How to Improve Small Firms' Payroll Tax Compliance? Evidence from a Randomized Field Experiment

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Abstract: Payroll tax evasion by firms is widespread and threatens the functioning of welfare systems in many countries around the world, yet very little is known about how to combat it. We report results from a large-scale RCT testing strategies to improve payroll tax compliance of small firms in a middle-income country. We randomize announced audit probabilities (1%, 10%, 40% or 60%) on the firm level and implement several novel types of moral appeals (varying information on the benefits of tax-financed public goods). Our monthly tax return data show that both deterrence and moral measures significantly improve payroll tax compliance. A high audit probability thereby generates 50% more additional tax revenue than moral appeals. An additional treatment with ambiguous audit probability shows that behavioral factors (such as probability neglect) play a minor role for increasing compliance in the deterrence treatments.

KEYWORDS. (Payroll) Tax Compliance, Firms, Audits, Moral Appeals, RCT.

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1. Introduction

To safeguard the functioning of society, high levels of tax compliance are important and improving tax compliance is therefore a key objective of governments and tax authorities around the globe (Slemrod, 2019, De Neve et al., 2021, Floyd et al., 2022).[1] While the existing tax compliance literature focuses on income taxes (personal income taxes of individuals and, to a lesser extent, business taxes) and VAT, recent research documents the prevalence and economic importance of payroll tax evasion in the form of underreported wages (Kumler et al., 2020, Feinmann et al., 2022). Payroll taxes (or social security contributions, henceforth *SSC*) are a function of employees' salaries and employers are responsible for remitting the tax. Payroll tax evasion plausibly occurs through a strategy where employers and employees collude and agree that employees' salaries are (partially) paid out in cash and remain unreported. The prevalence of such "payments under the table" (sometimes also referred to as "envelope wages") is particularly worrying given that payroll taxes are the second largest source of tax revenue in many countries (Eurostat, 2022). Despite its importance, literature on payroll tax evasion – and how to fight it – is very scarce.

There are at least two critical factors that underscore the importance of research exploring strategies to reduce payroll tax evasion by firms, even in the presence of existing research on individual taxpayers and other types of taxes. First, successful payroll tax evasion depends on the discretion of two parties (i.e., employers and employees) and reduces the tax burden of both parties, thereby aligning their incentives to remain silent and making fraud difficult to detect. The incentives and enforcement challenges are therefore different from those in other tax evasion situations, such as income tax evasion or VAT evasion. In the VAT context, for example, the incentives of sellers and buyers are misaligned and a built-in incentive structure exists that generates a third-party reported paper trail (Pomeranz, 2015, Naritomi, 2019). There is no such self-enforcing mechanism for payroll taxation, and its collusive nature undermines the power of third-party reporting (Paulus, 2015, Slemrod, 2019, Bjorneby et al., 2021).[2]

Second, payroll taxes have specific characteristics that do not apply to other types of taxes. Firms face a trade-off in their compliance decision: reporting lower wage costs saves payroll taxes, but is not advantageous for the profit tax burden, because unreported wages cannot be deducted from the profit tax base (see our theoretical intuition which is the first to model this trade-off). In addition, payroll taxes are "benefit taxes", where contributions paid today affect the benefits workers receive later, implying that they face a trade-off between between immediate and future income. Employees typically underestimate the loss of future benefits, and the untaxed portion of salary is usually not invested elsewhere for the future (as our follow-up survey shows, see Appendix

[1] Based on randomized audits, the most reliable approach to estimate evasion, the IRS (2016) reports a tax gap of 406 Billion USD for the US, corresponding to a share of 16.3% of tax liability that will never be remitted.

[2] In the context of personal income taxes, evasion is at the discretion of the employee and the employer does not directly benefit from employee evasion, implying that third-party reporting is sustainable and effective in preventing evasion (Kleven et al., 2011, Best, 2014).

G). Specifically, a notable 82% of employees receiving cash salaries have no financial assets and underestimate the potential net loss from payroll tax evasion by a factor of two to three. To summarize, because of these differences, existing studies of other types of taxes may provide limited insights into payroll tax evasion.

In this paper, we address the gap in the literature. We are the first to provide causal evidence from a RCT on how to reduce payroll tax evasion by small firms and how to break up collusion between employers and employees. We conducted a large-scale, preregistered randomized control trial (RCT) in cooperation with the federal tax authority of Bulgaria (National Revenue Agency, henceforth *NRA*) to explore how strategies that go beyond third-party reporting (Kleven et al., 2011, IRS, 2016) and specifically target firms can break up collusion and increase payroll tax compliance. We target firms in our experiment, because they play a key role in this context. First, remitting about 85% of all taxes (Milanez, 2017, Slemrod and Velayudhan, 2018), they are important, but understudied, actors in most tax evading situations. In addition, firms, especially small firms, are generally very difficult for tax authorities to penetrate (Slemrod, 2019). Second, firms are likely the initiators of collusive payroll tax evasion (as suggested by our survey evidence in Appendix G).

We conduct our experiment in Bulgaria, an EU country where approximately half of all firms are estimated to be at risk of engaging in collusive payroll tax evasion (see Williams and Horodnic, 2017 and Section 2.1). Bulgaria belongs to the majority of countries world-wide (108 out of 195) which are classified as middle-income countries by the World Bank. These countries are often characterized by weaker enforcement opportunities compared with developed, high-income, countries and face large challenges related to tax compliance (e.g., Carrillo et al., 2017). Compliance challenges in these countries are presumably linked to the prevalence of cash based transactions (Slemrod and Weber, 2012) and the relevance of small firms. For example, in Bulgaria, firms with less than 10 employees present 92% of all firms and SMEs employ 76% of the workforce (EU Commission, 2019).

In our RCT, we combine actual enforcement strategies and different communication treatments within the same population of firm taxpayers. Relying on the universe of VAT-registered small- and medium-sized firms in Bulgaria (about 172,000 firms), we randomly assign firms to three main groups. First, a control group of 10,000 firms which receive a placebo-type message conveying neutral information about the tax filing process. Second, 18,600 firms receive deterrence measures informing about the probability of becoming subject to an audit. The communicated audit probability randomly varies on the firm level and firms receive an audit with a probability of 1%, 10%, 40%, or 60%. Importantly, the tax authority truthfully implemented audits according to the audit probabilities. Third, 40,000 firms randomly receive one of four different moral appeal messages mentioning the role of tax revenue for financing public goods and emphasizing that taxpayers receive something in return for their tax money and social security contributions. The messages vary in the intensity with which they appeal to taxpayers' willingness to cooperate in the 'fiscal exchange' situation between taxpayers and the government (Buchanan, 1976), and they are informed by the behavioral economics literature showing that emphasizing the individual utility of pro-social behavior and cooperation increases pro-social behavior (e.g., Fisher et al., 1995, Chen et al., 2021). The

remaining firms are randomly assigned to either receive an invitation to participate in a survey studying firms' attitudes and beliefs about tax evasion (10,000 firms) or remain untreated (about 93,000 firms).[3] We conducted an additional follow-up survey to complement our RCT and to enhance our understanding of the determinants and perceived costs of payroll tax evasion (5000 firms and roughly 500 employees were invited to participate, see Appendix G for details).

Using administrative monthly tax return data provided by the NRA, we show that *both* deterrence messages and moral appeals can induce firms to break up collusion and improve payroll tax compliance relative to the control group. The treatment effect of stating a high audit probability (of 60%) is thereby approximately twice as large than the effect in the moral appeal treatment with the highest effect on compliance (USD 353 vs. USD 165, or 3% vs 5%, in additional monthly collected payroll tax compared with the control group). The treatment effects of our low audit probability and moral appeal treatments build up in the four months after the intervention, and diminish subsequently. These dynamics suggest that firms in these treatments do not make any permanent changes in response to low threat deterrence measures and moral appeals, but make temporary adjustments in the wake of the treatment reception (e.g., declaration of salaries or registering additional employees for a few months and returning to old habits subsequently). The effects of announcing high audit probabilities (i.e., 40% and 60%) are more persistent, indicating that high threats of deterrence measures have a more sustainable effect on payroll tax compliance. In the context of moral appeals, we find that the simplest form of our cooperation message has the most persistent effect.

Our experiment substantially boosted payroll tax revenue. A simple back-of-the-envelope cost-benefit analysis that accounts for the costs of the experimental interventions suggests that the experiment generated a return of about USD 298 per deterrence letter and USD 351 per moral appeal letter. Our most successful deterrence treatment stating high audit probabilities generated about 50% more in tax revenue per letter compared with our most effective moral treatment (USD 764 vs. USD 497). In total, our experimental interventions generated USD 10,856,280 in additional payroll tax revenue. To put this number into (a Bulgarian) perspective: the additional revenue generated in our experiment corresponds to closing 5% of the estimated revenue gap from payroll tax evasion (see Williams and Horodnic, 2017) and can fund the pensions of 5,210 Bulgarians over one year. Our estimates of the revenue effects are likely to be lower bounds because they do not take into account that higher payroll tax compliance also positively affects the income tax base reported by employees.

We show that our treatment effects are mostly driven by the larger firms in our sample (which are still small compared to most firms in OECD countries), while small firms have very little response to our treatment interventions (consistent with findings in Holz et al., 2023). We also study heterogeneity with respect to the volatility of pre-experimental tax payments and compliance risk (as estimated by the tax authorities).

[3] Because spillovers through tax professionals are important (Battaglini et al., 2020, Boning et al., 2020), our main sample consists of 95,508 firms (incl. untreated) that directly communicate with the tax authorities – see the discussion in Section 2.4. Our results are robust to including firms which correspond with the authorities through their tax professional (see Appendix D).

Firms which are presumed to be more risky and firms which fluctuate more in their pre-experimental tax payments respond more strongly to our treatment interventions. This suggests that our treatment messages are particularly effective among the 'usual suspects'.

Increased payroll tax compliance in the deterrence treatments is potentially driven by the mere threat of an audit *and* a calculus rationale that depends on the specified audit probability. The findings in previous literature such as Bérgolo et al. (2023) suggest that firms (in a VAT context) fall victim to probability neglect. To test for probability neglect explicitly (which is not done in existing work), our experiment includes an additional treatment in which we inform the firm that it faces the risk of an audit, while leaving the exact audit probability unspecified. Benchmarking this treatment against the treatments with specified probabilities, we find that the specification of large audit probabilities has an incremental effect. This finding is consistent with standard models of deterrence, but inconsistent with probability neglect.

We expand the research frontier by focusing on i) payroll taxes and ii) the evasion decisions of (small) firms, and by iii) combining actual deterrence and different (moral) communication treatments within one sample and one approach. Evasion of payroll taxes, which is different than with other type of taxes (see above), is generally understudied in the existing compliance literature. We show that targeting one of the two parties, the employer, in the collusive evasion situation can improve compliance, implying that employers are very often the initiators of under the table payments. In addition, we show that employees underestimate the potential losses from SSC evasion and rarely invest untaxed income to provide for the future.

Research studying *firm* tax evasion is generally very scarce (not only for payroll taxes), although firms are important actors in the compliance context. Indeed, Slemrod (2019) names "The Role of Firms" in the list of "Understudied Empirical Issues" in his recent survey of the tax compliance literature. The few existing compliance RCTs with firms mostly focus on VAT (Pomeranz, 2015, Bérgolo et al., 2023), where the compliance situation is different from payroll taxes. Work on *firm* compliance is pivotal even in light of existing evidence for *individuals*, because firm decision makers plausibly make different decisions in the work domain than in the private domain (see, e.g., Cohn et al., 2014, 2017), firm evasion has a different character (for example, because compliant firms reduce their competitiveness relative to non-compliant firms), and firms are important actors in an economy in general and are often responsible for remitting taxes.[4]

[4] Holz et al. (2023) focuses on corporate taxes and individual taxes of the self-employed. Studying the role of public disclosure and exposing taxpayers to reminders that tax evasion is potentially punished with prison sentences, they have a different focus than our paper. Focusing on the Covid-19 pandemic, Karver et al. (2022) study the effect of moral appeals and information about penalties in Albania. We further relate to Bjorneby et al. (2021) who study the impact of randomly performed audits on reported wages in Norway and Kumler et al. (2020) who find that a closer tie between pension benefits and reported wages led to a relative decline in underreporting (both non-RCT settings). Studying enforcement strategies addressing firms, we also relate to papers studying the role of increased third-party reporting and withholding (Carrillo et al., 2017, Slemrod et al., 2017, Adhikari et al., 2021, Waseem, 2022), VAT cuts (Waseem, 2023), the effect of different ways of delivery of messages (Ortega and Scartascini, 2020, Doerrenberg and Schmitz, 2017, Boning et al., 2020), the effects of audits (Lediga et al., 2020, Best et al., 2021), corporate tax administration reform

The tax compliance literature focusing on *individual* taxpayers (see Antinyan and Asatryan, 2024 for an overview) studies both strategies building on the Allingham and Sandmo (1972) economics-of-crime model (e.g., Slemrod et al., 2001, Kleven et al., 2011, Dwenger et al., 2016, De Neve et al., 2021) and strategies building on the observation that non-pecuniary intrinsic motivations (e.g., civic duty or tax morale) also shape the decision to pay taxes (e.g., Torgler, 2004, Fellner et al., 2013, Luttmer and Singhal, 2014, Hallsworth et al., 2017, Frimmel et al., 2018, Bott et al., 2020, Zhang et al., 2022). In contrast to our paper, this stream of literature does not regard payroll taxes, naturally leaves aside firm taxpayers and usually does not study both deterrence and moral appeal measures within one unified setting.

We are the first to show that moral appeals (which do not affect the rationale calculus) can influence *firm* tax compliance.[5] This finding contradicts the common assumption that firms are rational profit maximizers (Friedman, 2007) whose decisions are more "rational" than those of individuals (Charness and Sutter, 2012, Kocher et al., 2020, Fochmann et al., 2021). A novelty relative to all RCTs studying the effect of moral appeals on compliance – including either individual or firm taxpayers – is that we test different moral appeals and vary information about the benefit of paying taxes between treatments.[6] Our findings suggest that simple moral appeals (which stress the importance of public goods) are likely to generate more sustainable and sizable compliance effects than moral appeals that carry more information about the benefit of SSC and taxes for the individual taxpayer. This finding helps to inform future RCT designs as well as tax authorities and governments considering the use of moral messages to increase compliance.

With regards to deterrence measures, we add the finding that measures stating a high audit probability are most effective, suggesting that behavioral factors such as probability neglect seem to play only a minor role in our context. While in line with standard deterrence theories, it contrasts recent findings for firms (in a VAT context) in the compliance literature (Bérgolo et al., 2023). An advantage over most existing compliance studies (for firms and individuals) is our setting with monthly tax declarations and monthly tax return data, which allows us to study the dynamics immediately after the intervention as well as over a longer time span. Our dynamic results suggest that the annual data used in other work might mask effects that occur immediately after the treatment.

Comparing the cost effectiveness of costly deterrence and low-cost 'soft' measures is a further contribution of our study (De Neve et al., 2021 compare nudges to standard enforcement actions for individual taxpayers; see Chan et al., 2022 for lab evidence). Because of the higher cost of audits, moral messages directed at firms appear to generate

(Basri et al., 2021), or other papers (non-RCTs) on the effect of public disclosure and shaming (Hoopes et al., 2018, Dwenger and Treber, 2022). These papers do not consider payroll taxes (with the exception of Boning et al., 2020, Kumler et al., 2020 and Bjorneby et al., 2021).

[5] We are only aware of the studies by Bérgolo et al. (2023) and Pomeranz (2015) investigating the role of moral appeals for firm tax compliance. Both studies are in a VAT context and both find very little effects on VAT remittances.

[6] The only other paper that we know of which varies moral messages is Hallsworth et al. (2017). They, however, study individuals' timely payment of taxes which is conceptually different from tax evasion.

comparable amounts of tax revenue in the months following the intervention. Stark deterrence measures, by contrast, seem to be more persistent. Tax authorities can use our findings to combat collusive tax evasion and design campaigns to increase tax revenues.

2. The Field Experiment and Data

2.1 *Institutional Context*

Our RCT took place in Bulgaria, a post-communist country in southeastern Europe. With a GDP per capita of USD 8,366 in 2017, it is the poorest member state of the EU and ranks well below the OECD (USD 37,407) and EU (USD 33,024) averages. The World Bank classifies Bulgaria as a middle-income country (LINK). Within the 108 middle-income countries world-wide, Bulgaria is an upper-middle income country (and as such compares to countries like, for example, Brazil, Mexico, Domenican Republic, Thailand or Turkey). Marked by a transition from a centrally planned to a market economy, Bulgaria faces a wide variety of structural challenges that are more pressing than in other EU member states: corruption, informal economy, distrust in public institutions, and weak administration are believed to be serious problems (see OECD, 2021). Our experimental interventions are targeted at small firms who shape Bulgaria's economy. 99% of firms are small- and medium-sized and 92% of the firms have less than ten employees. Moreover, SMEs employ 76% of the workforce and are therefore particularly relevant for remitting payroll taxes. The most important industry sectors are wholesale and retail trade and manufacturing (EU Commission, 2019).

Background on Social Security Contributions (SSC). In our study, we focus on payroll tax evasion by firms. Firms file the majority of taxes in Bulgaria. They are responsible for remitting and withholding corporate income tax, VAT and SSC, as well as personal income taxes for their employees. Bulgaria has a simplified corporate and personal income tax system with low tax rates[7] that enhances the importance of SSC. SSC payments are the second most important source of tax revenue in Bulgaria (Eurostat, 2022) and account for 30% of total tax revenues which compares to about 8% of the GDP in 2021.

SSC are contributions paid by employees and employers to finance social security benefits (i.e., pension, occupational accident, illness, common disease, maternity, unemployment, and health insurance benefits). The tax rate is around 30% and applies to an employees' gross income.[8] Employers and employees share the tax burden, with employers bearing about two-third of the contributions. Firms have to monthly report their employees' gross income and pass the SSC due on it to the tax authority (no prepayments or the such). The monthly reported gross income also serves as the basis for calculating the employee's personal income tax.

Payroll tax evasion is a prevalent problem in Bulgaria. The Bulgarian tax authority estimates an annual loss of on average BGN 440 million (about USD 220 million) from payroll tax evasion (Williams and Horodnic, 2017). To evade payroll taxes, employers

[7] A flat tax of 10% applies to corporate and personal income. The VAT system is explained in Appendix E.
[8] The exact tax rate depends on the industry and an employee's occupation.

and employees collude and underreport employees' wages, and employees usually receive part of their salary in cash. These so-called 'envelope wages' are not part of an employee's labor contract and are thus not reported to the tax authority. Both employees and employers have an incentive to remain silent about the existence of envelope wages. While employees escape their share of social security and income tax payments, resulting in higher immediate income, employers benefit from reduced payroll contributions and lower wage costs.[9] This form of underdeclaring work is a widely applied practice in Bulgaria (EU Commission, 2020). The NRA estimates 47% of employers and 58% of employees to be at risk of engaging in underdeclared work (Williams and Horodnic, 2017). In a survey among the Bulgarian population, 15% of respondents reported receiving envelope wages stating the average amount undeclared at 30% (Yang and Williams, 2017).[10]

Results from Tax Survey I. To obtain an even better understanding of the institutional context and, specifically, the beliefs and attitudes towards taxes and tax evasion of firms in our sample, we invited 10,000 randomly selected firms, which are comparable to those receiving treatment mailings, to participate in a short survey. The survey was administered by the tax authorities alongside the experimental treatment mailings to ensure that we capture attitudes and beliefs that are in place at the time of the experiment. 1,725 (17.25%) firms responded to the survey invitation and answered at least some of the questions. The survey results reveal that 85% of the small firm respondents state that tax evasion is a problem in Bulgaria. In addition, firms in our survey indicate that about 22% of the revenue is evaded in their industry. Moreover, confirming a rather low compliance rate with payroll taxes, they believe that 23% of the wage bill is not officially declared and thus not subject to SSC. Informative for the effect sizes in our deterrence treatments, we also asked participants about their belief of receiving any form of tax inspection. Realistic responses (e.g., excluding extreme values) indicate that firms belief an inspection (any form of including audits) is likely to happen with a probability of 47%. Note however, that about 40% of respondents bunched their answers at a probability of around 50% (i.e., they pushed the slider which we used to ask for perceived probabilities to the middle of the scale). We discuss the implications of our survey findings in Section 4 where we present our findings. The full results of the survey and the design are presented in Appendix F.

Results from a Tax Survey II. To get an even better understanding about payroll tax evasion, we conducted a second survey in December 2023. The survey was again addressed at firms similar to the ones in the experiment (5000 firms were randomly invited by the tax authorities; response rate 7%) but we additionally surveyed employees of SMEs (448 employees invited by the market research firm dynata, 436 completes). Confirming the results from our first survey, a large share of respondents (around 50%) report that all or

[9] Note that payroll tax evasion may reduce future income of employees, i.e., pension payments. Thus, whether evasion of payroll taxes is actually income (and utility) maximizing for the employee in the long run is not clear.

[10] These estimates are lower-bound estimates as surveys tend to underreport sensitive questions.

part of the salary is paid/received in cash and thus prone to SSC evasion. Most informative for this paper are the results on the perceived financial loss from payroll tax evasion, the answers to the question whether employees have alternative financial assets besides their state pension and answers to the questions on who usually initiates cash salaries.

We asked employees to estimate the losses incurred by evading BGN 500 (or BGN 1000) per month (about USD 250 or USD 500 respectively) over 30 years. Reports reaffirm the non-strategic nature of payroll tax evasion by employees. Respondents underestimate the true net loss (which was provided by official calculations from the tax authorities) by 300% in the case of the lower monthly evasion and underestimate the loss by almost 100% in the case of the high monthly evasion of BGN 1000. In addition, only few employees report to have financial assets or other types of investments (82% of employees who receive their salary in cash have no financial investments of any kind). This underscores our intuition that under the table cash salaries may trap employees into receiving lower future income from pensions and lower insurance against adverse life events, e.g., in case of unemployment, employees will receive lower benefits. Finally, our survey results indicate that in the majority of cases (between 54% in the employer and 57% in the employee survey) employers are the driving force behind cash salaries (in another 15%-36% of the cases they are involved in the decision). This underscores the pivotal role employers play in collusive payroll tax evasion, emphasizing the significance of targeting employers in our efforts to combat evasion. Nevertheless, it's important to acknowledge that employees also contribute to the issue, as they report to be instigators of cash salaries in about 27% of the cases. More information on the survey and the results are presented in Appendix G.

Background on activities of the tax authorities. The NRA regularly conducts tax audits and imposes sanctions. The NRA informed us that they perform about 4,000 full audits and about 10,000 checks (a lighter form of an audit which may result in a full audit) with firms in our sample each year. Thus, firms in our sample face a probability of 6% for being investigated by the tax authorities. During an audit the tax authority looks at all tax and social security liabilities of the relevant period. Detected non-compliant taxpayers are liable to a fine of up to BGN 20,000 (USD 10,000), seizing of assets or imprisonment of up to eight years. The tax authorities collect about BGN 1,000 million (USD 500 million) in unpaid taxes each year and administer fines and sanctions of about BGN 1.3 million (about USD 650,000) a year. In addition, about BGN 300 million (USD 150 million) in interest for unpaid taxes are collected.

2.2 *Treatments*

We implement one baseline (control or placebo) treatment (Section 2.2.1), four treatments appealing to the morale of paying taxes and social security contributions (Section 2.2.2) and five deterrence treatments containing information about a firm's probability of receiving an audit in the following months (Section 2.2.3). All treatment letters (original Bulgarian along with English translation) are shown in Appendix B.

2.2.1 *Baseline Condition*

Our baseline (*Baseline*) mailing acts as the control condition for the moral appeal and deterrence treatments. We implement the baseline mailing because receiving any message by the tax authorities may already affect the behavior of taxpayers (e.g., because it raises awareness of filing taxes or an alert effect where taxpayers develop the feeling to be on the radar of the authorities). Using a group of taxpayers who do not receive any message as control group may report biased results (see Fellner et al., 2013 for similar arguments).

The baseline mailing was neutrally phrased as an informative message by the tax authorities; it provided a link to a government website that is helpful for the tax filing process of SMEs and that includes information about how to facilitate tax payments. The baseline mailing referred to our variables of interest: social security payments and tax payments (including VAT). The mail greeted the taxpayer and was electronically signed by the responsible person within the tax authorities. Importantly, all treatment mailings (moral appeal and deterrence treatments) contained the identical text from the baseline mailing. The main content of the mailing reads as follows:

Baseline: *We wish to make your payment of taxes and social insurance contributions as convenient as possible. In this regard, we would like to make you aware of our website (link to website included here) where you find much information relating to your tax payments and social insurance contributions. We hope you find our online appearance useful.*

2.2.2 *Moral Appeals*

The mailings in the moral appeal (*Moral*) treatments contained the identical text as the *Baseline*, but additionally included different types of moral appeals to pay taxes and social security contributions. The appeals differ in the intensity with which we emphasize the fiscal exchange between taxpayer and government. That is, our treatments emphasize the taxpayers' benefits from tax-financed public goods and SSC (see LINK for an overview of social insurance benefits in Bulgaria).

We implemented an appeal to cooperation in the *Cooperation* treatment. In this treatment, we simply mention the benefit of paying taxes for taxpayers. We increased the reference to the benefit from paying taxes in the *Example* treatment which contained the identical text as the *Cooperation* treatment, but additionally provided specific examples of public goods funded with tax money and social security contributions (e.g., roads, health care, education, etc.). In the *Necessity* treatment, the mailing contained the identical text as in the *Example* treatment, but we addressed the taxpayer directly, i.e., by mentioning that the individual firm is important for financing public goods. Finally, the *Picture* treatment was identical to the *Necessity* treatment, but we attached a picture of a tax-financed public good (a playground) to the mailing. Each of the treatments thus only adds one additional layer of information for the taxpayer. In summary, our *Moral* treatments added the following core information to the *Baseline* mailing:

Cooperation: Text from *Baseline* + *We would also like to remind you that paying taxes and social insurance contributions is a civic duty. Taxes and social security contributions are necessary to maintain and finance publicly provided public goods and services for you and everybody in Bulgaria.*
Example: Text from *Cooperation* + *You use public transportation? You use roads and public services such as health care? You have benefited from public education? Then you know that these goods and services require funding!*
Necessity: Text from *Example* + *Without your tax payments and social insurance contributions, we are not able to maintain, for example, public schools, kindergartens, hospitals and the social insurance system.*
Picture: Text from *Necessity* + A picture from a tax-financed playground for children.

2.2.3 *Deterrence Treatments*

The deterrence treatments (*Deterrence*) likewise build on the content of the *Baseline* mailing. In addition to the information from *Baseline*, we communicated the individual probability of receiving a tax audit for a firm. The audit probability varied across the different arms of the *Deterrence* treatments. The selected firms were truthfully audited in the months following our treatment mailings. The audits that were performed in the context of our experiment had the character of a 'check', which results in a full audit if anything is detected to be suspicious.

We implemented four treatments with specific audit probabilities: 1% in *Audit 1%*, 10% in *Audit 10%*, 40% in *Audit 40%* and 60% in *Audit 60%*. In addition, we set up one treatment with a positive alas not further specified, and hence ambiguous, individual audit probability (in *Ambiguous*). The core content of the treatment mailings with a specific audit probability and with the ambiguous audit probability is presented below (**X%** represents the respective audit probability in the different treatments, i.e., 1%, 10%, 40% and 60%).[11]

Audit 1%, ***Audit 10%***, ***Audit 40%***, ***Audit 60%***: Text from *Baseline* + *We would also like to remind you that the NRA takes steps and measures such as audits to ensure an effective tax collection. In this context, the NRA has randomly selected a group of taxpayers – including you – for a special investigation. X* ***out of 100 taxpayers in this group will randomly be selected to be subject to an audit during the next months.*** *In other words, there is a X* ***% probability*** *that you will be audited.*

Ambiguous: Text from *Baseline* + *We would also like to remind you that the NRA takes steps and measures such as audits to ensure an effective tax collection. In this context, the NRA has randomly selected a group of taxpayers – including you – for a special investigation. There is a chance that taxpayers in this group will be subject to an audit during the next months.*

[11] Note that we additionally performed two audit treatments (with an announced probability of 0% and 100%) which have no predictive power (because of the limited number of 100 observations in each treatment) but were interesting for the tax authorities from an exploratory point of view.

2.3 *Outcome Variables*

Relying on administrative monthly tax return data provided by the NRA, our key outcome variable of interest is the reported firm-level tax base of social security payments (tax base of SSC). We also consider the effect of our treatment interventions on tax base of VAT (see Appendix E for more details on the VAT variable and the corresponding VAT results). In addition to studying the effects of our treatments on the VAT base, we investigate potential spillover effects of treatment-induced SSC adjustments on VAT reporting behavior (see Appendix E).[12] The unit of observation in all analyses is the firm-month. We explore changes in reported tax base of SSC in the period prior to the experiment and the period after the experiment across the treatment interventions.

2.4 *Experimental Procedure, Randomization, and Sample*

The field experiment was designed in collaboration with the NRA in Bulgaria. The experiment was pre-registered on the AEA RCT Registry (RCT ID AEARCTR-0002390). Ethical approval was granted by the University of Lausanne (the home institution of one of the authors during the design phase).

We designed the experiment taking into account the capacities (e.g., for sending the mailings and performing the audits) and practical interests from the tax authorities (e.g., interest for the effect of specific audit probabilities and for moral appeals). For our field experiment the authorities were willing to sent out 68,580 mailings, perform 2,210 audits (which would otherwise have been conducted in a non-randomized way) and invite 10,000 firms into a separate and anonymous survey (see above). The number of firms in the different treatment conditions was thus influenced by academic interest and relevance, practical importance and the capacities of the tax authority.

To design the experiment and randomize firm taxpayers into treatments, the NRA provided pseudonomized information about the universe of small and medium sized 172,172 SMEs that are registered for VAT.[13] The data comprises monthly SSC and VAT information, information about the number of employees (in categories), a firm's industry for 2016, 2017 and the beginning of 2018 (until May 2018), and its assigned risk score (between 1- low-risk and 3 - high-risk). The data was provided in two batches. We

[12] While we transparently report and discuss VAT results in Appendix E, we focus on SSC payments throughout the paper because the literature on payroll tax evasion of firms is very scarce, whereas there do exist a few papers using RCTs to study VAT evasion (in particular Pomeranz, 2015, Bérgolo et al., 2023). We acknowledge that this is somewhat different than what we stated in our pre-registration, where we indicated that we study both SSC and VAT (without focus on SSC). However, in light of existing VAT evidence (much of which emerged after our pre-registration), non-existing payroll tax evidence, and because of important differences between these two taxes (e.g., trade-off due to deduction of wage costs in profit tax, paper trail, "benefit tax" character of payroll taxes and other differences in enforcement challenges), we believe that an improved understanding of payroll tax evasion deserves the majority of attention in our paper (without concealing the VAT results).

[13] There are approximately another 25,000 larger companies in Bulgaria that are VAT registered but that are not in our sample. These are large firms defined as meeting the following three criteria: i) balance sheet value of assets larger BGN 38 million, ii) net sales revenue over BGN 76 million, iii) more than 250 employees or firms for which the NRA did not have financial statements data when they compiled the data set.

received the 2016 data prior to the experiment (to perform the randomization) and received further data after the experiment (to analyse the effects).

We randomly assigned firms into treatments using blocked randomization. We used firms' 2016 tax data to perform the randomization. Firms were grouped into deciles based on their tax reporting and then randomly assigned to a treatment within each decile.[14] The treatments were balanced in terms of their 2016 SSC and VAT tax bases (see Appendix D.1 for summary statistics).

40,000 firms were randomly assigned to treatments involving moral appeals. 18,580 firms received deterrence messages and 10,000 served as the control group. Additionally, 10,000 firms were invited to participate in an anonymous survey administered by the research team. To validate the authenticity of the audit probabilities and to avoid deception, the firms receiving an audit in the deterrence treatments were randomly determined. Audit start dates were confirmed by the NRA to ensure proper implementation. Treatment mailings were sent in July 2017. The tax authority communicated with taxpayers and sent treatment mailings using an electronic mail service similar to standard commercial *email* providers and used for communication with taxpayers.

We conducted a second survey to gain additional insights about payroll tax evasion in December 2023. The survey was addressed at employees and employers. Following a similar procedure as in the first survey, we randomized employers into the survey and the tax authority invited the employers (5000) to participate by email. 361 employers (7%) started the survey and 212 completed it (response rate of 4%). Employees (448 started and 436 finished the survey) of Bulgarian SMEs were recruited by the market research firm dynata (LINK). See Appendix G for details and results.

The design of our experiment assumed direct communication between the tax authority and firms. However, after the treatment mailings were sent, the tax authorities discovered, and pointed out to us, that a total of 76,664 firms provided email addresses which were also used by other firms for communication. This indicates that some firms redirected communication to their tax accountant, leading to spillovers of treatments within the experiment (as shown in the literature; see, e.g., Battaglini et al., 2020, Boning et al., 2020) or confusion. Some accountants received multiple mailings with different content, which may have irritated them and reduced the effectiveness of our interventions. Tax authorities reported second-hand knowledge that some accountants were confused and did not consider the content of the mailings when filing taxes for clients.

