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Preliminary Results

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Abstract

How does the pace and sequencing of tax reforms influence taxpayer behavior? We design a laboratory experiment to evaluate how individuals respond to gradual versus abrupt tax increases and whether advance information about these reforms alters compliance. Participants face a multi-period tax schedule in which the statutory rate rises either all at once or in steps. We vary whether individuals are informed from the start about the entire path of tax changes or learn about each change only when it takes effect. While the overall magnitude of evasion does not differ significantly between abrupt and staggered reforms, we find that information plays a critical role: individuals with advance knowledge report significantly more income than those without. These findings highlight the importance of transparency and communication in the design of tax policy, particularly when compliance relies on voluntary reporting and enforcement is imperfect.

JEL classifications: H21, H26, C91
Keywords: Tax evasion, Fiscal reform, Laboratory experiment, Policy design

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1 Introduction

Rising global debt requires countries worldwide to undertake fiscal consolidation (International Monetary Fund, 2025; Powell and Valencia, 2023). Boosting revenues, optimizing public spending, and enhancing the progressiveness of fiscal policies dominate the policy discourse, particularly in developing countries (Izquierdo et al., 2018). However, comprehensive reforms tend to be elusive (Keefer and Scartascini, 2022). Widespread distrust in policymakers discourages citizens from endorsing reforms that bear immediate costs, even if they offer long-term benefits (Keefer et al., 2021, 2022). Broad reforms often encounter swift public opposition, with even minor changes facing resistance, sometimes culminating in civil unrest.[1]

Past studies in political economy literature suggest that crises often precipitate stabilization reforms and occasionally trigger structural reforms (Alesina et al., 2006; Drazen and Easterly, 2001; Drazen and Grilli, 1993; Hallerberg and Scartascini, 2015). The timing of the reform's implementation also appears crucial (e.g., the electoral calendar) (Alesina et al., 2006; Hallerberg and Scartascini, 2017), as well as the configuration of domestic political institutions (Ardanaz et al., 2020; Giuliano et al., 2013; Sturzenegger and Tommasi, 1998; Tommasi et al., 2014). The likelihood of reforms may not be independent of the depth and the path of the reform, an understudied topic because political and legal constraints prevent differentiated reform implementations. Some argue that one-time, extensive reforms are better because they concentrate attention and political costs. Others argue that several minor reforms are easier to pass because they attract less attention. Providing information on the schedule of changes can also affect the likelihood of reforms. While shocks may prevent organized collective action against the reform, they may exacerbate public distrust and derail current and future reforms (Keefer and Scartascini, 2022). On the contrary, information about future reforms may facilitate organizing against them, but it could also increase trust and legitimacy for the reform (Alessandro et al., 2021).

In this paper, we examine the effects of the pace of reform, as well as the role of ex-ante information about these reforms, in the context of a laboratory experiment. The lab offers a critical advantage: it allows us to randomize a policy reform that cannot be randomized in the field and can hardly be evaluated using observational data. Building on traditional income generation and tax evasion experiments (Friedland et al., 1978), we introduce two tax rate reform trajectories–gradual increments and a single substantial hike–with and without information about the upcoming reforms. This setting enables us to assess whether the pace of reform affects individual income-generating endeavors and their subsequent reporting behaviors. In this context, where there is no mechanism for protest or reversal of the reform, resistance to the reform is translated into a decrease in intrinsic motivation to pay taxes. We employ a tax

[1] For instance, an attempt to increase the metro fare in Chile in 2019 triggered prolonged protests, resulting in significant infrastructural damage and major economic setbacks. Similarly, opposition to a tax hike in Colombia in 2021 led to numerous casualties.

evasion experiment with a flat tax rate because it has been conducted for over 40 years (Alm and Malézieux, 2021); therefore, there is ample evidence against which to compare our baseline results and formulate our hypotheses. While the impacts of higher tax rates are somewhat predictable, with an increase in the tax rate resulting in a decrease in compliance, the influences of the reform pace and accompanying information are unknown.

For our analytical framework, we modify the standard tax evasion model of Allingham and Sandmo (1972) to encompass an effort component for income generation and an intrinsic tax payment motivation parameter. Consequently, reporting income voluntarily increases utility (i.e., tax morale) (Dwenger et al., 2016). Absent any path dependency of the intrinsic motivation based on the tax rate shifts, one would expect similar outcomes from gradual and abrupt rate changes once the final tax rate is reached. Our study also varies the extent of reform-related information provided to participants, examining its potential impact on outcomes. We incorporate the role of information into the analytical framework to evaluate the potential mechanisms through which information about reforms affects tax compliance in each period.

Experimentally, participants are divided into four groups, differentiated by their exposure to variations in tax rates and the availability of information. Two groups face a sharp tax rate increment (the *abrupt* treatment), while the other two experience it more gradually (the *staggered* treatment). At the experiment's onset, half of the participants (from both treatments) receive future tax rate information (the *information* treatment), while the others remain uninformed. The core outcome metric is tax compliance, which can be perceived as a measure of support for the reform. We also analyze how reforms impact participants' efforts to generate income.

Consistent with existing literature, our findings suggest that higher tax rates are associated with decreased tax reporting. Compared to the original 10% tax rate, a 40% tax rate increases non-compliance by about 9 percentage points–twice the effect observed with a 20% rate. There is negligible variance in tax compliance between groups exposed to staggered or abrupt tax hikes. Reaching the highest rate produces the same level of tax evasion, regardless of whether the reforms were implemented abruptly or staggered over time. However, receiving information about impending reforms in advance significantly mitigates the drops in compliance. For example, those who received information in advance are between 6 and 7 percentage points less likely to commit to full evasion than those who had no ex-ante information about the impending reforms.

According to our theoretical model, two potential mechanisms may explain the differences in compliance based on the information provided. First, information about current and future tax rates enables individuals to optimize intertemporally and decide how much effort to put in and how much to evade, not only according to the current tax rate but also future tax rates. Second, transparency about the future path of tax rates could affect trust in the government, and hence, tax morale. While both options are plausible theoretically, empirically, we show that individuals are not engaging in differential intertemporal optimization.

This paper contributes to the literature on tax evasion by incorporating tax reforms and infor-

mation about the future path of tax rates into the traditional model of tax evasion. That way, policymakers now have some stylized predictions about the impact of reforms and the impact of information about the reforms on compliance and effort.

Second, this paper complements a substantial body of literature on laboratory experiments that aims to understand tax compliance (Alm, 2019; Alm and Malézieux, 2021). While the literature has studied differential compliance in relation to tax rates (Alm et al., 1992), it has not incorporated changes to tax rates, different paths of reform, and the role of information or its absence.

Third, it contributes to the literature on information and public policy, demonstrating that anticipating information can be beneficial for governments in the context of tax reform. There are two noticeable groups of related literature. Closer to our paper is the literature on the impact of surprise and anticipated fiscal changes on economic activity (Mertens and Ravn, 2012; Ricco, 2015). The other is the literature on forward guidance and expectation formation when deciding and informing monetary and exchange rate policy (Jiang and Huang, 2023; Meade et al., 2015). Our results show that information is at least as influential as the design of the reform itself.

It also complements the literature on the role that information has in signaling policy paths to citizens. Central bank announcements simultaneously convey information about monetary policy and the central bank's assessment of the economic outlook. A surprise policy tightening does not have the same effect as one that had been anticipated (Jarociński and Karadi, 2020), and the effect of the shock is correlated with the level of uncertainty: medium- and long-term interest rates respond more to policy surprises when uncertainty is low (De Pooter et al., 2018). In this paper, we also consider shocks that can be anticipated more easily than others (the movement from 30% to 40% could have been easier to predict than the changes from 10% to 20% or from 10% to 40%). Unfortunately, it remains open to future research to randomize the unexpectedness of the shock while controlling for the magnitude of the change.

Finally, we provide clear policy lessons. In scenarios where public dissent against reforms is limited to tax compliance and effort reductions, that is, tax hikes can actually be implemented, abrupt reforms appear superior to gradual ones. Since the change in compliance between the different paths is not significant, abrupt reforms allow for the collection of more taxes earlier. Furthermore, forewarning citizens about upcoming reforms can decrease the negative effects on compliance and effort.

2 Theoretical Motivation

We modify the standard tax evasion model of Allingham and Sandmo (1972) and Srinivasan (1973) to include an effort component for income generation and an intrinsic motivation parameter for paying taxes. In our model, an individual decides how much income to generate z (think of z as $w * l$ where w is the given market wage rate and l the labor used for simplicity),

and how much income y to declare, on which she must pay a tax rate τ. The utility function has three components. First, the utility increases with income after tax that is transformed into consumption ($U'(c) > 0$ and $U''(c) < 0$). Second, the utility decreases with effort to produce income ($U'(z) < 0$ and $U''(z) < 0$) for given consumption. Finally, the individual has an intrinsic motivation to report their income and pay taxes ($S'(y) > 0$, $S''(y) < 0$, $U'(S(y)) > 0$, and $U''(S(y)) < 0$).

There is a possibility that the individual is audited with a probability p. If the individual is audited, she must pay a fine f on the undeclared tax, where $f > 1$. The individual's problem is to maximize the following function:

$$E\left[U\left(c, z, S\left(y\right)\right)\right] \equiv \underbrace{(1-p)U\left(z - y\tau, z, S\left(y\right)\right)}_{\substack{\text{Expected return} \\ \text{of not being audited}}} + \underbrace{pU\left(z - y\tau - (z-y)\tau f, z, S\left(y\right)\right)}_{\substack{\text{Expected return} \\ \text{of being audited}}}$$

The comparative statics of this model are standard: higher penalties and the probability of detection increase declared income, as does a higher intrinsic motivation. An increase in the tax rate generates an ambiguous effect on declaration (a higher tax increases the gain from evasion but also increases the cost if audited), but it has a potentially negative effect on income generation due to reduced post-tax marginal returns to effort.

Including information about the future path of tax rates in the model implies considering a finite-horizon model, where an individual chooses effort and compliance over T periods. Let us now call z_t the true income generated in period t, $y_t \leq z_t$, income reported (declared) in period t, τ_t, tax rate in period t, p_t: audit probability in period t, $f > 1$, penalty multiplier on evaded taxes if audited, $S(y_t, \tau_t)$, and $\delta \in (0, 1]$, the discount factor. Consumption in each period depends on whether the individual is audited:

$$\begin{aligned} c_t &= z_t - \tau_t y_t \quad \text{(not audited)} \\ c_t^{\text{audit}} &= z_t - \tau_t y_t - f\tau_t(z_t - y_t) \quad \text{(audited)} \end{aligned}$$

Let $U(c, z, S)$ be the utility function, increasing in c and S, decreasing in z (effort cost). Then, the expected utility in period t is:

$$EU_t = (1 - p_t)U(c_t, z_t, S(y_t, \tau_t)) + p_t U(c_t^{\text{audit}}, z_t, S(y_t, \tau_t))$$

The individual chooses $\{z_t, y_t\}_{t=1}^T$ to maximize expected discounted utility:

$$\max_{\{z_t, y_t\}_{t=1}^T} \sum_{t=1}^{T} \delta^{t-1} \left[(1 - p_t)U(z_t - \tau_t y_t, z_t, S(y_t, \tau_t)) + p_t U(z_t - \tau_t y_t - f\tau_t(z_t - y_t), z_t, S(y_t, \tau_t))\right]$$

How does the maximization problem differ for individuals who have information or not about the path of tax reforms? Basically, in the informed case, agents optimize using full knowledge of the tax trajectory: $\{\tau_s\}_{s=t}^{T}$ at $t = 1$. In the uninformed case, agents form expectations $\mathbb{E}[\tau_s]$ and solve under uncertainty. For simplicity, given that there is no reason for them to expect a tax change, particularly in the first period, let us assume that $\mathbb{E}[\tau_s] = \tau$. Informed individuals may smooth compliance or strategically front-load reporting, depending on the path of τ_s. For uninformed individuals, the intertemporal maximization problem would essentially be equivalent to the one-period problem.

