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Keywords
tax evasion, social information, audit probability, salience bias, laboratory experiment
JEL Codes
D83, D9, H2, H26

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February, 2023

Abstract

We experimentally test how information about the number of caught tax evaders, by interacting with individuals' prior beliefs, affect the decision to underreport taxes. Specifically, our results indicate that when individuals receive the information about the number of people caught evading taxes and perceive this as higher than prior beliefs, they evade less. When, instead, individuals consider the number of caught evaders as low with respect to their beliefs, they evade more. These findings suggest that when subjects are informed on how many people have been found evading taxes they infer the audit probability, rather than the tax evasion rate. Finally, we observe no salience bias effect when considering individuals to whom we highlighted information about others' norm violation nor when looking at those to whom we emphasised the probability of being audited.

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1 Introduction

The losses caused by the shadow economy and crimes such as corruption, tax evasion, fraud, extortion have been worried more and more governments. Among these, the fight against tax evasion has become a political priority for both the National tax administrations and the European Commission (**European Commission**, **2015**) over the last years. This concern has acquired even more attention after the economic crisis, as it becomes harder to reduce the budget deficits (**Lefebvre et al.**, **2015**; **European Commission**, **2019**). Also, for this reason data on this phenomenon – i.e., the yearly amount of taxes evaded, or the number of caught evaders- are often reported on both government institutions' website and newspaper (Internal Revenue Service (IRS)[1]; United States Sentencing Commission (USSC)[2]; The New York Times[3]; **Il sole 24 ore**, **2019**[4]; **Il Corriere della Sera**, **2020**[5]), possibly to raise citizens' awareness. However, how news on the reported tax evaders may affect the individual's behaviours or the overall tax compliance rate is still an open question.

In our paper, we want to inspect how the information on the number of caught evaders, by interacting with individuals' prior beliefs, affect the compliance rate. When a taxpayer gets to know that others have been audited and found guilty of evading taxes, she might infer either the tax evasion rate of her peers or the probability of being audited. While we do not exclude that a taxpayer, after being informed, may form other beliefs[6], we claim that information dissemination on enforcement outcomes – i.e., number of caught evaders – mainly induces individuals to form one of these two beliefs. Moreover, this study wants to explore whether highlighting either the tax evasion rate or the audit probability leads to a salience bias effect. Indeed, it could be that individuals' compliance behaviours can be influenced by emphasising one piece of information rather than another.

We expect that when the individual gets to know the number of caught evaders and she perceives this as relatively high compared to her prior beliefs, two different scenarios may occur depending on her inference. If the individual derives that the tax evasion rate is high, she will evade more. If, instead, the individual believes that the audit probability is high, she will comply more. On the contrary, when the individual perceives the number of people caught evading taxes as relatively low with respect to her prior beliefs, we assume the following. If the individual infers that most of the people pay the taxes, she will either comply more or as before[7]. Conversely, if the individual expects that the audit frequency is low, she will evade more.

It might be claimed that there exists a negative relationship between the audit probability and the empirical norm, and, therefore, that our question has an unambiguous answer. Indeed, according to the standard tax evasion model of **Allingham and Sandmo** (**1972**), the individual maximises her expected utility by evading the optimal amount of taxes, which in turn varies inversely with the audit probability. In other words, a (known) higher audit probability leads to both a decrease in tax evasion rate and in the amount of income evaded, and vice versa. Nonetheless, this negative relationship arises only if taxpayers have full information, both on the audit probability and on the tax evasion rate, are

[1] https://www.irs.gov/newsroom/irs-ci-counts-down-the-top-10-cases-of-2021.
[2] https://www.ussc.gov/sites/default/files/pdf/research-and-publications/quick-facts/Tax_Fraud_FY21.pdf.
[3] https://www.nytimes.com/2021/04/13/business/irs-tax-gap.html.
[4] https://www.ilsole24ore.com/art/lei-e-evasore-non-andra-carcere-ACgvDAn?refresh_ce=1.
[5] https://www.corriere.it/dataroom-milena-gabanelli/evasione-fiscale-italia-110-miliardi-tasse-non-pagate-cose-fare-subito-cashback-scontrino-elettronico/05f7119c-3984-11eb-97f0-6f118c19c928-va.shtml.
[6] For example, reading on a newspaper that several people have being found evading taxes may induce an individual to think that the government has increased the resource to uncover evaders; or that the tax authority is strategically using the media to frighten individuals and increase tax compliance.
[7] This has been called asymmetric effect of social information on tax compliance. We expose this later.

fully rational and show a similar risk aversion level. As observed by **Spicer and Thomas** (**1982**), the negative relationship between the audit probability and the propensity to evade emerges when individuals are fully or imprecisely informed. When citizens have no information[8], the negative relationship between the probability of being audited and the propensity to evade is not observed anymore (see also **Alm et al**., **1992a** and **Alm et al**., **2009**, on how uncertainty about the audit probability affect individuals' behaviours).

Furthermore, we assume that if we add an information either on the tax evasion rate or on the audit probability to the one on the number of caught evaders, the individual will behave in the same way. Indeed, even though we provide different pieces of information, the same conclusions can be drawn. If we inform the individuals about the number of caught evaders and the tax evasion rate, they can infer the audit probability. If, instead, we tell the subjects the number of people caught evading taxes and the audit probability, they can deduce the number of evaders. Hence, theoretically we should not find any difference. However, highlighting one piece of information rather than another could result in a salience bias effect (**Tversky and Kahneman**, **1973**).

In the naturally occurring world, the effect of knowing the number of caught evaders on compliance rate is unobservable, as is the hypothetical presence of a salience bias effect due to highlighting the information on the tax evasion rate or that on the audit probability. For these reasons, we adopt a laboratory experiment to answer our research questions. In our tax evasion game, participants earn their income by performing a task. In addition to this, they also receive a random income component. Then, individuals choose how much of their income to report, knowing that they can incur in an audit process with an exogenous probability. The tax evasion game is repeated for 20 periods, divided into two phases of 10 periods each. In the first phase of each treatment, the audit probability is unknown to participants. Before starting the 10 periods of the second phase, taxpayers are elicited beliefs on the first 10 periods of pilot sessions ("*pilot-p*" treatment). Specifically, participants are asked the number of caught evaders (N) and the tax evasion rate (E) they think occurred in these pilot sessions. After the elicitation task, subjects receive information according to the treatment they are randomly assigned to. In the "*info-caught*" treatment, individuals are informed on the average number of subjects per period, N, that have been caught evading taxes. In the "*info-evaders*" treatment, information is given to taxpayers about N and the average number of norm violators, E. In the "*info-prob*" treatment, individuals are informed about N, and the exact probability of being audited, p. In the "*full-info*" treatment, citizens are informed on N, on E and on the probability of being audited p. As for beliefs, the information provided in the treatments are drawn from the first 10 periods of the pilot sessions. We also run a "*no-info*" treatment, in which we do not elicit beliefs, nor we provide information to participants.

Our results indicate that individuals infer the audit probability rather than the tax evasion rate when receiving the information about the number of caught evaders. When individuals perceive that the observed number of caught evaders is higher than expected, they infer a higher audit probability and evade less. When, instead, participants are informed that the number of people caught evading taxes is lower than their prior beliefs, they deduce a lower audit probability and evade more. Moreover, we find no evidence of a salience bias effect when looking at individuals to whom we highlight the

[8] In the real-world individuals are unaware of the audit probability and the number of tax evaders. What they are informed of is the number of individuals caught evading taxes within their city, region or country. Furthermore, this can be extended to other violations, as traffic offences, thefts, parking tickets, extorsions, etc. In all these cases, individuals receive information on the number of caught individuals, and not on the audit probability or numbers of rule breakers.

information about others’ norm violation nor when considering those to whom we emphasise the audit probability.

2 Social information, audit probability and salience bias

Our paper relates to three main strands of the literature. The first one investigates the impact of social information – i.e., the information on others’ norm violation or norm compliance- on individuals’ unethical behaviours such as dishonesty, cheating, tax evasion. Several studies have found that observing norm violators is contagious (**Wilson and Kelling**, **1982**; **Keizer et al.**, **2008**; **Gino et al.**, **2009**; **Diekmann et al.**, **2015**; **Lefebvre et al.**, **2015**; **Dimant**, **2019**; **Alm et al. 2019**; **Garcia et al.**, **2020**; **Bicchieri et al.**, **2022**). The mechanism behind this effect has been explained in several ways.

According to **Diekmann et al.** (**2015**), individuals tend to obey a norm if they observe others to follow the norm and disrupt the norm if they get to know that others violate it. The authors define this mechanism as “conditional norm compliance”. Similarly, **Gino et al.** (**2009**) find that observing an individual violating the norm has an impact on own behaviours, however they make a further distinction between observing an in-group or an out-group violator. When a person detects that someone in her reference network behaves dishonestly, she will more probably violate the norm too. On the contrary, when the observed norm violator is an outsider, the probability of disrupting the norm decreases. By contrast, **Rauhut** (**2013**) observes that the decision to follow or violate the norm crucially depends on whether the subject under- or over-estimates the extent of the transgression. When individuals observe the proportion of liars, if they overestimate it, they lie less, instead if they underestimate it, the average number of liars increases.

Dimant, (**2019**) and **Bicchieri et al.** (**2022**) pointed to a different effect caused by the observation of norm followers rather than norm violators. As underlined in both studies, there exists an asymmetric effect of observability: an individual who detects norm violators more probably will break the rule, while an individual who observes norm compliers does not change her behaviour. **Bicchieri et al.** (**2022**) observe that this effect is weaken by social proximity[9]. The asymmetric effect has been found also within the literature of tax behaviour by **Lefebvre et al.**, **2015**: when individuals receive information of high compliance rate in past sessions, no significant effect is observed. On the contrary, when subjects are informed on low compliance rate, the proportion of tax evaders significantly increases[10].

Further explanations behind the norm contagious mechanism have been advanced by **Garcia et al**. (**2020**). They find that when individuals receive unofficial information about the compliance behaviours from their peers’ group, there is strong evidence in favour of social conformity effects. On the contrary, when the source of information is official, taxpayers show different behaviours depending on whether they were evaders or compliers in the previous period[11]. **Fortin et al**. (**2007**), instead, when studying the impact of social interactions on tax evasion, provide evidence of fairness

[9] The asymmetric effect seems in contrast with the previous cited “conditional norm compliance” (**Diekmann et al.**, **2015**). However, notice that in **Diekmann et al.**’s (**2015**) experiment, individuals in the treatment group observe only norm violations, and not pro-social behaviour.

[10] **Kamm et al.** (**2020**) find a similar result considering an history of either a good or bad quality institution. When a low-quality institution (exogenously) changes into a high-quality one, the compliance rate stays low. Instead, when the good-quality institution is replaced by the bad-quality, the overall tax evasion rate increases. Again, this could be seen as an asymmetric effect of past behaviours.

[11] If the taxpayer is a complier and gets to know that the average tax evasion rate of the group is high, in the next period she behaves against the social conformity effect. The opposite is not true though.

effect – i.e., when an individual perceives that her tax burden is unfair, she is more likely to underreport taxes to re-establish fairness – rather than social conformity.

Differently from previous works, that investigate the effect of disclosing the social information on unethical behaviours, we are interested in inspecting what is the effect of knowing the number of caught violators. To our best knowledge, only one paper explores how information on enforcement efforts can affect individuals' compliance. More specifically, **Alm et al**. (**2009**) study the effect on tax evasion rate of both official and unofficial (communications among participants) information about the audit results and about the total fines collected via audits. They also vary whether the participants are told the audit probability previous to reporting their income or not. **Alm et al**. (**2009**) find that when the audit probability is pre-announced, official information on the number of participants audited in the previous round increases tax compliance. Instead, when the audit rate is not pre-announced, official information negatively affect the individuals' behaviours in reporting taxes, while unofficial information about audit results reduces tax evasion. Our work differs in that we want to examine if informing the individuals on the number of caught evaders would allow them to deduce the tax evasion rate, and how this inference, conditional on prior beliefs, can impact compliance behaviours.

The second stream of studies we contribute to investigates the impact of the audit probability on unethical behaviours. The findings of **Berninghaus et al.** (**2012**) point in the direction of our study: the decision of individuals to engage in a corrupt action is not affected by their risk aversion but is influenced by their beliefs on others' behaviour. Moreover, a higher degree of strategic uncertainty reduces corruption. Analogous conclusions are made by **Tan and Yim** (**2014**). In their paper, they compare the efficiency of two audit rules: the flat audit probability and the "bounded rule"[12]. What emerges is that increasing the degree of strategic uncertainty among individuals leads to a lower level of tax evasion, without increasing the maximum number of possible audits in the "bounded rule" scheme.

Unlike in our experiment, **Berninghaus et al.** (**2012**) and **Tan and Yim** (**2014**) consider an audit rule that accounts for strategic uncertainty. Their auditing mechanism resembles more the real world one, in which there is a fixed amount of resources used to detect crimes and an increase of offenders lower the probability of catching them[13]. However, our focus is to observe whether individuals are able to infer the audit probability when they are informed on the number of caught evaders. Hence, such a design would make our experiment even more complex, without allowing us to accurately answer our research question.

