# Tax Enforcement Spillovers – Evidence from South Africa*

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**Abstract**

The purpose of this paper is to test for tax enforcement spillovers within economic and spatial networks. Using the population of corporate tax returns for the years 2009 to 2015, we can show that tax audits exert a positive and significant effect on the tax liability of non-targeted neighboring firms. Quantitatively, the results suggest that the audit of a close geographic neighbor increases corporate tax reporting by 0.7 percent. While the observed spillover effect decline in distance to the audited entity and are short run in nature, the implied aggregate revenue gains are non-negligible. Additional analyses show that the effect is driven by audit cases, where audited firms do *not* experience an upward revision in their tax owed in the course of the audit. This suggests that the observed effect is rooted in communication among taxpayers and is not driven by audit-related cost shocks of competitors or business partners.

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# 1 Introduction

Tax evasion is perceived to be a prevalent problem in many countries, in particular in the developing world (Besley and Persson, 2013, 2014). Recent years have seen growing academic and policy interest in designing optimal tax administration structures to combat evasion behavior (Slemrod, 2018). Next to information structures (like third-party reporting and verifiable paper trails), particular attention has been paid to taxpayer audits and their role in enforcing tax obligations. The design of effective audit systems requires explicit knowledge on the costs and benefits of tax audits (Keen and Slemrod, 2016). The latter are not straightforward to quantify. Alongside identifying evaded income and collecting unpaid taxes, [1] they may also affect future income reporting and excert spillover effects on other tax bases. Kleven et al. (2011), Advani et al. (2019), and DeBacker et al. (2018) show, for example, that audits increase the post-audit tax reporting of audited taxpayers. Birskyte (2013) and López-Luzuriaga and Scartascini (2019) find evidence of spillover effects across tax bases.

In this paper, we present complementary evidence suggesting that audits impact the tax reporting of *non-audited* taxpayers. While studies on the topic are scarce, theory suggests that such a link may exist: when firms of a given economic or spatial network get audited, reporting behavior of non-audited firms in the same network might, for example, change because taxpayers update on their own expected audit and fine propensity. In addition, non-audited firms might respond out of social norm considerations or because neighboring audits might induce audit-related cost shocks to competitors and business partners.

The aim of this paper is to empirically test for enforcement spillovers of tax audits. Testing ground are business taxpayers in South Africa. The analysis draws on the population of corporate tax returns for the tax years 2009 to 2015 which is provided by the South African Revenue Service (SARS). This data is linked to information on all business taxpayer audits conducted by SARS between 2008 and 2015. This comprehensive data set allows us to examine various taxpayer networks through which enforcement spillovers might work. Firstly, we look at spillover effects that can occur due to the industry affiliation of the firm. This includes the network of companies working in the same industry as well as the network of input- or output-related

[1] Related revenue effects can be directly calculated from authority accounts and are commonly used to justify audit-related expenses in authorities' annual reports. Many tax administrations, moreover, use audit case selection systems that are guided by the aim of optimizing these immediate revenue collections (e.g., Bloomquist, 2013).

companies. Spillover effects might, for example, stem from audit-related cost shocks of economically-connected firms (which might also include beneficial effects if a close competitor gets audited), information distributed by industry associations or communications flows between partners along the value chain. Secondly, using data on all South African tax practitioners allows us to study network effects that arise through tax preparer's networks. Tax professionals might serve as an informational hub and distribute evasion-relevant information to their clients. In addition, experiencing an audit of one of their clients could induce behavioral responses related to the preparation of tax returns more generally. We finally make use of precise bilateral distance data between audited and non-targeted firms to empirically assess the propensity for communicative interactions between firms that are spatially connected (see, e.g., Jaffe et al., 1993; Combes et al., 2005, 2011; Drago et al., 2020).[2]

As a first central result of the paper, we find that geographic proximity to audited taxpayers is the main driver for enforcement spillovers. The main analysis of the paper thus focuses on analyzing *spatial audit spillovers*. Methodologically, we estimate fixed effects models that compare changes in the tax reporting of firms with and without neighbors who are subject to business tax audits at a given point in time. The main threat to this identification strategy is that we might pick up effects related to the strategic assignment of audit resources across regions, conditional on regional tax reporting paths. To address this issue, we restrict the sample to firms that did not get audited themselves during our sample period. Neighbors are, moreover, defined based on precise bilateral distance data and all estimation models include a full set of municipality-year and sector-year fixed effects, respectively (thus absorbing common shocks to 250 municipalities). Put differently, the estimation strategy compares the tax reporting of firms in the same municipality whose neighboring businesses were and were not subject to an audit by SARS at a given point in time. In robustness checks, we estimate models with suburb-fixed effects (absorbing common shocks to about 10,000 suburbs) or municipality-sector-year-fixed effects, which additionally allow for SARS strategies that target specific industries in specific regions in a given tax year. All estimation models, furthermore, control for the average income of firms' direct corporate neighbors and are thus even able to absorb confounding effects related to locally refined tax reporting shocks.

We find clear evidence of positive audit spillovers on non-audited taxpayers in close

[2]Distance impedes firm trade (Combes et al., 2005) as well as an exchange of information and related knowledge spillovers (see, e.g., Jaffe et al., 1993). Evidence, moreover, points to the importance of local communication networks (see, e.g., Drago et al., 2020).

geographic proximity. If one additional fellow business in a 100-meter distance circle is audited, corporate tax liabilities are estimated to increase by around 0.7 percent, on average. This positive effect steeply declines in geographic distance. To test for the robustness of our baseline results, we conduct a battery of additional regressions, where we use different sets of fixed effects and alternative transformations of the outcome variables. The results even hold if we apply randomization inference to account for spatially correlated error terms in the most flexible way.

We also find consistent and robust results when restricting the sample to firms that experience only one single neighbor audit during the sample period. For these firms treated at the extensive margin, the spillover effect becomes eight times larger than the baseline effect. If such a firm experiences a neighbor audit, its tax liability increases by 5.8 percent. We also study effect dynamics. Compared to previous studies examining dynamic effects of own tax audits (see Advani et al., 2019; Beer et al., 2019), we find that enforcement spillover effects are rather short-run in nature and level off two years after the neighbor audit took place. In this regard, we also conduct an event-study-type analysis for the subset of firms that experiences exactly one neighboring tax audit. Given the data frame, we can look at the effect three years prior and two years after the treatment. Again, our results suggest that the spillover effects last for around two years and then starts declining in the third year after the neighbor audit.

The paper proceeds by dissecting potential underlying mechanisms that may establish the results. Specifically, we argue that the findings may either be rooted in taxpayer-to-taxpayer communication or may reflect behavioral adjustment in the wake of audit-related cost-shocks to economically-related firms. If audited firms tell others about their audit experience, fellow taxpayers may update on their own audit/fine propensity (which could go in either direction) or may change their reporting behavior out of social norm considerations. If taxpayers are connected economically, behavioral incentives may, in addition, change due to audit-related cost shocks or because authorities obtain evasion-relevant information on both the audited taxpayers as well as their related parties. To learn about the relative importance of these mechanisms, we compare their theoretical predictions with the observed empirical result pattern.

Our mechanism analyses yield clear evidence for effect heterogeneity. Most interestingly, spatial audit spillovers are driven by audits in which taxpayers are *not* subject to an upward revision of their tax liability. If business interactions were a relevant transmission channel, firms would be affected by cost shocks to audited business partners or competitors. As these cost shocks are plausibly stronger, or even restricted, to the subset of audited firms that experience an upward revision of their tax payments in the course of the audit, the theoretical predictions are inconsistent with the observed empirical findings.[3] The result pattern, in turn, matches with spillover effects established by taxpayer-to-taxpayer communication, coupled with social norm considerations. Specifically, audited taxpayers may tell others in their local networks about their audit experience but may do so selectively. To avoid being exposed as a tax evader, they may only report about audits that did not result in an upward revision of their business tax payments. Neighbor responses would be thus be limited to cases without an upward revision. Alternatively (or complementary), the result pattern is consistent with scenarios where the communication about audit experiences is non-selective but neighboring taxpayers' reporting response is asymmetric in the sense that their tax reporting increases when audited firms are observed to be tax-compliant, but does not decrease when they are observed to be non-compliant.

The identified audit spillovers have non-negligible revenue implications. Back-of-the-envelope calculations suggest that, within our sample period, audit spillovers increased aggregate corporate tax revenue by around 3.7 billion South African Rand (or 6.5% of the direct audit-related revenue yield). The results offer insights on the design of optimal tax audit strategies. Specifically, tax authorities worldwide rely on audit case selection systems which target tax returns based on risk indicators that correlate with taxpayers' expected evasion levels (Bloomquist, 2013). This strategy is designed to maximize the direct revenue collection through the audit. We show that audits also induce indirect revenue effects by raising fellow taxpayers' tax reporting. This implies that including the size of taxpayers' local communication networks and taxpayers' position therein as an additional criterion in case selection systems may raise audit-related revenue.

Our paper contributes to a flourishing literature on the link between tax enforcement activities and taxpayer behavior. Theory suggests that taxpayers become more compliant when audit frequencies and fine rates increase (see Allingham and Sandmo (1972) for their seminal work). In recent years, several papers have empirically tested for this presumption in both laboratory and field experiments (see, e.g., Mascagni (2018) for a survey). The latter studies commonly investigate income reporting after randomly selected taxpayers were treated with tax authority mailings that conveyed enforcement information or explicit audit threats. The results of these studies unanimously show that increasing the rates or salience of penalties and audits significantly raises indi-

[3] The tax cost of audited taxpayers may increase because of upward revisions of tax payments owed and fines levied in the course of the audit. On top of that, tax cost may rise as audited taxpayers increase their future tax reporting (see Kleven et al., 2011; Advani et al., 2019; DeBacker et al., 2018).

viduals' tax reporting relative to taxpayers in control groups (see, e.g., Drago et al. (2001), Kleven et al. (2011), Fellner et al. (2013) and Dwenger et al. (2016) as well as Hallsworth (2014) and Slemrod (2018) for surveys). Information on tax enforcement activities may, however, not only be conveyed through official communication by the tax authorities but may also be disseminated through unofficial communication among taxpayers. Evidence on the latter channel is scarce, however. Exceptions are the studies of Rincke and Traxler (2011), Boning et al. (2018) and Drago et al. (2020). Rincke and Traxler (2011) assess enforcement spillovers in the context of TV license fees in Austria and report that non-registered households' registration rates strongly respond to an increased enforcement of these fees in their vicinity. In a complementary paper, Drago et al. (2020) use the same testing ground to show that the content of mailings sent to potential evaders of TV license fees strongly impacts on the compliance of untreated households in the same local networks. Closest to our research is the study by Boning et al. (2018) which examines network effects of tax enforcement on withheld income and payroll tax remittance using a large-scale field experiment conducted by the US Internal Revenue Service. While they do not find spillover effects based on geographical networks (i.e. based on ZIP-code information), their results suggest that sharing an individual tax preparer significantly increases the tax remitted by two percent. They can also show that information flows within parent-subsidiary relationships such that subsidiaries of audited firms remit less taxes.[4]

Our paper contricutes to the literature in several ways. First, we assess the enforcement of a comprehensive nationwide tax that generates significant aggregate revenues rather than the enforcement of a fine. Second, our testing grounds are corporations rather than individuals. So far, there exist only few studies examining the effectiveness of enforcement measures for corporations and the economic literature on corporate tax audits is still scarce (e.g., Agostini and Martínez, 2014; Drago et al., 2020; Ariel, 2012; Bergman and Nevarez, 2006; Bérgolo et al., 2019).[5] Third, we study enforcement spillovers in an emerging economy context where fiscal capacities might be generally lower. The latter features link our paper to a small, but growing tax literaure, see, for example, Carrillo et al. (2017), Castro and Scartascini (2015), and Pomeranz (2015). While Pomeranz (2015), in line with our study, finds evidence of positive enforcement spillovers, her findings are suggested to be rooted in information generated through

[4]This result stems from 49 out of 12,172 forms in the treatment and control group which have at least one subsidiary.

[5]Please note that there is a strand of literature in business and accounting that examine the role of audits, see, for example, Allred et al. (2017) andBedi (2016).

"paper trails" established by the VAT system.[6] To the best of our knowledge, there is no study on the spillover effects of corporate taxes in less developed countries.

The paper proceeds as follows. Section 2 provides information on the institutional setting and the data used in the empirical analysis. Section 3 lays down our identification strategy to estimate enforcement spillovers in the context of tax audits. The network results are presented in Section 4. In Section 5, we examine spatial networks in more detail, while Section 6 discusses different transmission channels through which the estimated spillovers might work. Section 7 concludes.

# 2 Institutional Background and Data

## 2.1 Institutional Background

The World Bank categorizes the Republic of South Africa as an upper-middle-income economy (The World Bank, 2017) with a gross domestic product (GDP) per capita of US $5,744 in 2015 (which is the last year in our data set). The country is also characterized by extreme income and wealth inequality. South Africa's human development index value for 2015 was 0.66 putting the country on the 119th position out of 188 countries and territories (United Nations Development Programme, 2016). However, compared to other developing economies, the overall tax capacity is high: In 2015, the country's tax-to-GDP ratio amounted to 29 percent and thus well exceeded the tax-to-GDP ratio of other countries on the African continent (which amounted to 19.1 percent in 2015, see OECD Statistics, 2018). Nevertheless, the country's tax capacity still fell short of developed-country levels (the average tax-to-GDP-ratio in the OECD amounted to 34 percent in 2015). Similar to other less developed economies, corporate taxes are an important revenue source in South Africa, as indicated by a high corporate-tax-to-GDP ratio of 4.7 percent (OECD average: 2.7 percent), see Lediga et al. (2019) for further details. Income taxes are levied by the national government of South Africa under the Income Tax Act 58. During our sample period, businesses in South Africa were taxed at a proportional tax rate of 28 percent. The South African tax system further knows two types of small and medium enterprises, which benefit from special tax treatment. "Micro Businesses" face special tax dispensation in the

[6]See Alm et al. (2009) for a laboratory study, which assesses the effect of unofficial taxpayer communication on individuals' tax reporting. Furthermore, note that a more loosely related literature, provides evidence of spillovers of tax evasion behavior in social networks (see, e.g., Paetzold and Winner, 2016; Alm et al., 2017; Frimmel et al., 2018; Alstadsæter et al., 2019).

form of a turnover tax while "Small Business Corporations" benefit from progressive tax rates.[7]

SARS conducts audits on the tax affairs of selected taxpayers to verify compliance levels and deter non-compliance. The selection of companies for audits is risk based, and informed by historical analysis and identified risks.[8] An important approach to select companies for a tax audit is equitable coverage of companies in respect of sectors, tax types and geographical area. In addition, the risk profiling of companies is carried out using a range of financial and tax specific indicators, taxpayer's activities, risks pertinent to the industry, or cases of interest identified through media and other reports. While the economic analysis of non-random audits on the behavior of audited taxpayers is usually challenging because of this (unknown) selection process, we want to stress that as we do not consider audited taxpayers themselves, this selection problem is less prevalent in our spillover analysis. The identification strategy of our main analysis of spatial enforcement spillovers will only be threatened if the assignment of audit resources is based on very narrow geographical regions (conditional on regional tax reporting paths), which is very unlikely to be the case as discussed in Section 3.

