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Spatial Tax Enforcement Spillovers: Evidence from South Africa *

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Abstract

The purpose of this paper is to test for spatial enforcement spillovers. Empirical testing ground is the enforcement of business taxes in South Africa. The analysis relies on the population of business tax returns for the years 2009 to 2014 and data on all business taxpayer audits by the South African Revenue Services during that time period. The results suggest that audits significantly raise the tax reporting of non-audited neighboring firms. While the observed spillovers decline in geographic distance to the audited entity and are short-run in nature, the implied aggregate revenue gains are non-negligible. Additional analysis shows 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 roots in communication among taxpayers and is not driven by audit-related cost shocks to business partners.

Keywords: Taxation, audits, spillovers, neighbors

JEL Classification: H2, H7

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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 behaviour (Slemrod (2018)). Next to information structures (like third party reporting and verifiable paper trails), particular attention has been paid to tax payer audits and their role in enforcing tax obligations.

The design of effective audit systems, among others, requires knowledge on audit-related costs and benefits (Keen and Slemrod (2016)). The latter are not straight forward to quantify as audits - next to identifying evaded income and collecting unpaid taxes¹ - may also affect future income reporting and tax payments. Kleven et al. (2011a), Advani et al. (2017) and DeBacker et al. (2018) show that audits raise the post-audit tax reporting of audited taxpayers. In this paper, we present complementary evidence, which suggests that audits impact on the tax reporting of *non-audited* taxpayers. While studies on the topic are scarce (see the literature review below), theory suggests that such a link may exist: When fellows get audited, reporting behaviour might, for example, change because taxpayers update on their own expected audit and fine propensity or because audits and fines become salient.

The aim of this paper is to empirically test for such enforcement spillovers. Testing ground are business taxpayers in South Africa. The analysis draws on the population of corporate tax returns for the tax years 2009 to 2014, provided by the South African Revenue Services (SARS). This data is linked to information on all business taxpayer audits conducted by SARS between 2009 and 2015 and to data on bilateral distances between audited and non-audited firms, which allows us to model the propensity for communicative and business interactions between entities (see e.g. Jaffe et al. (1993), Combes et al. (2005), Combes et al. (2011) and Drago et al. (2018)).² In doing so, the analysis hence focuses on testing for potential spatial audit spillovers.

Methodologically, we estimate fixed effect models that compare changes in the tax reporting of firms with and without neighbors who are subject to business tax audits at

¹Related 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 to optimize these immediate revenue collections (e.g. Bloomquist (2013)).

²Distance impedes firm trade (Combes et al. (2005)) as well as 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. (2018)).

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 suburb-year fixed effects respectively (thus absorbing common shocks to 250 municipalities/10,000 suburbs in South Africa). Put differently, the estimation strategy compares the tax reporting of business taxpayers in the same municipality/suburb whose direct 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 region-industry-year fixed effects, which allow for possible SARS strategies that target specific industries in specific regions at a given point in time. All estimation models, furthermore, control for the average tax reporting of firms' direct corporate neighbors and are thus even able to absorb potential confounding effects related to locally refined tax reporting shocks and strategic audit responses.

We find evidence for positive audit spillovers on non-audited taxpayers in geographic proximity. If one additional fellow business in a 100 meter distance circle is audited, corporate tax payments are estimated to increase by around 1%. This positive effect steeply declines in geographic distance. If a fellow business in a 100 to 500 meter distance band gets audited, tax payments are estimated to increase by 0.5%. Still larger distance bands lead to a further decline in the estimated spillover effect. The observed effect is, moreover, short-run in nature. Quantitatively significant effects are centered around tax returns submitted in the first year after the audit and quickly subside for more distant post-audit periods. Placebo tests and sensitivity checks corroborate the validity of the estimation strategy and the robustness of the results. Finally, we find evidence for effect heterogeneity: Spatial audit spillovers are observed to be larger in less densely populated areas and the overall effect is shown to be driven by audits, in which taxpayers are *not* subject to an upward revision of their tax owed.³

The paper proceeds by discussing potential underlying mechanisms that may establish the result. Specifically, we argue that the findings may either root in taxpayer-to-taxpayer communication or may reflect behavioural adjustment in the wake of audit-related cost-shocks to business partners. If audited firms tell others about their audit experience, fellow taxpayers may, as described above, update on their own audit/fine

³The former result may reflect that spatial reference groups are smaller and local communication networks are more important in rural areas (see e.g. Drago et al. (2018)). Theoretical drivers of the result are discussed in the next paragraph.

propensity or may change their reporting behaviour because audits and fines become salient. If taxpayers are connected economically, behavioral incentives may, on top of that, change in the wake of audit-related cost shocks to connected parties 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 our observed empirical result pattern.