Although in our tax evasion game we do not use an audit scheme with strategic uncertainty, we include an unknown audit probability. As underlined by many (**Spicer and Thomas**, **1982**; **Alm**, **1988**; **Alm et al**, **2009**; **Lefevbre et al**, **2015)**, the probability of being audited is unknown in the real world and tax authority never publicly announces it. Despite this, most of the experimental studies done on tax evasion provide laboratory participants with the exact probability of being detected[14]. As in our study, **Choo et al**. (**2016**) investigated the effect of introducing an unknown audit probability but found no significant effect with respect to the treatment with a known audit probability[15]. Though,

[12] There is a fix amount of audits that tax authority can run (**Tan and Yim**, **2014**).

[13] Or, similarly, a high number of offenders decreases the amount of resources gathered and employed by the state to fight criminality.

[14] **Fortin et al.** (**2007**); **Alm et al.** (**2017**); **Alm et al.** (**2019**); **Garcia et al.** (**2020**).

[15] In their paper the audit probability, both when the this is public information and when is unknown, is equal to 20%. Instead, as we will explain in the next section, in our experiment the audit probability is equal to 30%.

in our paper, we inform citizens about the number of people caught evading taxes. From this information, individuals can form beliefs on the audit probability, which is not possible in the paper by **Choo et al**. (**2016**).

To the best of our knowledge, our research is the first one to investigate how individuals form beliefs on either the tax evasion rate or the audit probability when they are informed about the enforcement outcomes. **Alm et al**. (**2009**) indeed let the question open for further research. Specifically, they claim that "*The key policy issue is how information dissemination regarding enforcement efforts subsequently affects compliance. Our focus is on revealed behavior. It is certainly of interest to know how individuals incorporate information and adjust their prior probabilities*" (**Alm et al**., **2009**; footnote 19, pp. 398).

The third branch of studies we refer to looks at the impact of salience bias on individuals' decisions. As defined by the psychologists **Taylor and Thompson** (**1982**), salience (bias) describes the circumstance in which an individual overweight an information to which her attention has been directed to. This disproportionate overvalue of the information would lead the individual to take inefficient or irrational decisions. On the overweight of phenomena or information, a great contribution has been given by **Tversky and Kahneman**, (**1973**). By conducting several studies, they provide evidence of subjects' bias judgements on the frequency of events by availability of information. More recently, **Bordalo et al.** (**2012**) build on the salience bias effect a theoretical model that analytically explains several empirical phenomena[16].

Salience bias is considered one of the nine most important effects on individual's behaviour according to the MINDSPACE framework (**Dolan et al**., **2012**). Indeed, this structure has been increasingly used by policymakers and researchers to design interventions that can nudge people decisions. For example, in the well-known field experiment run in a grocery store by **Chetty et al**. (**2009**), they study whether people react when their attention is shifted on the sale tax. They show that when a tag with the tax-inclusive price[17] is added next to the one with the original price people decrease their demand compared to when they pay the sale tax at cashier's desk.

Despite the extended literature that have looked at the impact of information on either the tax evasion rate of others (**Lefebvre et al.**, **2015**; **Alm et al**., **2019**) or the audit probability (**Alm et al**, **2009**) on tax compliance, to our knowledge no paper seems to have explored a possible information salience bias. According to the mainstream theory, in which the individual is fully rational, we should find no effect in providing similar information but with different salience. However, according to the aforementioned literature, highlighting one information rather than another can lead individuals to different choices.

3 Experimental Design, information treatments and procedures

Our experiment consists of a "*pilot-p*" treatment, a "*no-info*" treatment and four information treatments[18]. In this section, we first present the experimental design of the "*pilot-p*" treatment. Then we explain the main differences between this treatment and the various treatments. Finally, we describe the experimental procedure.

[16] See also the model extensions in **Bordalo et al**. (**2013**, **2020**). Moreover, for a broader literature review on the salience bias theory, see **Bordalo et al**. (**2021**).
[17] They applied this to a basket of products.
[18] See the instructions in Appendix A.1.

3.1 Experimental design of "pilot-p" treatment

The experimental design of the "*pilot-p*" treatment is divided in three main parts. In the first one, individuals perform a real-effort task that is paid on a piece rate basis. It consists of counting the number of zeros within a table of 150 ones and zeros (**Abeler et al.**, **2011**). Subjects have to complete as many tables as possible within 4 minutes. Moreover, for each table they have 3 attempts to provide the correct number zeros. Whether they correctly count the zeros in a table, they gain 800 points, and a new table is generated; instead, if they fail the three attempts, they lose 800 points, unless they have a total number of points equal to 0. In this latter case, they lose nothing. We choose this task because it does not require any specific ability or prior knowledge. Moreover, the real-effort task is tedious, and so we expect that it requires a positive cost of effort for participants.

The second part of the experiment is further split up in two phases. Before the first phase begins, participants perform again the real-effort task they did in the first part[19], but now the earnings obtained represents the fixed component of their income. This fixed component of the income can be viewed as the fix wage an individual receives from her job (**Alm et al.**, **2019**). According to **Durham et al**. (**2014**) findings, when individuals perform a real-effort task within a tax evasion game they exhibit a stable compliance rate over time. Indeed, the experimental results of **Durham et al**. (**2014**) show that the interaction between earned income and loaded instruction (tax evasion frame) displays a steady dynamic across time of compliance level. Therefore, we consider a real-effort task in our design as we want that the variation in compliance from period to period is driven by a possible treatment effect rather than by a response to the experimental set-up. Moreover, **Durham et al**. (**2014**) observe that the joint presence of earned income and loaded instructions affects the link between income level and compliance rate: as the individual's income increases the compliance level decreases. Then, the first phase starts, and subjects are informed that they will play the game described below for 10 periods.

Each period is played in the same way and consists of four stages. In the first stage, individuals are endowed with the fixed component of their income. This amount is the same in each period. In addition to the fixed component of the income, subjects also receive a random component of their income. The latter is randomly extracted from a uniform distribution that has a lower bound of 2000 points and an upper bound of 5000 points. The random component of the income varies in each period. This can be seen as the variable amount of money individuals receive each year that is uncertain and cannot be predicted (**Alm et al.**, **2019**).
In the second stage, subjects report an integer amount of their total income $d_i \in [0, I_i]$ on which taxes are paid at the flat rate $\tau = 0.35$. The tax rate is common knowledge.
In the third stage, participants may be audited with probability $p = 0.30$, which is exogenous and unknown to the participants[20]. If the participant has reported all her total income and she is audited, nothing happens; if, instead, the participant has not declared all or part of her total income and she is controlled, she must pay a fine rate $\vartheta = 2$ per each income unit unreported[21]. Therefore, with (unknown) probability p she must pay a fine equal to $\vartheta \cdot \tau \cdot (I_i - d_i)$[22].

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[19] The real-effort task performed in the first part allows to measure for subjects' productivity.
[20] In the instructions we specified that the audit probability is independent from one period to another (Appendix A.1.).
[21] This is a variant of the pioneer tax evasion game of **Friedland et. al** (**1978**).
[22] In the standard theoretical model of income tax evasion (**Allingham and Sandmo**, **1972**), the individual maximises the expected utility of the evasion gamble. Hence, when considering this approach, a risk-neutral individual maximises the expected value of the gamble (see **Alm et al**., **1995**; **Alm et al**., **2019**). If no public good is considered, the expected value of the individual is: $EV_i = p \cdot [I_i - \tau \cdot d_i - \vartheta \cdot \tau \cdot (I_i - d_i)] + (1 - p) \cdot (I_i - \tau \cdot d_i)$. Maximizing this expression by the amount of income to report, d_i, we obtain that the optimal choice for a risk-neutral individual is to report $d_i = 0$

In the last stage, subjects receive feedback on how much they gain in the period, on whether they have been audited or not, and on the amount of the fine they have to pay in case they underreported part or all of their income and are audited[23].

Only after completing this first phase, participants are instructed that they will play a second phase that consists of other 10 periods. Before starting the first period of this second phase, they receive the information on the audit probability, which is, as before, equal to 0.30[24]. Hereinafter, we call this break between the first and the second phases the "informational interphase". Then, the 10 periods of the second phase are played. These are structured as before, and the fixed component of the individual's income is the same as the one in the first phase – i.e., the fixed component of the income is that determined with the real-effort task performed at the beginning of the second part of the experiment.

In the third and last part of the experiment, we control for individuals' risk preferences. We let participants to perform the BRET ("Bomb' risk elicitation task" by **Crosetto and Filippin**, **2013**) to measure their risk aversion. Moreover, they fill-in a questionnaire in which we ask socio-demographic characteristics.

3.2 Information treatments and "no-info" treatment

Considering the four information treatments- "*info-caught*", "*info-evaders*", "*info-prob*", and "*full-info*"- there are two main differences with respect to the "*pilot-p*" treatment: the information that subjects receive and the elicitation task they have to perform before receiving such information. In particular, the "informational interphase" – i.e., the interval between the first ten periods of the tax evasion game and the second ten periods – of these treatments is divided in two steps, of which the first one is the same for all the four information treatments. In this first step, participants are elicited beliefs on the first 10 periods of the "*pilot-p*" treatment. They are asked the average number of individuals per period they think have been caught evading taxes (N) and the average number of individuals per period they believe evaded taxes (E), no matter whether they got caught or not[25]. These beliefs are elicited with monetary incentives. If the subject correctly indicates the average number, she receives 2000 points. If she answers one number above or below, she gets 1000 points. Finally, if the participant writes two numbers above or two below, she wins 500 points.
In the second step of the "informational interphase", participants receive information depending on which of the four treatments they have been randomly assigned to. As for the beliefs, this information is drawn from the first 10 periods of the "*pilot-p*" treatment. In the "*info-caught*" treatment we informed participants on the average number of individuals per period that have been fined for evading taxes, N. In the "*info-evaders*" treatment subjects receive information on N, as in the "*info-caught*" treatment, and on the average number of subjects per period that evaded taxes, no matter if they have been audited or not, E – i.e., the tax evasion rate. In the "*info-prob*" treatment individuals

when $\vartheta \cdot \tau < 1$ (see **Alm et al**., **1995**; **Alm et al**., **2019**). Notice that this holds when the audit probability p is known. However, in the first phase of the second part of our experiment p is not common knowledge.

[23] Providing feedback at the end of each round may lead to the so-called "bomb crater effect" (**Mittone**, **2006**). This effect consists in a reduce compliance of an individual after she has been audited. We controlled for this effect.

[24] As in the first phase, we specified that this audit probability is independent across periods.

[25] In the instructions we called the first ten periods of the "*pilot-p*" treatment "other sessions". Moreover, we specified that in these "other sessions" individuals played the same game, with same rules and payoffs.

are provided with information on N and on the probability p of being audited. In the "*full-info*" treatment subjects are informed on N, on E and on the audit probability p.

We also run a "*no-info*" treatment, in which participants receive no information during the "informational interphase". Hence, differently from the "*pilot-p*" treatment, in the "*no-info*" treatment the audit probability is unknow in all the 20 periods of the second part of the experiment. Moreover, the "*no-info*" treatment diverges from the four information treatments in that during the "informational interphase" participants neither receive any information nor are elicited beliefs. This "*no-info*" treatment allows to observe whether there are any stop-and-go effects or learning process.

The rest of the experimental design is the same for all the treatments. Table 1 summaries the second part of the experiment of all the treatments.

Table 1- Summary of the second part of the experiment of all the treatments

	Treatment / Information	*Pilot-p*	*No-Info*	*Info-Caught*	*Info-Evaders*	*Info-Prob*	*Full-Info*
Phase 1	*Avg caught evaders (N)*	NO	NO	NO	NO	NO	NO
	Avg evaders (E)	NO	NO	NO	NO	NO	NO
	Audit probability (p)	NO	NO	NO	NO	NO	NO
Informational Interphase	*Step 1*	Receive information	No information	Elicitation beliefs	Elicitation beliefs	Elicitation beliefs	Elicitation beliefs
	Step 2			Receive information	Receive information	Receive information	Receive information
Phase 2	Treatment / Information	*Pilot-p*	*No-Info*	*Info-Caught*	*Info-Evaders*	*Info-Prob*	*Full-Info*
	Avg caught evaders (N)	NO	NO	**YES**	**YES**	**YES**	**YES**
	Avg evaders (E)	NO	NO	NO	**YES**	NO	**YES**
	Audit probability (p)	**YES**	NO	NO	NO	**YES**	**YES**

Notice that in all the treatments we run – i.e., the "*pilot-p*", "*no-info*" and the four information treatments – in the first phase of the second part of the experiment participants have the same information and they ignore the audit probability. Moreover, the audit probability applied in all the treatment is the same – i.e., $p = 0.30$. However, only in the "*pilot-p*", "*info-prob*" and "*full-info*" treatments participants are (explicitly) informed on this probability during the "informational interphase". In the "*info-evaders*" treatment subjects can infer the audit probability since they get to know the average number of caught evaders (N) and the tax evasion rate (E) before starting the second phase of the second part of the experiment. Similarly, in the "*info-prob*" treatment participants receive information on both N and the audit probability in the "informational interphase", hence they can deduce the tax evasion rate (E). Therefore, in the second phase of the second part of the experiment of the "*info-evaders*" and the "*info-prob*" treatments subjects (theoretically) have all the information, as in the "*full-info*" treatment. However, in the "*info-evaders*" treatment we highlight the tax evasion rate (E), while in the "*info-prob*" treatment we make salient the audit probability p.