In each period, a higher tax rate has two opposing effects. It increases the gain from evasion (since evading saves τ per unit of undeclared income), which would decrease y. It also increases the fine if caught, which increases the marginal disutility from being audited and increases y. Which effect dominates will depend on the level of risk aversion. If it is low, people tend to evade more; however, if it is high, they tend to evade less. This aligns with empirical results: evasion often increases with tax rate, especially in laboratory settings with modest audit penalties.

How would decisions at $t = 1$ differ for those with information about the future path of tax rates? If individuals are informed at $t = 1$ that future rates will be higher $\mathbb{E}[\tau_1] < \mathbb{E}[\tau_2] < \mathbb{E}[\tau_3] < \mathbb{E}[\tau_4]$, then, an increase in future tax rates makes evasion in the future more costly (via bigger fines if audited). Hence, future compliance becomes more attractive. But since compliance has diminishing returns, agents may shift some compliance to earlier periods. Thus, an increase in future tax rates may increase compliance today:

$$\frac{\partial y_1^{\text{informed}}}{\partial \tau_2}, \frac{\partial y_1^{\text{informed}}}{\partial \tau_3}, \frac{\partial y_1^{\text{informed}}}{\partial \tau_4} > 0$$

Still, if the agent knows that future tax rates will go up, and therefore evading in the future will be riskier, then evading today becomes relatively more attractive. So, in equilibrium, the agent prefers to evade more early and report more later.

As such, the resulting hypotheses are:

[H1] In period 1, there should be no statistically different behavior among those with no information, regardless of the future path of reforms:

$$y_1^{\text{Abrupt, No Info}} \simeq y_1^{\text{Staggered, No Info}}$$

[H2] For the uninformed, assuming that in each period they re-optimize myopically under the belief that no further change in taxes is coming, tax compliance should decrease as tax rates increase (unless they are very risk-averse):

$$y_1^{\text{No Info}} \geq y_2^{\text{No Info}} \geq y_3^{\text{No Info}} \geq y_4^{\text{No Info}}$$

[H3] For the informed, evading in the future will be riskier, therefore:

$$y_1^{\text{Info}} \leq y_2^{\text{Info}} \leq y_3^{\text{Info}} \leq y_4^{\text{Info}}$$

[H4] In period 1, those who know that tax rates will increase should present lower tax compliance, **only** if they maximize intertemporally:

$$y_1^{\text{Abrupt, Info}} \leq y_1^{\text{Abrupt, No Info}}$$

$$y_1^{\text{Staggered, Info}} \leq y_1^{\text{Staggered, No Info}}$$

In future periods,

$$y_{2,3,4}^{\text{Info}} \geq y_{2,3,4}^{\text{No Info}}$$

[H5] While the marginal cost of exerting effort remains constant when the tax rate changes, the marginal benefit of labor decreases proportionally as the tax rate increases.[2]

In the model, individuals have an intrinsic motivation to pay taxes that depends on the voluntarily reported income they receive. The intertemporal dimension and the information about tax changes bring to light another potential mechanism: the appropriateness of a potential tax rate increase $S(y, \tau)$. When the tax rate increases from τ to $\tau + \Delta\tau$, we define a parameter of dependency θ, so the individual perceived tax rate is $\tau + \theta\Delta\tau$, and the intrinsic motivation function becomes $S(y, \tau + \theta\Delta\tau)$.[3]

[H6] The information about future tax changes can affect the motivation to pay taxes. Shocks may decrease the motivation to pay taxes. Therefore,

$$y_4^{\text{Abrupt, Info}} \geq y_4^{\text{Abrupt, No Info}}$$

$$y_4^{\text{Staggered, Info}} \geq y_4^{\text{Staggered, No Info}}$$

[2] However, as we will describe, the model and our empirical approximation differ in one important respect. The theoretical model assumes that there is an opportunity cost of effort, hence, a disutility. In the lab, participants had no opportunity cost; hence, it is expected that they would exert effort no matter the rate, as that behavior maximizes their expected income.

[3] If the perception of the tax rate does not depend on the previous period's rate, the parameter θ should be equal to one. The intrinsic motivation should not be affected by the history of the tax reform, and the reported income in equilibrium should be the same in both tax schemes when the tax rate increases staggeringly or abruptly. It should also be independent of the individual's knowledge of future tax rate changes.

3 Experimental Design

To identify better ways of designing political reforms, we propose a tax evasion experiment with different tax rate increments. The experiment was conducted using oTree (Chen et al., 2016) involving 410 college students from various universities in Bogotá, Colombia. Participants were recruited through the Rosario Experimental and Behavioral Economics Lab (REβEL) between November 4, 2022, and March 2, 2023.[4]

3.1 Real-Effort Task (RET)

In the first part of the experiment, participants engaged in an encryption task as described by Erkal et al. (2011), which lasted for 5 minutes. Each participant was given a unique encryption table that assigned a random numerical value to each letter of the alphabet, with the letters arranged in a randomized sequence to ensure non-sequential and unpredictable assignments. The main task was to encrypt a 3-letter word using the provided table, substituting each letter with its corresponding number. After each word was correctly encoded, the encryption table changed, and a new word was presented. Each correctly encrypted word earned the participant one experimental point (EP).[5] Once the 5 minutes ended, participants learned about their performance and earnings. Overall, this task allowed participants to generate a non-taxed income and also allowed us to measure their baseline ability for this encryption task.

3.2 Tax Evasion Game (TEG)

In the second part of the experiment, participants engage in a 40-period tax evasion game (Alm and Malézieux, 2021) divided into four (4) blocks of 10 periods each. Each block begins with a 5-minute real-effort task (RET) that allows participants to generate an income, and once they have their generated income z_t, the tax evasion game (TEG) begins. The structure of each real-effort task implemented in the TEG follows the one presented in Section 3.1 with the same word length and conversion fee per word correct.

In each period of the TEG, we ask the participants to report their generated income z_t, and a tax is calculated based on the reported income y_t. It is worth noting that individuals are permitted to underreport their income, as the instructions do not stipulate that reporting must be accurate. Moreover, participants are informed that there is auditing and a penalty that all audited individuals must pay if they are caught under-reporting, which suggests that under-reporting is an option.

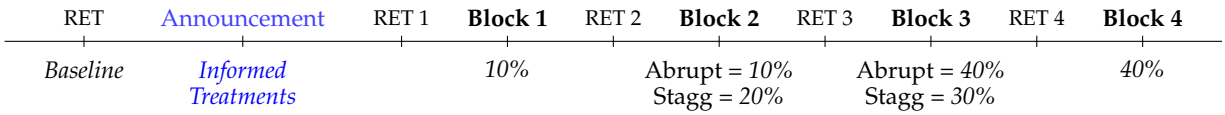
[4] The experimental protocol was approved by the Institutional Review Board (IRB) at Universidad del Rosario CS - 422, ensuring all ethical standards were adhered to.

[5] Each experimental point was valued at a fee of $0.05 USD.

After registering their reported income, each participant pressed a button to generate a random number, which determined whether they would be audited for the current period. Participants were aware that the audit probability was 20% and that it remained constant across periods.[6] Once they pressed the button, the system informed them if they were audited, and if audited, the computer verified the generated and reported incomes. If there were discrepancies, the computer imposed a penalty equal to four times the amount of the evaded tax. The penalty is calculated as: $\text{Penalty}_t = 4 \times \tau_t(\text{Generated income}_t - \text{Reported income}_t)$ with τ being the tax rate for the period t and with a constant penalty level of 4 across periods and treatments.

Finally, all taxes collected at the participant level were donated to a non-political, non-governmental organization selected by each participant at the end of the activity. The sequence of events displayed during the TEG is depicted in Figure 1.

Figure 1 - Timeline of the experiment



Note: Experimental timeline from left to right. (RET) = Real Effort Task (Erkal et al., 2011). The announcement reflects our information treatment, where treated participants receive information on the tax reform design. Finally, before each real effort task, participants were informed about the new tax rate for the respective block. Under each tick is depicted the income tax rate for each treatment.

3.3 Treatment Arms

We implement a 2 × 2 between-subjects design to identify the effect of the pace of the reform, and the effects of information about the pace of the reform. We introduce two tax rate reform trajectories: one with gradual increments (**stagger design**) and one with a single substantial hike (**abrupt design**) in the tax rate imposed over the reported income. Table 1 presents in detail the trajectories of the tax rate per design and block.

Table 1 Designs of the **Pace** of the Reform

	Tax rate (%)			
	Block 1	**Block 2**	**Block 3**	**Block 4**
Abrupt design	10%	10 %	40%	40%
Stagger design	10%	20 %	30%	40%

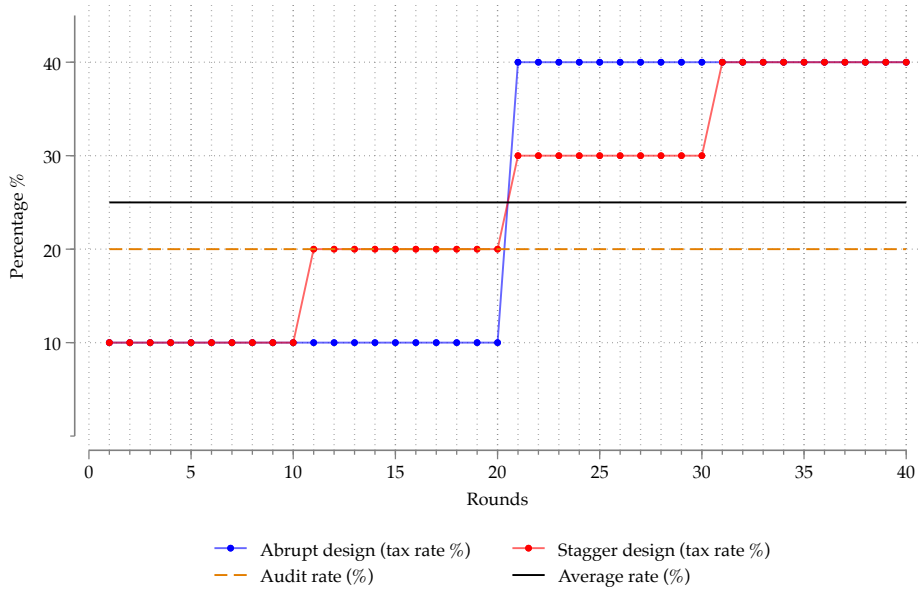
Note: Each cell describes the tax rate for each treatment

Regarding the effect of information, we introduce two different settings. The first one is the **information** design, where participants were informed about the entire trajectory of the tax rate, knowing the pace and depth of the reform since period 1. The second one is the **no information** design, where participants were unaware of the trajectory of the tax rate. Moreover, only informed participants were aware of changes in future tax rates, as we did not mention the

[6] Keeping tax evasion constant reduces the effect of institutional uncertainty (Kamm et al., 2021).

initial tax rate of 10% at any point.

Figure 2 Tax Reform Design



Note: In blue, is depicted the income tax rate for the abrupt treatments. In red, the income tax rate for the staggered treatments is illustrated. In black, is depicted the average income tax rate across the entire experiment, regardless of the treatment status (Average tax rate = 25%). In orange (dashed) is depicted the audit rate (Audit rate = 20%).

Participants were divided into four groups based on the **pace** of the tax rate change and the **information** they received about that pace. Table 2 presents the structure of our 2×2 between-subjects design. Of our 410-subject sample, 205 received information about the trajectory of the tax rate, while the other 205 were not informed about the structure of the reform. Additionally, 206 subjects participated in the abrupt design, while 204 participated in the staggered design.

Treatment status was assigned at the session level, so all participants within the same session received the same treatment. On average, each of the 16 sessions had 25 participants. Finally, at the end of the TEG activity, participants completed a socio-demographic questionnaire and provided the necessary information for payment.

Table 2 Treatments 2 2 between Subjects Design

	Information	
Pace	No information	Information
Abrupt	No information - Abrupt (C) (n_c =**101**)	Information - Abrupt (T_1) (n_1 =**105**)
Stagger	No information - Stagger (T_2) (n_2 =**104**)	Information - Stagger (T_3) (n_3 =**100**)

Note: Total sample = 410 subjects. In parentheses, the abbreviation of the treatment. In (**bold**), the number of participants assigned to the treatment.