[14] VAT and SSC payments in 2016 were highly correlated. We therefore decided to randomize firms into treatments based on the pre-experiment average VAT payment but checked whether the randomization was balanced with respect to all our variables of interests (VAT and SSC), i.e., whether the dependent variables significantly differed between experimental conditions. To randomly allocate firms into treatment, we ranked taxpayers according to mean VAT payments in the year 2016 in ascending order. We then divided the taxpayers in deciles; that is, we created ten groups where the first group consisted of the 10% taxpayers with the highest VAT payments in 2016 and the 10th group consisted of 10% of the taxpayers with the lowest VAT payments in 2016. Within each decile we randomly assigned a number to each firm and subsequently ranked firms within each decile by this random number (in ascending order). Based on this random ranking in each decile, we assigned the taxpayers to treatment groups by assigning blocks of $n/10$ firms to treatment group X, where X is one of the experimental conditions and n is the total number of firms we intended/were able to assign to group X (as per request of/discussion with the tax authorities). We repeated this procedure within each decile. We thus assigned $10 \times n/10 = n$ firms to group X in total.

We were unaware beforehand that firms may redirect communication to their tax accountant. The experimental design assumed direct communication of the tax authority with firms, even for firms with external or internal tax accountants (although we generally acknowledge the role of tax accountants for firms' compliance). As a matter of fact, if we had known during the design stage that the communication is through tax accountants for some firms, we would not have included these firms in the experiment in order to minimize spillover effects between treatments (and to meet the conventional SUTVA assumption). To avoid these concerns, our main specifications only consider firms that communicate directly with the tax authorities. Practically, we focus on firms which do not use the same email as another firm in their communication with the tax authority. This does not exclude the possibility that some firms still use tax accountants (e.g., 25% of surveyed firms reported having in-house accountants), but ensures that each firm directly receives only one treatment email.

Importantly, firms which communicate directly with the authority are equally distributed among treatment groups, maintaining the balance of randomization. We do not find significant pre-experiment differences in reported SSC and VAT tax bases across treatments among the 95,508 firms that communicate directly with the authority. Our main sample has the following number of observations in each treatment arm: *Baseline/ Control Group*: 5,540; all Moral treatments pooled: 22,268; *Cooperation*: 5,529; *Example*: 5,548; *Necessity*: 5,617; *Picture*: 5,574; all Audit treatments pooled: 10,249; *Audit 1%*: 2,908; *Audit 10%*: 2,810; *Audit 40%*: 1,124; *Audit 60%*: 650, ; *Ambiguous*: 2,757 (see Appendix A for more details for our main sample).[15]

Since we were not aware during the design stage that some firms communicate with the authority via a tax accountant, we did not pre-register that the main sample in our paper focuses on firms which communicate directly with the tax authority. To be fully transparent and to show results for the sample that we had pre-registered, we report all our results (including robustness checks) for the extended sample of all firms in our experiment (see Appendix D; also see Section 4 Experimental Results). Our results are generally robust and very similar when considering the full sample.

3. Expected Results

Our analyses focuses on the comparison of *Deterrence* and *Moral* treatments, respectively, with the *Baseline* as control condition, and on the comparison of effects within the *Deterrence* and the *Moral* treatments.[16]

Our moral appeals emphasize (and gradually increase across treatments) the taxpayers' benefit from infrastructure, healthcare, state education and other public goods provided by the state. The *Moral* treatments are thus based on the behavioral economic literature on cooperation (see Ledyard, 1995, Chaudhuri, 2011, for reviews) and reciprocity (Fehr and Schmidt, 1999, Bolton and Ockenfels, 2000, Charness and Rabin, 2002)

[15] We drop one firm with unreasonable reporting behavior in one month (pointing to a data error in this month) from all analyses.

[16] In light of their different nature, we do not derive any predictions for a comparison between the *Deterrence* and the *Moral* treatments. In Section 5.3, we discuss the direct empirical comparisons between the moral and deterrence treatments.

and also connected to literature on gift exchange in the field (Gneezy and List, 2006, Falk, 2007, Kube et al., 2012). Our treatments further relate to recent evidence showing that appealing to cooperation and the individual benefit from pro-social behavior may increase such behavior (List et al., 2021). Our treatments can also be viewed in the context of the 'fiscal exchange' paradigm (Buchanan, 1976, Alm and Jackson, 1993, Feld and Frey, 2007, Schaechtele et al., 2022) according to which the government provides public goods and the citizens are willing to pay taxes in exchange for consumption of the public goods.

Firms face a dilemma between honest reporting and tax evasion for maximum profits. Recent literature suggests that the behavior of (at least some) firms may contradict the standard textbook assumption of pure profit-maximization as they seem to make decisions on moral grounds, e.g., by reflecting preferences of their owners (see, e.g., Schmitz and Schrader, 2015, Grieder et al., 2021). They may therefore be responsive to our moral appeals, assuming that some firms (or decision-makers in firms) face moral costs from evasion. Our treatments aim to increase the visibility of these costs and encourage firms that value cooperation to reduce tax evasion and free-riding on public goods funded by taxes.

Our *Deterrence* treatments are inspired by the seminal work of Becker (1968) and Allingham and Sandmo (1972) who mainly focus on pecuniary motivations to pay taxes. Both theories assume that an increase in penalties or the detection probability reduces criminal and dishonest behavior. In our setting, the penalty for evading taxes is held constant, but the probability of detection is varied in the *Deterrence* treatments.[17]

To guide our analysis, we provide a short theoretical intuition to describe taxpayer behavior in the presence of deterrence and moral costs. We focus on firms as they are likely to have the key role in most of the decisions for under-the-table salaries. We thus assume that employees agreed to receive undocumented cash salaries and that their overall gross wage (reported plus undocumented) remains the same in the presence and the absence of evasion.[18] Only the way of receiving the salary changes for the employee. Our model thus accounts for the specifics of payroll taxes from a firm perspective. Firms face a trade-off: Underreporting wages saves payroll taxes, but it is disadvantageous for the profit tax bill because non-reported wages cannot be deducted from the profit tax base. We are not aware of prior literature that explicitly models this payroll tax specific trade-off. The modeling of moral evasion costs is inspired by Bott et al. (2020).

In our model, firms maximize their expected profit. Firms face two type of taxes. A profit tax τ (with $\tau \in [0,1]$) on profits and a payroll tax t (with $t \in [0,1]$) which is levied on the firm's wage costs. The firm has revenues R and true total wage costs W. The firm is legally required to pay profit taxes τ on profits $(R-W)$ and payroll taxes t on wage costs

[17] Our experiment used blocked randomization with equal distribution of firms (and large and small evaders) between treatment conditions, ensuring that firms on average face the same penalty. However, the probability of detection varied across treatments.

[18] Note that explicitly incorporating a probability that an employee rejects the offer to evade payroll taxes into our model would not affect our directional predictions. If employees reject to collude, firms report honestly. If they accept to collude the maximization problem from our firm-based model applies. As firms are randomly allocated to treatments, firms with employees who are willing to collude/unwilling to collude will be evenly spread across conditions.

W. However, the firm can hide wage costs from the tax authority and total true wage costs consist of reported wages F and unreported wages E (i.e., $W = F + E$). In our data, we see F, the reported base of the payroll tax (i.e., tax base of SSC). The *perceived* probability of an audit is p (with $p \in [0,1]$; see below for more). We assume that revenues R and true wage costs W are determined when the firm makes a decision about payroll tax evasion (that is, we do not model the determination of R and W and we do not model that the firm can hide revenue R from the tax authority). When evading, firms may incur subjective moral costs that depend on the extent of evasion: $s\beta(W-F)^2$ (see below for more interpretation).

If the firm is not audited, payroll tax evasion is not detected and payroll taxes are due on formally reported wages F. Unreported wages E cannot be claimed as expenses in the profit tax context and the profit tax base thus is $(R-F)$. The firm's profit thus is: $P^{nc} = (R-W) - \tau(R-F) - tF$ (with nc indicating the situation with no check by the authority). If the firm is subject to an audit, the tax authority is able to identify the firm's true wage costs. If the firm is caught evading (i.e., $F < W$), the tax authority charges a penalty which we model as an increase in the applicable tax rates: τ^c (with $\tau^c > \tau$) and t^c (with $t^c > t$). If the firm is audited and no evasion is detected (i.e., $F = W$), no penalty is charged and the usual tax rates apply (i.e., $\tau^c = \tau$ and $t^c = t$). As a result, in the case of an audit, honest firms are better off than evading firms. The firm's profit in the case of an audit is: $P^c = (R-W) - \tau^c(R-W) - t^cW$ (with c indicating the situation with check).

Firms thus maximize the following expected payoff function (which includes expected profits and moral costs) with respect to reported wages F:

$$\begin{aligned} E(\pi) &= pP^c + (1-p)P^{nc} - s\beta(W-F)^2 \\ &= p\Big((R-W) - \tau^c(R-W) - t^cW\Big) \\ &\quad + (1-p)\Big((R-W) - \tau(R-F) - tF\Big) - s\beta(W-F)^2 \end{aligned}$$

We assume that the perceived audit probability p consists of two parameters: i) parameter a (with $a \in \{0,1\}$) describes if the firm is aware of the possibility of an audit, and ii) parameter l (with $l \in [0,1]$) describes the perceived likelihood that such an audit might happen (conditional on audit awareness). Thus, $p = a \times l$ is zero if the firm is not aware of an audit possibility (because $a = 0$) and $p = a \times l$ is equal to the perceived likelihood l if the firm is aware of an audit (because $a = 1$). For simplicity, we assume that firms in the baseline treatment are not aware that an audit might happen (i.e., $a = 0$).[19] Our deterrence treatments then shift parameter a from 0 to 1 because they make firms aware that an audit can happen, and they additionally shift l and reveal the true probability of an audit for a firm.

s (with $s \in [0,1]$) captures the salience of the subjective moral evading costs and $\beta \geq 0$ is the weight attached to the moral costs of evasion if the moral costs are salient. We do not expect to manipulate the fundamental weight that is attached to the moral cost, β, through the treatment letters (Bott et al., 2020). Rather, in our moral treatments, we shift

[19] Note that our directional predictions hold when $a \in [0,1]$ and assuming that awareness of an audit a is lower in baseline than in the deterrence treatments.

the salience s of the moral costs: the salience is larger in any moral treatment relative to the baseline condition, i.e. $s(Moral) > s(Baseline)$. In addition, salience s varies between the moral treatments: $s(Cooperation) < s(Example) < s(Necessity) < s(Picture)$.

Maximizing $E(\pi)$ with respect to formally reported wages F yields the optimal level of reported wages, F^*, as a function of true wages:

$$F^* = W - \underbrace{\frac{(1-p)(t-\tau)}{2s\beta}}_{E^*}$$

The right term, E^*, describes the optimal underreported (evaded) amount of wage costs (recall: $W = F + E$). The firm underreports wages (i.e., $E^* > 0$ and thus $F^* < W$) if the profit tax τ is smaller than the payroll tax t (in this case, E^* is greater than zero). This finding occurs as a result of the previously described trade-off: underreported wages save payroll taxes, but they cannot be deducted from the profit tax base and therefore increase the due amount of profit taxes. In a situation in which profit taxes are higher than payroll taxes, reporting and deducting the true wage costs is advantageous. In the case of Bulgaria, firms have an incentive to underreport wages, because all profits (for both corporations and pass-through firms) are subject to a tax of 10%, whereas the payroll tax is 30% and the legal share of employers in this is $2/3$.

The remaining parts of the expression for F^* are intuitive as well: as the perceived audit probability p increases, reported income increases ($E^* = 0$ and $F^* = W$ for $p = 1$). Similarly, as the salience of the moral costs s increases, reported income increases (E^* decreases as s increases).

Our deterrence and moral treatments increase p and s, respectively. It is easy to see that these treatment induced shifts increase reported wages F and move reported wages towards true wages W, thus increasing tax honesty ($\frac{\delta F^*}{\delta p} > 0$ and $\frac{\delta F^*}{\delta s} > 0$). That is, we expect the tax base of the payroll tax F to increase through the treatment manipulations.

Expected Result for Moral Treatments: Given the differences in s between treatments, we hypothesize that the observed payroll tax base (F) is lowest in the *Baseline* condition and highest in the *Picture* condition.

Expected Result for Deterrence Treatments: Since $p = 0$ in the *Baseline* group, and $p > 0$ in any *Deterrence* group, we hypothesize that the observed payroll tax base (F) in the *Baseline* condition is lower than in any of the *Deterrence* conditions. In addition, we hypothesize that the reported tax base will be the higher, the higher the audit probability l that we communicate to firms in the audit letters, i.e., lowest tax base in the *Audit 1%* treatment and highest tax base in the *Audit 60%* treatment.[20]

4. Experimental Results

4.1 *Estimation Strategy*

Our administrative monthly tax return data include the outcome variable of interest before and after the experimental intervention. Following the practice in many RCTs on tax

[20] The *Deterrence* letters obviously include the *Ambiguity* condition. We will take a closer look at the *Ambiguity* condition as we study mechanisms in Section 5.

compliance (such as Pomeranz, 2015, Bjorneby et al., 2021, Bérgolo et al., 2023, and Holz et al., 2023), we use a difference-in-difference (DiD) approach that compares treated firms to control firms and the pre-treatment period to the post-treatment period to estimate treatment effects. As noted in McKenzie (2012) and Bérgolo et al. (2023), using pre- and post-treatment data reduces variance in the error term and thus gains statistical power.[21] We estimate both 'static' 2×2 DiD models and models that allow us to examine dynamics and pre-trends. Our results are very similar to those of a TWFE model with firm and month fixed effects (see Appendix C.2).[22] Our 'static' model has the following form:

$$Y_{i,t} = \beta_0 + \beta_1 POST_t + \beta_2 TREAT_i^j + \beta_3 (POST_t \times TREAT_i^j) + \mu X_i + \varepsilon_{i,t}. \quad (1)$$

$Y_{i,t}$ denotes our main outcome of interest, SSC tax base (F in our model), for firm i in month t. The unit of observation thus is the firm-month. Having the level of the SSC tax base as the outcome variable, we study intensive margin responses in the reporting decision. Because firms usually report some SSC, we do not consider the extensive margin decision of whether to report SSC at all. $TREAT_i^j$ is a dummy variable equal to one if firm i received treatment j and zero if it is in the baseline condition. $POST_t$ is a dummy variable indicating the months after the treatment, and X_i is a vector of two pre-defined control variables (pre-experimental 2016 values of number of employees – measured in categories – and a firm's industry) which we include to gain precision. Results are robust to excluding controls (see Appendix C.1). $\varepsilon_{i,t}$ is the error term.

Our coefficient of interest is β_3, the coefficient on the interaction term $POST_t \times TREAT_i^j$. It corresponds to the difference in reported SSC between treated and baseline firms between pre-treatment and post-treatment months. Hence, it represents the causal effect of our respective treatment mailings on the firms' tax base. We start with *pooled* regressions in which we benchmark firms in the *Baseline* condition against all firms who received a *Moral* treatment or a *Deterrence* treatment, respectively. Subsequently, we compare each experimental group separately relative to the *Baseline* condition. Obviously, the $TREAT_i^j$ dummy will be differently defined across these comparisons. We also test the audit treatments against the moral treatments (reported in Section 5.3). Standard errors are always clustered at the firm level. Our results are robust to using standard errors that are adjusted for multiple hypothesis testing (see Appendix C.3, where we use the MHT approach of Jones et al., 2019.)

We report absolute (level) effects and relative effects throughout all analyses. To study absolute effects, the outcome variable is measured as a money amount and regressions are estimated using OLS. The resulting coefficient of interest measures treatment effects in absolute money values (expressed in BGN). Relative effects are estimated using

[21] Estimates only using the post-treatment data generally align with our DiD estimates, though they are measured less precisely (see Appendix C.6).

[22] Note that all treated firms were treated at the same point of time, implying that our estimations are not subject to the concerns that were recently raised in the context of staggered DiD models where different units are treated at different points in time (Goodman-Bacon, 2021, Baker et al., 2022).

Poisson regressions and the coefficient of interest is interpreted as a semi-elasticity.[23] An important advantage of the Poission regression compared to a model with logged dependent variable is that there is no concern about zeros (Chen and Roth, 2023 highlight the problems of approaches such as $log(1+Y)$). The specification with relative effects takes into account that the treatment might not increase the wage bill by the same money amount for all firms independent of size.

We include four pre-treatment months (to account for monthly fluctuations and seasonality) in all specifications (results are robust to including different number of pre-treatment months, see Appendix C.4). We further differentiate between effects that occur in the months after the intervention and overall effects (spanning all post months in our data). We include four post-treatment months in the above regression to study the effects occurring directly in the months after the intervention and ten post-treatment months for the overall effects. To ensure that our results are not dependent on an arbitrarily chosen post-treatment horizon, we report results for two, four, six, eight and ten post-treatment months in Appendix C.5. The results are robust to including different numbers of post-treatment months. We do not consider very-immediate effects (say one post month) as firms need some time to respond to the treatment and adjust tax filing behavior.

To understand dynamic treatment effects over time, we run generalized DiD-models where we interact the treatment dummy with month dummies, thereby omitting the interaction with the month before the treatment (that is, we basically replace $POST_t \times TREAT_i^j$ in the above equation with interactions of the treatment dummy with month dummies). We include all months available in our data in these regressions and, in the figures, display the results for ten pre- and ten post-experimental months (consistent with the overall DiD specifications). Our main dynamics are in absolute money value terms. Appendix C.7 shows that the dynamic results look similar when we use relative effects in our Poisson regressions. Time trends prior to our treatment intervention are insignificant in all treatments.

We conduct an exploratory analysis to investigate heterogeneous treatment effects, as outlined in our pre-registration. Our analysis uses the static DiD model with sample splits based on pre-determined variables to compare treatment effects across all sources of heterogeneity that we can investigate in our data. Our sources of heterogeneity are: size (employees, 2016 tax base), industry, 2016 SSC tax base variance, and tax authority risk score. We only show relative effects (semi-elasticities estimated using Poisson) for comparability across firms and industries. The comparison groups in these sample split analyses always consist of control group firms with similar characteristics to the treatment group.

We report all analyses using the extended sample that includes firms which do not directly communicate with the authority in Appendix D. The results using this sample are generally robust, though sometimes smaller in size (which is consistent with the

[23] The Poisson approach allows for a percent-change effect interpretation provided that the coefficient is small enough (as done in e.g., Bérgolo et al., 2023). We also report (in the results description) relative effects based on relating the absolute level-coefficient to the control-group mean. The resulting effects are very similar to the Poission results.

explanation that some accountants were confused and/or ignored the treatment message). None of the conclusions that we draw in the paper differ from the conclusions that could be drawn from including firms that do not directly communicate with the authorities in the analysis.

4.2 *Moral treatments*

Static DiD. Table 1 presents the main results for the effect of the moral appeal treatments on SSC. Panel A depicts the immediate effects of our intervention (including four post-experimental months) and Panel B shows the results for the overall effects (including ten post-treatment months). Column (1) reports the regressions where we pool all *Moral* treatments. Columns (2-5) present the regression results in the *Cooperation, Example, Necessity, Picture* treatments, respectively. All reported effects are relative to the *Baseline* condition. The table always reports the coefficient of interest, $POST_t \times TREAT_i^j$, for both the absolute (level) effect and the relative effect.

TABLE 1. Treatment effects of moral treatments on SSC

Panel A:	**4 post-treatment months**				
	Moral all	*Cooperation*	*Example*	*Necessity*	*Picture*
	(1)	(2)	(3)	(4)	(5)
Absolute effect	278.195***	304.621**	267.742***	291.545***	242.031**
	(76.559)	(118.416)	(102.450)	(103.315)	(109.151)
Relative effect	0.031***	0.034***	0.038***	0.024**	0.027**
	(0.008)	(0.013)	(0.011)	(0.011)	(0.013)
Observations	186377	74366	74505	74850	74409
No of firms	27808	11069	11088	11157	11114
Panel B:	**10 post-treatment months**				
	Moral all	*Cooperation*	*Example*	*Necessity*	*Picture*
	(1)	(2)	(3)	(4)	(5)
Absolute effect	232.393**	329.416**	175.107	204.189	208.070
	(111.396)	(144.980)	(127.062)	(148.571)	(154.560)
Relative effect	0.024**	0.036**	0.029**	0.012	0.022
	(0.011)	(0.015)	(0.012)	(0.015)	(0.017)
Observations	321202	128123	128327	129002	128113
No of firms	27808	11069	11088	11157	11114

Notes: Treatment effects of moral messages on SSC. Displayed are DiD regression estimates of treatment messages on tax base SSC. The estimated treatment effects are relative to the baseline condition. Treatment effects are reported in absolute terms based on OLS regressions and in relative terms based on poisson regressions. SSC tax base is the monthly reported tax base for social security payments expressed in BGN. The pre-treatment time period includes four months before and the post-treatment time period four (Panel A) or ten (Panel B) months after the treatment. The pre-experiment control mean of tax base SSC in the baseline condition is BGN 8,004. A firm's number of employees and its industry affiliation are included as controls. Standard errors clustered on firm level are in parentheses. $^{*}\ p < 0.10, ^{**}\ p < 0.05, ^{***}\ p < 0.01$.

All $POST \times TREAT$ interaction terms in Panel A are positive and statistically significant, showing that firms in the *Moral* treatments raised payroll tax compliance relative to the control group in the months after the intervention. This is consistent with our predictions. In the specification where we pool all moral treatments, we find an average DiD coefficient of BGN 278 (USD 140) in the specifications with absolute effects. The relative effects show that this corresponds to a 3.1% increase in SSC tax base in each of the four months following the treatment intervention (Column 1). Consistent with this

Poisson regression coefficient, a relative comparison of the level effect to the level mean in the control group shows an increase of 3.5%.[24]

We find that the estimates for each type of moral appeal message (i.e., *Cooperation*, *Example*, *Necessity* and *Picture*) are positive and statistically significant (relative to baseline). However, all these coefficients are similar in magnitude and they are not significantly different from each other. Inconsistent with our predictions, this suggests that the type of message does not make a considerable difference; simple cooperation messages and messages that directly refer to the taxpayer and include a picture of a government financed children playground have similar effects on compliance.

As shown in Panel B, the pooled effect of all moral appeal messages sustains as we consider ten post-experimental months. For these overall effects, we detect differences across the different types of appeals. The *Cooperation* message generates more sustainable compliance, whereas the moral appeals with higher fiscal exchange character and direct appeals to the taxpayer (*Example*, *Necessity*, and *Picture*) do not (although the effects are directionally still positive; see below for more on dynamics). We summarize our main findings for the effects of moral appeals as follows:

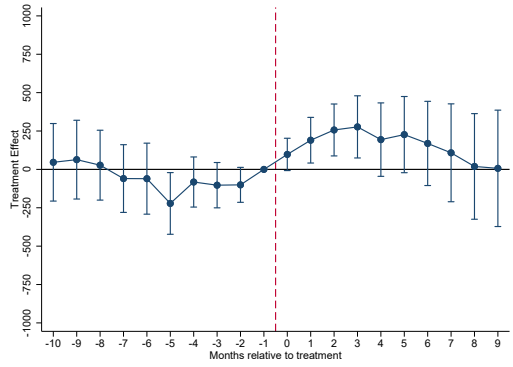
RESULT 1 (The effect of moral appeals on SSC compliance). *Moral appeals highlighting the individual benefit of paying taxes and social security contributions increase social security payments in the four (ten) months after our treatment intervention. Moral appeals providing concrete examples of public goods and appealing to the taxpayer directly do not have larger effects than simple cooperation messages.*

Dynamics. Figure 1 presents the monthly dynamics of the treatment effects for the specification where we pool all *Moral* treatments. The impact of our *Moral* treatments is immediate but diminishes over time. Treatment effects build up in the four months after the intervention and vanish subsequently. Panels (a)-(d) of Figure 2 present the dynamic effects for each of the *Moral* treatments separately. The effects of the different moral appeals align with the pooled treatment results. Treatment effects increase in the first months after the intervention but fade out in all but one of the *Moral* treatments.[25] Consistent with the static results, the *Cooperation* treatment is an exemption where the treatment effect appears to be more stable over time (see Panel (a) of the Figure).

Heterogeneity. To understand heterogeneous effects of our treatment messages, we perform exploratory analyses in which we estimate the pooled 'static' DiD model separately for different groups. To make firms of different sizes and across different industries comparable and to account for differences in initial tax bases across sub-samples,

[24] We find positive coefficients for the post-treatment indicator (not reported), indicating that firms in the baseline condition increased their payroll tax base after the intervention. This can be due to a summer effect (since treatment were sent in early summer), improved economic conditions in the second half of the year, or a combination of both. The Bulgarian economy improved in the latter half of 2017, as shown by the pre-experimental data and quarterly GDP data (GDP in million Euro: 2017Q1 10,724.7; 2017Q2 12,741.9; 2017Q3 14,302.0; 2017Q4 14,762.6; Source: Eurostat). Eventually, these results emphasize the need to have a randomized design with treatment and control group.

[25] Importantly, we do not observe a dynamic 'lift and shift' in tax compliance in the sense that the temporal increase in compliance is offset with a subsequent increase in evasion.



Notes: Pooled absolute monthly treatment effects of the moral appeal messages (Cooperation, Example, Necessity, Picture). The points plotted are the estimated DiD regression coefficients of treatment messages on tax base SSC by months relative to the treatment. Treatment effects are relative to the control message. SSC tax base is the monthly reported tax base for social security payments expressed in BGN. 95% confidence intervals are represented by the blue lines and based on standard errors clustered on firm level.

FIGURE 1. Dynamic absolute effects of moral treatments on SSC

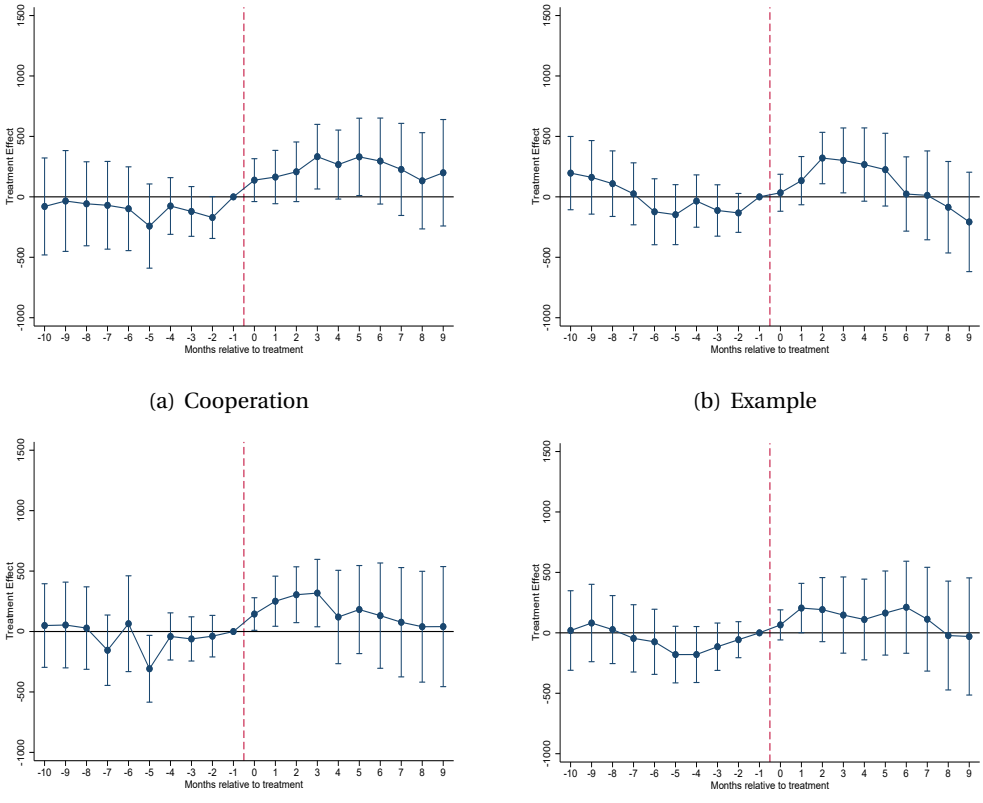
we focus on relative changes estimated using Poisson regressions. Our sources of heterogeneity are shaped by the variables that we have in our data. The corresponding results are presented in Figure 3 which plots the $POST \times TREAT$ interaction coefficient for the different sample splits. Specifically, we test if treatment effects depend on firm size in Panels (a) and (b) (based on the number of employees in 2016 in (a) and on pre-experimental tax base SSC quintiles in (b)), industry affiliation based on NACE codes in Panel (c), volatility of past tax payment behavior in Panel (d) and riskiness (from 1-low-risk to 3 - high-risk as assessed by the tax authorities) in Panel (e).

Our heterogeneity analysis shows that larger firms, riskier firms and firms with fluctuating SSC payments are more likely to change payroll tax reporting behavior in response to moral treatment interventions. In addition, we observe a higher compliance effect in the construction industry, which is usually believed to be a sector with "shadow" workers, and in manufacturing. We do not observe any other industry specific treatment effects.

The larger treatment effects for bigger firms may be driven by the larger scope for changing filing behavior of bigger firms. Supporting this explanation, our survey results do not point to strong differences in tax morale across firms with different size. The correlation between firm size and tax base volatility (in the pre-experiment periods) is also consistent with this explanation. An alternative explanation is that firms with more employees may benefit more from public goods and have closer connections to local communities, making them more responsive to the increased salience of the 'fiscal exchange' character of our treatment mailings.

4.3 *Deterrence treatments*

Static DiD. Table 2 presents the DiD estimates for our *Deterrence* treatments. The analysis and results presentation are organized in the same way as in the *Moral* treatments. Panel A of the Table reports the immediate treatment effects (including four months



(a) Cooperation

(b) Example

(c) Necessity

(d) Picture

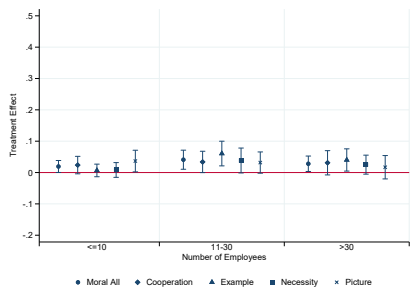
Notes: Monthly absolute treatment effects for each of the moral appeal messages (Cooperation, Example, Necessity, Picture) on SSC. The points plotted are the estimated DiD regression coefficients of treatment messages on tax base SSC by months relative to the treatment. Treatment effects are relative to the control message. SSC tax base is the monthly reported tax base for social security payments expressed in BGN. 95% confidence intervals are represented by the blue lines and based on standard errors clustered on firm level.

FIGURE 2. Dynamic absolute effects of moral treatments on SSC by sub-treatment

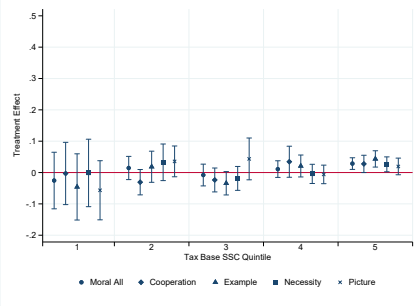
after the intervention) and Panel B reports overall effects from our RCT (including ten post-treatment months), and we report both absolute and relative effects.

Benchmarking all *Deterrence* treatments against the *Baseline* group, we find that the experimental interventions have a positive and statistically significant effect on SSC payments in the months after the intervention (Panel A, Column 1). This is consistent with our predictions. The treated firms increase their SSC tax base by BGN 277 (USD 138) or 3% per month relative to the baseline firms.[26] The regressions further show that higher announced audit probabilities are generally associated with larger treatment effects (an exception is the *Audit 10%* treatment). Firms in the *Audit 1%* treatment increase monthly SSC tax base by BGN 298 (USD 149; Column 2) and firms in the *Audit 40%* treatment raise their SSC tax base by BGN 437 (USD 219) per month (Column 4). In the *Audit 60%* treatment payroll tax payments increase by BGN 665 (USD 333) in the four months following

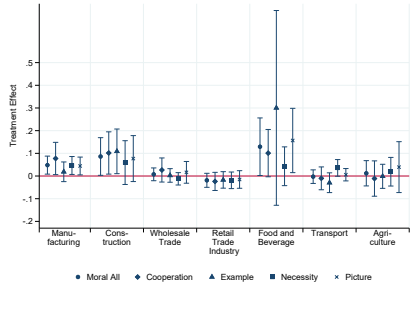
[26]Consistent with the relative Poisson regression coefficient, a relative comparison of the level effect to the level mean in the control group shows an increase of 3.5%.