Once a company is selected for an audit, the appointed auditor will issue a formal notification letter of audit to the taxpayer. The date on the letter is the issue date of the audit and the company has 21 days from that date to respond with required supporting documents. The complexity and type of the audit determines the standard turnaround time. On average, audits take approximately 30 working days to finalize once all supporting documents have been received. Depending on the complexity of the case and co-operation by the taxpayer, an audit is finalized within 30 working days or takes more than 12 months.

[7]The turnover tax has a graduated tax rate structure with a maximum marginal rate of 6 percent. To be eligible for this turnover tax, gross income for the year of assessment must not exceed an amount of R1 million and total assets must not exceed R5 million. To qualify as a SBC, a company must i) not have elected to be classified as a Micro Business for the year of tax assessment and ii) meet specific criteria. These include, among others, a gross income not exceeding R20 million (R14 million prior to the 2013 tax year) and limited shareholding.

[8]Prior to 2011 SARS audit and selection of cases was mainly done through the use of IT systems such as the SESAM system for computer auditing, South African Audit Risk Analysis Programme (SARAP) IT systems and through a systematic and strategically dependent way of selecting companies to audit. See SARS' Annual Reports for more information on SARS audit activities and direct revenue collections from the audits.

## 2.2 Data and Descriptive Statistics

The empirical analysis draws on the population of business tax returns in South Africa provided by SARS for the tax years 2009 to 2015. The data comprises around 3.73 million tax returns from 953,353 firms. To isolate audit spillovers on non-audited firms and to avoid results that are driven by "own-audit-effects," we have dropped all firms that have been audited themselves during the sample period. For each tax return, we observe the exact end of the financial year covered by the return (which may vary across firms but ends in February for most entities) as well as the date on which the return was processed by SARS (which usually corresponds to the submission date of the tax return).[9]

Unbalanced in its nature, the data set covers most items that are reported in the official tax return statement. Since we are interested in determining the effect of neighboring audits on firm behavior, our main outcomes are the taxpayers' reported taxable income and the resulting tax liability (both reported in South African Rand). The data set further includes information on sales, the cost of sales, current and non-current assets and a bunch of expense-related variables like financial depreciation, traveling expenses, repair and maintenance or donations. While our data is almost perfectly filled for the two outcome variables, missing data is more of an issue for the latter set of variables, including sales, cost of sales and deductions. This is, however, less problematic as the main focus of the paper is to determine whether taxpayers increase their taxable income and their tax liability if a neighbor went through an audit process.

Exploiting this rich firm-level data set, we are able to examine network effects of tax enforcement. Specifically, in a first step, we study the tax reporting of non-targeted firms that are linked to audited taxpayers by i) the same industry affiliation, ii) input-/output-relations, iii) geographic proximity, and iv) using the same tax professional. To determine input-/output relations, we use the official Input-output table for South Africa for the year 2013 which is provided by Statistics South Africa. For the spatial network, we link the tax return data to information on bilateral distances between entities. This allows us to identify taxpayers that are geographic neighbors which is used to determine whether a taxpayer has been treated, i.e. whether he has lived close to a neighbor that experienced a tax audit.

For each tax return $r$ submitted by firm $i$ at time $t$, we calculate the number of

[9] While taxpayers are obliged by law to submit the tax return for a given tax year within 12 months from the financial year end, we observe cases where taxpayers submit their returns late.

network audits $NA_{r,i,t}$:

$$NA_{r,i,t} = \sum_{j=1}^{J} \mathbb{1}(N_{ij}) \cdot D_{j,(t-n)} \tag{1}$$

where $\mathbb{1}(N_{ij})$ is an indicator function that equals one if firm $i$ and $j$ are in the same network, i.e. operate in the same industry, are input- or output-related[10], or hire the same tax professional. For the distance-based network, we consider the euclidean distance between firm $i$ and $j$ to define spatial networks. We start with a very narrow definition and declare firms as being neighbors if they are located within a distance band of 0-100 meters. To see the geographical extent of enforcement spillovers, we then add regressors for larger distance bands of 100–500 meters and 500–1000 meters (see Section 5.1).

$D_{j,(t-n)}$ equals one if firm $j$ was subject to an audit $n$ years prior to $t$ at maximum. The information on the completion date of an audit and the processing date of tax returns at SARS allows us to precisely identify those tax returns that were submitted $n$ years after the network audit was closed. To determine $D_{j(t-n)}$, we use all business tax audits conducted by SARS between 2008 and 2015, i.e. starting one year earlier than the begin of the tax return data. In total, we observe 28,588 business tax audits. After dropping outliers in the highest and lowest percentile of the distribution, the average adjustment per audit was 386,476 South African Rand (around 19,000 Euro); 41.5% of the audits were subject to an upward adjustment.[11] Our data comprises information on the audited firm, the tax return audited, the audit results (adjustments in the tax owed) and the closure date of the audit. While SARS conducts both "field audits," where tax officers visit the audited company at their premises to collect information, as well as "desk audits," where tax officers check submitted tax returns and complementary material without visits, our data does not allow us to distinguish between these cases. Our analysis, however, does not include so-called small-E audits, which automatically sent requests for adjustments of submitted tax returns or supplementary material to taxpayers, which submitted tax returns that the audit system flagged as inconsistent.[12]

[10]To classify firms as input- or output-related, we merged our industry classifications to the SIC code information in the official Input-output tables provided by Statistics South Africa. Based on input (output) coefficients, we label firms input (output) related if they operate in the most three important input (output) industries. This is an arbitrary choice, but results do not change if we, for example, use the seven on ten most important industries.

[11]Around 2.7% of the audits resulted in a downward revision.

[12]Such audits do not involve the trained auditors. Potentially submitted supplementary material is checked by administrative staff only.

In the baseline analysis, we set $n = 1$ such that we count all network audits that took place within a one-year time span before the submission of the considered tax return.

In a second step, we focus on analyzing spatial networks and test for potential mechanisms. We refine the definition of Equation (1) and only sum up *audited geographic neighbors* that feature certain characteristics. We start by counting neighbor audits with an audit result of zero. While this does not necessarily imply that these neighboring firms have been fully compliant with the tax law (as this would hinge on the assumption that the tax authority detects all misreported or hidden income during an audit), it allows for a good proxy for whether firms were compliant. We, moreover, reanalyze the baseline networks conditional on the geographical proximity. Again, we analyze whether the audited neighboring firm $j$ operates in the same industry as firm $i$. Using input-output tables for South Africa, we also determine whether the neighboring firm is in the direct value chain of the considered firm and finally, we count the number of neighboring firms with the same tax practitioner.

Table 1 reports descriptive statistics for the main variables of interest divided into dependent variables, calculated audit variables based on Equation (1) and control variables. Around 25 percent of all tax returns report a positive taxable income, which—given the existence of a tax-free allowance—results in a positive tax liability for only 21 percent of all returns. The average tax liability per firm amounts to 62,000 South African Rand (3122 EUR). However, there is a lot of variability as can be seen from the high standard deviation and the maximum value for both taxable income and tax liability.

The average number of audits within the industry network that took place one year before the submission of the respective tax return amounts to 462 audits. For the input- and output-related network, this number increases to around 1500. The number of audited firms that use the same tax practitioner is relatively small with 0.4 but seems reasonable as only 32 percent of all firms in South Africa hire a tax practitioner in general. Finally, for the geographic network, each firm experiences 0.70 neighboring audits within the narrow distance band of 100 meters. This number increases to 1.21 and 2.49 neighboring audits when looking at wider distance bands of 100-500 and 500-1000 meters, respectively. Having a deeper look at audit heterogeneity, 60 percent of neighbor audits within 100 meters have an audit result of zero, which we label "compliant" audits. Around 0.11 neighbor audits happen in the same industry, while 0.25 (0.26) audits are input (output) related. The number of neighboring audited firms within 100 meters that share a common tax practitioner is very small with only 0.03, on average.

# 3 Identification Strategy

The purpose of this paper is to test for spillover effects of taxpayer audits on the tax liabilities and income reporting of non-targeted firms. The observational unit is the tax return $r$ for tax year $\ell$, submitted by firm $i$ at time $t$. The estimation model takes on the following form

$$y_{r,i,m,k,t,\ell} = \alpha_1 + \alpha_2 NA_{r,i,t} + \alpha_3' X_{i,t,\ell} + \mu_i + \rho_{m,\ell} + \delta_{k,\ell} + \phi_t + u_{r,i,m,k,t,\ell}, \tag{2}$$

where $y_{r,i,m,k,t,\ell}$ depicts the tax liability (taxable income) for tax return $r$ submitted by firm $i$ for tax year $\ell$ that operates in industry $k$ and is located in municipality $m$. As this outcome features a lot of zeros, we apply the inverse hyperbolic sine (IHS) transformation $((ln(y + sqrt(y^2 + 1))$ which is similar to the standard (log+1)-transformation used in the previous literature to estimate proportional effects (see, e.g., Burbidge et al., 1988; Boning et al., 2018). The transformation ensures that we can keep observations with zero tax liability in the estimation sample. However, we will also present linear models using the level value of tax liabilities as well as a binary outcome indicating positive tax liabilities as dependent variables to see whether our results still hold. In robustness checks, we also run Poisson pseudo maximum likelihood (PPML) regressions.

The regressor of main interest is $NA_{r,i,t}$, which captures the number of network firms that experienced an audit in a given time frame before the submission of the considered tax return. In the baseline analysis, the definition of $NA_{r,i,t}$ accounts for audits within a one year-span before the submission date of the considered return $r$.[13] $X$ is a vector of control variables including total assets (in log), the number of neighboring firms and the average reported taxable income of these firms (in deciles). The model further includes firm-fixed effects $\mu_i$, municipality-year fixed effects $\rho_{m,\ell}$, sector-year fixed effects $\delta_{k,\ell}$ as well as submission year-fixed effects $\phi_t$.

The coefficient of main interest is $\alpha_2$ capturing the effect of an additional network audit on the tax liability (taxable income) reported. The main empirical identification concern relates to the non-random assignment of audit cases by SARS. As described above, SARS, in line with common tax authority practices, selects audit cases on a risk basis, implying that tax returns which feature characteristics that point to expected

[13] The construction relies on the date when return $r$ was processed by SARS (which is commonly within a couple of days of the actual submission date of the return) and on the date when the audit was closed. "Within a year-span" means that tax audits of neighbors are accounted for in the calculation of $NA_{r,i,t}$ if the gap between the two dates is less than 365 days.

irregularities and evaded income are audited at higher rates. Especially, as the focus will be on geographic networks, if risk-based targeting occurred on a regional, our identification strategy might be threatened. Consider, for example, the case where firms in a given region are subject to a negative income shock that lowers their reported income and tax payments below that of comparable firms in other regions. With risk-based targeting, these firms might be audited at increased rates and the coefficient $\alpha_2$ might capture both the audit effect plus region-specific tax reporting trends (in post-audit periods, tax payments might, due to mean reversal, grow at systematically higher rates than in other regions). Our estimation strategy accounts for this concern: We define neighboring firms based on bilateral distances between audited and non-audited entities. This allows us to augment the estimation model by region-year fixed effects, which absorb potentially confounding effects related to common regional tax trends and strategic audit assignments in space. Specifically, we run models with municipality-year and, in robustness analyses, suburb-year fixed effects, respectively, that account for common shocks to the 250 South Africa municipalities and 10,000 suburbs, respectively. Hence, we compare changes in the tax reporting of firms located in the same municipality whose direct neighbors were subject to tax audits with firms that did not experience such neighbor audits. To further refine the analysis, we also estimate models with municipality-industry-year fixed effects, which allow for the systematic targeting of two-digit industries in given municipalities by SARS.[14] All models, furthermore, include control variables for the number of direct neighbors within 100 meters and 100–1000 meters distance as well as the average taxable income of these neighbors for submission year $t$. The latter regressors are suitable for absorbing potential confounding effects related to potential SARS targeting of firms at very refined geographic levels, conditional on their tax reporting.

In additional analyses examining the spatial network, we refine the estimation model and include regressors for the number of audited neighbors in wider distance bands around the considered taxpayer of 100–500 meters and 500–1000 meters. Additionally, we assess dynamic effects and test for two-year and three-year-lagged responses to neighbor audits.[15] The estimation model then reads as:

$$y_{r,i,m,k,t,\ell} = \beta_1 + \sum_n \sum_q \gamma_q^n NA^q_{r,i,t-n} + \beta_3' X_{i,t} + \mu_i + \rho_{m,\ell} + \delta_{k,\ell} + \phi_t + \epsilon_{r,i,m,k,t,\ell}, \quad (3)$$

where $q$ indicates the distance bands, with $q \in \{0 - 100m, 100 - 500m, 500 - 1000m\}$

[14] Stated differently, the models compare changes in the tax reporting of firms in the same industry and municipality, whose neighbors are and are not subject to audits by SARS at a given point in time.

[15] Precisely, the two-year-lag (three-year-lag) regressor accounts for audit cases that were closed 366 to 730 days (731 to 1095 days) from the processing date of the considered tax return.

and $n$ indicates the year-lag between the tax return processing date and the closure of the audit, with $n \in \{1, 2, 3\}$.

