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Helsinki 2024

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Jarkko Harju:
Tampere University & Finnish Centre of Tax Systems Research (FIT) & CESifo,
jarkko.t.harju@tuni.fi

Kaisa Kotakorpi:
Tampere University & FIT & CESifo, kaisa.kotakorpi@tuni.fi

Tuomas Matikka:
VATT Institute for Economic Research & FIT & CESifo, tuomas.matikka@vatt.fi

Annika Nivala:
VATT & FIT, annika.nivala@vatt.fi

Suggested citation:
Harju Jarkko, Kotakorpi Kaisa, Matikka Tuomas, and Nivala Annika (2024). How Do Firms Respond to Risk-based Tax Audits? VATT Working Papers 165.

The same study has been published:
Harju Jarkko, Kotakorpi Kaisa, Matikka Tuomas, and Nivala Annika (2024). How Do Firms Respond to Risk-based Tax Audits? FIT Working Paper 22.

VATT Working Papers:
https://doria.fi/handle/10024/147862

Valtion taloudellinen tutkimuskeskus
VATT Institute for Economic Research
Arkadiankatu 7, 00100 Helsinki, Finland

Helsinki, June 2024

How Do Firms Respond to Risk-based Tax Audits?*

Jarkko Harju Kaisa Kotakorpi Tuomas Matikka Annika Nivala

May 17, 2024

Abstract

We analyze firm responses to risk-based tax audits – a central tool in regular tax enforcement – using full-population data on tax audits and tax returns in Finland. We find an immediate and persistent increase in reported profits by the audited firms after being audited compared to matched non-audited firms with a similar development in key outcomes before the audit. This is an indication of significant non-compliance in the baseline. We also examine the anatomy of non-compliance and find that both revenue and labor costs increase after audits, suggesting that some firms may follow a strategy of under-reporting their overall scale of operation. We use novel data on bankruptcy petitions and court decisions to investigate whether stricter tax enforcement has implications for real economic activity. We find a large increase in the likelihood of bankruptcy after audits among non-compliant firms, but no increase in bankruptcies for compliant firms.

JEL Codes: H26; H32; H83.

Keywords: tax compliance; tax evasion; tax enforcement; firm behavior

*We thank Matias Giaccobasso, Tuomas Kosonen, Jukka Pirttilä, Mazhar Waseem, Jason DeBacker, Chiara Lacava, Thor A. Thoresen and several conference participants and discussants for their comments and suggestions. Also, we thank the Finnish Tax Administration for their cooperation. All opinions, interpretations, and errors are our own. We are grateful to the Academy of Finland (grant no. 346252) for funding this project.

Harju: Tampere University & Finnish Centre of Tax Systems Research (FIT) & CESifo.

Kotakorpi: Tampere University & FIT & CESifo

Matikka: VATT Institute for Economic Research & FIT & CESifo

Nivala: VATT & FIT

1 Introduction

High tax compliance is essential for efficient tax collection and a fair distribution of the tax burden. Firms have a key role in tax compliance. They remit most of the tax revenue through value added tax or sales taxes, payroll taxes and business taxes. In addition, firms withhold income taxes from employees and provide associated information reports on wage income to tax administrations. However, it is well known that tax enforcement on firms is challenging due to limited third-party reporting of business income. There are a large number of recent studies examining the effects of particular enforcement tools, threat-of-audit letters and other types of campaigns on firm tax enforcement (Almunia and Lopez-Rodriguez 2018; Bérgolo et al. 2023; Boning et al. 2018; Brockmeyer et al. 2019; Carrillo et al. 2017; Pomeranz 2015; Naritomi 2019). However, comprehensive evidence is lacking on how firms respond to regular risk-based tax audits, even though such audits are the most fundamental tax enforcement and monitoring tools of tax administrations throughout the world.[1]

This paper provides new evidence on the effects of risk-based tax audits on firms using full-population administrative data and a matched comparison group. We conduct a comprehensive analysis of changes in tax compliance and the economic performance of firms after an audit. In addition, we study the anatomy of non-compliance by leveraging our detailed data including various items in firm tax returns. We provide novel evidence on the real effects of risk-based audits by analyzing their effects on firm exits and bankruptcies.

Risk-based tax audits are typically costly to implement. However, they can improve tax compliance through both unreported tax revenue uncovered directly in an audit and firms' compliance responses to audits in subsequent years. Nevertheless, tax audits might have negative effects on the economic performance of firms, given that successful tax enforcement may lead to a higher effective tax burden. Therefore, knowing how firms respond to risk-based audits is crucial for assessing the benefits and costs associated with

[1] We use the terms operational / risk-based / regular audits interchangeably to refer to audits carried out as part of the tax administration's regular tax auditing program, in contrast to e.g. randomized audits or targeted auditing campaigns.

these trade-offs, in order to better understand the welfare effects of audits and policy design regarding the scale and targeting of tax enforcement.

We combine full-population data on tax audits and annual tax returns of firms in Finland to study firm responses to risk-based tax audits. However, our data do not include risk scores or other direct indicators used by the tax administration in their audit selection. To construct a comparison group for the audited firms, we combine matching and difference-in-difference methods. Our comparison group includes non-audited firms with similar observed characteristics before the audit as the audited firms. In our analysis, we first show that the audited firms and the comparison group firms have very similar trends in the key outcomes 4 years prior to audit, supporting the validity of our empirical approach.

Furthermore, the matching approach has the following attractive features when studying risk-based audits: first, we restrict our empirical estimation to audited firms that have close matches in the data. Consequently, the largest firms that are very often audited and the smallest firms that are almost never audited drop out of our estimation sample. Our analysis thus captures firms that can be considered to be close to the margin of being audited. To the extent that our results can be interpreted causally, they correspond to the consequences of auditing one additional firm, thus providing important information for audit policy design. Second, the practical implication of our empirical strategy is that the firms in our analysis are small to medium-sized businesses that form the bulk of the firm population. These types of firms have great importance in terms of employment and economic growth.[2] This is also a subgroup of firms where further audits may be most effective and increasing audits is feasible, as the largest firms are already subject to frequent audits as well as relatively strict monitoring and reporting requirements.[3] Third, our empirical approach focuses on firms with real economic activity in the years prior to audit. This means that purely fraudulent "ghost firms" (see e.g. Carillo et al. 2023)

[2]See e.g. Haltiwanger et al. 2013 and Decker et al. 2014 who discuss that start-ups, which tend to be small or medium sized firms, are important for job creation and growth. Small and medium sized firms account for about 70% of employment and 60% of total revenue on average from 26 OECD countries (OECD, 2023).

[3]Relatedly, theoretical research suggests that tax evasion is more difficult to maintain in large firms (Kleven et al. 2016).

with no other function than to evade business taxes such as VAT are not included in our sample, or they receive at most a very small weight in the matching procedure. Our analysis is therefore restricted to the effects of audits among the general firm population.

We find that the audited firms' reported profits increase by 12% on average after being audited, compared to similar matched firms that were not audited. This response is statistically significant, immediate and persistent throughout our 5-year follow-up period after the audit. These results are consistent with significant non-compliance in tax payments: the sharp and immediate reporting response to audits represents a mirror image of initial tax non-compliance in the baseline. The persistent increase in reported profits in subsequent years after the audit indicates that audits have long-lasting effects on tax compliance, potentially through changes in perceived audit or monitoring probability. Furthermore, we find that the positive profit response is largely driven by those firms that were found to be non-compliant in the audit.

Regarding the channels of non-compliance, we find that both average revenue and labor costs increase after audit. This points to under-reporting of both revenue and labor costs in the baseline. This behavior can be rationalized, for example, by a firm strategy to under-report the scale of its operations in order to evade profit taxes, combined with an attempt to manipulate the audit probability by appearing small both in terms of revenues and costs. Also, under-reporting of labor costs is a signal of payroll tax evasion, and is consistent with collusive tax evasion between employers and employees.

In addition to reporting effects, stricter tax enforcement and increased compliance lead to a higher effective tax burden that may have negative real effects on business activity. In general, these real effects are notoriously difficult to measure using administrative data. By definition, tax reports provide inaccurate measures of the true scale of business activity for non-compliant firms, and the observed positive reporting effects after the audits may hide negative real effects. We study the potential real effects of audits by analyzing firm exits using full-population administrative data on bankruptcies. Tax audits may induce marginally profitable firms to exit when their effective tax rate increases, or if they experience liquidity issues when required to pay their tax deficit. We argue that studying firm bankruptcies captures a significant margin of real responses, since bankruptcy declarations are subject to a court decision and the process is costly for firms and their owners.

We observe a significant increase in bankruptcies following an audit. We find an increase of 39% in the likelihood of bankruptcy for the audited firms compared to a baseline bankruptcy rate of 4.6% among the matched non-audited firms. Importantly, we find that the increased bankruptcy rate is entirely due to non-compliant firms and those with detected grey economy activities as defined by the tax administration. We do not observe increased probability of bankruptcy or other types of exits for compliant firms, suggesting that these real effects of audits are limited to firms that were found to be fraudulent by the tax administration.

Our results are directly informative about the welfare consequences of audits, given that tax revenue implications are the relevant sufficient statistic for behavioral responses in tax policy analysis (Keen and Slemrod 2017; Hendren 2016). Our results, considering the effects on the VAT, payroll tax and corporate income tax bases, show that the overall increase in tax revenue arising through the longer-run behavioral responses of firms adds significantly to the direct revenue uncovered in the audit: our estimates suggest that the indirect effect of audits on tax revenue, arising through the reporting responses of firms over the 5-year-follow-up period after an audit, amounts to 38% (9000 euros) per firm, on top of the immediate average tax deficit detected in the audit (24,000 euros). This estimate is a lower bound, as we only include the main tax liabilities of firms and do not consider spillovers to the taxation of firm owners or employees. This tax revenue raised can be compared by tax administrators to the costs of audits to arrive at a full cost-benefit analysis. Further, we show that audits lead to changes in real economic activity that are likely to shift market share from non-compliant to compliant firms. Typically, reductions in real economic activity are considered as a drawback of tax policy. In our context, this reduction in potentially illicit activity can rather be considered a further benefit of audits beyond a simple tax revenue vs. audit cost calculation.

Our findings contribute to the previous literature on the effects of operational tax audits. Mazzolini et al. (2022), Beer et al. (2020) and D'Agosto et al. (2018) provide evidence on the responses of self-employed individuals to operational tax audits, and find increased tax compliance after audits. We focus on larger firms with potentially multiple owners, and find similar results in terms of increased compliance. We also investigate the channels of non-compliance, providing evidence on strategies that firms may use to evade taxes. Furthermore, we provide novel evidence on the real effects of audits by showing that they increase bankruptcies among non-compliant firms. DeBacker et al. (2015b) analyze a slightly different question and find a temporary increase in firms' tax aggressiveness after being audited, measured by the ratio of income tax paid to corporate net income. Our approach differs from this paper in using full-firm-population data and an empirical strategy that incorporates a comparison group of non-audited firms.[4]

Our paper complements the literature using field experiments or random audits to study how tax audits or threat-of-audit letters affect firm behavior (Pomeranz, 2015; Bérgolo et al., 2023; Boning et al., 2018; Bjørneby et al., 2021). While these studies reliably identify the effects due to randomization, their results may not be fully applicable to operational tax audits for two reasons: both the target population and the treatment may differ from risk-based audits. Our evidence, on the other hand, is directly relevant to policy-makers for determining how increasing the number of audits (i.e auditing one more firm in the context of the tax authority's regular audit program) affects firm tax reporting and other key outcomes.

Finally, our paper also relates to a broader literature on the compliance responses of firms to tax enforcement. Almunia and Lopez-Rodriguez (2018) study how monitoring intensity affects compliance using variation in auditing resources according to firm size. They find that higher audit rates increase compliance and many firms try to avoid higher auditing intensity by under-reporting their size. Similarly, our results indicate that firms under-report their scale of activity, and auditing more firms can lead to an increase in reported profits in the future. The role of third-party reporting and firm responses to

[4]In addition, Lediga et al. (2023) study the spillover effects of audits to neighboring firms, and Hanlon et al. (2007), Joulfaian (2000) and DeBacker et al. (2015a) study associations between tax evasion and manager and firm characteristics.

it, as well as compliance within value-added taxation, has been analyzed by Pomeranz (2015), Carrillo et al. (2017) and Naritomi (2019). Our results suggest that firms under-report both revenue and value added, and that tax audits can increase compliance within value-added taxation too.

The remainder of the paper is organized as follows: Section 2 describes the institutional framework and theoretical background on risk-based tax audits. In Section 3 we describe the data and our empirical strategy. Section 4 presents the results on tax reporting and Section 5 on firm exits. Section 6 provides a discussion on the tax revenue and welfare implications of audits, and Section 7 concludes.

2 Institutions and Conceptual Framework

2.1 Firm Tax Audits in Finland

In Finland, as in many other developed countries, firms report detailed revenue and cost items annually in their tax returns for the purpose of determining annual taxable profit. They are also required to file information not directly related to company taxes such as industry codes and the number of employees. Different tax schedules are used for different organizational forms. Sole proprietors' and partnerships' profits are taxed according to the personal income tax schedule, while corporations pay a flat corporate tax on taxable profits. In addition to annual income tax reporting, firms remit value-added taxes (VAT), and withhold personal income and payroll taxes from their employees' income each month. On top of tax reporting, firms must follow accounting principles with particular rules and guidelines in reporting their financial statements.[5]

Tax audits can be used to verify whether firms have filed taxes correctly in their tax returns. The goal of an audit is to determine the true tax liability of a firm and to collect the correct amount of taxes. Tax audits can also be used to improve future tax compliance by giving advice on how to correctly report taxes. For confidentiality reasons, the precise rules that the Finnish Tax Administration uses to select firms into audits are not available to the researcher. As we analyze risk-based audits in this paper, firms with discrepancies in their tax reporting are expected to be more likely to be selected for audit. We discuss this issue and how we address it in our empirical analysis in Section 3.