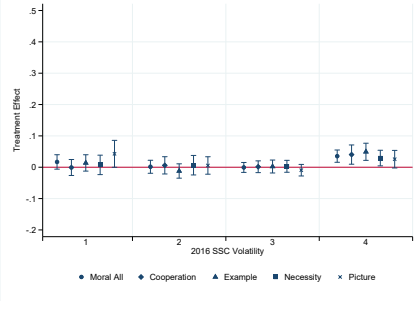
(a) Firm size: Number of employees



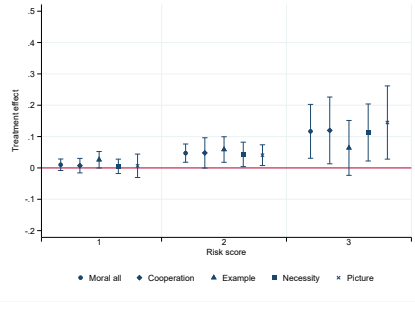
(b) Firm size: Pre-treatment tax base SSC



(c) Industry



(d) Volatility: 2016 tax base SSC



(e) Risk score

Notes: Panel (a): Heterogeneous effects of moral messages on SSC by the number of employees. Panel (b): Heterogeneous effects of moral messages by firm size in pre-treatment tax base SSC quintiles. Panel (c): Heterogeneous effects of moral messages on SSC by industry affiliation. Panel (d): Heterogeneous effects of moral messages on SSC by 2016 tax base SSC volatility quartiles. Panel (e): Heterogeneous effects of moral messages on SSC by the tax authorities' internal risk score (between 1-low risk to 3-high risk). The points plotted are the estimated DiD poisson regression coefficients of treatment messages on tax base SSC. Displayed are relative treatment effects compared to the control message. SSC tax base is the monthly reported tax base for social security payments expressed in BGN. The pre-treatment time period includes four months before and the post-treatment time period four months after the treatment. 95% confidence intervals are represented by the blue lines and based on standard errors clustered on firm level.

FIGURE 3. Heterogeneous effects of moral treatments on SSC (in percent)

our treatment mailings (Column 5). This indicates that higher audit probabilities trigger higher payroll tax compliance. Yet, although large in magnitude, the differences between the audit treatments are statistically insignificant. We also estimate positive treatment effects for those deterrence messages announcing an *Ambiguous* audit probability – we discuss these results in more detail in Section 5 below.

TABLE 2. Treatment effects of deterrence treatments on SSC

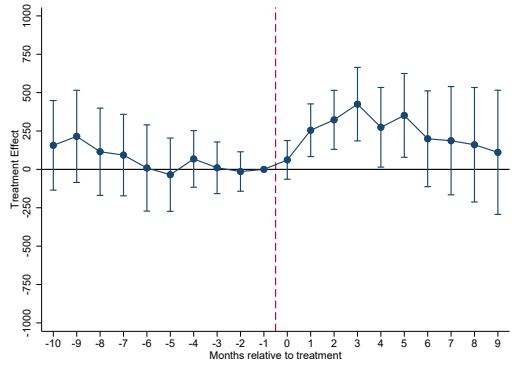
Panel A:	**4 post-treatment months**					
	Audit All (1)	*Audit 1%* (2)	*Audit 10%* (3)	*Audit 40%* (4)	*Audit 60%* (5)	*Ambiguous* (6)
Absolute effect	277.037***	297.985**	112.389	437.186***	664.887***	215.630**
	(91.270)	(134.778)	(127.836)	(166.276)	(248.315)	(108.930)
Relative effect	0.030***	0.024**	0.020	0.054***	0.051***	0.033***
	(0.009)	(0.010)	(0.015)	(0.020)	(0.017)	(0.013)
Observations	87553	56652	56147	44735	41772	55817
No of Firms	15789	8448	8350	6664	6190	8297
Panel B:	**10 post-treatment months**					
	Audit All (1)	*Audit 1%* (2)	*Audit 10%* (3)	*Audit 40%* (4)	*Audit 60%* (5)	*Ambiguous* (6)
Absolute effect	235.915*	230.592	76.163	381.102**	706.298**	132.387
	(123.973)	(145.757)	(177.653)	(189.863)	(329.200)	(132.282)
Relative effect	0.024*	0.015	0.019	0.049**	0.048**	0.024*
	(0.013)	(0.015)	(0.020)	(0.020)	(0.019)	(0.014)
Observations	150976	97605	96741	77045	71948	96166
No of Firms	15789	8448	8350	6664	6190	8297

Notes: Treatment effects of deterrence messages on SSC. Displayed are DiD regression estimates of treatment messages on tax base SSC. Displayed are DiD regression estimates of treatment messages on tax base SSC. The estimated treatment effects are relative to the baseline condition. Treatment effects are reported in absolute terms based on OLS regressions and in relative terms based on poisson regressions. SSC tax base is the monthly reported tax base for social security payments expressed in BGN. The pre-treatment time period includes four months before and the post-treatment time period four (Panel A) or ten (Panel B) months after the treatment. The pre-experiment control mean of tax base SSC in the baseline condition is BGN 8,004. A firm's number of employees and its industry affiliation are included as controls. Standard errors clustered on firm level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Including ten post-experimental months in the analysis (Panel B), we find less powerful effects of low audit probabilities (i.e., in the *Audit 1%*), suggesting that the effects are less sustainable (see more on the dynamic effects below). However, we still find statistically significant and economically meaningful treatment effects for the *Audit 40%* and *Audit 60%* treatment, again indicating that more severe deterrence measures have stronger and longer lasting effects. Thus, consistent with our predictions, the higher the announced audit probability, the more positive and sustainable the effect on reported tax base SSC. We summarize our main findings for the deterrence treatments in the following:

RESULT 2 (The effect of deterrence messages on SSC compliance). *Deterrence messages increase SSC payments. High deterrence messages (with larger announced audit probabilities) lead to economically larger and more sustainable effects than low deterrence messages.*

Dynamics. Figure 4 displays effects of the pooled *Deterrence* treatments on SSC reporting by month. The effects build up for five post-treatment months and then phase out in the following months. Panel (a)-(e) of Figure 5 present the dynamic treatment effects separately for each sub-treatment. The figures show that there are no significant pre-trends in the treatments. The Figure shows that treatment effects are short-lived when a low audit probability is announced. However, we see larger and more persistent effects in the *Audit 40%* group and especially in the *Audit 60%* treatment (Panel (c)-(d) of the



Notes: Pooled monthly absolute treatment effects of the audit probability messages (Audit 1% - Audit 60%). The points plotted are the estimated DiD regression coefficients of treatment messages on tax base SSC by months relative to the treatment. Treatment effects are relative to the control message. SSC tax base is the monthly reported tax base for social security payments expressed in BGN. 95% confidence intervals are represented by the blue lines and based on standard errors clustered on firm level.

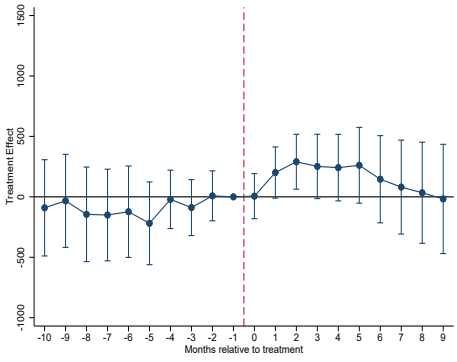
FIGURE 4. Dynamic effects of deterrence treatments on SSC

Figure). The dynamic treatment effects thus confirm the static DiD results and are consistent with our predictions: high audit probabilities lead to sizable and stable effects over time, while the effect of low audit probabilities on tax base SSC is smaller and less sustainable in our sample.[27]

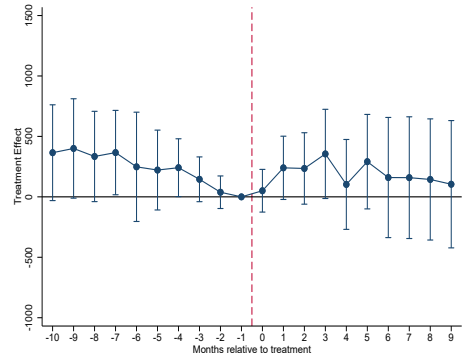
Heterogeneity. To study heterogeneous effects of the *Deterrence* treatments across different types of firms, we perform similar exploratory analyses as in the *Moral* treatments. The results are summarized in Figure 6. As before, we run our 'static' DiD model separately for different groups of firms (where the assignment is based on pre-experiment data) and consider relative changes to account for size differences across firms.

The treatment effects are particularly pronounced for large firms (with many employees and which are in higher 2016 tax base quintiles), firms that are classified as high-risk by tax authorities, and firms with volatile pre-experimental filing behavior. These findings align with Holz et al. (2023), who also report that large firms are more responsive to their treatment interventions. One possible interpretation for our finding is similar to that brought forward in the moral treatments: large firms have more employees, more resources, higher abilities and therefore more competence to engage in payroll tax evasion. Although it might be harder to implement the necessary collusion for payroll tax evasion as more employees are involved (Kleven et al., 2016), when changing to more compliant behavior their scope to adjust their SSC is higher compared with that of smaller firms. Holz et al. (2023) put forward the possible interpretation that threats are more credible for larger firms, which is also likely for firms who are classified as high-risk and with inconsistent filing behavior.

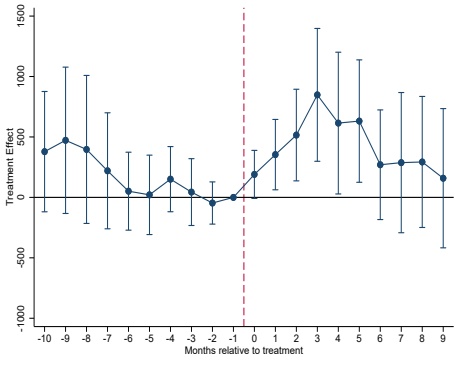
[27] Importantly, and similar to the *Moral* treatments, we do not observe a dynamic shifting in tax compliance. The temporal increase in compliance is not offset with a subsequent increase in evasion. Reported tax bases stay above the levels reported in the control group.



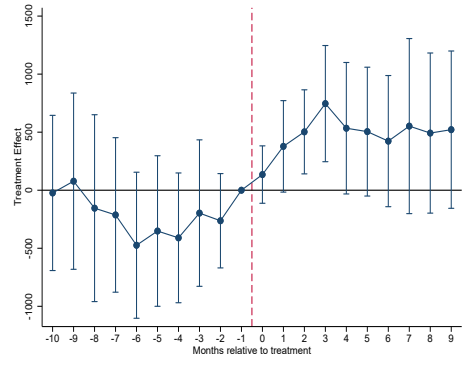
(a) Audit 1%



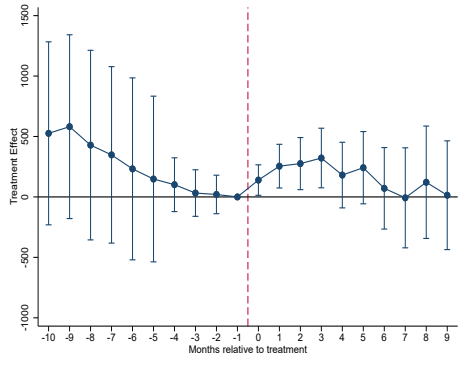
(b) Audit 10%



(c) Audit 40%



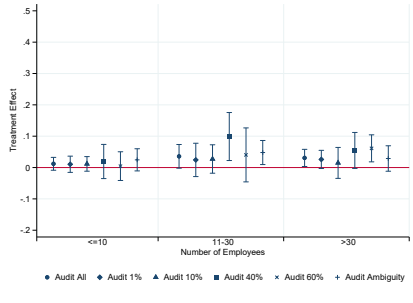
(d) Audit 60%



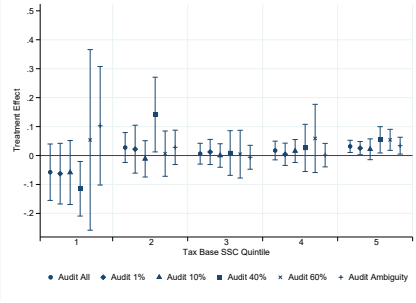
(e) Ambiguous

Notes: Monthly absolute treatment effects of the audit messages (Audit 1%, Audit 10%, Audit 40%, Audit 60%, Ambiguous) on SSC. The points plotted are the estimated DiD regression coefficients of treatment messages on tax base SSC by months relative to the treatment. Treatment effects are relative to the control message. SSC tax base is the monthly reported tax base for social security payments expressed in BGN. 95% confidence intervals are represented by the blue lines and based on standard errors clustered on firm level.

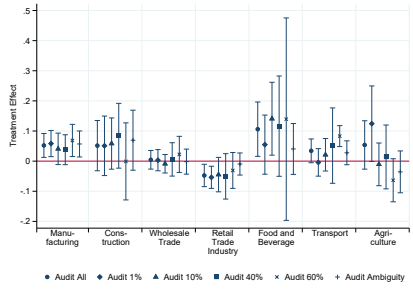
FIGURE 5. Dynamic absolute effects of deterrence treatments on SSC by sub-treatment



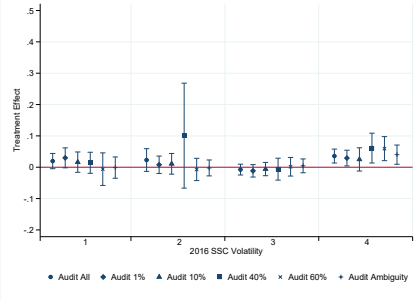
(a) Firm size: Number of employees



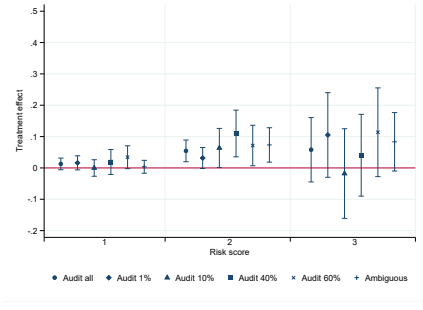
(b) Firm size: Pre-treatment tax base SSC



(c) Industry



(d) Volatility: 2016 tax base SSC



(e) Risk score

Notes: Panel (a): Heterogeneous effects of deterrence messages on SSC by the number of employees. Panel (b): Heterogeneous effects of deterrence messages by firm size in pre-treatment tax base SSC quintiles. Panel (c): Heterogeneous effects of deterrence messages on SSC by industry affiliation. Panel (d): Heterogeneous effects of deterrence messages on SSC by 2016 tax base SSC volatility quartiles. Panel (e): Heterogeneous effects of deterrence messages on SSC by the tax authorities' internal risk score (between 1-low risk to 3-high risk). The points plotted are the estimated DiD poisson regression coefficients of treatment messages on tax base SSC. Displayed are relative treatment effects compared to the control message. SSC tax base is the monthly reported tax base for social security payments expressed in BGN. The pre-treatment time period includes four months before and the post-treatment time period four months after the treatment. 95% confidence intervals are represented by the blue lines and based on standard errors clustered on firm level.

FIGURE 6. Heterogeneous effects of deterrence treatments on SSC (in percent)

5. MECHANISMS AND REVENUE EFFECTS

5.1 *Understanding the Effects of Moral Treatments*

The results reaffirm our conjecture on moral behavior of firms: the *Moral* treatments, which make firms aware that social security payments are a civic duty and that they receive something in return, trigger compliance. In the words of our model presented in

Section 3, the effect occurs because the treatment mailings increase salience s of the moral costs among firms who give some fundamental weight to morals (i.e., among firms whose β is larger than zero). It is consistent with our survey results that firms give some fundamental weight to morals and that increasing salience of the moral costs thus works: 82% of responding firms deem it generally moral to pay taxes and social security contributions and report that they should be paid honestly (see Table F.1 in Appendix F). This self-reported high tax morale is not necessarily ad odds with the self-resorted assessment that tax evasion is a problem in Bulgaria (85% of respondents believe this): firms may have some fundamental weight to morals, but the moral costs (and the quality of what they receive in return for paying honestly) are not salient to them. In addition, research by Guiso et al. (2013) and Brown et al. (forthcoming) demonstrate that social norms and moral constraints may differ from another and both can influence pro-social (cooperation) behavior.

Generally, our finding on moral appeals are in line with work such as List et al. (2021) who show that pro-social behavior (in the form of charitable giving by individuals) increases if people are reminded of the potential increase in (warm-glow) utility for themselves. The finding is likewise in line with results on fiscal exchange from laboratory experiments (see, e.g., Alm and Jackson, 1993) or field experiments with delinquent individual taxpayers (Eguino and Schächtele, 2020). Our findings on the dynamic effects are consistent with research studying contributions to public goods and conditional cooperation (Fischbacher et al., 2001) and resemble patterns in laboratory experiments (see, e.g., Ledyard, 1995, Chaudhuri, 2011, for reviews). Firms initially start to cooperate after having received a treatment letter in the moral treatments. They may then realize, however, that many other firms around them still do not cooperate (i.e., those in the control treatment or the untreated firms in their network). This induces firms who responded to our treatment interventions to revert back to initial compliance levels, explaining why effects fade out over time.

Contrary to our predictions, we do not observe significant differences between the four arms of the *Moral* treatments. Generally, this finding is in line with, for example, Dwenger et al. (2016) who do not find differences between two types of moral appeals in their study of *individuals'* compliance with local church fees. The similarity in effectiveness of our four *Moral* treatments in the four months after the intervention may be the result of a ceiling effect indicating that firms are either susceptible to moral appeals in general or not, and that the differences in the moral messages are not strong enough to further change taxpayers' behavior. Alternatively, our treatments may not trigger different compliance effects, because the appeals are not perceived to be different (in other words, the salience s does not differ between treatments).

While we do not observe differences from a statistical significance point of view, the point coefficient decreases as we move from the *Cooperation* treatment to the other *Moral* treatments. In addition, the dynamic results suggest that the *Cooperation* effects are more sustainable. The effectiveness of the *Necessity*, *Example*, and *Picture* message may be impaired compared with the *Cooperation* message, because the public goods mentioned may be perceived to be low quality.[28] Another possibility may be that shorter

[28] Note many public goods mentioned in the letters are financed through SSC (see LINK). An irrelevance of the mentioned public goods is therefore an unlikely explanation.

appeals like the one in the *Cooperation* treatment are generally more sticky compared with longer messages explaining the benefit of paying taxes in more detail. This could explain why we observe longer lasting effects in this treatment compared with the other *Moral* treatments.

5.2 *Understanding the Effects of Deterrence Treatments*

Our deterrence effects confirm our prediction that the threat of an audit increases compliance relative to our baseline control condition. The finding is consistent with a simple deterrence model and its interpretation is rather intuitive.

We further predicted that the compliance effect increases with the announced audit probability. While this prediction is based on standard deterrence models such as Allingham and Sandmo (1972), it conflicts with literature showing that people have problems to correctly assess probabilities (e.g., Tversky and Kahneman, 1974). Consistent with difficult probability assessment, some studies provide evidence that tax compliance effects do not increase with the announced audit probability, implying that taxpayers fall victim to probability neglect (Dwenger et al., 2016, Bérgolo et al., 2023).

Bérgolo et al. (2023) propose that their finding of probability neglect is consistent with a model of *risk-as-feelings* (Loewenstein et al., 2001), according to which responses to risks neglect underlying probabilities when fear is involved. Translated to our context, this implies that any message involving audit announcements has an effect on tax compliance, as it induces fear, and that the actual level of audit probability does not make a difference for the effect size. Our main results are not indicative of probability neglect and such a model of risk-as-feelings. For example, the point coefficient of announcing a 60% audit probability is more than twice as large as the point coefficient in the 1% treatment. Our results further suggest that high audit probabilities trigger a more persistent compliance effect than smaller probabilities.

Experimental Test of Probability Neglect and Risk-as-Feelings. To test probability neglect and a risk-as-feelings notion, our field experiment includes the *Ambiguous* treatment (see Section 2.2.3). In contrast to the other *Deterrence* messages, this treatment did not explicitly mention an audit probability. Considering that the treatment does mention the audit threat, thus inducing a fear to be audited, without specifying an audit probability, the risk-as-feelings model with probability neglect would predict that the *Ambiguous* treatment has a similar effect as a treatment that explicitly announces an audit probability.

Our theoretical intuition that we presented in Section 3 distinguishes between the mere threat/fear effect and the probability itself. Recall that in the model, the perceived probability of being detected, p (where $p = a \times l$ and $p \in [0,1]$), depends on two parameters: i) parameter a (with $a \in \{0,1\}$) describes if the firm is aware of the possibility of an audit, and ii) parameter l (with $l \in [0,1]$) describes the perceived likelihood that such an audit might happen (conditional on audit awareness). Probability neglect and the notion of risk-as-feelings would predict that an increase in the audit probability l does not affect compliance for taxpayers who are aware of an audit i.e., for whom $a = 1$), because the awareness already induces the feeling of fear and the probability itself is neglected.

So far, we compared treatments that mention audits (i.e., $a = 1$) along with a specific audit probability (i.e., l specified) to the baseline condition where taxpayers' awareness of audits is lower (we assume for simplicity $a = 0$). To disentangle the effect of a and l, we now consider the *Ambiguous* treatment. This treatment shifts a from 0 to 1, but it does not specify l. We can isolate the effect of l by comparing the treatments with specific audit probabilities to the *Ambiguous* treatment. With a risk-as-feelings notion, we would predict no differences across these groups – even as we compare the *Ambiguous* with a group with very high audit probability – , because the probabilities are neglected in a state of fear.

However, standard deterrence models predict that the effect between the *Ambiguous* treatment and the treatments with specific probabilities depends on the subjective belief about the audit risk in *Ambiguous*. In treatments where the announced probability is higher than the belief about the audit probability in *Ambiguous*, the standard deterrence model predicts that compliance increases. Beliefs about audit probabilities in the *Ambiguous* group are unobservable to us.[29] Assuming that the *Ambiguous* letter somehow increases the awareness of, and belief about, audit probabilities (compared to firms in our baseline) and considering that the NRA performs audits on a regular basis, it is conceivable that specified audit probabilities need to be sufficiently high to induce a difference between the *Ambiguous* treatment and treatment arms with specified probability in a standard model. While it is unclear whether these expectations translate into actual beliefs in the wake of receiving a treatment letter, past behavior of the NRA and the self reported expectations in our survey are supportive of this assumption. Based on the information provided by the NRA, the probability for a SME of being checked by the authorities is around 6%. The firm survey further indicates that firms assume that the likelihood for receiving a check of any kind is above 40%. Following the standard model and taking these potentially high priors into account, we hypothesize:

Expected Result for Ambiguous Treatment: Relative to the *Ambiguous* treatment, the compliance effect is positive for treatments announcing a high probability.

Table 3 features regression specifications (analogous to our previous analyses) in which we benchmark all treatments with a specified audit probability against the *Ambiguous* treatment. In specification (1), where we pool all audit treatments, the interaction coefficient of interest is positive, but small and insignificant. Considering the audit treatments separately, the point coefficient becomes larger as we increase the audit probability throughout specifications (2) to (5) (with the exception of the 10% treatment which had been insignificant before). However, while positive in magnitude, the effects of the 1% and 40% treatments are not precisely measured. The level effect for the 60%

[29] To maintain a clean comparison across all treatments (including those without audit probability) and for logistical reasons, we did not survey the participants in the respective treatment groups about their perceived audit probability. In the absence of probability neglect, the comparison between *Ambiguous* and the *Audit 1%*-*Audit 60%* treatments helps to identify whether the prior belief about receiving an audit is high or low. In case of high prior beliefs, the specific deterrence treatments will update the beliefs downwards and compliance will be higher in the *Ambiguous* condition compared with the deterrence treatment with audit probabilities below the prior belief.

treatment group is considerably higher than for the other groups (more than three times as large as for the 1% group and almost twice as large as the 40% group) and statistically significant at the 10% level, both in the specification with 4 (Panel A) and 10 (Panel B) post-treatment months. Thus, in our experiment, the announcement of a very high audit probability (60%) seems to have a positive compliance effect compared to a treatment with an ambiguous audit probability. The behavior of firms is thus inconsistent with probability neglect and a risk-as-feelings notion.

RESULT 3 (General audit threat versus specified audit probability). *Consistent with standard models of deterrence, announcing a specific high audit probability increases SSC compliance relative to a treatment (Ambiguous) that communicates an audit threat without specifying an audit probability.*

TABLE 3. Treatment effects of audit probability messages relative to ambiguous treatment on SSC

Panel A:	**4 post-treatment months**				
	Audit all (1)	*Audit 1%* (2)	*Audit 10%* (3)	*Audit 40%* (4)	*Audit 60%* (5)
Absolute effect	88.147	123.758	-91.363	235.713	442.883*
	(115.897)	(173.439)	(140.083)	(176.692)	(253.200)
Relative effect	-0.002	-0.007	-0.012	0.023	0.018
	(0.012)	(0.013)	(0.018)	(0.022)	(0.019)
Observations	68868	37967	37462	26050	23087
No of firms	10249	5665	5567	3881	3407
Panel B:	**10 post-treatment months**				
	Audit all (1)	*Audit 1%* (2)	*Audit 10%* (3)	*Audit 40%* (4)	*Audit 60%* (5)
Absolute effect	127.296	134.233	-41.441	253.383	562.706*
	(119.104)	(153.703)	(172.723)	(183.467)	(322.939)
Relative effect	0.001	-0.009	-0.005	0.025	0.024
	(0.012)	(0.014)	(0.020)	(0.020)	(0.019)
Observations	118900	65529	64665	44969	39872
No of firms	10249	5665	5567	3881	3407

Notes: Treatment effects of audit probability messages relative to the ambiguous message on SSC. Displayed are DiD regression estimates of treatment messages on tax base SSC. The estimated treatment effects are relative to the audit ambiguous treatment. Treatment effects are reported in absolute terms based on OLS regressions and in relative terms based on poisson regressions. SSC tax base is the monthly reported tax base for social security payments expressed in BGN. The pre-treatment time period includes four months before and the post-treatment time period four (Panel A) or ten (Panel B) months after the treatment. The pre-experiment control mean of tax base SSC in the ambiguous condition is BGN 7,136. A firm's number of employees and its industry affiliation are included as controls. Standard errors clustered on firm level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The Role of Performed Audits. We observe the highest compliance levels in the *Audit 40%* and *Audit 60%* treatments. Because our experiment is non-deceptive, by design the share of firms that received an audit is higher in these treatments than in treatments with lower announced audit probabilities. One potential explanation for our results could therefore be that the higher compliance levels are due to the performed audits rather than the announced audit probability. To disentangle the difference between the effects of announcing an audit probability and the effects of having received an audit, the NRA

provided us with data on the dates on which audits were implemented. We run our main DiD specifications for the deterrence treatments, but exclude all firm-months after the performance of an audit. The resulting estimates can therefore not be driven by the performed audit, but are necessarily due to the announcement of the audit.

The corresponding results presented in Table C.16 in Appendix C.8 are very similar to our main results. Firms in the audit treatments which did not (yet) receive an audit increase their reported tax base by more than the firms in the benchmark group. The effects are again larger for the higher audit probabilities and also the effects' sizes are very comparable. This suggests that firms report higher tax bases in expectation of potential upcoming audits.

The Role of Bankruptcies. Firms that comply due to the treatment may face a disadvantage compared to non-compliant competitors. To study if treated firms are more likely to go out of business, we compare the share of firms who cease reporting SSC in the ten months after the treatment mailing across experimental groups. No evidence of treatment-induced bankruptcies was found, with a roughly 5% share of zero reporting firms in all conditions and no statistically significant differences.

5.3 *Implications for Tax Revenue*

Using a simple back-of-the-envelope cost-benefit analysis, we determine the benefits in terms of increased tax revenue for the two main treatment groups and compare them to the costs of the interventions. Our results are based on reported SSC payments on the firm level without adjustments after audits and excluding penalties levied on non-compliant firms, and therefore likely pose lower bounds.[30]

To calculate the increase in payroll tax revenue from the RCT, we multiply the estimated treatment effects from Tables 1 and 2 by the number of treated firms, the number of post-treatment months, and the average payroll tax rate. The costs of the treatment interventions are then subtracted. For the moral appeals, costs are assumed to be minimal since the messages were prepared by researchers and sending requires minimal effort on the side of the tax authority.[31] Based on information about standard audit costs provided by the tax authority (based on the average duration and cost of an audit), we assume $20\ (hours)\ x\ 50\ (BGN/h) =$ BGN 1,000 (about USD 500) as costs for an audit in the deterrence treatments.[32]

Using the explained calculation ($[(DiDEstimate) \times (NoTreatedFirms) \times (Months) \times (TaxRate)]$) for the (pooled) *Moral* treatments, we arrive at RCT-induced revenue effects of BGN 7,433,816 (USD 3,739,165) in the four months after the intervention, and

[30] Note that we also do not account for higher personal income tax reporting of employees which increases with higher payroll tax compliance. We further do not include increases in VAT compliance in our simple back-of-the-envelope calculation. See Section E.3 for potential spillover effects of SSC compliance on VAT.

[31] Even assuming two working days for one individual with an hourly wage of 50 (BGN/h) for sending all the emails in the moral treatments only reduces the total revenue from all moral treatments together by BGN 800. This implies a negligible per-letter cost of BGN 0.035 (or roughly $ 0.02) in the moral treatments. Note that the assumed labor investment (16 hours) and wage (BGN 50/h) are likely overestimates.

[32] We refer to audits "during the next months". Framing the audit letters differently could have resulted in even stronger and longer-term effects, thus altering the cost-benefit calculation.

BGN 15,524,782 (USD 7,808,872) for the overall (10 months) treatment revenue. For the *Deterrence* treatments, we base our cost-benefit calculation on the following equation: $[(DiDEstimate) \times (NoTreatedFirms) \times (Months) \times (TaxRate)] - (AuditCosts)$. This results in BGN 2,229,973 (USD 1,121,663) revenue benefit occuring four months after the intervention and BGN 6,076,429 (USD 3,056,407) as the overall revenue from the RCT (10 months after).

The RCT triggered a substantial increase in collective social security payments of BGN 21,601,210 (approximately USD 10,856,280). The additional revenue is equivalent to closing 5% of the estimated revenue gap (Williams and Horodnic, 2017). Moreover, considering that the average pension in Bulgaria was BGN 345.46 (USD 147) during the time of the experiment (LINK), it is equivalent to yearly pensions for 5,210 individuals.

TABLE 4. Experiment induced extra SSC revenue per letter

Panel A:		**Moral Treatments**				
		Moral all (1)	*Cooperation* (2)	*Example* (3)	*Necessity* (4)	*Picture* (5)
Immediate revenue	(*in BGN*)	333.83	365.55	321.29	349.85	290.44
	(*in USD*)	167.92	183.87	161.61	175.97	146.09
Overall revenue	(*in BGN*)	697.18	988.25	525.32	612.57	624.21
	(*in USD*)	350.68	497.08	264.23	308.12	313.97

Panel B:		**Deterrence Treatments**					
		Audit all (1)	*Audit 1%* (2)	*Audit 10%* (3)	*Audit 40%* (4)	*Audit 60%* (5)	*Ambiguous* (6)
Immediate revenue	(*in BGN*)	217.58	347.58	34.87	124.62	197.86	248.76
	(*in USD*)	109.44	174.83	17.54	62.68	99.52	125.12
Overall revenue	(*in BGN*)	592.88	681.78	128.49	743.31	1,518.89	387.16
	(*in USD*)	298.22	342.93	64.63	373.88	763.99	194.74

Notes: Per mailing/letter revenue (in BGN) in the moral (Panel A) and deterrence (Panel B) treatments. The first two rows of each panel show the revenue up to four months after the intervention in BGN and USD respectively. The last two rows of each panel show the revenue up to ten months after the intervention in BGN and USD respectively. Revenue is calculated as *DiD Estimate* × *No of Treated Firms* × *Month after Intervention* × *Tax Rate* − *Intervention Cost*. The cost of the intervention is assumed to be zero in moral treatments and is equal BGN 1,000 per audit ($20\ hours \times$ BGN $50/hour$) in the deterrence treatments.

For tax authorities, understanding the extra benefit per letter/mailing sent in each treatment is relevant. This information helps assessing the cost-benefit of different policies and informs future interventions (e.g., how to scale efficiently; see List, 2022 on the importance of scaling).[33] To be implemented in practice, it is sometimes required that the benefits of the intervention exceed the costs by a certain margin. Table 4 displays the per letter revenue in the different treatment arms. The table shows that the revenue effects in our study are substantial in all treatment conditions. Panel A shows the results for *Moral* treatments and Panel B for the *Deterrence* treatments. The additional SSC are calculated in the same way as described above for the overall tax revenue. The table

[33] We acknowledge that tax revenue maximization is not an optimal tax policy and that optimal tax policy would expand enforcement activities to the point where their marginal cost equals their marginal social benefit (as shown in Slemrod and Yitzhaki, 1987). However, marginal social benefits can be difficult to calculate for tax authorities in practice and, therefore, weighting the revenue benefits against the enforcement costs may be a practical approach to shed light on the effectiveness of enforcement efforts.

shows that, because of the high cost of audits, the *Moral* treatments are overall more cost effective. They generate a per letter revenue of BGN 697 (USD 350) for ten post months, whereas the *Deterrence* treatments generate an additional overall SSC income of BGN 593 (USD 298). Confirming the results from the previous analysis, the *Cooperation* treatment and the (*Audit 60%*) treatment are effective and efficient. The *Cooperation* treatment generates an overall benefit of BGN 988 (USD 497) per letter. The *Audit 60%* treatment generates additional SSC of BGN 1,519 (USD 764) per letter. The most effective *Deterrence* treatment thus generates almost 50% more payroll tax revenue than the 'best' performing *Moral* treatment when considering the entire time span for which we have data.

To further examine the difference between deterrence and moral-appeal approaches, Appendix C.9 compares the empirical effects of moral appeals and our high-audit treatments. The specifications are comparable to the previous DiD analyses, except that they directly benchmark the moral treatments against different audit treatments. The table consistently shows positive and sizable coefficients for the high-audit treatments relative to the moral treatments. Comparing firms in any moral treatment (pooled) against firms in the groups with high audit probabilities (i.e., 40% or 60%), we obtain differences that are statistically significant from zero both four and ten month after the experiment.