3.3. Experimental procedures

Participants were recruited through the ORSEE platform (**Greiner**, **2004**). In total, 266 individuals (67.29% female) participated in the experiment and completed it[26]. Among them, 236 were students at Cà Foscari University in Venice, either from economics (129) or other tracks[27] (107). The other 30 participants were not students. We run 21 sessions and each subjects participated only once. In table 2 we present the details of these sessions[28].

Table 2- Details of the experimental sessions by treatment

Treatments	*Pilot-p*	*No-info*	*Info-caught*	*Info-evaders*	*Info-prob*	*Full-info*	*Total*
Total	*27 (2)*	*28 (2)*	*52 (4)*	*63 (5)*	*47 (4)*	*49 (4)*	*266 (21)*
%Female	*66.67*	*82.14*	*67.31*	*60.32*	*72.34*	*63.27*	*67.27*
Avg age	*24.29*	*24*	*24.42*	*23.04*	*22.57*	*23*	*23.45*

In the row of "Total", the numbers are those of participants, while the numbers of sessions are in parentheses.

Because of the Covid-19 pandemic emergency, we run the experiment through z-Tree Unleashed (**Fischbacher**, **2007**; **Duch et al.**, **2020**). This novel approach allows to run an experiment programmed on the z-Tree software outside the laboratory – i.e., online. Besides z-Tree Unleashed, we used the web platform Zoom to communicate with participants[29].

Participants received instructions in non-neutral terms[30] because we are interested in analysing income underreporting as misbehaviour, so that it was necessary to provide a context familiar to individuals[31]. Moreover, the instructions for the first part of the experiment were shown on the

[26] We had to exclude one participant because she could not finish the third part, the BRET (**Crosetto and Filippin**, **2013**).
[27] Students were from heterogenous degree courses, such as history, oriental languages, literature, etc.
[28] For further explanation on why the number of subjects differs across treatments see Appendix B- power analysis.
[29] See Appendix A.2 "*Further experimental procedure*" for a complete description of the online procedure.
[30] According to **Alm et al**.'s (**1992b**), individuals' compliance behaviour in experiments that use loaded instructions is not different from that observed in experiments that adopt a neutral language.
[31] We used terms such as tax, audit probability, income, fine, but we did not include words such as tax evasion, cheating, unlawful, fraud (see Lefebvre et al., 2015, page 410 footnote 14).

experiment page and read aloud. To ensure that individuals understood the rules of this first part, they had to correctly answer some comprehension questions before starting the task. Only after the completion of this first part, individuals were shown and read the instructions for the next part. At the beginning of the second part of the experiment, subjects were explained only about the first phase of this second part. Then, when all subjects have correctly responded to the comprehensive questionnaire of this part and completed the first phase of the second part, they received the instructions for the "information interphase" and the second phase. Once the second part of the experiment was over, participants were displayed the instructions of the third part of the experiment on the experiment page and the experimenter read them aloud. Before starting the BRET task, subjects were required to correctly answer comprehension questions relative to the rules of this last part.

Once the experiment was over, participants had to fill-in the socio-demographic questionnaire, in which they also reported their PayPal e-mail address. They were paid within 2-3 working days through PayPal and this was common knowledge (see Appendix A.1. Instructions). Average earnings were 16€, including a show-up fee of 3€.

4 Hypotheses

Our first main goal is to study how the information about the number of caught evaders, by interacting with individuals' prior beliefs, affect the tax compliance behaviours of individuals. We assume that disclosing information on the number of people that have been found evading taxes leads taxpayers to infer either the tax evasion rate of their peers or the audit frequency so that information on how many people have been found evading taxes suggests indeed the probability of being audited.

Suppose, for example, that the media report a certain number N of people denounced for evading taxes and that this is perceived as high by the citizen relatively to her prior beliefs. This taxpayer can infer either that i) most of the people evade taxes; or that ii) the audit probability is high. Providing information on norm violators have been found to decrease individuals' norm compliance (**Alm at al., 2009**; **Gino et al.**, **2009**; **Dieckmann et al.**, **2015**; **Lefebvre et al.**, **2015**; **Bicchieri et al.**, **2020**). Hence, we expect that, if the individual infers that the tax evasion rate among her peers is high, she will evade more. Instead, the effect of the audit probability on compliance have been observed to be controversial[32]. Notice that in our framework individuals are not directly informed on the audit probability, but they infer it from the information about caught tax evaders. In this regard, **Alm et al.** (**2009, p. 295**) observe that when taxpayers are not told the probability of being audited, "information reporting high audit activity will increase the subjective probability of an audit". Similarly, we expect that when the individual perceives that the number of caught evaders is high – relative to her prior beliefs – and infers that the audit probability is high, she will comply more. Based on these insights, we can state our first Hypothesis.

Hypothesis 1. *When individuals perceive that the number of caught evaders N is relatively high with respect to their prior beliefs, and they infer a high tax evasion rate, we expect that they evade more.*

[32] For example**, Alm et al.** (**2019**) find that a higher audit rate fails to increase compliance, rather it is marginally significant and negative correlated with the reported income. **Blackwell** (**2010**), instead, observes that increasing the audit probability leads to a lower tax evasion. In general, experimental studies suggest that the effect of increasing audit frequencies is non-linear: raising the audit frequency reduces its deterrent impact, and sometimes it can backfire (**Alm**, **2019**).

On the contrary, if individuals derive a higher audit probability from the information about the number of caught evaders N, we suppose that they evade less.

Suppose now that the taxpayers perceive the number of caught evaders N as relatively low with respect to their prior beliefs. Individuals can form either one of these two inferences: i) most of the people pay taxes; or ii) the audit probability is low. While empirical information on norm violations have been found to increase anti-social behaviours, the opposite is not always true (**Lefebvre et al., 2015; Kamm et al., 2017**; **Dimant**, **2019**; **Bicchieri et al.**, **2020**) [33]. It has been found that pro-social behaviours are not as contagious as anti-social behaviours. Indeed, if taxpayers suppose that the most of their peers comply, we expect that either individuals will increase their compliance[34] or that the compliance rate does not change significantly. Instead, if individuals infer a low audit probability, we assume that they will comply less (**Malézieux**, **2018**). According to these reasoning, we state our second hypothesis.

Hypothesis 2. *When individuals perceive that the number of caught evaders N is relatively low with respect to their prior beliefs, and they infer a low tax evasion rate, we expect that they evade less or as before. On the contrary, if individuals derive a lower audit probability from the information about the number of caught evaders N, we suppose that they evade more.*

To assess these two hypotheses, we will consider the "*info-caught*" treatment, in which individuals receive the information on the average number of caught evaders N after they played for 10 periods the tax evasion game. Hence, we will exploit the fact that individuals first play without information[35] and then they perform again the tax evasion game after receiving the information (i.e., within-subjects dimension). Moreover, the "*info-caught*" treatment allows to consider whether individuals perceive the observed number of caught evaders N as relatively high or relatively low with respect to their elicited beliefs. Looking at individuals' perceptions and at whether they evade more or less after receiving the information on the number of caught evaders, we can test both hypotheses.

The second aim of this paper is to test whether highlighting either the tax evasion rate or the audit probability can result in a salience bias effect. In the "*info-evaders*" treatment, in which individuals receive information on the number of caught evaders N and on the tax evasion rate E, subjects can infer the audit probability. In the "*info-prob*" treatment, in which individuals get to know both N and the audit probability, they can deduce the evasion rate. Therefore, these two treatments, the "*info-evaders*" and the "*info-prob*" treatments, theoretically provide the same information as the "*full-info*" treatment, in which we inform subjects on N, E and p. However, in the "*info-evaders*" treatment we highlight the tax evasion rate E and in the "*info-prob*" treatment we point out the audit probability, while in the "*full-info*" treatment all the information – i.e., on N, E and p – are explicitly given. When comparing the "*info-evaders*" with the "*full-info*" treatment, we should not observe any difference in tax compliance. Whether this is not the case, we can assume that there is a salience bias effect in highlighting the information on the evasion rate.

[33] Asymmetric effect of social norms and information on tax compliance.
[34] However, we predict that this increase in compliance is lower in magnitude than the decrease in compliance observed when individuals are exposed to norm violators (asymmetric effect).
[35] As explained before, they also ignore the applied audit probability.

Hypothesis 3a. *When comparing the compliance behaviours of individuals that receive information on the number of caught evaders N and on the tax evasion rate E with those who receive information on N, on E and the audit probability, we should find no differences.*

Similarly, when comparing the "*info-prob*" treatment with the "*full-info*" treatment there should be no difference in tax compliance rate. Whether we observe an effect in highlighting the information on the audit probability, we presume there is a salience bias effect.

Hypothesis 3b. *When comparing the compliance behaviours of individuals that receive information on the number of caught evaders N and on the audit probability p with those who receive information on N, on E and the audit probability, we should find no differences.*

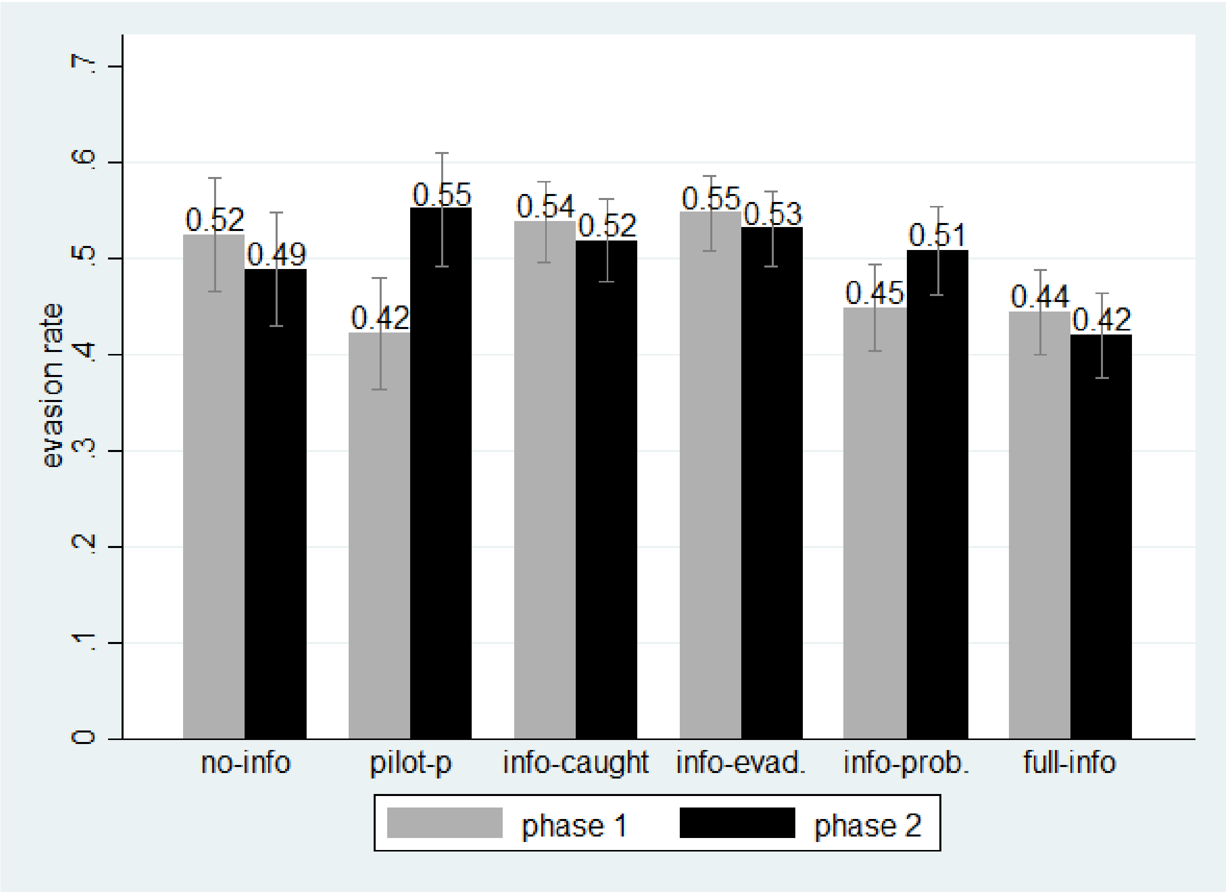
5 Results

In this section, we first present a descriptive analysis and a non-parametric test on the tax evasion rates from the various treatments. Then, we show the results from a regression analysis of the determinants of the decision to evade taxes. Finally, we report the estimation results on the choices of tax evasion from the comparisons of information conditions.

5.1 Descriptive analysis

Figure 1 shows the frequency of tax evaders relative to the total number of subjects by treatment and phase – i.e., by the first 10 periods of the tax evasion game, in which individuals receive no information, and the second 10 periods, in which individuals receive information[36]. We consider as evaders all the subjects that underreported either all or part of their total income.

Figure 1- Frequency of evaders relative to the total number of subjects by treatment and phase.