In a tax audit process, the auditors review information on the firm and its tax returns to find any discrepancies between them. The auditors first familiarize themselves with the industry and operating environment of the firm, review the information contained in

[5]Exceptions to these regulations include the following: firms with annual revenue under 8,500 euros (10,000 since 2016) are not VAT-liable and do not have to file VAT reports in Finland. However, these firms can voluntarily register for VAT. After 2010, firms with annual revenue below 25,000 euros are only required to report VAT annually. Firms registered as employers are required to report payroll taxes monthly. Firms are required to register as employers if they have at least two employees permanently or five employees temporarily or they can voluntarily register. Firms not registered as employers only report payroll taxes in the months they have employees. Firms for which at most one of the following conditions do not hold for two consecutive tax years are exempted from the requirement of having an external auditor for their accounts: total balance sheet is over 100,000 euros, revenue is over 200,000 euros, or there are over 3 employees on average during a year.

the tax returns, and previous decisions of the tax administration regarding the firm. The auditors contact the business to inform the firm about the audit and what accounting material is needed for it, and the firm submits relevant information such as significant changes in the business and how their accounting, payroll and financial management is organized. Then the actual audit follows, where the auditors review material such as accounting records, receipts, external auditors' reports and relevant business contracts. Finally, the tax auditors file a report on their findings and propose measures after a concluding discussion with the firm on what was detected in the audit, what the proposed actions are, and whether there are more issues to clarify before concluding the audit. The reports are filed officially even when there are no corrective measures. (Finnish Tax Administration, 2017).

If a firm has been found to be non-compliant in taxation, the tax administration can assign a tax increase of 10%, or 15% to 50% in more serious cases of tax evasion. Alternatively, the tax administration can report criminal tax evasion to the police or customs in the case of serious offenses. It is generally not possible both to assign a tax increase and a criminal investigation for individual taxpayers, but this is possible for corporations. If the negligence has been minor or the taxpayer has corrected their tax report without being prompted, the tax administration can impose a small delay fine (100 euros since 2018). Most of the actions after an audit follow a strict code of practice, but in some cases the tax auditor has the discretion to assign sanctions, especially in cases with less serious misconduct. (Finnish Tax Administration, 2020).

2.2 Theoretical Background

Firms may respond to tax audits in several ways. We are interested in both reporting responses – how does the audit change tax reporting and hence tax compliance by firms both in the short run and in subsequent years – as well as potential real responses – how does tax enforcement change the real economic activity of firms. Next, we discuss these channels in more detail.

Tax Reporting. To analyze the determinants of tax reporting behavior, we can apply the deterrence model of tax evasion by Allingham and Sandmo (1972) to firms. This model has been extended by Kleven et al. (2011) to account for the endogenous probability of detection and third-party information. In this simple theoretical discussion, we focus on profit taxes.[6] The model applies to the determinants of illegal tax evasion, but we briefly comment on other types of non-compliance, such as unintentional mistakes, below.

The firm makes a profit $\pi = R - C$, where R denotes revenue and C denotes costs, and reports profit $\hat{\pi} = \hat{R} - \hat{C}$ to the tax authority, where $\hat{R}$ and $\hat{C}$ may differ from the true values R and C. The tax rate on profits is denoted by τ, and if evasion is detected, the firm pays the evaded tax as well as a fine equal to a fraction θ of the evaded tax.

The probability that evasion is detected is denoted by $p(a)$, where a is the audit rule used by the tax authority. In the case of random audits, the audit probability is constant and the same for all firms. More generally, and in the context of this paper, the audit rule a, and hence p, depend on the firm's past reporting behavior in ways that are generally not fully known or observable to the firm (or the researcher). Note also that what matters for firm behavior is the firm's *perception* of the audit rule, and therefore in our context $p(a)$ is best thought of as the firm's perceived detection probability.

The firm chooses $\hat{R}$ and $\hat{C}$ to maximize expected after-tax profit, which is given by[7]

$$E[\pi] = (1 - p(a))(\pi - \tau\hat{\pi}) + p(a)((1 - \tau)\pi - \theta\tau(\pi - \hat{\pi})) \tag{1}$$

The firm can evade profit taxes either through understating $\hat{R}$ or by overstating $\hat{C}$ relative to R and C; both have an equivalent direct effect on reported profits and hence on profit tax liability. If the detection probability is independent of firm behavior (random audits), only this direct effect matters for reporting incentives. However, reporting

[6] The reporting incentives regarding VAT are similar to those regarding profits, but the incentives for payroll tax reporting are different. Here we focus solely on the reporting of profits, but comment below on the connection to payroll tax reporting.

[7] For simplicity, we view the firm as a single risk-neutral decision-maker that reports $\hat{R}$ and $\hat{C}$ to maximize expected after-tax profit. We therefore abstract from the agency problem within the firm, whereby the firm manager's reporting incentives may differ from those of the shareholders (Crocker and Slemrod 2005). This assumption may be justified if the incentive structure within the firm is set optimally.

incentives mediated through changes in audit probability make matters less straightforward. For example, if firms believe that the tax authority decides on audits based on the firm's size or the profit rate (ratio of profit over revenue), where a lower profit rate could trigger an audit, the firm's incentive to under-report revenue is amplified, and there is an incentive to under-report (rather than over-report) also costs to match the lower reported revenue. Carrillo et al. (2017) focus on this type of setting.[8] In the context of payroll tax reporting, the firm has the incentive to under-report wage costs to reduce payroll tax liability. Relatedly, the firm may fear that the tax authority compares its reports for profit tax and payroll tax purposes, which would limit the firm's incentive to over-report wage costs in order to evade profit taxes.[9]

In sum, if a firm engages in tax evasion, it is likely to have incentives to under-report revenue, whereas the incentives for under- or over-reporting costs depend on the details of the (perceived) audit rule. If the firm finds it optimal to understate the scale of its operations and under-report costs, it has bigger incentives to do so through under-reporting wages (due to linkages with payroll tax reporting), rather than other cost items. The third-party reporting of VAT by other firms on the costs and services used by the business can also limit the incentives for cost under-reporting.

Predictions on Responses to Tax Audits. If audits are effective, their direct effect is to reverse the under- or over-reporting of any given tax item. Therefore, the immediate responses to audits are informative about the magnitude and channel of tax non-compliance. In other words, immediate reporting responses to audits are a mirror image of tax non-compliance in the baseline.

The reported profit of non-compliant firms will increase post-audit. A similar prediction holds for the value added of the firm (revenue–costs). The anatomy of the change in reported profit – whether it is caused by changes on the revenue or cost side – is

[8] As another example, the audit rule may depend on past reported profits. In this case, the firm simply has an incentive to maintain a steady stream of reported profits over time, with unclear predictions about the optimal reporting strategy overall.

[9] The exact outcome also depends on (i) the level of the payroll tax vs. the profit tax and (ii) potential general equilibrium effects, whereby firms that underreport wages for payroll tax purposes may be able to collude with workers in order to reduce workers' personal income tax liability and pay lower wages. Bjørneby et al. (2021) focus on this type of collusive behaviour.

indicative of the firm's evasion strategy. As explained above, firms may misreport profit on the revenue or cost side or both. Thus, evading firms may under- or over-report costs depending on their selected evasion strategy and perceptions of the audit rule. First, *the reported revenue* of non-compliant firms will increase or stay constant after audit, depending on the channel of non-compliance. Second, *the reported costs* of non-compliant firms may decrease or increase post-audit. The latter possibility relates in particular to indirect effects arising through the firm's attempt to manipulate the detection or audit probability, which may cause under-reporting of costs in the baseline. If the perceived detection probability is exogenous (e.g. random audits), reported costs decrease post-audit due to increased cost reporting in order to evade taxes. If the perceived detection probability decreases in line with the level of reported costs (e.g. the audit probability decreases with the profit rate or the audit probability increases in line with the mismatch between income tax and payroll tax reporting), reported costs may increase post-audit.

In subsequent years, the effects of past audits depend on how the firm updates its beliefs about the audit rule and the monitoring and detection probability in the future, as discussed in more detail in DeBacker et al. (2015b). Depending on whether the firm updates the perceived probability of future audits upwards or downwards, effects in subsequent years either reinforce or counter the direct effects of the audit. Therefore, the longer-run reporting responses to audits can reveal their impact on the more permanent effects of audits on firm tax compliance.

In addition to reporting effects, we are interested in the potential *real effects* of tax enforcement. Stricter enforcement increases the effective tax rate and hence reduces after-tax profitability for non-compliant firms, but it should have no effect on firms that are compliant in the baseline.[10] In general, the profit tax should be neutral regarding the real scale of a firm's operations. However, for firms that are on the margin of being profitable, the shocks to profitability may be detrimental and, in the extreme, may cause a firm to exit. Also, paying the tax deficit discovered in the audit may have real effects through

[10] Stricter enforcement also affects the riskiness of the tax evasion gamble, and willingness to limit exposure to risk may also affect real decisions (see e.g. Slemrod and Gillitzer (2014), ch. 3.1.2). However, if we assume that firms are risk-neutral, this channel is not operational, and all effects on real behavior go through changes in expected after-tax profitability.

liquidity issues, which could lead the firm to exit. More generally, if we view business activity in the context of the owner's portfolio choice, lower profitability may induce the owner to invest in other types of activities instead.

Finally, the effects described above are relevant only for non-compliant firms, i.e. those firms that have reason to adjust their reporting when faced with an audit. However, even though the above discussion in the context of the deterrence model revolves around tax evasion, in practice non-compliance may be either intentional (evasion) or unintentional (e.g. honest mistakes). All audited firms may receive advice on correct reporting. Such information may be an additional reason for adjustments in reporting behavior after the audit. For example, firms may receive new information on what costs are eligible for tax deductions. However, it is not clear why honest mistakes should lead to systematic revisions in reporting in either direction.

3 Data and Empirical Strategy

3.1 Data

We use data on annual tax returns, tax audits and bankruptcies for the full population of Finnish firms. These data enable us to analyze firm reports several years before and after audits. First, we combine two data sets to construct a panel of tax reporting and audits: annual tax return data for 2000–2016, and tax audit data including all completed tax audits on firms in Finland in 2003–2016 from the Finnish Tax Administration. This study is the first in which these combined data are used for research purposes in Finland. The combined data constitute a unique data set of the full population of firm tax returns combined with the tax audit information on those firms that were audited. Second, we link data on bankruptcy court records from Statistics Finland to the firm panel. These data include court records on bankruptcy petitions and decisions. The key variables we use in our analysis are described in detail in Table A1 in Appendix A.

The combined tax audit and tax return data set includes almost one million unique firms with over six million firm-year observations. The overall audit rate is approximately

0.7% in our sample. There are in total 52,328 operational tax audits, and after applying our sample restriction criteria, we are left with 42,510 audited firms. First, we drop 4,104 observations where we are unable to match an audited firm to any firm ID in the tax return data.[11] Then we exclude firms that are audited after they have exited the tax return sample. This is because we want to study firms that are still active when they are selected for a tax audit. We define firms' exit year as first year after which the firm is no longer observed in the tax return data. This restriction excludes 1,606 audits that are conducted on firms that were no longer active one year before the audit. Finally, there are 4,012 firms that are audited multiple times, with 8,408 audits. We only include the first audit for these firms and they are excluded after the second audit. This allows us to focus solely on the implications of the first audit.

The audit data include detailed information on the outcomes and characteristics of the audits, but they do not not include direct information on selection into an audit, such as risk scores (we discuss this issue in more detail below). We use the beginning date of the audit to identify audit timing and the total detected tax deficit to identify firms that are found to be non-compliant. The tax return data include detailed information on firm characteristics and performance indicators as reported by the firm, such as company form, industry, revenue, costs, assets, the number of employees and dividends paid to shareholders.

For each firm in the bankruptcy data, we form indicators for bankruptcy petitions and declarations in a given year. We use the filing date to determine the year of the petition and the decision date of the declaration to define the year of the bankruptcy. This forms 55,699 firm-year observations for the period 2000–2017 from 37,899 unique firms.[12]

Finally, we restrict our sample to include incorporated and unincorporated businesses,

[11]We are unable to match 8,764 tax audit observations to firm-year observations in the tax return data. Out of those 4,104 observations cannot be matched to any firm ID in the tax return data. It is plausible that the unmatched observations are unregistered businesses that have never filed a business tax return because, according to the information from the tax audits, 92.8% of the unmatched observations are natural persons or estates and 2.7% are foreign firms, and have either no (93.5%) or very small reported revenue (5.9% in the micro category).

[12]There can be several observations per firm for a given year in this data set, as there can be several petitions. This is why we only construct a dummy variable for whether there is a bankruptcy petition and whether any of those petitions result in a bankruptcy decision for a given year. We are unable to link 2,040 of these firms to any firm ID in the tax return data.