6. Conclusion

We present findings from a field experiment on payroll tax compliance among firms in Bulgaria. Payroll tax evasion poses a challenge for governments and tax authorities. It is difficult to detect because of its collusive nature and the incentive for both employer and employee to remain silent. Moreover, it affects the social security system as a whole, and although the incentives between employers and employees to collude appear to be aligned, it may actually be harmful to employees. Since employees rarely invest the additional income from evasion, payroll tax evasion can lead to individuals retiring with (much) lower pensions or receiving lower health and unemployment insurance benefits (see, e.g., Brown et al., 2015 for literature on immediate vs. long term payment and pension streams and Bütler and Teppa, 2007, Heijdra and Romp, 2009 on retirement traps). Despite its importance, payroll tax evasion is not sufficiently studied in existing work. We narrow this research gap using a 'field experiment *across* firms' (Bandiera et al., 2011) which are infrequently used in existing compliance work.

In our experiment, we investigate whether and how deterrence measures and moral appeals targeted at the presumably stronger party in collusive payroll tax evasion break up collusion and increase compliance. Compared with an active control group, all types of moral appeals and deterrence threats significantly and substantially increase monthly payroll tax compliance in the months following our intervention. While the different moral messages used in our experiment largely have comparable effects on tax compliance, firms are more sensitive to deterrence messages with higher audit probabilities (compared to lower probabilities). We further find that specifying high audit probabilities triggers a larger effect than announcing unspecified audit probabilities, suggesting that firms respond to higher audit threats.

Our deterrence treatments contribute to the literature and to policy making by showing that long existing theories on tax compliance (Allingham and Sandmo, 1972) are applicable to the firm and SSC context and that it pays off for tax authorities to invest in audit and investigation capacities as effects are sustainable if threats are substantially high. Considering our moral appeal treatments, our results substantiate, and add to, previous findings which mostly cover individual taxpayers. We show that moral messages appealing to the fiscal exchange character of taxes and social security contributions increase compliance of firms. Firms (or decision-makers within firms) thus seem to have social preferences and react to messages highlighting the benefits from cooperation. This is, to some degree, remarkable as firms who comply with their taxes in a mostly non-compliant environment reduce their competitiveness as higher compliance comes with higher cost. However, it's important to note that the effects of most moral messages are more short-lived and only the simplest form of moral messages comes with a somewhat more persistent compliance effect. Many studies using moral messages and yearly data may therefore not have identified effects. Policymakers can use this simple and cheap intervention of sending emails to boost compliance in the immediate wake of treatment reception.

Our experiment was conducted in Bulgaria, which is classified as a middle-income country by the World Bank. While most countries around the globe are classified as middle-income and are thus somewhat comparable to Bulgaria, the literature mostly focuses on either the (OECD) high-income countries or very low-income countries, thereby somewhat neglecting the "middle class" of countries. We think, however, that research on this category of 'normal' countries is important and our experiment can help inform policies in similar countries. A well functioning tax collection may boost development and enable middle-income countries to close the gap to high-income countries. Our experiment constitutes an important first step in that direction.

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APPENDIX A: RANDOMIZATION AND SUMMARY STATISTICS

TABLE A.1. Summary statistics and balance across all treatments

	Main sample	*No letter*	*Baseline*	*Deterrence*	*Moral*	*Survey*	p-value test
Panel A:			**Outcome Variables**				
SSC 2016	8017.53	8089.55	8004.00	7788.17	8001.22	7848.12	0.9710
(Pre experiment averages 2016)	(40393.83)	(42514.68)	(33976.24)	(43732.58)	(37003.14)	(31669.35)	
VAT 2016	16276.30	15673.17	14344.14	16001.82	17131.56	20914.20	0.7132
(Pre experiment averages 2016)	(983559.57)	(1300714.86)	(167882.66)	(223994.03)	(364186.21)	(417082.61)	
Panel B:			**No of Employees**				
Share of firms with less than 11 employees	0.8561	0.8556	0.8572	0.8551	0.8567	0.8593	0.9459
	(0.3510)	(0.3514)	(0.3499)	(0.3520)	(0.3504)	(0.3478)	
Share of firms with 11 to 30 employees	0.0916	0.0913	0.0908	0.0938	0.0930	0.0860	0.4963
	(0.2885)	(0.2881)	(0.2873)	(0.2915)	(0.2904)	(0.2804)	
Share of firms with more than 30 employees	0.0520	0.0528	0.0516	0.0509	0.0500	0.0547	0.4558
	(0.2220)	(0.2237)	(0.2213)	(0.2199)	(0.2179)	(0.2274)	
Panel C:			**Industry Affiliation**				
Share of firms in manufacturing	0.1227	0.1224	0.1150	0.1212	0.1254	0.1245	0.2696
	(0.3281)	(0.3278)	(0.3190)	(0.3264)	(0.3312)	(0.3302)	
Share of firms in construction	0.0781	0.0793	0.0691	0.0776	0.0784	0.0754	0.0750
	(0.2683)	(0.2701)	(0.2537)	(0.2675)	(0.2687)	(0.2641)	
Share of firms in wholesale trade	0.1202	0.1208	0.1213	0.1208	0.1168	0.1254	0.3751
	(0.3252)	(0.3259)	(0.3265)	(0.3259)	(0.3211)	(0.3312)	
Share of firms in retail trade	0.2002	0.1990	0.2112	0.2053	0.2004	0.1895	0.0360
	(0.4001)	(0.3992)	(0.4082)	(0.4039)	(0.4003)	(0.3919)	
Share of firms in food and beverage	0.0393	0.0391	0.0440	0.0387	0.0385	0.0407	0.4418
	(0.1943)	(0.1937)	(0.2052)	(0.1930)	(0.1925)	(0.1975)	
Share of firms in transport	0.0617	0.0638	0.0606	0.0580	0.0594	0.0597	0.0548
	(0.2407)	(0.2445)	(0.2387)	(0.2337)	(0.2363)	(0.2370)	
Share of firms in agriculture	0.0592	0.0589	0.0596	0.0594	0.0603	0.0570	0.8979
	(0.2360)	(0.2354)	(0.2367)	(0.2364)	(0.2381)	(0.2319)	
No of firms	95508	51894	5540	10249	22268	5557	

Notes: Pre-experimental average for outcome variables and different firm characteristics across treatments with standard deviation in parentheses. The last column of each row presents the p-value testing the null hypothesis that the mean is equal for all treatment groups. Data on SSC and VAT tax bases and firm characteristics come from administrative tax records. SSC and VAT are expressed in BGN.

TABLE A.2. Summary statistics and balance across moral treatments

	Baseline	*Cooperation*	*Example*	*Necessity*	*Picture*	p-value test
Panel A:			**Outcome Variables**			
SSC	8003.999	7725.060	7112.225	8946.541	8204.141	0.2103
(Pre-experiment average tax base 2016)	(33976.243)	(35036.859)	(36546.137)	(42460.071)	(33236.669)	
VAT	14344.145	14871.930	18964.633	15979.228	18709.149	0.8008
(Pre-experiment average tax base 2016)	(167882.659)	(195274.907)	(575308.122)	(275328.999)	(293556.660)	
Panel B:			**No of Employees**			
Share of firms with less than 11 employees	0.857	0.859	0.865	0.852	0.850	0.1522
	(0.350)	(0.348)	(0.341)	(0.355)	(0.357)	
Share of firms with 11 to 30 employees	0.091	0.090	0.090	0.095	0.096	0.7211
	(0.287)	(0.287)	(0.287)	(0.294)	(0.295)	
Share of firms with more than 30 employees	0.052	0.050	0.044	0.052	0.054	0.1201
	(0.221)	(0.219)	(0.205)	(0.222)	(0.225)	
Panel C:			**Industry Affiliation**			
Share of firms in manufacturing	0.115	0.123	0.126	0.131	0.121	
	(0.319)	(0.328)	(0.332)	(0.338)	(0.326)	0.1039
Share of firms in construction	0.069	0.081	0.077	0.077	0.079	0.1609
	(0.254)	(0.272)	(0.266)	(0.266)	(0.270)	
Share of firms in wholesale trade	0.121	0.114	0.123	0.114	0.117	0.4333
	(0.327)	(0.318)	(0.328)	(0.317)	(0.321)	
Share of firms in retail trade	0.211	0.206	0.194	0.205	0.197	0.1278
	(0.408)	(0.404)	(0.395)	(0.404)	(0.398)	
Share of firms in food and beverage	0.044	0.035	0.036	0.043	0.039	0.0602
	(0.205)	(0.185)	(0.187)	(0.204)	(0.193)	
Share of firms in transport	0.061	0.062	0.058	0.060	0.057	0.8393
	(0.239)	(0.241)	(0.234)	(0.238)	(0.233)	
Share of firms in agriculture	0.060	0.057	0.065	0.055	0.065	0.0637
	(0.237)	(0.231)	(0.247)	(0.227)	(0.246)	
No of firms	5540	5529	5548	5617	5574	

Notes: Pre-experimental average for outcome variables and different firm characteristics across moral treatments with standard deviation in parentheses. The last column of each row presents the p-value testing the null hypothesis that the mean is equal for all treatment groups. Data on SSC and VAT tax bases and firm characteristics come from administrative tax records. SSC and VAT are expressed in BGN.

TABLE A.3. Summary statistics and balance across deterrence treatments

	Baseline	*Audit 1%*	*Audit 10%*	*Audit 40%*	*Audit 60%*	*Ambiguous*	p-value test
Panel A:				**Outcome Variables**			
SSC	8003.999	8453.453	7311.148	7527.440	10046.234	7136.360	0.5518
(Pre-experiment average tax base 2016)	(33976.243)	(65820.641)	(26178.217)	(35865.834)	(41969.878)	(30137.210)	
VAT	14344.145	16417.453	15362.765	14175.889	11240.609	18084.251	0.8710
(Pre-experiment average tax base 2016)	(167882.659)	(160523.255)	(302770.938)	(190603.144)	(151902.590)	(213604.991)	
Panel B:				**No of Employees**			
Share of firms with less than 11 employees	0.857	0.852	0.857	0.879	0.832	0.852	0.1098
	(0.350)	(0.355)	(0.350)	(0.326)	(0.374)	(0.356)	
Share of firms with 11 to 30 employees	0.091	0.093	0.094	0.069	0.102	0.103	0.0154
	(0.287)	(0.291)	(0.292)	(0.253)	(0.302)	(0.304)	
Share of firms with more than 30 employees	0.052	0.054	0.049	0.052	0.066	0.046	0.4047
	(0.221)	(0.226)	(0.215)	(0.223)	(0.249)	(0.209)	
Panel C:				**Industry Affiliation**			
Share of firms in manufacturing	0.115	0.127	0.126	0.124	0.123	0.108	0.1952
	(0.319)	(0.333)	(0.332)	(0.329)	(0.329)	(0.311)	
Share of firms in construction	0.069	0.075	0.079	0.070	0.085	0.080	0.3468
	(0.254)	(0.263)	(0.270)	(0.256)	(0.279)	(0.271)	
Share of firms in wholesale trade	0.121	0.126	0.117	0.117	0.097	0.127	0.2599
	(0.327)	(0.332)	(0.321)	(0.321)	(0.296)	(0.333)	
Share of firms in retail trade	0.211	0.205	0.205	0.188	0.198	0.215	0.4488
	(0.408)	(0.404)	(0.404)	(0.391)	(0.399)	(0.411)	
Share of firms in food and beverage	0.044	0.038	0.032	0.056	0.063	0.033	0.0003
	(0.205)	(0.192)	(0.177)	(0.230)	(0.243)	(0.179)	
Share of firms in transport	0.061	0.050	0.059	0.053	0.055	0.069	0.0626
	(0.239)	(0.217)	(0.235)	(0.225)	(0.229)	(0.253)	
Share of firms in agriculture	0.060	0.060	0.057	0.061	0.069	0.058	0.9256
	(0.237)	(0.237)	(0.232)	(0.240)	(0.254)	(0.234)	
No of firms	5540	2908	2810	1124	650	2757	

Notes: Pre-experimental average for outcome variables and different firm characteristics across deterrence treatments with standard deviation in parentheses. The last column of each row presents the p-value testing the null hypothesis that the mean is equal for all treatment groups. Data on SSC and VAT tax bases and firm characteristics come from administrative tax records. SSC and VAT are expressed in BGN.

APPENDIX B: TREATMENT LETTERS

B.1 *Baseline Condition*

ДО
ПРЕДСТАВИТЕЛ НА
АТ- [redacted] АМБУЛАТОРИЯ ЗА
ИНДИВИДУАЛНА ПРАКТИКА ЗА СПЕЦИАЛИЗИРАНА
МЕДИЦИНСКА ПОМОЩ ПО ДЕРМАТОЛОГИЯ И ВЕНЕРОЛОГИЯ

Уважаеми данъкоплатци,

Желаем да направим плащането на данъци и осигурителни вноски възможно най-удобно за Вас. В тази връзка, бихме искали да Ви информираме за нашия сайт (www.nra.bg), където може да откриете повече информация относно данъците и задължителните осигурителни вноски.

Надяваме се да намерите сайта ни за полезен!

С уважение,

Александър Георгиев – заместник изпълнителен директор на НАП

(a) Original email in Bulgarian

Dear taxpayer,

We wish to make your payment of taxes and social insurance contributions as convenient as possible. In this regard, we would like to make you aware of our website (include link) where you find much information relating to your tax payments and social insurance contributions. We hope you find our online appearance useful.

Yours sincerely,

SIGNATURE

(b) Email text in English

FIGURE B.1. Baseline condition

B.2 *Moral Appeals*

ДО
ПРЕДСТАВИТЕЛ НА
АТ- [redacted] АМБУЛАТОРИЯ ЗА
ИНДИВИДУАЛНА ПРАКТИКА ЗА СПЕЦИАЛИЗИРАНА
МЕДИЦИНСКА ПОМОЩ ПО ДЕРМАТОЛОГИЯ И ВЕНЕРОЛОГИЯ

Уважаеми данъкоплатци,

Желаем да направим плащането на данъци и осигурителни вноски възможно най-удобно за Вас. В тази връзка, бихме искали да Ви информираме за нашия сайт (www.nra.bg), където може да откриете повече информация относно данъците и задължителните осигурителни вноски.

Надяваме се да намерите сайта ни за полезен!

Бихме искали да Ви напомним също, че плащането на данъци и задължителни осигурителни вноски е граждански дълг. Данъците и осигурителните вноски са необходими за поддържането и финансирането на публично предоставени стоки и услуги за Вас и за всички други в Република България.

С уважение,

Александър Георгиев – заместник изпълнителен директор на НАП

(a) Original email in Bulgarian

Dear taxpayer,

We wish to make your payment of taxes and social insurance contributions as convenient as possible. In this regard, we would like to make you aware of our website (include link) where you find much information relating to your tax payments and social insurance contributions. We hope you find our online appearance useful.

We would also like to remind you that paying taxes and social insurance contributions is a civic duty. Taxes and social security contributions are necessary to maintain and finance publicly provided public goods and services for you and everybody in Bulgaria.

Yours sincerely, SIGNATURE

(b) Email text in English

FIGURE B.2. Cooperation treatment

ДО
ПРЕДСТАВИТЕЛ НА
АТ- [redacted] АМБУЛАТОРИЯ ЗА
ИНДИВИДУАЛНА ПРАКТИКА ЗА СПЕЦИАЛИЗИРАНА
МЕДИЦИНСКА ПОМОЩ ПО ДЕРМАТОЛОГИЯ И ВЕНЕРОЛОГИЯ

Уважаеми данъкоплатци,

Използвате ли публичен транспорт? Използвате ли пътищата и публични услуги като здравеопазването? Възползвате ли се от общественото образование? Тогава разбирате, че тези стоки и услуги изискват финансиране!

Желаем да направим плащането на данъци и осигурителни вноски възможно най-удобно за Вас. В тази връзка, бихме искали да Ви информираме за нашия сайт (www.nra.bg), където може да откриете повече информация относно данъците и задължителните осигурителни вноски.

Надяваме се да намерите сайта ни за полезен!

Бихме искали да Ви напомним също, че плащането на данъци и задължителни осигурителни вноски е граждански дълг. Данъците и осигурителните вноски са необходими за поддържането и финансирането на публично предоставени стоки и услуги за Вас и за всички други в Република България.

С уважение,

Александър Георгиев – заместник изпълнителен директор на НАП

(a) Original email in Bulgarian

Dear taxpayer,

We wish to make your payment of taxes and social insurance contributions as convenient as possible. In this regard, we would like to make you aware of our website (include link) where you find much information relating to your tax payments and social insurance contributions. We hope you find our online appearance useful.

We would also like to remind you that paying taxes and social insurance contributions is a civic duty. Taxes and social security contributions are necessary to maintain and finance publicly provided public goods and services for you and everybody in Bulgaria.

Yours sincerely, SIGNATURE

(b) Email text in English

FIGURE B.3. Example treatment

ДО
ПРЕДСТАВИТЕЛ НА
АТ-[redacted] АМБУЛАТОРИЯ ЗА
ИНДИВИДУАЛНА ПРАКТИКА ЗА СПЕЦИАЛИЗИРАНА
МЕДИЦИНСКА ПОМОЩ ПО ДЕРМАТОЛОГИЯ И ВЕНЕРОЛОГИЯ

Уважаеми данъкоплатци,

Използвате ли публичен транспорт? Използвате ли пътищата и публични услуги като здравеопазването? Възползвате ли се от общественото образование? Тогава разбирате, че тези стоки и услуги изискват финансиране!

Желаем да направим плащането на данъци и осигурителни вноски възможно най-удобно за Вас. В тази връзка, бихме искали да Ви информираме за нашия сайт (www.nra.bg), където може да откриете повече информация относно данъците и задължителните осигурителни вноски.

Надяваме се да намерите сайта ни за полезен!

Бихме искали да Ви напомним също, че плащането на данъци и задължителни осигурителни вноски е граждански дълг. Данъците и осигурителните вноски са необходими за поддържането и финансирането на публично предоставени стоки и услуги за Вас и за всички други в Република България.

Без Вашите данъчни плащания и задължителни осигурителни вноски, ние не сме в състояние да поддържаме, например обществените училища, детските градини, болниците и социалноосигурителната система.

С уважение,

Александър Георгиев – заместник изпълнителен директор на НАП

(a) Original email in Bulgarian

Dear taxpayer,

You use public transportation? You use roads and public services such as health care? You have benefited from public education? Then you know that these goods and services require funding!

We wish to make your payment of taxes and social insurance contributions as convenient as possible. In this regard, we would like to make you aware of our website (include link) where you find much information relating to your tax payments and social insurance contributions. We hope you find our online appearance useful.

We would also like to remind you that paying taxes and social insurance contributions is a civic duty. Taxes and social security contributions are necessary to maintain and finance publicly provided public goods and services for you and everybody in Bulgaria. Without your tax payments and social insurance contributions, we are not able to maintain, for example, public schools, kindergartens, hospitals and the social insurance system.

Yours sincerely,

SIGNATURE

(b) Email text in English

FIGURE B.4. Necessity treatment

ДО
ПРЕДСТАВИТЕЛ НА
АТ- [redacted] АМБУЛАТОРИЯ ЗА
ИНДИВИДУАЛНА ПРАКТИКА ЗА СПЕЦИАЛИЗИРАНА
МЕДИЦИНСКА ПОМОЩ ПО ДЕРМАТОЛОГИЯ И ВЕНЕРОЛОГИЯ

Уважаеми данъкоплатци,

Използвате ли публичен транспорт? Използвате ли пътищата и публични услуги като здравеопазването? Възползвате ли се от общественото образование? Тогава разбирате, че тези стоки и услуги изискват финансиране!

Желаем да направим плащането на данъци и осигурителни вноски възможно най-удобно за Вас. В тази връзка, бихме искали да Ви информираме за нашия сайт (www.nra.bg), където може да откриете повече информация относно данъците и задължителните осигурителни вноски.

Надяваме се да намерите сайта ни за полезен!

Бихме искали да Ви напомним също, че плащането на данъци и задължителни осигурителни вноски е граждански дълг. Данъците и осигурителните вноски са необходими за поддържането и финансирането на публично предоставени стоки и услуги за Вас и за всички други в Република България.

Без Вашите данъчни плащания и задължителни осигурителни вноски, ние не сме в състояние да поддържаме, например обществените училища, детските градини, болниците и социалноосигурителната система.

Моля, разгледайте тази снимка! Тя показва един пример за публично финансирани услуги.

С уважение,

Александър Георгиев – заместник изпълнителен директор на НАП

(a) Original email in Bulgarian

Dear taxpayer,

You use public transportation? You use roads and public services such as health care? You have benefited from public education? Then you know that these goods and services require funding!

We wish to make your payment of taxes and social insurance contributions as convenient as possible. In this regard, we would like to make you aware of our website (include link) where you find much information relating to your tax payments and social insurance contributions. We hope you find our online appearance useful.

We would also like to remind you that paying taxes and social insurance contributions is a civic duty. Taxes and social security contributions are necessary to maintain and finance publicly provided public goods and services for you and everybody in Bulgaria. Without your tax payments and social insurance contributions, we are not able to maintain, for example, public schools, kindergartens, hospitals and the social insurance system. Consider the attached picture! It shows an example of a tax-financed playground for children.

Yours sincerely, SIGNATURE

(b) Email text in English

FIGURE B.5. Picture treatment

B.3 *Deterrence Treatments*

ДО
ПРЕДСТАВИТЕЛ НА
АТ-[redacted] АМБУЛАТОРИЯ ЗА ИНДИВИДУАЛНА ПРАКТИКА ЗА СПЕЦИАЛИЗИРАНА МЕДИЦИНСКА ПОМОЩ ПО ДЕРМАТОЛОГИЯ И ВЕНЕРОЛОГИЯ

Уважаеми данъкоплатци,

Желаем да направим плащането на данъци и осигурителни вноски възможно най-удобно за Вас. В тази връзка, бихме искали да Ви информираме за нашия сайт (www.nra.bg), където може да откриете повече информация относно данъците и задължителните осигурителни вноски.

Бихме искали да Ви напомним също, че НАП предприема редица контролни мерки, за да гарантира ефективното събиране на данъците и задължителните осигурителни вноски.

В този контекст, НАП е подбрала произволно група от данъкоплатци, включително Вас, за специално проучване. **Четиридесетима от всеки 100 данъкоплатци в тази група ще бъдат избрани на случаен принцип и ще бъдат обект на контрол в следващите месеци**.

С други думи, налична е **40% вероятност** да бъдете проверен.

С уважение,

Александър Георгиев – заместник изпълнителен директор на НАП

(a) Original email in Bulgarian

Dear taxpayer,

We wish to make your payment of taxes and social insurance contributions as convenient as possible. In this regard, we would like to make you aware of our website (include link) where you find much information relating to your tax payments and social insurance contributions. We hope you find our online appearance useful.

We would also like to remind you that the NRA takes steps and measures such as audits to ensure an effective tax collection.

In this context, the NRA has randomly selected a group of taxpayers ? including you ? for a special investigation. ***40 out of 100 taxpayers in this group will randomly be selected to be subject to an audit during the next months****. In other words, there is a* ***40% probability*** *that you will be audited.*

Yours sincerely, SIGNATURE

(b) Email text in English

FIGURE B.6. Audit 40% treatment

ДО
ПРЕДСТАВИТЕЛ НА
АТ- [redacted] АМБУЛАТОРИЯ ЗА
ИНДИВИДУАЛНА ПРАКТИКА ЗА СПЕЦИАЛИЗИРАНА
МЕДИЦИНСКА ПОМОЩ ПО ДЕРМАТОЛОГИЯ И ВЕНЕРОЛОГИЯ

Уважаеми данъкоплатци,

Желаем да направим плащането на данъци и осигурителни вноски възможно най-удобно за Вас. В тази връзка, бихме искали да Ви информираме за нашия сайт (www.nra.bg), където може да откриете повече информация относно данъците и задължителните осигурителни вноски.

Надяваме се да намерите сайта ни за полезен!

Бихме искали да Ви напомним също, че НАП предприема редица контролни мерки, за да гарантира ефективното събиране на данъците и задължителните осигурителни вноски.

В този контекст, НАП е подбрала произволно група от данъкоплатци, включително Вас, за специално проучване. Има вероятност данъкоплатците в тази група да бъдат обект на контрол в рамките на следващите месеци.

С уважение,

Александър Георгиев – заместник изпълнителен директор на НАП

(a) Original email in Bulgarian

Dear taxpayer,

We wish to make your payment of taxes and social insurance contributions as convenient as possible. In this regard, we would like to make you aware of our website (include link) where you find much information relating to your tax payments and social insurance contributions. We hope you find our online appearance useful.

We would also like to remind you that the NRA takes steps and measures such as audits to ensure an effective tax collection.

In this context, the NRA has randomly selected a group of taxpayers ? including you ? for a special investigation. There is a chance that taxpayers in this group will be subject to an audit during the next months.

Yours sincerely, SIGNATURE

(b) Email text in English

FIGURE B.7. Ambiguous treatment

B.4 *Survey*

ПРЕДСТАВИТЕЛ НА
АТ- [redacted] АМБУЛАТОРИЯ ЗА ИНДИВИДУАЛНА ПРАКТИКА ЗА СПЕЦИАЛИЗИРАНА МЕДИЦИНСКА ПОМОЩ ПО ДЕРМАТОЛОГИЯ И ВЕНЕРОЛОГИЯ

Уважаеми *данъкоплатци*,

Каним Ви да участвате в кратък въпросник, администриран от НАП в сътрудничество с изследователи от Швейцарския федерален технологичен институт (ETH Zurich) и Центъра за европейски икономически изследвания.

Проучването ще Ви отнеме не повече от 10 минути. Ще Ви бъдат зададени няколко въпроса относно Вашето мнение за данъчното облагане в Република България.

Националната агенция за приходите си сътрудничи с научни изследователи с оглед подобряване на данъчната система в България. Целта на въпросника е придобиване на информация относно нагласите, свързани с данъчното облагане в България, сред българските дружества.

Отговорите, които ще предоставите при попълване на въпросника ще бъдат напълно анонимни. Никой никога няма да разкрие самоличността Ви, нито идентичността на Вашето дружество, както и никой няма да може да свърже Вашите отговори в проучването с данните на Вашето дружество.

С цел да гарантира Вашата анонимност, НАП, с настоящата покана, Ви изпраща линк към онлайн въпросника. Съдействащите изследователи не познават нито електронните адреси, нито идентичността на което и да било дружество, поканено да участва в проучването. От друга страна, изследователите извършват администрирането на самия въпросник, като отговорите, дадени от дружествата във въпросника са видими само по анонимен начин за изследователите.

Линкът, поместен по-долу, ще Ви пренасочи към онлайн проучване, до което НАП няма достъп. В проучването няма въпроси относно самоличността Ви. **Това означава, че НАП не разполага с достъп до самото проучване и предоставените в него отговори, както и че изследователите нямат достъп до личните данни и електронните адреси на дружествата.** Това гарантира спазване на абсолютна поверителност, както и че никой не би могъл никога да проследи Вашите отговори.

Данните, получени от Вашите отговори ще бъдат използвани единствено за изследователски цели.

Предварително благодарим за попълването на следния въпросник:

Линк към анкетата!

Или копирайте и поставете посочения по-долу адрес във Вашия интернет браузър:

https://descil.eu.qualtrics.com/jfe/form/SV_732FlMfh571DtyJ

С уважение,

Александър Георгиев – заместник изпълнителен директор на НАП

(a) Original email in Bulgarian

Dear taxpayer,

We invite you to participate in a short questionnaire administered by the NRA in cooperation with researchers from the ETH Zurich and the Center for European Economic Research.

The survey will not take more than 10 minutes and you will be asked a couple of questions regarding your opinion about taxation in Bulgaria. The NRA cooperates with researchers to improve the tax system in Bulgaria. The purpose of the questionnaire is to gain knowledge about the perceptions about taxation in Bulgaria among Bulgarian firms.

In the questionnaire, your answers are completely anonymous. No one will ever know your identity or the identity of your firm, and no one will be able to link your answers in the survey to the identity of your firm.

To ensure your anonymity, the NRA herewith sends out the link to the online questionnaire. The cooperating researchers do not know the email addresses or identities of any firms, which are asked to participate in the survey. On the other hand, the researchers administer the questionnaire itself, and the answers given by the firms in the questionnaire are only visible in an anonymous way to the researchers.

*The link below will redirect you to an online survey to which the NRA has no access. In the survey you are not asked for your identity.**This means, the NRA does not have access to the actual survey and the responses given and the researchers have no access to the identities and email addresses of the firms.** This ensures total anonymity and no one can ever track your responses.*

The data generated from your answers is used for research purposes only.

Thank you very much in advance for filling in the following questionnaire.

