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Tax policy levers and taxpayer decision-making: insights from a laboratory experiment on an evolutionary model of income tax evasion decisions

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ABSTRACT

An evolutionary game provides a parsimonious benchmark for repeated interactions between tax authorities and taxpayers in income-tax evasion decisions. The model relaxes the assumption of fully rational, forward-looking agents and characterizes the stationary shares of inspection and full compliance through payoff comparisons. It predicts that lower tax rates and higher penalties are associated with a larger share of fully honest taxpayers.

A laboratory experiment from the taxpayers' perspective examines how tax-authority policy instruments affect decisions across repeated rounds. Consistent with the model's comparative statics, a higher tax rate is associated with a lower probability of full honesty, whereas a higher penalty rate is associated with a higher probability of full honesty. The data also show patterns consistent with experience-based adjustment, including lower compliance after an audit, a pattern commonly labeled the “bomb-crater effect”. Because subjective beliefs were not elicited, we frame these behavioral mechanisms as interpretations of the observed patterns rather than as directly identified psychological channels.

1. Introduction

We present an evolutionary game to examine the role of repeated interactions between tax authorities and taxpayers in income tax evasion decisions. We test the model’s predictions with a laboratory experiment focusing on taxpayers’ reactions to policy levers in the hands of tax authorities. We also study the effects of repeated interaction on taxpayers to assess whether and how some form of “evolutionary pressure” affects their choices, thus supporting the assumption of limited rationality.

Allingham & Sandmo (1972), within the expected utility theory (EUT) framework, assume that rational, utility-maximizing, and risk-averse taxpayers make their tax evasion decisions in isolation, given the policy levers (i.e., inspections, penalties, and tax rates) set by fiscal institutions. The model predicts a negative relationship between the penalty rate and tax evasion. Moreover, under standard assumptions, it predicts that individuals may be less willing to evade when the tax rate is higher. This is an important insight, but it leads to the odd implication that governments can encourage greater tax compliance by increasing the tax burden. By contrast, a vast part of the experimental literature supports a positive impact of high tax rates on tax evasion (see Alm & Malézieux, 2021; Malézieux, 2018).

Evolutionary game theory (EGT) can address these paradoxical effects by relaxing the assumption of fully rational agents and by allowing strategies to evolve through experience and interaction. In our context, players adjust their behavior based on the relative attractiveness of available strategies (e.g., being honest versus cheating), as they learn from experience within a given enforcement environment. At the population level, these payoff differences translate into changes in strategy prevalence, and EGT provides a natural way to characterize a stable long-run outcome.

In economic terms, the equilibrium we focus on is a stationary benchmark analogous to a steady state in growth models (Barro & Sala-i-Martin, 2004). We derive this benchmark in a fully static way, without relying on a specific system of differential equations. We then interpret the stationary benchmark as evolutionarily stable in the standard “invasion” sense (Weibull-style): a small fraction of agents deviating from the incumbent behavior cannot obtain higher expected payoffs when the population is almost entirely at the incumbent state.

The model posited here assumes a population divided into two subpopulations (taxpayers and tax agencies). Individuals are not assumed to solve a fully rational, forward-looking optimization problem. Instead, taxpayers and agencies respond to incentives by making simple payoff-based comparisons among available strategies. Tax

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agencies are allowed to differ in their audit activities (reflecting enforcement discretion and constraints), and the theoretical analysis characterizes a stationary benchmark—derived statically—that summarizes the equilibrium shares of inspection and full compliance.

As for the tax authority's role, empirical evidence supports this assumption. There is no unique, centralized tax agency that uniformly controls taxpayers. Rather, tax agencies operate under common legal rules but may retain discretion because universal auditing is infeasible and audit criteria often allow targeted, partial, or no inspection when expected returns are low. This motivates the model's distinction between inspecting and non-inspecting agencies.

The model links repeated interaction to taxpayers' compliance choices through the distribution of compliance in the population. Empirically, we test this channel using tax and penalty rates, prior audit experience, and a lagged leave-one-out peer-compliance proxy, Beta avg. (see below, Section 4.1).

On these grounds, and given the compelling reasons to find realistic ways to combat tax evasion, we test the model in a laboratory experiment that mimics key features of the theoretical environment from the taxpayers' perspective. We examine how policy tools in the hands of tax authorities affect taxpayers' decisions in repeated interactions. In particular, we ask whether the proportions of honest and cheating taxpayers change when individuals face different tax and penalty rates, and whether observed behavior is associated with prior audit experience and the lagged peer-compliance proxy.

Our results show that the main experimental patterns are consistent with the theoretical comparative statics. Increasing the tax rate reduces the proportion of fully honest taxpayers, whereas increasing the penalty rate increases the likelihood of full honesty. The effect of the penalty rate also changes over time: the interaction between penalty rate and round is statistically significant. Finally, compliance falls after an audit in the previous round, a pattern consistent with the "bomb-crater effect" (Berger et al., 2021; DeBacker et al., 2015; Garrido & Mittone, 2013; Kastlunger et al., 2009; Kasper & Alm, 2022; Mittone, 2006; Mittone et al., 2017). Since the experiment did not elicit subjective beliefs, we interpret this behavioral mechanism cautiously and do not claim to identify the underlying belief-updating process directly.

2. Survey of the relevant literature

Allingham and Sandmo (1972) predict a negative relationship between the penalty rate and tax evasion. Moreover, as well as the subsequent expected utility theory (EUT) models of tax evasion, they predict that individuals are less willing to evade the higher tax rate levied (see works by Sandmo (2005) and Sandmo (2012), and the recent surveys by Alm (2019); Devos (2014); Dell'Anno (2022), and Slemrod (2019)). In this respect, however, much of the experimental literature supports the positive impact of high tax rates on tax evasion. Malézieux (2018) finds a positive link between tax rate and evasion in about 60% of the papers he considered (see also Alm & Malézieux, 2021).

To mitigate these paradoxical consequences, some formalization within the EUT setting has focused either on the interdependence among taxpayers and between taxpayers and tax authorities or on the role of the tax administration in forging taxpayer behavior in dynamic contexts. The results suggest that both social interactions, social norms, and repeated relationships between actors are relevant in tax evasion decisions. For example, within the EUT, Greenberg (1984) employs a dynamic, multiple-stage game with utility-maximizing individuals to investigate optimal audit activities and each individual's decision to be honest or cheat. Fortin et al. (2007) allow social interactions in dynamic repeated games. Regarding bargaining and agency models, see, for example, Cronshaw and Alm (1995); Erard and Feinstein (1994), and Graetz et al. (1986).

