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Abstract

Tax authorities around the world often are reluctant to disclose audit policy details. In particular, the US Internal Revenue Service (IRS) has the practice of releasing broad statistics like the audit rate of each income class but resists pressures demanding details on how different circumstances might result in a higher audit probability to taxpayers. This paper experimentally examines whether disclosing such details can reduce tax compliance. We compare a *Flat-rate* treatment, where taxpayers are told about the average audit probability, with a *Bounded* treatment, where taxpayers are fully informed of the contingent audit probability structure. Our findings do not support the potential concern against disclosing details. In an additional *Bounded*-hi-q treatment where multiple equilibria exist, the compliance level is even higher under full disclosure of the probability structure.

JEL codes: H26, M42, C9, C72

Keywords: Information disclosure, Government transparency, Audit policy, Tax auditing, Tax compliance, Laboratory experiment

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1 Introduction

This paper asks whether more transparency in government agencies compromises their commissioned objectives. Specifically, we study the impact of information disclosure, concerning audit policy details, by a tax authory like the US Internal Revenue Service (IRS) on the level of tax compliance. IRS has long been accused of having a “secret culture” (see Saxon (1994), Johnston (1995b), and Davis (1997)).[1] While the agency is not as opaque as before, what people know about IRS audits is still mainly from broad statistics provided on its web site (e.g., from the IRS Data Books).[2] Even though, following the enactment of the IRS Restructuring and Reform Act of 1998, the agency has made public the Internal Revenue Manual (IRM) describing the tax audit process (Gates (2000)), certain details of the audit policy remain undisclosed to taxpayers.[3]

Why does IRS disclose only broad statistics like the audit rate of each income class but not details of the audit policy? Apparently, the agency worries that the tax compliance level would fall should taxpayers know details of the audit policy (*New York Times* (1981a) and *New York Times* (1981b)). In this paper, we investigate whether a tax authority could

[1] For example, “the [US] Government’s chief keeper of historic records said [on 20 December 1995] that the Internal Revenue Service has, for at least two decades, violated Federal laws that require it to identify significant documents and turn them over to the National Archives. ... John W. Carlin, the Archivist of the United States, gave the I.R.S. 90 days to come up with a plan to identify, safeguard and eventually turn over to his office records that may have historic value. His 50-page evaluation cited "serious shortcomings" in I.R.S. record-keeping and questioned whether some important records had been lost or destroyed. "Numerous records that document both policy-making and high-profile programs" either are not scheduled to be released to the National Archives "or have not been located and identified," the evaluation said. ... Critics have long accused the I.R.S. of excessive secrecy, and historians, individual taxpayers and others have battled for access to statistical data.” (Johnston (1995b))

[2] Documentation of IRS audit practices in the academic literature is sparse. An example is Pentland and Carlile (1996).

[3] Evidence for this is not hard to find. For example, the actual operation of the discriminant function (DIF) formula used to identify the most suspicious tax returns for follow-up remains a “closely guarded secret” (Jones (2001)). It seems that IRS has released the entire IRM on its web site. But a careful look at the web manual shows that the section IRM 4.19.1.2.6, Form 1040 Individual Returns Scored by DIF System (Audit Code Definitions) is missing (see the Table of Contents of Part 4 of the IRM at http://www.irs.gov/irm/part4/index.html). According to IRM 4.1.3.2.2, the missing section contains Audit Codes used to identify returns “delivered [directly] to Examination as Automatics for manual screening,” regardless of the DIF scores. Apparently, IRS wants to keep the information secret.

Thoroughly searching over the web IRM can locate partial information about the Codes, e.g., in Exhibit 3.11.3-5. However, like multiple places of the section IRM 3.11.3 Individual Income Tax Returns (e.g., Exhibit 3.11.3-8 Examples of Reasonable Causes and Exhibit 3.11.3-6 Unallowable Codes), some details have been overwritten with equal signs (=). Similar blacked-out’s can be found in other IRS documents released to the public, e.g., pages 3-4, 3-8, 3-14, and 3-15 of the 2010 version of “IRS Processing Codes and Information” (IRS (2010)).

be no worse off by fully disclosing to taxpayers the structure of an audit policy, instead of merely telling them the average audit probability. Answering this question is important. It can provide evidence to support the information-withholding position of IRS, or otherwise give some assurance to the agency to become more transparent, as critics have demanded, without compromising the objective to increase tax compliance.

The tension between increasing government transparency and keeping appropriate levels of secrecy is not new (Ginsberg (2011)). Watchdog organizations like OMB Watch, Citizens for Responsibility and Ethics in Washington, First Amendment Coalition, and Taxpayers for Common Sense always press for more transparency and freedom of information. However, the demand for government transparency has never been stronger (Ornstein and Limor (2011)).

Since Barack Obama was elected as the US President, the administration has emphasized the committment to "creating an unprecedenteded level of openness in Government" (Obama (2009)). As a result of the Open Government Initiative, agencies are asked to increase disclosures (see, e.g., Department of the Treasury (2011)). Still, censorship of information prior to release is not unheard of (*The Associated Press* (2011)).

As for IRS, the reluctance to disclose information has not changed much in the last four decades.[4] The reservation is not only on open disclosure to the public but also on confidential

[4]In 1973, Ralph Nader of Tax Reform Research Group invoked the Freedom of Information Act (FOIA) in order to obtain some IRS documents. The agency refused and Nader responded with a suit before IRS reluctantly agreed to provide the documents (*Time* (1974)). A year before this, Susan Long and her husband started the litigation lasting for over 15 years, involving courts as high as the US Supreme Court, forcing the agency to be more open in releasing information. "Their first successful legal action set the principle that the I.R.S. could not withhold information like statistics on the audit rates for taxpayers in different income groups, nor its basic operating manual." (Saxon (1994)).

It was thought that the nearly two decades of litigation finally came to an end when IRS was ordered to pay the Longs' legal fees in 1991. But the battle reopened in 2004 when IRS told Susan Long that after extensive research, its lawyers concluded that "no court order existed and 'accordingly, the I.R.S. is not in violation of any standing injunctions'" by withholding information from her (Johnston (2006)). In 2006, Long went to court again to file a legal motion to require the agency to comply with prior court orders to turn over detailed data on its audit practices (Johnston (2006)). On 13 June 2008, the US District Court in Seattle granted her motion. IRS timely appealed the order. Finally, on 16 September 2010, the US Court of Appeals for the Ninth Circuit affirmed in part and reversed in part the 13 June 2008 order, ruling that some information taken from IRS's Form 5344 of one particular taxpayer, referred to as "cells of one," is confidential under 26 U.S. Code Section 6103(b) (United States Courts of Appeals for the Ninth Circuit (2010)).

Long was not the only one in battle with IRS for information disclosure in recent years. For example, Tax Analysts, the nonprofit publisher of *Tax Notes* magazine, went to court for obtaining e-mail messages in which tax auditors in the field were given advice on how to apply the law. "We won a unanimous court of appeals decision that they can't hide this stuff," Tax Analysts' president said, "but instead of complying with the order to produce it, they are playing games." (Johnston (2008)).

disclosure to researchers (see, e.g., Shackelford and Shevlin (2001), page 375). Intrigued by the puzzling attitude of IRS, we are interested in verifying whether disclosing audit policy details necessarily reduce tax compliance, or maybe it could actually increase compliance.

Laboratory tightly controls many factors that may affect behaviors. It also allows measuring certain personal characteristics, e.g., risk aversion level, that might be important to explaining behaviors but hard to measure outside laboratory. For these and other reasons, randomized experiments in laboratory are not subject to various limitations of observational experiments (Rosenbaum (2002)). Randomized experiment therefore offers an excellent methodology for us to answer the research question without worrying about confounding effects that might arise from using archival data.

Consistent with IRS's practice of disclosing only broad statistics like the audit rate of each income class, prior experimental studies on tax compliance usually consider settings where subjects are told to be audited independently at a known, constant probability (e.g., Allingham and Sandmo (1972), Yitzhaki (1974), Moser et al. (1995), Zimbelman and Waller (1999), Boylan and Sprinkle (2001), Kim et al. (2005), Kim and Waller (2005), Alm et al. (2009), and Kleven et al. (2011)). A recent theoretical study shows that such a *flat-rate* audit rule in equilibrium has the same deterrence effect as a variable-rate rule, referred to as the *bounded* rule (Yim (2009)).

Simply put, the bounded rule fully utilizes a given audit capacity to randomly select a sample of equally suspicious reports to check if the number of such reports exceeds the capacity, or otherwise audits all of such reports. Because the number of reports selected for audit is bounded by the audit capacity, the audit probability facing a taxpayer varies depending on the total number of suspicious reports filed by the taxpayer population. By setting the audit capacity appropriately, the compliance level induced by the bounded rule can be equivalent to that by the flat-rate rule. This theoretical equivalence together with the simple binary-income setting from which the bounded rule was derived makes comparing the two rules experimentally using human subjects a suitable way to answer our research question.

