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Nudging tax compliance: Evidence from a laboratory experiment☆,☆☆

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ABSTRACT

We test whether minimal, non-informative messages can nudge tax compliance beyond standard deterrence. In a within-subjects lab experiment, we randomize exposure to either a "reminder" that leaves audit probability unchanged or an informative "warning" tied to higher audit probability, and estimate effects on both the probability of evasion and the share of income evaded. A short non-informative reminder, holding incentives fixed, lowers the probability of evasion by about 16 percentage points, with no detectable effect on the evaded share among evaders; informative messages add at most marginal effects once audit probability is controlled for.

1. Introduction

We examine whether minimal, non-informative messages can nudge tax compliance beyond standard deterrence. A large body of evidence shows that tax compliance is sensitive to how tax authorities communicate with taxpayers, but it is often unclear whether observed effects operate through changes in perceived enforcement or through non-pecuniary channels such as salience and norm activation. We ask whether very simple non-informative messages can raise voluntary compliance over and above the classic deterrence channel that works by shifting perceived audit probability or penalties.

In their taxonomy of tax nudges, Antinyan and Asatryan (2024) distinguish (i) simple reminders, (ii) moral-suasion appeals, and (iii) deterrence-based messages, and show in a meta-analysis that these interventions have sizeable average effects, with substantial heterogeneity across message types. We focus on the simple reminder class and split it into two operational categories. Reminders are brief prompts that call attention to the payment obligation without conveying any new enforcement information, while warnings both remind and explicitly signal a higher likelihood of audit. This terminology enables a clean separation between norm/salience channels and pure deterrence.[2] What is new in our paper is the identification of a pure reminder effect separately from deterrence. We do not claim that reminders or warnings are novel interventions per se; rather, our contribution is to show how their effects can be disentangled within a single controlled design. The within-subject design orthogonalizes message content and enforcement by combining: (i) a non-informative message delivered at a fixed and common audit probability, so monetary incentives are unchanged; (ii) no-message decisions with exogenous variation in audit probability, which identify pure deterrence absent communication; and (iii) an informative message perfectly correlated with a higher realized audit probability, which captures warning effects. This structure allows

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[2] Reminders are used in field and lab experiments. See, among others, Advani et al. (2023), Alm et al. (1992), Alm and McKee (2006), Blumenthal et al. (2001), Carrillo et al. (2017), Charness et al. (2025), Chirico et al. (2019), Kleven et al. (2011), Ortega and Scartascini (2020), Pomeranz (2015), and Slemrod et al. (2017).

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us to estimate a pure reminder or nudge effect holding incentives fixed, a pure deterrence effect absent communication, and any incremental effect of informative messages beyond realized enforcement.

This distinction is substantively important, not merely methodological. Existing studies often identify reduced-form effects of communication packages that simultaneously alter perceived enforcement, salience, and moral framing, making it difficult to isolate the independent role of message content. By contrast, our design distinguishes a reminder or nudge channel, operating through salience and norm activation, from a deterrence channel, operating through perceived enforcement. By exposing the same participants to different message environments, we compare behavioral responses to a non-informative reminder and to an informative warning while holding constant individual-level determinants of compliance. This allows us to assess whether compliance changes are driven by the mere receipt of a message, by the additional enforcement information contained in the message, or by bundled deterrence effects that combine information, salience, and enforcement threats.

The question is policy-relevant. Across the EU and OECD, revenue agencies increasingly use proactive contact. Large reminder campaigns — such as those run by the Italian tax authority in 2016-17 — generated substantial additional revenue and exemplify a prevention-over-punishment approach. We show that a non-informative reminder reduces the incidence of evasion by about 16 percentage points, with no detectable effect on the intensity of evasion among evaders. By contrast, informative messages add little beyond the strong deterrence from higher audit probabilities. The takeaway is that communication design should distinguish between nudges that activate norms and messages that modify perceived audits; the former can be effective even when they convey no new information.

Our study connects to deterrence and moral suasion (Alm et al., 2023) and to audit-threat letters (Castro & Scartascini, 2015; Kleven et al., 2011; Slemrod et al., 2001). In the nudge tradition (Thaler & Sunstein, 2008), simple, timely, and personalized communications that leverage salience, simplification, and social norms have yielded small but policy-relevant improvements in payment timeliness and reported liabilities (Hallsworth et al., 2017; Kettle et al., 2016; Mascagni, 2018; Perez-Truglia & Troiano, 2018; Pomeranz, 2015). Meta-analyses find positive average effects with substantial heterogeneity by message content and context (Antinyan & Asatryan, 2024). De Neve et al. (2021) provide complementary evidence from population-wide experiments in Belgium showing that communication interventions affect compliance, with particularly strong effects of simplifying messages and additional positive effects of deterrence messages. We further show that brief non-informative reminders — holding stated audit probability and penalties fixed — can raise compliance, consistent with non-pecuniary channels (salience, norm activation) supported by evidence on emotions and shaming in evasion decisions (Coricelli et al., 2010, 2014).

Our framework builds on the economics-of-crime approach (Allingham & Sandmo, 1972; Becker, 1968; Yitzhaki, 1974), where individuals weigh the gains from underreporting against the costs of being audited and fined. We consider that the tax authority may send messages prior to filing. Even when such messages provide no verifiable new facts, they may still influence reporting by making enforcement salient or by activating norms—consistent with evidence on lie aversion in communication (Gneezy, 2005).

The paper proceeds as follows. Section 2 presents the conceptual framework and testable predictions. Section 3 describes the experimental design and procedures. Section 4 describes the empirical methodology. Section 5 reports summary statistics and preliminary tests and presents regression results on the decision to evade and the extent of evasion. Section 6 concludes.

