Can Taxes Shape Businesses' Sustainability?

Empirical Evidence on Three Dimensions

Inauguraldissertation zur Erlangung des akademischen Grades eines Doktors der Wirtschaftswissenschaften der Universität Mannheim

vorgelegt von

Alina Pfrang

Mannheim, 2024

Dekan: Joachim Lutz

Referent: Prof. Dr. Philipp Dörrenberg

Koreferent: Prof. Dr. Christoph Spengel

Koreferent: Prof. Dr. Johannes Voget

Verteidigt am 09.09.2024

Acknowledgements

I would like to extend my hearfelt gratitude to the whole tax team at the University of Mannheim. You accompanied me all the way through my doctorate, always lifted and cheered me up when things weren't going so well. Together we have shared numerous memorable experiences, both professionally and personally, fostering friendships that I deeply value. I would like to thank you for these friendships and your support. Special thanks to Jessica Müller, Katharina Schmidt, Inga Schulz, Sophia Wickel, Richard Winter and Sarah Winter for always being there for me. Thanks to Emilia Gschossmann and Christin Schmidt, you have been the most wonderful office colleagues.

I am deeply grateful to my three advisors, Philipp Dörrenberg, Christoph Spengel, and Johannes Voget. Your work initially inspired me during my studies, and your guidance and support during my PhD were crucial. I have learned a lot from you, whether through academic lessons or personal discussions. Thank you for your unwavering support and for giving me this opportunity.

I would also like to acknowledge my co-authors Philipp Dörrenberg, Emilia Gschossmann, Jan Schmitz, and Jan Zental. Without your collaboration, this dissertation would not have been possible. I have gained so much from our partnership. My sincere thanks to Lisa De Simone and Jaron Wilde for hosting me during my research stay. Your support and insightful comments significantly advanced my research. I am also grateful to Christine Bürner and Sandra Dumann for your invaluable support and encouragement.

A special thank you to my student research assistants who supported me throughout my doctorate. Having developed a passion for empirical tax research as a student assistant myself, it was important for me to pass on this enthusiasm and be a good mentor to you. I hope I succeeded. Thanks to Lotta Held, Alexandra Müller, Alexandra Pasdeloup, Christin Schmidt, and Mario Wagner.

I gratefully acknowledge the financial support from the Leibniz Science Campus

Mannheim-Taxation, the Graduate School of Economic and Social Sciences at the University of Mannheim, and the Women Go Abroad funding of the University of Mannheim.

Lastly, I want to express my immense gratitude to my friends and family. You are the best thing that could have happened to me. Thank you for the wonderful moments and countless conversations that have shaped me. Without you I wouldn't be here today. Thanks to my mom and dad, who I love more than anything. Thanks to my Oma Inge, my brother Luca, and my aunts and uncles, who are my biggest supporters. Thanks to my athletics girls who make my day (nearly) every Friday and to Ruhama, who shows me the world with her eyes every week. Thanks to Alina, Anne, Carina, Isabella, Julia, Linda, Melanie and Zoe, you are the best.

Mannheim, June 2024 *Alina Pfrang*

Contents

List of Figures

List of Tables

List of Abbreviations

2SLS Two-stage Least Squares.

AEA-RCT American Economic Association Randomized Control Trial.

AT Austria.

ATAD Anti-Tax Avoidance Directive.

BE Belgium.

BEPS Base Erosion and Profit Shifting.

BGN Bulgarian Lev.

CbCR Country-by-Country Reporting.

CFC Controlled Foreign Company.

CH Switzerland.

CIT Corporate Income Tax.

CO_2 Carbon Dioxide.

CPI Corruption Perception Index.

CZ Czech Republic.

DiD Difference-in-Differences.

DK Denmark.

EATR Effective Average Tax Rate.

EEA European Economic Area.

ERFT European Road Freight Transport.

EU European Union.

EUR Euro.

FE Fixed Effects.

GDP Gross Domestic Product.

GDPPC Gross Domestic Product per Capita.

GMAP Google Maps.

IP Intellectual Property.

IV Instrumental Variable.

LATE Local Average Treatment Effect.

LU Luxembourg.

MHT Multiple Hypothesis Test.

MNE Multinational Enterprise.

NL Netherlands.

NO_x Nitrogen Oxide.

NRA National Revenue Agency.

OECD Organisation for Economic Co-operation and Development.

OLS Ordinary Least Squares.

OS Open Source.

PL Poland.

PM Particulate Matter.

PPLM Poisson Pseudo Maximum Likelihood.

RCT Randomized Control Trial.

SDG Sustainable Development Goal.

SLCPs Short-lived Climate Pollutants.

SMEs Small and Medium-sized Enterprises.

SSC Social Security Contributions.

SUTVA Stable Unit Treatment Value Assumption.

TWFE Two-way Fixed Effects.

UBA Umweltbundesamt.

UN United Nations.

US United States.

USD United States Dollar.

VAT Value Added Tax.

ZEW Leibniz Centre for European Economic Research.

1 Introduction

Sustainability has emerged as a critical topic in contemporary discourse, profoundly influencing global policies and economic and societal behavior. Defined as meeting the needs of the present without compromising the ability of future generations to meet their own needs (United Nations 1978), the current debate on sustainability is largely driven by the global climate crisis and encompasses environmental, societal and economic dimensions such as pollution, poverty, inequality and fairness. In 2015, the United Nations (UN) agreed on 17 objectives to achieve a sustainable future, the Sustainable Development Goals (SDGs) (United Nations 2015).[1]

Taxes play a multifaceted role in promoting sustainable development through its three main functions (Avi-Yonah 2006). First, the revenue function enables governments to collect tax revenue to finance public investments, which are essential for infrastructure, education, healthcare, and other public services. Second, the redistributive function reallocates resources from high-income to low-income taxpayers, promoting equality and contributing to a fair tax system. Third, the steering function enables tax policies to be designed to incentivize or disincentivize certain behavior at both the individual and corporate level, for example, by encouraging environmentally-friendly consumption.

Thus, taxes are an important policy instrument for achieving the SDGs (Kopetzki et al. 2023; The Platform for Collaboration on Tax 2023). Effective tax policies can enhance fiscal sustainability, ensuring the solvency of the public sector through the revenue function, thereby contributing to SDGs 1 (No Poverty), 2 (Zero Hunger), 3 (Good Health and Well-being), 4 (Quality Education), and 6 (Clean Water and Sanitation). The redistributive function can help achieve SDGs 10 (Reduced Inequalities) and 16 (Peace, Justice,

[1] The 17 SDGs are defined as follows. Goal 1: No Poverty. Goal 2: Zero Hunger. Goal 3: Good Health and Well-being. Goal 4: Quality Education. Goal 5: Gender Equality. Goal 6: Clean Water and Sanitation. Goal 7: Affordable and Clean Energy. Goal 8: Decent Work and Economic Growth. Goal 9: Industry, Innovation and Infrastructure. Goal 10: Reduced Inequalities. Goal 11: Sustainable Cities and Communities. Goal 12: Responsible Consumption and Production. Goal 13: Climate Action. Goal 14: Life below Water. Goal 15: Life on Land. Goal 16: Peace, Justice and Strong Institutions. Goal 17: Partnerships for the Goals.

and Strong Institutions). Finally, using taxes as a regulatory steering tool can promote SDGs 7 (Affordable and Clean Energy), 9 (Industry, Innovation, and Infrastructure), 12 (Responsible Consumption and Production), and 13 (Climate Action).

Businesses play a major role in the sustainability debate. Their transformation towards sustainability is crucial as they are significant contributors to environmental damage and pollution. In 2023, firms were responsible for 20% of global greenhouse gas emissions (International Energy Agency 2024). Moreover, businesses are responsible for remitting about 85% of taxes (Milanez 2017; Slemrod and Velayudhan 2018), making them a significant factor for fiscal sustainability. Of particular concern in this context are issues such as tax avoidance by multinational enterprises (MNEs) (see, e.g., Hanlon and Heitzman 2010 or Wilde and Wilson 2018 for reviews) and tax evasion[2] (see, e.g., Slemrod 2019 for a review), that undermine both the revenue and redistributive functions of taxation, threatening key sustainability principles such as fairness, equality, and strong institutions.

Understanding how tax policies can influence businesses' sustainable development is vital. Effective taxation strategies can steer businesses towards more sustainable practices, ensuring they contribute positively to the environment and society. By addressing tax avoidance and evasion, policymakers can enhance the fairness and effectiveness of the tax system, ensuring adequate resources for public investments and reducing inequalities. Whether specific tax policies can shape the sustainable transformation of businesses is an empirical question that this dissertation aims to address by studying three different dimensions. Chapter Two analyzes how to fight small firms' tax evasion and how to foster their tax compliance with payroll taxes. Chapter Three evaluates the role of environmental taxes in the transportation sector. Chapter Four analyzes firms' responses to an anti-tax avoidance regulation mandating firms to pay their 'fair share' of taxes. By analyzing these three dimensions, this dissertation seeks to provide empirical evidence on the potential of

[2] Tax evasion refers to the illegal act of not paying taxes, while tax avoidance involves legally minimizing tax payments, which is generally considered to be inconsistent with the concept of sustainability (Avi-Yonah 2014; Bird and Davis-Nozemack 2018).

tax policies to foster businesses' sustainability.

In Chapter Two[3], we answer the question of how to improve payroll tax compliance among small firms. High levels of tax compliance are important to support social welfare systems worldwide, thereby promoting fiscal sustainability and social and economic equality (United Nations 2015; De Neve et al. 2021; Floyd et al. 2022). Tax evasion is a major problem in many countries around the world. For example, the International Revenue Service reported a tax gap of 406 billion United States Dollar (USD) for the United States (US) in 2016, representing 16.3% of tax liabilities that are never remitted. How to combat payroll tax evasion is especially important as payroll taxes are a significant source of revenue for many countries and affect both employers and employees. Despite its importance, the literature examining the firms' payroll tax compliance behavior is scarce.

Collaborating with the Bulgarian tax authorities, we conduct a randomized field experiment testing strategies to improve payroll tax compliance among small firms. Specifically, we send deterrence messages with different audit probabilities (1%, 10%, 40%, or 60%) and several novel moral appeal messages highlighting the benefits of tax-financed public goods to Bulgarian firms. Using monthly administrative tax return data, we find that both deterrence and moral appeals significantly improve firms' payroll tax compliance in the months following the intervention. Receiving a high audit probability has a treatment effect about 50% larger than receiving a moral appeal with substantial effects on larger firms and those deemed riskier by the tax authority. In a cost-benefit analysis we estimate that our experiment generated USD 10,856,280 in additional payroll tax revenue for the Bulgarian government. We conclude that the experiment increased businesses' tax compliance contributing to a more sustainable development.

In Chapter Three[4], we examine the impact of environmental taxes on commercial transportation and its environmental consequences. Specifically, we look at environmental

[3] This is joint work with Philipp Dörrenberg and Jan Schmitz and is available online as a Working Paper, Dörrenberg et al. 2024.

[4] This is a joint work with Jan Zental.

taxes imposed on diesel fuel and how they affect trucking companies in Europe. Road transport is a significant source of emissions, accounting for 15% of global Carbon Dioxide (CO_2) emissions (European Environment Agency 2022; International Energy Agency 2022). Thus, reducing its environmental impact is essential to mitigate the global climate crisis and achieving the SDGs (IPCC 2021). Environmental taxes on fuel are considered to be an important policy tool to incentivize sustainable transportation practices and discourage high-emission activities. However, understanding how commercial transportation responds to environmental taxes, especially when they vary across national borders, is a complex and understudied area.

We fill this gap and provide insights into the behavior of commercial transportation in response to environmental taxes, informing policymakers on strategies that promote sustainability in the transportation sector. First, we analyze how commercial trucks respond to differences in environmental taxes on diesel fuel across European borders, using administrative toll data on truck flows at German borders. Our results show that higher environmental tax rates on diesel fuel reduce truck traffic through a country. Second, we study truck journeys across Europe and find that higher environmental taxes at the same time cause trucks to take detours, leading to unintended environmental consequences. Supplementing our analysis with data on environmental emissions, we find that detours lead to increased pollution at the borders and an estimated additional 1 million tons of carbon emissions.

Chapter Four[5] evaluates the responses of MNEs to an anti-tax avoidance regulation implemented by the EU, specifically the Controlled Foreign Company (CFC) rules. Globalization and differing tax rates worldwide have led to tax avoidance by MNEs causing the erosion of national tax bases, which undermines a sustainable development. In response, public and political interest in curbing these aggressive tax planning practices has led to initiatives like the Organisation of Economic Cooperation and Development's (OECD)

[5] This is joint work with Emilia Gschossmann and is available online as a Working Paper, Gschossmann and Pfrang 2024.

Base Erosion and Profit Shifting (BEPS) program, which includes CFC regulations. The EU adopted these rules in 2016 under the Anti-Tax Avoidance Directive (ATAD) with the aim to discourage companies from shifting income to low-tax jurisdictions by subjecting that income to additional domestic taxation. We study whether this tax policy contributes to businesses' sustainability by assessing the effectiveness of this anti-avoidance rule in curbing profit shifting and identifying potential unintended consequences .

Using firm-level financial data and a difference-in-differences (DiD) research design, we examine the effects of CFC rules under the ATAD on the location, financial, and economic activities of MNEs. Our findings indicate that the CFC rules are only partially effective in reducing income shifting. While the number of low-taxed CFC subsidiaries decreases, the financial income of the remaining subsidiaries remains largely unchanged. Additionally, we observe increased costs for employees in CFC subsidiaries without altering the number of employees or investment, suggesting that MNEs circumvent the rules by artificially increasing economic activity in these locations and exploiting the exemptions included in the regulation.

Finally, Chapter Five provides a brief summary of the main findings and conclusions of each chapter of the dissertation.

The dissertation is based on papers originally written for publication in journals and other outlets. The following Table 1 lists the papers, indicating the co-authors, the publication status, and the contribution of the author of this dissertation.

Table 1: Contribution table

Paper	**How to Improve Small Firms' Payroll Tax Compliance? Evidence from a Randomized Field Experiment**
Co-Authors	Phillip Dörrenberg, Jan Schmitz
Status	SSRN Discussion Paper
Own (Key) Contribution	Preparation of administrative tax return data for analysis Qualitative literature survey and institutional background Development of hypothesis Introduction and positioning of the article Devising and conducting (poisson) difference-in-differences and event study regression analysis including comprehensive robustness and heterogeneity analysis Interpretation of results and derivation of economic implications
Paper	**The Impact of Environmental Taxes on Commercial Traffic and Its Environmental Consequences**
Co-Authors	Jan Zental
Status	Working Paper
Own (Key) Contribution	Development of hypothesis Preparation of administrative toll data, survey data and emissions data Collecting panel data on environmental tax rates on fuel Literature review and institutional background Introduction and positioning of the article Implementing geo-coding to analyse traffic data Devising and conducting instrumental variable regression analysis including comprehensive robustness analysis Interpretation of results and derivation of economic implications

Continued on next page

Continued from previous page

Paper	**Multinationals' Location, Financial and Real Responses to the EU-wide Implementation of CFC Rules by the ATAD**
Co-Authors	Emilia Gschossmann
Status	SSRN Discussion Paper
Own (Key)	Collection of ownership data and identifying ownership chains
Contribution	Qualitative literature survey and institutional background
	Development of the hypothesis
	Introduction and positioning of the article
	Devising and conducting poisson difference-in-differences and event study regression analysis including comprehensive robustness and heterogeneity analysis
	Interpretation of results and derivation of economic implications

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

Co-Authors: Philipp Doerrenberg and Jan Schmitz

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 Randomized Control Trial (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

Acknowledgements: We are very grateful to the tax authoritiy in Bulgaria (National Revenue Agency (NRA)), especially Kalin Atanasov and Nicolay Petkov, for giving us the opportunity to design and organize the field experiment, and for providing data access and great support throughout. The paper does not necessarily reflect the views of the NRA. Our contact to the Bulgarian tax authorities was established at European Commission workshops. Thanks to Thomas Hemmelgarn and Benedikt Herrmann for initiating these workshops and inviting us. We thank Albrecht Bohne, Jonas Fooken, Brian Galle, Benedikt Herrmann, Justin Holz, Jonathan Karver, Matthias Kasper, Henrik Kleven, Eric Ohrn, Andreas Peichl, Kunka Petkova, Sebastian Siegloch, Joel Slemrod, Christian Thöni, Andre van Hoorn, Felix Vetter, Johannes Voget, Mazhar Waseem, Utz Weitzel, Stefan Zeisberger, Christian Zehnder and participants at many workshops/seminars/conferences for helpful comments and suggestions. The field experiment is registered pre-trial in the American Economic Association Randomized Control Trial (AEA-RCT) registry under number AEARCTR-0002390. We obtained ethical approval from the University of Lausanne (Switzerland). Alina Pfrang gratefully acknowledges financial support from the Graduate School of Economics and Social Sciences of the University of Mannheim.

2.1 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 (De Neve et al. 2021; Floyd et al. 2022).[6] While the existing tax compliance literature focuses on income taxes (personal income taxes of individuals and, to a lesser extent, business taxes) and Value Added Tax (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 (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 can reduce 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

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

for payroll taxation, and its collusive nature undermines the power of third-party reporting (Paulus 2015; Slemrod 2019; Bjorneby et al. 2021).[7]

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 A.7). 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 Randomized Control Trial (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, pre-registered RCT in cooperation with the federal tax authority of Bulgaria (National Revenue Agency (NRA)) to explore how strategies that go beyond third-party reporting (Kleven et al. 2011; Internal Revenue Service 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

[7] 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).

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 A.7).

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 2.2.1). 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 Small and Medium-sized Enterprises (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).[8] 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 A.7 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.

[8] 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 2.2.4. Our results are robust to including firms which correspond with the authorities through their tax professional (see Appendix A.4).

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 (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 Organisation for Economic Co-operation and Development (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). 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; Cohn et al. 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.[9]

[9] 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

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.[10] 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.[11] 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

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 (Döerrenberg and Schmitz 2017; Boning et al. 2020; Ortega and Scartascini 2020), the effects of audits (Lediga et al. 2020; Best et al. 2021), corporate tax administration reform (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).

[10] 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.

[11] 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.

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. 2023 for lab evidence). Because of the higher cost of audits, moral messages directed at firms appear to generate 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.

2.2 The Field Experiment and Data

2.2.1 Institutional Context

Our RCT took place in Bulgaria, a post-communist country in southeastern Europe. With a Gross Domestic Product (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 European Union (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 2021b). 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[12] 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.[13] 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 pre-payments or the such). The monthly reported gross income also serves as the basis for calculating

[12] A flat tax of 10% applies to corporate and personal income. The VAT system is explained in Appendix A.5.
[13] The exact tax rate depends on the industry and an employee's occupation.

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 Bulgarian Lev (BGN) 440 million (about USD 220 million) from payroll tax evasion (Williams and Horodnic 2017). To evade payroll taxes, employers 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.[14] 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).[15]

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%

[14] 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.

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

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 believe 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 2.4 where we present our findings. The full results of the survey and the design are presented in Appendix A.6.

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 (5,000 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 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 employ-

ees 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 A.7.

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.

2.2.2 Treatments

We implement one baseline (control or placebo) treatment (Section 2.2.2), 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 2.2.2). All treatment letters (original Bulgarian along with English translation) are shown in Appendix A.2.

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.*

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.

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%).[16]

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*

[16] 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.

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.

2.2.3 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 A.5 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 A.5).[17] 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.

2.2.4 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

[17] While we transparently report and discuss VAT results in Appendix A.5, 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).

(e.g., interest for the effect of specific audit probabilities and for moral appeals). For our field experiment the authorities were willing to send 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 pseudonymized information about the universe of small and medium sized 172,172 SMEs that are registered for VAT.[18] 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 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.[19] The

[18] 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.

[19] 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.

treatments were balanced in terms of their 2016 SSC and VAT tax bases (see Appendix A.4 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 (5,000) 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 A.7 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.

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 Stable Unit Treatment Value Assumption (SUTVA)). 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.1 for more details for our main sample).[20]

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

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

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 A.4; also see Section 2.4 Experimental Results). Our results are generally robust and very similar when considering the full sample.

2.3 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.[21]

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) 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

[21] In light of their different nature, we do not derive any predictions for a comparison between the *Deterrence* and the *Moral* treatments. In Section 2.5.3, we discuss the direct empirical comparisons between the moral and deterrence treatments.

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.[22]

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.[23] 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

[22] 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.

[23] 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.

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 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:

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

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., $a = 0$).[24] 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 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

[24] 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.

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.[25]

[25] The *Deterrence* letters obviously include the *Ambiguity* condition. We will take a closer look at the *Ambiguity* condition as we study mechanisms in Section 2.5.

2.4 Experimental Results

2.4.1 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 compliance (such as Pomeranz 2015, Bjorneby et al. 2021, Bérgolo et al. 2023, and Holz et al. 2023), we use a Difference-in-Differences (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.[26] 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 Two-way Fixed Effects (TWFE) model with firm and month fixed effects (see Appendix A.3).[27] Our 'static' model has the following form:

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

$Y_{i,t}$ 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

[26] Estimates only using the post-treatment data generally align with our DiD estimates, though they are measured less precisely (see Appendix A.3).

[27] 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).

– and a firm's industry) which we include to gain precision. Results are robust to excluding controls (see Appendix A.3). $\varepsilon_{i,t}$ 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 2.5.3). 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 A.3, where we use the Multiple Hypothesis Test (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 Ordinary Least Squares (OLS). The resulting coefficient of interest measures treatment effects in absolute money values (expressed in BGN). Relative effects are estimated using Poisson regressions and the coefficient of interest is interpreted as a semi-elasticity.[28] An important advantage of the Poisson 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 sea-

[28] 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 Poisson results.

sonality) in all specifications (results are robust to including different number of pre-treatment months, see Appendix A.3). 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 A.3. 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 A.3 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 A.4. The results using this sample are generally robust, though sometimes smaller in size (which is consistent with the 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.

2.4.2 Moral Treatments

Static DiD. Table 2 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.

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%.[29]

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 im-

[29] 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.

Table 2: 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

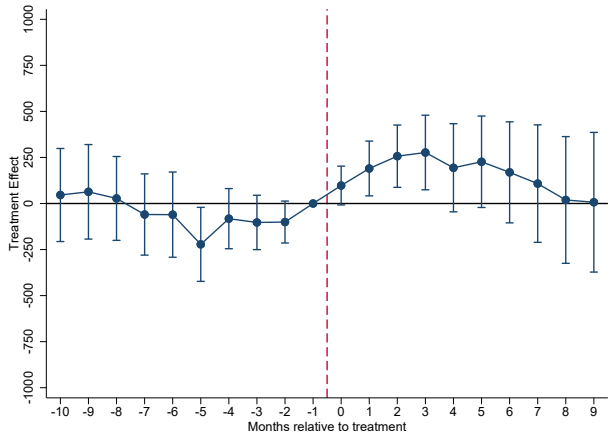
Note: Treatment effects of moral messages on SSC. Displayed are DiD regression estimates of treatment messages on tax base SSC. The estimated treatment effects are relative to the baseline condition. Treatment effects are reported in absolute terms based on OLS regressions and in relative terms based 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

mediate 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.[30] 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).

Figure 1: Dynamic absolute effects of moral treatments on SSC

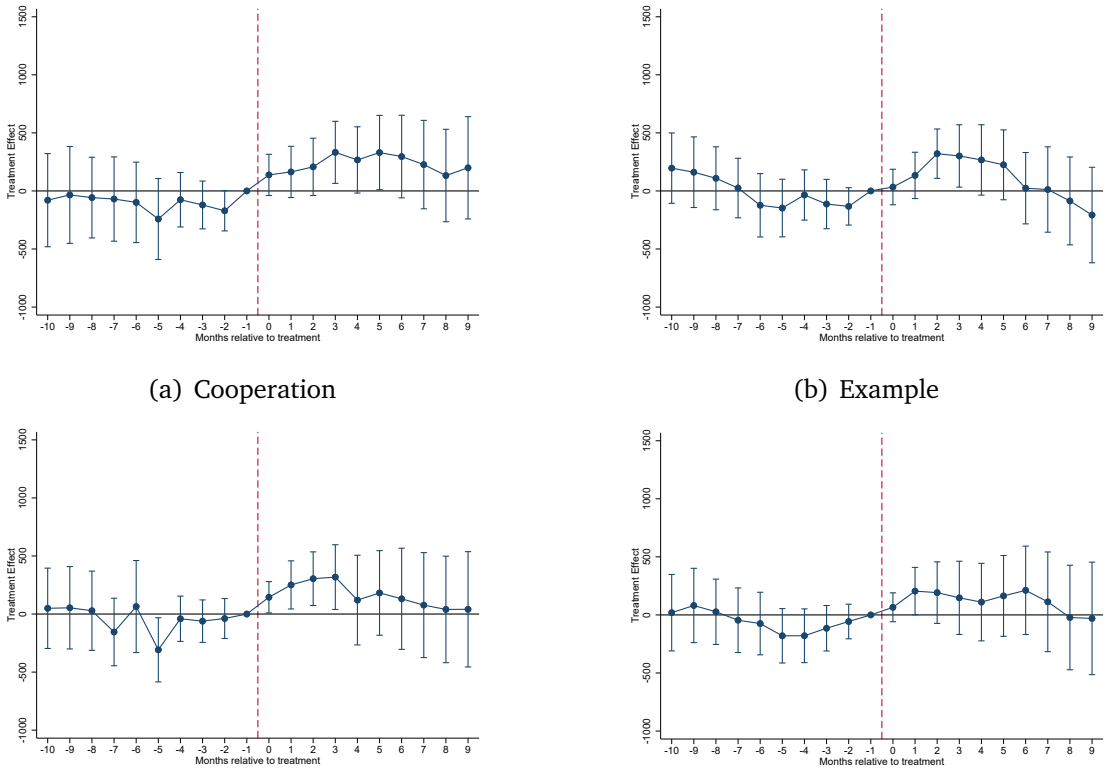


Note: Pooled absolute monthly treatment effects of the moral appeal messages (Cooperation, Example, Necessity, Picture). The points plotted are the estimated DiD regression coefficients of treatment messages on tax base SSC by months relative to the treatment. Treatment effects are relative to the control 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.

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, 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

[30] 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.

Figure 2: Dynamic absolute effects of moral treatments on SSC by sub-treatment



(a) Cooperation

(b) Example

(c) Necessity

(d) Picture

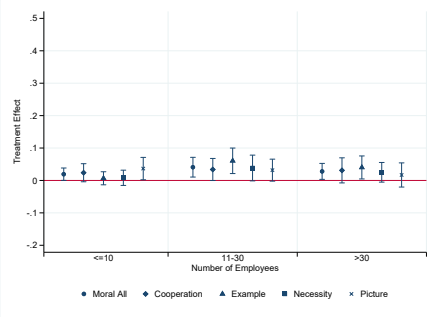
Note: Monthly absolute treatment effects for each of the moral appeal messages (Cooperation, Example, Necessity, Picture) on SSC. The points plotted are the estimated DiD regression coefficients of treatment messages on tax base SSC by months relative to the treatment. Treatment effects are relative 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.

payment behavior in Panel (d) and riskiness (from 1- low-risk to 3 - high-risk as assessed by the tax authorities) in Panel (e).

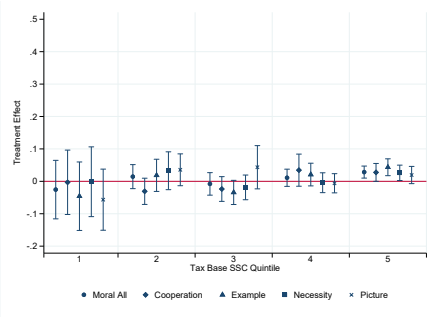
Our heterogeneity analysis shows that larger firms, riskier firms and firms with fluctuating SSC payments are more likely to change payroll tax reporting behavior in response to moral treatment interventions. In addition, we observe a higher compliance effect in the construction industry, which is usually 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

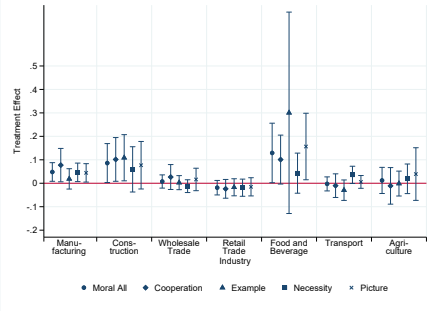
Figure 3: Heterogeneous effects of moral treatments on SSC (in percent)



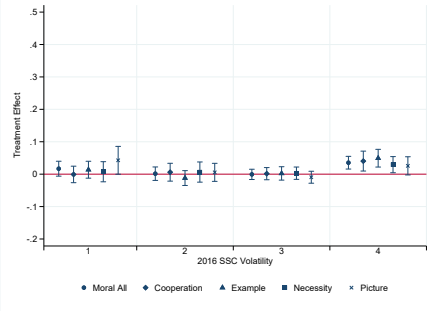
(a) Firm size: Number of employees



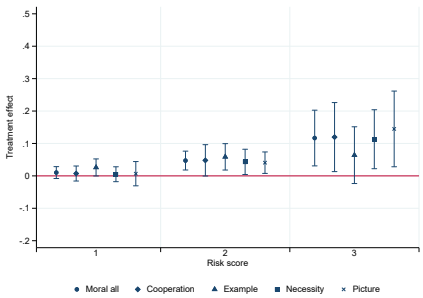
(b) Firm size: Pre-treatment tax base



(c) Industry



(d) Volatility: 2016 tax base



(e) Risk score

Note: 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.

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.

2.4.3 Deterrence treatments

Static DiD. Table 3 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 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.[31] 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 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 –

[31] 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%.

Table 3: 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	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	87,553	56,652	56,147	44,735	41,772	55,817
No of Firms	15,789	8,448	8,350	6,664	6,190	8,297
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	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	150,976	97,605	96,741	77,045	71,948	96,166
No of Firms	15,789	8,448	8,350	6,664	6,190	8,297

Note: Treatment effects of deterrence 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

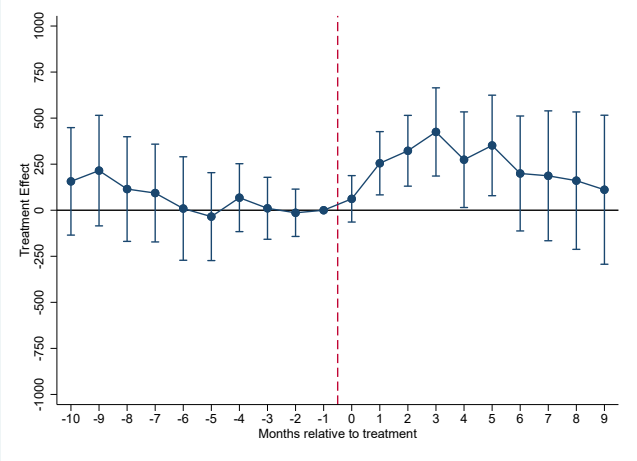
we discuss these results in more detail in Section 2.5 below.

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.*

Figure 4: Dynamic effects of deterrence treatments on SSC



Note: 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.

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

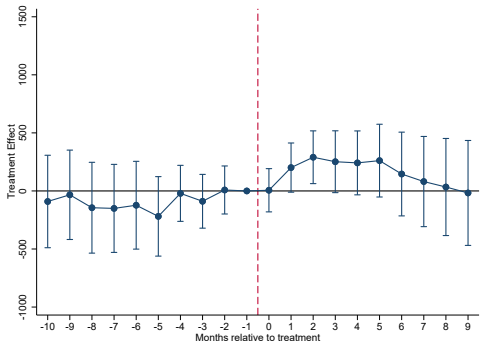
and especially in the *Audit 60%* treatment (Panel (c)-(d) of the Figure). The dynamic treatment effects thus confirm the static DiD results and are consistent with our predictions: high audit probabilities lead to sizable and stable effects over time, while the effect of low audit probabilities on tax base SSC is smaller and less sustainable in our sample.[32]

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.

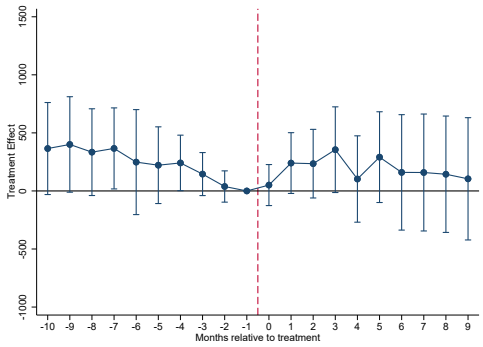
The treatment effects are particularly pronounced for large firms (with many employees and which are in higher 2016 tax base quintiles), firms that are classified as high-risk by tax authorities, and firms with volatile pre-experimental filing behavior. These findings align with Holz et al. 2023, who 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.

[32] 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.

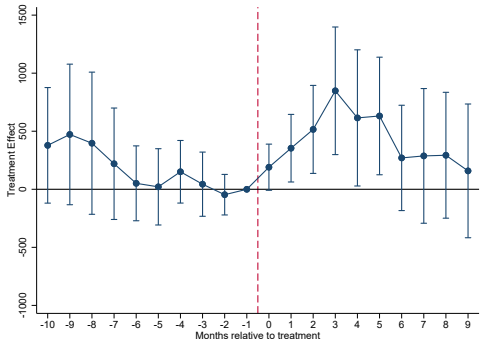
Figure 5: Dynamic absolute effects of deterrence treatments on SSC by sub-treatment



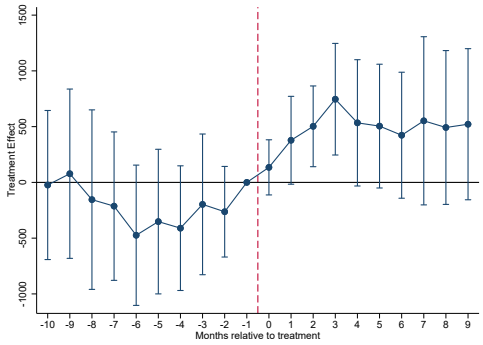
(a) Audit 1%



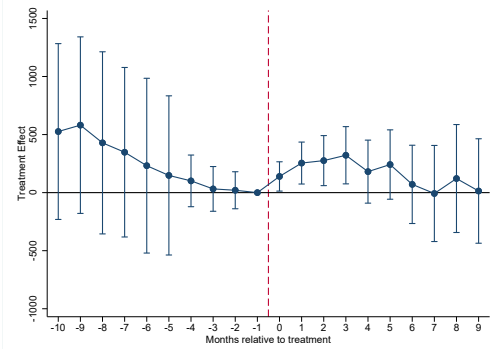
(b) Audit 10%



(c) Audit 40%



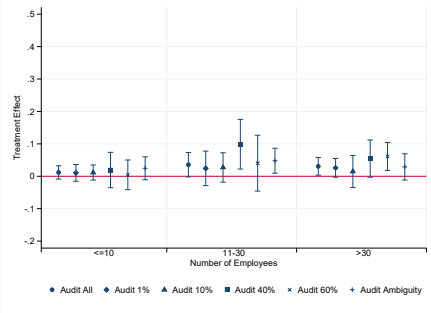
(d) Audit 60%



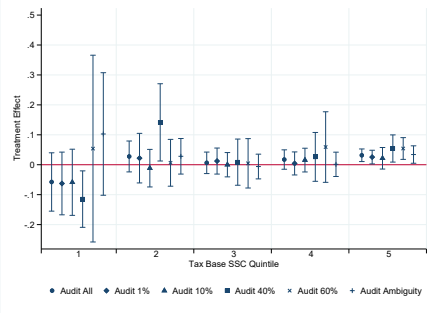
(e) Ambiguous

Note: 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.

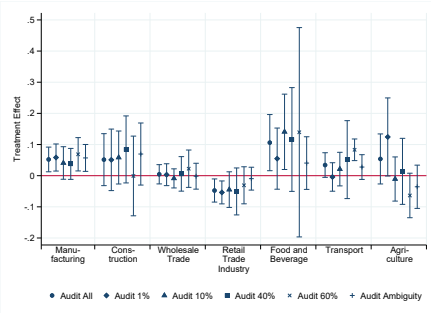
Figure 6: Heterogeneous effects of deterrence treatments on SSC (in percent)



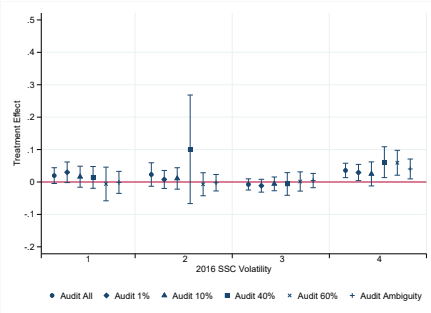
(a) Firm size: Number of employees



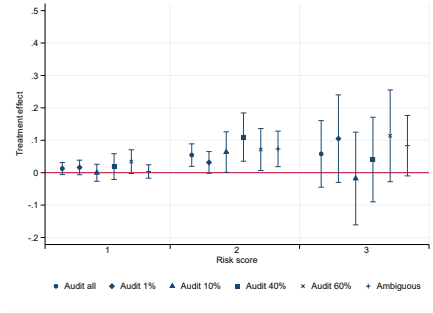
(b) Firm size: Pre-treatment tax base



(c) Industry



(d) Volatility: 2016 tax base



(e) Risk score

Note: 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.

2.5 Mechanisms and Revenue Effects

2.5.1 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 2.3, 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 A.36 in Appendix A.6). This self-reported high tax morale is not necessarily at odds with the self-resorted assessment that tax evasion is a problem in Bulgaria (85% of respondents believe this): firms may allocate 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. 2023 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.[33] Another possibility may be that shorter 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.

[33] 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.

2.5.2 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 2.2.2). 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 2.3 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.[34] Assuming that the *Ambiguous* letter somehow increases

[34] 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

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 4 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% 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.

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.

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.*

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 A.19 in Appendix A.3 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.

Table 4: Treatment effects of audit probability messages relative to ambiguous treatment on SSC

Panel A:	**4 post-treatment months**				
	Audit all	*Audit 1%*	*Audit 10%*	*Audit 40%*	*Audit 60%*
	(1)	(2)	(3)	(4)	(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	*Audit 1%*	*Audit 10%*	*Audit 40%*	*Audit 60%*
	(1)	(2)	(3)	(4)	(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

Note: 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 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.

2.5.3 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.[35]

To calculate the increase in payroll tax revenue from the RCT, we multiply the estimated treatment effects from Tables 2 and 3 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.[36] 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 $(hours) \times 50\ (BGN/h) =$ BGN 1,000 (about USD 500) as costs for an audit in the deterrence treatments.[37]

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 BGN 15,524,782 (USD 7,808,872) for the overall (10 months) treatment revenue. For the *Deterrence* treat-

[35] 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 90 for potential spillover effects of SSC compliance on VAT.

[36] 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 USD 0.02) in the moral treatments. Note that the assumed labor investment (16 hours) and wage (BGN 50/h) are likely overestimates.

[37] 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.

ments, 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 5: Experiment induced extra SSC revenue per letter

Panel A:		**Moral Treatments**					
		Moral all (1)	*Cooperation* (2)	*Example* (3)	*Necessity* (4)	*Picture* (5)	
Immediate	(*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	(*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	(*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	(*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

Note: 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* $\times$ 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).[38] To be implemented in practice, it is sometimes required that the benefits of the intervention exceed the costs by a certain margin. Table 5 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 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 A.3 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.

[38] 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.

2.6 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 au-

dit 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.

3 The Impact of Environmental Taxes on Commercial Traffic and Its Environmental Consequences

Co-Author: Jan Zental

Abstract: This paper examines how commercial trucks respond to differences in environmental taxes on diesel fuel across European borders. By analyzing truck flows at German borders using administrative toll data we study the impact of environmental taxes on truck traffic. Next, we investigate if trucks choose longer routes to avoid high environmental taxes by studying individual journeys across Europe. We find that commercial transport responds to changes in environmental taxes. An increase in a country's tax rate reduces truck traffic through that country. However, these tax changes also cause more trucks to take detours leading to unintended environmental consequences. Using air pollution data, we observe that pollution at the border increases as a result of tax-induced increases in cross-border truck flows. In addition, we estimate an additional 1 million tons of carbon emissions due to the detours taken.

Keywords: Environmental Taxes, Truck Traffic, Mobility, Tax Avoidance, Emissions

Acknowledgements: For their helpful comments and suggestions, we thank Lisa De Simone, Philipp Doerrenberg, Cristi Gleason, Max Pflitsch, Inga Schulz, Johannes Voget, Nicolas Woelfing, Jaron Wilde, Brady Williams, Richard Winter, the University of Texas Tax Readings Group and seminar participants at the Tax Mini Workshop, the Mannheim Taxation Campus Meeting, the Leibniz Centre for European Economic Research (ZEW) Taxes and Mobility Workshop, the Vienna Doctoral Consortium, the Barcelona Meeting on Transport Economics and Infrastructure, the University of Texas at Austin, the EU Women Tax Group, the University of Iowa. We gratefully appreciate the opportunity to work with the European Road Freight Transport microdata provided by Eurostat. The responsibility for all conclusions drawn from these data lies entirely with the authors We thank Jann Winter, Moritz Pilarski and Stella Hill for excellent research assistance. We gratefully acknowledge financial support from the Leibniz ScienceCampus MannheimTaxation and the Graduate School of Economics and Social Sciences of the University of Mannheim.

3.1 Introduction

The need to address the global climate crisis has sparked considerable interest in exploring effective policy instruments that can mitigate its consequences (IPCC 2021). As the transportation sector accounts for a significant portion of greenhouse gas emissions worldwide (European Environment Agency 2022; International Energy Agency 2022), finding strategies to reduce emissions from commercial trucks has become a critical objective for policymakers and researchers alike (see e.g., World Bank 2014; OECD 2021a; European Commission 2023). Environmental taxes on diesel fuel have emerged as a potential instrument to incentivize more sustainable transportation practices and discourage high-emission activities (International Energy Agency 2022).[39] However, understanding how commercial traffic responds to environmental taxes, particularly when they vary across national borders, is a complex and so far understudied area. In this study, we aim to fill this gap.

The transportation sector is a significant contributor to global greenhouse gas emissions, accounting for 20% of the total emissions. Within this sector, road transport is responsible for the majority of this burden, accounting for three-quarters of emissions, which equals to 15% of the overall global emissions (International Energy Agency 2022). Trucks, in particular, heavily rely on diesel fuel, notable for its substantially higher Carbon Dioxide (CO_2) emissions compared to the majority of passenger cars fueled by gasoline. Fuel costs, constituting roughly 20% of a truck's total operating expenses, play a crucial role in companies' strategic route planning. Taxes, which account for nearly half of the gross fuel price, wield a decisive influence over where transport companies choose to refuel. This influence becomes particularly pronounced when tax rates vary across countries, as observed in the European Union (EU) (Transport and Environment 2021), or across states, such as in the United States (US), incentivizing commercial trucks to strategically plan their re-

[39] An alternative instrument is the implementation of a cap-and-trade system. So far, no cap-and-trade system has been implemented in the road transportation sector.

fueling locations. Environmental taxes on fuel are designed to curb its demand, thereby aiming to reduce emissions and encourage a transition to cleaner alternatives. However, the existence of tax differentials across borders can lead to unintended consequences, such as trucks taking detours to access cheaper fuel in neighboring countries.

In this paper, we investigate how commercial trucks respond to differences in environmental taxes on diesel fuel across European borders. Our analysis proceeds in three steps to comprehensively assess the behavioral responses of truck operators to changes in tax rates and the resulting environmental consequences of such tax-induced responses. First, we use administrative toll data to examine whether cross-border truck traffic responds to changes in environmental taxes across countries. Next, we examine whether differences in tax rates between countries induce trucks to take detours to avoid high environmental taxes on fuel. Finally, we examine the environmental consequences of these actions by incorporating air pollution data and emissions.

We start our analysis by examining cross-border truck flows at German borders. We obtain comprehensive administrative data containing monthly truck counts from mid-2018 to the end of 2022 on detailed road segments in Germany, which allows us to measure the responsiveness of cross-border truck flows between Germany and its neighboring countries to differences in environmental taxes on fuel. To overcome the challenge of isolating the impact of environmental taxes on the gross price of diesel, we use an Instrumental Variable (IV) approach. We leverage the environmental tax differential between Germany's neighboring countries and Germany as an instrument to predict tax-induced changes in the gross price differential of diesel. These predicted price changes resulting from changes in environmental taxes on diesel allow us to estimate the tax-induced effects on cross-border transport.

We find that commercial traffic responds to changes in environmental taxes. An increase in the tax rate of a neighboring country leads to a decrease in truck traffic through that country. Specifically, we observe that a tax-induced increase in the average gross

price of diesel fuel by one euro cent per liter of fuel results in 466 fewer trucks per road segment at the border per month, suggesting a shift in truck traffic patterns away from more expensive cross-border routes. This finding implies that trucking companies prioritize fuel cost-optimized routes and underscores the influence of environmental taxes on their decision-making processes. Moreover, we show that this effect is more pronounced on highways that are heavily used by commercial traffic. The effect is consistently observed within the first six road-sections close to the border, but gradually diminishes as one moves away from the border region and more inland traffic intersects, underscoring the robustness of our results.

Subsequently, we focus on individual truck routes across Europe. We leverage an administrative survey data set spanning from 2011 to 2020 to investigate whether trucks strategically choose detours to to avoid countries with high environmental taxes while taking advantage of low environmental taxes in other countries. The European Road Freight Transport (ERFT) database allows us to track individual truck journeys from their starting point to their end point, while recording the countries crossed in between. To determine whether trucks intentionally detour to avoid high fuel taxes, we compare the actual route observed in our data with the fastest and shortest route obtained from Google Maps (GMAP) via web scraping. Our survey data set then includes the actual distance traveled per trip, the optimal distance, and the deviation between the two.

Leveraging environmental taxes on diesel fuel, averaged for each journey, we incorporate a linear probability model into our IV approach to examine whether an increase in the average environmental tax rate on diesel fuel for a journey affects the probability that a truck deviates from the fastest and shortest route available. Our results indicate that an increase in the average environmental tax on diesel significantly increases the probability that a truck deviates from the optimal route for the trip, suggesting that commercial trucking companies are indeed driving detours to save on fuel costs. Furthermore, we find that this behavior leads to an increase in total mileage, which includes both excess kilometers

and excess ton-kilometers, taking into account the size and environmental impact of the truck involved in the trip.

To further validate these findings, we take a closer look at the countries where detours are most common. Descriptively, we find that Luxembourg, along with Romania and Slovakia, are the main destinations for truck detours. Notably, these countries consistently have the lowest environmental tax rates on diesel on average in our data set. To provide empirical support for this observation, we focus on a subset of cross-border trips that involve detours. We regress the number of divergent truck journeys per country and quarter on the average environmental tax rate for that country and quarter. We include country and year-quarter fixed effects to account for the size of a country and general time trends. Our results show a negative relationship, indicating that the lower the environmental tax rate on diesel, the more trucks pass through a country. This supports the notion that environmental taxes on diesel have a significant impact on commercial trucking by influencing route planning decisions to minimize diesel costs.

Finally, we assess the environmental consequences of tax-induced responses in the context of cross-border truck traffic. Expanding on our initial study of truck flows at German borders, we incorporate sensor stations that measure local air pollution to investigate whether increased cross-border truck flows resulting from environmental tax differentials contribute to local increases in air pollution. The sensor data we collect provide information on Short-lived Climate Pollutants (SLCPs), with a particular focus on Particulate Matter (PM) emissions. PM is an important pollutant emitted during the combustion of fossil fuels that has adverse effects on both the environment and human health. Matching a cross-border road-section with the closest sensor station, we find that tax-induced increases in cross-border truck flows lead to local emission peaks at these border road-sections.

In addition, we use the results of our second analysis to quantify the environmental externalities resulting from the deviation of trucks to minimize fuel taxes. To do so, we es-

tablish a correlation between the excess ton-kilometers driven by trucks and the resulting emissions. Building on our earlier results, we employ a linear prediction model to estimate the cumulative excess ton-kilometers resulting from tax-induced deviations. We then calculate the associated CO_2 emissions by multiplying the total surplus by its emission factor. We find an increase in emissions of up to 1 million tons of CO_2 due to trucks taking detours to avoid high environmental taxes on diesel fuel.