The view that interactions between taxpayers and tax authorities are critical to individual compliance decisions is also supported by experimental studies showing that individuals behave as part of a collectivity, where social norms, moral suasion, peer pressure, and experience may drive their behavior. For example, Geeroms and Wilmots (1985) exploit the interdependence between taxpayer behavior when peers evade and the mixed moral effects of tax evasion imitation. Torgler (2003) experimentally identifies factors such as moral suasion that positively affect compliance decisions. Dwenger et al. (2016) show intrinsic motivations relevant to compliance decisions. Alm et al. (2017) stress the importance of peer effects, suggesting that neighboring behavior affects tax compliance behaviors. Battiston & Gamba (2016) investigate the role of social pressures on individuals' attitudes towards compliance within a field experiment conducted among bakeries in Italy. Vogel (1974) measures the effect of improved economic status on individuals' compliance behavior using a survey of Swedish taxpayers' attitudes, suggesting that peer effects matter: individuals' perceptions of their reference group's orientation toward tax evasion affect their decisions. In addition, recent studies present models that show taxpayers adjusting their tax compliance in response to their social reputation. To this end, they observe their neighbors' tax morale and social behavior as they are socially connected to other individuals in their network (Di Gioacchino & Fichera, 2020, 2022).

In this context, a crucial role is played by learning that has been widely studied in experimental economics to understand a variety of theoretical axioms in games, from the stability of mixed strategies equilibria over time (Erev & Roth, 1998) to individuals' initial predisposition towards specific strategies' updating rules (Camerer & Ho, 1999). Recent studies have shown particular interest in the learning process regarding tax compliance choices: Soliman et al. (2014) note that the dynamic interaction between audits and penalty rates may help build taxpayer memory, and that learning from memory represents a potential tool for addressing tax compliance. O'Doherty (2014) compares different audit strategies, suggesting that predictive analytics strategies – used in many OECD countries - are highly effective in reducing the tax gap and that effectiveness is highly related to the taxpayers' learning process estimation by fiscal authorities. Kastlunger et al. (2009) compare participants' immediate reactions to audits with their long-term tax plans, highlighting that inspections conducted at early stages are highly effective in increasing compliance levels compared to those conducted later. Their results also suggest a positive correlation between the level of tax evasion and the period during which individuals are not inspected.

As for the tax authority's role in individual tax evasion decisions, the experimental evidence provides contrasting explanations. It supports motley taxpayer attitudes toward tax planning, thereby threatening the hypothesis of taxpayers as entirely rational agents. Alm et al. (1990) find that detection tools and tax rates significantly impact compliance decisions. Tax revenues increase with lower marginal tax rates and, counterintuitively, with less compliance associated with higher penalties and audit probabilities. Ali et al. (2001) find a significant effect of tax and penalty rates on compliance. Baldry (1987) finds evidence that rejects the assumption that tax rate changes do not affect individuals' compliance decisions. Pommerehne & Weck-Hannemann (1996) find that audits positively affect compliance, whereas the penalty rate appears to be less significant. Alm et al. (1993) compare different tax enforcement settings and show that they induce higher compliance with purely random audits. In addition, Alm et al. (1995) find an increase in compliance levels in response to increases in both the audit rate and the fine. As for the effect of different tax rate magnitudes on compliance, comparing individuals' different sources of income, individual effort plays a role in compliance decisions in response to changes in tax policies and audit probabilities.

In this context, the evolutionary approach is relatively new, as despite the promising features of these models, very little has been done to apply them to decisions about income tax evasion. Several investigations have applied evolutionary games to the economy of sustainable resources (Zhao et al., 2016), poverty traps (Accinelli & Carrera, 2012), intellectual property rights (Yang et al., 2018), and the labor market (Araujo & De Souza, 2010). Regarding tax planning decisions, Chica et al. (2021) use an evolutionary computational model to investigate the features of fraud in consumption taxes. Carboni & Russu (2020) rely on an evolutionary model to evaluate optimal fiscal policies of the social planner, also accounting for corruption. Only recently, Petrohilos-Andrianos & Xepapadeas (2016) studied government regulations in the context of tax-evading agents who are not entirely rational. Related contributions have also studied tax evasion under behavioral assumptions (e.g., prospect theory) using analytical and simulation-based approaches (De Giovanni et al., 2019), as well as extensions that include bribe-prone auditors and adaptive behavior (De Giovanni et al., 2023). The papers of De Giovanni et al. (2019 and 2023) are purely analytical/simulation-based.

Our paper builds on this emerging literature by modeling the interaction between taxpayers and tax authorities as a two-population evolutionary game, without relying on EUT or prospect theory. The theoretical analysis focuses on a stationary benchmark characterized statically through payoff comparisons and interpreted in the standard invasion-resistance sense (Weibull, 1995). This benchmark predicts that higher tax rates increase evasion. The 45-round laboratory experiment provides evidence consistent with these comparative statics and with several forms of experience-based adjustment.

The conceptual framework is based on modeling a population of a large number of individuals, each with the same set of strategies, matched at random with another. In each interaction, individuals compare the expected payoffs associated with the available strategies (often referred to as fitness), where the relative attractiveness of a given behavior depends on the composition of behaviors in the population (Weibull, 1995). Therefore, evolutionary games can be an alternative to classical game theory and EUT in contexts where interactions indirectly influence behavior, mainly because the equilibrium benchmark does not require fully rational, forward-looking agents. Rather, it is consistent with agents driven by context, history, and perception.

3. The model

This section develops a parsimonious evolutionary game model of tax compliance. The model's main output is a stationary benchmark (p*, q*), where p* denotes the share of inspections and q* the share of fully compliant taxpayers. We derive this benchmark statically from payoff comparisons and use it to obtain transparent comparative statics for the laboratory analysis.

We model tax compliance as the outcome of a game between two large populations: tax agencies and taxpayers. Each encounter is a one-shot stage game in which the agency chooses whether to inspect, and the taxpayer chooses whether to be fully compliant or to evade. Because agents are boundedly rational, we do not interpret behavior as the solution to a fully forward-looking optimization problem. Instead, individuals are assumed to respond to incentives using simple payoff-based comparisons between available actions (e.g., comparing the expected payoff from compliance versus evasion, given the perceived enforcement environment). At the aggregate level, these payoff differentials determine which behaviors tend to be more prevalent in the population.

The population state is summarized by two shares. Let p denote the fraction (or probability) of agencies that inspect, and let q denote the fraction of taxpayers who are fully compliant. The pair (p, q), therefore, lies in the unit square and captures the strategic environment faced by a randomly matched agent. Our analysis focuses on a stationary benchmark (p*, q*) in which neither population has an incentive to shift away from its current mix of behaviors. This benchmark provides clear comparative statics with respect to policy parameters (tax and penalty rates) and enforcement frictions, which we take to the laboratory. The experimental protocol reproduces the taxpayer-side incentive structure over repeated rounds within fixed-parameter scenarios, allowing behavior to adjust with feedback; the empirical analysis tests the model's comparative statics using full honesty ($\beta_{it} = 1$) as the direct analog of q and uses Beta avg. only as a lagged ex-post measure of the surrounding peer-compliance environment.