In designing our experiment, we bear in mind the "Why People Pay Tax" (WPPT) puzzle documented in the tax compliance literature (Alm et al. (1992)). It is unclear why most people file tax returns honestly when the average audit probability is only 1% (Slemrod (2007)). Given this phenomenon, it is important to ensure that our experiment provides a sufficient incentive to lie. Otherwise, if nearly all participants behave honestly in our experiment, the result would bias toward a "no difference" conclusion. To avoid this bias, the baseline *Flat-rate* treatment of our experiment provides a strong enough incentive for participants to lie. It is so strong that theoretically all participants should lie, just like the key feature of the documented puzzle. However, also like the puzzle, the actual outcome is a compliance level much higher than 0%.

To compare with the *Flat-Rate* treatment that represents the practice of disclosing only the average audit probability, our *Bounded* treatment lets participants know how the audit probability is contingent on the total number of suspicious reports filed by taxpayers. Like the *Flat-rate* treatment, the *Bounded* treatment has a predicted compliance level of 0%. The actual outcome, again, is far above the theoretical prediction.

We find that compared to the *Flat-rate* treatment, the compliance level is not lower under full disclosure of the contingent audit probability structure in the *Bounded* treatment. Interestingly, it is actually higher in magnitude (43%, rather than 39% in the *Flat-Rate* treatment), though not statistically significant. Similar results continue to hold when confining to the last 10 periods where participants should have become familiar with the environment (47%, rather than nearly 42% in *Flat-Rate*). The findings support our hypothesis that there is no difference in the compliance levels under the bounded and flat-rate rules, which represent disclosing audit policy details (i.e., the contingent audit probability structure) versus merely the average audit probability. We conclude that disclosing audit policy details does not necessarily reduce tax compliance.

To see whether the conclusion might be sensitive to a parameter in the experiment, we contrast the *Bounded* treatment with the *Bounded*-hi-q treatment. This additional treatment captures the case where taxpayers in an area under the jurisdiction of an IRS District Office are more likely to have a high income.[5]. When the parameter q is high, there are multiple equilibria under the bounded rule in the tax compliance game of the experiment. One of the

[5]Consistent with the emphasis by Yim (2009), tax audits are administrated by IRS District Offices under audit capacity constraints. See further discussion in section 5.

equilibria involves all high-income taxpayers tacitly coordinating to lie. Another involves all of them reporting honestly. The third is a mixed equilibrium where each taxpayer randomizes to lie with the same probabiltiy.

We find that the conclusion from the first two treatments is not sensitive to the existence of multiple equilibria in the *Bounded*-hi-q treatment with a high q. We observe a higher level of compliance in this treatment (66% overall and 74% for the last 10 periods) than in both the *Flat-rate* and the *Bounded* treatment. We further conclude that fully disclosing the audit probability structure, rather than merely the average audit probability, can increase tax compliance, instead of reducing it.

Besides the main findings above, we also analyze the audit budget implications of the bounded rule to see whether they are broadly consistent with the theoretical insights of Yim (2009) where the rule was derived. The results suggest that the bounded rule on average conducts fewer audits than the flat-rate rule. If taking into account the budget commitment required to credibly implement the flat-rate rule, the bounded rule has a higher budget usage ratio than the flat-rate rule. Both results are in line with the theoretical insights about the bounded rule, suggesting no unexplained issue that might cause any concern.

Though consistent with the documented WPPT puzzle, the compliance levels observed in the experiment are under-predicted quite substantially by the standard theory. This leads us to conduct additional analyses to reconcile the discrepancy using alternative choice models under uncertainty. The observed behaviors can be satisfactorily explained by a loss aversion model. We are not aware of any unusual results from the analyses that might compromise the conclusion of our main analysis.

This paper adds to the literature on understanding how dissemination of enforcement information might affect taxpayers' behavior (e.g., Slemrod et al. (2001)). Focusing on the compliance impact of information dissemination regarding audit results, Alm et al. (2009) find that the effect of post-audit information is conditional on whether the taxpayer is well informed of the audit probability prior to filing. Unlike them, we do not consider disclosing population-wide audit results of the previous period before the filing in a period. Instead, we focus on the disclosure of the underlying contingent audit probability structure (*Bounded*), which has a deterrence effect theoretically equivalent to that of the average audit probability disclosed (*Flat-rate*). Alm et al. (2009) consider only the latter case to contrast with the alternative of no disclosure at all.

Research in the disclosure literature has predominantly concentrated on corporate transparency (e.g., Bushman et al. (2004) and Francis et al. (2009)). One of the main themes is that companies with more disclosure might enjoy the benefits from reducing information asymmetry, namely a lower cost of capital, a smaller bid-ask spread, etc (e.g., Botosan and Plumlee (2002)). This paper extends the literature to consider government transparency. What motivates government agencies' lack of transparency appears to be the potential benefits from being opaque. Our findings, however, suggest that a presumed benefit might not exist. Interestingly, there might even be some overlooked cost (in terms of foregone benefit) under certain circumstances (e.g., a high q).

Findings from accounting research suggest that investors do not fully exploit publicly available information, nor fully understand the implications of the information, in making investment decisions (e.g., Bartov et al. (2000), Dechow et al. (2008), and Landsman et al. (2011)). Possible reasons include limited attention or other information processing or transaction costs (e.g., Hirshleifer and Teoh (2003), Louis and Sun (2010), and Corwin and Coughenour (2008); see also the discussion by Schipper (2007)). Consistent with such findings, our results suggest that maybe IRS has overly worried about the impact of disclosing audit policy details.

To meet people's increasing demand for transparency in government, IRS can set out a plan to disclose more information about the audit policy on an annual basis. Each year the incremental disclosure should be about a clearly defined set of new information and be released on a specific date before the deadline of another round of tax return filing. This way researchers can precisely analyze the impact of the incremental information disclosure. Furher evidence can thus be provided to determine whether even more disclosure or IRS's current position of information withholding should be supported. Ultimately, such research may help IRS to understand how its commissioned objectives can be best fulfilled. The unintended monitoring functions of IRS on the financial market and financial reporting quality (see Hanlon and Heitzman (2010), page 138, El Ghoul et al. (2011), and Hanlon et al. (2011)) might also be enhanced.

The organization of this paper is as follows. We describe the experiment design and procedure in the next section, ending with our hypothesis for testing. Main results from the experiment are discussed in section 3. In section 4, we conduct additional analyses to reconcile the discrepancy between the actual compliance level observed in the experiment and that predicted by the standard theory. Section 5 reviews related tax compliance studies and further explains why we design the experiment based on the bounded rule. Section 6 contains concluding remarks. The theoretical analysis upon which our experimental study is based, technical details and proofs, and the experiment instructions are provided in the appendix.

2 Experiment and Hypothesis

2.1 Design

The tax compliance game in all treatments of our experiment 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 the report type ("high income" or "low income") to submit to an auditor, 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 true income. For instance, if H-type players submit a "low-income" report, they receive €22.5, instead of €12.5. Similarly,

[6]Experimental parameters concerning taxation are chosen to be in line with the 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.

L-type players receive −€2.5, instead of €7.5, if they submit a "high-income" report.[7] In the audit stage, the computer implements either a flat-rate rule or a bounded rule to audit "low-income" reports submitted. In the experiment, "high-income" reports are not audited. This is consistent with the IRM guidelines (see section 5 for further details).

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

Flat-rate: In this treatment, subjects are told that those filing "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 $K = 2$.[8] If subjects report honestly, nothing will happen to their final payoffs. If cheaters are caught by the auditor, they need to pay back the €10 of taxes evaded 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 the theoretical analysis provided in the appendix for details). Setting $K = 2$ means that if the number of "low-income" reports does not exceed two, then all of them 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 $L = 8$. Instead of merely disclosing the average audit probability, the contingent audit probability structure is fully disclosed to subjects through an audit probability table (see the experiment instructions provided in the appendix for details).

*Bounded-*hi-*q:* Except for the ex-ante probability q of receiving a high income, this treatment is the same as the *Bounded* treatment. The high $q = 0.9$ of this treatment represents the case of an area under the jurisdiction of an IRS District Office where taxpayers

[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 happens.

[8] Because the flat-rate rule induces all-or-none behavior in compliance, such a rule with an audit probability $a < 0.5$ theoretically has the same deterrence effect as the bounded rule, assuming the standard setup with perfectly rational, risk-neutral players.

are more likely to have a high income. Compared to the *Bounded* treatment, subjects lying in this treatment face a higher degree of uncertainty because fewer "low-income" reports will be submitted given the low probability of having low-income taxpayers. Consequently, there will be fewer honest "low-income" reports to pool with lying "low-income" reports, making lying easier to be detected by audits. 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 under the bounded rule when q is high.

2.2 Procedure

The experiment was conducted at the laboratory of a European university from October to December 2009. Most of the university students participating as subjects in the experiment were major in economics or business. The experiment instructions, provided in Appendix C.2, were modified from those in prior tax compliance studies, namely Alm et al. (2009), Kim et al. (2005), and Kim and Waller (2005). We used Z-Tree software (Fischbacher (2007)) to program and conduct the experiment.

Each treatment of the experiment consists of four sessions; each session has 16 subjects. The duration of a session is about 1 hour (including the initial instruction and final payment to subjects). The 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 an exercise making sure that they understand the rules of the tax compliance game.