3.4 Payment

Participants' payments comprise three elements: a participation fee, the income generated in the non-taxed encryption task, and the TEG activity payoff. The participation fee was a fixed amount of approximately $2.50 USD. Additionally, participants generated a non-taxed income by completing the first real effort task (henceforth *Baseline* task) and on average, participants encrypted 22 words, which represents approximately $1.1 USD. Finally, for the TEG activity, participants were aware that only 8 periods would be paid out. At the end of the activity, the computer selected randomly two periods per block, and the final payoff obtained during those periods would be defined as the TEG activity payoff. On average, participants received a final payoff of $12 USD[7], and all payments were made at the end of the session.

4 Results

We focus on two dimensions of taxpayer behavior: the effort to generate income that could be reported, and the actual compliance decision. We begin by examining the effect of the tax rate increase and our treatments on effort. We then turn to compliance. First, we examine the overall effect of the tax rate increase on compliance and analyze the effects of the **information** and **pace** of the reform by comparing results across treatment arms to understand how the compliance decline associated with higher tax rates can be mitigated through the design of the reform.

We begin by examining the effect of the tax rate and the treatments on participants' effort to generate income that could be declared. As shown in Table 3 and Table 4, we do not find statistically significant changes in effort levels as the tax rate increases. This result is likely due to features of our experimental setting: participants had the possibility to evade taxes, and the audit rate was fixed at 20%, making it more likely that individuals adjusted their reporting behavior rather than their effort. Additionally, because time in the laboratory does not carry an explicit opportunity cost (participants did not have an alternative activity to engage in), there was little incentive for participants to reduce their effort in response to higher taxes. As shown in Figure 3, there is also evidence of a learning curve in the encryption task, which may further attenuate any potential impact of tax rate changes on effort.

[7] Given that the minimum wage in Colombia is $ 1.42 USD/hour, the earnings obtained during the experiment are about 8.4 times the minimum wage.

Table 3 Effect of Tax Rate on Effort on the RET

Dep Var: **N° of encrypted words**	*Real Effort Task*				
	General Sample	Abrupt		Staggered	
		No information	Information	No information	Information
First task (ϵ_1)	1.449***	1.426***	1.229***	1.760***	1.380***
	(0.162)	(0.192)	(0.252)	(0.519)	(0.223)
Second task (ϵ_2)	2.301***	2.248***	2.105***	2.837***	2.042***
	(0.154)	(0.200)	(0.276)	(0.457)	(0.232)
Third task (ϵ_3)	2.557***	2.208***	2.324***	3.279***	2.457***
	(0.180)	(0.208)	(0.429)	(0.460)	(0.271)
Fourth task (ϵ_4)	2.611***	2.406***	2.448***	3.077***	2.561***
	(0.180)	(0.222)	(0.390)	(0.501)	(0.245)
Mean Dep. Variable	21.97	22.27	22.58	21.17	21.84
Controls	✓	✓	✓	✓	✓
Clusters	410	101	105	104	100
Observations	2041	505	525	520	491
Adjusted R-squared	0.05	0.07	0.10	0.12	0.08
p-value $H_0 : \epsilon_1 = \epsilon_2$	0.000	0.000	0.000	0.000	0.002
p-value $H_0 : \epsilon_1 = \epsilon_3$	0.000	0.000	0.007	0.000	0.000
p-value $H_0 : \epsilon_1 = \epsilon_4$	0.000	0.000	0.000	0.000	0.000
p-value $H_0 : \epsilon_2 = \epsilon_3$	0.041	0.827	0.537	0.023	0.073
p-value $H_0 : \epsilon_2 = \epsilon_4$	0.014	0.390	0.288	0.344	0.022
p-value $H_0 : \epsilon_3 = \epsilon_4$	0.705	0.276	0.775	0.427	0.592

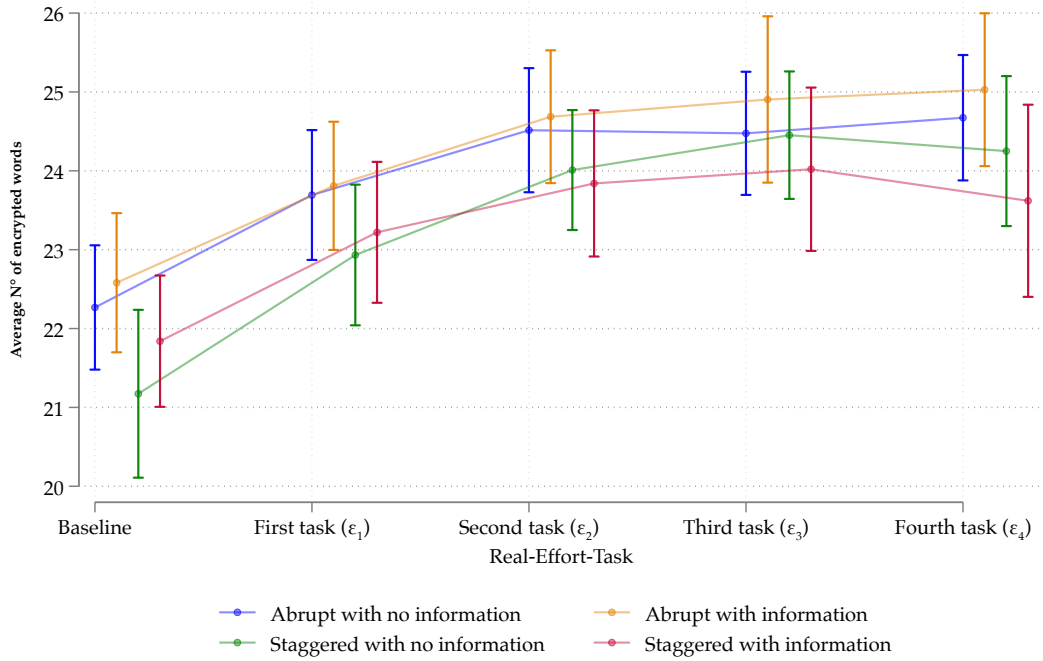
Notes: The baseline category is the *Baseline task*. The first through fourth tasks correspond to RET 1 through RET 4, denoted by (ϵ_1) to (ϵ_4), respectively. In the **Abrupt** treatment, tasks ϵ_1 and ϵ_2 were conducted under a tax rate of 10%, while ϵ_3 and ϵ_4 were conducted under 40%. In the **Staggered** treatment, ϵ_2, ϵ_3, and ϵ_4 were conducted under tax rates of 20%, 30%, and 40%, respectively. All specifications control for gender, age, socioeconomic stratum, risk aversion, weekly expenses, and political preferences. Standard clustered errors at the individual level are presented in parentheses. $^{***}p < 0.01$,** $p < 0.05$,* $p < 0.1$

Table 4 Treatment Effect on Effort by RET

Dep Var: **N° of encrypted words**	*Real Effort Task*				
	(Baseline)	(Block 1)	(Block 2)	(Block 3)	(Block 4)
Abrupt with information (T_1)	0.380	-0.044	-0.026	0.210	0.115
	(0.597)	(0.322)	(0.335)	(0.465)	(0.443)
Staggered with no information (T_2)	-1.110*	-0.014	0.251	0.748*	0.358
	(0.673)	(0.438)	(0.373)	(0.386)	(0.454)
Staggered with information (T_3)	-0.342	-0.168	-0.344	0.105	0.008
	(0.592)	(0.329)	(0.333)	(0.353)	(0.347)
Mean Dep. Variable	22.27	23.69	24.51	24.48	24.67
Controls	✓	✓	✓	✓	✓
Baseline performance control		✓	✓	✓	✓
Observations	410	410	409	406	406
Adjusted R-squared	0.03	0.54	0.58	0.49	0.49
p-value H_0: $T_1 = T_2$	0.034	0.948	0.495	0.295	0.661
p-value H_0: $T_1 = T_3$	0.238	0.725	0.400	0.833	0.815
p-value H_0: $T_2 = T_3$	0.270	0.743	0.157	0.151	0.469

Note: All specifications control for gender, age, socioeconomic stratum, risk aversion, weekly expenses, and political preferences. The *Baseline performance control* denotes the inclusion of the baseline performance as control for the estimation. The *Mean Dep. Variable* expresses the unconditional mean of the dependent variable for the control group: *Abrupt with no information*. Standard errors clustered at the individual level are presented in parentheses. $^{***}p < 0.01$,** $p < 0.05$,* $p < 0.1$

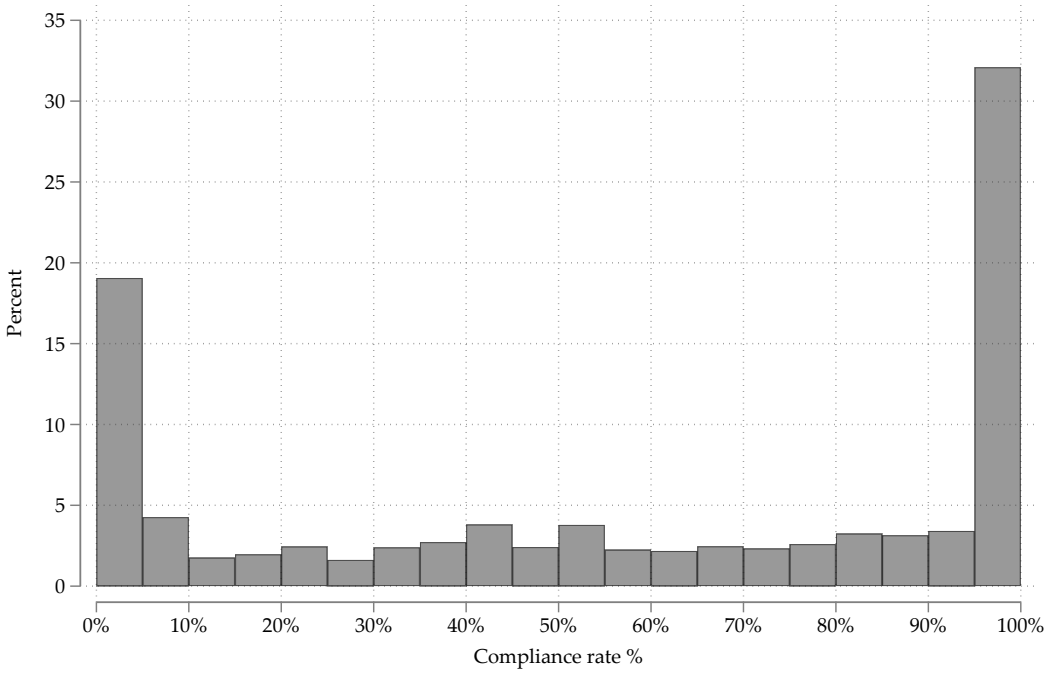
Figure 3 Performance in RET by Tasks (ϵ)



Notes: **Baseline** denotes the first real effort task. The first task (ϵ_1) corresponds to the real effort task under a tax rate of 10%. The second task (ϵ_2) corresponds to: for *Staggered* treatments, a tax rate of 20%; and for *Abrupt* treatments, a continued rate of 10%. The third task (ϵ_3) corresponds to: for *Staggered* treatments, a tax rate of 30%; and for *Abrupt* treatments, a jump to 40%. The fourth task (ϵ_4) corresponds to: for *Staggered* treatments, a tax rate of 40%; and for *Abrupt* treatments, a second task at 40%. 95% confidence intervals are shown.

We start by analyzing compliance behavior during the Tax Evasion Game (TEG), pooling participants across periods and treatments. Broadly speaking, our experiment shows that, as expected, compliance decreases as the tax rate increases. Overall, participants declared their total income (full compliance) 30% of the time (SD = 0.45). On the other hand, 19% of the time, they declared no income at all, which we refer to as full evasion. The average compliance rate is 57% (SD = 0.39). The distribution of the compliance rate is presented in Figure 4. This distribution shows an interesting pattern, where full compliance and full evasion coexist for the same experimental parameters, which is consistent with real-world data on compliance (Carrillo et al., 2021; Castro and Scartascini, 2015). This highlights the significant role of behavioral responses and biases, including loss aversion, overconfidence, and tax morale.