2. A simple reference framework

Following Allingham and Sandmo (1972) and Becker (1968), a taxpayer with true income I and tax rate t chooses reported income R to trade off the benefit from underreporting against the expected cost of detection and penalty. If not audited, after-tax income is $c_N = I - tR$. If audited, assuming a penalty rate $f > t$ applied to unpaid tax, after-tax income is $c_A = I - tR - (1+f)\,t\,(I-R)$. The baseline expected separable utility is then

$$\max_{R\in[0,I]} \quad (1-\alpha)\,u(c_N) \;+\; \alpha\,u(c_A), \tag{1}$$

where $u(\cdot)$ is increasing and concave and α is the (subjective) audit probability.

Eq. (1) models the decision to engage in illicit activity as a gamble, where the individual is aware of the probability of detection and the associated penalty for engaging in illegal activity. The model predicts a negative relationship between tax evasion and both the probability of detection (α) and the penalty rate (f). With a higher tax rate (t), individuals are also (weakly) less willing to evade.[3] In practice, information on audit probability may be unknown or time-varying. We allow the tax administration to send messages to taxpayers providing information (or reminders) before filing.[4]

We consider two message types: a *non-informative reminder* (a short contact that states nothing has changed but still reaches the participant); and an *informative message* (a brief notice that explicitly announces stricter enforcement, e.g., a higher likelihood of audit). We index communication by $M \in \{\varnothing, r, w\}$, where $\varnothing$ denotes no message received, r a non-informative message (reminder), and w an informative message (warning).

Messages can affect behavior through two channels. (i) **Nudge (norm) activation**, which adds a non-pecuniary disutility of underreporting, $g(E)$, increasing in the evasion rate $E = (I-R)/I$ with $g'(E) > 0$, $g''(E) \geq 0$, and $g(0) = 0$. (ii) **Deterrence**, which raises the effective enforcement weight $\alpha(M)$ through information or salience.

We assume that the proportion of undeclared income E entails a non-pecuniary cost $g(E)$, capturing psychological, moral, or normative disutility from evasion. The key condition for our analysis is that this cost is increasing in evasion, i.e., $g'(E) > 0$. This ensures that activating salience or norms through communication can reduce evasion even when monetary incentives are held fixed. For expositional convenience, we further impose $g''(E) \geq 0$ as a sufficient regularity condition that guarantees standard monotonicity and well-behaved comparative statics. However, our qualitative predictions do not hinge on convexity: allowing for $g''(E) < 0$ over some range would capture moral desensitization, whereby marginal non-pecuniary costs flatten as evasion increases, without affecting the identification or interpretation of the reminder effect tested in the experiment.

In problem (1), these channels map into (a) replacing the audit component with $\alpha(M)$, and (b) adding a message-dependent non-monetary cost $k_M g(E)$. The expected utility with messages is

$$\max_{R\in[0,I]} \underbrace{(1-\alpha(M))\,u(c_N) + \alpha(M)\,u(c_A)}_{\text{monetary payoff}} \;-\; \underbrace{k_M\,g(E)}_{\text{nudge}}, \tag{2}$$

where $\alpha(\varnothing) = \alpha$ and $k_\varnothing = 0$ (reducing (2) to (1) for $M = \varnothing$). We adopt an additively separable specification: the non-informative message induces a psychological cost measured in monetary units

[3] The counterintuitive effects of tax rates on compliance depend on the strength of substitution and income effects. A higher tax rate reduces both the return to compliance (substitution) and disposable income, with net effects depending on risk attitudes (Yitzhaki, 1974).

[4] Such preventive contact is not routine but is increasingly viewed as an effective compliance tool, even when message content is limited or imperfectly verifiable.

(local quasi-linear normalization), which legitimizes summing it with pecuniary payoffs.

For an interior solution, the optimal report R satisfies

$$\left[(1-\alpha(M))\,u'(c_N) - \alpha(M)\,f\;u'(c_A)\right] t = \frac{k_M}{I} g'(E). \tag{3}$$

The left-hand side is the difference between the marginal gain from hiding (in the no-audit state) and the risk-adjusted marginal penalty if audited. The right-hand side is the marginal non-monetary cost of lying. Higher effective enforcement ($\alpha(M)$), higher penalties (f), or stronger norm activation (k_M) tilt the balance toward lower E (higher compliance).

Eq. (3) makes explicit how communication affects compliance through two distinct channels. The left-hand side captures the standard deterrence mechanism: a higher perceived audit probability increases the expected monetary cost of evasion. The right-hand side captures the non-pecuniary or norm-based mechanism: messages may increase the psychological cost of underreporting even when monetary incentives remain unchanged.

We hypothesize that the effective enforcement weight is highest under an informative message and unchanged under a non-informative message, i.e., $\alpha(w) > \alpha(r) = \alpha$, because the warning announces stricter enforcement while the reminder states that enforcement is unchanged. We also hypothesize $k_r \geq k_w \geq 0$: a reminder may activate norms and salience without being perceived as a threat, whereas an informative message may be interpreted as purely instrumental and may crowd out intrinsic motivation, weakening the norm-activation component.

The model therefore highlights two mechanisms through which messages may affect compliance.

Prediction 1 (*Deterrence Channel*)**.** *If additional information is understood and believed, recipients revise upward their perceived audit probability. Ceteris paribus, informative messages reduce underreporting.*

Prediction 2 (*Nudge Channel*)**.** *Messages can activate social norms and add a non-pecuniary cost to underreporting. Ceteris paribus, messages reduce underreporting, with stronger effects the larger k_M (especially under non-informative reminders).*

The framework also yields distinct predictions for the extensive (whether to evade) and intensive (how much to evade conditional on evasion) margins of compliance. In Eq. (3), increases in either perceived enforcement ($\alpha(M)$) or the norm-activation component (k_M) raise the marginal cost of underreporting and can induce some individuals to switch from evasion to full compliance. This mechanism is expected to operate primarily on the extensive margin, especially for taxpayers located near the compliance threshold. By contrast, among individuals who continue to evade, the amount evaded may respond less strongly if remaining evaders are relatively insensitive to salience or already choose corner or near-corner reporting. The model therefore rationalizes why reminder effects may be more visible on the decision to evade than on the conditional amount evaded.