[36] In the first 10 periods, participants have the same information, and the audit probability is unknown.

The McNemar test indicates that participants evade significantly more in the second phase than in the first phase only in two treatments (Table 3). In particular, when they receive information on the probability of being audited – i.e., "*pilot-p*" treatment – and when they get to know the number of caught evaders N and the audit probability – i.e., "*info-prob*" treatment.

Table 3- McNemar test on the difference between the first and the second phase

Treatment	1st phase	2nd phase	Difference
No-Info	0.53 (0.5)	0.49 (0.5)	-0.04
Pilot-p	0.42 (0.49)	0.55 (0.5)	0.13***
Info-caught	0.53 (0.5)	0.52 (0.5)	-0.01
Info-evaders	0.55 (0.5)	0.53 (0.5)	-0.02
Info-prob	0.45 (0.5)	0.51 (0.5)	0.06**
Full-info	0.44 (0.5)	0.42 (0.49)	-0.02

Notes: * p<0.10, ** p<0.05, *** p<0.01. Standard deviations are in parenthesis.

Notwithstanding, the significant effect of these two treatments – i.e., the "*pilot-p*" and the "*info-prob*" treatments – should be taken with caution. As previously said, in the first 10 periods of all the treatments participants have the same information, and the audit probability is unknown. Therefore, we should observe that in the first phase the average evasion rate is similar across treatments. However, this is not the case: in the "*pilot-p*" and the "*info-prob*" treatments (and also in the "*full-info*" treatment) the average evasion rate in the first phase is lower than that in the first phase of the other treatments. The significant increase in the frequency of tax evaders resulting from the McNemar test could be due to the fact that in both the "*pilot-p*" and the "*info-prob*" treatments the evasion rate in the first phase is lower and in the second phase it reaches the average rate observed in other treatments.

However, it is worth pointing out that both figure 1 and table 3 do not consider individuals' prior beliefs about the number of caught evaders N, which are key features for our main research questions. In the following analysis, we take this into consideration by running a regression analysis of the impact on tax evasion of giving information about caught evaders and individuals' beliefs about this information.

5.2 A regression analysis of the impact of information about caught evaders on tax evasion

We exploit the within-subject design of the "*info-caught* treatment" – i.e., individuals receive information on the number of caught evaders N before starting the second phase – to test our hypotheses 1 and 2. In particular, we use a random effect probit model to estimate whether receiving the information on the number of caught evaders affect the individuals' compliance behaviours. We also investigate for other determinants that might affect tax compliance. Differently from the above preliminary analysis, we consider the individual's prior beliefs on the number of caught evaders N[37].

In table 5, we estimate three specifications of the individual's decision to evade taxes[38]. The binary dependent variable *Evasion* takes value equal to 1 if the participant has underreported her income, no matter which amount. Our main independent variables of interests are *Low N Beliefs*, "*Info-caught*" treatment and the interaction term *Low N Beliefs* × "*Info-caught*" treatment. In particular, the independent dummy variable *Low N Beliefs* takes value 1 when participants perceive the observed number of caught evaders N as low relatively to their beliefs[39] (beliefs $N > obs\ N$), and value 0 in the opposite case – i.e., when subjects perceive the observed N as high compared to their beliefs (beliefs $N < obs\ N$)[40]. "*Info-caught*" treatment is the independent dummy variable that takes value 0 in the first phase of the tax evasion game, when individuals have no information, and value 1 in the second phase, when they receive the information on N. Since we add the interaction term between the *Low N Beliefs* and the "*Info-caught*" treatment dummy variables, the coefficient of "*Info-caught*" treatment accounts for the effect of receiving the information on the number of caught evaders N conditional on perceiving N as high compared to individuals' elicited beliefs. Instead, the coefficient of the interaction term indicates how much individuals who perceive N as high and low, relative to prior beliefs, differ in the probability of evading taxes between each other and between before and after the informational treatment – i.e., before and after receiving the information on the number of caught evaders. The effect of being informed about the number of caught evaders N conditional on perceiving N as low compared to prior beliefs is given by the sum of the coefficient of the "*Info-caught*" treatment and that of the interaction term.

In specification 1, we include the logarithm of the total income, *Ln total income*, which is a continuous variable that considers the logarithm of the initial income (fixed and random components) a participant has at the beginning of the period[41]. We also consider two other independent dummy variables: *Evaded in prev. period*, which takes value 1 if the subject has evaded in the previous period, and *Controlled in prev. period*, which takes value 1 if the participant has been audited in the previous period. This latter dummy variable control for the so-called "bomb crater effect" (**Mittone**, **2006**) – i.e., individuals comply less in the current period when they have been audited in the previous one. Finally, we include the variable *Period*, which takes value from 1 (period=1) to 20 (period=20), and control for possible learning process over time.

[37] In Appendix C. we present the means and the standard deviations of beliefs on the number of caught evaders N and on the evasion rate E reported by participants in each treatment (table 4).

[38] Because subjects are observed for 20 periods, we consider random effect to control for unobserved heterogeneity. We can reject the null hypothesis of the absence of unobserved individual heterogeneity since the ρ coefficient in all the three specifications of Table 5 is significant.

[39] Hence, when the information on N received by individuals before starting the second phase of the tax evasion game is lower than their previously elicited beliefs on N.

[40] We dropped those participants, 6, whose elicited beliefs on N are equal to observed N (beliefs $N = obs\ N$).

[41] Recall that the random component changes each period, while the fixed component is always the same and is determined at the beginning of the second part of the experiment by the real-effort task.

Table 5 – random effects probit model

Evasion	(1)	(2)	(3)
Low N Beliefs	0.527	0.914	0.982
	(0.6972)	(0.6821)	(0.6655)
Info-caught treatment	-0.482*	-0.492*	-0.472*
	(0.2802)	(0.2801)	(0.2771)
Low N Beliefs × *Info-caught* treatment	0.668**	0.682**	0.669**
	(0.3073)	(0.3117)	(0.2992)
Ln total income	1.167***	1.197***	1.363***
	(0.3510)	(0.3616)	(0.3884)
Evaded in prev. period	0.190	0.191	0.188
	(0.1605)	(0.1607)	(0.1637)
Controlled in prev. period	0.091	0.087	0.098
	(0.1487)	(0.1491)	(0.1634)
Period	-0.029	-0.029	-0.031
	(0.0230)	(0.0230)	(0.0231)
BRET		-0.020	-0.014
		(0.0122)	(0.0126)
Age		0.070	0.061
		(0.0747)	(0.0568)
Male		-0.015	-0.235
		(0.4335)	(0.4688)
Study course			
Economics		Ref.	Ref.
Other tracks		-0.035	0.061
		(0.5628)	(0.5007)
No student		-1.029	-0.381
		(0.6883)	(0.6474)
Real-effort task (1^{st} part)			-0.000**
			(0.0002)
Ln final income prev. period			0.067
			(0.2690)
Questionnaire answers			
Progressive tax policy			-0.287
			(0.2258)
Rich pay too much taxes			0.300
			(0.2921)
Do you know tax evaders			0.260
			(0.5143)
Underreport investments profits			-0.314*
			(0.1719)
Underreport cash payments			0.090
			(0.1477)
High evasion when perceived low compliance			-0.385**
			(0.1634)
High evasion when perceived low audit prob.			0.083
			(0.1239)
Constant	-10.380***	-11.620***	-8.551***
	(2.9826)	(3.1815)	(2.8232)
N	874	874	874
L.L.	-387.918	-386.350	-379.732
Wald	19.88	22.97	38.15
Rho	.691***	.680**	.597**

Notes: * p<0.10, ** p<0.05, *** p<0.01. The dependent variable is *evasion*. Clustered standard errors at the subject level are in parenthesis. *Low N Beliefs*, *Info-caught* treatment, *Evaded in prev. period*, *Controlled in prev. period*, *Male* are dummy variables. *BRET* (bomb risk elicitation task) is given by the number of boxes collected by the individual and can take any integer value between 0 (high risk averse) and 100 (high risk seeking). *Ln total income* is a continuous variable and considers the logarithm of the initial income (fixed and random components) a participant has at the beginning of the period. *Study course* is a categorical variable that takes value 0 if the participant is in an economic track (reference point), 1 if s/he in a track different from economics and 2 if s/he is not a student. *Real-effort task* is a discrete variable that indicates the score the individual obtained in the task performed in the first part of the experiment. *Ln final income prev. period* is a continuous variable that considers the logarithm of the gains obtained by the participant at the end in the previous period. *Progressive tax policy* and *Rich pay too much taxes* can take value between 1 (strongly disagree) and 5 (strongly agree). *Do you know tax evaders* is a dummy variable. *Underreport investments profit* and *Underreport cash payments* can take value between 1 (this behaviour is perfectly acceptable) and 6 (this behaviour is not at all acceptable). *High evasion when perceived low compliance* and *High evasion when perceived low audit prob.* can take value between 0 (absolutely false) and 10 (absolutely true).

The results of specification 1 show that the coefficient of "*Info-caught*" treatment is significant at 10% level and negative, suggesting that when individuals receive the information on the number of caught evaders *N* and perceive this as high relatively to their prior beliefs, they evade less. According to our first hypothesis, this result indicates that individuals infer a higher audit probability. Instead, the coefficient of the interaction term – i.e., *Low N Beliefs* × "*Info-caught*" treatment – is significant and positive. When adding this to the coefficient of "*Info-caught*" treatment we still have a positive effect. This indicates that when the number of caught evaders *N* is perceived as low, participants evade more. Additionally, in this case it seems that participants make an inference on the audit probability, and specifically they deduce a lower audit probability (hypothesis 2). Moreover, we find that participants with a higher total income are more likely to evade taxes. Indeed, the *Ln total income* have a significant and positive effect on the decision to evade taxes. This result is in line with that of **Durham et al**. (**2014**): the joint presence of earned income and loaded instructions leads the individuals with a higher income to evade more. On the contrary, having evaded and having been audited in the previous period – i.e., respectively, *Evaded in prev. period* and *Controlled in prev. period* – do not affect the evasion choice[42].

In specification 2 of Table 5, we control for other independent variables. We add participants risk preferences that are captured by the BRET, "bomb risk elicitation task" (**Crosetto and Filippin**, **2013**). This variable counts the number of boxes collected by the individual within the task and can take any integer value between 0 (high risk averse) and 100 (high risk seeking). We also include other time-invariant independent variables that account for individual's characteristics. *Male* is a dummy variable that takes value 1 if the participant is a male and 0 if she is a female; *Age* assumes value from 17 to 52 (average 24,42)[43]; *Study course* is a categorical variable that takes value 0 if the participant is in an economic track (reference point), 1 if she is in a track different from economics and 2 if she is not a student. However, none of these additional controls either change the previous results (specification 1) nor have a significant impact on the probability to evade taxes. Indeed, both the coefficients of "*Info-caught*" treatment variable (hypothesis 1) and the interaction term between this and the *Low N Beliefs* dummy variable (hypothesis 2) are significant.

In specification 3 of Table 5, we include additional variables. *Real-effort task* is a discrete variable that indicates the points the individual obtained in the task performed in the first part of the experiment {0; 800; 1600; 2400; 3200; 4000; 4800}[44]. Even though this variable seems to negatively affect the probability to evade taxes, the coefficient is approximately zero. Moreover, we consider *Ln final income prev. period,* which is a continuous variable that considers the logarithm of the gains obtained by the participant at the end in the previous period – i.e., net of taxes and fines. This seems to have no effect on the individual's decision to evade taxes. The last set of independent variables consists of the answers to the questionnaires the participants fill-in at the end of the experiment[45]. Only the

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[42] We find no support for the "bomb crater effect" (**Mittone**, **2006**).

[43] This is true when considering the "*Info-caught*" treatment. Instead, when considering all the sessions, the participants age range between 17 and 60.

[44] Recall that in the real-effort task of the first part of the experiment, participants get 800 points whether they correctly indicate the number of 0 within a table, while they lose 800 points if they fail the three attempts, unless they have a total number of points equal to 0. Hence, this variable assumes values multiple of 800.

[45] The statements for *Progressive tax policy* and *Rich pay too much taxes* are, respectively, "Progressive taxation (a higher rate for the rich and a lower rate for the poor) is right because it allows the redistribution of wealth in society" and "Rich people have to pay too much tax". Participants could rate these two between 1 (strongly disagree) and 5 (strongly agree). *Do you know tax evaders* is a dummy variable that takes value 1 if the answer is "yes" and 0 if it is "no". The statements for *Underreport investments profit* and *Underreport cash payments* are, respectively, "Underreport some investment or interest gains the government would not be able to discover" and "Being paid in cash and then not reporting it on your tax form". Participants could rate these two between 1 (this behaviour is perfectly acceptable) and 6 (this behaviour is not

variables *Underreport investments profit* and *High evasion when perceived low compliance* show a significant and negative effect on the probability to evade, but these results must be taken with caution. Indeed, the variables obtained from the questionnaire answers may suffer from either endogeneity or justification bias, or both (**Lefebvre et al.**, **2015**). Again, the coefficients of our main independent variables of interests, "*Info-caught*" treatment and the interaction term *Low N Beliefs* × "*Info-caught*" treatment, are nearly unchanged with respect to those in specification 1 and 2.

