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The Bomb-Crater Effect of Tax Audits: Beyond Misperception of Chance*

Luigi Mittone[†] Fabrizio Panebianco[‡] Alessandro Santoro[§]

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Abstract

In this paper, we run a laboratory experiment where the information set is relatively rich, and, in particular, it includes audits on other taxpayers. At the same time, the implementation of the Bayesian updating process for the subjective probability to be audited is fairly simple. By doing so, we are able to elicit a range of consistent but heterogeneous probability beliefs and to distinguish between Bayesian and non-Bayesian subjects. We obtain two major results concerning Bayesian subjects. First, they exhibit strong and robust short-run BoCE. Second, they are seemingly not affected by audits on other taxpayers in their compliance decision. These results are robust to different definitions of Bayesianity and to different specifications. They conflict with the evidence that Bayesian agents do perceive correctly the chance to be audited. In turn, this suggests that existing explanations of the BoCE are not entirely satisfactory and that alternative theories, possibly based on the Duality approach, are needed.

KEYWORDS: Bomb-crater effect, Bayesian Updating, Behavioral Duality.
JEL CODES: C91,D81,H26

1 Introduction

Theoretical literature analyzing both tax compliance and audit probability has generally modeled this relationship as straightforward. The first neoclassical theoretical attempt to model the taxpayers' behavior goes back to the 1972 seminal paper by Allingham and Sandmo (1972) (AS72). The AS72 is an exercise of applied Expected Utility Maximization (EUT) (Von Neumann and Morgenstern, 1947), where the taxpayer is assumed to behave taking into account only three parameters: the probability to be audited, the amount of the fine to pay and the amount of taxes due. The decision maker knows these three ingredients, and she behaves increasing compliance with the audit probability. In spite of the relatively large debate that followed the AS72 paper, no revolutionary improvement to the theoretical frame has been subsequently added to the AS72 model until recently. Among few exceptions we signal Bernasconi and Zanardi (2004), Dhami and Al-Nowaihi (2007), Piolatto and Rablen (2013), and Piolatto and Trotin (2016). All these papers adopt a Prospect Theory (PT) approach (Kahneman and Tversky, 1979). In turn, EUT and PT approaches, along with models based on social interactions, tax morale and other social customs cover the wide range of theoretical approaches to the study of tax compliance (Hashimzade et al., 2013).

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According to both EUT and PT frameworks, one would expect that compliance is increasing in the probability to be audited. However, to verify this claim one needs to define and to measure the probability to be audited as *perceived* by the taxpayer. It is immediate to conjecture that this perception is primarily influenced by prior audits. As a matter of fact, the link between prior audits and compliance has been a subject of a large body of experimental studies.

As documented byMaciejovsky et al. (2007), the traditional experimental literature (e.g Spicer and Hero, 1985; Webley, 1987) shows that compliance is increasing in personal experience with being audited. A possible explanation is the so-called *availability-heuristic effect* (Tversky and Kahneman, 1975) which can be defined as the increase that prior audits generate on the subjective salience of audits and punishments, which lead to more compliance in the future. An alternative interpretation of the positive link between prior audits and subsequent tax compliance hinges on the update of subjective probability and it has been defined as the *target effect* i.e. the belief that an audit is likely to be followed by another one in the future (Hashimzade et al., 2013).

Looking more closely at the dynamics of tax evasion, more recent experimental literature has identified the *Bomb Crater Effect, BoCE*. The BoCE, first introduced by Mittone (2006), refers to the fact that tax compliance drops immediately after a taxpayer is audited. In war, troops under heavy enemy fire hid in the craters of recent explosions, believing that it would be highly unlikely for the next bombs to fall exactly at the same spot in a short time span. Something similar seems to happen in the context of tax audits. In the realm of lab experiments, the bomb-crater effect has been confirmed in various studies and has been tested through replicas of the original experiment in different countries (e.g. Garrido and Mittone, 2013) and modifying the sequences of audits to check its robustness (Kastlunger et al., 2009), and in field experiments (DeBacker et al., 2015).

From a theoretical perspective, the BoCE has been explained either by the *misperception of chance,* also known as gambler's fallacy, or by the *loss repair effect* (Maciejovsky et al., 2007) . In the former case, individuals assume that a random event, such as an audit, is more likely to occur because it has not happened for a while or it is less likely to occur because it recently happened. In the latter, taxpayers experiencing audits might try to repair their losses by engaging in tax evasion in subsequent filings. The former explanation *of the BoCE* is the prevailing one (Maciejovsky et al., 2007; Kastlunger et al., 2009).

The aim of this paper is to look more closely at the nature of the misperception of chance which may generate the BoCE. The primary source of this misperception is the inability to correctly compute the probability to be audited. In a dynamic setting, the natural reference is the Bayesian updating process. As Hashimzade et al. (2013) suggest, the BoCE, in opposition to the target effect, can be seen as a failure to use all the available information to update probability according to the Bayes' rule. Thus, when we observe BoCE, this may just reflect a given distribution of 'Bayesianity' among the subjects. In this paper we investigate whether the BoCE is due to this source of heterogeneity.

To perform our analysis, we run a laboratory experiment where the information set is relatively rich but, at the same time, the implementation of the Bayesian updating process is fairly simple. Taxpayers can collect information on audit probability from various sources, as stressed by Alm et al. (2009). In particular, this information includes the personal track of experiences, meaning when and how many audits have been directly experienced by the taxpayer herself, and the number of audits carried by the Tax Authority on the rest of the population of the taxpayers, or on a subset of it. We define the former as *own* audits and the latter as *others* audits and allow subjects to the experiment to be informed about the occurrences of both, according to different network structures.

Despite the presence of these multiple sources of information, by eliciting probability beliefs we are able to show that a significant number of the subjects do follow a Bayesian updating process, whilst the majority do not, so that we can capture the heterogeneity in Bayesianity among taxpayers and we can analyze subsets of observations and subjects characterized by different levels of Bayesianity. When we observe compliance in the whole population, a strong BoCE emerges with respect to own audits. Since there is no clear loss repair effect, one would expect that misperception of chance explains the BoCE. In turn, this would imply that the BoCE is originated by the Non-Bayesian taxpayers only. On the contrary, we find that the short-run BoCE is very strong also among Bayesian-subjects. Moreover, 'others' audits have no impact on Bayesian subjects. These are striking results since the information on 'own' and 'others' audits is seemingly used in the correct way by Bayesian subjects during the beliefs updating process. These results seem to imply that, contrary to what is commonly believed BoCE cannot be explained exclusively on the grounds of misperception of chance. Additional explanations are requires, and we suggest, as a possible explanation, the possibility of a sort of 'duality' in behavior *by Bayesian agents*: the probability to be audited is correctly computed, using all of the relevant information in the correct way, but, when the agent has to decide whether and how much to comply, she is driven only by the emotions which are triggered exclusively by own audits. We also indicate directions for future research.

The paper is organized as follows. Section 2 presents a simple measure of Bayesianity consistent with our framework, which enables us to perform our analysis. Section 3 describes the experimental design. Section 4 provides some descriptive statistics, while Sections 5 and 6 report, respectively, the results and their discussion. Section 7 ends with concluding remarks and directions for future research.

2 A Measure of Bayesianity

In this section we build a measure of Bayesianity to classify players and single observation with respect to their ability to correctly update subjective probability of being audited, given the information they receive. We consider a setting where each taxpayer:

1. Can observe only a subsample of other taxpayers and can get information about whether these taxpayers have been audited by the Tax Authority, but not about the outcome of the audit process;

2. Knows that, for every tax period, each taxpayer has the same probability of being audited and that this probability is the same for all the periods. In this way the taxpayer has the possibility to update at each period her subjective probability of being audited.

Feature 1 is a very realistic one, since there are many contexts in which the occurrence of an audit on other taxpayers can be observed, but its outcome is unobservable (Battiston et al., 2016). Feature 2 is relatively more abstract, since in the real-world Tax Authorities rarely use random audits, though common when lab experiments are conducted. It mainly serves our purpose to build a benchmark process of updating beliefs.

Our benchmark is a Bayesian taxpayer, i.e. a taxpayer who updates subjective probability using Bayes rule. In our setting, since audits are iid, the update is simple. At each point in time, each agent computes the subjective probability by considering the full past history, giving to each period the same weight, and giving the same weight to each audit, regardless whether it is of 'own' or 'others' type. In what follows we build the Bayesian subjective probability measure.