Finally, the calculation of standard errors accounts for clustering at the sector-year level in all baseline models.[16] In robustness checks, we test for potential changes in the significance of results when we alter assumptions on the correlation structure of the errors.

# 4 Network Results

Figure 1 in combination with Table 2 depict the baseline network results based on Equation (2), where the dependent variable is the IHS-transformed tax liability of the firm. Standard errors account for clustering at the sector-year level. In all baseline models, we winsorize the data at the top 99.9th percentile to account for outliers. For each network, Figure 1 shows the treatment effect $\alpha_2$ as well as 95 percent confidence bands. For the industry and input/output-related networks, we find a clear spillover effect of zero suggesting that—independent of distance—audits within this network do not exert a spillover effect on untreated firms. The confidence bands for the first three estimate are too small to be distinguishable from the point estimate implying that we can estimate a very precise zero effect, see also Specification (1) to (3) in Table 2. However, as can be easily inferred from the graph, there is a positive and significant spillover effect of tax audits on neighboring firms within a 100 meter radius. Stated differently, the nearby neighbor audits significantly increase the tax liabilities of firms. Quantitatively, Specification (4) of Table 2 suggests that one additional neighbor audit raises the firms' reported tax payments by around 0.8 percent. Sharing a common tax preparer is not associated with an increased tax liability if a network member gets audited. The point estimate is negative, but not significantly different from zero.

These results are in line with the well-established literature which shows that geographic distance shapes the intensity of communicative and business interactions (see, e.g., Jaffe et al., 1993; Combes et al., 2005; Drago et al., 2020). Based on the results of this analysis, in the rest of the paper, we will have a closer look at spatial spillover effects. In this regard, we will also reconsider industry, input- or output-related, and tax preparer networks at a more local level to examine potential transmission channels.

[16]We will also present results where we cluster at a different regional levels. To account for serial correlation, we also show robustness checks where we cluster at the firm level.

# 5 A Deeper Look at Geographic Networks

## 5.1 Baseline Results

For the rest of the paper, we turn our focus on spillover effects that run through geographic proximity. Figure 2 reexamines the geography-based network results in more detail. Figure 2a graphically shows the estimated spillover effects (with corresponding confidence bands) that can also be found in Specifications (1) to (3) in Table 3. In Specification (1), we only consider the number of neighboring audits within 100 meters that happened one year before the return submission at maximum (which corresponds to geographical spillover effect shown in the previous section). In Specifications (2) and (3) we add wider distance bands to test the geographical scope of the spillover effect. While we include regressors for 100–500 and 500-1000 meters in Specification (2), Specification (3) additionally accounts for neighbor audits within 1–10km, 10–50km and more than 50km.[17] In line with intuition, the spillover effect declines in geographical distance. For the 100-500 meters radius, the point estimate is halved and becomes marginally insignificant. It also reassuring that for the very large distances, we estimate a very precise zero effect as can be seen in Figure 2a where the point estimates for coefficients > 1km lie on the zero-effect line with confidence bands that are too small to be depicted in the graph.

Figure 2b presents the results shown in Specification (4) to (7) in Table 3 where we use different sets of fixed effects to hedge against the concern that regions or sectors might be specifically targeted in a given year (e.g. due to common negative income shocks). Specification (4) estimates the model in Equation (2) with firm-fixed effects and the set of baseline control variables only. Specification (5) adds sector-year fixed effects. In Specification (6), we include municipality-sector-year-fixed effects to control for municipality-sector-specific time trends. Finally, the last specification reports results where a full set of suburb-year fixed effects is included. As can be seen from the point estimates shown in Figure 2b, qualitatively and quantitatively similar results are derived in these specifications.[18] The spillover effect is most pronounced in the very

[17] This result is also confirmed if we winsorize at the 95th and 99th percentile, respectively. Robustness results are reported in Table 15. Trimming the data at these percentiles leaves the qualitative results unchanged, the point estimates are smaller though. However, as only 25 percent of firms have a positive tax payment, trimming the data reduces lots of variation which might explain this pattern.

[18] In Table 17 in the Appendix, we also show results using the Poisson Pseudo-Maximum Likelihood (PPML) estimation technique. The spillover effect for the smallest distance band of up to 100 meters stays significant at the 5 percent level. The point estimate reduced however to 0.4 percent as shown,

vicinity of the taxpayer and fades off in geographical distance.

To see whether the results stay robust for alternative definitons of the outcome variable, Table 4 reestimates our preferred specification using municipality-year and sector-year fixed effects for a radius up to 1000 meters using the level of tax liability in Specification (1). In Specification (2), we use a binary variable for having a positive tax liability as outcome. Specifications (3) and (4) repeat the same ecercise for taxable income whereby loss-making companies are excluded in Specification (3). There is a positive and significant effect of having a neighbor audit within 100 meters in all four regressions. If the number of neighboring tax audits increases by one, the average tax liability (taxable income) increases by around 1147 (5056) South African Rand. Interestingly, the point estimates for the two larger distance bands up to 1000 meters are significantly different from zero if we consider levels compared to proportional effects. Again, the spillover effect is declining in distance though with 496 and 394 South African Rand for the 100–500 and 500–1000 meters radius, respectively. The probability of having a positive tax liability (taxable income) increases by only around 0.005. Thus, the main effect might not be driven by firms that either report losses or report very low income which is tax-excempt up to around 60,000 Rand.[19]

Finally, in Table 16, we conduct further robustness checks where we restrict the sample and the control variables. In Specifications (1) and (2), we only consider firms observed over the entire data period. The next two columns show results where we exclude firms that report losses in every single period. In the last two specifications of Table 16, we run the baseline regression without including the spatial control of average taxable income. These modifications do not alter our results. As expected, the spillover effect is stronger for the balanced panel and without loss-making firms. For the former, the spillover effect is 1 percent compared to the baseline effect of 0.7 percent if all firms are considered.

Concluding, we present evidence of significant spatial spillover effects of business audits on non-audited taxpayers. The results also turn out to be quantitatively relevant. The findings suggest that one additional audited firm within a 100-meter distance radius increases the average reported tax liability by 0.7 percent or 1150 South African

for example, in Specification (2) of Table 17. However, please note that the algorithm failed to reach convergence due to the high number of fixed effects. Based on this numerical instability, this table has to be interpreted with caution and should only serve as a robustness test. The fact that the point coefficient using PPML decreases is in line with the results in Brockmeyer and Hernandez (2016), for example. See their Appendix for a detailed discussion.

[19]These tax thresholds where the tax liability jumps from zero to a positive amount changed over time. See Lediga et al. (2019) for further details.

Rand which is around 55 Euro per firm. Evaluated at the sample mean where firms have 112 neighboring firms within 100 meters, a simple back-of-the-envelope calculation suggests that the average spillover-related revenue gain per audit amounts to 128,800 South African Rand or 6200 Euro. For the 28,588 audits within our sample frame from 2008 to 2015, audit spillovers are hence suggested to have yielded 3.7 billion South African Rand in additional tax revenues (which corresponds to around 6.5 percent of the roughly 56.8 billion South African Rand in additional tax revenue collected through direct audit adjustments within our sample frame).

## 5.2 Examining Non-linear Effects

In this subsection, we drop the linearity assumption that each additional neighboring tax audit has the same effect on tax owed to the government. For all possible transmission channels, such a non-linear relationship where the deterrence effect decreases in the number of experienced neighboring tax audits seems plausible. In addition, especially when considering the audit as an exogenous positive shock to the subjective own audit probability, the spillover effect might be much more pronounced when the taxpayer gets his first neighbor audit compared to a situation where he faces a positive number audits in each period. Table 5 provides the results from specifications where we test for such non-linear effects. For simplicity and based on the results from the previous section, we only consider neighboring tax audits within 100 meters. In Specification (1), we include a squared term to test for a potential quadratic relationship. Albeit not significant, the coefficient of the squared regressor has the expected negative sign suggesting that the effect of an additional neighboring tax audit is slightly decreasing. While the results do not change if we exclude firms whose neighbors have never been subject to a tax audit (see Specification (2)), the effect is smaller and not statistically different from zero if we only consider firms with more than 20 neighboring tax audits in Column (3). In Column (4), we run the regression on the subsample of firms with more than one neighboring tax audit within 100 meters over the sample period. Here the effect stays significant with an estimated spillover effect of 0.9 percent.

The last two columns of Table 5 report the results where we restrict the sample to firms for which the number of experienced neighboring tax audits over the sample period does not exceed one (in Specification (5)) or is exactly one (in Specification (6)). Thus, the variation used to identify the spillover effect comes only from firms that are exposed to a one-time shock. In line with our intuition, the spillover effect is much more pronounced. Compared to the baseline effect of 0.7 percent, the effect is more than eight times larger for this subsample of firms. If a neighbor gets audited and if this audit is the first tax audit in the vicinity of the considered firm $i$, the tax liability of this firm increases by around 5.8 percent. Given this large spillover effect, it might, for example, be worthwhile as a tax authority with scarce resources to audit firms that have never been audited before as the potential spillover effects on neighboring firms, which experience their first neighboring audit, are relatively large. This result has thus direct implications for tax enforcement strategies, especially in countries where audit resources are scarce.

## 5.3 Effect Dynamics

In this section, we analyze the dynamics of the spillover effects arising from neighboring tax audits (see Advani et al., 2019) to see how long deterrence effects from enforcement spillovers might last. Exploiting data from random tax audits in the UK for the years 1999 to 2012, Advani et al. (2019) show that audits increase reported tax liabilities up to five years after the audit was completed. While the audit effect declines over time, they find that this decline is much faster for less auto-correlated income components. This finding is consistent with the idea that tax audits deliver new information to the tax authority and this information is more informative in predicting future outcomes for relatively stable income components. In our study, however, these dynamic effects might not be rooted in the fact that the tax authority has more information about the taxpayer as the firms that we consider do not get audited themselves. In our case, dynamic effects might arise from changes in the perceived audit probability or even in the perceived probability that evasion is detected.[20]

Table 6 reestimates our preferred specification (including municipality-year and sector-year fixed effects) in a dynamic setting, implying that the specifications comprise regressors that capture the number of neighbors in a given distance band that were audited one, two, and three years before the submission of the tax return, respectively.[21] As the audit data set comprises all audits that were completed between April 2008 and April 2015, we restrict the analysis of dynamic effects to the tax years 2011 to 2015. As apparent from Columns (1) and (2), the spillover effect lasts for more than one year, but already declines in the third year and even becomes insignificant when we include regressors for the 100-500 and 500-1000 meter radius. Interestingly, if we take

[20]The latter channel makes only sense if the firms i) knows the audit outcome of the neighboring tax audit and ii) has some information on the true level of income of its neighbor.

[21]Specifically, the one-, two-, three-year-lag is defined as the number of audits that were closed within 365, 366-730, 731-1095 days before the submission of the considered return.

the dynamic nature of these spillover effects into account, the point estimate of our baseline effect, i.e. the spillover effect in year-1, increases to around 1 percent. Moreover, the spillover effect for the two wider distance bands stays statistically significant with an effect isze of 0.6 and 0.3 percent, respectivly. Thus, compared to Advani et al. (2019), the spillover effects arising from neighboring audits are rather short-run in nature. This is also confirmed if we restrict the sample to firms with only one neighbor audit, as can be seen in Specification (3). Here, having a neighbor audit 365 days prior to the submission of the own tax return increases the tax payments by around 11 percent. The effect declines more than 50 percent in the second years after the event (to 5 percent) and becomes insignificant in the third year.

In the last two columns of Table 6, we present two placebo tests by including leads of the independent variable to check for increased tax liabilities prior to the treatment. As all coefficient are insignificant, firms that have a neighbor audit in year $t$ do not show increased tax liabilities in year $t-1$ and $t-2$ which speaks in favor of our identification strategy. In other words, after controlling for all fixed effects, neighbor audits are as good as randomly assigned.

Finally, in the spirit of Specification (3) and (5), we conduct an event-study-type analysis for the subset of firms that experiences exactly one neighboring tax audit. However, based on the limited years available, we are restricted in the number of lags and leads that we can use for the event study (see Schmidheiny and Siegloch (2019) for a detailed discussion of the methodology). We set an effect window of (-3,2) such that we can look at the effect three years prior and two years after the treatment. By including these lags and leads into the estimation model in a consistent manner, we have to restrict the analysis to the firms that experience their neighbor audit after April 2010 to ensure that we observe at least three pre-intervention periods for all firms.[22] We set the year $t-2$ as the baseline year such that we have to interpret the depicted point coefficients with respect to this year. The result is shown in Figure 3. Three things are worth noting. First, in line with the results from Table 6, the spillover effects last for around two years and then starts declining in the third year after the neighbor audit. Second, no coefficient in the pre-intervention period is significantly different from zero. Especially in year -3, the point estimate is effectively zero. Third, the point estimate in the year -1 is positive, albeit not significantly different from zero. This might be irritating at first sight as one might infer from this graph that firms have already responded prior to the treatment. There is, however, a plausible explanation for this pattern. Please remember that the regressor of interest in our baseline case is

[22] Treatment indicators were, moreover, binned at endpoints.

the number of neighboring audits that were *completed* within the last 365 days prior to the submission of the tax return. For our analysis, we have to rely on the completion date of the audit, as we do not observe the information on its issue date for most audit cases. From the cases where we observe both the completion and the issue date, we know that an audit takes, on average, 218 days. The median duration is 133 days and there is a non-negligible number of audit cases with more than a one-year gap between the issue and closure date of the audit. The positive effect in year $t-1$ thus includes cases where the neighbor audit was already issued and it is plausible that some firms might already know and react to the audit when it is issued.

