slightly larger but is still statistically insignificant ($p = 0.564$).

To see whether our conclusion on Hypothesis 1 is robust, we run a probit regression. The dependent variable equals 1 if an H-type subject underreports in a period, and is 0 otherwise. The independent variable indicates whether the observation comes from the *Bounded* or *Flat-rate* treatment (with or without social demographic controls). Regardless of the different standard error clustering methods (i.e., by subjects or by sessions), the estimated coefficient of the treatment variable is statistically insignificant at the 5% level. This further confirms that the underreporting rates in the two treatments are statistically indistinguishable.

The following is our result from the *Bounded*-hi-q treatment.

Result 2 *Hypothesis 2 is supported. The underreporting rate is significantly lower in the Bounded-hi-q treatment than in the Bounded treatment.*

Support: The average underreporting rate in the *Bounded*-hi-q treatment is 33.95% over all 30 periods and 26.16% over the last 10 periods. The compliance level in this treatment is the highest, as the underreporting rate is significantly lower compared to the *Bounded* treatment ($p < 0.05$). The difference is already salient in the first period and remains highly significant throughout the other periods of the game. This result is in line with what is found in the previous literature on coordination games. That is, the subjects in our experiment fail to coordinate on the payoff dominant equilibrium in which they all underreport their income.

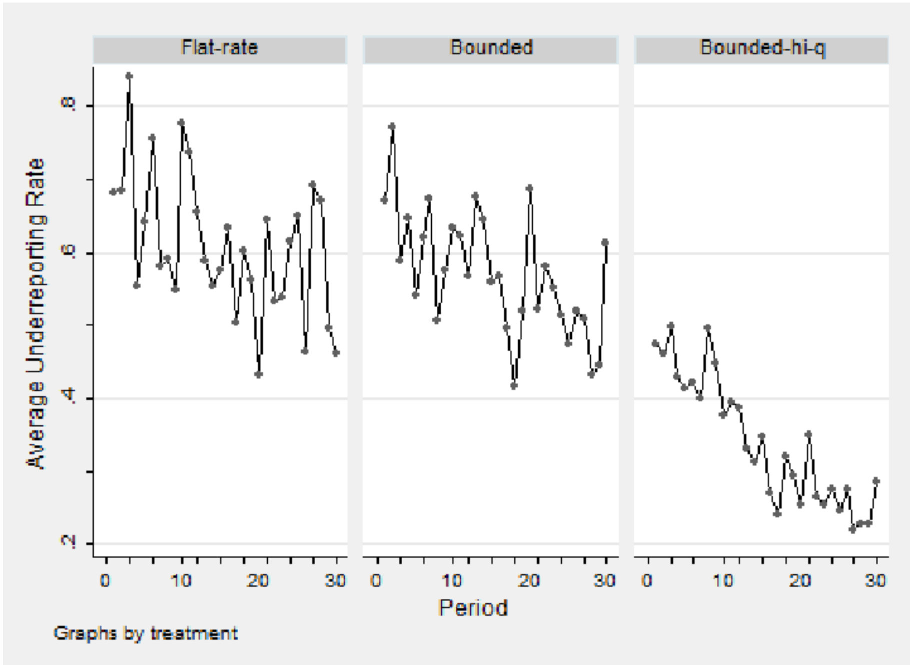
4 Behavioral Dynamics of Tax Evasion

This section focuses on individual-level compliance behavior, and in particular, the tax evasion dynamics. To provide a first impression of the data, Figure 1 depicts the average underreporting rates across treatments. The dynamics of the *Flat-rate* and *Bounded* treatments look similar. In contrast, the average underreporting rate in the *Bounded*-hi-q is visibly lower and declines steadily across periods.

One common feature shared by all treatments is that the aggregate underreporting rates fluctuate across periods. A potential explanation is that subjects attempt to assess the audit probability subjectively based on past audit experience. In the following, we explore how the experience of being audited in one period changes evasion decisions in subsequent periods, and whether responses differ with respect to the two auditing schemes.

So far, the two established patterns observed in many previous experiments are the "bomb crater" effect and the "echo" effect. The "bomb crater" effect addresses the immediate decline in compliance after a tax audit. This term derives from a phenomenon that soldiers in wars hide themselves in bomb craters with the belief that it is unlikely for a bomb to fall in the same place

Figure 1: Dynamics of underreporting rate over 30 periods



twice. In the context of tax evasion, the bomb crater effect predicts an immediate, high level of noncompliance following an audit (see, e.g., Guala and Mittone (2005), Kirchler et al. (2005), Mittone (2006), Bergman (2006)). Kastlunger et al. (2009) consider the noncompliance to be mainly driven by misperception of chance, since people believe that an audit is unlikely to take place consecutively, rather than by a motivation to repair losses in the previous period.

The echo effect emphasizes the importance of an early audit. If an audit takes place in an earlier period rather than a later period, it has a more prominent and persistent effect on tax compliance behavior (see Guala and Mittone (2005) and Mittone (2006)).