The game consists of 30 periods. At the beginning of each period, 16 subjects are randomly allocated into two groups of eight. The random re-matching protocol minimizes the chances that subjects encounter the same group of participants again. The purpose is to simulate a one-shot scenario but allows the subjects to be familiar with the game environment. This is particularly important for treatment sessions with the bounded rule. Each period can thus constitute a new observation of a one-shot game, rather than a snapshot of a multiple-period dynamic game. At the end of each period, a summary screen is presented to subjects with feedback information including the subject's true and reported income, and the final payoff for the period. Subjects are not informed of others' payoffs.

Upon finishing the tax compliance game part of the experiment, subjects are asked to complete a risk elicitation task similar to the one used by Holt and Laury (2002). The instructions for the risk elicitation task are handed out only after the tax compliance game. Hence, the subjects are not aware of its existence beforehand. Details of this task can be found in the experiment instructions. The task measures subjects' risk aversion levels, which could be useful in explaining their behaviors.

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 a 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.

We conclude this section by stating the hypothesis for testing, which is based on the prediction (Proposition 2) derived in the theoretical analysis given in the appendix.

Hypothesis 1 *The underreporting rates in the Flat-rate and Bounded treatments are the same.*

Because the game in the *Bounded*-hi-q treatment has multiple equilibria, we merely compare the underreporting rate in the treatment with those in the other two without advancing any hypotheses based on theoretical predictions.

3 Main Results

Figure 1 depicts the average underreporting rates across treatments. The dynamics in 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 over periods.

Table 2 summarizes the compliance behaviors and auditing statistics across experimental treatments. The first three columns contain averages over all 30 periods of play. The next three columns are averages of the last 10 periods, where subjects' behaviors are expected to be more stable after becoming familiar with the environment. Statistical testing on the treatement effects is based on the two-sided Wilcoxon rank-sum test. We adopt the strictest standard to use each session as an independent observation. This avoids any doubt that observations at more refined levels (e.g., by subject or by session-period) might not be completely independent. Such doubt arises from the fact that unlike indivdual decision-making experiments, subjects in our treatments under the bounded rule interact with each other, rather than make their own independent decisions; moreover, their behaviors might be correlated across periods.

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 the pre-specified levels. 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 lying H-type players, divided by 8). The ratio is around 80% in the two treatments.

The middle and the bottom panels of the table provide data for testing our hypothesis and examining the audit budget implications of the experiment results. 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 Wilcoxon 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 still statistically insignificant ($p = 0.564$).

Result 2 *The bounded rule is more cost-effective in the sense that on average*
(i) fewer audits are performed, and
(ii) the budget-usage ratio is higher
in the Bounded treatment than in the Flat-rate treatment.

Support: The difference in the *cheater detection rate*, namely the frequency that a tax cheater is caught during an audit stage, is not statistically significant in both treatments

($p = 0.113$ for all 30 periods; $p = 0.149$ for the last 10 periods). This means that the bounded and flat-rate rules are equally effectively in detecting cheaters.

Several pieces of evidence support that the bounded rule is more cost-effective. Assuming a constant cost per audit, we can use the number of audits performed in a treatment as a proxy for the audit resources consumed to achieve the compliance level observed. Both the *total and the average number of audits* performed are significantly lower in the *Bounded* treatment ($p < 0.05$).

We also look at the *audit selection rate*, which is defined as the proportion of "low-income" reports selected for audit, out of the total number of such reports received. This rate is significantly lower in the *Bounded* treatment, both for all 30 periods or only the last 10 periods ($p < 0.05$). These results suggest that auditing with the bounded rule can achieve the same compliance level at a lower cost.

Finally, we look at the *budget usage ratio*, which is defined as the percentage of audit resources actually used, out of the budget commitment required to credibly support an audit rule. The ratio is 100% in the *Bounded* treatment, which means that all resources committed are used at the full capacity in each period (i.e., two audits). Under the flat-rate rule, the budget-usage ratio is only 32%. The inefficiency is due to the fact that in order to credibly implement the flat-rate rule, the auditor must have the resources to be ready to do all eight audits in each period. However, much fewer audits are actually carried out.

In an equilibrium setting, Yim (2009) has analytically shown that even when the flat-rate rule can be implemented using large-sample random sampling, the budget usage ratio remains substantially below that of the bounded rule. Unfortunately, we cannot assess this theoretical insight with our experiment because the size of the experimental taxpayer population is only eight subjects.

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

Result 3 *The underreporting rate is significantly lower in the Bounded-hi-q treatment than in the Bounded and Flat-rate treatments. The higher compliance level is achieved with significantly fewer audits performed and with a higher budget-usage ratio.*

Support: The average underreporting rate in the *Bounded*-hi-q treatment is 33.95% over all 30 periods and 26.16% in the last 10 periods. The compliance level in this treatment is the highest, as the underreporting rate is significantly lower compared to the other two treatments ($p < 0.05$). The difference is already salient in the first period and remains highly significant throughout the other periods of the game.

Regarding auditing statistics, the total number of audits performed is smaller in this treatment than in the *Bounded* treatment ($p < 0.05$). However, the audit selection rate turns out to be significantly higher ($p < 0.05$), owing to fewer "low-income" reports received given the higher q in this treatment. The cheater detection rate is remarkably higher as well ($p < 0.05$). The budget-usage ratio is 95.63%, which is significantly higher than that in the *Flat-rate* treatment (32.03%).

4 Additional Analyses

While the main results discussed above have answered our research question concerning the impact of disclosing audit policy details, the observed compliance levels remain unexplained by the standard theory. In this section, we make an attempt to better understand individual-level compliance behavior. The purpose is to ensure that we have not overlooked anything that might lead to misinterpretation of the main results.

4.1 Stochastic Nature of Individual-level Behavior

Figure 2 displays the frequency distributions of the individual underreporting rate across treatments. The horizontal axis represents a subject's *individual underreporting rate*, i.e., the percentage of times where the subject when assigned as a high-income taxpayer submits a "low-income" report. The vertical axis represents the percentage of subjects with similar underreporting rates in a treatment.

The main message conveyed by Figure 2 is that the standard theory has limited explanatory power over the individual-level data of the *Flat-rate* and *Bounded* treatments. Only 29.13% of the subjects in the *Flat-rate* treatment and 23.43% of those in the *Bounded* treatment underreport whenever receiving a high income, behaving in accordance with the standard theory. The percentage of seemingly intrinsically honest subjects, who always report their income truthfully, is 12.5% in the *Flat-rate* treatment and 15.63% in *Bounded*. Even after correcting for the presence of seemingly intrinsically honest players, the standard theory still underpredicts the compliance levels observed in the treatments.

Figure 2 also indicates that around 60 percent of the subjects switch between the two options at various levels of frequency. This pattern is very similar in the two treatments (Mann-Whitney test: $p = 0.322$). In contrast, the distribution of the underreporting rate in *Bounded*-hi-q is significantly different (Mann-Whitney test: $p < 0.05$). Throughout this treatment, only about 7% of the H-type choose to submit "low-income" reports, whereas 33% of them honestly report a "high income."

Recognizing the highly stochastic nature of individual-level behavior, we conduct additional analyses to better understand the behavior using several choice models under uncertainty. Becasue the game in *Bounded*-hi-q has multiple equilibria and the compliance behavior observed in the treatment appears to follow a different pattern, we focus on the *Flat-rate* and *Bounded* treatments in our attempt to explain the stochastic component of the behavior.

4.2 Choice Models under Uncertainty

The standard theory predicts that strategic players will always choose to submit "low-income" reports. On the other hand, intrinsically honest players will always report the type of income they receive. In either case, the choices should be consistent across periods. In contrast, Figure 2 suggests that many participants in our experiment make stochastic choices, which is consistent with McFadden's discrete-choice framework (McFadden (2001)).

This framework relaxes the perfect rationality assumption to accommodate boundedly rational behavior. Models in this framework are motivated by empirical studies where observed decisions exhibit some noise (see, e.g., Fischbacher and Stefani (2007), Loomes (2005), Rieskamp (2008), and Wilcox (2011)). Such noise could come from observed sources like decision errors. It could also arise from unobserved or unmodeled channels such as individual perceptions of the game or sensitivity to payoff changes. The presence of such noise leads to people making decision errors and hence behaving inconsistently with their choices.

The *Flat-rate* treatment is essentially a non-strategic choice-under-uncertainty problem for

H-type players. Therefore, the classic individual discrete-choice model is a natural choice to explain the stochastic individual behavior. The *Bounded* treatment introduces interactions among subjects. A general way to incorporate decision errors into a strategic interaction setting is the quantal response equilibrium first proposed by McKelvey and Palfrey (1995). This equilibrium concept is based on McFadden (1973)'s random utility maximization model of the same framework.

Using the discrete-choice framework, we estimate and compare three choice models under uncertainty. They are risk aversion, loss aversion, and loss aversion with probability weighting. Brief descriptions of the models follow. (See the appendix for further details of the loss aversion model and of the discrete-choice framework applied to our experimental setting.)

