Tax Evasion and Social Interactions

Bernard Fortin, Guy Lacroix, Marie Claire Villeval

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Abstract

The paper extends the standard tax evasion model by allowing for social interactions. In Manski's (1993) nomenclature, our model takes into account *endogenous interactions*, *i.e.*, social conformity effects, *exogenous interactions, i.e.*, fairness effects, and *correlated effects*. Our model is tested using experimental data. Participants must decide how much income to report given individual and group tax rates and audit probabilities, and given a feedback on the other members' reporting behavior. Myopic and self-consistent expectations are considered in the analysis. In the latter case, the estimation is based on a two-limit simultaneous tobit with fixed group effects. A unique social equilibrium exists when the model satisfies coherency conditions. In line with Brock and Durlauf (2001b), the intrinsic nonlinearity between individual and group responses helps identify the model. Our results provide evidence of fairness effects but reject social conformity.

JEL: H26, D63, C24, C92, Z13.
Keywords: Social interactions, tax evasion, simultaneous tobit, laboratory experiments.

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[*]Département d'économique, Cirpée and Cirano, Université Laval, Québec, Canada.
E-mail: Bernard.Fortin@ecn.ulaval.ca

[†]Département d'économique, Cirpée and Cirano, Université Laval, Québec, Canada.
E-mail: Guy.Lacroix@ecn.ulaval.ca

[‡]GATE-CNRS, Lyon, France, and IZA, Bonn, Germany. E-mail: villeval@gate.cnrs.fr

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1 Introduction

In the standard model of tax evasion first proposed by Allingham and Sandmo (1972) and Yitzhaki (1974), the taxpayer is treated as an isolated expected utility maximizer who makes a portfolio decision under uncertainty. Cheating on taxes boils down to a risky activity whose payoff is either a lower tax burden or a penalty imposed by the tax authority. This theoretical framework assumes that the taxpayer is completely individualistic and amoral. His willingness to underreport income is not affected by social norms nor by any form of social interactions. Consequently, predicting the effects of tax or fraud prevention policies can be seriously mislead if social interactions do indeed play a significant role in tax evasion behavior. Thus, as is well known since Schelling (1978) and Akerlof (1980), interdependent behavior may generate multiple equilibria and exhibit contagion and epidemic features through a "social multiplier effect" [see Glaeser *et al.* (2003) for a recent discussion].

There are many reasons to believe that individual tax evasion decisions are affected by social norms and social interactions (*e.g.*, Andreoni *et al.* 1998). First, Erard and Feinstein (1994) insist on the role of guilt and shame in tax compliance behavior. Alm, McClelland and Schulze (1999) show that social norms play a crucial role and that voting on fiscal rules or communication can affect these norms. Likewise, Gordon (1989) and Myles and Naylor (1996) argue that an individual can derive a psychic payoff from adhering to the standard pattern of reporting behavior in his reference group (*social conformity effect*). Second, through learning from his peers, a taxpayer may find less costly ways to underreport income, to lower the risk of being caught or to reduce penalties associated with tax audits (*social learning effect*). Finally, the individual's perception of the fairness of his tax burden may influence his tax evasion decisions. Indeed, Spicer and Becker (1980) have provided evidence that those who believe they are treated unfairly by the tax system are more likely to evade taxes to restore equity (*fairness effect*).

While most economists probably agree with this taxonomy, there is certainly no consensus as to the magnitude of social interaction effects. Indeed, the very existence of these effects has become a controversial area of research in economics. Measuring social interactions effects raises difficult identification problems (Manski 1993) and they may prove hard to estimate when they are identifiable (Moffitt 2001, Blume and Durlauf 2005). Yet, even when appropriate data and econometric methods are used, they often turn out to be small or negligible determinants of individual outcomes (*e.g.*, Spicer and Hero 1985, Evans, *et al.* 1992, Aaronson 1998, Krauth 2006).

The identification problem arises from the fact that interdependent behavior takes different forms that are difficult to isolate. In Manski's (1993) terminology, the propensity of an individual to evade may genuinely vary according to the behavior of the group (*endogenous interactions* such as social conformity and social learning effects), but it may also vary with the exogenous characteristics of the group members (*exogenous interactions* such as fairness effects). Further, correlated outcomes need not arise from interdependent behavior alone. Indeed, members of a given group may behave similarly because they have similar unobserved characteristics or face similar institutional environments (*correlated effects*).

In a linear-in-means regression-like model with social interactions, Manski (1993) has shown that equilibrium outcomes cannot distinguish social effects (endogenous + exogenous) from correlated effects. Moreover, even in the absence of correlated effects, simultaneity in behavior of interacting agents introduces a collinearity between the mean outcome of the group and its mean characteristics. This so-called " *reflection problem*" hinders the identification of the endogenous effects from the exogenous effects. A number of researchers (*e.g.*, Brock and Durlauf 2001b, Moffitt 2001) have analyzed alternative models that allow for identification (*e.g.*, nonlinearity of the mean endogenous group effect on individual behavior, exclusion restrictions on exogenous interaction variables, randomized group composition). However, the validity of these models rests on the credibility of the identifying assumptions imposed to the model which in turn may depend on the nature of the data used to estimate the model.

Even when an interactions-based model is identified, its estimation raises serious econometric problems. In particular, the mean group decision, which appears as a regressor, is likely to be endogenous for two reasons. First, since individuals self-select within groups, they are likely to face common shocks and their unobserved characteristics are likely to be highly correlated (sorting bias). Second, because individual and group behavior feed on one another, the two variables are potentially simultaneously determined, at least when the groups are small (simultaneity bias).

Several studies that correct for the sorting bias show that the endogenous interaction effects shrinks and sometimes completely disappear. For example, based on micro-simulation estimation, Krauth (2006) has found that the actual peer effect on teen smoking is halved when compared to standard estimation procedures. This result suggests that papers reporting important peer effects should be taken cautiously if they ignore potential selection effects. Krauth has also shown that the simultaneity bias may be important in small groups. Therefore the use of appropriate data and econometric models is required to provide a robust test of the existence of social interactions effects.

In this paper we estimate the impact of social interactions on tax evasion based on the results of a laboratory experiment. Our approach takes into account both the identification and estimation problems related with the estimation of such a model. Moreover, we provide a test for the existence of multiple equilibria. Participants receive a fixed endowment and must decide how much income to report given their tax rate and audit probability, given those faced by the other members of their group, and given the group's mean reported income. Each game is repeated a sufficient number of periods to insure convergence to a (Nash) social equilibrium. We first develop a theoretical model of tax evasion with both endogenous and exogenous social interactions. We thus extend the standard Allingham-Sandmo-Yitzhaki model by allowing for social conformity and fairness effects.[1] The model allows two types of two corner solutions: no tax evasion or no tax compliance. This is important since 43% of all observations turn out to be censored in our data.

Social interaction effects crucially depend upon how agents anticipate group behavior. Two alternatives are considered. The first assumes *myopic expectations* whereby decisions are based on lagged group mean response. The second assumes *self-consistent expectations* whereby decisions are based on the contemporaneous group mean response. In the former case, a simple two-limit tobit model yields consistent parameter estimates under mild conditions. In the latter case, a two-limit *simultaneous* tobit model must be used to account for the endogeneity between individual and group responses. We exploit the non-linearities generated by the truncated normal distribution of the endogenous variables to help identify the model. We show that a unique social equilibrium exists when so-called "coherency conditions" (Gouriéroux *et al.* 1980) are satisfied. In a sense, this approach extends Brock and Durlauf's (2001a) discrete choice model to the case where the censored choice variable is a mix of discrete and continuous variables.

For the purpose of estimating the impact of social interactions on tax evasion, experimental data such as those we use have many advantages over alternative sources of information (audited tax returns or randomized surveys). In particular, they allow to control the reference group with whom individuals interact in the laboratory.[2] Hence, because group size is determined exogenously and membership assigned randomly, identification of social interaction effects is easier to achieve (Moffitt 2001).[3] Also, insofar as correlated effects persist, they can be dealt with though the use of group fixed effects, as long as many games are played by the participants. This also help identify the model.

Experiments are useful in circumventing problems that are intrinsic to audit and survey data. For instance the probability of auditing is generally related to the extent of underreporting. Analyzes that use either type of data have to control for potential endogeneity biases. In an experiment, the audit probability can be randomly assigned and unrelated to the intensity

[1] Because agents share the same information in our experimental setup, we do not consider social learning. We nevertheless present a simple test for dynamic social learning and reject it strongly. See section 5.