## 5.4 Robustness and Randomization Inference

Regarding the precision of estimated coefficients, one might be concerned that the significance of our results hinges on the choice of clustering standard errors. In the Appendix, we therefore present additional evidence for the robustness of our baseline results by reestimating Table 3 with different levels of clustering the standard errors. Tables 12 to 14 demonstrate that our baseline results do not change if we cluster on the firm-level (e.g. to account for serial correlation), on the suburb, or the municipality.[23]

However, if clustering the standard errors is insufficient to correct for the serial and spatial correlation of error terms, we might generally run the risk of getting too small standard errors. Therefore, as an additional robustness test, we rely on randomization inference as originated in Fisher (1935). This inferential technique does not hinge on any distributional assumptions about the structure of the error terms. Instead of the standard underlying idea of repeated sampling from an underlying population, randomization inference is based on fixed data and repeated permutations of the treatment variable. The general procedure consists of four steps. First, we compute the test statistic of interest for the original sample (in our case the t-statistic). We then randomly permute the treatment variable, that is the number of neighboring audits, and calculate the respective test statistic for each permutation. To take care of the potential treatment assignment process and possibly fixed audit resources per year and region, we strata by province-years and municipality-years, respectively. This randomization procedure is repeated 2000 times, which finally generates a distribution of the respective test statistic. In the last step, we compare the original test statistic from step one to the distribution obtained in step three. Please note that as the output

[23]The number of clusters is 513,154 for the firm-level clustering, 5,817 for the suburb and 212 for the municipality-level clustering.

of this procedure is solely a p-value, it is not possible to report standard confidence intervals.

The results of this exercise is shown in Figure 4. In Subgraphs (a) to (c) and (e), we plot the distribution of the t-statistic for the baseline specification including firm-fixed effects, municipality-year- and sector-year fixed effects for all firms. In Subgraphs (d) and (f), we only look at firms with exactly one neighbor audit. In addition, graphs (a) to (d) ((e) and (f)) present results where we permute the treatment variable within municipality-year (province-year) cells. The vertical line represents the t-statistic from the regressions shown in Column (2) of Table 3 and Column (6) of Table 5. In all six graphs, the reference distribution lies almost symmetrically around zero. For the small distance radius of 0-100 meters, the original t-statistic is "extreme" and placed at the right tail of the reference distribution suggesting that our results are significantly different from zero. It is further reassuring to see that for the wider distance bands up to 1000 meters, the original t-statistic is closer to zero and not as extreme as in the other cases. The graphical inspection is also confirmed when looking at the two-sided $p$-values, where we count the number of t-statistics that are—in absolute terms—larger than the original t-statistic to calculate $p$-values. For Figure 4a, for example, the two-sided $p$-value is 0.05, i.e. only 5 percent of all resampled t-statistics are larger than our originally estimated t-statistic.[24] Using province-year cells for the stratification is less restrictive and leads to a corresponding lower $p$-value of 0.01. This robustness exercise shows that the precision of our estimates does not hinge on the way we calculate our standard errors in the baseline analysis.

# 6 Mechanism Analysis

Understanding the transmission channels of enforcement spillovers is essential to draw policy conclusions. The link between neighbor audits and businesses' tax reporting may, broadly speaking, either be rooted in taxpayer-to-taxpayer communication (through a change in the perceived audit probability and/or social norm considerations) or in business interactions between firms.[25] There is, moreover, an additional transmission channel if firms share a common tax practitioner who might learn from tax audits of own clients himself. In the following, we want to explain these underlying transmission

[24]The two-sided $p$-value for the results shown in Figure 4b to f are 0.13, 0.25, 0.02, 0.01, 0.08, respectively.

[25]Please note that the latter is not relevant for tax audits of individuals but might play an important role in the corporate sector.

channels and compare their theoretical predictions with the observed empirical pattern.

## 6.1 Change in Perceived Audit Probability

Audited business owners may tell other taxpayers in their local network about their audit experience. In terms of the seminal work of Allingham and Sandmo (1972), observing an audit in their neighborhood, taxpayers may update their propensity for being selected for an audit as well as the expected fine rates. Behavioral responses may also emerge because fines and audits become salient when observed with others. It may be business owners who communicate with each other in personal or business networks (like local business associations) and these connections may form communication channels through which information on audit experiences is transmitted. Alternatively, information may flow through non-institutionalized interactions of employees of the businesses (either through personal and business relations) or through employer changes.

The change in the subjective audit probability arising by observing neighbors being audited is ambiguous (e.g., DeBacker et al., 2018). If the taxpayer's perceived audit probability increases in the number of neighboring audits or if he gets scared by having the tax inspectors nearby, we would observe an upward adjustment in the tax liability of the firm. On the other hand, if taxpayers have beliefs about the administrative resources of the tax authority being fixed, the perceived likelihood of an audit decreases if geographic neighbors get audited. This effect, known as the "bombcrater effect", would thus have the opposite sign resulting in a downward adjustment of the tax liability of the firm.

The positive spillover effects presented in the previous section clearly points against this bombcrater effect. In all specifications, having a neighbor audit increases the tax liability of the firm which would speak in favor of the first explanation. We want to stress though, that this mechanism is plausible if the firm gets informed about the neighbor audit. This is, for example, different to DeBacker et al. (2018) or Advani et al. (2019) which examine dynamic effects of own tax audits. Previous papers analyzing information flows within networks have already been able to document that the spread of information sharply declines in geographic distance. The study by Drago et al. (2020), e.g., suggest that enforcement spillovers vanish if the distance between the treated and the untreated household is more than 500 meters. This is also in line with our results. In most of our specifications, the spillover effect becomes statistically insignificant if we consider neighbor audits in more than 100 meter distance.

In addition to geographic distance, to the extent that the communication of audit experiences is a voluntary act, the propensity to tell others about an own audit experience may correlate with the audit outcome in combination with social norm considerations. This will be explained in the next section.

## 6.2 Social Norms

There is a growing academic literature on the effect of social norms in driving tax compliance decisions.[26] Despite the increasing number of laboratory and field experiments as well as survey-based studies in this strand of the literature, consensus about the link between tax compliance and social norms has not been reached (see, e.g., Alm et al., 2009; Bérgolo et al., 2019; Bott et al., 2019). The complex link between information on social norms and compliance behavior has also been documented in Del Carpio (2014) studying the property tax in Peru. For our study on enforcement spillover of corporate tax audits, the role of social norms in influencing the compliance decisions of untreated firms is twofold.

First, social norms may play a detrimental role for the communication of the tax audit. If tax evasion is seen as a socially unacceptable behavior, audited taxpayers may, out of social norm considerations, have a lower propensity for telling others about their audits if they were found non-compliant in the course of the audit. Contrary, the likelihood of communicating the tax audit might be higher if the audit did *not* identify them as a tax evader. If that held true, behavioral responses to neighbor audits would be expected to be quantitatively stronger, or even restricted, to taxpayer audits that did not result in upward revisions of the tax payments due.

Second, the results of neighbor audits disclose information about the social norms of being compliant within the local network of the taxpayer. This second line of argumentation is based on the assumption that the firms knows about the audit *and* its result. A negative audit result reveals direct information about the norm of being compliant as it shows that others abide the tax law. If anything, this would result in an increase in reported income of the firm. On the other hand, if the neighbor was found guilty and accused of being a tax evader, i.e. has a positive audit result, this may lead to a decrease in reported behavior because the taxpayer may adjust his own

[26] A social norm represents a pattern of behavior that is judged in a similar way by others, and is sustained in part by social approval or disapproval. Consequently, if others behave (do not behave) according to some socially accepted mode of behavior then the individual will (not) behave appropriately. See, for example, Alm et al. (1999).

behavior according to the social norm of noncompliance.[27]

Table 7 provides interesting insights and suggestive evidence about the plausibility of the above mentioned channels. In this table, we split the audit-regressor $NA_{r,i,t}$ by audit result, i.e. we count the number of neighboring audits with and without a positive audit result (labeled as "Non-Compliant" and "Compliant"). Columns (1) to (3) consider the full sample, while Columns (4) and (5) restrict the sample to firms that experienced one single neighbor audit during the sample period. In Columns (3) and (5), we replace the number of non-compliant neighbor audits with the count of audits with an "extreme" audit result, i.e an audit result above the 90th percentile.[28] Interestingly, the results shown in Table 7 suggest that the positive link between neighbor audits and firms' tax reporting is driven by audits where audited taxpayers have been cleared in the course of the tax audit. If the number of compliant tax audits within 100 meter distance increases by one, this leads to an increase in the tax liability of the firm by 1.3 percent. In this sub-analysis, the effect for the 100 to 500 meter distance band stays statistically significant with a point estimate of 0.5 percent. For firms with a first-time shock, the tax liability even increases by 9.1 percent. It also also worthwhile to note that—albeit statistically insignificant—the sign on the number of non-compliant audits is negative for the first two columns.

How can we reconcile this finding with the above-mentioned mechanism of social norms? There are two explanations in line with this finding. First, it may indeed be true that out of social norm considerations, taxpayers only communicate about the audit if they were cleared. In this case, only compliant audits are communicated to neighbors and we see an upward adjustment in the tax reporting of the firm - either because of an increased subjective audit probability or because of updating their belief about the compliance level in their neighborhood (which might be higher as expected because only compliant audits are communicated). The second explanation is rooted in an asymmetric behavioral response to social norms. The result pattern could also be explained if neighboring taxpayers are not selective in communicating their audits, but taxpayers only respond to an increase in their beliefs about the level of compliance while there is no or only a small response if they find out that their neighbors are cheating on their tax returns. Stated differently, if the taxpayer finds out that his neighborhood abides to the tax law, he becomes more compliant, while he does not become more

[27]Note that this is independent of the compliance level of the respective firm. If the firm would have been compliant, he may start engaging in tax evasion. If the taxpayer would have evaded anyway, he may increase the evaded amount of income.

[28]This amounts to 558,230 South African Rand.

non-compliant if he finds out the opposite. Of course, these two explanations do not rule each other out and could work simultaneously. In any case, from the perspective of the tax authority, knowing that spillover effects are driven by compliant audits may be an important and so far ignored determinant in choosing an optimal audit strategy.

### 6.3 Spillovers through Business Links

If economic relations were driving the main effect, enforcement spillovers could emerge through two channels. Firstly, an audit may be a positive cost shock to competitors or business partners, inducing behavior responses of connected parties. Specifically, such cost shocks may relate to the payment of tax arrears and fines, but also to potential increases in tax payments in post-audit periods (Kleven et al., 2011; Advani et al., 2019; DeBacker et al., 2018). Please note that this transmission channel of enforcement spillovers even works if the audited firm *does not* tell others about its experience (compared to the other channels laid out in this section). However, it is obvious that cost shocks are limited to audit cases which result in positive adjustments in the tax payments due (i.e., where audited taxpayers are identified as evaders). If business relations were a driver of the observed tax spillovers, theory would hence suggest that the observed effects are centered around (or are at least larger for) those audits which resulted in an upward revision of the audited taxpayers' taxable income. The response might, however, depend on the economic connection between the entities. For competitors, positive audit-related costs shocks to neighbors are expected to translate into higher firm profits (and hence tax payments) in standard competition models. If firms are business partners and are connected within production chains, cost shocks may negatively impact on firm profits, making the overall effect ambiguous.

Alternatively, tax authorities might obtain evasion-relevant information in the course of the audit. It is obvious that this information transmission channel is much more relevant for firms that are economically connected via ownership structures (i.e. firms might belong to a corporate group, see also Boning et al. (2018)) or along the value chain through an input- or output-related link. Pomeranz (2015), for example, finds evidence for enforcement spillovers which are rooted in information generated through "paper trails" established by the VAT system. In her paper, Pomeranz (2015) distinguishes between two mechanisms of the self-enforcing VAT paper trail which is based on the idea that firms have to ask for receipts in order to deduct input costs from their VAT payments.[29] While "collusive evasion" involves coordinated behavior between

[29] Please note that this self-enforcing mechanisms of the VAT breaks down at the final stage where

buyers and sellers such that the transaction does not show up in the books, "unilateral evasion" occurs when firms take the risk of underreporting their taxable income despite a paper trail being created. For our study, this implies two things. First, the paper trail transmission channel is only relevant if the audited firms inform their suppliers and clients about it. Second, in the case of collusive evasion, behavioral adjustments might be more likely to happen if the audited firm was found to be guilty of tax evasion. If the firm, however, engages in unilateral evasion, the reaction might be unrelated to the audit result of the neighboring firm.

To get an idea about the importance and the plausibility of the business channel, Table 8 presents results where we differentiate between neighbor audits of firms that do and do not operate in the same two-digit industry as the taxpayer under consideration. In addition, Tables 9 and 10 report analogous results where we count the number of neighbor audits of firms that are likely to be in an input-related or output-related industry.[30] To determine input-output linkages, we use the official Input-Output Table from Statistics South Africa for the year 2013 (Statistics South Africa, 2017). To be more precise, for each sector, we determine the three most relevant input (output) industries and then count the number of neighbor audits in these three industries.[31] Tables 8 to 10 have the same structure. In Specification (1), we consider neighbor audits of firms in the same/other industry within the 100 meter radius. Specification (2) looks at larger distance bands. We pool the 0-100 and 100-500 meter radius as the the number of firms operating in the same industry or in an input- or output-related industry within 100 meters is small such that observing an insignificant effect might be related to low statistical power. In Specification (3), we additionally split the regressor of interest by the audit result as in Table 7. Finally, in the last two columns, we only consider firms with one neighbor audit within 100 meters.

The result pattern presented in Tables 8 to 10 again suggests that enforcement spillover effects of neighbor audits are clustered around audits that did not result in an upward revision of the taxpayers' tax liability. The result pattern is thus largely at odds with spillover effects caused by cost-related shocks to business-related entities. Moreover, as apparent from Table 8, audits of firms in the same and other industries that did not result in upward adjustments, in turn, exert a quantitatively comparable effect on taxpayers' reported tax due (0.6 vs. 0.4 percent). In line with unilateral

firms sell to final consumers, see Naritomi (2019).

[30]In Table 18 we also show results where we combine input- und output-related industries in one category.

[31]Using the three most important input (output) industries is an arbitrary choice. Our results stay qualitatively robust if we use, for example, the five or ten most important industries.

evasion combined with increased communication flows to input- or output-related firms, we find stronger spillover effects for these type of neighbor audits. If the number of compliant audits of supplying firms increases by one, the tax liability of the considered firm increases by around 2 percent, see Specification (2) of Table 9. This effect is more than twice as high compared to the baseline effect of 0.7 percent. The effect is smaller and less clear for the case of neighbor audits in output-related industries.