3. The experiment

3.1. Experimental design

We designed our experiment to test a nudge-augmented version of the Allingham and Sandmo model of tax evasion. The key idea is that compliance may respond not only to monetary incentives — tax rates, audit probabilities, and penalties — but also to informational nudges. Our design allows us to study two distinct types of interventions: non-informative messages (reminders) that leave incentives unchanged, and informative messages (warnings) that explicitly change perceived enforcement. In the latter case, we are interested in whether messages generate additional nudge-type effects over the standard deterrence channel implied by the information they convey.

We run a laboratory tax-evasion experiment in which participants repeatedly decide how much of an earned income to declare, facing an uncertain audit and a monetary penalty if under-reporting is detected. Across rounds, we vary the communication regime in three ways. In some rounds, no message is sent, so behavior depends only on the standard incentives from tax rates, audit probability, and penalties. In other rounds, participants may receive a reminder: a message that does not change the audit probability and contains no information about enforcement. In a third set of rounds, participants may receive an informative message that explicitly signals differential audit probability for message recipients versus non-recipients. Within each message round, who receives the message is randomly determined and this is common knowledge among participants.

The experiment formally consists of 6 sessions with 32 participants each (192 subjects). Every session comprises 32 rounds, yielding 6144 individual decisions. The design is within-subjects. All amounts are denominated in tokens. At payment, 1 token is converted into euro 0.50. Each participant receives a fixed participation fee of 3 tokens. To preserve incentives while limiting stake effects, one round out of 32 is randomly selected for payment and added to the participation fee.

At the start of each round, each participant privately receives an income $I \in \{10, 20, 30, 40\}$ tokens. The participant reports $R \in [0, I]$, which is taxed at a round-specific rate $t \in \{0.30, 0.45, 0.60\}$ (common knowledge). If the participant is not audited, tax is paid only on the reported amount. If the participant is audited and $R < I$, tax is paid on the true income and a penalty equal to 66% of the unpaid tax is levied (i.e., $f = 0.66$, constant across rounds).[5]

In each round, we implement one of three communication regimes.

1. Non-informative message (NM). In these rounds, we send non-informative messages that act as reminders and do not change enforcement. Exactly 16 out of 32 participants are randomly assigned to receive a short message. Recipients and non-recipients face the same audit probability, 0.50 (8 of 16 each). Thus, the message carries no information about differential audit probability and leaves monetary incentives unchanged. Any behavioral difference between recipients and non-recipients in NM rounds identifies a nudge effect (salience and norm activation).
2. **Informative message (IM).** In these rounds, we send informative messages that change enforcement probabilities. As in NM rounds, 16 out of the 32 participants are randomly selected to receive a message. Recipients are audited with probability 0.75 (12 of 16), while non-recipients are audited with probability 0.25 (4 of 16). These messages explicitly alert individuals that the probability of being audited is higher (lower) if they (did not) receive the message. All players know that recipients and non-recipients face different audit probabilities.
3. **No message (NN).** In these rounds, no messages are sent. The audit probability for the round is either 0.25, 0.50, or 0.75 (common knowledge), identical for all 32 participants. These NN rounds provide the benchmark against which we evaluate the impact of informative and non-informative messages. They serve as control rounds for nudge effects (comparisons at fixed audit probability).

For ease of reference, Table 1 summarizes the three communication regimes, the corresponding audit probabilities, and the message assignment rule.

As described by Table 1, under both message regimes (NM and IM), the rule that "16 of 32 participants receive a message" is common knowledge, and the assignment of messages within each round is

[5] The penalty rate is set at the highest level that precludes negative post-audit income. Tax revenues are not redistributed to participants; instead, subjects are informed that these funds will be allocated to research support for future experiments.

Table 1
Experimental design overview.

Communication regimes, audit probabilities, and message assignment.			
	Msg	Audit probability α	Message assignment
NM	Yes	$\alpha = 0.50$ for everyone	16/32 receive a reminder (random; common knowledge)
IM	Yes	recipients: $\alpha = 0.75$; non-recipients: $\alpha = 0.25$	16/32 receive a warning (random; common knowledge)
NN	No	$\alpha \in \{0.25, 0.50, 0.75\}$ for everyone	No messages

random and independent across rounds. In NN rounds, no participant receives any message.

Each round follows a fixed sequence. (i) Communication regime (NM, IM, or NN) is publicly announced. Once the regime is introduced, the associated tax rate, penalty, and audit probability (in NN) become known. (ii) Each participant privately draws an income ($I \in \{10, 20, 30, 40\}$). Participants are aware that all subjects face the same probability of receiving each of the four incomes. (iii) In NM and IM, exactly 16 out of 32 participants are randomly assigned to receive a message, a fact that is itself common knowledge. (iv) Participants choose reported income, after which audits are conducted and payoffs realized.

This information structure is intentionally "clean." In NM, message receipt is non-informative because everyone faces ($\alpha = 0.50$). In IM, audit probabilities are explicitly and publicly tied to message receipt, after the communication stage, all participants — recipients and non-recipients alike — know their own audit probability and that of others (recipients: $\alpha = 0.75$; non-recipients: $\alpha = 0.25$). This removes uncertainty about the mapping between message receipt and enforcement, prevents confounding message effects with beliefs about assignment, and ensures direct comparability with NN benchmarks at the same realized α.

Conceptually, the design supports two complementary sets of nudge comparisons (**nudge channel on tax compliance**). First, based on NM versus NN at the same audit probability. In NM rounds, treated and untreated participants face identical monetary incentives ($\alpha = 0.50$ and the same tax/penalty schedule). Comparing recipients and non-recipients within NM rounds, and contrasting both groups with NN at $\alpha = 0.50$, isolates non-pecuniary effects of a purely non-informative reminder.[6] Second, based on IM versus NN. In IM rounds, recipients face a high audit probability ($\alpha = 0.75$) while non-recipients face a low audit probability ($\alpha = 0.25$). Comparing IM recipients with NN rounds at $\alpha = 0.75$ and IM non-recipients with NN rounds at $\alpha = 0.25$ allows us to test whether informative messages work solely by raising perceived enforcement or also exert an additional nudge-type effect.