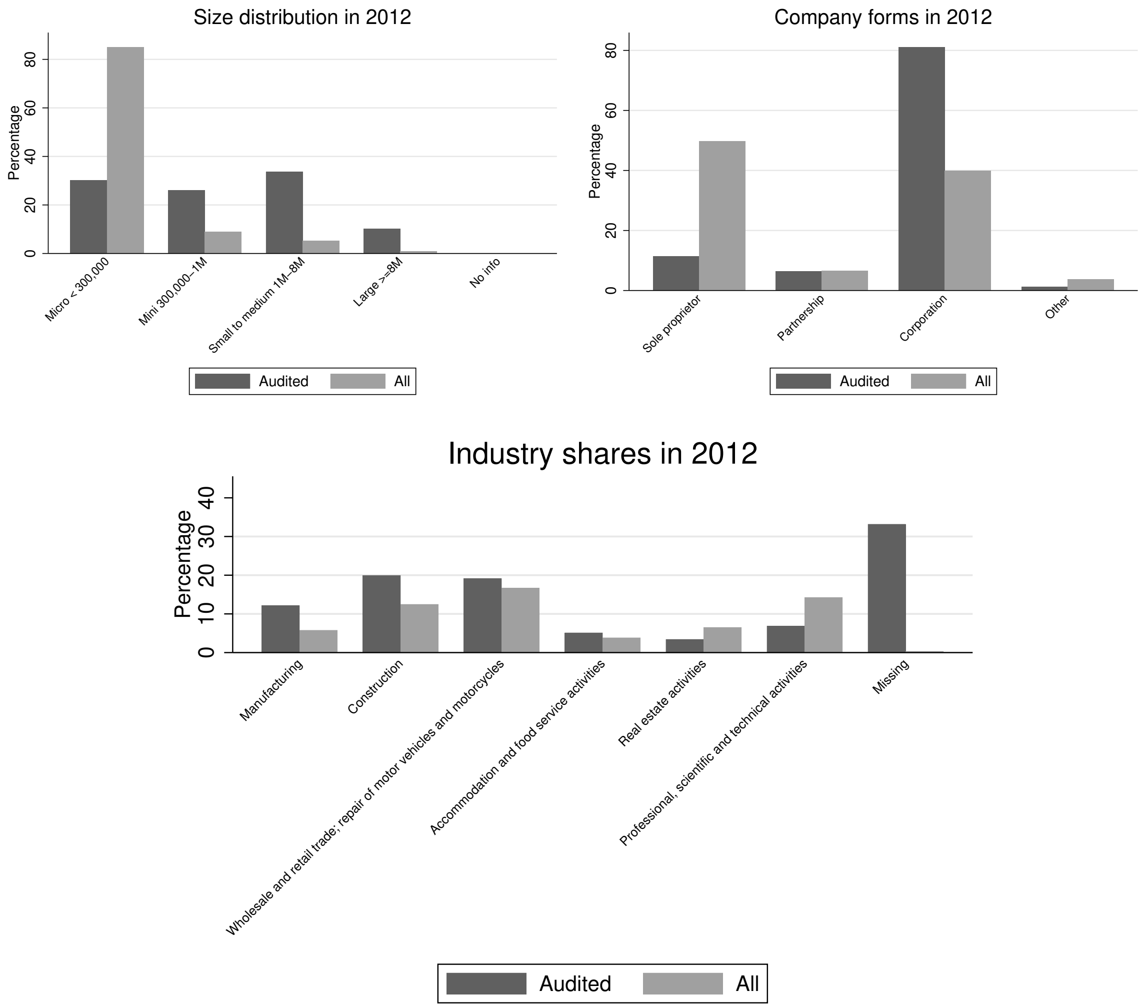
and exclude non-profit organizations, municipalities and other publicly owned organizations. To mitigate the potential impact of outlier observations, we winsorize all continuous variables at the 1st and 99th percentiles by year.

3.2 Empirical Strategy

With the tax return data, we can follow the audited firms' reported revenues, cost items and profits over time. The goal of our empirical analysis is to examine how these variables develop around the time of the audit and in subsequent years for the audited firms, relative to a benchmark group of non-audited firms. As we analyze risk-based audits and the audited firms comprise a highly selected group of all firms, our first and foremost task is to form a suitable comparison group that provides a reasonable benchmark to which the development of audited firms can be compared. Therefore, we begin by describing selection into audit and the observed differences between the audited and non-audited firms before turning to the matching strategy that forms an integral part of our empirical analysis.

Selection into Audit. We do not have access to information about the audit selection rule of the tax administration such as risk scores or other direct indicators of audit probability. However, we can observe from the data that the audited and non-audited firms differ from each other in many observable characteristics. Figure 1 presents descriptive evidence illustrating differences between audited firms and non-audited firms in key characteristics including size, company form and industry. The figure presents the relative distributions of audited and non-audited firms in 2012, and illustrates that larger firms and corporations are much more likely to be audited than other firms. Audits occur in all sectors, but firms operating in the manufacturing and construction industries are, on average, more likely to be audited in our raw data.

Figure 1: Distributions of audited firms and non-audited firms in 2012



Notes: The figure shows the relative distributions of all audited and non-audited firms in 2012 in terms of size (5 categories in terms of revenue), company form (sole proprietors, partnerships, corporation, other) and industries according to the Standard Industrial Classification used by Statistics Finland. The sample includes firms with positive revenue in 2012. Size according to revenue categories is defined in the year before the audit.

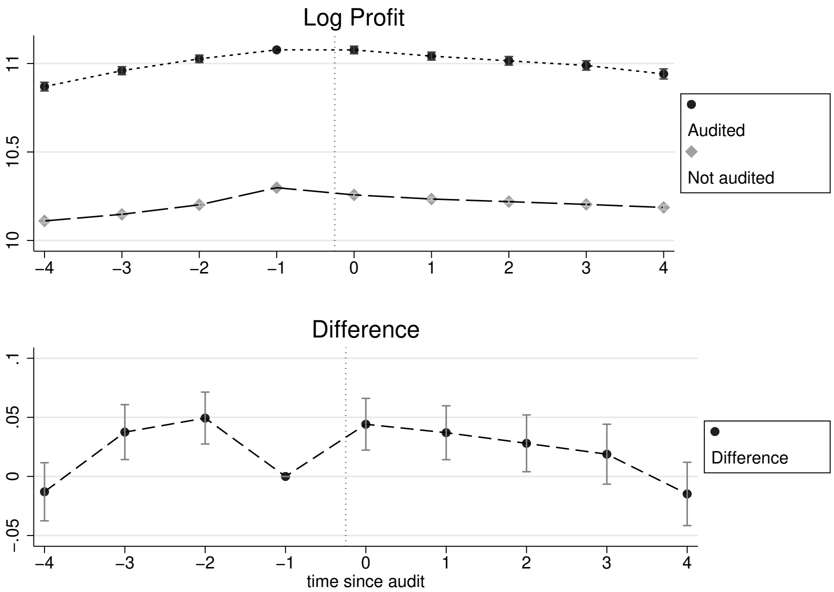
In addition to cross-sectional differences in observed characteristics, audited and non-audited firms may differ in terms of how their key outcomes develop over time. Certain patterns, such as a drop in reported profits or revenue, may induce the tax administration to conduct an audit. This means that being audited may be associated with a firm-specific trend in outcomes prior to audit.

To compare the development of audited and non-audited firms before and after the audit, we randomly allocate an observation year as a "placebo audit" year for the non-audited firms. We draw the placebo-audit year from a uniform probability distribution for the non-audited firms including all observation years after the firm enters the tax return data. Therefore, the non-audited firms do not receive any type of treatment in the randomly assigned placebo-audit year, but this procedure allows us to describe and compare the timelines of audited and non-audited firms in a similar fashion. This enables us to create a comparison group for the audited firms, which we also use in our difference-in-differences analysis described in more detail below.

Figure 2 plots the development of reported profits (in logs) for the audited firms and all non-audited firms with revenue between 100,000 and 10 million euros, and the difference in the development of profits between these two groups relative to one year before the audit and the placebo audit for the non-audited firms. This illustration is equivalent to testing for the randomness in the timing of an audit, since the timing of the placebo audit for the non-audited firms is randomly allocated. Hence, any differences in the development of profits prior to the audit year between the groups can reveal whether potential changes in profits are associated with the likelihood of being audited in the future.

The figure shows that the audited firms have larger profits and a higher profit growth rate before an audit, but they experience slower growth in profits just 1-2 years before being audited. This suggests that developments in key firm outcomes such as reported profits appear to be associated with the likelihood of an audit in the future.

Figure 2: Development of profits for audited firms and non-audited firms before and after the audit



Notes: The upper panel of the figure plots the development of log profits for the audited firms and non-audited firms from four years before to four years after the audit relative to the audit year, which is denoted by zero in the figure. The lower panel denotes the difference in the development of profits between these groups relative to one year year before the audit (year -1 in the figure) including the 95% confidence intervals. The development in log profits is estimated using equation (2) (explained in detail below) separately for the audited and non-audited firms using a sample that includes firms with revenue between 100,000 and 10 million euros in the year before the (placebo) audit. The placebo audit year for the non-audited firms is randomly drawn with an equal probability for each year after the firm is first observed in the administrative tax record data.

These findings have important implications for our empirical analysis. First, we cannot use an event-study approach where we would compare the development of the audited firms after an audit while using firms that were audited later as a control group. This type of timing-of-events approach relies on the timing of the audit being random (Borusyak et al. 2024). When this does not hold, the past development of those firms that are audited in the future does not represent a valid benchmark for the development of audited firms in the absence of an audit. Second, this type of selection into treatment can induce a well-known mean reversion problem, meaning that after a dip the outcomes tend to revert to their longer-run trajectories, which would further compromise identification of the effect of audits.

Therefore, instead of using a timing-of-events approach, we construct a matched comparison group of non-audited firms to provide a benchmark that describes the development of key firm outcomes in the absence of an audit. Our matching analysis aims at providing estimates of what might happen if the tax authority were to select an additional active firm into an audit. This is a key parameter in the actual policy of choosing the extent of audits and the resources of auditing activity. Next we discuss the matching procedure in detail.

Matching. We use a coarsened exact matching (CEM) strategy, introduced in Iacus et al. (2012). The CEM approach restricts the analysis to audited and non-audited firms that are sufficiently similar, defined as belonging to the same category or interval of selected matching variables before the audit. Consequently, the audited and non-audited firms that are similar in key observed characteristics are included in the treatment and comparison groups, while the audited firms that do not have similar non-audited counterparts are dropped from the analysis. Because CEM does not restrict the number of control group firms, we use CEM weights to equalize the number of comparison firms to the treated firms (see King 2012).[13]

We argue that the matching approach provides a reasonable empirical setting to analyze the development of firm outcomes after a tax audit. We cannot claim that our estimates are strictly causal, but we feel that a comparison of the development of firm outcomes for the audited and non-audited matched firms is of interest for several reasons. First, as we will show below, the pre-trends of the key outcomes for the matched audited and comparison firms are very similar. Therefore, the matched non-audited firms can be viewed as providing a comparison that describes the overall trends of key firm-level outcomes in the absence of an audit. Regarding the potential mean reversion problem, if there is mean reversion in firm outcomes after the audit, it should affect the treatment and comparison groups similarly, as their observed firm-level development is similar prior to the audit.

Second, there is likely to be some randomness in whether or not a firm is audited,

[13] CEM has various attractive properties compared to propensity score matching (King and Nielsen 2019): CEM is able to reduce imbalance and increase efficiency simultaneously, and there is no need to formally model selection into being audited. Furthermore, CEM automatically restricts the analysis to the common support of data. See Iacus et al. (2012) for more details on the CEM method.

especially between firms with similar observable tax return histories. The tax administration is likely to face resource constraints such that it cannot audit all the firms that may be non-compliant, and it often needs to rely on noisy signals of potential tax evasion to select audited firms. Table 2 below shows that no tax deficit was found in 42% of the audits conducted. This indicates that during the period covered by our data, selection into audit based solely on suspected non-compliance has been far from perfect.

Third, we argue that the matching procedure should identify firms that have a similar likelihood of being audited based on observable characteristics. In this sense, the resulting matched sample of comparison firms can be interpreted as including firms close to the margin of being audited. The matching procedure drops firms that are audited with either very high or very low probabilities (according to their observable characteristics), as there are not enough either treated or comparison firms in these groups. This implies that there are likely to be both non-compliant and compliant firms in our matched treatment and comparison groups.

Additionally, our matching strategy aims at focusing on firms with real economic activity before the audit. We compare audited and non-audited firms with similar revenues, costs, asset structures and histories in tax filing. This implies that our analysis will likely not include firms that exist solely to evade taxes, such as producing fake invoices to evade VAT. Firms with this type of purely fraudulent activity are also likely to face a relatively high audit rate in the first place. This means that these types of firms would at least be likely to receive only a small weight in our matching analysis, meaning that their impact on our results is small at best. Therefore, we argue that our approach captures responses to audits among the general firm population with real economic activity.

We use data from one year before the audit for the audited firms and the randomly assigned placebo audit year for the non-audited firms to form the matched groups of audited and comparison firms that we follow over time. The matching variables we use are revenue, change in revenue from $t-2$ to $t-1$ before the (placebo) audit, profits and net assets, and indicator variables for year, filing a tax return, positive revenue, 95th and 99th percentile points of revenue, organizational form, industry at the two-digit level, firm age category, and the number of years the firm is observed in the tax return data before the audit.

We use two criteria to select the preferred set of variables in the matching procedure. We select a specification that (i) improves the balance of covariates in the base year (one year prior to (placebo) audit) while still leaving enough firms in the matched sample of audited firms, and (ii) visually confirms the similarity of the pre-trends between the comparison and treatment groups in key outcomes, such as profits and revenue, before the audit. Therefore, we end up using rather strict criteria in choosing the preferred matching specification, as it includes both a comparison of the cross-sectional balance of the key variables between the treatment and benchmark groups and the parallel trends of key outcome variables between the groups before the audit. We present and discuss the characteristics of the audited and non-audited groups in more detail in Section 3.3. Our results and conclusions are not sensitive to the exact matching specification we select. The main results using alternative matching specifications are presented in Appendix B.

