The Effect of Perfect Monitoring of Matched Income on Sales Tax Compliance: An Experimental Investigation

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Abstract: Noncompliance is a quantitatively important phenomenon that significantly affects revenue sources for governments. This phenomenon raises challenging questions about the determinants of tax reporting and about the appropriate design of a tax system. This paper provides specific empirical insights using an experimental approach to evaluate the effects of systematic sales tax monitoring and the determinants of sales tax compliance. The results indicate that if perfect monitoring is instituted without other complementary policies, an increase in tax revenues is not the likely outcome. Once people have chosen their level of tax compliance, a stepped up policy of increased monitoring aimed at reducing fiscal fraud may not necessarily increase tax revenues. The reference-dependent effect observed in the data suggests that individuals will try to recover their losses following any policy changes even if it means taking more risks.

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1. Introduction

Noncompliance is a quantitatively important phenomenon that significantly affects revenue sources for governments. This phenomenon raises challenging questions about the determinants of tax reporting and also about the appropriate design of a tax system: how many resources should be devoted to auditing?

There have been a number of studies, theoretical and empirical, on the impact of audits on income tax compliance. Using random surveys (Fisher et al., 1989), and available tax databases (Clotfelter, 1983; Dubin et al, 1990; Erard and Ho 2001), researchers have identified characteristics of noncompliant taxpayers and what is likely to motivate tax compliance. Clotfelter (1983) provided an empirical analysis of taxpayer compliance with information from the Taxpayer Compliance Measurement Program (TCMP) of the Internal Revenue Service in the United States. He concluded that non-compliance is strongly related to the marginal tax rate. Dubin et al (1990) investigated the impact of audit rates and tax rates on tax compliance with state-level time series from 1977 to 1985. The authors observed that the continual decline in the audit rate over this period led to a significant decrease in IRS collections. Many experimental economic studies have been done on fraud and tax evasion concerning audit rates, penalties and tax compliance with earned versus endowed incomes (see Friedland et al., 1978; Webley et al 1991, Alm et al 1992, Boylan and Sprinkle 2001, Gërxhani and Schram, 2006, Cadsby et al, 2006, Alm and McKee, 2008 and Alm et al, 2009).

Most studies on compliance have focused on personal income tax. Despite the importance of the sales tax in state and local government budgets, surprisingly little academic research has focused on the subject of sales tax compliance with some exceptions (Mikesell 1985, Murray 1995, Alm et al 2004). Both the magnitude and determinants of sales tax noncompliance remain elusive targets. Murray (1995)

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has shown that taxpayers with greater opportunities to reduce their tax liabilities exploit these opportunities to their advantage. He also concluded that there is no obvious nor easy-to-implement policy to combat sales tax noncompliance.

The purpose of this paper is to provide specific empirical insights using an experimental approach to evaluate the effects of systematic sales tax monitoring and the determinants of sales tax compliance. The experimental approach makes it possible to measure exactly the rate of tax compliance. We investigate to what extent taxpayers would alter their compliance behavior in response to a change in the audit environment. In particular, we study whether perfect monitoring through electronic payment of retail sales income may improve tax compliance and raise the level of tax revenues. Perfect monitoring of sales income is analogous to increasing the audit rate on matched income to 100%. It is technically possible to match individual declarations of income to relevant third party information. In the US, this type of matching is reserved for audits and taxpayer compliance studies.\(^2\) The Canadian federal government matches random individual tax returns with third party information on earnings as part of an ongoing monitoring system of tax compliance.\(^3\)\(^4\)

The primary objective in instituting direct and automatic capture of the tax portion of sales paid through electronic payments is to increase revenues by reducing tax evasion. Third party reporting on matched income severely limits an individual’s ability to evade taxes. Higher detection probabilities reduce the marginal benefit of evasion and therefore make evasion less attractive. For example, in the US, small businesses and farms, which have less matched income than large businesses, have significantly higher rates of evasion. In France, it is estimated that 25% of the fiscal fraud over the

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2 Internal Revenue Service (1996).
3 Canada Revenue Agency, source: http://www.cra-arc.gc.ca/tax/individuals/topics/income-tax/reviews/menu-e.html
4 At present, financial institutions credit electronic purchases (including the levied taxes) to the merchant within 24 hours following the purchase. The merchant acts as an agent for the government and returns the levied taxes according to the frequency of his remittance agreement (either annual, quarterly or monthly). It is now possible for sales tax to be directly and immediately captured from every electronic transaction. Just as the value of the purchase can be credited to the merchant’s account, the corresponding taxes can be credited simultaneously to the taxing authority’s account.
1988-1992 period comes from unreported value added taxes. In Canada, a large part of the household repair work, the renovation and construction are sectors particularly affected by under-reporting of sales income (Fortin et al 1996). Canada’s Revenue Agency justifies higher audit rates on sole proprietorships by the fact that wage and salary earners present relatively few compliance problems. Salaried employees’ taxes are collected through payroll deductions and their contributions are readily verified by reference to information filed by their employers.

However, several studies have shown that an increase in audit probability does not necessarily lead to improved tax compliance. Whether increased audits and penalties are the best way to deal with this non-compliance depends on the reasons why taxpayers fail to comply. If taxpayers are “playing the audit lottery”, increasing penalty and audit rates should improve compliance. But if their objective is to maintain a certain level of income, increased audits might not necessarily induce higher compliance. For example, Slemrod et al (2001) show that an increase in auditing does not necessarily mean an increase in voluntary compliance as a result. If perfect monitoring increases tax revenues paid by some taxpayers by reducing the room for cheating, one might, however, suspect that such positive effect may be counterbalanced by perverse effects from other taxpayers. Faced with a drastic increase of the probability of audit, how will the merchant react? First, knowing that all non-cash transactions will be automatically reported, merchants may tend to under-report more on the unmonitored income (e.g. by not reporting all transactions in cash) in order to maintain a net expected income comparable similar to what they had before the introduction of monitored income. Second, the merchants can entertain discounted cash offers for goods and services rather than accept payment in a more traceable form of

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5 According to a report from “Inspection générale des finances”, 1997.
6 The Tax Audit, Circular No. 71-14R3, Canada Revenue Agency.
7 To test the impact on behavior of awareness of the likelihood of an audit, the Minnesota Department of Revenue carried out a controlled field study described in Slemrod et al (2001). A stratified sample was selected based on three income levels and split into a treatment and control group. The treatment group was informed by mail that their tax returns would be “closely examined.” The comparison between the treatment and control groups showed that the threat of examination increases reporting compliance among low and middle-income taxpayers, but had the opposite effect among high-income taxpayers.
currency. In this sense increasing the audit rate may contribute to the underground economy.\footnote{Several studies have shown that taxes are undeniably an important factor in the underground economy. There also appears to be a rare degree of unanimity on the empirical proposition that the underground economy has grown substantially as a percentage of GDP since early 1991. One key piece of evidence for this is the large increase in cash in circulation relative to reported incomes (Lippert and Walker 1997).} In addition to examining how dramatically increasing the audit rate affects compliance, our study also illustrates the negative effect of an announcing such a change in tax system. Indeed we anticipate that individuals may try to offset the eventual consequences of the introduction of a more binding tax system by taking advantage of their current, liberal environment.