Naturally, the design also allows us to isolate the **deterrence channel on tax compliance** by exploiting the NN cases for different α, where any behavioral differences can only be attributed to changes in the audit probability. In addition, comparing the NM and IM for observations in which no message is delivered with NN rounds provides a robustness check of the experimental design. It verifies that there are no confounding or framing effects for subjects who do not receive a message in a communication regime relative to the case where communication is not allowed.

The experimental sample is balanced at the round and session level with respect to regimes, audit probabilities, and tax rates. We ran 192 rounds (32 per session), with 32 decisions per round, for a total of 6144 observations. In each session, we run 3 rounds at each tax rate within each communication regime (NM and IM), for a total of 9 rounds under NM and 9 rounds under IM. The remaining 14 rounds are allocated to NN. Under this design, each tax rate is implemented for 4 or 5 rounds; we equalize exposure by balancing the number of 4-round blocks across tax rates. As sessions are 6, it follows that the NM and IM regimes each account for 54 rounds and NN accounts for 84 rounds. A schematic overview of the within-session allocation of rounds across tax rates and communication regimes is provided in Appendix A.

3.2. Experimental predictions

We begin by testing whether reminders decrease evasion. Evidence in favor of this hypothesis means that, conditional on identical incentives, average evasion is lower among subjects who receive the non-informative message than among those who do not.[7] Formally,

$$H_0 : E(0.50 \mid r) < E(0.50 \mid \varnothing) = E(0.50)$$

where $E(\alpha \mid M)$ denotes the average share of income under-reported by participants facing audit probability α when message condition M applies.[8] When no message is involved (NN), we write $E(\alpha)$. Within each NM round, message receipt is randomized at $\alpha = 0.50$ for everyone. Hence any systematic difference is a non-pecuniary per-se effect (nudge activation), not deterrence. Note that we also test the absence of potential framing effects across regimes. No frame effect is observed if evasion behavior does not differ between NN and NM when no messages are received: $E(0.50 \mid \varnothing) = E(0.50)$.

Second, we test the effects of warnings:

$$H_0 : E(0.75 \mid w) < E(0.25 \mid \varnothing) = E(0.25)$$

We compare IM at $\alpha = 0.75$ to $\alpha = 0.25$. Our H_0 supports the view that informative messages reduce under-reporting by strengthening perceived enforcement. Again, we also test the absence of potential frame effect, i.e., $E(0.25 \mid \varnothing) = E(0.25)$.

Finally, we test whether warnings work through deterrence only. Assuming no frame effect in IM, we compare evasion among IM recipients with NN at the same high probability.

$$H_0 : E(0.75 \mid w) < E(0.75)$$

A null finding supports pure deterrence; any remaining gap signals a per-se messaging effect. If H_0 does not hold, the cross-rate contrast $E(0.75 \mid w) < E(0.25 \mid \varnothing) = E(0.25)$ is only attributable to higher audit probability. By contrast, if H_0 holds, $E(0.75 \mid w) - E(0.75) < 0$ supports an additional nudge-type effect even for informative messages.

3.3. Procedures

The study took place at the *CIMEO Experimental Lab*, Sapienza University of Rome, and was programmed in *z-Tree* (Fischbacher, 2007). Participants were recruited from the undergraduate population.

Upon arrival, participants were randomly seated at isolated computer workstations. Sessions opened with the risk-attitude task (Charness & Gneezy, 2010). Each participant was endowed with 15 points (equivalent to 3 tokens or 1.5 euro), as show-up fee. Participants could invest part, all, or none of their participation fee in a risky lottery. They decided how many points to invest in a risky asset and how many to keep. If the rolled die is less than 4, the amount invested is lost but they receive the amount they did not invest; if the rolled die is greater

[6] Because message assignment is common knowledge within rounds (16/32 recipients), untreated subjects could, in principle, be indirectly affected. We therefore benchmark NM at $\alpha = 0.50$ against NN at $\alpha = 0.50$ and find no systematic differences for non-recipients.

[7] For brevity, we concentrate on the amount evaded (intensive margin), although all theoretical predictions naturally extend to the probability of evading (extensive margin).

[8] In line with our theoretical framework, $E(\alpha \mid r)$ and $E(\alpha \mid w)$ refer to the average share of income under-reported by participants who received non-informative and informative messages, respectively, and $E(\alpha \mid \varnothing)$ refers to participants who did not receive messages in NM or IM.

than 3, they earn 2.5 times the amount invested plus the amount they did not invest. A risk-neutral subject had to invest the entire sum, the one with the highest expected value. The smaller the amount invested, the greater the degree of risk aversion.

After completing the risk attitude test, written instructions for the main task were distributed, followed by a computerized comprehension quiz. During the session, experimenters answered only *factual* questions by reading the relevant passage from the instructions; no additional examples or evaluative comments were provided. For interpretive queries, participants were directed to re-read the instructions. Communication among participants was prohibited throughout.[9]

During each round, the interface included a checkbox that automatically computed, for any reported income, the corresponding net payoff both if an audit occurred and if it did not, given the applicable tax rate and penalty. Including this checkbox helped neutralize heterogeneity in participants' ability to process numerical information (including memory capacity) and broader differences in cognitive skills (Battigalli & Generoso, 2024; Delle Foglie & Papa, 2024). After each round, participants were informed of their earnings. At the end of the experiment, we randomly roll a die for each participant to establish the result of the risky lottery and all participants were paid according to the outcome of one randomly selected round, in addition they were given the outcome of the risky lottery.