## 6.4 Spillovers through Tax Preparers

Audit-related enforcement spillovers might also emerge if audited and non-audited firms hire the same tax practitioner for the preparation of their yearly tax return. Tax professionals may serve as an information hub to facilitate taxpayer-to-taxpayer communication. Besides, tax audits might directly influence the work of tax preparers who could learn about the behavior of the tax authority and the detection probabilities of different income items or deductions.[32] In Table 11, we report results where the regressor of interest is split into the number of neighbor audits of firms using the same tax preparer as the firm under consideration. In line with the results shown in Section 4, the results do not support the hypothesis that tax preparer networks might play an important role. Note, however, that the number of firms within 100 or even 500 meters distance that use the same tax preparer is very small such that this analysis may suffer from power problems. Albeit not significant, it is nevertheless interesting to see that the spillover effect of having a non-compliant neighboring firm using the same tax practitioner leads to point estimate of 0.13, see Specification (5). This might hint at potential spillovers through the network of tax prepares. There are two recent studies that indeed provide evidence for the existence of enforcement spillovers in tax preparers networks. Using the population of sole proprietorship taxpayers in Italy for seven years, Battaglini et al. (2019), for example, can document increased tax compliance of firms that share a common tax preparer. For the US, Boning et al. (2018) also find a positive small effect of around two percent, i.e. tax remitted by network firms increases by around two percent in the course of a network audit. While our analyses has to be interpreted with caution given the above-mentioned power problems, the results may also differ in the context of less developed economies where the number of firms engaging tax professionals, the regulation of tax professionals and the presence of corruption might be different.

[32]Of course, in the context of less developed countries, corruption might also be an important channel. In this sense, tax practitioners might learn about corruption and possible bribes.

# 7 Summary and Conclusion

In this paper, we provide evidence that business taxpayer audits exert a positive and significant effect on the tax liabilities of spatial neighbors. The empirical analysis relied on business tax returns and audit data from South Africa. Quantitatively, the results suggest that the audit of a close geographic neighbor increases corporate tax reporting by around 0.7%, on average. This translates into a non-negligible revenue gain at the aggregate level. As we find that the positive spatial spillovers are largely driven by audit cases that did not result in an upward revision of due business taxes, the findings also carry important implications for the optimal design of audit strategies: Revenue-maximizing audit selection systems are suggested to benefit from adding the size of taxpayers' local network to the set of audit selection criteria and, if taxpayers are targeted for spillover considerations, to focus on taxpayers that are expected to be compliant (as significant spillovers are limited to this group of firms).

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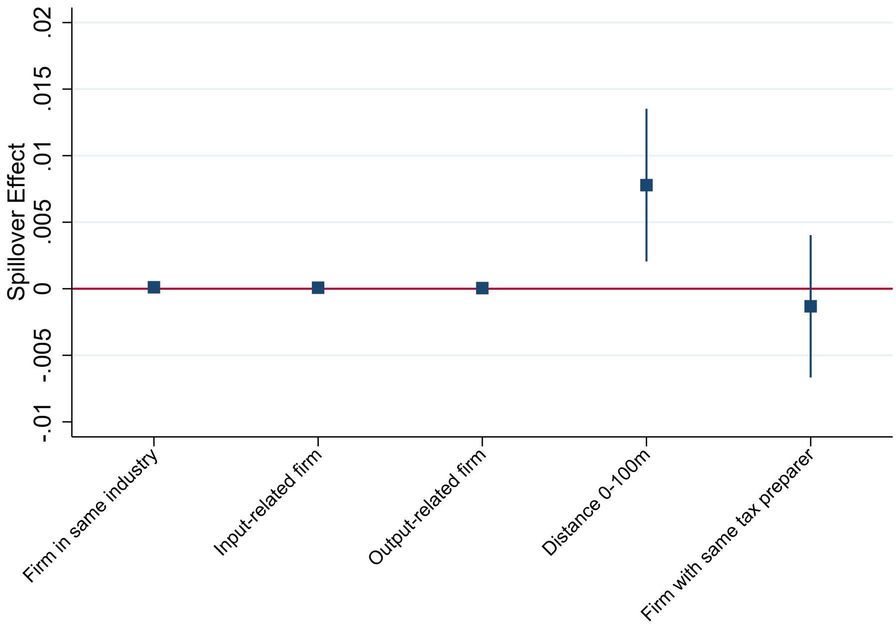
# Figures and Tables

Table 1: Descriptives

| | Mean | N | SD | Min | Max |
|---|---|---|---|---|---|
| *Dependent variables* | | | | | |
| Tax payment | 0.0622 | 3,737,629 | 2.07 | 0.00 | 2788.50 |
| P(tax payment)$>0$ | 0.2086 | 3,739,230 | 0.41 | 0.00 | 1.00 |
| Tax payment, sine transformed | 2.2636 | 3,737,629 | 4.54 | 0.00 | 22.44 |
| Taxable income | -0.2382 | 3,739,230 | 15.77 | -7058.95 | 9958.91 |
| P(taxable income)$>0$ | 0.2473 | 3,739,230 | 0.43 | 0.00 | 1.00 |
| *Audit variables* | | | | | |
| Audits, same industry | 462 | 3,735,368 | 529 | 0 | 3392 |
| Audits, input-related | 1479 | 3,739,230 | 1645 | 0 | 7453 |
| Audits, output-related | 1506 | 3,739,230 | 1600 | 0 | 7453 |
| Audits, same practitioner | 0.4073 | 3,735,368 | 1.8973 | 0 | 45 |
| Audits, 0-100m | 0.7003 | 3,735,368 | 2.2645 | 0 | 45 |
| Compliant Audits, 100m | 0.4185 | 3,735,368 | 1.4683 | 0 | 30 |
| Audits, same industry, 100m | 0.1103 | 3,735,368 | 0.5857 | 0 | 25 |
| Audits, input-related, 100m | 0.2494 | 3,739,230 | 1.2008 | 0 | 44 |
| Audits, output-related, 100m | 0.2599 | 3,739,230 | 1.2181 | 0 | 44 |
| Audits, same practitioner, 100m | 0.0284 | 3,735,368 | 0.4344 | 0 | 24 |
| Audits, 100-500m | 1.2094 | 3,735,368 | 3.1711 | 0 | 72 |
| Audits, 500-1000m | 2.4862 | 3,735,368 | 5.3666 | 0 | 114 |
| *Controls* | | | | | |
| Neighboring firms, 0-100m | 112.33 | 3,735,368 | 281.31 | 0 | 2684 |
| Neighboring firms, 100-1000m | 399.02 | 3,735,368 | 583.61 | 0 | 6323 |
| Total assets | 19.04 | 2,332,978 | 1653.60 | 0.00 | 899295.50 |
| Avg. taxable income neighbors, 0-100m | 0.05 | 3,481,761 | 17.69 | -6934.51 | 3166.02 |
| Avg. taxable income neighbors, 100-1000m | 0.08 | 3,399,176 | 4.35 | -226.89 | 1713.31 |

*Notes:* This table presents descriptive statistics of the main variables. All amounts are in South African Rand, expressed in million. Taxable income variables include loss-making firms. Number of audits, 0-100m (100-500m, 500-1000m) is the number of tax audits by firms that are located within a 0-100m (100-500m, 500-1000m) radius of the considered firm. Analogously, for each distance band, the number of tax audits of neighboring firms with the following characteristics is displayed: having a non-positive audit result ("compliant"), operating in the same sector ("same industry"), operating in an input or output-related industry ("input-related" and "output-related", having the same tax advisor ("tax practitioner"). When calculating average taxable income within 100m and 100-1000m, the considered firm was left out.
*Source:* Own calculation based on SARS data.

Figure 1: Results Networks - All Firms




(a) Main Results Network Effects

*Note:* Panel (a) plots regression coefficients from estimating enforcement spillovers in various networks. In all specifications, the dependent variable is sine-transformed tax liability which is regressed on the number if network tax audits. The dependent variable is winsorized at the top 99.9th percentile in all specifications. All regressions include a full set of firm-, municipality-year- and sector-year fixed effects. Standard errors are clustered by sector-year.

*Source:* Own calculation based on SARS data.

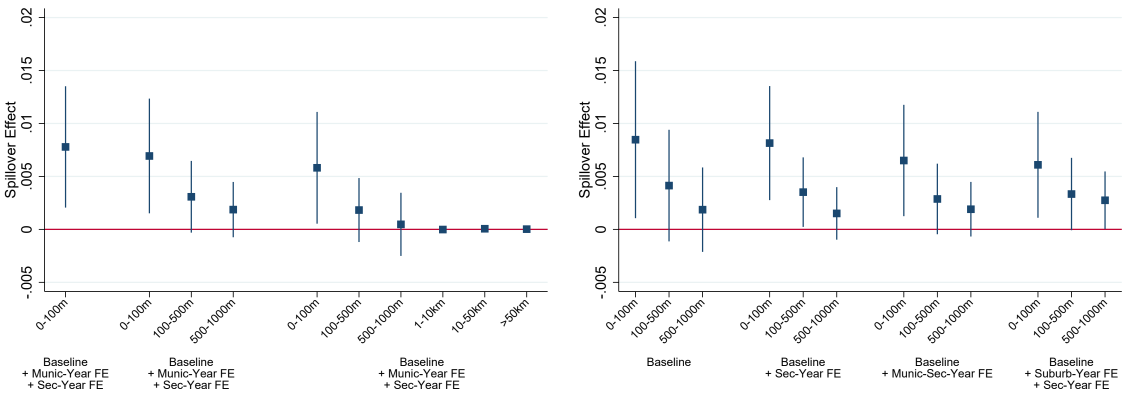
Table 2: Regression Results Network Effects

| Model | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Audited firm is/has | same industry | input-related | output-related | 100m distance | same tax preparer |
| Number of network audits | 0.0001*** | 0.0001*** | 0.0000 | 0.0078** | -0.0013 |
| | (0.0000) | (0.0000) | (0.0000) | (0.0035) | (0.0032) |
| Total assets (in log) | 0.2442*** | 0.2441*** | 0.2442*** | 0.2445*** | 0.2442*** |
| | (0.0073) | (0.0073) | (0.0073) | (0.0073) | (0.0073) |
| Observations | 1,910,180 | 1,910,180 | 1,910,180 | 1,910,180 | 1,910,180 |
| $R^2$ | 0.747 | 0.747 | 0.747 | 0.747 | 0.747 |
| Firm FE | x | x | x | x | x |
| Munic-Year FE | x | x | x | x | x |
| Sector-Year FE | x | x | x | x | x |

*Notes:* This table report the effects of network tax audits on the tax liability of non-targeted firms from 2009-2015 for all firms. In all specifications, the dependent variable is sine-transformed tax liability which is regressed on the number if network tax audits. The dependent variable is winsorized at the top 99.9th percentile in all specifications. All regression include a full set of firm-, municipality-year- and sector-year fixed effects. Standard errors are clustered by sector-year. ***,**,* indicate significance at the 1%, 5%, 10% levels.

*Source:* Own calculation based on SARS data.

Figure 2: Results Geographic Network - All Firms




(a) Main Results

(b) Different Fixed Effects

*Note:* Panel (a) plots regression coefficients from estimating Specification (1) to (3) of Table 3. From left to right, the result of three distinct regressions is shown, starting with a regression that only considers a distance band of 0-100m. In the mid specification, the 100-500m and 500-1000m radius is added, while in the right specification, firms located up to 50km and more are considered. Figure (b) displays the results from Columns (4) to (7) in Table 3 with different sets of fixed effects, see Section 5.1 for more details. Standard errors are clustered by sector-year.

*Source:* Own calculation based on SARS data.

Table 3: Main results

| Model | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| Audits, 0-100m | 0.0078** | 0.0069** | 0.0057* | 0.0085* | 0.0081** | 0.0065** | 0.0061** |
| | (0.0035) | (0.0033) | (0.0032) | (0.0045) | (0.0033) | (0.0032) | (0.0030) |
| Audits, 100-500m | | 0.0031 | 0.0018 | 0.0042 | 0.0035* | 0.0029 | 0.0034 |
| | | (0.0020) | (0.0018) | (0.0032) | (0.0020) | (0.0020) | (0.0021) |
| Audits, 500-1000m | | 0.0019 | 0.0005 | 0.0019 | 0.0015 | 0.0019 | 0.0027* |
| | | (0.0016) | (0.0018) | (0.0024) | (0.0015) | (0.0016) | (0.0016) |
| Audits, 1-10km | | | 0.0000 | | | | |
| | | | (0.0001) | | | | |
| Audits, 10-50km | | | 0.0001** | | | | |
| | | | (0.0000) | | | | |
| Audits, >50km | | | 0.0000*** | | | | |
| | | | (0.0000) | | | | |
| Total assets (in log) | 0.2445*** | 0.2445*** | 0.2442*** | 0.2470*** | 0.2445*** | 0.2442*** | 0.2439*** |
| | (0.0073) | (0.0073) | (0.0073) | (0.0074) | (0.0073) | (0.0073) | (0.0071) |
| Observations | 1,910,180 | 1,910,180 | 1,910,180 | 1,910,180 | 1,910,180 | 1,904,039 | 1,906,369 |
| $R^2$ | 0.747 | 0.747 | 0.747 | 0.746 | 0.747 | 0.751 | 0.752 |
| Baseline controls | x | x | x | x | x | x | x |
| Firm FE | x | x | x | x | x | x | x |
| Sector-Year FE | x | x | x | | x | | |
| Municipality-Year FE | x | x | x | | | | |
| Municipality-Sector-Year FE | | | | | | x | |
| Suburb-Year FE | | | | | | | x |

*Notes:* This table report the effects of tax audits of neighboring firms on tax liabilities from 2009-2015 for all firms. In all specifications, the dependent variable is sine-transformed tax liability which is regressed on the number if neighboring tax audits within the respective distance bands. The dependent variable is winsorized at the top 99.9th percentile in all specifications. In Column (1) to (3), firm-, municipality-year- and sector-year fixed effects are included. The combination of fixed effects is varied in Columns (4) to (7). Standard errors are clustered by sector-year. ***,**,* indicate significance at the 1%, 5%, 10% levels.
*Source:* Own calculation based on SARS data.