Call p_t^i the subjective probability of taxpayer i at time t, A_t^i a variable taking value 1 if the taxpayer i has been audited at time t and 0 otherwise. Call then N the set of all taxpayers (whose size n is known to each of them), N^i the set of i's neighbors and n^i the cardinality of N^i. Then, for each $j \in N^i$, A_t^j is a variable taking value 1 if the taxpayer j has been audited at time t and 0 otherwise. In what follows we build the subjective probability computed by a Bayesian taxpayer. We first define the following probability:

$$\tilde{p}_T^i \equiv \sum_{t=1}^{T-1} [\frac{1}{T-1} \cdot \frac{1}{n^i+1}(A_t^i + \sum_{j \in N^i} A_t^j)] \tag{1}$$

When at time T, each taxpayer has information about the $T-1$ periods before, so that she gives a weight $\frac{1}{T-1}$ to the information received in each period. In each period she receives n^i+1 pieces of information about audits: one for each of the n^i neighbors plus own audit experience. This means that the weight associated to each piece of information is $\frac{1}{n^i+1}$. As T increases, the weight associated to each piece of information decreases. However, it may be the case that this subjective probability, once computed, is lower than the highest number of audits ever observed in own neighborhood by the taxpayer.[1] This lower bound is given by:

$$\bar{p}_T^i \equiv \max_{t<T} \{ \frac{1}{n^i+1}(A_t^i + \sum_{j \in N^i} A_t^j)\} \tag{2}$$

Then, a Bayesian taxpayer knows that the number of audits is fixed along the rounds, so that it takes the maximum between $\tilde{p}_T^i$ and $\bar{p}_T^i$. So, call B_T^i the subjective probability computed by the Bayesian taxpayer. Then:

$$B_T^i = \max\{\tilde{p}_T^i, \bar{p}_T^i\} \tag{3}$$

In the long run a Bayesian taxpayer's subjective probability converges to the true probability.[2] It is crucial to understand which is the effect of an additional audit on B_T^i. Clearly, if $\tilde{p}_T^i > \bar{p}_T^i$, so that $B_T^i = \tilde{p}_T^i$, an additional audit has the effect of increasing B_T^i. Notice, however, that this is not always the case. If $\tilde{p}_T^i < \bar{p}_T^i$ then B_T^i does not change. In this case $B_T^i = \bar{p}_T^i$. An additional audit, while increasing $\tilde{p}_T^i$ may not be sufficient to make it larger than the lower bound, so that B_T^i is not affected by the additional control. In this sense we can say that, for a Bayesian agent, the effect of an additional audit on the subjective probability is non-negative.[3] This updating process is our benchmark hypothesis.

[1] Since audits are random, it can happen that in some period a large number of audits concentrate in i' neighborhood. Then, whatever $\tilde{p}_T^i$ a Bayesian taxpayer knows that the number of audits has to be at least as large than this number of audits.

[2] We highlight the fact that we assume agents to be bayesian if they display a subjective probability equal to the empirical probability computed just by looking at the distribution of audits in all the previous rounds. However, a Bayesian agent who starts with a belief about audit probability, cannot find any finite sequence of observations that can invalidate this belief.

[3] Note that we refer to the levels of subjective probability that, according to (3) is increasing on the number of audits. However, an additional audit can have a negative impact on the difference in probabilities observed in two subsequent periods. This can be verified by calculating first differences from equation (1).

Thus, abstracting aways from other individual characteristics, a Bayesian agent should never increase evasion as the number of audits she observes, 'own' or 'others', increases. To put it alternatively, a Bayesian agent should never display the BoCE. As for 'own' audits, this remark is consistent with Hashimzade et al. (2013)'s that a target effect is qualitatively similar to a Bayesian process of beliefs updating, so that the BoCE should arise only for non-Bayesian subjects. The role of 'others' audits is more subtle, though, since also the misperception of chance may lead to a positive impact of 'others' audits on compliance. This may happen if a sort of reverse bomb-crater is generated, so that the taxpayer expects other taxpayers not to be audited after they have been audited in the observed round.

To measure how taxpayers relate to the benchmark, we create an index of non-Bayesianity:

$$NB_t^i \equiv |B_T^i - p_T^i| \tag{4}$$

The minimum value of NB_t^i is 0, when, in the given round, the taxpayer reports a probability level which is exactly equal to the Bayesian one. The maximum value is 1 when the Bayesian probability is 0 (or 1) and the reported subjective probability is 1 (or 0). The lower NB_t^i, the more we say that the taxpayer conforms to the benchmark in the given round. We also talk about this specific observation as a Bayesian observation.[4]

Generally speaking, when a taxpayer, in a given round, behaves differently than the Bayesian one, and thus has a high NB_t^i, own and other audits could have a different impact on p_t^i. A Bayesian taxpayer has $NB_t^i = 0$. Note, however, that a non-Bayesian taxpayer may appear as a Bayesian when only the information about her subjective probability is observed. In particular, a non-Bayesian taxpayer may reshape and weight differently the information from 'own' and 'others' audits in the subject probability formation still getting $NB_t^i = 0$.[5]

3 Experimental Design

The experimental design was purported to reproduce in the most effective way the setting described in previous Section. Thus, it has two distinctive features. First, along the rounds of the experiment, subjects received information about 'own' and 'others' audits. Second, subjects were asked to report their subjective probability of being audited to keep tracks of their beliefs updating process.

The experiment was conducted in the Cognitive and Experimental Economics Laboratory at Trento

[4]We consider also more complex indices that control for the variance of the signal received, but all the results are unchanged.

[5]This process can be explained by using a simple algebraic argument. Assume that some taxpayers elaborate separately the information about own and others' audits and derive the following probabilities: $p^i_{t,Own}$ and $p^i_{t,others}$, the former being the audit probability a Bayesian subject would infer if has access to only own audits observation, while the latter is the probability computed by a Bayesian subject considering only the information about others' audits. Then, (1) can be rewritten as

$$\bar{p}_t^i = \alpha_t^i p^i_{t,Own} + (1 - \alpha_t^i) p^i_{t,others} \tag{5}$$

with an appropriate $\alpha_t^i \in (0, 1)$.[6] Notice now that in the data we elicit the subjective probability p_t^i and we compare it with $\bar{p}_t^i$ not observing $p^i_{t,Own}$ and $p^i_{t,others}$. It is just a matter of simple algebra to see that the elicited p_t^i can coincide with the Bayesian $\bar{p}_t^i$ for two reasons: i) the subject correctly computed $p^i_{t,Own}$ and $p^i_{t,others}$ and correctly weighted them; ii) the subject may bias the computation of $p^i_{t,Own}$ and $p^i_{t,others}$, bias the weight parameter and still get close to the Bayesian $\bar{p}_t^i$. We shall return to this point when discussing our results.

University. Overall 144 students (81 males and 63 females) with a mean age of 22.22 (SD=2.563) participated. In all experimental conditions subjects were students predominantly from social sciences.

Subjects had to file taxes in 30 consecutive rounds. Their regular income was 1000 ECU (Experimental Currency Units) per round and the tax liability was 400 ECU (corresponding to a tax rate of 40%). Subjects were explicitly informed that the number of rounds was unknown to them, that the number of audits at each period was determined at the first round and kept fixed for all the periods and that this was not communicated to them.

Moreover subjects were informed that the audits were random, and that the distribution of audits was independent at each round, i.e. it was not dependent on the outcome of audits conducted in the previous round. To be clear we make it explicit that the probability of being audited was the same for every subject and constant along the experiment and that the number of audits was fixed once for all at the beginning of the experiment[7] The actual understanding of this crucial point was checked by submitting a specific question before the beginning of the experiment.[8]

At each round subjects decided the amount of taxes to pay and communicate their belief about the number of audits implemented by the tax authority (subjective probability elicitation, on which we shall return briefly). In case of detection, evaded taxes had to be refunded and in addition the same amount had to be paid as a fine. Thus, evading 100 ECU and being caught would result in payment of 200 ECU. At the end of each round subjects were informed about own audit and its result, i.e. the sanction they had to pay in case they were caught evading.

Moreover, subjects were informed that they would receive information about the number of audits conducted on other 5 taxpayers, but not about the outcome of these audits. Subjects were also informed that the 5 taxpayers were the same along the experiment and that this information was reciprocal but exclusive, i.e. that each of the members of the network (in the instructions we define them as "taxpayers to which you are connected") could observe the occurrence of an audit on other members of their network only. We also made clear that no one receives information about the whole set of subjects playing.

We designed three different types of networks allowing for the possibility that information about others' audit is processed differently depending on whether neighbors are 'close', at an 'intermediate' distance, or 'far' from the taxpayer who receives the information. These are reported in Figure 2 in the Appendix, in which the red star represent the position of the subject within the network. On the one hand, a perfectly rational taxpayer should be indifferent to this distance, since every 'other' audit should have the same impact on the update of her probability beliefs. On the other hand, a non-Bayesian taxpayer may attach higher weight to an audit conducted on a "closer" taxpayer since she may interpret it as the sign of the tax Authority "getting closer". *However, this really rests on our ability to reproduce the spatial closeness in a convincing way in the lab.* [9].

Subjects were informed that, at the end of the session, two of the periods would be randomly drawn,

[7]See Instructions in Appendix

[8]The question was formulated as follows: Can the Tax Authority change the number of audits from round to round?

[9]Of course we could easily imagine a more sophisticated granularity of the information regarding the audits carried on other taxpayers. For example audits carried on taxpayers who are more or less close could be evaluated in different manners. Furthermore the same definition of closeness could play a role in improving the information gathered; we can imagine a spatial closeness (all the taxpayers of your neighborhood are being audited in these days), or a qualitative closeness (all the taxpayers of you kind are getting into some form of control by the tax Authority (e.g. you are a lawyer and all the lawyers are getting investigated in the last weeks) or other kinds of proximity/similarity. In the experiment, we will be looking the role of spatial closeness only.

and they would be remunerated with respect to the payoff in this respective round according to a fixed exchange rate of 1000 ECU = 8 Euro.