4. Econometric methodology

We explore the causal impact of informational nudges on tax compliance along two margins: (i) the extensive margin — the probability of evasion; and (ii) the intensive margin — the amount (or share) evaded conditional on evasion. In the spirit of the experimental and compliance literature on tax behavior in the economics-of-crime and social-norms traditions (see, e.g., Coricelli et al., 2010), we treat message exposure as exogenous and estimate treatment effects controlling for observables.

Let i index individuals and r rounds. We define two treatment indicators. First, d_{it} is an indicator for message delivery at the individual level in rounds where messages are sent: $d_{it} = 1$ if individual i receives a message, and $d_{it} = 0$, otherwise. Second, m_{it} is an indicator for being in a round in which messages are part of the treatment: $m_{it} = 1$ if it belongs to a round with messages, and $m_{it} = 0$ if it belongs to the benchmark without messages. In this framework, d_{it} measures the effect of actually receiving a message. By contrast, m_{it} measures whether behavior changes simply because the individual is in a round where messages might be sent, even if no message is received. It therefore captures any general contextual or framing effect of being in a "message round". If untreated individuals in message rounds behave like individuals in rounds with no messages, then the estimated effects can be attributed to message receipt itself, rather than to a generic increase in salience at the round level.

Let x_{it} denotes the vector of covariates comprising individual characteristics (gender, age, BA/Master student status, risk aversion), experimental controls (tax rate, income, decision time and its square), and the audit probability capturing the deterrence channel. The extensive-margin outcome is the evasion indicator $e_{it} \in \{0, 1\}$, while the intensive-margin outcome Y_{it} is the amount (percentage) evaded, which is observed only if $e_{it} = 1$. Because selection models require strong identifying variation, we use the Heckman correction mainly as a robustness check; results are very similar without the Mills ratio. Because the Heckman selection model is estimated without a strong exclusion restriction, identification of the inverse Mills ratio is relatively weak. We therefore use the selection correction only as a robustness check and verify that specifications without the Mills ratio yield very similar coefficients on the main variables and unchanged substantive conclusions.

Extensive margin: evasion decision. We first estimate a random–effects Probit model for the evasion decision:

$$e^*_{it} = \gamma_0 + \gamma_1 d_{it} + \gamma_2 m_{it} + x'_{it}\gamma + u_i + \varepsilon_{it}, \tag{4}$$

where e^*_{it} is a latent index, u_i is an individual random effect, and ε_{it} is an idiosyncratic error term. The observed indicator is $e_{it} = 1$ if $e^*_{it} > 0$, and $e_{it} = 0$ otherwise. We report average marginal effects (AMEs) for d_{it} and m_{it} to quantify the impact of message delivery and the message environment on the probability of evasion.

Intensive margin: conditional amount evaded. To address selection into evasion, we then estimate a random–effects linear model for y_{it} conditional on $e_{it} = 1$, augmenting the specification with the inverse Mills ratio from the first stage (Heckman two–step):

$$Y_{it} = \beta_0 + \beta_1 d_{it} + \beta_2 m_{it} + x'_{it}\beta + a_i + \rho\lambda_{it} + \eta_{it}, \qquad (e_{it} = 1), \tag{5}$$

where a_i is an individual random effect, λ_{it} is the inverse Mills ratio computed from the Probit model for e_{it}, and η_{it} is an idiosyncratic error.[10] The coefficients β_1 and β_2 identify treatment effects on the intensive margin, net of selection into evasion: β_1 measures the effect of actually receiving a message, while β_2 captures the effect of being in a message session (relative to the benchmark session without messages) conditional on evasion.

Standard errors are clustered at the individual level.[11] We present AMEs (with confidence intervals) from both stages and combine them in a two–panel figure for comparability. The same specification is applied to the two communication regimes so that messaging effects are estimated on a fully comparable basis. Overall, our approach mirrors standard practice in the experimental tax–compliance literature: extensive and intensive responses are estimated separately, while explicitly accounting for selection into evasion, in line with the logic used in studies that link behavioral frictions and compliance decisions (see, e.g., Coricelli et al. (2010)).

Summarizing, in the two regressions, the coefficients test for the existence of a nudge effect of the message on the decision whether to evade (extensive margin) and on the amount evaded conditional on evasion (intensive margin), respectively. The coefficients on m_{it}, instead, check whether the estimated effects are driven by message receipt rather than by being in a message round per se. In other words, they test whether the probability of evasion and the amount evaded by individuals who do not receive a message in a message round are the same as in the control group round, where no messages are ever sent.

5. Results

This section first describes the sample and main variables, and then presents the regression evidence on how communication and audit probabilities affect both the decision to evade and the amount of income evaded.

5.1. Summary statistics and preliminary tests

Table 2 summarizes the key variables. Males account for 49% of the sample; the mean age is 22.7 years, and 41% of participants are enrolled in a second-level (master) program (the remainder are BA students). On average, subjects take about 23 s to make a reporting decision. At the decision level, the average tax rate is 45%, income is 25 experimental currency units, and the audit probability is 0.50. Over half of the observations (56%) involve some evasion, with an average declared income of 16.1 and an average evasion rate of 0.35. Average payoffs are 15.5 per subject.

[9] Anonymity was guaranteed by using identification codes, and the participants' names remained unknown to all—including the experimenters and assistants.

[10] The selection and outcome equations use the same covariates; the Heckman specification is therefore intended only as a robustness check.

[11] We use random-effects specifications with standard errors clustered at the individual level to account for within-subject serial correlation.

Table 2
Summary statistics.

Variable	Average	St. dev.	Min	Max	Obs.
Male	0.49	0.50	0	1	192
Age	22.65	3.42	19	47	192
BA student	0.59	0.49	0	1	192
Risk neutral	0.73	0.45	0	1	192
Tax rate	0.45	0.12	0.30	0.60	6144
Income	25	11.18	10	40	6144
Audit probability	0.50	0.19	0.25	0.75	6144
Time	22.91	20.48	2	275	6144
Evaders	0.56	0.50	0	1	6144
Declared income	16.06	12.98	0	40	6144
Evaded income	8.94	11.57	0	40	6144
Evasion rate	0.35	0.40	0	1	6144
Payoffs	15.48	9.99	0.04	40	192

Table 3
Communication, audit probability and tax evasion.