Several treatments were conducted in our experiment in order to isolate these different effects. The treatments are organized in two sequences: “baseline-announcement-perfect monitoring without transfers” and “baseline-announcement-perfect monitoring with transfers”. In the baseline treatment, subjects receive income from two sources. To the participants these were represented as Source A (i.e. resulting from electronic transactions) and Source B (resulting from cash transactions). Subjects receive income each period and are asked to voluntarily report their income. Participants pay tax on the reported income. They are subject to an audit with some probability. The second treatment, referred to as the announcement treatment, is identical to the previous one except that after a specified period of play, an announcement is posted that a change in policy will take effect in the next treatment. Subjects are told that a change in policy will institute perfect monitoring of Source A income. The amount of Source B income will remain private. Participants are informed of the change in policy before the policy is instituted to ascertain if behavior changes significantly due to the expectation of imminent monitoring. In a third treatment, the perfect monitoring treatment without transfer, perfect monitoring of Source A is instituted. Finally, the last treatment, namely perfect monitoring with transfers, continues the perfect monitoring of Source A income but allows income earners to pay a premium to move income from Source A to Source B.
Our work is related to several previous laboratory experiments that aimed at testing compliance decisions. Alm et al (1992) used laboratory experiments to estimate individual responses to audit probability, taxes and penalties. They showed that reporting rates increase with audit and penalties rates but decline with taxes. Alm and McKee (2008) also investigated how individual tax reporting decisions may be affected by audit and also by announcements of audit. The authors found that announcements of audit increase compliance rates of those who are told that they will be audited while the compliance rate declines for those who are told that they would not be audited. In contrast to this study, we focus here on the negative impact induced by announcement of a policy change when such policy is not immediately implemented. Most experimental studies have assigned a single category of income with the exceptions of Gërxhani and Schram (2006) and Alm et al (2009). In Gërxhani and Schram’s paper, the participants choose between an income that is automatically audited, and an unregistered income that is affected by different probabilities of being audited. They found that subjects choose unregistered income more frequently. In our paper the two sources of income are assigned exogenously, except for one treatment. In that treatment, a participant could transfer the monitored source of income to the unmonitored category (but at a cost), which is not the case in Gërxhani and Schram’s paper. The paper by Alm et al is more similar to ours with regard to the focus on design of tax collection institutions. Alm et al use an experimental protocol to determine the impact of audit and tax rates on subjects whose incomes are not reported by their employers. Using a real task between subjects experiment, their results suggest that evasion is in part contingent on the source of income: the amount of evasion being higher for individuals with non-matched income.

Our study innovates on previous experimental work in several important ways. First, we focus on sales taxes rather than taxes on earned incomes. Sale taxes offer an interesting insight on tax evasion of

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9Gërxhani and Schram (2006) set the problem of tax evasion in the context of contributing to a public good, which is not considered in our experiment. They do not address directly the differential in tax compliance when the monitoring of income increases within subjects (sequentially).
sources of income that are difficult to trace, such as cash payments. Merchants generally receive sales income from two sources: conventional electronic payments and those paid using cash. While electronic payments will generally draw sales taxes, cash payments may go unreported and escape taxation.\textsuperscript{11} Second, we investigate to what extent individuals reduce the impact of monitoring by allowing subjects to transfer income from electronic to cash transactions. Such transfers are generally costly. Merchants are expected to accept lower cash amounts for unrecorded sales (Gordon, 1990). Third, our study seeks to examine the negative effects of announcing a change in auditing policy. In particular, assuming that a policy cannot be implemented immediately, we investigate to what extent such announcement may provoke individuals to report less income in the current period in order to counteract the future effects of this policy.

We find that taxpayer noncompliance is related to opportunities for cheating. Subjects report less of Source B income when monitored more perfectly and when presented with the opportunity to get rid of their observed income. Subjects more than compensate for the automatic taxes on Source A income by paying less overall on Source B income.

In section 2, the research objectives and the institutional setting are discussed. In section 3, we describe the experimental design and experimental protocol. Section 4 presents theoretical predictions and behavioral conjectures about the expected treatment effects. In section 5, the experimental results are presented and discussed. A last section concludes and presents our policy recommendations.

\textsuperscript{10} We thank a referee for this reference.

\textsuperscript{11} To some extent such tax evasion is close to those reported in self-employment activity. Self-employment activity that is well known to exhibit lower rates of compliance than taxpayers whose primary source of income are wages or salaries. A number of studies investigated differences in the reporting rates between salaried workers and self-employed (Clotfelter, 1983; Feinstein, 1991; Joulefaian and Rider, 1998; Bruce 2000).
2. Research Objectives and Institutional Setting

Using the Canadian context as reference, the objective of the research is to examine what factors influence merchant tax compliance, here acting as an agent for the government, with respect to the federal and provincial tax (GST – PST) collection. The basic issue is to establish whether the perfect monitoring of matched sales income can increase tax revenues. There are two ways an agent can obstruct this type of policy’s success. The agents can conceal, or under-report on the unobserved sales revenue (i.e. cash). The agents can shift sales with traceable forms of payment to cash.

It is likely that many merchants hide part of their sales and possible do not even pass on the levied taxes to the government. For such an individual, the perfect monitoring of electronic sales changes the environment in two important ways.

1. The merchant cannot avoid taxes levied on sales using an electronic payment method. There are costs and benefits for the merchant to generating automatic sales tax payment at point of sale. Automatic taxation saves the vender the cost of keeping up with and remitting the required taxes at a later date. On the downside, it disallows the use of those funds until tax time and any opportunity to evade the sales taxes from electronic sales.

Compulsory payment of taxes for all electronic transactions requires financial institutions to separate out the tax collected from electronic purchases and send it directly to the relevant agency rather than allow the merchant to keep the revenue until the required tax payment. Any policy of this type has two aims: first, at directly levying the consumers’ GST and PST on all transactions, and second, at persuading the consumers to pay their purchases by electronic payment methods as opposed to cash payments. When an electronic payment is done by credit card or using bank debit card, the GST and the PST levied on the purchases will be drawn automatically and almost instantly. At present, financial institutions associated with the employed credit or debit card return the entire amount of the transaction
(payment and the levied taxes) to the merchant within 24 hours following the purchase. The merchant acts as agent for the government and returns the levied taxes according to the frequency of his remittance (either annual, quarterly or monthly). The agent can therefore benefit from these tax sums until the actual tax payment date. It is worth noting that following the establishment of immediate GST and PST collection on all electronic transactions, the current tax collection delay will continue to apply for cash payment purchases.

2. Without perfect monitoring of sales matched income, the merchant could have hidden most sales if he wished to do so. But, once the taxes are automatically transferred to the government agency, it automatically indicates the merchant’s sales.

Faced with these new rules, how will the fraudulent merchant react? The only way he has left to defraud is with the cash payments. Will he conceal the part of his sales paid cash, and in addition, find a way to transfer sales from electronic transactions to cash? More specifically, in order to keep his income at pre-regime change, will he choose to defraud even more on the one part of his income on which he can still keep private, now that the room for cheating is limited?

According to contracts with financial institutions, a merchant cannot favor cash payment as opposed to electronic payment method to purchase goods and services, but clients are most likely unaware of this clause. Furthermore, it is not clear whether the clients would resist discounts offered by recalcitrant agents on purchases paid cash. Since one of the objectives of the perfect monitoring of sales income is to bring the buyers to use electronic payments as much as possible instead of cash payments, the buyers’ behavior is important to meet this objective. In the present study, we ignore, however, this buyers’ aspect. We also do not examine the impact of timing of sales and taxes paid. All taxes in our experiment are paid on a period by period basis with the exception of payment of back taxes resulting from an audit of a fraudulent merchant.
The experiments’ participants are paid according to their decisions. They face a stylized incentive structure that is comparable, as much as is possible, to the one described in the perfect monitoring of matched income. As a result, we can analyze and understand the potential difference that exists between the theoretical predictions at equilibrium and the experimental results as much as the daily life results.

3. The Experimental Design and Protocol

Experimental economics allows us to reproduce, in a controlled environment, a system of revenue declaration and monitoring. The experimenter has the advantage of observing actual income from different sources as well as reported income.

The primary question posed by this experiment is “Does the institution of perfect monitoring on matched income affect tax compliance?” To focus on this question, other interesting but complicating factors have not been incorporated into the experimental design such as the redistribution of taxes in the form of a public good, using earned income rather than endowed income, and the application of imperfect audit rules. Because the payment of taxes can be seen as a repeated action by the taxpayer, participants are fed a stream of income in the experiment, randomly realized one period at a time, and are asked to make a reporting decision each period.