Link

Yours sincerely,

SIGNATURE

(b) Email text in English

FIGURE B.8. Survey treatment

APPENDIX C: ROBUSTNESS CHECKS AND ADDITIONAL ANALYSES

C.1 *Treatment Effects without Control Variables*

TABLE C.1. Treatment effects of moral treatments on SSC without control variables

Panel A:	**4 post-treatment months**				
	Moral All (1)	*Cooperation* (2)	*Example* (3)	*Necessity* (4)	*Picture* (5)
Absolute effect	266.140***	240.981**	282.055**	297.134***	244.806**
	(87.973)	(120.067)	(113.613)	(114.640)	(117.876)
Relative effect	0.030***	0.029**	0.041***	0.026**	0.026**
	(0.010)	(0.013)	(0.012)	(0.012)	(0.013)
Observations	186457	74390	74537	74882	74449
No of firms	27808	11069	11088	11157	11114
Panel B:	**10 post-treatment months**				
	Moral All (1)	*Cooperation* (2)	*Example* (3)	*Necessity* (4)	*Picture* (5)
Absolute effect	175.584	210.514	167.069	134.917	189.770
	(117.816)	(145.012)	(136.155)	(161.637)	(159.775)
Relative effect	0.020*	0.026*	0.031**	0.007	0.019
	(0.012)	(0.014)	(0.013)	(0.016)	(0.016)
Observations	321342	128165	128383	129058	128183
No of firms	27808	11069	11088	11157	11114

Notes: Treatment effects of moral messages on SSC without control variables. Displayed are DiD regression estimates of treatment messages on tax base SSC. The estimated treatment effects are relative to the baseline condition. Treatment effects are reported in absolute terms based on OLS regressions and in relative terms based on poisson regressions. SSC tax base is the monthly reported tax base for social security payments expressed in BGN. The pre-treatment time period includes four months before and the post-treatment time period four (Panel A) or ten (Panel B) months after the treatment. The pre-experiment control mean of tax base SSC in the baseline condition is BGN 8,004. Standard errors clustered on firm level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE C.2. Treatment effects of deterrence treatments on SSC without control variables

Panel A:	**4 post-treatment months**					
	Audit All	*Audit 1%*	*Audit 10%*	*Audit 40%*	*Audit 60%*	*Ambiguous*
	(1)	(2)	(3)	(4)	(5)	(6)
Absolute effect	284.528***	274.304**	128.264	484.344***	645.506**	167.373
	(102.769)	(132.153)	(140.289)	(179.331)	(260.201)	(118.150)
Relative effect	0.032***	0.026**	0.021	0.060***	0.050***	0.029**
	(0.011)	(0.012)	(0.016)	(0.021)	(0.018)	(0.014)
Observations	87577	56668	56171	44751	41788	55833
No of Firms	15789	8448	8350	6664	6190	8297
Adjusted R^2	0.000	0.000	0.000	0.000	0.000	0.000
Panel B:	**10 post-treatment months**					
	Audit All	*Audit 1%*	*Audit 10%*	*Audit 40%*	*Audit 60%*	*Ambiguous*
	(1)	(2)	(3)	(4)	(5)	(6)
Absolute effect	202.359	146.583	82.940	377.377*	638.145*	38.130
	(132.662)	(145.885)	(190.202)	(204.159)	(344.545)	(138.751)
Relative effect	0.023*	0.010	0.019	0.049**	0.043**	0.017
	(0.014)	(0.016)	(0.021)	(0.021)	(0.020)	(0.014)
Observations	151018	97633	96783	77073	71976	96194
No of Firms	15789	8448	8350	6664	6190	8297

Notes: Treatment effects of deterrence messages on SSC without control variables. Displayed are DiD regression estimates of treatment messages on tax base SSC. Displayed are DiD regression estimates of treatment messages on tax base SSC. The estimated treatment effects are relative to the baseline condition. Treatment effects are reported in absolute terms based on OLS regressions and in relative terms based on poisson regressions. SSC tax base is the monthly reported tax base for social security payments expressed in BGN. The pre-treatment time period includes four months before and the post-treatment time period four (Panel A) or ten (Panel B) months after the treatment. The pre-experiment control mean of tax base SSC in the baseline condition is BGN 8,004. Standard errors clustered on firm level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C.2 *Treatment Effects Using TWFEs*

TABLE C.3. Treatment effects of moral treatments on SSC using TWFEs

Panel A:	**4 post-treatment months**				
	Moral All (1)	*Cooperation* (2)	*Example* (3)	*Necessity* (4)	*Picture* (5)
Absolute effect	248.342***	219.382*	302.001***	269.302**	202.049*
	(90.184)	(123.036)	(115.735)	(117.031)	(121.519)
Relative effect	0.028***	0.026**	0.042***	0.024**	0.021*
	(0.009)	(0.013)	(0.012)	(0.011)	(0.013)
Firm FE	X	X	X	X	X
Year-month FE	X	X	X	X	X
Observations	186457	74390	74537	74882	74449
No of firms	27808	11069	11088	11157	11114
Panel B:	**10 post-treatment months**				
	Moral All (1)	*Cooperation* (2)	*Example* (3)	*Necessity* (4)	*Picture* (5)
Absolute effect	193.683*	216.563	215.216*	184.367	158.230
	(110.421)	(137.156)	(126.095)	(129.086)	(155.437)
Relative effect	0.022*	0.026*	0.033***	0.015	0.016
	(0.011)	(0.014)	(0.013)	(0.013)	(0.016)
Firm FE	X	X	X	X	X
Year-month FE	X	X	X	X	X
Observations	321342	128165	128383	129058	128183
No of firms	27808	11069	11088	11157	11114

Notes: Treatment effects of moral messages on SSC using TWFEs. Displayed are DiD regression estimates of treatment messages on tax base SSC. The estimated treatment effects are relative to the baseline condition. Treatment effects are reported in absolute terms based on OLS regressions and in relative terms based on poisson regressions. SSC tax base is the monthly reported tax base for social security payments expressed in BGN. The pre-treatment time period includes four months before and the post-treatment time period four (Panel A) or ten (Panel B) months after the treatment. The pre-experiment control mean of tax base SSC in the baseline condition is BGN 8,004. Robust standard are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE C.4. Treatment effects of deterrence treatments on SSC using TWFEs

Panel A:	**4 post-treatment months**					
	Audit All (1)	*Audit 1%* (2)	*Audit 10%* (3)	*Audit 40%* (4)	*Audit 60%* (5)	*Ambiguous* (6)
Absolute effect	296.222***	282.121**	157.093	451.535**	679.372***	228.631*
	(103.165)	(122.323)	(145.552)	(180.709)	(261.865)	(121.898)
Relative effect	0.033***	0.027**	0.023	0.055***	0.056***	0.034**
	(0.010)	(0.011)	(0.016)	(0.021)	(0.017)	(0.013)
Firm FE	X	X	X	X	X	X
Year-month FE	X	X	X	X	X	X
Observations	87577	56668	56171	44751	41788	55833
No of Firms	15789	8448	8350	6664	6190	8297
Panel B:	**10 post-treatment months**					
	Audit All (1)	*Audit 1%* (2)	*Audit 10%* (3)	*Audit 40%* (4)	*Audit 60%* (5)	*Ambiguous* (6)
Absolute effect	243.025*	169.624	149.587	395.341**	691.094**	138.405
	(125.781)	(131.424)	(185.870)	(186.394)	(318.019)	(130.656)
Relative effect	0.027**	0.015	0.024	0.049**	0.053***	0.025*
	(0.013)	(0.015)	(0.020)	(0.020)	(0.019)	(0.014)
Firm FE	X	X	X	X	X	X
Year-month FE	X	X	X	X	X	X
Observations	151018	97633	96783	77073	71976	96194
No of Firms	15789	8448	8350	6664	6190	8297

Notes: Treatment effects of deterrence messages on SSC using TWFEs. Displayed are DiD regression estimates of treatment messages on tax base SSC. Displayed are DiD regression estimates of treatment messages on tax base SSC. The estimated treatment effects are relative to the baseline condition. Treatment effects are reported in absolute terms based on OLS regressions and in relative terms based on poisson regressions. SSC tax base is the monthly reported tax base for social security payments expressed in BGN. The pre-treatment time period includes four months before and the post-treatment time period four (Panel A) or ten (Panel B) months after the treatment. The pre-experiment control mean of tax base SSC in the baseline condition is BGN 8,004.Robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C.3 *Multiple Hypothesis Testing*

TABLE C.5. Multiple hypothesis testing moral treatments

Panel A:		**4 post-treatment months**				
		Moral All (1)	*Cooperation* (2)	*Example* (3)	*Necessity* (4)	*Picture* (5)
TREATMENT EFFECT		278.195	304.621	267.742	291.545	242.031
P-values	*Unadjusted*	0.000***	0.010**	0.009***	0.005***	0.027**
	Adjusted	0.001***	0.020*	0.018**	0.009***	0.053*
Panel B:		**10 post-treatment months**				
		Moral All (1)	*Cooperation* (2)	*Example* (3)	*Necessity* (4)	*Picture* (5)
TREATMENT EFFECT		232.393	329.416	175.107	204.189	208.070
P-values	*Unadjusted*	0.037**	0.023**	0.168	0.169	0.178
	Adjusted	0.074*	0.046**	0.336	0.169	0.357

Notes: Treatment effects and corresponding p-values for the moral treatments. Treatment effect represents the DiD coefficient regressing the treatment message on tax base SSC in BGN. Panel A reports Immediate effects for four months and Panel B Overall effects for ten months after the treatment intervention. A firm's number of employees and its industry affiliation are included as control variables. Unadjusted p-values are not corrected for testing multiple hypotheses. Adjusted p-values are corrected for testing multiple hypothesis. The command used is wyoung by Jones et al. (2019). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE C.6. Multiple hypothesis testing deterrence treatments

Panel A:		**4 post-treatment months**					
		Audit All (1)	*Audit 1%* (2)	*Audit 10%* (3)	*Audit 40%* (4)	*Audit 60%* (5)	*Ambiguous* (6)
TREATMENT EFFECT		277.037	297.985	112.389	437.186	664.887	215.630
P-values	*Unadjusted*	0.002***	0.027**	0.379	0.009***	0.007***	0.048**
	Adjusted	0.004***	0.054*	0.759	0.017**	0.015**	0.096*
Panel B:		**10 post-treatment months**					
		Audit All (1)	*Audit 1%* (2)	*Audit 10%* (3)	*Audit 40%* (4)	*Audit 60%* (5)	*Ambiguous* (6)
TREATMENT EFFECT		235.915	230.592	76.163	381.102	706.298	132.662
P-values	*Unadjusted*	0.059*	0.114	0.668	0.045**	0.032**	0.317
	Adjusted	0.117	0.227	0.793	0.089*	0.064*	0.634

Notes: Treatment effects and corresponding p-values for the deterrence treatments. Treatment effect represents the DiD coefficient regressing the treatment message on tax base SSC in BGN. Panel A reports immediate effects for four months and Panel B overall effects for ten months after the treatment intervention. A firm's number of employees and its industry affiliation are included as control variables. Unadjusted p-values are not corrected for testing multiple hypotheses. Adjusted p-values are corrected for testing multiple hypothesis. The command used is wyoung by Jones et al. (2019). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C.4 *Treatment Effects Varying the Pre-treatment Period*

TABLE C.7. Absolute treatment effects of moral treatments on SSC varying the pre-treatment period

Panel A:		**4 post-treatment months**				
	Pre-treatment months	*Moral All* (1)	*Cooperation* (2)	*Example* (3)	*Necessity* (4)	*Picture* (5)
Absolute effect	4	278.195***	304.621**	267.742***	291.545***	242.031**
		(76.559)	(118.416)	(102.450)	(103.315)	(109.151)
Observations		186377	74366	74505	74850	74409
Absolute effect	6	302.064***	330.848**	291.127***	320.968***	255.381**
		(81.651)	(129.831)	(107.433)	(110.306)	(114.281)
Observations		233877	93320	93421	93938	93382
Absolute effect	8	281.825***	315.919**	251.104**	321.102***	232.006**
		(86.373)	(140.618)	(106.131)	(116.192)	(118.163)
Observations		282031	112532	112600	113286	112645
Absolute effect	10	254.834***	305.432**	204.724*	296.658**	205.259*
		(91.334)	(153.188)	(107.145)	(119.584)	(120.907)
Observations		330618	131910	131953	132809	132084
No of Firms		27808	11069	11088	11157	11114
Panel B:		**10 post-treatment months**				
	Pre-treatment months	*Moral All* (1)	*Cooperation* (2)	*Example* (3)	*Necessity* (4)	*Picture* (5)
Absolute effect	4	232.393**	329.416**	175.107	204.189	208.070
		(111.396)	(144.980)	(127.062)	(148.571)	(154.560)
Observations		321202	128123	128327	129002	128113
Absolute effect	6	302.064***	330.848**	291.127***	320.968***	255.381**
		(81.651)	(129.831)	(107.433)	(110.306)	(114.281)
Observations		233877	93320	93421	93938	93382
Absolute effect	8	235.656**	340.078**	159.839	232.659	197.570
		(117.385)	(163.730)	(129.815)	(155.718)	(160.842)
Observations		416856	166289	166422	167438	166349
Absolute effect	10	208.627*	329.552*	113.804	208.073	170.939
		(122.858)	(176.177)	(135.331)	(161.998)	(164.553)
Observations		465443	185667	185775	186961	185788
No of Firms		27808	11069	11088	11157	11114

Notes: Absolute treatment effects of moral messages on SSC extending the pre-treatment period. Displayed are DiD regression estimates of treatment messages on tax base SSC. The estimated treatment effects are relative to the baseline condition. Treatment effects are reported in absolute terms based on OLS regressions. SSC tax base is the monthly reported tax base for social security payments expressed in BGN. The pre-treatment time period includes four, six, eight or ten months before and the post-treatment time period four (Panel A) or ten (Panel B) months after the treatment. The pre-experiment control mean of tax base SSC in the baseline condition is BGN 8,004. A firm's number of employees and its industry affiliation are included as control variables. Standard errors clustered on firm level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE C.8. Relative treatment effects of moral treatments on SSC varying the pre-treatment period

Panel A:		**4 post-treatment months**				
	Pre-treatment months	*Moral All* (1)	*Cooperation* (2)	*Example* (3)	*Necessity* (4)	*Picture* (5)
Relative effect	4	0.031***	0.034***	0.038***	0.024**	0.027**
		(0.008)	(0.013)	(0.011)	(0.011)	(0.013)
Observations		186377	74366	74505	74850	74409
Relative effect	6	0.033***	0.037***	0.041***	0.026**	0.028**
		(0.009)	(0.014)	(0.012)	(0.011)	(0.013)
Observations		233877	93320	93421	93938	93382
Relative effect	8	0.030***	0.035**	0.036***	0.025**	0.025*
		(0.009)	(0.015)	(0.011)	(0.011)	(0.014)
Observations		282031	112532	112600	113286	112645
Relative effect	10	0.027***	0.034**	0.031***	0.021*	0.022
		(0.009)	(0.016)	(0.011)	(0.011)	(0.013)
Observations		330618	131910	131953	132809	132084
No of Firms		27808	11069	11088	11157	11114
Panel B:		**10 post-treatment months**				
	Pre-treatment months	*Moral All* (1)	*Cooperation* (2)	*Example* (3)	*Necessity* (4)	*Picture* (5)
Relative effect	4	0.024**	0.036**	0.029**	0.012	0.022
		(0.011)	(0.015)	(0.012)	(0.015)	(0.017)
Observations		321202	128123	128327	129002	128113
Relative effect	6	0.027**	0.039**	0.032**	0.014	0.024
		(0.011)	(0.015)	(0.013)	(0.015)	(0.017)
Observations		233877	93320	93421	93938	93382
Relative effect	8	0.024**	0.037**	0.027**	0.013	0.021
		(0.011)	(0.016)	(0.012)	(0.015)	(0.017)
Observations		416856	166289	166422	167438	166349
Relative effect	10	0.021*	0.036**	0.021*	0.009	0.017
		(0.012)	(0.017)	(0.012)	(0.015)	(0.017)
Observations		465443	185667	185775	186961	185788
No of Firms		27808	11069	11088	11157	11114

Notes: Relative treatment effects of moral messages on SSC extending the pre-treatment period. Displayed are DiD regression estimates of treatment messages on tax base SSC. The estimated treatment effects are relative to the baseline condition. Treatment effects are reported in relative terms based on poisson regressions. SSC tax base is the monthly reported tax base for social security payments expressed in BGN. The pre-treatment time period includes four, six, eight or ten months before and the post-treatment time period four (Panel A) or ten (Panel B) months after the treatment. The pre-experiment control mean of tax base SSC in the baseline condition is BGN 8,004. A firm's number of employees and its industry affiliation are included as control variables. Standard errors clustered on firm level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE C.9. Absolute treatment effects of deterrence treatments on SSC varying pre-treatment period

Panel A:					**4 post-treatment months**		
	Pre-treatment months	*Audit All* (1)	*Audit 1%* (2)	*Audit 10%* (3)	*Audit 40%* (4)	*Audit 60%* (5)	*Ambiguous* (6)
Absolute effect	4	277.037***	297.985**	112.389	437.186***	664.887***	215.630**
		(91.270)	(134.778)	(127.836)	(166.276)	(248.315)	(108.930)
Observations		87553	56652	56147	44735	41772	55817
Absolute effect	6	290.132***	361.213**	69.386	437.087**	730.305***	164.477
		(97.549)	(155.765)	(130.839)	(173.830)	(252.150)	(165.002)
Observations		109852	71086	70427	56136	52387	69998
Absolute effect	8	274.282**	405.570**	18.434	368.826**	705.472***	87.776
		(107.480)	(182.735)	(142.031)	(175.473)	(259.653)	(215.013)
Observations		132461	85727	84913	67693	63160	84385
Absolute effect	10	252.257**	425.042**	-17.767	304.088*	647.605**	7.342
		(118.381)	(207.932)	(158.685)	(177.283)	(264.031)	(246.182)
Observations		155275	100509	99522	79358	74024	98894
No of Firms		15789	8448	8350	6664	6190	8297
Panel B:					**10 post-treatment months**		
	Pre-treatment months	*Audit All* (1)	*Audit 1%* (2)	*Audit 10%* (3)	*Audit 40%* (4)	*Audit 60%* (5)	*Ambiguous* (6)
Absolute effect	4	235.915*	230.592	76.163	381.102**	706.298**	132.387
		(124.699)	(145.757)	(177.653)	(189.863)	(329.200)	(132.282)
Observations		150976	97605	96741	77045	71948	96166
Absolute effect	6	249.376**	294.043*	34.016	380.885*	773.112**	82.336
		(127.177)	(162.529)	(176.308)	(195.669)	(331.847)	(180.312)
Observations		173275	112039	111021	88446	82563	110347
Absolute effect	8	233.773*	338.430*	-16.619	313.080	748.405**	6.319
		(135.566)	(186.564)	(186.237)	(198.867)	(341.085)	(229.506)
Observations		195884	126680	125507	100003	93336	124734
Absolute effect	10	211.796	357.832*	-52.576	248.544	690.228*	-73.629
		(146.048)	(209.746)	(203.269)	(210.360)	(353.748)	(261.665)
Observations		218698	141462	140116	111668	104200	139243
No of Firms		15789	8448	8350	6664	6190	8297

Notes: Absolute treatment effects of deterrence messages on SSC extending the pre-treatment period. Displayed are DiD regression estimates of treatment messages on tax base SSC. The estimated treatment effects are relative to the baseline condition. Treatment effects are reported in absolute terms based on OLS regressions. SSC tax base is the monthly reported tax base for social security payments expressed in BGN. The pre-treatment time period includes four, six, eight or ten months before and the post-treatment time period four (Panel A) or ten (Panel B) months after the treatment. The pre-experiment control mean of tax base SSC in the baseline condition is BGN 8,004. A firm's number of employees and its industry affiliation are included as control variables. Standard errors clustered on firm level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE C.10. Relative treatment effects of deterrence treatments on SSC varying pre-treatment period

Panel A:				**4 post-treatment months**			
	Pre-treatment months	*Audit All* (1)	*Audit 1%* (2)	*Audit 10%* (3)	*Audit 40%* (4)	*Audit 60%* (5)	*Ambiguous* (6)
Relative effect	4	0.030***	0.024**	0.020	0.054***	0.051***	0.033***
		(0.009)	(0.010)	(0.015)	(0.020)	(0.017)	(0.013)
Observations		87553	56652	56147	44735	41772	55817
Relative effect	6	0.030***	0.030**	0.013	0.054***	0.057***	0.025
		(0.010)	(0.012)	(0.016)	(0.020)	(0.017)	(0.020)
Observations		109852	71086	70427	56136	52387	69998
Relative effect	8	0.028***	0.033***	0.006	0.046**	0.052***	0.016
		(0.010)	(0.013)	(0.017)	(0.020)	(0.016)	(0.027)
Observations		132461	85727	84913	67693	63160	84385
Relative effect	10	0.025**	0.034**	0.002	0.038**	0.044***	0.006
		(0.011)	(0.013)	(0.019)	(0.019)	(0.015)	(0.031)
Observations		155275	100509	99522	79358	74024	98894
No of Firms		15789	8448	8350	6664	6190	8297
Panel B:				**10 post-treatment months**			
	Pre-treatment months	*Audit All* (1)	*Audit 1%* (2)	*Audit 10%* (3)	*Audit 40%* (4)	*Audit 60%* (5)	*Ambiguous* (6)
Relative effect	4	0.024*	0.015	0.019	0.049**	0.048**	0.024*
		(0.013)	(0.015)	(0.020)	(0.020)	(0.019)	(0.014)
Observations		150976	97605	96741	77045	71948	96166
Relative effect	6	0.025*	0.020	0.011	0.049**	0.054***	0.017
		(0.013)	(0.016)	(0.020)	(0.020)	(0.018)	(0.021)
Observations		173275	112039	111021	88446	82563	110347
Relative effect	8	0.023*	0.023	0.005	0.040**	0.049***	0.007
		(0.013)	(0.016)	(0.021)	(0.019)	(0.018)	(0.028)
Observations		195884	126680	125507	100003	93336	124734
Relative effect	10	0.019	0.024	0.000	0.033*	0.042**	-0.002
		(0.014)	(0.017)	(0.023)	(0.019)	(0.018)	(0.032)
Observations		218698	141462	140116	111668	104200	139243
No of Firms		15789	8448	8350	6664	6190	8297

Notes: Relative treatment effects of deterrence messages on SSC extending the pre-treatment period. Displayed are DiD regression estimates of treatment messages on tax base SSC. The estimated treatment effects are relative to the baseline condition. Treatment effects are reported in relative terms based on poisson regressions. SSC tax base is the monthly reported tax base for social security payments expressed in BGN. The pre-treatment time period includes four, six, eight or ten months before and the post-treatment time period four (Panel A) or ten (Panel B) months after the treatment. The pre-experiment control mean of tax base SSC in the baseline condition is BGN 8,004. A firm's number of employees and its industry affiliation are included as control variables. Standard errors clustered on firm level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C.5 *Treatment Effects Varying the Post-treatment Period*

TABLE C.11. Treatment effects of moral treatments on SSC varying the post-treatment period

	Post-treatment months	*Moral All* (1)	*Cooperation* (2)	*Example* (3)	*Necessity* (4)	*Picture* (5)
Absolute effect	2	216.775***	244.663**	153.530*	235.009***	225.348**
		(64.928)	(109.554)	(85.023)	(87.037)	(89.742)
Relative effect		0.024***	0.028**	0.024**	0.020**	0.026**
		(0.007)	(0.012)	(0.010)	(0.009)	(0.011)
Observations		140373	56019	56091	56359	56063
Absolute effect	4	278.195***	304.621**	267.742***	291.545***	242.031**
		(76.559)	(118.416)	(102.450)	(103.315)	(109.151)
Relative effect		0.031***	0.034***	0.038***	0.024**	0.027**
		(0.008)	(0.013)	(0.011)	(0.011)	(0.013)
Observations		186377	74366	74505	74850	74409
Absolute effect	6	280.934***	335.305***	284.036**	258.842**	237.640*
		(88.329)	(127.697)	(112.855)	(118.838)	(125.567)
Relative effect		0.030***	0.037***	0.041***	0.020	0.026*
		(0.010)	(0.014)	(0.012)	(0.012)	(0.015)
Observations		231952	92526	92727	93153	92578
Absolute effect	8	265.795***	343.128**	236.430**	232.348*	242.357*
		(99.969)	(137.542)	(119.137)	(134.984)	(141.264)
Relative effect		0.029***	0.038***	0.036***	0.017	0.026
		(0.010)	(0.014)	(0.012)	(0.014)	(0.016)
Observations		277005	110489	110725	111234	110532
Absolute effect	10	232.393**	329.416**	175.107	204.189	208.070
		(111.396)	(144.980)	(127.062)	(148.571)	(154.560)
Relative effect		0.024**	0.036**	0.029**	0.012	0.022
		(0.011)	(0.015)	(0.012)	(0.015)	(0.017)
Observations		321202	128123	128327	129002	128113
No of Firms		27808	11069	11088	11157	11114

Notes: Treatment effects of moral messages on SSC varying the post-treatment period. Displayed are DiD regression estimates of treatment messages on tax base SSC. The estimated treatment effects are relative to the baseline condition. Treatment effects are reported in absolute terms based on OLS regressions and in relative terms based on poisson regressions. SSC tax base is the monthly reported tax base for social security payments expressed in BGN. The pre-treatment time period includes four months before and the post-treatment time period two, four, six, eight and ten months after the treatment. The pre-experiment control mean of tax base SSC in the baseline condition is BGN 8,004. A firm's number of employees and its industry affiliation are included as control variables. Standard errors clustered on firm level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE C.12. Treatment effects of deterrence treatments on SSC varying the post-treatment period

	Pre-treatment months	*Audit All* (1)	*Audit 1%* (2)	*Audit 10%* (3)	*Audit 40%* (4)	*Audit 60%* (5)	*Ambiguous* (6)
Absolute effect	2	162.377**	188.123*	37.877	233.309*	477.739**	163.350*
		(75.674)	(111.105)	(102.785)	(131.891)	(206.510)	(96.729)
Relative effect		0.018**	0.014	0.011	0.031*	0.035**	0.026**
		(0.008)	(0.010)	(0.012)	(0.016)	(0.015)	(0.011)
Observations		65930	42672	42273	33694	31450	42008
Absolute effect	4	277.037***	297.985**	112.389	437.186***	664.887***	215.630**
		(91.270)	(134.778)	(127.836)	(166.276)	(248.315)	(108.930)
Relative effect		0.030***	0.024**	0.020	0.054***	0.051***	0.033***
		(0.009)	(0.010)	(0.015)	(0.020)	(0.017)	(0.013)
Observations		87553	56652	56147	44735	41772	55817
Absolute effect	6	303.700***	347.719**	104.312	485.718***	692.831**	204.253*
		(106.315)	(160.507)	(144.150)	(182.075)	(277.185)	(113.500)
Relative effect		0.033***	0.029***	0.020	0.060***	0.052***	0.032**
		(0.010)	(0.011)	(0.017)	(0.021)	(0.018)	(0.013)
Observations		108982	70507	69874	55652	51981	69470
Absolute effect	8	293.199**	352.784*	91.449	427.065**	700.090**	155.031
		(122.057)	(187.490)	(164.513)	(182.657)	(307.464)	(120.777)
Relative effect		0.031***	0.028**	0.020	0.054***	0.050***	0.027**
		(0.011)	(0.012)	(0.019)	(0.020)	(0.018)	(0.013)
Observations		130193	84203	83447	66449	62069	82945
Absolute effect	10	252.257**	425.042**	-17.767	304.088*	647.605**	7.342
		(118.381)	(207.932)	(158.685)	(177.283)	(264.031)	(246.182)
Relative effect		0.024*	0.015	0.019	0.049**	0.048**	0.024*
		(0.013)	(0.015)	(0.020)	(0.020)	(0.019)	(0.014)
Observations		155275	100509	99522	79358	74024	98894
No of Firms		15789	8448	8350	6664	6190	8297

Notes: Treatment effects of deterrence messages on SSC varying the post-treatment period. Displayed are DiD regression estimates of treatment messages on tax base SSC. The estimated treatment effects are relative to the baseline condition. Treatment effects are reported in absolute terms based on OLS regressions and in relative terms based on poisson regressions. SSC tax base is the monthly reported tax base for social security payments expressed in BGN. The pre-treatment time period includes four months before and the post-treatment time period two, four, six, eight and ten months after the treatment. The pre-experiment control mean of tax base SSC in the baseline condition is BGN 8,004. A firm's number of employees and its industry affiliation are included as control variables. Standard errors clustered on firm level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C.6 *Treatment Effects Using only Post-treatment Months*

TABLE C.13. Treatment effects of moral treatments on SSC using only post-treatment months

Panel A:	**4 post-treatment months**				
	Moral all	*Cooperation*	*Example*	*Necessity*	*Picture*
	(1)	(2)	(3)	(4)	(5)
Absolute effect	737.950	596.945	98.394	1497.036**	557.565
	(500.053)	(665.222)	(671.717)	(705.336)	(593.095)
Relative effect	0.084	0.083	0.025	0.152**	0.041
	(0.051)	(0.072)	(0.073)	(0.067)	(0.056)
Observations	92391	36856	36953	37118	36874
No of firms	27808	11069	11088	11157	11114
Panel B:	**10 post-treatment months**				
	Moral all	*Cooperation*	*Example*	*Necessity*	*Picture*
	(1)	(2)	(3)	(4)	(5)
Absolute effect	695.558	625.298	16.961	1411.472**	532.464
	(522.819)	(690.440)	(698.373)	(718.280)	(621.801)
Relative effect	0.078	0.085	0.016	0.140**	0.036
	(0.053)	(0.073)	(0.074)	(0.068)	(0.058)
Observations	227216	90613	90775	91270	90578
No of firms	27808	11069	11088	11157	11114

Notes: Treatment effects of moral messages on SSC using only post-treatment months. Displayed are regression estimates of treatment messages on tax base SSC. The estimated treatment effects are relative to the baseline condition including only the post-treatment period. Treatment effects are reported in absolute terms based on OLS regressions and in relative terms based on poisson regressions. SSC tax base is the monthly reported tax base for social security payments expressed in BGN. The post-treatment time period includes four (Panel A) or ten (Panel B) months after the treatment. The pre-experiment control mean of tax base SSC in the baseline condition is BGN 8,004. A firm's number of employees and its industry affiliation are included as controls. Standard errors clustered on firm level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE C.14. Treatment effects of deterrence treatments on SSC using only post-treatment months

Panel A:	**4 post-treatment months**					
	Audit All	*Audit 1%*	*Audit 10%*	*Audit 40%*	*Audit 60%*	
	(1)	(2)	(3)	(4)	(5)	
Absolute effect	645.337	1223.925	-310.360	152.599	2359.472	
	(768.000)	(1496.569)	(528.280)	(1068.309)	(1638.957)	
Relative effect	0.082	0.131	-0.028	0.033	0.249*	
	(0.077)	(0.132)	(0.056)	(0.115)	(0.145)	
Observations	43428	28090	27855	22173	20720	
No of Firms	13032	8448	8350	6664	6190	
Panel B:	**10 post-treatment months**					
	Audit All	*Audit 1%*	*Audit 10%*	*Audit 40%*	*Audit 60%*	
	(1)	(2)	(3)	(4)	(5)	(6)
Absolute effect	615.373	1166.515	-335.421	118.998	2421.440	
	(776.141)	(1474.323)	(565.625)	(1114.025)	(1730.664)	
Relative effect	0.078	0.123	-0.028	0.031	0.253*	
	(0.076)	(0.128)	(0.058)	(0.118)	(0.149)	
Observations	106851	69043	68449	54483	50896	
No of Firms	13032	8448	8350	6664	6190	

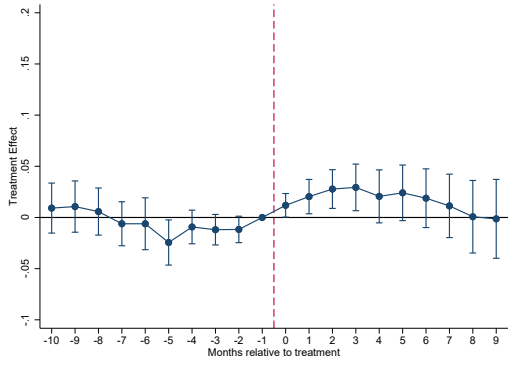
Notes: Treatment effects of deterrence messages on SSC using only post-treatment months. Displayed are regression estimates of treatment messages on tax base SSC. The estimated treatment effects are relative to the baseline condition including only the post-treatment period. Treatment effects are reported in absolute terms based on OLS regressions and in relative terms based on poisson regressions. SSC tax base is the monthly reported tax base for social security payments expressed in BGN. The post-treatment time period includes four (Panel A) or ten (Panel B) months after the treatment. The pre-experiment control mean of tax base SSC in the baseline condition is BGN 8,004. A firm's number of employees and its industry affiliation are included as controls. Standard errors clustered on firm level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE C.15. Treatment effects of audit probability messages relative to ambiguous treatment on SSC using only post-treatment months

Panel A:	**4 post-treatment months**				
	Audit all	*Audit 1%*	*Audit 10%*	*Audit 40%*	*Audit 60%*
	(1)	(2)	(3)	(4)	(5)
Absolute effect	630.239	1196.032	-85.256	-47.953	2319.743
	(698.105)	(1321.211)	(573.709)	(929.350)	(1612.312)
Relative effect	0.085	0.134	-0.007	0.013	0.253*
	(0.078)	(0.125)	(0.064)	(0.092)	(0.137)
Observations	34203	18865	18630	12948	11495
No of firms	10249	5665	5567	3881	3407
Panel B:	**10 post-treatment months**				
	Audit all	*Audit 1%*	*Audit 10%*	*Audit 40%*	*Audit 60%*
	(1)	(2)	(3)	(4)	(5)
Absolute effect	651.831	1206.509	-64.084	-60.807	2420.086
	(703.483)	(1296.689)	(609.825)	(967.523)	(1706.457)
Relative effect	0.087	0.134	-0.003	0.015	0.255*
	(0.077)	(0.122)	(0.067)	(0.093)	(0.143)
Observations	84235	46427	45833	31867	28280
No of firms	10249	5665	5567	3881	3407

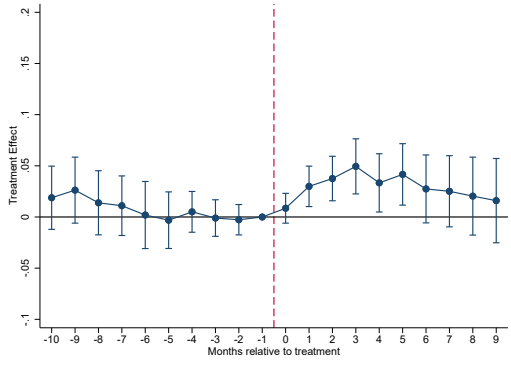
Notes: Treatment effects of audit probability messages relative to the ambiguous message on SSC using only post-treatment months. Displayed are regression estimates of treatment messages on tax base SSC. The estimated treatment effects are relative to the audit ambiguous message including only the post-treatment period. Treatment effects are reported in absolute terms based on OLS regressions and in relative terms based on poisson regressions. SSC tax base is the monthly reported tax base for social security payments expressed in BGN. The post-treatment time period includes four (Panel A) or ten (Panel B) months after the treatment. The pre-experiment control mean of tax base SSC in the baseline condition is BGN 8,004. A firm's number of employees and its industry affiliation are included as controls. Standard errors clustered on firm level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C.7 *Dynamic Relative Treatment Effects*



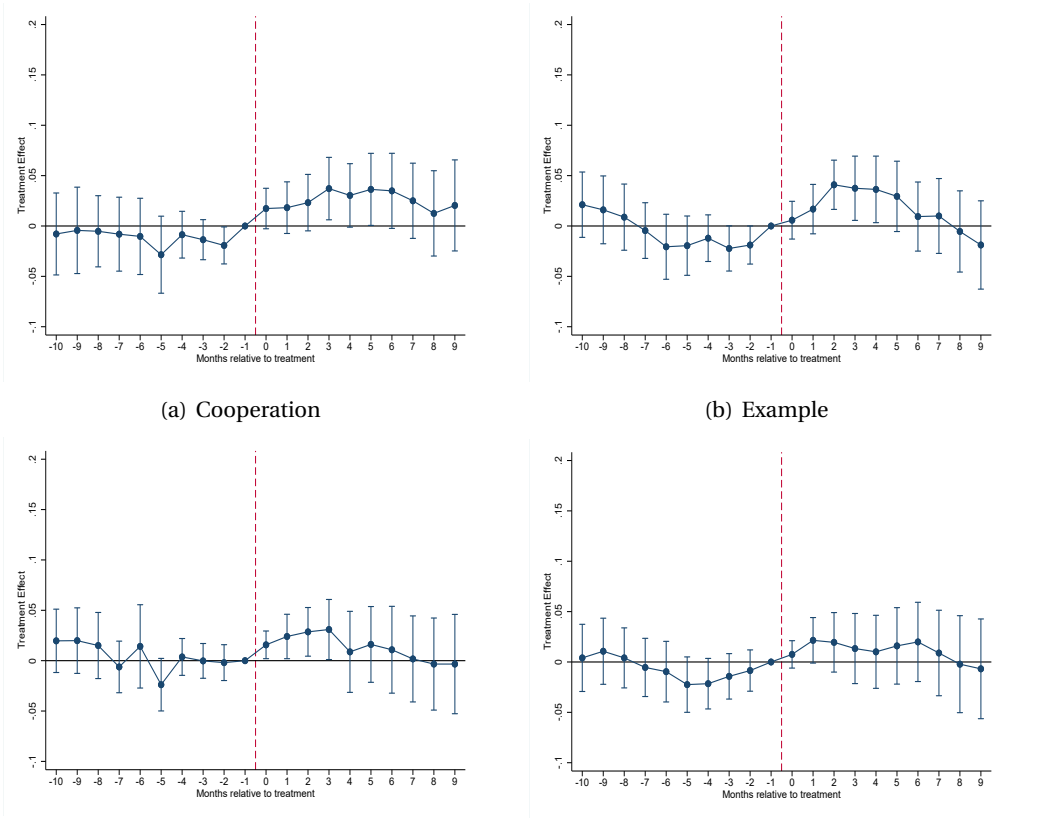
Notes: Pooled monthly relative treatment effects of the moral appeal messages (Cooperation - Picture) on SSC. The points plotted are the estimated DiD poisson regression coefficients of treatment messages on tax base SSC by months relative to the treatment. Treatment effects are relative to the control message. SSC tax base is the monthly reported tax base for social security payments expressed in BGN. The underlying sample includes firms that do not directly communicate with the tax authority. 95% confidence intervals are represented by the blue lines and based on standard errors clustered on firm level.