We further test the robustness of the findings regarding hypothesis 1 and 2 by restricting the analysis to those individuals who have similar prior beliefs on p[46]. Indeed, we elicit beliefs on both the number of caught evaders and the tax evasion rate[47], from which we can calculate the individuals implicit audit probability ($\frac{beliefs_N}{beliefs_E} = implicit\ p$). We first restrict our analysis to those who have an (implicit) belief on the audit probability lower than the actual one ($p = 0.3$). However, since few people in this treatment express a low perception of p, we could not run this estimation. Indeed, only 9 participants have an (implicit) belief on the audit probability lower than 0.3, hence the regression analysis has a low statistical power. Then, we restrict our analysis to participants who have an (implicit) belief on the audit probability higher than the actual one ($p = 0.3$)[48]. The robustness tests seem to confirm our main results for hypothesis 1 and 2.

5.3 Salience bias effect

We now proceed our analysis by testing hypotheses 3a and 3b. Therefore, we first want to analyse whether participants who receive information on the number of caught evaders N and on the evasion rate E – i.e., "*info-evaders*" treatment – behave differently from those who are assigned to the "*full-info*" treatment – i.e., get information on N, E and the audit probability p. Indeed, even though the "*info-evaders*" and the "*full-info*" treatments provide the same information, in the former the evasion rate is highlighted. Then, we want to test whether subjects who are informed on the number of caught evaders N and on the probability rate p – i.e., "*info-prob*" treatment – make different income reporting decisions with respect to those who have all the information ("*full-info*" treatment). Again, participants in the "*info-prob*" and in the "*full-info*" treatments receive the same information, however in the former we emphasise the probability rate[49]. To investigate these, we estimate two difference-in-difference-in-difference (DDD)[50] models with individual fixed effects (table 6 and 7). The use of this model is justified by the fact that the percentage of evaders in the first phases of the tax evasion

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at all acceptable). The statements for "High evasion when perceived low compliance" and "High evasion when perceived low audit prob." are, respectively, "In a State, a citizen does not pay taxes when he perceives that few people pay them" and "In a State, a citizen does not pay taxes when he perceives low audit probability". Participants could rate these two between 0 (absolutely false) and 10 (absolutely true).

[46] See appendix D. We consider again a random effects probit model (table 8). However, we find that ρ is no more significant in specification 2 and 3 of table 8. Because we cannot reject the null hypothesis of the absence of unobserved individual heterogeneity, the use of the random effect probit model is less justified. Hence, we also consider a linear mixed model (table 9). We find similar results.

[47] Recall that these beliefs are elicited on the first 10 periods of the second part of the "*pilot-p*" treatment.

[48] In the "*Info-caught*" treatment, 37 participants have an (implicit) belief on the audit probability higher than 0.3.

[49] As previously said, participants in the "*info-evaders*" treatment are able to infer the audit probability, while those in the "*info-prob*" can deduce the evasion rate.

[50] We employ a triple differences (DDD) model as we also consider the individual perception of N – i.e., we distinguish individuals that perceive the observed N as relatively high with respect to their prior beliefs from those who perceive it as low relatively to beliefs.

game of the *info-evaders*", "*info-prob.*" and "*full-info*" treatments are different from each other (see Table 3).

In table 6 and 7 we present the outcome of our DDD regressions, in which the dependent variable is *Evasion* that, as in the previous analysis (table 5), takes value 1 whenever an individual underreports all or part of her income, and 0 otherwise. Table 6 compares the "*info-evaders*" treatment with the "*full-info*" treatment in affecting the decision to evade. In this DDD regression, the interaction term comprises three variables. The first one is *Highlighting evasion rate* which takes value 1 when the individual is in the "*info-evaders*" treatment and 0 when she is in the "*full-info*" treatment. The variable *Phase* takes value 1 in the second phase of the tax evasion game, and 0 in the first phase. The third variable, *Low N Beliefs*, is the one seen previously.

Table 6 – triple differences model (DDD) with individual FE

Comparing "*Info-evaders*" with "*Full-info*"

Evasion	
Highlighting evasion rate × Phase	0.048
	(0.0837)
Highlighting evasion rate × Phase × *Low N Beliefs*	-0.054
	(0.0929)
Ln total income	0.252***
	(0.0740)
Evaded in prev. period	-0.025
	(0.0340)
Controlled in prev. period	0.058**
	(0.0256)
Period	-0.004
	(0.0033)
Ln final income in prev. period	-0.004
	(0.0330)
Constant	-1.655**
	(0.6737)
R-squared	0.5388
N	2014
Individual FE	✓

Notes: * p<0.10, ** p<0.05, *** p<0.01. The binary dependent variable is *Evasion*. Clustered standard errors at the subject level are in parenthesis. *Highlighting evasion rate, Low N Beliefs, phase, Evaded in prev. period, Controlled in prev. period* are dummy variables. *Ln total income* is a continuous variable and considers the logarithm of the initial income (fixed and random components) a participant has at the beginning of the period. *Ln final income prev. period* is a continuous variable that considers the logarithm of the gains obtained by the participant at the end in the previous period.

Results of table 6 shows that, when individuals receive the information on the number of caught evaders N and on the evasion rate E ("*info-evaders*" treatment), they behave as those who receive full information. This is true both when individuals perceive the number of caught evaders N higher than prior beliefs – i.e., the coefficient of *Highlighting evasion rate* × *Phase* is not significant – and when they consider the observed N lower with respect to what they thought – i.e., the sum between the coefficients *Highlighting evasion rate* × *Phase* and *Highlighting evasion rate* × *Phase* × *Low N Beliefs* is almost zero and not significant. Therefore, highlighting the information on the evasion rate does not have any salience effect, regardless of individuals' beliefs on the number of caught evaders N. In addition, we find that the coefficient of the variable *Ln total income* is significant and positive, indicating that a higher income increases the probability of evading taxes. Furthermore, the coefficient of *Controlled in prev. period* is significant and positive: this points to a "bomb crater effect" (**Mittone**, **2006**).

In table 7, we report the effect on tax evasion when comparing the "*info-prob*" treatment with the "*full-info*" treatment. We include the same independent variables of the previous regression (table 6), except for *Highlighting audit probability*. This dummy variable takes value 1 when the individual is in the "*info-prob*" treatment and 0 when she is in the "*full-info*" treatment. We find no significant effect on the decision to evade taxes when participants are informed on the number of caught evaders N and on the audit probability p with respect to those who receive full information. Again, the coefficient *Highlighting audit probability* × *Phase* and its sum with the coefficient *Highlighting audit probability* × *Phase* × *Low N Beliefs* are not significant, meaning that there is no salience bias effect both when individuals perceive N as high with respect to their prior beliefs and when they consider N as low relatively to elicited beliefs.

In table 7, we also observe that the coefficient of *Ln total income* is significant at 1% and positive: participants with a higher income are more inclined to evade taxes. Differently from the results in table 6, we find no more support for the "bomb crater effect", as the coefficient of *Controlled in prev. period* is no more significant. Instead, we find that the coefficient of the variable *Period* is significant and negative, indicating that participants evade less as the number of periods increases. Finally, results in table 7 show that a higher income obtained by the participant at the end of the previous period, net of taxes and fines, negatively affect the probability of evading taxes (the coefficient of *Ln final income prev. period* is significant and negative).

Table 7 – triple differences model (DDD) with individual FE

Comparing "*Info-prob*" with "*Full-info*"	
Evasion	
Highlighting audit probability × Phase	0.113
	(0.1296)
Highlighting audit probability × Phase × *Low N Beliefs*	-0.040
	(0.1408)
Ln total income	0.296***
	(0.0787)
Evaded in prev. period	-0.012
	(0.0407)
Controlled in prev. period	0.030
	(0.0286)
Period	-0.007*
	(0.0038)
Ln final income in prev. period	-0.089**
	(0.0377)
Constant	-1.358*
	(0.7073)
R-squared	0.5036
N	1748
Individual FE	✓

Notes: * p<0.10, ** p<0.05, *** p<0.01. The binary dependent variable is *Evasion*. Clustered standard errors at the subject level are in parenthesis. *Highlighting audit probability*, *Low N Beliefs*, *Phase*, *Evaded in prev. period*, *Controlled in prev. period* are dummy variables. *Ln total income* is a continuous variable and considers the logarithm of the initial income (fixed and random components) a participant has at the beginning of the period. *Ln final income prev. period* is a continuous variable that considers the logarithm of the gains obtained by the participant at the end in the previous period.

6 Conclusions

We run a laboratory experiment to study the effect of disclosing information about the enforcement outcomes – i.e., the number of individuals caught evading taxes – on the individuals' compliance behaviours. This method allows to control both for individuals' prior beliefs on the number of caught evaders and for the inference they make – on either the evasion rate or the audit probability – from the information about the enforcement outcomes. Moreover, our experimental design allows us to observe whether highlighting either the tax evasion rate or the audit probability can result in a salience bias effect.

One of the most important results of our study is that individuals' prior beliefs play a fundamental role in defining how information on the number of caught evaders affects individuals' tax evasion behaviour. Moreover, whether individuals perceive the information about the enforcement outcomes as relatively high or relatively low with respect to their beliefs, they always infer the audit probability. Indeed, we find that when an individual perceives that the observed number of caught evaders is higher than believed, she infers a higher audit probability and evade less. When, instead, an individual is informed that the number of people caught evading taxes is lower than supposed, she deduces a lower audit probability and evade more.

Another important finding of our paper is that we do not observe a salience bias effect when considering individuals to whom we highlight information about others' norm violation nor when looking at those to whom we emphasise the probability of being audited. In other words, when individuals receive information on the number of caught evaders and on the tax evasion rate, they infer the audit probability; while when participants get to know the number of people caught evading taxes and the audit probability, they deduce the evasion rate. Even though in the former case we highlight the evasion rate and in the second we emphasise the probability rate, participants in these two conditions behave as those who receive (explicitly) all information – i.e., on the number of caught evaders, on the evasion rate and on the probability rate.

Individuals are constantly exposed to information on how many people have been caught evading taxes, and in general on how many have been arrested or fined for other crimes or misbehaviours. However, little is known about how this information, by interacting with individuals' prior beliefs about the number of caught people, affects their misbehaviour in turn. Our findings suggest that should be posed caution in spreading information, as one should consider individuals' perceptions. Moreover, highlighting either the evasion rate or the audit probability, within the tax evasion context, seems to have no salience bias effect. Therefore, even though salience manipulation is an important tool used by policymakers (**The behavioural insight Team**[51]) to address individuals' (mis)behaviours (**Dolan et al**., **2012**), it might not apply when it comes to tax evasion.

As in most experimental papers, our study abstracts away from many elements of real life so to cleanly identify specific effects and motivations. Moreover, the number of participants that could be observed within a laboratory experiment is small. However, this limit can be easily overcome by replicating this study with an increasing number of subjects. A further limitation that can be addressed to our analysis concerns the fact that we only indirectly observed that individuals infer the audit probability when receiving information on the number of caught evaders. In fact, it could also be that the inference on the likelihood of being audited is due to a learning process. Still, the latter cannot fully explain our findings because individuals' prior beliefs seem to play an important role. It would be interesting for future research to directly study whether individuals infer the audit probability when

[51] https://www.bi.team/publications/mindspace/.

receiving the information on the number of caught evaders. Finally, our results indicate that participants with a higher income evade taxes with a higher probability. Further research could explore whether there is a different impact in receiving the information about the number of caught evaders between people with a high and a low income.

Appendix A.1 Instructions

Here below we present the instructions used in the experiment. The parts written in different colour (orange: "*pilot-p*"; purple: "info-caught"; red: "*info-evaders*"; blue: "*info-prob*"; green: "*full-info*") *are for the different treatment groups, while the "usual" black writing are the instructions common to all treatments and the "no-info" treatment.*

We thank you for taking part in this economic experiment. You will receive 3€ for showing up in addition to your earnings, which will depend on your decisions according to the rules we will further explain to you in details. Once the experiment is finished, your earnings will be paid within 2-3 days through PayPal.

During the entire experiment, your webcam and your microphone should be turned off.

If, at any time, you have any questions or problems please write in the zoom chat to one of the experimenters. We will answer to your doubts via chat. If necessary, we will give you the instructions to move to another virtual zoom room where there will be one of the experimenters with whom you can communicate privately the issues encountered. The experiment consists of three independent parts and will last for 90 minutes. The instructions for each part will be given at the beginning of each of them

In each of these three parts you will have the opportunity to gain an earning, according to the instructions that we will give you in a while, and your final payment consists of the sum of these earnings (plus the show up fee). In each of the three parts, your earnings consist of points. At the end of the experiment your total points will be converted in euro at the following rate: 1000 points = 1€

During the entire experiment, you are not allowed to communicate, with either the other participants of the experiment or other external people. Leave your mobile phone in another room and please do not allow other people to enter the room while you are taking part of the experiment: all your decisions must be taken in isolation.

Here below you are given the instructions for part one. The instructions for part two will be given once part one is finished, and those for part three once part two is completed.