We use the following random-effect probit model to examine these effects.

$$y_{it} = \gamma x_{it} + u_i + \varepsilon_{it} \ .$$

The variable y is equal to 1 if subjects decide to underreport, and is 0 otherwise. Furthermore, x is a vector of explanatory variables, the u_i represent individual random effects and γ is a vector of coefficients.

To examine the bomb crater effect, we regress the underreporting decision of subject i at period t on the previous audit experience when i received high income. The variable "past audit experience" equals to 1 if subject i was caught cheating on taxes in the previous period when he received high income, and 0 otherwise. As for the echo effect, we introduce a term called "early detection experience" which equals 1 if the latest previous experience of being caught occured within the first 10 periods.[13] "Gender" equals 1 if a subject is male. "Years of learning economics" represents the number of years a subject takes economic courses. "Econ experience × Game theory" counts for the experience of learning game theory. The four dummies for nationalities represent the four largest cohorts in our sample, with the baseline to be Europeans such as Italian, French and German. "Tax filing experience" equals 1 if a subject takes a part-time job and files taxes. "Degree of risk aversion" reports the total number of safe lotteries selected in the risk elicitation task. "Mach IV score" is based on the personality test by Christie and Geis (1970). MC1 through MC 4 document subjects' answers to questions 6-9 on treatment manipulations in Appendix B.2.2. For both effects, we run two specifications with and without controlling for repeated interactions, social background information (such as gender, major of study, nationality), personal characteristics (risk attitude, Mach-IV score) and belief data (e.g. how likely it is that a subject thinks of the decisions of the others when making his own decision). Results of the regressions are reported in Table 3.

[13] Other thresholds such as period 15 or period 5 do not change the significance of the coefficients for all treatments and all specifications.

Table 3: Probit regressions on the influences of past audits and other characteristics (Dependent variable y_{it}: 1 if underreport, 0 otherwise)

	Bomb crater effect			Bomb crater effect controlling for social characteristics and beliefs			Echo effect			Echo effect controlling for social characteristics and beliefs		
	Flat-rate	*Bounded*	*Bounded*-hi-*q*	*Flat-rate*	*Bounded*	*Bounded*-hi-*q*	*Flat-rate*	*Bounded*	*Bounded*-hi-*q*	*Flat-rate*	*Bounded*	*Bounded*-hi-*q*
Past audit experience	0.021	-0.434**	-0.468***	-0.121	-0.471***	-0.383***	-0.106	-0.509***	-0.660***	-0.056	-0.550***	-0.429***
	(0.150)	(0.170)	(0.131)	(0.149)	(0.171)	(0.136)	(0.170)	(0.190)	(0.144)	(0.175)	(0.202)	(0.167)
Early detection experience	–	–	–	–	–	–	0.049	0.262	0.598***	-0.221	0.261	0.116
							(0.252)	(0.301)	(0.183)	(0.304)	(0.357)	(0.248)
Period	–	–	–	-0.001	-0.084*	-0.093***	–	–	–	-0.013	-0.070	-0.084***
				(0.035)	(0.046)	(0.035)				(0.038)	(0.049)	(0.039)
Period^2	–	–	–	-0.0004	0.0026**	0.0019*	–	–	–	-0.0002	0.002**	0.002***
				(0.0011)	(0.0014)	(0.0011)				(0.0011)	(0.001)	(0.001)
Gender (1 for men)	–	–	–	0.664**	0.445	-0.301	–	–	–	0.659**	0.470	-0.302
				(0.283)	(0.469)	(0.271)				(0.281)	(0.473)	(0.272)
Years of learning economics	–	–	–	-0.130	0.515**	-0.011	–	–	–	-0.127	0.518**	-0.012
				(0.116)	(0.226)	(0.129)				(0.115)	(0.227)	(0.129)
Econ experience × Game theory	–	–	–	0.162	-0.338	0.102	–	–	–	0.160	-0.338	0.101
				(0.098)	(0.220)	(0.112)				(0.098)	(0.221)	(0.112)
Dummy for Eastern Europeans	–	–	–	-1.235**	-0.174	-0.594	–	–	–	-1.238**	0.118	-0.609
				(0.586)	(0.934)	(0.483)				(0.582)	(0.940)	(0.485)
Dummy for Dutch	–	–	–	-0.218	0.597	0.602	–	–	–	-0.228	0.589	0.594
				(0.468)	(0.666)	(0.407)				(0.466)	(0.668)	(0.408)
Dummy for Chinese	–	–	–	-0.112	0.199	0.596	–	–	–	-0.108	0.214	0.580
				(0.447)	(0.630)	(0.429)				(0.444)	(0.632)	(0.432)
Dummy for other Asian	–	–	–	-0.524	-0.557	0.212	–	–	–	-0.513	-0.546	0.200
				(0.529)	(0.741)	(0.513)				(0.526)	(0.744)	(0.515)
Tax filing experience	–	–	–	-0.302	-0.404	0.005	–	–	–	-0.300	-0.380	-0.009
				(0.320)	(0.389)	(0.344)				(0.318)	(0.392)	(0.346)
Degree of risk aversion	–	–	–	-0.066*	-0.181***	-0.003	–	–	–	-0.064*	-0.181***	0.003
				(0.037)	(0.056)	(0.043)				(0.037)	(0.056)	(0.043)
Mach-IV score	–	–	–	0.014	0.020	0.003	–	–	–	-0.005	-0.021	0.003
				(0.011)	(0.015)	(0.009)				(0.458)	(0.015)	(0.009)
MC1- Tax task complicated?	–	–	–	-0.107*	0.080	-0.035	–	–	–	-0.106*	0.083	-0.039
				(0.057)	(0.102)	(0.081)				(0.057)	(0.103)	(0.082)
MC2- Decision depend on others?	–	–	–	0.153	0.037	0.058	–	–	–	0.152	0.040	0.058
				(0.106)	(0.135)	(0.099)				(0.105)	(0.135)	(0.100)
MC3- Consider others when decide?	–	–	–	-0.156	-0.070	0.157	–	–	–	-0.155	-0.069	0.156
				(0.099)	(0.119)	(0.109)				(0.098)	(0.119)	(0.109)
MC4- Obliged to comply?	–	–	–	-0.175**	-0.059	-0.292***	–	–	–	-0.177**	-0.063	-0.293***
				(0.076)	(0.103)	(0.079)				(0.075)	(0.104)	(0.080)
Constant	0.846***	0.987***	0.516***	1.715	5.259**	0.392	0.876***	0.986***	0.460**	0.180	5.132**	0.363
	(0.187)	(0.190)	(0.167)	(1.517)	(2.316)	(1.389)	(0.178)	(0.192)	(0.182)	(1.512)	(2.331)	(1.394)
Log-likelihood	-256.829	-211.383	-315.721	-236.599	-196.051	-288.819	-256.461	-210.997	-310.105	-236.335	-195.783	-288.709
Observations	560	508	564	560	508	564	560	508	564	560	508	564