Our study provides evidence on the effects of environmental taxes on commercial transport and the resulting environmental impacts. Such evidence is scarce but policy-relevant as policymakers seek to promote sustainable practices in the transportation sector.[40] We uncover unintended consequences arising from unharmonized tax rates within the EU, particularly in a sector that is crucial to the EU's climate change (European Commission 2020). While our initial findings suggest that higher environmental taxes on diesel fuel lead to a reduction in commercial transport, in line with the objective of environmental taxes to reduce emissions from transport, we find in a subsequent analysis that trucks resort to deviations to avoid these higher taxes. As a result, they refuel in countries with lower tax rates, leading to increased mileage and fuel burned and thus higher overall emissions.

We contribute to the literature studying behavioral responses to environmental taxes in the transportation sector. The existing literature is relatively limited, with a notable absence of studies focusing on commercial transportation. Most research on environmental taxes in the transportation sector has focused on non-commercial drivers, examining aspects such as fuel consumption and consumer responses to tax and price changes (Rietveld et al. 2001; Banfi et al. 2005; Davis and Kilian 2011; Rivers and Schaufele 2015; Jansen and Jonker 2018; Bonnet et al. 2024). Notably, studies like Marion and Muehlegger 2011 and Li et al. 2014 utilizing fuel consumption data, find that consumers tend

[40] Alternative fuel options for the road transportation sector will not be available in the foreseeable future, primarily because freight transportation typically involves heavy loads, making electricity, for example, impractical. Additionally, shifting transportation to railways is not feasible due to the lack of sufficient freight rail infrastructure in most European countries.

to react more strongly to tax changes than to price changes. Gimenez-Nadal and Molina 2019 use variations in gasoline tax rates across US states and find that higher taxes lead to a greater number of people choosing public transportation instead of private cars for their commutes. Andersson 2019 examines the effect of a carbon tax on emissions in the Swedish transportation sector and finds a reduction in pollution. Our study fills a critical gap in research by addressing how commercial truck traffic reacts to cross-border changes in environmental taxes across borders.[41] We examine the mechanism that primarily affects fuel demand, namely driving behavior, using comprehensive data on traffic across Europe. This approach further distinguishes our study from previous literature that finds a net reduction in emissions in response to environmental taxes on fuel (e.g., Andersson 2019; Runst and Hoehle 2022), thereby overlooking shifts in driving behavior and emissions when only examining demand and emissions at the implementation location of the tax.[42]

Prior literature examining firms' responses to environmental taxes in different settings finds that firms generally reduce their own emissions in response to taxes and cap-and-trade systems (Martin et al. 2014; Dussaux 2020; Erbertseder et al. 2023; Martinsson et al. 2024). However, consistent with our study, firms reallocate resources to avoid paying or bearing environmental taxes and regulations without necessarily reducing emissions (Itzhak et al. 2021; Dechezleprêtre et al. 2022; Ecker and Keeve 2023; Kaenzig et al. 2023; Muûls et al. 2024). We add to this literature by documenting evidence of this relationship for commercial trucking companies. Using extensive but unexplored traffic data combined with emissions data, we quantify the impact on truck traffic and the excess kilometers driven by trucking companies to avoid high environmental taxes on fuel. We further estimate the incremental emissions caused by this shift in driving behavior.

[41] A related study by Luechinger and Roth 2016 differs from our study in that it examines how trucking in Switzerland responds to the introduction of a mileage tax that differs from environmental taxes on diesel fuel.

[42] Santos 2017 examines fuel taxes in Europe and calculates corrective fuel taxes aimed at internalizing externalities from road transport.

Furthermore, our study adds to the broader literature on the effects of taxes on mobility, drawing insights from works by Kleven et al. 2013, Schmidheiny and Slotwinski 2018, and Akcigit et al. 2016. This body of research has demonstrated that tax differentials and the mobility of the tax base can trigger various behavioral responses by individuals, including tax avoidance strategies. Our study builds upon this literature by showing that commercial truck drivers adjust their routes in response to cross-border tax differences, particularly within the context of environmental taxes.

3.2 Setting

3.2.1 Road Freight Transport in the EU

Road freight transportation plays a significant role within the transportation sector, accounting for a substantial share of 76.3% of total freight transportation in the EU as of 2019 (European Commission 2021). Emissions stemming from road transport make a considerable contribution, comprising approximately 15% of global CO_2 emissions (European Environment Agency 2022; International Energy Agency 2022). Trucks, in particular, heavily rely on diesel fuel, which is associated with higher levels of emittants compared to other fuels. Diesel engines emit a complex mixture of air pollutants, including CO_2, diesel PM and Nitrogen Oxide (NO_x), which contribute to both adverse health effects and climate change (OECD 2019).

A single truck tank filling can hold between 1,000 to 1,500 liters of diesel, allowing trucks to cover distances of up to 1,600 km.[43] On average, a truck filling in Europe costs between Euro (EUR) 1,400 and EUR 2,100. Thus, fuel costs represent a substantial portion, approximately 20%, of a truck's overall operating expenses, making it a critical factor in optimizing cost-efficient truck route planning (Trego and Murray 2010; American Transportation Research Institute 2018). Importantly, environmental taxes on fuel make a

[43] A distance of approximately 1,600 km can cover journeys such as traveling from Warsaw to Paris, from Amsterdam to Barcelona or from Hamburg to Rome.

significant contribution to the total fuel price, accounting for up to half of it (see Appendix Figure B.1 and Figure B.2). As these taxes are a function of the gross fuel price, they exert a substantial influence on the refueling decisions made by transportation companies (Izadi et al. 2020).

Refueling, given its substantial cost, is typically not left to the discretion of individual truck drivers in commercial trucking businesses. Instead, it is planned and managed by the company itself. Truck transport companies aim to strategically minimize fuel expenses by employing commercial transportation management systems. These systems plan truck journeys on the go, considering factors like saving fuel costs along with truck driver working hours, the average speed of the truck as well as road congestions. Anecdotal evidence further supports this notion, with several business models advertising diesel price tools designed to plan fuel-optimal routes across Europe and highlight the lowest diesel prices available (see, e.g. DKV Mobility[44] and Transport Topics).

Conversely, while road tolls are present in most EU countries, they are not among the cost factors that transport companies can easily avoid.[45] These tolls apply to both highways and federal roads, making it difficult for trucking companies to consistently navigate around them. To operate profitably, transport companies must select routes on well-developed roads. Choosing alternative routes on less-developed roads typically incurs a time surcharge that is more expensive than the toll costs. Additionally, toll charges remained stable throughout the EU during our sample period.

3.2.2 Environmental Taxes on Fuel

Environmental taxes on fuel are at the core of taxation within the transportation sector (OECD 2019). These taxes typically serve a dual purpose. Firstly, they are primarily designed to address the environmental externalities associated with fuel consumption.

[44] Appendix Figure B.3.

[45] Two different toll systems exist in the EU. Tolls are levied according to the distance travelled and the type of vehicle, while vignettes are scaled according to the duration of the use of the infrastructure.

Secondly, they generate revenue for the government. Taxes on fuel typically encompass both value-added tax (VAT) and excise taxes reflecting the environmental impact associated with fuel usage. At the pump excise taxes are added to the net fuel price, with the resulting sum being multiplied by the applicable VAT rate.[46] It is important to note here that VAT is not relevant in the context of freight transport, as companies can reclaim the VAT paid. As a result, we focus on excise taxes on fuel. In practice, different tax rates apply to the most commonly used fuels, diesel and gasoline. While diesel is predominantly used by trucks, gasoline is more common with private cars.

The landscape of environmental taxes on fuel (both diesel and gasoline) differs across nations. For instance, certain countries label their excise tax on fuel as mineral oil tax (e.g., Switzerland or Ireland), others include them in their energy tax (e.g., Germany). Either are applied per liter of fuel. Additionally, some countries impose a carbon tax based on the amount of CO_2 emissions produced during fuel combustion.[47] Following the classification by the European Commision, we combine all these taxes as environmental taxes on fuel (Eurostat 2024). The revenue generated from these environmental fuel taxes typically flows into the state's general budget, contributing to the financial resources of the government.[48] In 2021, tax revenue from environmental taxes on fuel within the EU amounted to EUR 60 billion, representing 18% of overall tax revenue from environmental taxes (European Commission 2023).[49]

As we narrow our focus to commercial traffic, our attention centers on environmental taxes concerning diesel fuel. Notably, environmental tax rates on diesel vary significantly between EU member states, with certain countries imposing lower taxes compared to others. These differences present opportunities for commercial truck companies to strategi-

[46] Gross price fuel = (Net price fuel + Environmental tax fuel) x (1 + VAT).

[47] An illustration involves the introduction of a carbon tax on fuels in Luxembourg and Germany in 2021, followed by Austria in September 2022. Notably, Switzerland excludes fuel usage from its carbon tax, while Slovenia places the liability for the carbon tax on fuel producers rather than fuel consumers. Meanwhile, France and Finland embed an explicit carbon component within their fuel excise tax.

[48] In contrast, the money generated by road tolls is used to maintain the road infrastructure.

[49] Other environmental taxes besides taxes on fuel are energy taxes, pollution taxes and resource taxes.

cally plan their journeys, opting to refuel in countries with lower diesel tax rates, thereby minimizing their operational costs. In some EU countries, including Belgium, France, Hungary, Ireland, Italy, Romania, Slovenia, and Spain, commercial trucking businesses can partially recover the diesel taxes they pay through state-issued tax rebates.[50]

We use these variations in environmental taxes and the availability of tax rebates to investigate the responsiveness of commercial trucks to differences in these tax regimes. Our analysis begins by examining cross-border traffic in Germany. Germany serves as an ideal setting due to its prominent role in intra-European land transport. Its geographical location at the heart of Europe coupled with a well-developed transportation infrastructure including highways and federal roads results in a constant flow of goods between Germany and its neighbors. Moreover, Germany's environmental tax on diesel remained stable throughout our sample period, making it an ideal choice as our baseline country.[51] Figure 7 illustrates the monthly evolution of environmental taxes on diesel fuel in euro cent per liter across Germany and its neighboring countries from 2011 to 2022. Notably, Germany imposes rather high taxes on diesel, while Poland and Luxembourg consistently maintain lower rates. Additionally, Belgium and France have comparatively higher taxes on diesel fuel for cars but offer tax rebates, as depicted by the dashed line, for commercial trucks.[52]

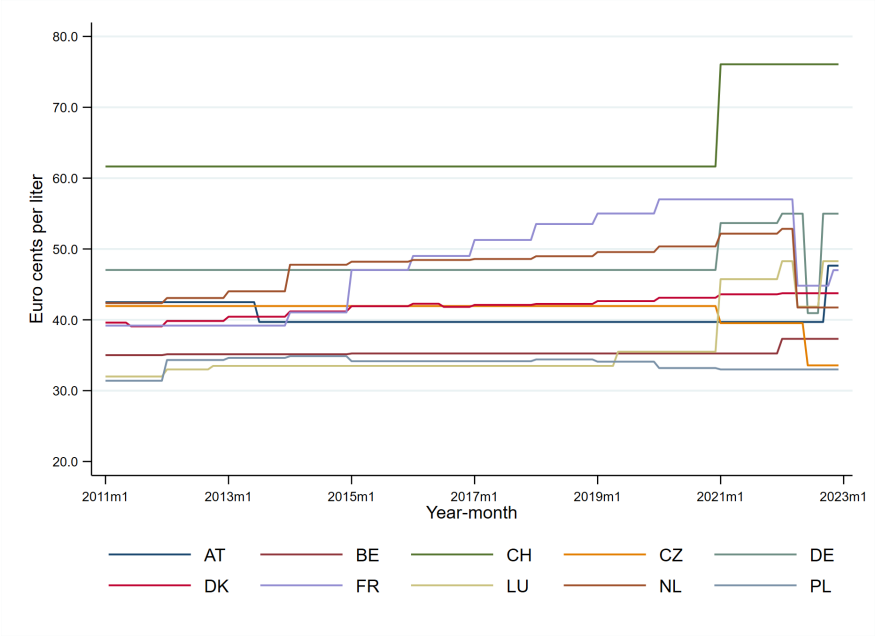
Our first analysis leverages the variations in environmental taxes on diesel fuel relative to the German diesel tax rate. We subtract the environmental tax of Germany from that of each of its neighboring countries per month (tax in neighboring country - tax in Germany). Figure 7 presents the development of diesel taxes compared to Germany, encompassing Germany's neighboring countries and the time period mid-2018 to 2022 for

[50] In 2018, the total amount of truck diesel tax rebates paid by these EU countries amounted to approximately EUR 4 billion. Currently, trucks in the EU pay an average diesel tax of EUR 0.43 per liter, which is EUR 0.07 below the rate imposed on cars (Transport and Environment 2021).

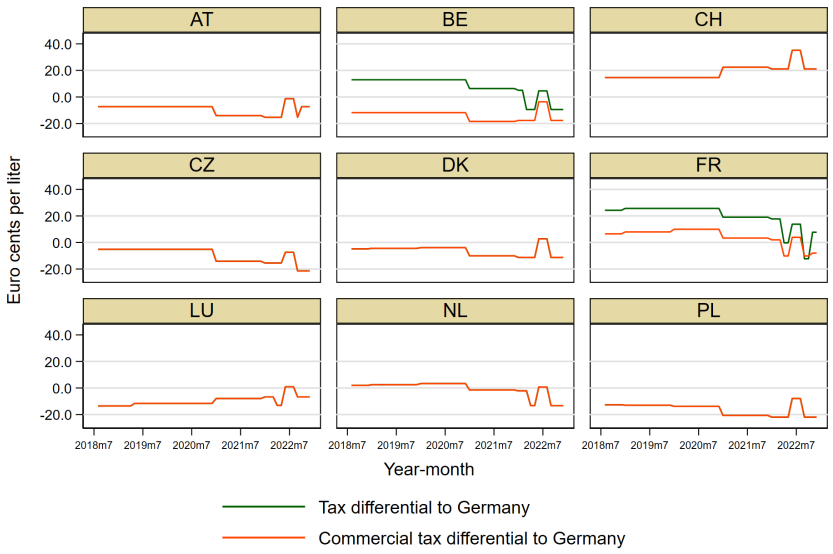
[51] Germany adjusted its environmental taxes on fuel only in 2022, a response prompted by the events surrounding the Ukraine war.

[52] The drops observed in 2022 were implemented as a response to the Ukraine war, during which several nations reduced their taxes on fuel as a countermeasure against rising oil prices.

Figure 7: Development of environmental taxes on diesel



(a) In Germany and neighboring countries



(b) Neighboring countries relative to Germany

Note: The figures plot the development of environmental taxes on diesel in Germany and its neighboring countries. Panel (a) plots the countries' monthly tax rates in euro cent per liter over the period 2011 to 2022. Panel (b) plots the countries' tax differentials relative to Germany (tax in neighboring country - tax in Germany) in euro cent per liter of diesel fuel over the period mid-2018 to 2022. For Belgium and France that apply different tax rates on commercial vs. private use the green line is the tax differential excluding commercial tax rebates and the orange line is the tax differential including commercial tax rebates.

Table 6: Summary statistics of environmental taxes on diesel by country

	Obs	Mean	SD	Min	Max	Median
Austria (AT)	144	40.45	1.55	39.70	47.65	39.70
Belgium (BE)	144	35.38	0.59	35.02	37.32	35.25
Bulgaria (BG)	144	32.75	0.49	31.44	33.01	33.01
Switzerland (CH)	144	64.05	5.39	61.65	76.07	61.65
Czech Republic (CZ)	144	41.26	1.91	33.57	41.95	41.95
Germany (DE)	144	47.96	2.75	40.95	54.99	47.04
Denmark (DK)	144	41.85	1.37	39.08	43.75	42.11
Estonia (EE)	144	41.54	4.82	37.20	49.30	39.29
Spain (ES)	144	31.29	1.28	30.39	35.19	30.39
Finland (FI)	144	47.18	3.90	36.28	51.25	46.28
France (FR)	144	48.06	6.87	39.19	57.01	49.02
Greece (GR)	144	38.20	3.86	33.00	41.20	41.00
Croatia (HR)	114	39.47	2.18	34.03	40.67	40.67
Hungary (HU)	144	34.36	3.50	22.05	38.88	35.54
Italy (IT)	144	40.13	0.72	37.51	40.32	40.32
Lithuania (LT)	96	35.01	1.83	33.02	37.20	34.70
Luxemburg (LU)	144	35.65	4.67	32.00	48.28	33.50
Latvia (LV)	108	37.04	3.39	33.29	41.40	37.20
Netherlands (NL)	144	47.34	3.29	41.75	52.85	48.45
Poland (PL)	144	33.78	0.93	31.40	34.87	34.16
Portugal (PT)	144	42.22	7.21	27.84	51.33	45.59
Romania (RO)	144	34.67	2.63	30.44	38.45	33.79
Sweden (SE)	144	47.27	7.87	30.29	58.66	46.22
Slovenia (SI)	144	33.00	0.00	33.00	33.00	33.00
Slovakia (SK)	144	37.89	0.91	36.80	38.64	38.64
Total	3486	40.43	8.00	22.05	76.07	39.29

Note: The table reports summary statistics of (commercial) diesel tax rates (in euro cent per liter) by country during the period 2011 to 2022. The statistics include the number of observations, mean, standard deviation, minimum, maximum and median diesel tax rates by country.

which we possess traffic data from German roads. These relative differences in environmental tax rates on diesel serve as the main explanatory variable in the initial part of our empirical analysis, aiming to investigate the response of commercial traffic to changes in

environmental taxes.

In our subsequent analysis, we delve into whether truck drivers take detours to circumvent high environmental taxes. For this purpose, we once again employ the diesel tax rates, now encompassing all continental European countries, and extending the time frame from 2011 to 2022. Table 6 shows summary statistics of (commercial) diesel tax rates by all European countries included in our sample. This expanded data set on environmental tax rates on diesel allows us to examine truck routes throughout Europe and explore potential deviations undertaken by drivers to avoid regions with high environmental taxes.

3.3 Data and Descriptives

3.3.1 Traffic: Toll Data

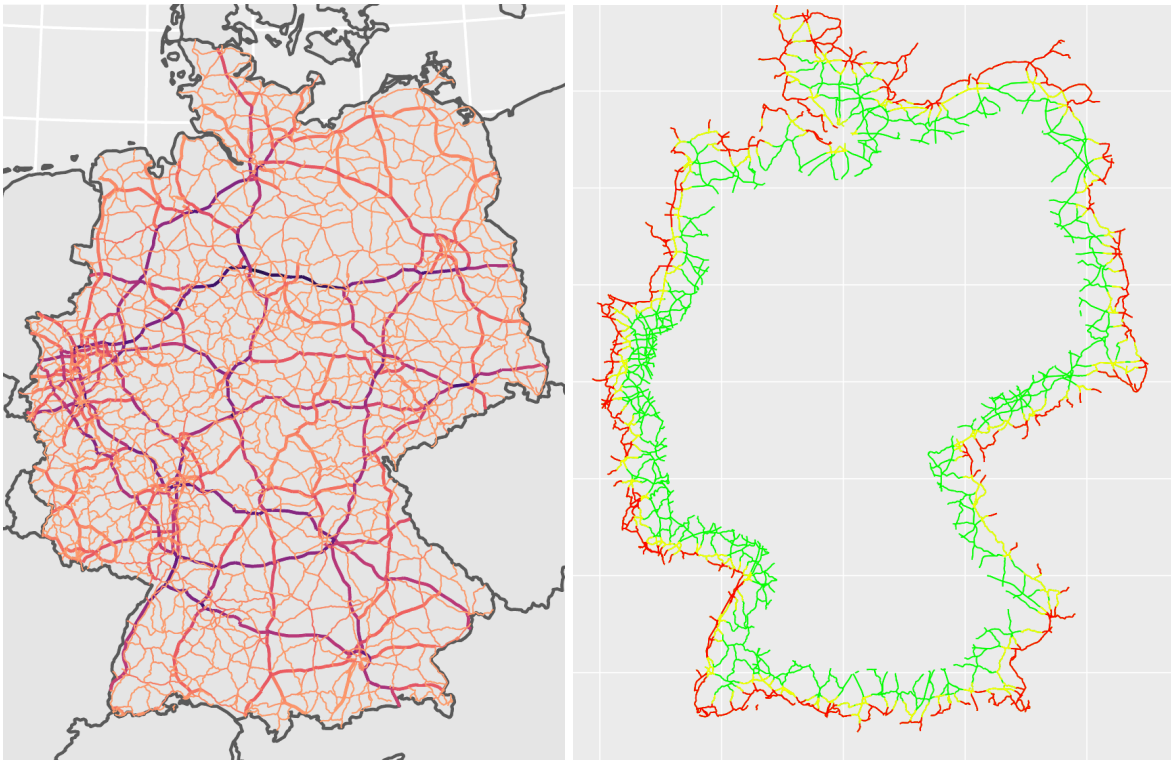
To assess the impact of environmental tax rate differences on cross-border truck traffic flows between Germany and its neighboring countries, we utilize administrative toll data for Germany. The data provide us with monthly information on all trucks passing through Germany at a highly detailed spatial level, covering both highways and federal roads.[53] By observing the monthly count of trucks traveling on each road-section per month, we can assess how sensitive truck traffic flow is to changes in environmental taxes on fuel. Specifically, we observe the number of trucks traversing a given road-section for each direction independently resulting in two monthly observations per road-section. The data available spans from mid-2018 to the end of 2022.[54] Given that the data include information on the number of axes (i.e. truck size), we restrict our analyses to trucks with more than four axes, representing about 80% of the truck count in our sample (see Appendix Figure B.5). Road freight transportation heavily relies on trucks equipped with four or more axes, which are also the predominant vehicles commonly observed on high-

[53] In our study, we use the term highway to refer to the German "Autobahnen" while federal roads are German "Bundesstraßen".

[54] Data availability is limited to mid 2018 until the end of 2022 as the German toll system for trucks was implemented incrementally. The implementation started in April 2017 on highways, and was extended to federal roads by July 2018. As a result, data for the period before July 2018 is not available for analysis.

ways.[55] Our data further includes the road type driven (i.e. highway vs. federal roads). This enables us to analyze heterogeneity in tax sensitivity between road types, with highways possibly being the preferred choice for truck drivers in terms of delivery speed and infrastructure.

Figure 8: Geographic plots of road-sections covered by toll data



(a) Highways and federal roads in Germany (b) Highways and federal roads at borders

Note: The figures plot the toll data geographically. Panel (a) shows all highway and federal road road-sections present in the data set. The thick lines represent highways, and the thin lines represent federal highways. The darker the color, the more traffic a road-section has on average. Panel (b) shows only highways and federal roads around borders. Red lines are all road-sections within 5 kilometers of the border, yellow are all between 5 and 10 kilometers, and green are within 10 to 15 kilometers of the border.

Figure 8 displays all highways and federal roads in Germany included in our data set, while Table 7 provides descriptive statistics such as the total number of observations, which amounts to 690,792, and the average length of a road-section, which stands at 731 meters. Notably, 96% of the road-sections in our data set are composed of federal roads,

[55] Trucks featuring three axes, such as car cranes, and those with two axes, like tractors or combined harvesters, serve different specialized purposes.

while the remaining 4% pertain to highways. Road-sections on highways are, however, on average ten times longer than those on federal roads. Moreover, monthly truck count statistics underline the significance of German "Autobahnen" (highways), with a monthly average of 79,273 trucks passing through each highway section, in contrast to an average of 4,856 trucks on federal roads.

Given our focus on cross-border flows, Figure 8 (b) identifies the border corridors we use to examine the impact of environmental taxes on cross-border truck traffic. We analyze road-sections that directly cross the border, as well as those within 5 km, 10 km, and 15 km of the border. A total of 135 highways and federal roads intersect the German border, connecting to neighboring countries. These roads serve as crucial transportation links, facilitating cross-border movement and trade between Germany and its neighboring countries. Appendix Table B.1 presents the average monthly truck traffic flow by distance to the border, while also differentiating traffic intensity by country of destination.

3.3.2 Traffic: Survey Data

In our second analysis we utilize a large sample of administrative route survey data to track truck drivers on their journeys across Europe. The ERFT database provided by EUROSTAT enables us to trace individual truck drivers from their starting point to their final destination while capturing the countries they cross in between.[56] This second data set allows us to observe the actual routes commercial trucks drive through Europe. For anonymity purposes the starting and end point of each journey is reported in NUTS-2 (Nomenclature of Territorial Units for Statistics) format, which corresponds to district-sized geographical regions.[57] Moreover, the ERFT database contains information on the

[56] According to a regulation by the European Commission, each member state is required to collect data on the carriage of goods by road. This data is obtained by surveying owners of road vehicles registered within their respective member states. It includes the journeys made by the vehicles, including the start and end points, as well as the countries crossed. Countries are required to report quarterly surveyed micro-data on selected vehicles. See Article 1 of Regulation (EC) 70/2012, as of 18 January 2012 (European Commission 2012).

[57] The NUTS format classifies Europe's territory in a hierarchical order, where NUTS-1 corresponds to larger regions such as federal states, while areas covered by NUTS-2 and NUTS-3 are more granular.

Table 7: Summary statistics of toll data

	Obs	Mean	SD	P1	P50	P99
Road-section length						
Overall	691,098	731	1,272	100	300	6,800
Highway	25,478	5,050	3,493	300	4,300	16,400
Federal road	665,620	565	687	100	300	3,300
Truck count						
Overall	691,098	7,599	17,841	49	2,320	108,671
Highway	25,478	79,273	43,794	4,881	75,079	171,241
Federal road	665,620	4,855	7,273	48	2,182	38,018
Cross-border truck count						
Overall	13,094	17,728	32,222	2	2,786	145,806
Highway	2,550	69,578	40,841	7,898	68,524	161,426
Federal road	10,544	5,188	8,865	2	1,653	46,952

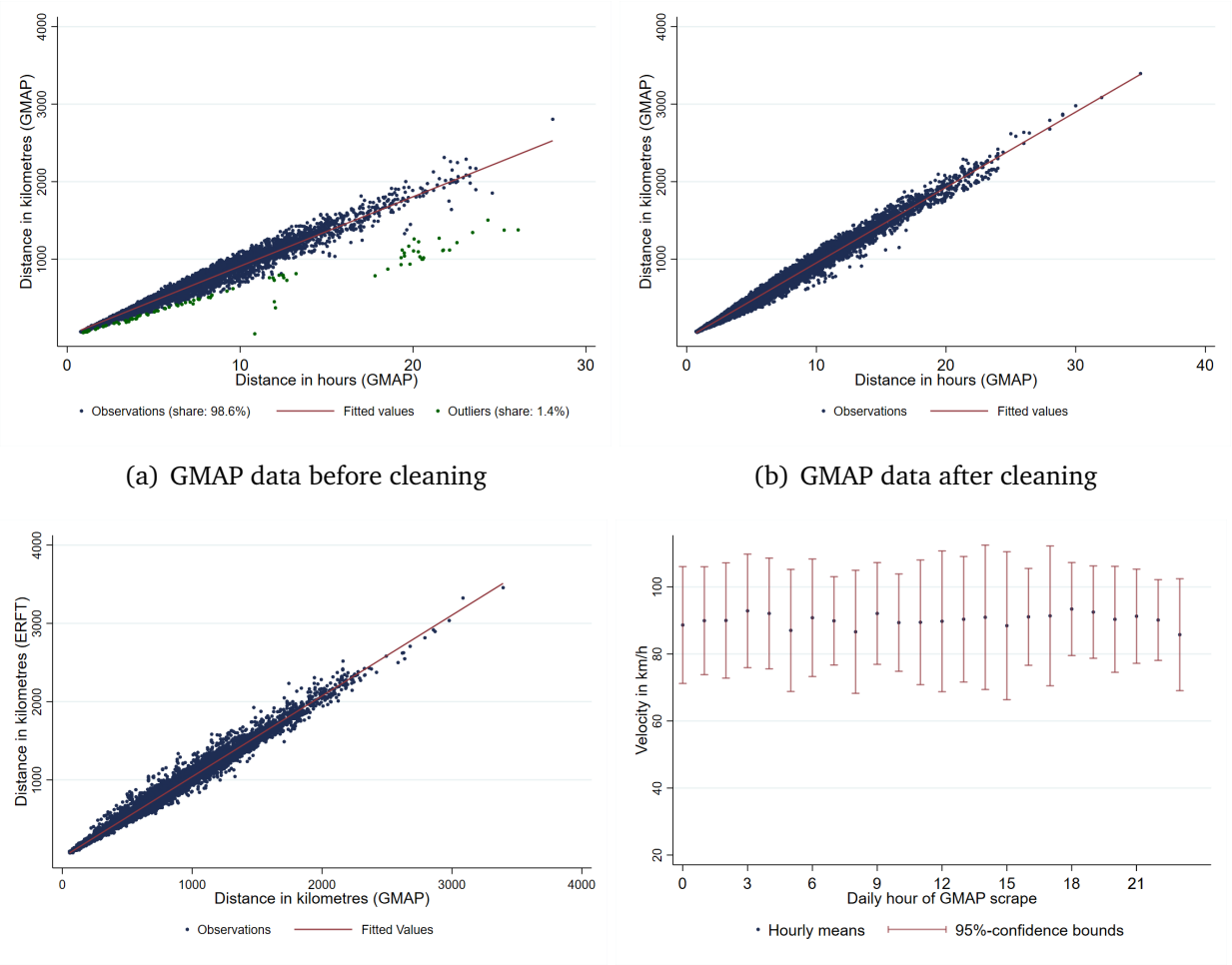
Note: The table reports summary statistics for the toll data. Displayed are monthly averages spanning the time period mid-2018 till the end of 2022. Road-section length is measured in meters. Truck count is measured in the number of trucks passing the road-section each month. Cross-border truck count restricts the sample to road-sections crossing the border. The number of observations is presented in road-section-months.

actual distance traveled per journey (in kilometers), the type of journey (whether the truck is laden or unladen), and the type of transport (whether the journey was for hire or on own account). In total, we observe more than 46 million journeys from 426.115 unique vehicles during the time period from 2011 to 2020.[58] As we are interested in journeys that drive through continental Europe, we filter the data and find 1,182,339 cross-border journeys provided by 307,241 unique respondents.

Using this second data set we investigate whether trucks deliberately take detours to avoid high environmental taxes. For this, we compare the actual route taken by each truck driver with the fastest and shortest route possible. The detour is quantified by measuring the difference between these two distances while also noting the specific countries trucks deviate to. Given that the ERFT data set originally only provides information on the actual

[58] Release date: 10 November 2021, version 1.

Figure 9: Visualization of GMAP and ERFT data



(a) GMAP data before cleaning

(b) GMAP data after cleaning

(c) Comparison distance ERFT/GMAP data

(d) GMAP Daily route velocity

Note: The figures show properties of the actual ERFT data and the scraped GMAP data. Panels (a) and (b) depict the scraped GMAPs data before and after cleaning. Plotted observations are aggregated at the NUTS2-pair level for the start and end points of each route, provided suchs pairs contain at least 10 observations per route (approx. 95 percent of all routes). Panel (c) provides a comparison of distance lengths between ERFT and GMAP, again aggregated at the level of the NUTS2-start-end-pairs. Panel (d) visualizes the influence of daily scrape hours on average route velocity.

route taken for each journey, we employ a web scraping technique to extract the fastest and shortest route from GMAP for every journey. Our web scraper utilizes the journey's start and end points from the data, searching for the most direct route in GMAP. Since the start and end points are classified under the NUTS-2 system, we execute this search using the centroids of each NUTS-2 district. Additionally, we ensure that GMAP identifies

a suitable location near these centroids, ensuring that simulated routes commence from areas suitable for road traffic. Simultaneously, we collect information on the countries traversed during each journey. This allows us to determine the potential detours taken for the specific journeys.

Table 8: Summary statistics of survey data

	Obs	Mean	SD	P1	P50	P99
Actual distance (ERFT)						
Overall	1,185,176	624.05	468.91	87	491	2,106
Deviating	71,793	511.22	488.22	69	286	2,108
Optimal distance (GMAP)						
Overall	1,185,176	448.24	368.87	72	323	1,770
Deviating	71,793	473.10	457.95	66	272	1,946
Deviation distance						
Overall	1,185,176	24.39	86.27	-143	10	341
Deviating	71,793	38.12	102.63	-167	22	432
Hired journey						
Overall	1,185,176	0.88	0.32	0	1	1
Deviating	71,793	0.85	0.36	0	1	1
Single-trip						
Overall	1,185,176	0.71	0.45	0	1	1
Deviating	71,793	0.64	0.48	0	1	1
Laden journey						
Overall	1,185,176	0.81	0.39	0	1	1
Deviating	71,793	0.80	0.40	0	1	1
Laden weight						
Overall	1,185,176	136.76	102.33	0	150	313
Deviating	71,793	124.77	103.79	0	114	352

Note: The table reports summary statistics for the survey data. Displayed are journey averages spanning the time period 2011 till the end of 2020. Overall refers to all cross-border journeys in the data set, with deviating being those that deviate from the optimal route by crossing another or a different country. Distances are measured in kilometers. The actual distance per journey refers to the ERFT survey data. Optimal distance per journey refers to the fastest route according to GMAP. Deviation is the measured deviation between actual and optimal journey in kilometers. Hired, laden and Single-trip journeys are displayed in percentage points. Laden weight is displayed in 100 kg and corresponds to the weight of the laden goods.

To validate the accuracy of our web scraping approach we show the properties of the actual ERFT data, including the actual distances driven, and the scraped GMAP data,

including the fastest and shortest route feasible, in Figure 9. As Panel (a) depicts, we identify 1.4% of journeys as outliers, classifying them as such due to their significantly longer duration relative to the distance driven.[59] We exclude these outliers from our sample, enhancing the reliability of our analysis (see Panel (b)). Figure 9 Panel (c) further illustrates a comparison of the distances of journeys between the ERFT and GMAP data. The alignment of these distances provides additional validation for our scraping approach. To address concerns regarding potential temporal biases in the GMAP data due to the time of scraping, Panel (d) displays the hourly averages of the scraped data. Notably, these averages remain consistent across different times of the day, mitigating any potential time-related influences on our results.

We present summary statistics for our refined survey data set in Table 8, providing insights into both all cross-border journeys and those that involve deviations. On average, each journey covers a distance of 624.05 kilometers, while the optimal journey distance is 448.24 kilometers, resulting in an average deviation distance of 24.39 kilometers. Among the cross-border routes in our sample 71,793 deviate by traversing different or additional countries compared to the optimal route. The majority of journeys involve laden and hired trucks. The countries crossed most frequently are Germany, the Netherlands, and Belgium (see Appendix Table B.8).

3.3.3 Emissions: Sensor Data

In the second part of the paper we aim to estimate the environmental consequences of trucking businesses' tax-induced reactions. To link the tax-induced increases in truck traffic to local increases in environmental emissions, we rely on two data sources: first, we use administrative air pollution data provided by the German Federal Environment Agency (henceforth, we use the abbreviation of its German name 'Umweltbundesamt', i.e. UBA). Second, we employ publicly available, Open Source (OS) air pollution data provided by the

[59] These deviations are typically triggered by extraordinary events such as extensive traffic congestion, road closures, or truck breakdowns.

Sensor Community.[60] Both databases collect sensor data on Short-lived Climate Pollutants, with particulate matter being the key pollutant collected. PM is emitted when fossil fuel is combusted. Road traffic is the main source of PM (World Health Organization 2005). PM is classified in different categories of fineness (i.e. PM2.5, PM10) and is measured in particles (in g/m^3). In our analyses we focus on PM10 as it is the most consistently measured air pollutant in both data sets and combined with our traffic data. SLCPs such as PM have serious consequences for health and climate change. Black carbon, a component of PM, is one of the largest contributors to global warming. It warms the earth's atmosphere by absorbing sunlight, thereby accelerating the melting of snow and ice (Ramanathan and Carmichael 2008). Moreover, PM can cause harm to human health. PM can be carried deep into the lungs where it can cause inflammation and worsen the condition of people with heart and lung diseases (European Environment Agency 2023).

To investigate the impact of increased truck traffic on environmental emissions, we combine our traffic data and emissions data. Leveraging our toll data set, we link sensors to corresponding road-sections based on geographical coordinates. We hereby focus on road-sections located closer to the border (i.e. up to a road-order of 60 inside Germany). The advantage of the administrative UBA data lies in its complete time coverage and measurement reliability, implying no outlier observations in any direction and a fully balanced panel. Conversely, the measurement data provided by OS is significantly larger, allowing us to identify emission responses to taxation at a very granular, local level. However, station coverage changes over time and the share of outliers for PM10 is substantial. We thus restrict our sample as follows: First, to reduce imbalance over time, we require each OS emission station to have at least 40 year-month observations (equal to the sample median). Second, we winsorize the year-month values of PM10 at the level of both lower and upper 5%. This yields a similar tail distribution of PM10 between the OS and the UBA

[60] The Sensor Community project originated from Luftdaten.info, an open knowledge initiative located in Stuttgart, Germany. It began as a regional project in 2015 but has grown significantly in recent years and now covers air pollution sensors across the world. Citizens install sensors that measure air pollutants, and the Sensor Community generates a constantly updating particulate matter map from the transmitted data.

data. In order to balance the advantages and limitations of both emission data sources, for our further analysis we require that PM10 is measured at the closest UBA or OS emission sensor station.

Table 9 shows the summary statistics for the distance of a road-section to a sensor station and the monthly PM10 emissions for highways and federal roads. The median distance between a sensor and the nearest cross-border road-section is equal to 500 meters for the full sample, but increases to 6.4 kilometers when considering only cross-border road-sections. Given that PM can be reliably monitored within a range of up to 1,000 meters, this setup still allows us to evaluate environmental emissions for subsets of emission stations located closely to road-sections at highways and federal roads in Germany.

3.4 Effect of Environmental Taxes on Commercial Traffic

3.4.1 Toll Data

Empirical Strategy. To investigate the effect of environmental taxes on cross-border truck traffic, we examine changes in the environmental tax differential between Germany and its neighboring countries. Environmental taxes on diesel fuel influence a trucking company's refueling choices by being part of the gross price of diesel. Diesel prices are endogenously determined by market dynamics, which involve both supply and demand factors. Consequently, the gross price of diesel is influenced not only by the environmental tax rate but also by variables such as oil prices and decisions made by local fuel station owners. To address endogeneity issues due to simultaneity or imperfect pass-through and to isolate the variation in the diesel gross price attributable to environmental taxes, we employ an instrumental variable strategy. We instrument the gross price differential with the environmental tax differential (see a similar approach, e.g., in Bonnet et al. 2024). This enables us to isolate the tax-induced impact of gross diesel price differences on cross-border truck traffic.

Table 9: Summary statistics of emission data

	Obs	Mean	SD	P1	P50	P99
Distance to emission station						
Full sample						
Overall	401,435	1,554	2	23	547	9,839
Highway	25,209	1,962	1	640	1,452	6,154
Federal road	376,226	1,526	2	23	429	9,839
Cross-border road-sections						
Overall	10,226	8,915	7	251	6,375	28,050
Highway	2,550	7,631	6	668	5,062	26,017
Federal road	7,676	9,341	8	232	6,593	28,481
Monthly emissions (PM10)						
Full sample						
Overall	339,798	12.505	6.6	3.277	11.085	33.127
Highway	21,377	13.221	6.7	3.539	11.928	32.653
Federal road	318,421	12.457	6.6	3.276	11.044	33.138
Cross-border road-sections						
Overall	8,865	12.530	6.8	3.216	11.074	33.180
Highway	2,190	12.873	7.0	3.256	11.116	33.726
Federal road	6,675	12.417	6.7	3.201	11.051	32.653

Note: The table reports summary statistics for the emission data for the full sample and the cross-border road-sections by road type. The upper half of the table displays distances between the road-sections and the emission sensor stations. The lower half of the table depicts the measured values for PM10. Observations span the time period from mid-2018 until September 2022. Distance between road-sections and emission sensor stations is measured in meters. Monthly PM10 is measured in $\mu g/m^3$. The number of observations is presented in road-section-months.

An effective instrument must satisfy two key criteria. First, the instrument must be correlated with the endogenous variable (*relevance condition*). In our context, this means that environmental taxes on diesel must be correlated with the gross price of diesel. Our first stage regression (see Table 10 Panel A) confirms this correlation. Second, the *exclusion restriction* necessitates that, conditional on covariates, the instrument influences the outcome only through the endogenous variable. In our case, environmental taxes on diesel exclusively affect cross-border truck traffic through their influence on the gross price of diesel. Given that environmental taxes are integrated into the gross price and that

truck drivers ultimately consider the gross price when making decisions, this assumption appears reasonable. Moreover, as environmental tax revenues are not used to maintain road infrastructure, we can mitigate concerns that road quality is a correlated omitted variable.

One remaining identification challenge is the potential policy endogeneity and omitted variable bias that arise from using policies as sources of variation. To address this, we provide evidence that our tax variable is plausibly exogenous. Figure B.4 shows that the average monthly net and gross price of diesel in Germany and its neighboring countries follows a similar pattern during our sample period. This suggests that the net price of diesel is primarily influenced by the oil price, while gross price differences between countries are driven by changes in tax rates. We further conduct an additional robustness check excluding the months following the outbreak of the Ukraine war (February 2022). In response to the war, many countries significantly lowered their taxes on diesel, potentially leading to confounding. Our results remain consistent (see Table B.4).

We implement the IV estimator using Two-stage Least Squares (2SLS). In the first stage, we regress the gross price differential of diesel on our instrument, the environmental tax differential. The gross price and tax differentials are computed as the difference between the gross price or environmental tax in the neighboring country and the corresponding values in Germany. Germany is selected as our baseline country as we are examining traffic at German borders. Moreover its location, infrastructure, and relatively stable environmental tax on diesel fuel make it a suitable reference point. In the second stage, we regress our outcome variable of interest, commercial cross-border traffic, on the predicted gross price differential.

To estimate the first stage regression, we set up the following equation:

$$\textit{Gross price differential}_{icm} = \alpha_i + \beta_m + \gamma \times \textit{Env. tax differential}_{icm} + \varepsilon_{icm}. \tag{2}$$

$\textit{Gross price differential}_{icm}$ represents the diesel gross price difference between country c

and Germany on road-section i during a particular year and month m. Our instrument *Env. tax differential*$_{icm}$ captures the variation in environmental taxes on diesel. Using the predicted values of the first stage regression, we estimate the following second stage equation:

$$\textit{Cross-border traffic}_{icm} = \alpha_i + \beta_m + \delta \times \textit{Gross price } \widehat{\textit{differential}}_{icm} + \varepsilon_{icm}. \tag{3}$$

Cross-border traffic$_{icm}$ represents the number of trucks driving on a specific cross-border road-section i during a particular year and month m.[61] Our variable of interest is the instrumented diesel tax rate differential *Gross price* $\widehat{\textit{differential}}_{icm}$ that captures the tax-induced variation in diesel gross price between a neighboring country and Germany. By construction Equation 3 estimates the Local Average Treatment Effect (LATE). The coefficient of interest δ examines how a tax-induced change in the gross price differential of diesel affects commercial traffic flow at the border. Put differently, it estimates the effect on cross-border truck traffic for price changes that are caused by environmental tax changes on diesel. In all our specifications, we include road-section fixed effects (α_i) to account for factors that remain constant over time and are specific to a road-section. We also include year-month fixed effects (β_m) to control for variations in time and cluster standard errors by road-section.

Finally, to examine the direct effect of the environmental tax differential on cross-border traffic, we specify the following reduced form equation:

$$\textit{Cross-border traffic}_{icm} = \alpha_i + \beta_m + \lambda \times \textit{Env. tax differential}_{icm} + \varepsilon_{icm}. \tag{4}$$

Since truck traffic predominantly occurs on highways (see Table 7), we conduct our regression analyses separately for two groups. First, we examine the entire set of cross-border road-sections on highways and federal roads, which includes our full sample. Second, we narrow our focus to cross-border road-sections situated on highways, reflecting the most prominent roads for truck traffic. To broaden our perspective and provide a more

[61] We refrain from using a Poisson specification because the second stage of our 2SLS analysis cannot be non-linear, even though our outcome variable is a count variable.

comprehensive understanding of the effects, we further expand our analysis to include road-sections located at different distances from the border. To mitigate concerns that our results are confounded by the Covid-19 pandemic, we exclude affected months (March and April 2020) in a robustness test.[62]

Results. Table 10 Panel A presents the results of the first stage regression, exploring the relation between the environmental tax differential on diesel and the gross price differential. Column (1) displays the results for the full sample, while Column (2) focuses on highways. In both specifications, we observe a significant positive effect of the tax differential on the gross price differential. This suggests that our instrument, the environmental tax differential, accurately predicts the gross price differential. The high F-statistics support this notion and suggests that our instrument is not weak.

In Table 10 Panel B, we explore the reduced form directly regressing cross-border truck traffic on the environmental tax differential on diesel. Again, Column (1) presents results for the full sample, while Column (2) narrows the focus to highway traffic. The estimated coefficients are statistically significant showing that commercial cross-border traffic reacts negatively to an increase in the tax differential. An increase in the tax differential by one euro cent per liter of fuel (referring to an environmental tax increase in the neighboring country) is associated with an average decrease of 47 trucks per month on a cross-border road-section in the full sample, with this reduction increasing to 327 trucks per month on highways.

Table 10 Panel C presents results for the Ordinary Least Squares (OLS) specification, regressing cross-border traffic on the gross price differential for the full sample and highways. For the full sample, we find a positive coefficient that is statistically insignificant. In contrast, for the highway sample, we observe a significantly negative coefficient. Under

[62] We already include year-month fixed effects in our analysis, which should control for pandemic factors that affected cross-border traffic across neighboring countries equally. In this robustness exercise, we hence test specifically for the possibility that, e.g., border closures that occurred only for a subset of neighboring countries, might affect our results.

our preferred IV specification in Panel D, both 2SLS coefficients are negative and statistically significant. We find that an increase in the environmental tax rate on diesel in a neighboring country leads to a substantial reduction in cross-border traffic toward and away from that country. More precisely, a tax-induced increase in the price differential by one euro cent per liter of fuel (i.e., the neighboring country experiences an environmental tax-induced increase in diesel price by one euro cent relative to Germany) is associated with an average monthly decrease of 53 trucks on all cross-border roads and a significant decrease of 466 trucks on highways. These findings align with our initial expectations, demonstrating that truck traffic responds to changes in environmental tax rates on fuel. An increase in the environmental tax rate on diesel in one country decreases truck traffic flow through that country, while a decrease in the tax rate leads to an increase in traffic flow. This result is stronger on highways, highlighting the importance of highways for trucking companies.

To calculate the overall increase in truck traffic caused by tax differences, we consider the mean environmental tax rate difference of -6.5 euro cent per liter of fuel between two neighboring countries and multiply it by our estimated coefficient. We predict approximately 1,177 additional trucks per month per cross-border road-section and 3,029 additional trucks per month on highway road-sections. For Germany as a whole, this amounts to an annual excess of approximately 1,177 trucks x 12 months x 2 incoming and outgoing road-section x 133 cross-border roads, which results in an increase of around 4 million truck commutes per year. This highlights the substantial impact that even small shifts in environmental tax rates on diesel in neighboring countries can exert on the volume of cross-border truck traffic in Germany.