Figure 4 (*Full Sample*) Histogram of Compliance



Note: Compliance rate (%) denotes the fraction of endowment declared by the participant. Compliance rate of 0% denotes **total evasion**, while 100% denotes **full compliance**. The histogram contains all decisions made by participants across all rounds. Full compliance of our sample = 33% (Full compliance in other settings = 45% (Alm and Malézieux, 2021)). Full evasion of our sample = 18% (Full evasion in other settings = 19% (Alm and Malézieux, 2021))

Table 5 presents the average compliance rate across all periods by treatment. The first column shows results for the full sample, while the remaining columns display results by treatment arm. Across specifications, higher tax rates are associated with lower compliance rates.

When the sample is pooled across treatments, the mean compliance rate under the 10% tax rate is 61.8%. Relative to this baseline, increasing the tax rate to 20% and 30% leads to statistically significant reductions in compliance of 4.3 and 4.5 percentage points, respectively. When the tax rate increases to 40%, the decline becomes more pronounced, with a reduction of 8.8 percentage points.

Although a decline in compliance is expected as the tax rate increases-—due to stronger incentives to evade-—how the increase is implemented can mitigate this effect. The smallest reduction in compliance is observed in the abrupt treatment with information, where compliance falls by 7.3 percentage points at the 40% tax rate. In contrast, the staggered treatment without information results in the largest declines in compliance (13 percentage points).

To better understand taxpayer responses to tax rate changes, we examine compliance behavior along the extensive and intensive margins. The extensive margin captures the binary decision of whether to report any income at all, while the intensive margin reflects the continuous choice of how much income to report, conditional on reporting something. These two dimensions of compliance represent different types of taxpayer behavior and may respond differently to

Table 5 Effect of Tax Rate on **Compliance** Rate

Dep Var: **Compliance rate**	General sample	Staggered		Abrupt	
		No information	Information	No information	Information
	(1)	(2)	(3)	(4)	(5)
20% tax rate	-0.043**	-0.068***	-0.066***		
	(0.020)	(0.019)	(0.016)		
30% tax rate	-0.045**	-0.088***	-0.048**		
	(0.021)	(0.025)	(0.020)		
40% tax rate	-0.088***	-0.130***	-0.076***	-0.088***	-0.073***
	(0.012)	(0.029)	(0.024)	(0.024)	(0.020)
Mean compliance with 10% tax rate	0.618	0.649	0.630	0.581	0.632
Controls	✓	✓	✓	✓	✓
Clusters	410	104	100	101	105
Observations	15901	4037	3850	3939	4075
Adjusted R-squared	0.04	0.06	0.08	0.03	0.05
p-value H_0 : 20% tax rate = 30% tax rate	0.871	0.241	0.341		
p-value H_0 : 20% tax rate = 40% tax rate	0.027	0.006	0.663		
p-value H_0 : 30% tax rate = 40% tax rate	0.030	0.025	0.077		

Notes: *Baseline (abrupt - no information):* The tax rate is 10% for the first two blocks (periods 1-20) and 40% for the remaining two blocks (periods 21-40). *Treatment 1 (abrupt - information):* The change in the tax rate is the same as the Baseline, but before the first block, the subjects are informed about the tax rate for each period. *Treatment 2 (incremental - no information):* The tax rate is 10% for the first block (periods 1-10), 10% for the second block (periods 11-20), 30% for the third block (periods 21-30), and 40% for the remaining block (periods 31-40). *Treatment 3 (Incremental - Information):* The change in the tax rate is the same as Treatment 2, but before the first block, the subjects are informed about the tax rate for each period. We include controls for female, age, stratum, risk aversion, weekly expenses, and political preferences. Robust standard errors in parentheses.

changes in tax rates and the design of tax reforms. To capture these distinct decision-making processes, we employ two complementary measures: the probability of full evasion (reporting zero income), which reflects behavior at the extensive margin, and the compliance rate (the fraction of actual income reported), which captures the intensive margin of tax compliance. This distinction enables us to determine whether our treatments primarily affect the decision to declare at all or the degree of honesty among those who choose to report some income.

Providing information about the reform trajectory significantly reduces the probability of full evasion, with effects that are most pronounced when tax rates are high. Table 6 and Figure 5 present the effects of our treatment arms using the abrupt with no information treatment as a comparison on the likelihood of full evasion, disaggregated by tax rate. Columns (1)–(4) report estimates for a 10% tax rate, while Columns (5)–(8) show results for the 40% tax rate. The outcome variable is a binary indicator equal to one if participants declared zero income.

At the 10% tax rate, treatment effects are small and not statistically significant across all specifications, as expected. The mean rate of full evasion in this setting is around 10%. At the 40% tax rate, the overall level of full evasion increases to 20%. In this higher-tax setting, participants receiving advance information about future tax rates are less likely to report no income, with statistically significant reductions in full evasion of around 7 percentage points. This information effect is consistent across both abrupt and staggered reform designs, suggesting that transparency about upcoming policy changes helps maintain taxpayer participation in the system at the extensive margin, independent of the pace at which reforms are implemented.

Table 6 Effect of Each Treatment on **Full Evasion**

Dep Var: **Full evasion**	Block							
	Tax rate = 10%				Tax rate = 40%			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Abrupt with information (T_1)	-0.051	-0.041	-0.041	-0.041	-0.078*	-0.056	-0.056	-0.071*
	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)
Staggered with no information (T_2)	-0.026	-0.026	-0.026	-0.022	-0.003	-0.001	-0.001	-0.017
	(0.03)	(0.03)	(0.03)	(0.03)	(0.05)	(0.04)	(0.04)	(0.05)
Staggered with information (T_3)	-0.031	-0.021	-0.021	-0.028	-0.072*	-0.054	-0.054	-0.076*
	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)
Mean Dep. Variable	0.10	0.10	0.10	0.11	0.20	0.20	0.20	0.20
Effect size (%)	-48.3	-39.3	-39.3	-37.4	-39.8	-28.3	-28.3	-36.2
Controls		✓	✓			✓	✓	
Round FE			✓	✓			✓	✓
LASSO Controls				✓				✓
Observations	4100	4100	4100	3690	4060	4060	4060	4060
Clusters	410	410	410	410	406	406	406	406
Adjusted R-squared	0.00	0.03	0.03		0.01	0.07	0.07	
p-value $H_0: T_1 = T_2$	0.311	0.524	0.525	0.445	0.061	0.153	0.153	0.169
p-value $H_0: T_1 = T_3$	0.466	0.427	0.428	0.646	0.871	0.974	0.974	0.881
p-value $H_0: T_2 = T_3$	0.867	0.844	0.844	0.823	0.094	0.171	0.171	0.147

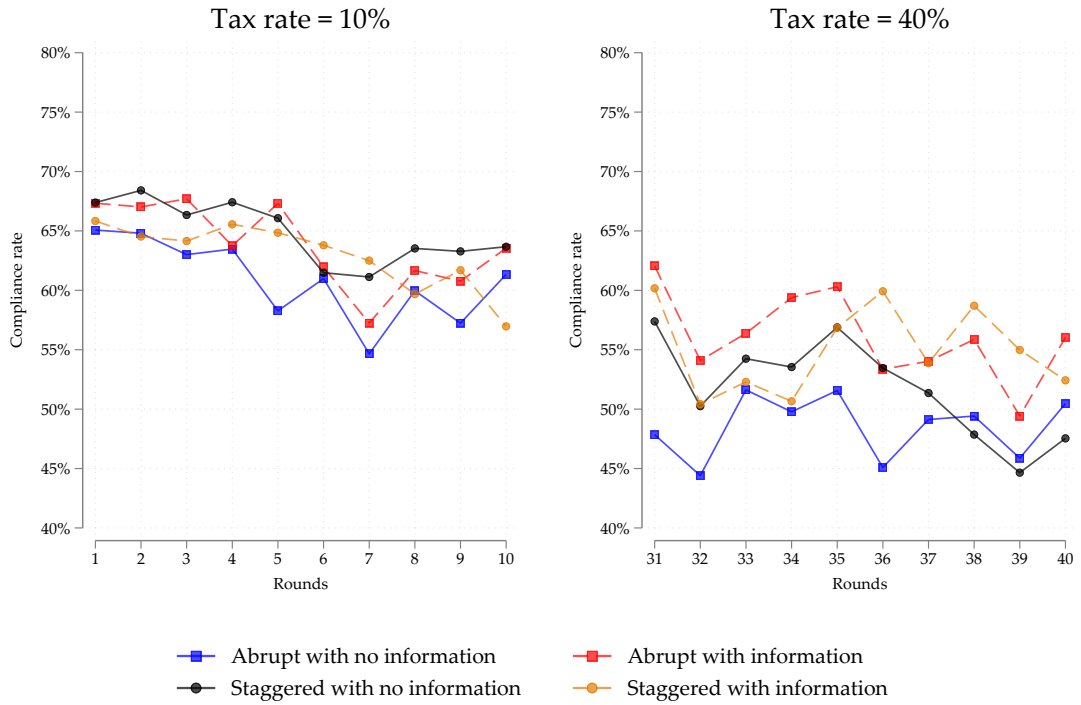
Notes: *Baseline (abrupt - no information):* The tax rate is 10% for the first two blocks (periods 1-20) and 40% for the remaining two blocks (periods 21-40). *Treatment 1 (abrupt - information):* The change in the tax rate is the same as the Baseline, but before the first block, the subjects are informed about the tax rate for each period. *Treatment 2 (incremental - no information):* The tax rate is 10% for the first block (periods 1-10), 10% for the second block (periods 11-20), 30% for the third block (periods 21-30), and 40% for the remaining block (periods 31-40). *Treatment 3 (Incremental - Information):* The change in the tax rate is the same as Treatment 2, but before the first block, the subjects are informed about the tax rate for each period. We include controls for female, age, stratum, risk aversion, weekly expenses, and political preferences. Robust standard errors in parentheses.

Figure 5 Probability of Full Evasion across Rounds by Treatment

Note: Tax collection (%) denotes the fraction of endowment collected by the national authority. In squares (■), the average compliance rate for **abrupt** treatments. In circles (●), the average compliance rate for **staggered** treatments. Dashed, **informed** treatments. Solid, **uninformed** treatments. **Left panel:** denotes choices when tax rate is 10% and **Right panel:** denotes choices when tax rate is 40%.

We repeat the same analysis for the compliance rate, which captures behavior at the intensive margin of tax compliance. Unlike the results for full evasion, we find no statistically significant effects of either the information treatment or the pace of reform on the compliance rate. As shown in Figure 6 and Table 7

Figure 6 Average Compliance Rate across Rounds by Treatment



Note: Tax collection (%) denotes the fraction of endowment collected by the national authority. In squares (■), the average compliance rate for **abrupt** treatments. In circles (●), the average compliance rate for **staggered** treatments. Dashed, **informed** treatments. Solid, **uninformed** treatments. **Left panel:** denotes choices when tax rate is 10% and **Right panel:** denotes choices when tax rate is 40%.

However, when we perform an ex-post power calculation, we find that our sample size may not be sufficiently large to detect meaningful changes in the compliance rate, which exhibits substantially higher variance compared to the binary full evasion measure.[8] Given the significant results for the probability of declaring zero income and the fact that individuals are randomly assigned to treatment arms, we pool the treatment arms across the information component to double our effective sample size.[9] Specifically, we combine the abrupt and staggered treatments within each information condition, creating two groups: those who received advance information about

[8] We compute ex-post minimum detectable effect with command `clustersampi`. We estimate intracluster correlation across participants' choices ICC $= 0.505$, and compute the minimum detectable effect with an 80% power, a statistical significance level $\alpha = 0.05$, and a standard deviation of $SD = 0.41$, which denotes the standard deviation of compliance for the control group. The minimum detectable effect is set to be MDE $= 0.12$

[9] By doubling our sample size, and assuming a standard deviation on compliance of $SD = 0.41$, the minimum detectable effect with an 80% power is set to be MDE $= 0.09$.