Risk aversion. The first model we consider simply relaxes the assumption of risk neutrality. In the risk aversion model, subjects are assumed to have a constant relative risk aversion (CRRA) utility function: $u(\pi) = (\pi^{1-r}) / (1-r)$, where π is the disposable income (i.e., after-tax income) and r is the CRRA coefficient. This model offers the possibility of explicitly testing the assumption of risk neutrality. If the estimated r is significantly different from zero, then the null hypothesis that subjects are risk neutral can be rejected. We have also considered alternative utility forms such as constant absolute risk aversion (CARA) and power-expo. There is little change in the goodness of fit to the data.

Loss aversion. While the observed compliance behavior can be explained by risk attitude, it is also consistent with the notion of loss aversion. Recent research has shown that loss aversion provides a much better account of tax evasion both in the lab and in the field (see, e.g., Elffers and Hessing (1997), Yaniv (1999), King and Sheffrin (2002), and Dhami and Al Nowaihi (2007, 2010)). The loss aversion model characterizes individuals as loss-averse in terms of the disposable income relative to some reference income. For a given amount of such relative income $x > 0$ and a value function $v(x)$, losses are weighted more than gains, i.e., $|-v(-x)| > v(x)$. We consider Tversky and Kahneman (1992)'s specification of the value function: $v(x) = x^{\alpha}$ if $x \geq 0$, and $v(x) = -\lambda(-x)^{\beta}$ if $x < 0$. The α and β are the parameters determining the curvature of the function, and λ is the coefficient of loss aversion. Subjects are considered loss-averse if $\lambda > 1$.[9]

[9] Given a fixed payoff structure, data from the tax compliance game alone contain only two moments

Loss aversion with probability weighting. Besides the value function, subjects could also have a nonlinear transformation of the probability scale. For example, Kahneman and Tversky (1979) find that people overestimate low probabilities and underestimate high probabilities. To examine the effect of subjective probability weighting, we also estimate a model of loss aversion with probability weighting. In particular, we consider a popular form of the one-parameter probability-weighting function: $w(\gamma) = \gamma^\delta/(\gamma^\delta + (1-\gamma)^\delta)$, where γ is a probability and $\delta \geq 0$ is the weighting parameter. Note that if $\delta < 1$, the weighting function has an inverted-S shape, which is concave for low probabilities and convex for high probabilities, and crosses the diagonal at the probability of 1/3.

Effectively speaking, H-type players' reporting decision is like choosing between a safe option (honest reporting) and a risky lottery (underreporting), with known, constant probabilities in the *Flat-rate* treatment but unknown, endogenous probabilities in the *Bounded* treatment. Thus, the reporting choice in the *Bounded* treatment is affected by the subjects' *perceived* average audit probability, denoted by $\widehat{a}$. Our analyses let us infer an estimate of $\widehat{a}$. With the estimate, we can answer the following questions: What average audit probability of a flat-rate rule would induce the same level of compliance as observed in the *Bounded* treatment? Moreover, how do risk aversion, loss aversion, and probability weighting influence the subjects' perception of the average audit probability in the *Bounded* treatment?

4.3 Additional Result

Table 3 reports the estimation results of the three models based on the *Flat-rate* and *Bounded* treatments. All coefficient estimates of the models are highly significant (at the 1% level), suggesting that all of them are useful in explaining the compliance levels observed in the treatments. For instance, the risk aversion specification suggests that subjects are risk averse in both treatments, as the CRRA coefficient r is significantly larger than zero. It indicates that risk aversion helps in explaining the data. The perceived audit probability in the *Bounded* treatment is 0.336. In other words, a flat-rate rule with an audit probability of

(i.e., the percentages of subjects selecting the "risky" lottery in the *Flat-rate* and *Bounded* treatments). They lack sufficient identification power to estimate three parameters jointly. Therefore, we pool together the data from the risk elicitation task and the tax compliance game to jointly estimate the parameters.

0.336, rather than 0.4, would induce such risk-averse subjects to comply at a level similar to what has been observed in the *Bounded* treatment.

Results of the loss aversion specification suggest that subjects are loss-averse. The estimated coefficient of loss aversion λ is larger than 1 in both treatments, which means that subjects are more sensitive to a loss than a gain of the same magnitude. The estimated slope coefficients of the value function indicate concavity in the gain domain (α) and convexity in the loss domain (β). Moreover, a Vuong test on non-nested models favors the loss aversion model over the risk aversion model ($p < 0.05$). For loss-averse subjects, a flat-rate rule with an audit probability of 0.306 would induce the same compliance level as observed in the *Bounded* treatment.

The third specification combines loss aversion with probability weighting. We find that this specification does not improve the goodness of fit significantly. Moreover, the probability-weighting parameter δ is not significantly different from 1 for both treatments ($p = 0.438$ and 0.397 for *Flat-rate* and *Bounded*, respectively). So the subjectively weighted probabilities used by the subjects on average are in line with the objective probabilities. Overall, the results suggest that what drives the observed compliance level is likely to be the way the subjects view losses and gains, rather than how they assess probabilities.

Figure 3 displays the predicted underreporting rates based on different models and the actual rates observed in the treatments. Because probability weighting adds little to the loss aversion model, the predicted underreporting rate of this model is based on coefficients estimated without probability weighting. The compliance behaviors in our experiment are best explained by the loss aversion model, compared to the alternatives, namely the risk aversion model and the standard theory with perfectly rational, risk-neutral players (with and without correction for seemingly intrinsically honest subjects). We conclude this section with the following result.

Result 4 *The compliance levels observed in the Flat-rate and Bounded treatments can be satisfactorily explained by a loss aversion model under the discrete-choice framework.*

The additional analyses in this section solve the otherwise unexplained levels of compliance observed in the experiment. Throughout the process, we do not find anything that might compromise the conclusion from our main analysis.

5 Relation to Tax Compliance Literature

IRS has the practice of disclosing only broad statistics such as the audit rate of each income class. In line with this, many tax compliance studies consider settings where taxpayers are told to be audited independently at a known, constant probability (see literature reviews by Andreoni et al. (1998), Alm and McKee (1998), and Slemrod and Yitzhaki (2002)). Such a setting is captured by the *Flat-rate* treatment in our experiment.

In the vast majority of tax compliance studies, the attention is on the interaction between the auditor and a taxpayer, without considering the interaction with the whole taxpayer population, or the interactions among taxpayers. A notable exception is Alm and McKee (2004), who experimentally study a "DIF" rule that represents 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 of the incomes reported by all other players. This audit rule induces a coordination problem for taxpayers who want to cheat on taxes. In their experiment, all participants receive the same level of income in any given period. This is not the case in our experiment. Besides this distinction leading to a different coordination problem in the *Bounded* treatment, another difference is that the interaction induced by the bounded rule among taxpayers does not always lead to a coordination game.

Tax compliance studies rarely explicitly consider audit budget. Unlike others, Yim (2009) emphasizes the importance of the budget commitment required to support an audit policy and the implication to the structure of the policy. Using a setting similar to the classic tax compliance game (Graetz et al. (1986)), he shows that the equilibrium audit policy that minimizes the required committed budget takes the form of the bounded rule. Such a binary-income setting, or similar discrete-type extensions, have been used in many studies (e.g., Mills et al. (2010), Mills and Sansing (2000), and some others cited in footnote 4 of Yim (2009)).

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 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.

Besides Yim (2009), Erard and Feinstein (1994) also explicitly consider audit budget. However, like other tax compliance studies, they focus on the interaction between the auditor and an atomic taxpayer in the population. This effectively reduces the whole taxpayer population into a representative taxpayer. The complexity of the model gives rise to the characterization of the equilibrium by a second-order differential equation The equation does not have a closed-form solution and hence can only be solved numerically. In contrast, the setting of the classic tax compliance game is much simpler. Morever, the bounded rule that constitutes an equilibrium audit strategy has a simple structure determined by the audit capacity constraint.

Indeed, audit capacity is an important concern in IRS's operations. Guidelines in the IRM suggest that a subtantial part of the agency's operations is done at the District Office level, referred to as "[geographical] Area" in the manual. The audit capacity of each District Office, namely the staff force constituting mainly of revenue agents, tax compliance officers, return classifiers, etc (referred to as "posts-of-duty (POD)" in the manual), is determined based on the approved national examination plan constrained by resources requested in the Congressional Budget (see IRM 4.1.1.2 (10-24-2006) "Examination Plan").

Besides audit capacity, the bounded rule or the binary-income setting has other stylized features resembling IRS's audit policy. To point out the similarity, it is useful to begin with a quick overview of the audit selection procedure in reality. According to the IRM, tax returns are first computer-scored using the DIF System (see IRM 4.1.3.2 (10-24-2006) "DIF Overview"). Then with the national minimum cutoff score determined by National Headquarters each year, returns above the cutoff are added to the DIF inventory (see IRM 4.1.1.3 (10-24-2006) "Minimum DIF Cutoff Score").