Estimation. To evaluate the development of key firm outcomes over time around the audits, we estimate the following equation using data four years before and five years after an audit for the matched audited and non-audited firms:

$$y_{itk} = \sum_{m=-4}^{4} \lambda_m I(t = k + m) + \sum_{m=-4}^{4} \delta_m (I(t = k + m) * Audited_i) + \alpha_i + \gamma_t + \epsilon_{itk} \qquad (2)$$

where y is the outcome for firm i in year t at event year k, which is the year of audit for audited firms and the placebo-audit year for the non-audited firms.[14] $I(t = k + m)$ is an indicator for the observation year m before or after the audit year k, with negative numbers denoting years before the (placebo) audit. In the equation, λ_m capture the common trend component of the audited and non-audited firms relative to the audit year, γ_t are year fixed effects and α_i are firm fixed effects. $Audited_i$ is an indicator for the

[14]For firms that are audited more than once during the event window included in our analysis, we define the audit year as the year in which the firm is audited for the first time. Whether the firm is audited again or not within our event window can be viewed as a potential outcome of the first audit, and hence we do not control for or select firms based on whether they face subsequent audits.

audited firms. Therefore, the coefficients δ_m represent the differences in the development of outcomes for audited and non-audited firms around the audit. We omit the event time dummy at $k-1$ so that the estimated event time coefficients δ_m represent the outcome development relative to the year before the (placebo) audit. Our baseline analysis uses an unbalanced panel of firms that allows firms to exit from the sample, but we conduct the analysis including only firms that are active throughout the whole observation period (balanced panel) in Appendix A in Figure A2 as a robustness check. Our results are robust to using this data specification.

We estimate the following difference-in-differences equation for each of the outcome variables to measure the average changes after the audit for the audited firms compared to the matched comparison firms:

$$y_{it} = \delta(After_{it} * Audited_i) + \beta X_{it} + \alpha_i + \gamma_t + \epsilon_{it} \tag{3}$$

where $After_{it}$ is an indicator for the years after an audit (or the placebo audit for the comparison group), and X_{it} includes additional firm covariates (age category, organizational form, industry, and an indicator for being located in the capital city area), and ϵ_{it} is the error term.[15] To compare the size of the estimates of different outcomes to each other, we translate the regression estimates into euros by using the mean of each outcome in the year just before the audit. These means are reported on the last row of each table reporting the regression estimates.[16]

[15]We categorize firms based on the number of years since the first observation in the panel in the following age categories: young firms (1-3 years), middle-aged firms (4-9 years) and old firms (10 or more years), or age unknown.

[16]Our empirical strategy is essentially a staggered difference-in-differences set-up with firm fixed effects, where there is a separate comparison group for each treatment year. This makes our strategy different from the standard two-way fixed effects (TWFE) design, which suffers from well-documented problems if there are heterogeneous or dynamic treatment effects (see e.g. Callaway and Sant'Anna 2021; Goodman-Bacon 2021; Borusyak et al. 2024). Our comparison group consists of never-treated firms, and we have a post-treatment period and time relative to treatment defined for the comparison group, allowing us to estimate the dynamics related to treatment timing, not just differences in those treated earlier vs. later. Because of this, our set-up is analogous to the difference-in-differences with no difference in treatment timing, as the parameters δ_m identify the development relative to the treatment year k, not relative to time t, and the development over time is controlled for by the year fixed effects, which are separate from the treatment period effects λ_m. Common TWFE estimation strategies do not allow for estimating λ_m as the treatment year is not defined for the never-treated group. Also, our matching strategy matches the comparison firms by year, identifying a comparison group for firms audited in each year. The challenges in the standard staggered difference-in-differences arise because

3.3 Descriptive Statistics

Table 1 presents the descriptive statistics for the audited and non-audited firms. We present the statistics for the full firm population in columns (1) and (2), and separately for the matched firms in columns (3)–(5). In addition, column (6) shows the characteristics of those audited firms that are not matched to any non-audited firm in the CEM procedure. After matching we have 19,425 audited matched firms in our treatment group, comprising almost half of the audited firms in our full data. In the comparison group, we have 354,048 non-audited matched firms.

Table 1 shows that the audited firms are on average much larger in all characteristics than other firms that are not audited (columns 1 and 2). The descriptive statistics illustrate that the matching procedure clearly reduces the observed differences between the audited and non-audited firms, especially by dropping the largest audited firms from the sample (columns 3 and 4). Using the CEM-weighted averages, differences in size and other key observed characteristics between the matched audited firms (column 3) and the comparison firms (column 5) are largely removed one year before the (placebo) audit.

units have pre- and post-treatment periods of different duration, and hence get different implicit weights in the regression according to the duration of the treatment period. Our approach using the randomly assigned placebo audit year for the comparison group avoids these challenges, because it creates separate comparison groups for the firms audited in different years and hence the pre- and post-treatment periods have the same length.

Table 1: Descriptive statistics

	All		Matched			Not Matched
	(1)	(2)	(3)	(4)	(5)	(6)
	Audited	Non-audited	Audited	Non-audited	Non-audited weighted	Audited
Revenue	1,914,071	188,716	572,996	100,688	541,043	2,880,207
	(2,819,692)	(735,679)	(1,618,662)	(347,769)	(1,624,492)	(3,092,254)
Profit	88,532	15,610	29,255	8,576	28,889	133,965
	(163,480)	(50,543)	(87,111)	(22,387)	(87,102)	(191,408)
Value added	807,853	106,512	267,072	60,326	254,536	1,197,442
	(1,140,723)	(384,878)	(647,948)	(151,085)	(653,717)	(1,254,514)
Variable costs	1,273,966	121,420	396,925	53,753	381,030	1,865,745
	(2,147,153)	(568,208)	(1,302,645)	(284,779)	(1,317,595)	(2,388,927)
Labor costs	513,317	72,270	181,537	39,497	170,706	723,199
	(766,099)	(235,765)	(469,321)	(116,900)	(451,425)	(839,749)
Wage costs	371,623	39,822	140,435	25,657	133,401	521,614
	(597,420)	(157,001)	(424,245)	(93,087)	(411,525)	(643,845)
Employees	15	1.7	5.9	1	5.1	21
	(19)	(5.8)	(11)	(3.4)	(11)	(21)
Corporation	.74	.27	.61	.32	.61	.85
	(.44)	(.44)	(.49)	(.47)	(.49)	(.36)
N	42,510	865,181	19,425	354,048	354,048	23,085

Notes: Table presents the descriptive statistics from one year before the audit for the audited firms and the randomly assigned placebo audit year for the non-audited firms. Table presents the mean values and standard deviations in parentheses. The full population of firms is included in columns (1) and (2), and the matched sample in columns (3)–(4). Column (5) includes the statistics for the matched non-audited firms with CEM weights. The characteristics for the audited non-matched firms are presented in column (6). Table A1 in Appendix A presents the detailed definitions of the variables. The CEM matching variables used in the weighting procedure include (bins of) revenue, change in revenue from $t-2$ to $t-1$ before the audit, profits and net assets, and indicator variables for year, filing a tax return, positive revenue, 95th and 99th percentile points of revenue, organizational form, industry at the two-digit level, firm age category, and the number of years the firm is observed in the tax data before the (placebo) audit year.

Table 2 shows the descriptive statistics of tax audits for all audited firms and the audited firms in our matched sample. Overall, the descriptive statistics for the audits are similar for all audited firms and the firms included in the matched sample. The average total detected tax deficit in an audit is approximately 22,000 euros for all audited firms and 21,000 euros for the matched firms. The detected tax deficit represents 12% of the average firm-level total revenue in the year before the audit for all firms and 19% for the matched sample. A positive deficit is found on average in 58% of all audits and almost the same percentage, 60%, for the matched firms. For more serious offenses the fractions are slightly larger for the matched firms compared to all audited firms: shadow economy activities, as defined by the tax administration, were found in 29% vs. 20% of the audits, and 13% vs. 9% of the cases ended up in criminal investigations for the matched sample and all audited firms, respectively. On average, tax audits lasted 123 days for all audited firms and 128 days for the firms in the matched sample, and the average number of tax years audited is around 1.5, meaning that the detected tax deficit has accumulated over a span of about one or two years on average.

Table 2: Descriptive statistics of tax audits

	(1) All	(2) Matched
Tax deficit (in euros)	22,370	21,490
	(61,545)	(58,550)
Tax deficit per revenue	.12	.19
	(.44)	(.55)
Positive tax deficit detected	.58	.6
	(.49)	(.49)
Measures undertaken due to audit	.67	.68
	(.47)	(.47)
Shadow economy activities	.2	.29
	(.4)	(.45)
Criminal charges	.086	.13
	(.28)	(.34)
Duration of audit (in days)	123	128
	(146)	(157)
Number of years audited	1.4	1.5
	(1.1)	(1.1)
Observations	42,512	19,425

Notes: Table presents the mean values (standard deviations in parentheses) of the key variables and characteristics of tax audits in 2003–2016 for all audited firms in column (1) and for the audited firms in the matched sample in column (2). Table A1 in Appendix A presents the detailed definitions of the variables.

4 Results on Tax Reporting

We begin this section by presenting our baseline results on the changes in the development of firm profits and value added around the audits. Value added captures the scale of business in terms of producing goods and services, and profits is a measure of the income generated for the owners of businesses after all cost items in production. These two outcomes represent the most relevant firm-level tax bases from a policy point of view. We then analyze the potential mechanisms behind the responses and examine the heterogeneity of the results by observed tax compliance and key firm-level characteristics such as size and company form.

4.1 Reported Profits and Value Added

The upper panel of Figure 3a plots the development of taxable profits in logs separately for matched audited and non-audited firms, and the lower panel shows the respective difference-in-differences estimates using equation (2) with 95% confidence intervals for each year. We follow the development of firms from four years before to five years after the audit, including the audit year. In the figure, the estimates are presented relative to one year before the (placebo) audit (year -1 on the horizontal axis).

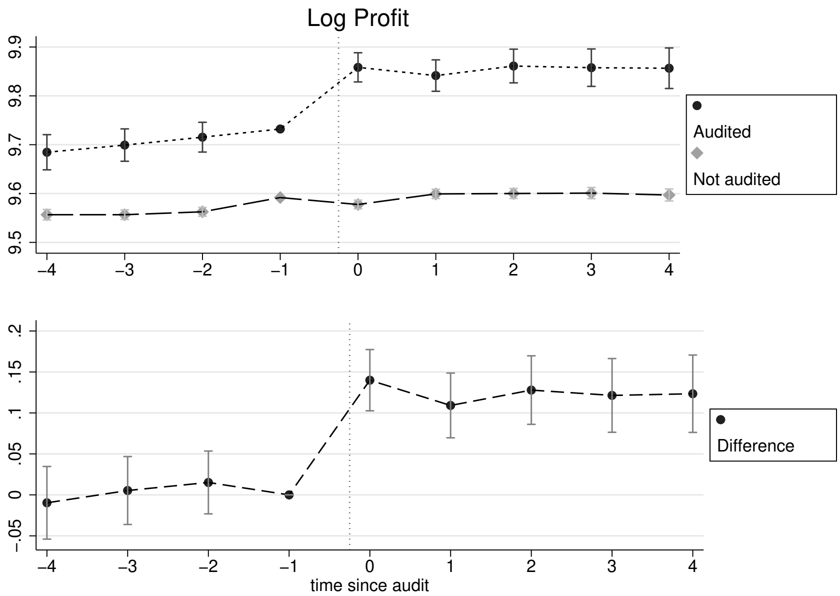
Figure 3a shows that the profits of the matched audited and non-audited firms developed very similarly before the (placebo) audit. The figure thus illustrates that our matching strategy is successful in eliminating the pre-trend differences between the audited and non-audited firms observed in the raw data (see Figure 2). Therefore, we can argue that in this sense the matched non-audited firms provide a meaningful comparison for the economic development of the matched audited firms.

As our main result, we find a clear increase in reported profits right after the audit. Profits increase by more than 10% immediately in the year of the audit, and this increase is persistent and statistically significant for the whole follow-up period. This illustrates that audits are associated with a clear and consistent increase in reported profits among the audited firms relative to the matched comparison group.

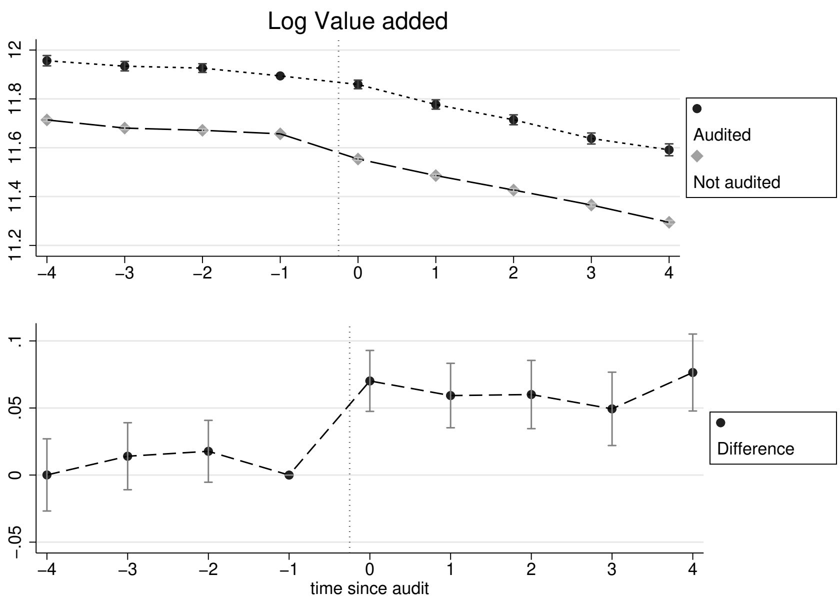
Figure 3b shows the development of value added (revenue minus costs of materials and services) over time following the same structure as in Figure 3a. The figure illustrates that the development of value added is similar for both matched audited and non-audited firms before the audit. Right after the audit, reported value added is on average approximately 5% larger for the audited firms compared to non-audited firms, and remains larger throughout the follow-up period. Therefore, the results on value added are very similar to those for profits presented above.