Next, we broaden our perspective by including road-sections located further away from the border. Figure 10 illustrates the 2SLS estimates on the tax-induced price differential by the distance to the border. Figure 10 (a) estimates Equation 3 separately by road-section. Road-section 0 is the section directly located at the border, while road-section

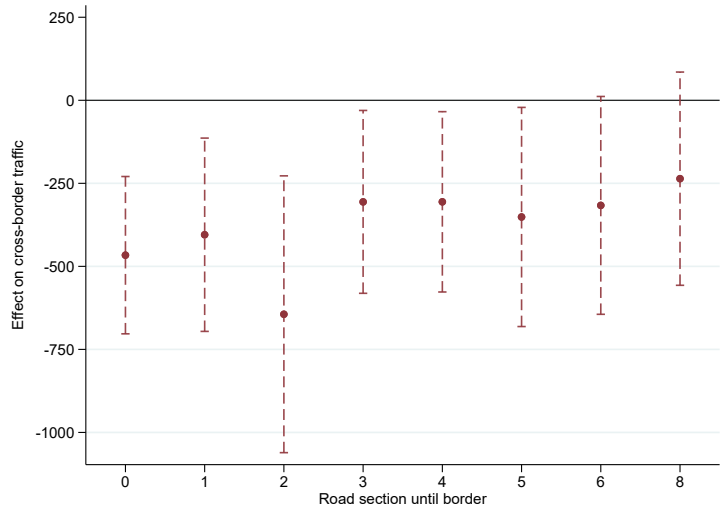
Table 10: Toll data: Effect of environmental taxes on commercial traffic

	Panel A: First stage Diesel gross price differential		**Panel B: Reduced form** Cross-border traffic	
	Full sample	*Highway*	*Full sample*	*Highway*
	(1)	(2)	(1)	(2)
Env. tax differential	0.932***	0.591***	-46.737**	-327.138***
	(0.080)	(0.141)	(18.091)	(87.593)
Year-month FE	X	X	X	X
Road-section FE	X	X	X	X
Observations	13,094	2,550	13,094	2,550
F-statistic	135.66	17.58	-	-
	Panel C: OLS Cross-border traffic		**Panel D: Second stage** Cross-border traffic	
	Full sample	*Highway*	*Full sample*	*Highway*
	(1)	(2)	(1)	(2)
Gross price differential	7.134	-181.164***	-53.930**	-466.317***
	(11.804)	(49.050)	(21.828)	(117.849)
Year-month FE	X	X	X	X
Road-section FE	X	X	X	X
Observations	13,094	2,550	13,094	2,550

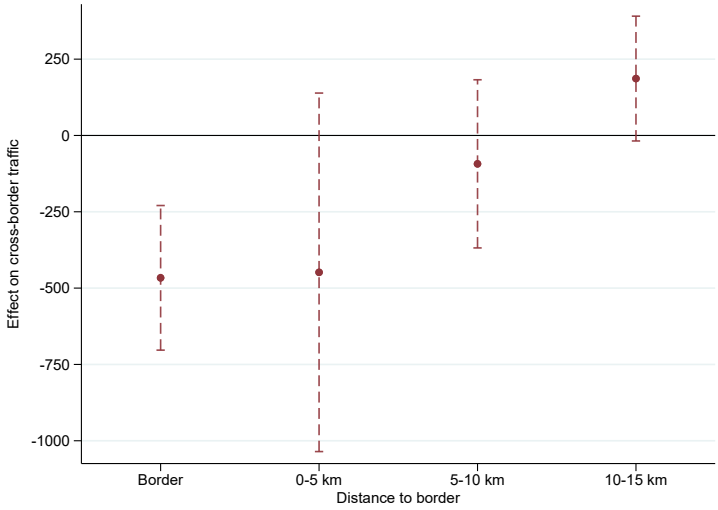
Note: The table presents the main results of our toll data analysis. Panel A reports first stage results regressing the gross price differential on the environmental tax differential. Panel B reports estimates of the reduced form equation. The dependent variable is the monthly truck count on a cross-border road-section. The independent variable is the diesel tax differential between Germany and its neighboring countries. Panel C reports results from a OLS specification and Panel D results from the second stage. The dependent variable is the monthly truck count on a cross-border road-section. The independent variable is the (instrumented) gross price differential. The environmental tax differential and the gross price differential are calculated relative to Germany (tax/price in neighboring country - tax/price in Germany) and measured in euro cent per liter of diesel fuel. In all specifications we include road-section and year-month fixed effects. Columns (1) display the results for the full sample, Columns (2) for the highways Standard errors are clustered on the road-section level and shown in parenthesis. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

1 is one section away from the border. Notably, the coefficients for the first six sections are all negative and statistically significant. However, the magnitude of the coefficient diminishes as we move farther from the border. We attribute this trend to an increasing presence

Figure 10: Toll data: Effect by distance to border



(a) By road-section until border



(b) By distance to border

Note: The figure plots the effect of environmental taxes on commercial highway traffic by road-section and distance to the border. The points plotted are the 2SLS estimates regressing the monthly truck count on the instrumented gross price differential. The gross price differential is calculated relative to Germany (price in neighboring country - price in Germany) and measured in euro cent per liter of diesel fuel. Panel (a) presents the effects by road-section until border. Panel (b) presents the estimates by grouping road-section by their distance to the border. 95% confidence intervals are represented by the dashed red lines and based on standard errors clustered on the road-section level.

of inland traffic on road-sections located at a greater distance to the border. As more domestic traffic intersects with these roads, the impact of tax differentials on cross-border

truck traffic gradually diminishes. In Figure 10 (b) we separately estimate point estimates for road-sections directly at the border, 0 to 5 km, 5 to 10 km, and 10 to 15 km from the border. We confirm that the negative effect gradually decreases the farther we move away from the border and add that it is most pronounced within the first 5 kilometers to the border.

We acknowledge that our results could be contaminated by the impact of concurring events. Our results are unlikely to be biased by the COVID-19 pandemic and associated border closures in March and April 2020. While borders were closed for private travelers, they remained open for transport vehicles. Additionally, some countries relaxed Sunday driving bans for trucks, opened green lanes for faster truck passage at borders, and eased driving and rest requirements. Therefore, we believe that the pandemic did not have a significant impact on transport operations due to these special measures. If trucks were unable to respond to environmental tax differences due to border closures during this period, our results would be biased toward zero. To address this concern, we exclude the first two months of the pandemic (March and April 2020) from our sample. We document in Appendix Table B.3 that our estimated coefficients remain consistent.

Another potential confounding event is the outbreak of the war in Ukraine, which caused fuel prices to spike throughout 2022. At the same time, European governments responded by lowering environmental taxes on fuel across the board. To ensure that our results are not affected by this confounding event in February 2022, we test whether our results hold when excluding all months after January 2022. We expect to find even stronger results for this shortened sample period, as tax changes that occurred due to the Ukraine war are more closely aligned across countries. Indeed, in Appendix Table B.4 we observe even stronger responses when we exclude the period of the Ukraine war.

3.4.2 Survey Data

Empirical Strategy. Next, we investigate whether differences in tax rates across countries induce trucking companies to take detours. To this end, we examine individual truck journeys across continental Europe. We begin by observing the actual distance traveled by a truck during its trip and the countries it crosses, as recorded in the survey data. By leveraging data scraped from GMAP, which includes the fastest and shortest routes along with the associated countries, we can compare these two routes to determine whether a truck deviates from the optimal route and assess if this deviation is driven by fuel tax differences.

First, we assess whether the likelihood of deviation increases with higher environmental taxes on diesel along the journey. We begin by creating a binary variable that indicates whether a truck's journey deviates from the fastest and shortest route available. A journey is classified as deviating if it involves driving a longer distance and traveling through an additional or different country compared to the optimal route. Subsequently, we regress this deviation indicator on the average gross price of diesel along the journey. To account for potential endogeneity, we instrument the gross price with the average environmental tax on diesel to isolate the tax-induced component of the gross diesel price. We then apply a linear probability model[63] to estimate the following 2SLS regression:

$$Deviation_{jnq} = \alpha_n + \beta_q + \delta \times \widehat{Gross\ price\ average}_{jnq} + \varepsilon_{jnq} \tag{5}$$

The dependent variable $Deviation_{jnq}$ is coded as one if the truck journey j deviates from the optimal route in quarter q by passing through an additional or different country, and zero otherwise. We define the $Gross\ price\ average_{jnq}$ as the average gross price of diesel along journey j in quarter q, instrumented by the average environmental diesel tax in

[63] We choose a linear probability model over a logit or probit specification to avoid the incidental parameter problem, which would pose challenges in including fixed effects (Greene 2002). For robustness, we provide the results using a probit specification without fixed effects in Appendix Table B.7, where our results remain consistent.

the first stage. We expect a positive coefficient ($\delta > 0$) for our coefficient of interest δ, supporting the hypothesis that trucks are more likely to deviate when the average tax rate along the route increases. Our model incorporates robust standard errors. Additionally, we introduce year-quarter fixed effects (β_q) to capture time-specific variations and NUTS2-start-end-point pair fixed effects (α_n) to account for factors specific to a journey from one region to another. This addresses concerns that our estimated effect might be influenced by factors other than taxes, such as geographic characteristics or tolls, that could determine the optimal route. With this demanding fixed-effect structure, we account for variation in tax rates over time while holding the specific journey in NUTS2 classification constant. However, recognizing that fixing our results on NUTS2-start-end-point pairs leads to a loss of variation, we also present the results without any Fixed Effects.

Next, we quantify the additional distance traveled by trucks due to variations in environmental taxes. To achieve this, we replace the deviation indicator variable with the journey's deviation distance, measured both in kilometers and ton-kilometers to account for the size and environmental impact of the truck involved in a journey. On the right-hand side of Equation 5 we introduce an interaction term, which incorporates the deviation indicator. This adjustment allows us to quantify how far truck companies deviate when faced with an increase in environmental taxes on diesel. As before, we employ 2SLS to isolate the tax-induced changes in the gross price of diesel.

Subsequently, we seek to gain a more profound understanding of the countries through which trucks deviate from their optimal routes. Initially, we conduct a descriptive analysis to identify countries where detours are most prevalent. In this step, we compile our data into a country-quarter panel, capturing the number of journeys passing through a country and the number of deviations within that country. By computing the share of these two metrics, we gain insights into the countries most frequently deviated through by controlling for the overall number of trucks passing that country. We empirically assess whether these deviations are influenced by tax considerations. We regress the number of deviating

truck journeys per country and quarter on the tax-induced average gross price of diesel for that specific country and quarter using our 2SLS model. Our specification incorporates county and year-quarter FEs accounting for variations in a country's size and across time.

Results. We initiate the analysis of the survey data using a linear probability model to assess whether differences in the environmental taxes on diesel influence the likelihood of truck companies deviating from the fastest and shortest route. The results are detailed in Table 11. Panel A displays the outcomes of the first stage, regressing the average diesel gross price on the average environmental tax on diesel. Panel B presents the results of the reduced form, regressing the deviation indicator on the average environmental tax on diesel. We perform an OLS specification, directly regressing the deviation indicator on the gross price average of diesel, in Panel C. Finally, we present the second stage results, where the deviation indicator is regressed on the tax-induced average gross price of diesel, in Panel D. Columns (1) do not include fixed effects, and Columns (2) incorporate both year-quarter and NUTS2-start-end-point fixed effects.

To validate the relevance condition, Table 11 Panel A displays the results of the first stage regression. The positive and significant coefficients confirm the existence of a correlation and, consequently, thereby affirming the relevance of the instrument. Additionally, the sufficiently high F-statistics support this notion, confirming that our instrument is not weakly identified.

The results from the reduced form in Table 11 Panel B reveal a positive and significant relation between the probability of deviating from the shortest and fastest feasible route and the average environmental tax rate on diesel. This suggests that truck drivers are more inclined to deviate from the optimal journey when faced with higher average tax rates on that route. To be precise, a one euro cent per liter of fuel increase in the average environmental tax corresponds to a 0.43% (0.18%) increase in the probability of a truck deviating from its optimal route, without (with) the inclusion of fixed effects.

Table 11: Survey data: Effect of environmental taxes on commercial traffic

	Panel A: First stage Average diesel gross price		**Panel B: Reduced form** Deviation	
	(1)	(2)	(1)	(2)
Env. tax average	0.43870***	1.40913***	0.00429***	0.00177***
	(0.04465)	(0.05370)	(0.00086)	(0.00048)
Year-quarter FE		X		X
NUTS2-start-end FE		X		X
Observations	1,182,316	1,177,246	1,182,339	1,177,269
F-statistic	123.17	649.84	-	-
	Panel C: OLS Devation		**Panel D: Second stage** Deviation	
	(1)	(2)	(1)	(2)
Gross price average	0.00019	0.00041***	0.00974***	0.00126**
	(0.00025)	(0.00013)	(0.00286)	(0.00039)
Year-quarter FE		X		X
NUTS2-start-end FE		X		X
Observations	1,185,153	1,180,083	1,182,316	1,177,246

Note: The table presents the results of our survey data analysis estimating a linear probability model. Panel A reports first stage results regressing the average diesel gross price on the average environmental tax on diesel. Panel B reports estimates of the reduced form equation. The dependent variable is the deviation indicator and the independent variable is the average environmental tax on diesel on the journey. Panel C reports results from a OLS specification and Panel D results from the second stage. The dependent variable is the deviation indicator and the independent variable is the (instrumented) average diesel gross price on the journey. Columns (2) include year-quarter and NUTS2-start-end FEs. Robust standard errors are included in parenthesis. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Finally, the results of the second stage regression in Table 11 Panel D validate the positive relationship highlighted by the reduced form. We observe that a tax-induced increase in the average gross diesel price by one euro cent per liter of fuel corresponds to a 0.97% (0.13%) increase in the probability of a detour, without (with) the inclusion of fixed effects. Initially, in our toll data analysis, we established that truck drivers respond to changes in environmental taxes by reducing traffic when taxes are raised. This second part

Table 12: Survey data: Deviation distance

	Panel A: Deviation distance Excess kilometers		
	2SLS (1)	*2SLS* (2)	*RF* (3)
Env. tax average			0.169
			(0.196)
Env. tax average x Deviation			0.398***
			(0.031)
Gross price average	-2.148***	0.140	
	(0.532)	(0.137)	
Gross price average x Deviation	0.128***	0.132***	
	(0.024)	(0.010)	
Year-quarter FE		X	X
NUTS2-start-end FE		X	X
Observations	1,182,316	1,177,246	1,177,269
	Panel B: Deviation ton-kilometers Excess ton-kilometers		
	2SLS (1)	*2SLS* (2)	*RF* (3)
Env. tax average			5.078
			(3.052)
Env. tax average x Deviation			6.155***
			(0.540)
Gross price average	-37.845***	3.916*	
	(9.437)	(2.106)	
Gross price average x Deviation	1.594***	2.041***	
	(0.416)	(0.180)	
Year-quarter FE		X	X
NUTS2-start-end FE		X	X
Observations	1,182,316	1,177,246	1,177,269

Note: The table presents results from our survey data set estimating the effect of environmental taxes on deviation distance. Panel A reports the results using the deviation distance in kilometers as outcome variable. Panel B reports estimates using deviation in ton-kilometers as outcome variable. The independent variables are the average environmental tax on diesel on the journey and its interaction with the deviation indicator for the reduced form in Columns (3) and the (instrumented) diesel gross price on the journey and its interaction with the deviation indicator for the 2SLS approach in Columns (1) and (2). In Columns (2) and (3) we include year-quarter and NUTS2-start-end FEs. Robust standard errors are included in parenthesis. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

demonstrates that our first results, a reduction in traffic causing less cross-border traffic, doesn't mean trucks are driving less overall. Instead, the probability of transport companies deviating to refuel in a country with lower environmental taxes on fuel increases when differences in environmental taxes exist.

We delve deeper into the implications of these findings by quantifying the extent of the detour. To achieve this, we replace the deviation indicator variable with a variable containing the deviation distance and re-estimate our 2SLS IV regression, as illustrated in Table 12. Panel A shows the deviation distance in kilometers as the dependent variable, while Panel B measures deviation in ton-kilometers, accounting for the size and freight a truck is transporting. For the deviating journeys, we find that a tax-induced increase in the average gross diesel price by one euro cent per liter increases the deviation distance by 132 meters in the 2SLS specification and by 398 meters in the reduced form. Looking at excess ton-kilometers, we confirm a positive relationship, observing that a one euro cent per liter increase in the tax-induced average gross diesel price leads to an increase of 2.04 ton-kilometers.

Next, our focus shifts to identifying countries where detours are most prevalent. Initially, we descriptively examine the number of trucks deviating through a country, scaled by the overall number of trucks passing through that country. Our observations highlight Luxembourg, along with Romania and Slovakia, as primary destinations for truck detours (see Appendix Table B.8). These countries exhibit the lowest environmental tax rates on diesel on average in our data set.

To empirically confirm this observation, we focus on a subset of cross-border journeys involving deviations. We regress the number of deviating truck journeys per country and quarter on the average tax-induced gross diesel price for that country and quarter. Our model incorporates controls for the total number of journeys passing through each country per quarter, as well as the country's population, to account for variations in size and traffic. The results in Table 13 reveal negative coefficient estimates, indicating that the

Table 13: Survey data: Country effects

	No. deviations	**Log(No. deviations)**	**Share deviations**
	2SLS	*2SLS*	*2SLS*
	(1)	(2)	(3)
Gross price average	-2.179	-0.028	-0.007
	(1.732)	(0.022)	(0.006)
Year-quarter FE	X	X	X
Country FE	X	X	X
Observations	920	757	920

Note: The table presents the effects of environmental taxes on deviations by country. The dependent variable is the number of deviations in Column (1), the log(number of deviations) in Column (2) and the share of deviating journeys per country and year-quarter in Column (4). The independent variable is the (instrumented) average diesel gross price per country and year-quarter. All specifications include year-quarter and country FEs. Standard errors clustered on country level are included in parenthesis. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

volume of trucks deviating through a country increases as the environmental tax in that country decreases. This finding substantiates the notion that environmental taxes on diesel significantly impact commercial trucking operations, influencing route planning decisions to minimize diesel costs.

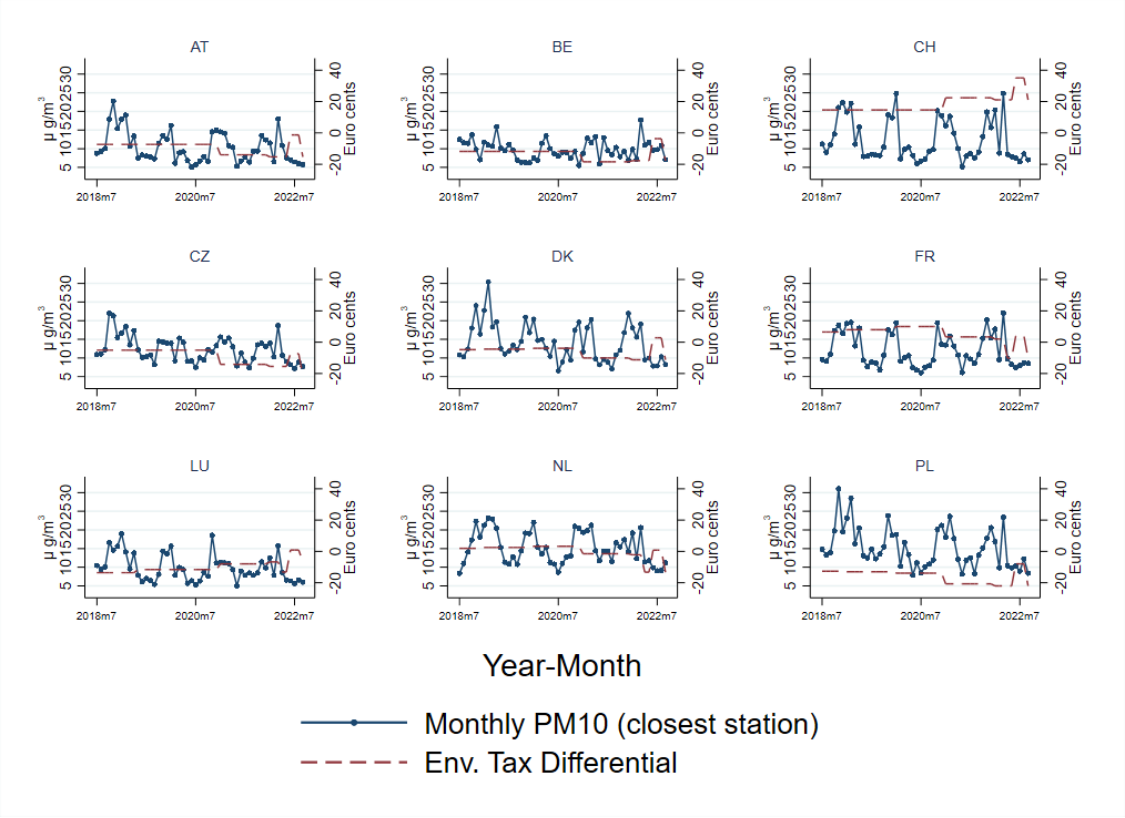
3.5 Environmental Consequences

3.5.1 Combining Toll and Emission Data

To assess the environmental implications of tax-induced shifts in truck traffic, we link our toll data set to emissions data collected by sensor stations near specific road-sections. Focusing on the German cross-border setting, we examine emissions from roads leaving and entering Germany. To gain a first understanding of the relationship between environmental taxes and emissions, we plot the tax differentials between Germany and its neighboring countries against the average emissions on cross-border road-sections over our study period (mid-2018 to 2022) in Figure 11. The figure suggests that PM10 emis-

sions generally increase on average when the tax in the neighboring country decreases, particularly evident when the tax differential becomes more negative, favoring lower environmental taxes in neighboring countries (e.g., Austria (AT); Belgium (BE); Czech Republic (CZ); Denmark (DK); Netherlands (NL); Poland (PL)). Conversely, we observe a decrease in average emissions during periods where the environmental tax differential increases, indicating that neighboring countries have raised their environmental taxes on fuel compared to Germany (e.g., Austria (AT); Belgium (BE); Switzerland (CH); Denmark (DK); Luxembourg (LU); Netherlands (NL); Poland (PL)). The figure thus indicates a shifting effect of emissions towards the country with lower tax rates.

Figure 11: Emission data: Environmental tax differentials and emissions



Note: The figure plots average environmental emissions on cross-border roads and the corresponding environmental tax differential between Germany and its neighboring countries over the time period from July 2018 until September 2022. Monthly PM10 is measured in $\mu g/m^3$ for the closest emission station. The environmental tax differential is calculated relative to Germany (tax in neighboring country - tax in Germany) and measured in Euro cents per liter of diesel fuel.

To analyze how local emission pollution reacts to tax-induced variations in cross-border truck traffic, we employ the reduced form approach utilized in Equation 4, with PM10, measured at the closest emission sensor station, being the dependent variable. Our analysis is limited by the requirement that PM10 must be measured at a close distance, i.e., in a radius of not more than one kilometer. However, Table 9 shows that this is only the case for a small share of our road-sections. We thus estimate our reduced form empirical model for subsets of our overall cross-border road-sections in a stepwise procedure. We start with including only values for PM10 that come from emission stations within a 750 meter perimeter. We subsequently increase this perimeter by 250 meters for each following regression until a perimeter of two kilometers. The further away from the threshold of one kilometer, we cannot expect the value of PM10 being measured precisely. Our empirical specification thus faces the trade-off between obtaining a sufficiently large sample for a reliable estimation of our coefficient of interest on the one hand, and measuring our outcome variable PM10 reliably on the other hand. Moreover, the small sample limits our ability to conduct two-way clustering of standard errors, as our individual road-sections reduce to less than fifty individual clusters. We thus use one-way clustered standard errors at the year-month level.

Figure 12, Panel A depicts our regression results visually for cross-border road-sections (see also Appendix Table B.9). We find a negative and statistically significant coefficient for small perimeters until a perimeter of 1.5 kilometers, above which the coefficient becomes insignificant. This is in line with our concern that measurement precision of PM10 decreases increasingly for perimeters larger than one kilometer. Our findings imply that a decrease in the environmental tax differential relative to a neighboring country by one percentage point increases local PM10 emissions at the respective cross-border road-section by between 0.09 and 0.16 $\mu g/m^3$.

As a placebo test, we estimate the identical regression for a subset of road-sections located further inside Germany, i.e., at a larger distance from the border. We choose a

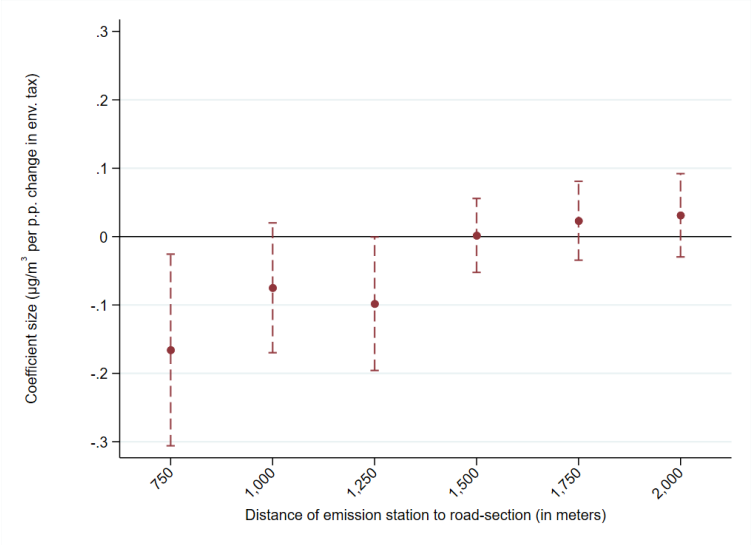
set of road-sections with orders between 10 and 40, because the road-sections below the order of 10 provide a risk that the closest, matched sensor station is identical to a cross-border road-section. Figure 12, Panel B depicts our placebo regression results. There is no discernible effect in any direction of cross-border tax differentials on local PM10 emissions. We conclude that environmental tax differentials lead to higher local pollution due to divergence in truck routes, in line with our findings from the prior section.

3.5.2 Predicting Emissions with Survey Data

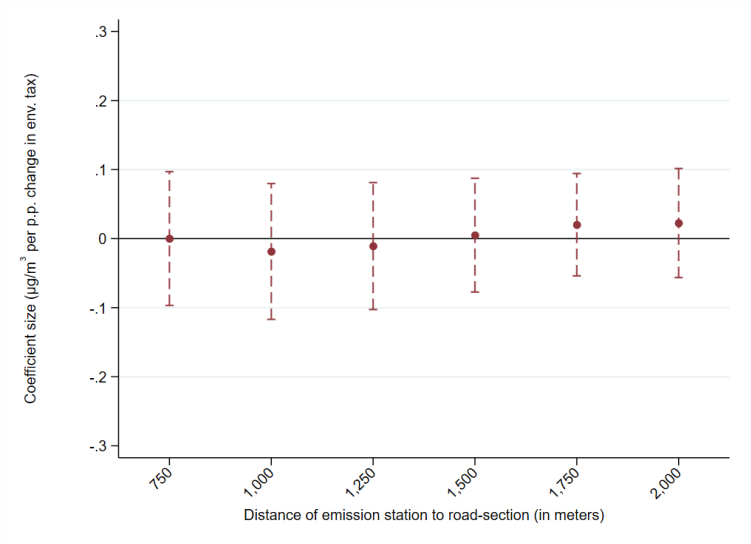
The findings from our second analysis, utilizing the Europe-wide truck journey data, enable us to quantify the environmental externalities arising from trucks deviating to minimize fuel taxes. We start by considering the excess ton-kilometers driven, as estimated in Table 12. Expanding on our previous findings, we employ a linear prediction model to estimate the cumulative surplus ton-kilometers resulting from tax-induced adjustments within the cross-border trucking sector in Europe. We use the 2SLS point estimate from Table 12 Panel B, along with a lower and upper bound derived from the 95%-confidence interval. We predict the deviation excess-ton kilometers using our sample of journeys throughout Europe. In the subsequent step, we adjust the result based on the sampling rate of the survey data. Finally, we calculate the predicted excess ton-kilometers by multiplying them by the associated tons of CO_2 emissions per ton-kilometer, considering varying levels of CO_2 emission intensity.

The results are displayed in Table 14, showing a range of excess emissions from 426,000 tons to 1,070,000 tons CO_2 emissions caused by tax-induced deviations in truck transport. These additional emissions result from trucks driving longer routes to save on fuel, highlighting an unintended consequence of non-harmonized environmental tax rates on diesel fuel. To contextualize these findings, it's notable that the total yearly CO_2 emissions caused by road transport in the EU amount to 199 million tons (European Environment Agency 2024). Thus, tax-induced deviation contribute to approximately 0.54%

Figure 12: Emission data: Effect of tax differentials on emissions



(a) On cross-border road-sections



(b) On inland road-sections

Note: The figure plots the effect of environmental taxes on emissions by distance of emission station to road-section. The points plotted are coefficient estimates of a reduced form regression of local PM10 on the environmental tax differential between Germany and its neighboring countries. Panel (a) presents the reduced form estimates for stepwise increases in perimeter size around the cross-border road-sections. Panel (b) repeats the same analysis with a focus on inland road-sections, i.e. those with a road order between 10 and 40. 95% confidence intervals are represented by the dashed red lines lines and based on standard errors clustered at the year-month level.

of these emissions. Overall, the differences in environmental tax rates on diesel thus lead to an increase in emissions.

Table 14: Survey data: Excess emissions from deviations

	Low CO2-Intensity	Medium CO2-Intensity	High CO2-Intensity
Lower Bound	426,540.7	785,732.8	1,046,147.1
Point estimate	431,679.1	795,198,4	1,058,749.8
Upper Bound	436,817.5	804,663.9	1,071,352.5

Note: The table presents the additional emissions resulting from tax-induced differences in diesel prices across the EU. The estimates are derived from the point estimates and 95%-confidence interval obtained from Table 12, Panel B, using a linear prediction model. The values are then scaled up by the sampling rate and multiplied by the excess ton-kilometers, along with their corresponding tons of CO_2 emissions. We include a low, medium and high CO_2 emissions multiplier.

3.6 Discussion and Conclusion

This study quantifies the impact of environmental taxes on commercial truck traffic and its environmental consequences. First, we use administrative toll data to analyze cross-border truck flows at German borders and assess the response of truck traffic to changes in environmental taxes. Second, we examine individual truck trips across Europe using administrative survey data to investigate whether trucks take detours to avoid high environmental taxes. In both analyses, we use an IV approach to isolate the effect of environmental taxes on the gross price of diesel. Specifically, we instrument the gross price of diesel with the environmental tax to disentangle the changes in the price of diesel induced by environmental taxes. In the second part, we assess the environmental consequences of these tax-induced responses by incorporating data on air pollution and CO_2 emissions.

We find that trucking companies respond significantly to environmental taxes on diesel. While we initially observe a decrease in cross-border truck traffic, especially on heavily congested highways near borders, we do not witness an overall reduction in traffic. Instead, trucking companies drive detours to refuel in countries with lower fuel taxes, leading to increased mileage and exacerbating environmental externalities. Moreover, our investigation into the environmental consequences reveals spikes in air pollution in response to tax-induced changes in commercial traffic at borders. We quantify that the

increased truck mileage lead to additional emissions of up to 1 million tons of CO_2.

Our comprehensive investigation into the effects of environmental taxes on commercial truck traffic makes a unique contribution to the existing literature. Responding to the timely and highly relevant call for more research on the impacts of environmental taxes (Lester and Olbert 2024), our study enhances the understanding of how such taxes influence commercial traffic and provides valuable insights into potential policy solutions to mitigate environmental consequences. The evidence we present suggests that harmonizing tax rates across the EU could be a viable strategy to prevent commercial trucks from deviating to other countries. Such cross-border harmonization could potentially decrease traffic and foster more sustainable practices in the transportation sector.

Our findings extend beyond Europe and the specific environmental taxes on fuel. Similar variations in fuel taxes exist across US states (Davis and Kilian 2011; Li et al. 2014), and the broader implications of our results can be generalized to scenarios with non-harmonized tax rates on different mobile tax bases. The key takeaway from our study is that when the tax base is mobile and opportunities for tax avoidance exist, there is a risk of shifting behavior. This issue is particularly critical in the context of environmental regulations aimed at reducing emissions to mitigate climate change, as it can lead to leakage and an overall increase in emissions.

4 Multinationals' Location, Financial and Real Responses to the EU-wide Implementation of CFC Rules by the ATAD

Co-Author: Emilia Gschossmann

Abstract: We examine how the introduction of Controlled Foreign Company (CFC) rules by the Anti-Tax Avoidance Directive (ATAD) in the European Union impacts Multinational Enterprise (MNE). Using firm-level financial data and a difference-in-differences research design, we study whether the implementation of CFC rules in the context of the ATAD alters MNEs' location, financial and economic activity decisions. Our results reveal that the newly implemented CFC rules were only partly effective in reducing income shifting. While the share of CFC subsidiaries decreases, the financial income of the persisting subsidiaries remains largely unchanged. Moreover, we observe positive effects on the costs of employees assigned to a CFC subsidiary, suggesting that the economic activity exemptions introduced by the ATAD allows MNEs to circumvent the rules by opting for a simple approach of enhancing economic activity in these locations.

Keywords: CFC legislation, Tax avoidance, Multinational firms, Corporate taxation

Acknowledgements: We thank Lisa De Simone, Philipp Doerrenberg, Jessica Mueller, Marcel Olbert, Christoph Spengel and Richard Winter for helpful comments and suggestions. We gratefully acknowledge comments and suggestions from seminar participants at the IESE Tax Conference, the Virtual Doctoral Tax Seminar and the University of Mannheim. We thank Felix Jungmann and Alexandra Mueller for excellent research assistance. We gratefully acknowledge financial support from the Graduate School of Economics and Social Sciences of the University of Mannheim.

4.1 Introduction

Globalisation and differing tax rates worldwide have provided multinationals with tax planning opportunities. The strategic shift of income from high-tax to low-tax countries by Multinational Enterprises (MNEs) has led to the erosion of countries' tax bases, resulting in substantial revenue losses. Data leaks like the LuxLeaks coupled with empirical literature (see, e.g., Riedel 2018, Dharmapala 2019, Dyreng and Hanlon 2023, for a review) have brought to light these aggressive tax planning practices, arousing public and political interest in curbing MNEs income shifting strategies. To counteract these tax avoidance practices of MNEs, Organisation for Economic Co-operation and Development (OECD) recommended the implementation of Controlled Foreign Company (CFC) rules as part of its broader Base Erosion and Profit Shifting (BEPS) project in 2015 (OECD 2015). By attributing the income of a low-taxed subsidiary to the higher-taxed parent company and subjecting this income to immediate taxation at the parent level, CFC rules seek to discourage MNEs from profit shifting.

Building on the OECD's recommendations, the European Union (EU) adopted the Anti-Tax Avoidance Directive (ATAD) in 2016 (European Union 2016). This directive mandated EU member states to implement and adjust various anti-tax avoidance measures, including CFC rules, which had to be introduced by December 31, 2018. While CFC rules were already in place as a unilateral tax avoidance measure in some countries, the mandatory introduction of these rules by the ATAD marked a significant shift for MNEs in the EU. The directive required a harmonized implementation across all EU countries, albeit with the flexibility for countries to adopt either model A or model B. Both models vary in terms of the income affected and the exemptions granted. Despite generally having the objective to act as a deterrent to income shifting, the CFC rules implemented by the ATAD comprise exemptions that provide MNEs with avenues to navigate around them.[64] In this paper, we

[64] Applied exemptions depend on the model chosen. Under model A, passive income of the low-taxed subsidiary is allocated to the parent company. However, if the subsidiary can prove that it carries out a significant economic activity or that its passive income accounts for less than one third of total income, it

investigate whether MNEs change their location, financial and economic activity decisions in response to the EU-wide introduction of CFC rules.

We use a Difference-in-Differences (DiD) design to empirically examine firms' reporting (income) and real (economic activity) responses to the introduction of CFC rules by the ATAD. Leveraging financial statement and ownership data from Orbis covering the years 2012 to 2020, we identify multinational groups encompassing both the parent company and its majority-owned subsidiaries.[65] We distinguish between CFC subsidiaries that are low-taxed and targeted by the anti-tax avoidance regulation and their indirectly affected sister companies, which we refer to as non-CFC subsidiaries.[66] Utilizing the announcement of the mandatory implementation of CFC rules in the EU in 2016, we compare MNEs before and after the implementation announcement (first difference) located in EU countries that newly implemented CFC rules under the ATAD to MNEs located in EU countries that had CFC rules in place prior to the ATAD (second difference).

Our analysis proceeds in three steps. We start by investigating whether multinational groups alter their location decisions, specifically by closing down CFC subsidiaries and instead opening up non-CFC subsidiaries. Next, we examine whether multinational groups change their income reporting behavior by focusing on CFC and non-CFC subsidiaries that did not close down due to the new regulation. Specifically, we examine financial income, as it is subject to immediate CFC taxation once earned in a low-tax country. Finally, we analyze the real effects on the economic activity of MNEs' subsidiaries, specifically exploring investment and employment. Throughout our analyses, we consider both directly affected CFC subsidiaries and indirectly affected non-CFC subsidiaries to assess whether MNEs respond by reallocating income or investment. Additionally, we exploit heterogeneities be-

can be exempted from the application of the CFC rule. Under model B, all non-distributed income from non-genuine arrangements that have been put in place for the essential purpose of obtaining a tax advantage is attributed to the parent company if certain accounting profit thresholds are exceeded. We provide further details and intuitions on these exemptions in Section 4.2.

[65] In contrast to many prior studies, we rely on annual historical ownership data allowing us to track the development of ownership relations over time (Prettl 2017; Clifford 2019; Schenkelberg 2020).

[66] We refer to the multinational group as the 'MNE' or the 'group' and its majority-owned entities as the 'subsidiaries'.

tween the two models A and B that member states could implement and examine how MNEs responded to the implemented exemptions.

The CFC regulations introduced by the ATAD could influence corporate decisions in several dimensions. The overall increase in the MNEs' tax burden may result in alterations to location decisions or even the liquidation of entities that no longer provide tax advantages (Barrios et al. 2012; Clifford 2019; Prettl and Hagen 2023). Similarly, MNEs may strategically reallocate financial income to navigate around CFC rules (Altshuler and Hubbard 2003; Ruf and Weichenrieder 2013). If multinational groups aim to minimize taxes, they might shift financial income to non-CFC subsidiaries positioned just above the tax threshold (Clifford 2019; Schenkelberg 2020). Additionally, higher overall tax burdens resulting from reduced tax avoidance could lead to decreased investments (Hall and Jorgenson 1967; Djankov et al. 2010; Ohrn 2018). Alternatively, to circumvent the application of the new regulations, MNEs may respond to the introduction of CFC rules by making investments that better align their taxation with economic activity (Bilicka et al. 2022; De Simone et al. 2022; Drake et al. 2022). While model A provides for an explicit exemption for subsidiaries with significant economic activity, model B applies only to arrangements without genuine business activity. Increased investments in countries with low tax rates could thus help to justify the income shifted to these countries and prevent this income from falling under the CFC regulations.

Our first set of tests examines the location responses of MNEs to the implementation of CFC rules under the ATAD. Using a sample of EU-based MNEs, we track the annual count of worldwide subsidiaries, categorizing those in low-tax countries as CFC subsidiaries and the remaining subsidiaries as non-CFC subsidiaries. A subsidiary is considered low-taxed if the host country's tax rate falls below the tax level criterion stipulated in the CFC rule of the parent company's home country. We then compare the share of (non-)CFC to total subsidiaries before and after the introduction of CFC rules due to the ATAD between MNEs located in countries that newly implemented CFC rules and those located in countries that

had CFC rules in place prior to the ATAD. Our findings reveal that the share of CFC subsidiaries in affected groups significantly decreases, while the share of non-CFC subsidiaries simultaneously increases in response to the implementation of CFC rules. The result is robust to alternative specifications and when examining the number of CFC subsidiaries directly. The reduction in the group's number of CFC subsidiaries is particularly notable among MNEs located in model A countries.

Having established that multinational groups alter their location decisions, we subsequently examine responses at the subsidiary level of affected groups, focusing specifically on subsidiaries that persist throughout our sample period. This sample criterion ensures that subsidiaries that have either been liquidated or newly established in response to the CFC rules do not bias our results. Our DiD specifications capture the differential effect between pre- and post-reform periods for (non-)CFC subsidiaries with parent companies located in countries that newly implemented CFC rules, relative to (non-)CFC subsidiaries with parents located in countries that already had CFC rules in place prior to the ATAD.

We start by exploring firms' reporting responses, specifically whether they change their income reporting. We focus on financial income at the subsidiary level as our outcome variable, as this income is subject to additional taxation at the parent company if CFC regulations apply. Previous studies suggest that MNEs shift financial income from CFC to non-CFC subsidiaries in response to the implementation of CFC rules (Prettl 2017; Clifford 2019). However, contrary to these earlier findings, we observe that the financial income of affected (non-)CFC subsidiaries does not change significantly in response to the CFC rule implementation compared to unaffected (non-)CFC subsidiaries. We thus document evidence consistent with firms not altering their income reporting behavior in persisting CFC subsidiaries.

Finally, we investigate whether MNEs adjust their real economic activity in response to CFC rules. Our objective is to understand whether, as suggested by prior literature, MNEs reduce their economic activity due to increased tax rates resulting from the CFC

rules (Ruf and Weichenrieder 2012; Egger and Wamser 2015; Prettl 2017). As an alternative channel, we explore whether these MNEs strategically substantiate their low-taxed subsidiaries to take advantage of exemptions provided in the ATAD. To assess whether MNEs exploit these exemptions to mitigate the application of CFC rules, we evaluate the effects on investment and employment at the subsidiary level. While we do not find clear evidence that treated CFC subsidiaries alter their investment behavior, we observe significant increases in employee costs. Moreover, while personnel costs of affected CFC subsidiaries rise, those of treated non-CFC subsidiaries decrease. This suggests that labor is more mobile and adjustable than capital (Giroud and Rauh 2019; De Simone and Olbert 2022). Additionally, we find that the number of employees at treated CFC subsidiaries slightly decreases. This is consistent with prior research from the Intellectual Property (IP) box literature, indicating that MNEs reallocate wages rather than expand the workforce (Bornemann et al. 2023; Chen et al. 2023).

We provide several additional tests to ensure that our results are not influenced by coinciding events that affected MNEs during that time, namely additional anti-tax avoidance regulations implemented with the ATAD and Country-by-Country Reporting (CbCR) targeting EU-based MNEs. To address these concerns, we demonstrate that our results remain robust to alternative samples, control groups, and outcome variables. By design, our results are unlikely to be driven by the introduction of CbCR, as this regulation applies to to all multinationals above the applicable size threshold in the EU (De Simone and Olbert 2022). In our research design, we directly identify groups and their subsidiaries that were newly subject to CFC rules and compare them to similar entities that were already affected beforehand. To address concerns regarding outcome measures and control variables simultaneously affected by the policy change, we demonstrate the robustness of our results using pre-period covariates (Lester and Olbert 2024).

Our findings suggest that the introduction of CFC rules had a limited impact on MNE's income shifting behavior. While we observe a reduction in the share of CFC subsidiaries,

the financial income of remaining CFC subsidiaries remains unchanged. Interestingly, we find that MNEs substantiate real economic activity by increasing employee costs within CFC subsidiaries, which allows them to fall under the exemptions provided by the ATAD. However, the absence of significant increases in investments or the expansion of the workforce implies that MNEs are opting for a less cost-intensive strategy to circumvent the application of CFC rules. Instead of making costly investments or expanding their workforce, firms are choosing to elevate employee costs to circumvent the anti-tax avoidance regulation.

Our study contributes to the literature in three ways. First, we add to the research on firms' real reporting responses to taxation, as proposed by Lester and Olbert 2024. We investigate both real activity and reporting outcomes separately within one setting by exploiting the EU-wide implementation of CFC rules by the ATAD. Previous research on the interrelation between firms' real activity and reporting responses has shown mixed results. Some studies suggest that firms make real decisions to successfully implement income-shifting strategies (Williams 2018; De Simone and Olbert 2022; Drake et al. 2022), while other studies focus on the consequences of income shifting for subsequent investment showing that income-shifting affects the level and efficiency of investment (Suárez Serrato 2019; Bilicka et al. 2022; De Simone et al. 2022). We provide evidence that, in the face of an anti-avoidance regulation, MNEs can simultaneously back up existing income-shifting strategies on their balance sheets with economic activity.

Second, we contribute to the broader literature in accounting and economics on the effects of CFC regulations. Prior research has primarily focused on investigating CFC rules before the implementation of the ATAD, generally finding them effective in curbing income shifting activities (Altshuler and Hubbard 2003; Prettl 2017; Clifford 2019; Schenkelberg 2020), leading to decreased investments in low-tax countries (Ruf and Weichenrieder 2012; Egger and Wamser 2015; Clifford 2019; Prettl and Hagen 2023). Our study goes further by evaluating the widespread implementation of CFC rules within the EU during

the post-BEPS and post-ATAD period. This time frame marks a shift where CFC rules are no longer unilateral measures but have become a collective initiative, under which a minimum harmonization is achieved. Given the large-scale and harmonized implementation of CFC rules, EU-based MNEs can no longer avoid the application of the rules by relocating their headquarters to EU member states without CFC rules (Voget 2011). This coordinated approach aims to close remaining loopholes within the EU, desirably leading to significantly less tax avoidance. Unlike prior studies focusing solely on the effects on CFC subsidiaries (Ruf and Weichenrieder 2013; Egger and Wamser 2015), our approach considers the multinational group as a whole, encompassing both CFC and non-CFC subsidiaries.[67] We explore responses at both the group and subsidiary levels, comparing changes made to financial income, investment, and employment within the group. Additionally, we introduce a novel dimension by examining whether MNEs exploit exemptions provided under the ATAD to circumvent the CFC rules. Our findings indicate that MNEs strategically enhance their economic presence by increasing costs of employees to circumvent the new regulation.

Third, we contribute to the ongoing debate surrounding the economic consequences of newly implemented anti-tax avoidance measures (Jacob 2022). Our study assesses the impact of the CFC rules under the ATAD on firms' real reporting responses, providing essential information for policymakers. We are the first to provide an extensive study of coordinated CFC rules within the EU, shedding light on the effects of different models and offering policy-relevant insights into how the rules' design influences their impact on income shifting and economic activity. While our findings align with the EU's intended goal of better aligning economic activity in low-tax countries, we observe that MNEs do not alter their income-shifting behavior but tend to opt for simple ways to substantiate economic activity by adjusting the costs of employees. This trend contradicts the EU's intended goals. Importantly, as discussions on a global minimum tax unfold, our research

[67] Bilicka et al. 2022 and Bilicka et al. 2023 also investigate alternative anti-tax avoidance regulations and consider affiliates that are indirectly affected.

underscores the ongoing importance of evaluating the impact of coordinated CFC rules (Bilicka et al. 2023; Overesch et al. 2024). With the likelihood of these rules coexisting with a global minimum tax, our study emphasises the necessity of continued evaluation to guide policy-making and ensure the effectiveness of anti-tax avoidance measures.

4.2 Institutional Background

CFC rules are designed to discourage MNEs from transferring income from their high-tax home country to foreign subsidiaries with lower tax rates. This is achieved by enabling the home country of the parent company to tax the income of the foreign subsidiary. Specifically, when a foreign subsidiary is identified as both controlled and low-taxed, its profits are attributed to the parent company and taxed on an accrual basis.[68] The foreign income is incorporated into the parent company's tax base in the year of realization, making it subject to the high Corporate Income Tax (CIT) rate of the home country, while the taxes paid in the host country are credited. Consequently, CFC rules impose a tax penalty on the parent company for engaging in income-shifting activities, equal to the difference between the high taxes of the home country and the lower taxes paid in the host country.

In its effort to reduce tax avoidance, the EU mandated all member states to adopt or adjust their existing CFC rules to comply with the provisions set out in the ATAD by December 31, 2018 (European Union 2016). Generally, the CFC rule of the ATAD applies to entities subject to corporate taxes in one or more EU member states (Art. 1 ATAD). For an MNE to fall under the application of CFC rules, two criteria have to be fulfilled: the control criterion and the tax level criterion. The control criterion dictates that a subsidiary can only qualify as a CFC if it is under the control of the parent company. Control is defined as the direct or indirect majority ownership of voting rights, capital, or profit entitlement (Art. 7 (1) (a) ATAD). Thus, CFC rules are applicable to foreign entities whose

[68] The amount of profit that is attributed to the parent company is determined in proportion to the parent company's participation in the subsidiary.

decisions are influenced by the parent company, making them susceptible to income shifting. The tax level criterion stipulates that the provisions should only apply to low-taxed subsidiaries. The rationale behind this criterion is to address tax avoidance by focusing on foreign subsidiaries subject to low taxation. According to Art. 7 (1) (b) ATAD, a subsidiary is considered low-taxed if the actual CIT paid is less than 50% of the CIT that would have been levied under the corporate tax system of the parent company's home country. Some countries additionally establish black or white lists that determine whether a country is considered low-taxed or not. This is possible because the ATAD sets minimum requirements, leaving member states the flexibility to set lower control or higher tax level thresholds.