FIGURE C.1. Dynamic Relative effect s of moral treatments on SSC



Notes: Pooled monthly relative treatment effects of the audit probability messages (Audit 1% - Audit 60%) on SSC. The points plotted are the estimated DiD poisson regression coefficients of treatment messages on tax base SSC by months relative to the treatment. Treatment effects are relative to the control message. SSC tax base is the monthly reported tax base for social security payments expressed in BGN. The underlying sample includes firms that do not directly communicate with the tax authority. 95% confidence intervals are represented by the blue lines and based on standard errors clustered on firm level.

FIGURE C.2. Extended sample: Dynamic Relative effect s of deterrence treatments on SSC



(a) Cooperation

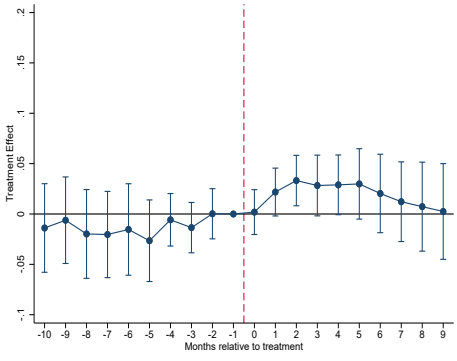
(b) Example

(c) Necessity

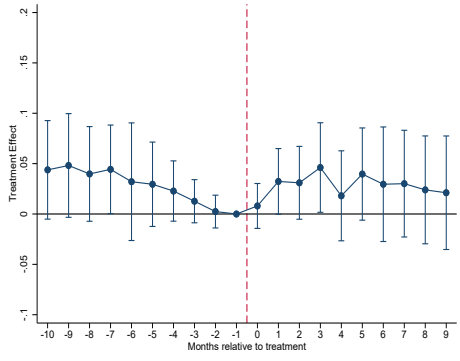
(d) Picture

Notes: Monthly relative treatment effects of moral appeal messages (Cooperation, Example, Necessity, Picture) on SSC. The points plotted are the estimated DiD poisson regression coefficients of treatment messages on tax base SSC by months relative to the treatment. Treatment effects are relative to the control message. SSC tax base is the monthly reported tax base for social security payments expressed in BGN. The underlying sample includes firms that do not directly communicate with the tax authority. 95% confidence intervals are represented by the blue lines and based on standard errors clustered on firm level.

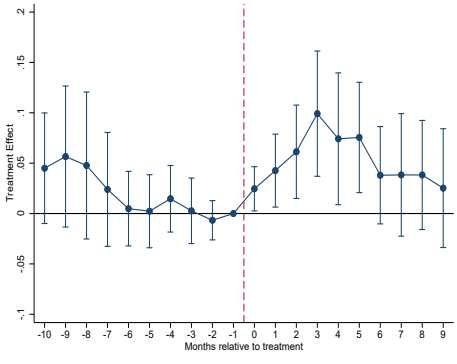
FIGURE C.3. Extended sample: Dynamic Relative effect s of moral treatments on SSC by sub--treatment



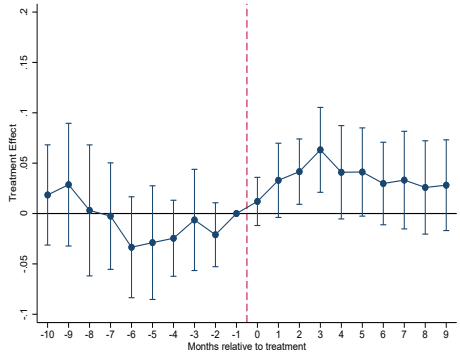
(a) Audit 1%



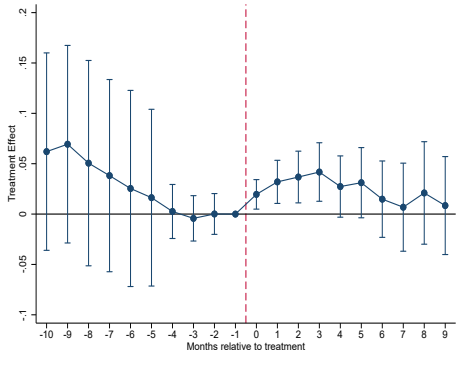
(b) Audit 10%



(c) Audit 40%



(d) Audit 60%



(e) Ambiguous

Notes: Monthly relative treatment effects of audit messages (Audit 1%, Audit 10%, Audit 40%, Audit 60%, Ambiguous) on SSC. The points plotted are the estimated DiD poisson regression coefficients of treatment messages on tax base SSC by months relative to the treatment. Treatment effects are relative to the control message. SSC tax base is the monthly reported tax base for social security payments expressed in BGN. The underlying sample includes firms that do not directly communicate with the tax authority. 95% confidence intervals are represented by the blue lines and based on standard errors clustered on firm level.

FIGURE C.4. Dynamic Relative effect s of deterrence treatments on SSC by sub-treatment

C.8 *Treatment Effects Excluding Audited Firms*

TABLE C.16. Treatment effects of deterrence treatments on SSC excluding audited firms

Panel A:	**4 post-treatment months**					
	Audit all	*Audit 1%*	*Audit 10%*	*Audit 40%*	*Audit 60%*	*Ambiguous*
	(1)	(2)	(3)	(4)	(5)	(6)
Absolute effect	277.037***	297.985**	112.389	437.186***	664.887***	215.630**
	(91.270)	(134.778)	(127.836)	(166.276)	(248.315)	(108.930)
Relative effect	0.030***	0.024**	0.020	0.054***	0.051***	0.033***
	(0.009)	(0.010)	(0.015)	(0.020)	(0.017)	(0.013)
Observations	87553	56652	56147	44735	41772	55817
No of firms	15789	8448	8350	6664	6190	8297
Panel B:	**10 post-treatment months**					
	Audit all	*Audit 1%*	*Audit 10%*	*Audit 40%*	*Audit 60%*	*Ambiguous*
	(1)	(2)	(3)	(4)	(5)	(6)
Absolute effect	267.183**	271.928*	113.970	464.023*	740.744**	192.879
	(130.996)	(146.080)	(180.933)	(268.042)	(333.989)	(131.455)
Relative effect	0.027**	0.019	0.023	0.049*	0.065**	0.031**
	(0.013)	(0.015)	(0.020)	(0.026)	(0.032)	(0.014)
Observations	147811	97348	95864	75764	70793	95880
No of firms	15789	8448	8350	6664	6190	8297

Notes: Treatment effects of deterrence treatments on SSC excluding audited firms' observations after the audit has ended. Displayed are DiD regression estimates of treatment messages on tax base SSC. The estimated treatment effects are relative to the baseline condition.Treatment effects are reported in absolute terms based on OLS regressions and in relative terms based on poisson regressions. SSC tax base is the monthly reported tax base for social security payments expressed in BGN. The pre-treatment time period includes four months before and the post-treatment time period four (Panel A) or ten (Panel B) months after the treatment. The pre-experiment control mean of tax base SSC in the baseline condition is BGN 8,004. A firm's number of employees and its industry affiliation are included as controls. Standard errors clustered on firm level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C.9 *Testing Moral vs. Deterrence Treatments*

TABLE C.17. Testing moral vs. deterrence treatments

Panel A:	**4 post-treatment months**				
	Moral All	*Cooperation*	*Example*	*Necessity*	*Picture*
Audit 40% & 60%					
Absolute effect	254.335*	218.623	251.762	239.516	306.523*
	(140.580)	(167.681)	(155.811)	(156.101)	(161.944)
Relative effect	0.023*	0.018	0.014	0.029*	0.027
	(0.014)	(0.017)	(0.016)	(0.015)	(0.017)
Audit 40%					
Absolute effect	162.424	135.212	166.831	143.879	204.410
	(160.309)	(185.479)	(174.233)	(174.179)	(179.159)
Relative effect	0.024	0.021	0.016	0.031	0.029
	(0.020)	(0.022)	(0.021)	(0.021)	(0.022)
Audit 60%					
Absolute effect	391.206	339.170	376.396	379.905	460.088*
	(244.818)	(259.870)	(253.445)	(253.047)	(261.512)
Relative effect	0.020	0.015	0.011	0.027	0.024
	(0.016)	(0.019)	(0.018)	(0.017)	(0.019)
Panel B:	**10 post-treatment months**				
	Moral All	*Cooperation*	*Example*	*Necessity*	*Picture*
Audit 40% & 60%					
Absolute effect	283.412*	175.720	324.758*	306.612	326.733*
	(165.693)	(190.696)	(175.478)	(191.746)	(198.374)
Relative effect	0.025*	0.013	0.019	0.038**	0.028
	(0.013)	(0.016)	(0.014)	(0.016)	(0.018)
Audit 40%					
Absolute effect	154.205	54.738	204.572	176.939	185.298
	(170.636)	(196.908)	(180.290)	(196.525)	(202.543)
Relative effect	0.025	0.014	0.020	0.038*	0.029
	(0.018)	(0.021)	(0.019)	(0.021)	(0.022)
Audit 60%					
Absolute effect	481.484	358.326	506.451	498.780	547.920
	(317.777)	(329.704)	(323.221)	(330.113)	(340.483)
Relative effect	0.024	0.011	0.018	0.037*	0.027
	(0.017)	(0.020)	(0.018)	(0.020)	(0.021)

Notes: Comparing treatment effects of moral messages with audit messages. The estimated treatment effects for the audit treatments are relative to the respective moral treatments. Teatment effects are reported in absolute terms based on OLS regressions and in relative terms based on poisson regressions. SSC tax base is the monthly reported tax base for social security payments expressed in BGN. The pre-treatment time period includes four months before and the post-treatment time period four (Panel A) or ten (Panel B) months after the treatment. A firm's number of employees and its industry affiliation are included as controls. Standard errors clustered on firm level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

APPENDIX D: EXTENDED SAMPLE (INCLUDING FIRMS THAT DO NOT DIRECTLY COMMUNICATE WITH THE TAX AUTHORITY)

D.1 *Randomization and Summary Statistics*

TABLE D.1. Extended sample: Summary statistics and balance across all treatments

	Extended sample	*No letter*	*Baseline*	*Deterrence*	*Moral*	*Survey*	p-value test
Panel A:			**Outcome Variables**				
SSC 2016	7133.60	7179.88	7124.08	7039.36	7091.88	7051.96	0.9858
(Pre experiment averages 2016)	(34988.56)	(35445.20)	(32199.65)	(41958.72)	(32734.68)	(26893.92)	
VAT 2016	14423.42	13992.75	13542.00	13292.23	15675.03	16430.15	0.5668
(Pre experiment averages 2016)	(748001.59)	(982843.64)	(153140.11)	(211855.91)	(305902.26)	(333256.46)	
Panel B:			**No of Employees**				
Share of firms with less than 11 employees	0.8660	0.8655	0.8693	0.8658	0.8663	0.8668	0.8751
	(0.3406)	(0.3411)	(0.3371)	(0.3409)	(0.3403)	(0.3398)	
Share of firms with 11 to 30 employees	0.0882	0.0879	0.0862	0.0900	0.0892	0.0845	0.4604
	(0.2835)	(0.2832)	(0.2807)	(0.2862)	(0.2851)	(0.2782)	
Share of firms with more than 30 employees	0.0456	0.0463	0.0442	0.0441	0.0442	0.0486	0.1710
	(0.2086)	(0.2102)	(0.2055)	(0.2053)	(0.2055)	(0.2150)	
Panel C:			**Industry Affiliation**				
Share of firms in manufacturing	0.1104	0.1105	0.1046	0.1097	0.1111	0.1134	0.3064
	(0.3134)	(0.3135)	(0.3061)	(0.3125)	(0.3143)	(0.3171)	
Share of firms in construction	0.0748	0.0756	0.0688	0.0748	0.0743	0.0754	0.1595
	(0.2631)	(0.2643)	(0.2531)	(0.2630)	(0.2623)	(0.2640)	
Share of firms in wholesale trade	0.1203	0.1198	0.1239	0.1202	0.1195	0.1242	0.5509
	(0.3253)	(0.3248)	(0.3295)	(0.3252)	(0.3244)	(0.3298)	
Share of firms in retail trade	0.1941	0.1935	0.2051	0.1966	0.1929	0.1880	0.0250
	(0.3955)	(0.3951)	(0.4038)	(0.3974)	(0.3946)	(0.3907)	
Share of firms in food and beverage	0.0444	0.0447	0.0467	0.0441	0.0434	0.6605	0.2448
	(0.2060)	(0.2067)	(0.2110)	(0.2053)	(0.2038)	(0.2049)	
Share of firms in transport	0.0669	0.0685	0.0650	0.0649	0.0649	0.0654	0.0687
	(0.2499)	(0.2527)	(0.2465)	(0.2463)	(0.2464)	(0.2472)	
Share of firms in agriculture	0.0607	0.0605	0.0606	0.0611	0.0613	0.0597	0.9658
	(0.2388)	(0.2384)	(0.2386)	(0.2395)	(0.2399)	(0.2369)	
No of firms	172172	93592	10000	18580	40000	10000	

Notes: Pre-experimental average for outcome variables and different firm characteristics across treatments with standard deviation in parentheses. The last column of each row presents the p-value testing the null hypothesis that the mean is equal for all treatment groups. Data on SSC and VAT tax bases and firm characteristics come from administrative tax records. SSC and VAT are expressed in BGN. The underlying sample includes firms that do not directly communicate with the tax authority.

TABLE D.2. Extended Sample: Summary statistics and balance across moral appeals

	Baseline	*Cooperation*	*Example*	*Necessity*	*Picture*	p-value test
Panel A:			**Outcome Variables**			
SSC	7124.079	6795.104	6612.200	7756.223	7198.823	0.255
(Pre-experiment average tax base 2016)	(32199.652)	(29531.641)	(32223.483)	(39490.918)	(28483.175)	
VAT	13541.999	14624.856	16815.067	15669.303	15590.647	0.810
(Pre-experiment average tax base 2016)	(153140.108)	(189886.149)	(446229.406)	(245860.217)	(280462.937)	
Panel B:			**No of Employees**			
Share of firms with less than 11 employees	0.869	0.869	0.871	0.864	0.861	0.174
	(0.337)	(0.337)	(0.335)	(0.342)	(0.346)	
Share of firms with 11 to 30 employees	0.086	0.088	0.088	0.090	0.091	0.760
	(0.281)	(0.283)	(0.283)	(0.286)	(0.288)	
Share of firms with more than 30 employees	0.044	0.043	0.041	0.045	0.048	0.131
	(0.206)	(0.202)	(0.198)	(0.208)	(0.214)	
Panel C:			**Industry Affiliation**			
Share of firms in manufacturing	0.105	0.108	0.112	0.111	0.113	0.316
	(0.306)	(0.311)	(0.315)	(0.315)	(0.317)	
Share of firms in construction	0.069	0.079	0.074	0.071	0.073	0.087
	(0.253)	(0.270)	(0.261)	(0.258)	(0.260)	
Share of firms in wholesale trade	0.124	0.119	0.121	0.120	0.117	0.696
	(0.329)	(0.324)	(0.326)	(0.325)	(0.322)	
Share of firms in retail trade	0.205	0.195	0.186	0.200	0.191	0.007
	(0.404)	(0.396)	(0.389)	(0.400)	(0.393)	
Share of firms in food and beverage	0.047	0.043	0.042	0.045	0.044	0.409
	(0.211)	(0.202)	(0.199)	(0.208)	(0.206)	
Share of firms in transport	0.065	0.067	0.064	0.066	0.064	0.906
	(0.247)	(0.250)	(0.244)	(0.247)	(0.244)	
Share of firms in agriculture	0.061	0.058	0.064	0.059	0.064	0.220
	(0.239)	(0.234)	(0.245)	(0.236)	(0.245)	
No of Firms	10000	10000	10000	10000	10000	

Notes: Pre-experimental average for outcome variables and different firm characteristics across moral treatments with standard deviation in parentheses. The last column of each row presents the p-value testing the null hypothesis that the mean is equal for all treatment groups. Data on SSC and VAT tax bases and firm characteristics come from administrative tax records. SSC and VAT are expressed in BGN. The underlying sample includes firms that do not directly communicate with the tax authority

TABLE D.3. Extended Sample: Summary statistics and balance across deterrence treatments

	Baseline	*Audit 1%*	*Audit 10%*	*Audit 40%*	*Audit 60%*	*Ambiguous*	p-value test
Panel A:				**Outcome Variables**			
SSC	7124.079	7241.176	6530.959	6652.373	9049.614	7096.467	0.471
(Pre-experiment average tax base 2016)	(32199.652)	(51260.970)	(28338.409)	(29415.911)	(38889.374)	(47961.841)	
VAT	13541.999	15303.190	12096.689	12496.899	8111.472	13456.484	0.750
(Pre-experiment average tax base 2016)	(153140.108)	(225394.888)	(240932.829)	(178280.742)	(215361.926)	(173708.093)	
Panel B:				**No of Employees**			
Share of firms with less than 11 employees	0.869	0.864	0.870	0.875	0.854	0.860	0.302
	(0.337)	(0.343)	(0.336)	(0.330)	(0.353)	(0.347)	
Share of firms with 11 to 30 employees	0.086	0.089	0.087	0.084	0.091	0.097	0.310
	(0.281)	(0.285)	(0.282)	(0.277)	(0.287)	(0.297)	
Share of firms with more than 30 employees	0.044	0.047	0.042	0.041	0.055	0.042	0.399
	(0.206)	(0.211)	(0.201)	(0.198)	(0.228)	(0.201)	
Panel C:				**Industry Affiliation**			
Share of firms in manufacturing	0.105	0.116	0.110	0.111	0.103	0.104	0.310
	(0.306)	(0.320)	(0.313)	(0.314)	(0.303)	(0.306)	
Share of firms in construction	0.069	0.073	0.074	0.072	0.081	0.078	0.381
	(0.253)	(0.260)	(0.262)	(0.259)	(0.272)	(0.268)	
Share of firms in wholesale trade	0.124	0.121	0.119	0.126	0.114	0.119	0.835
	(0.329)	(0.326)	(0.324)	(0.332)	(0.317)	(0.324)	
Share of firms in retail trade	0.205	0.201	0.197	0.185	0.189	0.200	0.327
	(0.404)	(0.401)	(0.398)	(0.388)	(0.392)	(0.400)	
Share of firms in food and beverage	0.047	0.043	0.039	0.057	0.053	0.042	0.326
	(0.211)	(0.203)	(0.193)	(0.232)	(0.223)	(0.201)	
Share of firms in transport	0.065	0.062	0.063	0.059	0.069	0.071	0.364
	(0.247)	(0.240)	(0.244)	(0.237)	(0.254)	(0.257)	
Share of firms in agriculture	0.061	0.058	0.061	0.069	0.065	0.059	0.606
	(0.239)	(0.233)	(0.239)	(0.254)	(0.247)	(0.236)	
No of firms	10000	5200	5000	2000	1180	5000	

Notes: Pre-experimental average for outcome variables and different firm characteristics across deterrence treatments with standard deviation in parentheses. The last column of each row presents the p-value testing the null hypothesis that the mean is equal for all treatment groups. Data on SSC and VAT tax bases and firm characteristics come from administrative tax records. SSC and VAT are expressed in BGN. The underlying sample includes firms that do not directly communicate with the tax authority.

D.2 *Static DiD*

TABLE D.4. Extended sample: Treatment effects of moral appeals on SSC

Panel A:	**4 post-treatment months**				
	Moral all (1)	*Cooperation* (2)	*Example* (3)	*Necessity* (4)	*Picture* (5)
Absolute effect	155.161**	164.369*	160.947*	158.100*	150.170
	(78.519)	(95.715)	(96.108)	(95.192)	(93.985)
Relative effect	0.019**	0.020*	0.026**	0.012	0.021*
	(0.009)	(0.012)	(0.012)	(0.011)	(0.012)
Observations	326681	130802	130913	131216	130220
No of firms	50000	20000	20000	20000	20000
Panel B:	**10 post-treatment months**				
	Moral all (1)	*Cooperation* (2)	*Example* (3)	*Necessity* (4)	*Picture* (5)
Absolute effect	149.626	190.241*	107.894	183.461	134.820
	(95.866)	(112.034)	(107.608)	(120.525)	(115.219)
Relative effect	0.017	0.023*	0.019	0.013	0.019
	(0.011)	(0.013)	(0.012)	(0.013)	(0.014)
Observations	562293	224963	225239	225753	223994
No of firms	50000	20000	20000	20000	20000

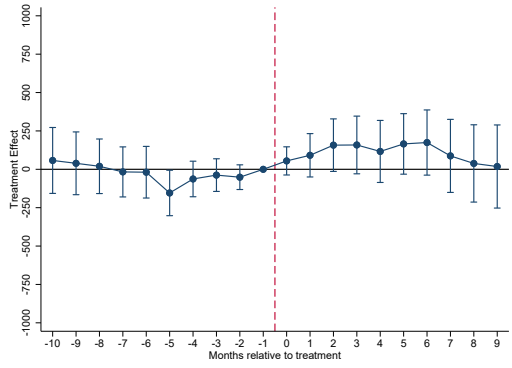
Notes: Treatment effects of moral messages on SSC. Displayed are DiD regression estimates of treatment messages on tax base SSC. Displayed are DiD regression estimates of treatment messages on tax base SSC. The estimated treatment effects are relative to the baseline condition. Treatment effects are reported in absolute terms based on OLS regressions and in relative terms based on poisson regressions. SSC tax base is the monthly reported tax base for social security payments expressed in BGN. The pre-treatment time period includes four months before and the post-treatment time period four (Panel A) or ten (Panel B) months after the treatment. The pre-experiment control mean of tax base SSC in the baseline condition is BGN 7,124. A firm's number of employees and its industry affiliation are included as controls. The underlying sample includes firms that do not directly communicate with the tax authority. Standard errors clustered on firm level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE D.5. Extended sample: Treatment effects of deterrence treatments on SSC

Panel A:	**4 post-treatment months**					
	Audit all	*Audit 1%*	*Audit 10%*	*Audit 40%*	*Audit 60%*	*Ambiguous*
	(1)	(2)	(3)	(4)	(5)	(6)
Absolute effect	130.429	78.836	56.523	256.982**	467.609**	107.283
	(85.245)	(108.854)	(99.986)	(131.026)	(187.046)	(96.450)
Relative effect	0.016	0.006	0.013	0.037**	0.034**	0.014
	(0.010)	(0.012)	(0.013)	(0.017)	(0.015)	(0.013)
Observations	153231	99431	98333	78603	73334	98383
No of firms	28380	15200	15000	12000	11180	15000
Panel B:	**10 post-treatment months**					
	Audit all	*Audit 1%*	*Audit 10%*	*Audit 40%*	*Audit 60%*	*Ambiguous*
	(1)	(2)	(3)	(4)	(5)	(6)
Absolute effect	140.179	61.333	91.811	225.109	546.349**	136.650
	(103.929)	(121.064)	(130.939)	(145.474)	(242.774)	(131.389)
Relative effect	0.016	0.001	0.019	0.032*	0.036**	0.020
	(0.012)	(0.014)	(0.015)	(0.017)	(0.018)	(0.014)
Observations	263664	171057	169087	135060	126116	169278
No of firms	28380	15200	15000	12000	11180	15000

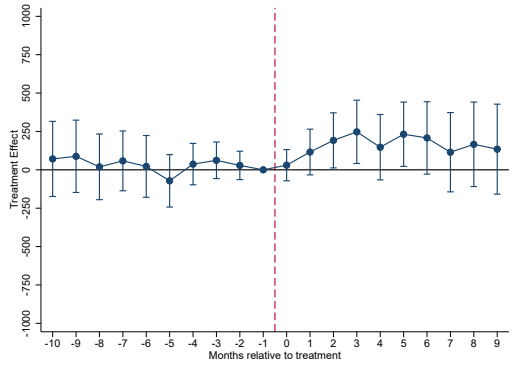
Notes: Treatment effects of moral messages on SSC. Displayed are DiD regression estimates of treatment messages on tax base SSC. Displayed are DiD regression estimates of treatment messages on tax base SSC. The estimated treatment effects are relative to the baseline condition. Treatment effects are reported in absolute terms based on OLS regressions and in relative terms based on poisson regressions. SSC tax base is the monthly reported tax base for social security payments expressed in BGN. The pre-treatment time period includes four months before and the post-treatment time period four (Panel A) or ten (Panel B) months after the treatment. The pre-experiment control mean of tax base SSC in the baseline condition is BGN 7,124. A firm's number of employees and its industry affiliation are included as controls. The underlying sample includes firms that do not directly communicate with the tax authority. Standard errors clustered on firm level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

D.3 *Dynamics*



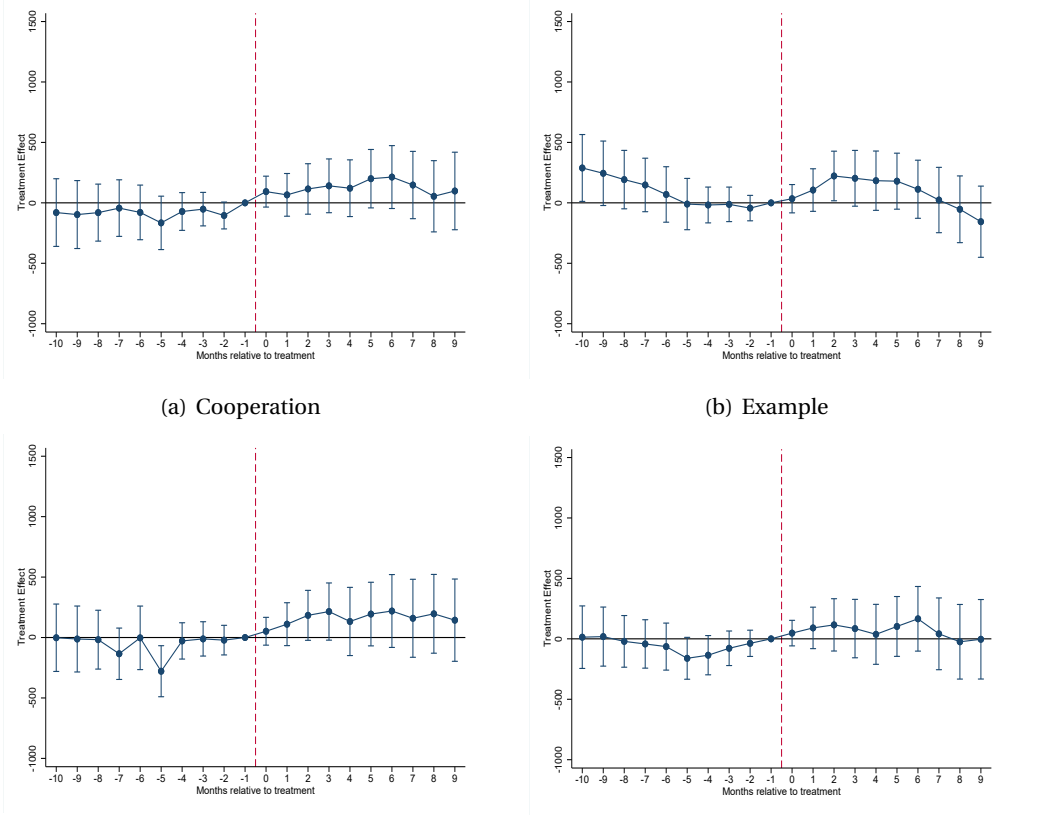
Notes: Pooled monthly absolute treatment effects of the moral appeal messages (Cooperation - Picture) on SSC. The points plotted are the estimated DiD regression coefficients of treatment messages on tax base SSC by months relative to the treatment. Treatment effects are relative to the control message. SSC tax base is the monthly reported tax base for social security payments expressed in BGN. The underlying sample includes firms that do not directly communicate with the tax authority. 95% confidence intervals are represented by the blue lines and based on standard errors clustered on firm level.

FIGURE D.1. Extended sample: Dynamic absolute effects of moral treatments on SSC



Notes: Pooled monthly absolute treatment effects of the audit probability messages (Audit 1% - Audit 60%) on SSC. The points plotted are the estimated DiD regression coefficients of treatment messages on tax base SSC by months relative to the treatment. Treatment effects are relative to the control message. SSC tax base is the monthly reported tax base for social security payments expressed in BGN. The underlying sample includes firms that do not directly communicate with the tax authority. 95% confidence intervals are represented by the blue lines and based on standard errors clustered on firm level.

FIGURE D.2. Extended sample: Dynamic absolute effects of deterrence treatments on SSC



(a) Cooperation

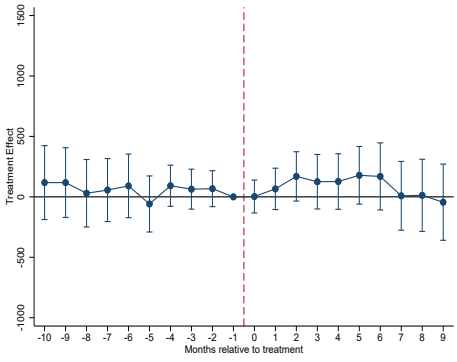
(b) Example

(c) Necessity

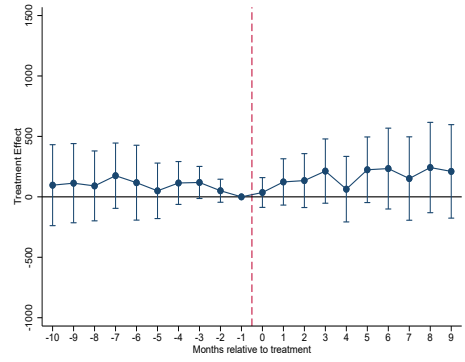
(d) Picture

Notes: Monthly absolute treatment effects of moral appeal messages (Cooperation, Example, Necessity, Picture) on SSC. The points plotted are the estimated DiD regression coefficients of treatment messages on tax base SSC by months relative to the treatment. Treatment effects are relative to the control message. SSC tax base is the monthly reported tax base for social security payments expressed in BGN. The underlying sample includes firms that do not directly communicate with the tax authority. 95% confidence intervals are represented by the blue lines and based on standard errors clustered on firm level.

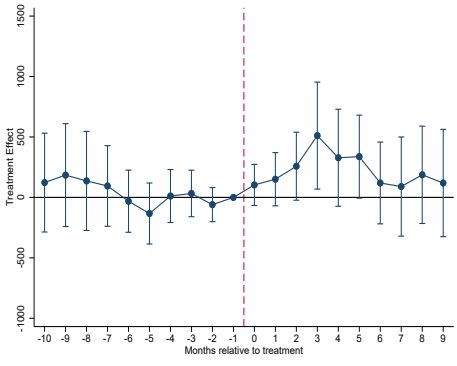
FIGURE D.3. Extended sample: Dynamic absolute effects of moral treatments on SSC by sub--treatment



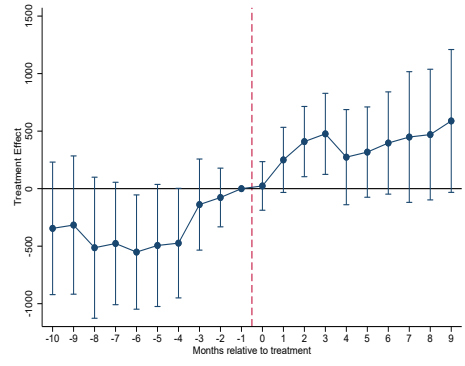
(a) Audit 1%



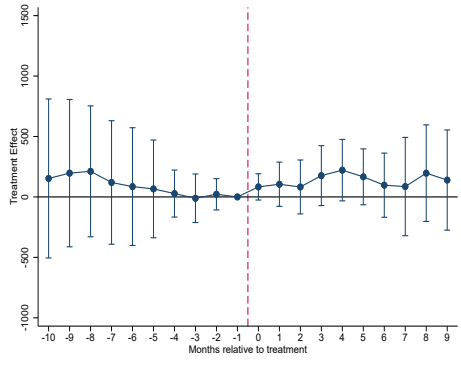
(b) Audit 10%



(c) Audit 40%



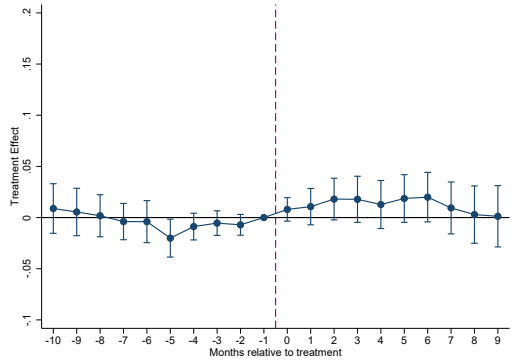
(d) Audit 60%



(e) Ambiguous

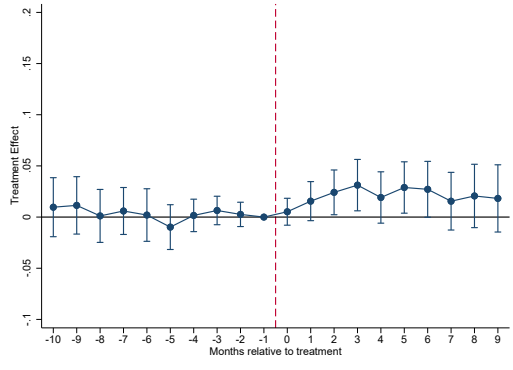
Notes: Monthly absolute treatment effects of audit messages (Audit 1%, Audit 10%, Audit 40%, Audit 60%, Ambiguous) on SSC. The points plotted are the estimated DiD regression coefficients of treatment messages on tax base SSC by months relative to the treatment. Treatment effects are relative to the control message. SSC tax base is the monthly reported tax base for social security payments expressed in BGN. The underlying sample includes firms that do not directly communicate with the tax authority. 95% confidence intervals are represented by the blue lines and based on standard errors clustered on firm level.