3.1. *Strategies, notation, and empirical mapping of β*

There are two large populations: taxpayers (T) and tax agencies (A). In each encounter, one taxpayer is randomly matched with one agency. Actions are chosen simultaneously. Tax agencies choose between: Inspect (I) and Not inspect (NI). The action NI also captures the practical possibility that a taxpayer is not audited because the number of inspectors is limited.

Taxpayers choose between two discrete strategies: Honest (H), corresponding to full compliance (declared share $\beta = 1$), and Cheat (C), corresponding to evasion (declared share $\beta = \overline{\beta} < 1$). Importantly, in the theoretical model, β under strategy C is not an idiosyncratic continuous choice: the model is a 2×2 game, and all taxpayers who choose C are characterized by the same declared share $\overline{\beta}$, common across evaders in a given environment. In the experiment, instead, participants choose a declaration share β_{it} in [0,1] in each round. We therefore use the indicator $\beta_{it} = 1$ as the direct empirical counterpart of H, while Beta avg. is used as a lagged ex-post proxy for the peer-compliance environment.

Let $Y > 0$ denote true income (for simplicity, common across taxpayers). Let $\tau \in (0,1)$ be the tax rate applied to declared income, and let $\varphi > 0$ be the penalty rate applied to evaded tax when evasion is detected. The tax agency incurs an inspection cost equal to $Z \cdot \tau \cdot Y$, with $Z > 0$. If an evader is not inspected, the agency bears an opportunity cost proportional to undeclared income, equal to $K \cdot (1 - \overline{\beta}) \cdot Y$, with $K > 0$, where under strategy C we set $\beta = \overline{\beta} < 1$.

3.2. *A. stage-game payoffs*

Tax agency payoffs are denoted $\pi_A(\cdot,\cdot)$; taxpayer payoffs are denoted $\pi_T(\cdot,\cdot)$. The specific payoff structure of the game is the following.

	"honest" $\beta = 1$	"cheat" $\beta = \overline{\beta} < 1$
"inspect"	π_A(I,H), π_T(I,H)	π_A(I,C), π_T(I,C)
"non-inspect"	π_A(NI,H), π_T(NI,H)	π_A(NI,C), π_T(NI,C)

When an inspecting agency is matched with an honest taxpayer, the agency payoff πA(I,H) is tax revenue on true income net of inspection costs, $\tau \cdot Y - Z \cdot \tau \cdot Y$, as in Eq. (1). The taxpayer payoff πT(I,H) is after-tax income, $(1-\tau) \cdot Y$, as in Eq. (2).

$$\pi_A(I,H) = \tau \cdot Y - Z \cdot \tau \cdot Y \tag{1}$$

$$\pi_T(I,H) = (1 - \tau) \cdot Y \tag{2}$$

When an inspecting agency is matched with a cheating taxpayer, the agency receives tax revenue on true income plus fines on evaded tax, net of inspection costs, as in Eq. (3). The cheating taxpayer receives disposable income net of taxes and fines, as in Eq. (4). Under strategy C, $\beta = \beta^{-} < 1$.

$$\pi_A(I,C) = \tau \cdot Y + \varphi \cdot \tau \cdot (1 - \beta) \cdot Y - Z \cdot \tau \cdot Y \tag{3}$$

$$\pi_T(I,C) = (1 - \tau) \cdot Y - \varphi \cdot \tau \cdot (1 - \beta) \cdot Y \tag{4}$$

When a non-inspecting agency is matched with an honest taxpayer, the agency receives the tax due as in Eq. (5), while the taxpayer receives after-tax income as in Eq. (6).

$$\pi_A(NI,H) = \tau \cdot Y \tag{5}$$

$$\pi_T(NI,H) = (1 - \tau) \cdot Y \tag{6}$$

When a non-inspecting agency is matched with a cheating taxpayer, the taxpayer receives disposable income after tax on under-declared income, as in Eq. (8). The agency receives revenue on the declared income net of the opportunity cost of missed inspection, as in Eq. (7). Under strategy C, $\beta = \bar{\beta} < 1$.

$$\pi_A(NI, C) = \tau \cdot \beta \cdot Y - K \cdot (1-\beta) \cdot Y \tag{7}$$

$$\pi_T(NI, C) = (1 - \tau \cdot \beta) \cdot Y \tag{8}$$

The distinction between inspecting and non-inspecting agencies captures the practical fact that tax authorities cannot audit all taxpayers and may target audits selectively. Universal auditing is infeasible because the taxpayer population is much larger than the enforcement capacity. Moreover, audit rules often allow partial, targeted, or no inspection when expected returns are low. This motivates the opportunity cost of missed audits in Eq. (7).

At a given population state, taxpayers and tax agencies are randomly matched in one-shot pairwise encounters. A match with a non-inspecting agency also captures the practical case in which the number of agencies is smaller than the number of taxpayers. Actions are chosen simultaneously, and neither side observes ex ante which action the other side has chosen.

In evolutionary game studies, the expected payoff of a strategy, given the distribution of strategies in the other subpopulation, is called its "fitness function." For each action—honesty or cheating for taxpayers, inspection or non-inspection for agencies—it equals the weighted average of the stage-game payoffs, with weights given by the probability of meeting each opponent type.

3.3. Population shares and expected payoffs

Let $p \in [0,1]$ denote the share of inspecting agencies in the agency population (equivalently, the probability that a randomly matched agency inspects). Let $q \in [0,1]$ denote the share of fully honest taxpayers in the taxpayer population (probability that a randomly matched taxpayer is honest). Hence, $(1-p)$ is the share of non-inspecting agencies, and $(1-q)$ is the share of evaders.

Given random matching, expected payoffs (fitness) conditional on each action are the following.

3.4. Expected payoffs (fitness) given population shares p and q

Let p be the share of inspecting agencies and q the share of fully honest taxpayers. Expected payoffs conditional on each action are:

$$\Pi_A(I|q) = q \cdot \pi_A(I, H) + (1-q) \cdot \pi_A(I, C) \tag{9}$$

$$\Pi_A(NI|q) = q \cdot \pi_A(NI, H) + (1-q) \cdot \pi_A(NI, C) \tag{10}$$

$$\Pi_T(H|p) = p \cdot \pi_T(I, H) + (1-p) \cdot \pi_T(NI, H) = (1-\tau) \cdot Y \tag{11}$$

$$\Pi_T(C|p) = p \cdot \pi_T(I, C) + (1-p) \cdot \pi_T(NI, C) \tag{12}$$

A conceptual clarification is needed. At the individual level, behavior is guided by comparisons between the expected payoffs of the available strategies (H vs. C for taxpayers; I vs. NI for agencies), conditional on the opponent population state (p, q). References to performance above or below the population mean payoff are population-level statements used by evolutionary selection dynamics to describe changes in strategy shares; they are not an individual decision rule.

3.5. Static equilibrium and stationary benchmark (p*, q*)

We focus on an interior stationary point $(p^*,q^*)\in(0,1)^2$ of the two-population state space. The mixed benchmark reported below follows from the indifference conditions between H and C in each population. Boundary points are also stationary under standard evolutionary dynamics; their interpretation and stability are discussed in Appendix A1, which reports the corresponding replicator dynamics and boundary rest points.