Table 3 summarizes the difference-in-differences estimates for profits and value added using equation (3). Columns (1) and (2) of the table show the results for log profits and log value added. Following an audit, the reported profits of audited firms increase by 12.3% on average, which, given the outcome mean in the bottom row of the table, translates into an average increase of approximately 2030 euros per year. Reported value added increases by 5.7% on average, which translates into an increase of approximately 5240 euros.

Figure 3: Firm responses to audits: Reported profit and value added



(a) Reported profits



(b) Value added

Notes: Figure plots the development of log profits and value added for the matched audited firms and non-audited firms relative to the audit year (denoted by zero in the figure), and the difference in profits between the firm groups relative to one year before the (placebo) audit. The upper graph of each panel plots the regression coefficients λ_m using equation (2) including 95% confidence intervals estimated separately for the audited and non-audited firms. The lower graphs plot the regression coefficients δ_m with 95% confidence intervals. The regressions include firm and year fixed effects and are estimated using CEM weights.

Table 3: Difference-in-differences results: Reported profits and value added

	(1) Log Profit Profit	(2) Log Value added	(3) Log 3-year moving average of profits	(4) Profits in euros
Audited * After audit	0.123***	0.057***	0.071***	2,701*
	(0.015)	(0.009)	(0.012)	(1,122)
Observations	1,006,090	1,223,151	1,092,855	1,617,032
Adjusted R^2	.0032	.038	.085	.008
Outcome mean for the audited firms	9.65	11.4	9.37	29,255

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Table presents the difference-in-differences estimates δ using equation (3) and the means of the outcome variables for the audited firms in the year before the audit. The regressions control for the common trend λ, year fixed effects, firm age category, and location in the capital city area. The regressions are estimated using CEM weights.

One potential concern when using log profits as an outcome variable is that it ignores firms reporting zero profits or losses. This conceals a potentially important channel if, for example, a significant proportion of firms become unprofitable after an audit. Therefore, we use two alternative profit measures which alleviate these concerns in columns (3) and (4) of Table 3. Our results for the log of the three-year moving average of profits and profits in euros suggest qualitatively very similar changes in taxable profits as in column (1), indicating that our baseline results are robust to the choice of how we measure our main outcome variable. The results using the three-year moving average of profits suggest a slightly smaller response compared to our baseline estimate, which can be expected since any changes in this variable are attenuated in the first years after the audit by definition. The euro-profit result implies a slightly larger increase in profits than our baseline estimate (2700 vs. 2000 euros).[17] Table A2 in the Appendix similarly summarizes the results using log of three-year moving average of value added and value added in euros. The estimate for three-year moving average is slightly smaller at 2% and statistically significant, but for value added in euros the estimate is larger at €18,000 but not statistically significant due to the large standard error.

Overall, our main results show a rapid and persistent increase in reported profits and value added following a tax audit. This finding indicates substantial non-compliance. The immediate increase in reported profits in the year of the audit indicates misreporting of

[17]Figure A1 in Appendix A illustrates that the development of the alternative profit measures before and after the audit is qualitatively very similar to our baseline measure of log profits.

both profit and value added taxes in the baseline. The fact that the increase in reported taxes persists over time after the audit suggests that audits have long-lasting effects on tax compliance beyond the direct tax deficit detected in the audit. This means that the audited firms fundamentally change their reporting behavior in future years. This observation is similar to the findings for self-employed individuals in some earlier studies (Mazzolini et al. 2022; Beer et al. 2020; D'Agosto et al. 2018). One potential mechanism behind this is that firms may update their perceived audit probability or the likelihood of other types of monitoring upwards after an operational tax audit.

However, our results contrast with the "bomb-crater" effect of audits observed in some studies, which should lead to reduced compliance after an audit. This type of pattern is documented, for example, by DeBacker et al. (2015b) who use a measure of effective tax rates to capture developments in the tax aggressiveness of firms. Furthermore, our results suggest that the potential negative real effects of audits in subsequent years are not large enough to counter the positive reporting response. Nevertheless, the observed positive response could hide potential negative real effects on firm performance. We return to this issue using firm bankruptcies as a measure of real responses in Section 5.

4.2 Mechanisms of Non-compliance

Next, we study the potential mechanisms behind the observed changes in reported profits. The increase in profits after an audit is an indication of tax non-compliance in the baseline, while analyzing the different components of firm reporting may be informative of the channels of non-compliance, as discussed in more detail in Section 2.2.

Our main outcome variables in this analysis are (log) revenue, labor costs, variable costs, and the number of employees. Figure 4 shows the development of these outcomes before and after the audit. The pre-trends for some of these variables are admittedly not as clean as for our main outcomes. Nevertheless, we observe clear and persistent increases in both revenue (panel a) and labor costs (panel b) after the audit for the audited firms in comparison to matched non-audited firms. Also, the number of employees (panel d) increases slightly after the audit. However, interestingly, there appears to be no clear change in variable costs (panel c) after audits. This suggests that misreporting of costs is concentrated in labor costs rather than intermediate goods and services used by the firms. One potential explanation for this is the third-party reporting of VAT, which may limit the extent to which firms are willing to misreport their costs (see e.g. Waseem 2022).[18]

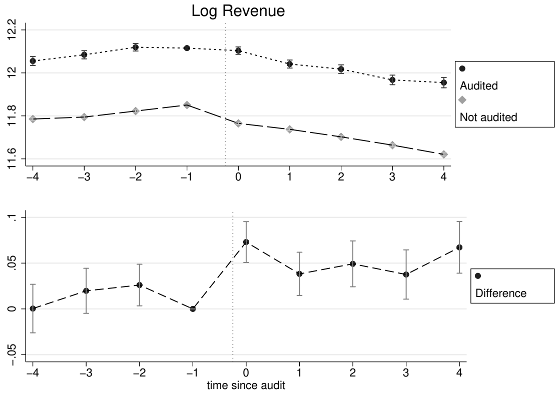
The corresponding regression estimates are presented in Table 4. The estimates show a 4.5% increase in revenue, 6.1% increase in labor costs, 6.8% increase in wage costs (not including payroll taxes and social security contributions) and 2.8% increase in the reported number of employees. Also, the estimates show an insignificant 1.8% increase in variable costs. These estimates are line with the visual observations above.

These observed responses paint a fairly consistent picture that can be understood in the light of the theoretical considerations outlined in Section 2.2. The observed behavior indicates under-reporting of revenue as the key channel of tax non-compliance. Also, there appears to be under-reporting of costs, which may be a sign of under-reporting of the overall scale of the business. This observation is consistent with an attempt to manipulate audit probabilities, even though firms' intentions cannot of course be directly observed from the administrative data.

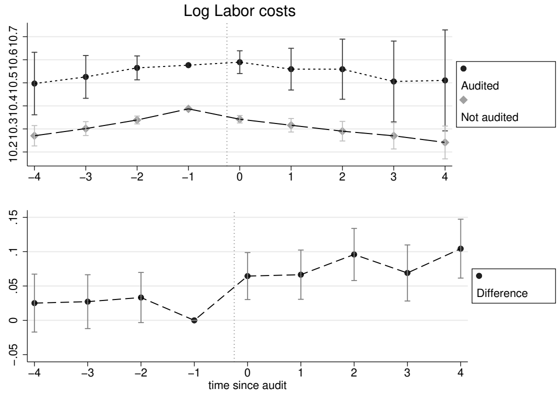
Cost under-reporting applies in particular to labor costs rather than other variable costs. As discussed in Section 2.2, if firms under-report the scale of their operations, there are incentives to do so via under-reporting of labor costs: under-reporting labor costs has the additional benefit of reducing payroll tax payments and the costs associated with other mandatory employer obligations, as well as potentially enabling employees to under-report personal income taxes. This can be particularly relevant in Finland, where the payroll tax rate is relatively high (25%). The simultaneous increase in the number of reported workers further supports the labor channel being the most relevant in terms of cost misreporting.

[18] Figure A2 in Appendix A plots the development of the main variables for firms that are active throughout our full observation period. The patterns are similar in this specification to our baseline unbalanced panel sample.

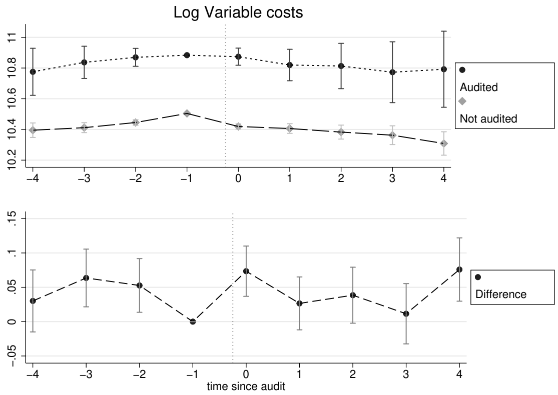
Figure 4: Firm responses to audits: Other outcomes



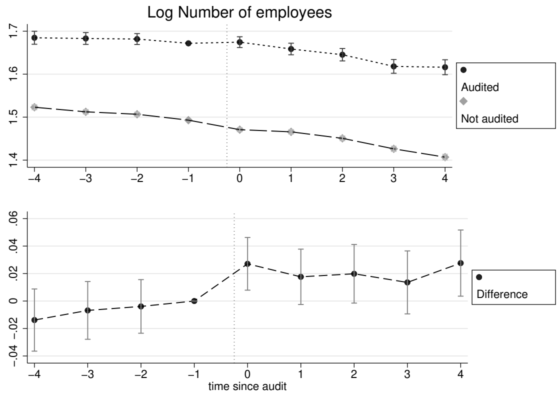
(a) Revenue



(b) Labor costs



(c) Variable costs



(d) Number of employees

Notes: Figure plots the development of log revenue, labor costs, variable costs and the number of employees for the matched audited firms and non-audited firms relative to the audit year (denoted by zero in the figure), and the difference in these variables between the firm groups relative to one year before the (placebo) audit. The upper panels plot the regression coefficients λ_m from equation (2) including 95% confidence intervals estimated separately for the audited and non-audited firms. The lower panel plots the regression coefficients δ_m with 95% confidence intervals. The regressions include firm and year fixed effects and are estimated using CEM weights.

Table 4: Difference-in-differences results: Other outcomes

	(1) Log Revenue	(2) Log Variable costs	(3) Log Labor costs	(4) Log Wage costs	(5) Log Number of employees
Audited * After audit	0.045***	0.018	0.061***	0.068***	0.028***
	(0.010)	(0.016)	(0.014)	(0.012)	(0.007)
Observations	1,259,532	996,210	850,289	568,687	557,249
Adjusted R^2	.01	.0055	.0061	.0098	.011
Outcome mean for the audited firms	12	10.9	10.5	10.8	1.59

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Table presents the difference-in-differences estimates δ using equation (3) and the means of the outcome variables for the audited firms in the year before the audit. The regressions control for the common trend λ, year fixed effects, firm age category, and location in the capital city area. The regressions are estimated using CEM weights.

4.3 Heterogeneity

Next, we study how responses to audits differ by firm type. To do this, we estimate the following equation, which adds an indicator for firm group (E_i) to equation (3) presented above:

$$y_{it} = \lambda_1(After_{it} * E_i) + \delta_1(After_{it} * A_i) + \delta_2(After_{it} * A_i * E_i) + \alpha_i + \gamma_t + \beta X_{it} + \epsilon_{it} \quad (4)$$

In the following regression tables we report δ_1, which denotes the average response to a tax audit for the firms not included in each group, and δ_2, which is the response for the studied firm group in addition to δ_1.

First, we use the data on audit outcomes, which are rarely accessible to researchers, to split the audited firms into two groups: those that were found to have a positive tax deficit and those with no deficit detected in the audit. We compare these two groups of firms to the matched non-audited group, as we naturally do not observe a compliance status for the non-audited firms.

The regression estimates presented in Table 5 show that both of these groups increase reported profits following an audit, but the response is much larger for firms with a detected positive deficit compared to those with no detected tax deficit. The estimates indicate that the log profit of audited firms with no tax deficit increased by 6.5%, while the log profit of audited firms with a positive tax deficit increased by 11% in addition to that, implying a 17.5% and statistically significant increase for the non-compliant group (see column (1) of Table 5). In contrast, there is no difference in reported value added between these groups after the audit (column 2). Using the three-year moving average of log profits as the outcome, the results are very similar to the baseline estimate (column 3). The overall increase in euro-profits and the difference between the deficit and non-deficit groups are not significantly different from zero statistically, but the difference in magnitude is significant given that the positive deficit firm group has a lower baseline of reported profits (column 4).[19]

Table 5: Difference-in-differences results by detected tax deficit

	(1) Log Profit	(2) Log Value added	(3) Log 3-year moving average of profits	(4) Profits in euros
Audited * After audit	0.065***	0.061***	0.038*	1941
	(0.019)	(0.012)	(0.016)	(1226)
Difference: Positive deficit	0.108***	−0.008	0.059**	1361
	(0.022)	(0.015)	(0.020)	(913)
Observations	1,006,090	1,223,151	1,092,855	1,617,032
Adjusted R^2	.0033	.038	.085	.008
Outcome mean for the audited firms	9.7	11.6	9.47	35,699
Outcome mean for the audited firms with positive deficit	9.61	11.3	9.29	24,088

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Notes: Table presents the difference-in-differences estimates δ_1 and δ_2 using equation (4) and the means of the outcome variables for the audited firms in the year before the audit. Table shows the difference of the estimates between those audited firms with no detected tax deficit in the audit (Audited * After audit) and those with a detected tax deficit (Difference: Positive deficit). The regressions control for the common trend λ, year fixed effects, firm age category, and location in the capital city area. The regressions are estimated using CEM weights.