Considerations mainly build on the fact that observed enforcement spillovers are driven by audit cases, where taxpayers are cleared in the audit process. This is largely at odds with spillover effects that emerge through business interactions: 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 limited, 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.⁴ 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 out of social norm considerations: To avoid being exposed as a tax evader, they may only report on audits that did not result in an upward revision of their business tax payments. If that held true, neighbor responses would be limited to these cases, as observed in the data. Alternatively (or complementary), the result pattern is consistent with scenarios where 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 may, furthermore, have non-negligible revenue implications. Back-of-the-envelope calculation suggests that, within our sample period, audit spillovers have increased aggregate business tax revenues by around 222.5 million South African Rand (or 0.4% of the direct audit-related revenue yield). The results also offer insights for the design of optimal tax audit strategies. Specifically, tax authorities

⁴The 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. (2011a), Advani et al. (2017) and DeBacker et al. (2018)). Note that the first effect is limited to taxpayers that are identified as cheaters in the auditing process and experience an upward revision in the tax due. Empirical evidence, moreover, suggests that the latter effect is also larger, the larger the upward tax adjustment in the course of the audit (see e.g. Kleven et al. (2011a)).

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 impact on future tax revenues 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 audit case selection systems may raise audit-related revenue yields. As audit spillovers in our study are, on top of that, observed to be centered around audits that do not result in an upward revision of the audited businesses' tax payments, the findings imply that tax authorities, when targeting taxpayers for spillover considerations, should select firms that are expected to be tax compliant.

Our paper contributes to a flourishing literature on the link between tax enforcement activities and taxpayer behaviour. Theory suggests that taxpayers become more compliant when audit frequencies rise and fine rates increase (see Allingham and Sandmo (1972) for the seminal work). In recent years, several papers 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 convey 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 treated individuals' tax reporting relative to taxpayers in the control group (see e.g., Drago et al. (2001), Kleven et al. (2011b), 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. Two exceptions are Rincke and Traxler (2011) and Drago et al. (2018). 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 increased enforcement of these fees in their vicinity. In a complementary paper, Drago et al. (2018) 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. Our paper differs from these studies 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 ground are corporations rather than individuals and, third,

we study enforcement spillovers in an emerging economy context. The latter features link our paper to Pomeranz (2015) who studies value added taxation (VAT) in Chile and provides evidence for positive spillovers of VAT enforcement within production chains. While Pomeranz (2015), in line with our study, finds evidence for enforcement spillovers, her findings are suggested to root in information generated through 'paper trails' established by the VAT system.⁵

The rest of the paper is structured as follows: Section 2 presents information on institutional background and data. The empirical identification strategy is outlined in Section 3. Section 4 presents results and Section 5 concludes.

2 Institutional Background and Data

The aim of this paper is to test for spatial spillover effects of taxpayer audits on the tax reporting of non-audited taxpayers. Our testing ground is business taxation in South Africa. In the following, we describe the institutional background of the study and the data used for the empirical analysis.

2.1 Institutional Background

South Africa is an upper-middle-income economy with a gross domestic product (GDP) per capita of 5,744 US dollars in 2015. The country's overall tax capacity is high compared to other developing economies: In 2015, its tax-to-GDP ratio amounted to 29% and thus well exceeded the tax-to-GDP ratio of other countries on the African continent (which amounted to 19.1% in 2015, see OECD Statistics (2018)). The country's tax capacity, nevertheless, still fell short of developed-country levels (- the average tax-to-GDP-ratio in the OECD amounted to 34% in 2015). Similar to other less developed economies, corporate taxes are, however, an important revenue source in South Africa as indicated by a high corporate-tax-to-GDP ratio of 4.7% (OECD average: 2.7%). See Lediga et al. (2018) for further details.

Income taxes are levied by the national government of South Africa under the Income Tax Act 58 of 1962. During our sample period (2009 to 2015), businesses in South

⁵Also 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 for spillovers of tax evasion behaviour in social networks (see e.g. Paetzold and Winner (2016), Alm et al. (2017), Frimmel et al. (2018) and Alstadster et al. (2018)).