Taxpayers' indifference (derivation of p*):

$$\Pi_T(H|p*) = \Pi_T(C|p*) \tag{13}$$

$$p* = 1/(1+\varphi) \tag{14}$$

Thus, higher penalties reduce the equilibrium inspection share: when φ increases, less inspection is required to sustain deterrence.

Agencies' indifference (derivation of q*):

$$\Pi_A(I|q*) = \Pi_A(NI|q*) \tag{15}$$

$$q* \ = 1 - \frac{\tau Z}{(\varphi\tau + \mathrm{K} + \tau)(1 - \bar{\beta})} \tag{16}$$

An interior solution requires $0 < q^* < 1$, which holds when:

$$0 < \tau \cdot Z < (\varphi \cdot \tau + \tau + K) \cdot (1 - \bar{\beta}) \tag{17}$$

If $\tau \cdot Z$ is too large relative to the right-hand side, enforcement is too costly and the interior expression would imply $q^* \leq 0$; in that case the feasible stationary benchmark collapses to $q^* = 0$ (no full compliance). Conversely, when enforcement costs are low ($\tau \cdot Z$ close to 0), the benchmark approaches q^* close to 1.

Notice that the interior stationary point obtained under replicator dynamics coincides with the mixed equilibrium characterized by mutual indifference between strategies in each population. Hence, the equilibrium shares (p^*, q^*) can be derived statically without specifying any particular dynamic.

3.6. Comparative statics and economic interpretation

The indifference conditions leading to Eqs. (14) and (16) deliver sharp comparative statics (see Appendix A2). The inspection share p^* decreases in φ. The share of full compliance q^* increases in φ, decreases in τ, decreases in Z and increases in K. Moreover, q^* decreases in $\bar{\beta}$: when evaders declare a larger fraction of income under strategy C (higher $\bar{\beta}$), the term $(1-\bar{\beta})$ shrinks, the fiscal/penalty gains from auditing weaken, and full compliance becomes less prevalent in the stationary benchmark.

We interpret (p^*, q^*) as a static invasion-resistance benchmark in the Weibull sense. The formal invasion inequalities and the link with standard replicator dynamics are reported in Appendix A1, so the main text can focus on the payoff comparisons and comparative statics used in the empirical analysis.

4. The experiment

4.1. Experimental design

Within the model's conceptual framework, repeated exposure to the same policy environment may be associated with experience-based adjustment. The experiment, therefore, assesses whether the proportions of fully honest and cheating taxpayers move in the directions predicted by the model when subjects face specific policy levers of the tax authority.

We connect the static invasion-resistance benchmark to the repeated experiment as follows. The stationary benchmark (p^*, q^*) is derived from payoff comparisons and does not rely on a specific adjustment dynamic. Repeated laboratory rounds are used to test the model's comparative statics under fixed policy parameters, with full honesty (βit $= 1$) mapped to H.

The experimental design examines only taxpayers' decisions, treating the enforcement environment as given. In particular, the audit probability is exogenously fixed at $p = 0.5$ in every round and scenario. The central issues investigated are as follows.

First, we verify whether the model's predictions regarding taxpayers' behavior, as presented in Table 1, hold for the policy parameters under

Table 1
Direction of the effects on the equilibrium values of the relevant parameter changes, ceteris paribus.

Equilibrium values	Penalty rate (φ)	Tax rate (τ)	Declared share under strategy C ($\overline{\beta}$)	Inspection cost (Z)	Opportunity cost of missed audit (K)
q^*	>0	<0	<0	<0	>0
$1-q^*$	<0	>0	>0	>0	<0
p^*	<0				
$1-p^*$	>0				

policymakers' control, namely the tax rate and the penalty rate. In addition, we examine the role of Beta avg. as a lagged peer-compliance proxy. Participants do not observe others' declarations and cannot communicate during the experiment; therefore, Beta avg. is not an informational variable directly available to subjects. It is constructed ex post from the experimental data and should be interpreted as a proxy, not as an elicited subjective belief. For each participant i and round t, Beta avg. (i, t − 1) is defined as the average declared income share in round t − 1 across all other participants j ≠ i.

Second, we explore whether the reported income percentage changes with repeated exposure to the same tax authority behavior, under fixed audit probability and fixed policy parameters within each scenario.

The experimental design adapts the conceptual framework of evolutionary tax evasion games to 45 rounds of individual repeated choices. These 45 rounds are divided into five scenarios, each with different combinations of tax and penalty rates and a fixed audit probability p = 0.5. Participants were divided into two groups. The sequence of rounds, scenarios, and treatments is summarized in Table 2 (Part 2). The two groups follow different sequences of scenarios after the common baseline scenario, as summarized in Table 2.

As in the model, the tax system is flat, with rates ranging from 0.25 to 0.60, reflecting perceived tax burdens in various countries. Fines for evaded taxes, i.e., the penalty applied to the evaded income tax of the participant if caught evading, are calculated as a proportion of the evaded tax at a rate ranging from 0.25 to 0.60, together with the full payment of the tax due (see Sandmo, 2005, for the role of the penalty rate relative to the tax rate). Because the experiment focuses on

Table 2
The experimental design.

ID assignment to each participant		Treatments	
		Group 1	Group 2
Part 1		**Risk aversion test**	
Part 2	General instructions for Part 2		
	Instructions for rounds 1 to 9		
	Trial run for rounds 1 to 9		
	Rounds 1 to 9	**Tax rate: 45%**	**Tax rate: 45%**
		Penalty rate: 45%	**Penalty rate: 45%**
	Instructions for rounds 10 to 18		
	Trial run for rounds 10 to 18		
	Rounds 10 to 18	**Tax rate: 60%**	**Tax rate: 45%**
		Penalty rate: 45%	**Penalty rate: 60%**
	Instructions for rounds 19 to 27		
	Trial run for rounds 19 to 27		
	Rounds 19–27	**Tax rate: 25%**	**Tax rate: 45%**
		Penalty rate: 45%	**Penalty rate: 25%**
	Instructions for rounds 28 to 36		
	Trial run for rounds 28 to 36		
	Rounds 28–36	**Tax rate: 60%**	**Tax rate: 25%**
		Penalty rate: 25%	**Penalty rate: 60%**
	Instructions for rounds 37 to 45		
	Trial run for rounds 37 to 45		
	Rounds 37–45	**Tax rate: 25%**	**Tax rate: 60%**
		Penalty rate: 25%	**Penalty rate: 60%**

taxpayers' behavior, we keep the audit probability fixed and exogenous, thus testing the model's comparative statics with respect to tax and penalty rates conditional on a given enforcement intensity.