Alm and McKee (2004) have studied a "DIF" rule that triggers an audit to a taxpayer based on the "deviation between his or her reported income and the average reported income" in an experiment session. Anecdotal evidence, however, indicates that what matters most is not the reported income of a return relative to others' average. "[T]ax professionals, who are familiar with I.R.S. procedures, say that the [DIF] formula examines the relationships between those income and deduction items that the I.R.S. has found to be the best indicators of compliance, chiseling and cheating." (Johnston (1996)). In line with this, a statistics professor Aczel (1994) has used a "supercomputer and modern statistical techniques like logistic regression or classification and regression trees to determine which kinds of returns get audited" (Johnston (1996)) and found that "taxpayers whose Schedule A deductions are less than 35 percent of income are almost never audited, while those who deduct 44 percent or more of income are almost certain to be audited. Those who fall in between those figures are at risk of being audited, depending on which type of deductions they take." (Johnston (1995a)).

Thus, whether certain deduction items have been claimed and their amounts relative to the reported income of the return seems to be most important. A return would have little chance to be added to the DIF inventory if "suspicious" deduction items were not claimed. The red-flag nature of claiming "suspicious" deduction items is similar to the pooling of "low-income" reports by lying taxpayers with those by honest low-income taxpayers in the binary-income setting of the experiment.

Not every return added to the central DIF inventory will eventually be audited. To be selected for audit, a return must first be among those ordered by a relevant Area for classification into *accepted as filed* or *selected for examination* (i.e., audit) (see IRM 4.1.5.1.3

(10-24-2006) "Sorting of Classified Returns"). Areas might have different selection rates for a variety of reasons (e.g., local issues, classifiers' judgment, etc). Therefore, to meet the audit target of an Area in the Examination Plan, "[t]he PSP [(i.e., Planning and Special Programs Territory Manager)] will calculate the Area DIF cutoff score ... giving consideration to the selection rate." (see IRM 4.1.1.3.1 (10-24-2006) "DIF Cutoff Score"). With the Area DIF cutoff, returns of an Area are divided into two groups: *above-cutoff* returns (analogous to the "low-income" reports in the experiment) and *below-cutoff* (analogous to the "high-income" reports).

Areas order returns from the cerntral DIF inventory based on their specific cutoffs. After classification, returns selected for audit are catergorized into "Field Examination" (i.e., visits at taxpayers' sites) or "Office Examination" (i.e., interviews at IRS offices) (see IRM 4.1.5.1.3 (10-24-2006) "Sorting of Classified Returns"). The returns are added to the Examination inventory (see IRM 4.1.1.6.3 (10-24-2006) "Inventory Monitoring"). Later the audits of these returns are assigned to POD's (i.e., revenue agents, tax compliance officers, etc) "based on ZIP codes [on the returns] using the ZIP/POD Lookup Table" (see IRM 4.1.1.7 (10-24-2006) "ZIP/POD Tables").

The IRM has guidelines to regulate the flow of orders in accordance with the Examination Plan. A POD Supplement Order is allowed as an exception if "there is a workload shortage at a specific POD" (see IRM 4.1.3.4 (10-24-2006) "Guidelines for Ordering Returns"). Nonetheless, the IRM specifies that if such orders "result in the delivery of returns that are below the [Area] DIF cutoff score", "not more than 10% of the returns ordered for any POD should be below the DIF cutoff score." (see IRM 4.1.1.3.2 (10-24-2006) "Use of DIF Cutoff Score for Return Orders"). In other words, aside from the 10% flexibility, a POD is not permitted to audit below-cutoff returns even when the POD has audited all the above-cutoff returns assigned to it, with idle capacity to audit more. This feature is similar to the key characteristic of the bounded rule: audit as many as possible if the number of suspicious reports exceeds the given capacity, or otherwise audit all such reports but none of the unsuspicious despite under-utilized capacity.

Because of the simple setting, the similarity with key features of the reality, and the theoretical equivalence to the flat-rate rule's deterrence effect, we use the bounded rule to represent the underlying audit policy of a tax athority that discloses merely the average audit probability to taxpayers.

6 Concluding Remarks

Tax authorities around the world often are reluctant to disclose audit policy details.[10] In particular, the US IRS has the practice of releasing broad statistics like the audit rate of each income class but opposes pressures demanding details on how different circumstances might result in a higher audit probability to taxpayers. In this paper, we ask whether the potential adverse impact on tax compliance could be a serious concern justifying the reluctance of tax authorities like IRS to disclose audit policy details.

To answer the question, we carefully consider the theoretical deterrence-equivalence of two audit rules and the documented WPPT puzzle in designing the treatments of our experiment. In the *Flat-rate* treatment, participants are told that they independently face a known audit probability. By constrast, participants in the *Bounded* treatment are fully informed of the contingent audit probability structure. We first show that according to the standard theory, participants should have a sufficiently strong incentive to lie about their income, regardless of the treatments. Based on this theoretical prediction that is consistent with the WPPT puzzle, we develop the hypothesis for testing.

Our findings show that consistent with the WPPT puzzle, the observed compliance levels are substantially higher than the theoretically predicted levels. Most important, the compliance levels of the two treatments that represent merely disclosing the average audit probability versus fully disclosing the audit policy details are not significantly different. This main result supports our hypothesis, suggesting that disclosing audit policy details do not necessarily reduce tax compliance. The examination with a third treatment to assess the sensitivity of our results to the existence of multiple equilibria suggests that disclosing audit policy details can increase, rather than reduce tax compliance.

We check two things to ensure that behaviors observed in the experiment are consistent

[10] "[M]ost [tax] agencies undertake substantial precautions to maintain the secrecy of their audit selection procedures." (Andreoni et al. (1998)). See OECD (2004) for an overview of various countries' audit selection systems.

with what we know from theories, and hence our main results are not compromised by anything that we could not explain. First, we verify that the audit budget implications of the observed behaviors are broadly consistent with the theoretical insights of the study where the equivalence between the bounded and flat-rate rules was derived. Then we use alternative choice models under uncertainty to explain the observed compliance levels under-predicted by the standard theory. Results from these exercises confirm what we know from theories. We therefore believe that our main result is not affected by some unknown factor.

Obviously, the evidence collected from one experiment cannot constitute a strong ground for tax authorities (sharing IRS's concern) to change their disclosure practices. Nevertheless, given the trend in increasingly stronger demand for government transparency, the evidence from this experiment does provide a reasonable basis for tax authorities to be more open-minded in viewing the issue. Compared to IRS, some agencies in other countries appear to be more liberal and transparent (see, e.g., Canada Revenue Agency and Australian Tax Office discussed in Hasseldine (2007) and Leviner (2008)). However, unless tax authorities let researchers examine more accurately and thoroughly the impacts of disclosing audit policy details, no one can tell what level of disclosure is best for society.

Let us re-iterate our suggestion already made in the introduction: IRS can set out a plan to disclose on a properly selected date of each year more information about the audit policy. This way researchers can precisely analyze the impact of the incremental information disclosure. Furher evidence can thus be provided to determine whether even more disclosure or IRS's current position of information withholding should be supported.

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Figure 1: Dynamics of underreporting rate over 30 periods