Let us now explain how probability beliefs were elicited.

The focus of our experiment is on the ability to update the probability to be audited. In general, one can assume that each taxpayer computes the subjective probability of being audited and decides consequently how much taxes to pay according to the scheme in Figure 1.

[FIGURE 1 ABOUT HERE]

This process corresponds to our treatment 1. In details, in treatment 1 at each round we elicited the subjective probability, subjects choose the tax to pay and then are given the information about audits. Thus, beginning from round 2 onwards, the subjective probability is declared immediately after having received the information about audits and the compliance decision follows, exactly as depicted in Figure 1. However, we wanted to avoid to frame the process of forming beliefs in a specific way. Thus, in treatment 2 subjects first chose the tax to pay, then the subjective probability was elicited and, at the end, the information about audits have been provided. In this case, from round 2 onward, the decision of compliance is placed in between the information about audits and the elicitation of subjective probability.

For both treatments, the probability belief was elicited by asking the following question: "You must indicate how many audits, in your opinion, the Tax Authority has carried out in the round just played, writing a number from 0 to 24 (maximum number of subjects). At the end of the experiment we will assign a prize of 250 UMS to the one who will guess for the greater amount of times the correct number of tax audits actually carried on in each round. In case of *ex-aequo* winners the 250 UMS prize will be assigned to each one of them."[10]

It is important to note that, by formulating the question in this way, each subject was requested to guess the number of audits that would be carried on the whole population of subjects, and was *not* asked if she would be personally audited in the active round. This is a crucial point for the interpretation of our results.

4 Some Descriptives

In our dataset there are 144 subjects, i.e. 72 for each treatment, observed for 30 rounds, for a total of 4,320 observations. The average value of evasion amounts to 166.5 euros, i.e. approximately 41.6% of due taxes.

Although this value may appear unrealistically high, consider that the actual number of audits is fixed and set to 6 for all the rounds in every session, so that the true probability is equal to 6/24 or 25%. On the other hand, the sanction rate is equal to 100% of amount evaded so that a risk-neutral perfectly informed taxpayer should evade all of her income. As it is customary in lab experiments, we observe some taxpayers, 15, who are honest by definition (i.e they never evade) and some others, 7, who are total evaders (i.e they always evade all of their income).

To distinguish between short and long-term effects, we use two pairs of variables to measure both 'own' and 'others' audits: *Own Audit Stock* and *Own Audit Lag* for the former and *Others Audit Stock*

[10] See instructions in Appendix

and *Others Audit Lag* for the latter. Given round t, *Lag* variables capture own and others audit in $t-1$, while *Stock* variables consider the total number of audits from round 1 to round $t-2$.

As discussed before, the impact of 'others' audits, can change as the type of network changes accordingly to Figure 2, so we construct the variable *Neigh.Type* to take this into account.

[TABLE 1 HERE]

Clearly, the perceived probability to be audited is not the only variable on which tax compliance depends. To take into account all possible determinants of compliance a wide range of additional variables should be considered (Hashimzade et al., 2013). Among them, the tax rate, the degree of risk aversion, the operation of tax consultants, the sector and the size of the economic activity, the level of tax morale and the perceived fairness of the whole tax system. We can observe some proxies of them, such as gender and wealth, which are usually associated with risk aversion. In particular, we expect evasion to be higher among males, as well as among richer subjects.

Moreover, we can control for the 'loss repair' effect, by keeping tracks of the stock of sanctions (*Sanctions Stock*) and of the amount of sanctions raised in previous round (*Sanctions Lag*). On the other hand, we are not able to replicate many relevant features of the real-world such as the presence of a tax consultant or the kind of economic activity. Most importantly, we are not able to measure neither tax morale nor proxies of the perception of fairness of the tax system. We shall take these limitations into account when we choose the empirical approach to analyse the data.

[TABLE 2 HERE]

5 Results and Discussion

To properly address the research question we set out at the beginning of the paper we need to replicate a meaningful distribution in Bayesianity levels. In particular, we need to show that we were able to generate consistent but also heterogeneous probability beliefs. After showing this, we can turn to the analysis of the effect of prior 'own' and 'others' audits on subsequent compliance.

5.1 The Heterogeneity in Bayesianity

The reported probability range is from 0 (i.e. the subject expected the Tax Authority not to perform any control) to 1 (i.e. the subject expected the Tax Authority to control everyone). The average value of audits expected by agents is 7.57, so that subjective probability observed in the data is 31.5% which, in relative terms, is higher than the true one, 24% by approximately one-third.

We measure Bayesianity using two variables

- NB index, defined for every observation and calculated as in equation 4;
- *Meanratio*, defined for every subject as the mean of her NB values over the entire experiment.

Some of the relevant values of these two distributions are reported in Table 3

[TABLE 3 HERE]

After ranking observations or agents in ascending order of NB or Meanratio, respectively, we provide two definitions of Bayesianity:

- *Restrictive Bayesianity* so that an observation or a subject is defined as Bayesian if the corresponding value of NB or Meanratio belongs to the first quartile of the distribution;

- *Median Bayesianity*, so that an observation or a subject is defined as Bayesian if the corresponding value of NB or Meanratio belongs to the lowest 50% of the distribution.

Thus, according to the *Restrictive* definition, an observation is Bayesian if the corresponding value of NB is below 3% while according to the *Median* definition an observation is Bayesian if the corresponding value of NB is below 6.9%. On the other hand, a subject is defined Bayesian if her value of Meanratio is below 5.2% or if its below 8.1% according to the *Restrictive* or to the *Median* definition, respectively. Considering that the mean value of both variables is equal to 11.2% all of these definitions seem to allow, on the one hand, to discriminate to a good extent between Bayesian and non-Bayesian behaviors and, on the other hand, to allow for meaningful regressions within every subset. Figure 3 provides a graphical comparison of four representative subjects.

[FIGURE 4 HERE]

The most Bayesian agent of the sample has an average value of NB which is very close to 0, i.e. 1.1%. Indeed, her subjective probability, shown by the solid line, follows very closely the Bayesian one, also when the latter varies[11], i.e. until round 18th.

If the restrictive definition is adopted, the least Bayesian agent is the one having a value of Meanratio equal to 5.2%. She also displays a quite remarkable ability to update correctly the probability to be audited, although she also shows some erratic behavior in the first and last rounds. The median agent, who belongs to the non-Bayesian subgroup if the restrictive definition is adopted, while is the last Bayesian agent according to the *median definition* is able to capture, to some extent, the trend of the probability, but is very erratic in behavior. Finally, the least Bayesian agent, for which the probability is almost constant, clearly randomizes when she reports the subjective probability to be audited.

We need to verify that Bayesian subjects actually do compute probabilities in the way predicted, i.e. that the probability they report is non-negatively correlated with both 'own' and 'others'.

[TABLE 4 HERE]
[TABLE 5 HERE]

For Bayesian subjects, correlations are significant and of the correct sign for 'own' and 'others' audits in previous round, as well as for the stock of 'own' audits, for both definitions. The only anomaly is the negative and significant correlation with the stock of 'others' audits.

For non-Bayesian taxpayers, on the other hand, correlations are always non significant or have the sign opposite to that predicted by the Bayesian updating process, with the only exception of the correlation with 'own' audits in the short run.

[11] This is an important remark since the value of NB may be low simply because the variance of the signal is low, i.e. there are too few observable audits. More in general, when we adjust NB by the variance in the signal the rank of subjects does not vary greatly and results are unchanged.

To sum up, there is a subset of subjects who are seemingly able to perceive quite correctly the probability to be audited on the basis of the information available. On the other hand, the majority of subjects do misperceive the chance to be audited.

5.2 The impact of own and others audits

We start our analysis by regressing, for the whole sample, evasion on audits and on variables introduced in Section 4, using both a random and a fixed effects model.

Results from both models (Table 6) indicate that:

a. 'Own' audits have an impact which is consistent with BoCE, i.e. they *increase* evasion in the short run (positive and significant coefficient of *Own Audit Lag*) and *decrease* evasion in the longer run (negative and significant coefficient of *Own Audit Stock*) ;

b. An increase in the number of 'others' audits tends to *reduce* evasion in the short as well as in the long run, but this effect is not or only weakly significant;

c. The type of network is significant only with p-value of 10% with a negative sign, but even this slight significance disappears when it is interacted with the variable *Others Audit Lag*[12].

Note also that the 'loss repair effect' is visible at this level in the random effects model, where coefficient of both Sanctions variable are positive and significant (p-value 1%), while it is less evident in the fixed effects specification, where the coefficient of SanctionsStock is not significant and that on SanctionsLag is positive but less significant (p-value 10%). When the random effects model is considered, the impact of personal features is in line with what expected, since evasion is higher among males and wealthier subjects.

While result [c] seems to cast some doubts on our ability to reproduce within the lab the real-world spatial closeness, results [a] and [b] can be analyzed using the approach illustrated in previous sections. To do so, we disaggregate the analysis between Bayesian and non-Bayesian subsets of observations and of agents.

