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# Appealing, threatening or nudging? Assessing various communication strategies to promote tax compliance

Andris Saulitis<sup>1</sup>  Philipp Chapkovski<sup>2</sup>

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## Abstract

This study examines the effect of various communication strategies on wage underreporting and tax compliance. Employing a field experiment with 3813 businesses in Latvia—a country marked by substantial wage underreporting—this research utilizes advanced data analytics to disseminate messages from the tax authority to firms whose declared wages substantially lag behind industry and regional averages. Messages ranged from normative appeals to audit probabilities and nudges. The immediate result was a notable increase in compliance in the first four months after the intervention, with firms elevating average wage levels. While the specific content of messages did not result in distinct long-term compliance behavior, the overall effectiveness of sending messages was affirmed. We identify a message combining 5% audit probability with normative appeals as the most effective one in enhancing tax revenues and triggering minimal negative feedback from the message receivers.

**Keywords** Tax collection · Shadow economy · Prosocial behaviour · Tax audits · Wage underreporting

**JEL Classification** C93 · D03 · D22 · H26 · H32 · H83

## 1 Introduction

Underreporting of wages contributes significantly to the shadow economy, with firms complying with minimum requirements like minimum wages while paying a substantial portion of salaries unofficially (Franić, 2019; Horodnic et al., 2023; Meriküll & Staehr, 2010; Williams, 2008, 2010; Williams & Padmore, 2013). Recent data indicates a trend towards undeclared work across Europe, moving from entirely unreported labor to

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underreporting actual labor income (European Commission, 2019, p. 75). This shift underscores a challenge in the fight against the shadow economy, highlighting the need for targeted strategies that address not just the presence of undeclared workers but the complex issue of labor income underreporting.

Advanced data analytics in tax administration offers a promising solution for identifying wage underreporting. For instance, Gavaille and Zasova (2022) demonstrate the potential of machine learning for classifying compliant and non-compliant firms concerning labor taxes. When paired with tailored communication strategies, data analytics and other technologies are a promising way to effectively nudge firms towards compliant behavior (de la Feria & Grau Ruiz, 2022; Martinez-Vazquez et al., 2023; Williams, 2023). This study presents a novel attempt to combine data analysis with diverse communication strategies to address wage underreporting.

We employ a field experiment with small businesses in Latvia, where traditional methods to fight the underreporting of wages have had limited success. Latvia has the highest rate of individuals in the European Union who admit receiving unofficial payments from employers even though issuing so-called ‘envelope wages’ (cash-in-hand payments) can lead to criminal charges (European Commission, 2019, p. 71). The Central Bank of Latvia estimates that underreporting of wages among low-wage earners and those officially earning the minimum wage affects around 70% of employees (Zalamane, 2021). Significantly, ‘envelope wages’ contribute nearly half (47%) of Latvia’s shadow economy, which represents an estimated 20% at least of the national GDP, making it one of the largest in the eurozone (Putniņš & Sauka, 2015; Sauka & Putniņš, 2022; Schneider, 2022).

Our study selected 3,813 businesses with average salary levels significantly below the averages of their industry and region at the time of the intervention. We randomly assigned some of those businesses to receive one of several possible messages. Alongside the baseline treatment message that simply informs on this fact and appeals to increase salaries in the future, we randomly assigned firms to six additional treatments. Three of these treatments introduced varying audit probabilities. The other three conditions incorporated different nudges, i.e., contextual information or encouraging prosocial behavior. We are interested in which of the various communication approaches, if any, is the most effective way to increase wage levels: simply appealing to increase the average wages in compliance with a social norm, threatening firms with audits in the future, or nudging them with contextual cues.

Mindful of critiques around the limited scope of many field experiments (Beshears & Kosowsky, 2020; Bicchieri & Dimant, 2022; Deaton & Cartwright, 2018; Slemrod, 2019; Viscusi, 2022), our study considers both the long-term implications on wage reporting of our interventions, as well critically examines the possibility of scaling the examined approach. First, we monitor the firms’ monthly wage declarations during the sixteen months following the intervention. Second, we discuss the scalability of our designed interventions by looking at the unexpected findings of our study: the spontaneous feedback received in response to our interventions. Finally, we incorporate these insights into our calculations of the net welfare effects of our interventions.

The focus of our study is the interaction between public institutions and private businesses to improve governance and reduce shadow economy activities. Public choice scholars have long argued that taxation and public finance are resolved through political channels, reflecting the balance or imbalance of political forces and institutions (Plott, 2014; Poterba, 1998; Torgler, 2022). The quality of institutions plays a crucial role in shaping these political channels and, consequently, has a significant impact on the size of the shadow economy. Empirical studies such as those by Buehn and Schneider (2012) and Allam et al.

(2023) show that institutional inefficiencies contribute to higher levels of tax evasion and a larger shadow economy. Furthermore, Gërxhani and Wintrobe (2021) argue that trust-based political exchange is critical, and tax compliance is influenced by citizens' trust in government to provide services and their belief in fellow citizens' compliance.

Strong rule of law and government legitimacy can reduce tax evasion by building greater trust in public institutions (Mason et al., 2020; Richardson, 2008). Our study seeks to understand the effects of appeals, threats, and nudges to foster fiscal exchange between the government and firms. In examining the various communication strategies in tax collection, we are mindful that the government's pursuit of maximizing tax revenues through its policies may inadvertently undermine tax compliance. Our insights from firms' written responses to the interventions show how certain communication strategies can backfire on firms' tax morale. The findings underscore the importance of a balanced approach that incorporates fine-grained trust-building measures and enforcement mechanisms to achieve a sustainable fiscal capacity of a state, i.e., the ability to collect taxes.

The paper presented here introduces various nudging interventions that intend to change individual (or, in our case, company) behavior. However, public choice researchers have warned of the potential downsides of nudging, particularly normative nudging, which can sometimes elicit contrary feelings when people are reminded of how many others are involved in non-cooperative behavior (Bicchieri & Dimant, 2022). Alm et al. (2023) reviewed the application of nudges in tax compliance and found mixed effects. They suggest that while the design and implementation of nudges are crucial, these interventions can complement traditional deterrence or enforcement approaches. This paper contributes to this debate by showing that norm-referenced messages have limited potential benefits in changing tax avoidance behavior.

Particularly, we find that messages delivered a positive fiscal impact in the first six months after the intervention. However, it seems that sending a message matters much more than what kind of message is sent, as the differences between threats of audits and nudges that do not warn of audit probability are not substantive. This would suggest that nudging might be the best approach, as it saves resources that would be spent on audits carried out by the tax authority. However, we observe that nudges and relatively large audit probabilities (33% & 66%) trigger large feedback from firms. The 5% audit probability does not, suggesting that clear and transparent information on the future steps of the tax authority minimizes the negative externalities of sending messages to the taxpayers. In the long term, our results indicate a dynamic response to the intervention, where the initial positive effect of messages on average salaries diminishes over time, but the overall trend remains higher than in the control group, with no significant long-term difference between different types of messages. Ultimately, we identify the 5% audit message as the most effective one due to its combination of a positive fiscal effect, low feedback, and a sustained impact over a long-term period.

The paper is organized as follows. We begin by laying out our theoretical framework and explaining the design of the study. Following this, we report on the behavior among the firms. We use a rich dataset that includes information on average wages, employees, turnover, and claimed tax deductions for each firm to observe the possible changes in wage-setting behavior and tax evasion due to intervention. Then, we estimate the possibility of scaling the experimental results, considering the immediate reactions of firms post-intervention and gleaned insights from their feedback. In concluding the paper, we summarize and discuss our findings within the existing literature on wage underreporting.

## 2 Communication strategies to foster tax compliance

We have identified several communication strategies employed to mitigate tax evasion. Even though fear of detection and punishment, as well as the perceived benefits of cash-in-hand payments, are significant factors in accepting ‘envelope wages’, several studies highlight the role of tax morale and social norms in such behavior (Franić, 2019, 2020; Horodnic et al., 2023; Williams & Horodnic, 2017). In our baseline message, we prime firms to the existing social norms by providing information on others’ behavior. Social norms are one of the most studied and ambiguous interventions, with many contextual factors affecting taxpayers’ behavior (Bobek et al., 2013; Górecki & Letki, 2021; Onu & Oats, 2015). For example, Hallsworth et al. (2017) have found that descriptive social norms — information that reveals the behavior of the majority — are the most effective among other interventions in persuading tax payments. In other tax compliance studies, social norms have no effect (Carpio, 2014; Cranor et al., 2020; Perez-Truglia & Troiano, 2018), and in some cases, even backfire tax collection efforts (De Neve et al., 2021; John & Blume, 2018). In our case, the baseline message offers information on firms’ wage practices in the previous six months compared to industry and regional averages, highlighting deviations from the wage reporting patterns over a significant period.

For the additional six treatment messages, our baseline message is complemented and reinforced with additional paragraphs based either on threats or contextual cues, i.e., nudges. Our second communication strategy mixes normative appeals with various audit probabilities (5%, 33%, 66%) that we disclose to the recipients. Audits and sanctions mostly demonstrate enhanced compliance (Bíró et al., 2022; Bjørneby et al., 2021). However, threatening taxpayers with audits risks undermining trust and eliciting adverse reactions (Gangl et al., 2020; Hofmann et al., 2017; Mendoza et al., 2017).

Our third communication strategy is inspired by studies that have demonstrated the positive effect of nudges (Abdukadirov, 2016; John et al., 2011; Thaler & Sunstein, 2008). When employed judiciously, nudges can bridge the gap between civic duty and immediate self-interest, guiding taxpayers towards more compliant behaviors (Benartzi et al., 2017; Hallsworth, 2014; Hallsworth et al., 2017). However, field experiments have provided mixed evidence on the effectiveness of nudging on labor taxes. While some indicate positive effects when firms are informed about their below-average salary levels, others highlight the fleeting nature of these impacts (Boning et al., 2020; Vainre et al., 2020).

It would seem that the effectiveness of various strategies varies due to the influence of contextual factors. Our study allows us to further investigate the factors that influence the decisions on tax compliance. Through the insights gained, we strive to contribute to a more holistic, nuanced understanding of the role of data-driven and targeted communication strategies in tax compliance, particularly among businesses with an identified risk of having underreported wages.

## 3 Experimental design of the study

At the core of our experiment, we provide firms with critical information on average salary levels by asking them to reconsider their wage-setting practices in the future. We then examine these normative appeals in conjunction with diverse enforcement strategies that pivot on either audit probabilities or nudges. It enables us to scrutinize how different combinations of deterrence and non-deterrence strategies influence compliance behavior. In particular, we investigate how the power of normative appeals changes with varying audit

probabilities and whether imbuing our normative appeals with additional contextual cues can elicit enhanced wage levels. In this section, we outline the specifics of our experimental design, starting with the experimental setting and then discussing its various components, including treatment messages and empirical strategy for analyzing the experimental results.

### 3.1 Experimental setting

The communication between businesses and tax administration in Latvia is mostly digital via the Electronic Declaration System (EDS) hosted by the State Revenue Service (SRS). Within this platform, enterprises submit monthly tax declarations consisting of information on the number of employees, salaries for each employee, and turnover. The system automatically calculates the taxes due: information on wages is used to estimate the labor taxes, while the turnover is essential to calculate the amount of value-added tax owed after claimed deductions.