Part I

In this first part you have to perform a task, that from now will be called "counting zeros task". You have to count the number of zeros in a table that contains 150 between zeros and ones, randomly displaced. You have three attempts to provide the correct number of zeros. When you insert the correct number of zeros a new table is generated. For each table you solve correctly you will receive 800 points. If you fail the three attempts per table, so you enter for three times the wrong number of zeros, 800 points will be subtracted from the earnings accumulated up to that point. However, if your total earnings at the time you fail the three attempts for a table is 0, you will not lose points and go negative, but you will remain at 0 points. At the end of the experiment, you will be shown your earnings, namely your points converted in euros according to the initially indicated conversion rate (1000 points = 1 €).

Here there are two examples of possible outcomes of part 1. These examples are just to show you two different situations:

Examples 1. You correctly solved two tables and you failed three times to give the correct answer for a table. Your earnings are:

- 2 x 800 points for the two correct tables
- −1x 800 points for the mistaken table.

thus a total of 800 points

Example 2. You correctly solved two tables and you fail three times to give the correct answer three tables. Your earnings are:

- 2 x 800 points for the two correct tables
- −3 x 800 points for the mistaken three tables

thus a total of 0 points.

You will have 4 minutes (240 seconds) to complete as many tables as possible. The remaining time will be shown on the upper right-hand corner of the screen. Once the time will be over, click on the 'Next' button and you will receive the instructions for the second part of the experiment

Before starting the "table task", you have to answer the following control questions. You will not be able to move on until you have answered all the questions correctly. You will have the opportunity to consult a summary table of the instructions for this first part.

Understanding questions:

1. How many parts does the experiments consists of?
   __
2. What do you have to do in the task:
   - Count the number of ones;
   - Count the number of zeros;
   - Subtract the number of zeros from the number of ones and report the difference
3. When you correctly solve a table:
   - You earn nothing;
   - You finish part 1;
   - You earn 800 points and you are given another table

4. How many attempts you have for each table:
   - 1
   - 2
   - 3
5. If you have zero points and you fail all the available attempts you have for a table:
   - Your total earnings stay at zero points
   - You lose 800 points and have negative points

- You end the experiment.

Part II

This part consists of 10 independent periods.

At the end of Part II the computer will randomly select one period and the earnings you got in this period will be sum up to the earnings of Part I and to the show-up fee and will be paid at the end of the experiment.

At the beginning of each of the 10 periods you will receive a "total income". In each period, the "total income" consists of a "fixed component" and a "random component".

- The "fixed component" of your income will always remain the same for each of the 10 periods and is determined as follow. At the beginning of this second part, you will have to repeat the "counting zeros task" done in the first part again. Your income obtained in the "counting zeros task" will then determine the "fixed component" of your income.
  As before, you will have three attempts to indicate the correct number of zeros contained in a table in which 150 between ones and zeros are randomly displayed. If you answer correctly within the three attempts, a new table will be generated. For each correctly solved table you will receive 800 points. If you fail the three attempts per table, so you enter for three times the wrong number of zeros, 800 points will be subtracted from the earnings accumulated up to that point. However, if your total earnings at the time you fail the three attempts for a table is 0, you will not lose points and go negative, but you will remain at 0 points.

- The "random component" of your income will be a random amount between 2000 and 5000 points and will vary from one period to another, unlike the "fixed component" of your income which remains the same for all 10 periods.

[*Phase I, 10 periods*]

We now describe in detail the rules for each period of the second part. Each period is then divided in the following three stages:

i) STAGE 1: At the beginning of each period, you will receive the "fix component" of your income, which is determined by the points obtained in the "counting zeros task", and the "random component" of your income, which varies in each period and ranges between 2000 points and 5000 points. This amount will be randomly draw at the beginning of each period. The sum of these two components is your "total income".

ii) STAGE 2: You are then asked to report your income in order to pay taxes on it. On the reported income you will have to pay a flat tax of 35%.

iii) STAGE 3: Your reported income might be then subject to an audit with a certain probability. This probability is independent from one period to the other, which means that if you, for example, are audited in period 2, you will have the same probability of being audited in the next period; similarly, if, for example, you are not audited in one period, you will have the same probability of being audited in the next period.

In case your reported income is equal to your "total income" then your earnings for the period do not depend on whether your reported income will be audited or not. In any cases, your earnings for the period will be equal to your "total income" minus the taxes paid on the "total income" (which in this case is equal to the reported income).

In case your reported income is lower than your "total income" then your earnings will depend on whether your reported income will be audited or not.

- In case you are **not audited**, your earnings for the period are equal to your "total income" minus the taxes paid on the reported (if any) income.
- In case you are **audited**, you will have to pay a sanction. Specifically, this will be equal to the 35% tax paid on the unreported income times 2. So, in this case, your earnings for the period will be equal to your "total income" minus the taxes paid on the reported income minus the taxes paid on undeclared income times 2.

At the end of each period you will be told whether you were audited or not and you will receive information on your net final income for the period.

Please note that in the second part only one period will be randomly extracted for payment so that the earnings you got in this period will be summed up to the earning of the first part and paid to you at the end of the experiment.

Before starting the "counting zeros task", you have to answer following control questions. Recall that the examples we propose here are only for explanation purpose, these are not suggestions.

Examples

Example 1. Suppose that in a period of this second part you get a "total income" of 2000 points and suppose you report an income (reported income) of 2000 points.

- ➢ The taxes paid will be equal to 700 points, i.e., 35% of the reported income.
- ➢ Regardless of the auditing procedure, your earnings for the period are 1300 points ("total income" - taxes calculated on reported income, i.e., 2000 - 700).

Example 2. Suppose now that in a period of this second part you a "total income" of 2000 points and suppose you report an income (reported income) of 1000 points.

- ➢ The taxes paid will be equal to 350 points, i.e., 35% of the reported income.
- ➢ In case you are audited, you have to pay a sanction. This is equal to the 35% tax paid on the unreported income times 2, i.e., 2 x 0,35 x (2000 − 1000) = 700 points. In this case, your earnings for the period are equal to 2000 – 350 − 700 = 950 points.
- ➢ In case you are not audited, your earnings for the period are equal to 2000 – 350 = 1650 points.

Before starting the second part of the experiment, you have to answer the following control questions. You will not be able to move on until you have answered all the questions correctly. You will have the opportunity to consult a summary table of the instructions for this first part.

Understanding questions:

1. The "table task" you perform at the beginning of Part II:
   - differs from the one you perform in Part I because now you have to count the number of ones;
   - it is the same as the one you did in Part I but changes in the points you gain in case you solve the table and in the points you lose in case you fail the three attempts per table;
   - the two tasks are identical, both in what you count (number of zeros) and amount of win and loss points.
2. Your "total income" at the beginning of each period is given by:
   - a random component;
   - either a random component or a fix component
   - both a "fix component", which is the same in each period and determined by the "table task", and a "random component", which varies in each period;
   - a "fix component", which varies in each period and is determined by the "table task" that you perform each time;
3. If you have a "total income" of 4000 points and you report 4000 points, the total amount of taxes paid is:
   - 1400
   - 2500
   - 1700

   And your final income is:
   - 1900
   - 2600
   - 2300
4. If you have a "total income" of 4000 points and you report 2000 points, the total amount of taxes paid is:
   - 700
   - 300
   - 800
5. If you have a "total income" of 4000 points, you report 2000 points and you are not audited, your final income is:
   - 3100
   - 3700
   - 3300
6. When do you incur in the fine rate:
   - When you report all your income and are audited
   - When you underreport all your income or part of it and are audited
   - When you underreport part of your income or part of your income and you are not audited
7. If you have a "total income" of 4000 points, you report 2000 points and you are audited, what is your final income after you pay the taxes on declare income and the fine rate (recall: you have to subtract to the "total income" the taxes you paid on the declared income and the fine which is equal to 2x0,35xundereported income):
   - 1500
   - 1900
   - 2100

[*Phase II, 10 periods*:

As for phase I, also in phase II each period is divided in three stages, however there is a change in the 11th period only. Individuals are not told this; they only receive in period 11th information that varies depending on which treatment they have been assigned to.]

[Beliefs elicitation]

You will now have to play another 10 periods. The rules for each period are the same. Also the "fixed component" of your income is equal to the gain obtained with the "counting zero task" you did at the beginning of this second part. Again, at the end of the period you will be told if you have been audited or not and what is your final earnings for the period just played. As before, one of these second 10 periods will be randomly selected and the total earnings will be shown to you at the end of the experiment along with those for show up fee, the earnings of the first part and the earnings obtained in the period randomly selected from the first 10 periods played in the second part of the experiment.

Only for "*pilot-p*":

The audit probability in these second 10 periods that you are going to play is equal to 30%. As before, this probability is independent from one period to the other, which means that if you, for example, are audited in period 12, you will have the same probability of being audited in the next period; similarly, if, for example, you are not audited in one period, you will have the same probability of being audited in the next period.

Only for the four treatments:

Before continuing with the next 10 periods, we ask you to answer the following questions. If you answer correctly, you will get an additional earning. This will be paid at the end of the experiment, together with the show up fee, the earnings of the first part of the experiment and the earnings of the second part of the experiment.

Before this experimental session, we run other sessions identical to this one, that we call "other sessions" where 27 **other** people participated. In this "other session" individuals earned the "fixed income" of their income and received the "random component" of their income in the same way; hence, they were endowed with the "total income" in the same way. Moreover, these other participants participated to the second part of the experiment playing with the same rules you are playing now in this session

We now ask you to report your expectations about what happened in these "other sessions", noting that you can increase your earnings depending on your answer, as explained in the following. You will have to answer two questions, for each of them you will have to provide an integer number. The earning explained here below is relative to one correct answer. If you correctly answer both questions you will gain twice the earning relative to one correct answer.

The right answer could be a number between 0 and 27 (total number of people participating in the "other sessions"). Whenever your answer, to one question, is correct- i.e., you write the exact number in the box- then you will receive 2000 points. If, instead, your answer is of **one** number above or of **one** number below the right answer, you gain 1000 points. Finally, if your answer is of **two** numbers above or of **two** answers below the right answer, you gain 500 points.

Answer to the two following questions:

- What do you think was the average number of individuals per period in these "other sessions" who reported an income lower than their "total income" or who did not report any income, **no matter** they were audited or no? __

- What do you think was the average number of individuals per period in these "other sessions" who reported an income lower than their "total income" or who did not report any income **and** have been audited and fined? __

[Information treatments]

Now we will give you information about these "other sessions"

In "other sessions", where 27 people participated, a total number of 4 individuals underreported either part or all of their income and have been audited.

In "other sessions", where 27 people participated, a total number of 4 individuals underreported either part or all of their income and have been audited. Moreover, a total number of 11 individuals underreported either part or all of their income, considering both those that have been audited and those that have not been audited.

In "other sessions", where 27 people participated, a total number of 4 individuals underreported either part or all of their income and have been audited. Moreover, the probability of being audited, which is the same in this session, was equal to 30%.

In "other sessions", where 27 people participated, a total number of 4 individuals underreported either part or all of their income and have been audited. Moreover, a total number of E individuals have underreported either part or all of their income, considering both those that have been audited and those that have not been audited. The probability of being audited, which is the same in this session, was equal to 30%.

Part III

[*The following part individuals will perform a task in order to observe and control for their risk preferences and fill-in a questionnaire. The task we will use to measure individuals' risk preferences is that of* ***Crosetto*** *and* ***Filippini*** *(****2013****). In particular, we will use the dynamic "Bomb elicitation risk task" as it has several advantages, such as minimal numeracy skills, it presents the lotteries in a sequential way, the visual version is easily understandable. Moreover, the instructions are those used by the authors, adapted in some parts to our experiment*[52]*. The post-experimental questionnaire asks for socio-economic information and will be used for controls as well.*]

[52] You can find the instructions used by **Crosetto** and **Filippin** (**2013**) at the following web page: https://paolocrosetto.wordpress.com/research/bret/

The second part of the experiment is finished. Now the third and last part of the experiment will start.

Please read carefully.

As soon as you will press continue, on your PC screen will appear a squared formed by 10x10 cells. Each cell represents a box; hence the total number of boxes is 100.

As soon as you press start, from the top-left corner you will start to collect boxes, one every second. You earn 50 points for every box that is collected. Once collected, the box disappears from the screen and your earnings are updated accordingly. At any moment you can see the amount earned up to that point.

Careful: these earnings are only potential! Indeed, inside one of these boxes there is a time bomb that destroys everything if you collect it.

You do not know where this time bomb is. You only know that the time bomb can be in any place with equal probability. Moreover, even if you collect the time bomb, you will not know it until the end of the experiment.

Your task is to choose when to stop the collecting process. You do so by clicking 'Stop' at any time.

At the end of the experiment, we will randomly determine the number of the box containing the time bomb by means of a bag containing 100 numbered tokens.

If you happen to have collected the box where the time bomb is located, you will earn zero. If the time bomb is located in a box that you did not collect you will earn the amount of money accumulated when you click on the 'Stop' buttom.

Before starting the second part of the experiment, you have to answer the following control questions. You will not be able to move on until you have answered all the questions correctly. You will have the opportunity to consult a summary table of the instructions for this first part.