Table 7 Effect of Each Treatment on **Compliance** Rate

Dep Var: **Compliance rate**	Block							
	Tax rate = 10%				Tax rate = 40%			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Abrupt with information (T_1)	0.029	0.018	0.018	0.020	0.076*	0.064	0.064	0.068
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Staggered with no information (T_2)	0.040	0.038	0.038	0.035	0.032	0.027	0.027	0.026
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Staggered with information (T_3)	0.021	0.015	0.015	0.027	0.065	0.057	0.057	0.066
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Mean Dep. Variable	0.61	0.61	0.61	0.60	0.49	0.49	0.49	0.49
Effect size (%)	4.8	2.9	2.9	3.2	15.6	13.3	13.3	14.0
Controls		✓	✓			✓	✓	
Round FE			✓	✓			✓	✓
LASSO Controls				✓				✓
Observations	4090	4090	4090	3681	4040	4040	4040	4040
Clusters	409	409	409	409	404	404	404	404
Adjusted R-squared	0.00	0.03	0.03		0.01	0.05	0.05	
p-value $H_0 : T_1 = T_2$	0.773	0.574	0.575	0.674	0.288	0.336	0.337	0.299
p-value $H_0 : T_1 = T_3$	0.822	0.944	0.944	0.855	0.802	0.860	0.860	0.969
p-value $H_0 : T_2 = T_3$	0.608	0.531	0.532	0.832	0.419	0.431	0.432	0.317

Notes: *Baseline (abrupt - no information):* The tax rate is 10% for the first two blocks (periods 1-20) and 40% for the remaining two blocks (periods 21-40). *Treatment 1 (abrupt - information):* The change in the tax rate is the same as the Baseline, but before the first block, the subjects are informed about the tax rate for each period. *Treatment 2 (incremental - no information):* The tax rate is 10% for the first block (periods 1-10), 10% for the second block (periods 11-20), 30% for the third block (periods 21-30), and 40% for the remaining block (periods 31-40). *Treatment 3 (Incremental - Information):* The change in the tax rate is the same as Treatment 2, but before the first block, the subjects are informed about the tax rate for each period. We include controls for female, age, stratum, risk aversion, weekly expenses, and political preferences. Robust standard errors in parentheses.

the reform trajectory and those who did not. With this increased statistical power, we find that providing information before the reform about how the tax rate will increase significantly mitigates the decline in compliance associated with higher tax rates–both at the extensive margin, reducing the probability of declaring zero income (full evasion) by 6 percentage points (Table 8), and at the intensive margin, increasing the average compliance rate by 6 percentage points (Table 9).

Table 8 Effect of the Information Treatment on **Full Evasion**

Dep Var: **Full evasion**	Block							
	Tax rate = 10%				Tax rate = 40%			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Information treatment	-0.028	-0.018	-0.018	-0.022	-0.074**	-0.054*	-0.054*	-0.061**
	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)
Mean dep. var	0.09	0.09	0.09	0.09	0.19	0.19	0.19	0.19
Effect size (%)	-30.3	-19.5	-19.5	-22.8	-37.9	-27.9	-27.9	-31.3
Controls		✓	✓			✓	✓	
Round FE			✓	✓			✓	✓
LASSO Controls				✓				✓
Observations	4100	4100	4100	3690	4060	4060	4060	4060
Clusters	410	410	410	410	406	406	406	406
Adjusted R-squared	0.00	0.03	0.03		0.01	0.07	0.07	

Notes:

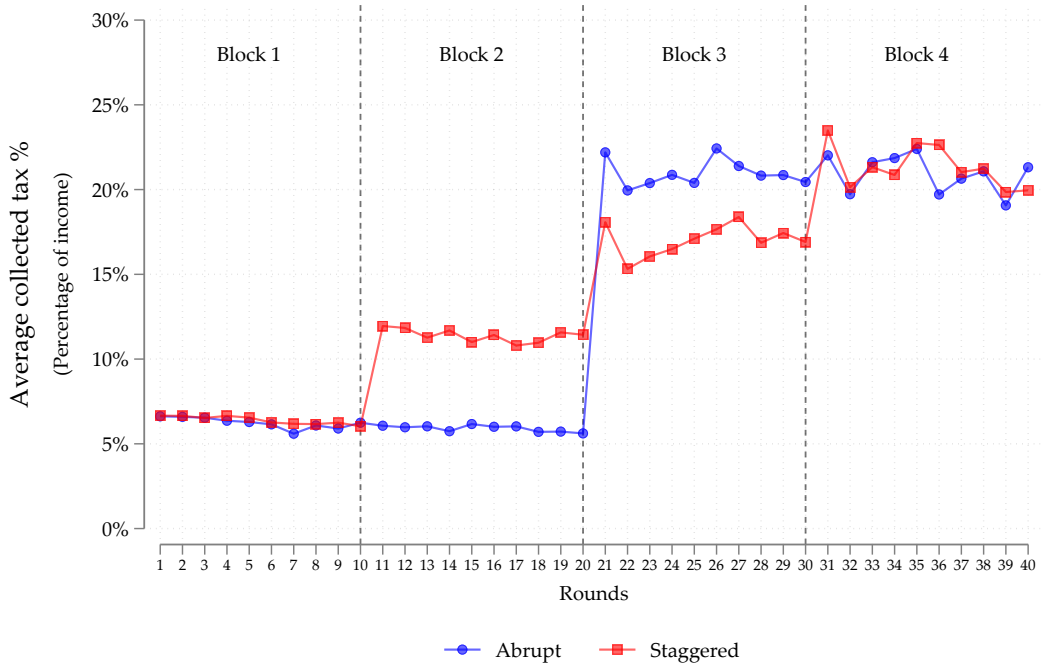
Table 9 Effect of the Information Treatment on **Compliance** Rate

Dep Var: **Compliance rate**	Block							
	Tax rate = 10%				Tax rate = 40%			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Information treatment	0.005	-0.002	-0.002	0.001	0.054*	0.049*	0.049*	0.061**
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Mean dep. var	0.63	0.63	0.63	0.63	0.50	0.50	0.50	0.50
Effect size (%)	0.8	-0.3	-0.3	0.1	10.9	9.7	9.7	12.1
Controls		✓	✓			✓	✓	
Round FE			✓	✓			✓	✓
LASSO Controls				✓				✓
Observations	4090	3681	3681	3681	4040	4040	4040	4040
Clusters	409	409	409	409	404	404	404	404
Adjusted R-squared	0.00	0.03	0.03		0.00	0.05	0.05	

Notes: *Baseline (abrupt - no information):* The tax rate is 10% for the first two blocks (periods 1-20) and 40% for the remaining two blocks (periods 21-40). *Treatment 1 (abrupt - information):* The change in the tax rate is the same as the Baseline, but before the first block, the subjects are informed about the tax rate for each period. *Treatment 2 (incremental - no information):* The tax rate is 10% for the first block (periods 1-10), 10% for the second block (periods 11-20), 30% for the third block (periods 21-30), and 40% for the remaining block (periods 31-40). *Treatment 3 (Incremental - Information):* The change in the tax rate is the same as Treatment 2, but before the first block, the subjects are informed about the tax rate for each period. We include controls for female, age, stratum, risk aversion, weekly expenses, and political preferences. Robust standard errors in parentheses.

These findings yield policy recommendations for the design and implementation of tax reforms. Our results demonstrate that the pace of reform implementation does not significantly affect compliance levels when tax rates increase. Whether tax rates are raised abruptly or gradually through a staggered approach, the ultimate compliance outcomes are statistically equivalent. However, providing advanced information about the reform trajectory before implementation has a substantial and significant impact on taxpayer behavior. Information provision reduces both the probability of complete tax evasion and mitigates the decline in compliance rates typically associated with higher tax burdens. Therefore, from a revenue maximization perspective, policymakers should prioritize implementing faster, more direct reforms while ensuring that citizens are fully informed about future tax rate increases. This approach combines the administrative efficiency of abrupt implementation with the compliance benefits of transparency, ultimately maximizing tax collection (Figure 7).

Figure 7 Average Tax Collection across Rounds by Staggered Paths



Note: Tax collection (%) denotes the fraction of endowment collected by the national authority. In red , the average compliance rate for **staggered** treatments, and in blue , the average compliance rate for **abrupt** treatments. Tax rates by blocks: **Block 1:** (10% - all treatments). **Block 2:** (Staggered, 20%)(Abrupt, 10%). **Block 3:** (Staggered, 30%)(Abrupt, 40%). **Block 4:** (40% - all treatments).

5 Conclusion

We explored how variations in the pace of a tax rate increase, as well as the availability of information on the path of reforms, influence individual behavior in a controlled experimental setting. Our results reveal that the pace of reform, whether abrupt or gradual, has a negligible influence on labor effort or tax compliance. This suggests that once reforms are approved and implemented, the manner in which they are introduced over time might not play a pivotal role in shaping individual behaviors. However, the provision of prior information regarding the path of such reforms serves as a mitigating factor against decreased compliance.

In light of these findings, policymakers could infer that if public resistance against reforms is primarily manifested through decreased tax compliance and effort, then abrupt reforms may have a strategic edge because they yield higher overall tax collections, due to imposition of higher tax rates sooner. Moreover, informing the public in advance about impending reforms could be a key strategy to buffer against potential negative repercussions for compliance.

In broader terms, our study provides a deeper understanding of the dynamics at play when fiscal reforms are introduced, reaffirming the importance of both intrinsic motivation to comply with tax mandates and the role of clear communication from policymakers. It also highlights

the need to continually refine our understanding of fiscal reform implementation strategies.

This paper serves as a stepping stone to test even more complex reforms and environments. First, our laboratory experiment abstracts from the reform process itself. Providing information about the reforms may increase compliance once they are introduced, but it can also provide fertile ground for more active collective action to begin with. In future research, we aim to introduce another possible action: protesting the reform itself and allowing for the possibility of partial or full reform reversal.

Second, in addition to protests, large tax reforms may generate effects on enforcement (Kanbur and Ronconi, 2018). If tax agents, who are taxpayers themselves, believe that the reform is unjust, they may adjust their effort to collect taxes, making the probability of detection endogenous to the tax rate. Future work could randomize the detection and penalty parameters while changing tax rates.

Third, in our laboratory setting, individuals have a very low opportunity cost of effort. As such, if they were offered a worthy alternative to collecting money, they might have taken it when the tax rate increased. Fourth, while we have evaluated the role of the pace of change, due to a lack of power, we did not randomize the depth. That is, instead of one large or several equivalent jumps, future work could work with different types of jumps and adjustments. Moreover, randomizing the different step sizes would reduce the potential for learning and increase uncertainty, which may help test similar hypotheses to those evaluated in the literature on monetary and exchange rate policy in the context of tax compliance.