The experiment consists of 48 periods of income declaration. In each period, subjects receive income from two sources, voluntarily report the amount of their total income they desire, pay tax on the reported income, are subject to an audit with some probability and are able to examine their own income, income declaration, tax payment, audit history and penalty history. Participants are told that for each period, their assigned income is randomly (from a uniform distribution) drawn between 10 and 110 experimental units of currency (eu). Our study uses two categories of income simply designated to the participants as Source A and Source B.
To examine the influence of different proportions of matched income, each participant is assigned randomly one of three types at the beginning of the experiment. The endowment of Source A income is fixed at either 80%, 50%, or 20% of total income for types I, II and III. All three types are present in each treatment. Participants are told that the government does not know their true earned income nor whether their income from source A is initially from types I, II or III of total income. The tax rate \( t \) is 40% on reported income. Audits are always successful at exposing unreported income. Penalties, \( f \), for under-reporting are an additional 50% on unpaid taxes and an automatic audit for the previous two periods. The chances of being audited in any one period is 10% and bumps up to 20% if a participant reports income less than median reported income. Using these parameters and the maximal audit rate of 20%, we still cannot induce an expected income maximizing agent to report positive income. This may not be the case for individuals who are risk averse. Even though audit rates of 10% and 20% might seem high, they are not unlikely in some economic sectors. Using lower audit rates would increase the incentive to cheat. Moreover, the levied taxes are not returned to the participants in any way. This would add to the motivation to under-report as well.

To examine the effects on behavior of instituting perfect monitoring on a portion of income, the participant learns after several periods that Source A will be perfectly monitored by the government. Participants are informed of the change in policy before the policy is instituted to ascertain if behavior changes significantly due to the expectation of imminent monitoring.

The experiment includes several treatments. The baseline treatment consists of 21 periods. In each period, subjects receive income, report how much of their total income they desire, pay tax on the

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12 Participants are differentiated by their type of income from Source A. The differences in income level come from the draws at each round. To avoid too much variation and to better control the income conditions in the experiment, we have imposed the same level of income to each participant at each round. This last point was not known to the participants.

13 The set up of the experiment borrows from current practices of recovery agencies in Canada.

14 Two pilot sessions of 12 participants each were conducted to ascertain if declaration rates were stable for participants during 48 periods of receiving and reporting income. After 10 periods of play, there was no detectable change in the pattern
reported income, and are subject to an audit with some probability. Immediately following the baseline play, subjects play the announcement treatment during 6 periods. In this treatment, participants are exposed to announcement that the change to the new Source A income monitoring system will begin with period 28. The announcement of the new tax collection policy is simply implemented by posting an announcement at the beginning of period 22. Participants are told it will take six periods to implement the new policy. During the announcement phase, we attempt to observe whether facing the imminent implementation of the perfect monitoring of matched income induces some participants to change their behavior. This question is relevant since we know that perfect monitoring of matched income will not be come into effect without a realistic delay.

Lastly, participants play 21 periods under the new policy (perfect monitoring of Source A income). Once perfect monitoring of Source A income is implemented, participants are told that any income received from Source A and not reported would result in an automatic audit of total income. We insist on the fact that the government still will not know each participant’s true total income, nor which type of player he is. The two perfect monitoring treatments (perfect monitoring with or without transfers) differ by the level of fluidity between Source A income and Source B income for the participants. In the perfect monitoring treatment without transfer, participants are not allowed to transfer units of income from Source A to Source B. Keeping the income sources stringent in this simplified setting allows the observer to determine whether evasion behavior changes and if it changes in such a way to affect tax revenues. A variant of the Perfect monitoring treatment called Perfect monitoring treatment with transfers allows participants to transfer income from the perfectly monitored source to the unmonitored source at the rate of 6 units for 5 units. The goal of this treatment is to observe the “shift towards cash payments.” The other components of the protocol are the same as for the previous monitoring treatment.
Sixteen sessions of 12 participants were conducted for a total of 192 participants. Each session had four of each type of player (20%, 50% and 80% Source A income). Eight sessions were conducted with the perfect monitoring treatment without transfers and eight sessions were conducted with opportunity of transfers.

Prior to taking part in any experimental session, all participants are required to go through a series of computerized instructions that simulated each potential event during the experiment: a reporting period without audit, a reporting period with audit and no repercussions, and a reporting period with a successful audit and resulting penalty. The participants are informed that the experimenter does not know their total income, nor which type of player they are. In fact, the only information the experimenter has is declared income for the group, which is used to assign audits and the value of total income after an audit.

Actual payoff to each subject for participation is exactly his or her income from one period of play. The total income appears to participants as the sum of income from Source A and Source B, randomly drawn from a uniform distribution and ranged anywhere from 10 to 110 experimental units. At the conclusion of the experiment, one period is randomly drawn by the experimenter for payment and is compensated at the rate of $0.50 CAD/eu. Participants earned an average of $25 including a $10 show-up fee for less than 90 minutes of participation.

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16 was set for 21 periods, well beyond any anticipated adjustment by the participants.

15 Note that two participants over-report their income in more than 60% of the periods. They were drop from the initial database, which means that the data analysis is conducted with 190 players. Keeping those observations does not significantly affect the results.

16 Instructions to participants are presented in Appendix B.
4. Theoretical Considerations

We provide in this section a brief theoretical discussion of the individual compliance decisions and behavioral assumptions.

4.1 Theoretical predictions

Our experiment borrows from the seminal work of Allingham and Sandmo (1972) and Yitzhaki’s (1974) based on the expected utility model.

Let’s consider first the baseline treatment. $y$ denotes the subject’s gross income drawn between 10 and 110, which expected value is 60. This is common knowledge to all participants. The expected probability to be audited is

$$p = 0.5^*A_{Low} + 0.5^*A_{High}. \tag{1}$$

$A_{Low}$ is the audit rate for those reporting income below the median and $A_{High}$ is the audit rate for income reported above the median.

Let $t$ the tax rate and $f$ the penalty rate. For simplicity, consider the decision of a risk neutral participant who decides to report an amount $R$ of his gross income $y$. His expected net income, $ENI$, is:

$$(1 - p)(y - Rt) + p[y - Rt - (y - R)t - (y - R)tf]$$

$$\Leftrightarrow y - \left[ p \cdot t \cdot y - R + ft \cdot y - R + tR \right] = ENI \tag{2}$$

If an individual chooses to report his full income ($R = y$), then his net income, $NI$, is simply:

$$y(1 - t) = NI \tag{3}$$

When his choice is to report no income ($R = 0$), his expected net income is :

$$(1 - p)y + p \cdot 1 - t - ft \cdot y = 1 - pt \cdot 1 + f \cdot y \tag{4}$$
The expected rate of return from reporting no income \((R = 0\) in equation (2)) relatively to reporting all his income is:

\[
\frac{ENI \text{ with } R = 0 - NI}{NI}
\]  

(5)

With \(t = 40\%, \ f = 50\%, \ p = 0.15\) (with \(A_{low} = 10\%\) and \(A_{high} = 20\%\)) and \(y = 60\), then an individual who report his full income receives an expected payoff of 36 units but receives 54.6 units if he reports nothing at period 1, a return on evasion of 51.6\%. For the periods 3 to 48, the rate of return for cheating reduces to 21.67\% since if audited an automatic audits for the previous two periods occur\(^{17}\). This is a strong incentive not to report any income to the tax authority in the baseline treatment and full reporting is always a dominated strategy.

Based on this simple model, let’s consider now the theoretical predictions from the announcement treatment. Since parameters remain unchanged in the announcement treatments, theoretical predictions are exactly the same as in the baseline treatment: individuals should not report their incomes.

Turning next to the perfect monitoring treatment, we can easily see that the overall compliance rate be higher in this treatment compared to the baseline since individuals should still evade taxes from income of source B (since the expected rate of return is unchanged for income B) but should always report their income from source A as the probability of an audit in equal to 1 for under-reporting A (since \(p = 1\) in eq. (2)).