Analytical results are reported in Tables from 7 to 10. Here we focus on the main messages.

First, the 'loss repair' effect disappears, for both subsets, when fixed effects are used, since coefficient of sanctions are not significant. This is important since the fixed effects model is able to capture time-invariant personal features, such as tax morale and perceptions of fairness of the tax system, that are not proxied by our variables and, therefore, are hidden in the error term of the random effects model. Thus, the 'loss repair effect' is unlikely to provide an exhaustive explanation of the BoCE. In particular, if the misperception of chance is the source of the BoCE, one would expect this effect to be originated within the subsets non-Bayesian observations and subjects. However, results are not entirely in line with these expectations since :

c. both Bayesian and non-Bayesian subsets exhibit a very strong BoCE in the short run: the coefficient of *Own Audit Lag* is always positive and very significant in all regressions;

[12]More precisely, both the coefficient of Neigh.Type on its own and all of the coefficient of interacted terms are not significant. These additional results are available upon request

d for Non-Bayesian observations (and subsets) the BoCE is also apparent in the longer run, since the coefficient of *Own Audits Stock* is negative and significant; on the contrary, no robust BoCE appears in the long run for Bayesian subsets;

e. the coefficient of 'others' audits tend to be not significant within subsets.

Thus, although, as conjectured, non-Bayesian subjects do generate a substantial part of the BoCE that we observe in the whole sample, Bayesian subjects also exhibit strong BoCE with respect to 'own' audits, although only in the short run. Moreover, it is worth noting that, although Bayesian subjects tend to react in the predicted way to 'others' audits, this impact is, on average, not significant. We shall now focus on these results.

5.3 Illustration and interpretation of behavior of Bayesian agents

To further illustrate previous results concerning Bayesian agents we use a graphical approach. In Figure 4 we report two pairs of graphs each comparing the impact of 'own' and 'others' audits on, respectively, the subjective probability and evasion as chosen by Bayesian agents, according to the restrictive as well as to the median definition.

[FIGURE 4 HERE]

Graphs for subjective probability are obtained from results reported in Table 11. Thus, on the vertical axis is reported the difference between the probability of an audit reported by a Bayesian taxpayer who has been audited after period 0 and before period 1 and the probability that the same taxpayer would report if she had not been audited. This difference is obtained from estimated coefficients of audit variables. The solid line considers only own audits, while the dashed line considers also others audits. Difference in period 1 is obtained as the sum of the short-run effects (i.e lag variables), while differences in period 2 is obtained as the sum of coefficients of stock variables. In period 3 the impact is the same as in period 2 but discounted by 10%.

Graphs for evasion are obtained in a similar way from results reported in Tables 9 and 10 for the FE specification. In every graph, the distance between the dashed and the solid line indicates the additional impact of 'others' audits, which is, in general, to increase the perceived probability and to decrease evasion. Two things clearly emerge.

First, Bayesian agents do conform to predictions when the *updating process* is considered, as the two graphs in the first line of Figure (4) show. In particular, the impact of 'own' audits on subjective probability is always positive, since all curves are in the positive quadrant. However, the impact of 'others' audits is not always significant, as shown by the fact that the distance between the dashed and the solid line is always positive but, for some periods, very small (and thus, non significant).

Second, Bayesian agents do not conform to predictions when the *compliance decision* is considered, as the two graphs in the second line of Figure (4) show. In particular, the impact of 'own' audits is of the opposite sign in the short term, since evasion tends to increase immediately after an audit, and this is the graphical representation of the BoCE. Note also that in period 2 the impact of own audits is zero (and largely non significant) while only in period 3 an audit conducted before period 1 tends to have a negative impact on evasion (but again non significant). Moreover, although the impact of 'others' audits on evasion is of the predicted sign, it is so small to be non significant (i.e the dashed is below but very close to the solid line).

The greater impact of 'own' audits could be driven by focalization, which is crucial in determining the formation of predictions and judgments (Kahneman et al. (2006)). If a subject is induced to focalize on 'own' audits, i.e. to attach to these audits a higher weight, she might appear as Bayesian when she is actually misperceiving the chance to be audited, as shown in equation (5). This weighting process, in turn, may generate the BoCE also among (apparent) Bayesian subjects.

However, this interpretation cannot be accepted for two reasons.

First, as stressed before, in our experiment we asked agents to guess the number of audits that would be carried on the *whole* population of subjects. The reason of this choice is that we didn't want to focalize our subjects to consider only, or prevalently, their own information track. Given that the tax audits were never extended to the whole population, and that the average number of agents audited was constant round by round, the BoCE should not influence in any way the number of audits guessed for the whole population. This is to say that the subjects, when asked to guess the audit probability for the overall population, where not focalized on what was just happened to themselves and therefore were not sensitive to BoCE when answering to our question.

Second, the coefficients of 'own' and 'others' audits in Table (11) are quite close, especially in the short run.

Thus, the most plausible explanation of our results is based on the psychological positioning of the taxpayer towards the occurrence of an audit. Our results suggest two things.

First, a tax audit becomes a *salient attribute* for the decision maker if it regards herself, while a tax audit on other taxpayers is much less relevant for the compliance decision, although both of them enter the probability to be audited. The intuition is that information regarding other taxpayers does not produce an equivalently intense involvement as a personal experience does, i.e. it is not salient.

Note that, as reported in the Introduction, salience was quoted by the traditional experimental literature, as the main explanation of *availability effect*, which is the contrary of BoCE. In our case, however, we need to interpret salience consistently with the fact that BoCE is found also among Bayesian subjects.

Using the dual processes theory, we could explain this by hypothesizing the existence of some form of shifting in the cognitive process. More precisely one could suppose that computing the expected audit probability for the subjects' population, or deciding to evade taxes are different mental tasks and therefore activate two separate cognitive processes. Kahneman (2011) states that we use two separate systems to take decisions: system one and system two. System one is characterized by fast and automatic mental processes, often linked to emotions, while system two uses slow and controlled mental processes, which are almost totally uninfluenced by emotions. Our results could be explained by this duality: Bayesian subjects use their system one when they have to decide how much to comply and system two when they have to compute probabilities.

Note that this interpretation relates somehow to a recent evolution of the gambler's fallacy (GF) literature which is based on neuro-sciences techniques.

Xue et al. (2012) try to falsify the idea that the GF heuristic should be "*supported by a fast, emotional and intuitive system, and that it can be overcome by deliberative reasoning*". More precisely what Xue et al. (2012) seem to demonstrate, using a mix of techniques aimed to measure computational skills and affective skills, is that subjects with strong cognitive ability are *more sensible* to GF and, at the same time, that subjects with strong affective decision-making capacity are less conditioned by GF. The Authors explain this counterintuitive result stating that the GF strategy, compared with a conservative strategy, that the Authors call "win stay-loss-shift", requires in fact a stronger cognitive ability be able to decide to discard the steady state behavior. Transferring this intuition to our experiment we could say that BoCE represents a shift in the prevailing behavioral pattern used by the subjects and, therefore, accordingly with the results by Xue et al. (2012), it is strong also in Bayesian subjects (who represent in our sample the most computationally skilled ones).

6 Concluding Remarks and Directions for Future Research

As mentioned in the Introduction, the BoCE has been so far explained in the literature by appealing to the misperception of chance, while the loss repair effect is a less favoured explanation. In this paper, we confirm that the loss repair effect is not very robust, and that it appears only when some unobservable features of agents are not appropriately taken into account. However, we also show that the misperception of chance cannot be interpreted as 'inability' to use all the available information to update probability of beliefs since (short-term) BoCE is very robust also when only Bayesian subjects are considered. Morover, although 'others' audits are used to update probability beliefs by Bayesian subjects, they do not appear to be relevant for the compliance decision..

We suggest, as a possible explanation, the possibility of a sort of 'duality' in behavior *by Bayesian agents*: the probability to be audited is correctly computed, using all of the relevant information in the correct way, but, when the agent has to decide whether and how much to comply, she is driven only by the emotions which are triggered exclusively by own audits. To test it, we could use functional magnetic resonance image (FMRI) techniques, as suggested by Shao et al. (2016), to investigate on the activation of different brain areas while the subjects where required to take risky decisions. The brain area more extensively activated when GF type choice are taken by the subjects is the right inferior parietal lobule (IPL). IPL is involved in quite different tasks, for example is activated when we try to interpret emotions through observing facial stimuli, but the functions more relevant for our issue performed through the activation of IPL are language and mathematical operations. Thus, we could look at activations of this area to check for different responses by Bayesian and non-Bayesian taxpayers.

The policy relevance of the present paper, and more in general of the lab experimental literature which finds the BoCE, rests with its external validity. A recent stream of research has focussed on randomized field experiments which are deemed to yield more reliable results while, at the same time, avoiding the endogeneity bias affecting standard econometric analyses of real-world data. It is then interesting to note that in the paper by DeBacker et al. (2015) which looks at the impact of prior audits on subsequent compliance by corporations, a robust BoCE is found. Since corporations are usually believed to take their compliance decisions in a 'rational' way, as opposite to 'non-rational' individuals, this result is to some extent similar to the one we obtain here, although in a completely different setting, i.e. a BoCE characterizing the compliance decision by Bayesian agents.