*10% significance; **5% significance, ***1% significance.We only include observations which players receive high income. To account for within group correlation, the standard errors are clustered on individuals.

The left-hand side of Table 3 presents regression results in examination of the bomb crater effect. We find evidence for the bomb crater effect in the *Flat-rate* treatment. The coefficient for past auditing experience is positive but insignificant, indicating that the subjects do not exhibit a lower propensity to evade taxes immediately after being audited. However, the bomb crater effect does not exist under the bounded rule mechanism. For both the *Bounded* and *Bounded*-hi-q treatment, if a high-income subject was caught in underreporting taxes, he is significantly less likely to underreport in the subsequent period when he receives high income. This result is robust even after controlling for the experience of play (time trend), social and personal characteristics and belief data.

The above results show a distinctive impact of the previous auditing experience on the subsequent evasion for the two auditing mechanisms. We replicate the bomb crater effect for the *Flat-rate* treatment, which shares common features with previous studies: Subjects are explicitly told that they will be audited with a constant probability. As long as they are aware that the uncertainty only comes from nature, they tend to overlook the fact that the likelihood of the next audit comes from a known distribution rather than recent audits (Tversky and Kahneman (1974)).

However, probability assessment processes change for the bounded rule, as the subjects are not informed about the exact audit probability. Apart from nature, the audit probability is also associated with others' beliefs and actions. Consequently, when subjects are audited, they might overweight the probability that the audit comes from the actions of other players rather than from nature. For conditionally honest players, the overestimation of others' honesty directly leads to a self-fulfilling prophecy that they should comply in the subsequent period like most others do. This also explains why the downward sloping trend in the *Bounded*-hi-q treatment is much more salient and steady compared to that of the *Bounded* treatment: When the level of strategic uncertainty is high, subjects are much more likely to attribute an experience of being caught to the honesty of the decisions of others rather than to nature.

The right-hand panel reports the examination of the echo effect. If early auditing experience has an extra effect on compliance behavior, the coefficient for the variable "early detection experience" should be significant and negative. However, we do not find this effect for all treatments. In the *Bounded*-hi-q treatment, an early audit experience is even less effective, although this effect disappears after controlling for periods of play, social and personal characteristics and belief data. This result contradicts the findings of earlier studies by Mittone (2006) and Kastlunger et al. (2009). A potential explanation is that the time horizon of our setup is not long enough to fully examine this question. In our 30-period experiment, a subject receives high income only about 15 times in the *Flat-rate* and the *Bounded* treatments. On the other hand, in studies such as Mittone (2006) and Kastlunger et al. (2009), players interact for 60 periods. For the *Bounded*-hi-q treatment, although subjects have more experiences of being an H-type, the distinctive probability assessment procedure discussed before makes the dynamics of the game quite different from those in the previous studies.

Result 3 summarizes the above findings.