Finally let’s consider the theoretical predictions from the perfect monitoring treatment with opportunity of transfers. For the perfect monitoring treatment with opportunity of transfers, the overall compliance rate should be close to the baseline treatment. The reason is that it is always optimal for a participant to

\(^{17}\) For the first period, the expected rate of return per dollar of income not reported against the alternative of reported that dollar is 51.6\% as an audit in the first period does not implies a fine in the previous two periods (per $, the rate of return is: \((1 -0.15 \times 0.40) \times [1 + 0.50] - (1 - 0.40)) / (1 - 0.40)\) where 0.15 is the average probability of being audited , 0.40 the tax rate and 0.50 the fine). It reduces to 36.7\% in the second period (the term in square is multiply by 2 to account for the fine in the
buy the maximum possible of units of B with units of A. Indeed, purchasing additional units of B costs 20% more than the units of B that the participants already own (buying additional units of income B costs 1.20 units of income A). However, such transfers allow the player to avoid the automatic auditing. The trade off between avoidance of automatic auditing and paying an extra cost to buy additional units of B can be easily expressed in terms of expected rate of return. Precisely, the expected rate of return of the purchased units of B is the same as the expected rate of return for initial units of B minus the cost of purchasing extra units. The expected rate of return of the purchased units of B is therefore 1.67% (i.e. 21.67% minus 20%). Thus it remains optimal to buy extra units B and not reporting them. The expected rate of return for tax evasion for units of B that the participants already own is unchanged at 21.67%.\textsuperscript{18}

The predictions discussed above are from a very simple version of the Allingham- Sandmo – Yitzhaki models. Assuming risk averse participants, introducing the social and moral consideration of paying taxes and the stigma associated with being caught will significantly modify those predictions. Recently authors have referred to non expected utility models to explain why people pay or decide not to pay taxes (see for example, Dhami and al-Nowaihi, 2007, referring to prospect theory). The next section will consider this approach in the context of behavioral predictions.\textsuperscript{19}

4.2 Behavioral predictions

A possible objection to the expectation of a positive relationship between compliance and perfect monitoring without transfers is that individuals may counteract the consequences of a policy change to perfect monitoring, by taking on more risks by not declaring their imperfectly monitored income. Such

\textsuperscript{18} Note that because an individual cannot pay twice for non reporting her income if audited, therefore when one is hit by an audit, the expected rate of return of cheating is the maximum of 51.6%, reaching 31.6% in the perfect monitoring treatment with transfers for the purchased units of B.

\textsuperscript{19} It is outside the scope of this paper to formalize those models.
an effect is akin to the idea of reference point in prospect theory.\textsuperscript{20} According to loss aversion in prospect theory and the reference-dependent effect, individuals will try to recover their losses following any policy changes, which may mean taking more risks. This is a familiar finding among gamblers and traders who set specific financial targets.\textsuperscript{21} Our conjecture is stated more precisely in H1.

\textbf{H1}: The increase in tax revenue due to the implementation of perfect monitoring on income of one class (Source A) may be offset by reporting less income from another class (Source B).

Our second conjecture is that, for similar reasons as presented above, some participants may report less income in the announcement treatment than in the baseline treatment in order to get a head start on offsetting the future tax burden of the perfect monitoring policy. This is summarized in H2.

\textbf{H2}: Participants may be willing to counteract the future consequences of the perfect monitoring policy by reporting less income in the current announcement treatment than in the baseline.

Audit experience may also influence perceptions of future audits, which is theoretically incorrect since instructions of the experiment made it clear that audits were randomly assigned each period, not conditionally on past behavior. A possible reason for this misperception is that a participant’s attention may be occupied with the bad outcome itself and neglect the fact that such event is very unlikely to occur. This effect is generally termed the “probability neglect” effect. The idea is that, “when intense emotions are engaged, people tend to focus on the adverse outcome, not on its likelihood, which may lead to significant distortions in both private and public arenas” (Sunstein, 2002 p.61). Moreover

\textsuperscript{20} The reference point consists of the individual's point of comparison against which each alternative is compared. In our case, this can be the initial outcome obtained in the baseline treatment. According to the prospect theory, individuals will 'code' each alternative as a gain or a loss in utility relative to this reference point rather than using the absolute value of the outcome. The resulting losses or gains are then weighed by their perceived probabilities of occurrence, forming a non-linear value function. Because people typically approach gains and losses differently, generally acting risk-averse on gains and risk-seeking on losses (Kahneman and Tversky, 1979), the result is a non-linear utility function.

\textsuperscript{21} A similar situation is with the concept of a target income and the behavioral model of labor supply developed by Altman (2001). See also Camerer et al (1997) on income target related to the supply of taxis.
people may feel a disproportionate fear of risk when the associated risks are hard to control (Slovic, 2000; Sustein, 2002). Finally, the occurrence of an audit may also induce the opposite effect, by provoking individuals to underestimate future audits. This effect, generally termed the “Gambler’s fallacy” or “Monte Carlo fallacy”, relies on the impression that a certain stochastic event is less likely to happen following the occurrence of an event or a series of events. Again, this is obviously a fallacy since the occurrence of past events do not impact the probability that similar random event will occur in the future. According to Tversky and Kahneman, Gambler's fallacy may be induced by a psychological heuristic. This is stated in H3.

H3: Audit experience may influence perceptions of risk and therefore influence current decisions.

In addition to risk perception, other behavioral factors may also influence tax compliance. For example, some individuals may desire to conform to a norm of truth-telling. So called honest individuals would avoid the emotional cost associated with deviation from this norm by reporting a non null part or all of their income (Coricelli et al, 2008). This is summarized in assumption H4.

H4: Irrespective of pecuniary incentives, non-pecuniary factors such as norms of truth-telling may motivate subject to report non null amounts of income in both the baseline and the announcement treatments.

5. Experimental Results

5.1 Descriptive statistics

22 The intuition behind Gambler fallacy is that people may expect that deviations from average should balance out. For example, if a fair coin is tossed repeatedly and tails comes up a larger number of times than is expected, a gambler may incorrectly believe that this means that heads is more likely in future tosses. Such an event is often referred to as being "due.
Figures 1a and 1b show the time path of the average reporting rates by period, in the different treatments for sessions 1-8 (without opportunity of transfer) and sessions 9-16 (with transfers), respectively. The average reporting rate is shown on the vertical axis. The period number is given on the horizontal axis. To facilitate comparisons across treatments, average reporting rates are depicted graphically on the same time scale.

Average reporting rates are significantly different from zero in contradiction with the simple version of the A-S-H models. Both figures indicate that in most of treatments, reporting rates are rather stable over time. Comparing treatments, both figures indicate important differences. Figure 1a shows that the announcement phase induces a significant reduction of overall reporting rate. Introducing monitoring policy without opportunity of transfers does not lead to improve reporting rates. In contrast, Figure 1b shows that introducing perfect monitoring with opportunity of transfers induces a negative effect on the reporting rate level. Figure 1b indicates that the negative effect of announcement seems to be lower in sessions 9-16 than in sessions 1-8. These results seem to confirm the idea that introducing a perfect monitoring policy does not necessarily lead to an improvement of tax compliance because agents may be willing to counteract the effects of such policy by reporting less income from other sources, which translates into less overall reporting rate. In general, those results support our behavioral predictions.

Table 1 presents descriptive statistics on the basic variables related to our experimental parameters and behaviors exhibited by the participants (definition and construction of all the variables are presented in appendix A). It shows that the reporting rate significantly decreases with perfect monitoring and opportunity of transfers. The reporting rate is 76.59% in the baseline treatment.\(^{23}\) This percentage falls to 70.30% in the monitoring treatment with transfers and is significantly lower than the baseline result \((z = 4.452; p = 0.0001, \text{Wilcoxon signed-rank tests})\). In contrast, we find no significant difference

\(^{23}\) Note that a Mann-Whitney-Wilcoxon test indicates no significant difference between the two baseline treatments in sessions with and without transfers \((z = 1.131; p > 1)\).
between the baseline treatment and the monitoring treatment without transfers ($z = -0.490; p > 0.1$). We also observe that the reporting rate is significantly lower in the announcement stage preceding monitoring without transfers (67.67%) compared to the baseline treatment ($z = 3.456; p = 0.0005$, Wilcoxon signed-rank). We speculate that the difference in behavior between the two treatments is that the participants are trying to counteract the expected consequences of the perfect monitoring policy by taking advantage of their current situation. In contrast no significant difference is found between the baseline treatment and the announcement treatment with transfers ($z = 0.941; p > 0.1$). A possible explanation is that participants anticipate that they can avoid future taxes under the implementation of perfect monitoring when transfers are permitted. Consequently, they are not motivated to “stock up” before the new policy is implemented. The non parametric tests confirm the previous findings.