The EDS also provides an opportunity for communication between the taxpayer and the tax administration. We conducted our study in this communication environment, where we could send messages and monitor the responses. We cooperated with the SRS to develop several messages and examine their effect on increasing the average wage levels paid by firms.<sup>1</sup>

The businesses included in the study were already pre-selected by the SRS to inquire about the average declared salary of less than 70% of the average in the industry and region. The methodology for calculating the average salary was based on the total sum of salaries paid to all employees within a month, divided by the number of employees declared to have received a salary in that month. This calculation did not account for employees without a declared salary or distinguish between those employed full- or part-time but focused solely on those with declared earnings. This approach allowed for a consistent measure across firms, ensuring comparability while addressing the concern of wage underreporting.

Given this methodology, firms might be tempted to increase average wages by reducing the number of lower-paid or part-time employees. For this reason, we focus our analysis not only on the changes in the average salaries each month but also on the average number of employees over time. Doing so ensures that the findings reflect genuine changes in wage levels rather than statistical artifacts created by employment shifts.

### 3.2 Treatment messages

In the baseline message, businesses were only informed that, in their case, the average salary in the first half of 2021 has been below 70% of the average in the region and sector in which the firm operates. Consequently, it was explained that this estimate is considered a risk of engaging in the shadow economy. At the end of the first paragraph, firms were asked to minimize the abovementioned risk by considering raising the average salary in the next declarations submitted to the SRS (see Appendix A for the baseline message, translated into English). In addition to the baseline message, we designed six behaviorally informed treatment texts that were included in the messages (see Table 1). These treatment texts were one paragraph long, with three varying audit probabilities and the remaining three employing prosocial messages.

<sup>1</sup> The experimental design and the subsequent analysis of the field experiment was pre-registered ([reference partly hidden for preserving anonymity of authors for the reviewers] 2021).

**Table 1** Treatment texts in the experiment

Approach	Treatment	Additional treatment paragraph
No message	Control group	No message sent
Baseline	Baseline	No additional treatment paragraph included (see Appendix A for the text sent to the baseline message recipients)
Audit	5% audit 33% audit 66% audit	We inform you that starting from January 2022, a randomly selected share [5%/33%/66%] of companies from the recipients of this letter will be invited to provide the SRS Tax Payment Compliance Division with explanations and information about the correctness of labor taxes and compliance with the requirements of the law in the declarations submitted during the period from October 2021 to December 2021
Prosocial	Social norm	Most people pay taxes: according to a survey conducted by <i>Latvijas Fakti</i> last fall, 84% of Latvian residents and their employers have honestly reported all earned income and have not received envelope wages
	Trust in SRS	Trust-oriented communication and cooperation is the strategic goal of the SRS. Latvian citizens increasingly trust the SRS and appreciate the “Consult first” principle implemented in its operations. In both 2018 and 2019, the SRS was awarded the initiative’s “Consult First” award “Leader in the monitoring strategy”. We invite you to trust the SRS as your ally in tax matters for honest business and public welfare!
	Combating the shadow economy	The shadow economy is diminishing: in the last ten years, the share of envelope wages in Latvia has decreased by more than ten percentage points. There is also an improvement in the segments of other taxes, and the collection of taxes is improving. SRS estimates for the so-called VAT gap show that it will have decreased from 9.4% to 8.6% in 2020, continuing the declining trend for several years

The examination of explicit audit threats was based on the study by Harju et al. (2014), where they varied the audit probabilities (5% and 33%) in the letters and analyzed their effect on tax compliance among hairdressers. In their study, only a high probability (33%) of the audit had a positive effect. We extended this research by varying the audit probabilities at three different levels (5%, 33%, and 66%) to investigate whether increased audit probability would improve tax compliance, as suggested by the economics-of-crime-based tax compliance model (Allingham & Sandmo, 1972). It is important to note that even when audit probabilities were explicitly stated, it was emphasized that audits would not be carried out earlier than January 2022 and for a period no earlier than September 2021. In other words, the audits would be performed only on tax declarations submitted in the future, giving firms time to reconsider their wage policies.<sup>2</sup>

The remaining three treatments provided additional contextual cues on top of the baseline message. In one of the messages, we used descriptive social norms that “characterize the perception of what most people do” (Cialdini et al., 1991, p. 203). Specifically, the letter stated that, according to a recent survey, 84% of Latvians have truthfully reported their income and have not received cash in an envelope. Another treatment message emphasized

<sup>2</sup> In January 2022, a random list of enterprises from the sample was prepared for audit by the SRS. As such, there was no deception involved. However, there was no timeline when and how exactly the audits will be carried out and the process is out of our control.



that trust in the SRS has increased. The last treatment message included information about the successful fight against the shadow economy. It emphasized a recent decrease in the size of the shadow economy and the estimated VAT gap. An additional control group did not receive any messages.

### 3.3 Experimental procedure

At first, we developed the messages and examined them in a pilot study carried out two months before the intervention. The pilot indicated that around one in four firms is writing or calling back to the SRS. As a result, the number of businesses included in the trial was limited to the estimated number of received replies to the messages and the ability of the SRS to respond to these inquiries in legally binding time. In total, 3,929 firms were selected for the trial.

In addition to an average salary level 70% below the average in the industry and region, several additional factors were used as the selection criteria (see Fig. 1). During the randomization procedure, the firms were blocked by the statistical region and balanced on the key control variables (see Appendix B, Table 7 for descriptive statistics and balance tests). Consequently, the treatment messages were sent in three waves (one-third of the sample each week) during September 2021, starting September 7. The treatment arms were used as blocks in the random assignment to the waves. The last messages were sent on September 21, roughly a month before the next tax declaration was due to be submitted. After the intervention, no audits were carried out for the firms in the sample. The exception was detecting fraud or any other illegal activity requiring immediate action. In such a case, the case was dropped from the sample, as was the case if the business was closed.<sup>3</sup>

### 3.4 Empirical strategy: difference-in-difference specification

Our empirical strategy uses a difference-in-differences approach using monthly panel data at the employer level. This approach allows us to compare tax compliance behavior across different groups of employers that differ in their exposure to specific tax compliance messages.

Our primary empirical specification is as follows:

$$Y_{it} = \beta_0 + \sum_{y=-3}^3 \beta_y M_{it} \times I(t - t_i^* = y) + F_i + T_t + \gamma C_{it} + \varepsilon_{it}$$

Here,  $Y_{it}$  denotes the outcome of interest—that is, the average declared salary per employer  $i$  in month  $t$ .  $M_{it}$  is an indicator variable for the treatment group, i.e., employers who received a specific tax compliance message. The treatment is a binary indicator equal to 1 for ‘treated’ firms that received a message from the SRS and 0 for ‘non-treated’ firms that received no message. This variable distinguishes firms that were subjected to the intervention (tax compliance message) from those that were not.

<sup>3</sup> The ex-post simulations of difference-in-difference models with the given effect between treated ( $N=3,282$ ) and untreated ( $N=470$ ) companies with the observed effect (+20.68 euro) showed that for the full population size (100% share) we can detect a statistically significant effect at the  $\alpha=0.05$  level in 98% of cases and at  $\alpha=0.01$  in 92% of cases. The statistical power with half of the population (50% share) remains at 81% at the  $\alpha=0.05$  level. The detailed distributions of simulations are available in Appendix E and the code for replication is provided in the OSF repository: <https://osf.io/yaetk/>.



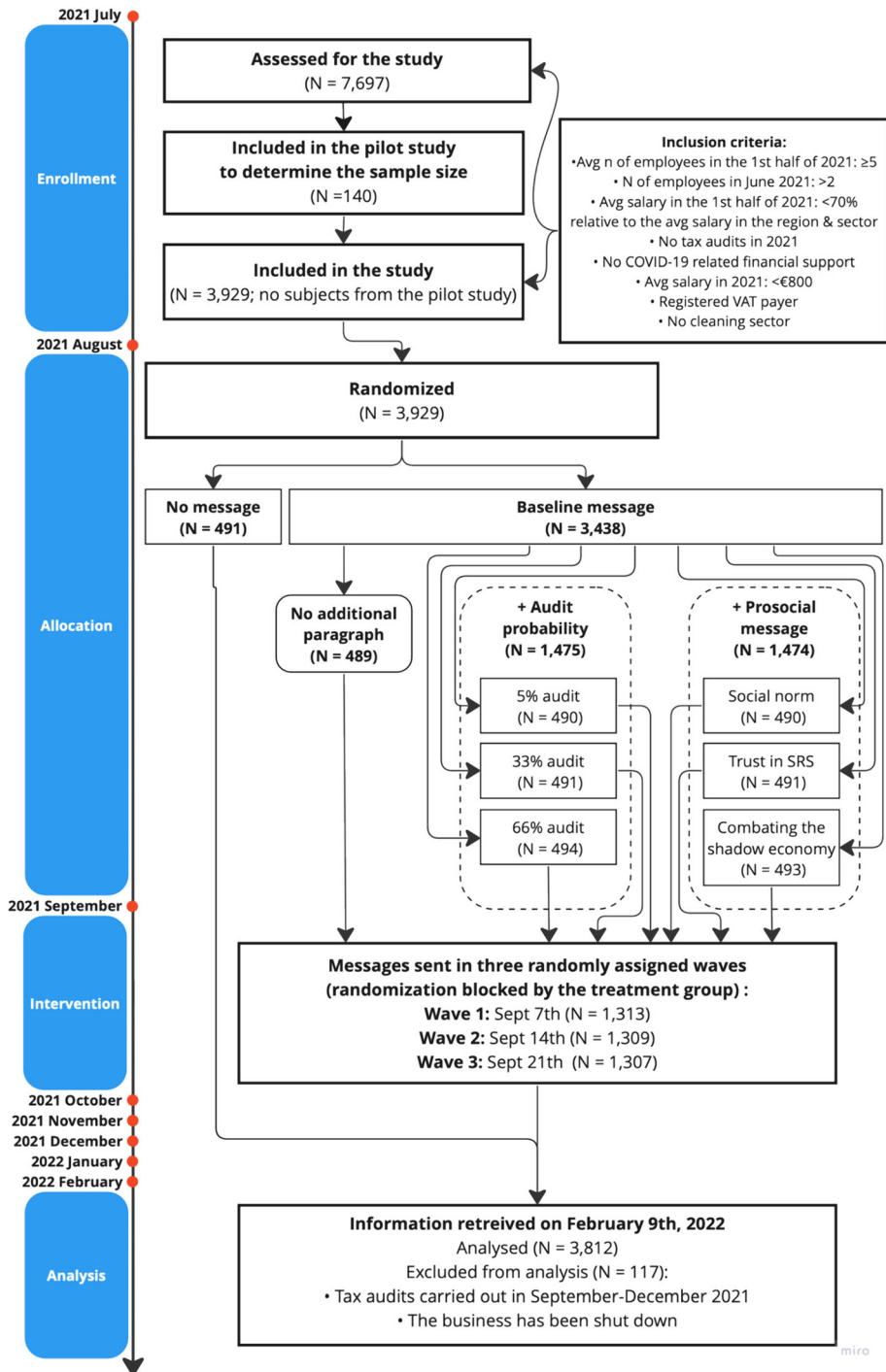


Fig. 1 Flowchart and timeline of the experiment

Indicator variables  $I(t - t_i^* = y)$  measure the time relative to the intervention month  $t_i^*$  (September 2021). Again, we should emphasize that the data reflects the reporting months; the reports for any given month  $t$  will be submitted next month. This explains why we can observe some effects of the intervention in September, as the reports for that month are actually submitted in October of the year. The variable  $y$  represents the months relative to the intervention, with different indicators for each month in your dataset: three months before the intervention (June, July, August 2021), the intervention month itself (September 2021), and three months after the intervention (October, November, December 2021).  $y = -1$ , August 2021, the month immediately before the intervention, is the omitted category: the coefficients for the other months are interpreted relative to this baseline period. We expect that the coefficients  $\beta_i$  for the months prior to the intervention will not be statistically significantly different from zero.