We also analyze the heterogeneity of responses for different types of firms whose tax evasion opportunities, and hence potential responses to tax audits, might differ: i) larger versus smaller firms and ii) corporations versus other company forms. First, larger firms tend to be more frequently audited by the tax administration and may respond to this underlying higher perceived probability of a tax audit by higher compliance. Compliance

[19]Figure A3 in Appendix A presents the development of reported profits and value added for the positive deficit and no deficit firm groups relative to the non-audited matched firms.

may also be higher due to the more stringent reporting and accounting requirements faced by larger firms. In addition, the mechanisms of tax non-compliance may differ as larger firms may engage in more sophisticated forms of tax evasion that are more difficult to detect. These might show up as smaller observed responses to tax audits. In the analysis, we split our sample into two groups based on the median value of revenue before the audit, and the firm is denoted as large if it has revenue above that value one year before the audit year.

Second, incorporated firms have different tax reporting rules and may have different incentives for tax evasion than unincorporated firms. The more detailed tax reporting requirements for incorporated firms could reduce both their incentives and possibilities for evasion compared to unincorporated firms, which have much more simplified and less detailed tax reporting requirements. In addition, corporations tend to be more likely to use an external auditor for their accounts compared to unincorporated firms.

The results of these analyses are presented in Table 6. Note that here we can estimate a full triple-difference model, as there are non-zero values for the indicator variable for the comparison group too. The estimates show that larger firms seem to have smaller responses in reported profits and value added compared to smaller firms. The differences are quantitatively meaningful but not statistically significant, except for value added. Also, corporations have a smaller increase in log profits and valued added, but the differences to the unincorporated firms are not statistically significant. These results imply that in general the increases in reported profits after audits are somewhat larger among firm groups that are ex-ante more likely and able to engage in tax evasion.

Table 6: Difference-in-differences results by firm type

	(1) Log Profit	(2) Log Value added	(3) Log 3-year moving average of profits	(4) Profits in euros
Audited * After audit	0.167***	0.141***	0.106***	886.662
	(0.034)	(0.027)	(0.032)	(642.499)
Difference: Large firm	−0.050	−0.098***	−0.041	2,230.148
	(0.037)	(0.029)	(0.035)	(1,442.667)
Observations	1,006,090	1,223,151	1,092,855	1,617,032
Adjusted R^2	0.003	0.038	0.085	0.008
Audited * After audit	0.147***	0.090***	0.092***	2,867.264***
	(0.017)	(0.014)	(0.016)	(392.455)
Difference: Corporation	−0.039	−0.049**	−0.029	−229.286
	(0.027)	(0.019)	(0.023)	(1,592.742)
Observations	1,006,090	1,223,151	1,092,855	1,617,032
Adjusted R^2	0.004	0.038	0.086	0.008

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Table presents the difference-in-differences estimates δ_1 and δ_2 using equation (4). Table shows the difference of the estimates between audited firms below and above median revenue in the year before the audit (Difference: Large firm), and for unincorporated (partnerships and sole proprietors) and incorporated firms (Difference: Corporation). The regressions control for the common trend λ, year fixed effects, firm age category, and location in the capital city area. The regressions are estimated using CEM weights.

5 Bankruptcies and Firm Survival

Next, we examine whether there are changes in firm survival after an audit. As discussed above in Section 2.2, tax audits can induce real responses among the audited firms in addition to reporting effects. However, separating real and reporting responses is challenging, as the observed changes in, for example, reported profits and revenue are a combination of reporting and real responses, meaning that potential negative real response could be concealed behind an increase in reported profits.

Firm exits can be considered a straightforward indicator of changes in real economic activity. This is especially so in the Finnish context, where the role of informal businesses is negligible. To analyze real responses through forced exits, we use a novel approach where we combine the tax audit data with administrative data on bankruptcies. We argue that bankruptcy is likely to measure a real firm exit, as it is an extreme outcome for a firm. First, a firm cannot simply decide to go bankrupt. A bankruptcy petition must first be filed to a court either by the firm itself or its creditor(s). Then the court decides on the outcome of the petition. Second, bankruptcy is expensive and undesirable for a firm: it is a long process lasting typically from 2 to 5 years, and once bankruptcy is declared, the owner can no longer decide on how to use the firm's assets, as they are then liquidated and paid to the creditors. In addition, bankruptcy entails reputational harm for the firm and its owners, as bankruptcy petitions and decisions are public information in Finland and are posted online and in a newspaper.

Our data consist of all bankruptcy petitions including the court decisions. There are three main outcomes: i) a petition may be cancelled if the firm is considered viable, or two types of firm exits can be enforced: ii) bankruptcy or iii) lapse of bankruptcy, where the business is ended without bankruptcy proceedings due to lack of sufficient funds. After its decision the court appoints an estate administrator to distribute the firm's assets to the creditors. If the creditors agree that there are not enough funds to go through with bankruptcy proceedings, the bankruptcy lapses and the firm is simply removed from the company registers. In addition, the court can decide to continue the bankruptcy under iv) public receivership even without sufficient funds if there are reasons to gather more information on the firm. This action is mainly used to combat the grey economy or economic crime. Hence public receivership is of additional interest in studying the impacts of tax audits.

As a justification for the costliness of bankruptcy procedures, bankruptcy petitions and decisions are rare in the overall firm population in Finland. On average, only 1% of firms are filed for bankruptcy in a given year. About 58% of these firms end up in forced exit: 20% are declared bankrupt and in 38% of the cases there is a lapse of bankruptcy. The bankruptcy petition is cancelled in 23% of the cases.

Panels (a) and (b) of Figure 5 depict the development of the bankruptcy outcomes for matched audited and non-audited firms, including bankruptcy petitions and all forced exits combining declared, lapsed and public receivership bankruptcies. It should be noted that because we analyze firms that still exist one year prior to the (placebo) audit, we do not have a meaningful estimate for the difference in exit rates before the audit. Hence, our estimates rather describe the difference in bankruptcies between audited and non-audited firms that are similar in observed economic development before the (placebo) audit.

The figure shows that there is a clear increase in all of these outcomes among audited firms after the audit. Bankruptcy outcomes peak at one or two years after the audit. This suggests that the likelihood of bankruptcy procedures increases significantly right after the audit.

Panels (c) and (d) of Figure 5 show the same outcomes as above separately for firms that were observed to be compliant and non-compliant in the audit, defined similarly as in Section 4.3 above. The figure reveals that the increase in bankruptcy petitions and associated forced exits is completely driven by firms with a tax deficit detected in the audit. In contrast, the bankruptcy rate for audited firms with no tax deficit follows very closely that for the comparison group of matched non-audited firms.

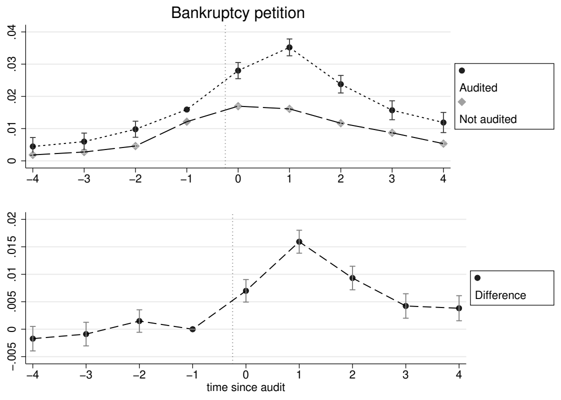
To numerically assess the development of bankruptcy rates, we form a cumulative indicator for these outcomes before the audit (in years -4 to -1) and after the audit (0 to +4) for the firms and use it in our regression analysis. Table 7 shows the regression results separately for all the different bankruptcy outcomes (petitions, all forced exits, declared and lapsed bankruptcies, and public receivership). The results confirm the visual findings above: there is a statistically significant and relatively sizable increase in all bankruptcy outcomes for the audited firms. We find a 3 percentage-point increase in bankruptcy petitions relative to the 5.2% mean of the non-audited firms after the (placebo) audit and a 1.8 percentage-point increase in total forced exits relative to a 4.6% mean. The increases in lapsed and public receivership bankruptcies are very large relative to the non-audited firms' outcome means (38% and 400%, respectively), but the absolute magnitudes are still rather small at 1.1 and 0.3 percentage points.

The bottom panel of Table 7 shows the results separately by the observed compliance status in the audit using a similar approach as in Section 4.3. Additionally, the table shows the results for the audited firms in which grey economy activities were detected by the tax administration. The regression results highlight that the observed increase in bankruptcies after audits is completely driven by fraudulent firms, and particularly by those firms associated with more severe tax evasion activities.[20]

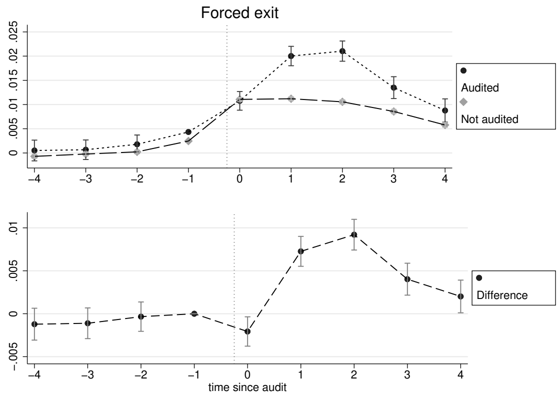
To sum up, our results suggest that there can be significant negative real effects associated with audits through increased bankruptcy rates. However, this effect appears to be solely concentrated among the non-compliant firms. In contrast, there is no observable increase in forced exits for compliant firms. This indicates that the negative real effects measured by firm exits are apparent only for fraudulent firms, and particularly for those with grey economy activities. While we typically consider that the real effects of higher tax enforcement negatively impact the economy, the exits of fraudulent firms can even increase efficiency and the market outcomes of compliant firms. A caveat to our findings is that bankruptcy procedures might themselves trigger an audit, particularly related to grey economy activities. This would mean that our estimates represent an upper bound of the potential effect of audits on subsequent bankruptcies. Nevertheless, our results clearly illustrate that audits do not induce increased firm exits among compliant firms, suggesting that audits do not significantly reduce the real economic activity of honest taxpayers.

[20] As an alternative measure for firm exits, we use an indicator variable of whether a firm has reported positive revenue or not. Appendix A Table A3 collects these results, and shows that they are qualitatively in line with the bankruptcy results, suggesting that tax audits increase firm exits.

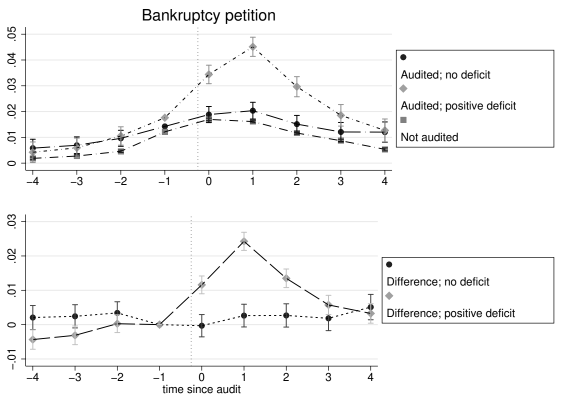
Figure 5: Firm responses to audits: Bankruptcies



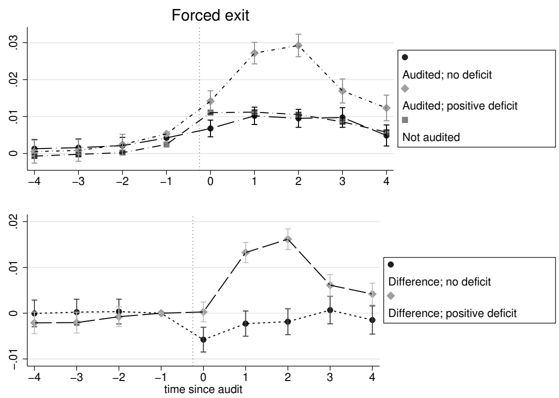
(a) Bankcruptcy petition



(b) Forced exit



(c) Bankcruptcy petition by tax deficit status



(d) Forced exit by tax deficit status

Notes: Panels (a) and (b) of the figure plot the development of bankruptcy petitions and forced exits for the matched audited firms and non-audited firms relative to the audit year (denoted by zero in the figure), and the difference in the outcomes between the firm groups relative to one year before the (placebo) audit. Forced exits include exits due to declared, lapsed and public receivership bankruptcies. Panels (c) and (d) in the figure separate out audited firms with and without a positive tax deficit detected in the audit by the tax administration. The upper graph of each panel plots the regression coefficients λ_m using equation (2) including 95% confidence intervals estimated separately for the audited and non-audited firms. The lower graphs plot the regression coefficients δ_m with 95% confidence intervals. The regressions include firm and year fixed effects and are estimated using CEM weights The panel is balanced by filling zeros for firms in the year they are not observed in the data.