Result 3 *The bounded rule and the flat-rate rule result in different behavioral dynamics. The bounded rule suppresses the bomb crater effect that otherwise exists in the flat-rate rule. The "echo" effect is not found in any treatment.*

Apart from the previous auditing experience, we also detect some interesting findings from socio-demographics and beliefs on the evasion decision. In the *Flat-rate* treatment, being female, coming from eastern Europe, choosing more safe options in the risk elicitation task, considering the tax evasion decision to be complex and feeling obligated to report truthfully all decrease the likelihood of cheating on taxes. In the *Bounded* treatment, however, social and personal characteristics seem to impact less on behavior. Apart from the risk attitude, the only social information that affects behavior is training in economics: Players who have spent more years studying economics are more likely to underreport. This result seems to suggest that training in economics results in behavior more in line with the predictions made by game theory. In the *Bounded*-hi-q treatment, social characteristic data have no impact on evasion behavior except for the variable that measures personal norms. The more a subject expresses "obliged to report truthfully" in the post-game questionnaire, the less they cheat on taxes in general. This shows that their reported answers are consistent with their actual behavior in the experiment.

5 Concluding Remarks

This paper experimentally examines the bounded rule as an alternative auditing mechanism that naturally integrates game theory into the modeling of taxpayer interactions. In a tax compliance game, subjects receive either high- or low income with a predetermined probability. On knowing a certain auditing rule, they report income to the tax agency. In the *Flat-rate* treatment, participants are told that they independently face a known audit probability. In contrast, participants in the *Bounded* treatment are informed of the maximum number of audits. The experimental results indicate that the compliance rate in the bounded rule is the same as that in the traditional rule when the level of strategic uncertainty is low, but becomes much higher when the level of strategic uncertainty is high. In the presence of multiple equilibria, the bounded rule deters subjects from coordinating on the payoff-dominant equilibrium without any increase in the maximum number of audits.

Similar to previous experimental studies on the coordination game, the underreporting rate declines drastically in the *Bounded*-hi-q treatment, demonstrating again the attraction of the safe strategy. These findings could be explained by the fact that people are generally strategic uncertainty averse (see, e.g., Heinemann et al. (2009)). According to Brandenburger (1996)'s definition, strategic uncertainty arises when "there is uncertainty concerning the purposeful behavior of players in an interactive decision situation", as opposed to a game against nature. When people are strategic uncertainty averse, they will prefer a sure, safe outcome to a better but riskier one with a realized probability depending on the decisions of others. In the bounded rule treatment, even though jointly underreporting income yields a higher ex post income for every H-type subject, it is difficult for them to fully ensure that others also think in the same way. Lacking the opportunity to communicate or pre-commit to the risky decision, subjects prefer to choose a safe strategy. In summary, strategic uncertainty aversion should be further explored as a powerful source of deterrence in audit mechanism design.

The results also show that individual behavioral dynamics are institution-dependent. We find that the bounded rule decreases the bomb crater effect – possibly via the individuals' perception of the auditing experience. Although we do not elicit beliefs in our current setup, we could still infer from the reactions of the participants that they are more likely to associate the audit experience with the compliance decisions of others than with probability assessment (otherwise, we would observe similar results as in the *Flat-rate* treatment). Our paper echoes a finding of Kastlunger et al. (2009) that the bomb crater effect could disappear by merely changing the sequence of random audits. Future studies are needed to explore the robustness of behavior dynamics to the changes of institutional environment such as auditing rules, time horizon, etc.

This study is the first step into the investigation of the bounded rule empirically. In our current setup, taxpayers can only decide whether to underreport or honestly report. In future studies, the model could be extended to allow choices as to the extent of underreporting. Another possible extension might involve introducing a human auditor to further examine the strategic interactions. In our current setup, subjects are not allowed to exchange information with each other in order to be consistent with most of the tax compliance experiments. Yet, in reality, taxpayers do have opportunities to communicate with each other. For instance, Alm and McKee (2004) show that such cheap-talk communication could help taxpayers to coordinate on a zero-compliance (payoff-dominant) equilibrium. However, if a strategic auditor could observe this, she could adjust the audit capacity accordingly to combat collusion among taxpayers.

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Appendix

A Technical Details and Proofs

A.1 Proof of Proposition 1

This subsection contains two parts. The first part proves that given that all players are rational, strategic expected profit maximizers, the game introduced by the bounded rule is dominance solvable. The second part shows that this claim still holds by introducing conditionally or intrinsically honest players.

The proof is trivial that reporting high income is a dominated strategy for the L-type players. To prove that the best response of H-type players is underreporting given that L-type players display dominance, the expected payoff from underreporting should be strictly larger than the sure payoff from reporting truthfully. Moreover, this holds regardless of the beliefs that H-type players hold towards the other H-types.