Our specification controls for company-level fixed effects ( $F_i$ ) to account for all time-invariant characteristics of each firm, such as company size, industry, region, wave of message dissemination, and message type. By including firm fixed effects, we implicitly account for these time-invariant characteristics. The specification also includes month-level fixed effects ( $T_t$ ) to capture month-specific variance. Additionally, we incorporate time-varying  $\gamma C_{it}$ , to control for characteristics that change over time, such as turnover, number of employees, and the total amount of declared taxes for each firm.

The empirical strategy of this study requires a thorough understanding of the timing involved in tax declaration submissions and its implications for our analysis. Tax declarations for a given month must be submitted by the 20th day of the subsequent month. This procedural nuance is crucial for interpreting the effects of our September 2021 intervention. We anticipate potential behavioral changes to manifest in the immediate aftermath of the intervention. Specifically, for our study, this pertains to tax declarations due by October 20, 2021, which reflect wage levels for September 2021. Recognizing this timing is essential to fully grasp the direct impact of our intervention on tax compliance behavior immediately after its implementation.

### 3.5 Analysis of the feedback by firms

The analysis of calls and messages received from firms by the tax authority is integral to our study for several reasons. First, it provides direct insights into firms' immediate reactions and intentions following the intervention, offering a nuanced understanding of their behavioral responses beyond mere statistical changes in tax declarations. While it is true that firms can adjust their reported taxes without any direct communication with the tax authority, the act of reaching out—whether to clarify, commit, or contest the information provided—signals a level of engagement and receptivity to the intervention that cannot be captured through tax data alone. Furthermore, this feedback represents a rich qualitative dataset that can elucidate firms' motivations, challenges, and decision-making processes as they navigate tax compliance and wage reporting. Analyzing these communications allows us to identify the prevalent themes, concerns, and misconceptions among firms, which can inform future interventions and policy designs, including calculating the net welfare effect. In essence, this part of our research highlights the interactive aspect of tax compliance, emphasizing the importance of dialogue between tax authorities and taxpayers in fostering a cooperative compliance environment.

For the quantitative content analysis, we systematically classified the feedback received from firms through phone calls or messages in the EDS. Tax administration personnel

scrutinized the responses, categorizing them into four distinct groups: (1) no response, indicating no explicit commitment towards changing wage levels; (2) a promise to increase salaries, showcasing a commitment to adjust wage levels upwards; (3) a declaration of inability to raise wages, suggesting financial or operational constraints; and (4) an indefinite answer, reflecting uncertainty or non-committal stances regarding wage adjustments. The personnel involved in categorizing these responses were blinded to the specific type of message each firm received, implementing a single-blind experimental design to mitigate potential bias. To discern any disparities in declared salary growth across these response categories, we employed non-parametric Kruskal–Wallis tests, allowing for an unbiased comparison irrespective of the distribution of salary changes.

After the experimental intervention, the SRS handed over anonymized versions of the written feedback it had accrued from firms. It is important to note that the feedback conveyed through calls was not systematically recorded or transcribed for analysis. Consequently, our qualitative examination of firm feedback is exclusively based on those entities that opted to communicate in writing. This methodological choice inherently limits our qualitative analysis to a subset of the total feedback, providing insights into firm behavior and perceptions through the lens of those who chose to engage with the tax authority via written correspondence.

## 4 Experimental results

We present the results of our study in two subsections. First, we focus our investigation on the impact of the treatment on reported salaries, examining the short-term effects (within the first four months after the intervention) and then looking at the long-term trends over the next year. We then report on companies' responses to our interventions by presenting the results of the content analysis and providing insights into the communication between the tax administration and the companies.

### 4.1 Treatment effect on declared salaries

In our analysis, we examine the impact of tax compliance messages on the monthly salary declared by firms. We employ a difference-in-differences approach to compare 'treated' firms—those that received any type of message—with 'non-treated' firms that did not receive a message before and after the intervention. The coefficients for the months before the intervention,  $\text{treated} \times \text{month} = -3$  and  $-2$  (June and July 2021, respectively), are not significantly statistically different from zero in most regressions, which is consistent with our expectations that there should be no difference in the declared salaries between the groups before we disseminated the messages.

The intervention month (September 2021) is indicated by  $\text{treated} \times \text{month} = 0$ , and the subsequent months till December are marked as  $\text{treated} \times \text{month} = 1, 2$ , and  $3$ . The coefficients for these months across all columns are positive and statistically significant, suggesting a noticeable increase in the average declared salary after receiving the tax compliance messages. In Table 2, we present two regressions (columns 1 and 2) in which we compare a non-treated sample of firms with a pooled sample of firms with no distinction based on the type of treatment message received, as well as regressions in which treated firms are selected from specific subsets of treatment message. Column 3 compares non-treated firms with recipients of the baseline message; column 4 takes recipients of audit

**Table 2** Fixed effects (difference-in-difference) regressions on log of average declared salaries

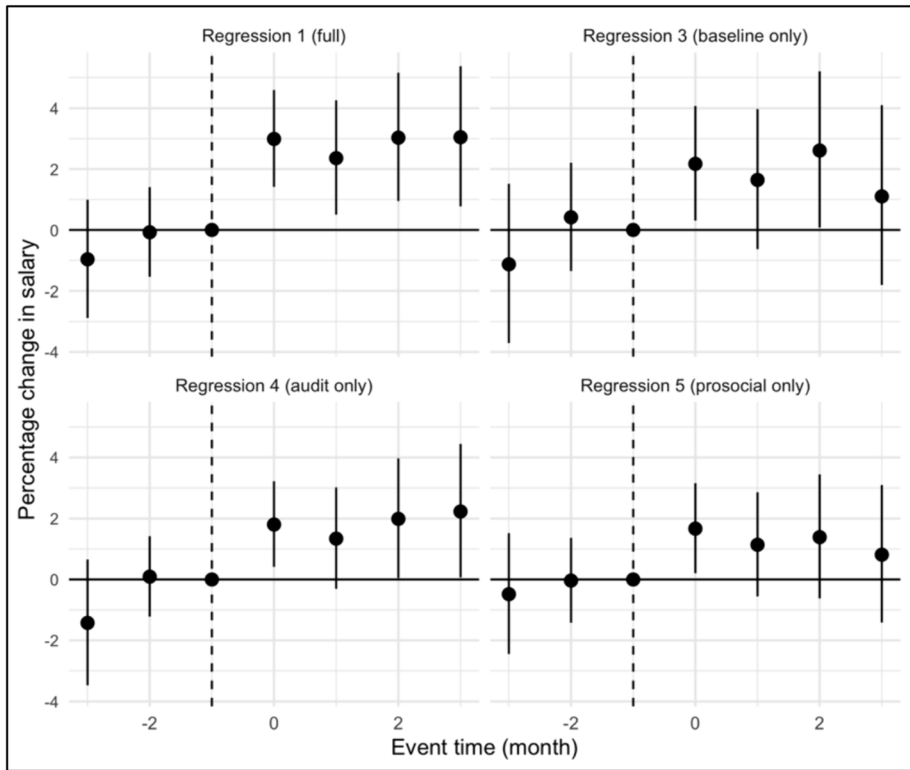
	(1)	(2)	(3)	(4)	(5)
Treated x Month=-3	-0.01 (0.01)	-0.01 (0.01)	-0.01 (0.01)	-0.01 (0.01)	0.00 (0.01)
Treated x Month=-2	0.00 (0.01)	0.00 (0.01)	0.00 (0.01)	0.00 (0.01)	0.00 (0.01)
Treated x Month=0	0.03*** (0.01)	0.02** (0.01)	0.02* (0.01)	0.02* (0.01)	0.02* (0.01)
Treated x Month=1	0.02* (0.01)	0.01 (0.01)	0.02 (0.01)	0.01 (0.01)	0.01 (0.01)
Treated x Month=2	0.03** (0.01)	0.02* (0.01)	0.03* (0.01)	0.02* (0.01)	0.01 (0.01)
Treated x Month=3	0.03** (0.01)	0.01 (0.01)	0.01 (0.01)	0.02* (0.01)	0.01 (0.01)
Turnout	No	Yes	Yes	Yes	Yes
Number of employees	No	Yes	Yes	Yes	Yes
Declared taxes	No	Yes	Yes	Yes	Yes
<i>Fixed-effects</i>					
Firm	Yes	Yes	Yes	Yes	Yes
Month	Yes	Yes	Yes	Yes	Yes
Observations	26,264	25,919	6,494	12,957	12,978
R2	0.805	0.845	0.855	0.854	0.841
Within R2	0.001	0.217	0.196	0.247	0.191

Columns 1 to 2 report difference between non-treated ('No message') group and all other treatments; Column 3 reports difference between non-treated group and baseline message recipients; Column 4 reports difference between non-treated group and audit message recipients; Column 5 reports difference between non-treated group and prosocial message recipients. Standard errors clustered at the firm level. \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$

messages, and column 5 compares non-treated firms only recipients of prosocial types of messages.

We start with the simplest specification (column 1), which contains only company and month level fixed effects. Including these fixed effects allows us to control for time-invariant characteristics of firms and time-specific shocks that could otherwise confound our results. Additional firm-month-specific variables, such as turnover and the number of employees, are included in columns 2 through 5. Overall, our findings suggest that tax compliance messages have a significant positive effect on the average salary declared by firms immediately after the intervention. This effect persists and even increases in the subsequent months, indicating a potential lasting impact of the messages on firms' tax compliance behavior. Specifically, there is an overall 3% increase in average salary levels among firms receiving such messages compared to those that did not (see Fig. 2). This effect is the largest for those firms that received messages warning them about a potential audit, albeit the coefficients for different messages are very similar, indicating that the mere act of sending a message is more impactful than the specific content of the message.

It is important to reiterate that our data correspond to the reporting months, with submissions due by the 20th day of the subsequent month. For example, the tax declaration for September must be submitted by October 20. This scheduling detail clarifies the observed effects of the intervention within September itself (month 0 in Fig. 2)—since the declarations for this month are submitted post-intervention, allowing the intervention's impact to be reflected immediately in the reports. This timing aspect is crucial to understand how the interventions' effects are captured in our analysis.