Table 4: Other outcomes

| Model | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Dependent variable | Tax liability | P(tax liability)> 0 | Taxable income | P(taxable income)> 0 |
| Audits, 0-100m | 1144.4136*** | 0.0005* | 5032.0658*** | 0.0005* |
| | (338.9206) | (0.0003) | (1761.2368) | (0.0003) |
| Audits, 100-500m | 498.8787** | 0.0002 | 548.1251 | 0.0001 |
| | (226.4201) | (0.0002) | (1241.2910) | (0.0002) |
| Audits, 500-1000m | 395.9227* | 0.0001 | 1805.8551 | 0.0001 |
| | (202.0368) | (0.0002) | (1120.0677) | (0.0002) |
| Total assets (in log) | 4520.7811*** | 0.0225*** | 30391.5746*** | 0.0291*** |
| | (334.8048) | (0.0007) | (2041.3928) | (0.0014) |
| Observations | 1,910,180 | 1,910,180 | 1,088,702 | 1,910,180 |
| $R^2$ | 0.761 | 0.715 | 0.829 | 0.722 |

*Notes:* This table reports the effects of tax audits of neighboring firms on (1) the level of tax liabilities (in South African Rand), (2) a dummy indicator for having a positive tax liability, (3) the level of taxable income and (4) a dummy indicator for reporting positive taxable income. In Specification (3), loss-making firms are excluded from the analysis. For Specification (1) and (3), the data is winsorized at the 99.9th percentile. All regressions shown in this table include firm-, municipality-year- and sector-year fixed effects. Standard errors are clustered by sector-year. ***,**,* indicate significance at the 1%, 5%, 10% levels.
*Source:* Own calculation based on SARS data.

Table 5: Results - Non-linear Effects

| Model | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Sample | All firms | No never | # audits $\geq$ 20 | # audits > 1 | # audits $\leq$ 1 | # audits = 1 |
| Audits, 0-100m | 0.0109** | 0.0095*** | 0.0062 | 0.0088*** | 0.0586** | 0.0581** |
| | (0.0049) | (0.0034) | (0.0039) | (0.0034) | (0.0228) | (0.0233) |
| Audits, squared, 0-100m | -0.0001 | | | | | |
| | (0.0001) | | | | | |
| Total assets (in log) | 0.2445*** | 0.2515*** | 0.2588*** | 0.2485*** | 0.2421*** | 0.2623*** |
| | (0.0073) | (0.0087) | (0.0113) | (0.0089) | (0.0069) | (0.0094) |
| Observations | 1,910,074 | 860,314 | 98,891 | 671,991 | 1,238,014 | 188,227 |
| $R^2$ | 0.747 | 0.746 | 0.746 | 0.746 | 0.747 | 0.746 |

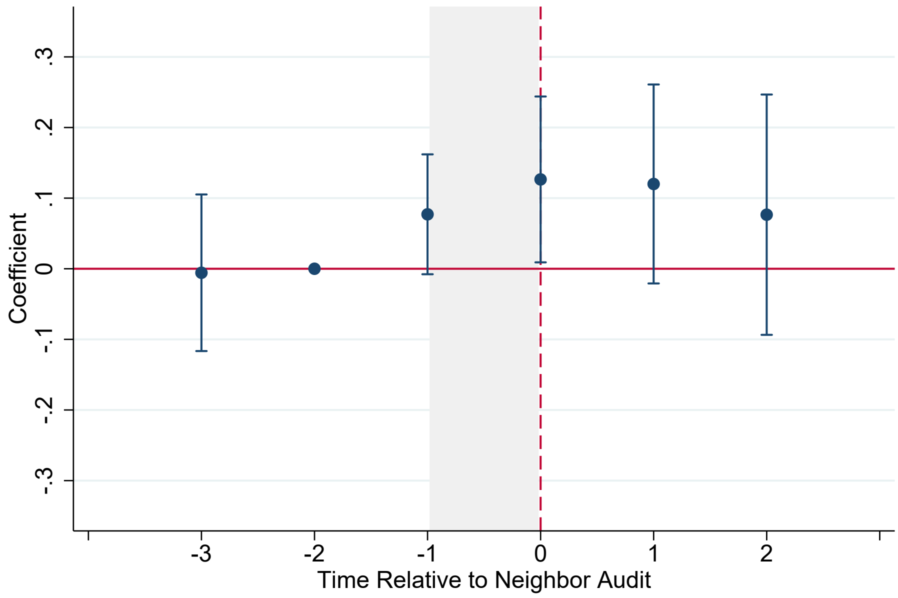
*Notes:* This table shows results from regressions analyzing a non-linear relation between the number of tax audits (within 100m radius) and the tax liability. In all specifications, the dependent variable is the sine-transformed tax liability which is winsorized at the top 99.9th percentile. In Column (1), a squared term for the number of neighboring audits is added to the regression, while in Columns (2) to (6), the sample is restricted by the total number of neighbor audits within the sample period 2009-2015. Column (2) excludes firms that have never experienced a neighbor audit. Columns (3) and (4) restrict the sample to firms with more than 19 and more than one neighbor audits. In the last two columns, the regression is conducted for firms with maximal one (Specification (5)) and exactly one (Specification (6)) neighbor audit. Standard errors are clustered by sector-year. ***,**,* indicate significance at the 1%, 5%, 10% levels.
*Source:* Own calculation based on SARS data.

Table 6: Effect dynamics

| Model | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Sample | All firms | All firms | # audits = 1 | All firms | # audits = 1 |
| Audits, year-1, 100m | 0.0115*** | 0.0095** | 0.1066*** | | |
| | (0.0039) | (0.0038) | (0.0342) | | |
| Audits, year-2, 100m | 0.0112*** | 0.0109*** | 0.0493* | | |
| | (0.0041) | (0.0039) | (0.0279) | | |
| Audits, year-3, 100m | 0.0074* | 0.0073 | 0.0051 | | |
| | (0.0044) | (0.0045) | (0.0256) | | |
| Audits, year-1, 100-500m | | 0.0059** | | | |
| | | (0.0023) | | | |
| Audits, year-2, 100-500m | | 0.0033 | | | |
| | | (0.0027) | | | |
| Audits, year-3, 100-500m | | 0.0032 | | | |
| | | (0.0046) | | | |
| Audits, year-1, 500-1000m | | 0.0030* | | | |
| | | (0.0017) | | | |
| Audits, year-2, 500-1000m | | 0.0011 | | | |
| | | (0.0024) | | | |
| Audits, year-3, 500-1000m | | 0.0005 | | | |
| | | (0.0024) | | | |
| Audits, year+1, 100m | | | | -0.0017 | 0.0105 |
| | | | | (0.0025) | (0.0210) |
| Audits, year+2, 100m | | | | 0.0016 | -0.0220 |
| | | | | (0.0023) | (0.0219) |
| Total assets (in log) | 0.2472*** | 0.2471*** | 0.2722*** | 0.2163*** | 0.2321*** |
| | (0.0077) | (0.0077) | (0.0098) | (0.0057) | (0.0093) |
| Observations | 1,327,512 | 1,327,512 | 121,643 | 1,565,305 | 152,738 |
| $R^2$ | 0.791 | 0.791 | 0.794 | 0.786 | 0.785 |

*Notes:* This table depicts the dynamic effects of tax audits of neighboring firms on tax liabilities from 2009-2015. In all specifications, the dependent variable is sine-transformed tax liability winsorized at the top 99.9th percentile. The regressions further include firm-, municipality-year- and sector-year fixed effects. Compared to the baseline specification, Column (1) and (2) present results from regressions that additionally include the number of neighbor audits that happened two ("year-2") and three ("year-3") years before the submission date of the tax return. In Column (3), only firms with exactly one neighbor audit over the sample period are considered. Columns (4) and (5) present placebo tests where the tax liability is regressed on the number of neighbor audits that happened one and two years after the return submission. Standard errors are clustered by sector-year. ***,**,* indicate significance at the 1%, 5%, 10% levels.
*Source:* Own calculation based on SARS data.

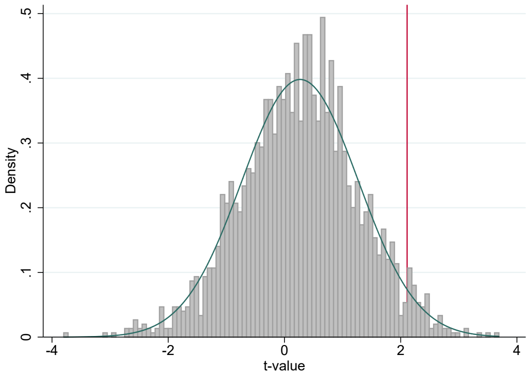
Figure 3: Results Event Study



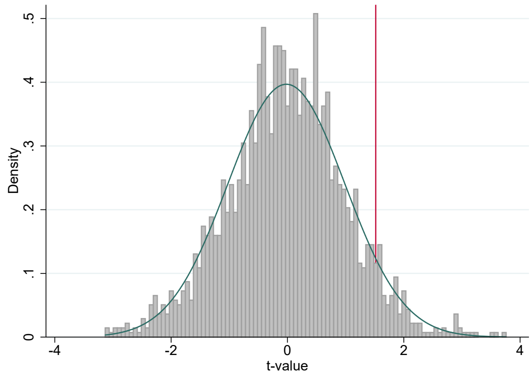

*Note:* This graph depicts the results (point coefficients and 95 percent confidence bands) of the event study as explained in Section 5.3. The dependent variable is the IHS-transformed tax liability which is winsorized at the top 99.9th percentile. The sample includes the subset of firms that experience exactly one neighboring tax audit and is further restricted to tax returns submitted after April 2010 to ensure that we observe at least three pre-intervention periods for all firms. The effect window goes from (-3,2) to display the spillover effect three years prior and two years after the treatment. The coefficient year -2 is left out and set as the base effect. Standard errors are clustered by sector-year.

*Source:* Own calculation based on SARS data.

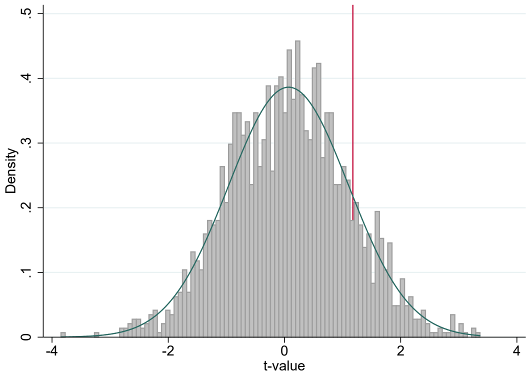
Figure 4: Results Randomization Inference



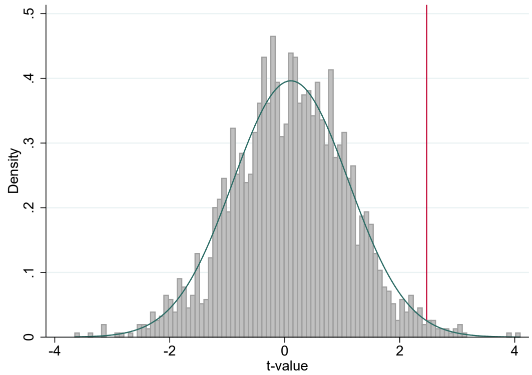

(a) All firms, 0-100m (municipality-year)



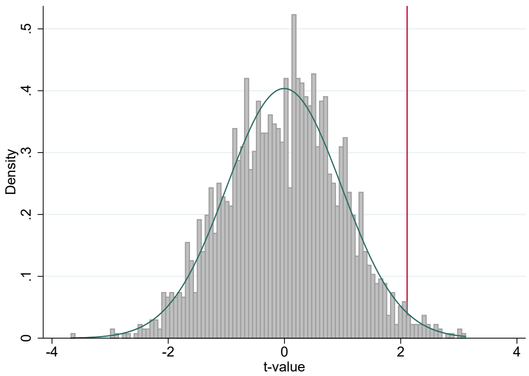

(b) All firms, 100-500m (municipality-year)



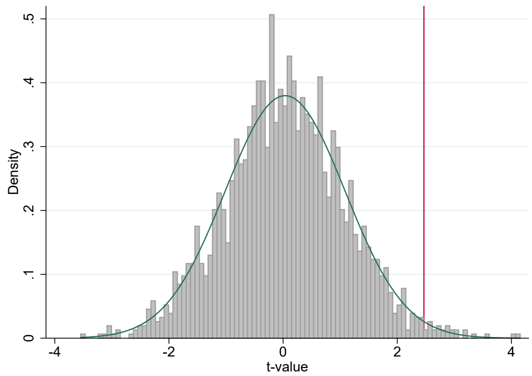

(c) All firms, 500-1000m (municipality-year)




(d) # audits = 1, 0-100m (municipality-year)




(e) All firms, 0-100m (province-year)




(f) # audits = 1, 0-100m (province-year)

*Note:* The six graphs depict the original t-statistic (vertical red lines) and randomization distributions (gray bars) with 2,000 permutations from estimating the effect of having a neighbor tax audit on the tax liability of the firm. Subfigures (a) to (d) present results where the treatment variable was permuted within municipality-year cells, while in Subfigures (e) and (f) province-year cells were used. In Subgraphs (d) and (f), only firms with exactly one neighbor audit are included. The respective t-statistic comes from the regressions shown in Column (2) of Table 3 and Column (6) of Table 5. The corresponding two-sided RI p-values for the treatment effects are 0.05, 0.13, 0.25, 0.02, 0.01, 0.08, respectively.