First assume that an H-type player anticipates that nobody else will underreport. That is, $\overline{b_0} = (b_1, b_2, ..., b_{N-1}) = (0, 0., , , 0)$. In this situation, "low-income" reports are submitted by only L-types. Since the probability of being an L-type is $q = 0.5$ for every other player, the probability that exactly n out of $N-1$ players submit "low-income" reports follows the binomial distribution $\mathbf{Bin}\ (n, N-1; q) = \mathbf{Bin}\ (n, 7; 0.5)$. The expected payoff from underreporting is therefore:

$$\begin{aligned}
E(\pi_l|\overline{b_0}) &= \sum_{n=0}^{N-1} \mathbf{Bin}(n; N-1, q) \times \{\min(\frac{2}{n+1}, 1) \times \pi_F + [1 - \min(\frac{2}{n+1}, 1)] \times \pi_S\} \\
&= \pi_S - (\pi_S - \pi_F) \times \sum_{n=0}^{N-1} \mathbf{Bin}(n; N-1, B_i) \times \min(\frac{2}{n+1}, 1) \\
&= 22.5 - 20 \times \sum_{n=0}^{7} \mathbf{Bin}(n; 7, 0.5) \times \min(\frac{2}{n+1}, 1) \\
&= 12.698
\end{aligned}$$

The sure payoff of reporting truthfully is 12.5. Hence, a self-interested, risk neutral H-type player will underreport.

The remaining proof shows that for any given set of beliefs held by an H-type player, the expected payoff from underreporting is always at least $E(\pi_l|\overline{b_0})$. Assume that player N thinks that the first $N-1$ players underreport with probability $\overline{b} = (b_1, b_2, ..., b_{N-1})$. The probability that player i submits "low-income" is $B_i = 1 - q + qb_i = \frac{1}{2}(1 + b_i)$. Note that $B_i \in [\frac{1}{2}, 1]$. To facilitate notation, define an index vector $\mathbf{I} = (i_1, i_2, ...i_7)$, with $i_1 \neq i_2 \neq ...i_7$. Each index takes a value from the set $\{1, 2, ..., 7\}$. The probability that n out of 7 other players submit "low-income" reports is:

$$\Pr(n|\bar{b}) = \sum_{s=1}^{C_7^s} \prod_{j=1}^{s} B_{i_j} \prod_{k=s+1}^{i_7} (1 - B_{i_k})$$

The expected payoff from underreporting is therefore:

$$E(\pi_l|\bar{b}) = \sum_{n=0}^{N-1} \Pr(n|\bar{b}) \times \{\min(\frac{2}{n+1}, 1) \times \pi_F + [1 - \min(\frac{2}{n+1}, 1)] \times \pi_S\}$$

It turns out that for any given b_i, $\partial E(\pi_l)/\partial b_i = (\partial E(\pi_l)/\partial B_i) \cdot (\partial B_i/\partial b_i) > 0$.[14] This means that the expected payoff from underreporting is increasing in the (subjective) propensity to evade taxes. Hence, given any set of beliefs $\bar{b} = (b_1, b_2, ..., b_{N-1})$, $E(\pi_l|b) \geq E(\pi_l|\overline{b_0})$. Hence, the best response of the H-type players is to underreport. ∎

The second part of this subsection proves that the introduction of conditionally or intrinsically honest players does not change the directions of treatment difference.

Let ρ be the probability that a player is conditionally honest, and $1 - \rho$ be the probability that a player is a strategic, self-regarding profit maximizer, where $0 \leq \rho < 1$. We do not allow $\rho = 1$, since at least one strategic player is thinking of this problem. In our setting, in particular, the number of conditionally honest players ρN can be any number from 0 to 7 out of 8 players. We further assume that the ρ is the same in both treatments.

The strategy of the conditionally honest players is as follows. When they receive low income, they will always report truthfully. When they receive high income, they will honestly report their income if they think that the number of players cheating on taxes $(1 - \rho)N$ is not higher than a certain threshold $\lambda \in [0, 7]$, and will underreport their income otherwise.

To prove the statement, we first show that the inclusion of these players does not affect the strategy of the profit maximizers. When the strategic players are assigned to be L-types, they gain a higher payoff by reporting truthfully, regardless of the auditing rule implemented. In the *Flat-rate* treatment, H-type profit maximizers only compare a sure payoff of reporting truthfully and the expected payoff from the tax evasion gamble if they underreport. Hence, the existence of honest players will not affect their choices. In the *Bounded* treatment, the subjective beliefs of strategic, H-type players regarding the number of "low-income" reports now become $B_i = (1-q) + q(1-\rho)b$. Given that $q = 0.5$, $0 \leq \rho < 1$, B still lies in the interval $[\frac{1}{2}, 1]$. Therefore, Proposition 2 still holds.

[14] Calculations are available upon request.

Anticipating that strategic profit maximizers will cheat when they receive high income, the conditional honest players will assess the proportion of self-interested profit maximizers in the population. If the proportion is $(1-\rho)N \leqslant \lambda$, they will honestly report their income. If $(1-\rho)N > \lambda$, they will underreport.