Africa were taxed at a proportional tax rate of 28 percent. Since 2001, the business tax system furthermore granted lower business tax rates to so-called 'Small Business Corporations' (SBCs). To qualify as an SBC, a company must meet specific criteria. These include that the businesses' gross income does not exceed R20 million (R14 million prior to the 2013 tax year) as well as that the company's shareholders are natural persons and do not own shares in any other company, except other SBCs. Entities classified as personal service providers in the tax law, moreover, do not qualify for the SBC regime. A table with the SBC tax schedule in our sample years is presented in Table B1 in the Appendix.

2.2 Data and Descriptive Statistics

To establish the causal effect of business taxpayer audits on non-audited taxpayers in geographic vicinity, we combine three datasets.

First, the analysis draws on the population of business tax returns in South Africa provided by the South African Revenue Services (SARS) for the tax year 2009 to 2014. The data comprises around 3.2 million tax returns from 774,112 firms (where 759,127 of these firms were non-SBCs in all or a fraction of our sample years). Since we are interested in determining the effect of taxpayer audits on firm behaviour, our main variables of interest are the taxpayers' reported taxable income and the tax owed (both reported in South African Rand). To isolate audit spillovers on non-audited entities and avoid results that are driven by 'own-audit-effects', we disregard firms that were themselves audited during our sample period. Figure 1 present kernel densities of firms' tax payments and taxable income for all sample years. Table 1 reports descriptive statistics.⁶ Note that for each tax return, we observe the financial year covered by the return (which may vary across firms but ends in February for a large fraction of the entities) as well as the date at which the return was processed by SARS (which commonly corresponds to the date of submission of the tax return or a couple of days after). 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.

This data is linked to information on bilateral distances between entities, which was generated from precise geo-coordinates of firms' geographic location. This allows us to identify taxpayers that are geographic neighbors. Precisely, for each tax return r

⁶Note that our data only includes information on business tax payers' tax owed. In the following, we will also refer to the tax owed as 'tax payments'.

submitted by business taxpayer i at time t , we calculate the number of audited firms in a given distance band

$$NrAu_{rit} = \sum_{j=1}^J D_j \cdot A_{jt} \quad (1)$$

where D_j is an dummy variable indicating that firm $j \neq i$ is located within a given distance band of taxpayer i and A_{jt} indicates that firm j was subject to an audit at time t . In the baseline analysis, we define firms to be neighbors if they are located within a distance band of 100 meters. Robustness checks add regressors for audited firms in larger distance of 100-500 meters and 500-1000 meters. As sketched in the Introduction and further spelled out in the method part below, our analysis hence focuses on spatial spillover effects of taxpayer audits. This follows the notion that geographic distance shapes the intensity of communicative and business interactions (see e.g. Jaffe et al. (1993), Combes et al. (2005) and Drago et al. (2018) as well as Footnote 2). For additional analyses, spelled out in detail below, we, moreover, refine the definition of (2) and only sum up those audited neighbors that feature certain characteristics, indicated by F_{jit} , e.g. referring to i and j being in the same industry.

$$NrAuF_{rit} = \sum_{j=1}^J F_{jit} \cdot D_j \cdot A_{jt} \quad (2)$$

Complementarily, we will calculate the number of neighboring firms in a given distance band $Nr_{it} = \sum_{j=1}^J D_j$ and the average taxable income \overline{I}_{it} and average tax owed $\overline{\tau}_{it}$ of neighboring firms in a given distance band

$$\overline{\tau}_{it} = \frac{\sum_{j=1}^J D_j \cdot \tau_j}{\sum_{j=1}^J D_j}, \quad \overline{I}_{it} = \frac{\sum_{j=1}^J D_j \cdot I_j}{\sum_{j=1}^J D_j}$$

where τ_j and I_j indicate the tax owed and taxable income reported by firm $j \neq i$. These variables will be used as control regressors in the empirical estimation strategy as explained below. Descriptive statistics are reported in Table 1.

To determine A_{jt} , we, moreover, make use of all business tax audits conducted by SARS between 2009 and 2015. In total, we observe the audit of 28,588 business tax returns within that time frame. After dropping outliers in the highest and lowest percentile of the distribution, the average adjustment per audit was 386,475.5 South African Rand; 41.5% of the audits were subject to an upward adjustment. 2.7% of the audits resulted in a downward revision. Also see SARS' Annual Reports for more

information on SARS audit activities and direct revenue collections from the audits. Our data comprises information on the audited entity, 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.⁷ Furthermore note that SARS, similar to tax authorities in other countries, operates audit case selection systems, where tax returns are assigned for audits based on characteristics that correlate with expected levels of evaded taxable income. This may give rise to endogeneity issues when identifying the causal link between taxpayer audits and the tax reporting of non-audited fellow businesses that are discussed in detailed in the following section.