# 8 Channels

Table 7: Compliant vs non-compliant firms

| Model | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Sample | All firms | All firms | All firms | # audits = 1 | # audits = 1 |
| Compliant Audits, 0-100m | 0.0127*** | 0.0111*** | 0.0125*** | 0.0914*** | 0.0913*** |
| | (0.0041) | (0.0039) | (0.0042) | (0.0304) | (0.0304) |
| Non-Compliant Audits, 0-100m | -0.0022 | -0.0020 | | 0.0185 | |
| | (0.0053) | (0.0053) | | (0.0299) | |
| Compliant Audits, 100-500m | | 0.0047** | | | |
| | | (0.0020) | | | |
| Non-Compliant Audits, 100-500m | | -0.0011 | | | |
| | | (0.0038) | | | |
| Compliant Audits, 500-1000m | | 0.0014 | | | |
| | | (0.0017) | | | |
| Non-Compliant Audits, 500-1000m | | 0.0013 | | | |
| | | (0.0039) | | | |
| Audits with extreme auditresult, 0-100m | | | 0.0028 | | -0.0596 |
| | | | (0.0132) | | (0.0760) |
| Total assets (in log) | 0.2445*** | 0.2444*** | 0.2445*** | 0.2623*** | 0.2623*** |
| | (0.0073) | (0.0073) | (0.0073) | (0.0094) | (0.0094) |
| Observations | 1,910,074 | 1,910,074 | 1,910,074 | 188,227 | 188,227 |
| $R^2$ | 0.747 | 0.747 | 0.747 | 0.746 | 0.746 |

*Notes:* This table extends the baseline results by splitting the regressor of interest into neighbor audits of firms that experienced an upward adjustment in the course of the audit ("non-compliant") and firms that did not ("compliant"). Columns (3) and (5), only non-compliant audits with an extreme audit results, i.e. with an audit result larger than the 90th percentile, are considered. In all specifications, the dependent variable is sine-transformed tax liability which is regressed on the number of neighboring tax audits within the respective distance bands. Standard errors are clustered by sector-year. ***,**,* indicate significance at the 1%, 5%, 10% levels.
*Source:* Own calculation based on SARS data.

Table 8: Heterogeneity - same vs. other industry

| Model | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Sample | All firms | All firms | All firms | # audits = 1 | # audits = 1 |
| Audits, same ind, 0-100m | 0.0029 | | | 0.0834** | |
| | (0.0054) | | | (0.0363) | |
| Audits, other ind, 0-100m | 0.0090** | | | 0.0509* | |
| | (0.0038) | | | (0.0262) | |
| Audits, same ind, 0-500m | | 0.0061* | | | |
| | | (0.0034) | | | |
| Audits, other ind, 0-500m | | 0.0039** | | | |
| | | (0.0019) | | | |
| Audits, same ind, 500-1000m | | 0.0015 | | | |
| | | (0.0032) | | | |
| Audits other ind, 500-1000m | | 0.0016 | | | |
| | | (0.0015) | | | |
| Compliant Audits, same ind, 0-500m | | | 0.0075** | | |
| | | | (0.0032) | | |
| Non-Compliant Audits, same ind, 0-500m | | | -0.0002 | | |
| | | | (0.0049) | | |
| Compliant Audits, other ind, 0-500m | | | 0.0065** | | |
| | | | (0.0025) | | |
| Non-Compliant Audits, other ind, 0-500m | | | -0.0014 | | |
| | | | (0.0030) | | |
| Compliant Audits, same ind, 500-1000m | | | 0.0021 | | |
| | | | (0.0025) | | |
| Non-Compliant Audits, same ind, 500-1000m | | | -0.0028 | | |
| | | | (0.0079) | | |
| Compliant Audits, other ind, 500-1000m | | | 0.0004 | | |
| | | | (0.0020) | | |
| Non-Compliant Audits, other ind, 500-1000m | | | 0.0025 | | |
| | | | (0.0035) | | |
| Compliant Audits, same ind, 0-100m | | | | | 0.0940* |
| | | | | | (0.0530) |
| Non-Compliant Audits, same ind, 0-100m | | | | | 0.0695 |
| | | | | | (0.0643) |
| Compliant Audits, other ind, 0-100m | | | | | 0.0901** |
| | | | | | (0.0368) |
| Non-Compliant Audits, other ind, 0-100m | | | | | 0.0062 |
| | | | | | (0.0336) |
| Total assets (in log) | 0.2446*** | 0.2446*** | 0.2446*** | 0.2623*** | 0.2623*** |
| | (0.0073) | (0.0073) | (0.0073) | (0.0094) | (0.0094) |
| Observations | 1,909,674 | 1,909,674 | 1,909,674 | 188,204 | 188,204 |
| $R^2$ | 0.747 | 0.747 | 0.747 | 0.746 | 0.746 |

*Notes:* This table extends the baseline results by splitting the regressor of interest into neighbor audits that happen in firms operating in the same vs other sector(s) than firm under consideration. Columns (3) and (5), moreover, count the number of compliant/non-compliant audits within each group. In all specifications, the dependent variable is sine-transformed tax liability which is regressed on the number of neighboring tax audits within the respective distance bands. Standard errors are clustered by sector-year. ***,**,* indicate significance at the 1%, 5%, 10% levels.
*Source:* Own calculation based on SARS data.

Table 9: Heterogeneity - input-related vs. other firms

| Model | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Sample | All firms | All firms | All firms | # audits = 1 | # audits = 1 |
| Audits, input, 0-100m | 0.0166 | | | 0.0964** | |
| | (0.0107) | | | (0.0431) | |
| Audits, other, 0-100m | 0.0056* | | | 0.0472* | |
| | (0.0034) | | | (0.0245) | |
| Audits, input, 0-500m | | 0.0117** | | | |
| | | (0.0056) | | | |
| Audits, other, 0-500m | | 0.0025 | | | |
| | | (0.0017) | | | |
| Audits, input, 500-1000m | | 0.0081*** | | | |
| | | (0.0026) | | | |
| Audits, other, 500-1000m | | -0.0000 | | | |
| | | (0.0018) | | | |
| Compliant Audits, input, 0-500m | | | 0.0192*** | | |
| | | | (0.0062) | | |
| Non-Compliant Audits, input, 0-500m | | | -0.0032 | | |
| | | | (0.0060) | | |
| Compliant Audits, other, 0-500m | | | 0.0036 | | |
| | | | (0.0023) | | |
| Non-Compliant Audits, other, 0-500m | | | -0.0007 | | |
| | | | (0.0033) | | |
| Compliant Audits, input, 500-1000m | | | 0.0073 | | |
| | | | (0.0047) | | |
| Non-Compliant Audits, input, 500-1000m | | | 0.0059 | | |
| | | | (0.0073) | | |
| Compliant Audits, other, 500-1000m | | | -0.0006 | | |
| | | | (0.0022) | | |
| Non-Compliant Audits, other, 500-1000m | | | 0.0001 | | |
| | | | (0.0042) | | |
| Compliant Audits, input, 0-100m | | | | | 0.1072* |
| | | | | | (0.0607) |
| Non-Compliant Audits, input, 0-100m | | | | | 0.0835 |
| | | | | | (0.0605) |
| Compliant Audits, other, 0-100m | | | | | 0.0868*** |
| | | | | | (0.0334) |
| Non-Compliant Audits, other, 0-100m | | | | | 0.0008 |
| | | | | | (0.0322) |
| Total assets (in log) | 0.2445*** | 0.2444*** | 0.2444*** | 0.2623*** | 0.2623*** |
| | (0.0073) | (0.0073) | (0.0073) | (0.0094) | (0.0094) |
| Observations | 1,910,074 | 1,910,074 | 1,910,074 | 188,227 | 188,227 |
| $R^2$ | 0.747 | 0.747 | 0.747 | 0.746 | 0.746 |

*Notes:* This table extends the baseline results by splitting the regressor of interest into neighbor audits that happen in firms that are most probably connected via input vs noninput linkages. See Section 6.3 for more information. Columns (3) and (5), moreover, count the number of compliant/non-compliant audits within each group. In all specifications, the dependent variable is sine-transformed tax liability which is regressed on the number of neighboring tax audits within the respective distance bands. Standard errors are clustered by sector-year. ***,**,* indicate significance at the 1%, 5%, 10% levels.

*Source:* Own calculation based on SARS data.

Table 10: Heterogeneity - output-related vs. other firms

| Model | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Sample | All firms | All firms | All firms | # audits = 1 | # audits = 1 |
| Audits, output, 0-100m | 0.0086 | | | 0.0434 | |
| | (0.0085) | | | (0.0546) | |
| Audits, other, 0-100m | 0.0075** | | | 0.0630** | |
| | (0.0034) | | | (0.0252) | |
| Audits, output, 0-500m | | 0.0108** | | | |
| | | (0.0054) | | | |
| Audits, other, 0-500m | | 0.0027 | | | |
| | | (0.0021) | | | |
| Audits, output, 500-1000m | | 0.0029 | | | |
| | | (0.0036) | | | |
| Audits, other, 500-1000m | | 0.0014 | | | |
| | | (0.0016) | | | |
| Compliant Audits, output, 0-500m | | | 0.0137 | | |
| | | | (0.0094) | | |
| Non-Compliant Audits, output, 0-500m | | | 0.0063 | | |
| | | | (0.0051) | | |
| Compliant Audits, other, 0-500m | | | 0.0051** | | |
| | | | (0.0022) | | |
| Non-Compliant Audits, other, 0-500m | | | -0.0037 | | |
| | | | (0.0036) | | |
| Compliant Audits, output, 500-1000m | | | -0.0025 | | |
| | | | (0.0044) | | |
| Non-Compliant Audits, output, 500-1000m | | | 0.0097 | | |
| | | | (0.0073) | | |
| Compliant Audits, other, 500-1000m | | | 0.0019 | | |
| | | | (0.0021) | | |
| Non-Compliant Audits, other, 500-1000m | | | -0.0009 | | |
| | | | (0.0038) | | |
| Compliant Audits, output, 0-100m | | | | | 0.0891 |
| | | | | | (0.0688) |
| Non-Compliant Audits, output, 0-100m | | | | | -0.0142 |
| | | | | | (0.0730) |
| Compliant Audits, other, 0-100m | | | | | 0.0922*** |
| | | | | | (0.0354) |
| Non-Compliant Audits, other, 0-100m | | | | | 0.0290 |
| | | | | | (0.0382) |
| Total assets (in log) | 0.2445*** | 0.2444*** | 0.2444*** | 0.2623*** | 0.2623*** |
| | (0.0073) | (0.0073) | (0.0073) | (0.0094) | (0.0094) |
| Observations | 1,910,074 | 1,910,074 | 1,910,074 | 188,227 | 188,227 |
| $R^2$ | 0.747 | 0.747 | 0.747 | 0.746 | 0.746 |

*Notes:* This table extends the baseline results by splitting the regressor of interest into neighbor audits that happen in firms that are most probably connected via output vs non-output linkages. See Section 6.3 for more information. Columns (3) and (5), moreover, count the number of compliant/non-compliant audits within each group. In all specifications, the dependent variable is sine-transformed tax liability which is regressed on the number of neighboring tax audits within the respective distance bands. Standard errors are clustered by sector-year. ***,**,* indicate significance at the 1%, 5%, 10% levels.

*Source:* Own calculation based on SARS data.

Table 11: Heterogeneity - same tax practitioner

| Model | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Sample | All firms | All firms | All firms | # audits = 1 | # audits = 1 |
| Audits, same, 0-100m | 0.0074 | | | 0.0936 | |
| | (0.0144) | | | (0.0700) | |
| Audits, other/no, 0-100m | 0.0078** | | | 0.0551** | |
| | (0.0035) | | | (0.0249) | |
| Audits, same, 0-500m | | 0.0030 | | | |
| | | (0.0114) | | | |
| Audits, other/no, 0-500m | | 0.0046** | | | |
| | | (0.0020) | | | |
| Audits, same, 500-1000m | | -0.0027 | | | |
| | | (0.0326) | | | |
| Audits, other/no, 500-1000m | | 0.0016 | | | |
| | | (0.0015) | | | |
| Compliant Audits, same, 0-500m | | | 0.0013 | | |
| | | | (0.0122) | | |
| Non-Compliant Audits, same, 0-500m | | | 0.0068 | | |
| | | | (0.0166) | | |
| Compliant Audits, other/no, 0-500m | | | 0.0073*** | | |
| | | | (0.0021) | | |
| Non-Compliant Audits, other/no, 0-500m | | | -0.0017 | | |
| | | | (0.0030) | | |
| Compliant Audits, same, 500-1000m | | | -0.0229 | | |
| | | | (0.0363) | | |
| Non-Compliant Audits, same, 500-1000m | | | 0.0398 | | |
| | | | (0.0596) | | |
| Compliant Audits, other/no, 500-1000m | | | 0.0010 | | |
| | | | (0.0016) | | |
| Non-Compliant Audits, other/no, 500-1000m | | | 0.0012 | | |
| | | | (0.0038) | | |
| Compliant Audits, same, 0-100m | | | | | 0.0747 |
| | | | | | (0.1090) |
| Non-Compliant Audits, same, 0-100m | | | | | 0.1265 |
| | | | | | (0.1283) |
| Compliant Audits, other/no, 0-100m | | | | | 0.0929*** |
| | | | | | (0.0314) |
| Non-Compliant Audits, other/no, 0-100m | | | | | 0.0110 |
| | | | | | (0.0339) |
| Total assets (in log) | 0.2445*** | 0.2444*** | 0.2444*** | 0.2623*** | 0.2623*** |
| | (0.0073) | (0.0073) | (0.0073) | (0.0094) | (0.0094) |
| Observations | 1,910,074 | 1,910,074 | 1,910,074 | 188,227 | 188,227 |
| $R^2$ | 0.747 | 0.747 | 0.747 | 0.746 | 0.746 |

*Notes:* This table extends the baseline results by splitting the regressor of interest into neighbor audits that happen in firms with the same tax adivsor and firms with another/no tax advisor. See Section 6.3 for more information. Columns (3) and (5), moreover, count the number of compliant/non-compliant audits within each group. In all specifications, the dependent variable is sine-transformed tax liability which is regressed on the number of neighboring tax audits within the respective distance bands. Standard errors are clustered by sector-year. ***,**,* indicate significance at the 1%, 5%, 10% levels.

*Source:* Own calculation based on SARS data.