Understanding questions:

1. In this third part of the experiment, you will have to:
    - click on each box starting from the top-left in order to collect them and when you finish click on “Stop”;
    - click on “Stop” when you want to stop the collecting process. Indeed, when the task will start, each box will disappear from the screen starting from the top-left,;
    - click randomly on the boxes to collect them and when you finish click on “Stop”
2. Your earnings:
    - are zero if you collect the box where the time bomb is located or equal to the amount of money accumulated, until you press “Stop”, when you did not collect the box where the bomb is;
    - are half of the amount of money accumulated, until you press “Stop”, if you collect the box where the time bomb is located or equal to the amount of money accumulated, until you press “Stop”, when you did not collect the box where the bomb is;
    - are zero if you collect the box where the time bomb is located or equal to the amount of money accumulated, until you press “Stop”, times 3 when you did not collect the box where the bomb is

Post-experiment questionnaire

1. Have you ever participated in one or more economic experiments before today?
   a. Yes
   b. No
2. How old are you?
   __
3. Gender?
   a. F
   b. M
4. Nationality
   __
5. Study course
   a. Business
   b. Economics
   c. Computer or engineering science
   d. Psychology
   e. Math, physics, or chemistry
   f. Medicine
   g. Other __
   h. Not a student
6. Which year are you enrolled in?
   a. 1st (bachelor)
   b. 2nd (bachelor)
   c. 3rd (bachelor)
   d. 4th (only if a study course of four years- i.e., quadriennale)
   e. 1st (master)
   f. 2nd (master)
   g. I am not enrolled in a study course
7. On a scale between 1 and 10, how do you view your family's income?
   “1 (very low)", "2", "3", "4", "5", "6", "7", "8", "9","10 (very high)”
8. On a scale between 1 and 10, how do you view your family's wealth with respect to the Italian average?
   “1 (very low)", "2", "3", "4", "5", "6", "7", "8", "9","10 (very high)”
9. What do you think is the percentage of taxation that weigh on your family's income?
   a. "Less than 10%",
   b. "Between 10% and 15%",
   c. " Between 15% and 20%",
   d. " Between 20% and 25%",
   e. " Between 25% and 30%",
   f. " Between 30% and 35%",
   g. " Between 35% and 40%",
   h. " Between 40% and 45%",
   i. " Between 45% and 50%",
   j. " Between 50% and 55%",
   k. " Between 55% and 60%",
   l. "Over 60%"

10. What do you think should be a fair percentage of taxation for your family's income?
    a. "Less than 10%",
    b. "Between 10% and 15%",
    c. " Between 15% and 20%",
    d. " Between 20% and 25%",
    e. " Between 25% and 30%",
    f. " Between 30% and 35%",
    g. " Between 35% and 40%",
    h. " Between 40% and 45%",
    i. " Between 45% and 50%",
    j. " Between 50% and 55%",
    k. " Between 55% and 60%",
    l. "Over 60%"
11. Progressive taxation (a higher rate for the rich and a lower rate for the poor) is right because it allows the redistribution of wealth in society”
    “1 (strongly disagree)", "2", "3", "5 (strongly agree)"
12. Rich people have to pay too much tax.
    “1 (strongly disagree)", "2", "3", "5 (strongly agree)"
13. Tax evaders are usually high-income citizens.
    “1 (strongly disagree)", "2", "3", "5 (strongly agree)"
14. What do you think is the percentage of the working population that does not declare part or all of its income to the National tax administrations (in Italian, Agenzia delle Entrate) responsible for collecting taxes?
    a. Less than 1 %
    b. Between 1 % and 5 %
    c. Between 5 % and 10 %
    d. Between 10 % and 20 %
    e. Between 20 % and 30 %
    f. Between 30 % and 40 %
    g. Between 40 % and 50 %
    h. More than 50 %
15. Do you personally know someone or someone who works and does not report part or all of his/her income to the National tax administrations (in Italian, Agenzia delle Entrate)?
    a. Yes
    b. No
16. On a scale between 1 and 10, where 1 is “absolutely false” and 10 is “absolutely true”, value the following statements:
    - In a State, a citizen does not pay taxes when s/he perceives low audit probability.
    - In a state, a citizen does not pay taxes when s/he has limited moral sense.
    - In a State, a citizen does not pay taxes when s/he perceives that few people pay them.
    - In a state, a citizen does not pay taxes when s/he perceives that the tax revenue is used inefficiently.
    - In a state, a citizen does not pay taxes when the tax rate is too high.
17. On a scale between 1 and 6, where 1 is “perfectly acceptable” and 6 is “not at all acceptable”, value the following statements:

- Trade or exchange goods and services with a friend or a neighbour and do not report it on the tax form.
- Report your main income but underreport some small revenues.
- Underreport cash payments.
- Underreport some investment or interest gains the government would not be able to discover.

18. On a scale between 1 and 6, where 1 is "it does not describe me at all" and 6 is "it describes me perfectly", value the following statements:
    - I consider myself a sensitive person and worried about people less lucky than me.
    - Sometimes I do not feel sorry or sympathise with people who have problems.
    - When I see someone taking advantage of others, I become protective of them.
    - When I see someone being treated unfairly, sometimes I do not feel much compassion for him/her.
19. On a scale between 1 and 10, how much do you think you can trust other people?
    "1 (you cannot absolutely trust others)", "2", "3", "4", "5", "6", "7", "8", "9","10 (you can always trust others)"
20. On a scale between 1 and 10, how much you think helping others is a moral duty?
    "1 (helping others is not a moral duty)", "2", "3", "4", "5", "6", "7", "8", "9","10 (helping others by any means is a moral duty)"

Appendix A.2 Further experimental procedures

The procedure before starting the experiment was as follow: few minutes before the session was scheduled, subjects clicked on the zoom link previously[53] sent out via e-mail and were asked to enter an online waiting room. In this waiting room, participants could not talk between each other, nor see each other. Then, we let one subject at the time to enter the zoom main room and we checked his or her ID. During the identification procedure, we also asked the participant to turn off the camera and the audio for the entire duration of the experiment[54] and we changed his or her zoom name. Next, we sent him/her in a breaking-room- i.e., a virtual room parallel to the main one. Only when the identification process of an individual ended and she was sent to the breaking room, another subject was allowed to enter the main room from the waiting room to proceed with a new identification. In this way, subjects could never see each other with cameras and audio on. Moreover, in the breaking room there was another experimenter controlling that the subjects within this parallel virtual room were keeping the camera and the audio off.

After all participants have been sent, one by one, to the break-out room, we moved them back to the main room of zoom. We then provided the link to connect to z-Tree through the chat. Furthermore, whenever individuals have questions, they could write privately to one of the experimenters through this chat or could ask to talk with one of the two experimenters in a private zoom room[55].

[53] A few hours before the session started, we sent the zoom link to the participants who signed-up for that session.

[54] We decided to ask subjects to turn-off the cameras because we did not want that individual decision was affected by the view of others during the game. This came at a cost: we could not see whether participants were doing other things, such as chatting through the mobile phone, during the experiment. However, when we communicated with the subjects and asked if they understood the instructions, they responded quickly via chat, which meant they were paying attention and focused on the experiment.

[55] We set the zoom calls such that participants were allowed to write only to the experimenter and not to other participants.

Appendix B. Power analysis

It has become a general rule to set the alpha (probability of type I error) at the 0,05 and 1-beta (where beta is the probability of committing a type II error), which is the power of the test, at the 80%. In order to perform the power analysis and obtain the number of individuals needed to run the experiment and have a high probability of observing an effect, we consider four different papers that employ a tax evasion game and provide information to individuals, and we use their dependent variable sample mean and standard deviation. These papers are: **Alm et al.** (**2019**); **Choo et al**. (**2016**); **Alm et al.** (**2009**); **Fortin et al**. (**2007**).

We use the "Control" treatment of the paper of **Alm et al**. (**2019**) to obtain the sample size of our "*no-info*" treatment, because in this treatment the authors provide no information on neither the descriptive nor injunctive norms in all the thirty rounds. In particular, we employ the means and standard errors[56] of the dependent variable "reported taxes" of the first and second half of their "Control" treatment (see Table 2 *Simple Descriptive Statistics* of their paper) to run the power analysis. In STATA, we implement the following command to estimate the paired sample t-test when an individual is observed twice:

```
. power pairedmeans 3241.53 3162.33 , sd1(715) sd2(737) corr(0.5) oneside
```

We Furthermore, we set a positive correlation of 0.5: it is reasonable to assume that a subject is likely to behave similarly in the first and the second phase when she does not receive any information. The estimated sample size for the "*no-info*" treatment is 26.

In order to set the optimal sample size of the "*pilot-p*" treatment, and further assess the one of the "*no-info*" treatment, we consider the paper of **Choo et al**. (**2016**). Indeed, in their paper they run a "P20" treatment, in which the audit probability is known and set at 20% and a "UP" treatment, where the audit probability is unknow and equal to 20%. We consider the average compliance rates and the standard deviations of the student sample[57] of these two treatments (see Table 2 *Average compliance rate* of their paper) to calculate the number of subjects needed for our "*pilot-p*" and "*no-info*" treatment. The STATA command used to run the power analysis is:

```
. power twomeans 0.61 0.7 , sd1(0.35) sd2(0.3) oneside p(0.8)
```

Differently from the "power pairedmeams" we run before, here we have a situation of an independent-samples t-test – i.e., the individuals are assigned to only one of the two treatments and are observed only once. According to the results, the sample size of the "*pilot-p*" and the "*no-info*" treatments should be equal to 16 subjects each. However, when considering the paper of **Alm et al**. (**2019**), the "*no-info*" treatment should have 26 participants. Hence, we rely on the power analysis that provides a higher number of subjects. Therefore, for both the "*no-info*" and the "*pilot-p*" treatments we consider as optimal sample size 26 participants. Nevertheless, we have 27 and 28 participants for, respectively, the "*pilot-p*" and the "*no-info*" treatments. This because we invited to the experimental sessions more participants than needed and we decided to have few more subjects in each treatment.

[56] Notice that they report the standard errors and not the standard deviations. To obtain the latter and run the power analysis, we transform the standard errors by using this formula: $s.d. = S.E.\times \sqrt{n}$. In their paper n=120.

[57] **Choo et al**. (**2016**) consider three different samples: a student sample, a PAYE sample and a self-assessed sample. Since we recruited mainly students, we consider the average compliance rates and the standard deviations of the student sample.

We consider the mean and the standard deviations of the amount reported in the “NOINFO” and the “INFO” treatments (see Table 1 *Descriptive statistics* of their paper) of the paper by **Fortin et al**. (**2007**) to obtain the optimal sample size of our “*info-evaders*” treatment. Indeed, in their experimental design, in part 1 (“NOINFO” treatment) they allow participants to play the tax evasion game with no information on others tax compliance. Then, in part 2 (“INFO” treatment) they provide subjects with feedback about the others’ compliance behaviours in the previous period. Since our “*info-evaders*” treatment has some features similar to the experimental design of **Fortin et al**. (**2007**), we run the following paired sample t-test as individuals are observed twice:

```
. power pairedmeans 53.92 50.15 , sd1(37.54) sd2(38.68) corr(0.1) oneside
```

The correlation we consider here is lower, 0.1, than the one used to estimate the sample size of the “*no-info*” treatment because subjects in **Fortin e al**. (**2007**) “INFO” treatment receive an information on the evasion rate. Hence, even though participants are observed twice, the information individuals have in the first and second part of the experiment is different allowing for less correlated behaviours in the two parts. Results indicate that we should have 57 subjects in the “*info-evaders*” treatment. Since we had some technical problem with z-tree Unleashed (one session of this treatment crashed down[58]), we had to run one extra session and for precaution we invited more individuals to be sure of having at least a total of 57/58 subjects for the “*info-evaders*” treatment. We ended up having 63 subjects for this treatment.

In order to choose the sample size of the second phases of “*info-caught*” and the “*info-prob*” treatments, we consider the paper of **Alm et al**. (**2009**) since they run a treatment in which individuals are informed only on the audit results – i.e., number of people audited in the previous round – and another in which participants know also the audit probability. In particular, we use the mean and the standard deviations of the declared income of the "Serie A Setting", in which the probability is pre-announced, and the "Serie B Setting", in which the probability is not pre-announced. In STATA, we estimated the independent-samples t-test as follow:

```
. power twomeans 38.69 51.7 , sd1(36.77) sd2(35.49) oneside p(0.8)
```

The results indicate that the optimal sample size for the “*info-caught*” and the “*info-prob*” treatments should be of 10 individuals each. This sounds too little. For this reason, we decided to consider a higher number of subjects for these two treatments. We chose to set a sample size for the “*info-caught*” and the “*info-prob*” treatments lower[59], around 50 individuals[60], than that for the “*info-evaders*” treatment, as the latter need a higher sample size to reach a power of at least 80%.

Concerning the “*full-info*” treatment, we chose as optimal sample size the same number of participants used for the “*info-prob*” treatment. The reason for this choice is that, in the “*full-info*” treatment, the information on the evasion rate is added to the "*info-prob*" treatment, and therefore we expect that with 50 individuals[61] the power of 80% would be satisfied also in this case.