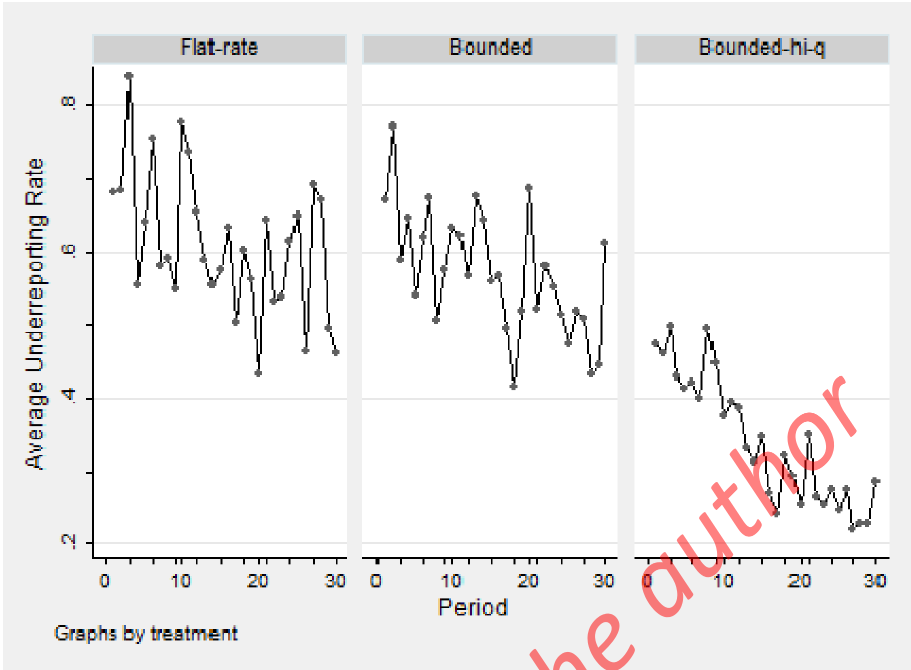


Figure 2: Frequency distributions of individual underreporting rate

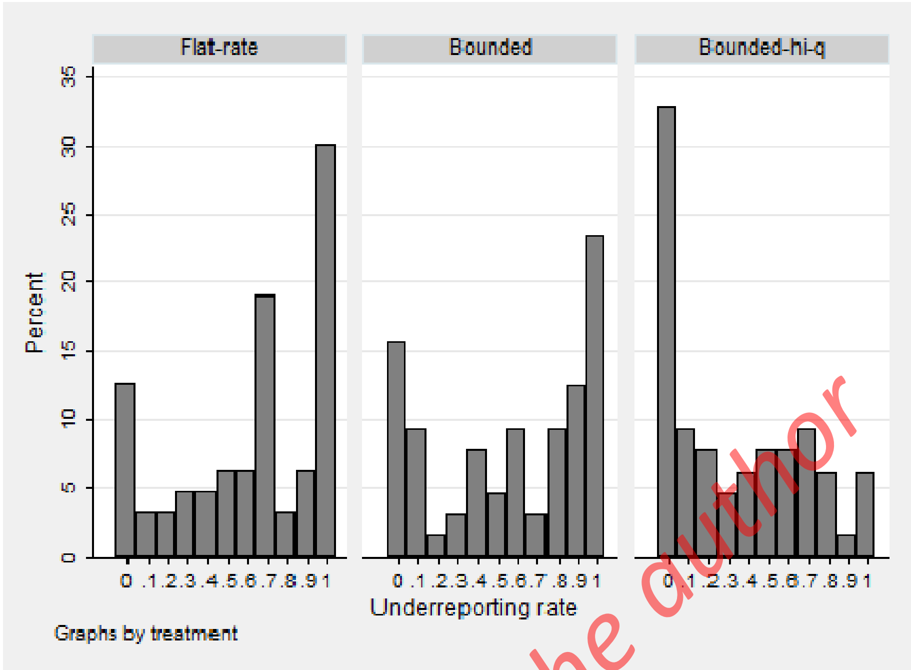


Figure 3: Model predictions of underreporting rate versus actual observations

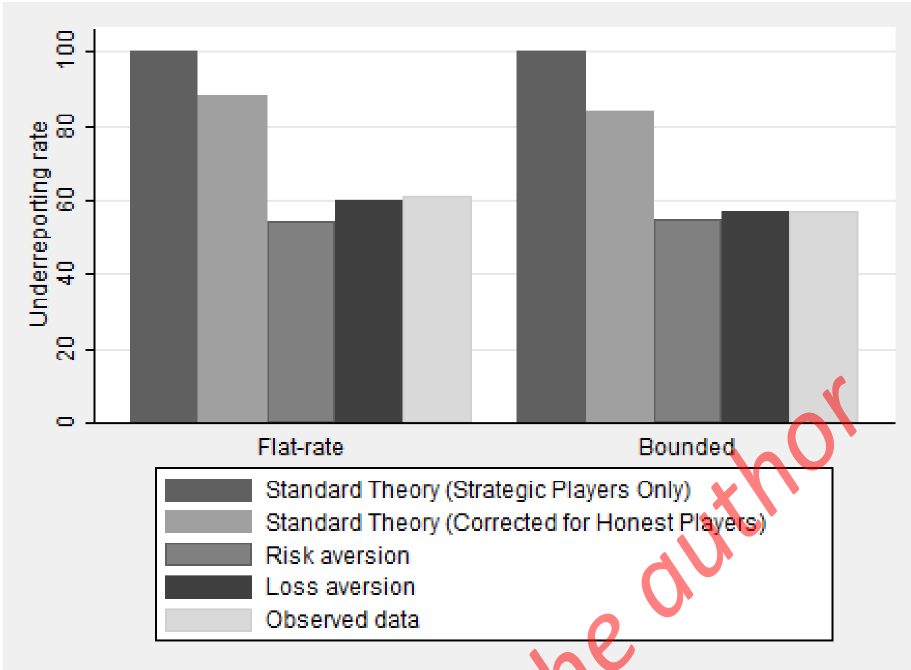


Table 1: Experimental treatments

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

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*		$p < 0.05$			$p < 0.05$	
Auditing statistics						
Cheater detection rate	38.76%	33.13%	73.27%	42.08%	31.88%	70.97%
	(0.065)	(0.043)	(0.025)	(0.107)	(0.125)	(0.105)
Bounded v. Flat-rate	$p = 0.113$			$p = 0.149$		
Bounded-hi-q *v. Bounded*		$p < 0.05$			$p < 0.05$	
Total no. of audits	153.8	120	114.8	53.75	40	37
	(18.14)	(0.000)	(4.500)	(8.098)	(0.000)	(2.160)
Bounded v. Flat-rate	$p < 0.05$			$p < 0.05$		
Bounded-hi-q *v. Bounded*		$p < 0.05$			$p < 0.05$	
Avg. no. of audits (per group per period)	2.56	2	1.91	2.69	2	1.85
	(0.300)	(0.000)	(0.065)	(0.414)	(0.000)	(0.093)
Bounded v. Flat-rate	$p < 0.05$			$p < 0.05$		
Bounded-hi-q *v. Bounded*		$p < 0.05$			$p < 0.05$	
Audit selection rate	40.16%	31.71%	59.99%	42.96%	32.94%	71.31%
	(0.030)	(0.006)	(0.062)	(0.038)	(0.007)	(0.095)
Bounded v. Flat-rate	$p < 0.05$			$p < 0.05$		
Bounded-hi-q *v. Bounded*		$p < 0.05$			$p < 0.05$	
Budget usage ratio	32.03%	100%	95.63%	32.09%	100%	92.54%
	(0.181)	(0.000)	(0.032)	(0.181)	(0.000)	(0.033)
Bounded v. Flat-rate	$p < 0.05$			$p < 0.05$		
Bounded-hi-q *v. Bounded*		$p < 0.05$			$p < 0.05$	

Note: Standard errors are in the parentheses. Statistical testing on the treatement effects is based on the two-sided Wilcoxon rank-sum test, with each session constituting an independent observation. *High-income frequency* is the actual frequency of the subjects being assigned as a high-income taxpayer 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 lying high-income taxpayers. *Underreporting rate* is the percentage of times where subjects when assigned as a high-income taxpayer submit a "low-income" report. *Cheater detection rate* is the frequency that a tax cheater is caught during an audit stage. *Audit selection rate* is the proportion of "low-income" reports selected for audit, out of the total number of such reports received. *Budget usage ratio* is the percentage of audit resources actually used, out of the budget commitment required to credibly support an audit rule.

Table 3: Estimation of choice models under uncertainty

	Risk aversion		**Loss aversion**		**Loss aversion with Prob. Weighting**	
	Flat-rate	*Bounded*	*Flat-rate*	*Bounded*	*Flat-rate*	*Bounded*
CRRA coefficient r	0.366 (0.350)	0.594 (0.055)				
Gain domain curvature α			0.445 (0.034)	0.428 (0.038)	0.640 (0.459)	0.533 (0.075)
Loss domain curvature β			0.548 (0.052)	0.708 (0.030)	0.586 (0.068)	0.858 (0.073)
Loss aversion coefficient λ			1.100 (0.802)	1.148 (0.030)	1.674 (0.123)	1.283 (0.171)
Weighting parameter δ					1.150 (0.193)	0.899 (0.120)
Perceived audit prob. $\widehat{a}$		0.336 (0.017)		0.305 (0.007)		0.240 (0.023)
Log-likelihood	-1163.773	-1087.292	-1141.710	-1082.473	-1141.353	-1082.111
Observations	2331	2287	2331	2287	2331	2287

Note: All coefficient estimates in this table are statistically significant at the 1% level. To account for within-group correlation, the standard errors are clustered by individual. The *risk aversion* specification is based on a constant relative risk aversion (CRRA) utility function: $u(\pi) = (\pi^{1-r})/(1-r)$, where π is the disposable income (i.e., after-tax income) and r is the CRRA coefficient. The *loss aversion* specification is based on Tversky and Kahneman (1992)'s specification of the value function: $v(x) = x^{\alpha}$ if $x \geq 0$, and $v(x) = -\lambda(-x)^{\beta}$ if $x < 0$, where α and β are the parameters determining the curvature of the function in the gain and loss domains, respectively, and λ is the coefficient of loss aversion. The *loss aversion with probability weighting* specification is based on a popular one-parameter probability-weighting function: $w(\gamma) = \gamma^{\delta}/(\gamma^{\delta}+(1-\gamma)^{\delta})$, where γ is a probability and $\delta \geq 0$ is the weighting parameter. The *perceived* audit probability $\widehat{a}$ is the audit probability of a flat-rate rule that would induce the same level of compliance as observed in the *Bounded* treatment.

Appendix

A Theoretical Analysis

A.1 Model Setup

The theoretical model setup underlying our experiment is similar to that in Yim (2009). He derived the bounded rule using the classic tax compliance game setting. A comparison of our setup with Yim's is provided at the end of this section. Below we first describe the model setup and then explain the predictions in different treatments of the experiment.