[Table 1, Figures 1a and 1b about here]

In addition to the differences in the overall reporting rate across the treatments discussed above, we also observe that the zero compliant participants, although small in number, are more numerous in the monitoring treatment with transfers (1.55%), than the monitoring treatment without transfers (0.05%). Figures 2a and 2b offer information about the effects of announcement and monitoring with respect to level of income for sessions 1-8 (perfect monitoring without transfers) and sessions 9-16 (perfect monitoring with transfers), respectively.

[Figures 2a and 2b about here]

As illustrated in Figures 2a and 2b, reporting in the baseline is greater than 70% for every income range. Given the substantial incentives to under-report, positive and relatively high reporting rates indicate that the participants are either generally quite risk averse or that other suggested models of tax compliance are at play here. As the stakes increase, we also observe that, in all treatments, reporting rate decreases with the level of income.
Figure 2a shows that the announcement induces a significant reduction of overall reporting rate. In contrast, perfect monitoring without transfer seems to have two opposing effects depending on the level of income. An increase in monitoring seems to have a negative effect on reporting rate for lower incomes whereas it has a positive effect on this ratio for higher incomes. With Figure 2b, we observe that perfect monitoring with transfers induces a reduction in the reporting rate level. Consistent with our previous results, the negative effect of announcement is lower in this case compared to figure 2a, which indicates that subjects are aware that the introduction of perfect monitoring is less dramatic when they have the opportunity to transfer income to the unmonitored source. Finally, the negative effect of perfect monitoring (significant at 5%), compared to the baseline treatment, indicates that increasing monitoring does not necessarily improve the reporting rates when individuals have the opportunity to transfer income to an unmonitored source.

Table 2 provides, by participant’s income type and policy treatment, descriptive statistics on income, reported income, taxes paid, percentage of auditing, etc. Table 2 shows that the participants who had 80% of their income generated from Source A were obliged to report more income than they would have if not monitored so closely.

[Table 2 and Figure 3 about here]

Figures 3a and 3b provide information concerning reporting rates for the three types of players across all three experimental phases with and without transfers, respectively. Figure 3a shows that without transfers, highly monitored participants who had 80% of their income from Source A tend to increase their reporting rate on average in the monitoring treatment and in the announcement treatment compared to the baseline treatment. In sharp contrast for the other two types of participants (e.g. with 50% and 20% of income from source A), the reporting rates decline through the announcement stage
and move back upward throughout the perfect monitoring treatment.\textsuperscript{24} When future transfers are possible (Figure 3b), highly monitored participants tend to stabilize their reporting rates while the other two types of participants significantly reduce them in the announcement and the monitoring treatments.

Figures 4, 5 and 6 compare the two monitoring treatments with and without an opportunity of transfers for each participant’s income type. For those with 80\% and 50\% of income from Source A (Figures 4 and 5), the declaration rates are generally lower for higher income levels and especially when participants had the opportunity to transfer income from Source A to Source B. For the least monitored participants, those with only 20\% of total income derived from Source A, the difference between the two treatments is insignificant. The participants’ inclination to avoid being in the reported income’s lower bracket and by the same token to face a 20\% auditing rate, explains this result. Actually, since buying extra units of B is costly, it would be useless to do so if one had to report it. The participant knows that, having 80\% of his or her total income already not systematically audited, part of this income will have to be reported to try to avoid the higher auditing rate.

[Figures 4, 5 and 6 about here]

\textit{5.2 Econometric results}

The descriptive statistics imply that the perfect monitoring of matched income does not bring in greater tax revenues for the government.\textsuperscript{25} In fact, tax revenues will decrease with perfect monitoring with respect to the parameters of the experiment. Two primary factors can explain this result.

First, as the descriptive statistics for the monitoring treatment without opportunity of transfer show, participants tend to cheat more on unmonitored income when they cannot under-report monitored income. The only exception in this study is the subgroup of participants whose income from Source A

\textsuperscript{24} Alm and McKee (2006) found that announcement increases compliance of those told they will be audited, but reduces compliance of those knowing they will not be audited; the net effect is that overall compliance falls.
represents 80% of their total income. Second, there is a shift towards unmonitored income (Source B) when this option is available for the highly monitored participants (80% and 50% from Source A). A reasonable explanation for these observations is that participants are willing to assume more risks when they are closely monitored in order to maintain a net expected income comparable to what they had before the introduction of monitored income.

In this section, the data are analysed with parametric regressions. Table 3 consists of two panels. The left panel displays the result of five regressions in which the dependent variable is the percentage of total income reported to the tax authority by individual \( i \) at period \( t \). It shows the estimates of the determinants of reporting rates by treatment with random effects Tobits.\(^{26}\) The use of Tobit models is justified by the number of left and right censored observations in the sample. The right panel (column 6) presents the results of the determinants of tax revenue from a random effects Feasible-Generalized-Least-Squares regression.\(^{27}\) We control for demographics (not reported in the estimates but available upon request).\(^{28}\)

The estimates support earlier observations that tax reporting declines with total income level. We also observe that the participants reduce their reported income in the period following an audited period.

\(^{25}\) Tax revenues are the tax paid on the reported income. They do not include penalties.

\(^{26}\) In a panel Tobit, the error component splits into a time invariant individual random effect, \( \alpha_i \) and a time-varying idiosyncratic random error \( \varepsilon_{it} \).

\(^{27}\) Given \( RF_{it} \) measuring the taxes paid (tax revenues) by participant \( i \) at period \( t \). Tax revenues are described by a vector of exogenous variables \( z_{it} \) and the corresponding vector with parameters \( \delta \) and a random variable that can be divided into a random individual effect \( \alpha_i \), and a pure random variable \( \varepsilon_{it} \): \( RF_{it} = z_{it}\delta + \alpha_i + \varepsilon_{it}, i = 1, \ldots, n, \ t = 1, \ldots, T \). Assuming uncorrelated errors terms and that the \( z_{it} \) are also uncorrelated with the errors terms, an appropriate estimation technique is the Feasible-Generelarized Least-Squares discussed in Greene (2008, chapter 9).

\(^{28}\) Demographics include various participant characteristics: age (mean: 25.4; sd: 5.95); male (53.1%) previous participation is a binary variable for whether the participant has already taken part in an experiment other than the current one (1= participated in a previous experiment (38.5%), 0 otherwise.); gamble indicates whether the participant chose to earn a guaranteed $5 show-up fee or a gamble with a 50% probability of earning $11 and a 50% probability of earning $0. Participants who choose the gamble are considered relatively less risk averse than those who chose the $5 show-up fee (73.4%); instruction feedback describes the participant’s assessment of the clarity of the instructions on a scale of 0 to 10, 10 being “very clear” (mean: 8.29; sd:1.46); and a list of binary variables indicating whether the participant is a worker (8.85%), unemployed (6.25%), a student (80.2%), a graduate student (22.4%) or a student with prior mathematical training...
This reaction to previous audit is in accordance with our discussion on optimal strategy. This result is also consistent with the reference-dependence effect in prospect theory. Participants may be willing to compensate for the losses suffered in the audit, even at the expense of taking more risks (Tversky and Kahneman 1991). This occurrence could also be explained by the “Gambler’s fallacy” concept, which states that the participants believe that after being audited once, the probability to be audited again soon after is smaller than the described probability. This is of course incorrect, the auditing probabilities being independent from a period to the next. Columns (1) and (2) indicate that there is no significant difference among player types in the baseline and announcement treatments. In contrast, as shown by column (3), the perfect monitoring without transfers treatment reveals that players of types II and III report significantly less than players of type I, which is consistent with our previous findings. The lower constant term suggests that on average, people report less income in the perfect monitoring treatment with transfers.

With respect to the reference variable that is the baseline treatment preceding the announcement and policy implementation treatments, column (5) reveals that the tax rate reporting decreases for all types of players under perfect monitoring when transfers from Source A to Source B income are permitted. If the participants are not able to shift income from Source A to Source B, the tax reporting increases for those participants with 80% of their initial income coming from Source A under perfect monitoring when compared to the baseline treatment. The tax rate reporting is stable for participants with 50% of their income coming from Source A and decreases for those with 20% of their income coming from Source A.