[2] Audited tax returns usually do not reveal the nature of the reference group within which an individual may interact. This information is required to estimate social conformity effects (Manski 2000). Also, though randomized surveys can provide subjective information on the taxpayer's reference group (*e.g.*, Sheffrin and Triest 1992), a substantial fraction of tax evasion activities are likely to be underreported in these data (Elffers *et al.* 1987). Moreover tax evaders may overestimate the amount unreported by their peer group in order to better justify their own behavior (cognitive dissonance bias).

[3] In practice however, random assignment may not wash away entirely correlated effects since participants are usually drawn from a restricted pool of volunteers who are likely to have similar unobserved characteristics (*e.g.*, students from a business school and from engineering schools, as in our experiment).

of evasion, thus avoiding the problem. In addition, the use of computerized devices avoids measurement errors likely to distort field data since decisions in the laboratory are perfectly recorded.

Few attempts have been made to document the impact of social interactions on tax compliance using experimental data. What little evidence exists is rather inconclusive. Recent attempts have focused on criminal activities such as stealing (Falk and Fischbacher 2002) or free riding in public goods games (Falk *et al.* 2004), but none has focused on tax compliance *per se*. One notable exception is Bosco and Mittone (1997). In their setting, individuals receive a public good commensurate to the tax contributions of all group members. They have found strong evidence that individual compliance is influenced by the reporting behavior of other group members. In an older experiment, Alm *et al.* (1992) have similarly found that compliance increases when taxpayers receive a public good in exchange for their payment. In our setup, contrary to most experimental studies, individual monetary payoffs do not depend on the other participants' behavior. This allows us to better isolate the effect of social interactions. To our knowledge, this is the first attempt to analyze the reflection problem using experimental data.

The rest of the paper is organized as follows. In Section 2, we present the theoretical model. Section 3 describes the experiment. Section 4 discusses the econometric approach. Section 5 discusses the main features of the data and presents the econometric results. According to our findings, equilibrium outcomes are consistent with (anti-)conformity effects when expectations are assumed to be self-consistent and the endogeneity of group response is not accounted for. When expectations are assumed to be myopic, the (anti-)conformity effect is still significant but much less important. Finally, it is no longer statistically significant when endogeneity is taken into account and self-consistent expectations are assumed. Our results also provide some evidence of fairness effects but systematically reject correlated effects. As expected, individual tax rates, audit probabilities, gender and inequality aversion all have a significant influence on tax compliance behavior. Section 6 concludes the paper.

2 A Model of Tax Evasion with Social Interactions

2.1 Modelling the individual tax evasion decision

In this section we introduce endogenous and exogenous social interactions among taxpayers into the standard Allingham-Sandmo-Yitzhaki tax evasion model.[4] Consider individual i who

[4]In the literature, not all models rely on the expected utility theory. For instance, Dahmi and al-Howaini (2003) show that the positive link between tax rates and evaded taxes can be rationalized by the "prospect theory".

belongs to a reference group of size N, N being exogenous. His decision horizon is one period. His before-tax income I, normalized to 1, is unknown to the tax authority and is exogenous. For simplicity, assume all individuals in the group have the same income. The individual faces a flat tax rate t_i on his reported income, D_i. He must decide how much income to report knowing that with probability p_i his tax return will be audited. If caught cheating he must pay the amount of evaded tax, $t_i F_i$, with $F_i = 1 - D_i$, plus a commensurate penalty $\theta t_i F_i$, with $\theta > 0$. For simplicity, the penalty rate is assumed the same for everyone. If the individual is not audited, his net income will be $1 - t_i D_i$. If he is audited his net income will be $1 - t_i D_i - (1+\theta) t_i F_i = 1 - t_i D_i - (1+\theta) t_i (I - D_i)$. Expected utility, EU_i, is assumed to consist of two separable components:

$$EU_i = \{(1-p_i)u(1-t_iD_i) \ + p_i u(1-t_iD_i-(1+\theta)t_i(1-D_i))\} + S(D_i, X_i). \qquad (1)$$

The first component within braces is the private expected utility associated with tax compliance behavior, that is, with a choice of D_i. Assuming that the individual is risk averse, private utility $u(\cdot)$ is increasing and concave in consumption. The second component, $S(D_i, X_i)$, is the social (*ex-ante*) utility associated with tax compliance. This component is assumed to depend on reported income, D_i, and on a vector X_i of exogenous variables to be defined below.[5] The marginal social utility of tax compliance, $s_i \equiv \partial S/\partial D_i$, is assumed to depend only on $X_i : s_i = s(X_i)$. Therefore $S(D_i, X_i)$ is an affine function of D_i and can be written as:

$$\begin{aligned} S(D_i, X_i) &= s(X_i)D_i & (2) \\ &= s(\overline{D}^e_{-i}, \overline{t}_{-i}, \overline{p}_{-i}, A_i, \overline{A}_{-i}, \varepsilon_i)D_i. & (3) \end{aligned}$$

The vector X_i includes a number of variables. First, we assume that the marginal social utility of tax compliance depends on $\overline{D}^e_{-i}$, individual i's subjective expectation of the mean tax compliance of the other members of his reference group. A positive effect corresponds to a social conformity effect.[6] In that case, preferences exhibit so-called strategic complementarities (Brock and Durlauf 2001a). A negative effect corresponds to a social anti-conformity effect (strategic substitutabilities). In that case the individual prefers to deviate from the tax compliance behavior of his reference group. Second, given that participants receive the same

Both Kahneman and Tversky (1979) and Alm, McClelland and Schulze (1992) provide some evidence that individuals overweight low probabilities of audit. Robben, Webley, Weigel et al. (1990) show that taxpayers evade more when they perceive the tax system to be unfair and the audit probability low ("equity theory"). On the other hand, King and Sheffrin (2002) have conducted an experiment based on framed questions that lead them to conclude that subjects behave more in accordance with expected utility theory than with either the prospect theory or the equity theory.

[5]The separability assumption between private and social utilities is relatively common. See Brock and Durlauf (2001a).

[6]Myles and Naylor (1996) assume that the conformity effect is limited to the evasion decision. In our more general approach, the evaded amount (not only the evasion decision) can be influenced by the behavior of other group members (see Gordon 1989).

before-tax income, the individual's marginal social utility is assumed to be increasing with his group's tax rate, $\overline{t}_{-i}$, and his group's audit probability, $\overline{p}_{-i}$, given his own tax rate and audit probability rate (fairness effects).[7] Finally, X_i includes a sub-vector A_i of observable attributes (*e.g.,* gender), a sub-vector $\overline{A}_{-i}$ of the corresponding mean observable attributes of the other members and the realization of a random term ϵ_i that captures unobservable individual-specific attributes and attributes that are common to all individuals in the group.[8] The theoretical model and its econometric counterpart are linked through ε_i.

We assume that the public goods funded by the tax receipts do not enter the individual's utility and therefore have no bearing on tax evasion decisions. Substituting equations (3) and (2) into (1) and assuming that preferences satisfy the Von Neuman-Morgenstern axioms, the individual's problem is to choose how much income to report, D_i, so as to maximize his expected utility (1) subject to the inequality conditions $0 \leq D_i \leq 1$.

The optimal level of reported income can be derived from the Kuhn-Tucker conditions. Instead, we present an equivalent formulation that is more in line with our econometric specification. Let us first solve the optimization problem while ignoring the inequality conditions on D_i. The equation for D_i^*, the latent variable associated with D_i, can be written as:

$$D_i^* = D^*(t_i, p_i, \overline{D}^e_{-i}, \overline{t}_{-i}, \overline{p}_{-i}, A_i, \overline{A}_{-i}, \varepsilon_i). \tag{4}$$

Because the individual's income and penalty rate θ are assumed constant they are omitted from (4). Given the inequality conditions on D_i , the relationship between the (observed) variable D_i and the latent (unobserved) variable D_i^* is:

$$D_i = 1\!\!I(0 < D_i^* < 1)D_i^* + 1\!\!I(D_i^* \geq 1), \tag{5}$$

where $1\!\!I(a)$ is an indicator function for the event a.