If the subsidiary meets the control and tax level criteria, it is considered as CFC, triggering the application of CFC rules. The income generated by the low-taxed subsidiary is assigned to the parent company and immediately subject to CFC taxation. The allocation of income depends on the CFC model chosen, as outlined in Article 7 (2) of the ATAD. Member states have the flexibility to opt for one of two models, each differing in the nature of income to be attributed. Model A assigns non-distributed passive income, including interest, royalties, dividends, income from financial leasing, income from financial activities, and income from invoicing companies, to the parent company (Article 7 (2) (a) ATAD). This category of income is highly mobile, making it ideal to shift income. Under model B, all non-distributed income from non-genuine arrangements established primarily for the purpose of securing a tax advantage is allocated to the parent company (Article 7 (2) (b) ATAD). However, such non-genuine arrangements are not explicitly defined, resulting in an ambiguous scope for the application of model B.

The ATAD contains exemptions for low-taxed subsidiaries that demonstrate genuine economic activities or only little mobile income. Specifically, member states applying model A must provide an exemption for CFCs within the European Economic Area (EEA) that show evidence of substantive economic activities. Optionally, this substantive eco-

nomic activity exemption can be extended to CFCs located in third countries. To streamline administrative processes, member states using model A have the discretion to exclude subsidiaries from their CFC regulations if their passive income constitutes less than one-third of the total income. Member states opting for model B may apply exemptions for CFCs that report accounting profits and non-trading income below Euro (EUR) 750.000 and EUR 75.000, respectively, or accounting profits below 10% of total operating costs.

Before the ATAD mandated the implementation of CFC rules across all EU countries, some member states already had pre-existing CFC regulations. These countries either maintained their existing rules or made only minor adjustments to align with the ATAD's provisions. In contrast, other countries introduced CFC regulations for the first time.[69] A total of 15 countries newly adopted CFC rules following the ATAD, namely Austria, Belgium, Bulgaria, Cyprus, Czech Republic, Estonia, Croatia, Ireland, Luxembourg, Latvia, Malta, Netherlands, Romania, Slovenia and Slovakia. With the exception of Romania, all of these countries implemented the provisions at the end of 2018. The distribution of the model chosen is as follows: six countries adopted model A (Austria, Czech Republic, Croatia, Netherlands, Slovenia, Romania), while nine countries opted for model B (Belgium, Bulgaria, Cyprus, Estonia, Ireland, Luxembourg, Latvia, Malta, Slovakia). Twelve countries had pre-existing CFC rules before the ATAD (Denmark, Finland, France, Germany, Greece, Hungary, Italy, Lithuania, Poland, Portugal, Spain, Sweden). Among these, eight countries operate under model A (Denmark, Germany, Greece, Italy, Lithuania, Poland, Portugal, Spain) and two under model B (Hungary, Sweden). France and Finland combined the two models.

In terms of exemptions, all countries that implemented model A had to introduce the economic activity exemption for EEA countries. However, only some of these countries extended this exemption to third countries. Concerning the one-third income exemption,

[69] Table C.1 provides an overview of the CFC legislations in the EU member states with the respective year of enactment. The legislative information for the study is mainly drawn from IBFD (2014-2021), OECD, PwC and KPMG.

most model A countries introduced this provision or adopted an even stricter proportion. As for model B countries, the majority implemented the exemptions related to low accounting profits and non-trading income..

All EU member states were required to implement CFC rules by the end of 2018, with the regulations entering into force in January 2019. MNEs were informed about these impending regulations first with the ATAD's proposal in January 2016 and its final adoption in July 2016. This timeline allowed multinational groups to anticipate the application of the regulation from the financial year 2019 onward, enabling them to plan possible reporting and economic adjustments as early as 2016. Countries that had already implemented CFC regulations made only minor adjustments to their existing regulations, such as extending the scope of the rules to include permanent establishments. These changes were largely implemented at a later date.[70]

4.3 Literature and Hypotheses

In the absence of anti-tax avoidance regulations, MNEs can strategically position income within their subsidiaries in low-tax countries (Dharmapala 2019; Dyreng and Hanlon 2023). Through strategies like transfer pricing, multinational groups shift income to foreign subsidiaries subject to lower tax rates, thereby reducing their overall tax burden (Jacob 1996; Grubert 2003). However, with the introduction of CFC rules, the tax rate on foreign income rises significantly, aligning it with the tax rate of the parent company's jurisdiction. Consequently, this results in a substantial increase in both the foreign effective tax rate and the overall tax burden of the MNE, leading to higher costs of capital for the corporation (Hall and Jorgenson 1967).

To maintain low tax levels and further maximize after-tax income, MNEs can respond in various dimensions following the implementation of CFC regulations. Anti-tax avoid-

[70] E.g., Germany and Luxembourg adjusted its CFC regulations in 2022, while Denmark, France, Greece, and Spain did so in 2021.

ance measures impact firms' decisions both at the corporate group level and at the subsidiary level. Multinational groups can respond by altering their location decisions, income reporting, and real economic activity. An MNE comprises the parent company and several subsidiaries, which can be further differentiated into CFC and non-CFC subsidiaries. CFC subsidiaries are those directly falling under the purview of the CFC regulation. In contrast, non-CFC subsidiaries essentially function as sister entities to CFC subsidiaries, closely connected and susceptible to being utilized by the parent company to reroute income, assets, or personnel in reaction to anti-tax avoidance measures.

First, our analysis focuses on the group level, assessing how multinational groups respond to CFC rules by investigating shifts in location patterns within the MNE. As controlled subsidiaries that fall below the tax threshold are less attractive for income shifting, MNEs might consider closing such CFCs (Barrios et al. 2012). Conversely, to still achieve a low tax rate, MNEs could potentially reroute income to existing non-CFC subsidiaries that are taxed at a lower rate compared to the parent company. MNEs might even explore the option of opening new subsidiaries in countries just above the tax threshold. Existing research provides evidence that CFC rules influence MNEs to be less likely to establish subsidiaries in low-tax countries that fall under the tax level criterion. For instance, Clifford 2019 examines how MNEs respond to CFC rules in terms of location decisions, revealing that MNEs relocate their subsidiaries, resulting in fewer subsidiaries falling below the tax level threshold and more positioned right above it. Additionally, findings from Prettl and Hagen 2023 indicate that CFC rules adversely impact the likelihood of choosing a low-tax country as the target country in mergers and acquisitions. Therefore, we anticipate a reduction in the share of CFC subsidiaries, while we expect the share of non-CFC subsidiaries to increase.

Hypothesis 1a: The share of CFC subsidiaries of affected MNEs decreases in response to the implementation of CFC rules relative to unaffected MNEs.

Hypothesis 1b: The share of non-CFC subsidiaries of affected MNEs increases in response

to the implementation of CFC rules relative to unaffected MNEs.

Second, we focus on the income reporting response at the subsidiary level (Lester and Olbert 2024). Altshuler and Hubbard 2003 observe a decrease in the incentive to place financial assets in low-tax countries due to CFC rules. Similarly, Ruf and Weichenrieder 2013 investigates the implementation of CFC rules in Germany and concludes that CFC rules deter the placement of financial assets in low-tax countries. Conversely, Clifford 2019 and Schenkelberg 2020 explore the response of sister subsidiaries that are not directly subject to CFC regulation, revealing that multinationals reroute income into subsidiaries just above the tax level threshold. Therefore, we formulate the following predictions.

Hypothesis 2a: The financial income of affected CFC subsidiaries decreases in response to the implementation of CFC rules relative to unaffected subsidiaries.

Hypothesis 2b: The financial income of affected non-CFC subsidiaries increases in response to the implementation of CFC rules relative to unaffected subsidiaries.

Third, we analyze the real effects on economic activity. Prior literature has consistently shown that increasing MNEs' costs of profit shifting lead to a reduction in investment (Jacob 2022).[71] This pattern extends to the implementation of CFC rules, as evidenced by, e.g., Egger and Wamser 2015, which find that the introduction of CFC rules in Germany in 2001 was associated with affiliates in low-tax countries decreasing total fixed assets. However, tax authorities increasingly demand firms to substantiate income in low-tax countries to align taxation with real economic activity. This can be achieved by increasing investment (Bilicka et al. 2022; De Simone et al. 2022; Drake et al. 2022; Bilicka et al. 2023). In line with this, the CFC rule of the ATAD only applies to subsidiaries with no genuine economic activity, incentivizing MNEs to substantiate subsidiaries' businesses if they want to benefit from low tax rates. Balancing these two conflicting ideas, we frame our hypotheses in the null form.

Hypothesis 3a: The economic activity of affected CFC subsidiaries does not change in

[71] Refer to Djankov et al. Djankov et al. 2010; Ljungqvist and Smolyansky 2018; Ohrn 2018; Giroud and Rauh 2019 for papers that show a negative relation between taxes and investment and employment.

response to the implementation of CFC rules relative to unaffected subsidiaries.

Hypothesis 3b: The economic activity of affected non-CFC subsidiaries does not change in response to the implementation of CFC rules relative to unaffected subsidiaries.

4.4 Data and Empirics

4.4.1 Data

To analyze the effects of the CFC regulations implemented by the ATAD, we leverage firm-level data from the Orbis database provided by Bureau van Dijk. The Orbis database combines financial information with ownership information of reported firms, which allows us to determine whether a subsidiary meets the control criterion specified by the ATAD. The control criterion necessitates direct or indirect majority ownership of voting rights, capital, or profit entitlement. In our study, we concentrate on the connection between a subsidiary and its global ultimate owner, i.e., the highest-ranking shareholder in the corporate structure who holds the majority of the subsidiary. Unlike previous studies, we utilize annual historical ownership data spanning from 2012 to 2020.[72] This enables us to track ownership relations over time instead of using just a snapshot of the ownership network and assuming it to remain constant.

The tax level criterion of the ATAD identifies a subsidiary as low-taxed if its actual taxes paid are more than 50% lower than what would be owed if taxed in the parent company's home country. We use the statutory CIT rate of the subsidiary in 2019 to approximate actual taxes paid by the subsidiary in the host country. The statutory CIT rate of the parent company's home country effective in 2019 is used to estimate taxes in the home country. We also conduct robustness checks using the Effective Average Tax Rate (EATR). The choice of 2019 as a benchmark aligns with the initial year of application for the new regulations. In evaluating whether a subsidiary is subject to CFC rules, we further consider

[72] For instance, the studies by Clifford (Prettl 2017; Clifford 2019; Schenkelberg 2020) rely solely on ownership information from the final year of their sample periods. This approach may introduce bias into their findings since group structures typically undergo variations over time.

relevant black or white lists established in the MNE's home country. As we are interested in the consequences of the implementation of CFC rules in the EU member states due to the ATAD, our analysis focuses on EU-based MNEs and their worldwide subsidiaries.

We complement the ownership information with financial data for the years 2012 to 2020, using consolidated financial data for the multinational groups and unconsolidated financial variables for the subsidiaries. We exclude firms in the banking and insurance sector from the data due to distinct tax treatment.

We compile information on the MNEs' home countries and the subsidiaries' host countries from various sources. Worldwide statutory CIT rates are hand-collected from the OECD[73], the Tax Foundation[74] and EY. Countries' Gross Domestic Product (GDP), Gross Domestic Product per Capita (GDPPC) and unemployment rates are drawn from the World Development Indicators Database of the World Bank.[75] As the variables GDP and GDPPC are expressed in current USD, the annual average exchange rates published by Eurostat are applied for conversion to EUR.[76] Data on countries' Corruption Perception Index (CPI), reflecting rankings from highly corrupt (zero) to very clean (100), is obtained from Transparency International.[77]

4.4.2 Sample and Descriptives

To comprehensively assess MNEs' responses, we establish two distinct samples: a group sample and a subsidiary sample. The group sample, organized by group-years as the unit of observation, includes the annual share of CFC and non-CFC subsidiaries within each group. We require that all groups in the sample must have at least one CFC subsidiary at some point in our observation period. Additionally, observations featuring negative values

[73] OECD, https://data-explorer.oecd.org/vis?tenant=archive&df[ds]=DisseminateArchiveDMZ&df[id]=DF_CTS_CIT&df[ag]=OECD&lo=5&lom=LASTNPERIODS&dq=.&to[TIME_PERIOD]=false.
[74] Tax Foundation, https://files.taxfoundation.org/20211208141411/1980-2021-Corporate-Tax-Rates-Around-the-World.xlsx.
[75] World Bank, https://databank.worldbank.org/source/world-development-indicators.
[76] Eurostat, https://ec.europa.eu/eurostat/web/exchange-and-interest-rates/database.
[77] Transparency International, https://www.transparency.org/en/cpi/2020.

in the control variables are excluded. The sample creation steps are shown in Appendix Table C.2. Using the group sample, we examine MNEs' location responses. Specifically, we investigate whether the share of both CFC and non-CFC subsidiaries changes in response to the implementation of CFC rules under the ATAD (addressing *Hypotheses 1a* and *1b*).

For a more granular analysis of persistent subsidiaries, the subsidiary sample necessitates firms to have observations in both 2012 and 2020, marking the start and end of our sample period.[78] The subsidiary sample is based on subsidiary-year observations and includes subsidiaries' financial income, assets, costs and numbers of employees. We drop observations with negative values in the dependent and independent variables. Appendix Table C.3 depicts the sample creation. Using the subsidiary sample, we analyze MNEs' income reporting responses, delving into whether CFC and non-CFC subsidiaries change the reporting of financial income (*Hypotheses 2a* and *2b*). Additionally, we investigate economic activity, assessing whether subsidiaries make adjustments to tangible fixed assets or the costs associated with employees (*Hypotheses 3a* and *3b*).

Table 15 presents descriptive statistics for both the group sample (Panel A) and the subsidiary sample (Panel B), further divided into CFC and non-CFC subsidiaries. The group sample encompasses 24,919 observations from 3,450 distinct MNEs. On average, CFC subsidiaries account for 13% of the total number of subsidiaries in a group, while non-CFC subsidiaries make up 86%. The subsidiary sample contains 90,641 CFC subsidiary observations from 10,722 distinct subsidiaries. In comparison, non-CFC subsidiary observations reach 4,521,635 subsidiary-years, spanning 528,379 unique subsidiaries. The difference in observations between CFC and non-CFC subsidiaries can be attributed to the tax level criterion of the ATAD, according to which most subsidiaries' host countries exceed the low-tax threshold specified by the parent companies' home countries, resulting in a higher count of non-CFC subsidiaries in the sample.

[78] Restricting our sample to subsidiaries that survive throughout the sample period allows us to account for survivorship bias. We want to examine the intensive margin responses of those subsidiaries that did not close in response to the introduction of CFC rules.

Table 15: Descriptive statistics

	Obs	Mean	SD	P25	P50	P75
Panel A: Group sample						
Share of CFC subsidiaries	24,919	0.13	0.17	0.01	0.07	0.20
Share of non-CFC subsidiaries	24,919	0.86	0.17	0.80	0.92	0.99
Number of total subsidiaries	24,919	79.13	313.01	3.00	12.00	38.00
Number of CFC subsidiaries	24,919	5.94	43.84	0.00	1.00	2.00
Number of non-CFC subsidiaries	24,919	72.94	294.84	2.00	10.00	34.00
Total assets (mio. EUR)	24,777	2,966	13,505	63.08	191.00	792.90
Intangibility	23,231	0.11	0.16	0.01	0.04	0.15
Leverage	21,577	0.19	0.30	0.06	0.15	0.26
Average foreign tax rate (percent)	18,551	24.59	5.44	21.62	25.00	27.81
Home-country GDP (bil. EUR)	24,919	1,955	1,044	1,078	2,228	3,026
Home-country GDPPC (thous. EUR)	24,919	33.58	9.12	28.16	33.39	37.66
Home-country CIT rate (percent)	24,919	29.36	4.38	28.00	30.18	30.18
Panel B: Subsidiary sample						
CFC subsidiaries						
Total assets (mio. EUR)	90,102	19.28	332.88	0.03	0.22	1.45
Tangible fixed assets (mio. EUR)	74,837	4.03	59.09	0.00	0.03	0.35
Number of employees	47,631	69.33	437.81	1.00	5.00	29.00
Costs of employees (mio. EUR)	36,337	2.16	11.26	0.03	0.19	0.98
Financial income (mio. EUR)	48,230	0.79	15.90	0.00	0.00	0.02
Operating income (mio. EUR)	51,950	21.69	273.71	0.03	0.47	3.99
Host-country GDP (bil. EUR)	90,641	1,019	1,114	131.37	277.48	2,158
Host-country GDPPC (thous. EUR)	90,641	25.67	19.78	9.13	16.67	39.80
Host-country CIT rate (percent)	90,641	18.86	3.66	16.00	19.00	20.00
Host-country unemployment rate (percent)	90,641	6.15	2.88	4.33	5.56	7.10
Host-country CPI	73,498	63.48	16.69	48.00	60.00	80.00
Non-CFC subsidiaries						
Total assets (mio. EUR)	4,489,309	10.60	296.64	0.27	0.76	2.21
Tangible fixed assets (mio. EUR)	4,326,498	2.41	103.29	0.01	0.08	0.44
Number of employees	2861980	30.83	409.74	2.00	6.00	17.00
Costs of employees (mio. EUR)	2,365,427	1.44	23.58	0.04	0.15	0.50
Financial income (mio. EUR)	2,706,939	0.45	15.01	0.00	0.00	0.00
Operating income (mio. EUR)	3,292,177	7.16	181.03	0.11	0.47	1.80
Host-country GDP (bil. EUR)	4,521,635	1,720	929.72	1,113	1,655	2,490
Host-country GDPPC (thous. EUR)	4,521,635	29.21	8.02	24.11	27.72	36.47
Host-country CIT rate (percent)	4,521,644	28.79	3.77	27.81	30.00	31.29
Host-country unemployment rate (percent)	4,521,635	9.52	5.16	5.23	9.40	11.90
Host-country CPI	3,503,625	64.66	14.95	52.00	62.00	80.00

Note: The table displays the summary statistics for the group sample and the subsidiary sample. The subsidiary sample is split into subsidiaries classified as CFC subsidiaries and subsidiaries classified as non-CFC subsidiaries. The variables are defined in Appendix C.5.

4.4.3 Empirics

The implementation of CFC regulations in several EU countries at the end of 2018 creates a quasi-experimental setting, allowing us to empirically study companies' reactions through a DiD design. In our analyses, we compare changes over time between firms newly affected by the CFC regulations due to the ATAD (treatment group) and those that had already been subject to CFC regulations prior to the ATAD (control group). The identifying assumption is that treated and control firms would have followed a similar trend in the absence of the implementation of CFC rules resulting from the ATAD. Despite the rules taking effect at the close of 2018, we expect anticipation effects in the years leading up to it. The ATAD proposal was unveiled in January 2016 and officially adopted in July 2016. Consequently, companies were aware as early as 2016 that they would be subject to CFC rules by the end of 2018, providing ample time for anticipatory adjustments. We further support this notion by conducting a media coverage analysis with Factiva, which highlights the spike in newspaper attention throughout 2016 (see Appendix Figure C.1). To accommodate firms' anticipation of the rule, we designate the year 2016 as the initial treatment year.

First, we examine MNEs as a whole using the group sample. To assess the impact of the introduction of CFC rules in response to the ATAD on MNEs' location decisions, we explore a group's share of CFC and non-CFC subsidiaries before and after the implementation. The treatment group consists of MNEs for which the newly implemented CFC rules became binding in 2019. To be part of the treatment group, a MNE needs to be located in one of twelve EU member states that newly introduced CFC rules at the end of 2018.[79] MNEs in the control group did not experience significant changes in CFC legislation. The control group consists of MNEs located in one of nine EU member states that have not

[79] We consider twelve out of 15 EU member states that newly implemented CFC rules due to the ATAD in our treatment group. Romania introduced the CFC rules one year earlier, i.e., at the end of 2017, and is thus not included in our empirical analysis. Furthermore, the CFC regulations in Estonia and Latvia do not specify a tax level criterion to determine whether a subsidiary is low-taxed. As we cannot identify CFC subsidiaries of Estonian and Latvian MNEs, we exclude these two countries from the analysis.

implemented CFC rules at the end of 2018, because similar provisions had already been in place.[80] Although some of these home countries amended their existing CFC provisions at the end of 2018, the legislative changes made, e.g., the extension of CFC rules to permanent establishments, are only of minor relevance for the dependent variables considered in our analyses and are assumed to have a negligible effect. To address potential concerns, we exclude control countries that changed their CFC regulations in robustness tests and find similar results.

We employ a Poisson Pseudo Maximum Likelihood (PPLM) model for this analysis as econometric theory suggests that PPML regressions are particularly suited for underlying count variables (Santos Silva and Tenreyro 2006; Cohn et al. 2022; Chen and Roth 2023; Wooldridge 2023). Our underlying outcome variable, the number of (non-)CFC subsidiaries, is likely to be nonlinear and characterized by a large proportion of zero values. We include an exposure variable, the total number of subsidiaries, to examine the effect on the share of (non-)CFC subsidiaries.[81]

$$\begin{aligned} &\textit{Share of (non-) CFC subsidiaries}_{gmt} = \\ &exp(\beta_1 \times (\textit{MNE Treat}_m \times \textit{Post}_t) + \gamma_1 \times W_{gt} + \gamma_2 \times W_{mt} + \lambda_g + \omega_t) + \varepsilon_{gmt} \end{aligned} \tag{6}$$

The *Share of (non-) CFC subsidiaries* of a multinational group g located in home country m in year t is the outcome of interest. $\textit{MNE Treat}_m$ is an indicator variable equal to 1 for MNEs located in EU member states that newly implemented CFC rules by the ATAD. $\textit{POST}_t$ is an indicator variable equal to 1 for financial years starting on or after January 1, 2016. W_{gt} are time-varying group control variables and W_{mt} are time-varying home-country controls. Following existing literature, we include the statutory CIT rate as well

[80] Twelve EU member states already had CFC regulations in place before the ATAD. In our analysis, we exclude Greece, Lithuania and Poland as they introduced their CFC rules in 2014 and 2015, shortly before the ATAD was adopted, and would therefore distort our results.

[81] In Poisson regressions, the effects are proportional and can be interpreted as semi-elasticities. The Poisson model can be solved using the natural logarithm $log(Y) = \alpha + \beta X + \varepsilon$. The effect of a unit change can then be expressed in log-units of the dependent variable. The implied estimate of the proportional treatment effect is $exp(\beta_1) - 1 * 100$. When an exposure variable is added, Poisson regressions estimate the semi-elasticity of the rate of outcome per unit of exposure.

as the GDP and GDPPC as home country controls (Markle and Robinson 2012; De Simone and Olbert 2022). To account for group characteristics, we include leverage, total assets, intangible assets scaled by total assets, and the average foreign CIT rate across all the group's subsidiaries (Markle and Robinson 2012). Moreover, the specifications contain group fixed effects (λ_g) to control for time-invariant unobserved group heterogeneity. We also include year fixed effects (ω_t) to account for aggregate time trends.[82] The coefficient of interest β_1 captures the differential effect on MNEs' share of (non-)CFC subsidiaries between pre- and post-reform periods for the treatment group relative to the control group.

Next, we delve into the subsidiary-level analysis, focusing only on those CFC and non-CFC subsidiaries that persist throughout the implementation of CFC rules. As we are interested in investigating the effects on CFC and non-CFC subsidiaries individually, we conduct separate analyses for each. Non-CFC treatment subsidiaries, though not meeting the CFC classification, are integral parts of groups that own at least one CFC subsidiary. Consequently, these non-CFC subsidiaries essentially function as sister entities to CFC subsidiaries, adding another dimension to our exploration, as companies might redirect income, assets, or personnel to these entities in response to the implementation of CFC rules. In our treatment group, we include subsidiaries of MNEs located in countries that newly introduced CFC rules, while in our control group, we include subsidiaries of MNEs located in countries that already had CFC rules in place before the ATAD. Thus, we compare CFC subsidiaries of treated MNEs to CFC subsidiaries of control MNEs and non-CFC subsidiaries of treated MNEs to non-CFC subsidiaries of control MNEs.[83]

To investigate firms' real and reporting responses at the subsidiary level, we examine variables from subsidiaries' financial statements. Initially, we explore changes in subsidiaries' income reporting behavior, utilizing financial income subject to CFC taxation as the outcome variable. Subsequently, we investigate the effects on economic activity of

[82] In a robustness check, we also include industry-year fixed effects to absorb differential time trends across industries.

[83] See Appendix Figure C.2 for illustrations of the set-up of the two different treatment dummies.

subsidiaries, focusing on tangible fixed assets and the cost of employees. We estimate the following PPML regression for subsidiary i located in host country h in year t.[84]

$$\begin{aligned} &\textit{Financial variable (non-)CFC subsidiary}_{iht} = \\ &exp(\beta_1 \times (\textit{(Non-)CFC Treat}_i \times \textit{Post}_t) + \gamma_1 \times W_{it} + \gamma_2 \times W_{ht} + \lambda_i + \omega_t) + \varepsilon_{iht} \end{aligned} \tag{7}$$

The coefficient of interest $beta_1$ captures the differential effect between pre- and post-reform periods for the treatment group relative to the control group. We include time-varying subsidiary control variables (W_{it}) and time-varying host country control variables (W_{ht}). As host country controls, we include the GDP and GDPPC as well as the unemployment rate, the CPI and the statutory CIT rate (Schenkelberg 2020). To account for subsidiary size, we include operating income and the number of employees when examining tangible fixed assets and operating income and total assets when examining employee costs. To absorb unobserved time-invariant subsidiary and time-specific trends, we again include year (ω_t) and subsidiary fixed effects (λ_i).

One potential concern is that the dependent variables of treatment and control firms may exhibit differential trends in the pre-treatment period. To mitigate this concern, we additionally estimate all our specifications in an event-study DiD model, replacing the $POST_t$ indicator with separate years. We show that the parallel trends assumption holds during the pre-reform years. For identification, we further assume that no other correlated factors impacted our treated and control MNEs differently around the ATAD implementation. Our DiD approach exhibits several strengths that support this identifying assumption. We identify EU MNEs and their subsidiaries that fall under the purview of the CFC regulation and compare newly treated firms within the same group ((non-)CFC subsidiaries to (non-)CFC subsidiaries) to those that have already been treated. Thus, by design, our results cannot be driven solely by CbCR, as it applied to all EU MNEs at the same time.

[84] Poisson regressions are well-suited even when the outcome variable is continuous (Santos Silva and Tenreyro 2011; Cohn et al. 2022; Wooldridge 2023). We calculate the estimated treatment effect as $(exp(\beta_1) - 1) * 100$.

Moreover, further tax enforcement applied by the European Commission at the same time targeted primarily US MNEs that are not under the scope of the ATAD's CFC rules (Fox et al. 2022). To address concerns that outcome and control variables in our subsidiary sample are simultaneously affected by the policy change, we use pre-period covariates in a robustness test (Lester and Olbert 2024). We further test the robustness of our results by delaying the treatment timing to 2019 when the regulation became formally effective, omitting the years 2016 to 2018 because of anticipation effects. In additional robustness tests, we use alternative outcome variables, modify the control group, and include industry-year fixed effects.

4.5 Empirical Results

4.5.1 Location Response

Parallel Trends. We begin by examining whether MNEs alter their location decisions in response to the introduction of CFC rules under the ATAD. Figure 13 (a) displays the coefficients for our event-study DiD specification, with the share of CFC subsidiaries as the dependent variable. In the pre-ATAD adoption years (2012-2015), the coefficients are insignificant, indicating parallel trends in the share of CFC subsidiaries in the treatment and control group, thus meeting the identifying assumption of our DiD design. Following the ATAD implementation, we observe a declining trend in the share of CFC subsidiaries starting in 2017. Treated MNEs exhibit a delayed response, significantly reducing their share of CFC subsidiaries compared to the control group. Notably, this response occurs before the actual enforcement of CFC rules in 2019, supporting our assertion that MNEs could anticipate the forthcoming regulations.

The results for the share of non-CFC subsidiaries are depicted in Figure 13 (b). The coefficients remain stable and insignificant before the ATAD adoption. After the ATAD implementation, we observe increasing coefficient estimates on the share of non-CFC sub-

sidiaries, offsetting the concurrent decrease in CFC subsidiaries. Again, the response to the implementation of CFC rules occurs in the period preceding the actual enforcement.

Results. Our regression results presented in Table 16 confirm these findings. Columns (1) and (2) display the results for the share of CFC subsidiaries with and without the inclusion of home-country and group control variables. In both specifications, the coefficient of the interaction term is economically and statistically significantly negative. Incorporating controls, the coefficient on the DiD indicator MNE $\text{Treat}_m \times \text{Post}_t$ is -0.2041 and suggests that the share of CFC subsidiaries in the treatment group decreases by 18.46% relative to the control group. The observed decline indicates that affected MNEs react to the adoption of the ATAD by closing subsidiaries that would come under the purview of CFC taxation from 2019 onwards. Consequently, the results *Hypothesis 1a* and are in line with prior literature (Clifford 2019).

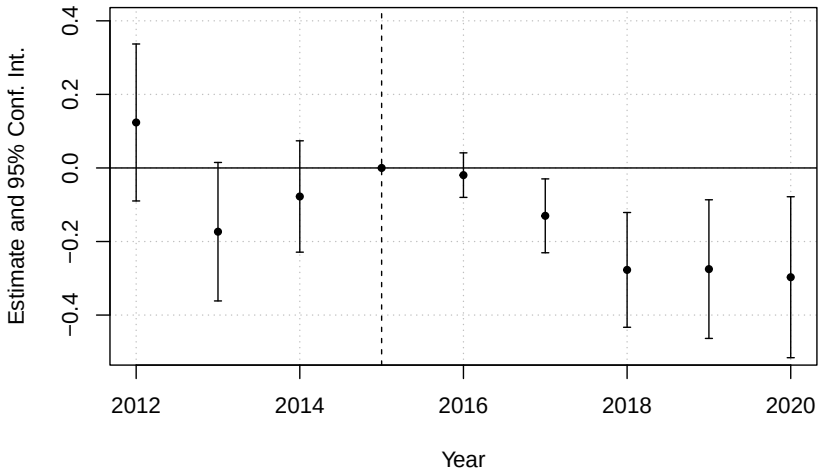
Columns (3) and (4) present the regression results for the share of non-CFC subsidiaries. In Column (3), no control variables are included, while control variables are added in Column (4). The coefficient of the interaction term is positive but insignificant in both cases. Although we see a slight increase in the share of non-CFC subsidiaries in the event-study design, this result suggests that the share of non-CFC subsidiaries in affected MNEs does not exhibit significant changes after the implementation of the ATAD.

Robustness. We conduct several tests to show the robustness of our results. Table 17 shows the results for the robustness tests when considering the share of CFC subsidiaries as dependent variable.[85] First, we include industry-year fixed effects to absorb differential time trends across industries. Column (1) shows that the coefficient on the interaction term remains similar in size and statistical significance compared to our baseline regression estimates.

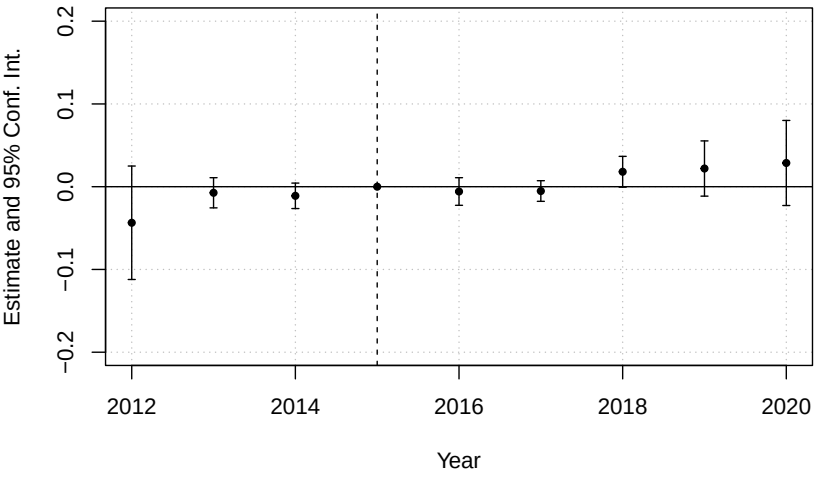
Second, we use an alternative dependent variable, specifically the number of a group's

[85] The corresponding tests for non-CFC subsidiaries are depicted in Appendix Table C.4.

Figure 13: Location response



(a) Share of CFC subsidiaries



(b) Share of non-CFC subsidiaries

Note: This figure shows event-study DiD estimates for the effect of the ATAD's CFC rules on the share of (a) CFC subsidiaries and (b) non-CFC subsidiaries to total subsidiaries estimated with PPML. The effect is relative to the control group. The specifications include the control variables presented in Equation (1), group fixed effects and year fixed effects. Standard errors are clustered at the MNE level. 95% confidence intervals are presented by the black lines.

Table 16: Location response

	Share of CFC subsidiaries		Share of non-CFC subsidiaries	
	(1)	(2)	(3)	(4)
MNE Treat x Post	-0.1939***	-0.2041***	0.0175	0.0208
	(0.0580)	(0.0697)	(0.0140)	(0.0185)
Home-country GDP		0.0000**		-0.0000
		(0.0000)		(0.0000)
Home-country GDPPC		0.0000		-0.0000
		(0.0000)		(0.0000)
Home-country CIT		-0.0307**		0.0030*
		(0.0148)		(0.0017)
Group total assets		-0.0000		0.0000
		(0.0000)		(0.0000)
Group leverage		0.1753*		-0.0105
		(0.1016)		(0.0108)
Group intangibility		0.1001		-0.0016
		(0.2366)		(0.0203)
Group foreign average CIT		-0.0735***		0.0055***
		(0.0104)		(0.0013)
Group FE	X	X	X	X
Year FE	X	X	X	X
Observations	19,873	15,161	19,790	15,185

Note: This table presents results of the DiD regressions estimated with PPML. The dependent variable is the share of CFC to total subsidiaries in Columns (1) and (2) and the share of non-CFC to total subsidiaries in Columns (3) and (4). The estimated effects are relative to the control group. The pre-treatment period spans from 2012 to 2015 while the post-treatment period spans from 2016 to 2020. MNE Treat is a dummy variable set to one for all MNEs in the treatment group and to zero for all MNEs in the control group. Robust standard errors adjusted for clustering at the MNE level are reported in parentheses. ***, ** and * indicate significance at the 1%, 5% and 10% level.

CFC subsidiaries. In Column (2), we present the regression results for this variable. The coefficient of the interaction term retains its negative sign and is statistical significant at the 1% level, mirroring the findings from our baseline analysis.

In Column (3), we adopt an alternative control group. The initial control group comprises EU member states that already had CFC rules in place before the ATAD and made

only minor adjustments to comply with the ATAD. Three countries, namely Germany, Denmark, and Spain, already had fully ATAD-compliant regulations and thus did not make any adjustments until 2021. To ensure that our results are robust and not influenced by treatment effects on the control group, we rerun our baseline regression using only observations from MNEs located in Germany, Denmark, and Spain as the control group. The resulting coefficient for the interaction term remains negative and statistically significant at the 1% level. Furthermore, the magnitude of the coefficient aligns with that of the baseline analysis, reinforcing the credibility of our results and suggesting that they are not driven by control group effects.

To isolate the effect occurring after the implementation of CFC rules, we modify the treatment timing in Column (4). More specifically, we delay the treatment timing until 2019, the year in which the CFC rules officially entered into force and exclude all observations in the years 2016, 2017, and 2018. Again, we find a negative and statistically significant effect for MNEs newly targeted by CFC rules.

Heterogeneity. When implementing the ATAD's CFC rules, EU member states had the flexibility to choose between model A and model B, as detailed in Section 4.2. These two models differ in the attribution of income to the parent company and the resulting taxation, as well as in the granted tax exemptions. While our baseline analysis encompasses MNEs from all EU member states, Appendix Table C.5 breaks down the treatment group's observations into those from model A and model B countries.[86] Columns (1) and (2) present the regression results for the share of CFC subsidiaries of MNEs located in model A and model B countries, respectively. The coefficient of the interaction term is negative in both regressions, however, only the coefficient for MNEs in model A countries is statis-

[86] We refrain from conducting a comprehensive sample split. All countries with pre-existing CFC rules before the ATAD adoption adhere to model A, except for Finland, France, Hungary, and Sweden. In a complete sample split, Finland and France would be excluded due to their CFC rules being a combination of both models A and B. Hungary and Sweden, having adopted model B, would represent the only two countries eligible for the control group, resulting in a limited number of observations.

Table 17: Location response: Robustness

	Share of CFC subsidiaries			
	Industry-year fixed effects	*Alternative dependent variable*	*Alternative control group*	*Delayed treatment*
	(1)	(2)	(3)	(4)
MNE Treat x Post	-0.3082***	-0.4920***	-0.2204***	-0.2996***
	(0.0589)	(0.1469)	(0.0574)	(0.1039)
Group controls	X	X	X	X
Home-country controls	X	X	X	X
Group FE	X	X	X	X
Year FE		X	X	X
Industry-year FE	X			
Observations	15,136	15,161	10,859	9,754

Note: This table presents robustness tests for the location response. The dependent variable is the share of CFC subsidiaries to total subsidiaries. In Column (1), we use industry-year fixed effects. Column (2) uses an alternative dependent variable, the number of CFC subsidiaries. In Column (3), we adjust the control group containing only countries that did not change their CFC rules until 2021, including Germany, Denmark and Spain. Column (4) changes the post-period to 2019 and 2020. All specifications include home-country and group control variables. The home-country controls comprise GDP, GDPPC and the CIT rate. As group controls, we include total assets, leverage, intangible assets scaled by total assets and the average CIT rate across all foreign subsidiaries. Robust standard errors adjusted for clustering at the MNE level are reported in parentheses. ***, ** and * indicate significance at the 1%, 5% and 10% level.

tically significant. This finding suggests that the choice of the CFC model does not alter the direction of the response by affected groups, observing a negative coefficient for both models. However, only MNEs in model A countries exhibit a significant reduction in the share of CFC subsidiaries. One possible explanation for the more pronounced reduction in the share of CFC subsidiaries of parent firms in model A countries can be attributed to the specification of the income that falls under CFC rules. While the ATAD clearly specifies that passive income falls under the provision under model A, the definition for model B remains somewhat unspecific. Further, the economic activity exemption introduced with model A states clearly whether a subsidiary and its parent company fall under the obligation to pay

CFC taxes. As a result, MNEs in model A countries could clearly anticipate which of its subsidiaries will be targeted by the new regulation and close these in response.

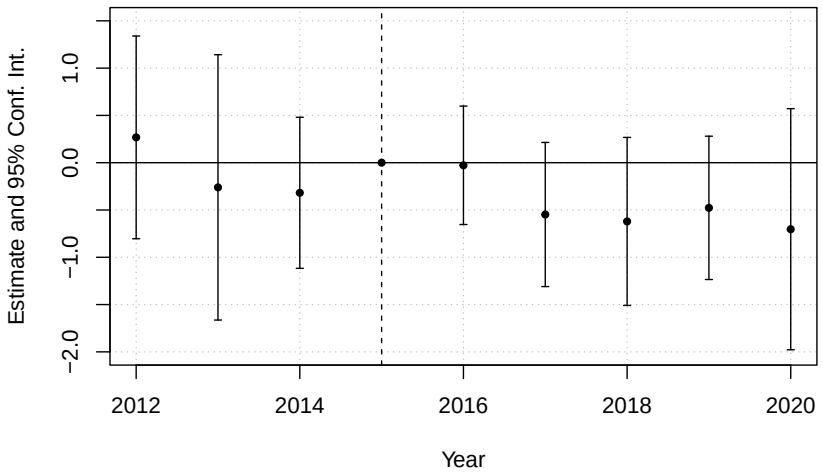
4.5.2 Financial Response

Parallel Trends. We turn our attention to the reporting response at the subsidiary level. While we observe that affected MNEs decrease their share of CFC subsidiaries, it remains unclear whether these MNEs also adjust the degree of income shifting within the CFC subsidiaries that continue to operate. Given that additional taxation under the ATAD's CFC rules only applies to subsidiaries' passive income (under model A) or income from non-genuine arrangements (under model B), we use financial income as a proxy. Moreover, financial income is often regarded as mobile and closely linked to profit shifting activities.

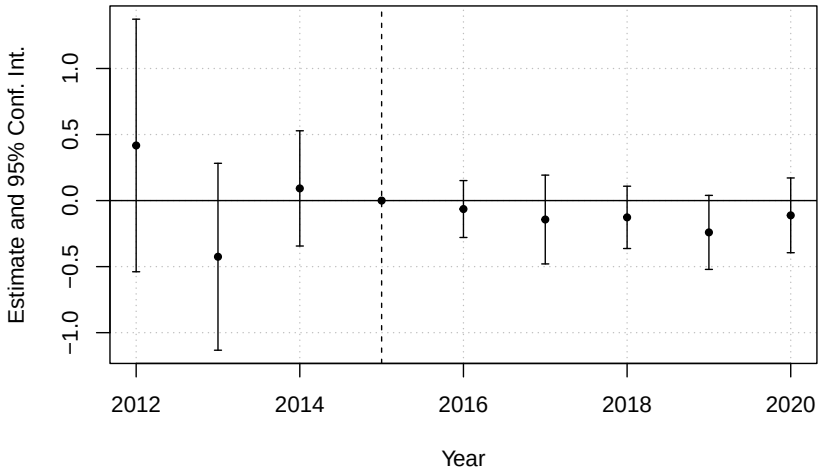
Figure 14 illustrates the dynamic effects on financial income for CFC and non-CFC subsidiaries, respectively, estimated from the event-study specification. The coefficients before the ATAD implementation are insignificant, indicating that financial income evolves in parallel and meeting the identifying assumption of our DiD design. Following the adoption of the ATAD, financial income of treated (non-)CFC subsidiaries does not change significantly relative to the control group.

Results. The results from our standard DiD design are presented in Table 18. Columns (1) and (2) depict the effects on the financial income of CFC subsidiaries, while Columns (3) and (4) show the results for non-CFC subsidiaries. For both CFC subsidiaries and non-CFC subsidiaries, the coefficients are negative but lack statistical significance confirming the null result documented above. In contrast to earlier studies that find a significant drop in financial income in response to CFC rules (Prettl 2017; Clifford 2019), we observe no significant change in subsidiaries' income reporting behavior in response to the implementation of CFC rules through the ATAD. Our results suggest that the financial income of persisting CFC subsidiaries has not undergone significant adjustments in response to the

Figure 14: Financial response



(a) CFC subsidiaries



(b) Non-CFC subsidiaries

Note: This figure shows event-study DiD estimates for the effect of the ATAD's CFC rules on financial income of (a) CFC subsidiaries and (b) non-CFC subsidiaries estimated with PPML. The effect is relative to the control group. The specifications include the control variables presented in Equation (2), subsidiary fixed effects and year fixed effects. Standard errors are clustered at the subsidiary level. 95% confidence intervals are presented by the black lines.

policy change. Moreover, the financial income of non-CFC subsidiaries does not exhibit a significant reaction to the implementation of CFC rules either. Consequently, we cannot confirm our *Hypotheses 2a* and *2b*, which propose that MNEs might redirect financial income from CFC subsidiaries to non-CFC subsidiaries.

Robustness. We perform several robustness tests, as outlined in Appendix Table C.6 for CFC subsidiaries and in Appendix Table C.7 for non-CFC subsidiaries. In Columns (1) of both tables, we use industry-year fixed effects instead of year fixed effects. In the regressions in Columns (2), we replace the time-varying control variables at the subsidiary level with constant pre-treatment averages to mitigate concerns that our outcome and control variables are simultaneously affected. In Columns (3), we use financial profit and loss as the dependent variable instead of the financial income. To ensure the robustness of our results to variations in the control group, we adopt more comprehensive control groups in Columns (4) containing all unaffected subsidiaries, irrespective of their classification as CFC or non-CFC. This means that the control group used in Columns (4) of both tables remains consistent among the CFC and non-CFC analysis. In Columns (5), we construct a new treatment dummy based on subsidiaries' EATRs[87] to validate that the treatment dummy definition based on statutory CIT rates does not drive our results. Lastly, we postpone the treatment time to 2018 in Columns (6).

Regarding CFC subsidiaries, the robustness tests largely align with the baseline results. In most specifications, the coefficient estimates are negative and statistically insignificant. Only the coefficients in Columns (1) and (5) are insignificant positive. For non-CFC subsidiaries, the estimates are consistently negative and mostly insignificant. Overall, the tests demonstrate that our baseline results are robust to variations in the empirical setting.

[87] The EATRs are calculated using the Devereux/Griffith methodology and are taken from `https://www.zew.de/mannheim-tax-index#c14749`.

Table 18: Financial response

	Financial income			
	CFC subsidiaries		*Non-CFC subsidiaries*	
	(1)	(2)	(3)	(4)
CFC Treat x Post	-0.3583	-0.3484		
	(0.2178)	(0.3851)		
Non-CFC Treat x Post			-0.1191	-0.1803
			(0.1227)	(0.1427)
Host-country GDP		0.0000		-0.0000
		(0.0000)		(0.0000)
Host-country GDPPC		-0.0000		0.0000
		(0.0000)		(0.0000)
Host-country CIT		-0.0045		0.0197
		(0.0559)		(0.0179)
Host-country unemployment rate		-0.0159		0.0152
		(0.0816)		(0.0150)
Host-country CPI		-0.0385		0.0165**
		(0.0436)		(0.0075)
Subsidiary total assets		0.0000		0.0000***
		(0.0000)		(0.0000)
Subsidiary number of employees		-0.0000		0.0000**
		(0.0002)		(0.0000)
Subsidiary operating income		-0.0000		-0.0000
		(0.0000)		(0.0000)
Subsidiary FE	X	X	X	X
Year FE	X	X	X	X
Observations	43,831	26,390	2,523,207	1,280,758

Note: This table presents results of the DiD regressions estimated with PPML. The dependent variable is the financial income of CFC subsidiaries in Columns (1) and (2) and of non-CFC subsidiaries in Columns (3) and (4). The estimated effects are relative to the control group. The pre-treatment period spans from 2012 to 2015 while the post-treatment period spans from 2016 to 2020. (Non-)CFC Treat is a dummy variable set to one for all subsidiaries in the treatment group and to zero for all subsidiaries in the control group. Robust standard errors adjusted for clustering at the subsidiary level are reported in parentheses. ***, ** and * indicate significance at the 1%, 5% and 10% level.

Heterogeneity. In Appendix Table C.8, when differentiating between CFC subsidiaries affiliated with MNEs from model A or model B countries, we observe a nuanced response based on the model that applies. Albeit statistically insignificant, the negative coefficient in Column (1) indicates a decrease in financial income of subsidiaries affiliated to model A countries while the positive coefficient in Column (2) suggests that financial income of model B-affiliated subsidiaries rather increases. Conversely, we obtain a significant negative coefficient for non-CFC subsidiaries associated with MNEs from model B countries (Column (4)), while the coefficient for non-CFC subsidiaries linked to MNEs from model A countries is insignificantly positive (Column (3)). The signs of the coefficient estimates suggest that MNEs located in model A countries tend to respond to the introduction of CFC rules by shifting financial income from CFC subsidiaries to non-CFC subsidiaries.

4.5.3 Economic Activity

Parallel Trends. Prior research has demonstrated that anti-tax avoidance measures can impact not only companies' financial positions but also lead to adverse real effects (Jacob 2022; Lester and Olbert 2024). Our null findings on financial income and the exemptions that have been implemented under the ATAD's CFC rules raise the question of whether MNEs are avoiding the application of CFC rules by ensuring that their CFC subsidiaries engage in genuine economic activities. Subsidiaries with substantial economic activity are exempt from CFC rules under model A, while model B favors subsidiaries with economic activity by applying CFC rules exclusively to non-genuine arrangements. To address this question, we examine investment and employment responses, measured by tangible fixed assets and the costs of employees, within both CFC and non-CFC subsidiaries.