**Fig. 2** Percentage Change in Declared Salaries per Company between Non-Treated and Treated Groups. *Notes:* the coefficients are based on regressions from the Table 2 (Regression numbers correspond to columns in Table 2). Vertical lines show 95% confidence intervals

The assumption of a parallel trend is crucial for our specification. There is no obvious pre-trend in the months prior to the intervention. We checked the balance of covariates at the company level (e.g., industry affiliation, region, indicators of economic activity in the previous year) and the monthly level before the intervention (i.e., before September 2021). Tables 3 and 4 show no significant differences between the treated and untreated groups of companies. The almost identical trends in average salaries reported four months before the intervention in both groups also confirm the plausibility of the assumption of a parallel trend (Fig. 3).

We also looked at the number of employees to examine possible adverse effects, as the pressure toward firms to increase salaries can push businesses into the shadow economy (Arsić et al., 2015; Davidescu & Schneider, 2019). Specifically, they could move the workers with the lowest salaries to the shadow economy to increase the declared average wage level. However, we did not find statistically significant differences in turnover or the number of employees declared among the firms in the post-treatment period (see Table 5 and Fig. 9 in the Appendix).

Also, to compensate for the increase in labor taxes to be paid as a result of increased wages, firms could declare smaller turnover and, as a result, pay less value-added tax that is calculated in relation to the turnover. Additionally, firms might claim higher tax deductions for value-added tax, as was evident in a study by Ariel (2012). As with changes in the

**Table 3** Firm-level covariates

Characteristic	Control N=477 <sup>1</sup>	Treated N=3,335 <sup>1</sup>	Difference <sup>2</sup>	95% CI <sup>2,3</sup>	p value <sup>2</sup>
Average salary for firms in the industry & region in 2021 Q1 (euros)	1,064 (307)	1,065 (280)	−0.94	−30, 28	>0.9
Average salary in 2021 months 1–7 (euros)	467 (123)	470 (117)	−3.3	−15, 8.5	0.6
Number of employees in 2021 months 1–7	13 (21)	14 (22)	−0.73	−2.7, 1.3	0.5
Number of employees in 2020	12 (14)	13 (21)	−1.3	−2.7, 0.19	0.088
Company turnover in first seven months of 2021 (euros)	359,244 (513,428)	404,930 (1,041,932)	−45,686	−103,828, 12,457	0.12
Company turnover in 2020 (euros)	566,930 (821,583)	621,446 (1,294,700)	−54,516	−141,008, 31,976	0.2
Paid taxes in 2021.g. by 31 August 2021 (euros)	39,529 (99,999)	41,333 (125,347)	−1,804	−11,752, 8,144	0.7
Taxes paid in 2020 (euros)	52,745 (121,710)	59,336 (193,831)	−6,591	−19,378, 6,196	0.3
Grants received (euros)	4,531 (17,720)	4,429 (19,096)	102	−1,619, 1,822	>0.9
Age of the enterprise (years)	15 (9)	15 (9)	0.17	−0.69, 1.0	0.7

<sup>1</sup>Mean (SD); n (%); <sup>2</sup>Welch Two Sample t-test; Pearson's Chi-squared test; <sup>3</sup>CI=Confidence Interval

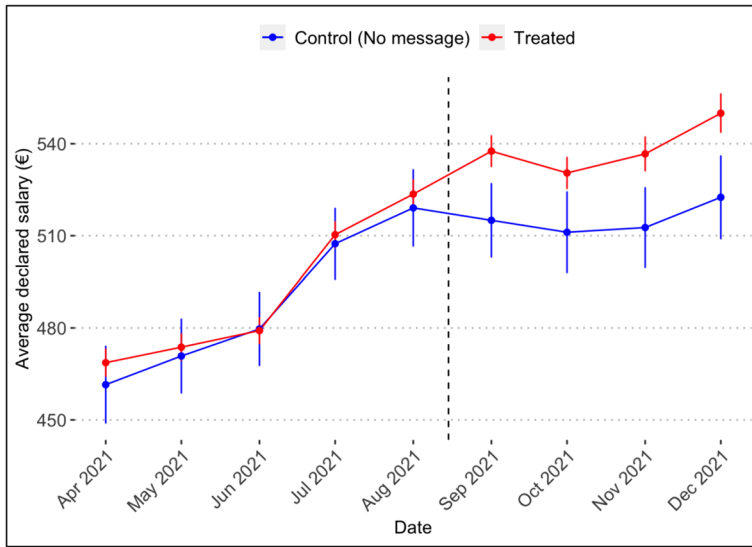
**Table 4** Firm-month level covariates before intervention (June–August 2021)

Characteristic	Control N=1,907 <sup>1</sup>	Treated N=13,336 <sup>1</sup>	Difference <sup>2</sup>	95% CI <sup>2,3</sup>	p value <sup>2</sup>
Average salary	494 (136)	497 (134)	−2.5	−8.9, 4.0	0.5
Taxes paid to the single tax account	6,213 (13,223)	6,379 (16,992)	−166	−1,084, 752	0.7
Returned claimed tax deductions from SRS	895 (3,801)	791 (3,615)	104	−98, 305	0.3
Turnover	59,306 (84,576)	66,679 (169,607)	−7,373	−16,380, 1,635	0.11
N of employees	14 (25)	14 (23)	−0.55	−1.8, 0.74	0.4
Total sum of declared taxes	3,450 (7,163)	3,495 (5,543)	−45	−365, 275	0.8

<sup>1</sup>Mean (SD); <sup>2</sup>One-way ANOVA; <sup>3</sup>CI=Confidence Interval

average number of workers, we find no differences in firm behavior regarding VAT in the post-intervention four-month period relative to the ‘No message’ condition (see Table 6 and Fig. 10 in the Appendix).

Overall, we find that messages had a fiscally positive effect with no evidence that firms would engage in a shadow economy, claim more tax deductions, or that a message triggers any other financially adverse effect. Instead, each of the 3,335 treated firms declared, on



**Fig. 3** Trends in average salaries before and after intervention. *Notes:* Vertical lines show 95% confidence intervals. Trends for each treatment can be found in Appendix (Fig. 8)

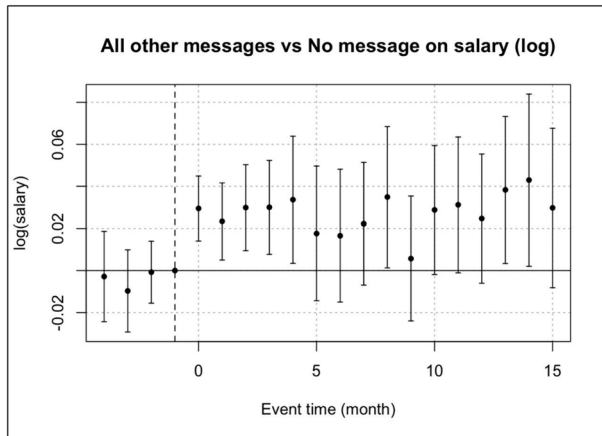
average, around 130 euros more in labor taxes each month over four months post-intervention. This is on top of the 3,490 euros that untreated firms paid on average, marking a nearly 4% gain in tax revenues from the treated firms. This gain is closely aligned with the 3% increase in the average salary level among firms that received a message compared to those who did not in the post-intervention period. A simple back-of-the-envelope calculation, considering the additional labor taxes declared by treated firms during the post-intervention period (September–December) minus the difference they paid compared to non-treated firms during the pre-intervention period (June–August), reveals a total fiscal effect of approximately 1.7 million euros.

## 4.2 Long-term effect of the messaging strategies

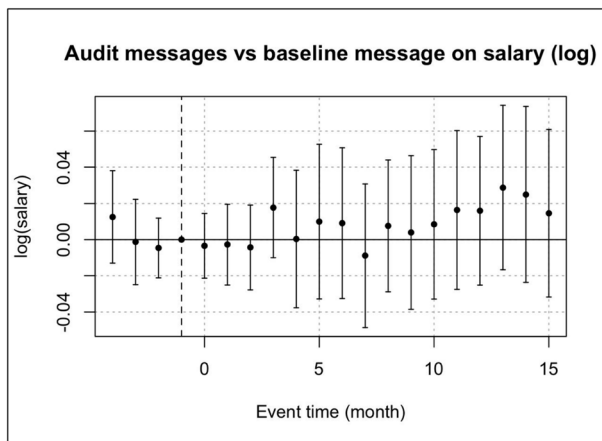
In a non-pre-registered extension of our study, we examine the long-term impact of our behavioral messaging strategy on tax compliance. This analysis is based on data from the SRS on tax returns filed by the firms in our study in the year following our intervention, 2022. Our central question is to determine the duration of the impact: Is it a short-lived reaction triggered by a perceived threat of a tax audit after receiving a letter from the SRS, regardless of whether a tax audit was explicitly mentioned? Or does it represent a more profound shift that endures over time, implying a re-evaluation of social norms and a subsequent decision to increase salaries? We address these questions in the following analysis.

Our results, shown in Fig. 4, emphasize a dynamic response to the intervention over time. The figure shows the results of the difference-in-differences regression, where the baseline group is the group that received no message at all. We compare the treatment interaction with the months after treatment between these two groups. The dependent variable is log (salary), so the y-axis shows the percentage difference between treated and untreated groups per month.





**Fig. 4** Average declared salary (log): difference between treated and untreated groups in the long term. *Note:* Whiskers show 95% confidence intervals



**Fig. 5** Effect of Pooled Audit Risk Messages (5%, 33%, 66%) Compared to Baseline Message on Average Salary Over Time. *Note:* Whiskers show 95% confidence intervals

In the first five months after the intervention, the firms that received the messages show a persistent increase in average salaries compared to the firms that did not receive any messages (which corresponds to the similar graph of the top-left panel of Fig. 2). This trend diminishes in the following months, where the differences, while still positive, decrease in magnitude, suggesting a temporary adjustment period in which companies may be trying to compensate for increased labour costs. Nevertheless, the overall trend shows that they are consistently higher than the salary reported by the firms in the control group, even if the difference is not statistically significant due to the large variance.

To investigate whether there are long-term differences between recipients of different message types, which were not observed in the short run, we also performed an analysis comparing the baseline with the audit messages. Our analysis, illustrated in Fig. 5, compares those who received a baseline message with those who received more stringent messages

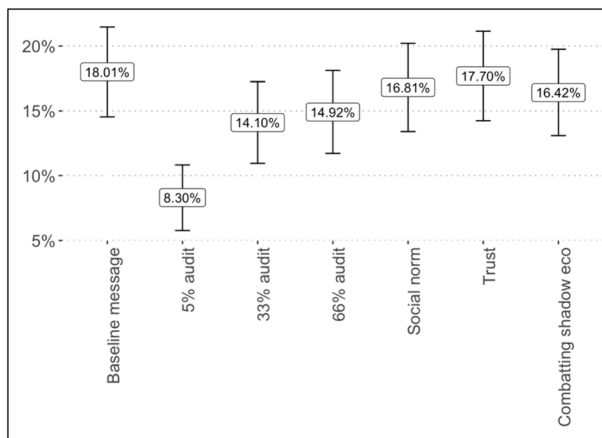
about different audit probabilities. The results show that there is no long-term difference in reported compensation between those who received the baseline message and those who were assigned to various audit probabilities.