From this model we can derive six predictions regarding tax evasion:

1. A risk-averse individual will always underreport his income (*i.e.,* $D_i < 1$) whenever $1 - s(X_i)/tu'(1-t) - p_i(1+\theta) > 0$, that is, whenever the expected return on evaded taxes is strictly positive, with due allowance for the marginal social cost of tax evasion, $s(X_i)/tu'(1-t)$.The latter will be positive if tax compliance yields a positive social marginal utility $(s(X_i) > 0)$. Interestingly, simple expected utility models predict much lower compliance rates than what is usually observed in practice (see Andreoni *et al.*

[7]To simplify the presentation, we do not include t_i and p_i in the vector X_i, that is, we ignore the fairness effects associated with changes in the individual's tax rate or audit probability. However, these effects are taken into account in the estimation of the reduced-form model.

[8]The vectors A and $\overline{A}_{-i}$ and the scalar ε_i could also influence the private utility component of the individual's expected utility. However this would not change the comparative statics of the model in any significant way.

1998). The difference may be partly attributable to the omission of this marginal social cost.

The next five predictions concern the impact of exogenous variables on the amount reported by individual i assuming an interior solution:

2. $\partial D_i/\partial t_i = ?$, assuming decreasing absolute risk aversion;
3. $\partial D_i/\partial p_i \geq 0$;
4. $\partial D_i/\partial \overline{D}^e_{-i} = ?$;
5. $\partial D_i/\partial \bar{t}_{-i} \geq 0$;
6. $\partial D_i/\partial \overline{p}_{-i} \geq 0$.

Proposition (2) states that the impact of an increase in the tax rate on tax compliance can be positive or negative. The impact can be decomposed into two components of opposite sign. The first is positive (see Yitzhaki 1974) and has raised a lot of discussion in the literature since it is rather counter-intuitive. It arises because the penalty is proportional to the amount of evaded tax. Therefore an increase in the tax rate involves no substitution effect between the individual's private consumption when he is audited and when he is not. Because it reduces income, however, the individual is induced to cheat less if his absolute risk aversion decreases with income. The second effect derives from the social component in the utility function. Because the marginal cost (in terms of paid taxes) of tax compliance increases with the tax rate, the individual reduces his level of tax compliance. Therefore, our model shows that adding a social component to the utility function may generate a positive relationship between tax rates and tax evasion.

Proposition (3) was first derived by Allingham and Sandmo (1972) and states that an increase in the audit probability increases tax compliance. Proposition (4) states that an increase in the mean reported income by the reference group may increase tax compliance (social conformity effect) or decrease it (anti-conformity effect). Finally, propositions (5) and (6) indicate that an increase in the tax rate or in the audit probability of an individual's group increases his tax compliance (fairness effects). Propositions (4)–(6) derive from the fact that an increase in the marginal social utility of tax compliance, s_i, induces the individual to report more income to the tax authority.

2.2 Social equilibrium with tax evasion

We assume that an individual acts non-cooperatively and does not take into account the effect of his decision on the choices made by the others. In order words, he makes his tax compliance decision conditional upon his expectations about his group's mean reported income, $\overline{D}^e_{-i}$. To close the model, we must state explicitly how individuals form their expectations and in particular how these relate to the information available at the time the decision is made. This issue is crucial because the estimates of the social conformity effect are intimately related to the expectations formation mechanism.

Prior to discussing how expectations are formed, some elements of the experimental setup must first be described. Each group plays five separate interactive rounds. At the beginning of each round, tax rates and audit probabilities are randomly assigned to group members and remain constant for the duration of the round. Each round is broken-down into periods. At the end of each period, once every group member has recorded his decision, $\overline{D}_{-i}$ is computed and reported to each member i. The game is repeated a sufficient number of periods to insure convergence is reached. The convergence criterion is expressed as $\left|(\overline{D}^{\tau-1} - \overline{D}^{\tau-2})/\overline{D}^{\tau-2}\right| \leq .05$.[9]

Given the above experimental setup, we consider two alternative expectation formation mechanisms. The first assumes expectations are based on the group's mean reported income in the preceding period (myopic expectations). An important advantage of assuming myopic expectations is its simplicity. If one further assumes that the random terms are not autocorrelated, the variable $\overline{D}^e_{-i}$ (which, in this case, is equal to $\overline{D}^{\tau-1}_{-i}$ for $t \geq 2$) is exogenous. [10] The model is thus identified if the correlated effects can be accounted for by group fixed effects. The main weakness of this approach are that is it based on an *ad hoc* assumption about the appropriate lag length. Also, it implies that individuals can make systematic forecasting errors without ever adjusting their expectations.

The second alternative assumes that expectations are based on the contemporaneous group mean reported income (*self-consistent* expectations). If there exists a social equilibrium, it follows that $\overline{D}^e_{-i} \simeq \overline{D}_{-i}$ which is a property of the (Nash) social equilibrium.[11] This approach thus allows us to assume self-consistent beliefs when estimating the model. The equilibrium condition of the model is thus obtained by setting $\overline{D}^e_{-i} = \overline{D}_{-i}$ and replacing $\overline{D}_{-i}$ by $\frac{1}{N-1}(\overline{D}N - D_i)$ in the latent equation (4). Substituting this equation into (5) and solving for D_i as a function of $\overline{D}$ and x_i, we get $D_i = D(\overline{D}, x_i)$. Adding over N and dividing by N we

[9] Obviously the convergence criterion can only be computed after two periods. After ten iterations, the round is stopped if no convergence has been reached and the round is discarded. See Section 3.

[10] Our econometric model allows for autocorrelation due to unobservable (to the econometrician) invariant group characteristics, since the latter are taken into account by group fixed effects.

[11] If there are innovations in the random term ε_i at each period (ε_i becomes ε_{it}) and individuals do not communicate, the perfect foresight equilibrium will be replaced by a rational expectations equilibrium with: $\overline{D}^e_{-i} = E(D_{-i})$.

finally get:

$$\overline{D} = \frac{\sum_{i=1}^{N} D_i(\overline{D}, x_i)}{N} = G(\overline{D}, x), \tag{6}$$

where x is the vector of all exogenous individual and policy variables of the model. Since $G(\overline{D}, x)$ is continuous and the support of $\overline{D}$ is compact, it follows from Brouwer's fixed point theorem that there must exist at least one solution for $\overline{D}$ that satisfies this condition. As argued by Brock and Durlauf (2001b), multiple equilibria are a common feature of interactions-based models such as (6). We will take up this issue in Section 4 and we will show that it is related to the coherency conditions in the model to be estimated.

3 The Experiment

The purpose of our experiment is to generate data to estimate and test our model of tax evasion with endogenous and exogenous social interactions. This section presents the experimental design and discusses its external validity, that is, how its results could be generalized to the larger population.

3.1 Experimental Design

Our experiment comprises two parts (see instructions in Appendix A). The first part of the experiment, which is mainly used to help participants to learn how to play the games, consists of 5 rounds. To facilitate decision-making, this part excludes endogenous social interactions and therefore information on group behavior is not disclosed ("NOINFO" treatment). Each group is composed of 15 participants. At the beginning of each round, each participant receives the same initial exogenous "endowment" of 100 experimental currency units (ECU) which constitutes his income. He is requested to give back a percentage of his income (a "deduction rate"). There are 5 different tax rates, with each rate randomly assigned to 3 participants. This is common knowledge. Each participant is told that these paybacks will go into scientific research funds (*i.e.*, the lab gets this amount of money back). To satisfy this request, the participant must report an amount between 0 and 100 that will be partly taxed back. He is informed that his reported income can be audited according to a certain probability and that this audit will entail the payment of a fine (a "penalty") if the reported income is less than his endowment. The penalty is fixed at 100% of unpaid taxes. There are 5 audit probabilities, with each audit probability randomly assigned to 3 participants.[12] This is also common knowledge. The participants are informed that the probabilities are independent of the reported amounts. It should be noted that the distribution of individual tax rates is independent of the distribution of the audit probabilities.

To simplify decision-making, a scrollbar on the computer screen indicates for each possible value of reported income the payoffs if not audited and if caught cheating.[13] At the end of each round, once all participants have validated their decision on the keyboard, a new round starts automatically. There is no feed-back about actual audits and payoffs before the whole session is completed. This limits the presence of wealth effects during the experiment that may distort compliance behavior. At the beginning of each new round a new series of tax rates and audit probabilities are reassigned to the group members. We deliberately alternate between medium, low and high tax and audit regimes and this order is kept constant across sessions to ease comparisons. This was however not common knowledge. Even if subjects had been able to anticipate this order, it could not affect their behavior after being audited since they received no feed back on actual audits.