Figure 15 depicts the event-study graphs for the investment effects of CFC rules for CFC and non-CFC subsidiaries. For both types of subsidiaries, the pre-treatment coefficients are insignificant, aligning with the parallel trends assumption. In the years following the ATAD adoption (from 2016 onward), the coefficients for CFC subsidiaries slightly

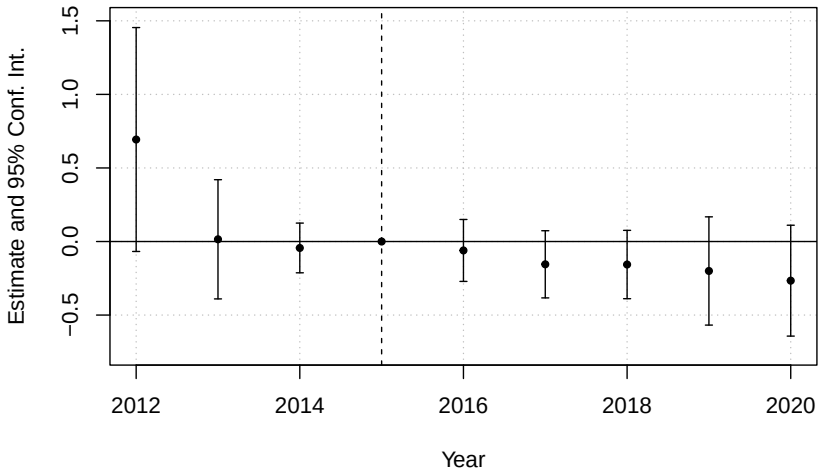
decrease, however, still lack statistical significance. For non-CFC subsidiaries, we observe non-significant effects around zero.

The event-study plots for employment are shown in Figure 16. Considering CFC subsidiaries, we find insignificant coefficients in the pre-regulation years, supporting the parallel trends assumption. In the post-ATAD adoption years, the coefficients increase and are statistically significant. This suggests that employee-related costs increase among treated CFC subsidiaries compared to control CFC subsidiaries. For non-CFC subsidiaries, we observe insignificant coefficients in the pre-ATAD period. In the post-period, we observe a decrease in non-CFC subsidiaries' costs of employees, which is however only borderline significant.

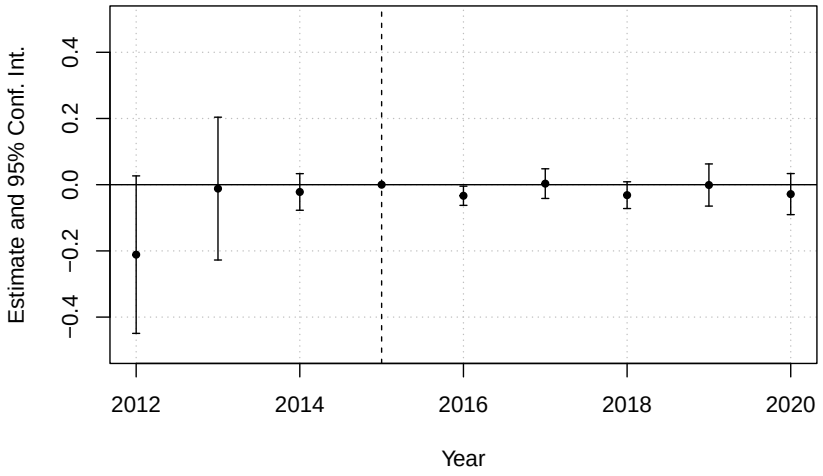
Results. We present the results of our baseline DiD specification in Table 19. Panel A displays the regression outcomes for investment, utilizing tangible fixed assets as the outcome variable, while Panel B illustrates the results for employment, using costs of employees as the outcome variable. Employing our more comprehensive specification including control variables, we do not observe a significant effect on the level of tangible fixed assets of CFC subsidiaries. In contrast, for employment, we document a positive and significant coefficient on the interaction term. Our more sophisticated estimation in Panel B, Column (2), suggests a rise in employee costs by 8.50% for affected CFC subsidiaries relative to unaffected ones. These findings support the notion that affected CFC subsidiaries tend to boost employment costs as a strategy to navigate around CFC rule application and thus contradict findings from earlier literature when CFC rules where not adopted collectively (Ruf and Weichenrieder 2012; Egger and Wamser 2015; Prettl 2017).

Regarding tangible fixed assets of non-CFC subsidiaries in Panel A, the coefficients are statistically insignificant and economically small. Moreover, the signs of the coefficients are mixed depending on whether control variables are included in the regression. For employment, we observe negative coefficients for costs of employees of non-CFC sub-

Figure 15: Economic activity: Investment



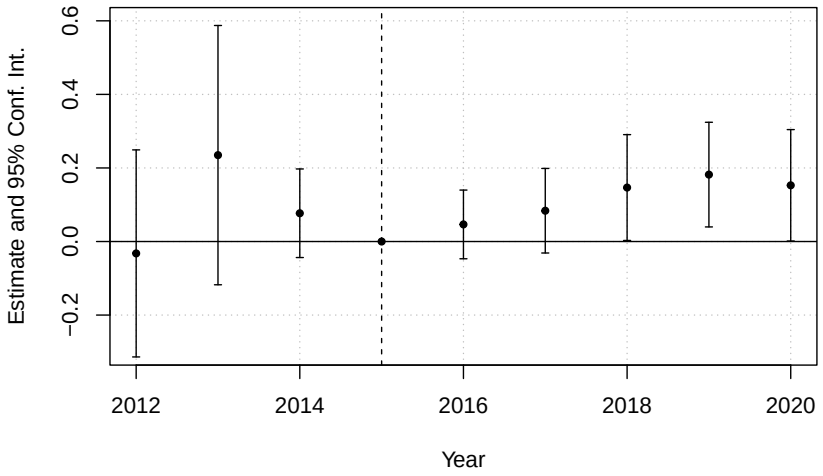
(a) CFC subsidiaries



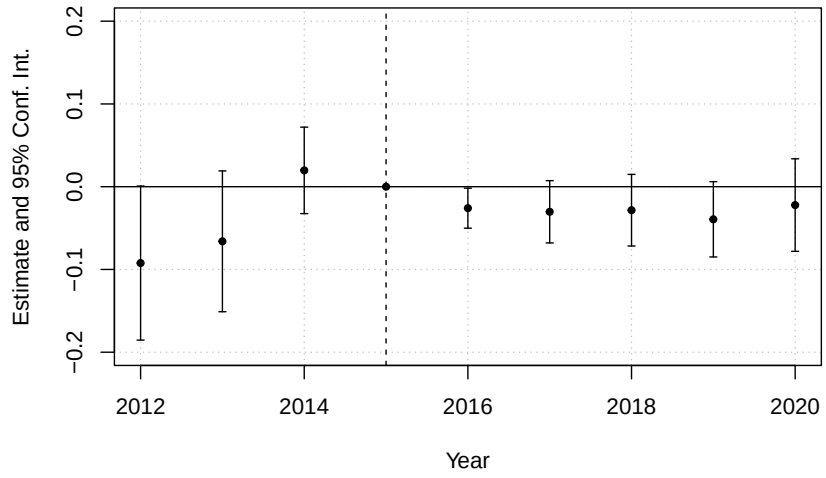
(b) Non-CFC subsidiaries

Note: This figure shows event-study DiD estimates for the effect of the ATAD's CFC rules on tangible fixed assets of (a) CFC subsidiaries and (b) non-CFC subsidiaries estimated with PPML. The effect is relative to the control group. The specifications include the control variables presented in Equation (2), subsidiary fixed effects and year fixed effects. Standard errors are clustered at the subsidiary level. 95% confidence intervals are presented by the black lines.

Figure 16: Economic activity: Employment



(a) CFC subsidiaries



(b) Non-CFC subsidiaries

Note: This figure shows event-study DiD estimates for the effect of the ATAD's CFC rules on costs of employees of (a) CFC subsidiaries and (b) non-CFC subsidiaries estimated with PPML. The effect is relative to the control group. The specifications include the control variables presented in Equation (2), subsidiary fixed effects and year fixed effects. Standard errors are clustered at the subsidiary level. 95% confidence intervals are presented by the black lines.

sidiaries. This finding indicates that MNEs may be reallocating employment costs from their non-CFC subsidiaries to their CFC subsidiaries. However, the findings also reveal that affected MNEs did not change investment in their subsidiaries relative to unaffected MNEs. Our findings are consistent with previous studies investigating investment and employment effects of tax rules in distinct settings, which provide initial evidence that labor may be more mobile than capital (Giroud and Rauh 2019; De Simone and Olbert 2022). Additionally, we confirm prior work indicating that low foreign tax rates not only attract employment but also enable multinationals to engage in more aggressive income shifting, as the presence of labor, and in our case labor costs, helps to substantiate tax planning strategies (Williams 2018; De Simone et al. 2022; De Simone and Olbert 2022; Drake et al. 2022).

Robustness. To validate our results on economic activity, we perform several robustness tests. We report the results for CFC subsidiaries in Tables 21 and 22, for investment and employment respectively.[88] In Column (1), we replace year fixed effects by industry-year fixed effects. The estimates for investment and employment are in line with the baseline results, with the coefficient for employee costs remaining positive and significant at the 10% level.

To address the concern that our outcome and control variables are simultaneously affected by the policy change, we run our baseline specification including pre-treatment average subsidiary-level covariates (Lester and Olbert 2024). Column (2) shows that our results are robust to this specification.

We use alternative dependent variables in Column (3). We examine the effect on total assets in the investment analysis and the impact on the number of employees in the employment analysis. We find a negative and insignificant effect for total assets. Interestingly, the coefficient for the number of employees is negative, indicating that treated CFC

[88] Appendix Tables C.9 and C.10 presents the results of the robustness tests for non-CFC subsidiaries.

Table 19: Economic activity: Investment

	Fixed assets			
	CFC subsidiaries		Non-CFC subsidiaries	
	(1)	(2)	(3)	(4)
CFC Treat x Post	-0.2199*	-0.1704		
	(0.1147)	(0.1214)		
Non-CFC Treat x Post			0.0444	-0.0059
			(0.0499)	(0.0271)
Host-country GDP		-0.0000***		0.0000
		(0.0000)		(0.0000)
Host-country GDPPC		0.0000**		0.0000
		(0.0000)		(0.0000)
Host-country CIT		-0.0050		0.0026
		(0.0067)		(0.0072)
Host-country unemployment rate		0.0032		0.0083
		(0.0117)		(0.0058)
Host-country CPI		0.0080*		-0.0071
		(0.0048)		(0.0065)
Subsidiary operating income		0.0000***		0.0000***
		(0.0000)		(0.0000)
Subsidiary number of employees		0.0001		0.0000***
		(0.0000)		(0.0000)
Subsidiary FE	X	X	X	X
Year FE	X	X	X	X
Observations	68,077	29,835	4,139,128	1,775,562

Note: This table presents results of the DiD regressions estimated with PPML. The dependent variable is tangible fixed assets. Columns (1) and (2) depict the results for CFC subsidiaries while Columns (3) and (4) show the results for non-CFC subsidiaries. The estimated effects are relative to the control group. The pre-treatment period spans from 2012 to 2015 while the post-treatment period spans from 2016 to 2020. (Non-)CFC Treat is a dummy variable set to one for all subsidiaries in the treatment group and to zero for all subsidiaries in the control group. Robust standard errors adjusted for clustering at the subsidiary level are reported in parentheses. ***, ** and * indicate significance at the 1%, 5% and 10% level.

subsidiaries increase personnel costs, possibly through higher salaries or workforce adjust-

Table 20: Economic activity: Employment

	Costs of employees			
	CFC subsidiaries		*Non-CFC subsidiaries*	
	(1)	(2)	(3)	(4)
CFC Treat x Post	0.1224**	0.0816*		
	(0.0607)	(0.0481)		
Non-CFC Treat x Post			-0.0491*	-0.0171
			(0.0287)	(0.0240)
Host-country GDP		-0.0000		0.0000**
		(0.0000)		(0.0000)
Host-country GDPPC		-0.0000		-0.0000
		(0.0000)		(0.0000)
Host-country CIT		-0.0014		0.0121***
		(0.0045)		(0.0026)
Host-country unemployment rate		-0.0555***		-0.0071*
		(0.0114)		(0.0040)
Host-country CPI		0.0093***		0.0024*
		(0.0030)		(0.0012)
Subsidiary operating income		0.0000**		0.0000*
		(0.0000)		(0.0000)
Subsidiary total assets		0.0000***		-0.0000
		(0.0000)		(0.0000)
Subsidiary FE	X	X	X	X
Year FE	X	X	X	X
Observations	36,337	28,310	2,365,425	1,753,770

Note: This table presents results of the DiD regressions estimated with PPML. The dependent variable is the costs of employees. Columns (1) and (2) depict the results for CFC subsidiaries while Columns (3) and (4) show the results for non-CFC subsidiaries. The estimated effects are relative to the control group. The pre-treatment period spans from 2012 to 2015 while the post-treatment period spans from 2016 to 2020. (Non-)CFC Treat is a dummy variable set to one for all subsidiaries in the treatment group and to zero for all subsidiaries in the control group. Robust standard errors adjusted for clustering at the subsidiary level are reported in parentheses. ***, ** and * indicate significance at the 1%, 5% and 10% level.

ments, while the total number of employees decreases slightly.[89] This finding underlines

[89] This finding is consistent with the literature on intellectual property (IP) box regimes. For instance, Chen et al. 2023 find no change in the average number of employees in IP-box relative to non-IP box countries.

the earlier observation that companies do not significantly alter their business operations to substantiate economic activity and avoid CFC rules. Instead, they adopt a less intensive approach. Rather than increasing the number of employees in entities subject to CFC regulation, they reallocate or enhance compensation levels, reflecting changes in the labor mix.

In Column (4) we use the same more comprehensive control group that we already implemented in the robustness tests for the financial response (see Section 4.5.2). The results for the more comprehensive control group are in line with the baseline results for both the investment and employment analysis.

To mitigate concerns that our results are driven by the way how the treatment dummy is defined, we use an alternative treatment dummy that is based on the EATR (see Section 4.5.2) in the specifications in Column (5). The effect on investment remains insignificant. For employment, the coefficient does not change compared to the baseline result and is statistically significant.

In Column (6), we delay the treatment timing to isolate the effects that occur after the CFC rules came officially into force. The coefficient for employment is significantly positive, indicating substantial adjustments by affected CFC subsidiaries post-2018. The larger coefficient magnitude compared to our baseline specification aligns with our expectations, considering that substantial economic activity was mandated only from the date on which the CFC regulations were officially enforced.

Heterogeneity. In Appendix Tables C.11 and C.12 we split the treated subsidiaries into those affiliated with MNEs in model A and model B countries. We find that the positive effect on the costs of employees is more pronounced for CFC subsidiaries in model A countries. However, for model B, we observe reversing results for both CFC and non-CFC subsidiaries regarding the costs of employees. While there is an increase in labor costs

Instead, they find an increase in labor costs. Additionally, Bornemann et al. 2023 find that the number of skilled employees increases following the introduction of an IP box regime.

for CFC subsidiaries, there is a decrease in labor costs for non-CFC subsidiaries. Although the coefficients align in size, they are not statistically significant. This might indicate some shifting of employee costs from non-CFC to CFC subsidiaries occurring in MNEs with parent firms located in model B countries.

The distinct response between model A and model B groups can be explained by the differences between the models. Model A exempts subsidiaries with substantive economic activity from additional CFC taxation at the parent company level. To fall under this exemption, MNEs in model A countries increase their employment costs in CFC subsidiaries without significantly changing the rest of their business model, opting for the least intensive approach to substantiate economic presence. On the other hand, Model B applies CFC taxation only to non-genuine arrangements. In response, MNEs in model B countries rearrange their business model by shifting some of their labor from CFC to non-CFC subsidiaries, thereby changing the structure of their business model to genuine arrangements in CFC countries. Overall, the heterogeneous effects for the two CFC models show that both models open up the possibility for MNEs to circumvent CFC rule application by substantiating economic activity.

Table 21: Economic activity: Robustness investment

	Fixed assets – CFC subsidiaries					
	Industry-year fixed effects	*Pre-treatment controls*	*Alternative dependent variable*	*Comprehensive control group*	*Alternative treatment definition*	*Delayed treatment*
	(1)	(2)	(3)	(4)	(5)	(6)
CFC Treat x Post	-0.1785	-0.1919	-0.1316	-0.1487	0.0980	-0.2199
	(0.1493)	(0.1235)	(0.1137)	(0.1214)	(0.1924)	(0.1748)
Subsidiary controls	X	X	X	X	X	X
Host-country controls	X	X	X	X	X	X
Subsidiary FE	X		X	X	X	X
Year FE		X	X	X	X	X
Industry-year FE	X					
Observations	29,327	31,120	33,191	1,818,253	33,188	17,659

Note: This table presents robustness tests for the economic activity of CFC subsidiaries. The dependent variable is tangible fixed assets. In Column (1), we use industry-year fixed effects. In Column (2), we replace the time-variant subsidiary-level controls with constant pre-treatment averages. Column (3) uses an alternative dependent variable, total assets. In Column (4), we use a comprehensive control group that contains all unaffected subsidiaries, regardless of whether they classify as CFC or non-CFC. Column (5) uses an adjusted treatment definition based on the EATR instead of the statutory CIT rate. Column (6) changes the post-period to 2019 and 2020. All specifications include host-country and subsidiary control variables. The host-country controls comprise GDP, GDPPC, CIT rate, unemployment rate and CPI. As subsidiary controls, we include number of employees and operating revenue. Robust standard errors adjusted for clustering at the subsidiary level are reported in parentheses. ***, ** and * indicate significance at the 1%, 5% and 10% level.

Table 22: Economic activity: Robustness employment

	Costs of employees – CFC subsidiaries					
	Industry-year fixed effects	*Pre-treatment controls*	*Alternative dependent variable*	*Comprehensive control group*	*Alternative treatment definition*	*Delayed treatment*
	(1)	(2)	(3)	(4)	(5)	(6)
CFC Treat x Post	0.0872*	0.1062*	-0.0863*	0.2267***	0.1201***	0.1593**
	(0.0480)	(0.0576)	(0.0449)	(0.0495)	(0.0420)	(0.0699)
Subsidiary controls	X	X	X	X	X	X
Host-country controls	X	X	X	X	X	X
Subsidiary FE	X		X	X	X	X
Year FE		X	X	X	X	X
Industry-year FE	X					
Observations	28,031	28,568	28,644	1,805,660	32,601	17,313

Note: This table presents robustness tests for the economic activity of CFC subsidiaries. The dependent variable is costs of employees. In Column (1), we use industry-year fixed effects. In Column (2), we replace the time-variant subsidiary-level controls with constant pre-treatment averages. Column (3) uses an alternative dependent variable, the number of employees. In Column (4), we use a comprehensive control group that contains all unaffected subsidiaries, regardless of whether they classify as CFC or non-CFC. Column (5) uses an adjusted treatment definition based on the EATR instead of the statutory CIT rate. Column (6) changes the post-period to 2019 and 2020. All specifications include host-country and subsidiary control variables. The host-country controls comprise GDP, GDPPC, CIT rate, unemployment rate and CPI. As subsidiary controls, we include total assets and operating revenue. Robust standard errors adjusted for clustering at the subsidiary level are reported in parentheses. ***, ** and * indicate significance at the 1%, 5% and 10% level.

4.6 Discussion and Conclusion

This study investigates the location, financial, and economic activity responses of EU-based MNEs following the widespread implementation of CFC rules due to the ATAD. By employing a DiD design, we compare newly treated MNEs and their subsidiaries to those that were already subject to CFC rules prior to the ATAD, both before and after the ATAD adoption in 2016. We hypothesize that the implementation of CFC rules increases the costs of income shifting and thereby reduce income shifting. However, the policy change may also cause increases in economic activity given the newly introduced exemptions.

Our findings provide evidence of MNEs decreasing the share of subsidiaries subject to the newly implemented CFC regulations and reallocating their economic activity. These results align with the intended goals of reducing profit shifting and better aligning economic activity with taxation, which are the primary objectives of the ATAD. However, the observed adjustments suggest that MNEs tend to opt for simple ways to substantiate economic activity in low-tax countries, primarily by adjusting the costs of employees rather than altering the number of employees or investment. This trend contradicts the intended goals of the EU member states and argues in favor of MNEs likely continuing to maximize shareholder value by lowering tax costs. Furthermore, our analysis does not identify statistically significant evidence of MNEs rerouting financial income in response to the introduction of CFC rules. Our results remain robust across various alternative specifications. While we acknowledge data limitations associated with using Orbis data, we anticipate that future research with larger samples and administrative tax data can explore additional outcomes related to corporate responses to CFC rules.

Our comprehensive and simultaneous exploration of multinationals' reporting and economic responses at both the group and subsidiary levels in response to taxes and in particular anti-tax avoidance regulations is novel (Lester and Olbert 2024). We further address the call for more research on the real effects of new anti-tax avoidance measures (Riedel 2018; Jacob 2022; Dyreng and Hanlon 2023). Our evidence suggests that the

widespread introduction of CFC rules in the EU was only partly effective in reducing income shifting. We interpret our results as indicative of MNEs substantiating their tax avoidance strategies by increasing economic activities in countries with preferential tax regimes, while still opting for the least intensive approach. Simultaneously, firms reduce the number of subsidiaries in low-tax countries. Our findings have important policy implications guiding the implementation of effective anti-tax avoidance regulations.

5 Summary of Main Findings

Can taxes shape the sustainable transformation of businesses? This dissertation provides new empirical evidence on three critical dimensions of how tax policies influence businesses' sustainable development.

Chapter Two investigates how tax policy tools can combat payroll tax evasion, a significant challenge for governments and tax authorities. Payroll tax evasion is difficult to detect due to its collusive nature, where both employers and employees have an incentive to remain silent. This issue not only affects government budgets and fiscal sustainability but also impacts the redistributive function of taxation, with social security payments being directly affected. Thus, improving payroll tax compliance is an essential dimension of how taxes can shape businesses' sustainability.

Conducting a field experiment across firms in Bulgaria, we find that both deterrence measures (audit probabilities) and moral appeals effectively break up collusion and improve firms' tax compliance. While different moral messages have comparable effects on compliance, firms are more responsive to deterrence messages with higher audit probabilities. Overall, our experiment generated an additional USD 10,856,280 in tax revenue for the Bulgarian tax authority, closing 5% of the estimated revenue gap (Williams and Horodnic 2017) and covering the yearly pensions of 5,210 individuals during the time of the experiment.

These findings demonstrate that tax enforcement actions are a promising tool to enhance tax compliance and sustainability among firms. Therefore, ongoing research and policy efforts must continue to identify strategies to combat tax evasion, and our study contributes to this by presenting successful enforcement strategies in the context of payroll tax evasion.

Chapter Three focuses on the steering function of taxation, examining whether environmental taxes on diesel fuel promote sustainable transformation in the transportation sector. The transportation sector is responsible for 20% of global greenhouse gas emis-

sions and constitutes the only sector where emissions have continued to grow over the last decade (International Energy Agency 2022). Thus, finding effective policy tools to reduce emissions in this sector is crucial.

We evaluate the impact of environmental taxes on commercial truck traffic and its environmental consequences using comprehensive data on truck traffic, journeys, and emissions. First, we assess the general response of truck traffic to environmental taxes. Second, we investigate whether trucking companies avoid high environmental taxes by taking detours. Finally, we quantify the environmental consequences of these tax-induced reactions. We find that trucking companies significantly respond to environmental taxes on diesel. While we initially observe a decrease in cross-border truck traffic in response to increased taxes, there is no overall reduction in traffic. Instead, trucks take detours to refuel in countries with lower taxes, leading to increased mileage, emissions and air pollution.

Our findings show that while environmental taxes on fuel can influence commercial trucking businesses' sustainability, how these taxes are imposed, especially across countries, is crucial to achieving this goal. Our evidence suggests that harmonizing environmental tax rates across the EU could foster more sustainable practices in the transportation sector. We contribute to the critical discussion on environmental regulations aimed at reducing emissions to mitigate climate change.

Chapter Four examines the responses of MNEs to the EU-wide implementation of CFC rules designed to reduce tax avoidance by subjecting low-taxed foreign income to the domestic taxation of the parent company. This analysis addresses a third important dimension of how taxes can shape sustainability by evaluating the effectiveness of anti-tax avoidance regulations targeting multinational firms.

We combine ownership and financial data of MNEs to measure their location, financial, and real economic responses to the newly implemented CFC regulation. We find that CFC rules lead to a reduction in the share of low-taxed CFC subsidiaries and the reallo-

cation of economic activity. However, our results indicate that MNEs tend to substantiate economic activity by increasing employee costs rather than the number of employees or investments. This behavior contradicts the intended goals of the anti-tax avoidance measure, especially as we do not observe evidence of firms rerouting financial income in response to the CFC rules.

These findings suggest that the widespread implementation of CFC rules in the EU has been only partially effective in reducing tax avoidance. Our results have significant policy implications, guiding the implementation of effective anti-tax avoidance regulations to promote the sustainable development of businesses and achieving the SDGs.

References

Adhikari, B., Alm, J., and Harris, T. F. (2021). Small business tax compliance under third-party reporting. Journal of Public Economics 203, 104514. DOI: 10.1016/j.jpubeco.2021.104514.

Akcigit, U., Baslandze, S., and Stantcheva, S. (2016). Taxation and the International Mobility of Inventors. American Economic Review 106(10), 2930–2981. DOI: 10.1257/aer.20150237.

Allingham, M. G. and Sandmo, A. (1972). Income tax evasion: A theoretical analysis. Journal of Public Economics 1(3-4), 323–338. DOI: 10.1016/0047-2727(72)90010-2.

Alm, J. and Jackson, B. R. (1993). Fiscal exchange, collective decision institutions, and tax compliance. Journal of Economic Behavior and Organization 22(3), 285–303. DOI: 10.1016/0167-2681(93)90003-8.

Altshuler, R. and Hubbard, R. G. (2003). The effect of the tax reform act of 1986 on the location of assets in financial services firms. Journal of Public Economics 87(1), 109–127. DOI: 10.1016/S0047-2727(01)00174-8.

American Transportation Research Institute (2018). An Analysis of the Operational Costs of Trucking: 2018 Update.

Andersson, J. J. (2019). Carbon taxes and CO? emissions: Sweden as a case study. American Economic Journal: Economic Policy 11(4), 1–30. DOI: 10.1257/pol.20170144.

Antinyan, A. and Asatryan, Z. (2024). Nudging for Tax Compliance: A Meta-Analysis. Available at SSRN 3500744. DOI: 10.2139/ssrn.3500744.

Avi-Yonah, R. S. (2014). Corporate taxation and corporate social responsibility. New York University Journal of Law & Business 11(1).

Avi-Yonah, R. S. (2006). The three goals of taxation. Tax Law Review 60(1), 1–28.

Baker, A. C., Larcker, D. F., and Wang, C. C. (2022). How much should we trust staggered difference-in-differences estimates? Journal of Financial Economics 144(2), 370–395. DOI: 10.1016/j.jfineco.2022.01.004.

Bandiera, O., Barankay, I., and Rasul, I. (2011). Field Experiments with Firms. Journal of Economic Perspectives 25(3), 63–82. DOI: 10.1257/jep.25.3.63.

Banfi, S., Filippini, M., and Hunt, L. C. (2005). Fuel tourism in border regions: The case of Switzerland. Energy Economics 27(5), 689–707. DOI: 10.1016/j.eneco.2005.04.006.

Barrios, S., Huizinga, H., Laeven, L., and Nicodème, G. (2012). International taxation and multinational firm location decisions. Journal of Public Economics 96(11-12), 946–958. DOI: 10.1016/j.jpubeco.2012.06.004.

Basri, M. C., Felix, M., Hanna, R., and Olken, B. A. (2021). Tax Administration versus Tax Rates: Evidence from Corporate Taxation in Indonesia. American Economic Review 111(12), 3827–71. DOI: 10.1257/aer.20201237.

Battaglini, M., Guiso, L., Lacava, C., and Patacchini, E. (2020). Tax Professionals and Tax Evasion. NBER Working Paper 25745. DOI: 10.3386/w25745.

Becker, G. S. (1968). Crime and Punishment: An Economic Approach. Journal of Political Economy 76(2), 169–217. DOI: 10.1086/259394.

Bérgolo, M., Ceni, R., Cruces, G., Giaccobasso, M., and Perez-Truglia, R. (2023). Tax audits as scarecrows: Evidence from a large-scale field experiment. American Economic Journal: Economic Policy 15(1). DOI: 10.1257/pol.20200321.

Best, M., Shah, J., and Waseem, M. (2021). Detection Without Deterrence: Long-Run Effects of Tax Audit on Firm Behavior. Unpublished Paper.

Best, M. C. (2014). The role of firms in workers' earnings responses to taxes: Evidence from Pakistan. Unpublished Paper.

Bilicka, K., Devereux, M., and Güceri, I. (2023). Tax Policy, Investment and Profit-Shifting. Available at https://www.law.columbia.edu/sites/default/files/2023-03/BDG_2023_03_23.pdf.

Bilicka, K., Qi, Y., and Xing, J. (2022). Real responses to anti-tax avoidance: Evidence from the UK Worldwide Debt Cap. Journal of Public Economics 214, 104742. DOI: 10.1016/j.jpubeco.2022.104742.

Bird, R. and Davis-Nozemack, K. (2018). Tax avoidance as a sustainability problem. Journal of business ethics 151, 1009–1025. DOI: 10.1007/s10551-016-3162-2.

Bjorneby, M., Alstadsaeter, A., and Telle, K. (2021). Limits to third-party reporting: Evidence from a randomized field experiment in Norway. Journal of Public Economics 203, 104512. DOI: 10.1016/j.jpubeco.2021.104512.

Blanken, I., Ven, N. van de, and Zeelenberg, M. (2015). A meta-analytic review of moral licensing. Personality and Social Psychology Bulletin 41(4), 540–558. DOI: 10.1177/0146167215572134.

Bolton, G. E. and Ockenfels, A. (2000). ERC: A theory of equity, reciprocity, and competition. American Economic Review, 166–193.

Boning, W. C., Guyton, J., Hodge, R. H., Slemrod, J., Troiano, U., et al. (2020). Heard it through the grapevine: The direct and network effects of a tax enforcement field experiment on firms. Journal of Public Economics 190, 104261.

Bonnet, O., Fize, É., Loisel, T., and Wilner, L. (2024). Is Carbon Tax Truly More Salient? Evidence from Fuel Tourism at the France-Germany Border. Available at SSRN 4717742. DOI: 10.2139/ssrn.4717742.

Bornemann, T., Kelley Laplante, S. O., and Osswald, B. (2023). The Effect of Intellectual Property Boxes on Innovative Activity and Tax Benefits. Journal of the American Taxation Association 45(2), 7–33. DOI: 10.2308/JATA-2021-013.

Bott, K. M., Cappelen, A. W., Sørensen, E. Ø., and Tungodden, B. (2020). You ve got mail: A randomized field experiment on tax evasion. Management Science 66(7), 2801–2819. DOI: 10.1287/mnsc.2019.3390.

Brown, J. R., Ivković, Z., and Weisbenner, S. (2015). Empirical determinants of intertemporal choice. Journal of Financial Economics 116(3), 473–486.

Brown, M., Schmitz, J., and Zehnder, C. (2023). Moral constraints, social norm enforcement and strategic default in weak and strong economic conditions. Journal of Money, Credit and Banking. DOI: 10.1111/jmcb.13165.

Buchanan, J. M. (1976). Taxation in fiscal exchange. Journal of Public Economics 6(1), 17–29. DOI: 10.1016/0047-2727(76)90038-4.

Bütler, M. and Teppa, F. (2007). The choice between an annuity and a lump sum: Results from Swiss pension funds. Journal of Public Economics 91(10), 1944–1966. DOI: 10.1016/j.jpubeco.2007.09.003.

Carrillo, P., Pomeranz, D., and Singhal, M. (2017). Dodging the Taxman: Firm Misreporting and Limits to Tax Enforcement. American Economic Journal: Applied Economics 9(2), 144–64. DOI: 10.1257/app.20140495.

Chan, H. F., Dulleck, U., Fooken, J., Moy, N., and Torgler, B. (2023). Cash and the hidden economy: Experimental evidence on fighting tax evasion in small business transactions. Journal of Business Ethics, 1–26. DOI: 10.1007/s10551-022-05186-y.

Charness, G. and Rabin, M. (2002). Understanding social preferences with simple tests. The Quarterly Journal of Economics 117(3), 817–869.

Charness, G. and Sutter, M. (2012). Groups Make Better Self-Interested Decisions. Journal of Economic Perspectives 26(3), 157–76. DOI: 10.1257/jep.26.3.157.

Chaudhuri, A. (2011). Sustaining cooperation in laboratory public goods experiments: a selective survey of the literature. Experimental Economics 14(1), 47–83.

Chen, J. and Roth, J. (2023). Logs with Zeros? Some Problems and Solutions. The Quarterly Journal of Economics 139(2), 891–936. DOI: 10.1093/qje/qjad054.

Chen, S., De Simone, L., Hanlon, M., and Lester, R. (2023). The Effect of Innovation Box Regimes on Investment and Employment Activity. The Accounting Review 98(5), 187–214. DOI: 10.2308/TAR-2019-0338.

Chen, Y., Cramton, P., List, J. A., and Ockenfels, A. (2021). Market design, human behavior, and management. Management Science 67(9), 5301–5967. DOI: 10.1287/mnsc.2020.3659.

Clifford, S. (2019). Taxing multinationals beyond borders: Financial and locational responses to CFC rules. Journal of Public Economics 173, 44–71. DOI: 10.1016/j.jpubeco.2019.01.010.

Cohn, A., Fehr, E., and Maréchal, M. A. (2014). Business culture and dishonesty in the banking industry. Nature 516(7529), 86–89. DOI: 10.1038/nature13977.

Cohn, A., Fehr, E., and Maréchal, M. A. (2017). Do professional norms in the banking industry favor risk-taking? The Review of Financial Studies 30(11), 3801–3823. DOI: 10.1093/rfs/hhx003.

Cohn, J. B., Liu, Z., and Wardlaw, M. I. (2022). Count (and count-like) data in finance. Journal of Financial Economics 146(2), 529–551. DOI: 10.1016/j.jfineco.2022.08.004.

Davis, L. W. and Kilian, L. (2011). Estimating the effect of a gasoline tax on carbon emissions. Journal of Applied Econometrics 26(7), 1187–1214. DOI: 10.1002/jae.1156.

De Neve, J.-E., Imbert, C., Spinnewijn, J., Tsankova, T., and Luts, M. (2021). How to Improve Tax Compliance? Evidence from Population-Wide Experiments in Belgium. Journal of Political Economy 129(5), 1425–1463. DOI: 10.1086/713096.

De Simone, L., Klassen, K. J., and Seidman, J. K. (2022). The effect of income-shifting aggressiveness on corporate investment. Journal of Accounting and Economics 74(1), 101491. DOI: 10.1016/j.jacceco.2022.101491.

De Simone, L. and Olbert, M. (2022). Real Effects of Private Country-by-Country Disclosure. The Accounting Review 97(6), 201–232. DOI: 10.2308/TAR-2020-0714.

Dechezleprêtre, A., Gennaioli, C., Martin, R., Muûls, M., and Stoerk, T. (2022). Searching for carbon leaks in multinational companies. Journal of Environmental Economics and Management 112, 102601. DOI: 10.1016/j.jeem.2021.102601.

Dharmapala, D. (2019). Profit Shifting in a Globalized World. AEA Papers and Proceedings 109, 488–492. DOI: 10.1257/pandp.20191043.

Djankov, S., Ganser, T., McLiesh, C., Ramalho, R., and Shleifer, A. (2010). The Effect of Corporate Taxes on Investment and Entrepreneurship. American Economic Journal: Macroeconomics 2(3), 31–64. DOI: 10.1257/mac.2.3.31.

Döerrenberg, P. and Schmitz, J. (2017). Tax compliance and information provision - A field experiment with small firms. Journal of Behavioral Economics for Policy 1(1), 47–54.

Dörrenberg, P., Pfrang, A., and Schmitz, J. (2024). How to improve small firms' payroll tax compliance? Evidence from a randomized field experiment. Available at SSRN 4130097. DOI: 10.2139/ssrn.4130097.

Drake, K. D., Goldman, N. C., and Murphy, F. (2022). Foreign Employment, Income Shifting, and Tax Uncertainty. The Accounting Review 97(2), 183–212. DOI: 10.2308/TAR-2019-0047.

Dussaux, D. (2020). The joint effects of energy prices and carbon taxes on environmental and economic performance: Evidence from the French manufacturing sector. Available at OECD Environment Working Papers 154. DOI: 10.1787/b84b1b7d-en.

Dwenger, N., Kleven, H., Rasul, I., and Rincke, J. (2016). Extrinsic and intrinsic motivations for tax compliance: Evidence from a field experiment in Germany. American Economic Journal: Economic Policy 8(3), 203–32.

Dwenger, N. and Treber, L. (2022). Shaming for tax enforcement. Management Science 68(11), 8202–8233. DOI: 10.1287/mnsc.2021.4295.

Dyreng, S. and Hanlon, M. (2023). Tax Avoidance and Multinational Firm Behavior. Available at SSRN 4359219. DOI: 10.2139/ssrn.4359219.

Ecker, F. and Keeve, T. (2023). Managing Corporate Emission Disclosures Through Divestitures. Available at: SSRN 4517339. DOI: 10.2139/ssrn.4517339.

Egger, P. H. and Wamser, G. (2015). The impact of controlled foreign company legislation on real investments abroad. A multi-dimensional regression discontinuity design. Journal of Public Economics 129, 77–91. DOI: 10.1016/j.jpubeco.2015.07.006.

Eguino, H. and Schächtele, S. (2020). A playground for tax compliance? Testing fiscal exchange in an RCT in Argentina. Tech. rep. IDB Working Paper Series.

Erbertseder, T., Jacob, M., Taubenböck, H., and Zerwer, K. (2023). How effective are emission taxes in reducing air pollution? Available at SSRN 4353315. DOI: 10.2139/ssrn.4353315.

EU Commission (2019). SBA Fact Sheet Bulgaria 2019. Available at: https://ec.europa.eu/growth/smes/sme-strategy/sme-performance-review_en.

EU Commission (2020). Undeclared Work in the European Union. Special Eurobarometer 498. Available at: https://europa.eu/eurobarometer/surveys/detail/2250.

European Commission (2020). Communication from the commission to the european parliament, the council, the european economic and social commitee and the commitee of regions: Stepping up europe's 2030 climate ambition investing in a climate-neutral future for the benefit of our people. COM/2020/562. Available at: https://eur-lex.europa.eu/legal-content/EN/TXT/?uri=CELEX:52020DC0562.

European Commission (2021). Eurostat Freight transport statistics. Available at: https://ec.europa.eu/eurostat/statistics-explained/index.php?title=Freight_transport_statistics_-_modal_split.

European Commission (2012). Regulation 70/2012/EU of the European Parliament and of the Council of 18 January 2012 on statistical returns in respect of the carriage of goods by road. Available at: http://data.europa.eu/eli/reg/2012/70/oj.

European Commission (2023). Regulation of the European Parliament and of the council amending Regulation (EU) 2019/631 as regards strengthening the CO2 emission performance standards for new passenger cars and new light commercial vehicles in

line with the Union's increased climate ambition. Available at: https://data.consilium.europa.eu/doc/document/PE-66-2022-INIT/en/pdf.

European Environment Agency (2024). Greenhouse gas emissions by sector. Available at: https://doi.org/10.2908/ENV_AIR_GGE.

European Environment Agency (2022). Greenhouse gas emissions from transport in Europe. Available at: https://www.eea.europa.eu/ims/greenhouse-gas-emissions-from-transport.

European Environment Agency (2023). How air pollution affects our health. Available at: https://www.eea.europa.eu/en/topics/in-depth/air-pollution/eow-it-affects-our-health.

European Union (2016). Council Directive - laying down rules against tax avoidance practices that directly affect the functioning of the internal market.

Eurostat (2024). Environmental taxes – A statistical guide. Available at: https://ec.europa.eu/eurostat/documents/3859598/18358719/KS-GQ-23-016-EN-N.pdf/011c3fcd-c6c3-604c-320f-bba5da058efe?version=1.0&t=1705926723782. DOI: 10.2785/730717.

Eurostat (2022). EU Tax Revenue Statistics. Available at: https://ec.europa.eu/eurostat/statistics-explained/index.php?title=Tax_revenue_statistics.

Falk, A. (2007). Gift exchange in the field. Econometrica 75(5), 1501–1511. DOI: 10.1111/j.1468-0262.2007.00800.x.

Fehr, E. and Schmidt, K. M. (1999). A theory of fairness, competition, and cooperation. The Quarterly Journal of Economics 114(3), 817–868. DOI: 10.1162/003355399556151.

Feinmann, J., Hsu Rocha, R., and Lauletta, M. (2022). Payments Under the Table. Available at SSRN 4270665. DOI: 10.2139/ssrn.4270665.

Feld, L. and Frey, B. (2007). Tax Compliance as the Result of a Psychological Tax Contract: The Role of Incentives and Responsive Regulation. Law & Policy 29(1), 102–120. DOI: 10.1111/j.1467-9930.2007.00248.x.

Fellner, G., Sausgruber, R., and Traxler, C. (2013). Testing enforcement strategies in the field: Threat, moral appeal and social information. Journal of the European Economic Association 11(3), 634–660. DOI: 10.1111/jeea.12013.

Fischbacher, U., Gächter, S., and Fehr, E. (2001). Are people conditionally cooperative? Evidence from a public goods experiment. Economics Letters 71(3), 397–404. DOI: 10.1016/S0165-1765(01)00394-9.

Fisher, J., Isaac, R. M., Schatzberg, J. W., and Walker, J. M. (1995). Heterogenous demand for public goods: Behavior in the voluntary contributions mechanism. Public Choice 85(3), 249–266. DOI: 10.1007/BF01048198.

Floyd, E., Hallsworth, M., List, J. A., Metcalfe, R. D., Rotaru, K., and Vlaev, I. (2022). What motivates people to pay their taxes? Evidence from four experiments on tax compliance. Available at: http://fieldexperiments-papers2.s3.amazonaws.com/papers/00750.pdf.

Fochmann, M., Fochmann, N., Kocher, M. G., and Müller, N. (2021). Dishonesty and risk-taking: Compliance decisions of individuals and groups. Journal of Economic Behavior & Organization 185, 250–286. DOI: 10.1016/j.jebo.2021.02.018.

Fox, Z. D., Jacob, M., Wilde, J. H., and Wilson, R. J. (2022). Beyond Borders: Uncertainty in Supragovernmental Tax Enforcement and Corporate Investment. The Accounting Review 97(6), 233–261. DOI: 10.2308/TAR-2021-0050.

Friedman, M. (2007). The social responsibility of business is to increase its profits. *Corporate ethics and corporate governance*. Springer, Berlin, Heidelberg, 173–178. DOI: 10.1007/978-3-540-70818-6_14.

Frimmel, W., Halla, M., and Paetzold, J. (2018). The Intergenerational Causal Effect of Tax Evasion: Evidence from the Commuter Tax Allowance in Austria. Journal of the European Economic Association 17(6), 1843–1880. DOI: 10.1093/jeea/jvy033.

Gimenez-Nadal, J. I. and Molina, J. A. (2019). Green Commuting and Gasoline Taxes in the United States. Energy Policy 132. DOI: 10.1016/j.enpol.2019.05.048.

Giroud, X. and Rauh, J. (2019). State Taxation and the Reallocation of Business Activity: Evidence from Establishment-Level Data. Journal of Political Economy 127(3), 1262–1316. DOI: 10.1086/701357.

Gneezy, U. and List, J. A. (2006). Putting behavioral economics to work: Testing for gift exchange in labor markets using field experiments. Econometrica 74(5), 1365–1384. DOI: 10.1111/j.1468-0262.2006.00707.x.

Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. Journal of Econometrics 225(2), 254–277. DOI: 10.1016/j.jeconom.2021.03.014.

Greene, W. (2002). The Bias of the Fixed Effects Estimator in Nonlinear Models. Available at: https://pages.stern.nyu.edu/~wgreene/nonlinearfixedeffects.pdf.

Grieder, M., Kistler, D., and Schmitz, J. (2021). Environmental Decision Making in Small Companies: A Behavioral Economics Perspective. Sustainability and Environmental Decision Making, 129–151. DOI: 10.1007/978-981-15-9287-4_13.

Grubert, H. (2003). Intangible Income, Intercompany Transactions, Income Shifting, and the Choice of Location. National Tax Journal 56(1), 221–242. DOI: 10.17310/ntj.2003.1S.05.

Gschossmann, E. and Pfrang, A. (2024). Multinationals' location, financial and real responses to the EU-wide implementation of CFC Rules by the ATAD. Available at SSRN 4735272. DOI: 10.2139/ssrn.4735272.

Guiso, L., Sapienza, P., and Zingales, L. (2013). The determinants of attitudes toward strategic default on mortgages. The Journal of Finance 68(4), 1473–1515. DOI: 10.1111/jofi.12044.

Hall, R. E. and Jorgenson, D. W. (1967). Tax Policy and Investment Behavior. The American Economic Review 57(3), 391–414.

Hallsworth, M., List, J. A., Metcalfe, R. D., and Vlaev, I. (2017). The behavioralist as tax collector: Using natural field experiments to enhance tax compliance. Journal of Public Economics 148, 14–31. DOI: 10.1016/j.jpubeco.2017.02.003.

Hanlon, M. and Heitzman, S. (2010). A review of tax research. Journal of accounting and Economics 50(2-3), 127–178. DOI: 10.1016/j.jacceco.2010.09.002.

Heijdra, B. J. and Romp, W. E. (2009). Retirement, pensions, and ageing. Journal of Public Economics 93(3-4), 586–604. DOI: 10.1016/j.jpubeco.2008.10.009.

Holz, J. E., List, J. A., Zentner, A., Cardoza, M., and Zentner, J. E. (2023). The $100 Million Nudge: Increasing Tax Compliance of Businesses and the Self-Employed using a Natural Field Experiment. Journal of Public Economics 218, 104779. DOI: 10.1016/j.jpubeco.2022.104779.

Hoopes, J. L., Robinson, L., and Slemrod, J. (2018). Public tax-return disclosure. Journal of Accounting and Economics 66(1), 142–162. DOI: 10.1016/j.jacceco.2018.04.001.

Internal Revenue Service (2016). Federal Tax Compliance Research: Tax Gap Estimates for Tax Years 2008–2010.

International Energy Agency (2024). CO_2 Emissions in 2023. Available at: https://www.iea.org/reports/co2-emissions-in-2023.

International Energy Agency (2022). Transport. Available at: https://www.iea.org/reports/transport.

IPCC (2021). Global Warming of 1.5°C: An IPCC Special Report on the impacts of global warming of 1.5°C above pre-industrial levels and related global greenhouse gas emission pathways, in the context of strengthening the global response to the threat of climate change, sustainable development, and efforts to eradicate poverty. Chapter 3. Available at: https://www.ipcc.ch/sr15/.

Itzhak, B.-D., Jang, Y., Kleimeier, S., and Viehs, M. (2021). Exporting pollution: where do multinational firms emit CO2? Economic Policy 36(107), 377–437. DOI: 10.1093/epolic/eiab009.

Izadi, A., Nabipour, M., and Titidezh, O. (2020). Cost Models and Cost Factors of Road Freight Transportation: A Literature Review and Model Structure. Fuzzy Information and Engineering 11(3), 257–278. DOI: 10.1080/16168658.2019.1706960.

Jacob, J. (1996). Taxes and Transfer Pricing: Income Shifting and the Volume of Intrafirm Transfers. Journal of Accounting Research 34(2), 301–312. DOI: 10.2307/2491504.