Table 7: Difference-in-differences results: Bankruptcies

	(1) Bankruptcy petition	(2) Forced exit	(3) Bankruptcy declared	(4) Bankruptcy lapsed	(5) Bankruptcy public receivership
Audited * After audit	0.030***	0.018***	0.004**	0.011***	0.003***
	(0.003)	(0.002)	(0.001)	(0.002)	(0.001)
Observations	746,946	746,946	746,946	746,946	746,946
Adjusted R^2	.021	.038	.012	.026	.0016
Outcome mean: audited	.026	.00695	.00165	.00479	.000515
Outcome mean: non-audited	.0516	.0464	.017	.0294	.000754
Audited * After audit	−0.001	−0.011***	−0.003*	−0.010***	0.001*
	(0.003)	(0.002)	(0.001)	(0.002)	(0.000)
Difference: Positive deficit	0.043***	0.034***	0.010***	0.023***	0.002*
	(0.005)	(0.004)	(0.002)	(0.003)	(0.001)
Difference: Grey economy	0.063***	0.067***	0.013***	0.049***	0.005***
	(0.006)	(0.005)	(0.003)	(0.004)	(0.001)
Observations	746,946	746,946	746,946	746,946	746,946
Adjusted R^2	.022	.038	.013	.027	.0019
Outcome mean: audited	.0149	.00361	.000374	.00311	.000125
Outcome mean: audited & positive deficit	.0249	.00692	.0019	.00485	.000173
Outcome mean: audited & grey economy activities	.0429	.0118	.0032	.00712	.00142
Outcome mean: non-audited	.0516	.0464	.017	.0294	.000754

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: The upper panel in the table shows the baseline coefficients for all audited firms, and the bottom panel includes the difference of the estimates between audited firms with and without a detected tax deficit in the audit (Difference: Positive deficit) and firms with and without grey economy activities as defined by the tax administration (Difference: Grey economy). The regressions control for the common trend λ, year fixed effects, firm age category, and location in the capital city area. The regressions are estimated using CEM weights. Outcome means for the audited firms are measured one year before the audit and for non-audited firms one year after the randomly assigned placebo audit

6 Tax Revenue and Welfare

Next, we provide a back-of-an-envelope calculation of the increase in tax revenue from an audited firm based on the development in tax reporting after the audit. The tax revenue implications of audits are naturally of interest to the tax administration, but increases in tax revenue have broader significance. Keen and Slemrod (2017) show that the elasticity of tax revenue is a sufficient statistic for the behavioral response to administrative interventions, and therefore a crucial ingredient in the welfare analysis of tax enforcement measures. Hendren (2016) has developed a similar analysis and metric for a broader class of policy changes, with an application for tax enforcement provided by Boning et al. (2023).[21]

In calculating the tax revenue implications of the firms' behavioral response to audits, we include the three most important tax bases: profit, value added and payroll taxes. This calculation, therefore, provides a lower bound of the change in tax revenue following an audit. First, it does not include all taxes the firm is liable for (e.g. property tax or excise taxes), or spillovers to other taxpayers such as other firms, or the employees or owners of the audited firm. To the extent that our estimation results can be interpreted causally, our calculation corresponds to the additional tax revenue collected from auditing one more firm according to the usual auditing practice, over and above the evaded revenue recovered directly at the audit.

In Table 8 we construct the estimated change in tax revenue due to behavioral responses as follows: first, we convert our difference-in-differences estimates of the change in the log of the tax bases (profits, value added and wage costs) into euro amounts based on the treatment group average in the year prior to the audit. Second, we account for the extensive margin response – the probability of filing a positive value in the tax report – as the log outcomes only include positive values.[22] We calculate the probability by adding the change in the probability of a positive report after an audit to the baseline probability in the year before the audit. Then we obtain the average total change in the tax bases by multiplying the euro amount by the probability. Finally, we multiply the changes in the tax bases by the tax rates to obtain the total change in implied tax revenue. We use the standard VAT rate of 22% in 1994–2010, the corporate tax rate of 26% in 2005–2011, and an average payroll tax rate of 25% in the calculations.

[21]Such welfare analysis naturally hinges on an estimate of the causal effect of the policy in question. Our estimates can provide an input to welfare analysis to the extent that the estimates can be viewed as causal.

[22]The results on the extensive margin regressions are summarized in Table A3 in Appendix A.

Table 8: Change in tax revenue due to responses to audits

	Profit	Value added	Payroll	Total
Change in tax base (log coef.)	0.123***	0.057***	0.068***	
Change in tax base in euros	2038	5396	3422	10,856
Change in probability of positive report	0.005	−0.008*	0.005	
Probability of positive report after audit	0.65	0.77	0.57	
Total change in tax base (in euros)	1316	4179	1934	
Tax rate	26	22	25	
Tax revenue per year (in euros)	342	919	484	1745
5-year sum of change in tax revenue (in euros)	1711	4597	2418	**8726**

Notes: The table presents the numbers used for estimating the change in tax revenue (profit, value added and payroll taxes) due to firm responses to audits. We convert our difference-in-differences estimates (top row) of the changes in the log of the tax bases into euro amounts based on the treatment group average in the year prior to the audit. We account for the extensive margin response by adding to the estimates the change in the probability of a positive report after an audit. We obtain the average total change in the tax base by multiplying the euro amount by the probability. We multiply the change in the tax bases by the tax rates to get the total change in implied tax revenue. We use the standard VAT rate of 22% in 1994–2010, the corporate tax rate of 26% in 2005–2011, and an average payroll tax rate of 25% in the calculations.

The table shows that there is an average increase per firm of approximately 8700 euros in tax revenue during the five-year period after the audit. This is not yet a full-fledged welfare analysis of audits, but to the extent that our estimates can be interpreted as causal effects of audits, the above number may provide a crucial ingredient in a welfare analysis.

To obtain a measure of the overall societal benefits of audits, we first need to add an estimate of tax revenue recovered directly in the audit, which is not included in the behavioral response that we have estimated. Our best estimate of this is currently the average tax deficit found at audit, which is approximately 21,000 euros (see Table 2). It is noteworthy that the revenue increase due to the firms' behavioral response to audits is approximately 40% of this direct revenue effect. If one were to consider the direct effects only – as one typically would do in the absence of the type of analysis we have carried out in this paper – one would, therefore, understate the revenue effect of audits by as much as 40%.

The approximate total revenue effect per audit of 30,000 euros would then need to be compared with the relevant costs. This would require information on the costs of additional audits to the tax administration (information not available to the researcher), and private compliance costs associated with audits borne by the audited firms. The latter cost should be included only to the extent that the private costs reflect true resource costs to firms, and not transfers in the form of fines, or shifts of market share from non-compliant to compliant firms.[23]

Finally, we show above that an important consequence of audits is increased exits of non-compliant firms. Such a reduction in potentially illicit activity is likely to have societal benefits not captured in the above type of welfare calculus. Therefore, the overall impact of removing fraudulent firms from the market to the benefit of honest firms and their customers would need to be considered for an overall welfare analysis of risk-based tax audits.

7 Conclusions

We use full-population data on risk-based firm tax audits and tax returns in Finland to analyze how firms respond to operational, risk-based tax audits. To study how the development of key outcomes of the audited firms changes after the audit, we use a combination of matching and difference-in-differences methods. The matching procedure restricts our analysis to audited (treated) and non-audited (comparison) firms that have similar observable characteristics and a similar pre-audit development of key outcomes. Our approach focuses on small to medium-sized firms, which are the relevant target group when the tax administration decides on additional audits, as larger firms are typically already subject to more frequent audits and other stricter tax enforcement measures in many countries.

We find that the audited firms increase their reported profits immediately and persistently following an audit compared to the firms in the comparison group. This indicates that tax compliance increases among audited firms after the audit. Additionally, we observe increases in reported revenue, value added, labor costs, and the number of employees, suggesting that the under-reporting of revenue and labor costs (payroll taxes) are the key

[23] These types of costs would be included in a welfarist welfare calculus, as noted by Keen and Slemrod (2016). It is open to debate whether there is a case in the present context for excluding costs incurred by dishonest firms, whose actions are harmful from a societal perspective; this would be a case of a non-welfarist welfare calculus.

channels of tax non-compliance.

Our results indicate that firm responses to risk-based audits lead to an increase in tax revenue from profit, value added and payroll taxes of approximately 9000 euros on average per audited firm, which amounts to about 40% of the average unpaid tax revenue uncovered directly in the audit. Therefore, considering only the direct revenue effects of audits significantly understates their tax revenue effects.

Finally, we provide a novel analysis of the real effects of tax enforcement by examining firm survival after audits using data on bankruptcy petitions and decisions. We find a significant increase in bankruptcies for the audited non-compliant firms after the audit. These findings are consistent with negative real effects of audits on fraudulent firms. In contrast, we do not find evidence of negative real effects of audits on the business activity of compliant firms. Therefore, it appears that in addition to raising tax revenue, auditing activity has the additional societal benefit of shifting market share from non-compliant to compliant firms.

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A Appendix

Table A1: Definitions of key variables used in the analysis

Variable	Definition
Company form	Legal form of the firm (sole proprietors, partnerships, corporations).
Industry	Standard Industry Classification of 2008 at the five-digit level.
Age of the firm categories	Young: 1-3 years, Middle: 4-9 years, Old: 10 or more, or unknown. Firm age is based on years since the first observation in the tax record data.
Revenue	Annual revenue of the firm from products and services minus VAT and other direct taxes.
Variable costs	Annual euro value of the costs used as intermediate inputs in production, such as materials and services used.
Value added	Annual euro value of revenue minus variable costs.
Labor Costs	Total labor costs directly determined by wages or salaries including social security contributions.
Wage costs	Total costs on wages or salaries excluding social security contributions.
Profit	Reported firm profits. Revenue minus variable costs, labor costs and capital depreciations.
Number of employees	Reported number of employees in the firm.
Net assets	Total assets minus total liabilities.
Tax deficit	Sum of tax deficit detected from the audited tax items.
Measures undertaken due to audit	Indicator for whether any corrective measures are undertaken at the audit.
Shadow economy activities	Indicator for whether shadow economy activities were discovered at the audit according to the tax administration.
Criminal charges	Indicator for whether criminal charges are filed due to the audit.
Duration of audit (in days)	Audit duration from the reported start date to end date.
Number of years audited	Number of tax years audited at the audit.
Bankruptcy petition	Bankruptcy filed to the court by creditor(s) or firm owners.
Bankruptcy declared	Decision by the court on declared bankruptcy.
Bankruptcy lapsed	Decision by the court where the business is ended without bankruptcy proceedings due to lack of funds in the firm.
Bankruptcy public receivership	Decision by the court to continue bankruptcy proceedings even without sufficient funds.
Forced exit	Bankruptcy decisions combining declared, lapsed and public receivership bankruptcies.

Table A2: Difference-in-differences results: alternative specification of value added

	(1) Log 3 year ma of Value added	(2) Value added
Audited * After audit	0.019^{*}	17,529.1
	(0.009)	(9801.3)
Observations	1,320,936	1,352,168
Adjusted R^2	.088	.073
Outcome mean of the audited in t-1	11.2	267,072

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Table presents the difference-in-differences estimates δ using equation (3) in the main text, and the means of the outcome variables for the audited firms in the year before the audit. The table shows the estimates for the log 3 year moving average of value added and value added (in level) after an audit in comparison to the period before that. The regressions control for the common trend λ, year fixed effects, firm age category, and location in the capital city area. The regressions are estimated using CEM weights. The results show that the likelihood of reporting positive value added and revenue are smaller after the audit for the audited firms in comparison to the non-audited matched firms. There is no statistically significant difference in positive reports regarding profits or wage costs.

Table A3: Difference-in-differences results: Reporting at the extensive margin

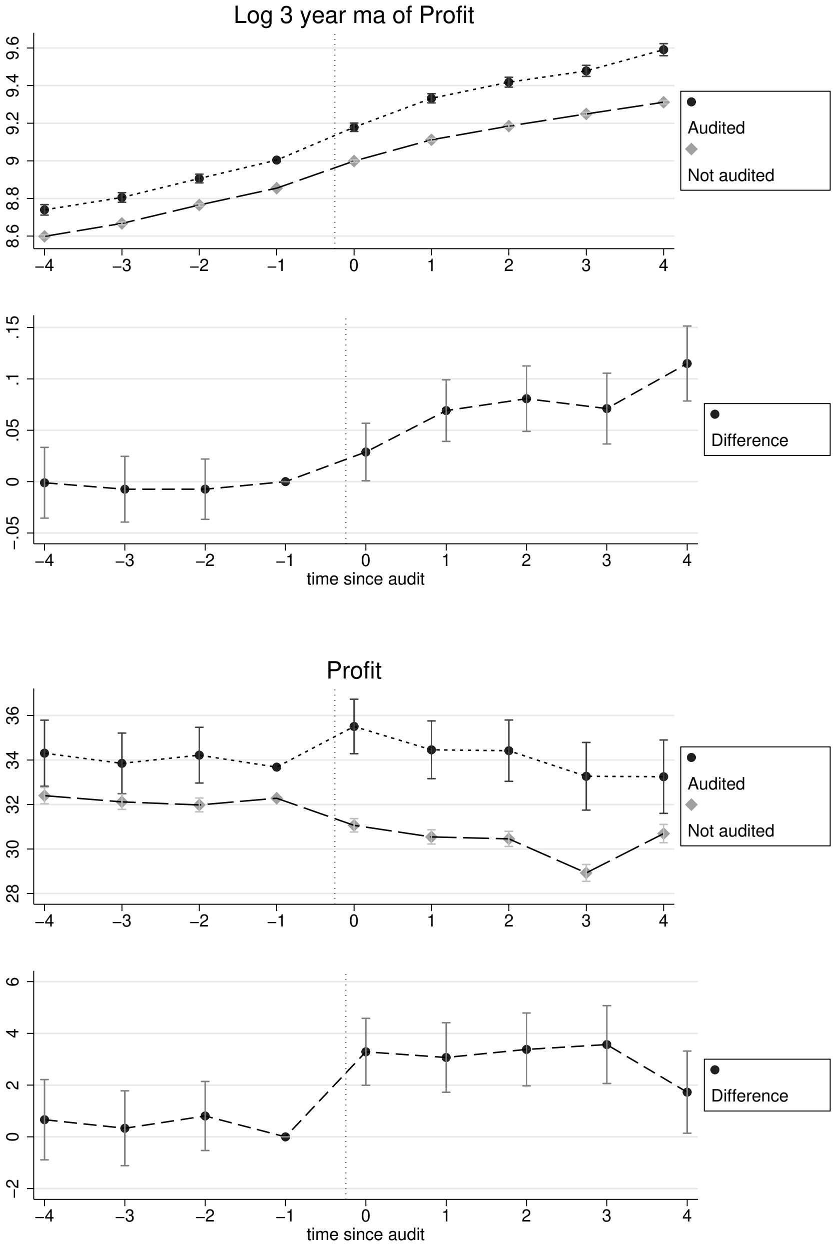
	(1) Positive profit	(2) Positive value added	(3) Positive revenue	(4) Positive wage costs
Audited * After audit	0.005	-0.008^{*}	-0.010^{**}	0.005
	(0.003)	(0.003)	(0.003)	(0.003)
Observations	2,934,601	2,934,601	2,934,601	2,934,601
Adjusted R^2	.14	.31	.33	.16
Outcome mean for the audited firms	.641	.782	.799	.56