Finally note that information on the date of closure of an audit case and the processing date of tax returns at SARS allows us to identify those tax returns that were submitted to SARS within a given time frame after a specific audit. In the baseline analysis, $NrAu_{it}$ accounts for all neighbor audits A_{jt} that took place within a one-year time-span before the submission of the considered tax return. The time indicator t hence refers to the year in which the considered tax return was submitted. Additional analyses will account for the number of audits that took place two-years and three years before the submission of the considered tax return respectively. This allows us to investigate the dynamics of spatial taxpayer spillovers.

3 Empirical Strategy

As described above, the purpose of this paper is to test for spillover effects of taxpayer audits on the tax payments and income reporting of neighboring firms. Observational unit is the tax return r for tax year ℓ , submitted by firm i at time t . The estimation model takes on the following form

$$y_{r,i,m,k,t,\ell} = \alpha_1 + \alpha_2 NrAu_{r,i,t} + \alpha_3' X_{i,t} + \rho_{m,k,t} + \mu_i + \phi_\ell + u_{i,m,k,t,\ell}. \quad (3)$$

⁷Furthermore note that our analysis does not account for so-called small-E audits, which were implemented by SARS after 2011 and 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. Such audits do not involve the trained auditors. Potentially submitted supplementary material is checked by administrative staff only.

where $y_{i,m,k,t,\ell}$ depicts the tax and income reported in tax return r submitted by firm i that operates in industry k and is located in region m . The regressor of main interest is $NrAu_{r,i,t}$, which captures the number of neighboring firms that experienced an audit in a given time frame before the submission of the considered tax return (see the definition above). In the baseline analysis, the definition of $NrAu_{r,i,t}$ accounts for neighbors in a distance radius of 100 meters and for audits within a year-span before the date of submission of the considered return r .⁸ In additional models, the neighbor definition allows for other distance bands and we, additionally, determine dynamic effects by including variables that capture neighbor audits two and three years (366-730 and 731-1095 days) before the submission date of the considered return respectively.

The coefficient of main interest is α_2 that captures the effect of neighbor audits on the tax and income reported on return r of firm i at time t . 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 practice, selects audit cases on a risk basis, implying that tax returns which feature characteristics that point to expected irregularities and evaded income are audited at higher rates. If risk-based targeting occurred on a regional basis, our identification strategy might be affected. Consider, for example, the case where firms in a given region are subject to a negative 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 α_2 might capture both, the audit effect plus region specific tax reporting trends (- in post-audit periods, tax payments might, due to mean reversal, e.g. grow at systematically higher rates than in other regions).

Our estimation strategy accounts for this concern: We define neighboring firms based on data on bilateral distances between audited and non-audited entities. In the baseline analysis, we define neighbors to be firms within a 100 meter distance-band of the considered business. 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 suburb-year fixed effects respectively that account for common shocks to the 250 South Africa municipalities and 10,000 suburbs respectively. Put differently, the model compares changes in the tax reporting of firms located in the

⁸The construction relies on the date when return r was processed by SARS (which commonly is 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 $NrAu_{r,i,t}$ if the gap between the two dates is less than 365 days.

same municipality and suburb respectively whose direct neighbors are and are not subject to tax audits by SARS. To further refine the analysis, we also estimate models with region-industry-year fixed effects, which allow for systematic targeting of 2-digit industries in given municipalities by SARS.⁹ The models, furthermore, include control variables for the number of direct neighbors within a given distance radius as well as the average tax reporting of these neighbors at a given point in time. The latter regressors are suited to absorb potential confounding effects related to potential SARS targeting of firms at very refined geographic levels, conditional on their tax reporting. Finally note that all specifications account for a full set of fixed effects indicating the year of the tax return.

In additional analyses, we refine the estimation model and additionally 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.¹⁰ The estimation model then reads

$$y_{r,i,m,k,t,\ell} = \beta + \sum_n \sum_q \delta_q^n NrAu_{r,i,t-n}^q + \gamma' X_{i,t} + \rho_{m,k,t} + \mu_i + \epsilon_{i,m,k,t,\ell}, \quad (4)$$

where q indicates the distance bands, with $q \in \{0-100m, 100-500m, 500-1000m\}$ and n indicates the year-lag between the tax return processing date and the closure of the audit, with $n \in \{1, 2, 3\}$. The definition of the other variables and the estimation strategy remain unchanged. Finally note that the calculation of standard errors accounts for clustering at the municipality level in all baseline models. 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 Results

The baseline results are presented Tables 2 and 3. Standard errors account for clustering at the municipality level and are presented in parentheses. The dependent variable in Table 2 is the log of firms' tax due (where cases of zero tax payments are kept in the sample and treated as zeros, see also Brockmeyer et al. (2018)) and the level of the tax

⁹Put 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.