All participants receive (are assumed to earn) the exact income of 1000 Euros per round and have to decide how much of that income to declare for taxes, knowing that the taxes finance specific, reasonable national health expenditures. Experimental design features (e.g., redistribution mechanisms) are known to affect compliance; our design follows the standard 'no-redistribution' setup used in the literature. After nine rounds, a new scenario with different tax or penalty rates is introduced. We use a random audit with fixed probability, rather than strategic or risk-based audit rules. Participants were fully informed about their income, the audit probability (p = 0.5), and the applicable tax and penalty rates in each scenario. At the end of each round, participants learn individually whether they were audited and the outcome of their choice.

A key novelty of this experiment is its monitoring of individual decision-making processes over multiple rounds of a repeated decision environment, aiming to capture behavioral adjustment under repeated exposure and feedback. The multiple rounds allow us to test whether memory is formed at the individual level through repeated exposure to the same enforcement parameters and round-by-round feedback. This is consistent with adaptive behavior and limited rationality, while the theoretical model provides the stationary benchmark and comparative statics used to structure the hypotheses.

4.2. Procedures

The experiment was conducted online using Qualtrics and facilitated via Zoom and Google Meet by the Faculty of Economics at Sapienza University of Rome. A detailed description of how the experiment was set up in Qualtrics is available from the authors upon request. After preregistration (second-year undergraduate students of the Faculty of Economics of Sapienza were invited to participate and asked to preregister to choose the preferred session among the available dates), 106 participants, all students, took part in five sessions held from 28/12/2020 to 23/1/2021. Each session comprised 45 rounds, yielding 4770 individual observations. Participants were divided into two groups (Group 1 and Group 2) with balanced gender representation (50 females and 56 males).

Each experiment session had two parts. Upon arrival/connection with the virtual lab at the chosen time, the session began (Part 1) with a standard risk aversion test (Levy, 1994; Quattrone & Tversky, 1988), involving decisions about investing an endowment of 50 Euros in a coin toss, given that they would receive the invested amount multiplied by 2.5 if the result were heads and zero if the result was tails.

Following the risk attitude test, the experimenter provided instructions on Part 2, explaining that participants were part of a community of students expected to pay taxes on a fixed income of 1000 Euros per round, with taxes funding national health expenditures. Instructions detailed tax and fine calculations, and participants knew there was always a 50% chance of being inspected each round and that inspections were randomized to ensure consistent 50% inspection rates. Note that we chose a high p = 0.5 to ensure a salient and easily understood audit probability for a student sample (see Abeler et al., 2019).

Participants could simulate the monetary effects of different tax and penalty rate combinations on the income they chose to report (i.e., the amount of disposable income remaining after taxes and any penalties if inspected) before each scenario change. After each round, they were informed individually of the inspection outcomes and their disposable income. The complete text of the experiment is available from the authors upon request.

Table 2 (Part 2) outlines that the first nine rounds, common to both groups, had a tax and penalty rate of 45%. After nine rounds, participants were notified of changes in either tax or penalty rates and could trial the new rates' effects and, when ready, play again for nine rounds.

This structure continued for subsequent scenarios, with the final reward (randomly drawn from each individual's performance) in the form of online bookstore vouchers sent via email. All decisions were anonymized using identification codes to ensure participants' names remained unknown to the experimenters. Recruitment and payment details are in Appendix A3.

4.3. Descriptive statistics

We now present descriptive statistics of the experimental data and some preliminary results. Table 3 presents summary statistics for the full panel of 106 individuals, each playing 45 rounds, making 4770 observations. It also reports statistics for the two groups of players.

Table 3
Summary statistics for the whole panel of 106 individuals and for the two groups of players.

(1) Whole sample =group 1 *plus* group 2					
Variable	Mean	Std. dev.	Min	Max	Obs.
β_{it} = individual share of total income declared	0.75	0.36	0	1	4,770
Share of fully honest taxpayers (β_{it} = 1)	0.53	0.50	0	1	4,770
Share of cheaters ($0 < \beta_{it} < 1$)	0.41	0.49	0	1	4,770
Share of ghosts (β_{it} = 0)	0.06	0.24	0	1	4,770
Tax rate	0.44	0.14	0.25	0.60	4,770
Penalty rate	0.44	0.14	0.25	0.60	4,770
Inspection	0.50	0.50	0	1	4,768
Beta avg. (lagged peer proxy)	0.75	0.01	0.73	0.80	4,240
Risk aversion	22.80	11.80	0	50.00	4,770
Share of males	0.53	0.50	0	1	4,770
(2) **Group 1**					
Variable	**Mean**	**Std. dev.**	**Min**	**Max**	**Obs.**
β_{it} = individual share of total income declared	0.74	0.37	0	1	2,385
Share of fully honest taxpayers (β_{it} = 1)	0.54	0.50	0	1	2,385
Share of cheaters ($0 < \beta_{it} < 1$)	0.39	0.49	0	1	2,385
Share of ghosts (β_{it} = 0)	0.07	0.26	0	1	2,385
Tax rate	0.43	0.16	0.25	0.60	2,385
Penalty rate	0.37	0.10	0.25	0.45	2,385
Inspection	0.50	0.50	0	1	2,384
Beta avg. (lagged peer proxy)	0.74	0.01	0.73	0.77	2,120
Risk aversion	24.64	12.55	0	50.00	2,385
Share of males	0.49	0.50	0	1	2,385
(3) **Group 2**					
Variable	**Mean**	**Std. dev.**	**Min**	**Max**	**Obs.**
β_{it} = individual share of total income declared	0.75	0.35	0	1	2,385
Share of fully honest taxpayers (β_{it} = 1)	0.52	0.50	0	1	2,385
Share of cheaters ($0 < \beta_{it} < 1$)	0.43	0.49	0	1	2,385
Share of ghosts (βit = 0)	0.05	0.22	0	1	2,385
Tax rate	0.44	0.11	0.25	0.60	2,385
Penalty rate	0.50	0.14	0.25	0.60	2,385
Inspection	0.50	0.50	0	1	2,384
Beta avg. (lagged peer proxy)	0.76	0.01	0.75	0.80	2,120
Risk aversion	20.96	10.68	0	50.00	2,385
Share of males	0.57	0.50	0	1	2,385

In Table 3, the individual share of total income declared corresponds to βit, the declaration share chosen by individual i in round t. Beta avg. is the lagged leave-one-out peer-compliance proxy defined above. The share of fully honest taxpayers ($\beta_{it} = 1$) corresponds to full compliance in the model (q), whereas cheaters and ghosts correspond to evasion outcomes ($1-q$). Inspection is a dummy equal to 1 when a taxpayer is audited in a given round and zero otherwise. Risk aversion takes values from 0 to 50, with 50 representing the maximum level of risk aversion.

Preliminary results - not reported but available from the authors upon request - were obtained using the Kruskal-Wallis (KW) test.[1] We measure the effect of different combinations of tax and penalty rates on the proportions of honest and cheating individuals to confirm the model results. When subjected to different tax and penalty rates, they significantly differ in the proportions of honest, cheating, and ghost individuals. Moreover, considering that the subsamples compared with the KW test, concerning the different scenarios proposed, may not be independent of each other, and that the observations at the individual level may not be independent across rounds, we also performed the nonparametric Wilcoxon signed rank test on the average observations at the session level on the average proportion of income declared. Again, the results, which are not reported but are available upon request, highlight two elements: First, changes in treatment have a statistically significant effect on the average share of income reported by individuals. Second, individuals adjust their reporting based on their experience in previous rounds, regardless of the combination of tax and penalty rates to which they are subject.