We assume that belief is mutually rational in equilibrium. Hence, in the presence of conditionally honest players, the non-compliance rates of both treatments become:

$$\sum \mathbf{Bin}(n; N, q)(1-\rho) = \begin{cases} (1-\rho) & \text{if} \quad (1-\rho)N \leqslant \lambda \\ 1 & \text{if} \quad (1-\rho)N > \lambda \end{cases}$$

The analysis of intrinsically honest players is simpler, since their strategies could be reformulated by setting $\lambda = 7$. As $(1-\rho)N \leqslant 7$ always holds, the compliance rates of both treatments with intrinsically honest players become:

$$\sum \mathbf{Bin}(n; N, q)(1-\rho) = (1-\rho)$$

A.2 The Existence of Coordination

If this game is a coordination game, there exists a $b \in [0, 1]$ such that the payoff from underreporting is equal to the honest payoff:

$$\begin{aligned} E(\pi_u; N, q, K, b_i) &= \sum_{n=0}^{N-1} \mathbf{Bin}(n, N-1; B_i) \left[(1-a_{BD}) \times (I_H - T_L) + a_{BD} \times (I_H - T_H - F)\right] \\ &= I_H - T_H \quad . \end{aligned}$$

Due to the discrete nature of the distribution, a direct proof is difficult. However, just for illustration purposes, if N is large, the expected number of "low-income" reports is $B_i N = [(1-q) + q b_i]N$. The expected profit from underreporting could be simplified as

$$\begin{aligned} E(\pi_u) &= \frac{K}{B_i N}(I_H - T_H - F) + (1 - \frac{K}{B_i N})(I_H - T_L) \\ &= I_H - T_H \quad . \end{aligned}$$

Solving the equation yields $B_i = K(T_H + F - T_L)/N(T_H - T_L)$. Hence, there exists a set of parameters K, T_H, F, T_L, N and q such that $B_i \in (0, 1)$. Thus, in certain parameter domains, the H-type players under the bounded rule find themselves indifferent between underreporting and honestly reporting if $b_i = \bar{b} = \frac{B_i-(1-q)}{q}$. If $b_i > \bar{b}$, then the H-types all underreport; if $b_i < \bar{b}$, then the H-types all report honestly.

A.3 Proof of Proposition 2

Let $\sigma_i(j)$ be the probability that type i player (H-type or L-type) will use strategy j (u or h). There are two pure Nash equilibria and one mixed-strategy equilibrium in this treatment:

$$\{(\sigma_H(u) = 1, \sigma_L(h) = 1), (\sigma_H(h) = 1, \sigma_L(h) = 1), (\sigma_H(u) = 0.432, \sigma_L(h) = 1)\}.$$

In other words, the two pure Nash equilibria are 1) all H-type players underreport and 2) all H-type players report honestly. L-type players always report honestly.

Let us examine the former case. Given that an H-type player thinks that all other H-types choose strategy u, he will have an expected payoff of 17.5 by playing strategy l. By deviating to h, the payoff decreases to 12.5. Since we assume symmetry among players, no one has an incentive to deviate from underreporting, which constitutes an NE. A highly similar analysis applies to the latter case. Given that all other H-type players play strategy h, a strategy deviation from h to l will yield a lower expected payoff for H-type players (from 12.5 to 3.59). Hence, no one has an incentive to deviate.

On top of the two pure equilibria, the game also has a mixed-strategy equilibrium in which each H-type player is indifferent between the strategy of reporting honestly and underreporting. Given the game parameters, the underreporting probability b that induces utility indifference is $b^*_{SE} = 0.432$.

B Instructions

B.1 Instructions Comparison

The instructions given in the next subsection are for the *Bounded* treatment. These instructions differ from those given for the other treatments as follows:

- *Flat-rate* treatment

    1. The second bullet (concerning matching protocol) of the list under "Task Description" in the instructions for the "Tax Compliance Game" is absent.
    2. The "Audit Probability Table" is absent.
    3. The phrase "see audit prob. table" in the "Payoff Table" becomes 0.4.

- *Bounded*-hi-q treatment

    1. In the third bullet of the list under "Task Description" in the instructions for the "Tax Compliance Game", the probability of receiving €25 becomes 0.9; accordingly, the probability of receiving 10 becomes 0.1.
    2. In the "Payoff Table" (immediately before "Payment Method" in the instructions for the "Tax Compliance Game"), the probabilities in the second column become 0.9 and 0.1, respectively.

B.2 Instructions for the *Bounded* Treatment

- Please read these instructions carefully!
- Please do not talk to your neighbours and remain quiet during the entire experiment.
- If you have a question, please raise your hand. We will come to you to answer it.
- You will receive a show-up fee of €3 for completing all tasks in the experiment, independent of your performance.

Task Description

- This session consists of 30 periods of play; each period is completely independent of the others.

- Of the participants in the room, two groups of 8 participants will be randomly formed at the beginning of each period. You will not know the identity of the other players in your group in any period.

- At the beginning of each period, you will receive a taxable income of either €25 or €10. The probability of receiving €25 is 0.5; the probability of receiving €10 is 0.5.

- Your task is to report your income to the auditor, which is played by a computer. The amount that you report is your decision. You can report either €25 or €10, regardless of your received income.

After-tax Income Determination

Your after-tax income in this period is determined by the following two steps: tax payment and an audit.

Step One: Tax payment

The tax rate is 50% for those who reported €25 and 25% for those who reported €10. Suppose the income you received is €25:

- If you report €25 to the auditor, the auditor will charge €12.5 (50% of €25) as tax. So your after-tax income in this period equals €25 – €12.5 = €12.5.