Consider a taxpayer population of a given income class of size N. For simplicity, we assume two income levels: high and low, denoted by I_{H} and I_{L}, respectively, where $I_{\mathrm{L}} < I_{\mathrm{H}}$. Each taxpayer has a probability q of having a high income (H-type) and $1 - q$ of low income (L-type), where $0 < q < 1$. Taxpayers know the type distribution as well as their own types, but they do not know the types of the others. Each taxpayer has to decide simultaneously and privately whether to report a "high income" or "low income" to the tax authority. Let T_{H} and T_{L} be the tax payments for taxpayers filing "high-income" and "low-income" reports, respectively, where $T_{\mathrm{H}} < I_{\mathrm{H}}$, $T_{\mathrm{L}} < I_{\mathrm{L}}$, and $T_{\mathrm{L}} < T_{\mathrm{H}}$. If cheaters are audited, they will be caught with a fine $F > 0$ imposed on top of the tax they need to pay. Taxpayers who report truthfully are assumed to incur no cost if they are audited. The theoretical analysis in this section is based on the simplest setting with perfectly rational, risk-neutral taxpayers maximizing the disposable income (i.e., after tax and fine, if applicable).

Flat-rate rule. Any taxpayer filing a "low-income" report will independently face a flat audit probability a_{FR}. Since reporting truthfully does not incur any cost when being audited, L-type players always report their income truthfully. If they report a "high income," the report will not be audited. They will be taxed T_{H}, which is larger than the tax T_{L} on a honestly reported income. For H-type players, the honest-reporting payoff is $I_{\mathrm{H}} - T_{\mathrm{H}}$. Underreporting gives them a payoff of $I_{\mathrm{H}} - T_{\mathrm{L}}$ if they are not audited, and $I_{\mathrm{H}} - T_{\mathrm{H}} - F$ if they are audited. Therefore, underreporting is an optimal choice when the expected payoff from it is larger than the payoff from honest reporting:

$$(1 - a_{FR})(I_{\mathrm{H}} - T_{\mathrm{L}}) + a_{FR}(I_{\mathrm{H}} - T_{\mathrm{H}} - F) > (I_{\mathrm{H}} - T_{\mathrm{H}}).$$

That is to say, H-type players will underreport if the audit probability is less than the threshold $\bar{a}$ defined below

$$\bar{a} = (T_{\mathrm{H}} - T_{\mathrm{L}})/(F + T_{\mathrm{H}} - T_{\mathrm{L}});$$

otherwise, honest reporting is an optimal choice.

Owing to its "coin-flipping" nature, the flat-rate rule cannot be credibly implemented unless the auditor has a budget commitment to be ready to audit N "low-income" reports, if indeed received. In contrast, the bounded rule described below is characterized by an audit capacity $K < N$.

Bounded rule. This rule, like the flat-rate rule, never audits "high-income" reports. Let L denote the number of "low-income" reports received by the auditor. If L is smaller than or equal to the audit capacity K, the auditor will audit all L reports. However, if L is larger than K, the auditor will randomly audit K of the reports. Consequently, a taxpayer filing a "low-income" report faces a contingent audit probability under the bounded rule:

$$a_{BD} = \begin{cases} 1 & \text{if} \quad L \leq K \\ K/L & \text{if} \quad L > K \end{cases} \quad \text{for } L = 0, 1, ...N.$$

A key feature of the bounded rule is that the audit probability a_{BD} is no longer exogenously given. Instead, it depends on the audit capacity K and the number L of "low-income" reports submitted. The latter is affected by the population size N and the ex-ante probability q of a taxpayer having a "high income." The following proposition characterizes an important property of the bounded rule.

Proposition 1 *For any given N and q, the auditor can always choose an audit capacity K for the bounded rule such that it induces the same compliance level as the flat-rate rule.*

The intuition of Proposition 1 is as follows. Any audit probability a_{FR} under the flat-rate rule induces all-or-none compliance behavior. If the maximum number of K is so high that all "low-income" reports will always be audited for sure, H-type players will have no incentive to underreport. On the other hand, if K is zero (meaning that no audit is conducted regardless of the number of "low-income" reports submitted), then H-type players will underreport with certainty. Between these two extreme cases there exists a threshold $\bar{K}$ such that any $K > \bar{K}$ sustains compliance behavior regardless of the actual income-realization parameter q. That is, even in the scenario where all taxpayers file "low income" reports, the audit probability is still high enough to deter tax evasion.

To induce full compliance, however, the committed budget K does not always need to be larger than $\bar{K}$. Put it differently, even when $K < \bar{K}$, the bounded rule is still able to induce full compliance. Depending on the parameters, the interactions among taxpayers induced by the bounded rule could either be a dominance-solvable game with one unique equilibrium, or a coordination game with multiple equilibria. We construct two treatments to empirically examine the deterrence effect of the bounded rule under each situation.

A.2 Predictions in Treatments

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 A.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 a 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 underreport.

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,

[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.

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 her/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 truth-telling 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: $B_i = 1 - q + qb_i$.

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-type obey dominance, then the strategy of reporting truthfully (h) is dominated. That is, even when a 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) reports a "low income" truthfully. The calculation guarantees that evading taxes is always a best response for a H-type player when L-type players obey dominance. Proposition 2 stated below provides the theoretical foundation for our hypothesis for testing.

Proposition 2 *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.*

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. We focus on the symmetric equilibria because asymmetric equilibria, though exist in this setting, require unrealistic coordination among the ex ante homogenous players.

Proposition 3 *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 each 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.*

Now consider that some players are intrinsically honest. They report their income truthfully, regardless of their income type. This assumption does not change the treatment differences, as long as the percentage of intrinsically honest players is identical in both treatments. Recall that in the *Bounded* treatment, the optimal strategy of the H-type players does not depend on their beliefs towards other H-type players. As long as they believe that L-types will not play dominated strategy (i.e. reporting a "high income"), they can form expectations on the proportion of "low-income" reports submitted to the auditor. Given that the ex-ante probability of being a L-type player is sufficiently high ($q = 0.5$), the sure payoff for a H-type player from reporting honestly is lower than the expected payoff from underreporting, even when s/he does not expect any other H-types to underreport. This ensures that all H-type players in the *Bounded* treatment will continue to underreport with or without intrinsically honest players. In the *Flat-rate* treatment, players make decisions independently. The audit probability facing a taxpayer is not influenced by the existence of intrinsically honest players. In sum, if the percentage of intrinsically honest players is assumed to be the same in both treatments, the treatment difference in the underreporting rate is unaffected.

In the *Bounded*-hi-q treatment, suppose that each taxpayer has a known probability ρ of being an intrinsically honest player. If ρ is sufficiently large, strategic players will find underreporting too risky to be worth the attempt. If that is the case, this modification could be considered as a refinement of the coordination game. However, if ρ is small, the payoff-dominant Nash equilibrium still exists, provided that a strategic player has a strong belief in the noncompliance behavior of other strategic players. We find that (seemingly) intrinsically honest players consist of 15% of all subjects in the *Flat-rate* treatment. Assuming strategic players correctly anticipate that $\rho = 0.15$, the threshold belief inducing underreporting behavior changes from 0.432 to 0.508. Nonetheless, the two pure-strategy equilibria remain the same.

A.3 Relation to Yim (2009)

In Yim (2009), the auditor interacts strategically with taxpayers by choosing an audit capacity without openly committing to it before taxpayers making reporting choices. Therefore, like the classic tax compliance game, deterministic underreporting by all H-type taxpayers cannot constitute an equilibrium.

In theory, we can structure the parameters of the two audit rules such that in equilibrium the induced compliance levels are identical, and then we design a fully-strategic game experiment accordingly. However, this requires a demanding understanding about the game and a mutual belief towards each other's choice. Any off-equilibrium decisions by subjects taking the auditor role will have unpredictable impacts on others taking the taxpayer role, leading to unmanageable complications in comparing the treatment results.

Considering such complications, we let the auditor commit to an audit rule and focus on taxpayers' reactions. Therefore, our experiment is not a direct test of Yim's model. Instead, we regard our examination of the budget implications of the audit rules as a simple check on the robustness of the theoretical insights, i.e., to see whether they still largely hold outside the original setting where the bounded rule was derived.

B Technical Details and Proofs

B.1 Technical Details: Discrete-choice Framework and Loss Aversion Models

Discrete-choice framework applied to the experimental setting. According to the discrete-choice framework, H-type players will choose to underreport if and only if the difference in the expected utilities is sufficiently large to exceed a stochastic error denoted by ε. Formally, this is expressed as

$$EU(\pi_u) - EU(\pi_h) > \varepsilon.$$

where π_u and π_h denote the disposable income from underreporting and honest reporting, respectively. The stochastic error ε is commonly assumed to be independently and identically distributed across players and actions with a Type 1 extreme value (i.e., logit) distribution. The error can come from many sources, including the inability to calculate the expected payoff or trembling hands during decision making.