## 5 Scaling the experimental results: spontaneous feedback and its contents

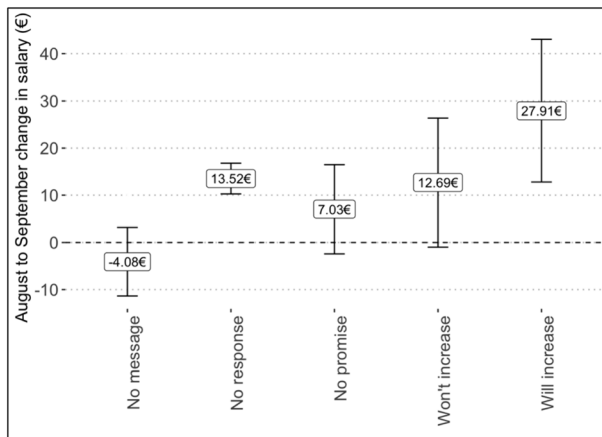
In this section, we investigate the broader impact of our intervention strategy, extending beyond just the fiscal effects and tax declarations submitted by firms. We examine spontaneous and unexpected feedback following the intervention, which presents crucial insights but also potential challenges for scaling the examined approach. This step aligns with best practices in adopting mixed methodologies to gain a deeper understanding of the underlying motives driving observed behavioral changes (see, for instance, Ganzach & Karsahi, 1995; Hallsworth et al., 2017; Roll et al., 2019). It provides a qualitative depth that reveals the considerations, challenges and interaction of companies with tax compliance strategies, thus enhancing our understanding of the dynamics between tax authorities and businesses. Additionally, it explores the practicalities and challenges of scaling experimental results to broader applications and their welfare effects, a critical aspect in behavioral policymaking (List, 2022; Soman, 2024).

Upon receiving communication from the SRS, around one in six firms (15%) responded through a call or in writing. However, it is noteworthy that messages indicating a 5% probability of audit elicited a considerably lower response rate than other messages (see Fig. 6). Specifically, the response rate in this group was a mere 8%, which is about half the rate compared to other treatment groups. This divergence could be attributed to a perception of lower risk associated with the 5% probability of audit, making firms feel less compelled to seek immediate clarification or express concerns.

Regardless of the treatment message received, most firms did not explicitly commit to a course of future action in their communicated responses. Almost half (46%) left their future wage decisions unspecified, while about 27% of firms promised to raise wages, and an equal proportion communicated that a wage increase was impossible. We observe that the pledge



**Fig. 6** Share of companies that provided feedback on the messages they received, broken down by the treatment. *Note:* Whiskers show 95% confidence intervals



**Fig. 7** Change in the average salary from August to September 2021 by type of feedback from a company. Note: Whiskers indicate 95% confidence intervals

given by firms to the SRS regarding salary increases is not merely lip service. These firms significantly increased their average salaries compared to firms that either refrained from making such a commitment or did not respond (see Fig. 7). As we can see from this figure, the increase from one month before the treatment (August 2021) to the next was greatest for those companies from which the tax authorities received a response that was interpreted as a promise to increase the reported salary.

Companies that did not provide feedback were also above average in terms of reported salaries, while those whose main feedback was unclear or interpreted as a refusal to increase were not significantly different from the untreated group of companies. This behavior indicates that when firms can raise salaries, they are highly likely to proactively inform the SRS of this decision, thus demonstrating compliance. It aligns with the intent to minimize the probability of being audited, even when such a consequence is not explicitly outlined in the letter. Meanwhile, this pattern could represent the successful application of commitment principles in tax compliance, where a stated pledge or promise emerges as an influential mechanism to initiate behavioral change (Borrmann & Wessels, 2017; Mittone & Saredi, 2016).

In addition to the content analysis results, we were able to review the firms' written responses to explore how the communication was perceived. While the few ethnographic illustrations are just examples, they give voice to the individuals under study, showcasing diverse interpretations that our messages triggered among the recipients (see Silverman, 2011). These examples do not serve as direct empirical evidence; nevertheless, they enhance our understanding of people's economic decisions and the economic outcomes resulting from these choices (Piore, 2006).

The feedback covered a broad spectrum of responses, from gratitude for being informed about the prevailing social norms to curiosity about the SRS's reasons for contacting specific firms and even more critical viewpoints that questioned the motivations behind the outreach. These responses help illuminate how firms perceive and interpret norm enforcement, hinting at a relationship that deviates from the idealized models of cooperation often put forward in the service paradigm approach (Alm & Torgler, 2011; Lee et al., 2014) or the "service and customer" relationship (Kirchler, 2007). We identify two aspects that are

important to this study in understanding norm enforcement in taxation, which we consider when calculating the net welfare effect in the proceeding section.

First, rather than a simplistic dichotomy between compliance and evasion, firms' narratives suggest that their strategic decisions often weigh the viability of continuing operations against the financial burdens imposed by tax policies, a sentiment that is common among businesses in communication with tax administrations (see Smith, 2020). This complexity is reflected in communications with the tax authority, where the underlying message is not merely about tax levels but hinges on the existential threat to their business continuity in the face of increased fiscal demands. For example, one firm, upon being marked at risk for underreporting salaries, expressed dismay, citing the pandemic's adverse effects on their operations and the resultant layoffs:

[Name of the firm] received a notification from the State Revenue Service that it was included in the envelope salary risk group. A company board member admits that he was unpleasantly surprised when he received such a statement. [...] Unfortunately, due to the COVID-19 situation, the number of orders significantly decreased, and our company was forced to lay off most of the employees. In conclusion, as the net turnover decreases, we have to observe a trend that soon we will no longer have the opportunity to continue our economic activity and will be forced to terminate it.

/ID 24; Condition: Trust towards SRS/

Another firm highlighted the untenability of wage increases amid declining turnover and operational losses, suggesting that additional financial strain could lead to business closure. These responses indicate not an outright denial of tax obligations but a deeper concern over the sustainability of their business under current tax structures:

Our turnover dropped dramatically during the pandemic, and we had to close several units and lay off people. There is only one small grocery store that does not yet generate sufficient profit [in order to be able] to raise wages. On the contrary, we are operating at a loss, also due to minimum wage and tax increases. There is a big risk of closing this last store and with it the whole company. So there can be no question of any salary in envelopes!

/ID: 4301; Condition: 5% audit/

Second, we notice an interesting dynamic concerning wage responsibility. Firms often shift the onus of earning above the minimum wage onto employees. In the narratives, employers frame themselves as third-party reporters of wages, effectively transferring the responsibility of reaching or exceeding the average wage to workers. This is visible in the quote from ID 61, which attributes the shortcoming of not earning specified contract wages to the workers' lack of responsibility or commitment:

Unfortunately, in our business sector, we are facing a shortage of qualified workers; the company lacks workers who would fulfil their duties honestly; part of the existing workforce is a bunch of people who do not come to work and therefore do not earn the wages specified in the contract. [...] "Stable" employees, who regularly and honestly perform the duties stipulated in the employment contract, have adequate wages above the minimum wage set by the state; this can be seen in the submitted reports on labour taxes.

/ID 61; Condition: Social norm/

This response underscores a perspective in which employers perceive their role as merely reporting the wages that employees have earned through their hard work and dedication. It

absolves firms of responsibility for low average wages, shifting it instead onto employee performance and work ethics. Furthermore, these narratives reveal another compelling pattern: firms' strict adherence to the bare legal requirements regarding wages. Employers frequently emphasize their compliance with minimum wage laws while simultaneously downplaying any obligation towards ensuring or increasing average wages. This can be seen in the case of ID 5640, in which the writer proudly affirms compliance of its firm with the law and prompt tax payments but subtly avoids committing to any wage increase:

As a member of the board of [Name of the firm], I am happy to be able to still pay the minimum wage set by the law, and, so far, I have not delayed the tax payments.

/ID 5640; Condition: Combating the shadow economy/

The explicit adherence to the legally set minimum wage and the sidestepping of average wage discussions underscore an interpretation of tax compliance rooted in a legal obligation. As expressed in responses to the SRS, this legal-centric mindset indicates that norm enforcement could fall short without explicit legal requirements.

Overall, incorporating the nuanced insights from firms' feedback into our analysis sheds light on the complex dynamics between tax authorities and businesses. Although the short-term fiscal effect of our messaging strategy indicated a promising path to enhanced tax revenues, the qualitative feedback reveals notable challenges. Messages were sometimes interpreted as coercive, even if they did not include threats of audit, leading to negative perceptions among firms. Negative feelings, such as anger, are common in taxation-related matters, especially among small businesses, and significantly affect tax compliance decisions (Enachescu et al., 2019; Kamleitner et al., 2012). In the responses to the tax administration, many express discontent about being suspected of tax evasion in the very first sentences:

After reading your message, I was devastated that I had to read words such as envelope wages. I can say that we are one of those who pay all taxes and don't use envelopes. You can ask any employee if there are envelope wages. Let us respect honest people and not put everyone in the same bag as dishonest. I hope for your understanding.

/ID 2667; Condition: Social norm/

The gap between the intention behind the nudges, which should be subtle and mimic the service paradigm, and their reception, underscores the delicate balance needed in communication strategies. Interestingly, the intervention involving a 5% probability of audit elicited significantly less feedback, suggesting that it was an optimal strategy to minimize negative reactions while improving compliance. Had we not analyzed the feedback in depth, we might have incorrectly concluded that the baseline or prosocial messages should be implemented as the communication strategy with firms, as they deliver the same fiscal effect as the interventions with audit probabilities. However, the findings of this subsection highlight the 5% audit probability message as the most effective, as it not only delivers improved tax revenues but also has the potential to minimize negative feedback.

## 6 Conclusions and discussion

This study used a natural setting to conduct an experimental study and investigate the behavior of businesses with the risk of engaging with the shadow economy. The study revealed that normative appeals effectively catalysed change, prompting many firms to

reassess their wage policies. Particularly noticeable was the efficacy of this approach among smaller firms, which are often more challenging to audit and more likely to engage in the shadow economy (Gavoille & Zasova, 2022; Kleven et al., 2016). Our findings hint at the potential effectiveness of these interventions in curbing tax evasion and addressing wage underreporting. Although the subject of our study represents a minority group in terms of labour tax payments, the insights obtained have wider implications for a broader taxpayer base.

By informing firms about their standing in relation to industry peers concerning average salary levels, we were able to stimulate behavioral change. The response was significant, with a 3% improvement in the average salary level among firms receiving such messages compared to those who did not. Our findings underscore the effectiveness of using social comparisons in messaging strategies to nudge firms towards better compliance. Contrary to concerns that pressuring firms to increase salaries could push them into the shadow economy, we found no evidence to support this. Our interventions did not lead to significant changes in declared turnover or number of employees, and did not increase claimed tax deductions.

At the same time, we found that a significant number of businesses start active communication with the tax administration after receiving a message. It allowed us to critically examine whether and how messages promote the “service” paradigm in the daily operations of the tax administration and foster tax morale (Alm & Torgler, 2011). Our qualitative analysis of the feedback reveals that many businesses perceived the delivered message as coercive. Others do not consider tax compliance a field where normative appeals are at play and respond that only legal requirements, such as minimum wage, must be followed. Notably, integrating any of the prosocial messages did not significantly improve tax compliance compared to the baseline message. Instead, such messages evoked responsiveness comparable to those with an audit probability of 33% or more. Yet, introducing a minimal (5%) audit probability appeared to mitigate these unintended repercussions, at least with respect to calls and emails to the tax administration.