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Online Appendix

Table 1 Randomization Testing Effect of Information on **Compliance**

Dep Var: **Compliance rate**	Block	
	Tax rate = 10%	Tax rate = 40%
Information treatment	0.005	0.054*
	(0.028)	(0.030)
Constant	0.629***	0.501***
	(0.020)	(0.021)
Randomization inference **p-value**	0.87	0.07
N° of samples **higher** than observed	867	65
N° of samples (reps)	1000	1000

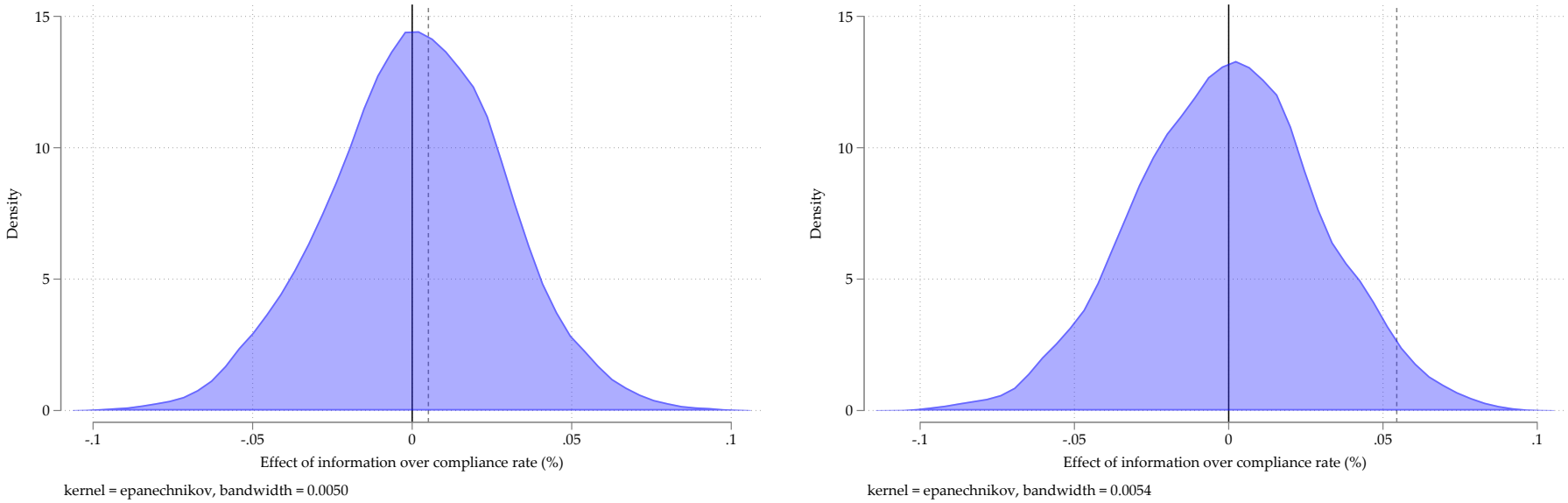
Notes: We employed randomization testing as a robustness check analysis. We resample the treatment assignment to compute 3,000 possible *t distributions* and then compare the observed effect with the simulated effect. We employed the STATA command *ritest* to compute the Monte Carlo simulations. We do not include controls for our estimation.

Table 2 Starting and Ending Effect: Robustness Check (**Rounds 2 - 38**)

Dep Var: **Compliance rate**	Block							
	Tax rate = 10%				Tax rate = 40%			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Information treatment	0.007	-0.001	-0.001	0.003	0.053*	0.048*	0.048*	0.056*
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Mean dep. var	0.62	0.62	0.62	0.62	0.51	0.51	0.51	0.51
Effect size (%)	1.1	-0.1	-0.1	0.4	10.4	9.5	9.5	11.0
Controls		✓	✓			✓	✓	
Round FE			✓	✓			✓	✓
LASSO Controls				✓				✓
Observations	3272	3272	3272	3272	3232	3232	3232	3232
Clusters	409	409	409	409	404	404	404	404
Adjusted R-squared	0.00	0.03	0.03		0.00	0.05	0.05	

Notes: *Baseline (abrupt - no information):* The tax rate is 10% for the first two blocks (periods 1-20) and 40% for the remaining two blocks (periods 21-40). *Treatment 1 (abrupt - information):* The change in the tax rate is the same as the Baseline, but before the first block, the subjects are informed about the tax rate for each period. *Treatment 2 (incremental - no information):* The tax rate is 10% for the first block (periods 1-10), 10% for the second block (periods 11-20), 30% for the third block (periods 21-30), and 40% for the remaining block (periods 31-40). *Treatment 3 (Incremental - Information):* The change in the tax rate is the same as Treatment 2, but before the first block, the subjects are informed about the tax rate for each period. We include controls for female, age, stratum, risk aversion, weekly expenses, and political preferences. Robust standard errors in parentheses.

Figure 1 Randomization Inference Analysis

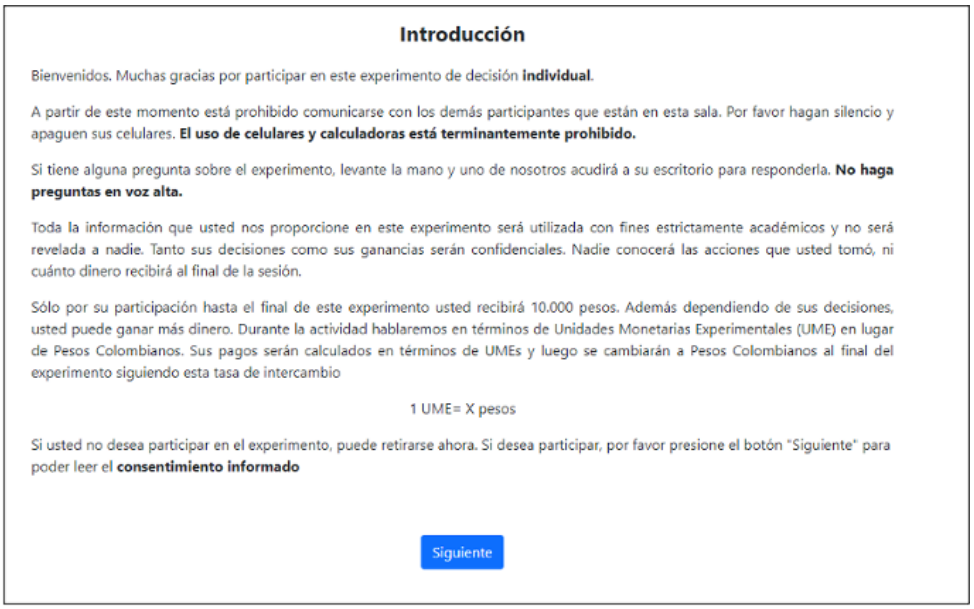


(a) Treatment Effect with Tax Rate of **10%**

(b) Treatment Effect with Tax Rate of **40%**

Note: All estimations are based on data from the round *before shock*. The *x-axis* denotes the average of the receivers endowment ($\overline{E^r}$), the *y-axis* denotes the expected sent amount by other senders ($\sum_{m\neq i}^{S} M_m^s$), and the *z-axis* denotes the amount sent by the altruistic sender (M_i^s). **Left Figure ??:** has an expected returned amount of $\overline{\gamma^r} = 0.45$. **Right Figure ??:** has an expected returned amount of $\overline{\gamma^r} = 0.55$. In violet = low levels of sent amount; in yellow = high levels of sent amount. The following parameters were fixed at: $R = 2$, $S = 2$, $\alpha = 1$, $\kappa = 4$. Finally, $\overline{\gamma^c} = 0.5$

Figure 2 Screen – Introduction



Note: The experiment was conducted using oTree (Chen et al., 2016) involving 410 college students from various universities in Bogotá, Colombia. Participants were recruited through the Rosario Experimental and Behavioral Economics Lab (REβEL) between November 4, 2022, and March 2, 2023. The instructions are in Spanish.

Figure 3 Screen – Informed Consent

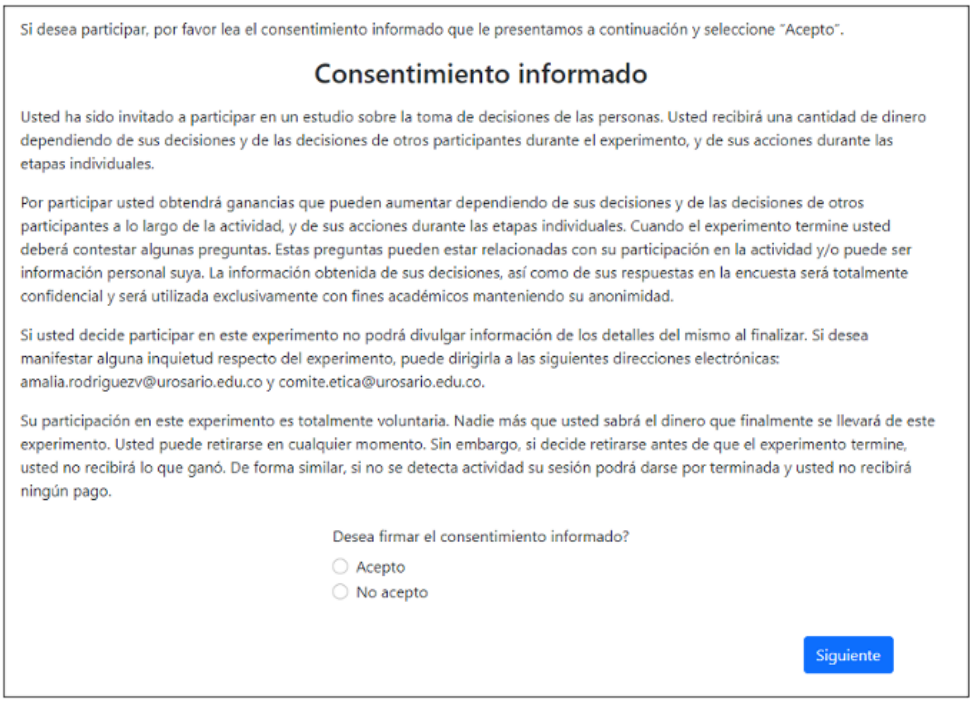


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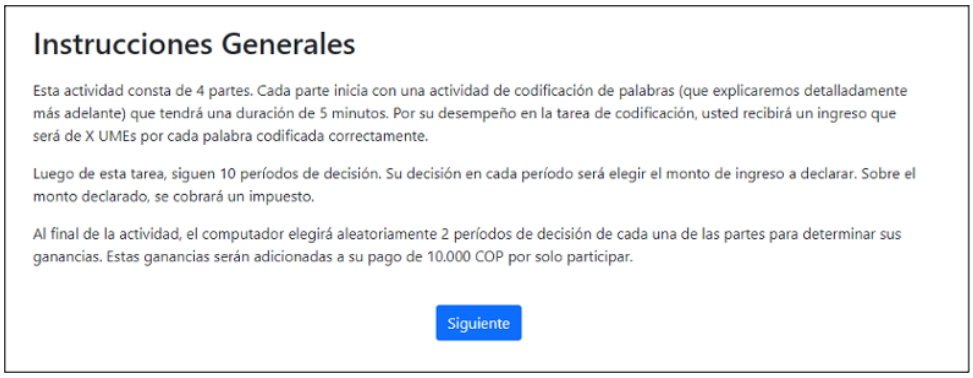
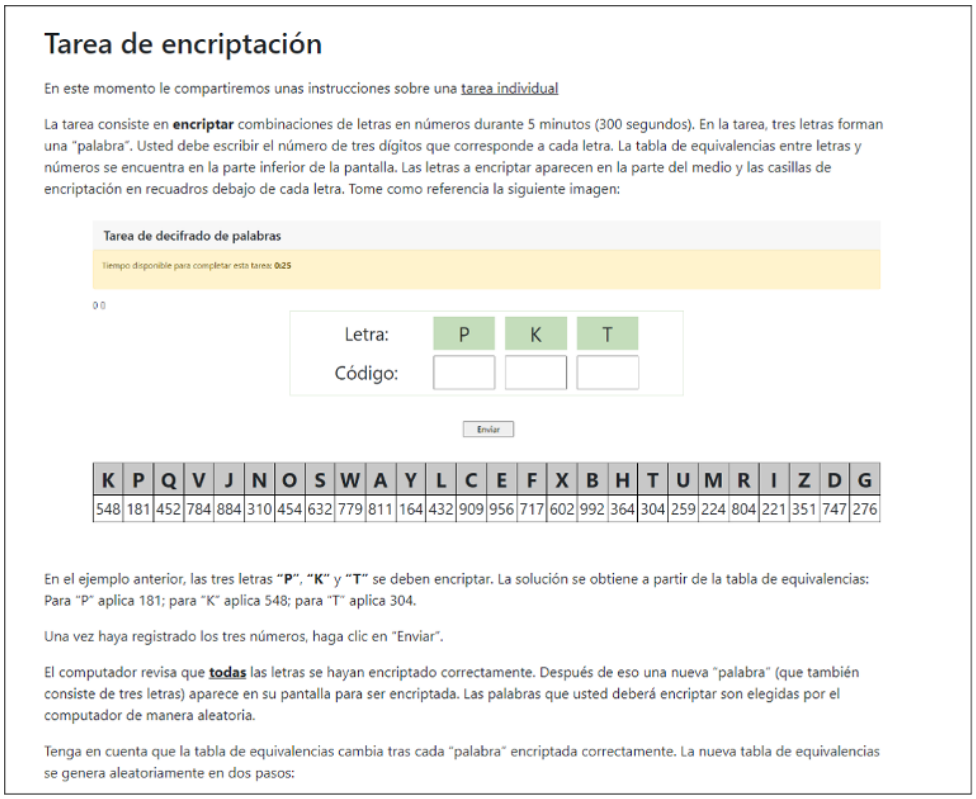
Figure 4 Screen – General Instructions