(66.1%). The introduction of demographic variables does not affect the estimated coefficients of the experimental variables. Most of these variables are not statistically significant.

29 Here behavioral economics is an explanation of the results and not the basis for a policy recommendation. McCaffery (2006) has discussed in the context of policies aimed at increasing the saving rate in the US, the marriage of behavioural economics and fundamental tax reform.
In the announcement treatment preceding the perfect monitoring without the transfers treatment, the tax rate reporting decreases with respect to the baseline treatment. In the announcement treatment preceding the introduction of the perfect monitoring with transfers, there is no diminution in tax reporting as already noted earlier.

[Table 3 about here]

Turning next to the determinants of tax revenues, column (6) reports a positive and significant coefficient associated to the “total income” variable. We have seen earlier that the reporting rates decrease with the income level. Nonetheless, column (6) shows that tax revenues increase with the income since the income raise compensates for the diminution of the reported income. Consistent with our previous results, column (6) shows that the tax revenues decrease under perfect monitoring when transfers from Source A to Source B income are permitted when compared to the baseline. The tax revenues decrease by 0.907 eu, 3.134 eu and 2.5866 eu with respect to the baseline treatment for participants with respectively 80%, 50% and 20% of their initial income coming from Source A.\(^\text{30}\) If the participants are not allowed to shift income from Source A to Source B, the tax revenues increase by 3.498 eu for those participants with 80% of their initial income coming from Source A under perfect monitoring when compared to the baseline treatment. Tax revenues are stable for participants with 50% of their income coming from Source A and they decrease by 1.607 eu for those with 20% of their income coming from Source A.

6. Conclusion

\(^{30}\)To illustrate for the type II participant: with respect to the reference state, we add the coefficient -0.0145 of type II (both treatments)” to the coefficient – 3.1195 of “Type II and transfer”.

The motivation for this study was to examine the impact of perfect monitoring of matched income on tax compliance. To address this question, we used a laboratory experiment to observe actual and reported income behavior in a repeated-decision framework. Two treatments were used to observe the change in monitoring with and without the opportunity to transfer income from the perfectly monitored source to the unmonitored source.

Our experiment contributes to the previous existing literature on tax compliance by showing that increasing the probability of audit does not necessarily lead to a reduction of tax evasion because agents may seek to counteract the consequences of an increase of probability of audit by reporting less income from other sources that are less monitored (reference-dependent effect) and by transferring incomes toward these alternative sources of income when it is possible. This result is consistent with Gerxhani and Schram (2006) who found a significant shift from perfectly monitored (registered) income towards unregistered income when tax evasion is made possible. It is also in line with Alm et al (2009) suggesting that the amount of evasion is higher for individuals with non-matched income.

We have three key findings.

First, on average the less monitored income types (50% and 20% from Source A) and all income types when allowed to transfer income had higher rates of non-compliance. The only group that had the same rate of compliance was the group of participants who derived 80% of their income from Source A without the opportunity to transfer incomes.

Second, when the participants have the opportunity to get rid of automatically monitored income, they more than compensate for the rigid taxes on Source A income and report less overall on Source B income.

Third, we find a significant decrease in tax revenues with the opportunity of transfer. In this experiment we observe approximately a 15% decline in tax revenues with the realistic policy treatment with transfers of income. We are not claiming that a 15% tax reduction in tax revenues will be seen, but our
experiment suggests that, if perfect monitoring is instituted without some other complementary policies, an increase in tax revenues is not the likely outcome.\textsuperscript{31}

Our study stresses that a successful policy aimed at reducing fiscal fraud might be a difficult task once people have decided their equilibrium level of tax compliance. The reference-dependent effect and loss aversion in prospect theory suggest that individuals will try to recover their losses following any policy changes even if it means taking more risks. Maybe the only solution is to assess what taxpayers consider a “fair” level of taxation, which balances their relative level of tax contributions with what they expect to gain from them.

\textsuperscript{31} This echoes McCaffery’s (2006) call for fundamental tax reform based on rigorous thinking and theory.
References


### Table 1: Descriptive statistics by treatment*

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>Monitoring treatment without opportunity of transfer</th>
<th>Monitoring treatment with opportunity of transfer</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Type I 80% Source A</td>
<td>Type II 50% Source A</td>
</tr>
<tr>
<td></td>
<td>Average  Std Dev.</td>
<td>Average  Std Dev.</td>
</tr>
<tr>
<td>Total income</td>
<td>58.02  27.18</td>
<td>58.02  27.18</td>
</tr>
<tr>
<td>Reported income</td>
<td>52.11  24.51</td>
<td>43.70  24.00</td>
</tr>
<tr>
<td>Overall reporting rate (%)</td>
<td>90.38% 9.72%</td>
<td>76.44% 22.48%</td>
</tr>
<tr>
<td>Reporting rate on B * (%)</td>
<td>54.23% 44.76%</td>
<td>53.49% 44.22%</td>
</tr>
<tr>
<td>Amount of B purchased</td>
<td>7.74   14.32</td>
<td>7.93   11.25</td>
</tr>
<tr>
<td>Amount B purchased / Amount B available</td>
<td>14.29% 12.80%</td>
<td>15.05%</td>
</tr>
<tr>
<td>% reporting no income</td>
<td>42.41% 41.82%</td>
<td>35.33%</td>
</tr>
<tr>
<td>% reporting no income on B</td>
<td>29.76% 27.53%</td>
<td>17.67%</td>
</tr>
<tr>
<td>% Auditing</td>
<td>14.29%</td>
<td>15.05%</td>
</tr>
</tbody>
</table>

* (reported income – income from Source A) / (income from Source B)

### Table 2: Descriptive statistics by income type and monitoring treatment

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>Baseline treatment (both sessions 1-8 and 9-16)</th>
<th>Announcement treat. preceding monitoring without transfers (sessions 1-8)</th>
<th>Announcement treat. preceding monitoring with transfers (sessions 9-16)</th>
<th>Perfect Monitoring treat. without transfers (sessions 9-16)</th>
<th>Perfect Monitoring treat. with transfers (sessions 9-16)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Average  Std Dev.</td>
<td>Average  Std Dev.</td>
<td>Average  Std Dev.</td>
<td>Average  Std Dev.</td>
<td>Average  Std Dev.</td>
</tr>
<tr>
<td>Reported income</td>
<td>45.54  29.89</td>
<td>34.64  26.80</td>
<td>45.63  29.33</td>
<td>43.52  25.16</td>
<td>40.15  27.30</td>
</tr>
<tr>
<td>Overall reporting rate (%)</td>
<td>76.59% 32.64%</td>
<td>67.67% 36.58%</td>
<td>76.56% 30.72%</td>
<td>76.94% 26.38%</td>
<td>70.30% 33.54%</td>
</tr>
<tr>
<td>Reporting rate on B * (%)</td>
<td>63.66% 43.58%</td>
<td>52.05% 44.97%</td>
<td>59.30% 43.68%</td>
<td>54.22% 43.62%</td>
<td>54.91% 43.38%</td>
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<tr>
<td>Taxes paid on the reported income</td>
<td>17.95  11.84</td>
<td>13.69  10.75</td>
<td>17.99  11.76</td>
<td>17.18  10.02</td>
<td>15.88  10.99</td>
</tr>
<tr>
<td>Amount of B purchased</td>
<td>17.95  11.84</td>
<td>13.69  10.75</td>
<td>17.99  11.76</td>
<td>17.18  10.02</td>
<td>15.88  10.99</td>
</tr>
<tr>
<td>Amount B purchased / Amount B available</td>
<td>% reporting no income</td>
<td>% reporting no income</td>
<td>% reporting no income</td>
<td>% reporting no income</td>
<td>% reporting no income</td>
</tr>
<tr>
<td></td>
<td>5.51%  9.82%</td>
<td>4.91%  0.05%</td>
<td>4.91%  0.05%</td>
<td>4.91%  0.05%</td>
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<tr>
<td></td>
<td>47.54% 39.12%</td>
<td>42.81% 39.90%</td>
<td>42.81% 39.90%</td>
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<td>42.81% 39.90%</td>
</tr>
<tr>
<td></td>
<td>26.44% 31.75%</td>
<td>25.44% 25.06%</td>
<td>25.44% 25.06%</td>
<td>25.44% 25.06%</td>
<td>25.44% 25.06%</td>
</tr>
</tbody>
</table>