4.4. Regression results

Based on previous preliminary evidence, we now empirically test the experimental results as follows. First, we use a probit regression to estimate the probability that an individual is a fully honest taxpayer ($\beta_{it} = 1$). This provides a direct test of the theoretical comparative statics in Table 1 for tax and penalty rates and a cautious test of the association with Beta avg. In the theoretical model, $\beta^{-} < 1$ characterizes the fixed declared share under strategy C; in the experiment, individuals choose β_{it} each round. Second, we use random-effects panel regressions to examine correlates of individual compliance (β_{it}) in repeated interactions, focusing on tax and penalty rates, prior audit experience (inspection at $t-1$), and Beta avg.

In order to provide a straightforward test of the theoretical model's qualitative predictions, we proceed as follows. In Table 4, we estimate a base probit model (Model 1) that includes the constant term and the variables corresponding to the theoretical environment: Beta avg., the tax rate, the penalty rate, and inspection at $t-1$. In Model 2, we extend the set of regressors by controlling for gender (sex) and risk aversion. Finally, Model 3 introduces time/round fixed effects to capture possible shocks that simultaneously affect all individuals within a given scenario. Standard errors are clustered at the individual (participant) level.

Note that the estimates in Table 4 are relatively stable across the three model specifications. The results from Models 1, 2, and 3 are comparable, suggesting that the sign and magnitude of the estimated coefficients are robust to different specifications. In each case, an increase in the tax rate reduces the probability of being a fully honest taxpayer. Increasing the penalty rate increases the probability of being fully honest. Having been audited in the previous round ($t-1$) reduces the probability of being fully honest. Unlike the model where the inspection share is endogenous, in the experiment, the tax authority's behavior is fixed (audit probability $p = 0.5$ in all rounds and scenarios) in order to study taxpayers' choices in repeated games. Therefore, the negative effect of inspection at ($t-1$) reflects behavioral updating from experienced audits, rather than changes in the objective audit probability.

[1] The KW test is used because the observations do not follow the normal distribution; therefore, the parametric t-test or the ANOVA test is not applicable. Since the null hypothesis tested with the KW test represents the circumstance in which the mean ranks of two subsamples are equal, the test deals with whether two subsamples come from the same distribution. Thus, in this context, the null hypothesis represents the case where the proportions of honest, cheating, and ghost participants do not vary between scenarios. In contrast, the alternative to be tested represents the case in which a change in scenario affects the proportions of honest, cheating, and ghost participants.

Table 4
Estimated marginal effects from probit regressions of the probability of being fully honest (β_{it}=1).

	Model 1		Model 2		Model 3	
tax rate	−0.278	***	−0.278	***	−0.282	***
	(0.083)		(0.083)		(0.083)	
penalty rate	0.190	**	0.190	**	0.206	***
	(0.076)		(0.076)		(0.078)	
inspection (t − 1)	−0.075	***	−0.075	***	−0.076	***
	(0.022)		(0.022)		(0.022)	
Beta avg. (lagged peer proxy)	−0.012	*	−0.012	*	−0.076	***
	(0.006)		(0.006)		(0.016)	
risk aversion			−0.001		−0.002	
			(0.003)		(0.003)	
Sex			0.012		0.026	
			(0.060)		(0.057)	
Round fixed effects					Yes	
Constant	Yes		Yes		Yes	

Notes: Robust standard errors in parentheses. Reported figures are marginal effects. Standard errors are clustered at the individual (participant) level. All probit models include a constant; with round fixed effects (Model 3) one category is omitted for collinearity with the constant. Note that, since inspection(t − 1) is included, the first round is excluded by construction. *p < 0.10; **p < 0.05; ***p < 0.01.

The recent experimental literature on tax evasion has identified the "bomb-crater effect" (Mittone, 2006), which refers to tax compliance declining immediately after a taxpayer is audited. In war, troops under heavy enemy fire hide in the craters of recent explosions, believing that it is doubtful that subsequent bombs will fall in precisely the same place in a short period. Something similar may happen in the context of tax audits: subjects may temporarily underestimate the near-term likelihood of another audit after being audited, even though audits are randomized.

Model 3 controls for round fixed effects, capturing round-specific shocks that simultaneously affect all participants within a given scenario. When round fixed effects are included, the one-round dummy is omitted to avoid collinearity with the constant. Since inspection(t − 1) is included, the first round is excluded by construction.

Model 3 mirrors the qualitative directions in Table 1: the probability of being fully honest increases when the tax rate decreases, the penalty rate increases, and Beta avg. decreases. A 10-percentage-point increase in the tax rate (penalty rate) correlates with a 2.82 percentage-point decrease (2.06 percentage-point increase) in the probability of full honesty. The coefficient for Beta avg. is negative, indicating that higher peer compliance in the previous round is associated with a lower probability of full honesty by the individual in the current round. The negative coefficient on inspection at t − 1 is consistent with the empirical pattern commonly labeled the bomb-crater effect, although the experiment does not directly identify the belief-updating process behind it.

Tables 5 and 6 use random-effects estimates to explore further the correlates of individual compliance, measured by β_{it}. We focus on prior audit experience, time within scenario, policy parameters, and the lagged peer-compliance proxy (Beta avg.), while keeping a clear distinction between estimated associations and behavioral interpretations.

In Table 5, Model 1 incorporates the variables corresponding to the theoretical environment: Beta avg., tax rate, penalty rate, and inspection at t − 1. Model 2 adds sex and risk aversion. Model 3 includes round fixed effects. Standard errors are clustered at the individual (participant) level. As in the probit models, the constant term captures round 2 fixed effects as the one-round dummy has to be excluded to avoid collinearity.

Including round fixed effects affects the magnitude of some coefficients, which is consistent with fixed effects absorbing round-specific shocks. Importantly, the sign and statistical significance of the main policy effects (tax and penalty rates) and of Beta avg. remain unchanged across specifications, supporting the robustness of the qualitative conclusions.

Table 5
Regression of the share of declared income.

	Model 1		Model 2		Model 3	
tax rate	−0.185	***	−0.185	***	−0.174	**
	(0.070)		(0.070)		(0.070)	
penalty rate	0.311	***	0.311	***	0.450	***
	(0.063)		(0.064)		(0.065)	
inspection (t − 1)	−0.091	***	−0.089	***	−0.090	***
	(0.018)		(0.018)		(0.017)	
Beta avg. (lagged peer proxy)	−0.045	***	−0.043	***	−0.090	***
	(0.011)		(0.010)		(0.016)	
risk aversion			0.000		0.000	
			(0.002)		(0.002)	
sex			−0.039		−0.029	
			(0.040)		(0.039)	
Constant	Yes		Yes		Yes	
Round fixed effects					Yes	

Notes: Standard errors are clustered at the individual (participant) level. A constant term is included in all models; with round fixed effects (Model 3), one category is omitted for collinearity with the constant. *p < 0.10; **p < 0.05; ***p < 0.01.