The implications of such a result, if confirmed, can change significantly the contribution of the experimental literature to the study of tax compliance. At its beginnings, this literature claimed that "the more precise is the information available regarding probabilities of detection, the more likely it is that taxpayers' responses to changing audit probabilities will conform to the predictions of conventional economic models" i.e. to AS72, since "in the absence of precise information regarding audit probabilities, it is impossible to select the amount of evasion that maximizes expected utility" (Spicer and Thomas, 1982).

Moroever it was argued that "it is questionable whether taxpayers make the burdensome calculations necessary to determine an optimal level of tax evasion" and consequently that "the rules which individuals use in place of optimization procedures are of interest" (Spicer and Hero, 1985).

Our paper shows that these rules are still of great interest even if precise information about audit probabilities is provided and even for subjects who are able to use this information to correctly perform probability calculations.

Finally, the evidence provided in this paper seems to indicate that the type of network in which the agent is inserted and from which the information on 'others' audits is obtained is not relevant for non-Bayesian agents, while it might be more significant for Bayesian ones. This result is also, to some extent, contrary to theoretical premises but, to further explore it, it probably requires to simulate 'closeness' in a more convincing way.

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7 Appendix 1: Experiment instructions

INSTRUCTIONS TO PARTICIPANTS (translated version)

The purpose of this experiment is to study tax evasion. The experiment makes use of a network of personal computers and is based upon the participation of volunteers who are sitting in this classroom along with you. The software in use allows participants to interact by exchanging information between them.This software is entirely automatized and it does not allow researchers to observe your choices for the whole experiment. Results are automatically generated and collected in a database which will be kept rigorously anonymous. To guarantee anonymity, at the beginning of the experiment you will receive a secret identification code that you will be able to use to obtain your compensation from Administrative Offices.

The final compensation will be the sum of a fixed participation fee of 2 euros plus a variable part whose amount depend on the performance during the experiment.

The experiment replicates a real-world situation: you are a worker who receives a gross income of 1000 UMS (Unit of Experimental Currency) per year, that are subject to a tax rate of 40%. During the experiment you will have to fill in your income declarations, while the revenue agency will run some audits and apply sanctions, if due.At the end of the experiment, UMS will be converted in Euro on the basis of a ratio 1000:8 (1000 UMS=8 euros).

During the experiment you will be involved in a number of rounds that represent a number of tax periods (you can assume a round as a year). The number of total rounds is not known and will not be known to you until the end of the experiment.

At the beginning of each round you will receive your income of 1000 UMS and you will have to declare your year income, where the Revenue Agency informs you that you will have to pay 40% of declared of income in taxes.

At every round, your income will be shown on the screen and, using the computer keyboard, you will have to write in the dedicated frame the amount of taxes you want to pay. In the case you have decided to pay nothing it will suffice to insert "0".

Immediately after, you must indicate how many audits, in your opinion, the Tax Authority has carried out in the round just played, writing a number from 0 to 24 (maximum number of subjects). At the end of the experiment we will assign a prize of 250 UMS to the one who will guess for the greater amount of times the correct number of tax audits actually carried on in each round. In case of *ex-aequo* winners the 250 UMS prize will be assigned to each one of them. If no participant is able to correctly guess the number of audits in any round, the prize will be lost.

Following this, the Tax Authority will carry over audits. With regard to these, it is important that everybody knows that:

- Audited subjects are selected by the software in a totally random way and researchers cannot intervene in any way in the choice of subjects to be audited. The guarantee that this is actually true stems from the fact that researchers' objective is not to find tax evaders but to simulate a context which is as close as possible to the real one, in which obviously no one can clearly know with certainty who is evading taxes.

- At the beginning of each round a number of participants to be audited is chosen. This number is fixed and it does not change from round to round, but it will not be disclosed to any participant. Consequently, the probability of being audited is the same one for all participants, but it is not known by any of these.

- The audit concerns only the taxes paid in the present round. If you are selected for an audit, the compute will send you a screenshot and will tell you the amount of the sanction that you will have to pay if you have decided to evade.

- The sanction increases as the evaded amount increases (see the attached table). In the case you are audited and an evasion is found, an amount equal to the tax evaded plus 100% of this will be detracted from your income.

Your final compensation will be determined by randomly extracting two rounds and giving you the income associated to these two rounds.

In addition to information concerning your income you will be informed about what is happening to other participants. More precisely, you are know in a classroom which simulates a geographical context characterized by the presence of 4 fiscal districts, each composed of 6 taxpayers. The figure you see on the screen represent the spatial distribution of taxpayers, a red star indicates the point where you are. The map you see on the computer is purely theoretical, as positions do not correspond to places in the lab.

During the experiment, at the end of each round you will receive some information concerning what happened to you and to other taxpayers during previous round.More precisely, you will know: a) if you have been audited; b) how many of the taxpayers to which you are connected, and that will be indicated by corresponding *frecce* on your monitor, have been audited by the Tax Authority .

As you will receive information about what happened to other taxpayers, other taxpayers will receive information about what happened to you. More precisely, some taxpayers will know that you have been audited in the previous round. However, it is important that you know that it will never occur that all of other participants will know what happened to you. In any case, the information will remain strictly confidential, i.e. they will circulate only within the group of taxpayers as indicated by the red star.

ATTACHED TABLE

Income in every round: 1000 UMS. Tax rate 40%. Ta x due=400 UMS.

CASE 1. You reported an amount of due taxes equal to 400. You are not audited so that your income will be equal to your initial income less paid taxes (1000-400=600).

CASE 2. You reported an amount of due taxes equal to 400. You are not audited but no evasion is found so that you won't receive any sanction, and your income will be equal to your initial income less paid taxes (1000-400=600).

CASE 3. You reported an amount of due taxes equal to 121. You are not audited so that your income will be equal to your initial income less paid taxes (1000-121=879).

CASE 4. You reported an amount of due taxes equal to 121. You are audited and the Tax Authority detects the evasion. Thus, you will have to pay the taxes you did not pay (279) plus a sanction equal to your evaded tax (279). Your final income will then be equal to 1000-121-279-279=321

CASE 5. You reported an amount of due taxes equal to 0. You are not audited so that your income will be equal to your initial income less paid taxes (1000-0=1000).

CASE 6. You reported an amount of due taxes equal to 0. You are audited and the Tax Authority detects the evasion. Thus, you will have to pay the taxes you did not pay (400) plus a sanction equal to your evaded tax (400). Your final income will then be equal to 1000-400-400=200

ISTRUZIONI PER I PARTECIPANTI(original italian version)

Lo scopo di questo esperimento e' di studiare il fenomeno dell'evasione fiscale. L'esperimento ricorre all'uso di piu' computer collegati tra loro in rete e prevede il coinvolgimento di piu' volontari che si trovano nella vostra stessa aula. Il software utilizzato per l'esperimento consente ai partecipanti di interagire scambiandosi informazioni. Il software e' totalmente automatizzato e non permette ai ricercatori di osservare le vostre scelte per l'intera durata dell'esperimento. I risultati sono automaticamente raccolti in un data-base che rimarra' rigorosamente anonimo. Per garantire l'anonimato all'inizio dell'esperimento vi sara' attribuito un codice segreto d'identificazione che vi servira' per incassare il vostro compenso presso gli uffici amministrativi.

Il compenso finale sara' composto da una quota di partecipazione fissa di Euro 2 piu' una parte variabile che dipende dalla performance durante l'esperimento.

L'esperimento simula un contesto reale: tu sei un lavoratore che percepisce un reddito lordo di 1000 UMS (Unita' di Moneta Sperimentale) annuali su cui prevista una imposizione fiscale del 40%. Nel corso dell'esperimento dovrai compilare le dichiarazioni dei redditi, l'agenzia delle entrate effettuera' dei controlli e imporra' le eventuali sanzioni. Alla fine dell'esperimento le UMS saranno convertite in Euro sulla base di un rapporto di 1000 a 8 (1000 UMS = 8 Euro).

Nell'esperimento parteciperai a pi round che rappresentano diversi periodi di tempo (potete immaginare ogni round come un anno). Il numero totale di round non vi noto n vi sara' reso noto se non alla fine dell'esperimento stesso.

All'inizio di ciascun round percepirai il tuo reddito di 1000 UMS e dovrai fare la tua dichiarazione annuale delle tasse, nella quale l'Agenzia delle Entrate ti informa che, per legge, dovrai versare il 40% del tuo reddito in tasse.

Ad ogni round comparir sullo schermo il reddito e, usando la tastiera del computer, dovrai scrivere nell'apposita finestra l'ammontare di tasse che hai deciso di versare. Nel caso avessi deciso di non versare dichiarare nulla ti sar sufficiente inserire uno "0".

Immediatamente dopo dovrai indicare quanti controlli effettuera', secondo te, l'Agenzia delle Entrate sulle dichiarazioni presentate nel round appena giocato, indicando un numero da 0 a 24 (massimo numero di partecipanti). Alla fine dell'esperimento assegneremo un premio di 250 UMS a chi indovinera' il maggior numero di volte il numero effettivo di controlli che stato effettuato in ogni round. Nel caso ci fossero vincitori ex-aequo, il premio di 250 UMS sara' assegnato a ciascuno di loro. Nel caso in cui nessuno indovinasse in nessun round il numero di controlli, il premio verra' perso.