		Prop. evaders		Prop. evaded income	
		(1)	(2)	(3)	(4)
(a) Non-informative communication					
		NM	NN	NM	NN
$\alpha = 0.50$	$d_{it} = 1$	0.47	–	0.50	–
		(0.50/864)		(0.33/408)	
$\alpha = 0.50$	$d_{it} = 0$	0.63	0.61	0.60	0.60
		(0.48/864)	(0.49/960)	(0.33/540)	(0.33/582)
(b) Informative communication					
		IM	NN	IM	NN
$\alpha = 0.75$	$d_{it} = 1$	0.31	0.38	0.48	0.49
		(0.46/864)	(0.49/864)	(0.35/268)	(0.34/327)
$\alpha = 0.25$	$d_{it} = 0$	0.77	0.79	0.74	0.75
		(0.42/864)	(0.41/864)	(0.30/667)	(0.30/681)

Table 3 summarizes how communication and audit probability jointly affect tax evasion at both the extensive margin (proportion of evaders) and the intensive margin (proportion of income evaded among evaders). The table contrasts the outcomes of NM and IM regimes ($d = 1$ if a message is delivered, $d = 0$ otherwise) with NN regime. The upper block refers to the NM regime with $\alpha = 0.50$, while the lower block reports the IM regime, where messages are coupled with changes in α. The table reports the proportion of evaders in columns (1) and (2); the proportion of income evaded by evaders in columns (3) and (4). In parentheses, we report the standard error and the number of observations. In columns (3) and (4), the number of observations is smaller because the reported values refer only to evaders.

In the IM regime, raising α from 0.25 to 0.75 reduces the proportion of evaders from 0.77 to 0.31 ($Z = 2.20$, $p = 0.027$) and lowers the share of income evaded from 0.74 to 0.48 ($Z = 2.20$, $p = 0.027$), indicating a strong negative response of both margins to higher audit intensity, in line with Becker–Allingham–Sandmo predictions. Different probabilities of being audited matter. The deterrence channel can also be cleanly identified by comparing NN outcomes across audit probabilities.[12] All reported statistics adopt the Wilcoxon signed rank test, which compares averages at the session level. Our data are independent at session level, but not at the individual level. The Wilcoxon signed rank tests accounts for such structure in the data.

The table allows us to isolate the intrinsic message effect conditional on a given α. In the case of non-informative communication, receiving a reminder ($d = 1$) substantially reduces both the incidence of evasion (0.47 vs. 0.61–0.63: $Z = 1.99$, $p = 0.046$ and $Z = 2.20$, $p = 0.027$) and the fraction of income evaded (0.50 vs. 0.60, $Z = 1.99$, $p = 0.046$), even though incentives are unchanged. This points to a genuine per se nudging effect. By contrast, in the case of informative messages, the proportion of subjects who received a warning ($d = 1$) is almost the same as those considered in the same NN case (0.31 vs. 0.38: $Z = 1.69$, $p = 0.091$). The rate of evasion is also essentially the same (0.48 vs. 0.49). Hence, we do not provide evidence for nudging for warnings. In both cases, as expected, no frame effect is observed. No significant difference is found between the proportion of evaders who did not receive a message in both NM and IM compared to NN regime. The same holds for the evasion rate.

Summarizing, non-informative messages reduce evasion even holding audit probability fixed, whereas informative messages have little or no effect once we condition on enforcement. We also find no framing effects when no message is received in communication setups. The absence of a nudge effect for informative messages may reflect diminishing marginal effects at higher enforcement levels: when deterrence is strong, compliance is driven mainly by incentives, leaving little scope for an additional reminder effect. Alternatively, because the warning conveys new information, it may redirect subjects' attention to the incentive change itself, crowding out any purely non-informative (reminder) channel.

5.2. Regression analysis

We estimate a random-effects (RE) probit for the evasion decision and a random-effects GLS for the share of income evaded among evaders, correcting for selection with a Heckman two-step (the inverse Mills ratio from Stage 1 enters the evaders-only GLS). Table 4 reports four specifications: (1) Non-informative communication — RE probit for evasion; (2) Non-informative communication — RE GLS for amount/share evaded (Heckman second stage); (3) Informative communication with an explicit audit-probability control — RE probit; (4) Informative communication — RE GLS (Heckman second stage). Figs. 1 and 2 show average marginal effects (AMEs) of message delivered (MD: $d_{it} = 1$ and $m_{it} = 1$) and message not delivered (MN: $d_{it} = 0$ and $m_{it} = 1$) with 95% confidence intervals. Fig. 1 plots the extensive margin (evasion probability), and Fig. 2 the intensive margin (amount/share evaded conditional on evasion).

The three variables of interest are *message delivered*, *message* , and the *audit probability*. In the non-informative specifications — columns (1) and (2) — the coefficient/AME on message delivered isolates a pure nudge/salience effect: the impact of receiving a short reminder when monetary incentives are unchanged. In the pooled specifications with audit control — columns (3) and (4) — we add the audit probability to absorb the deterrence channel associated with higher audit intensity (the "informative" component). In this setting, message delivered tests whether being explicitly told about enforcement has an additional, intrinsic effect beyond the realized audit probability. Symmetrically, message is a within-regime placebo: it checks whether subjects who do not receive a message behave the same when messages are possible but not received as when no messages exist at all, ceteris paribus. A message estimate statistically indistinguishable from zero confirms this equivalence.

In the non-informative communication, a short reminder significantly lowers the incidence of evasion: the AME of Message delivered is about −0.16 ($p < 0.01$), a reduction of about 16 percentage points holding covariates fixed. By contrast, the intensive margin is unaffected: conditional on evasion, the coefficient on Message delivered is small and insignificant. Message not delivered is null on both margins. This pattern is consistent with a nudge/salience/norm-activation channel that shifts the decision whether to evade but not how much to under-report.