Table 6
Regression of the share of declared income on selected covariates and interaction terms.

	Model 4	Model 5	Model 6
tax rate	−0.145	−0.174***	−0.145
	(0.105)	(0.070)	(0.105)
penalty rate	0.674***	0.674***	0.450***
	(0.123)	(0.123)	(0.065)
inspection (t − 1)	−0.090***	−0.090***	−0.090***
	(0.017)	(0.017)	(0.017)
Beta avg. (lagged peer proxy)	−0.092***	−0.092***	−0.090***
	(0.017)	(0.017)	(0.016)
risk aversion	0.000	0.000	0.000
	(0.002)	(0.002)	(0.002)
sex	−0.029	−0.029	−0.029
	(0.039)	(0.039)	(0.039)
Constant	Yes	Yes	Yes
Round fixed effects	Yes	Yes	Yes
Interaction terms:			
tax rate × round	−0.005		−0.005
	(0.015)		(0.015)
penalty rate × round	−0.040**	−0.040***	
	(0.016)	(0.016)	

Notes: Standard errors are clustered at the individual (participant) level. *p < 0.10; **p < 0.05; ***p < 0.01.

The share of declared income is negatively associated with the tax rate. For instance, Models 1 and 2 show that a 10-percentage-point increase in the tax rate is associated with a 1.85 percentage-point decrease in the share of reported income. As suggested by the theoretical benchmark, Beta avg. enters negatively in all specifications. Increased penalty rates are associated with higher β_{it}, whereas being audited in the previous round (t − 1) is associated with lower β_{it}. This latter pattern is consistent with the bomb-crater effect discussed in the literature, but we treat the mechanism cautiously because subjective beliefs about audit probabilities were not elicited. Risk aversion and gender are not statistically significant in these specifications.

In Table 6, we test whether the associations between compliance and the tax and penalty rates change with time within each scenario. Model 4 includes both interactions jointly, while Models 5 and 6 test the penalty-rate and tax-rate interactions separately.

Model 4 signals that only the interaction between the penalty rate and time is statistically significant, whereas the interaction between tax rate and time is not statistically significant. We do not find any nonlinearity in the effects of the tax rate on the share of declared income over time, even when we separately consider, in Model 6, the effect of the interaction between tax rate and time. This suggests that individuals are unlikely to change their behavior over time in response to changes in the tax rate within a scenario.

For the penalty-rate interaction with time, Models 4 and 5 show that the positive association between the penalty rate and declared income declines over rounds within a scenario. For each additional round, the association falls by 0.04. This pattern is consistent with experience-based adjustment to a fixed policy environment, but it should not be read as direct identification of a specific learning mechanism.

Accordingly, we avoid interpreting this coefficient as proof of a distinct gambler's fallacy. A more cautious interpretation is that repeated exposure changes the observed association between penalties and compliance, in a way that is compatible with bounded rationality and experience-based adjustment. The data do not allow us to determine whether this adjustment reflects misperception of expected punishment, loss repair, changing attention to sanctions, or another behavioral channel. These mechanisms remain plausible interpretations rather than directly identified effects.

Considering the estimated coefficients, the association between the penalty rate and the declared income share is 0.634 in the first round of a scenario (0.674 - 0.040). By the ninth round, this association is less than half of its initial magnitude. Since the probability of inspection is fixed at $p = 0.5$ by design, this time pattern suggests that subjects' responses to a fixed penalty environment change with experience. Because subjective beliefs were not elicited, however, we interpret this as evidence consistent with bounded rationality in repeated choices, not as direct evidence of a particular belief-bias mechanism.

The interaction between time and the penalty rate is therefore best described as suggestive of an experience-based adjustment in compliance behavior. It indicates that individuals react differently to the same penalty environment as a scenario unfolds, but the data do not identify whether this reflects changing perceived audit probabilities, changing perceived sanction severity, or another behavioral process.

Overall, the experimental evidence supports a cautious behavioral interpretation: compliance is associated with prior audit experience and with time-varying responses to penalties. These findings are consistent with bounded rationality and motivate future work that directly elicits subjective beliefs about audit probabilities, sanctions, and peer compliance.

5. Conclusions

Evolutionary games have been applied in various economic contexts, but relatively little work has examined income tax evasion decisions through this lens. This study presents an evolutionary game model centered on repeated interactions between tax authorities and taxpayers. The model departs from full rationality and shows how the distribution of compliance in the population can affect the stationary share of full compliance. In the experiment, the corresponding empirical measure is a lagged peer-compliance proxy, Beta avg., which is constructed ex post and interpreted with appropriate caution.

The laboratory experiment tests the model's comparative statics from the taxpayers' perspective. We examine whether the proportions of fully honest and cheating taxpayers change when subjects face different tax and penalty rates, while the audit probability is fixed by design. We also study whether individual decisions are associated with prior audit experience and the lagged peer-compliance proxy.

The experimental results are consistent with the main predictions of the theoretical benchmark. Increasing the tax rate reduces the proportion of fully honest taxpayers, whereas increasing the penalty rate increases the likelihood of full honesty. The effect of the penalty rate changes over time within a scenario, suggesting experience-based adjustment to a fixed policy environment. Compliance also declines after an audit, a pattern consistent with the bomb-crater effect. Because the experiment did not elicit subjective beliefs, these behavioral mechanisms should be read as cautious interpretations of the observed data rather than as directly identified psychological channels.

Our study offers several important contributions to the literature on tax evasion. First, unlike standard theory, we have shown that high tax rates can positively affect tax evasion. Second, our results indicate that policy tools, such as penalty rates, can increase tax compliance, albeit at a declining rate over time within a scenario. Third, the lagged peer-compliance environment, proxied by Beta avg., is significantly associated with individual reporting behavior; this should be interpreted as evidence on the compliance environment rather than as direct evidence on subjective beliefs.

Our study also has important implications for tax authorities and policymakers. First, tax authorities should be cautious when increasing tax rates, as this may have unintended consequences for evasion. Second, the time-varying effect of penalty rates suggests that taxpayers' responses to policy levers may change with repeated exposure. Third, policy design should account for the peer-compliance environment and social norms, while recognizing that subjective beliefs were not directly elicited in our experiment.

CRediT authorship contribution statement

Silvia Fedeli: Writing – review & editing, Writing – original draft, Resources, Project administration, Methodology, Funding acquisition, Formal analysis, Conceptualization. **Francesco Bloise:** Writing – review & editing, Methodology, Investigation, Data curation. **Elena Costarelli:** Software, Methodology, Data curation.