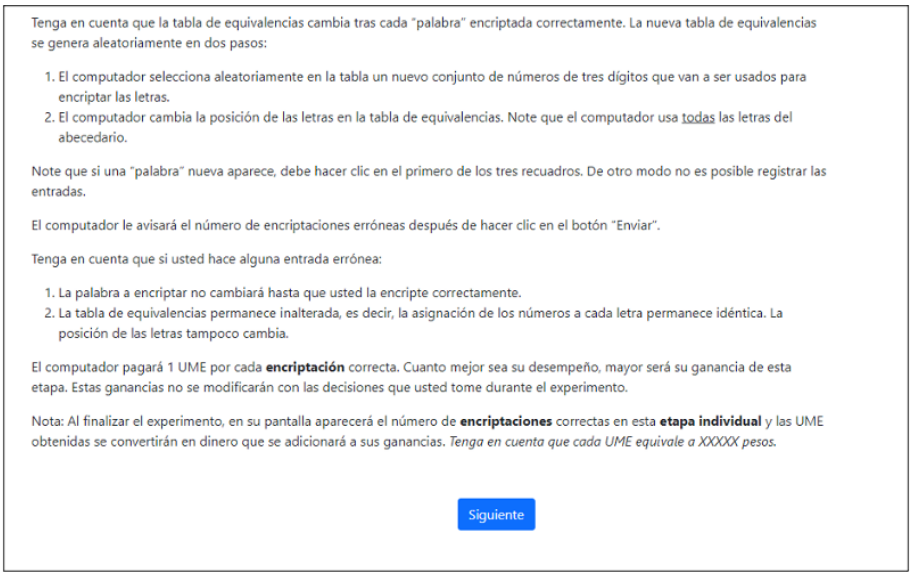
Note: The experiment was conducted using oTree (Chen et al., 2016) involving 410 college students from various universities in Bogotá, Colombia. Participants were recruited through the Rosario Experimental and Behavioral Economics Lab (REβEL) between November 4, 2022, and March 2, 2023. The instructions are in Spanish.

Figure 5 Screen – Encryption Task Instructions I



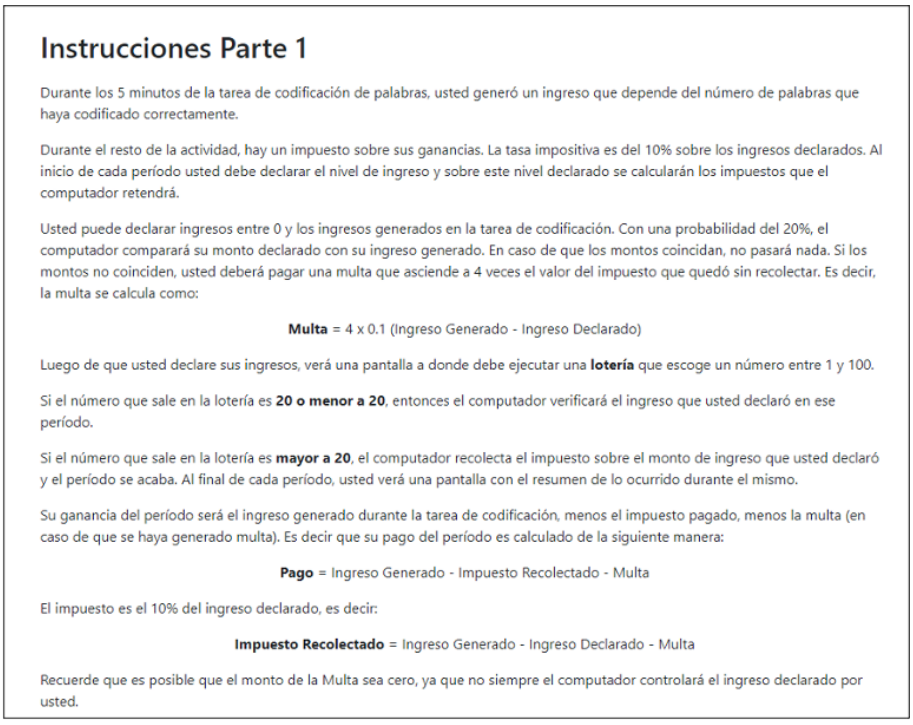
Note: The experiment was conducted using oTree (Chen et al., 2016) involving 410 college students from various universities in Bogotá, Colombia. Participants were recruited through the Rosario Experimental and Behavioral Economics Lab (REβEL) between November 4, 2022, and March 2, 2023. The instructions are in Spanish.

Figure 6 Screen – Encryption Task Instructions II



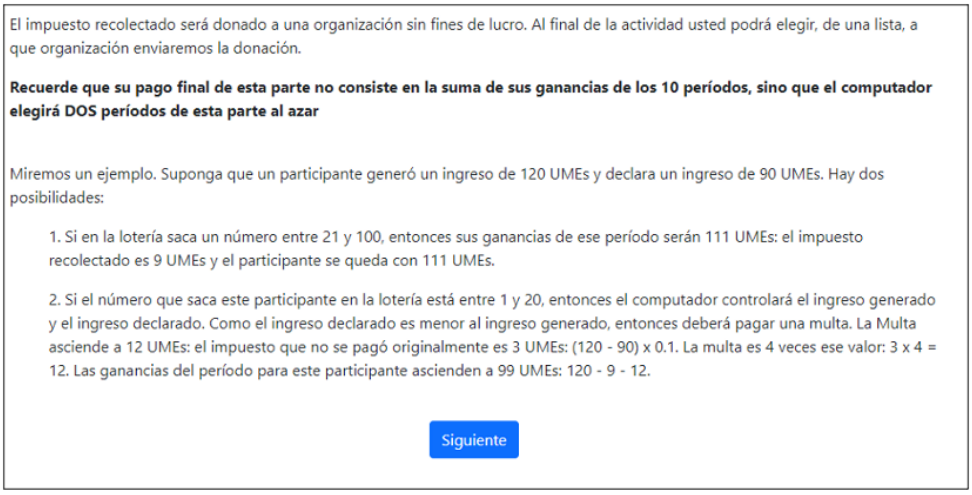
Note: The experiment was conducted using oTree (Chen et al., 2016) involving 410 college students from various universities in Bogotá, Colombia. Participants were recruited through the Rosario Experimental and Behavioral Economics Lab (REβEL) between November 4, 2022, and March 2, 2023. The instructions are in Spanish.

Figure 7 Screen – Tax Declaration Instructions I



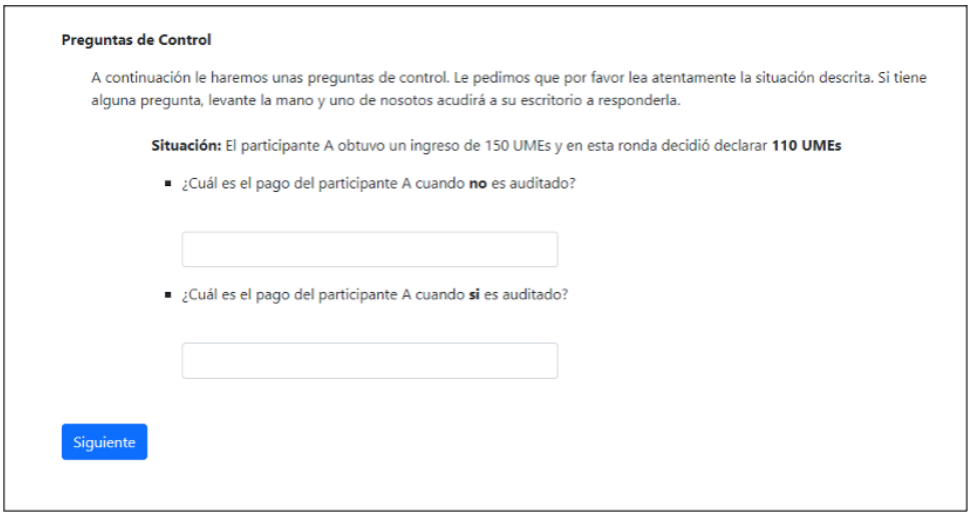
Note: The experiment was conducted using oTree (Chen et al., 2016) involving 410 college students from various universities in Bogotá, Colombia. Participants were recruited through the Rosario Experimental and Behavioral Economics Lab (REβEL) between November 4, 2022, and March 2, 2023. The instructions are in Spanish.

Figure 8 Screen – Tax Declaration Instructions II



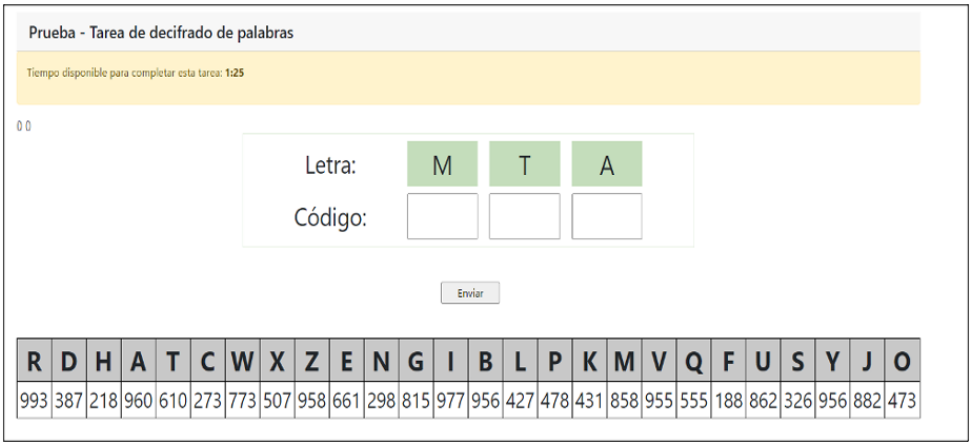
Note: The experiment was conducted using oTree (Chen et al., 2016) involving 410 college students from various universities in Bogotá, Colombia. Participants were recruited through the Rosario Experimental and Behavioral Economics Lab (REβEL) between November 4, 2022, and March 2, 2023. The instructions are in Spanish.

Figure 9 Screen – Control Question



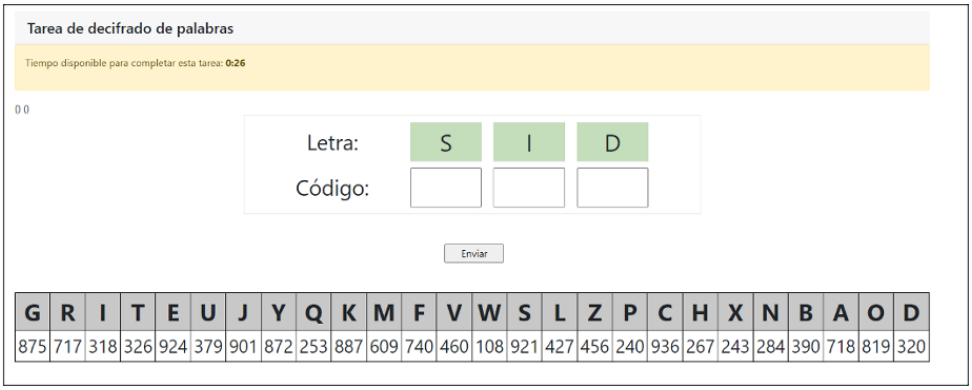
Note: The experiment was conducted using oTree (Chen et al., 2016) involving 410 college students from various universities in Bogotá, Colombia. Participants were recruited through the Rosario Experimental and Behavioral Economics Lab (REβEL) between November 4, 2022, and March 2, 2023. The instructions are in Spanish.