We have also conducted a detailed welfare analysis, evaluating the costs and benefits of each intervention based on Keen and Slemrod (2017) and its application in Meiselman (2018). Our analysis incorporates the impact of negative feedback from firms into the marginal social value of public spending, accounting for the broader societal implications of taxpayer dissatisfaction. By including this dynamic element, our welfare analysis extends beyond fiscal outcomes, illustrating how different strategies impact both economic and social dimensions of tax compliance. Our findings indicate that the prosocial and 5% audit treatments deliver positive net welfare effects in the highest number of scenarios (see Appendix F). Prosocial messages are effective due to the cost savings for the tax agency not conducting extensive audits and for firms responding to such audits. In the case of the 5% audit treatment, the combination of low audit levels and low levels of feedback from firms contributes to its positive net welfare effect.

Our study provides more than just immediate outcomes; it uncovers the long-term effectiveness of the interventions. Initially, the messages showed a persistent increase in average salaries for the first five months. However, this trend diminishes over time, though the overall levels remain higher than in the control group. Notably, our results indicate no significant long-term differences between various types of messages, suggesting that while the initial impact of the messages is substantial, the specific content—whether audit warnings or normative appeals—does not result in distinct long-term compliance behavior. This highlights the overall effectiveness of sending messages to improve compliance, rather than the nature of the message itself.

Our adaptation of the optimal enforcement condition reveals that certain messaging strategies, particularly those with minimal audit probabilities and social norm cues, deliver positive net welfare effects. This nuanced result, especially the superior performance of the 5% audit probability treatment in balancing net welfare gains against the burdens placed on firms, provides a critical perspective for policymakers. It underscores the effectiveness of subtly calibrated threats in achieving compliance objectives while minimising negative feedback and potential resentment from the taxpayer community.

Long-term behavioral shifts, coupled with the net welfare analysis, suggest a pathway for tax authorities to enhance compliance without exacerbating the adversarial relationship with taxpayers. The balance between inducing compliance and fostering a cooperative compliance environment emerges as a pivotal consideration. Our analysis of the feedback highlights firms' varied perceptions of nudges and threats, further emphasizing the importance of carefully designed communication strategies that respect the complex dynamics of interactions between taxpayers and tax administration.

Although our results indicate a clear direction, they also raise several intriguing questions for future research. For instance, it remains unclear to what extent our intervention diminished the shadow economy or motivated mostly small businesses to increase salaries at a marginal cost after updating their knowledge about the average wage levels. Our study has illuminated a pathway towards enhancing tax compliance through targeted messaging based on advanced data analytics. This strategy proved successful, not only in conveying the message but in ensuring that it was relevant and specific to each recipient. This success contrasts with the potential outcome of a more generic notification, such as saying that the firm had been flagged as high-risk for an unspecified reason or randomly assigned to a potential audit without a detailed reasoning. Such a message could have led to significant discontent and an overwhelming volume of feedback similar to what was observed in treatments without clear and transparent communication on future actions by the SRS. We therefore emphasize the paramount importance of transparent communication. It is crucial in every step of intervention: both in selecting firms for intervention and in clarifying the intended future steps of the tax authority. As such, we advocate for an approach that enhances understanding and cooperation between tax authorities and firms, thereby fostering a more compliant and equitable tax environment even if any message from the tax authority is considered as a threat.

## Appendix A. Treatment texts in the experiment

### Baseline message

Welcome! The data analysis by the State Revenue Service (hereinafter, SRS) on the submitted tax reports for the 1st half of 2021 shows that the salary in your company does not reach 70% of the average salary in the sector and region, according to the data from the Central Statistics Office (hereinafter—CSB) on the 1st quarter of 2021.

Companies whose average wages are significantly lower than the industry average in the region are at higher risk of envelope wages. Therefore, we invite you to consider the possibility of raising the salary of the company's employees in the coming months, bringing it closer to the average salary of the industry in the region, thus reducing the risk of envelope wages. Information about the average salary of the specific industry in the region is publicly available on the CSB website:



[https://data.stat.gov.lv/pxweb/lv/OSP\\_PUB/START\\_EMP\\_DS\\_DSV/DSV050c/table/tableViewLayout1](https://data.stat.gov.lv/pxweb/lv/OSP_PUB/START_EMP_DS_DSV/DSV050c/table/tableViewLayout1)

[Treatment texts for the experimental groups go here; see Table 1].

Per the regulatory enactments, the task of the SRS is to control the correctness of the calculation and payment of taxes, duties, as well as other payments determined by the state<sup>4</sup>, as well as to ensure the collection of taxes, duties and other mandatory payments determined by the state in the territory of Latvia<sup>5</sup>. To ensure the fulfilment of these tasks, the SRS monitors the economic and financial activities of any legal and natural persons<sup>6</sup>.

In case of additional questions or uncertainties, please call 67121369, write to the Electronic Declaration System or the email address.

Sincerely,

State Revenue Service

## Appendix B. Summary statistics

See Table 5.

<sup>4</sup> The law “On Taxes and Fees”, Article 18, the first part of Clause 2.

<sup>5</sup> The law “On the State Revenue Service”, Article 2, Clause 1.

<sup>6</sup> The law “On the State Revenue Service”, Article 8, Clause 8.

**Table 5** Summary statistics

Approach	No message				Baseline				Audit				Prosocial			
	N	Mean	SD		N	Mean	SD		N	Mean	SD		N	Mean	SD	Test
The average salary in 2021 (Months I–VI)	477	466.953	123.224	477	469.007	118.392	1425	472.102	116.634	1433	468.849	117.966	1433	468.849	117.966	F=0.31
The average number of employees in 2021 (Months I–VI)	477	13.055	20.817	477	14.382	27.127	1425	13.201	14.855	1433	14.179	25.631	1433	14.179	25.631	F=0.773
The average number of employees in 2020	474	11.788	13.952	471	14.699	35.961	1413	12.323	14.736	1423	13.228	19.642	1423	13.228	19.642	F=2.213*
Turnover 07 2021	467	59.233.507	84,458.432	459	74,861.996	223,052.138	1394	65,361.476	168,052.159	1392	66,135.015	178,388.861	1392	66,135.015	178,388.861	F=0.654
Turnover 06 2021	477	53.577.84	74,952.083	477	71,926.677	227,398.667	1424	60,209.494	113,180.014	1433	63,509.959	174,457.023	1433	63,509.959	174,457.023	F=1.262
Turnover 2021	477	359,244.024	513,428.105	477	453,021.202	1,523,749.682	1425	393,002.693	789,688.98	1433	400,781.927	1,065,235.834	1433	400,781.927	1,065,235.834	F=0.746
Turnover 2021	471	566,930.051	821,582.87	470	690,167.566	1,989,123.096	1409	594,757.566	999,664.786	1416	625,193.373	1,262,566.007	1416	625,193.373	1,262,566.007	F=0.96
Employees in 07 2021	477	13.597	23.402	477	14.532	27.864	1425	13.688	15.036	1433	14.665	27.428	1433	14.665	27.428	F=0.563
Employees in 06 2021	477	13.899	28.548	477	14.849	27.937	1425	13.703	15.944	1433	14.742	27.115	1433	14.742	27.115	F=0.585
The average salary in 07 2021	477	507.376	129.725	477	512.789	130.942	1425	512.589	128.272	1433	507.225	129.349	1433	507.225	129.349	F=0.551
The average salary in 06 2021	477	479.67	134.119	477	478.809	130.905	1425	480.75	129.816	1433	477.641	127.886	1433	477.641	127.886	F=0.14
The average salary in 05 2021	477	470.833	135.285	477	467.826	127.062	1425	476.561	129.872	1433	472.877	132.323	1433	472.877	132.323	F=0.632
The average salary in 04 2021	476	461.492	140.471	476	464.902	127.129	1422	469.968	133.972	1431	468.633	136.493	1431	468.633	136.493	F=0.561
Taxes paid in 2021 (Months I to VI)	477	39,528.636	99,998.581	477	45,098.647	115,137.769	1425	37,485.602	64,430.662	1433	43,905.092	167,391.65	1433	43,905.092	167,391.65	F=0.86
Taxes paid in 2020	476	52,744.918	121,710.48	474	70,344.074	235,321.408	1418	54,074.926	102,632.105	1426	60,908.065	241,924.584	1426	60,908.065	241,924.584	F=1.136
Grants received	477	4531.385	17,720.359	477	5058.344	23,285.134	1425	4445.52	16,550.296	1433	4204.143	19,904.871	1433	4204.143	19,904.871	F=0.247
Region	477			477			1425			1433			1433			X <sup>2</sup> =0.438
Kurzeme	51	10.7%		50	10.5%		150	10.5%		149	10.4%		149	10.4%		