Jacob, M. (2022). Real Effects of Corporate Taxation: A Review. European Accounting Review 31(1), 269–296. DOI: 10.1080/09638180.2021.1934055.

Jansen, D.-J. and Jonker, N. (2018). Fuel tourism in Dutch border regions: Are only salient price differentials relevant? Energy Economics 74, 143–153. DOI: 10.1016/j.eneco.2018.05.036.

Jones, D., Molitor, D., and Reif, J. (2019). What do workplace wellness programs do? Evidence from the Illinois workplace wellness study. The Quarterly Journal of Economics 134(4), 1747–1791. DOI: 10.1093/qje/qjz023.

Kaenzig, D. R., Marenz, J., and Olbert, M. (2023). Carbon Leakage to Developing Countries. Available at SSRN 4833343. DOI: 10.2139/ssrn.4833343.

Karver, J. G., Shijaku, H., and Ungerer, C. T. (2022). Nudging in the Time of the Coronavirus : Evidence from an Experimental Tax Trial in Albania at the Onset of a Global Pandemic. Available at the World Bank Policy Research Working Paper Series 9961.

Kleven, H. J., Knudsen, M. B., Kreiner, C. T., Pedersen, S., and Saez, E. (2011). Unwilling or unable to cheat? Evidence from a tax audit experiment in Denmark. Econometrica 79(3), 651–692. DOI: 10.3982/ECTA9113.

Kleven, H. J., Kreiner, C. T., and Saez, E. (2016). Why can modern governments tax so much? An agency model of firms as fiscal intermediaries. Economica 83(330), 219–246. DOI: 10.1111/ecca.12182.

Kleven, H. J., Landais, C., and Saez, E. (1, 2013). Taxation and International Migration of Superstars: Evidence from the European Football Market. American Economic Review 103(5), 1892–1924. DOI: 10.1257/aer.103.5.1892.

Kocher, M. G., Praxmarer, M., and Sutter, M. (2020). Team Decision-Making. *Handbook of Labor, Human Resources and Population Economics*. Ed. by K. F. Zimmermann. Springer International Publishing, 1–25.

Kopetzki, L. C., Spengel, C., and Weck, S. (2023). Moving forward with tax sustainability reporting in the EU – a quantitative descriptive analysis. World Tax Journal 15(2), 291–320.

Kube, S., Maréchal, M. A., and Puppe, C. (2012). The currency of reciprocity: Gift exchange in the workplace. American Economic Review 102(4), 1644–62. DOI: 10.1257/aer.102.4.1644.

Kumler, T., Verhoogen, E., and Frías, J. (2020). Enlisting Employees in Improving Payroll Tax Compliance: Evidence from Mexico. The Review of Economics and Statistics 102(5), 881–896. DOI: 10.1162/rest_a_00907.

Lediga, C., Riedel, N., and Strohmaier, K. (2020). Tax enforcement spillovers–evidence from South Africa. Available at SSRN 3616817. DOI: 10.2139/ssrn.3616817.

Ledyard, J. O. (1995). Is there a problem with public goods provision. The Handbook of Experimental Economics, 111–194.

Lester, R. and Olbert, M. (2024). Firms' Real and Reporting Responses to Taxation: A Review. Available at: https://ssrn.com/abstract=4779893. DOI: 10.2139/ssrn.4779893.

Li, S., Linn, J., and Muehlegger, E. (2014). Gasoline taxes and consumer behavior. American Economic Journal: Economic Policy 6(4), 302–42. DOI: 10.1257/pol.6.4.302.

List, J. A. (2022). The Voltage Effect. Vol. 58. Penguin UK, 3–8. DOI: 10.1057/s11369-022-00298-9.

List, J. A., Murphy, J. J., Price, M. K., and James, A. G. (2021). An experimental test of fundraising appeals targeting donor and recipient benefits. Nature Human Behaviour 5, 1339–1348. DOI: 10.1038/s41562-021-01095-8.

Ljungqvist, A. and Smolyansky, M. (2018). To cut or not to cut? On the impact of corporate taxes on employment and income. Tech. rep. DOI: 10.3386/w20753.

Loewenstein, G. F., Weber, E. U., Hsee, C. K., and Welch, N. (2001). Risk as feelings. Psychological Bulletin 127(2), 267–286.

Luechinger, S. and Roth, F. (2016). Effects of a mileage tax for trucks. Journal of Urban Economics 92, 1–15. DOI: 10.1016/j.jue.2015.09.005.

Luttmer, E. F. P. and Singhal, M. (2014). Tax Morale. Journal of Economic Perspectives 28(4), 149–68.

Marion, J. and Muehlegger, E. (2011). Fuel tax incidence and supply conditions. Journal of public economics 95(9), 1202–1212.

Markle, K. and Robinson, L. (2012). Tax haven use Across International Tax Regimes. Available at https://faculty.tuck.dartmouth.edu/images/uploads/faculty/leslie-robinson/marklerobinson.pdf.

Martin, R., Muûls, M., De Preux, L. B., and Wagner, U. J. (2014). Industry compensation under relocation risk: A firm-level analysis of the EU emissions trading scheme. American Economic Review 104(8), 2482–2508. DOI: 10.1257/aer.104.8.2482.

Martinsson, G., Sajtos, L., Strömberg, P., and Thomann, C. (2024). The Effect of Carbon Pricing on Firm Emissions: Evidence from the Swedish CO2 Tax. The Review of Financial Studies 37(6), 1884–1886. DOI: 10.1093/rfs/hhad097.

McKenzie, D. (2012). Beyond baseline and follow-up: The case for more T in experiments. Journal of Development Economics 99(2), 210–221. DOI: 10.1016/j.jdeveco.2012.01.002.

Milanez, A. (2017). Legal Tax Liability, Legal Remittance Responsibility and Tax Incidence: Three Dimensions of Business Taxation. DOI: 10.1787/22235558.

Muûls, M., Martin, R., Colmer, J., and Wagner, U. (2024). Does pricing carbon mitigate climate change? Firm-level evidence from the European Union Emissions Trading Scheme. Review of Economic Studies. DOI: 10.1093/restud/rdae055.

Naritomi, J. (2019). Consumers as Tax Auditors. American Economic Review 109(9), 3031–3072. DOI: 10.1257/aer.20160658.

OECD (2015). Designing Effective Controlled Foreign Company Rules, Action 3 - 2015 Final Report. Paris: Organisation for Economic Co-operation and Development.

OECD (2021a). ITF Transport Outlook 2021. Available at: https://www.oecd-ilibrary.org/transport/itf-transport-outlook-2021_16826a30-en.

OECD (2021b). OECD Economic Surveys: Bulgaria 2021: Economic Assessment. Available at: https://doi.org/10.1787/1fe2940d-en. OECD Publishing, Paris.

OECD (2019). Taxing vehicles, fuels, and road use: Opportunities for improving transport tax practices. Available at: https://doi.org/10.1787/22235558.

Ohrn, E. (2018). The Effect of Corporate Taxation on Investment and Financial Policy: Evidence from the DPAD. American Economic Journal: Economic Policy 10(2), 272–301. DOI: 10.1257/pol.20150378.

Ortega, D. and Scartascini, C. (2020). Don't blame the messenger. The Delivery Method of a Message Matters. Journal of Economic Behavior & Organization 170, 286–300. DOI: 10.1016/j.jebo.2019.12.008.

Overesch, M., Schindler, D., and Wamser, G. (2024). Design and Consequences of CFC and GILTI Rules: A Review and Potential Lessons for the Global Minimum Tax. Available at SSRN 4783698. DOI: 10.2139/ssrn.4783698.

Paulus, A. (2015). Tax evasion and measurement error: An econometric analysis of survey data linked with tax records. Available at ISER Working Paper 2015-10.

Pomeranz, D. (2015). No taxation without information: Deterrence and self-enforcement in the value added tax. American Economic Review 105(8), 2539–69. DOI: 10.1257/aer.20130393.

Prettl, A. (2017). Influence of anti-tax avoidance rules on profit shifting and real FDI-examining CFC rules. DOI: 10.2139/ssrn.2893907.

Prettl, A. and Hagen, D. von (2023). Multinational ownership patterns and anti-tax avoidance legislation. International Tax and Public Finance 30(3), 565–634. DOI: 10.1007/s10797-021-09719-5.

Ramanathan, V. and Carmichael, G. (2008). Global and regional climate changes due to black carbon. Nature Geoscience 1(4), 221–227. DOI: 10.1038/ngeo156.

Riedel, N. (2018). Quantifying International Tax Avoidance: A Review of the Academic Literature. Review of Economics 69(2), 169–181. DOI: 10.1515/roe-2018-0004.

Rietveld, P., Bruinsma, F., and Van Vuuren, D. (2001). Spatial graduation of fuel taxes; consequences for cross-border and domestic fuelling. Transportation Research Part A: Policy and Practice 35(5), 433–457. DOI: 10.1016/S0965-8564(00)00002-1.

Rivers, N. and Schaufele, B. (2015). Salience of Carbon Taxes in the Gasoline Market. Journal of Environmental Economics and management 74, 23–36. DOI: 10.1016/j.jeem.2015.07.002.

Ruf, M. and Weichenrieder, A. (2013). CFC legislation, passive assets and the impact of the ECJ's Cadbury Schweppes Decision. Tech. rep. DOI: 10.2139/ssrn.2398309.

Ruf, M. and Weichenrieder, A. J. (2012). The taxation of passive foreign investment: Lessons from German experience. Canadian Journal of Economics 45(4), 1504–1528. DOI: 10.1111/J.1540-5982.2012.01737.X.

Runst, P. and Hoehle, D. (2022). The German eco tax and its impact on CO_2 emissions. Energy Policy 160, 112655. DOI: 10.1016/j.enpol.2021.112655.

Santos, G. (2017). Road fuel taxes in Europe: Do they internalize road transport externalities? Transport Policy 53, 120–134. DOI: 10.1016/j.tranpol.2016.09.009.

Santos Silva, J. M. C. and Tenreyro, S. (2011). Further simulation evidence on the performance of the Poisson pseudo-maximum likelihood estimator. Economics Letters 112(2), 220–222. DOI: 10.1016/j.econlet.2011.05.008.

Santos Silva, J. M. C. and Tenreyro, S. (2006). The Log of Gravity. The Review of Economics and Statistics 88(4), 641–658. DOI: 10.1162/rest.88.4.641.

Schaechtele, S., Eguino, H., and Roman, S. (2022). Fiscal Exchange and Tax Compliance: Evidence from a Field Experiment. Journal of Policy Analysis and Management 42(3). DOI: 10.1002/pam.22460.

Schenkelberg, S. (2020). The Cadbury Schweppes judgment and its implications on profit shifting activities within Europe. International Tax and Public Finance 27(1), 1–31. DOI: 10.1007/s10797-019-09553-w.

Schmidheiny, K. and Slotwinski, M. (1, 2018). Tax-induced mobility: Evidence from a foreigners' tax scheme in Switzerland. Journal of Public Economics 167, 293–324. DOI: 10.1016/j.jpubeco.2018.04.002.

Schmitz, J. and Schrader, J. (2015). Corporate social responsibility: A microeconomic review of the literature. Journal of Economic Surveys 29(1), 27–45. DOI: 10.1111/joes.12043.

Slemrod, J. and Yitzhaki, S. (1987). The optimal size of a tax collection agency. Scandinavian Journal of Economics 89(2), 183–192. DOI: 10.2307/3440063.

Slemrod, J. (2019). Tax compliance and enforcement. Journal of Economic Literature 57(4), 904–54. DOI: 10.1257/jel.20181437.

Slemrod, J., Blumenthal, M., and Christian, C. (2001). Taxpayer response to an increased probability of audit: Evidence from a controlled experiment in Minnesota. Journal of Public Economics 79(3), 455–483. DOI: 10.1016/S0047-2727(99)00107-3.

Slemrod, J., Collins, B., Hoopes, J. L., Reck, D., and Sebastiani, M. (2017). Does credit-card information reporting improve small-business tax compliance? Journal of Public Economics 149, 1–19. DOI: 10.1016/j.jpubeco.2017.02.010.

Slemrod, J. and Velayudhan, T. (2018). Do Firms Remit at Least 85% of Tax Everywhere? New Evidence from India. Journal of Tax Adminsitration 4(1), 124–137.

Slemrod, J. and Weber, C. (2012). Evidence of the invisible: toward a credibility revolution in the empirical analysis of tax evasion and the informal economy. International Tax and Public Finance 19, 25–53. DOI: 10.1007/s10797-011-9181-0.

Suárez Serrato, J. C. (2019). Unintended Consequences of Eliminating Tax Havens. Availabe at NBER Working Paper 24850. DOI: 10.3386/w24850.

The Platform for Collaboration on Tax (2023). PCT Progress Report 2023. Available at: https://www.tax-platform.org/sites/pct/files/publications/PCT-Progress-Report-2023.pdf.

Tiefenbeck, V., Staake, T., Roth, K., and Sachs, O. (2013). For better or for worse? Empirical evidence of moral licensing in a behavioral energy conservation campaign. Energy Policy 57, 160–171. DOI: 10.1016/j.enpol.2013.01.021.

Torgler, B. (2004). Moral suasion: An alternative tax policy strategy? Evidence from a controlled field experiment in Switzerland. Economics of Governance 5(3), 235–253. DOI: 10.1007/s10101-004-0077-7.

Transport and Environment (2021). Fuel taxes. Available at: https://www.transport-environment.org/challenges/climate-tools/fuel-taxes/.

Trego, T. G. and Murray, D. (2010). An Analysis of the Operational Costs of Trucking. Transportation Research Board 2010 Annual Meetings 18,20. Available at: https://trid.trb.org/View/910437.

Tversky, A. and Kahneman, D. (1974). Judgment under Uncertainty: Heuristics and Biases. Science 185(4157), 1124–1131. DOI: 10.1126/science.185.4157.1124.

United Nations (1978). Report of the World Commission on Environment and Development: Our Common Future. Available at: http://www.un-documents.net/our-common-future.pdf.

United Nations (2015). Transforming Our World: The 2030 Agenda for Sustainable Development. Available at: https://sdgs.un.org/2030agenda.

Voget, J. (2011). Relocation of headquarters and international taxation. Journal of Public Economics 95(9), 1067–1081. DOI: 10.1016/J.JPUBECO.2010.11.019.

Waseem, M. (2023). Overclaimed refunds, undeclared sales, and invoice mills: Nature and extent of noncompliance in a value-added tax. Journal of Public Economics 218, 104783.

Waseem, M. (2022). The Role of Withholding in the Self-Enforcement of a Value-Added Tax: Evidence from Pakistan. The Review of Economics and Statistics 104(2), 336–354. DOI: 10.1162/rest_a_00959.

Wilde, J. H. and Wilson, R. J. (2018). Perspectives on corporate tax planning: Observations from the past decade. The Journal of the American Taxation Association 40(2), 63–81. DOI: 10.2308/atax-51993.

Williams, B. M. (2018). Multinational tax incentives and offshored U.S. jobs. Accounting Review 93(5), 293–324. DOI: 10.2308/accr-52008.

Williams, C. and Horodnic, I. A. (2017). Under-declaring work, falsely declaring work: Under-declared employment in the European Union. Available at SSRN 3053148.

Wooldridge, J. M. (2023). Simple approaches to nonlinear difference-in-differences with panel data. The Econometrics Journal 26(3). DOI: 10.1093/ectj/utad016.

World Bank (2014). Reducing black carbon emissions from diesel vehicles: Impacts, control strategies, and cost-benefit analysis. Available at: https://documents1.worldbank.org/curated/en/199821468324280897/pdf/WPS4818.pdf.

World Health Organization (2005). Health effects of transport-related air pollution. Available at: https://apps.who.int/iris/handle/10665/328088.

Yang, J. and Williams, C. C. (2017). Tackling Falsely-Declared Salaries in Bulgaria: Evidence from a 2015 Survey. Economic Alternatives 3, 333–351.

Zhang, C. Y., Hemmeter, J., Kessler, J. B., Metcalfe, R. D., and Weathers, R. (2022). Nudging Timely Wage Reporting: Field Experimental Evidence from the US Supplemental Security Income Program. Management Science 69(3), 1341–1351. DOI: 10.1287/mnsc.2022.4645.

A Appendix to Chapter 2

A.1 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	

Note: 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	

Note: 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

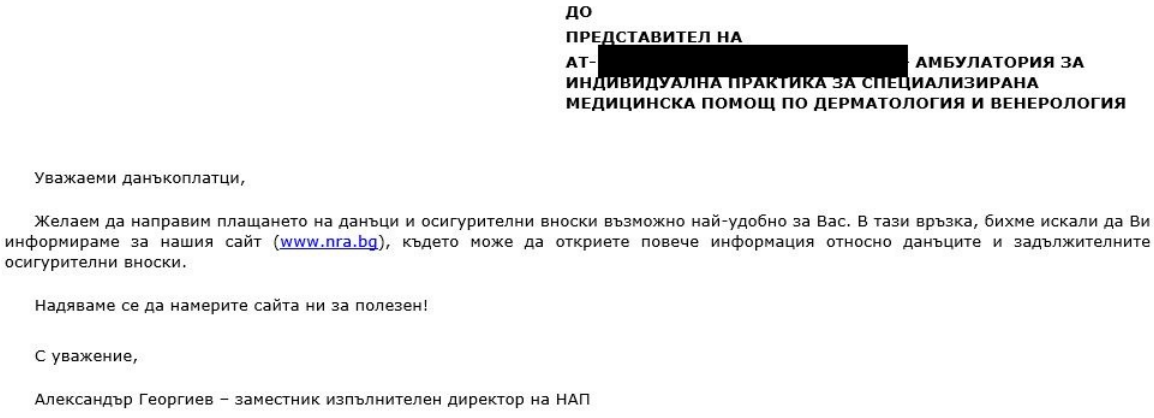
	Baseline	*Audit 1%*	*Audit 10%*	*Audit 40%*	*Audit 60%*	*Ambiguous*	p-value test
Panel A:				**Outcome Variables**			
SSC	8003.999	8453.453	7311.148	7527.440	10046.234	7136.360	0.5518
(Pre-experiment average tax base 2016)	(33976.243)	(65820.641)	(26178.217)	(35865.834)	(41969.878)	(30137.210)	
VAT	14344.145	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	

Note: 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.

A.2 Treatment Letters

Baseline Condition

Figure A.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 A.2: Cooperation 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 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 A.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.

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.

Yours sincerely, SIGNATURE

(b) Email text in English

Figure A.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.

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 A.5: Picture 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

Deterrence Treatments

Figure A.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.

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 A.7: Ambiguous 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

Survey

Figure A.8: Survey treatment

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

A.3 Robustness Checks and Additional Analyses

Treatment Effects without Control Variables

Table A.4: Treatment effects of moral treatments on SSC without control variables

Panel A:	**4 post-treatment months**				
	Moral All	*Cooperation*	*Example*	*Necessity*	*Picture*
	(1)	(2)	(3)	(4)	(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	*Cooperation*	*Example*	*Necessity*	*Picture*
	(1)	(2)	(3)	(4)	(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

Note: 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 A.5: 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
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

Note: Treatment effects of deterrence 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Treatment Effects Using TWFEs

Table A.6: Treatment effects of moral treatments on SSC using TWFEs

Panel A:	**4 post-treatment months**				
	Moral All	*Cooperation*	*Example*	*Necessity*	*Picture*
	(1)	(2)	(3)	(4)	(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	*Cooperation*	*Example*	*Necessity*	*Picture*
	(1)	(2)	(3)	(4)	(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

Note: 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 A.7: Treatment effects of deterrence treatments on SSC using TWFEs

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	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	*Audit 1%*	*Audit 10%*	*Audit 40%*	*Audit 60%*	*Ambiguous*
	(1)	(2)	(3)	(4)	(5)	(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

Note: Treatment effects of deterrence 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 errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Multiple Hypothesis Testing

Table A.8: Multiple hypothesis testing moral treatments

Panel A:		**4 post-treatment months**				
		Moral All	*Cooperation*	*Example*	*Necessity*	*Picture*
		(1)	(2)	(3)	(4)	(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	*Cooperation*	*Example*	*Necessity*	*Picture*
		(1)	(2)	(3)	(4)	(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

Note: 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 A.9: 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

Note: 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Treatment Effects Varying the Pre-treatment Period

Table A.10: 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

Note: 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 and 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 A.11: 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

Note: 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 A.12: 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

Note: 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 A.13: 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

Note: 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Treatment Effects Varying the Post-treatment Period

Table A.14: 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

Note: 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 A.15: 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

Note: 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Treatment Effects Using only Post-treatment Months

Table A.16: 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

Note: 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 A.17: Treatment effects of moral 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)
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

Note: 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

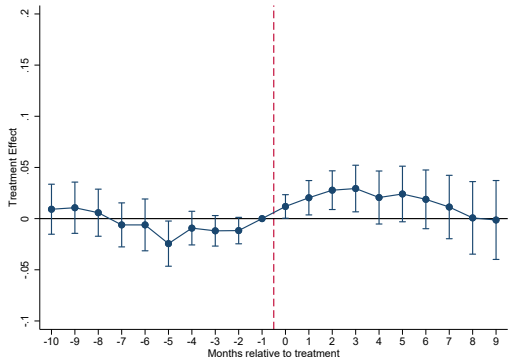
Table A.18: Treatment effects of audit probability messages relative to ambiguous treatment on SSC using only post-treatment months

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

Note: 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

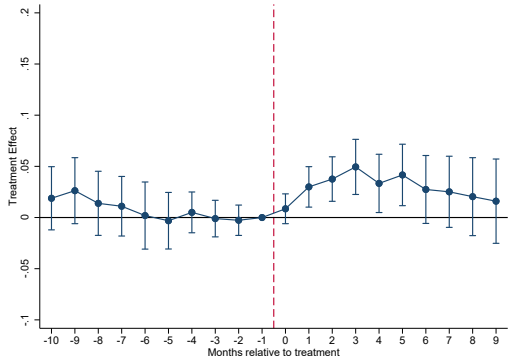
Dynamic Relative Treatment Effects

Figure A.9: Dynamic Relative effects of moral treatments on SSC



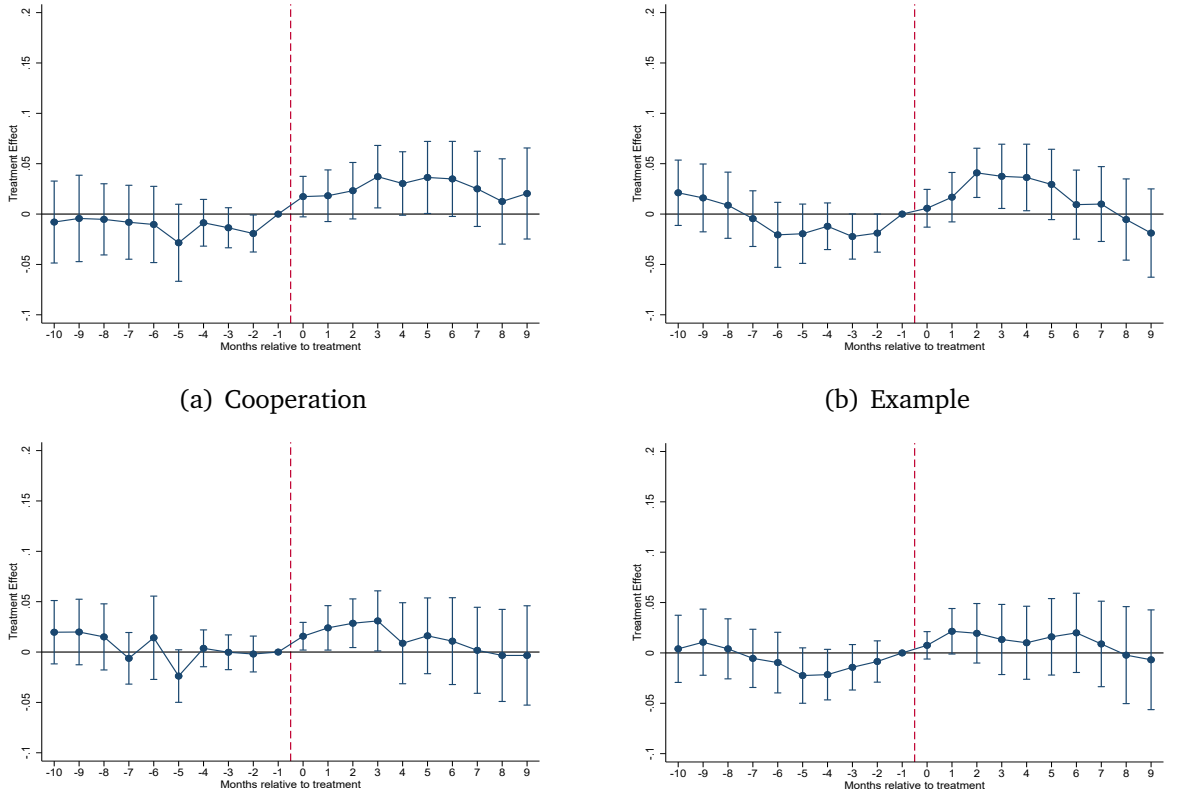
Note: Pooled monthly relative treatment effects of the moral appeal messages (Cooperation - Picture) on SSC. The points plotted are the estimated DiD poisson regression coefficients of treatment messages on tax base SSC by months relative to the treatment. Treatment effects are relative to the control 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 A.10: Dynamic Relative effect s of deterrence treatments on SSC



Note: 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 A.11: Dynamic Relative effect s of moral treatments on SSC by sub-treatment



(a) Cooperation

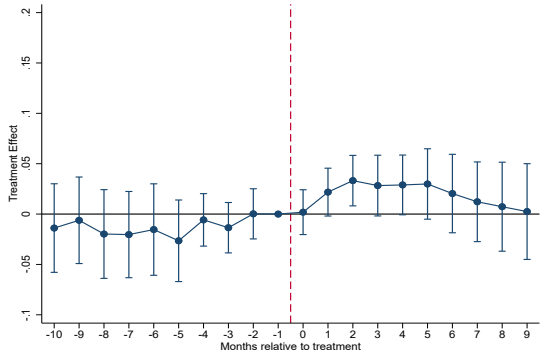
(b) Example

(c) Necessity

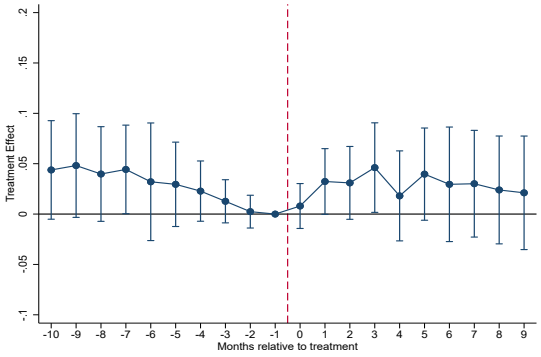
(d) Picture

Note: 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.

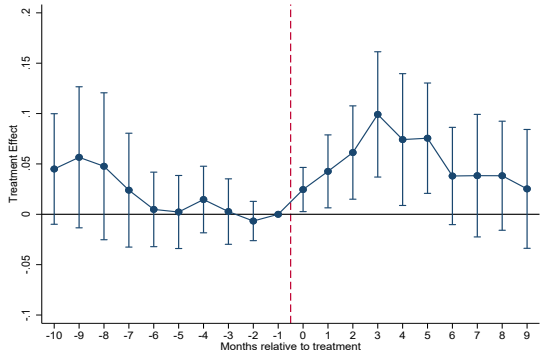
Figure A.12: Dynamic Relative effect s of deterrence treatments on SSC by sub-treatment



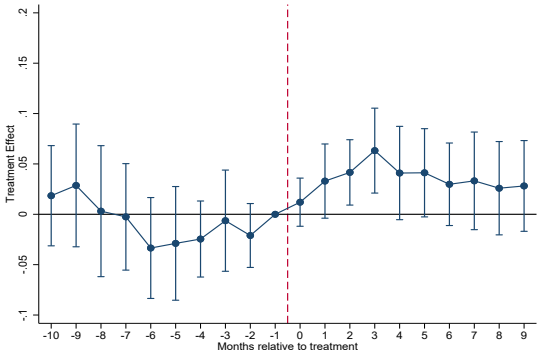
(a) Audit 1%



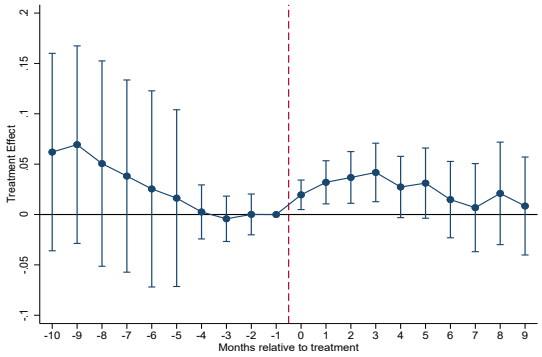
(b) Audit 10%



(c) Audit 40%



(d) Audit 60%



(e) Ambiguous

Note: 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.

Treatment Effects Excluding Audited Firms

Table A.19: 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

Note: 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Testing Moral vs. Deterrence Treatments

Table A.20: 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)

Note: 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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

Randomization and Summary Statistics

Table A.21: 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	

Note: 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 A.22: 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	

Note: 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 A.23: 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	

Note: 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.

Static DiD

Table A.24: Extended sample: Treatment effects of moral appeals on SSC

Panel A:	**4 post-treatment months**				
	Moral all	*Cooperation*	*Example*	*Necessity*	*Picture*
	(1)	(2)	(3)	(4)	(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	*Cooperation*	*Example*	*Necessity*	*Picture*
	(1)	(2)	(3)	(4)	(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

Note: 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

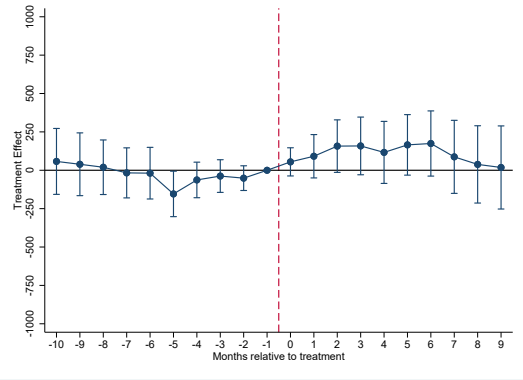
Table A.25: Extended sample: Treatment effects of deterrence treatments on SSC

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

Note: 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 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

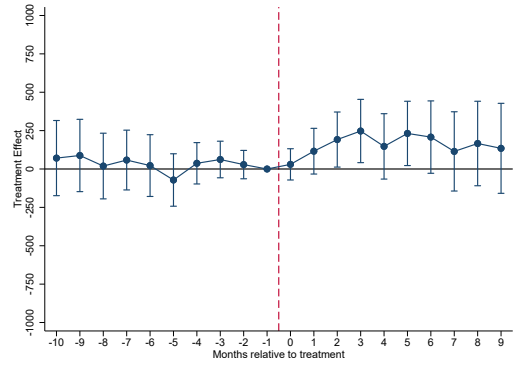
Dynamics

Figure A.13: Extended sample: Dynamic absolute effects of moral treatments on SSC



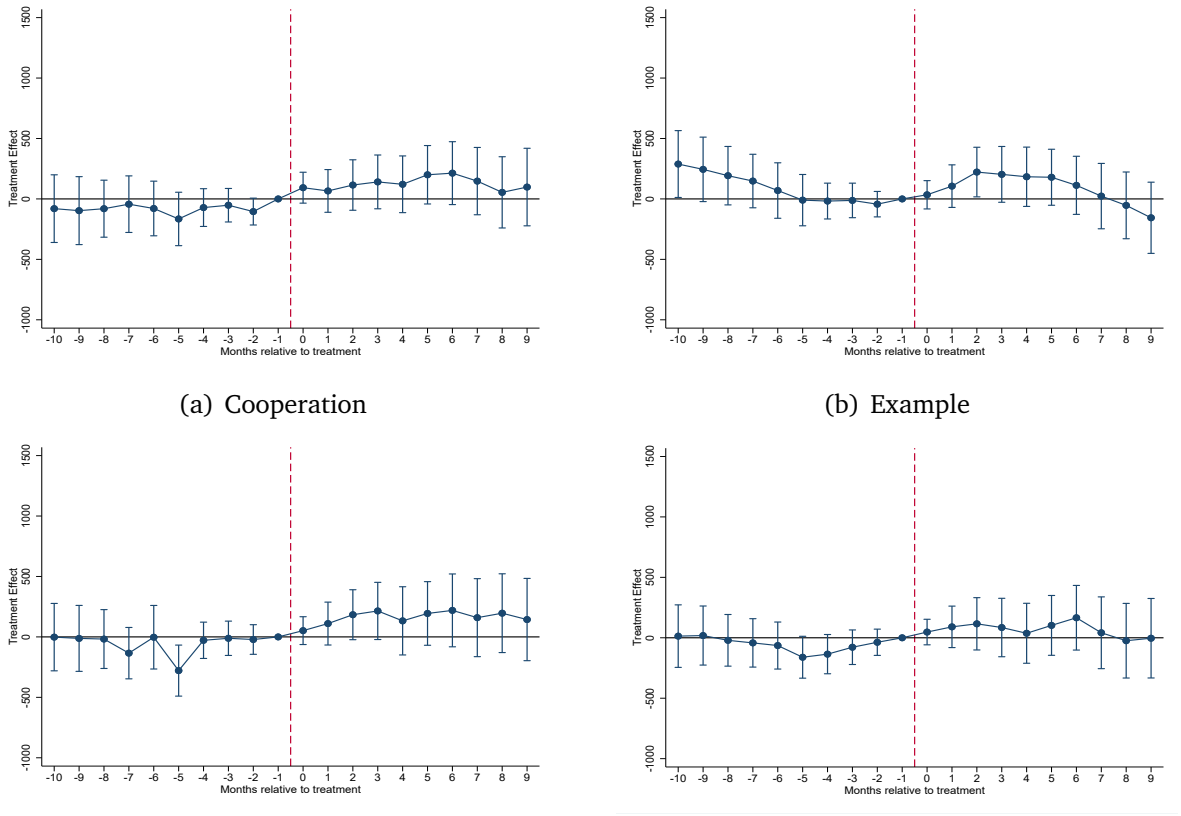
Note: Pooled monthly absolute treatment effects of the moral appeal messages (Cooperation - Picture) on SSC. The points plotted are the estimated DiD regression coefficients of treatment messages on tax base SSC by months relative to the treatment. Treatment effects are relative to the control message. 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 A.14: Extended sample: Dynamic absolute effects of deterrence treatments on SSC



Note: 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 A.15: Extended sample: Dynamic absolute effects of moral treatments on SSC by sub-treatment



(a) Cooperation

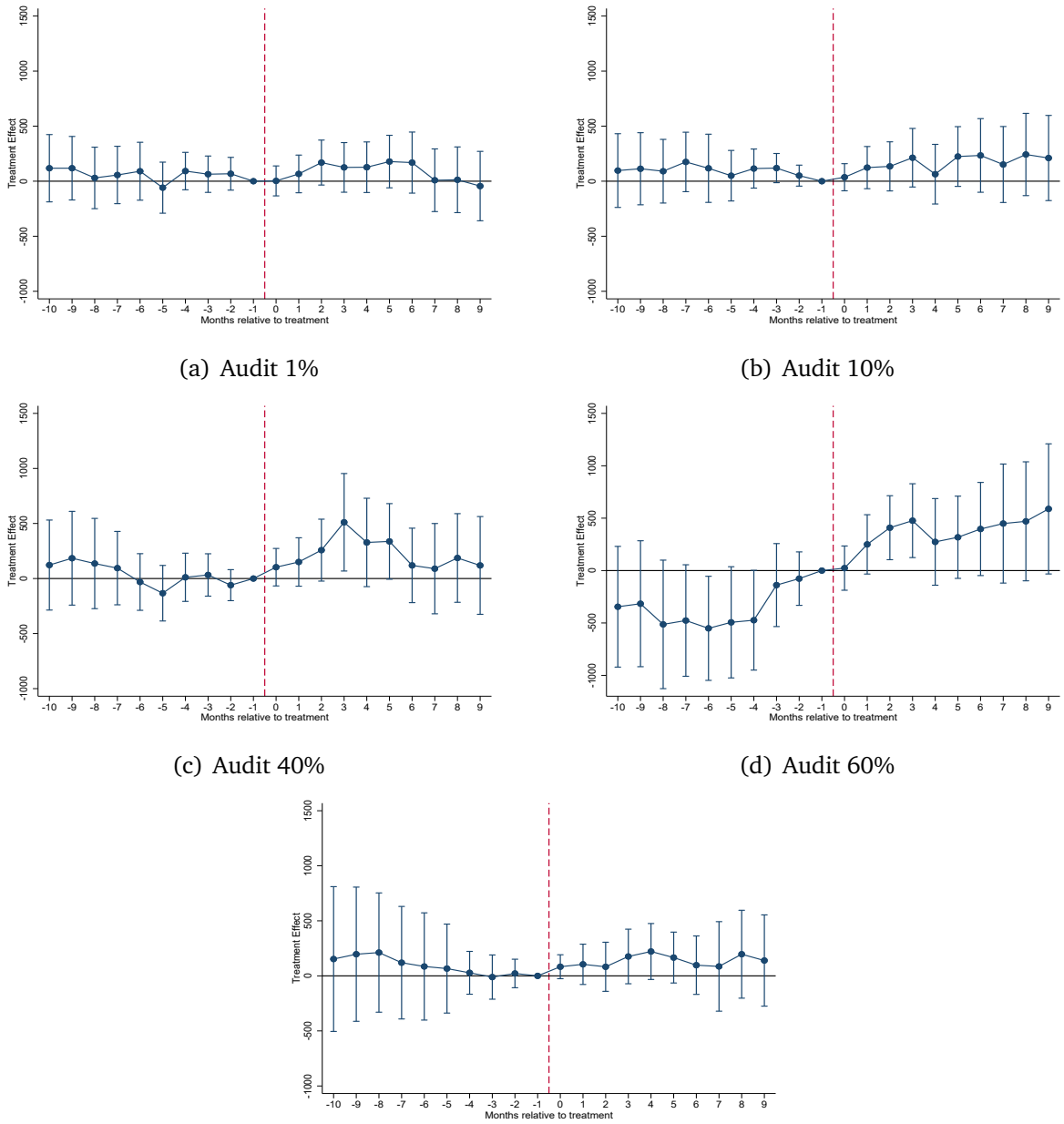
(b) Example

(c) Necessity

(d) Picture

Note: 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 A.16: Extended sample: Dynamic absolute effects of deterrence treatments on SSC by sub-treatment



(a) Audit 1%

(b) Audit 10%

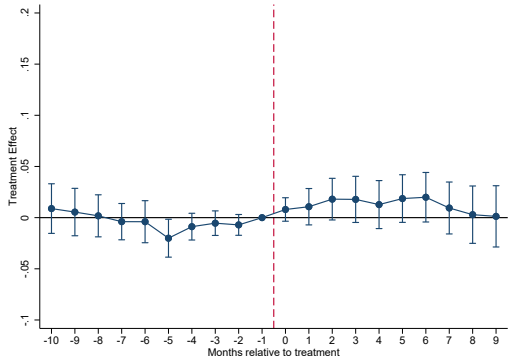
(c) Audit 40%

(d) Audit 60%

(e) Ambiguous

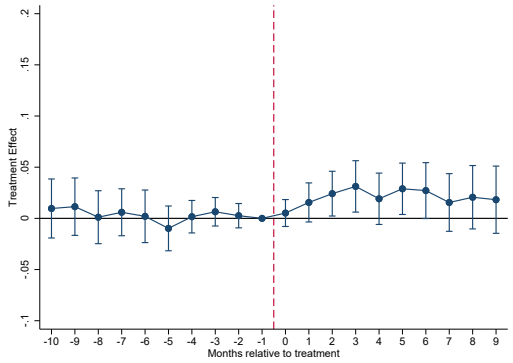
Note: Monthly absolute treatment effects of audit messages (Audit 1%, Audit 10%, Audit 40%, Audit 60%, Ambiguous) on SSC. The points plotted are the estimated DiD regression coefficients of treatment messages on tax base SSC by months relative to the treatment. Treatment effects are relative to the control 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 A.17: Extended sample: Dynamic relative effects of moral treatments on SSC



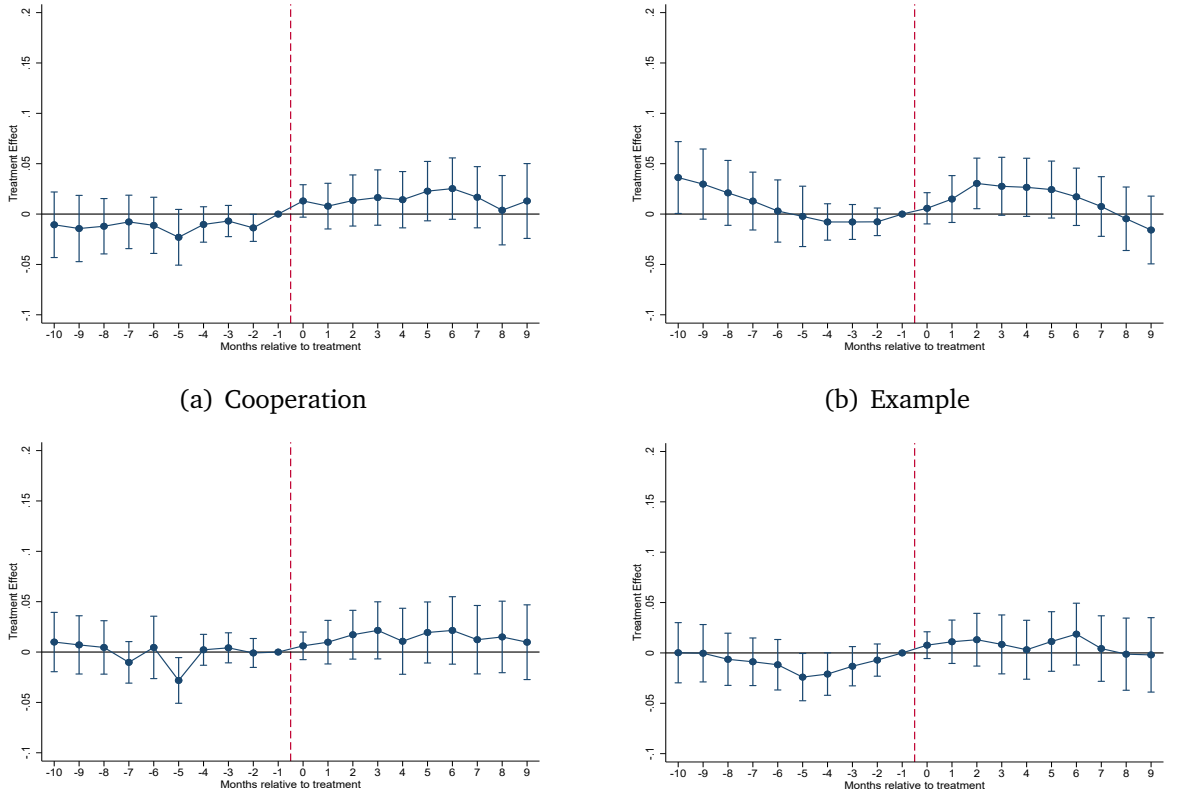
Note: Pooled monthly relative treatment effects of the moral appeal messages (Cooperation - Picture) on SSC. The points plotted are the estimated DiD poisson regression coefficients of treatment messages on tax base SSC by months relative to the treatment. Treatment effects are relative to the control 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 A.18: Extended sample: Dynamic relative effects of deterrence treatments on SSC



Note: 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 A.19: Extended sample: Dynamic relative effects of moral treatments on SSC by sub-treatment



(a) Cooperation

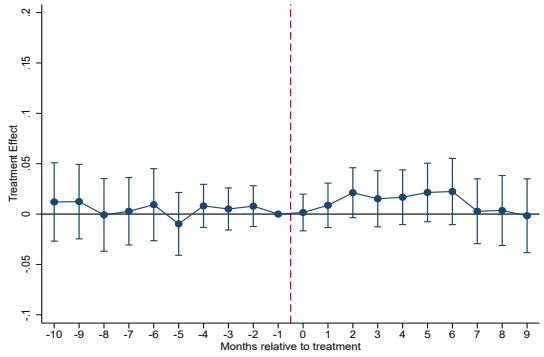
(b) Example

(c) Necessity

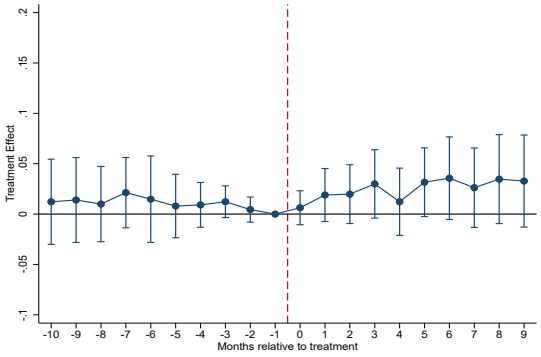
(d) Picture

Note: 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.

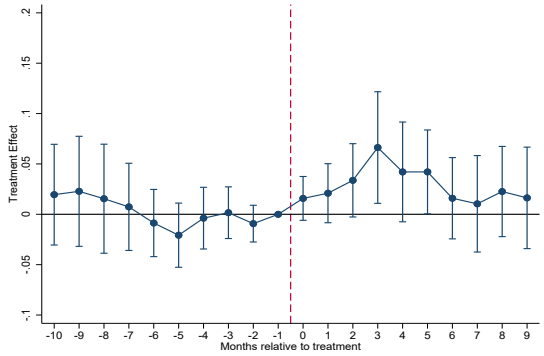
Figure A.20: Extended sample: Dynamic relative effects of deterrence treatments on SSC by sub-treatment



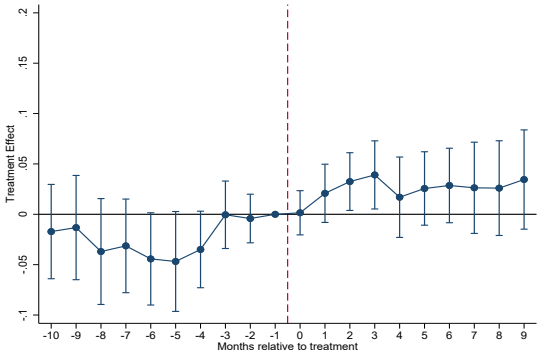
(a) Audit 1%



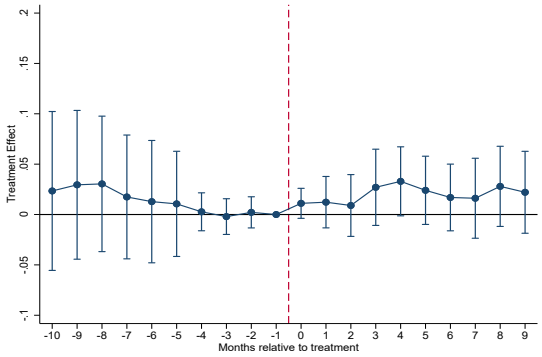
(b) Audit 10%



(c) Audit 40%



(d) Audit 60%

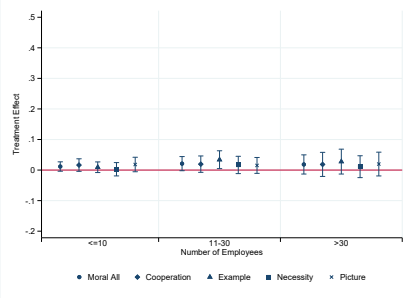


(e) Ambiguous

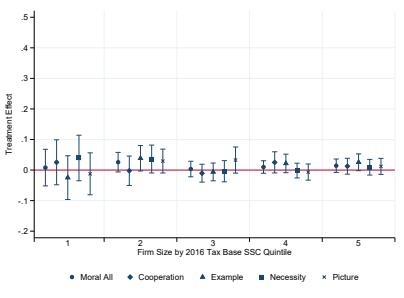
Note: Monthly relative treatment effects of audit messages (Audit 1%, Audit 10%, Audit 40%, Audit 60%, Ambiguous) on SSC. The points plotted are the estimated DiD poisson regression coefficients of treatment messages on tax base SSC by months relative to the treatment. Treatment effects are relative 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.