* (reported income – income from Source A) / (income from Source B)
Table 3: Determinants of reporting rates and tax revenue

<table>
<thead>
<tr>
<th>Treatment:</th>
<th>Baseline</th>
<th>Announcement Perfect monitoring without transfers</th>
<th>Announcement Perfect monitoring with transfers</th>
<th>All treatments</th>
<th>All treatments</th>
</tr>
</thead>
<tbody>
<tr>
<td>Dep. variable</td>
<td>Reporting rate</td>
<td>Reporting rate</td>
<td>Reporting rate</td>
<td>Tax revenue</td>
<td>Tax revenue</td>
</tr>
<tr>
<td>Models</td>
<td>RE Tobit$^a$</td>
<td>RE Tobit$^a$</td>
<td>RE Tobit$^a$</td>
<td>RE Tobit$^a$</td>
<td>RE Tobit$^a$</td>
</tr>
<tr>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
<td>(6)</td>
</tr>
<tr>
<td>Total income</td>
<td>-0.0057***</td>
<td>-0.0055***</td>
<td>-0.0033***</td>
<td>-0.0041***</td>
<td>-0.0046***</td>
</tr>
<tr>
<td>Previous period audit</td>
<td>-0.0640***</td>
<td>-0.0961**</td>
<td>-0.0766***</td>
<td>-0.0878***</td>
<td>-0.0797***</td>
</tr>
<tr>
<td>Type II</td>
<td>0.0222</td>
<td>-0.0918</td>
<td>-0.1367*</td>
<td>-0.1203</td>
<td>0.9877***</td>
</tr>
<tr>
<td>Type III</td>
<td>0.0747</td>
<td>-0.1045</td>
<td>-0.2703***</td>
<td>-0.0572</td>
<td></td>
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<tr>
<td>Dum. Sessions 7-16</td>
<td>0.1128</td>
<td>0.2171***</td>
<td>(0.0687)</td>
<td>(0.0724)</td>
<td></td>
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<tr>
<td>Baseline</td>
<td>Ref.</td>
<td>Ref.</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Announcement (both with and without transfers)</td>
<td>-0.1531***</td>
<td>-1.5388***</td>
<td></td>
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<td>Announcement and transfer</td>
<td>0.1050***</td>
<td>0.6518</td>
<td></td>
<td></td>
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<tr>
<td>Type I (monitoring both with and without transfers)</td>
<td>0.1699***</td>
<td>3.4980***</td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>Type I and monitoring with transfer</td>
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<td>-4.4050***</td>
<td></td>
<td></td>
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<td>Type II (monitoring both with and without transfers)</td>
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<td>-0.0145</td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>Type II and monitoring with transfer</td>
<td>-0.1724***</td>
<td>-3.1195***</td>
<td></td>
<td></td>
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<tr>
<td>Type III (monitoring both with and without transfers)</td>
<td>-0.0854***</td>
<td>-1.6076***</td>
<td></td>
<td></td>
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<td>Type III and monitoring with transfer</td>
<td>-0.1108***</td>
<td>-0.9790*</td>
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<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
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<tr>
<td>Constant</td>
<td>1.1701***</td>
<td>1.1823***</td>
<td>1.3912***</td>
<td>0.9283***</td>
<td>1.1009***</td>
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<tr>
<td>p</td>
<td>(0.1880)</td>
<td>(0.1976)</td>
<td>(0.2353)</td>
<td>(0.2201)</td>
<td>(0.1518)</td>
</tr>
<tr>
<td>$\rho$</td>
<td>0.303**</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$\sigma_\alpha$</td>
<td>0.4114***</td>
<td>0.4177***</td>
<td>0.2445***</td>
<td>3715***</td>
<td>0.3627***</td>
</tr>
<tr>
<td>$\sigma_\varepsilon$</td>
<td>0.4446***</td>
<td>0.3439***</td>
<td>0.2292***</td>
<td>0.2931***</td>
<td>0.3693***</td>
</tr>
<tr>
<td>Observations</td>
<td>3800</td>
<td>1140</td>
<td>1995</td>
<td>1995</td>
<td>8930</td>
</tr>
<tr>
<td>Log likelihood</td>
<td>-2413.3947</td>
<td>-689.1120</td>
<td>-553.2483</td>
<td>-811.4883</td>
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<tr>
<td>Left censored obs.</td>
<td>214</td>
<td>84</td>
<td>1</td>
<td>31</td>
<td>330</td>
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<tr>
<td>Right censored obs.</td>
<td>1886</td>
<td>481</td>
<td>814</td>
<td>778</td>
<td>3959</td>
</tr>
</tbody>
</table>

Notes: $^a$RE Tobit=Random Effects Tobit; $^b$RE FGLS : Random Effects Feasible-Generalized-Least-Squares. Standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. The first period of observation was eliminated due to the construction of the lagged audit variable. The correlation coefficients $\rho$ is statistically significantly different from $\varrho$ according to the Lagrange multiplier test with a 5% critical threshold.
Figure 1a: Overall reporting rate by treatment over time (sessions 1-8)

Figure 1b: Overall reporting rate by treatment over time (sessions 9-16)
Figure 2a: Ratio of reported income per level of income (session 1-8)

Figure 2b: Ratio of reported income per level of income (session 9-16)
Figure 3a: Overall reporting rate by treatment and type of player (sessions 1-8)

Figure 3b: Overall reporting rate by treatment and type of player (sessions 9-16)
Figure 4: Ratio of the reported income for those having 80% of their income automatically audited (Source A)

Figure 5: Ratio of the reported income for those having 50% of their income automatically audited (Source A)

Figure 6: Ratio of the reported income for those having 20% of their income automatically audited (Source A)
## Appendix A: Definitions of Variables

<table>
<thead>
<tr>
<th>Name</th>
<th>Definition</th>
</tr>
</thead>
<tbody>
<tr>
<td>Basic (Baseline)</td>
<td>1 if Baseline treatment is played and 0 otherwise</td>
</tr>
<tr>
<td>Announcement</td>
<td>1 if Announcement treatment is played and 0 otherwise</td>
</tr>
<tr>
<td>Announcement with transfers</td>
<td>1 if Announcement treatment preceding monitoring with transfers is played and 0 otherwise.</td>
</tr>
<tr>
<td>Announcement without transfers</td>
<td>1 if Announcement treatment preceding monitoring without transfers is played and 0 otherwise.</td>
</tr>
<tr>
<td>Monitoring with transfers</td>
<td>1 if Monitoring with transfer treatment is played and 0 otherwise</td>
</tr>
<tr>
<td>Monitoring without Transfer</td>
<td>1 if Monitoring with transfer treatment is played and 0 otherwise</td>
</tr>
<tr>
<td>Perfect monitoring</td>
<td>1 if Monitoring is possible (i.e in monitoring treatments with and without transfers) and 0 otherwise.</td>
</tr>
<tr>
<td>Type I</td>
<td>Player’s source A income is 80% of her total income</td>
</tr>
<tr>
<td>Type II</td>
<td>Player’s source A income is 50% of her total income</td>
</tr>
<tr>
<td>Type III</td>
<td>Player’s source A income is 20% of her total income</td>
</tr>
<tr>
<td>Type I and Monitoring</td>
<td>Interaction variables</td>
</tr>
<tr>
<td>Total income</td>
<td>Player’s income randomly drawn between 10 and 110 experimental units.</td>
</tr>
<tr>
<td>Reported Income</td>
<td>Reported income by each player</td>
</tr>
<tr>
<td>Overall reporting rate</td>
<td>Reported income/total income</td>
</tr>
<tr>
<td>Audit</td>
<td>1 if the participant is audited and 0 otherwise</td>
</tr>
<tr>
<td>Age</td>
<td>Player’s age</td>
</tr>
<tr>
<td>Men</td>
<td>1 if participant is a male and 0 otherwise</td>
</tr>
<tr>
<td>Previous participation</td>
<td>1 if the player has already taken part in an experiment other than the current one and 0 otherwise.</td>
</tr>
<tr>
<td>Gamble</td>
<td>1 if the participant chose a gamble (50% probability of earning $11 and a 50% probability of earning $0) and 0 if she chose to earn a guaranteed $5 show up fee.</td>
</tr>
<tr>
<td>Instruction Feedback</td>
<td>Describes the participant’s assessment of the clarity of the instructions (from 0 to 10)</td>
</tr>
<tr>
<td>Worker</td>
<td>1 if the participant is a worker and 0 otherwise</td>
</tr>
<tr>
<td>Unemployed</td>
<td>1 if the participant is unemployed and 0 otherwise</td>
</tr>
<tr>
<td>Student</td>
<td>1 if the participant is a student and 0 otherwise</td>
</tr>
<tr>
<td>Graduate student</td>
<td>1 if the participant is a graduate student and 0 otherwise</td>
</tr>
<tr>
<td>Mathematical training</td>
<td>1 if the student with prior mathematical training and 0 otherwise</td>
</tr>
</tbody>
</table>
Appendix B: Participant instructions (translated from French)