FIGURE D.4. Extended sample: Dynamic absolute effects of deterrence treatments on SSC by sub-treatment



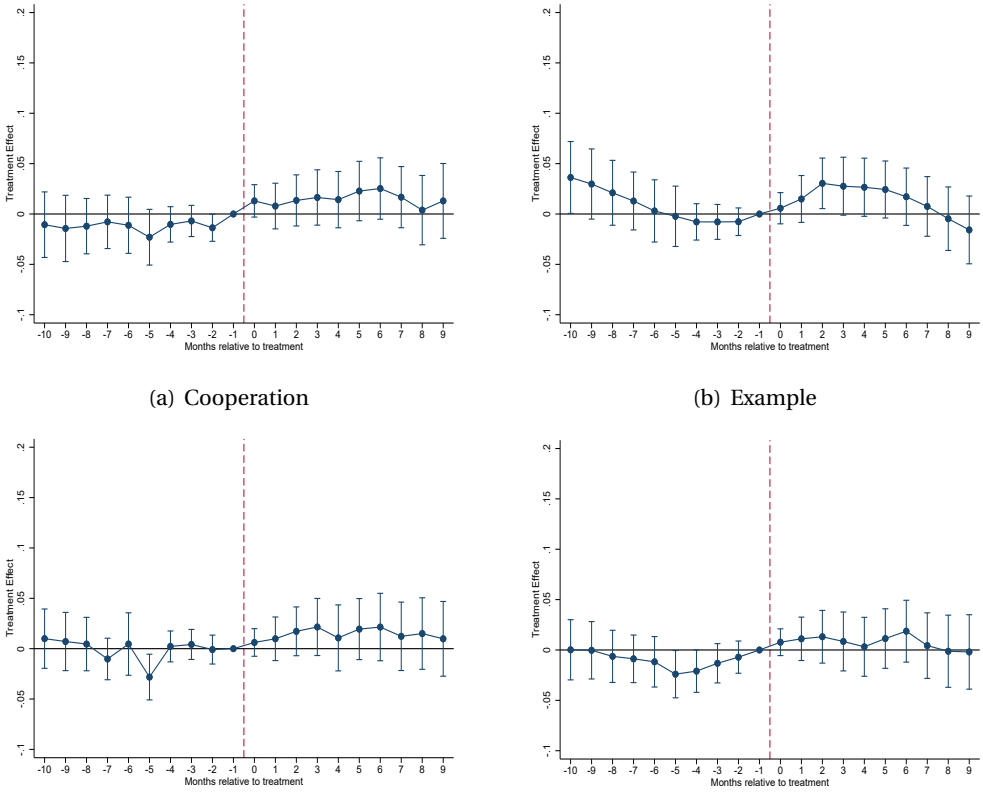
Notes: Pooled monthly relative treatment effects of the moral appeal messages (Cooperation - Picture) on SSC. The points plotted are the estimated DiD poisson regression coefficients of treatment messages on tax base SSC by months relative to the treatment. Treatment effects are relative to the control message. SSC tax base is the monthly reported tax base for social security payments expressed in BGN. The underlying sample includes firms that do not directly communicate with the tax authority. 95% confidence intervals are represented by the blue lines and based on standard errors clustered on firm level.

FIGURE D.5. Extended sample: Dynamic relative effects of moral treatments on SSC



Notes: Pooled monthly relative treatment effects of the audit probability messages (Audit 1% - Audit 60%) on SSC. The points plotted are the estimated DiD poisson regression coefficients of treatment messages on tax base SSC by months relative to the treatment. Treatment effects are relative to the control message. SSC tax base is the monthly reported tax base for social security payments expressed in BGN. The underlying sample includes firms that do not directly communicate with the tax authority. 95% confidence intervals are represented by the blue lines and based on standard errors clustered on firm level.

FIGURE D.6. Extended sample: Dynamic relative effects of deterrence treatments on SSC



(a) Cooperation

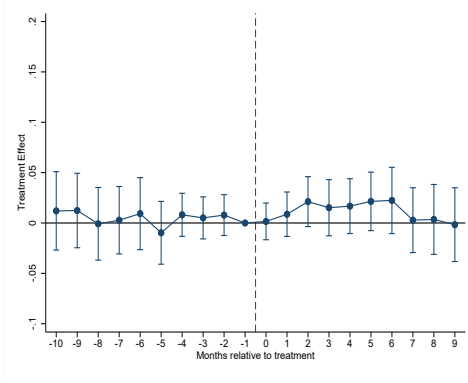
(b) Example

(c) Necessity

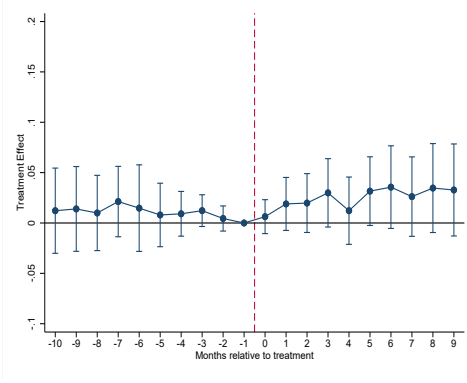
(d) Picture

Notes: Monthly relative treatment effects of moral appeal messages (Cooperation, Example, Necessity, Picture) on SSC. The points plotted are the estimated DiD poisson regression coefficients of treatment messages on tax base SSC by months relative to the treatment. Treatment effects are relative to the control message. SSC tax base is the monthly reported tax base for social security payments expressed in BGN. The underlying sample includes firms that do not directly communicate with the tax authority. 95% confidence intervals are represented by the blue lines and based on standard errors clustered on firm level.

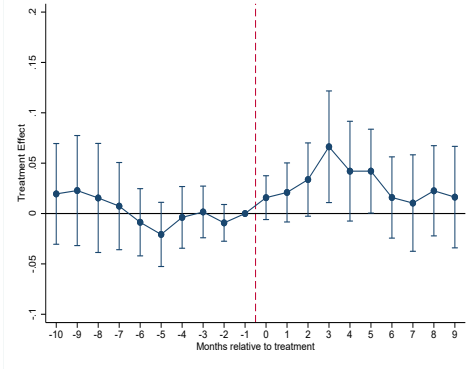
FIGURE D.7. Extended sample: Dynamic relative effects of moral treatments on SSC by sub--treatment



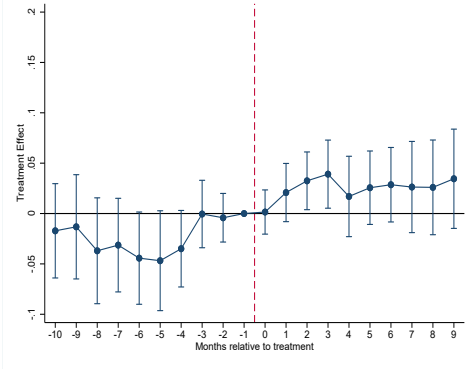
(a) Audit 1%



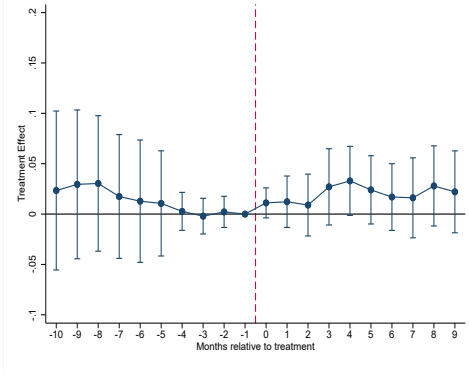
(b) Audit 10%



(c) Audit 40%



(d) Audit 60%

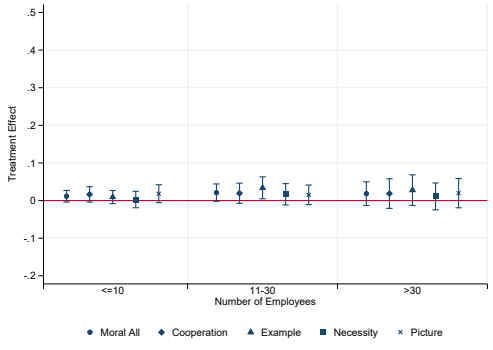


(e) Ambiguous

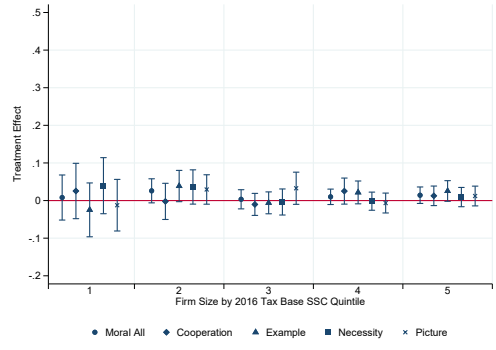
Notes: Monthly relative treatment effects of audit messages (Audit 1%, Audit 10%, Audit 40%, Audit 60%, Ambiguous) on SSC. The points plotted are the estimated DiD poisson regression coefficients of treatment messages on tax base SSC by months relative to the treatment. Treatment effects are relative to the control message. SSC tax base is the monthly reported tax base for social security payments expressed in BGN. The underlying sample includes firms that do not directly communicate with the tax authority. 95% confidence intervals are represented by the blue lines and based on standard errors clustered on firm level.

FIGURE D.8. Extended sample: Dynamic relative effects of deterrence treatments on SSC by sub--treatment

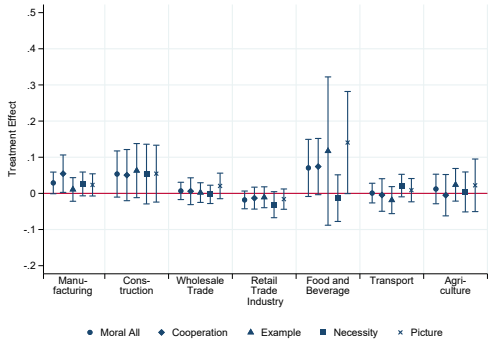
D.4 *Heterogeneity*



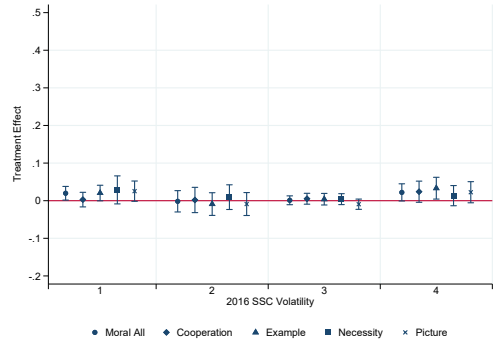
(a) Firm size: Number of employees



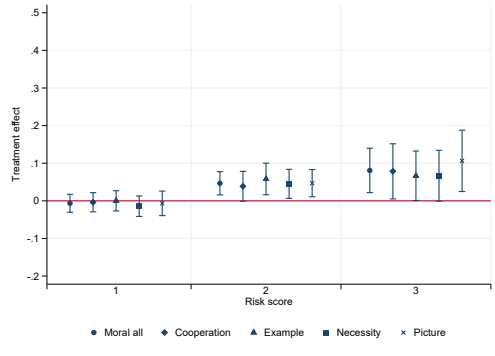
(b) Firm size: Pre-treatment tax base SSC



(c) Industry



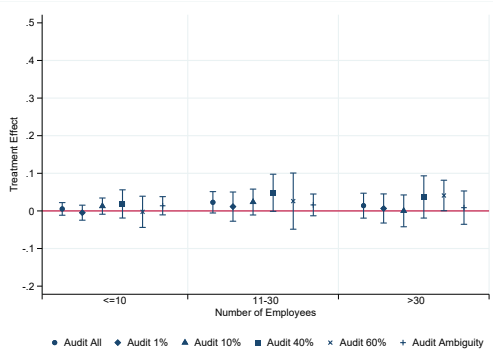
(d) Volatility: 2016 tax base SSC



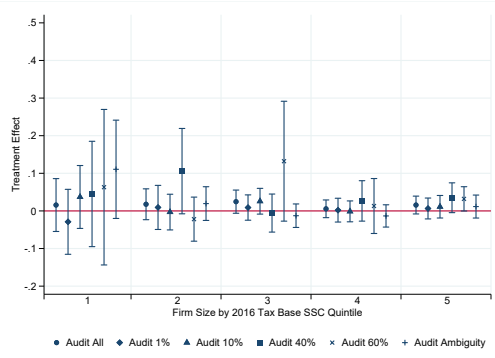
(e) Risk score

Notes: Panel (a): Heterogeneous effects of moral messages on SSC by the number of employees. Panel (b): Heterogeneous effects of moral messages by firm size in pre-treatment tax base SSC quintiles. Panel (c): Heterogeneous effects of moral messages on SSC by industry affiliation. Panel (d): Heterogeneous effects of moral messages on SSC by 2016 tax base SSC volatility quartiles. Panel (e): Heterogeneous effects of moral messages on SSC by the tax authorities' internal risk score (between 1-low risk to 3-high risk). The points plotted are the estimated DiD poisson regression coefficients of treatment messages on tax base SSC. Displayed are relative treatment effects compared to the control message. SSC tax base is the monthly reported tax base for social security payments expressed in BGN. The pre-treatment time period includes four months before and the post-treatment time period four months after the treatment. 95% confidence intervals are represented by the blue lines and based on standard errors clustered on firm level.

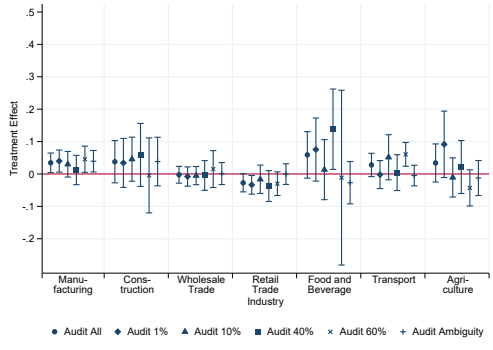
FIGURE D.9. Extended sample: Heterogeneous effects of moral treatments on SSC



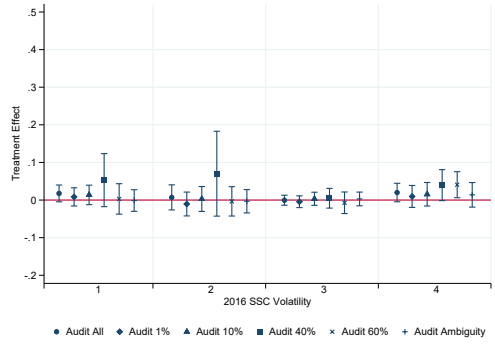
(a) Firm size: Number of employees



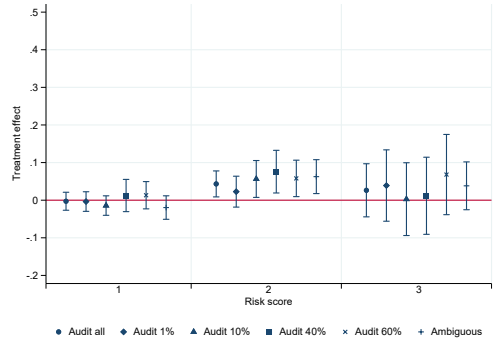
(b) Firm size: Pre-treatment tax base SSC



(c) Industry



(d) Volatility: 2016 tax base SSC



(e) Risk score

Notes: Panel (a): Heterogeneous effects of deterrence messages on SSC by the number of employees. Panel (b): Heterogeneous effects of deterrence messages by firm size in pre-treatment tax base SSC quintiles. Panel (c): Heterogeneous effects of deterrence messages on SSC by industry affiliation. Panel (d): Heterogeneous effects of deterrence messages on SSC by 2016 tax base SSC volatility quartiles. Panel (e): Heterogeneous effects of deterrence messages on SSC by the tax authorities' internal risk score (between 1-low risk to 3-high risk). The points plotted are the estimated DiD poisson regression coefficients of treatment messages on tax base SSC. Displayed are relative treatment effects compared to the control message. SSC tax base is the monthly reported tax base for social security payments expressed in BGN. The pre-treatment time period includes four months before and the post-treatment time period four months after the treatment. 95% confidence intervals are represented by the blue lines and based on standard errors clustered on firm level.

FIGURE D.10. Extended sample: Heterogeneous effects of deterrence treatments on SSC

D.5 *Mechanisms and Revenue Effects*

TABLE D.6. Extended sample: Treatment effects of audit probability messages relative to ambiguous message on SSC

Panel A:	**4 post-treatment months**				
	Audit all (1)	*Audit 1%* (2)	*Audit 10%* (3)	*Audit 40%* (4)	*Audit 60%* (5)
Absolute effect	28.002	-22.767	-58.333	148.260	344.472*
	(79.784)	(107.911)	(95.673)	(128.574)	(184.851)
Relative effect	0.002	-0.008	-0.003	0.024	0.019
	(0.012)	(0.013)	(0.014)	(0.018)	(0.016)
Observations	68868	37967	37462	26050	23087
No of firms	18380	10200	10000	7000	6180
Panel B:	**10 post-treatment months**				
	Audit all (1)	*Audit 1%* (2)	*Audit 10%* (3)	*Audit 40%* (4)	*Audit 60%* (5)
Absolute effect	8.155	-73.844	-52.896	59.143	357.054
	(109.269)	(135.427)	(143.052)	(170.296)	(254.422)
Relative effect	-0.003	-0.016	-0.002	0.013	0.016
	(0.011)	(0.013)	(0.016)	(0.016)	(0.017)
Observations	207838	115231	113261	79234	70290
No of firms	18380	10200	10000	7000	6180

Notes: Trreatment effects of audit probability messages relative to the ambiguous message on SSC. Displayed are DiD regression estimates of treatment messages on tax base SSC. The estimated treatment effects are relative to the audit ambiguous treatment. Treatment effects are reported in absolute terms based on OLS regressions and in relative terms based on poisson regressions. SSC tax base is the monthly reported tax base for social security payments expressed in BGN. The pre-treatment time period includes four months before and the post-treatment time period four (Panel A) or ten (Panel B) months after the treatment. The pre-experiment control mean of tax base SSC in the ambiguous condition is BGN 7,096. A firm's number of employees and its industry affiliation are included as controls. The underlying sample includes firms that do not directly communicate with the tax authority. Standard errors clustered on firm level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE D.7. Extended sample: Additional SSC revenue per letter

Panel A:		**Moral Treatments**				
		Moral all (1)	*Cooperation* (2)	*Example* (3)	*Necessity* (4)	*Picture* (5)
Immediate revenue	(*in BGN*)	186.19	197.24	193.14	189.72	180.20
	(*in USD*)	93.65	99.21	97.15	95.43	90.64
Overall revenue	(*in BGN*)	448.88	570.72	323.68	550.38	404.46
	(*in USD*)	225.78	287.07	162.81	276.84	203.44

Panel B:		**Deterrence Treatments**					
		Audit all (1)	*Audit 1%* (2)	*Audit 10%* (3)	*Audit 40%* (4)	*Audit 60%* (5)	*Ambiguous* (6)
Immediate revenue	(*in BGN*)	41.42	84.60	-32.17	-91.62	-38.87	118.74
	(*in USD*)	20.83	42.55	-16.18	-46.09	-19.55	59.73
Overall revenue	(*in BGN*)	305.74	174.00	175.43	275.33	1,039.05	399.95
	(*in USD*)	153.78	87.52	88.24	138.49	522.63	201.17

Notes: Per mailing/letter revenue (in BGN) in the moral (Panel A) and deterrence (Panel B) treatments. The first two rows of each panel show the revenue up to four months after the intervention in BGN and USD respectively. The last two rows of each panel show the revenue up to ten months after the intervention in BGN and USD respectively. Revenue is calculated as *DiD Estimate* $\times$ *No of Treated Firms* $\times$ *Month after Intervention* $\times$ *Tax Rate* $-$ *Intervention Cost*. The cost of the intervention is assumed to be zero in moral treatments and is equal BGN 1,000 per audit ($20\,hours \cdot times$ BGN $50/hour$) in the deterrence treatments. The underlying sample includes firms that do not directly communicate with the tax authority.

APPENDIX E: VAT

We focus on the SSC tax base (payroll taxes) throughout the main body of our paper. However, our treatment letters mention VAT payments alongside SSC and underreporting SSC contributions potentially allows firms to credibly lower reported revenue to save on VAT. Our conjecture is therefore that firms potentially change behavior in both domains and we thus also report results for the VAT tax base. Below, we first briefly explain the Bulgarian VAT system (E.1). We then present the main effects of our experimental interventions on the reported VAT base (E.2). Finally, we investigate potential spillover effects of treatment-induced SSC adjustments on VAT reporting behavior (E.3).

E.1 *Institutional Background*

VAT payments account for 50% of total tax revenues in Bulgaria. VAT is levied on the sale of goods and the provision of services. The tax rate applied in Bulgaria is 20%.[34] Firms collect VAT paid by customers for their goods and services and pay VAT on purchasing goods and services. VAT paid on input costs (VAT credit) is credited against the VAT collected from customers (VAT debit). The difference comprises a firm's VAT tax base. Bulgarian firms have to file a monthly VAT return that contains all this information. A key distinction between VAT and payroll taxes is the degree to which third-party reporting is enforced. Firms additionally have to submit the ledgers of account with their VAT return including a purchase day and a sales day book and a list that details all the customers and the values of sales made to them. This creates an information trail that the Bulgarian tax authority can use to cross-check revenues. Thus, the NRA observes every sale or purchase along the value chain making the case for VAT evasion more difficult.

E.2 *Treatment Effects*

Table E.1 (for the *Moral* treatments) and Table E.2 (for the *Deterrence* treatments) below depict how the treatment mailings affect VAT payments. The empirical specifications are analogous to the DiD specifications we presented before, but replace the SSC base with the monthly VAT base as outcome variable. Overall, we find insignificant effects (with very large standard errors relative to coefficients) of our treatments on VAT payments for all treatment groups and both time spans that we consider in our analysis.

One possible interpretation for the overall insignificant effect on VAT is rooted in the strong enforcement possibilities associated with VAT (mentioned above). In addition, there is a paper trail for VAT payments (see the well established results by Pomeranz, 2015 in this context). These features allow the tax authority to cross-check revenues quite properly. For SSC, by contrast, both employers and employees have an incentive to evade payroll taxes. By teaming up, employers and employees can easily circumvent the third-party enforcement mechanism and evade part of the social security payments. As a result, initial evasion levels are possibly different across the two tax bases and SSC payments have more scope for being improved in response to treatment mailings.

[34] A reduced tax rate of 9% applies only to hotel accommodations and similar establishments.

TABLE E.1. Treatment effects of moral treatments on VAT

Panel A:	**4 post-treatment months**				
	Moral all (1)	*Cooperation* (2)	*Example* (3)	*Necessity* (4)	*Picture* (5)
Absolute effect	244.637	-1632.795	4088.705	86.007	-1582.751
	(1609.127)	(1930.412)	(3174.903)	(1794.257)	(2097.713)
Observations	218112	86838	86971	87647	87201
No of firms	27808	11069	11088	11157	11114
Panel B:	**10 post-treatment months**				
	Moral all (1)	*Cooperation* (2)	*Example* (3)	*Necessity* (4)	*Picture* (5)
Absolute effect	-2809.223	-155.144	3494.675	963.021	-15590.462
	(3758.232)	(1740.725)	(2845.856)	(1717.734)	(13880.197)
Observations	378391	150718	150894	152087	151348
No of firms	27808	11069	11088	11157	11114

Notes: Treatment effects of moral messages on VAT. Displayed are DiD regression estimates of treatment messages on tax base VAT. The estimated treatment effects are relative to the baseline condition. Treatment effects are reported in absolute terms based on OLS regressions and in relative terms based on poisson regressions. VAT tax base is the monthly reported tax base for VAT payments expressed in BGN. The pre-treatment time period includes four months before and the post-treatment time period four (Panel A) or ten (Panel B) months after the treatment. The pre-experiment control mean of tax base VAT in the baseline condition is BGN 14,344. A firm's number of employees and its industry affiliation are included as controls. Standard errors clustered on firm level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE E.2. Treatment effects of deterrence treatments on VAT

Panel A:	**4 post-treatment months**					
	Audit All (1)	*Audit 1%* (2)	*Audit 10%* (3)	*Audit 40%* (4)	*Audit 60%* (5)	*Ambiguous* (6)
Absolute effect	-1616.486	-6138.736	-132.946	6230.884	-1425.218	-1031.338
	(3104.482)	(6720.442)	(2902.376)	(4786.333)	(2491.336)	(1830.890)
Observations	102205	66215	65619	52290	48626	65219
No of Firms	15789	8448	8350	6664	6190	8297
Panel B:	**10 post-treatment months**					
	Audit All (1)	*Audit 1%* (2)	*Audit 10%* (3)	*Audit 40%* (4)	*Audit 60%* (5)	*Ambiguous* (6)
Absolute effect	-1050.593	-5526.369	1922.547	4150.965	-2875.171	-895.856
	(3021.118)	(6708.561)	(2782.274)	(2837.191)	(2901.436)	(2139.042)
Observations	177382	114911	113936	90777	84414	113196
No of Firms	15789	8448	8350	6664	6190	8297

Notes: Treatment effects of deterrence treatments on VAT. Displayed are DiD regression estimates of treatment messages on tax base VAT. The estimated treatment effects are relative to the baseline condition. Treatment effects are reported in absolute terms based on OLS regressions and in relative terms based on poisson regressions. VAT tax base is the monthly reported tax base for VAT payments expressed in BGN. The pre-treatment time period includes four months before and the post-treatment time period four (Panel A) or ten (Panel B) months after the treatment. The pre-experiment control mean of tax base VAT in the baseline condition is BGN 14,344. A firm's number of employees and its industry affiliation are included as controls. Standard errors clustered on firm level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

E.3 *VAT Spillover*

To shed more light on firm behavior and to complement our analysis, we also consider potential spillovers between SSC and VAT. To this end, we check whether firms which increase their SSC payments in response to our treatments also change their behavior with regard to VAT payments. To the best of our knowledge, our data on SSC and VAT are unique in that we can identify such spillovers, which may – depending on their

direction – be worrisome or encouraging for policymakers. On the one hand, if firms, for instance, increase tax base SSC in response of our treatments, but reduce tax base VAT, our interventions will be overall less effective from a tax revenue perspective. On the other hand, firms could also behave consistently and increase honesty in both categories which might render our interventions more effective than when looking at SSC alone.[35]

To test spillovers between SSC and VAT, we run VAT regressions in which the treatment group is restricted to firms which increase SSC in response to our treatment messages. For this, we calculate the average before-after difference (comparing average tax base SSC in the four months before and after the experiment) for the firms in our control condition. We then only keep treatment firms that have a SSC change that is larger than the average SSC change in the control group. We then run our static DiD regression with VAT as the dependent variable only including these treatment firms and the usual control firms. Based on this approach, we find support for consistent treatment effects on both SSC and VAT. Firms which increase tax base SSC also report significantly higher tax base VAT for both the *Moral* and the *Deterrence* treatments. Table E.3 (for *Moral* treatments) and E.4 (for *Deterrence treatments*) below show our DiD results for this analysis.

Although the effects are not strong enough to affect VAT payments directly (see Tables E.1 and E.2), this finding is encouraging news for tax authorities and adds an important and missing piece to the literature as it suggests that, in our sample, there is no offsetting of compliant behavior in one domain of tax payments with higher evasion in another. This indicates that reported tax revenue in other studies may report lower bounds with respect to the actual tax revenue. Because of positive spillover effects through compliance on other, not directly studied taxes revenue effects may be larger.

[35] Economic arguments on substitution effects between SSC compliance and VAT evasion could result in negative treatment effects on tax base VAT. Firms might want to offset the higher cost from SSC compliance with lower reported tax base VAT (similar to the offsetting effects identified in Carrillo et al., 2017). Literature from behavioral economics and social psychology on moral licensing likewise provides arguments for negative spillovers (see, e.g., Blanken et al., 2015, Tiefenbeck et al., 2013).