¹⁰Precisely, 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.

owed and the taxable income reported in Table 3 (where outliers with taxable income and tax owed above the 99th percentile respectively as well as taxable income below the 1st percentile of the sample distribution are dropped in the latter specifications). Specification (1) in Panel A estimates the model in Equation (3) with firm fixed effects and the set of control variables spelled out above. Specification (2) of Panel B adds municipality-year and industry-year fixed effects and hence controls for municipality and industry-specific trends in tax reporting. Panel C replaces the latter set of fixed effects with a full set of fixed effects at the municipality-2 digit industry-year level and Panel D reports analogous results for specifications with a full set of suburb-year fixed effects and industry-year fixed effects.

In the baseline specifications, the calculation of the $A_{r,i,t}$ regressor refers to neighbors within a 100 meter distance radius that got audited within a one-year frame before the submission of the considered tax return. The results in Column (1) of all panels suggest that neighbor audits significantly raise the tax reporting of firms. Quantitatively, Specification (1) of Panel A suggests that one additional neighbor audit raises the firms' reported tax payments by around 1.4%. Qualitatively and quantitatively similar results are derived in specifications with region-year and industry-year fixed effects (as well as the combination of the two).

Specifications (2) and (3) add additional regressors for the number of neighbor audits within a 100-500m and a 500m-1km distance radius (in a one-year time frame before the submission of the considered tax return) respectively. In line with intuition, the coefficient estimates capturing the effect of taxpayer audits in larger distance to the considered firm turn out to be quantitatively smaller than the baseline estimate for the 100 meter distance radius, albeit statistically significant. Table 3 reestimates the baseline specifications in Table 2, using the level of firms' tax payments and taxable income (outlier-corrected at the 1%-level, see above) as dependent variable. This yields results that are comparable to our baseline estimates. Quantitatively, one additional audited taxpayer in a 100 meter distance radius is suggested to raise firms' reported taxable income and tax payments due by around 261 and 38 South African Rand respectively (cf. Specifications (1) and (4) of Panel D in Table 3).

Table 4, moreover, reestimates the specifications in Panel C of Table 2 (including municipality-industry-year fixed effects) in a dynamic setting, implying that the specifications comprise regressors that capture the number neighbors in a given distance band that were audited one, two and three years before the submission of the tax return respectively.¹¹ Specifications (1)-(3) include regressors for the two-year lag audits,

¹¹Specifically, the one/two/three year lag is defined as the number of audits that were closed 0-

Specifications (4)-(6) regressors for the two and three year lag audits. Table A1 in the appendix shows that similar results emerge if we drop the industry-region-year fixed effects or account for suburb-year and industry-year fixed effects instead.

In Table 5, we test for effect heterogeneity differentiating between taxpayers that live in areas of different economic density. The underlying presumption is that one additional taxpayer audit may have a larger impact on the tax reporting of neighboring firms in areas that are characterized by limited economic density relative to dense places with a lot of firm activity. Specifically, the interaction between individual taxpayers (in terms of communication as well as business interactions) is likely a declining function of the number of taxpayers located in a given area. While our baseline specifications include the number of firms within a given distance band as control variable (see above), the specifications in Table 5 reestimate the baseline models in Panel C of Table 2 separately for all firms (Specifications (1), (4) and (7)), firms with less than 700 neighbors in a one kilometer distance radius (Specifications (2), (5) and (8)) and less than 300 firms in a one kilometer distance radius (cf. Specifications (3), (6) and (9)). In line with intuition, we find that the effect of additional tax payer audits on firms' business tax payments grows larger the lower the density of business activity in a given area (as determined by the number of firms). See also Table A2 in the appendix for specifications that control for suburb-year fixed effects.

Table 6, moreover, presents a placebo test. Specifically, we redefine the dependent variable to take on values two years prior to the audit closure.¹² In line with expectation, the coefficient estimate for all audit-variables turn out small (substantially smaller than the baseline estimates) and do not statistically differ from zero. Again, the findings are confirmed in Table A3 in the appendix, where we rerun the same specifications but with a different modelling of the region-year and industry-year regressors.