The second part of the experiment also consists of 5 rounds. It corresponds to the so-called information condition ("INFO" treatment). Two main changes are introduced in the protocol. The first change relates to the structure of the rounds. The second to the informational feedback. Each round now includes up to 10 periods. The idea is to allow convergence in decision making to reach social equilibrium. In the first period of a new round, new tax and audit regimes are assigned for the whole round. From the second period on, each participant receives a feedback about the group behavior in the previous period. Hence, the number of evaders among the 14 other group members and their mean reported income appear on the screen. During a round, individual tax rates and audit probabilities are fixed. A new period is launched until the convergence criteria is equal to or lower than 5% in absolute value. All the other parameters of the protocol remain unchanged during a round. If convergence is not achieved within 10 periods, a new round is initiated.

By combining various tax rates and audit probabilities the experiment mimics a large range of tax regimes (see Appendix B). A total of 12 sessions were carried out, each involving 10 rounds. The sessions were subdivided into 3 sets. For each one, 3 different tax and audit regimes (high, medium, low) were used. In all, we thus experimented with 9 tax regimes and 9 audit regimes, yielding as many as 45 individual tax and audit rates.

[12] Allocating audit probabilities as a function of past detected cheating behavior would have increased the realism of the experiment and enabled social learning, but it would also have made the results less comparable with past experiments, the rules of the game considerably more complex and the convergence process slower.

[13] Therefore, during the experiment, the subjects were fully aware of the risk of losses associated with each of their decision. 61 observations indicate a loss in a round but these losses were largely compensated by earnings in other rounds. All subjects earned considerably more than their show-up fee and nobody left the laboratory with having to pay money out of their pocket.

At the end of their session, participants were asked to fill an anonymous post-experimental questionnaire. This questionnaire aimed at collecting information about individual characteristics such as age, gender and college major. An additional item was added to elicit the individual degree of inequality aversion. Participants had to imagine a situation involving the sharing of a pie among two persons (excluding themselves in order to get less emotional decision). They were asked to indicate their favorite share among two possibilities. They had to make three consecutive choices. The alternative shares were $(50, 50)$ against $(55, 65)$, $(50, 50)$ against $(45, 70)$, and $(50, 50)$ against $(35, 85)$. Rejection of a greater pie but more unequally shared can be considered a signal of a high inequality aversion. An index of inequality aversion (between 0 and 2) is included in some specifications of the model as a control variable.[14]

This experiment was performed at GATE (Groupe d'Analyse et de Théorie Economique, France) using the Regate software (Zeiliger 2000). Participants were volunteer undergraduate and graduate students from four business and engineer schools and university. A total of 180 students participated in this experiment. Since each session consisted of 10 rounds, this provides a total of 1800 observations (900 for each of NOINFO and INFO treatments). Excluding rounds which did not achieve convergence leaves a total of 795 observations for the INFO treatment. Participants were paid in cash in a separate room. A show-up fee of 3 €, was added to cover participation expenses and participants who answered the questions on inequality aversion received an additional 1.5 €. The average earning was 13.77 €.

3.2 External Validity

Laboratory experiments are often criticized because they may not properly reflect natural environments. In our particular setting, one may question the artificial character of the reference groups, the contextual framework and the nature of the information made available to the participants in each round. We address each in turn.

We acknowledge that interacting with an artificial reference group may be a poor proxy for real-life interactions. In our game, the reference group is exogenously imposed and consists of all those who happen to show-up at a given session. Yet this approach presents an important advantage: Every subject interacts with a single well-defined reference group of the same size. Analyzing social interactions is thus made much easier than when using survey data. Indeed, because these hardly ever provide any information about reference groups, the analyst often assumes individuals interact with those who share similar attributes: age, education, income, vicinity, *etc.* (*e.g.*, van Praag and Frijters 1999). This is precisely what we do in our exper-

[14]The index, F, is constructed as followed. We first define three dummies $V_i(i = 1, 2, 3)$ associated to the three consecutive choices above, with $V_i = 1$ when the choice is $(50, 50)$. We set $F = 0$ when $V_1 = 0, V_2 = 0$ and $V_3 = 0$ or when $V_1 = 0, V_2 = 0$ and $V_3 = 1$ (low aversion), $F = 2$ when $V_1 = 1, V_2 = 1$ and $V_3 = 1$ or when $V_1 = 0, V_2 = 1$ and $V_3 = 1$ (high aversion), and $F = 1$ in all other cases (average aversion).

iment: participants are all students of nearly the same age, enrolled in the local engineering and business schools, and living in the vicinity of Lyon. Despite being artificially generated, laboratory reference groups may nevertheless closely mimic real-life reference groups.

In designing the experiment, we have purposely chosen not to contextualize the game: Participants were not told the decisions were about tax evasion and neutral wording was used throughout to avoid framing effects.[15] Although we have refrained from using terms such as "fraud" or "tax evasion", we can reasonably assume that subjects understood they were required to report the entirety of their endowment to avoid punishment. The notion of "penalty" and "fine" should have made it clear that under-reporting meant cheating. Lastly, contrary to previous experiments, subjects were informed about their peer's behavior. It was nevertheless important to keep the rest of the protocol as standard as possible so that our results could be compared to those of previous experiments. This is why tax rates or audit probabilities were randomly assigned.

Finally, the external validity of the experiment may be jeopardized by the fact that participants are probably more informed than real-life tax evaders. Indeed, tax evasion is usually not observed or revealed to peers or outsiders to avoid guilt, shame or being reported to the tax authority. The reason participants are provided information about the true mean reported income and the number of evaders is to avoid strategic behavior. Allowing individual misreporting would have made the mean uninformative and would have seriously compromise our ability to identify the impact of social interactions.

4 Econometric model

In this section we discuss the econometric methodology used to estimate the model. In the presentation, we assume that individual expectations are based on contemporaneous values of the group's mean reported income (self-consistent expectations).[16] To ease our task, the latent equation (4) is linearized as follows:

$$D_{ik}^{g*} = x_{ik}^{g}\beta + \gamma \overline{D}_{-ik}^{g} + \overline{x}_{-ik}^{g}\delta + c^{g} + \eta_{ik}^{g}, \tag{7}$$

where D_{ik}^{g*} is a latent variable for the desired amount of income reported by individual i in group g at round k, $i = 1, ..., N$, $g = 1, ...G$, $k = 1...K$; x_{ik}^{g} is a corresponding row vector of

[15] Alm (1988) suggests that decisions made in a such a setting better reflect subjects' preferences. Alm *et al.* (1992) conclude that the use of neutral rather loaded wording does not change behavior, while King and Sheffrin (2002) show that subjects are reluctant to evade taxes even when using frames to stress the unfairness of the tax regimes.

[16] When expectations are assumed to be based on lagged group averages, a simple two-limit tobit is used to estimate the model.

observable exogenous variables (including a constant term), β and δ are vectors of parameters, c^g represents unobservable group-specific attributes and η_{ik}^g is an error term capturing the effects of unobservable individual-specific attributes that may vary across rounds. Since the correlation between participants is taken into account by the component c^g, one assumes that η_{ik}^g is distributed as $N(0, \sigma^2)$. The normal density and cumulative function of η_{ik}^g are denoted by $f(\eta_{ik}^g)$ and $F(\eta_{ik}^g)$ respectively. In addition, let

$$\overline{D}_{-ik}^g = \frac{1}{N-1} \sum_{\substack{j=1 \\ j \neq i}}^{N} D_{jk}^g, \qquad \overline{x}_{-ik}^g = \frac{1}{N-1} \sum_{\substack{j=1 \\ j \neq i}}^{N} x_{jk}^g.$$