A questo punto l'Agenzia delle Entrate procedera' con i controlli. A questo proposito e' importante che tutti sappiate che:

- Il software seleziona gli indagati seguendo una procedura totalmente casuale senza che i ricercatori possano in nessun modo intervenire nella scelta dei soggetti da ispezionare. La garanzia che cio' sia effettivamente vero discende dal fatto che l'obiettivo che i ricercatori perseguono non e' di smascherare gli evasori bensi' di simulare un contesto che sia il pi possibile vicino a quello reale, nel quale nessuno puo' ovviamente sapere con assoluta certezza chi stia effettivamente evadendo.

- All'inizio di ogni round e' selezionato un numero di partecipanti che saranno indagati. Questo numero fisso e non cambia di round in round, ma non sar reso noto ad alcun partecipante. Ne consegue che la probabilita' di essere indagati e' la stessa per tutti i partecipanti, ma non e' nota a nessuno di essi.

- L'ispezione riguarda solo le imposte pagate nel round in corso. Nel caso tu fossi estratto per un'ispezione fiscale il computer ti inviera' una schermata e ti comunichera' l'importo della multa che dovrai pagare qualora avessi deciso di evadere.

- La multa applicata cresce al crescere dell'importo evaso (vedi schema allegato alle istruzioni). Nel caso in cui siate indagati e sia riscontrata un'evasione, sara' detratta dal vostro reddito accumulato una quantita' di denaro pari alla somma della quantita' evasa piu' una multa pari al 100% della quantita' evasa stessa.

Per determinare la vostra ricompensa finale saranno estratti due round a caso e vi sara' assegnata la vincita relativa ai round.

Oltre alle informazioni relative al tuo reddito sarai messo al corrente di cosa sta avvenendo agli altri partecipanti. Piu' precisamente, tu ti trovi in un'aula che simula un contesto geografico caratterizzato dall'esistenza di 4 distretti fiscali, ciascuno composto da 6 contribuenti. La figura che vedi sul monitor rappresenta la distribuzione spaziale dei contribuenti, una stella rossa illustra il punto nel quale ti trovi. La mappa che vedi sul monitor e' puramente teorica, e le posizioni non corrispondono alle postazioni di gioco.

Durante l'esperimento, al termine di ogni round riceverai informazioni relative a cosa e' accaduto a te e ad altri contribuenti fiscali durante il round precedente. Piu' precisamente saprai: a) se sei stato controllato tu; b) quanti dei contribuenti a cui sei collegato, e che ti saranno indicati da altrettante frecce sul monitor del computer, sono stati controllati dall'Agenzia delle Entrate.

Cosi' come riceverai informazioni in merito a cosa e' accaduto ad altri contribuenti fiscali anche gli altri potranno ricevere informazioni in merito a cosa accaduto a te. Piu' precisamente, alcuni contribuenti potranno essere informati in merito al fatto che sei stato indagato nel round precedente. Tuttavia e' importante che tu sappia che non accadra' mai che tutti gli altri partecipanti siano informati in merito a quanto ti accaduto. Le informazioni in ogni caso resteranno strettamente confidenziali, vale a dire che circoleranno solo nel contesto del gruppo di partecipanti indicato dalle frecce.

SCHEMA ALLEGATO

Reddito per round: 1000 UMS. Imposizione fiscale 40%=400UMS

CASO 1. Hai dichiarato di voler pagare tasse per 400. Non essendo stato controllato il tuo reddito sara' dato dal reddito iniziale meno le tasse pagate (1000-400=600).

CASO 2. Hai dichiarato di voler pagare tasse per 400. Essendo stato controllato ma non riscontrando alcuna evasione non riceverai nessuna multa, e il tuo reddito sara' dato dal reddito iniziale meno le tasse pagate (1000-400=600).

CASO 3. Hai dichiarato di voler pagare tasse per 121. Non essendo stato controllato il tuo reddito sara' dato dal reddito iniziale meno le tasse pagate (1000-121=879).

CASO 4. Hai dichiarato di voler pagare tasse per 121. Sei pero' stato controllato, e l'Agenzia riscontra l'infrazione. Dovrai percio' pagare le tasse non pagate (279) pi una multa corrispondente a 1 volta le tasse evase (279). Il reddito finale sara' percio' dato da 1000-121-279-279=321

CASO 5. Hai dichiarato di voler pagare tasse per 0. Non essendo stato controllato il tuo reddito sara' dato dal reddito iniziale meno le tasse pagate (1000-0=1000).

CASO 6. Hai dichiarato di voler pagare tasse per 0. Sei pero' stato controllato, e l'Agenzia riscontra l'infrazione. Dovrai percio' pagare le tasse non pagate (400) piu' una multa corrispondente a 1 volta le tasse evase (400). Il reddito finale sara' percio' dato da 1000-400-400=200.

8 Appendix 2: Figures and Tables

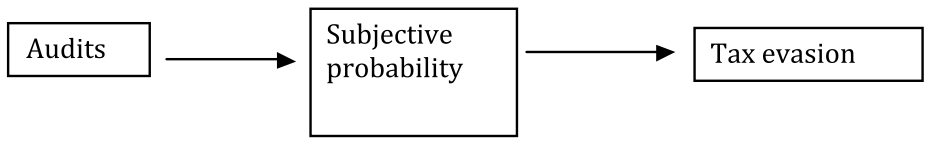

Figure 1: The steps of compliance choice

Table 1: Variables Description

Variable	Mean
Evasion	Chosen level of evasion
Own Audit Stock	Number of own audits in the round interval $[0, t-2]$
Others Audit Stock	Number of audits in own neighborhood in the round interval $[0, t-2]$
Own Audit Lag	Subject audited or not in previous round
Others Audit Lag	Number of audits in own neighborhood in the previous round
non-Bayesianity	Index in (4)
Wealth	Cumulated income in all previous rounds
Sanctions Stock	Cumulated amount of sanctions paid by the subject in the round interval $[0, t-2]$
Sanctions Lag	Sanctions paid by the subject in previous round
Round	Sequential number of the rounds
Treatment	"0" if subjects first choose evasion level and then have "Subj. Prob." elicited,
	"1" if subjects have first "Subj. Prob." elicited and then choose evasion level.
Neigh. Type	1 if neighbors were displayed as close
	3 if neighbors were displayed as distant
	2 if neighbors were displayed as middle distance
Male	1 if male, 0 if female
Familiarity	1 if with an economic background, 0 else
Age	Age of the subject