[58] At the time we run the experiment (March and April 2021) z-Tree Unleashed was just launched and some bugs were not fixed yet.
[59] Moreover, we also had a budget constraint and we decided to have a higher number of subjects in the “*info-evaders*” treatment rather than the “*info-caught*” and the “*info-prob*” treatments.
[60] When running the experiment, we ended up having 52 subjects in the “*info-caught*” treatments and 47 in the” info-*prob*” treatment due to different show up rates.
[61] An individual did not show-up, so at the end we have 49 subjects for the “full-info” treatment.

Appendix C. Means and standard deviations of beliefs on N and E by treatment.

In table 4 we report the means and standard deviations of the beliefs on the number of caught evaders N and the evasion rates E expressed by participants in the beliefs elicitation task. The beliefs are presented by treatment and only for the informational treatments, as in the "*no-info*" and in the "*pilot-p*" treatments we did not elicit individual's beliefs. In order to test whether the means of beliefs on N and E differ across treatments, we perform the Mann-Whitney non-parametric test[62].

Table 4 – Means and standard deviations of beliefs on N and E

Treatments	*Info-caught*	*Info-evaders*	*Info-prob*	*Full-info*
Beliefs on N	11.10 (1.02)	10.89 (0.85)	11.70 (0.96)	9.61 (0.90)
Beliefs on E	18.6 (0.96)	18.68 (0.94)	17.87 (1.06)	15.98 (1.07)

Standard deviations are in parenthesis

When looking at the means of beliefs on the number of caught evaders N, we observe that participants in the "*full-info*" treatment reported on average lower beliefs on N compared to those in the other three informational treatments. However, we find that only the means of the beliefs on N expressed in the "*info-prob*" and in the "*full-info*" treatments significantly differ between each other ($p - value = 0.076$). Similarly, when considering the beliefs on the number of people who evaded (no matter whether they got caught or not) E, participants assigned to the "*full-info*" treatment reported on average lower beliefs on E compared to people in the other treatments. Indeed, the mean of beliefs on E in the "*full-info*" treatment is significantly lower than that in the "*info-caught*" treatment ($p - value = 0.089$) and that in the "*info-evaders*" treatment ($p - value = 0.046$). Instead, there is no significance difference between beliefs on E expressed in the "*full-info*" and in the "*info-prob*" treatments ($p - value = 0.209$).

[62] All the Mann-Whitney tests we run are two-tailed. Moreover, we perform the Mann-Whitney tests considering the treatments two by two.

Appendix D. Robustness check

To test the robustness of our findings regarding hypotheses 1 and 2, we consider again a random effect probit model to estimate whether receiving the information on the number of caught evaders – i.e., being assigned to the "*info-caught*" treatment – affect the individuals' compliance behaviours. However, we now restrict our analysis to those individuals who have similar prior beliefs on p[63] to study whether they still infer the audit probability rather than the tax evasion rate when receiving the information about the number of caught evaders. As before, in this further analysis we consider the individuals' perception of the number of caught evaders N – i.e., whether they perceive N as relatively high or low with respect to prior beliefs.

As previously anticipated, we could not restrict our analysis to those participants who have an (implicit) belief on the audit probability lower than the actual one ($p = 0.3$) because only 9 participants in the "*info-caught*" treatment have an (implicit) belief on the audit probability lower than 0.3. Hence, we perform the robustness checks of our findings only considering those participants, 37, who exhibit an (implicit) belief on the audit probability higher than the actual one ($p = 0.3$).

Table 8 shows the results of three specifications that consider the same independent variables of the three specifications seen in table 5. The dependent variable is again *Evasion*, which as before takes value equal to 1 if the participant has underreported her income, either all or part of it. As for table 5, our independent variables of interests are *Low N Beliefs*, "*Info-caught*" treatment and the interaction term *Low N Beliefs* × "*Info-caught*" treatment.

The results in specification 1, 2 and 3 of table 8 are similar to the ones of table 5. In particular, the coefficient of "*Info-caught*" treatment is negative and significant at the 1% in all three specifications. This indicates that when individuals, who have implicit beliefs on the audit probability higher than the actual one, receive information on the number of caught evaders N and perceive it as relatively high with respect to their prior beliefs, they evade less. Therefore, according to our hypothesis 1, individuals infer a higher audit probability. Instead, in all the three columns, the coefficient of the interaction term – i.e., *Low N Beliefs* × "*Info-caught*" treatment – is significant and positive, and the sum between this and the coefficient of "*Info-caught*" treatment is positive. This suggest that when subjects – who have implicit beliefs on the audit probability higher than 0.3 – perceive the number of caught evaders as low, they infer a lower audit probability and evade more. Furthermore, all three specifications show a positive and significant effect of the total income – i.e., *Ln total income* – indicating that richer individuals are more likely to evade taxes.

When controlling for *Real-effort task*, we find a significant and negative effect. However, the impact of this variable on the probability to evade taxes is very little: the coefficient of *Real-effort task* is equal to zero. Moreover, the variables *Underreport investments profit* and *High evasion when perceived low compliance* show a significant and negative effect on the probability to evade, while the coefficient of *High evasion when perceived low audit prob.* is significant but positive. Again, these results must be taken with caution because questionnaire answers may suffer from endogeneity (**Lefebvre et al.**, **2015**)[64].

[63] We calculate the individual's implicit probability considering both the elicited beliefs on caught evaders N and on evasion rate E: $\frac{beliefs_N}{beliefs_E} = implicit\ p$).

[64] In table 5 we observe that the only the coefficient of *Underreport investments profit* and *High evasion when perceived low compliance* are negative and significant.

Table 8 – random effects probit model restricting to participants with belief on the audit probability higher than 0.3

Evasion	(1)	(2)	(3)
Low N Beliefs	1.037***	1.397**	0.467
	(0.2875)	(0.6452)	(1.0644)
Info-caught treatment	-0.599***	-0.598***	-0.592**
	(0.2275)	(0.2284)	(0.2314)
Low N Beliefs × *Info-caught* treatment	0.696***	0.697***	0.699***
	(0.1763)	(0.1765)	(0.1786)
Ln total income	1.162***	1.207***	1.340***
	(0.3984)	(0.4192)	(0.4497)
Evaded in prev. period	0.108	0.109	0.114
	(0.1781)	(0.1784)	(0.1858)
Controlled in prev. period	0.094	0.093	0.083
	(0.1669)	(0.1672)	(0.1718)
Period	-0.028	-0.028	-0.029
	(0.0229)	(0.0230)	(0.0233)
BRET		-0.009	-0.012
		(0.0142)	(0.0146)
Age		0.008	-0.018
		(0.0505)	(0.0427)
Male		-0.011	0.009
		(0.5094)	(0.5353)
Study course			
Economics		Ref.	Ref.
Other tracks		-0.066	0.219
		(0.6741)	(0.6046)
No student		-0.785	-0.032
		(0.6341)	(0.7094)
Real-effort task (1^{st} part)			-0.000*
			(0.0003)
Ln final income prev. period			-0.000
			(0.2930)
Questionnaire answers			
Progressive tax policy			-0.195
			(0.2507)
Rich pay too much taxes			0.413
			(0.3579)
Do you know tax evaders			0.345
			(0.6548)
Underreport investments profits			-0.299*
			(0.1749)
Underreport cash payments			0.138
			(0.1605)
High evasion when perceived low compliance			-0.457***
			(0.1544)
High evasion when perceived low audit prob.			0.233*
			(0.1255)
Constant	-10.660***	-11.035***	-6.790**
	(3.4894)	(3.7900)	(3.1209)
N	703	703	703
L.L.	-314.785	-314.182	-308.523
Wald	13.54	14.73	27.36
Rho	.693*	.686	.593

Notes: * p<0.10, ** p<0.05, *** p<0.01. The dependent variable is Evasion. Clustered standard errors at the subject level are in parenthesis. *Low N Beliefs*, *info-caught* treatment, *Evaded in prev. period*, *Controlled in prev. period*, *Male* are dummy variables. *BRET* (bomb risk elicitation task) is given by the number of boxes collected by the individual and can take any integer value between 0 (high risk averse) and 100 (high risk seeking). *Ln total income* is a continuous variable and considers the logarithm of the initial income (fixed and random components) a participant has at the beginning of the period. *Study course* is a categorical variable that takes value 0 if the participant is in an economic track (reference point), 1 if s/he in a track different from economics and 2 if s/he is not a student. *Real-effort task* is a discrete variable that indicates the score the individual obtained in the task performed in the first part of the experiment. *Ln final income prev. period* is a continuous variable that considers the logarithm of the gains obtained by the participant at the end in the previous period. *Progressive tax policy* and *Rich pay too much taxes* can take value between 1 (strongly disagree) and 5 (strongly agree). *Do you know tax evaders* is a dummy variable. *Underreport investments profit* and *Underreport cash payments* can take value between 1 (this behaviour is perfectly acceptable) and 6 (this behaviour is not at all acceptable). *High evasion when perceived low compliance* and *High evasion when perceived low audit prob.* can take value between 0 (absolutely false) and 10 (absolutely true).

The estimation results of table 8 are similar to the ones presented in table 5. However, the ρ coefficient is not significant in specifications 2 and 3 of table 8. This indicates that the null hypothesis of the absence of unobserved individual heterogeneity cannot be rejected. Hence, we also consider a linear mixed model (table 9), in which we restrict the analysis to those individuals who have an (implicit) belief on the audit probability lower than the actual one.

Results in table 9 are similar to those in table 8 (and table 5). The coefficient of "*Info-caught*" treatment is negative and significant, while that of the interaction term *Low N Beliefs* × "*Info-caught*" treatment is positive and significant and its sum with the "*Info-caught*" treatment coefficient is positive. These again indicates that individuals seem to infer the audit probability rather than the tax evasion rate when receiving the information about the number of caught evaders. When considering the control variables, in table 9 only the coefficient *Real-effort task* is no more significant. Instead, *Ln total income*, *Underreport investments profit*, *High evasion when perceived low compliance* and *High evasion when perceived low audit prob.* are again significant in affecting the individuals' probability of evading taxes.

Table 9 – linear mixed model restricting to participants with belief on the audit probability higher than 0.3

Evasion	(1)	(2)	(3)
Low N Beliefs	0.289***	0.348**	0.220
	(0.0611)	(0.1470)	(0.2275)
Info-caught treatment	-0.154***	-0.154***	-0.153***
	(0.0500)	(0.0502)	(0.0498)
Low N Beliefs × *Info-caught* treatment	0.192***	0.192***	0.192***
	(0.0402)	(0.0402)	(0.0404)
Ln total income	0.263***	0.270***	0.296***
	(0.0895)	(0.0939)	(0.0971)
Evasion in prev. period	0.036	0.037	0.038
	(0.0549)	(0.0548)	(0.0566)
Controlled in prev. period	0.024	0.024	0.019
	(0.0366)	(0.0367)	(0.0381)
Period	-0.007	-0.007	-0.007
	(0.0049)	(0.0049)	(0.0049)
BRET		-0.001	-0.003
		(0.0032)	(0.0028)
Age		-0.003	-0.008
		(0.0110)	(0.0093)
Male		0.016	0.049
		(0.1136)	(0.1122)
Study course			
Economics		Ref.	Ref.
Other tracks		-0.010	0.059
		(0.1376)	(0.1238)
No student		-0.130	0.026
		(0.1533)	(0.1675)
Real-effort task (1^{st})			-0.000
			(0.0001)
Ln final income prev. period			-0.008
			(0.0652)
Questionnaire answers			
Progressive tax policy			-0.041
			(0.0607)
Rich pay too much taxes			0.083
			(0.0781)
Do you know tax evaders			0.061
			(0.1328)
Underreport investments profits			-0.078**
			(0.0335)
Underreport cash payments			0.035
			(0.0369)
High evasion when perceived low compliance			-0.116***
			(0.0343)
High evasion when perceived low low audit prob.			0.070**
			(0.0279)
Constant	-1.979**	-1.966**	-0.953
	(0.7795)	(0.8316)	(0.7548)
R-squared	0.0756	0.1023	0.3781
N	703	703	703

Notes: * p<0.10, ** p<0.05, *** p<0.01. The dependent variable is evasion. Clustered standard errors at the subject level are in parenthesis. *Low N Beliefs*, *info-caught* treatment, *evaded in prev. period*, *controlled in prev. period*, *male* are dummy variables. *BRET* (bomb risk elicitation task) is given by the number of boxes collected by the individual and can take any integer value between 0 (high risk averse) and 100 (high risk seeking). *Ln total income* is a continuous variable and considers the logarithm of the initial income (fixed and random components) a participant has at the beginning of the period. *Study course* is a categorical variable that takes value 0 if the participant is in an economic track (reference point), 1 if s/he in a track different from economics and 2 if s/he is not a student. *Real-effort task* is a discrete variable that indicates the score the individual obtained in the task performed in the first part of the experiment. *Ln final income prev. period* is a continuous variable that considers the logarithm of the gains obtained by the participant at the end in the previous period. *Progressive tax policy* and *Rich pay too much taxes* can take value between 1 (strongly disagree) and 5 (strongly agree). *Do you know tax evaders* is a dummy variable. *Underreport investments profit* and *Underreport cash payments* can take value between 1 (this behaviour is perfectly acceptable) and 6 (this behaviour is not at all acceptable). *High evasion when perceived low compliance* and *High evasion when perceived low audit prob.* can take value between 0 (absolutely false) and 10 (absolutely true).

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