In this model γ is the endogenous social interaction effect. If positive, participants conform to group behavior, while if negative, they deviate from the group behavior. The vector δ captures the exogenous effects (including the fairness effect). To model the correlated effects two approaches can be used. The *group random effects* approach treats c^g as a random term assuming it is orthogonal to the exogenous variables: $\epsilon_{ik}^g = c^g + \eta_{ik}^g$. The *group fixed effects* approach allows c^g to be arbitrarily correlated with the exogenous variables. This method is more general and in fact much easier to implement than the former approach. We follow Aronsson *et al.* (1999) and use the group fixed effects approach. There are thus $G-1$ dummy variables to be estimated, one for each group, save one to allow identification. Following Kooreman (2006), the N equations in (7) corresponding to those associated with round k of session g can be written in matrix notation as

$$D_k^{g*} = X_k^g \beta + \Gamma D_k^g + \overline{X}_{-k}^g \delta + C^g d^g \boldsymbol{\iota}_N + \eta_k^g, \qquad \text{for} \quad g = 1, \cdots, G; \ k = 1, \cdots, K, \quad (8)$$

where

$$\Gamma = \begin{bmatrix} 0 & \frac{\gamma}{N-1} & \cdots & \frac{\gamma}{N-1} \\ \frac{\gamma}{N-1} & 0 & \cdots & \frac{\gamma}{N-1} \\ \vdots & \vdots & \ddots & \vdots \\ \frac{\gamma}{N-1} & \frac{\gamma}{N-1} & \cdots & 0 \end{bmatrix},$$

C^g is a $(G-1)$ row vector of group-specific fixed effects, d^g is a $(G-1)$ column vector of dummy variables, $\boldsymbol{\iota}_N$ is a $N \times 1$ column vector of ones, and η_k^g is a vector of error terms distributed as $N(0, \sigma^2 I)$. Recall that the reported income is normalized between 0 and 1. The relationship between the observed vector D_k^g of reported incomes and the corresponding latent vector is given by $D_k^g = \mathbb{1}(0 < D_i^{g*} < 1) D_i^{g*} + \mathbb{1}(D_i^{g*} \geq 1)$, where as before $\mathbb{1}(\cdot)$ is a vector of indicator functions which take the value one or zero. Equation (8) corresponds to a simultaneous equation two-limit tobit with within- and cross-equation restrictions on parameters (see matrix Γ) and with error terms uncorrelated across equations. It involves both latent variables and their observed counterparts. Amemyia (1974) was the first to consider such mixed models and the approach we use to estimate our system is based on his work.

The estimation of (8) raises two distinct problems that must be addressed separately: the so-called coherency problem and the identification problem. The coherency problem (see Gouriéroux *et al.* 1980) consists in finding the condition which guarantees the system has a well-defined unique reduced form. In a general linear-in-means model, the coherency condition reduces to the invertibility of $I - \Gamma$, the matrix of coefficients of the endogenous variables. In a latent linear-in-means model with censored endogenous variables such as (8) the coherency condition is more restrictive. Indeed, Amemyia (1974) has shown that every principal minor of the matrix $I - \Gamma$ must be positive. This coherency condition clearly implies the existence of a unique social equilibrium at each round k of session g. In the empirical section, this condition is verified for each specification of the structural model.

In a sense, the coherency problem precedes the identification problem. Indeed, the latter refers to the uniqueness of the parameters of the structural model given the parameters of the reduced form model. Identification therefore assumes the existence of a well-defined reduced form. As discussed above, estimating social interactions models raises serious identification problems. Results from Manski (1993) imply that it is impossible to identify the structural parameters β, γ, δ and c^g ($g = 1, ..., G-1$) when the model involves no censored endogenous variables ($D_{ki}^{g*} = D_{ki}^{g}$ for all i, k and g) and without *a priori* restrictions on the parameters of δ or on the distribution of η_k^g. The reason is that the order condition for identification in a structural linear model is not satisfied (Moffitt 2001).

Two reasons explains why our model is identified. First, models with endogenous censored variables such as (8) may be easier to identify than linear-in-means models. Due to the nonlinear relationship between observed reported income and the corresponding latent variable, the model imposes a nonlinear relationship between the individual behavior and the mean behavior of the reference group. As emphasized by Brock and Durlauf (2001b), this is likely to solve the identification problem since nonlinear models with self-consistent beliefs are most likely to be identified.[17] From the econometric point of view, this result is consistent with the idea that nonlinearity generally helps rather than hampers identification. It is important to note however that identification hinges on knowing the specific form of nonlinearity which, in our case, depends on the assumption of normality of the error terms. Second, our model imposes restrictions on the covariance matrix of η_k^g since $\eta_k^g \sim N(0, \sigma^2 I)$. These restrictions follow from the fact that the correlations between individual disturbances are assumed to be taken into account by the group fixed effects. The latter can be estimated because participants play many games.

To derive the likelihood function of our model, let $Z_{ik}^g = (x_{ik}^g, \overline{D}_{-ik}^g, \overline{x}_{-ik}^g, 1)$ and $\alpha = (\beta, \gamma, \delta, c^g)'$ so that from (7) we can write: $D_{ik}^{g*} = Z_{ik}^g \alpha + \eta_{ik}^g$. For any given round k in

[17]They derive conditions for identification in the case of a discrete-choice generalized logistic model of social interactions and show that they are much less restrictive than for the linear-in-means model. However they do not analyze the case of a mixed discrete-continuous tobit-type model such as the one used in this paper.

session g define:

$$\begin{array}{lcl} R_k^g & : & \text{the number of players who reported } 0 < D_{ik}^g < 1, \\ S_k^g & : & \text{the number of players who reported } D_{ik}^g = 0, \\ T_k^g & : & \text{the number of players who reported , } D_{ik}^g = 1. \end{array}$$

with $R_k^g + S_k^g + T_k^g = N$.

Divide the observations on all the rounds (for $k = 1, ..., K$ and $g = 1, ..., G$) into seven subsets:

$$\begin{array}{lcl} S_1 & : & R_k^g > 0, S_k^g = 0, T_k^g = 0. \\ S_2 & : & R_k^g > 0, S_k^g > 0, T_k^g = 0. \\ S_3 & : & R_k^g > 0, S_k^g = 0, T_k^g > 0. \\ S_4 & : & R_k^g > 0, S_k^g > 0, T_k^g > 0. \\ S_5 & : & R_k^g = 0, S_k^g > 0, T_k^g = 0. \\ S_6 & : & R_k^g = 0, S_k^g = 0, T_k^g > 0. \\ S_7 & : & R_k^g = 0, S_k^g > 0, T_k^g > 0. \end{array}$$

The likelihood function of the model (7) is given by:

$$\begin{aligned} L \;=\; & \prod_{S_1} |B_N| \left[\prod_N f(D_{ik}^g - Z_{ik}^g \alpha) \right] \times \qquad (9) \\ & \prod_{S_2} \left[\prod_{R_k^g} \left| B_{R_k^g} \right| f(D_{ik}^g - Z_{ik}^g \alpha) \prod_{S_k^g} F(-Z_{ik}^g \alpha) \right] \times \\ & \prod_{S_3} \left[\prod_{R_k^g} \left| B_{R_k^g} \right| f(D_{ik}^g - Z_{ik}^g \alpha) \prod_{T_k^g} F(Z_{ik}^g \alpha - 1) \right] \times \\ & \prod_{S_4} \left[\prod_{R_k^g} \left| B_{R_k^g} \right| f(D_{ik}^g - Z_{ik}^g \alpha) \prod_{S_k^g} F(-Z_{ik}^g \alpha) \prod_{T_k^g} F(Z_{ik}^g \alpha - 1) \right] \times \\ & \prod_{S_5} \left[\prod_{S_k^g} F(-Z_{ik}^g \alpha) \right] \times \\ & \prod_{S_6} \left[\prod_{T_k^g} F(Z_{ik}^g \alpha - 1) \right] \times \\ & \prod_{S_7} \left[\prod_{S_k^g} F(-Z_{ik}^g \alpha) \prod_{T_k^g} F(Z_{ik}^g \alpha - 1) \right], \end{aligned}$$

with

$$\left|B_{R_k^g}\right| = \begin{vmatrix} 1 & -\frac{\gamma}{N-1} & \cdots & -\frac{\gamma}{N-1} \\ -\frac{\gamma}{N-1} & 1 & \cdots & -\frac{\gamma}{N-1} \\ \vdots & \vdots & \ddots & \vdots \\ -\frac{\gamma}{N-1} & -\frac{\gamma}{N-1} & \cdots & 1 \end{vmatrix},$$

the determinant of the corresponding matrix $[R_k^g \times R_k^g]$. Maximizing the log of (9) with respect to α and σ yields the full information maximum likelihood estimates of the model. Under standard regularity assumptions, these estimates are consistent and asymptotically efficient.