[12] Individuals facing a higher audit probability (75%) are significantly less likely to become tax evaders than those facing a lower probability (50%): 0.38 vs. 0.61 ($Z = 2.20$, $p = 0.027$). A similar pattern emerges when comparing an audit probability of 50% with an even lower probability of 25%: subjects in the 50% condition are less likely to evade (0.61 vs. 0.79; $Z = 2.20$, $p = 0.027$). Taken together, these contrasts provide clear evidence in favor of a strong deterrence channel.

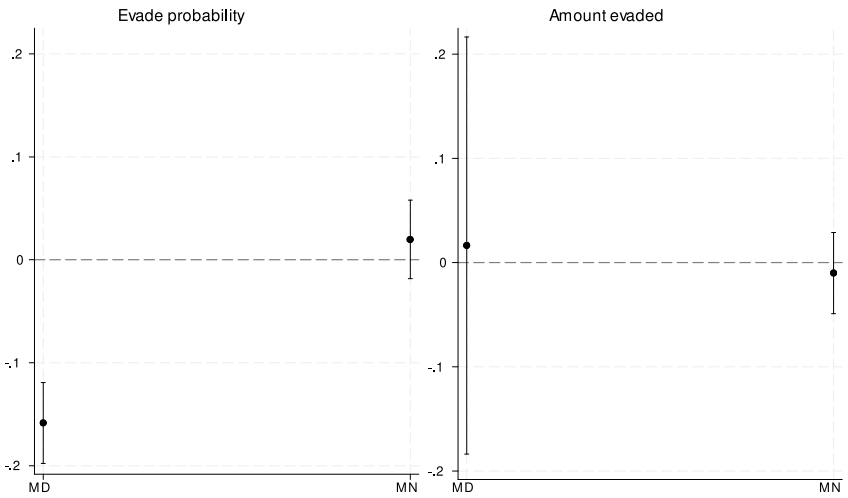

Fig. 1. Reminders. Average marginal effects (probability points) on evasion and evasion intensity. Error bars denote 95% confidence intervals.
Legend. Left panel: AMEs of Message delivered and Message not delivered on the probability of evasion from the random-effects probit; vertical bars are 95% CIs. Right panel: corresponding AMEs on the amount/share evaded among evaders (random-effects GLS, Heckman second step).

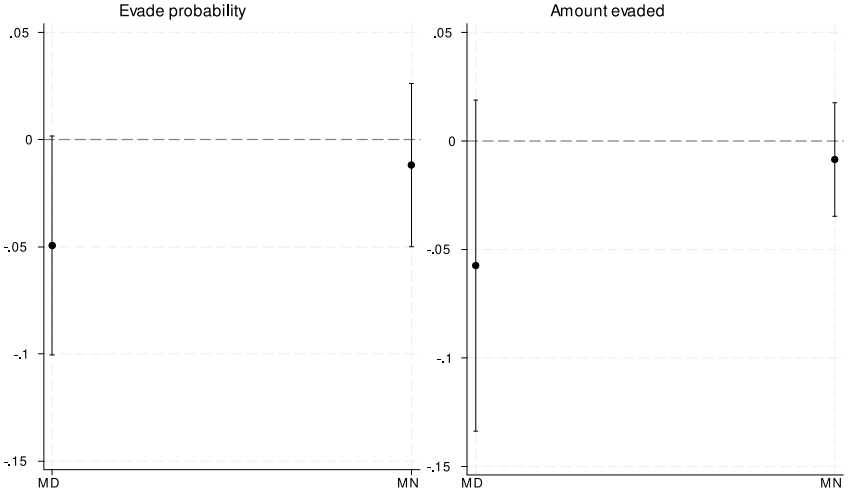

Fig. 2. Warnings. Average marginal effects (probability points) on evasion and evasion intensity. Error bars denote 95% confidence intervals.
Legend. Left panel: AMEs on the probability of evasion controlling directly for realized audit probability; right panel: AMEs on the amount/share evaded among evaders.

In the informative specifications that control for audit probability, the deterrence channel is strong: higher audit intensity is associated with a markedly lower probability of evasion, as in Becker–Allingham–Sandmo. On top of this, message delivered — now "informative" because it co-moves with enforcement — has a small, borderline effect on the extensive margin (≈ -0.05, $p < 0.06$) and remains insignificant on the intensive margin; message is null. Hence, in our setting, informative messages appear to operate mainly through actual enforcement, with at most limited additional intrinsic effects.

Controls behave as expected. Among experimental controls, income is positively and significantly associated with evasion on the extensive margin, while the tax rate is never significant. Decision time enters nonlinearly, implying a concave pattern in which moderate deliberation is associated with more evasion but very long decision times with lower outcomes. Regarding individual characteristics, age is negatively related to evasion. Men display higher evasion intensity once deterrence and selection are accounted for. BA students display lower outcomes and more risk-averse subjects evade less. In the Heckman step, the inverse Mills ratio is insignificant in the NM regime but positive and significant in the pooled specification, indicating non-random selection into the sample of evaders there. The random-effects share ($\rho \approx 0.33$-0.43) points to substantial persistent individual heterogeneity.

An important result is that reminder messages affect primarily the extensive margin of compliance. While reminders reduce the probability of evasion, we do not detect significant changes in the amount concealed among those who continue to evade. This pattern is consistent with the idea that non-informative messages mainly influence individuals who are close to the compliance threshold by increasing salience or activating moral costs of evasion. By contrast, taxpayers who continue to evade may be less responsive to such behavioral cues or may already choose reporting strategies anchored to monetary incentives. From a policy perspective, these findings suggest that low-cost reminder campaigns may be effective at increasing broad-based compliance, but are less likely to substantially alter the behavior of persistent or high-intensity evaders.