Heterogeneity

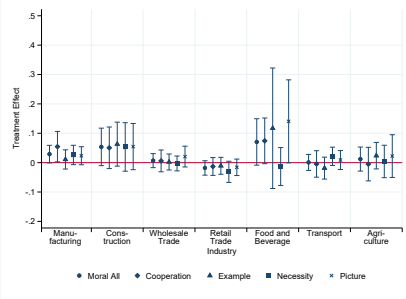
Figure A.21: Extended sample: Heterogeneous effects of moral treatments on SSC



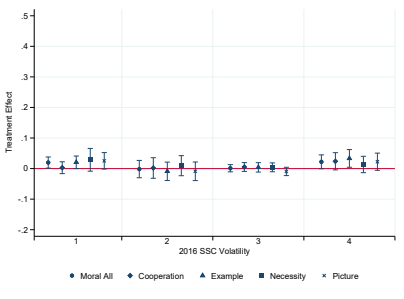
(a) Firm size: Number of employees



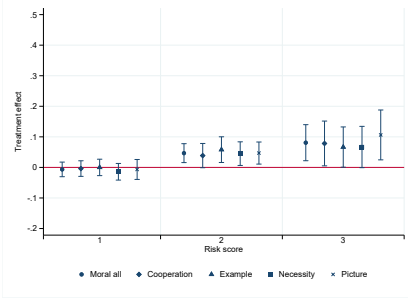
(b) Firm size: Pre-treatment tax base SSC



(c) Industry



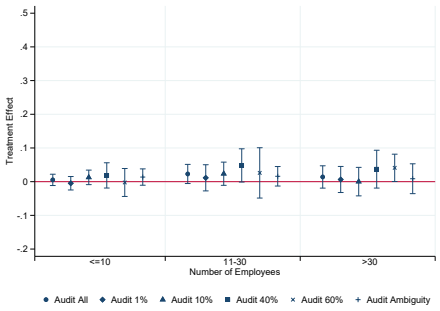
(d) Volatility: 2016 tax base SSC



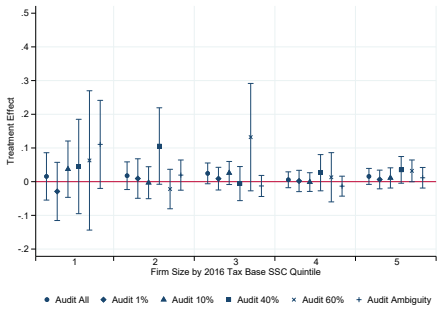
(e) Risk score

Note: 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.

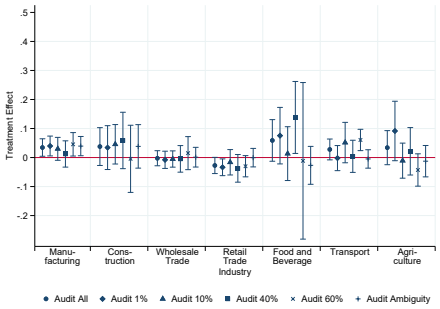
Figure A.22: Extended sample: Heterogeneous effects of deterrence treatments on SSC



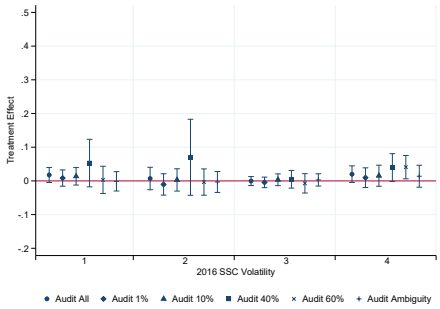
(a) Firm size: Number of employees



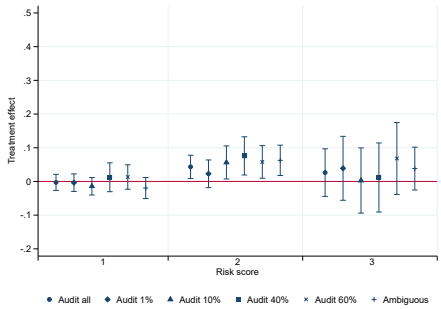
(b) Firm size: Pre-treatment tax base SSC



(c) Industry



(d) Volatility: 2016 tax base SSC



(e) Risk score

Note: 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.

Table A.26: Extended sample: Treatment effects of audit probability messages relative to ambiguous message on SSC

Panel A:	**4 post-treatment months**				
	Audit all	*Audit 1%*	*Audit 10%*	*Audit 40%*	*Audit 60%*
	(1)	(2)	(3)	(4)	(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	*Audit 1%*	*Audit 10%*	*Audit 40%*	*Audit 60%*
	(1)	(2)	(3)	(4)	(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

Note: 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 A.27: Extended sample: Additional SSC revenue per letter

Panel A:		**Moral Treatments**				
		Moral all	*Cooperation*	*Example*	*Necessity*	*Picture*
		(1)	(2)	(3)	(4)	(5)
Immediate	(*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	(*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	*Audit 1%*	*Audit 10%*	*Audit 40%*	*Audit 60%*	*Ambiguous*
		(1)	(2)	(3)	(4)	(5)	(6)
Immediate	(*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	(*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

Note: 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* $\times$ BGN *50/hour*) in the deterrence treatments. The underlying sample includes firms that do not directly communicate with the tax authority.

A.5 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 (A.5). We then present the main effects of our experimental interventions on the reported VAT base (90). Finally, we investigate potential spillover effects of treatment-induced SSC adjustments on VAT reporting behavior (90).

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%.[90] 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.

Table A.28: Treatment effects of moral treatments on VAT

Panel A:	**4 post-treatment months**				
	Moral all	*Cooperation*	*Example*	*Necessity*	*Picture*
	(1)	(2)	(3)	(4)	(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	*Cooperation*	*Example*	*Necessity*	*Picture*
	(1)	(2)	(3)	(4)	(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

Note: 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Treatment Effects

Table A.28 (for the *Moral* treatments) and Table A.29 (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.

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

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.

Table A.29: Treatment effects of deterrence treatments on VAT

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	-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	*Audit 1%*	*Audit 10%*	*Audit 40%*	*Audit 60%*	*Ambiguous*
	(1)	(2)	(3)	(4)	(5)	(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

Note: 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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.[91]

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 A.30 (for *Moral* treatments) and A.31 (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 A.28 and A.29), this finding is encouraging news for tax authorities and adds an important

[91] 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).

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.

Table A.30: VAT spillover: Treatment effects of moral treatments on VAT

Panel A:	**4 post-treatment months**				
	Moral all	*Cooperation*	*Example*	*Necessity*	*Picture*
	(1)	(2)	(3)	(4)	(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	*Cooperation*	*Example*	*Necessity*	*Picture*
	(1)	(2)	(3)	(4)	(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

Note: 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 A.31: VAT spillover: Treatment effects of deterrence treatments on VAT

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	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	*Audit 1%*	*Audit 10%*	*Audit 40%*	*Audit 60%*	*Ambiguous*
	(1)	(2)	(3)	(4)	(5)	(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

Note: 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Extended Sample (Including Firms that Do Not Directly Communicate with the Tax Authority)

Table A.32: Extended sample: Treatment effects of moral treatments on VAT

Panel A:	**4 post-treatment months**				
	Moral all	*Cooperation*	*Example*	*Necessity*	*Picture*
	(1)	(2)	(3)	(4)	(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	*Cooperation*	*Example*	*Necessity*	*Picture*
	(1)	(2)	(3)	(4)	(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

Note: 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 A.33: Extended sample: Treatment effects of deterrence treatments on VAT

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	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	*Audit 1%*	*Audit 10%*	*Audit 40%*	*Audit 60%*	*Ambiguous*
	(1)	(2)	(3)	(4)	(5)	(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

Note: 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 A.34: Extended sample: VAT spillover: Treatment effects of moral treatments on VAT

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

Note: 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 A.35: Extended sample: VAT spillover: Treatment effects of deterrence treatments on VAT

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	17679.286***	16254.251*	17407.880*	28850.086**	5788.243	8930.415**
	(5222.601)	(8347.274)	(9350.934)	(12877.308)	(6441.780)	(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	*Audit 1%*	*Audit 10%*	*Audit 40%*	*Audit 60%*	*Ambiguous*
	(1)	(2)	(3)	(4)	(5)	(6)
Absolute effect	11678.533***	8621.237*	14803.069**	20739.543***	-3075.322	5354.490
	(3571.895)	(4736.911)	(7318.754)	(7282.721)	(7350.106)	(4952.397)
Observations	171532	150013	148950	141490	139229	149680
No of firms	13545	11004	10933	10392	10230	10986

Note: 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.6 Tax Survey I

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 A.36 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 A.2. 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.[92]

[92] The original survey is available upon request.

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.[93]

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 A.37 limits results to the control group of the survey experiment only. Results are consistent with those reported for the full survey population.

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 A.36 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 A.36). Here, we elicited four different dimensions of intrinsic tax morale. In particular, we asked survey participants to rate (on a scale from 1 - strongly

[93] 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).

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 A.36). 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 A.23 and Figure A.24 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 A.23 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 disagree that cheating is justified. The bottom-left panel of Figure A.23 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 A.24 presents detailed information about firms opinion about cash payments as a means for tax evasion. 55% of the respondents indeed believe that cash

Table A.36: 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

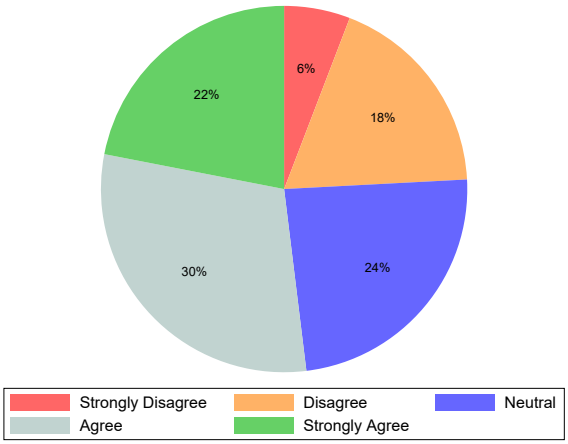
Note: Averages with standard deviation in parentheses. Column 1: All questions answered. Column 2: Not all questions answered. Column 3: Average over all respondents. **Panel A:** Mean of variables reflecting tax morale. **Panel B:** Questions reflect statements concerning the evasion channel. In Panel 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.

payments are used to evade taxes.

Figure A.23: Tax Morale I–Tax Morale IV

Note: 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 A.24: Cash payments as a means for tax evasion



Note: 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 A.37: 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

Note: 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.

A.7 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.[94] 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.[95] We therefore only report results from questions related to this paper.[96] 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 A.25 and Figure A.26 graphically illustrate how employers and employees report how they pay and currently receive their salary and, in case of a salary in cash, who

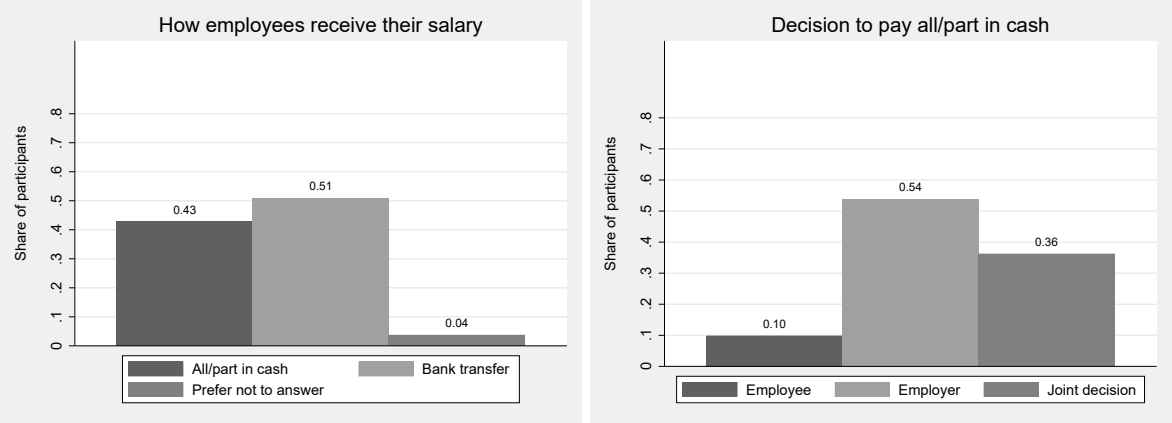
[94] Note that we targeted to receive 250 completed survey responses but received more completes.
[95] We received ethical approval by the Ethics Assessment Committee Faculty of Law and Nijmegen School of Management (ECLAM); EACLM Ref No: 2023.43.
[96] The full survey and all the results are available upon request.

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 A.27 and A.28 solidify our statement. We ask employees whether they privately engage in consumption smoothing and have any financial assets/investments to provide for their 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 A.27). 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 A.28 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 A.28).

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 A.25: Employers: Form of salary and driving force for cash salary

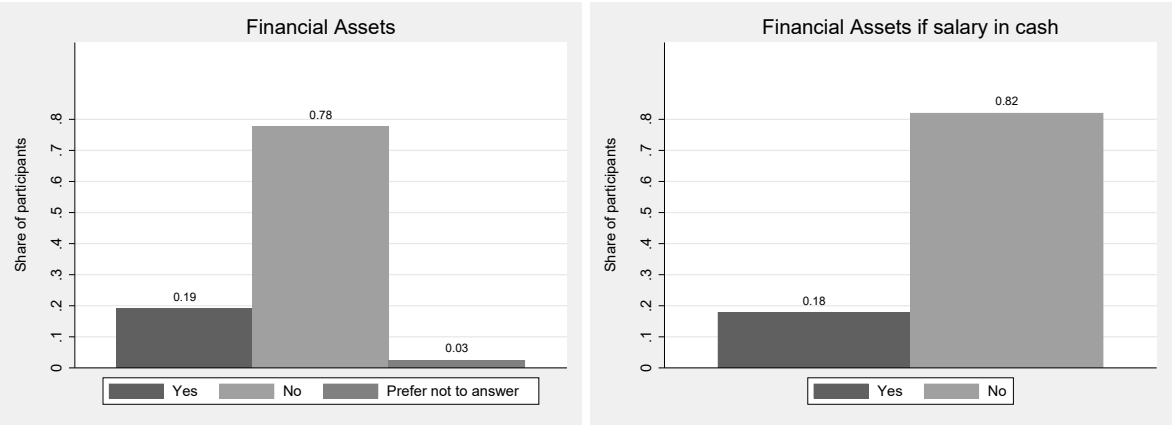


Note: Self reported way of paying salary (left panel), driving force for paying cash salaries conditional on paying part/all of the salary in cash (right panel).

Figure A.26: Employees: Form of salary and driving force for cash salary

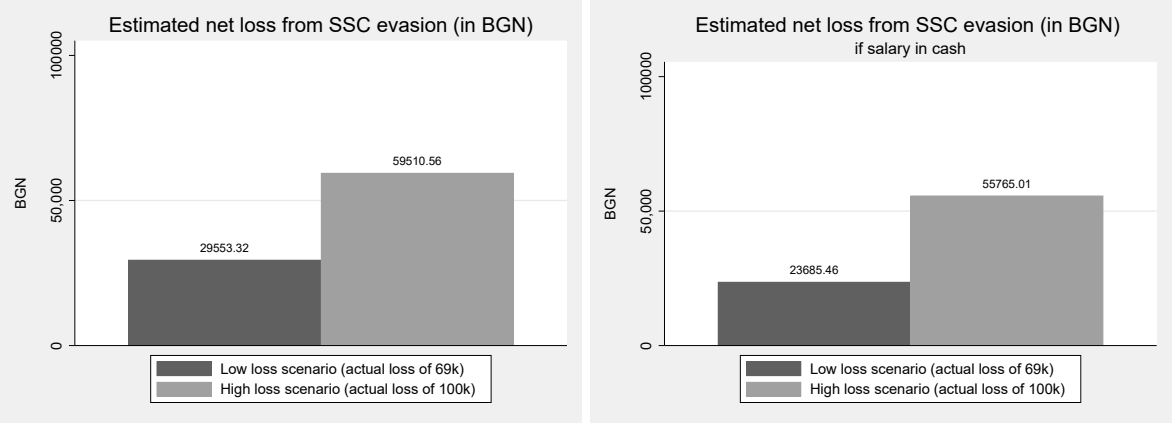
Note: Self reported way of receiving the salary (left panel), driving force for receiving the salary in cash conditional on being payed part/all of the salary in cash (right panel).

Figure A.27: Share of employees reporting to have financial assets



Note: 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 A.28: Estimated net loss of future benefits from SSC evasion



Note: 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.

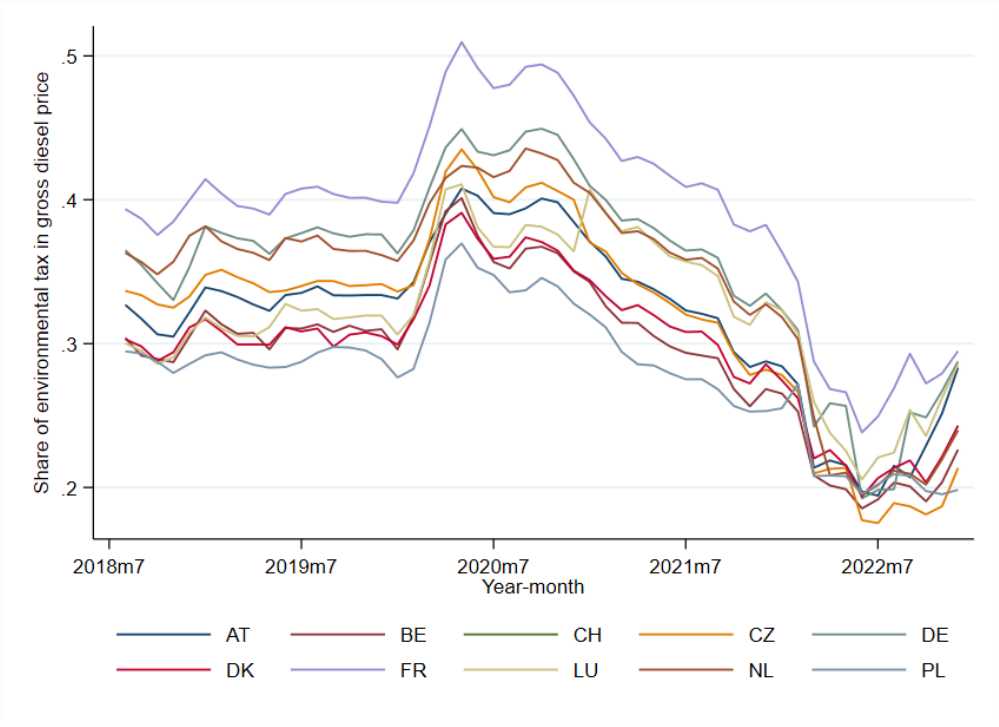
A.8 Variable Definitions

Variable	Definition
SSC tax base	SSC tax base for firm i in month t.
TREAT	Indicator variable equal to one if firm i received treatment j and zero if it is in the baseline condition.
POST	Indicator variable indicating the months t after the treatment.
Number of employees	Pre-experimental 2016 values of firm i's number of employees measured in categories (less than 11, 11 to 30, more than 30).
Industry	Firm i's industry.
VAT tax base	VAT tax base for firm i in month t.

B Appendix to Chapter 3

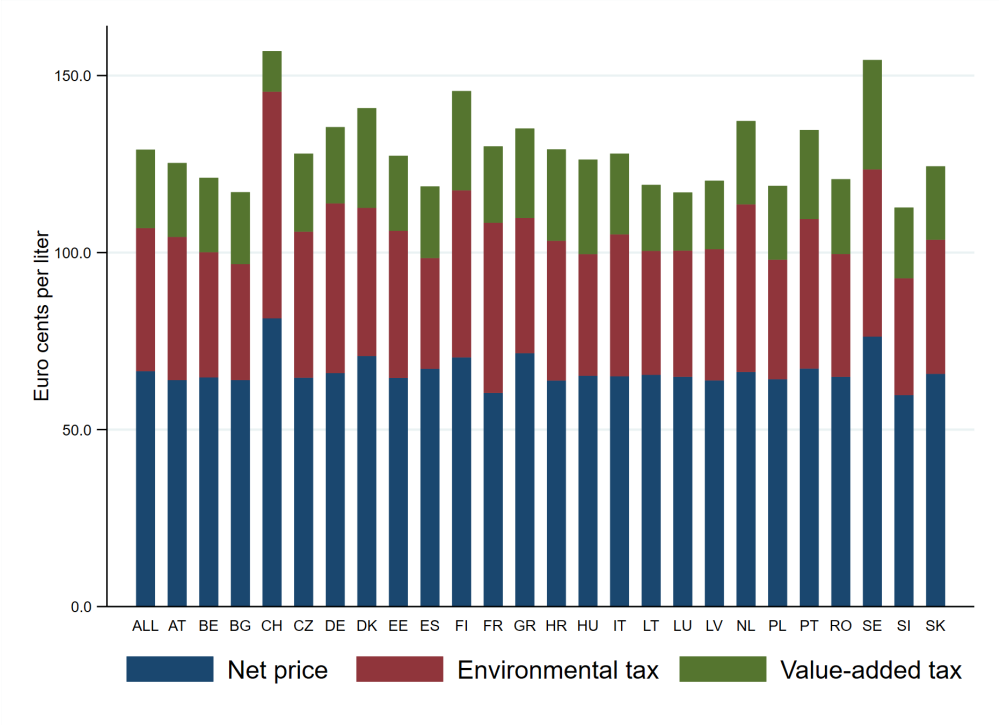
B.1 Environmental Taxes and Diesel Prices

Figure B.1: Share of environmental taxes in gross diesel price



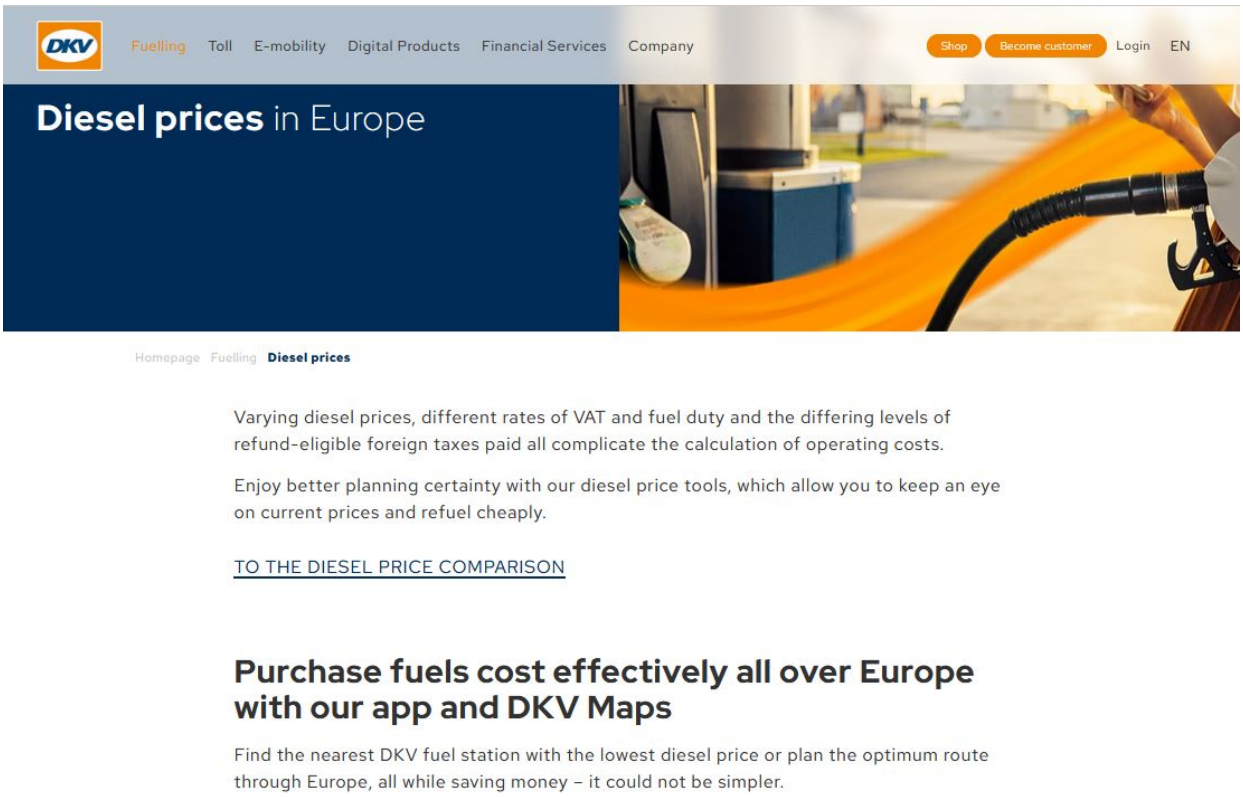
Note: The figure illustrates the share of environmental taxes in the total gross price of diesel in Germany and its neighboring countries during the period spanning from mid-2018 to 2022.

Figure B.2: Diesel price components by country



Note: The figure illustrates the components of the total gross price of diesel by continental European countries. The numbers displayed are averages over the time period 2011 to 2022. The components are the net price of diesel, the environmental tax on diesel and the value-added tax. The price components are displayed in euro cents per liter of diesel.

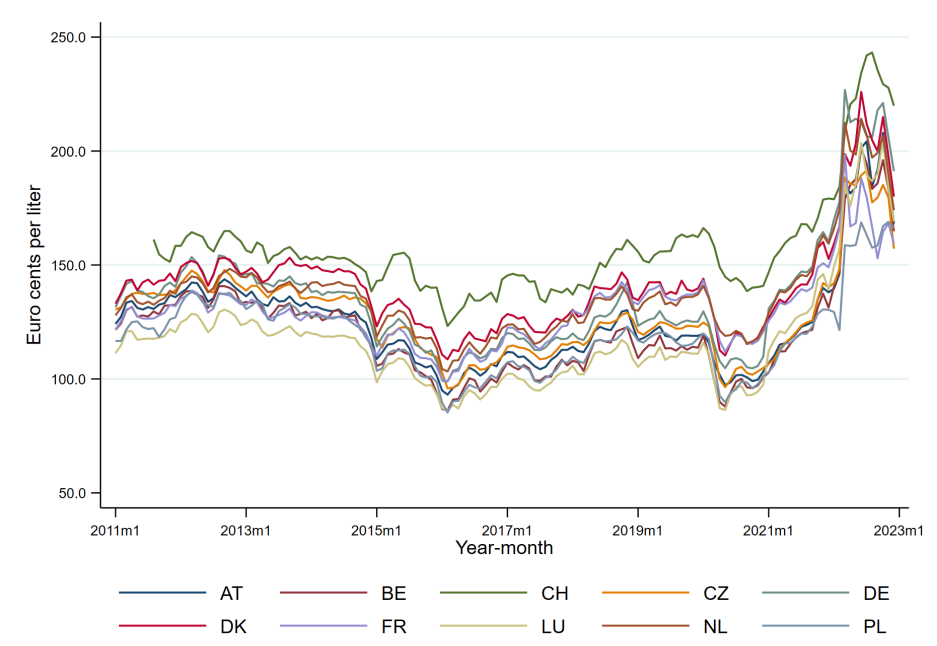
Figure B.3: Advertising diesel price tools for trucking companies



Note: The figure shows anecdotal evidence how the company DKV Mobility advertises diesel price tools for trucking companies.

Figure B.4: Net and gross diesel price by country

(a) Net price diesel

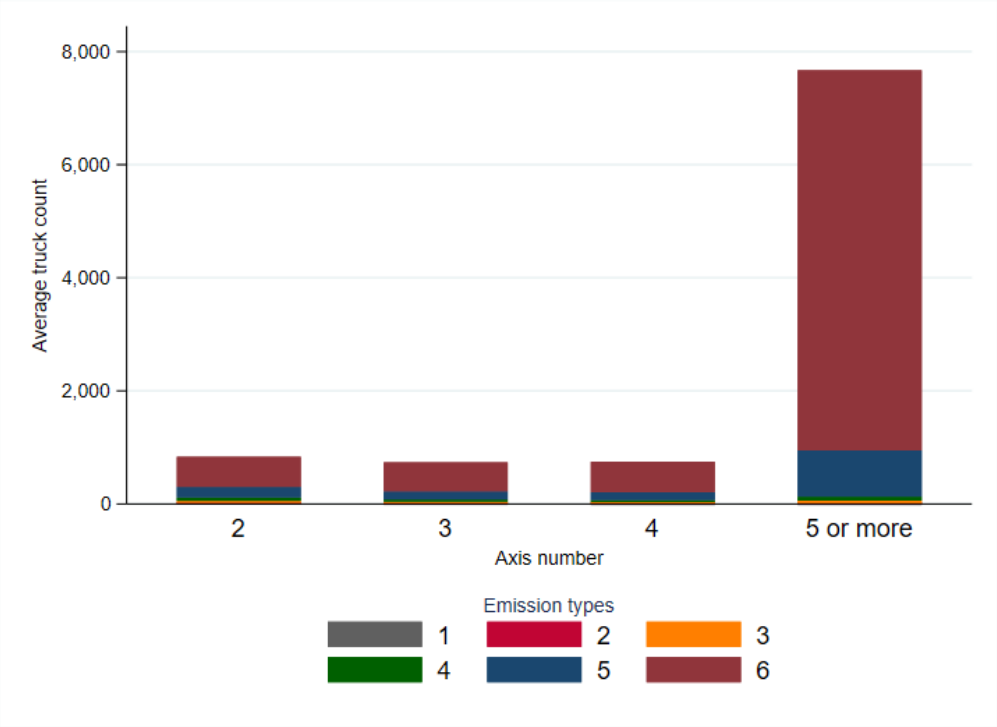


(b) Gross price diesel

Note: The figure plots the average monthly diesel price in Germany and its neighboring countries for our sample period 2011 to 2022. Panel (a) shows the average monthly net price by country. Panel (b) displays the average monthly gross price including VAT and environmental taxes by country. Prices are displayed in euro cents per liter of diesel.

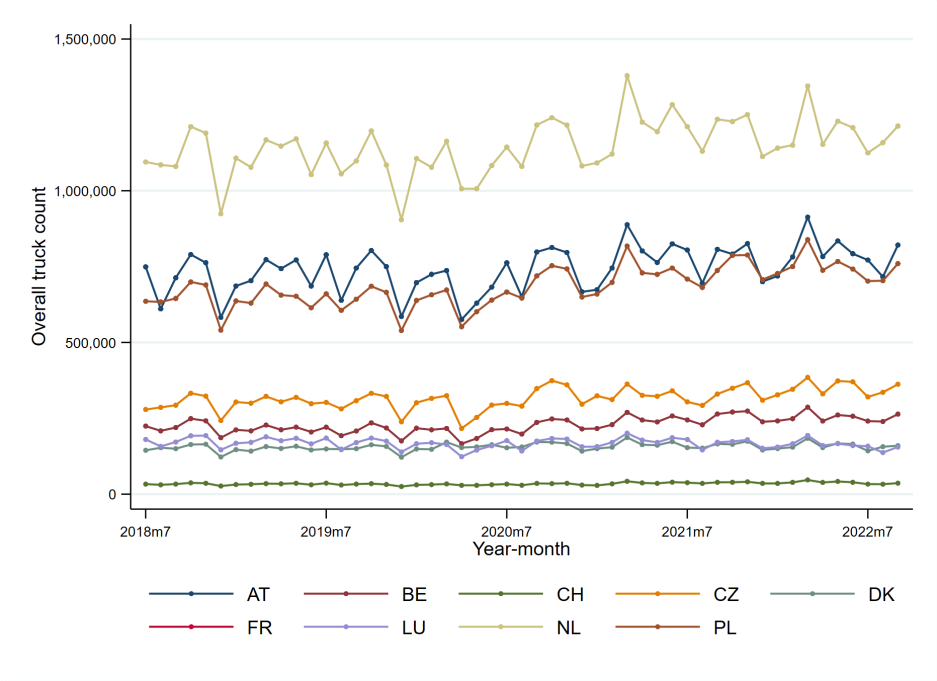
B.2 Toll Data

Figure B.5: Toll data: Average truck count by axes and emission type

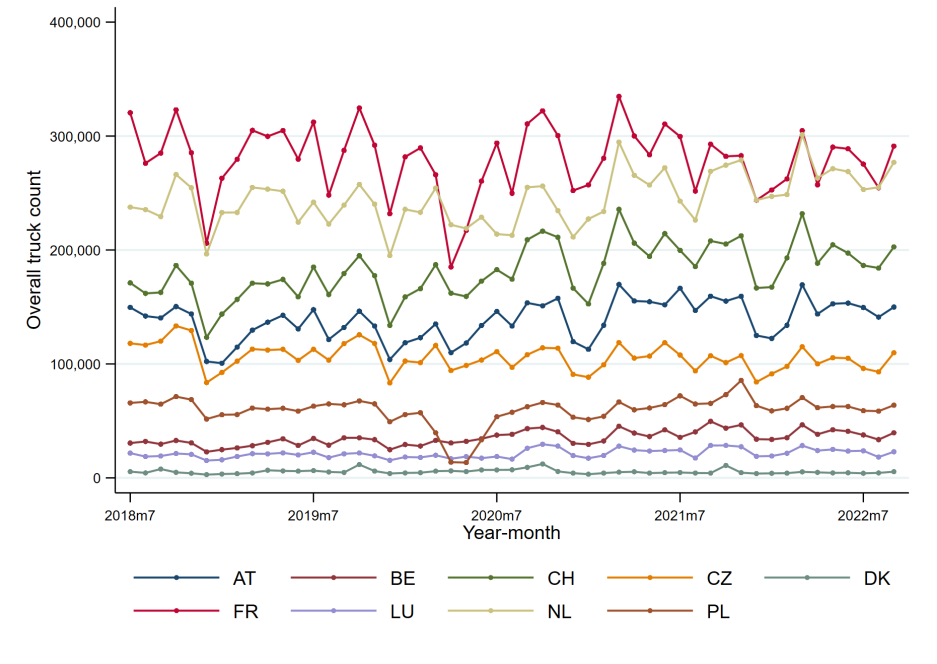


Note: The figure illustrates the average monthly truck count in our toll data categorized by axis number and emission type. Vehicles with two axes are for example tractors or combined harvesters. Vehicles with three axes are for example car cranes. Trucks in our sample are considered vehicles with more than four axes that are heavily used for road freight transportation. The emission type is based on the european standard for exhaust emissions, Euro 1 to Euro 6. The standards were implemented over time, with Euro 6 being the most recent requirement for newly registered vehicles.

Figure B.6: Toll data: Truck count at border



(a) Highway



(b) Federal road

Note: The figure plots the monthly overall truck count at the borders to Germany's neighboring countries. Panel (a) illustrates the monthly truck count at cross-border road-section on highways. Panel (b) displays the monthly truck count at cross-border road-section on federal roads.

Table B.1: Toll data: Average monthly truck count by distance to border and country

		Full Sample	Border	Below 5 km	5-10 km	10-15 km
Overall	*Mean*	7598.59	17727.79	6174.56	6199.36	6469.74
	SD	(17840.57)	(32221.78)	(16257.18)	(13889.69)	(15924.99)
	Obs	691,098	13,094	86,137	72,248	67,178
Austria (AT)	*Mean*	6031.66	19210.92	7125.17	5649.30	4558.28
	SD	(15317.13)	(35621.10)	(19603.19)	(11698.00)	(10663.45)
	Obs	144,389	2,340	13,846	15,897	17,663
Belgium (BE)	*Mean*	3663.81	10165.92	3561.25	1807.36	2825.70
	SD	(11303.86)	(24675.20)	(14278.20)	(2595.45)	(8642.24)
	Obs	65,091	1,326	9,984	7,929	7,242
Switzerland (CH)	*Mean*	6054.93	6765.68	7436.03	6568.55	5141.10
	StD	(6160.11)	(7476.39)	(7616.15)	(6548.51)	(4835.80)
	Obs	77,530	1,632	20,444	14,908	40,546
Czech Republic (CZ)	*Mean*	6542.46	19319.05	5632.14	5446.97	5883.29
	SD	(13095.61)	(29455.54)	(11010.86)	(13949.86)	(14674.59)
	Obs	89,293	1,116	8,964	7,603	9,781
Denmark (DK)	*Mean*	7347.47	26952.02	2344.33	4184.25	5366.37
	SD	(15446.31)	(36443.93)	(1354.03)	(2856.95)	(2612.75)
	Obs	20,880	306	1,836	990	1,632
France (FR)	*Mean*	5481.52	6989.18	4812.66	4697.90	5082.10
	SD	(10744.58)	(12618.05)	(10025.92)	(9632.76)	(9708.28)
	Obs	102,787	2,039	17,118	12,600	7,905
Luxembourg (LU)	*Mean*	3933.19	11826.22	1402.98	4193.85	3626.22
	SD	(8430.01)	(18383.80)	(1439.32)	(6019.25)	(3874.70)
	Obs	44,873	816	5,428	4,831	6,287
Netherlands (NL)	*Mean*	18485.58	33117.67	10999.48	11264.38	20448.70
	SD	(32107.47)	(41153.58)	(27004.83)	(23948.83)	(37228.94)
	Obs	92,305	2,142	12,203	10,196	6,836
Poland (PL)	*Mean*	9058.41	27600.60	7362.24	9457.13	4795.32
	SD	(23800.40)	(46053.79)	(22162.41)	(23624.03)	(6926.83)
	Obs	53,950	1,377	4,790	3,726	3,804

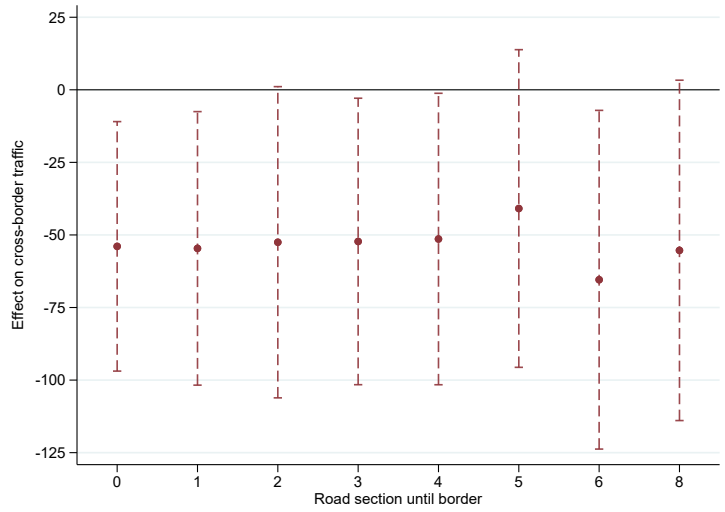
Note: The table reports average monthly truck counts categorized by road-section and destination country. Full sample includes all road-sections up to 50 km from the border. Border includes only the direct road-section at the border itself. Below 5 km, 5-10 km and 10-15 km comprise all road-sections within the respective corridor distances from the border.

Table B.2: Toll data: Alternative specification

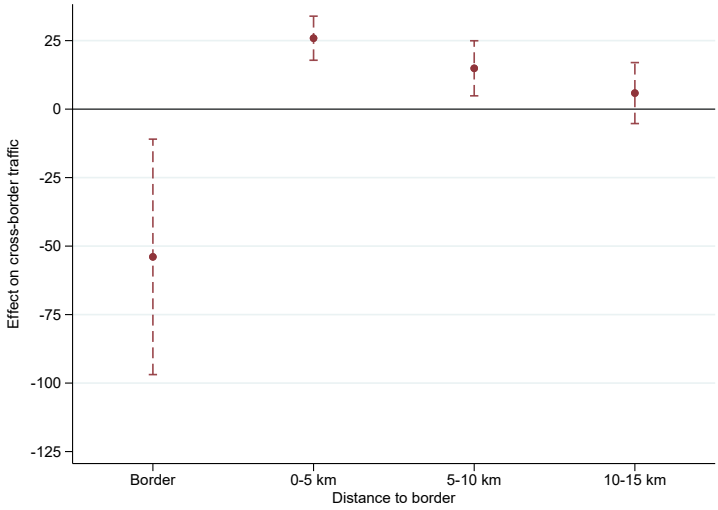
	Log(Cross-border traffic)		
	Highway		
	OLS	*2SLS*	*RF*
	(1)	(2)	(3)
Env. tax differential			-0.002
			(0.002)
Gross price differential	-0.002***	-0.003***	
	(0.001)	(0.001)	
Year-month FE	X	X	X
road-section FE	X	X	X
Observations	2,550	2,550	2,550

Note: The table reports results from the toll data analysis using a log specification for the dependent variable cross-border traffic. Column (1) shows results from an OLS specification and (2) from an IV 2SLS regression. The dependent variable is log of the monthly truck count on a cross-border road-section. The independent variable is the (instrumented) gross price differential. Column (3) shows the results of the reduced form regressing log(Cross-border traffic) on the environmental tax differential. The environmental tax differential and the gross price differential are calculated relative to Germany (tax/price in neighboring country - tax/price in Germany) and measured in euro cent per liter of diesel fuel. In all specifications we include road-section and year-month fixed effects. Standard errors are clustered on the road-section level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure B.7: Toll data: Full sample effect by distance to border



(a) By road-section until border



(b) By distance to border

Note: The figure plots the effect of environmental taxes on commercial traffic by road-section and distance to the border for the full sample including highways and federal roads. The points plotted are the 2SLS estimates regressing the monthly truck count on the instrumented gross price differential. The gross price differential is calculated relative to Germany (price in neighboring country - price in Germany) and measured in euro cent per liter of diesel fuel. Panel (a) presents the effects by road-section until border. Panel (b) presents the estimates by grouping road-section by their distance to the border. 95% confidence intervals are represented by the dashed red lines lines and based on standard errors clustered on the road-section level.

Table B.3: Toll data: Robustness Covid-19

	Panel A: Second stage Cross-border traffic		**Panel B: Reduced form** Cross-border traffic	
	Full sample	*Highway*	*Full sample*	*Highway*
	(1)	(2)	(1)	(2)
Env. tax differential			-44.254**	-326.573***
			(18.049)	(88.811)
Gross price differential	-49.237**	-459.794***		
	(20.815)	(111.871)		
Year-month FE	X	X	X	X
road-section FE	X	X	X	X
Observations	12585	2450	12585	2450

Note: The table reports robustness tests of the toll data analysis excluding all months (032020 - 04020) that are affected by the Covid-19 pandemic. Panel A shows the results from the second stage regression. Panel B shows the results form the reduced form. The dependent variable is the monthly truck count on a cross-border road-section. The independent variable is the instrumented gross price differential in Panel A and the environmental tax differential in Panel B. The environmental tax differential and the gross price differential are calculated relative to Germany (tax/price in neighboring country - tax/price in Germany) and measured in euro cent per liter of diesel fuel. Columns (1) show the results for the full sample, Columns (2) for highways. In all specifications we include road-section and year-month fixed effects. Standard errors are clustered on the road-section level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.4: Toll data: Robustness Ukraine war

	Panel A: Second stage Cross-border traffic		**Panel B: Reduced form** Cross-border traffic	
	Full sample	*Highway*	*Full sample*	*Highway*
	(1)	(2)	(1)	(2)
Env. tax differential			-68.992***	-484.053***
			(24.746)	(99.859)
Gross price differential	-165.044***	-552.531***		
	(61.591)	(146.755)		
Year-month FE	X	X	X	X
road-section FE	X	X	X	X
Observations	11034	2150	11034	2150

Note: The table reports robustness tests of the toll data analysis excluding all months (022022 - 12022) that are affected by the Ukraine. Panel A shows the results from the second stage regression. Column (2) the results form the reduced form. The dependent variable is the monthly truck count on a cross-border road-section. The independent variable is the instrumented gross price differential in Panel A and the environmental tax differential in Panel B. The environmental tax differential and the gross price differential are calculated relative to Germany (tax/price in neighboring country - tax/price in Germany) and measured in euro cent per liter of diesel fuel. Columns (1) show the results for the full sample, Columns (2) for highways. In all specifications we include road-section and year-month fixed effects. Standard errors are clustered on the road-section level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.5: Toll data: Highway effect by country

		Cross-border traffic		
		Second stage (1)	*Reduced form* (2)	*Observations* (3)
Austria (AT)	*Env. tax differential*		-708.090***	510
			(145.250)	
	Gross price differential	-335.246***		510
		(65.176)		
Belgium (BE)	*Env. tax differential*		-670.545**	204
			(133.209)	
	Gross price differential	-411.137***		204
		(70.559)		
Switzerland (CH)	*Env. tax differential*		157.447	102
			(137.209)	
	Gross price differential	-206.558		102
		(126.654)		
Czech Republic (CZ)	*Env. tax differential*		-882.522***	204
			(60.238)	
	Gross price differential	-403.141***		204
		(23.772)		
Denmark (DK)	*Env. tax differential*		-453.825**	102
			(9.138)	
	Gross price differential	-209.418***		102
		(2.967)		
Luxembourg (LU)	*Env. tax differential*		-207.071***	204
			(34.003)	
	Gross price differential	9285.859***		204
		(1317.286)		
Netherlands (NL)	*Env. tax differential*		-514.451***	816
			(89.554)	
	Gross price differential	-291.295***		816
		(49.067)		
Poland (PL)	*Env. tax differential*		-1090.498***	408
			(65.176)	
	Gross price differential	-509.069***		408
		(205.568)		

Note: The table reports results of the toll data analysis by country on highways. Column (1) shows the results from the second stage regression. Column (2) the results form the reduced form. The dependent variable is the monthly truck count on a cross-border road-section. The independent variable is the instrumented gross price differential in the second stage specification and the environmental tax differential in the reduced form specification. The environmental tax differential and the gross price differential are calculated relative to Germany (tax/price in neighboring country - tax/price in Germany) and measured in euro cent per liter of diesel fuel. Standard errors are clustered on the road-section level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B.3 Survey Data

Table B.6: Survey data: Meta-information

	Global sampling rate	Response rate (2012)
Austria (AT)	0.8	98.5
Belgium (BE)	0.9	65.3
Bulgaria (BG)	0.2	69.8
Switzerland (CH)	0.3	58.6
Czech Republic (CZ)	0.3	91.9
Germany (DE)	0.4	96.3
Denmark (DK)	0.4	99.0
Estonia (EE)	0.7	75.5
Spain (ES)	0.3	94.3
Finland (FI)	0.1	58.1
France (FR)	0.3	77.9
Greece (GR)	0.1	83.8
Croatia (HR)	0.8	80.1
Hungary (HU)	1.3	87.5
Italy (IT)	0.4	25.5
Lithuania (LT)	0.6	90.1
Luxembourg (LU)	3.3	91.9
Latvia (LV)	0.5	78.4
Netherlands (NL)	0.5	76.6
Norway (NO)	0.3	95.6
Poland (PL)	0.1	84.7
Portugal (PT)	0.9	75.4
Romania (RO)	0.7	97.3
Sweden (SE)	0.4	70.1
Slovenia (SI)	0.7	74.5
Slovakia (SK)	0.1	87.5
Average	0.7	80.5

Note: The table reports meta-data about the European Road Freight Transport Statistics. The figures are obtained from the 2014 EUROSTAT publication "Methodologies used in surveys of road freight transport in Member States, EFTA and Candidate Countries". The global sampling rate and the response rate are expressed as a percentage across time (yearly weeks) and space (countries).