TABLE E.3. VAT spillover: Treatment effects of moral treatments on VAT

Panel A:	**4 post-treatment months**				
	Moral all (1)	*Cooperation* (2)	*Example* (3)	*Necessity* (4)	*Picture* (5)
Absolute effect	15491.372***	6505.355*	26825.518**	15884.618***	12977.819***
	(3416.805)	(3849.253)	(11061.161)	(3779.072)	(4596.098)
Observations	78446	52116	51973	52415	52487
No of firms	9938	6623	6605	6663	6667
Panel B:	**10 post-treatment months**				
	Moral all (1)	*Cooperation* (2)	*Example* (3)	*Necessity* (4)	*Picture* (5)
Absolute effect	12195.037***	5870.988*	23447.110***	15038.429***	4621.560
	(3029.321)	(3396.449)	(8102.087)	(4424.540)	(5631.713)
Observations	136461	90560	90305	91053	91199
No of firms	9938	6623	6605	6663	6667

Notes: Treatment effects of moral messages on VAT for firms that improved SSC compliance in response to the experiment. The underlying sample includes only firms that have an larger than average before-after difference in tax base SSC compared to firms in the baseline condition. Displayed are DiD regression estimates of treatment messages on tax base VAT. The estimated treatment effects are relative to the baseline condition. Treatment effects are reported in absolute terms based on OLS regressions and in relative terms based on poisson regressions. VAT tax base is the monthly reported tax base for VAT payments expressed in BGN. The pre-treatment time period includes four months before and the post-treatment time period four (Panel A) or ten (Panel B) months after the treatment. The pre-experiment control mean of tax base VAT in the baseline condition is BGN 14,344. A firm's number of employees and its industry affiliation are included as controls. Standard errors clustered on firm level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE E.4. VAT spillover: Treatment effects of deterrence treatments on VAT

Panel A:	**4 post-treatment months**					
	Audit All (1)	*Audit 1%* (2)	*Audit 10%* (3)	*Audit 40%* (4)	*Audit 60%* (5)	*Ambiguous* (6)
Absolute effect	15456.998**	10467.670*	13854.814	37898.718*	6290.882	5306.107
	(6179.663)	(5446.722)	(12961.416)	(21263.330)	(8866.546)	(5370.669)
Observations	55530	48359	47748	45349	44619	48039
No of Firms	7050	6148	6073	5770	5679	6107
Panel B:	**10 post-treatment months**					
	Audit All (1)	*Audit 1%* (2)	*Audit 10%* (3)	*Audit 40%* (4)	*Audit 60%* (5)	*Ambiguous* (6)
Absolute effect	10568.014**	4847.844	16079.482	23467.184**	-6915.401	2583.828
	(5219.102)	(5307.213)	(11756.860)	(11603.665)	(11595.159)	(7896.123)
Observations	96520	84010	82944	78748	77474	83440
No of Firms	7050	6148	6073	5770	5679	6107

Notes: Treatment effects of deterrence treatments on VAT for firms with improved SSC compliance. Displayed are DiD regression estimates of treatment messages on tax base VAT. The estimated treatment effects are relative to the baseline condition. Treatment effects are reported in absolute terms based on OLS regressions and in relative terms based on poisson regressions. VAT tax base is the monthly reported tax base for VAT payments expressed in BGN. The pre-treatment time period includes four months before and the post-treatment time period four (Panel A) or ten (Panel B) months after the treatment. The pre-experiment control mean of tax base VAT in the baseline condition in BGN 14,344. A firm's number of employees and its industry affiliation are included as controls. Standard errors clustered on firm level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

E.4 *Extended Sample (Including Firms that Do Not Directly Communicate with the Tax Authority)*

TABLE E.5. Extended sample: Treatment effects of moral treatments on VAT

Panel A:	**4 post-treatment months**				
	Moral all (1)	*Cooperation* (2)	*Example* (3)	*Necessity* (4)	*Picture* (5)
Absolute effect	1990.294	195.725	3713.783*	3000.049	1038.731
	(1247.771)	(1448.355)	(2043.386)	(2030.845)	(1598.431)
Observations	391472	156741	156602	156766	156614
No of Firms	50000	20000	20000	20000	20000
Panel B:	**10 post-treatment months**				
	Moral all (1)	*Cooperation* (2)	*Example* (3)	*Necessity* (4)	*Picture* (5)
Absolute effect	-97.812	1421.228	2597.006	2608.250*	-7055.039
	(2195.052)	(1236.866)	(1771.625)	(1535.294)	(7810.568)
Observations	678931	271887	271597	271972	271625
No of firms	50000	20000	20000	20000	20000

Notes: Treatment effects of moral messages on VAT. Displayed are DiD regression estimates of treatment messages on tax base SSC. The estimated treatment effects are relative to the baseline condition. VAT tax base is the monthly reported tax base for VAT payments expressed in BGN. The pre-treatment time period includes four months before and the post-treatment time period four (Panel A) or ten (Panel B) months after the treatment. The pre-experiment control mean of tax base VAT in the baseline condition is BGN 14,344. A firm's number of employees and its industry affiliation are included as controls. The underlying sample includes firms that do not directly communicate with the tax authority. Standard errors clustered on firm level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE E.6. Extended sample: Treatment effects of deterrence treatments on VAT

Panel A:	**4 post-treatment months**					
	Audit all (1)	*Audit 1%* (2)	*Audit 10%* (3)	*Audit 40%* (4)	*Audit 60%* (5)	*Ambiguous* (6)
Absolute effect	583.392	-1721.508	1292.293	5607.974*	-829.078	457.178
	(2065.454)	(4230.012)	(2096.536)	(3408.441)	(1801.319)	(1345.792)
Observations	183152	118986	117719	94020	87678	117613
No of firms	28380	15200	15000	12000	11180	15000
Panel B:	**10 post-treatment months**					
	Audit all (1)	*Audit 1%* (2)	*Audit 10%* (3)	*Audit 40%* (4)	*Audit 60%* (5)	*Ambiguous* (6)
Absolute effect	726.099	-1743.092	1883.239	5423.745**	-1326.660	98.810
	(1847.429)	(3919.810)	(1744.532)	(2435.266)	(1863.200)	(1376.008)
Observations	317670	206426	204258	163065	152071	203984
No of firms	28380	15200	15000	12000	11180	15000

Notes: Treatment effects of deterrence messages on VAT. Displayed are DiD regression estimates of treatment messages on tax base SSC. The estimated treatment effects are relative to the baseline condition. VAT tax base is the monthly reported tax base for VAT payments expressed in BGN. The pre-treatment time period includes four months before and the post-treatment time period four (Panel A) or ten (Panel B) months after the treatment. The pre-experiment control mean of tax base VAT in the baseline condition is BGN 14,344. A firm's number of employees and its industry affiliation are included as controls. The underlying sample includes firms that do not directly communicate with the tax authority. Standard errors clustered on firm level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE E.7. Extended sample: VAT spillover: Treatment effects of moral treatments on VAT

Panel A:	**4 post-treatment months**				
	Moral all (1)	*Cooperation* (2)	*Example* (3)	*Necessity* (4)	*Picture* (5)
Absolute effect	18523.791*** (3159.063)	10501.807*** (3302.960)	24020.778*** (6732.639)	23395.549*** (8332.522)	16046.850*** (4616.825)
Observations	138599	93393	93514	93540	93403
No of firms	17580	11887	11900	11906	11887
Panel B:	**10 post-treatment months**				
	Moral all (1)	*Cooperation* (2)	*Example* (3)	*Necessity* (4)	*Picture* (5)
Absolute effect	14389.691*** (2362.350)	10898.498*** (2717.819)	19145.349*** (4829.440)	17558.215*** (5693.160)	9826.246** (4051.885)
Observations	240979	162156	162375	162413	162185
No of firms	17580	11887	11900	11906	11887

Notes: Treatment effects of moral messages on VAT for firms that improved SSC compliance in response to the experiment. The underlying sample includes only firms that have an larger than average before-after difference in tax base SSC compared to firms in the baseline condition. Displayed are DiD regression estimates of treatment messages on tax base VAT. The estimated treatment effects are relative to the baseline condition. VAT tax base is the monthly reported tax base for VAT payments expressed in BGN. The pre-treatment time period includes four months before and the post-treatment time period four (Panel A) or ten (Panel B) months after the treatment. The pre-experiment control mean of tax base VAT in the baseline condition is BGN 14,344. A firm's number of employees and its industry affiliation are included as controls. The underlying sample includes firms that do not directly communicate with the tax authority. Standard errors clustered on firm level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

TABLE E.8. Extended sample: VAT spillover: Treatment effects of deterrence treatments on VAT

Panel A:	**4 post-treatment months**					
	Audit all (1)	*Audit 1%* (2)	*Audit 10%* (3)	*Audit 40%* (4)	*Audit 60%* (5)	*Ambiguous* (6)
Absolute effect	17679.286*** (5222.601)	16254.251* (8347.274)	17407.880* (9350.934)	28850.086** (12877.308)	5788.243 (6441.780)	8930.415** (3975.135)
Observations	98774	86416	85824	81541	80244	86247
No of firms	13545	11004	10933	10392	10230	10986
Panel B:	**10 post-treatment months**					
	Audit all (1)	*Audit 1%* (2)	*Audit 10%* (3)	*Audit 40%* (4)	*Audit 60%* (5)	*Ambiguous* (6)
Absolute effect	11678.533*** (3571.895)	8621.237* (4736.911)	14803.069** (7318.754)	20739.543*** (7282.721)	-3075.322 (7350.106)	5354.490 (4952.397)
Observations	171532	150013	148950	141490	139229	149680
No of firms	13545	11004	10933	10392	10230	10986

Notes: Treatment effects of deterrence messages on VAT for firms that improved SSC compliance in response to the experiment. The underlying sample includes only firms that have an larger than average before-after difference in tax base SSC compared to firms in the baseline condition. Displayed are DiD regression estimates of treatment messages on tax base VAT. The estimated treatment effects are relative to the baseline condition. VAT tax base is the monthly reported tax base for VAT payments expressed in BGN. The pre-treatment time period includes four months before and the post-treatment time period four (Panel A) or ten (Panel B) months after the treatment. The pre-experiment control mean of tax base VAT in the baseline condition is BGN 14,344. A firm's number of employees and its industry affiliation are included as controls. The underlying sample includes firms that do not directly communicate with the tax authority. Standard errors clustered on firm level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

APPENDIX F: TAX SURVEY I

F.1 *Survey Design and Implementation*

To get a more detailed understanding about the beliefs and tax moral of firms in Bulgaria, we conducted a brief survey with SMEs which were comparable to those in the experimental conditions but were not subject to the interventions.

Table A.1 randomization provides an overview of the firms invited to participate in the survey compared to the general population of similar firms (in terms of size and revenue) in Bulgaria and firms in our experimental conditions. Firms which have been invited to participate in the survey are comparable (and statistically not different) to the population of SMEs in Bulgaria in terms of tax base VAT, social security payments, number of employees. Consequentially, the invited firms are comparable to the firms in our RCT population.

In the survey we asked participants to indicate their moral attitudes towards paying taxes, whether they think that tax evasion is a problem in Bulgaria, their beliefs about receiving a tax inspection and their beliefs about evasion channels and behaviors in their industry. Questions on tax morale were (in part) adapted from the World Values Survey (LINK). Additionally, the firms were asked whether they use a tax accountant (in-house or externally) or file their taxes without expert help (see Table F.1 for survey questions and results).

The tax authorities invited 10.000 firms to participate in the survey but it was explained to firms that the survey was organized and conducted by the research team. The survey invitation (in Bulgarian and an English translation) is available in Appendix B. Only aggregated data was shared with the tax authorities and no individual firm-level information was asked from participants. Specifically, firms were invited to follow a link redirecting them to the survey software Qualtrics (LINK) with which we administered the survey.[36]

The survey invitation was sent out at the same day as the treatment mailings to gather meaningful data at the time of the main study, i.e., representing beliefs, opinions and attitudes at the time of our experiment.[37]

Note that the survey also entailed an experimental component: At the beginning of the survey, some firms (randomly determined) were presented with the current distribution of tax spending at the time of the survey (with and without the possibility to indicate preferred changes in that distribution). Other firms (again randomly determined) did not see the distribution and proceeded to the questionnaire directly. Since differences in reporting behavior were minimal between conditions, we report pooled results over all conditions in this paper. Table F.2 limits results to the control group of the survey experiment only. Results are consistent with those reported for the full survey population.

[36] The original survey is available upon request.

[37] Responses may be different if the survey was conducted at a different point in time because, e.g., attitudes with respect to tax morale etc. may change because of exogenous events (like tax scandals) or may seasonably vary (more optimistic in the summer and pessimistic in the winter or vice versa).

F.2 *Survey Results*

Of the 10,000 invited firms, 1,725 (17.25%) SMEs initiated their participation in the survey. 54.6% (942) of the firms that started the survey answered all questions. As noted above, the survey (and the experiment) were a collaboration between the tax authorities and us researchers. Not all survey questions are therefore relevant and informative for the paper. The main purpose of the survey was to shed light on different aspects of firm tax evasion, in particular i) attitudes towards tax evasion along different dimensions (tax morale), ii) the extent of tax evasion in Bulgaria, and iii) firms' belief about the likelihood of receiving a tax inspection. Table F.1 provides an overview of our survey variables.

Because of the different dimensions, we disseminate the results of the survey in several blocks. First, we present the results of our questions concerning the attitudes towards tax evasion (Panel A of Table F.1). Here, we elicited four different dimensions of intrinsic tax morale. In particular, we asked survey participants to rate (on a scale from 1 - strongly disagree to 5 - strongly agree) their opinion to the following six statements: i.) 'One should honestly declare all income on the tax return.' ii.) 'It is acceptable to overstate deductions on the tax return.' iii.) 'Legally avoiding to pay taxes if possible is acceptable.' iv.) 'It can always be justified to cheat on taxes if there is a chance'. The purpose of eliciting different dimensions of tax morale is to gain a more nuanced impression of tax-evasion attitudes. Second, we focus on the extent of evasion, evasion channels and beliefs about audit probabilities in (Panel B, Panel C and, Panel D of Table F.1). Two widely discussed devices for tax evasion are side payments in cash (for example as hidden wages to employees) and e-commerce business activities. The survey asks participants about the prevalence of these two evasion channels in Bulgaria. In particular, respondents had to indicate their degree of agreement with the following two statements: i) 'Making payments in cash to evade tax and insurance contributions is a common offense.' ii) 'In e-commerce (internet commerce), tax evasion is easier.' In addition, firms were asked to indicate their opinion whether they think that i.) 'avoiding tax payments is a problem in Bulgaria?'(Panel B). They also needed to answer: ii.) 'In your opinion, what part of the profits (in percent) is officially declared in your industry?', iii.) 'In your opinion, what part of the revenues of the companies (in percent) in your industry is related to cash payments, in which the due taxes and social security contributions are evaded.', iv.) 'In your opinion, what part of the profits from e-commerce (in percent) is officially declared in your industry?' and, v.) 'In your opinion, what proportion of wages (in percent) in your industry are paid "in an envelope" without being declared?' (Panel C). We asked firms about their belief about receiving a tax audit (Panel D): i.) 'In your opinion, how high is the probability (in percent) that a company in your industry is subject to a tax audit/inspection?', ii.) 'In your opinion, how high is the probability (in percent) of your company or you personally being subject to tax audit / inspection?'. Finally, we also asked firms to indicate their number of employees and whether they use a tax accountant (Panel E).

Finally, we also asked firms how many employees they have and whether they use a tax accountant. Firms report to have on average between 1-20 employees. About 42% of the firms that completed the survey reported to use the service of an external tax

accountant. 25% of the firms have an internal accountant and 7% of the firms do not have a tax accountant. These percentages match reasonably well with our observation from the field experiment where a large fraction of the firms in the sample redirect their communication and use an external accounting service.

Of particular interest are answers reported in Panel C and D of the Table. The Panel C results indicate that the vast majority of firms in our sample (85%) think that tax evasion is a problem in Bulgaria. A large fraction further states that about 22% of earnings are evaded and, most importantly, about 22% of the wages are paid without reporting social security contributions. It may be, however, that firms underreport in Panel C for strategic reasons. That is, they downplay (knowing that they need to report somewhat realistic numbers) the percentage of social security payments evaded to avoid further scrutiny. The Panel D results are informative for our deterrence treatments. Respondents who give realistic responses (i.e., no extreme values such as zero or 100) indicate that they belief an inspection (any form of including audits) is likely to happen with a probability of 47%. These are remarkably high beliefs which can be explained in four ways: First, about 40% of respondents bunched their answers at a probability around 50% (i.e., they pushed the slider which we used to ask for perceived probabilities to the middle of the scale). Second, respondents interpret any type of check as inspection (e.g., that the reported tax return is at least looked at and checked for consistency). The tax authorities report that in this regard, the reported probability may not be unrealistic. Third, it may be that respondents actually believe that the audit probability is in between 40% and 50%. This explains why treatments with high (i.e., 40% and 60%) announced audit probabilities in the RCT are most effective and treatments with low announced audit probabilities (i.e., 1% and 10%) are not effective in establishing sustainable SSC compliance. Fourth, it may be that respondents report relatively high percentages for strategic reasons as they know that aggregate results of the survey will be shared with the authorities and high beliefs about inspection may signal that the tax authority is doing a good job and future investments in deterrence measures are not needed.

Figure F.1 and Figure F.2 provide detailed information on the Panel A and Panel B questions concerned with firms' reported tax moral and beliefs about cash payments as a potential evasion channel. The Figures report the frequency of answers on the 5-point Likert Scale (which ranged from 1-strongly disagree to 5-strongly agree). The upper-left panel of Figure F.1 shows survey respondents with respect to the very general question regarding tax morale. Expressed tax morale among small Bulgarian firms and self-employed is fairly high: 97% of all respondents strongly agree or agree that 'everyone should honestly declare their income in the tax return'. The picture becomes more dispersed as we turn to more nuanced questions about tax-evasion attitudes. The upper-right panel of the same Figure depicts the results for the survey question on acceptance of over-reporting deductions in the tax return. Since over-reporting of costs is effectively an action of tax evasion, we would expect similar results as for the previous statement on the general desirability of tax honesty. The share of respondents who strongly disagree or disagree that cost over-reporting is acceptable is 62%. The bottom-right panel of the Figure shows results for the justifiability of tax cheating. The pattern here is similar to the pattern that is observed for over-reporting of costs: 59% disagree or strongly

TABLE F.1. Summary of survey variables - All participants

	Completed responses	Partial responses	All responses
Panel A:		**Tax Morale**	
Everyone should honestly declare their income in the tax return:	4.656	4.574	4.643
(Strongly disagree 1 – Strongly agree 5)	(0.762)	(0.904)	(0.787)
It is acceptable to overstate costs in the tax return:	1.828	1.770	1.819
(Strongly disagree 1 – Strongly agree 5)	(0.930)	(0.884)	(0.923)
Legal avoidance of tax payments, if possible, is acceptable:	2.978	2.749	2.940
(Strongly disagree 1 – Strongly agree 5)	(1.366)	(1.347)	(1.365)
Cheating on taxes, if they exist, can always be justified:	1.825	1.880	1.834
(Strongly disagree 1 – Strongly agree 5)	(0.995)	(1.067)	(1.007)
Panel B:		**Evasion channels**	
Making payments in cash to evade taxes is a common offense:	2.868	2.765	2.852
(Strongly disagree 1 – Strongly agree 5)	(1.278)	(1.315)	(1.284)
In e-commerce (internet commerce), tax evasion is easier:	3.062	3.005	3.052
(Strongly disagree 1 – Strongly agree 5)	(1.157)	(1.198)	(1.164)
Panel C:		**Beliefs about tax evasion**	
% of firms who think tax evasion is a problem	85.46	90.06	86.16
(0% – 100%)	(35.27)	(30.01)	(34.54)
% Evasion of earnings in the own industry:	22.39	14.75	22.26
(0% – 100%)	(24.56)	(17.79)	(24.48)
% Revenue related to cash payments that hide taxes and SSC:	19.39	28.94	19.55
(0% – 100%)	(21.90)	(30.76)	(22.09)
% Evasion in e-commerce	42.86	34.44	42.72
(0% – 100%)	(36.11)	(31.13)	(36.03)
% Evasion of wages	21.45	27.88	21.56
(0% – 100%)	(22.93)	(30.92)	(23.08)
Panel D:		**Beliefs about audit probabilities**	
% Belief audit/inspection industry	51.76	48.36	51.47
(0% – 100%)	(19.43)	(22.18)	(19.68)
% Belief audit/inspection own firm	47.86	42.45	47.47
(0% – 100%)	(20.24)	(20.89)	(20.32)
Panel E:		**No of Employees/use of accountant**	
Number of employees	2.487	2.786	2.493
(1=0,2 = 1-10, 3 = 11-20,4 = 21-30,5 = 31-50,6 = 50+)	(1.245)	(1.578)	(1.252)
Use tax accounting firm	0.424	-	0.231
(0=No or 1=Yes)	(0.494)	-	(0.422)
In-house tax accountant	0.248	-	0.136
(0=No or 1=Yes)	(0.432)	-	(0.343)
No accountant	0.0722	-	0.0394
(0=No or 1=Yes)	(0.259)	-	(0.195)
Observations	942	783	1725

Notes: Averages with standard deviation in parentheses. Column 1: All questions answered. Column 2: Not all questions answered. Column 3: Average over all respondents. **Panel A:** Mean of variables reflecting tax morale. **Panel B:** Questions reflect statements concerning the evasion channel. In Panel A and B: High values (max 5) indicate that respondents strongly agree with the statement. Low values (min 1) indicate strong disagreement. **Panel C:** Mean beliefs about tax evasion. **Panel D:** Mean of realistic (i.e., excluding extreme values <1% and >89%) % belief about the likelihood of receiving a tax inspection in the own industry and in the own firm. **Panel E:** General survey information about number of employees and the use of an (external or internal) tax accounting service.

disagree that cheating is justified. The bottom-left panel of Figure F.1 deals with a survey question regarding the acceptability of legal tax avoidance. As one would expect, we see a higher acceptability than for illegal cheating behavior. However, roughly 20% of respondents express that they do not even find legal avoidance acceptable. Jointly the results suggest that individual tax morale is high and prevalent in the survey population. This may explain why our moral appeal treatments have been effective means to increase tax compliance.

Finally, Figure F.2 presents detailed information about firms opinion about cash payments as a means for tax evasion. 55% of the respondents indeed believe that cash payments are used to evade taxes.

FIGURE F.1. Tax Morale I–Tax Morale IV

Notes: Answers to statements about tax morale by question and category. Answers limited to respondents who completed the survey. **Tax Morale I:** Taxes should be paid honestly. **Tax Morale II:** Over-reporting cost is acceptable. **Tax Morale III:** Legal avoidance is acceptable. **Tax Morale IV:** Cheating on taxes is acceptable. Categories are: Strongly disagree (red), Disagree (orange), Neutral (blue), Agree (lime) and, Strongly agree (green). Size and percentage number in pie-slices indicates mean frequency of responses.

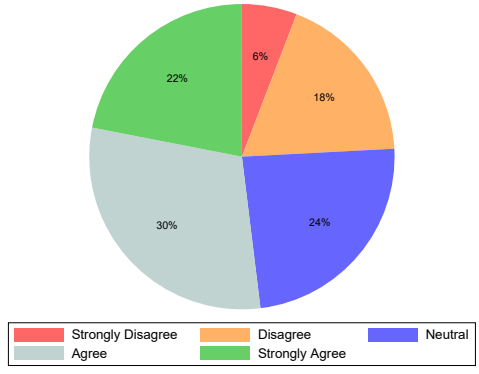


FIGURE F.2. Cash payments as a means for tax evasion

Notes: Answers to statements about cash payments as an evasion channel. Categories are: Strongly disagree (red), Disagree (orange), Neutral (blue), Agree (lime) and, Strongly agree (green). Size and percentage number in pie-slices indicates mean frequency of responses.

TABLE F.2. Summary of survey variables: Control Group

	Completed responses	Partial responses	All responses
Panel A:		**Tax Morale**	
Everyone should honestly declare their income in the tax return:	4.776	4.566	4.726
(Strongly disagree 1 – Strongly agree 5)	(0.645)	(0.957)	(0.736)
It is acceptable to overstate costs in the tax return:	1.739	1.645	1.716
(Strongly disagree 1 – Strongly agree 5)	(0.905)	(0.860)	(0.894)
Legal avoidance of tax payments, if possible, is acceptable:	3.133	2.711	3.032
(Strongly disagree 1 – Strongly agree 5)	(1.384)	(1.374)	(1.391)
Cheating on taxes, if they exist, can always be justified:	1.718	1.908	1.763
(Strongly disagree 1 – Strongly agree 5)	(0.924)	(1.061)	(0.960)
Panel B:		**Evasion channels**	
Making payments in cash to evade taxes is a common offense:	2.863	2.592	2.798
(Strongly disagree 1 – Strongly agree 5)	(1.285)	(1.308)	(1.294)
In e-commerce (internet commerce), tax evasion is easier:	3.029	2.895	2.997
(Strongly disagree 1 – Strongly agree 5)	(1.123)	(1.228)	(1.149)
Panel C:		**Beliefs about tax evasion**	
% of firms who think tax evasion is a problem	86.31	91.30	87.42
(0% – 100%)	(34.45)	(28.38)	(33.22)
% Evasion of earnings in the own industry:	22.15	12.58	21.70
(0% – 100%)	(24.55)	(18.59)	(24.36)
% Revenue related to cash payments that hide taxes and SSC:	20.82	31.92	21.35
(0% – 100%)	(24.00)	(33.58)	(24.57)
% Evasion in e-commerce	44	29.25	43.30
(0% – 100%)	(36.30)	(29.04)	(36.08)
% Evasion of wages	22.89	26.25	23.05
(0% – 100%)	(25.10)	(31.86)	(25.40)
Panel D:		**Beliefs about audit probabilities**	
% Belief audit/inspection industry	51.76	48.36	51.47
(0% – 100%)	(19.43)	(22.18)	(19.68)
% Belief audit/inspection own firm	49.00	43.28	58.32
(0% – 100%)	(19.43)	(21.21)	(19.63)
Panel E:		**No of Employees**	
Number of employees	2.461	2.583	2.466
(1=0,2 = 1-10, 3 = 11-20,4 = 21-30,5 = 31-50,6 = 50+)	(1.218)	(1.379)	(1.223)
Observations	241	198	439

Notes: Averages with standard deviation in parentheses. Gontrol group from survey experiment only. Column 1: All questions answered. Column 2: Not all questions answered. Column 3: Average over all respondents. **Panel A:** Mean of variables reflecting tax morale. **Panel B:** Questions reflect statements concerning the evasion channel. In Panel A and B: High values (max 5) indicate that respondents strongly agree with the statement. Low values (min 1) indicate strong disagreement. **Panel C:** Mean beliefs about tax evasion. **Panel D:** Mean of realistic (i.e., excluding extreme values <1% and >89%) % belief about the likelihood of receiving a tax inspection in the own industry and in the own firm. **Panel E:** General survey information about number of employees. Note: due to a mistake in the survey software the answers about the use of an (external or internal) tax accounting service was not elicited in this condition.

APPENDIX G: TAX SURVEY II: SURVEY WITH EMPLOYERS AND EMPLOYEES

We run a second survey specifically targeted at employers (owners or senior management of firms) and employees in Bulgaria. Following a similar procedure as in the first survey, we randomized employers whose firms were comparable to the general population of SMEs in our data into the survey. The tax authority invited the employers (5000) to participate by email. 361 employers (7%) started the survey and 212 completed it (response rate of 4%). Employees of Bulgarian SMEs were recruited by the market research firm dynata (LINK). Dynata invited employees who were comparable to the general the workforce of SMEs. 448 employees of Bulgarian SMEs started the survey and 436 finished.[38] In both cases, participants were redirected to our anonymous and confidential survey which was administered through Qualtrics (LINK).

The survey was conducted mainly for a follow up project.[39] We therefore only report results from questions related to this paper.[40] The results from the previous survey already indicated that a large share of firms report that wages are underreported to evade SSC (about 22%). Moreover, 20% of firms reported that cash payments are used to evade SSC. We dig deeper in to these channels and report the answers to questions on the form of salary payments of employees. Specifically, we report the answers to the question whether employees receive part or all of their salary in cash and, in case of a cash salary, the answers to the question who initiated the cash salary, the employer or the employee? We also asked this question for behavior in the past. Finally, we asked participants whether they have financial assets and for their estimate about potential income losses from payroll tax evasion.

Figure G.1 and Figure G.2 graphically illustrate how employers and employees report how they pay and currently receive their salary and, in case of a salary in cash, who decided that the salary was, at least partially, paid in cash. Although there are some differences in how employers and employees answer these questions three facts are imminent. First, a large share of the salary is paid in cash (between 27% and 43%). Second, in the majority of cases the employer decides upon the form of salary. 54% of employers report that they decide and 57% of employees report that their employer decides on the cash salary. Third, in spite of the main role of the employer, the figures highlight the collusive nature of cash salaries. In 27% (im employees are asked) and 10% (in the employer survey) employees initiate cash salaries. Notably, in another 15% (for the employers) or 35% (for employees) both play an equally important role in the decision process. Results for past behavior indicate that cash payments have been more prevalent in the past (62%) and that employers played an even greater role (employers involved in 85-90% of cash salary decisions).

We briefly mention that payroll taxes evasion hurts future income (i.e., pensions) and health care benefits as it undermines an employees SSC in the main text. Figure G.3 and G.4 solidify our statement. We ask employees whether they privately engage in consumption smoothing and have any financial assets/investments to provide for their

[38] Note that we targeted to receive 250 completed survey responses but received more completes.

[39] We received ethical approval by the Ethics Assessment Committee Faculty of Law and Nijmegen School of Management (ECLAM); EACLM Ref No: 2023.43.

[40] The full survey and all the results are available upon request.

pension or any other unforeseen life events. The vast majority (78%) report to have no financial assets which makes strategic evasion of payroll taxes from an employees perspective unlikely (see left panel of Figure G.3). The right-hand side of the figure confirms that evasion of payroll taxes is non-strategic. 82% of respondents who receive their salary in cash report to have no financial assets. Figure G.4 shows that employees substantially underestimate the loss in future income and benefits through payroll tax evasion. We asked participants to estimate the loss incurred by payroll tax evasion of BGN 500 (or BGN 1000) per month over 30 years. Actual losses were provided by the tax authorities and amount to about BGN 69,000 (or BGN 100,000). The Figure shows that participants substantially underestimate the losses (by up to 300% for participats who receive their salary in cash, see right hand panel of Figure G.4).

These findings underpin the potential size of the problem showcasing that, in 2023, still a large share of the salary is paid in cash which is prone to SSC evasion. The findings further show that while employers are the main responsible for cash salaries, employees play a crucial role in the decision process. Further, employees are likely unaware that under the table cash salaries hurt their future income and insurance benefits. The survey also shows that they do not take precaution to cushion these lower benefits in the future by having (financial) assets and privately engaging in consumption smoothing.

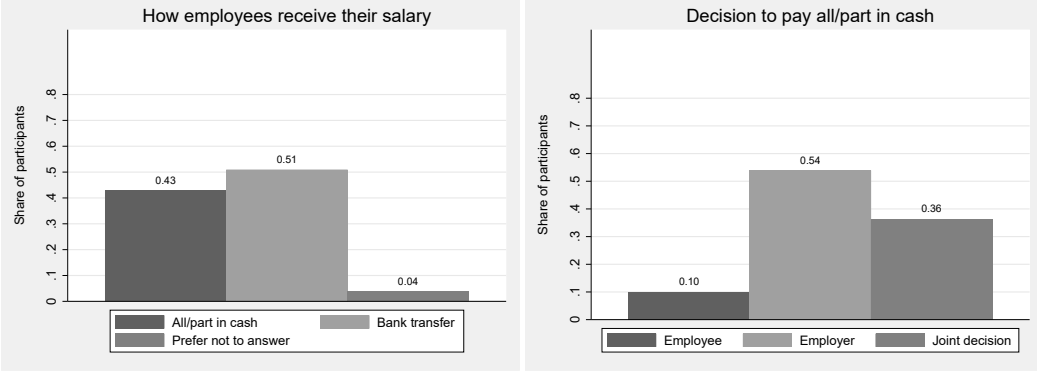


FIGURE G.1. Employers: Form of salary and driving force for cash salary

Notes: Self reported way of paying salary (left panel), driving force for paying cash salaries conditional on paying part/all of the salary in cash (right panel).

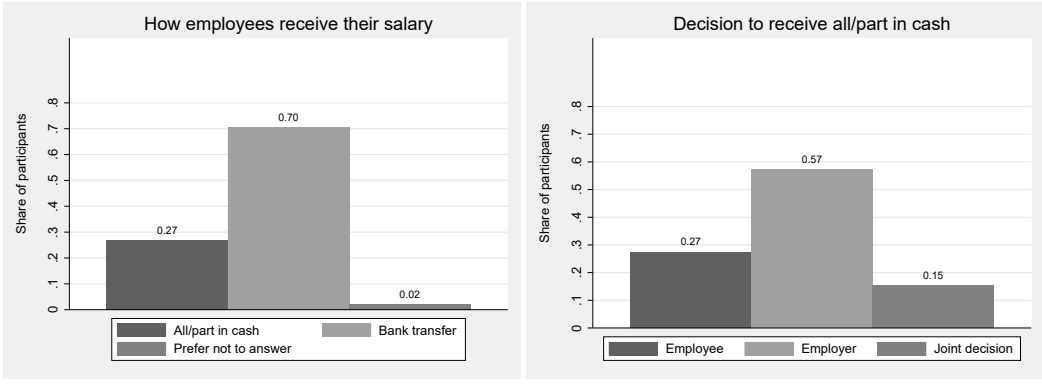


FIGURE G.2. Employees: Form of salary and driving force for cash salary

Notes: Self reported way of receiving the salary (left panel), driving force for receiving the salary in cash conditional on being payed part/all of the salary in cash (right panel).

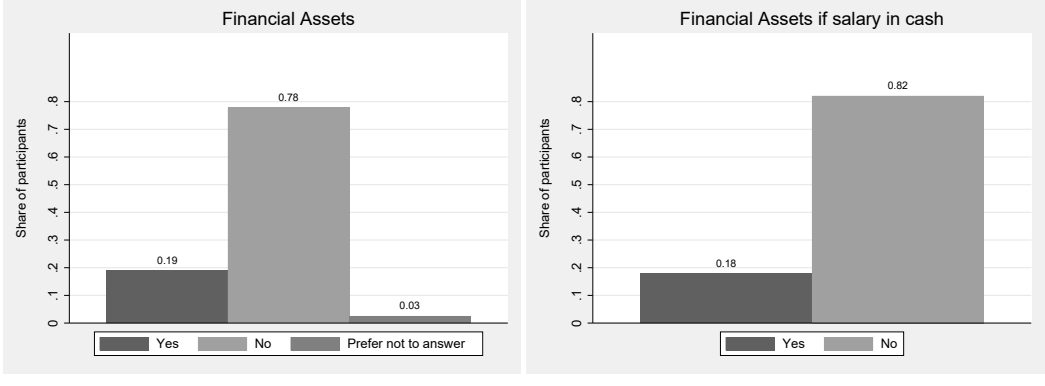


FIGURE G.3. Share of employees reporting to have financial assets

Notes: Share of employees with financial assets. Left panel: all employees. Right panel: Employees who report to receive their salary in cash. Note that none of the employees who receive their salary in cash answers with "Prefer not to answer".

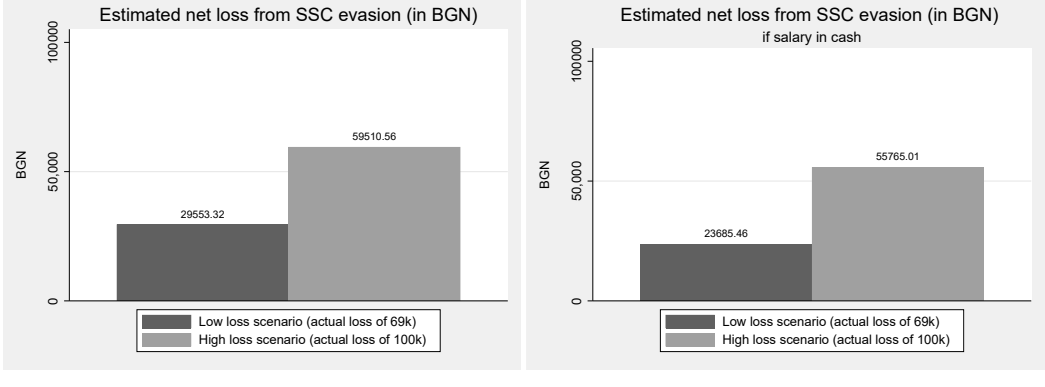


FIGURE G.4. Estimated net loss of future benefits from SSC evasion

Notes: Estimated average net loss (including benefits from higher cash salary) from payroll tax evasion for a period of 30 years. Low loss scenario: cash salary of BGN 500/month). High loss scenario: cash salary of BGN 1000/month. Left panel: all participants. Right panel: participants who report to receive salary in cash.