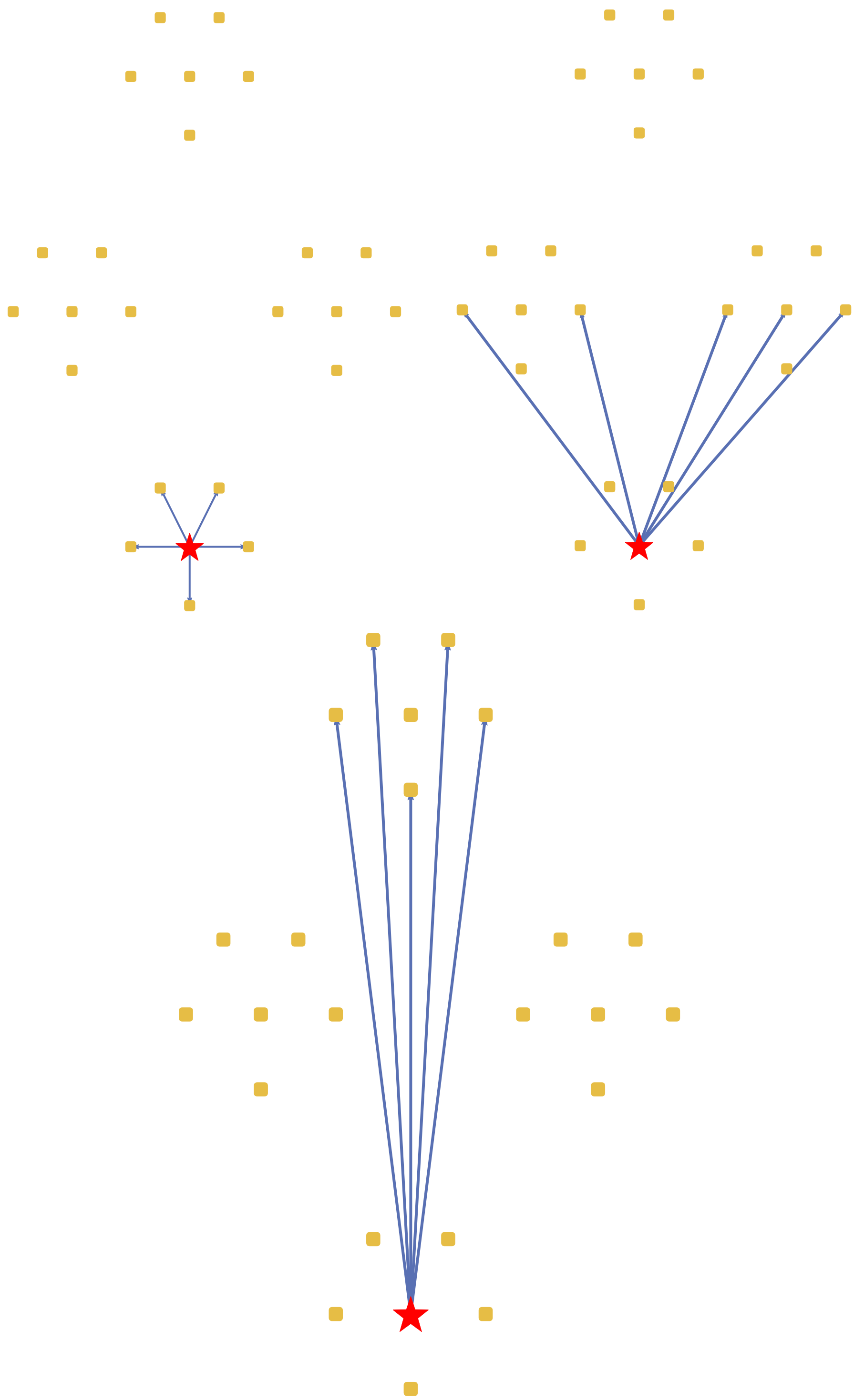

Figure 2: Type of networks: 'close', 'intermediate' and 'far' neighbors

Table 2: Summary Statistics

Variable	**Mean**	**(Std. Dev.)**	**Min.**	**Max.**	**N**
Evasion	166.479	(175.278)	0	400	4320
Own Audit Stock	3.625	(2.817)	0	13	4032
Others Audit Stock	18.125	(10.536)	0	46	4032
Own Audit Lag	0.25	(0.433)	0	1	4176
Others Audit Lag	1.25	(0.819)	0	4	4176
non-Bayesianity	0.114	(0.129)	0	0,802	4176
Wealth	9887.776	(6070.805)	0	27400	4320
Sanctions Stock	1058.306	(1459.745)	0	10400	4320
Sanctions Lag	83.994	(228.06)	0	800	4176
Round	15.5	(8.656)	1	30	4320
Treatment	0.5	(0.5)	0	1	4320
Neigh. Type	2	(0.817)	1	3	4320
Male	0.563	(0.496)	0	1	4320
Familiarity	0.639	(0.48)	0	1	4320
Age	22.552	(2.563)	19	31	4290

Table 3: Distribution of NB and of Meanratio

Quartile	Maximum value of NB	Maximum value of Meanratio
1	3.0%	5.2%
2	6.9%	8.1%
3	15.1%	14.7%
4	80.9%	51.3%

Table 4: Correlation between Subjective Probabilities and Audits: coefficients and p-values
Restrictive definition of Bayesianity for Subjects

	Bayesian (25% with lowest Meanratio)	non-Bayesian
Own Audit Lag	11.33%(0.000)	1.88%(0.292)
Others Audit Lag	19.52% (0.000)	7.58%(0.000)
Own Audit Stock	8.79% (0.0053)	-1.76%(0.334)
Others Audit Stock	-5.77%(0.0673)	-0.49% (0.768)

Table 5: Correlation between Subjective Probabilities and Audits: coefficients and pvalues
Median definition of Bayesianity for Subjects

	Bayesian (50% with lowest Meanratio)	non-Bayesian
Own Audit Lag	9.98% (0.000)	0.47%(0.829)
Others Audit Lag	13.47% (0.000)	7.43%(0.007)
Own Audit Stock	4.21%(0.059)	-3.14% (0.159)
Others Audit Stock	-7.19%(0.012)	2.01%(0.367)

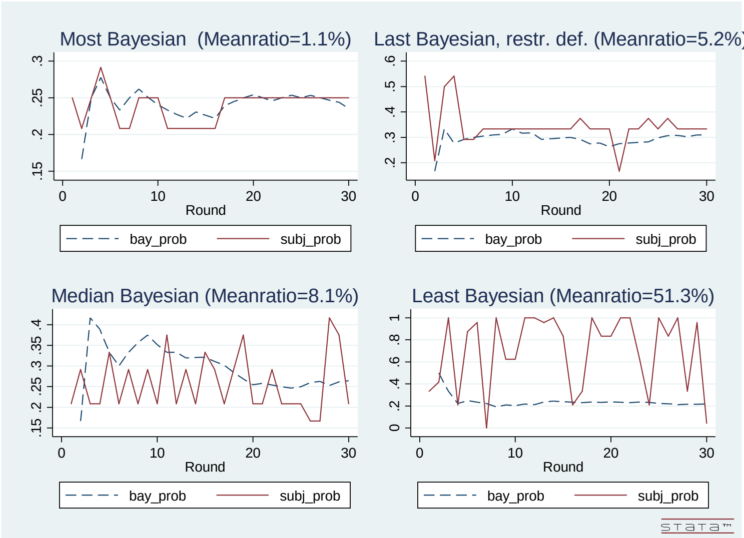

Figure 3: Four representative agents

Table 6: Regression for the whole sample (dep.var: evasion)

VARIABLES	(1) Tot. Sample FE	(2) Tot. Sample RE
OwnAuditStock	-9.110***	-5.194***
	(2.778)	(1.743)
OthersAuditStock	-0.670	-0.775
	(1.606)	(0.766)
OwnAuditLag	57.07***	39.44***
	(10.49)	(6.629)
OthersAuditLag	-5.840*	-6.279**
	(3.035)	(2.636)
Non_Bayesianity	-3.923***	-3.212***
	(1.175)	(0.730)
wealth	-0.00169	0.0503***
	(0.00362)	(0.00212)
SanctionsStock	0.00182	0.0412***
	(0.00365)	(0.00234)
SanctionsLag	0.0296*	0.129***
	(0.0179)	(0.0127)
Treatment		-0.865
		(4.345)
Round	5.162	-34.69***
	(3.336)	(1.975)
Neigh. Type		-4.555*
		(2.689)
Male		12.14***
		(4.480)
Familiarity		1.690
		(5.159)
age		-3.609***
		(0.981)
Constant	144.6***	273.9***
	(7.962)	(26.02)
Observations	4,032	4,004
R^2	0.075	
Number of id	144	143

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 7: Subsamples of Bayesian and non-Bayesian observations (dep. var: evasion). Restrictive definition of Bayesianity

VARIABLES	(1) Bayesian FE	(2) Bayesian RE	(3) Non Bayesian FE	(4) Non Bayesian RE
OwnAuditStock	-12.76**	-5.528	-7.424**	-6.771***
	(6.008)	(4.551)	(2.983)	(2.102)
OthersAuditStock	-1.042	-1.644	0.680	-1.101
	(3.693)	(2.009)	(1.665)	(0.913)
OwnAuditLag	51.66***	39.31***	62.07***	41.63***
	(16.92)	(12.38)	(12.06)	(7.729)
OthersAuditLag	-5.486	-7.113	-4.305	-5.982*
	(7.146)	(5.023)	(3.081)	(3.064)
Non_Bayesianity	17.95	18.97	-4.387***	-3.806***
	(15.87)	(16.96)	(1.248)	(0.849)
wealth	-0.00935	0.0309***	0.00119	0.0479***
	(0.00589)	(0.00582)	(0.00411)	(0.00252)
SanctionsStock	0.00920	0.0267***	-0.00120	0.0403***
	(0.00601)	(0.00542)	(0.00411)	(0.00293)
SanctionsLag	0.0318	0.0955***	0.0213	0.125***
	(0.0335)	(0.0229)	(0.0200)	(0.0149)
Treatment		14.57		-3.479
		(14.32)		(5.315)
Round	11.29	-18.99***	1.186	-32.22***
	(7.027)	(5.152)	(3.756)	(2.394)
Neigh. Type		-21.21**		-1.293
		(8.517)		(3.336)
Male		22.10		11.49**
		(14.72)		(5.438)
Familiarity		16.49		-0.497
		(16.37)		(6.247)
age		-8.540***		-3.191***
		(3.067)		(1.203)
Constant	131.8***	371.7***	150.7***	259.8***
	(17.04)	(79.67)	(9.577)	(31.86)
Observations	1,024	1,022	3,008	2,982
R^2	0.085		0.073	
Number of id	124	123	143	142

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 8: Subsamples of Bayesian and non-Bayesian observations (dep. var: evasion). Median definition of Bayesianity