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Appendix A1. Why the static derivation delivers the same interior equilibrium as replicator dynamics

This appendix clarifies why the stationary benchmark (p^*, q^*) derived in the main text via payoff indifference coincides with the interior rest point obtained under standard replicator dynamics.

Consider a two-population setting with two strategies per population. Let p in [0,1] be the share of Inspect (I) in the agency population, and q in [0,1] be the share of Honest (H) in the taxpayer population. Let $\Pi_A(I \mid q)$ and $\Pi_A(NI \mid q)$ denote agencies' expected payoffs from I and NI, given q; let $\Pi_T(H \mid p)$ and $\Pi_T(C \mid p)$ denote taxpayers' expected payoffs from H and C, given p. These expected payoffs are exactly those defined in Section 3 (Eqs. (9)–(12)).

Under standard two-strategy replicator dynamics, the evolution of strategy shares can be written in the compact form:

$$dp/dt = p(1-p)\cdot[\Pi_A(I|q) - \Pi_A(NI|q)] \tag{A1}$$

$$dq/dt = q(1-q)\cdot[\Pi_T(H|p) - \Pi_T(C|p)] \tag{A2}$$

These expressions imply an immediate equivalence for interior stationary points. If $0 < p < 1$, then $p(1-p) > 0$, and therefore $dp/dt = 0$ if and only if:

$$\Pi_A(I|q) = \Pi_A(NI|q) \tag{A3}$$

Similarly, if $0 < q < 1$, then $q(1-q) > 0$, and therefore $dq/dt = 0$ if and only if:

$$\Pi_T(H|p) = \Pi_T(C|p) \tag{A4}$$

Hence, any interior rest point of the replicator dynamics must satisfy mutual indifference between the two strategies in each population. Conversely, any (p, q) satisfying (A3) and (A4) is an interior stationary point of the replicator dynamics. Therefore, the interior equilibrium shares (p*, q*) can be obtained either dynamically (as the interior rest point of (A1)–(A2)) or statically by solving the indifference conditions (A3)–(A4), corresponding to Eqs. (13) and (15), which yield Eqs. (14) and (16) in the main text.

Finally, note that the replicator dynamics also admits boundary rest points at p in {0,1} and q in {0,1} because of the multiplicative terms $p(1-p)$ and $q(1-q)$. This is why, in the main text, we focus on the interior solution characterized by A3–(A4) and its feasibility conditions, which deliver the comparative statics used in the empirical analysis.

Static stationarity interpretation (Weibull-style) without committing to a specific dynamic

We interpret (p*, q*) as evolutionarily stable in the classic static 'invasion' sense. Consider the incumbent state (p*, q*). Let a small fraction ε of agencies adopt an alternative inspection mixture p′ and a small fraction ε of taxpayers adopt an alternative honesty share q′, with $\varepsilon > 0$ small.

Define average expected payoffs for mixed behavior:

$$\Pi_A(p, q) = p\cdot\Pi_A(I|q) + (1-p)\cdot\Pi_A(NI|q) \tag{A5}$$

$$\Pi_T(q, p) = q\cdot\Pi_T(H|p) + (1-q)\cdot\Pi_T(C|p) \tag{A6}$$

Then (p*, q*) is evolutionarily stable if, for any (p′, q′) different from (p*, q*), there exists $\bar{\varepsilon} > 0$ such that for all ε in $(0, \bar{\varepsilon})$ incumbents strictly outperform mutants in the slightly perturbed populations:

Invasion inequalities (for small $\varepsilon > 0$): We consider a small fraction ε of agents deviating to alternative behaviors. The equilibrium (p*, q*) is evolutionarily stable if such rare deviators earn a strictly lower expected payoff than incumbents when interacting in an almost entirely incumbent population.

$$\Pi_A(p*, (1-\varepsilon)\cdot q* + \varepsilon\cdot q') > \Pi_A(p', (1-\varepsilon)\cdot q* + \varepsilon\cdot q') \tag{A7}$$

$$\Pi_T(q*, (1-\varepsilon)\cdot p* + \varepsilon\cdot p') > \Pi_T(q', (1-\varepsilon)\cdot p* + \varepsilon\cdot p') \tag{A8}$$

This is a purely static stability notion and does not require specifying a particular adjustment dynamic (replicator equations, imitation, reinforcement learning, etc.). Dynamic interpretations can be provided as optional microfoundations, but the equilibrium characterization and comparative statics used in the remainder of the paper do not depend on any differential equation.

Appendix A2. Comparative statics

$$\frac{\partial q*}{\partial \tau} = -\frac{KZ}{(1-\overline{\beta})(K+\varphi\tau+\tau)^2} < 0$$

$$\frac{\partial q*}{\partial \varphi} = \frac{Z\tau^2}{(1-\overline{\beta})(K+\varphi\tau+\tau)^2} > 0$$

$$\frac{\partial q*}{\partial \overline{\beta}} = -\frac{Z\tau}{(K+\varphi\tau+\tau)(1-\overline{\beta})^2} < 0$$

$$\frac{\partial q*}{\partial K} = \frac{\tau Z}{(1-\overline{\beta})(K+\varphi\tau+\tau)^2} > 0$$

$$\frac{\partial q*}{\partial Z} = -\frac{\tau}{(1-\overline{\beta})(K+\varphi\tau+\tau)} < 0$$

$$\frac{\partial p*}{\partial \varphi} = -\frac{1}{(\varphi+1)^2} < 0$$

Appendix A3. Information on the experiment

Participants were recruited from among the students enrolled in Public Finance and Economic Policy courses at the Faculty of Economics of Sapienza University of Rome. They received an email invitation to participate in the experiment, along with instructions for taking part. They were asked to send their contact information to the experimenters in advance so that they could register. All sessions were conducted after the online lectures, after which the registered participants were invited to remain connected to the virtual room.

The computational part of the experiment was programmed in Qualtrics, and a single customized link was sent to each registered participant through the Qualtrics platform. Before the experiment began, the experimenter explained the instructions and asked the participants to take note of the personal ID number shown to each of them on the screen at the end to get payment after the experiment. During the experiment, students were connected and could communicate with the experimenters in a chat room if they had technical problems. Students were allowed to ask the experimenters to repeat the instructions or part of them (and the experimenter could repeat them partially or totally). No communication between participants was allowed. Once finished, participants left the virtual room.

As described in Table A1, the payment for participation is a voucher ranging from 5 to 20 euros commensurate with one of the results randomly extracted among the results each participant obtained during the experiment and paid according to the distribution reported in the following table. In order to guarantee anonymity, participants were paid by the administrative office of the Department of Economics and Law of Sapienza—University of Rome using their codes. Participants knew officers were unaware of the details and reasons for the payments or anything concerning the experiment.

Table A1
Distribution of vouchers assigned to the 106 participants of the experiment.

Voucher amount	Result in the experiment	Number of participants obtaining the voucher
5 euros	0–550 euros	56
10 euros	551–700 euros	33
20 euros	701–1000 euros	17

Data availability

Data will be made available on request.

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