Table 5 continued

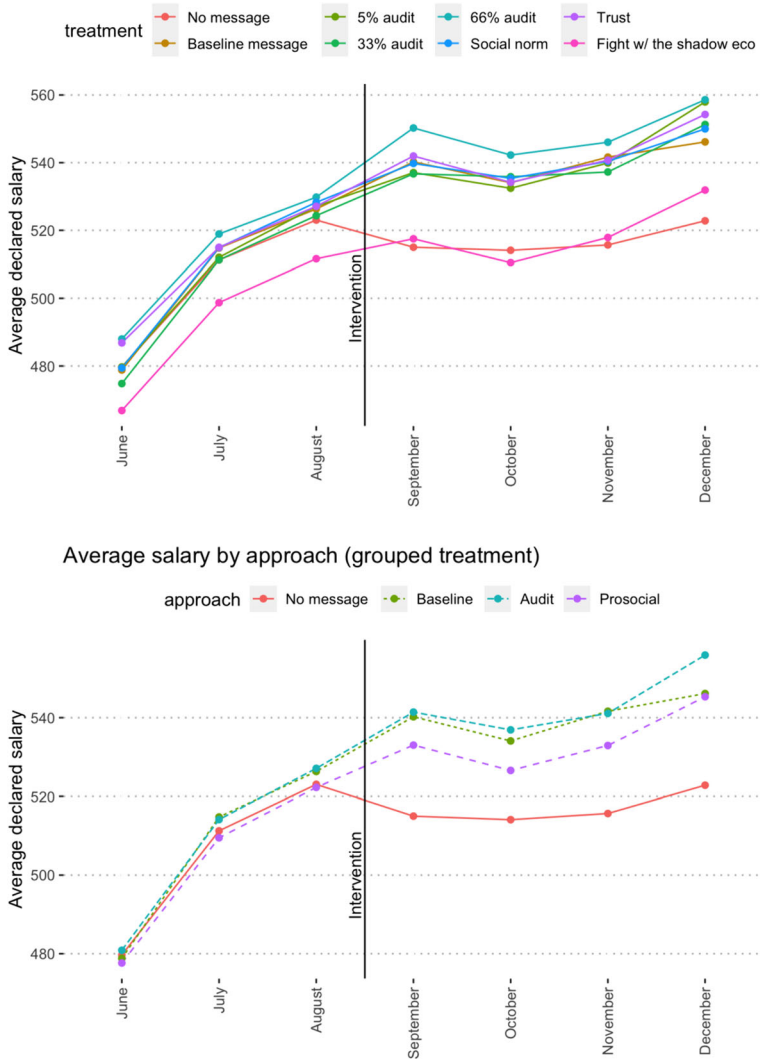
Approach	No message			Baseline			Audit			Prosocial		
	N	Mean	SD	N	Mean	SD	N	Mean	SD	N	Mean	SD
Latgale	53	11.1%		51	10.7%		149	10.5%		149	10.4%	
Pienīga	86	18%		87	18.2%		263	18.5%		267	18.6%	
Rīga	199	41.7%		201	42.1%		605	42.5%		605	42.2%	
Vidzeme	39	8.2%		39	8.2%		116	8.1%		119	8.3%	
Zemgale	49	10.3%		49	10.3%		142	10%		144	10%	
Industry	477			477			1425			1433		
												X <sup>2</sup> = 30.058**
Retail	111	23.3%		114	23.9%		329	23.1%		321	22.4%	
Construction	96	20.1%		123	25.8%		249	17.5%		272	19%	
Manufacturing	77	16.1%		80	16.8%		253	17.8%		268	18.7%	
Agriculture	40	8.4%		39	8.2%		133	9.3%		132	9.2%	
Transport & Logistics	26	5.5%		19	4%		81	5.7%		61	4.3%	
Services	76	15.9%		75	15.7%		228	16%		236	16.5%	
Other	51	10.7%		27	5.7%		152	10.7%		143	10%	
Grants received (binary)	477	0.119	0.325	477	0.147	0.354	1425	0.139	0.346	1433	0.123	0.328 F=1.07
Age of the firm	477	15.407	8.965	477	14.804	8.61	1425	15.23	8.932	1433	15.387	8.862 F=0.566
Ratio to the average salary in the industry & region in 2021 (months I to VI)	477	0.458	0.129	477	0.457	0.126	1425	0.464	0.132	1433	0.459	0.13 F=0.57
Ratio to the average salary in the industry & region in July 2021	477	0.5	0.146	477	0.502	0.147	1425	0.505	0.151	1433	0.497	0.146 F=0.631
Wave	477	2.01	0.819	477	2.004	0.817	1425	2.001	0.818	1433	1.999	0.818 F=0.027
The average salary in June 2021	477	479.67	134.119	477	478.809	130.905	1424	480.874	129.778	1433	477.641	127.886 F=0.151
The average salary in July 2021	467	511.269	127.129	459	514.788	129.121	1394	514.132	127.251	1392	509.494	127.293 F=0.387

Table 5 continued

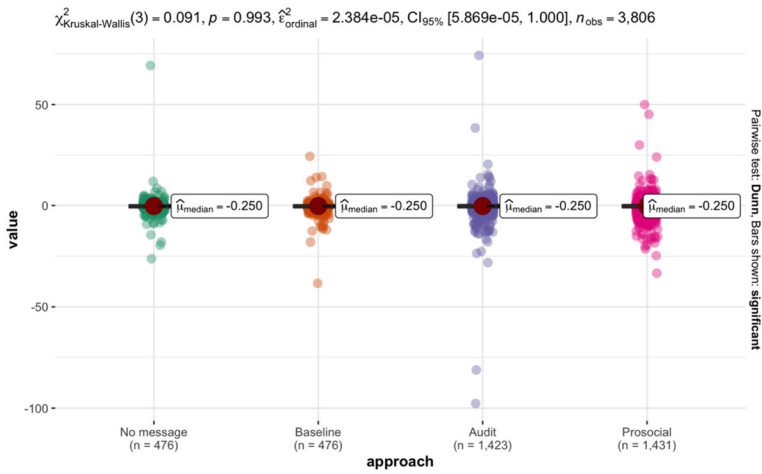
Approach Variable	No message			Baseline			Audit			Prosocial			Test
	N	Mean	SD	N	Mean	SD	N	Mean	SD	N	Mean	SD	
The average salary in August 2021	467	523.031	137.333	459	526.307	144.65	1398	527.096	139.89	1401	522.259	136.215	The average
The average salary in September 2021	476	515.008	134.522	475	540.227	152.641	1421	541.411	152.669	1431	533.009	151.385	F = 3.956***
The average salary in October 2021	467	514.104	145.729	457	534.069	153.666	1381	536.889	152.239	1385	526.571	147.822	F = 3.072**
The average salary in November 2021	466	515.675	143.367	460	541.629	160.942	1391	541.085	161.419	1396	532.914	162.698	F = 3.291**
The average salary in December 2021	470	522.816	150.706	474	546.131	176.831	1407	555.93	176.751	1418	545.321	197.733	F = 3.931***

## Appendix C. Statistical Analyses by Periods and Treatment Effects

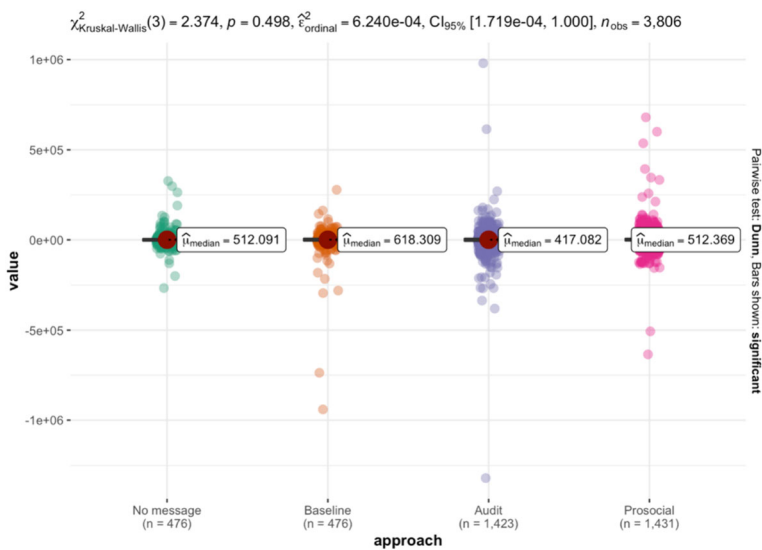
See Figs. 8, 9, 10, 11 and Tables 6 and 7



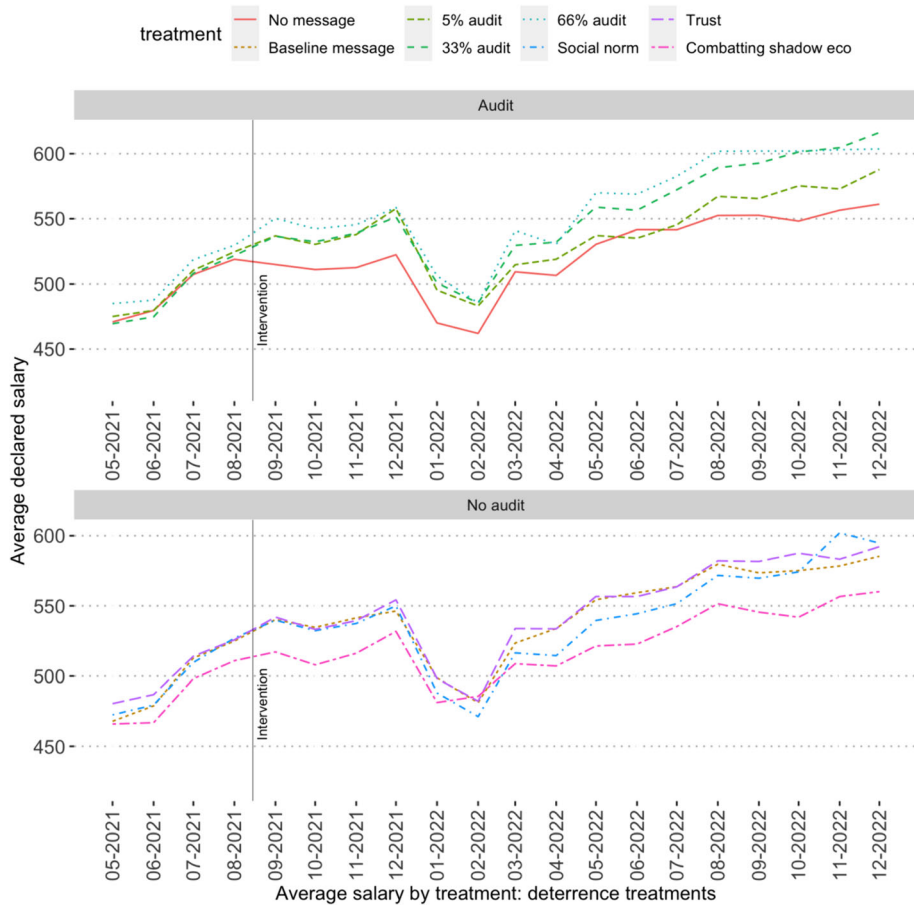
**Fig. 8** Average absolute values of declared salaries per month (in €)



**Fig. 9** Difference in number of employees (change from June–August to September–December)



**Fig. 10** Differences in declared turnover (change from June–August to September–December)



**Fig. 11** Long-term consequences of treatment intervention: data for 2021–2022



**Table 6** Non-parametric Kruskal–Wallis tests for difference across approaches in number of employees (change from June–August to September–December)

Group1	Group2	n1	n2	Kruskal–Wallis H statistic	<i>p</i>	<i>p</i> .adj	<i>p</i> .adj.signif
No message	Baseline	477	477	113,372.0	0.984	0.984	ns
No message	Audit	477	1425	336,451.5	0.830	0.984	ns
No message	Prosocial	477	1433	339,804.5	0.941	0.984	ns
Baseline	Audit	477	1425	336,028.5	0.798	0.984	ns
Baseline	Prosocial	477	1433	339,487.5	0.916	0.984	ns
Audit	Prosocial	1425	1433	1,022,307.0	0.850	0.984	ns

Dependent variable: number of employees per company in a single month; \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$ ; ns:  $p > 0.1$

**Table 7** Pairwise Wilcoxon tests for the difference in means of declared turnover (change from June–August to September–December)

Group1	Group2	n1	n2	Wilcoxon rank-sum test statistic	<i>p</i>	<i>p</i> .adj	<i>p</i> .adj.signif
No message	Baseline	477	477	109,764.0	0.406	0.676	ns
No message	Audit	477	1425	340,419.0	0.866	0.866	ns
No message	Prosocial	477	1433	332,736.0	0.451	0.676	ns
Baseline	Audit	477	1425	350,679.0	0.246	0.676	ns
Baseline	Prosocial	477	1433	343,184.5	0.802	0.866	ns
Audit	Prosocial	1425	1433	990,305.0	0.206	0.676	ns

Dependent variable: average monthly declared turnover per company: pre- vs. post-treatment; \* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$ ; ns:  $p > 0.1$

## Appendix D. Analysis of the feedback

See Tables 8, 9 and 10

**Table 8** Mean differences in declared salary by response and treatment

Approach	No response	No promise	Will not increase	Will increase
No message	13.34576	NA	NA	NA
Baseline	36.43133	13.52687	24.29079	39.53743
Audit	37.79587	26.49876	25.76213	62.05795
Prosocial	30.71394	25.68260	46.00127	51.13736