General Instructions
You are asked to participate in a series of decisions. This experiment is made of several periods. You will be asked to make a decision in each period. You must complete one period before moving on to the next period. Your earnings depend on what you decide. Therefore, it is important to read these instructions carefully. At the end of the experimental session, you will make a random draw from all the played periods. Your earnings for the session will be a function of your “net final income” from that period. What you earn is completely private. You will be paid for one randomly selected decision (period) in private at the end of the experiment.

Period Breakdown.
Every period, you will receive your income. This income is randomly drawn by the computer and is between 10 eu (experimental units) and 110 eu.

Two sources of income
Your income comes from two different sources: A and B.
There are three participant types randomly determined by the computer. They are characterized by whether most of their income comes from source A, source B, or equally from both sources. That is

- Type 1: 80% of your total income comes from source A and 20%, from source B.
- Type 2: your total income comes equally from sources A and B.
- Type 3: 20% of your total income comes from source A and 80%, from source B.

For example, suppose that you are a type 3 player. If your total income is 50 eu, 10 eu would come from source A and 40 eu would come from source B. This information is confidential. You remain of the same type for the entire experiment.

Paying taxes
Only you know your true income. Once you know your true income, you will be asked to report your income to the government and pay taxes on the reported income. There is no restriction on how much you can report (apart from it being a non-negative whole number – no decimals). You can report more, the same or less than your true income.

Your earnings at the end of the experiment depend on your true income less taxes paid and possible fines. Once you have reported your income, a 40% tax is levied on this reported income.

Auditing
The government does not know your true earned income nor whether your income from source A is 80%, 50%, or 20% of total income. Only you know this. The government could audit you and everyone else in the room and discover your income, but this is very expensive and useless if you already reported your true income. Therefore, the government randomly audits people and the probability to be audited is a function of the following rule:
- If your reported income is in the bottom 50% of the reported incomes, the probability to be audited is 20%.
- If your reported income is in the top 50% of the reported incomes, the probability to be audited is 10%.

If you are audited and it reveals that you have under-reported your total income, you will have to pay:

1) the taxes on the non-reported income, and
2) a fine corresponding to 50% of the overdue taxes that you had not reported.

In other words, you would be paying one and half times the unpaid taxes. Additionally, if audited, you will automatically be audited for the previous 2 periods. If you had under-reported your income in these periods, you will have to pay 1.5 times the unpaid taxes for each one of these periods as well. Hence, your “net final income” for the audited period is equal to your reported income less the taxes paid
on your reported income less the sum of the overdue taxes and fines for the current period (t) and the previous 2
(t-1 and t-2), if applicable.

Note that you can not be audited (and fined) twice for the same period.

**How your earnings from the experiment are determined**
Every period, your “net final income” is determined by the computer the following way:

- **If you are not audited:**
  "Final net income” = Total income – Taxes on the reported income
- **If you are audited and have reported your total income:**
  "Final net income” = Total income – Taxes on the reported income
- **If you are audited and have not reported your final income:**
  "Final net income” = Total income – Taxes on the reported income – Taxes on the non-reported income (in t, t-1 and t-2) – Fines (in t, t-1 and t-2)

As a reminder, taxes on the reported income are 40% of the reported income, the taxes on the non-reported
income (payable if audited) correspond to: 40% X (total income – reported income). Finally, the fine
corresponds to 50% of the taxes on the non-reported income. (note that our program rounds up the decimals).

At the end of the experimental session, you will randomly draw a number corresponding to one of the played
periods. Each number has an equal probability to be drawn. Your earnings in eu for the drawn period will be
converted with a $1 eu = 0.50 $CAN rate.

**Additional information**
We recommend you to re-read these instructions. After taking place in front of your computer, please raise your
hand if you have questions regarding the instructions. We will come and answer them privately, and will then
inform everybody in the room of both the question and the answer. The questions should not try to validate a
decision strategy, but only attempt to understand the instructions better.
Before we start the experimental session, we will ask you some questions regarding your age, gender, level of
education and field of study, universities or schools currently attended, or your situation on the work market, and
whether you have already participated in such an experiment. This information will remain anonymous. We will
then begin the experiment.
We will also so ask you some comprehension questions on these instructions.

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**1st Announcement treatment. (sessions 1-8 ; given prior to period 22)**

We are only advising you that starting 6 periods from now, a new technology will allow the government to
automatically know all the income you will earn from source A. However, as a reminder, the government
will not know your total income nor which type of player you are (i.e. whether your income from source A is
80%, 50% or 20% of your income).
Following the introduction of this technology, since the government will know all the income coming from
source A, you will be audited automatically if you report income less than what you receive from source A.
We remind you that, as in the previous periods, when you are audited for a given period, you will be
retroactively audited for the two previous periods as well. In other words, if you are audited in this upcoming
period, you will automatically be audited for the last two periods before this new technology was announced.

**2nd Perfect Monitoring (sessions 1-8 ; given prior to period 28)**
As promised, the new technology that allows the government to automatically know all the income you will earn from source A is in service. From now on, if you report income lower than received from source A, you will be automatically audited. The other conditions regarding the taxes, fines and audit are still in effect.

1st Announcement. (sessions 9-16; given prior to period 22)

We are only advising you that starting 6 periods from now, a new technology will allow the government to automatically know all the income you will earn from source A. However, as a reminder, the government will not know your total income nor which type of player you are (i.e. whether your income from source A is 80%, 50% or 20% of your income). Furthermore, you will be able to purchase experimental units of source B with units of source A, at the specified rate. Following the introduction of this technology, since the government will know all the income coming from source A, you will be audited automatically if you report income less than what you receive from source A. We remind you that, as in the previous periods, when you are audited for a given period, you will be retroactively audited for the two previous periods as well. In other words, if you are audited in this upcoming period, you will automatically be audited for the last two two periods before this new technology was announced.

2nd Perfect Monitoring treatment with transfers. (sessions 9-16; given prior to period 28)

As promised, the new technology that allows the government to automatically know all the income you will earn from source A is in service, you are able to purchase units of source B with units of source A. You must expend 1.20 units of A to obtain 1 unit of B. We specify that this technology allows the government to know your final number of units from A, but it does not know how much you initially had. Without auditing you, the government does not know your income from source B. If you decide to do such transactions, your gross income will be modified, but your taxable income will remain your initial given income. From now on, if you report income lower than received from source A, you will be automatically audited. The other conditions regarding the taxes, fines and audit are still in effect. If you have not purchased units of B with units of A, your “final net income” is calculated by the computer the same way as before. If you have purchased units of B with units of A, your “final net income” is calculated by the computer the following way:

- **If you are not audited:**
  “Final net income” = Total income – Cost to purchase units of B – Taxes on the reported income

- **If you are audited and have reported your total income:**
  “Final net income” = Total income – Cost to purchase units of B – Taxes on the reported income

- **If you are audited and have not reported your total income:**
  “Final net income” = Total income – Cost to purchase units of B – Taxes on the reported income – Taxes on the non-reported (initial) total income (in t. t-1 and t-2) – Fines (in t. t-1 and t-2)