# Appendix

## A Different Levels of Clustering

Table 12: Main results (cluster id)

| Model | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Audits, 100m | 0.0078*** | 0.0069** | 0.0085*** | 0.0081*** | 0.0065** | 0.0061** |
| | (0.0028) | (0.0028) | (0.0027) | (0.0027) | (0.0028) | (0.0030) |
| Audits, 100-500m | | 0.0031 | 0.0041* | 0.0035* | 0.0029 | 0.0033 |
| | | (0.0022) | (0.0021) | (0.0021) | (0.0022) | (0.0023) |
| Audits, 500-1000m | | 0.0019 | 0.0019 | 0.0015 | 0.0019 | 0.0027* |
| | | (0.0014) | (0.0013) | (0.0013) | (0.0014) | (0.0014) |
| Total assets (in log) | 0.2445*** | 0.2444*** | 0.2470*** | 0.2445*** | 0.2442*** | 0.2438*** |
| | (0.0015) | (0.0015) | (0.0015) | (0.0015) | (0.0015) | (0.0015) |
| Observations | 1,910,074 | 1,910,074 | 1,910,074 | 1,910,074 | 1,903,933 | 1,906,263 |
| $R^2$ | 0.747 | 0.747 | 0.746 | 0.747 | 0.751 | 0.752 |
| Baseline controls | x | x | x | x | x | x |
| Firm FE | x | x | x | x | x | x |
| Sector-Year FE | x | x | | x | | |
| Municipality-Year FE | x | x | | | | |
| Municipality-Sector-Year FE | | | | | x | |
| Suburb-Year FE | | | | | | x |

*Notes:* This table reports the effects of tax audits of neighboring firms on tax liabilities from 2009-2015 for all firms. In all specifications, the dependent variable is the sine-transformed tax liability which is regressed on the number of neighboring tax audits within the respective distance bands. The dependent variable is winsorized at the top 99.9th percentile in all specifications. In Columns (1) and (2), firm-, municipality-year- and sector-year fixed effects are included. The combination of fixed effects is varied in Columns (3) to (6). Standard errors are clustered on the firm level. ***,**,* indicate significance at the 1%, 5%, 10% levels.
*Source:* Own calculation based on SARS data.

Table 13: Main results (cluster suburb)

| Model | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Audits, 100m | 0.0078** | 0.0069** | 0.0085** | 0.0081** | 0.0065* | 0.0061 |
| | (0.0036) | (0.0034) | (0.0034) | (0.0033) | (0.0034) | (0.0040) |
| Audits, 100-500m | | 0.0031 | 0.0041* | 0.0035 | 0.0029 | 0.0033 |
| | | (0.0022) | (0.0023) | (0.0022) | (0.0022) | (0.0024) |
| Audits, 500-1000m | | 0.0019 | 0.0019 | 0.0015 | 0.0019 | 0.0027* |
| | | (0.0014) | (0.0014) | (0.0014) | (0.0014) | (0.0014) |
| Total assets (in log) | 0.2445*** | 0.2444*** | 0.2470*** | 0.2445*** | 0.2442*** | 0.2438*** |
| | (0.0026) | (0.0026) | (0.0025) | (0.0026) | (0.0026) | (0.0025) |
| Observations | 1,909,922 | 1,909,922 | 1,909,922 | 1,909,922 | 1,903,780 | 1,906,263 |
| $R^2$ | 0.747 | 0.747 | 0.746 | 0.747 | 0.751 | 0.752 |
| Baseline controls | x | x | x | x | x | x |
| Firm FE | x | x | x | x | x | x |
| Sector-Year FE | x | x | | x | | |
| Municipality-Year FE | x | x | | | | |
| Municipality-Sector-Year FE | | | | | x | |
| Suburb-Year FE | | | | | | x |

*Notes:* This table reports the effects of tax audits of neighboring firms on tax liabilities from 2009-2015 for all firms. In all specifications, the dependent variable is the sine-transformed tax liability which is regressed on the number of neighboring tax audits within the respective distance bands. The dependent variable is winsorized at the top 99.9th percentile in all specifications. In Columns (1) and (2), firm-, municipality-year- and sector-year fixed effects are included. The combination of fixed effects is varied in Columns (3) to (6). Standard errors are clustered by suburb. ***,**,* indicate significance at the 1%, 5%, 10% levels.
*Source:* Own calculation based on SARS data.

Table 14: Main results (cluster municipality)

| Model | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Audits, 100m | 0.0078** | 0.0069** | 0.0085** | 0.0081** | 0.0065* | 0.0061 |
| | (0.0039) | (0.0034) | (0.0033) | (0.0032) | (0.0034) | (0.0038) |
| Audits, 100-500m | | 0.0031 | 0.0041* | 0.0035* | 0.0029 | 0.0033** |
| | | (0.0022) | (0.0025) | (0.0020) | (0.0022) | (0.0017) |
| Audits, 500-1000m | | 0.0019*** | 0.0019*** | 0.0015** | 0.0019*** | 0.0027*** |
| | | (0.0006) | (0.0007) | (0.0006) | (0.0007) | (0.0008) |
| Total assets (in log) | 0.2445*** | 0.2444*** | 0.2470*** | 0.2445*** | 0.2442*** | 0.2438*** |
| | (0.0076) | (0.0076) | (0.0076) | (0.0076) | (0.0078) | (0.0075) |
| Observations | 1,910,074 | 1,910,074 | 1,910,074 | 1,910,074 | 1,903,933 | 1,906,263 |
| $R^2$ | 0.747 | 0.747 | 0.746 | 0.747 | 0.751 | 0.752 |
| Baseline controls | x | x | x | x | x | x |
| Firm FE | x | x | x | x | x | x |
| Sector-Year FE | x | x | | x | | |
| Municipality-Year FE | x | x | | | | |
| Municipality-Sector-Year FE | | | | | x | |
| Suburb-Year FE | | | | | | x |

*Notes:* This table reports the effects of tax audits of neighboring firms on tax liabilities from 2009-2015 for all firms. In all specifications, the dependent variable is the sine-transformed tax liability which is regressed on the number of neighboring tax audits within the respective distance bands. The dependent variable is winsorized at the top 99.9th percentile in all specifications. In Columns (1) and (2), firm-, municipality-year- and sector-year fixed effects are included. The combination of fixed effects is varied in Columns (3) to (6). Standard errors are clustered by municipality. ***,**,* indicate significance at the 1%, 5%, 10% levels.
*Source:* Own calculation based on SARS data.

# B Further Robustness Analyses

Table 15: Robustness - Outlier

| Model | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | Winsorized at | | | | Trimmed at | | | |
| | 95th pctile | | 99th pctile | | 95th pctile | | 99th pctile | |
| Audits, 100m | 0.0065** | 0.0058* | 0.0074** | 0.0066** | 0.0032 | 0.0031 | 0.0055* | 0.0049* |
| | (0.0032) | (0.0031) | (0.0034) | (0.0032) | (0.0030) | (0.0028) | (0.0031) | (0.0029) |
| Audits, 100-500m | | 0.0027 | | 0.0029 | | 0.0005 | | 0.0020 |
| | | (0.0020) | | (0.0020) | | (0.0019) | | (0.0019) |
| Audits, 500-1000m | | 0.0016 | | 0.0017 | | 0.0000 | | 0.0013 |
| | | (0.0016) | | (0.0016) | | (0.0020) | | (0.0018) |
| Total assets (in log) | 0.2386*** | 0.2385*** | 0.2436*** | 0.2435*** | 0.1986*** | 0.1986*** | 0.2354*** | 0.2354*** |
| | (0.0070) | (0.0070) | (0.0072) | (0.0072) | (0.0058) | (0.0058) | (0.0069) | (0.0069) |
| Observations | 1,910,074 | 1,910,074 | 1,910,074 | 1,910,074 | 1,738,656 | 1,738,656 | 1,876,057 | 1,876,057 |
| $R^2$ | 0.741 | 0.741 | 0.746 | 0.746 | 0.700 | 0.700 | 0.737 | 0.737 |

*Notes:* This table shows the results from reestimating Specification (1) and (2) from Table **??** with different outlier corrections. In Columns (1) to (4), the data is winsorized at the 95th and 99th percentile, respectively. Columns (5) to (8) show results from trimmed data. Standard errors are clustered by sector-year. ***,**,* indicate significance at the 1%, 5%, 10% levels.

*Source:* Own calculation based on SARS data.

Table 16: Robustness - Sample and control restrictions

| Model | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Sample | Balanced panel | | No loss | | No spatial control | |
| Audits, 0-100m | 0.0103** | 0.0095** | 0.0093** | 0.0083** | 0.0071** | 0.0062* |
| | (0.0043) | (0.0040) | (0.0043) | (0.0041) | (0.0034) | (0.0032) |
| Audits, 100-500m | | 0.0049** | | 0.0033 | | 0.0033 |
| | | (0.0023) | | (0.0025) | | (0.0020) |
| Audits, 500-1000m | | 0.0006 | | 0.0026 | | 0.0019 |
| | | (0.0017) | | (0.0019) | | (0.0016) |
| Total assets (in log) | 0.2801*** | 0.2800*** | 0.2735*** | 0.2734*** | 0.2445*** | 0.2445*** |
| | (0.0092) | (0.0092) | (0.0076) | (0.0076) | (0.0073) | (0.0073) |
| Observations | 1,140,171 | 1,140,171 | 1,495,581 | 1,495,581 | 1,910,074 | 1,910,074 |
| $R^2$ | 0.738 | 0.738 | 0.717 | 0.717 | 0.747 | 0.747 |

*Notes:* This table shows robustness results from reestimating Specification (1) and (2) from Table **??**. Columns (1) and (2) only include firms which are observed in every year from 2009 to 2015. Columns (3) and (4) exclude firms that report losses in every year from 2009 to 2015. In Columns (5) and (6), the regression is run without controlling for the average taxable income within 0-100m and 100-1000m. Standard errors are clustered by sector-year. ***,**,* indicate significance at the 1%, 5%, 10% levels.
*Source:* Own calculation based on SARS data.

Table 17: Results PPML

| Model | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Sample | All firms | All firms | No spatial control | All firms | # audits = 1 | # audits = 1 |
| Audits, 0-100m | 0.0035** | 0.0035** | 0.0034** | | 0.0336** | |
| | (0.0015) | (0.0015) | (0.0015) | | (0.0136) | |
| Audits, 100-500m | | -0.0001 | -0.0000 | | | |
| | | (0.0009) | (0.0009) | | | |
| Audits, 500-1000m | | 0.0000 | 0.0000 | | | |
| | | (0.0007) | (0.0006) | | | |
| Compliant Audits, 0-100m | | | | 0.0038** | | 0.0326** |
| | | | | (0.0017) | | (0.0165) |
| Non-Compliant Audits, 0-100m | | | | 0.0023 | | 0.0352 |
| | | | | (0.0031) | | (0.0220) |
| Total assets (in log) | 0.1947*** | 0.1947*** | 0.1947*** | 0.1947*** | 0.1761*** | 0.1761*** |
| | (0.0271) | (0.0271) | (0.0272) | (0.0271) | (0.0268) | (0.0268) |
| Observations | 882,788 | 882,788 | 882,788 | 882,788 | 101,776 | 101,776 |

*Notes:* This table shows results from reestimating the most important specifications using the Poisson Pseudo-Maximum Likelihood (PPML) estimation technique. Please note that the algorithm failed to reach convergence due to the high number of fixed effects. In all specifications, standard errors are clustered by sector-year. ***,**,* indicate significance at the 1%, 5%, 10% levels.
*Source:* Own calculation based on SARS data.

Table 18: Heterogeneity - input- or output-related vs. other firms

| Model | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Sample | All firms | All firms | All firms | # audits = 1 | # audits = 1 |
| Audits, in- or output, 0-100m | 0.0091 | | | 0.0599 | |
| | (0.0084) | | | (0.0381) | |
| Audits, other, 0-100m | 0.0074** | | | 0.0599** | |
| | (0.0035) | | | (0.0266) | |
| Audits, in- or output, 0-500m | | 0.0080* | | | |
| | | (0.0045) | | | |
| Audits, other, 0-500m | | 0.0025 | | | |
| | | (0.0020) | | | |
| Audits, in- or output, 500-1000m | | 0.0060*** | | | |
| | | (0.0021) | | | |
| Audits, other, 500-1000m | | -0.0005 | | | |
| | | (0.0020) | | | |
| Compliant Audits, in- or output, 0-500m | | | 0.0132** | | |
| | | | (0.0061) | | |
| Non-Compliant Audits, in- or output, 0-500m | | | -0.0020 | | |
| | | | (0.0041) | | |
| Compliant Audits, other, 0-500m | | | 0.0036 | | |
| | | | (0.0026) | | |
| Non-Compliant Audits, other, 0-500m | | | -0.0011 | | |
| | | | (0.0037) | | |
| Compliant Audits, in- or output, 500-1000m | | | 0.0046 | | |
| | | | (0.0035) | | |
| Non-Compliant Audits, in- or output, 500-1000m | | | 0.0061 | | |
| | | | (0.0059) | | |
| Compliant Audits, other, 500-1000m | | | -0.0007 | | |
| | | | (0.0026) | | |
| Non-Compliant Audits, other, 500-1000m | | | -0.0012 | | |
| | | | (0.0041) | | |
| Compliant Audits, in- or output, 0-100m | | | | | 0.0796* |
| | | | | | (0.0475) |
| Non-Compliant Audits, in- or output, 0-100m | | | | | 0.0354 |
| | | | | | (0.0495) |
| Compliant Audits, other, 0-100m | | | | | 0.0985*** |
| | | | | | (0.0356) |
| Non-Compliant Audits, other, 0-100m | | | | | 0.0091 |
| | | | | | (0.0417) |
| Total assets (in log) | 0.2445*** | 0.2444*** | 0.2444*** | 0.2623*** | 0.2623*** |
| | (0.0073) | (0.0073) | (0.0073) | (0.0094) | (0.0094) |
| Observations | 1,910,074 | 1,910,074 | 1,910,074 | 188,227 | 188,227 |
| $R^2$ | 0.747 | 0.747 | 0.747 | 0.746 | 0.746 |

*Notes:* This table extends the baseline results by splitting the regressor of interest into neighbor audits that happen in firms that are most probably connected via input/output vs firms that are not directly connected via the value chain. See Section 6.3 for more information. Columns (3) and (5), moreover, count the number of compliant/non-compliant audits within each group. In all specifications, the dependent variable is sine-transformed tax liability which is regressed on the number if neighboring tax audits within the respective distance bands. Standard errors are clustered by sector-year. ***,**,* indicate significance at the 1%, 5%, 10% levels.
*Source:* Own calculation based on SARS data.