5 Results

Table 1 provides descriptive statistics of our sample. Most subjects are young and males outnumber females. Both tax rates and audit probabilities display a large standard deviation (see also Appendix B). This helps to identify their impact on tax compliance behavior. Over 88% (53/60) of all rounds with feedback information on the tax compliance satisfy the convergence criterion and therefore correspond to a social equilibrium. This leaves 795 observations on reported income in part II of the experiment (INFO treatment) out of 900 potential observations.[18] In the INFO treatment, 24.5% of these observations (195) are censored at zero while 19% (151 observations) are censored at 100 for a total of 43.5% censored observations (346). The corresponding percentages in the NOINFO treatments are 18% (164 observations), 21% (189 observations) and 39% (353), respectively. Finally, the average reported income in the INFO treatment (50.15) is about half the initial endowment and slightly lower than the average reported income in the NOINFO treatment (53.92).

Table 2 reports detailed estimation results for various specifications of the model. The econometric results focus mainly on the INFO treatment, though the last column of the table (column (8)) reports results from the NOINFO treatment. Column (1) provides results for a full linear-in-means model in which individual and corresponding group mean variables are included as regressors. There are thus no exclusion restrictions on exogenous interactions variables. Correlated effects are taken into account through 11 group dummies. While the

[18]In the econometric analysis, we excluded rounds that did not converge after ten rounds to avoid a misspecification bias. Basically, we assumed (but could not test) that these rounds would have converged to a social equilibrium had we allowed the rounds to include more than ten periods. Of course, this may generate a selection problem (due to choice-based sampling). To provide some evidence on this point, we estimated the probability that round j converges as a function of the characteristics of the round and of the participants in this round (age, gender, individual tax rate, the probability of being audited,...) and none of these variables was significant at the 5% level. This indicates that the presence of a selection bias is not likely to be an important problem here.

audit probability, p_i, is entered linearly, the tax rate, t_i, is entered with a linear and a quadratic term since our theoretical model predicts its impact on tax compliance can be decomposed into the sum of two opposite effects. No other individual characteristics are included in this specification. Importantly, results in column (1) assume that individual expectations are based on contemporaneous group mean reported income (self-consistent expectations). The model is estimated by a simple two-limit tobit model and thus ignores the potential simultaneity between group and individual behavior. Since there are only 15 participants in each session, this omission may significantly bias the estimate of the endogenous social effect. Results in column (1) show that, contrary to expectations, the parameter estimate of $\overline{D}_{-i}$ is negative and statistically significant at the 5% level. This is opposite to the social conformity effect because an increase in mean group tax evasion induces individuals to be more compliant.

There are at least four reasons why such a result may obtain. First, tax evasion behavior may induce a social anti-conformity rather than a social conformity effect. In other words, participants may be inclined to deviate from the reference group's behavior. Wenzel (2004) argues that, at least in the field of tax evasion, social norms may induce deviation from mean group response if the latter is inconsistent with individuals' internal norms. Kooreman (2006) obtains such an anti-conformity effect when studying student self-esteem: the lower the group self-esteem, the higher the individual self-esteem. In our tax evasion experiment social anti-conformity is unlikely, although it can not be completely ruled out. Second, since the tax yields are used to finance scientific research, altruistic behavior may induce individuals to contribute more when the others reduce their contribution. This explanation is also unlikely to explain much of the tax evasion behavior of the participants. A third interpretation is that individuals may reduce their tax evasion whenever the group evades more out of fear this may trigger a higher audit rate in further rounds. This is also unlikely because participants were instructed at the outset that audit regimes are exogenous. Finally, the most likely explanation is that the parameter estimate of $\overline{D}_{-i}$ is biased because the simple two-limit tobit omits the potential simultaneity between individual and group responses. Recall that this bias may arise from the fact that individual and group behavior feed on one another. Moffitt (2001) and Krauth (2006) insist on the potential importance of this bias when the number of individuals in the reference group is small.

As mentioned earlier, two alternative approaches are used to tackle the simultaneity problem. Results in columns (2) and (3) assume that expectations are myopic and that the error terms are not autocorrelated. Under these assumptions, a simple two-limit tobit provides consistent estimators. In column (2), the parameter estimate of $\overline{D}_{-i}$ ($= \overline{D}_{-i}^{\tau-1}$), while still negative, is now much smaller (-0.813 rather than -2.966). Moreover, it is no longer statistically significant at the 5% level, though it remains significant at the 10% level (Student $t = 1.74$). This indicates that the simultaneity problem is potentially important and may partly explain the strong negative coefficient in column (1). The second approach assumes self-consistent expectations and uses a simultaneous two-limit tobit model (see equation (9)). Columns (4) to (6) provide results based on this approach. Note first that the model in column (4) imposes no exclusion restrictions. Identification of the parameter estimates thus rests entirely on the response variables being censored and on the error terms assumed to be normally distributed with zero covariances. The second thing to notice is that the specifications of columns (4)–(6) all satisfy the coherency condition since the principal minors of the matrix $I-\Gamma$ are positive in each case. This implies that there are no multiple equilibria in our experiment. In column (4) the parameter estimate of $\overline{D}_{-i}$ is now close to zero ($=-0.046$) and is no longer statistically significant even at the 10% level (Student $t=0.99$). This result is robust to changes in model specification and indicates that there is no endogenous interaction effects in our experiment, either under the assumption of myopic expectations (at the 5% level) or self-consistent expectations (even at the 10% level). These results are consistent with the hypothesis that when the mean behavior of the group does not affect the individual monetary payoffs, there is no endogenous social interactions associated with tax evasion (Spicer and Hero 1985).

The results allow us to investigate the existence of exogenous interactions since the tax rate and the audit probability are included both at the individual, $(t_i,\, p_i)$, and group, ($\bar{t}_{-i}$, $\overline{p}_{-i}$), levels in all specifications. Because the parameter estimates of $\bar{t}_{-i}$ and $\overline{t^2_{-i}}$ are not individually significant in specification (2) but are jointly significant at the 5% level (χ^2 $=6.37 \sim \chi^2(2, .05)=5.99$), we have excluded $\overline{t^2_{-i}}$ in specification (3) to ease interpretation. The parameter estimate of $\bar{t}_{-i}$ is positive and significant at the 5% level.This lends support to the existence of a fairness effect in terms of horizontal equity because individuals are inclined to report more when the fiscal treatment of their group worsens. Spicer and Becker (1980) have also found that individuals who were told their tax rates were below average reported relatively higher amounts.[19] On the other hand, the parameter estimate of $\overline{p}_{-i}$ is not significantly different from zero in all specifications (except in column (1)) thereby rejecting fairness effects relative to the fraud preventing policy.

According to the parameter estimates of t_i and t_i^2 in column (5), a one percentage point increase in the individual tax rate decreases desired reported income by a small amount ($=-0.0426$) at the mean tax rate ($\bar{t}=0.38$). The estimates also predict that the positive impact occurs only at tax rates above 39.1%. Below that level the negative effect dominates and induces more tax evasion. As discussed in the theoretical model, both positive and negative effects are consistent with our model when tax compliance yields a positive social marginal utility.

Interestingly, experimental results on the impact of tax rates on compliance are not clear cut. Some studies have found that increased tax rates decrease compliance (Friedland *et al* 1978; Collins and Plumlee 1991), while others have found the opposite to hold (Alm *et al.*

[19]In an experiment similar to that of Spicer and Becker (1980), Webley *et al.* (1991) found no such fairness effect.

1995). Studies based on survey data yield similarly contradictory results (see Andreoni *et al.* 1998). Feinstein (1991) and Alm *et al.* (1993) conclude that an increase in the marginal tax rate lowers tax evasion, whereas Clotfelter (1983) obtains a positive elasticity of underreporting with respect to the marginal tax rate.

In all specifications, the parameter estimate of p_i is positive and significant at the 5% level. This result is consistent with the Alingham-Sandmo proposition according to which an increase in the audit probability reduces tax evasion, but also with evidence based on survey data (Friedland *et al.* 1978, Klepper and Nagin 1989, Dubin, Graetz and Wilde 1990, Slemrod *et al.* 2001).