6. Conclusions

The experiment shows that a brief, non-informative reminder reduces the incidence of tax evasion by about 16 percentage points, with no detectable change in the intensity of evasion among those who still under-report. Informative messages, by contrast, add little beyond the deterrence generated by higher audit probabilities. Overall,

Table 4
Random-effects Probit and GLS estimates.

	(1)	(2)	(3)	(4)
Message delivered	−0.562***	0.017	−0.202*	−0.057
	(0.073)	(0.102)	(0.107)	(0.039)
Message	0.070	−0.010	−0.049	−0.009
	(0.069)	(0.020)	(0.079)	(0.013)
Audit probability	–	–	−2.999***	−0.943***
			(0.156)	(0.203)
Male	0.120	0.037	0.222	0.104***
	(0.141)	(0.036)	(0.138)	(0.032)
Age	−0.057**	0.014	−0.061**	−0.008
	(0.022)	(0.012)	(0.022)	(0.006)
BA student	−0.155	−0.032	−0.115	−0.094**
	(0.154)	(0.044)	(0.150)	(0.031)
Risk aversion	0.061	−0.113***	0.144	−0.069**
	(0.158)	(0.035)	(0.155)	(0.028)
Tax rate	−0.345	0.013	0.078	−0.066
	(0.228)	(0.083)	(0.215)	(0.051)
Income	0.022***	−0.006	0.011***	−0.001
	(0.003)	(0.004)	(0.002)	(0.001)
Time	0.013	−0.008	0.0159***	−0.004***
	(0.003)	(0.002)	(0.003)	(0.001)
Time^2 (×1000)	−0.054**	0.038***	−0.096***	0.011
	(0.000)	(0.000)	(0.000)	(0.000)
Heckman λ (Mills)	–	−0.284	–	0.308**
		(0.303)		(0.130)
Constant	0.997*	0.864***	2.455***	1.265***
	(0.582)	(0.109)	(0.576)	(0.120)
Observations	2688	1530	3456	1943
Groups (person)	192	181	192	184
Log-likelihood	−1508	–	−1684	–
R^2 (overall)	–	0.132	–	0.253
ρ (RE share)	0.431	0.362	0.426	0.334

Robust standard errors clustered at the individual level in parentheses, columns (2)–(4).
* $p < 0.10$.
** $p < 0.05$.
*** $p < 0.01$.

the evidence suggests that simple reminders work primarily through salience and norm activation, shifting the decision of whether to evade rather than how much income to conceal.

Our findings should be interpreted with the usual external-validity caveats. Our subjects are students making incentivized, but relatively low-stakes, decisions in a controlled laboratory environment. Real-world taxpayers may respond differently (particularly, older individuals, the self-employed, or firms facing higher stakes and greater complexity). Moreover, audit probabilities in the lab are higher and more salient than in most tax systems, so deterrence effects in the field are likely weaker and less transparent.

This contrast may be especially relevant for informative warnings. When enforcement is already salient — as in the lab — warnings may add little beyond the realized audit probability. In the field, where audit probability is typically low and opaque, the same messages may matter more by updating beliefs about enforcement. By comparison, non-informative reminders that operate through attention, salience, or norm activation may be more robust across enforcement environments, although their magnitude remains an empirical question.

Several features of our setting help keep communication salient and credible. In the field, however, attention and trust depend on the communication channel, timing, framing, and the sender's reputation, and responses may vary with cultural norms and local compliance histories. Our within-subjects design also allows for learning and potential carryover across rounds, and a message not delivered condition may be interpreted differently when messages are part of a broader enforcement environment. Finally, while we directly observe the intensity of evasion in the lab, this margin is typically difficult to measure in administrative data unless detailed third-party reporting or audit records are available. Accordingly, our findings should be interpreted as identifying mechanisms under controlled conditions rather than as point estimates that can be transferred directly to administrative settings.

Despite the limitations, a clear policy implication emerges from our experiment: low-cost reminders can reduce the number of evaders and should be regarded as complements to, rather than substitutes for, traditional enforcement tools. Assessing their practical relevance requires field validation under realistic conditions—namely, preregistered randomized controlled trials that orthogonalize message content and audit selection, use audit rates of the magnitude observed in practice, vary delivery channels and senders, and track heterogeneous and persistent effects over time. Where the intensive margin is not observable, reminder campaigns should be paired with third-party information or random post-filing audits, and their cost-effectiveness evaluated in terms of additional revenue per euro spent.

In sum, our evidence indicates that minimalist reminder communications can meaningfully shift the extensive margin of compliance even when incentives are held fixed, whereas informative messages in this setting appear to work primarily through deterrence. The broader implication is that communication policies should not be evaluated only in reduced-form terms: whether a message works depends on whether it changes attention, norms, or perceived enforcement. Translating this decomposition into policy requires field validation, but the distinction is conceptually important for the design of scalable compliance interventions.

CRediT authorship contribution statement

Giovanni Di Bartolomeo: Writing – original draft, Methodology, Formal analysis, Conceptualization. **Silvia Fedeli:** Writing – review & editing, Investigation, Conceptualization. **Stefano Papa:** Writing – review & editing, Project administration, Formal analysis, Data curation.

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Appendix A

Table A.1 reports, for each tax rate, the per-session allocation of rounds to NM, IM, and NN. Sessions were implemented using alternative sequences of round types (i.e., different within-session orderings).

Table A.1
Sessions and communication regimes.

	taxrate	Sessions						
		1	2	3	4	5	6	
Non-informative NM [54]	0.30	3	3	3	3	3	3	18
	0.45	3	3	3	3	3	3	18
	0.60	3	3	3	3	3	3	18
Informative IM [54]	0.30	3	3	3	3	3	3	18
	0.45	3	3	3	3	3	3	18
	0.60	3	3	3	3	3	3	18
No messages NN [84]	0.30	5	4	5	4	5	5	28
	0.45	5	5	4	5	5	4	28
	0.60	4	5	5	5	4	5	28
		32	32	32	32	32	32	

Appendix B. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.socec.2026.102624.

Data availability

Data will be made available on request.

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