VARIABLES	(1) Bayesian FE	(2) Bayesian RE	(3) Non Bayesian FE	(4) Non Bayesian RE
OwnAuditStock	-7.076*	-4.342	-8.380**	-7.958***
	(4.222)	(3.268)	(4.088)	(2.714)
OthersAuditStock	-1.888	-2.607	0.962	-0.349
	(2.275)	(1.604)	(2.146)	(1.340)
OwnAuditLag	49.65***	33.34***	66.16***	54.25***
	(11.89)	(11.86)	(15.48)	(15.86)
OthersAuditLag	-7.212	-7.862	-5.557	-6.083
	(4.839)	(4.946)	(4.164)	(3.833)
Non_Bayesianity	5.603	5.073	-4.046***	-3.787***
	(7.590)	(7.631)	(1.265)	(1.036)
wealth	-0.00573	0.0346***	9.16e-07	0.0465***
	(0.00446)	(0.00392)	(0.00549)	(0.00292)
SanctionsStock	0.00158	0.0267***	-0.00425	0.0395***
	(0.00391)	(0.00376)	(0.00595)	(0.00414)
SanctionsLag	0.0314	0.106***	0.0238	0.109***
	(0.0213)	(0.0190)	(0.0255)	(0.0247)
Treatment		1.576		1.951
		(12.32)		(7.875)
Round	9.339**	-20.50***	1.813	-32.02***
	(4.489)	(3.437)	(4.880)	(3.144)
Neigh. Type		-15.77**		2.334
		(7.544)		(4.760)
Male		27.21**		11.14
		(13.32)		(8.120)
Familiarity		1.688		10.62
		(15.26)		(9.945)
age		-9.134***		-0.371
		(2.418)		(1.944)
Constant	133.0***	391.6***	151.0***	180.9***
	(13.11)	(65.47)	(12.61)	(47.91)
Observations	2,061	2,059	1,971	1,945
R^2	0.072		0.078	
Number of id	139	138	138	137

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 9: Subsamples of Bayesian and non-Bayesian subjects (dep. var: evasion). Restrictive definition of Bayesianity

VARIABLES	(1) Bayesian FE	(2) Bayesian RE	(3) Non Bayesian FE	(4) Non Bayesian RE
OwnAuditStock	0.945	-1.911	-10.55***	-6.729***
	(8.141)	(3.762)	(3.109)	(2.037)
OthersAuditStock	-3.923	-0.271	0.285	-1.343
	(3.586)	(1.593)	(1.854)	(0.926)
OwnAuditLag	65.57***	31.93***	56.08***	41.79***
	(18.24)	(12.03)	(12.70)	(7.874)
OthersAuditLag	-10.80	-6.068	-4.234	-6.662**
	(7.379)	(4.673)	(3.190)	(3.160)
Non_Bayesianity	-3.433	-2.381	-3.982***	-3.574***
	(4.796)	(5.270)	(1.196)	(0.811)
wealth	0.00406	0.0487***	-0.00198	0.0490***
	(0.00548)	(0.00443)	(0.00449)	(0.00255)
SanctionsStock	-0.00472	0.0382***	0.00253	0.0418***
	(0.00676)	(0.00424)	(0.00509)	(0.00296)
SanctionsLag	0.0219	0.147***	0.0298	0.119***
	(0.0320)	(0.0218)	(0.0213)	(0.0153)
Treatment		18.20**		-6.719
		(8.358)		(5.201)
Round	3.504	-35.12***	4.453	-32.67***
	(5.824)	(3.556)	(4.002)	(2.482)
Neigh. Type		-4.494		-2.613
		(4.703)		(3.334)
Male		-4.714		14.94***
		(9.997)		(5.322)
Familiarity		-4.469		2.580
		(11.03)		(6.088)
age		-7.934***		-2.311**
		(2.030)		(1.174)
Constant	150.2***	382.3***	143.8***	240.0***
	(17.02)	(54.53)	(9.197)	(31.06)
Observations	1,008	1,008	3,024	2,996
R^2	0.112		0.069	
Number of id	36	36	108	107

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 10: Subsamples of Bayesian and non-Bayesian subjects (dep. var: evasion). Median definition of Bayesianity

Subsample of Bayesian and Non-Bayesian subjects (dep.var: evasion)

VARIABLES	(1) Bayesian FE	(2) Bayesian RE	(3) Non Bayesian FE	(4) Non Bayesian RE
OwnAuditStock	-6.767	-0.544	-9.073**	-8.046***
	(4.759)	(3.999)	(3.604)	(2.295)
OthersAuditStock	-3.994*	-0.439	2.103	-1.540
	(2.366)	(1.476)	(2.155)	(1.374)
OwnAuditLag	64.49***	43.44***	52.96***	37.18**
	(13.79)	(14.83)	(15.65)	(15.93)
OthersAuditLag	-7.397	-4.067	-4.772	-9.125**
	(4.789)	(4.737)	(3.789)	(3.758)
Non_Bayesianity	0.326	1.113	-4.396***	-3.996***
	(3.161)	(3.867)	(1.265)	(1.052)
wealth	-0.00194	0.0524***	0.00113	0.0496***
	(0.00388)	(0.00316)	(0.00620)	(0.00341)
SanctionsStock	0.00382	0.0408***	-0.00446	0.0375***
	(0.00446)	(0.00445)	(0.00681)	(0.00430)
SanctionsLag	0.0163	0.122***	0.0392	0.131***
	(0.0228)	(0.0209)	(0.0279)	(0.0261)
Treatment		-1.878		-0.758
		(8.985)		(8.575)
Round	9.004**	-38.05***	0.0544	-31.80***
	(4.024)	(3.193)	(5.051)	(3.305)
Neigh. Type		-5.841		1.425
		(5.221)		(5.231)
Male		13.52		15.51*
		(10.45)		(9.290)
Familiarity		1.509		3.265
		(12.06)		(11.04)
age		-4.719**		-1.357
		(1.846)		(2.385)
Constant	138.3***	303.6***	147.9***	204.3***
	(11.18)	(54.37)	(11.06)	(56.10)
Observations	2,016	2,016	2,016	1,988
R^2	0.082		0.076	
Number of id	72	72	72	71

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 11: Subsamples of Bayesian subjects (dep. var: Subjective probability). FE models

VARIABLES	(1) Bayesian Subjects restr. def	(2) Bayesian Subjects median def
OwnAuditStock	0.112	0.153*
	(0.137)	(0.0825)
OthersAuditStock	0.0368	0.0681**
	(0.0392)	(0.0300)
OwnAuditLag	0.265**	0.260**
	(0.120)	(0.115)
OthersAuditLag	0.251***	0.241***
	(0.0534)	(0.0498)
Non_Bayesianity	0.141	0.374***
	(0.172)	(0.111)
wealth	2.15e-05	0.000104
	(9.72e-05)	(8.90e-05)
SanctionsStock	-8.76e-05	-1.83e-05
	(0.000148)	(8.78e-05)
SanctionsLag	-9.45e-05	9.02e-05
	(0.000166)	(0.000187)
round	-0.101	-0.216**
	(0.100)	(0.0832)
Constant	6.325***	6.001***
	(0.248)	(0.243)
Observations	1,008	2,016
R^2	0.102	0.151
Number of id	36	72

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

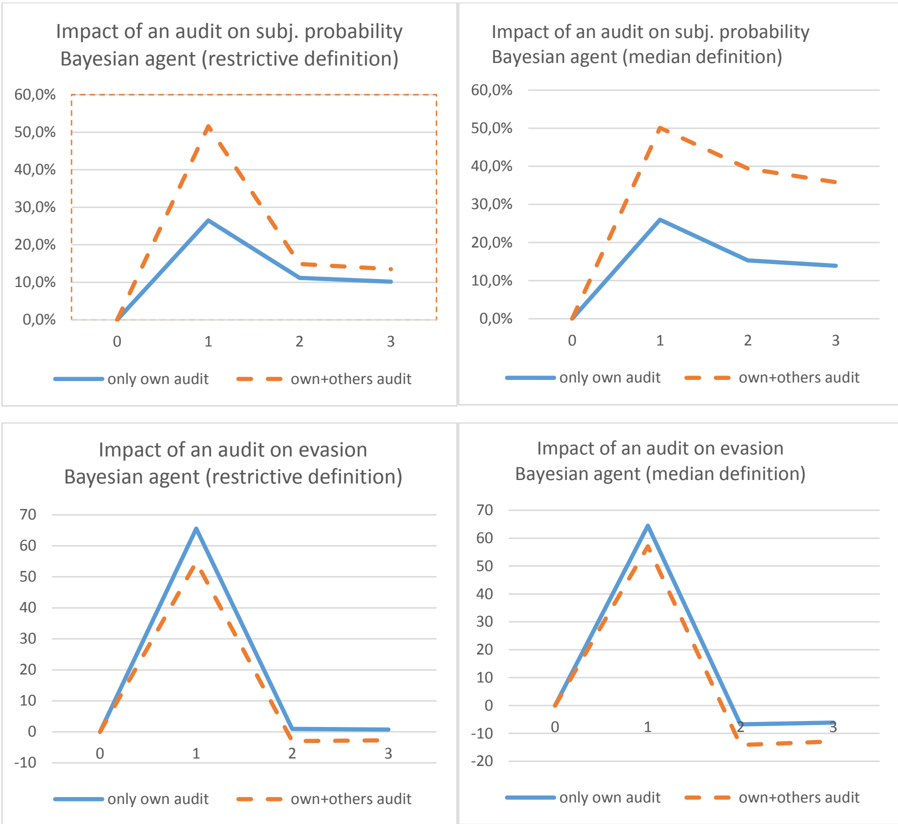

Figure 4: Comparison between the impact of an audit on subjective probability and evasion for Bayesian taxpayers