In column (6), the gender variable (Sex_i) and the inequality aversion index ($Avers_i$) are added to the model with self-consistent expectations. [20] As in many studies, women are found to evade less than men (*e.g.,* Spicer and Becker 1980, Baldry 1986). According to the parameter estimate of gender, *ceteris paribus,* females report on average 14.7 more units than males. As for the inequality index, its parameter estimate is positive and significant at the 5% level, thus indicating that those with a high inequality aversion are likely to evade less, *ceteris paribus.* Note however that including the gender variable and the inequality aversion index has little impact on the other parameter estimates of the model (compare columns (5) and (6)).[21] With respect to correlated effects, only one group dummy (g_9) is significant at the 5% level. However, a likelihood ratio test based on column (6) rejects the null assumption that all group dummies are zero. Our results are thus consistent with the presence of correlated effects.[22]

Column (7) reports estimation results where endogenous effects are assumed away. As expected, the fairness effect on taxation is significant at the 5% level and its value is close to the one obtained when expectations are assumed to be myopic (see column (3)).

Finally, column (8) provides results of a simple two-limit tobit using information from the NOINFO treatment (900 observations). Results are qualitatively quite similar to those obtained using the INFO treatment (*e.g.,* compare with columns (6) or (7)) though the coefficients of t_i and t_i^2 change signs. Interestingly, the coefficients associated with the audit probability, the gender and the inequality aversion index are all smaller in the NOINFO treatment. This is consistent with the existence of a social multiplier effect. However it would be premature to draw any strong conclusion from these observations since learning effects

[20] The estimation never converged when we included the group mean gender variable. This variable was thus excluded from the specification. Besides, no other individual characteristic was significant.

[21] We also tested for "dynamic social learning effects" by including dummy variables for each round. None were ever significant at the 5% level.

[22] In the specification where the group dummies are removed, the mean group gender variable ($\overline{sex}_{-i}$) becomes significant at the 5% level. This indicates that gender composition is presumably correlated with group dummies.

in the latter treatment (that was implemented earlier in the experiment)are likely to be much important than in the INFO treatment.

6 Conclusion

Research on tax evasion usually ignores “peer effects” or “social interactions effects”. This omission is due to the fact that testing for such effects is notoriously difficult for two reasons. First, outcomes data rarely reveal the reference group composition, whether it is the family, the neighborhood, or work colleagues. Second, even when the group composition is known, estimating interaction-based models raises severe identification problems.

The identification problem arises from the fact that interdependent behavior takes different forms that are difficult to isolate. In a linear-in-means regression-like model with social interactions, equilibrium outcomes cannot distinguish between endogenous effects from exogenous effects or correlated effects. Furthermore, even when an interactions-based model is identified, its estimation raises serious econometric problems.

In this paper we argue that laboratory experiments can be useful in solving these problems. Reference groups are naturally defined as participants in each particular session. Randomization of participants across groups limits correlated effects and sorting biases. Moreover, group fixed effects can be used to take into account correlated effects. This clearly helps identify the endogenous and exogenous interactions effects. The particular setup of our experiment has an added benefit. Because it generates censored data, it naturally implies a nonlinear relationship between individual and group responses, assuming normality of the error terms. Assuming that the group fixed effects wash away the correlated effects, this nonlinearity allows identification of the model without the need to impose any identifying restrictions.

In line with the recent empirical literature on social interactions, we find that the estimation method is crucial in obtaining consistent estimates of interaction effects. Thus when we assume that individual tax evasion behavior is affected by contemporaneous mean group behavior (self-consistent expectations) but ignore the simultaneity of individual and group responses, we find strong evidence of social anti-conformity effects. This effect is considerably reduced when we assume expectations are based on group mean lagged response (myopic expectations). Moreover, the effect completely disappears under self-consistent expectations once the simultaneity between individual and group responses is taken into account using an appropriate estimation method (two-limit simultaneous tobit).

We also find fairness effects in term of horizontal equity: for a given gross income and a given personal tax rate, the individual will report less when facing a reduction in the mean tax rate of his group. Perceived unfair taxation may thus lead to increased tax evasion. At the policy level this means that a taxation system that is more horizontally equitable is likely to improve tax compliance.

As noted by many (*e.g.,* Manski 2000), experimental research has its own limitations. In our experiment groups are formed artificially for the sake of the experiment. Caution must thus be exercised when extrapolating our findings to the population of taxpayers.

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Appendix A: Instructions

You will be taking part in an experiment on decision-making under the aegis of both Université Laval in Québec and Université Lumi ère Lyon 2. The experiment is designed so that your earnings will depend on your decisions.

The session consists of 10 rounds. The first five rounds involve a single-period, *i.e.* require a single decision. The next five rounds include several periods, with each period requiring one decision. In each round you will receive a score based on your decisions. The average score over entire session will determine your earnings. Scores are converted into Euros at the following rate: 100 experimental currency units = 15 €. In addition, you will receive a show-up fee of 1.5 €. Your earnings will be paid in cash at the end of the session in a separate room to preserve the confidentiality of your earnings.

You will be part of a group of 15 participants from the same school. All your decisions are anonymous. Talking is not allowed throughout the entire session. Any violation of this rule will result in being excluded from the session and not receiving any payment. If you have any question regarding these instructions, please raise your hand; your question will be answered publicly.

ROUNDS 1 to 5

Each round consists of a single period. At the beginning of each round, each participant receives an endowment of 100 experimental currency units . You are requested to give back a percentage of this endowment according to a "rate of deduction". There are 5 different rates and each of these is randomly assigned to 3 participants. The sum of the deductions from the group members serves to finance scientific projects.

The rate of reduction will be applied to the amount you report. You must use the scrollbar to report any amount between 0 and 100 (100 corresponding to the endowment that you received).

This amount can be audited according to a certain audit probability and this audit may entail a penalty. There are 5 different audit probabilities and each of these is randomly assigned to 3 participants. The consequences of an audit are indicated below. There are 3 possible cases.

- If the amount you report is not audited, your deduction rate will apply to your reported amount. In this case, no penalty applies. Your payoff is given by the following formula:

$$\begin{aligned} \text{Payoff} &= \text{Endowment - Deduction} \\ \text{Deduction} &= \text{Deduction rate} \times \text{Reported amount} \end{aligned}$$

- If the amount you report is audited and is equal to your endowment, your deduction rate applies to this amount and consequently no penalty applies. Your payoff is given by the following formula:

$$\begin{aligned} \text{Payoff} &= \text{Endowment - Deduction} \\ \text{Deduction} &= \text{Deduction rate} \times \text{Endowment} \end{aligned}$$

- If the amount you report is audited and is less than your endowment, your deduction rate applies to your endowment. In addition, you will be charged a penalty equal to your deduction rate times the non reported fraction of your endowment. Your payoff is then equivalent to:

$$
\begin{aligned}
\text{Payoff} &= \text{Endowment - Deduction - Penalty} \\
\text{Deduction} &= \text{Deduction rate} \times \text{Endowment} \\
\text{Penalty} &= \text{Deduction rate} \times \text{Endowment - Reported amount}
\end{aligned}
$$

What information do you receive at the beginning of each round?

At the beginning of each round, you are informed about the following:

-
- the 5 different deduction rates.
- your own deduction rate.
- the 5 different audit probabilities.
- your own audit probability.

On your computer screen you will find a scrollbar ranging from 0 to 100 which you must use to indicate the amount you wish to report. As you move the scrollbar, you will see both your payoffs if audited or not. To validate your decision, you must stop the scrollbar on the desired amount and then click the ”OK” button. Once all the participants have clicked the ”OK” button, the next round will begin automatically.

You will be informed about the following at the end of the session only:

-
- the actual audit of your reported amounts and the number of the rounds in which an audit actually took place.
- the payment of a penalty when applicable.
- your payoff.

What changes from one round to the next ?

Each round is independent from the previous ones. At the beginning of each new round, you will receive a new endowment of 100 experimental currency units. Likewise, new deduction rates and audit probabilities will be assigned randomly to participants.

[THE FOLLOWING INSTRUCTIONS WERE DISTRIBUTED TO THE PARTICIPANTS ONLY AFTER ROUND 5 WAS COMPLETED]

ROUNDS 6 to 10

The session will continue in a moment, but with two changes however.

1. From now on, each round consists of several periods.

   At the beginning of each new period of a round, you will receive an endowment of 100 experimental currency units. Everyone keeps the same deduction rate and audit probability for all the periods of a given round. When a new round begins, new deduction rates and audit probabilities will be assigned randomly to all participants, including you.

2. As of the second period of a round, and for each successive periods, you will be given two additional pieces of information:

   -
   - how many participants among the other 14 participants have reported less than their endowment in the preceding period.
   - the average amount reported by the other 14 participants in the preceding period.