**Table 9** Population size by response and treatment

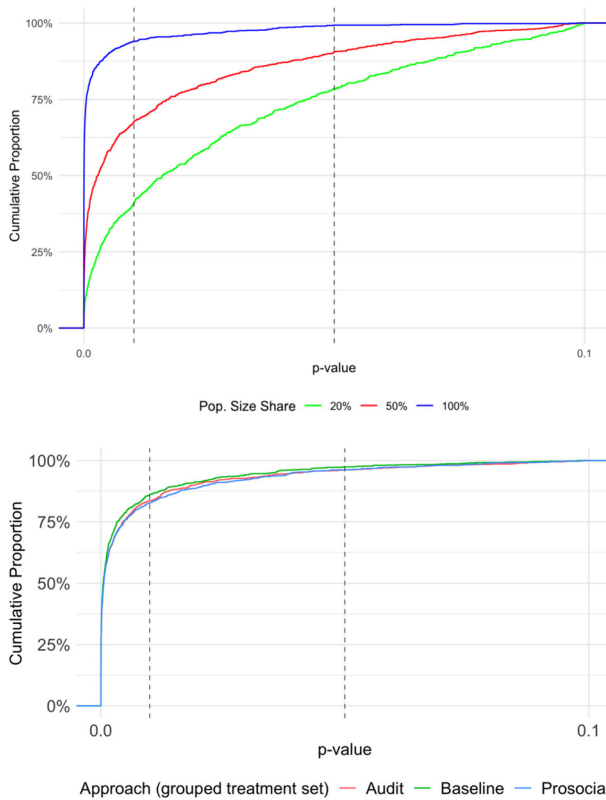
Approach	No response	No promise	Will not increase	Will increase
Baseline	392 (82%)	40 (8%)	13 (3%)	32 (7%)
Audit	1250 (88%)	94 (7%)	46 (3%)	35 (2%)
Prosocial	1193 (83%)	96 (7%)	76 (5%)	68 (5%)
Total	2835 (85%)	230 (7%)	135 (4%)	135 (4%)

**Table 10** Wilcoxon test for difference in declared salaries (Before vs. After treatment) by response type

Estimate	Group1	Group2	n1	n2	Statistic	<i>p</i>	<i>p</i> .adj	<i>p</i> .adj. signif
7.357488	No response	No promise	2835	230	348,742	0.071000	0.212	ns
2.861858	No response	Will not increase	2835	135	196,055	0.605000	0.992	ns
−17.134553	No response	Will increase	2835	135	160,615	0.002000	0.009	**
−4.753350	No promise	Will not increase	230	135	14,862	0.496000	0.992	ns
−24.071999	No promise	Will increase	230	135	11,914	0.000207	0.001	**
−20.495996	Will not increase	Will increase	135	135	7472	0.011000	0.042	**

## Appendix E. Post-hoc statistical power calculations

See Fig. 12  
and Table 11.



**Fig. 12** Simulations results. *Note:* Simulations were run in R (see the OSF repository for the replication code). For the pooled data (treated vs untreated), we draw the effect size (20.68206), the baseline average salary (502.5818), monthly trend (19.36584) and treated ( $N=3,282$ ), and untreated ( $N=470$ ) population sizes from the real data. Similarly, we drew the number of treated per approach from the real data for the subgraph with approaches

**Table 11** Simulations results

Population share	$p$ value < 0.01 (%)	$p$ value < 0.05 (%)	
0.2	24	46	
0.5	60	80	
1.0	93	98	
Approach (grouped treatments set)	$p$ value < 0.01 (%)	$p$ value < 0.05 (%)	N
Audit	81.2	93.4	1402
Baseline	83.7	94.8	472
Prosocial	79.8	92.8	1408

The percentage of simulations showing p-values below the threshold values (0.01 and 0.05) are shown. For the per-approach simulations, full sample sizes per each approach group were used, while for the pooled data, we tested 20%, 50% and 100% population sizes

## Appendix F. Expected welfare effects

In this appendix, we estimate the direct expected welfare effect of our interventions on tax compliance, incorporating the approach from Keen and Slemrod (2017) and its application in Meiselman (2018). This estimation leverages an optimal enforcement condition, guiding when a tax authority's actions—specifically, the dissemination of communication to potential non-filers—result in a net positive change in societal welfare. The condition is formalized as follows:

$$\phi [\Delta \text{Revenue} - \Delta \text{Administrative cost}] - \Delta \text{Private cost} > 0.$$

Here,  $\phi$  symbolizes the marginal social value of public spending, a crucial multiplier that brings the marginal revenue, after accounting for administrative costs, into alignment with privately held euros for comparison. Essentially, this parameter underscores the premise that a euro in the hands of tax authorities has a greater social value than one retained privately, necessitating  $\phi > 1$ . The specification simplifies by assuming the marginal utility of a euro remains constant across taxpayers, and similarly, the marginal social value of public expenditure does not vary. The specification also does not account for the potential additional profits that might be realized from conducting audits. Nevertheless, the specification succinctly captures the dual appearance of foregone consumption—both as a gain in revenue and as a part of the private cost—highlighting the balance between the fiscal benefits of tax collection and the incurred costs on both public and private fronts. This methodology allows us to quantitatively assess the welfare implications of our messaging strategy, considering the broader impacts beyond mere fiscal outcomes.

We calculate the increase in declared labour taxes by examining the data from September to December and comparing it to the baseline figures from August. This surplus is then adjusted by subtracting the corresponding increase observed in the control group ('No message' condition) over the same period. This adjustment allows us to distinctly attribute any observed changes in tax compliance directly to our intervention, thereby isolating the differential impact of each messaging strategy.

To estimate the costs of the SRS running the experiment, we rely on the fact that the preparation of the data file, an effort requiring minimal resources, was completed within a single working day, leading us to assign a negligible cost to this activity. Sending a message via the system is free of charge and therefore not included in the calculations. The effort to address firms' feedback was estimated to consume approximately four hours, while audits were projected to require a more substantial commitment of 40 working hours. The hourly rates for these activities were derived from the SRS's publicly available salary data as of January 2022, which lists the average salaries for data scientists and senior tax inspectors, inclusive of all labour-related taxes.

For the private costs incurred by firms, we reference the hourly costs associated with legal and accounting services, pegged at €11.87 in the first quarter of 2022. This estimate assumes that responding to a letter or undergoing an audit demands four and forty working hours, respectively, from both the tax administration and the firm involved. Moreover, as suggested by the feedback analysis, we introduce a dynamic element to the marginal social value ( $\phi$ ) to account for the potential negative impact on the SRS and public administration in general. Specifically, we adjust  $\phi$  by deducting the proportion of feedback received in each experimental condition, thereby quantifying the broader social implications of taxpayer discontent.

Our analysis reveals that, except for treatments involving high audit probabilities (33% and 66%), all intervention strategies demonstrate positive net welfare effects in case of the

**Table 12** Net welfare (in euros) per firm with the marginal social value of public spending ( $\phi$ ) at 1.5 level

		Baseline	5% audit	33% audit	66% audit	Social norm	Trust	Combating shadow eco	Audit	Prosocial	All treatments
Labour tax cumulative profit	[1]	509.33	519.19	495.25	452.12	594.76	324.16	178.80	488.85	365.91	439.09
Data preparation	[2]	0.02	0.02	0.02	0.02	0.02	0.02	0.02	0.02	0.02	0.02
Processing responses	[3]	7.17	3.30	5.61	5.94	6.69	7.05	6.54	4.95	6.76	6.04
Auditing	[4]	0	19.90	131.36	262.71	0	0	0	137.99	0	59.14
Net revenue	[5]=[1]-[2]-[3]- [4]	502.15	495.96	358.27	183.45	588.05	317.10	172.25	345.89	359.13	373.89
Social value	[6]= $\phi$ x [5]	662.79	702.78	486.88	247.80	783.22	419.52	230.09	476.36	478.54	504.79
Private cost	[7]	517.69	546.78	658.48	772.41	602.56	332.37	186.42	636.75	379.42	510.31
Net welfare	[8]=[6]-[7]	<b>145.10</b>	<b>156.00</b>	-171.59	-524.60	<b>180.67</b>	<b>87.15</b>	<b>43.67</b>	-160.38	<b>99.12</b>	-5.52

Positive values are in bold

**Table 13** Net welfare (in euros) per firm under alternative assumptions

Marginal social value	Baseline	5% audit	33% audit	66% audit	Social norm	Trust	Combating shadow eco	Audit	Prosocial 1	All treatments
$\phi=4.5$	<b>1651.54</b>	<b>1643.89</b>	<b>903.21</b>	<b>25.74</b>	<b>1944.82</b>	<b>1038.44</b>	<b>560.43</b>	<b>877.30</b>	<b>1176.52</b>	<b>1116.14</b>
$\phi=1.5$	<b>145.10</b>	<b>156.00</b>	-171.59	-524.60	<b>180.67</b>	<b>87.15</b>	<b>43.67</b>	-160.38	<b>99.12</b>	-5.52
$\phi=1.4$	<b>94.89</b>	<b>106.41</b>	-207.42	-542.95	<b>121.86</b>	<b>55.44</b>	<b>26.45</b>	-194.97	<b>63.21</b>	-42.91
$\phi=1.3$	<b>44.67</b>	<b>56.81</b>	-243.25	-561.29	<b>63.06</b>	<b>23.73</b>	<b>9.22</b>	-229.56	<b>27.29</b>	-80.30
$\phi=1.2$	-5.54	<b>7.21</b>	-279.07	-579.64	<b>4.25</b>	-7.98	-8.00	-264.15	-8.62	-117.69
$\phi=1.1$	-55.76	-42.38	-314.90	-597.98	-54.55	-39.69	-25.23	-298.74	-44.53	-155.08

Positive values are in bold

marginal social value of 1.5, as shown in Table 12. The notable costs associated with conducting audits largely account for the negative welfare impacts of the higher audit probability treatments. Among the more effective strategies, the social norm treatment emerges as the most beneficial, generating a net welfare of €180.67 per firm, closely followed by the 5% audit treatment, which achieves a net welfare of €156 per firm. This advantage of the 5% audit treatment is partly attributable to its significantly lower incidence of negative feedback compared to other treatments. It is noteworthy that the 5% audit also incurs lower private costs than the social norm treatment, posing a critical challenge for policymakers: how to optimally balance net welfare improvements against the increased burdens these strategies impose on firms.

Adjusting the marginal social value of public funds to 1.2 modifies the calculus significantly (see Table 13). Under these conditions, the 5% audit treatment emerges with a net welfare effect of €7.21 per firm, surpassing the social norm treatment, which registers at €4.25 per firm. This recalibration underscores how feedback from firms can influence the net welfare outcomes, emphasising that the effectiveness of a treatment is not solely its direct fiscal impact but also the subsequent feedback it generates. In this refined analysis, the preference shifts towards the 5% audit, which sustains positive net welfare effects when the marginal social value is relatively small.

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## Declarations

**Conflict of interest** None reported.

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