Figure 10 Screen – Encryption Task



Note: The experiment was conducted using oTree (Chen et al., 2016) involving 410 college students from various universities in Bogotá, Colombia. Participants were recruited through the Rosario Experimental and Behavioral Economics Lab (REβEL) between November 4, 2022, and March 2, 2023. The instructions are in Spanish.

Figure 11 Screen – Encryption Result

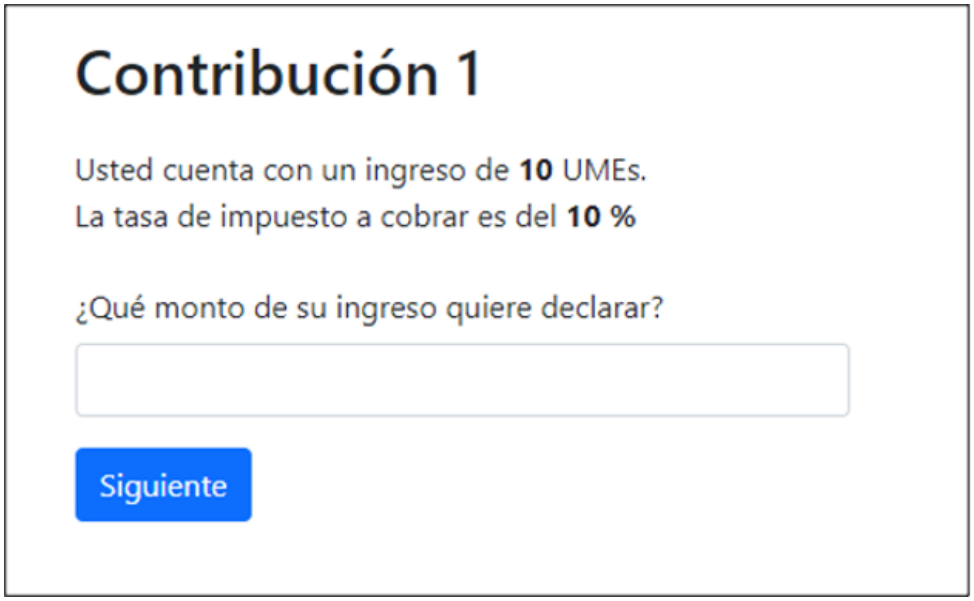


Note: The experiment was conducted using oTree (Chen et al., 2016) involving 410 college students from various universities in Bogotá, Colombia. Participants were recruited through the Rosario Experimental and Behavioral Economics Lab (REβEL) between November 4, 2022, and March 2, 2023. The instructions are in Spanish.

Figure 12 Screen – Results of the Encryption Task

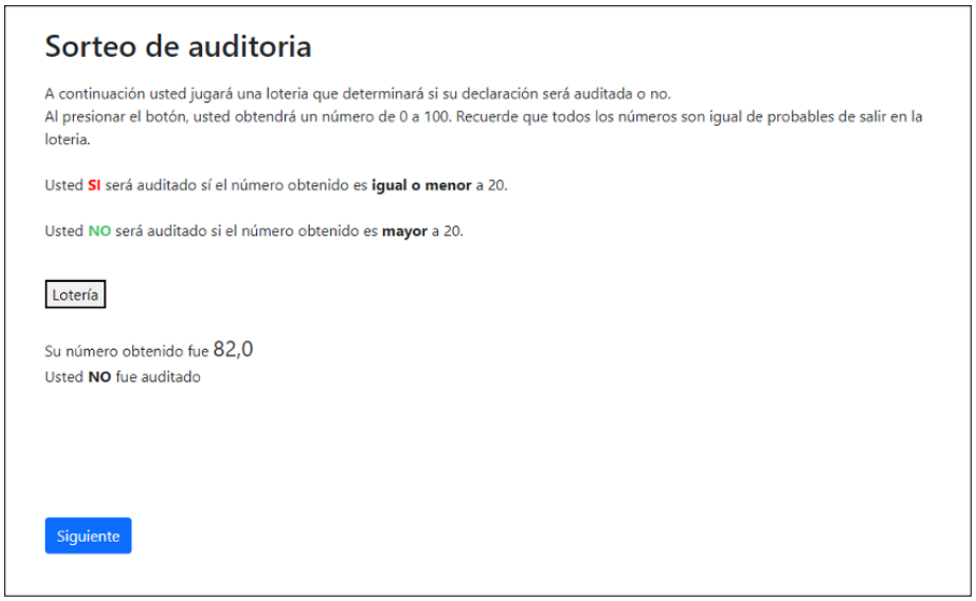
Note: The experiment was conducted using oTree (Chen et al., 2016) involving 410 college students from various universities in Bogotá, Colombia. Participants were recruited through the Rosario Experimental and Behavioral Economics Lab (REβEL) between November 4, 2022, and March 2, 2023. The instructions are in Spanish.

Figure 13 Screen – Declaration



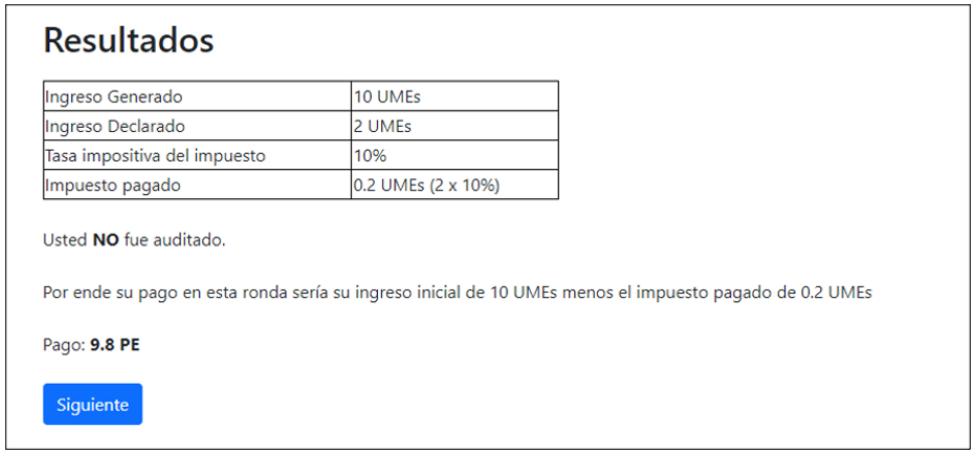
Note: The experiment was conducted using oTree (Chen et al., 2016) involving 410 college students from various universities in Bogotá, Colombia. Participants were recruited through the Rosario Experimental and Behavioral Economics Lab (REβEL) between November 4, 2022, and March 2, 2023. The instructions are in Spanish.

Figure 14 Screen – Audit Lottery



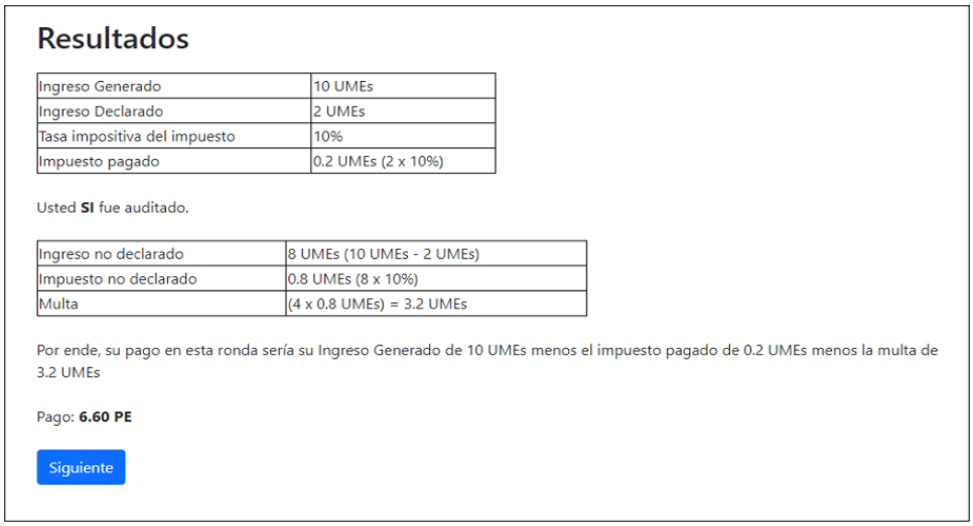
Note: The experiment was conducted using oTree (Chen et al., 2016) involving 410 college students from various universities in Bogotá, Colombia. Participants were recruited through the Rosario Experimental and Behavioral Economics Lab (REβEL) between November 4, 2022, and March 2, 2023. The instructions are in Spanish.

Figure 15 Screen – Results When Not Audited



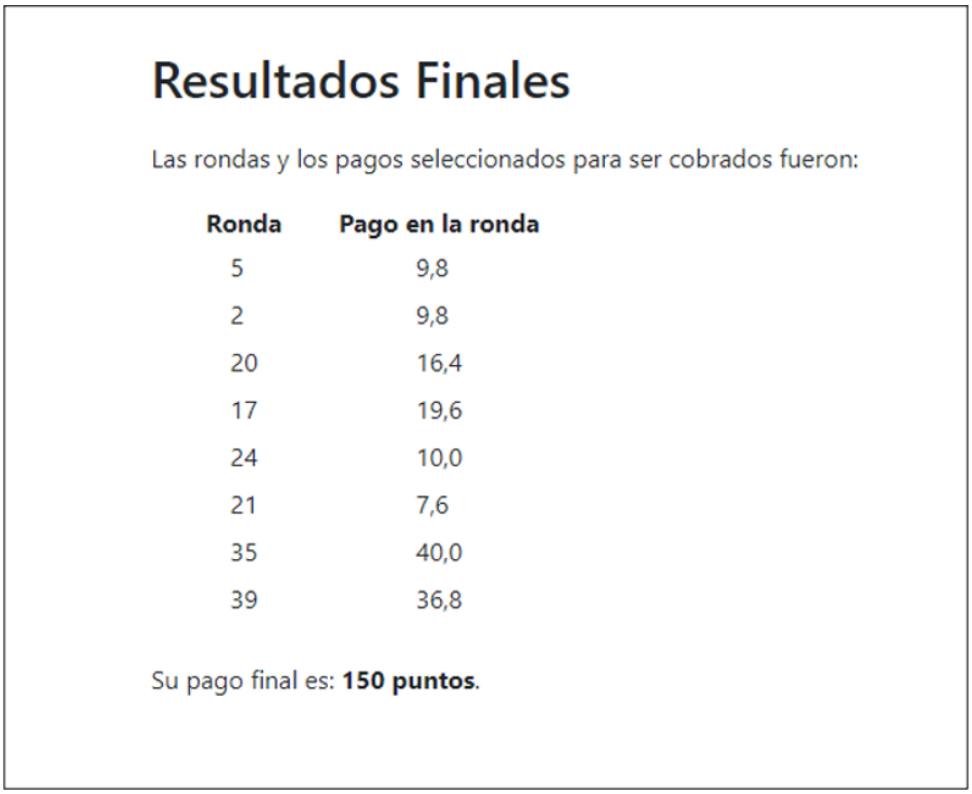
Note: The experiment was conducted using oTree (Chen et al., 2016) involving 410 college students from various universities in Bogotá, Colombia. Participants were recruited through the Rosario Experimental and Behavioral Economics Lab (REβEL) between November 4, 2022, and March 2, 2023. The instructions are in Spanish.

Figure 16 Screen – Results When Audited



Note: The experiment was conducted using oTree (Chen et al., 2016) involving 410 college students from various universities in Bogotá, Colombia. Participants were recruited through the Rosario Experimental and Behavioral Economics Lab (REβEL) between November 4, 2022, and March 2, 2023. The instructions are in Spanish.

Figure 17 Screen – Final Results



Note: The experiment was conducted using oTree (Chen et al., 2016) involving 410 college students from various universities in Bogotá, Colombia. Participants were recruited through the Rosario Experimental and Behavioral Economics Lab (REβEL) between November 4, 2022, and March 2, 2023. The instructions are in Spanish.