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

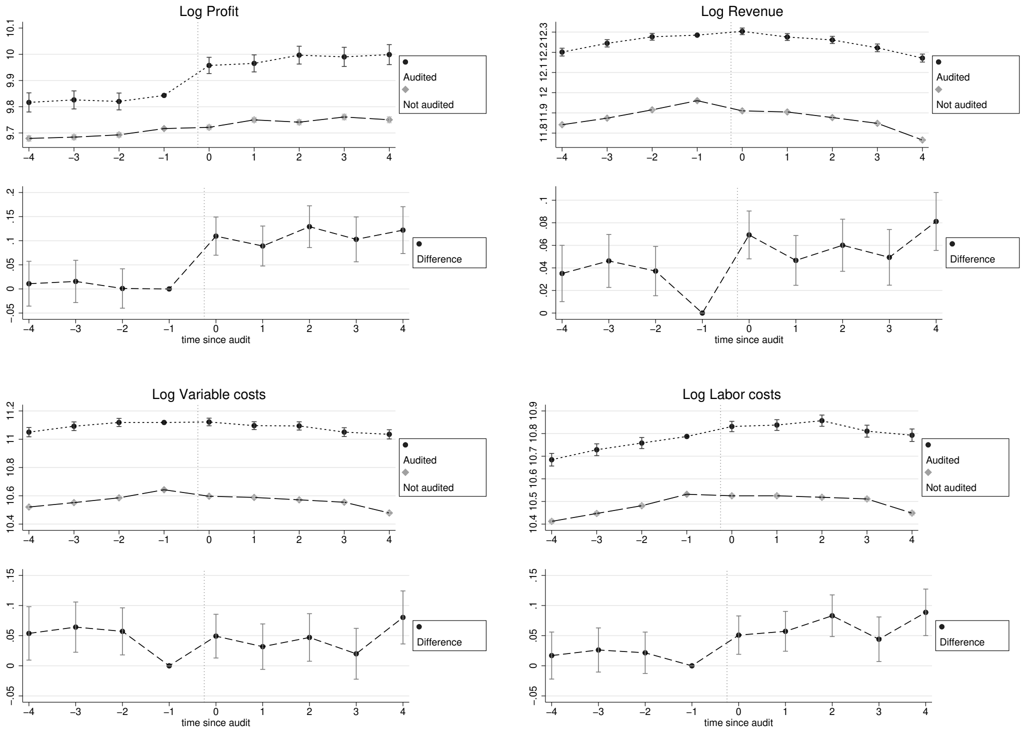
Notes: Table presents the difference-in-differences estimates δ using equation (3) in the main text, and the means of the outcome variables for the audited firms in the year before the audit. The table shows the estimates for the likelihood of reporting positive profits, value added, revenue and wage costs after an audit in comparison to the period before that. The regressions control for the common trend λ, year fixed effects, firm age category, and location in the capital city area. The regressions are estimated using CEM weights. The results show that the likelihood of reporting positive value added and revenue are smaller after the audit for the audited firms in comparison to the non-audited matched firms. There is no statistically significant difference in positive reports regarding profits or wage costs.

Figure A1: Firm responses to audits: Additional profit measures



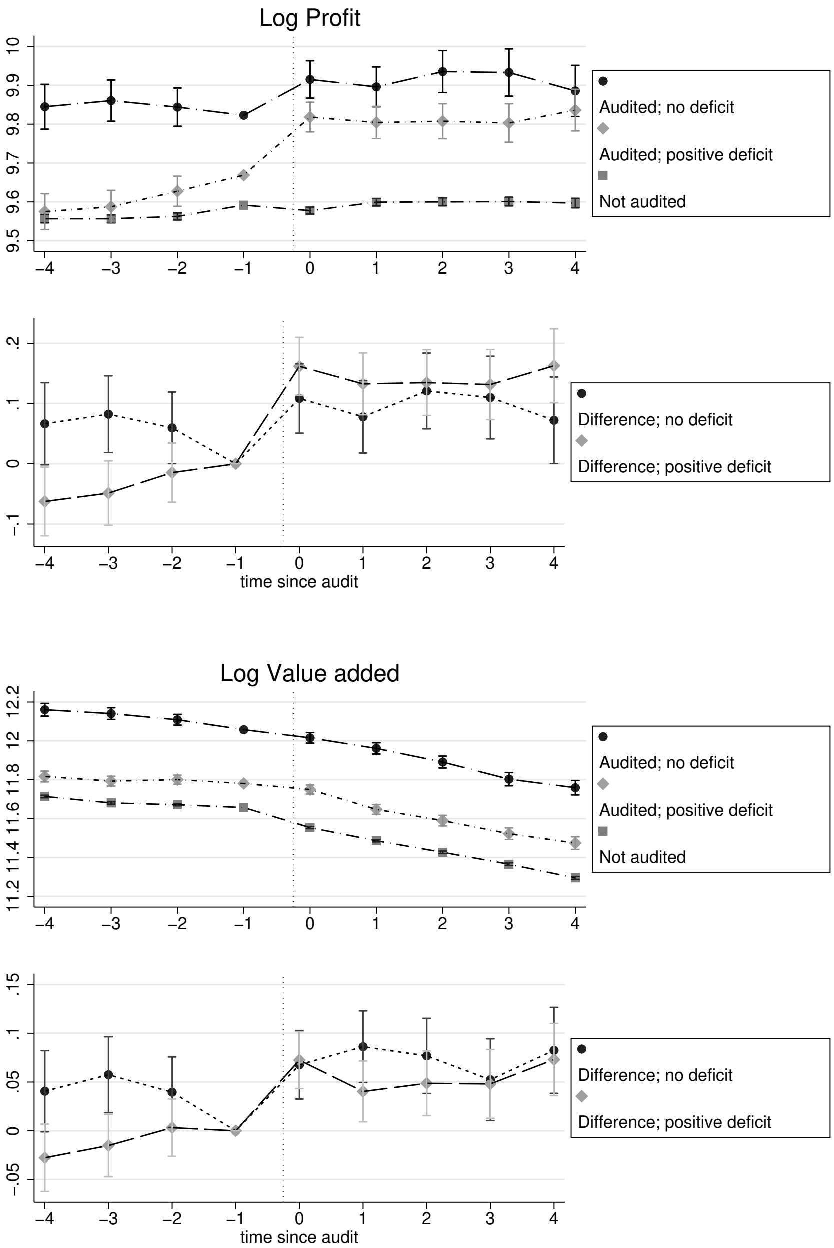
Notes: Figure plots the development of the log of the 3-year moving average of profits and profits in thousands of euros for the matched audited firms and non-audited firms relative to the audit year (denoted by zero in the figure), and the difference in these variables between the firm groups relative to one year before the (placebo) audit. The upper panels plot the regression coefficients λ_m using equation (2) in the main text including 95% confidence intervals, estimated separately for the audited and non-audited firms. The lower panel plots the regression coefficients δ_m with 95% confidence intervals. The regressions include firm and year fixed effects and are estimated using CEM weights. The figure shows that these alternative profit measures develop similarly in these groups around the audits as in the baseline Figure 3 in the main text.

Figure A2: Firm responses to audits using balanced panel data



Notes: Figure plots the development of log profits, revenue, variable costs and labor costs for the matched audited firms and non-audited firms relative to the audit year (denoted by zero in the figure) using balanced panel data of firms that we observe in the register data throughout the observation period (from 4 years before to 5 years after the (placebo) audit). The upper panels plot the regression coefficients λ_m using equation (2) in the main text including 95% confidence intervals, estimated separately for the audited and non-audited firms. The lower panel plots the regression coefficients δ_m with 95% confidence intervals. The regressions include firm and year fixed effects and are estimated using CEM weights. The figure shows that the development of the key outcomes around the audits is similar when using this sample and when using unbalanced panel data in the main text.

Figure A3: Firm responses to audits by detected tax deficit: Reported profits and value added



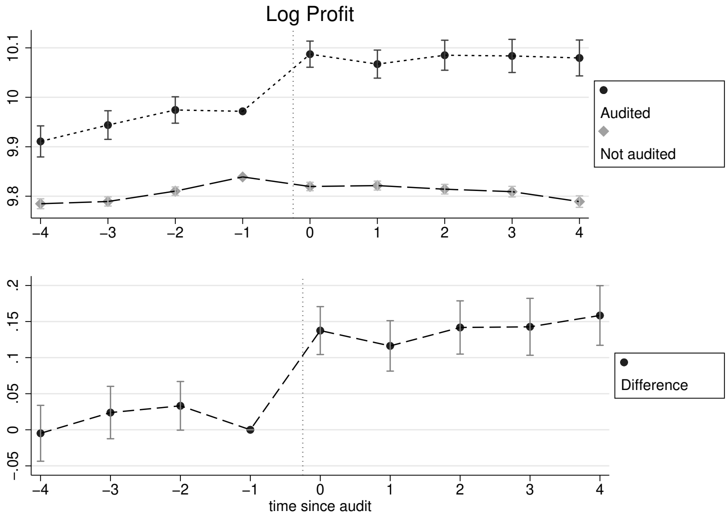
Notes: Figure plots the development of log profits and value added for the matched audited firms and non-audited firms relative to the audit year (denoted by zero in the figure) separately for those audited firms with and without a positive tax deficit detected in the audit. The upper panels plot the regression coefficients λ_m using equation (2) in the main text including 95% confidence intervals, estimated separately for the audited and non-audited firms. The lower panel plots the regression coefficients δ_m with 95% confidence intervals. The regressions include firm and year fixed effects and are estimated using CEM weights.

B Main results using alternative matching specifications

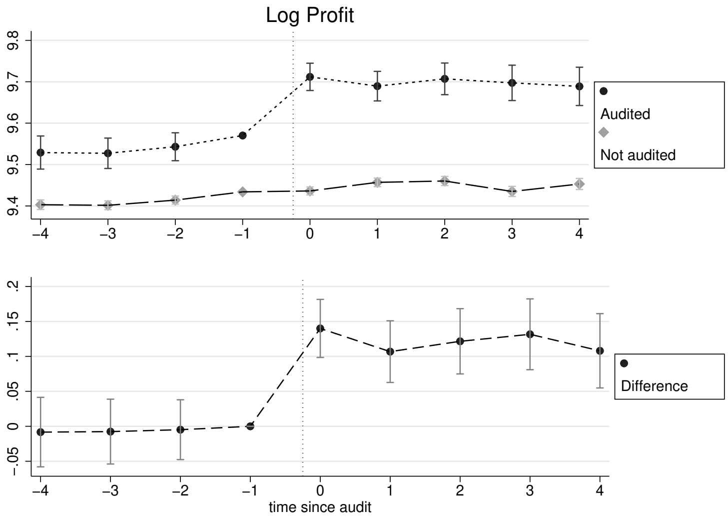
We have conducted several different matching specifications to asses to robustness of our results. The criteria for choosing the preferred specification are explained in the main text. The differences in firm development after an audit are qualitatively similar between the different matching specifications. However, including the dummies for the 95th and 99th percentiles of revenue are crucial in improving the similarity in the trends of revenue between the treatment (audited firms) and comparison (not audited firms) groups before the (placebo) audit. This can be the result of the largest audited firms, which are over-represented in the group of audited firms, driving the difference in the development before the audit. Including more variables in the matching procedure, such as location or other size or trend measures, does not significantly improve the pre-trends or the balance between the groups in the year before the (placebo) audit, but excludes many observations from the matched sample.

Figure B1 plots the development of reported profits for the treatment and comparison groups using two alternative matching specifications: one with less matching variables in panel (a) and another with more variables added in the matching procedure in panel (b). The matching variables used in each specification are listed in the note of the figure. The matching procedure with less variables provides a slightly less similar trends in profits between the groups before the audit, but still indicates a clear jump in profits right after the audit for the treatment group. Having more variables included in the matching procedure provides similar trends in profits as the baseline specification we use in the main text, but excludes about 4,000 more firms from the treatment group.

Figure B1: Development of reported profits with alternative matching specifications



(a) Less matching variables



(b) More matching variables

Notes: Figure plots the development of log profits for the matched audited firms and non-audited firms relative to the audit year (denoted by zero in the figure), and the difference in profits between the firm groups relative to one year before the (placebo) audit. The upper part of each panel plots the regression coefficients λ_m using equation (2) including 95% confidence intervals estimated separately for the audited and non-audited firms. The lower part plots the regression coefficients δ_m with 95% confidence intervals. The regressions include firm and year fixed effects and are estimated using CEM weights. The matching variables in panel (a) include revenue, profits, net assets, positive revenue, revenue 95th percentile, revenue 99th percentile, age category, filed tax return, company form and year. The matching variables in panel (b) include revenue, change in revenue, labor costs, change in labor costs, profits, net assets, positive revenue, revenue 95th percentile, revenue 99th percentile, age category, filed tax return, company form, industry and year.