Appendix B: Values of tax rates and audit probabilities

The values of the tax rates and the audit probabilities used in each session are the following:

SESSIONS 1 TO 4

Distribution of the audit probabilities

Regime	Individual Probability					Mean	Standard Deviation
Low	0.04	0.08	0.11	0.33	0.37	0.18	12.2
Medium	0.07	0.22	0.27	0.32	0.37	0.25	10.3
High	0.24	0.27	0.33	0.37	0.43	0.33	6.2

Distribution of the tax rates

Regime	Individual tax rates					Mean	Standard Deviation
Low	0.05	0.10	0.15	0.30	0.70	0.26	23.5
Medium	0.20	0.35	0.40	0.50	0.55	0.40	12
High	0.40	0.45	0.50	0.55	0.60	0.50	7

SESSIONS 5 TO 8

Distribution of the audit probabilities

Regime	Individual Probability					Mean	Standard Deviation
Low	0.08	0.12	0.15	0.37	0.41	0.23	13.6
Medium	0.13	0.27	0.32	0.37	0.42	0.30	9.95
High	0.28	0.30	0.37	0.40	0.47	0.36	6.89

Distribution of the tax rates

Regime	Individual tax rates					Mean	Standard Deviation
Low	0.10	0.15	0.20	0.35	0.75	0.31	23.54
Medium	0.25	0.40	0.45	0.55	0.60	0.45	12.25
High	0.45	0.50	0.55	0.60	0.65	0.55	7.07

SESSIONS 9 TO 12

Distribution of the audit probabilities

Regime	Individual Probability					Mean	Standard Deviation
Low	0.02	0.04	0.07	0.29	0.33	0.15	13.22
Medium	0.03	0.18	0.23	0.28	0.33	0.21	10.29
High	0.20	0.23	0.29	0.33	0.40	0.29	7.13

Distribution of the tax rates

Regime	Individual tax rates					Mean	Standard Deviation
Low	0.05	0.10	0.15	0.25	0.65	0.24	21.54
Medium	0.15	0.30	0.35	0.45	0.50	0.35	12.25
High	0.35	0.40	0.45	0.50	0.55	0.45	7.07

Table 1
Descriptive Statistics

	Mean	Stand. Dev.	Min	Max
Amount reported in Part I	53.92	37.54	0	100
Amount reported in Part II	50.15	38.68	0	100
Age	23.61	5.94	17	50
Tax rate	0.38	0.16	0.05	0.75
Audit probability	0.25	0.11	0.02	0.47
Sex (Female=1)	0.40	0.49	0	1
Inequality aversion index	1.33	0.84	0	2
	Number of observations			
Groups	12			
Rounds Part I + Part II	120			
Rounds that converged in Part II	53			
Participants per group	15			
Observations on amount reported in Part I	900			
Observations on amount reported in Part II*	795			
- Censored at 0 in Part I (Part II)	164 (195)			
- Censored at 100 in Part I (Part II)	189 (151)			
- Not censored in Part I (Part II)	547 (449)			

∗ Observations are limited to games that converged.

Table 2
Estimation Results of the Tax Compliance Model
Dependent variable: Reported Individual Income ($D_i/100$)

	(1) Two-Limit Tobit Group: $\bar{D}^{\tau}_{-i}$		(2) Two-Limit Tobit Group: $\bar{D}^{\tau-1}_{-i}$		(3) Two-Limit Tobit Group: $\bar{D}^{\tau-1}_{-i}$		(4) Simultaneous Tobit Group: $\bar{D}^{\tau}_{-i}$	
	Para.	Std. Error	Para.	Std. Error	Para.	Std. Error	Para.	Std. Error
Intercept	0.692	0.529	0.039	0.461	0.344	0.202	-0.021	0.405
t_i	-0.969	0.782	-1.348	0.792	-1.139	0.725	-1.524	0.701
t_i^2	1.311	1.017	1.797	1.026	1.505	0.923	1.955	0.907
p_i	1.780	0.224	1.802	0.235	1.808	0.231	1.914	0.210
$\bar{t}_{-i}$	1.576	2.930	2.871	2.710	1.046	0.433	1.248	2.261
$\bar{t}^2_{-i}$	-0.393	3.683	-2.321	3.404			-0.747	2.836
$\bar{p}_{-i}$	2.479	0.683	0.420	0.834	0.447	0.700	-0.304	0.524
g_1	0.455	0.134	0.199	0.139	0.202	0.138	0.117	0.116
g_2	0.099	0.120	0.010	0.124	0.015	0.122	0.012	0.110
g_3	0.094	0.127	0.008	0.132	0.011	0.131	0.000	0.117
g_4	-0.061	0.133	0.061	0.137	0.068	0.135	-0.077	0.121
g_5	-0.148	0.131	-0.143	0.136	-0.143	0.135	-0.112	0.122
g_6	-0.636	0.140	-0.397	0.151	-0.397	0.144	-0.285	0.123
g_7	-0.413	0.134	-0.272	0.142	-0.273	0.138	-0.222	0.122
g_8	-0.014	0.150	-0.091	0.155	-0.097	0.154	-0.033	0.138
g_9	0.634	0.126	0.343	0.130	0.344	0.129	0.231	0.104
g_{10}	0.283	0.120	0.190	0.124	0.186	0.123	0.086	0.110
g_{11}	0.487	0.115	0.322	0.117	0.322	0.117	0.144	0.102
$\bar{D}_{-i}$	-2.966	0.407	-0.813	0.468	-0.817	0.414	-0.046	0.046
σ	0.630	0.024	0.651	0.024	0.651	0.024	0.588	0.022
Log-Lik	-745.556		-770.544		-770.809		-727.665	
Obs.	795		795		795		795	

Table 2
(continued)

	(5) Simultaneous Tobit Group: $\bar{D}^{\tau}_{-i}$		(6) Simultaneous Tobit Group: $\bar{D}^{\tau}_{-i}$		(7) Two-Limit Tobit Group: Nil		(8) Two-Limit Tobit (NOINFO) Group: Nil	
	Para.	Std. Error	Para.	Std. Error	Para.	Std. Error	Para.	Std. Error
Intercept	0.076	0.185	-0.092	0.341	-0.164	5.368	-0.204	1.213
t_i	-1.457	0.627	-1.529	0.643	-1.306	0.723	1.294	0.560
t_i^2	1.861	0.806	1.899	0.821	1.666	0.921	-1.578	0.709
p_i	1.915	0.212	1.913	0.207	1.835	0.230	1.597	0.173
$\bar{t}_{-i}$	0.661	0.403	0.727	0.385	1.005	0.429	-0.193	0.319
$\bar{p}_{-i}$	-0.297	0.660	-0.300	0.530	-0.324	0.577	0.165	0.452
g_1	0.118	0.128	0.117	0.116	0.088	0.298	0.222	0.111
g_2	0.014	0.103	0.041	0.108	0.001	0.121	0.121	0.097
g_3	0.001	0.116	-0.007	0.116	-0.021	0.551	0.220	0.159
g_4	-0.075	0.119	-0.100	0.149	0.007	1.616	0.092	0.377
g_5	-0.112	0.118	-0.058	0.122	-0.113	0.136	0.059	0.109
g_6	-0.285	0.130	-0.212	0.123	-0.228	0.567	0.182	0.178
g_7	-0.222	0.122	-0.197	0.141	-0.250	1.083	0.060	0.258
g_8	-0.035	0.129	-0.007	0.138	-0.091	0.556	0.155	0.172
g_9	0.231	0.109	0.226	0.103	0.240	0.290	0.165	0.112
g_{10}	0.085	0.103	0.117	0.120	0.214	1.078	0.421	0.266
g_{11}	0.145	0.104	0.109	0.102	0.232	0.292	0.329	0.110
Sex_i(1=female)			0.147	0.049	0.105	0.056	0.098	0.042
$Avers_i$			0.082	0.030	0.088	0.270	0.046	0.065
$\overline{Avers}_{-i}$			-0.013	0.188	0.144	3.756	0.144	0.865
$\bar{D}_{-i}$	-0.046	0.389	-0.045	0.046				
σ	0.588	0.023	0.578	0.022	0.648	0.024	0.531	0.023
Log-Lik	-727.698		-717.043		-766.723		-676.465	
Obs.	795		795		795		900	