Because expected utility is unique up to an affine transformation, a standard result of the discrete-choice framework is that under the error distributional assumptions above, the underreporting probability $\widehat{b}$ is given as follows:

$$\begin{aligned}\widehat{b} &= \Pr\{EU(\pi_u) - EU(\pi_h) > \mu\varepsilon\} \\ &= \frac{1}{1+\exp[-(EU(\pi_u) - EU(\pi_h))/\mu]},\end{aligned} \tag{1}$$

where $\mu > 0$ representing the sensitivity of a subject's reporting choice to the relative payoffs of the two choices. When μ approaches infinity, players choose underreporting and honest reporting with equal probability, independent of the relative expected payoffs. As μ decreases, players put less probability weight on choices that yield suboptimal payoffs. When μ approaches 0, the probability of their selecting the optimal choice converges to 1. Simply put, μ reflects the magnitude of the measurement error when subjects calculate expected utilities from underreporting and honest reporting.

Our baseline specification assumes risk-neutral players, i.e., $U(\pi) = \pi$. So for this specification, the conditional log-likelihood function used for estimation is as follows:

$$\ln L = \sum_{i,t} \left\{ y_{it} \ln \left(\frac{1}{1+\exp[(E(\pi_h)-E(\pi_u))/\mu]} \right) + (1-y_{it}) \ln \left(\frac{\exp[(E(\pi_h)-E(\pi_u))/\mu]}{1+\exp[(E(\pi_h)-E(\pi_u))/\mu]} \right) \right\},$$

where

$$E(\pi_u) = \begin{cases} 0.6 \times 22.5 + 0.4 \times 2.5 & \text{for the } \textit{Flat-rate} \text{ treatment} \\ (1-\widehat{a}) \times 22.5 + \widehat{a} \times 2.5 & \text{for the } \textit{Bounded} \text{ treatment,} \end{cases}$$

$y_{i,t} \in \{0,1\}$ indicates whether subject i underreports (1) or honestly reports (0) in the tax compliance game in period t, and $\widehat{a}$ is the perceived audit probability estimated jointly with μ. For the other models, we change the specification of $E(\pi)$ accordingly to fit the assumption of risk aversion, or loss aversion, with and without probabilitly weighting.

Loss aversion with and without probability weighting. We follow Dhami and al-Nowaihi (2007, 2010) to use the true disposable income as the reference income $R = I_{\mathrm{H}} - T_{\mathrm{H}}$. With this reference point, we define the relative income as

$$x = \begin{cases} I_{\mathrm{H}} - T_{\mathrm{H}} - F - R & \text{if caught.} \\ I_{\mathrm{H}} - T_{\mathrm{L}} - R & \text{if not caught.} \end{cases}$$

The rationale for defining the relative income this way is as follows. If the reference income was specified differently, say, using the initial income or the income after cheating detection, taxpayers would always be in the domain of losses or in the domain of gains. Hence, the asymmetry of losses and gains cannot affect their behaviors, and the loss aversion model would fall back into some kind of expected-utility framework. Such a framework is referred to as rank-dependent expected utility theory. It may be seen as the expected utility theory applied with a transformed cumulative probability distribution (see Dhami and al-Nowaihi 2007 for more details).

B.2 Proof of Proposition 1

Let $\bar{K} = [\bar{a}N]$, as K needs to be an integer. Thus, $a_{BD} = \min\{1, \overline{K}/L\} = \min\{1, [\bar{a}N]/L\}$. Since $L \leqslant N$, $a_{BD} \geqslant \bar{a}$. That means, in the scenario where all players declare low income, the audit probability a_{BD} is equal to $\bar{a}$. The H-type players are indifferent between the decisions of underreporting and reporting honestly. If $K > \bar{K}$, that means the lowest probability of being audited is strictly larger than $\bar{a}$. Hence, any $K > \bar{K}$ is sufficient to support full compliance.

The simplest case to induce zero compliance is to set $K = 0$. Because of zero audit, self-regarding, profit-maximizing H-type players always report low income, regardless of their beliefs towards other H-types. More generally, if $K < [\bar{a}]$, the bounded rule cannot induce any compliance for strategic players regardless of the income distribution. In other words, in the worst-case scenario in which only one H-type player claims low income, the audit probability he or she faces is lower than $[\bar{a}]$. Hence, strategic H-type players will underreport.

B.3 Proof of Proposition 2

This subsection contains two parts. The first part proves that given 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 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 comply 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 a H-type player anticipates that no body other than him or her 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 L-type. Since the probability of being a 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 $\textbf{Bin}\ (n, N-1; q) = \textbf{Bin}\ (n, 7; 0.5)$. 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-interest, risk neutral H-type player will underreport.

The remaining proof shows that for any given set of beliefs held by a H-type player, the expected payoff from underreporting is always not less than $E(\pi_l|\overline{b_0})$. Assume that player N thinks the first $N-1$ players underreport with probability $\bar{b} = (b_1, b_2, ..., b_{N-1})$. The probability that player i submit "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, $\partial E(\pi_l)/\partial b_i = (\partial E(\pi_l)/\partial B_i) \cdot (\partial B_i/\partial b_i) > 0$.[12] This means that the expected payoff from underreporting is increasing in the (subjective) propensity to evade taxes. Hence, given any set of belief $\bar{b} = (b_1, b_2, ..., b_{N-1})$, $E(\pi_l|b) \geq E(\pi_l|\overline{b_0})$. Hence, the best response of the H-type players is to underreport. ∎

The second part of this subsection proves that the introduction of intrinsically honest players does not change the directions of treatment difference. Let ρ be the probability that

[12] Calculation is available upon request.

a player is intrinsically 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 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.

To prove the statement, we only need to show that the inclusion of honest 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 of 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.

In the presence of honest players, the non-compliance rate of both treatments becomes:

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

B.4 The Existence of Coordination

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

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

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

$B_iN = [(1-q)+qb_i]N$. The expected profit from underreporting could be simplified as

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

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.

B.5 Proof of Proposition 3

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

Let us examine the former case. Given that a H-type player thinks that all other H-types choose strategy u, s/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 the incentive to deviate from underreporting, which constitutes a 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 has also a mixed-strategy equilibrium in which each H-type player is indifferent between the strategy of honest-reporting and underreporting. Given the game parameters, the underreporting probability b that induces utility indifference is $b^*_{SE} = 0.432$. ∎

C Instructions

C.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, and 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.

C.2 Instructions for *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 to €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 to €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 to €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 to €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, it 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

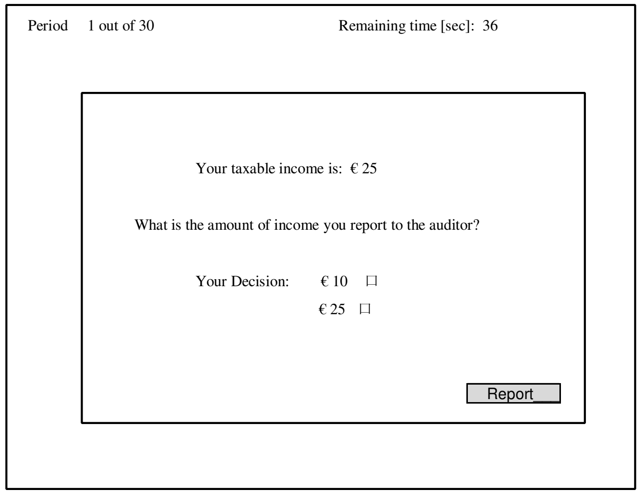
Received Income	**Probability**	**Reported Income**	**Audit Probability**	**After-tax Income if audited**	**After-tax Income if NOT audited**
€25	0.5	€25	0	€12.5	€12.5
		€10	see audit prob. table	€2.5	€22.5
€10	0.5	€10	see audit prob. table	€7.5	€7.5
		€25	0	−€2.5	−€2.5

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.

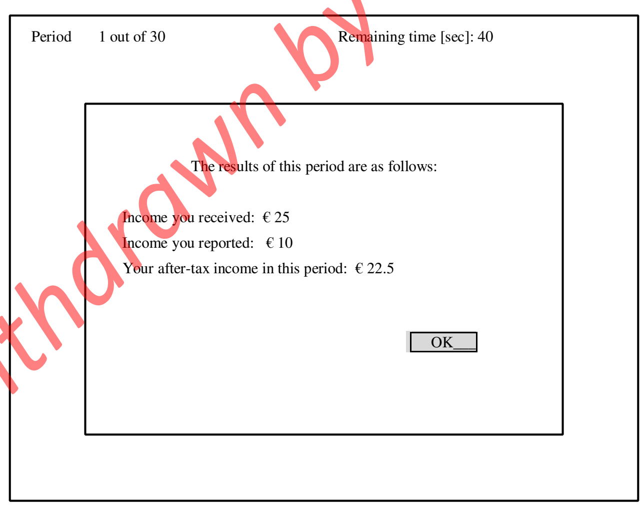
SCREEN 1



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. Your 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.

C.2.1 Risk Elicitation Task[13]

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.

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

C.2.2 Post-experimental Questions

Questions on Treatment Manipulation

Please evaluate the following statements with respect to the tax reporting task:[14]

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

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 as 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%.[15]

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?

[14] 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.

[15] In the *Bounded-Hq* treatment, the chance should be 90%, instead of 50%.

2. What is your nationality?

3. How many years have you already studied in economics?

4. Have you ever had a course related to game theory?

5. Have you ever had a part-time job?