- If you report €10 to the auditor, the auditor will charge €2.5 (25% of €10) as tax. So your after-tax income in this period equals €25 – €2.5 = €22.5.

Suppose the income you received is €10:

- If you report €10 to the auditor, the auditor will charge €2.5 (25% of €10) as tax. So your after-tax income in this period equals €10 – €2.5 = €7.5.

- If you report €25 to the auditor, the auditor will charge €12.5 (50% of €25) as tax. So your after-tax income in this period equals €10 – €12.5 = -€2.5.

- In sum, the auditor charges tax based on your reported income, instead of your received income.

Step Two: Audit

The auditor does not know your received income unless your report is audited later.

Auditing procedure:

- If your reported income is €25, you will not be audited. That means what you have earned in step one (€12.5 or -€2.5) will be your after-tax income (if your received income is €25 and €10, respectively).
- Regardless of your received income, if your reported income is €10, there is a chance that your report will be audited. The outcome is as follows:
  - Suppose your reported income is €10 AND your received income is also €10. Then what you have earned in step one (€7.5) will be your after-tax income, no matter whether your report is audited or not.
  - Suppose your reported income is €10 AND your received income is €25. If your report is not audited, you will keep the €22.5 earned in step one; if audited, you will get €2.5.

Auditing probability:

The number of reports the auditor will audit depends on the number of players reporting an income of €10 in a group.

- If the number of €10 income reports is equal to two or less, the auditor will audit all of the €10 reports.

- If the number of €10 income reports is three or more, then two out of such reports will be randomly selected for audit.

- The "Audit Probability Table" below shows the audit probabilities for a player who reported an income of €10.

Audit Probability Table

Number of €10 reports	0	1	2	3	4	5	6	7	8
Audit Probability	100%	100%	100%	66.7%	50%	40%	33.3%	28.6%	25%

- The "Payoff Table" below summarizes all of the possible scenarios you may encounter in one period and the related payoffs:

Payoff Table

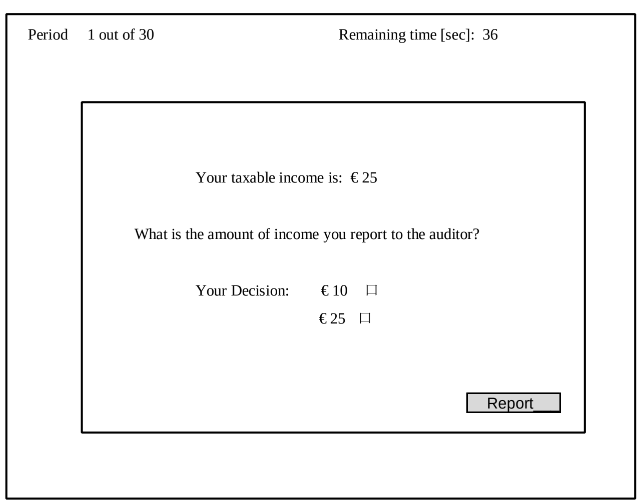
Received Income	Probability	Reported Income	Audit Probability	After-tax Income if audited	After-tax Income if NOT audited
€25	0.5	€25	0	€12.5	€12.5
		€10	see audit prob. table	€2.5	€22.5
€10	0.5	€10	see audit prob. table	€7.5	€7.5
		€25	0	−€2.5	−€2.5

Payment Method

- At the end of this experiment, one out of 30 periods will be selected to determine your payoff for this task. The computer program will generate a random number from 1 to 30. This number will determine one of the 30 periods. Your performance in that period determines your payoff.
- You will be paid based on your after-tax income for the randomly selected period.
- Because each period is equally likely to be selected for payment determination, you should make your decision in each period as if that period would be selected for payment.
- Your payoff will be paid out in cash at the end of the experiment along with your earnings in the other task(s).

We will now show you what the computer screens look like.

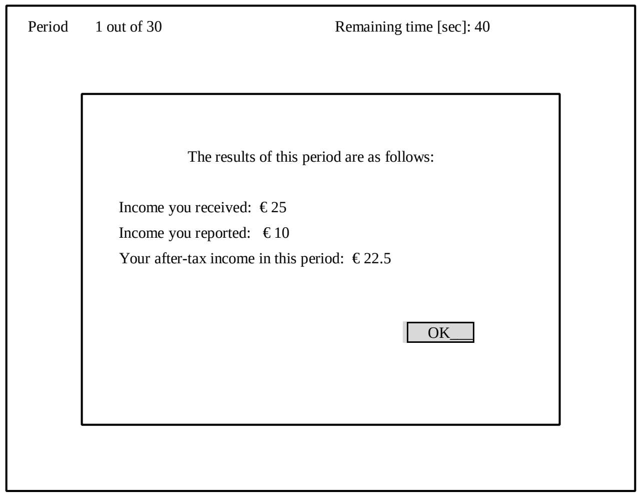
SCREEN 1



In “Screen 1”, you can decide the amount of income to report to the auditor. Please select either “€10” or “€25”, and confirm your choice by pressing the “Report” button.