Table B.7: Survey data: Probit specification

	Panel A: Second stage Deviation	**Panel B: Reduced form** Deviation
	(1)	(1)
Env. tax average		0.034***
		(0.007)
Gross price average	0.054***	
	(0.007)	
Observations	1,182,316	1,182,339

Note: The table reports results from the survey data analysis using a probit model. Panel A shows results from the second stage regression, Panel B from the reduced form. The dependent variable is the deviation indicator and the independent variable is the instrumented average diesel gross price on the journey for the second stage and the environmental tax differential for the reduced form. Robust standard errors are included in parenthesis. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

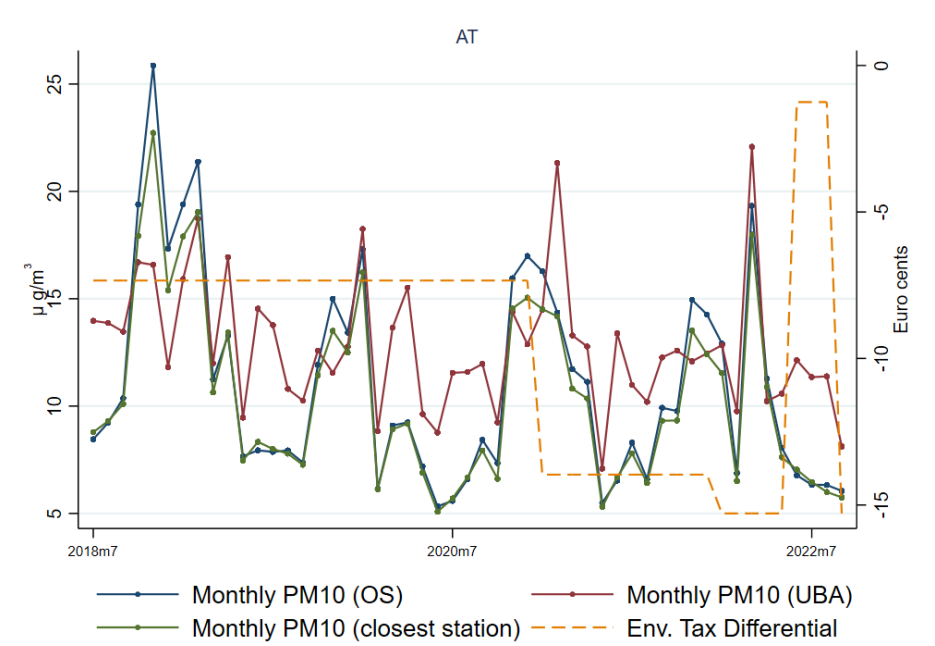
Table B.8: Survey data: Share of routes by country

	Share overall routes	Share deviating routes	Ratio per country
Austria (AT)	0.110	0.279	2.531
Belgium (BE)	0.352	0.414	1.177
Bulgaria (BG)	0.001	0.000	0.000
Switzerland (CH)	0.043	0.075	1.733
Czech Republic (CZ)	0.072	0.092	1.274
Germany (DE)	0.522	0.709	1.359
Denmark (DK)	0.013	0.001	0.095
Estonia (EE)	0.007	0.000	0.052
Spain (ES)	0.056	0.012	0.223
Finland (FI)	0.001	0.000	0.285
France (FR)	0.242	0.223	0.919
Greece (GR)	0.000	0.000	0.000
Croatia (HR)	0.008	0.015	1.887
Hungary (HU)	0.065	0.069	1.064
Italy (IT)	0.074	0.113	1.538
Lithuania (LT)	0.022	0.006	0.295
Luxembourg (LU)	0.075	0.135	1.801
Latvia (LV)	0.019	0.004	0.199
Netherlands (NL)	0.377	0.487	1.291
Norway (NO)	0.000	0.000	0.000
Poland (PL)	0.070	0.062	0.882
Portugal (PT)	0.019	0.002	0.130
Romania (RO)	0.009	0.014	1.522
Sweden (SE)	0.003	0.000	0.171
Slovenia (SI)	0.037	0.045	1.220
Slovakia (SK)	0.047	0.068	1.443
Observations	1,285,633	80,687	1,285,633

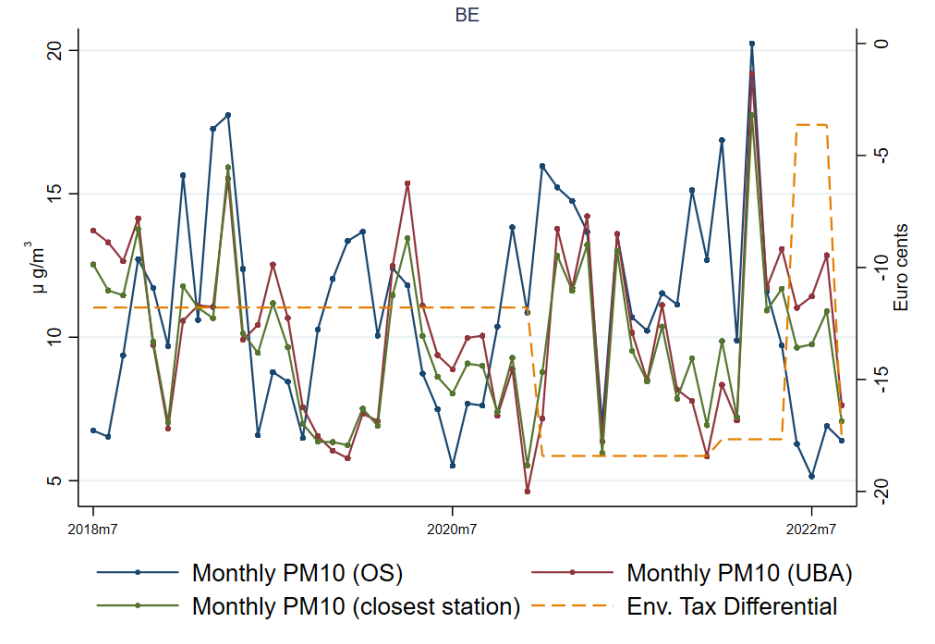
Note: This table shows the country share of truck traffic routes in Europe. The first column provides the share of overall journeys passing through a particular country. The second colum provides the same statistic, but only for the subset of routes which diverge from the optimal route calculated via GMAP. The third and last column contains the ratio between the first two columns, indicating whether a specific country is part of detours relative to its overall importance in international truck traffic.

B.4 Emission Data

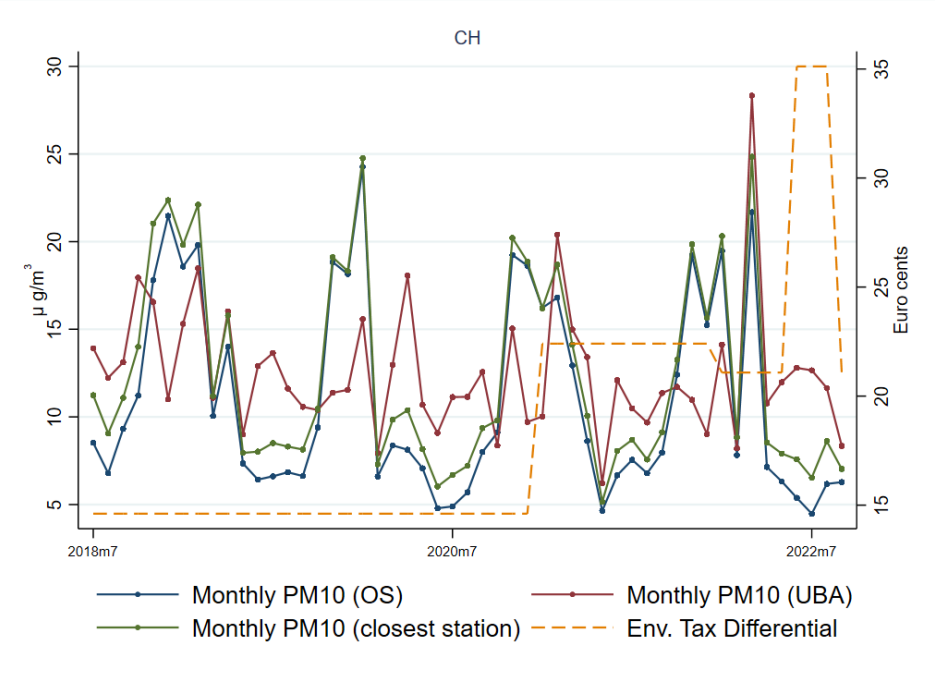
Figure B.8: Emission data: Environmental tax differentials and emissions by border-country



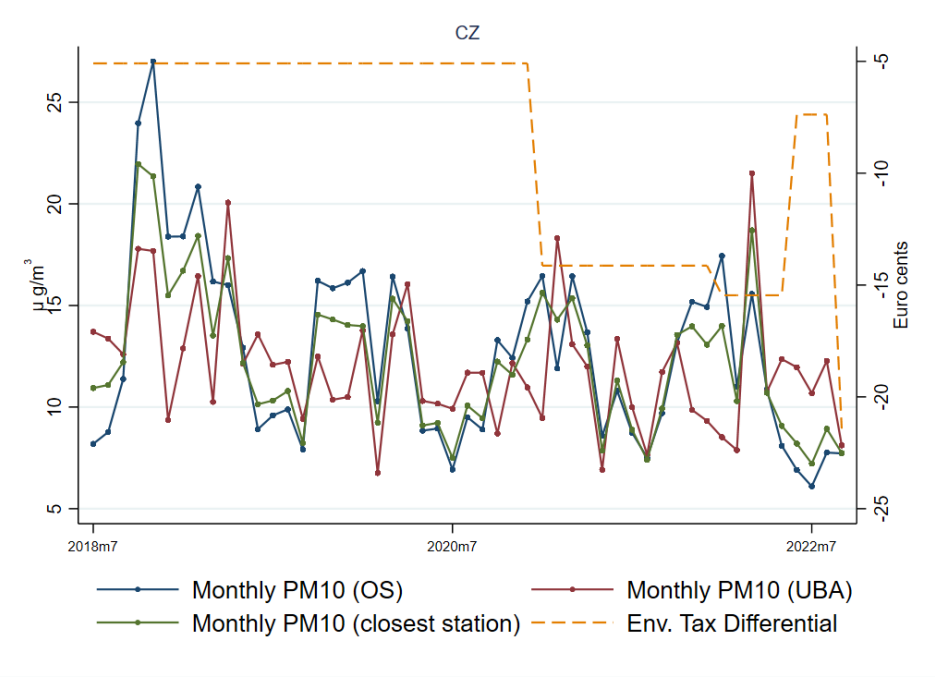
(a) Austria



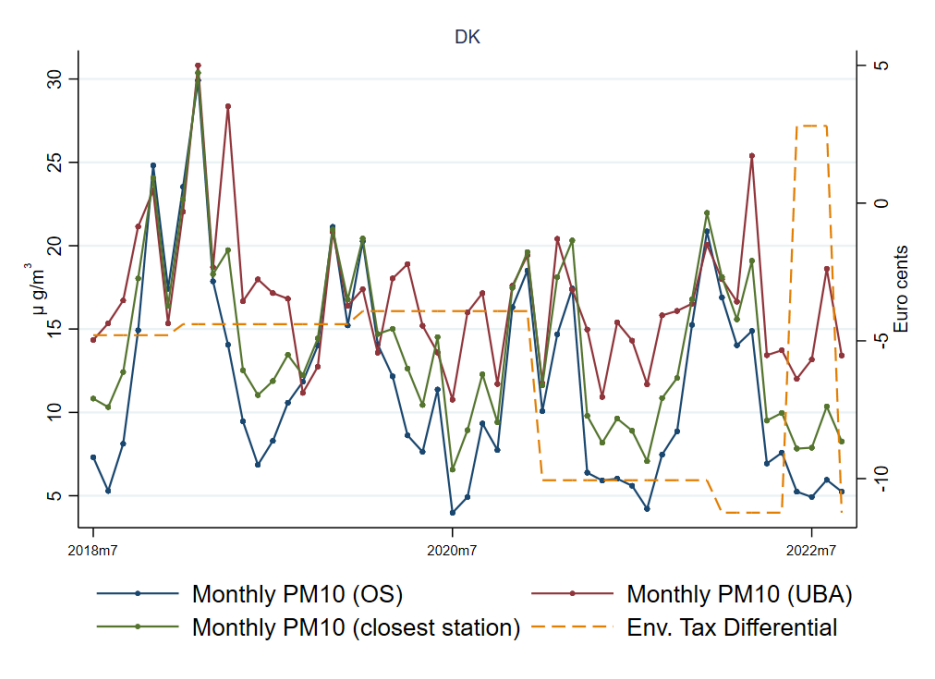
(b) Belgium



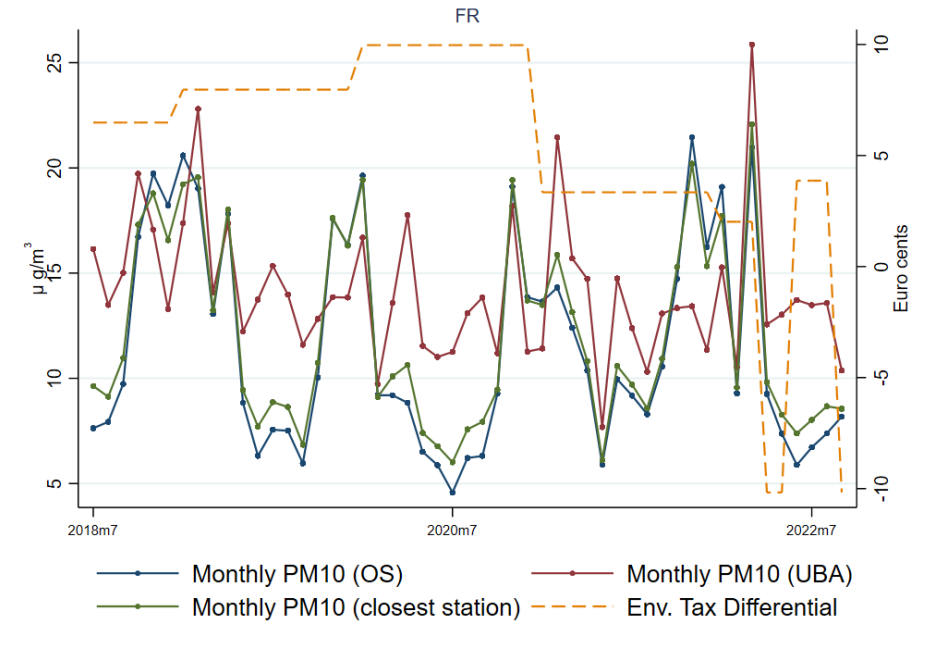
(c) Switzerland



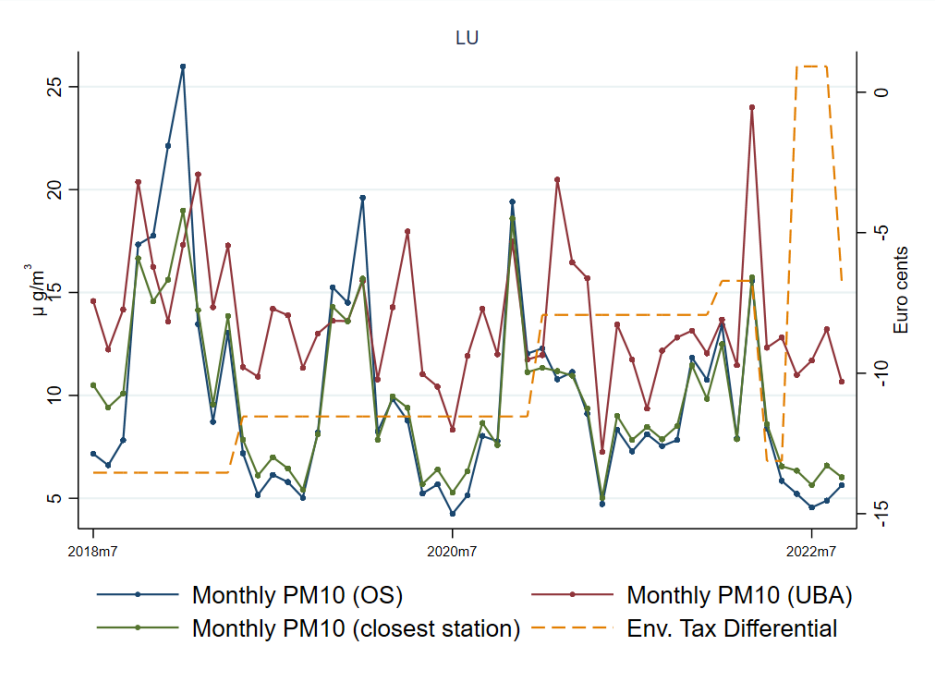
(d) Czech Republic



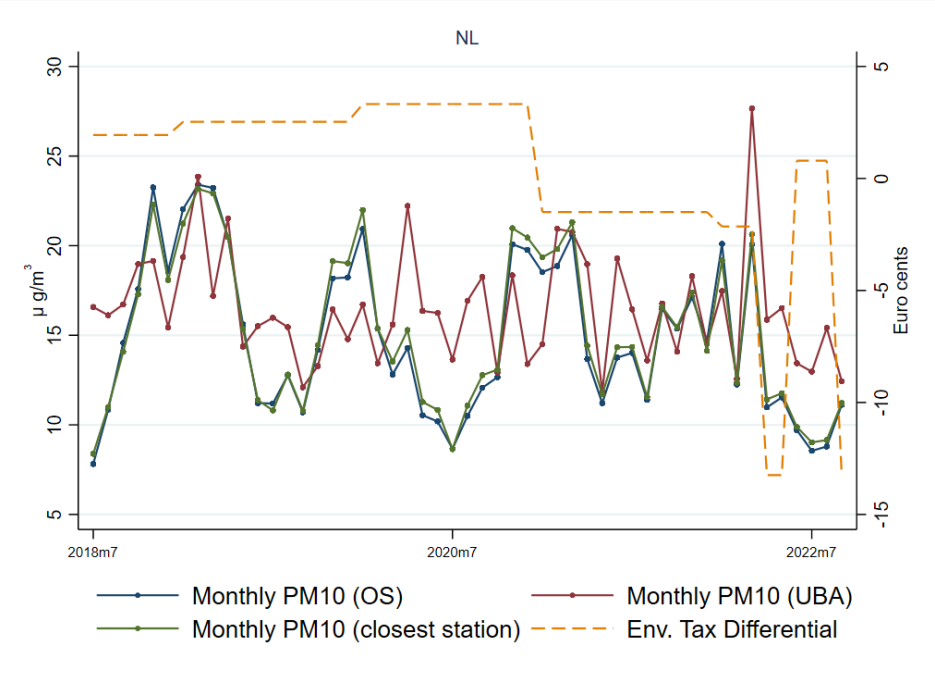
(e) Denmark



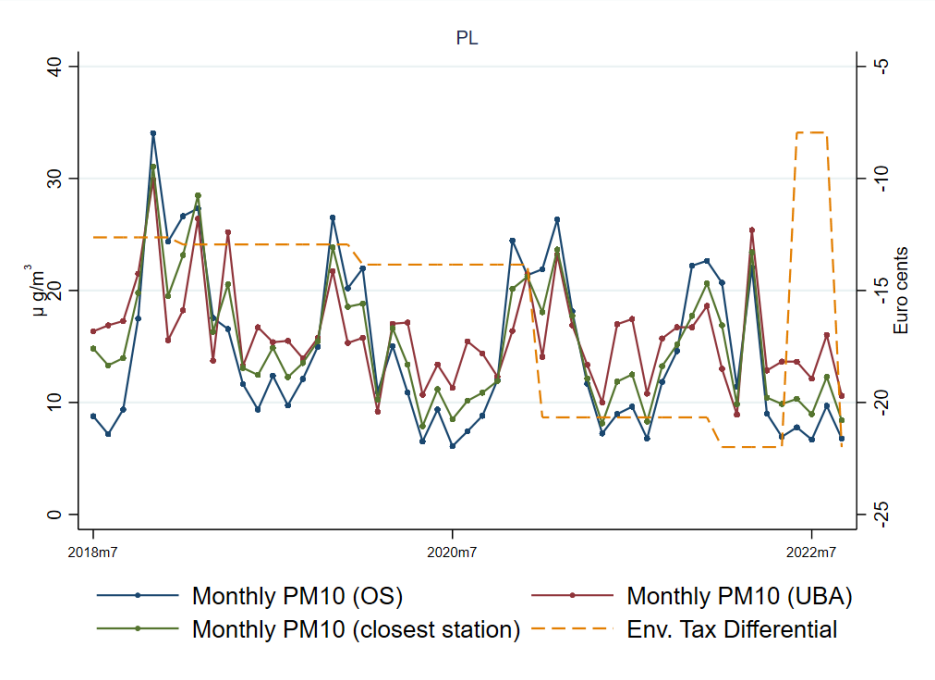
(f) France



(g) Luxembourg



(h) Netherlands



(i) Poland

Note: The figure plots average environmental emissions on cross-border roads and the corresponding environmental tax differential between Germany and its neighboring countries over the time period from July 2018 until September 2022. Monthly PM10 is measured in $\mu g/m^3$, and shown for each data source separately. The environmental tax differential is relative to Germany (tax in neighboring country - tax in Germany) and measured in euro cent per liter of diesel fuel.

Table B.9: Emission data: Effect of tax differential on local pollution

	PM10					
Distance to	750	1,000	1,250	1,500	1,750	2,000
sensor station	(1)	(2)	(3)	(4)	(5)	(6)
Env. tax differential	-0.166**	-0.0749	-0.0982**	0.00165	0.0231	0.0312
	(0.07)	(0.05)	(0.05)	(0.03)	(0.03)	(0.03)
Constant	13.46***	13.02***	12.77***	12.52***	12.96***	13.23***
	(0.51)	(0.15)	(0.09)	(0.02)	(0.05)	(0.11)
Observations	278	493	539	885	1,168	1,689

Note: The table reports reduced form estimates of regressing local, cross-border measures of PM10 on cross-border environmental tax differentials using OLS. The term meters refers to the road-section distance to the closest emission sensor station. One-way clustered standard errors are reported in parentheses. All columns include road-section and year-month fixed effects. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

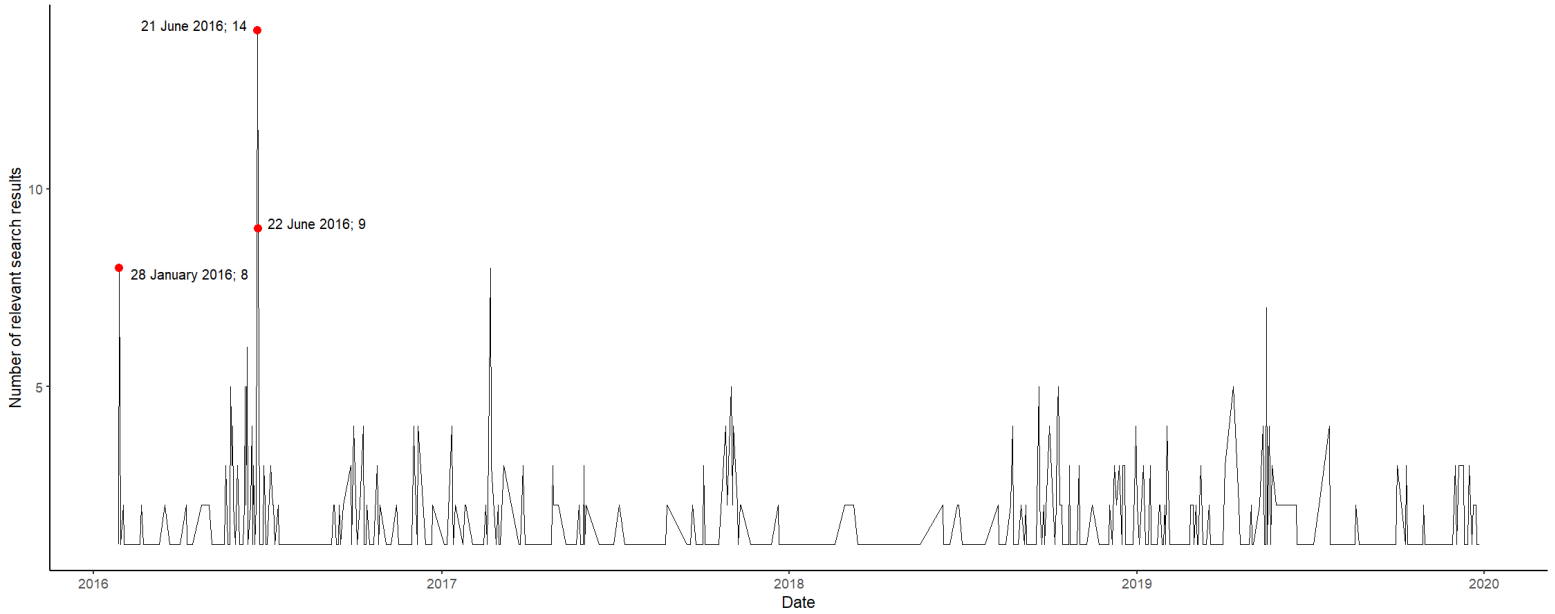
B.5 Variable Definitions

Variable	Definition
Toll data	
Gross price differential	Difference in the diesel gross price between Germany's neighboring country c and Germany in year-month m.
Env. tax differential	Difference in environmental tax rate on diesel between Germany's neighboring country c and Germany in year-month m.
Cross-border traffic	The number of trucks passing a road-section i in year-month m.
Survey data	
Gross price average	Average diesel gross price on journey j in year-month m.
Env. tax average	Average environmental tax rate on journey j in year-month m.
Deviation	Indicator variable equal to one if the truck deviated on a journey in distance and to another country.
Emission data	
Emissions	PM10 emissions measured by the closest sensor station to road-section i in year-month m.

C Appendix to Chapter 4

C.1 Institutional Background and Empirics

Figure C.1: Media coverage analysis



Note: This figure shows a media coverage analysis conducted with Factiva. We plot the number of relevant media articles over time when searching for 'anti-tax avoidance directive'.

Table C.1: Overview of CFC rules

Panel A: EU countries that newly introduced CFC rules		
	Enactment	Model
Austria (AT)	December 31, 2018	A
Belgium (BE)	December 31, 2018	B
Bulgaria (BG)	December 31, 2018	B
Cyprus (CY)	December 31, 2018	B
Czech Republic (CZ)	December 31, 2018	A
Estonia (EE)	December 31, 2018	B
Croatia (HR)	December 31, 2018	A
Ireland (IE)	December 31, 2018	B
Luxembourg (LU)	December 31, 2018	B
Latvia (LV)	December 31, 2018	B
Malta (MT)	December 31, 2018	B
Netherlands (NL)	December 31, 2018	A
Romania (RO)	December 31, 2017	A
Slovenia (SI)	December 31, 2018	A
Slovakia (SK)	December 31, 2018	B
Panel B: EU countries that already had CFC rules		
	Enactment	Model
Denmark (DK)	1995	A
Finland (FI)	1995	A/B
France (FR)	1980	A/B
Germany (DE)	1972	A
Greece (GR)	2014	A
Hungary (HU)	1997	B
Italy (IT)	2001	A
Lithuania (LT)	2014	A
Poland (PL)	2015	A
Portugal (PT)	1995	A
Spain (ES)	1995	A
Sweden (SE)	1989	B

Note: The table gives an overview of the enactment timing of CFC rules in EU countries and the corresponding model chosen. Panel A lists EU countries that newly introdued CFC rules. Panel B lists EU countries that had already implemented CFC rules prior to the ATAD.

Table C.2: Group sample: Sample creation table

		Group-year observations	Unique groups
		(1)	(2)
1)	EU-based groups with consolidated financial information	287,280	58,015
2)	Drop groups not within treatment or control group	269,567	54,755
3)	Drop bank and insurance groups	267,233	54,352
4)	Require groups to have at least one CFC subsidiary in any of the years	24,919	3,450
5)	Drop observations with negative values in control variables	24,919	3,450

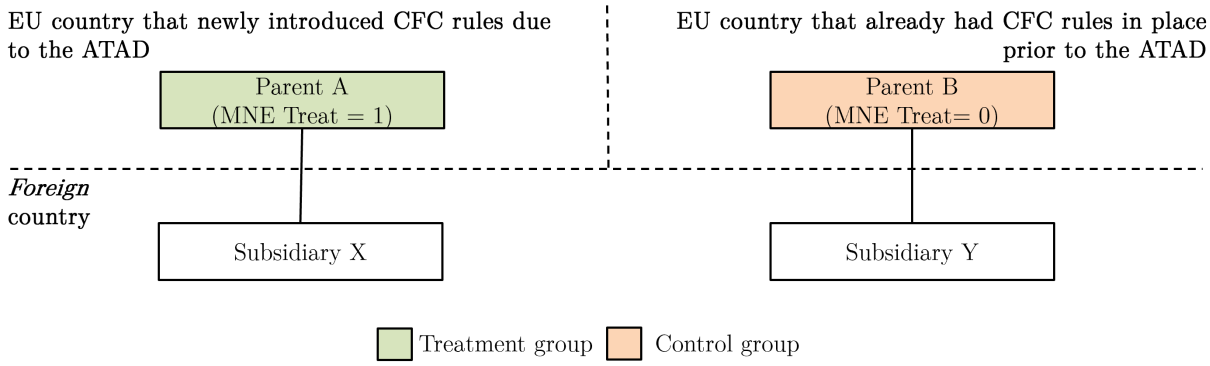
Note:The table displays the steps in the construction of the group sample.

Table C.3: Subsidiary sample: Sample creation table

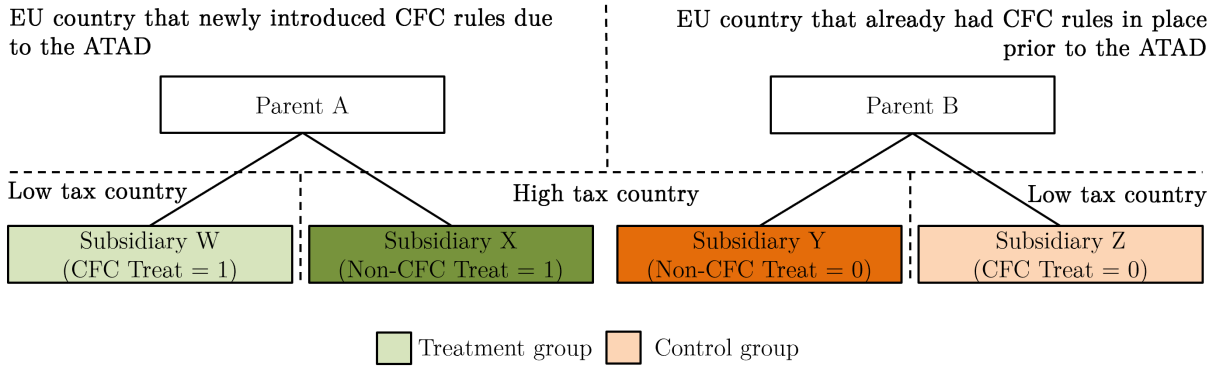
		Subsidiary-year observations	Unique subsidiaries
		(1)	(2)
1)	Subsidiaries of EU-based groups with unconsolidated financial information	26,846,438	5,950,445
2)	Drop subsidiaries not within treatment or control group	13,132,918	2,971,398
3)	Drop bank and insurance groups and subsidiaries	13,101,906	2,964,821
4)	Require subsidiaries to have an observation in 2012 and 2020	5,469,813	633,830
5)	Drop observations with negative values in dependent and control variables	5,452,747	633,783

Note: The table displays the steps in the construction of the subsidiary sample.

Figure C.2: Treatment and control group



(a) Group sample



(b) Subsidiary sample

Note: This figure shows the treatment and control groups for (a) the group sample and (b) the subsidiary sample.

C.2 Location Response

Table C.4: Location response: Robustness

	Share of non-CFC subsidiaries			
	Industry-year fixed effects	*Alternative dependent variable*	*Alternative control group*	*Delayed treatment*
	(1)	(2)	(3)	(4)
MNE Treat x Post	0.0210	-0.0904	0.0187	0.0421
	(0.0190)	(0.0620)	(0.0144)	(0.0320)
Group controls	X	X	X	X
Home-country controls	X	X	X	X
Group FE	X	X	X	X
Year FE		X	X	X
Industry-year FE	X			
Observations	15,161	15,185	10,852	9,993

Note: This table presents robustness tests for the location response. The dependent variable is the share of non-CFC subsidiaries to total subsidiaries. In Column (1) we use industry-year fixed effects. Column (2) uses an alternative dependent variable, the number of non-CFC subsidiaries. In Column (3) we adjust the control group containing only countries that did not change their CFC rules until 2021, including Germany, Denmark and Spain. Column (4) changes the post-period to 2019 and 2020. All specifications include home-country and group control variables. The home-country controls comprise GDP, GDPPC and the CIT rate. As group controls, we include total assets, leverage, intangible assets scaled by total assets and the average CIT rate across all foreign subsidiaries. Robust standard errors adjusted for clustering at the MNE level are reported in parentheses. ***, ** and * indicate significance at the 1%, 5% and 10% level.

Table C.5: Location response: Heterogeneity

	Share of CFC subsidiaries		Share of non-CFC subs.	
	Model A	*Model B*	*Model A*	*Model B*
	(1)	(2)	(3)	(4)
MNE Treat x Post	-0.1892**	-0.1368	0.0411	-0.0011
	(0.0745)	(0.0922)	(0.0361)	(0.0047)
Group controls	X	X	X	X
Home-country controls	X	X	X	X
Group FE	X	X	X	X
Year FE	X	X	X	X
Observations	14,128	14,356	14,138	14,370

Note: This table presents heterogeneity results of the DiD regressions estimated with PPML for model A and model B countries. The dependent variable is the share of CFC to total subsidiaries in Columns (1) and (2) and the share of non-CFC to total subsidiaries in Columns (3) and (4). The estimated effects are relative to the control group. The pre-treatment period spans from 2012 to 2015 while the post-treatment period spans from 2016 to 2020. MNE Treat is a dummy variable set to one for all MNEs in the treatment group and to zero for all MNEs in the control group. All specifications include home-country and group control variables. The home-country controls comprise GDP, GDPPC and the CIT rate. As group controls, we include total assets, leverage, intangible assets scaled by total assets and the average CIT rate across all foreign subsidiaries. Robust standard errors adjusted for clustering at the MNE level are reported in parentheses. ***, ** and * indicate significance at the 1%, 5% and 10% level.

C.3 Financial Response

Table C.6: Financial response: Robustness

	Financial income – CFC subsidiaries					
	Industry-year fixed effects	*Pre-treatment controls*	*Alternative dependent variable*	*Comprehensive control group*	*Alternative treatment definition*	*Delayed treatment*
	(1)	(2)	(3)	(4)	(5)	(6)
CFC Treat x Post	0.130	-0.474	-0.673	-0.167	0.403	-0.268
	(0.416)	(0.495)	(0.810)	(0.209)	(0.263)	(0.513)
Subsidiary controls	X	X	X	X	X	X
Host-country controls	X	X	X	X	X	X
Subsidiary FE	X		X	X	X	X
Year FE		X	X	X	X	X
Industry-year FE	X					
Observations	25,928	27,181	10,318	1,319,507	29,211	15,381

Note: This table presents robustness tests for the financial response of CFC subsidiaries. The dependent variable is financial income. In Column (1), we use industry-year fixed effects. In Column (2), we replace the time-variant subsidiary-level controls with constant pre-treatment averages. Column (3) uses an alternative dependent variable, the financial profit and loss. In Column (4), we use a comprehensive control group that contains all unaffected subsidiaries, regardless of whether they classify as CFC or non-CFC. Column (5) uses an adjusted treatment definition based on the EATR instead of the statutory CIT rate. Column (6) changes the post-period to 2019 and 2020. All specifications include host-country and subsidiary control variables. The host-country controls comprise GDP, GDPPC, CIT rate, unemployment rate and CPI. As subsidiary controls, we include total assets, number of employees and operating revenue. Robust standard errors adjusted for clustering at the subsidiary level are reported in parentheses. ***, ** and * indicate significance at the 1%, 5% and 10% level.

Table C.7: Financial response: Robustness

	Financial income – Non-CFC subsidiaries					
	Industry-year fixed effects	*Pre-treatment controls*	*Alternative dependent variable*	*Comprehensive control group*	*Alternative treatment definition*	*Delayed treatment*
	(1)	(2)	(3)	(4)	(5)	(6)
Non-CFC Treat x Post	-0.070	-0.162	-0.427**	-0.188	-0.277**	-0.249
	(0.122)	(0.182)	(0.199)	(0.149)	(0.138)	(0.186)
Subsidiary controls	X	X	X	X	X	X
Host-country controls	X	X	X	X	X	X
Subsidiary FE	X		X	X	X	X
Year FE		X	X	X	X	X
Industry-year FE	X					
Observations	1,278,123	1,446,320	322,363	1,323,753	1,248,735	711,605

Note: This table presents robustness tests for the financial response of non-CFC subsidiaries. The dependent variable is financial income. In Column (1), we use industry-year fixed effects. In Column (2), we replace the time-variant subsidiary-level controls with constant pre-treatment averages. Column (3) uses an alternative dependent variable, the financial profit and loss. In Column (4), we use a comprehensive control group that contains all unaffected subsidiaries, regardless of whether they classify as CFC or non-CFC. Column (5) uses an adjusted treatment definition based on the EATR instead of the statutory CIT rate. Column (6) changes the post-period to 2019 and 2020. All specifications include host-country and subsidiary control variables. The host-country controls comprise GDP, GDPPC, CIT rate, unemployment rate and CPI. As subsidiary controls, we include total assets, number of employees and operating revenue. Robust standard errors adjusted for clustering at the subsidiary level are reported in parentheses. ***, ** and * indicate significance at the 1%, 5% and 10% level.

Table C.8: Financial response: Heterogeneity

	Financial income			
	CFC subsidiaries		*Non-CFC subsidiaries*	
	Model A	Model B	Model A	Model B
	(1)	(2)	(3)	(4)
CFC Treat x Post	-0.4724	0.0291		
	(0.4518)	(0.2825)		
Non-CFC Treat x Post			0.0772	-0.2735*
			(0.1579)	(0.1631)
Subsidiary controls	X	X	X	X
Host-country controls	X	X	X	X
Subsidiary FE	X	X	X	X
Year FE	X	X	X	X
Observations	26,022	25,727	1,278,560	1,277,687

Note: This table presents heterogeneity results of the DiD regressions estimated with PPML for model A and model B countries. The dependent variable is the financial income of CFC subsidiaries in Columns (1) and (2) and of non-CFC subsidiaries in Columns (3) and (4). The estimated effects are relative to the control group. The pre-treatment period spans from 2012 to 2015 while the post-treatment period spans from 2016 to 2020. (Non-)CFC Treat is a dummy variable set to one for all subsidiaries in the treatment group and to zero for all subsidiaries in the control group. All specifications include host-country and subsidiary control variables. The host-country controls comprise GDP, GDPPC, CIT rate, unemployment rate and CPI. As subsidiary controls, we include total assets, number of employees and operating revenue. Robust standard errors adjusted for clustering at the subsidiary level are reported in parentheses. ***, ** and * indicate significance at the 1%, 5% and 10% level.

C.4 Economic Activity

Table C.9: Economic activity: Robustness investment

	Fixed assets – Non-CFC subsidiaries					
	Industry-year fixed effects	*Pre-treatment controls*	*Alternative dependent variable*	*Comprehensive control group*	*Alternative treatment definition*	*Delayed treatment*
	(1)	(2)	(3)	(4)	(5)	(6)
Non-CFC Treat x Post	0.0138	0.0192	-0.1706***	-0.0109	0.0462	0.0007
	(0.0300)	(0.0335)	(0.0657)	(0.0265)	(0.0745)	(0.0400)
Subsidiary controls	X	X	X	X	X	X
Host-country controls	X	X	X	X	X	X
Subsidiary FE	X		X	X	X	X
Year FE		X	X	X	X	X
Industry-year FE	X					
Observations	1,774,232	1,915,048	1,851,566	1,822,737	1,731,697	1,002,301

Note: This table presents robustness tests for the economic activity of non-CFC subsidiaries. The dependent variable is tangible fixed assets. In Column (1), we use industry-year fixed effects. In Column (2), we replace the time-variant subsidiary-level controls with constant pre-treatment averages. Column (3) uses an alternative dependent variable, total assets. In Column (4), we use a comprehensive control group that contains all unaffected subsidiaries, regardless of whether they classify as CFC or non-CFC. Column (5) uses an adjusted treatment definition based on the EATR instead of the statutory CIT rate. Column (6) changes the post-period to 2019 and 2020. All specifications include host-country and subsidiary control variables. The host-country controls comprise GDP, GDPPC, CIT rate, unemployment rate and CPI. As subsidiary controls, we include number of employees and operating revenue. Robust standard errors adjusted for clustering at the subsidiary level are reported in parentheses. ***, ** and * indicate significance at the 1%, 5% and 10% level.

Table C.10: Economic activity: Robustness employment

	Costs of employees – Non-CFC subsidiaries					
	Industry-year fixed effects	*Pre-treatment controls*	*Alternative dependent variable*	*Comprehensive control group*	*Alternative treatment definition*	*Delayed treatment*
	(1)	(2)	(3)	(4)	(5)	(6)
Non-CFC Treat x Post	0.0130	-0.0118	-0.0832**	-0.0353	-0.0307	-0.0270
	(0.0256)	(0.0238)	(0.0387)	(0.0239)	(0.0320)	(0.0275)
Subsidiary controls	X	X	X	X	X	X
Host-country controls	X	X	X	X	X	X
Subsidiary FE	X		X	X	X	X
Year FE		X	X	X	X	X
Industry-year FE	X					
Observations	1,752,992	1,763,567	1,803,242	1,810,716	1,704,828	1,012,427

Note: This table presents robustness tests for the economic activity of non-CFC subsidiaries. The dependent variables is costs of employees. In Column (1), we use industry-year fixed effects. In Column (2), we replace the time-variant subsidiary-level controls with constant pre-treatment averages. Column (3) uses an alternative dependent variable, number of employees. In Column (4), we use a comprehensive control group that contains all unaffected subsidiaries, regardless of whether they classify as CFC or non-CFC. Column (5) uses an adjusted treatment definition based on the EATR instead of the statutory CIT rate. Column (6) changes the post-period to 2019 and 2020. All specifications include host-country and subsidiary control variables. The host-country controls comprise GDP, GDPPC, CIT rate, unemployment rate and CPI. As subsidiary controls, we include total assets and operating revenue. Robust standard errors adjusted for clustering at the subsidiary level are reported in parentheses. ***, ** and * indicate significance at the 1%, 5% and 10% level.

Table C.11: Economic activity: Heterogeneity investment

	Fixed assets			
	CFC subsidiaries		*Non-CFC subsidiaries*	
	Model A	Model B	Model A	Model B
	(1)	(2)	(3)	(4)
CFC Treat x Post	-0.2077	0.1152		
	(0.1299)	(0.1313)		
Non-CFC Treat x Post			-0.0360	0.1102**
			(0.0234)	(0.0547)
Subsidiary controls	X	X	X	X
Host-country controls	X	X	X	X
Subsidiary FE	X	X	X	X
Year FE	X	X	X	X
Observations	29,400	29,088	1,773,294	1,772,164

Note: This table presents heterogeneity results of the DiD regressions estimated with PPML for model A and model B countries. The dependent variable is tangible fixed assets. Columns (1) and (2) depict the results for CFC subsidiaries while Columns (3) and (4) show the results for non-CFC subsidiaries. The pre-treatment period spans from 2012 to 2015 while the post-treatment period spans from 2016 to 2020. (Non-)CFC Treat is a dummy variable set to one for all subsidiaries in the treatment group and to zero for all subsidiaries in the control group. All specifications include host-country and subsidiary control variables. The host-country controls comprise GDP, GDPPC, CIT rate, unemployment rate and CPI. As subsidiary controls, we include number of employees and operating revenue. Robust standard errors adjusted for clustering at the subsidiary level are reported in parentheses. ***, ** and * indicate significance at the 1%, 5% and 10% level.

Table C.12: Economic activity: Heterogeneity employment

	Costs of employees			
	CFC subsidiaries		*Non-CFC subsidiaries*	
	Model A	Model B	Model A	Model B
	(1)	(2)	(3)	(4)
CFC Treat x Post	0.0938*	0.0456		
	(0.0480)	(0.1278)		
Non-CFC Treat x Post			0.0136	-0.0641**
			(0.0280)	(0.0310)
Subsidiary controls	X	X	X	X
Host-country controls	X	X	X	X
Subsidiary FE	X	X	X	X
Year FE	X	X	X	X
Observations	27,848	27,439	1,751,310	1,749,841

Note: This table presents heterogeneity results of the DiD regressions estimated with PPML for model A and model B countries. The dependent variable is the costs of employees in Panel B. Columns (1) and (2) depict the results for CFC subsidiaries while Columns (3) and (4) show the results for non-CFC subsidiaries. The pre-treatment period spans from 2012 to 2015 while the post-treatment period spans from 2016 to 2020. (Non-)CFC Treat is a dummy variable set to one for all subsidiaries in the treatment group and to zero for all subsidiaries in the control group. All specifications include host-country and subsidiary control variables. The host-country controls comprise GDP, GDPPC, CIT rate, unemployment rate and CPI. As subsidiary controls, we include total assets and operating revenue. Robust standard errors adjusted for clustering at the subsidiary level are reported in parentheses. ***, ** and * indicate significance at the 1%, 5% and 10% level.

C.5 Variable Definitions

Variable	Definition
Group sample	
Share of (non-) CFC subsidiaries	Multinational group g's share of (non-)CFC subsidiaries in year t.
MNE Treat	Indicator equal to 1 for multinational group g located in treated home country m.
Post	Indicator equal to 1 for post-treatment years t.
Number of total subsidiaries	Multinational group g's number of CFC subsidiaries in year t.
Number of (non-)CFC subsidiaries	Multinational group g's number of non-CFC subsidiaries in year t.
Intangibility	Multinational group g's intangible assets divdided by total assets in year t.
Leverage	Multinational group g's debt divided by total assets in year t.
Home country GDP	Total gross domestic product in multinational group g's home country m in year t.
Home country GDPPC	Total gross domestic product per capita in multinational group g's home country m in year t.
Home country CIT rate	Corporate income tax rate applicable in multinational group g's home country m in year t.
Subsidiary sample	
(Non-)CFC Treat	Indicator equal to 1 for subsidiary i located in treated
Post	Indicator equal to 1 for post-treatment years t.
Total assets	Subsidiary i's total assets in year t.
Tangible fixed assets	Subsidiary i's tangible fixed assets in year t.
Number of employees	Subsidiary i's number of employees assets in year t.
Costs of employees	Subsidiary i's costs of employees in year t.
Financial income	Subsidiary i's financial income in year t.
Operating income	Subsidiary i's operating income in year t.
Host country GDP	Total gross domestic product in subsidiary i's host country h in year t.

Continued on next page

Continued from previous page

Variable	**Definition**
Host country GDPPC	Total gross domestic product per capita in subsidiary *i*'s host country *h* in year *t*.
Host country CIT	Corporate income tax rate applicable to subsidiary *i* in host country *h* in year *t*.
Host country unemployment rate	Unemployment rate in subsidiary *i*'s host country *h* in year *t*.
Host country corruption perception index	Corruption perception index in subsidiary *i*'s host country *h* in year *t*.

Kurzlebenslauf

09/2020 - 06/2025	**Universität Mannheim** Wissenschaftliche Mitarbeiterin und Doktorandin am Lehrstuhl für Allgemeine Betriebswirtschaftslehre und Betriebswirtschaftliche Steuerlehre I & II Prof. Dr. Philipp Dörrenberg & Prof. Dr. Christoph Spengel Promotionsstudium an der Graduate School of Economics and Social Sciences (GESS), Centre for Doctoral Studies in Business
03/2025 - 06/2025	**University of Iowa, USA** Lehr- und Forschungsaufenthalt am Tippie College of Business Prof. Ryan Wilson
04/2024 - 06/2024	**University of Iowa, USA** Forschungsaufenthalt am Tippie College of Business Prof. Jaron Wilde
01/2024 - 03/2024	**University of Texas at Austin, USA** Forschungsaufenthalt an der McComb School of Business Prof. Lisa De Simone
09/2018 - 08/2020	**Universität Mannheim** M.Sc. Mannheim Master in Management
10/2015 – 08/2018	**Universität Passau** B.Sc. Business Administration and Economics