Warning: Before pressing the button, make sure your choice is correct. You cannot change your decision after you have pressed OK.

SCREEN 2



“Screen 2” is the feedback table you will receive regarding your after-tax income. You will find information on the initial taxable income you received, the income you reported and your after-tax income in this period.

Click on OK when you finish checking the information.

Note that the purpose of the screen shots is to clarify the procedure, rather than provide advice about how to act. You should make the decisions that are best for you.

B.2.1 Risk Elicitation Task[15]

Task Description

In this task, you are asked to make decisions related to 21 choice pairs. In each choice pair, you need to select between two lotteries labeled "Lottery A" and "Lottery B". Please, take your time and read each choice pair carefully. An example of a typical choice pair is given below:

Choice No.1	Lottery A	€5.5 with probability 0.5 or €3.5 with probability 0.5	Your choice:	Lottery A ☐
	Lottery B	€9 with probability 0.5 or €0.5 with probability 0.5		Lottery B ☐

Payment Method

- You need to make choices for all 21 choice pairs. However, only one of the 21 choices you have made will be chosen for the payoff determination of this task. First, the computer program will generate a random number from 1 to 21. This number will determine a choice pair. Then, the computer program will simulate the lottery you have chosen and reveal the outcome on your screen. The outcome of this lottery will determine your payoff.

- For example, suppose that the computer program has generated a random number 2. It will then check what you have selected in choice pair number 2. Suppose that you have chosen Lottery A in that choice pair. Then the computer program will simulate Lottery A and reveal your payoff (either €5.5 or €3.5). Your payoff will be paid out in cash at the end of the experiment along with your earnings for the other task.

It is important that you fully understand the lottery selection task. Please raise your hand if you have any questions at this moment.

B.2.2 Post-experimental Questions

Questions on Treatment Manipulation Please evaluate the following statements with respect to the tax reporting task:[16]

1=strongly disagree, 2=somewhat disagree, 3=slightly disagree, 4=no opinion, 5=slightly agree, 6=somewhat agree, 7=strongly agree

[15] The risk elicitation task is conducted after the tax compliance game. However, the subjects do not know the existence of this task when they are playing the game.

[16] The first five questions are used to understand the subjects' perception about the experimental setup and instructions in general. We do not expect to find differences across treatments. The last five questions focus on capturing different types of manipulations of the treatments; therefore, we expect to see differences across manipulations.

1. The instructions were clearly formulated.
2. I felt that I performed well on the task.
3. I received plenty of time to carry out the task.
4. I was motivated to do well on the task.
5. The task was fun to perform, motivating me to achieve a payoff as high as possible.
6. I considered the tax reporting task to be fairly complex.
7. My payoff is determined not only by my own decision, but also by the decisions of the other players.
8. When making my decision, I thought about what other players might do.
9. I feel obliged to report the received income in each period.
10. The chance I have received €25 is about 50%.[17]

Questions on Background Information

Please answer the following survey questions. Your answers will be used for this study only. Individual data will not be exposed.

1. What is your gender?
2. What is your nationality?
3. How many years have you already studied economics?
4. Have you ever had a course related to game theory?
5. Have you ever had a part-time job?

[17] In the *Bounded*-hi-q treatment, the chance should be 90%, instead of 50%.

Questions on Mach IV Scale[18]

In the following you will find a list of statements. Please read them carefully and indicate to what extent you agree or disagree. Even if in some cases you would like to say that your answers depend on the circumstances, you should only choose one of the answers. Since all responses are anonymous, you can answer freely. There is nobody on whom you need to make a good impression. Only if you answer very honestly can the results be used.

1=strongly disagree, 2=somewhat disagree, 3=slightly disagree, 4=no opinion, 5=slightly agree, 6=somewhat agree, 7=strongly disagree

1. Never tell anyone the real reason you did something unless it is useful to do so.
2. The best way to handle people is to tell them what they want to hear.
3. One should take action only when sure it is morally right.
4. Most people are basically good and kind.
5. It is safest to assume that all people have a vicious streak and it will come out when they are given a chance.
6. Honesty is the best policy in all cases.
7. There is no excuse for lying to someone else.
8. Generally speaking, people won't work hard unless they're forced to do so.
9. All in all, it is better to be humble and honest than to be important and dishonest.
10. When you ask someone to do something for you, it is best to give the real reasons for wanting it rather than giving reasons which carry more weight.
11. Most people who get ahead in the world lead clean, moral lives.
12. Anyone who completely trusts anyone else is asking for trouble.
13. The biggest difference between most criminals and other people is that the criminals are stupid enough to get caught.

[18] Questions 3, 4, 6, 7, 9, 10, 11, 14, 16 and 17 are reverse coded.

14. Most people are brave.

15. It is wise to flatter important people.

16. It is possible to be good in all respects.

17. Barnum was wrong when he said that there's a sucker born every minute.

18. It is hard to get ahead without cutting corners here and there.

19. People suffering from incurable diseases should have the choice of being put painlessly to death.

20. Most people forget more easily the death of their parents than the loss of their property.