Table 7 redefines the $A_{r,i,t}$ -regressor in two variables, where one counts the number of neighboring businesses in a given distance radius (100 meter) that were subject to a SARS audit at a given point in time and experienced an upward revision of their tax owed and the other counts the number of neighboring businesses that were subject to a SARS audit and did not experience an upward revision of their tax owed. The

365/366-730/731-1095 days before the submission of the considered return.

¹²Note that we present specifications where the dependent variable is lagged by two years (rather than one year) as our data only includes information on the closure date of the audit but not on the issue date. As anecdotal evidence as well as a subset of cases for which this data is available in our files suggest that there is a non-negligible number of audit cases with more than a one-year gap between issue and closure date of the audit, a two-year gap is a rare exception.

results in Table 7 suggest that the positive link between neighbor audits and firms' tax reporting is largely driven by audits where taxpayers did not experience an upward adjustment in their tax owed. This result is confirmed in Table A4 in the Appendix, where we reestimate the specifications in Table 7 but control for a full set of suburb-year and industry-year fixed effects. Similar results, moreover, emerge when we - next to differentiating between audits with and without an upward revision in the tax due - differentiate between audits of firms that do and do not operate in the same 2-digit industry as the taxpayer under consideration. The result pattern again suggests that positive effects of neighbor audits on firms' tax reporting are clustered around audits that did not result in an upward revision of the taxpayers' tax due. 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.

To obtain a tentative notion of the importance of different underlying mechanisms that may give rise to the observed audit spillovers, we will, in the following, compare the observed result pattern with the theoretical predictions of different transmission channels. As sketched in the Introduction, the link between neighbor audits and businesses' tax reporting may, broadly speaking, either root in taxpayer-to-taxpayer communication or in business interactions between firms. Taxpayer-to-taxpayer communication predicts that audited business owners may tell other taxpayers in their local network about their audit experience. The latter may then update on their own propensity to be selected for an audit and their expected fine rates, which may result in adjustments of their reporting behaviour. Alternatively, behavioural responses may emerge because fines and audits become salient when observed with others.

As sketched above, it may be business owners who communicate with each others 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 interactions of employees of the businesses (either through personal and business relations) or through employer changes. To the extent that the communication of audit experiences is a voluntary act, the propensity to tell others about an audit experience may correlate with characteristics of the audit. Most importantly, if tax evasion is no socially accepted behaviour, audited taxpayers may, out of social norm considerations, have a higher propensity to tell others about their audits if they were cleared in the audit and the audit did *not* identify them as a tax evader.¹³ If that held true, behavioural responses to neighbor

¹³A social norm represents a pattern of behaviour 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 be-

audits would expected to be quantitatively stronger, or even limited, to taxpayer audits that did not result in upward revisions of the tax payments due.¹⁴

The result pattern is, in turn, largely at odds with spillover effects caused by business interactions. In this dimension, spillovers could, from a theoretical perspective, emerge through two channels: Firstly, the audit may be a positive cost shock to competitors or business partners, which may change the behaviour of connected parties. Specifically, such cost shocks may relate to adjustments in the tax payments due in the audit and related fine payments or to potential increases in tax payments in post-audit periods (Kleven et al. (2011a), Advani et al. (2017) and DeBacker et al. (2018)). Note that the former costs are limited to audit cases which result in positive adjustments in the tax payments due (i.e. where audited taxpayers are identified to have evaded taxable income) and that evidence suggests that the latter effect is also larger, the higher the direct upward revision of the tax owed in the course of the audit (see e.g. Kleven et al. (2011a)). 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. As we observe the reverse result pattern, the findings suggest that this effect is of minor importance.¹⁵

Alternatively, tax authorities might obtain evasion-relevant information in the course of the audit. It is less obvious that such information is obtained w.r.t. business taxation than, for example, with regard to value added taxation that leaves a paper trail and involves third-party reported information. Even if such information is obtained, it is, from our perspective, plausible to assume that the distribution of such information is

have) according to some socially accepted mode of behaviour, then the individual will (not) behave appropriately. See, for example, Alm et al. (1999).

¹⁴Note that the line of argumentation presupposes that observing audits of fellow taxpayers raises tax reporting through upward adjustments of expected audit and fine propensities or salience effects. In theory, the reverse effects might also apply: If taxpayers expect audit resources to be fixed, they may expect that their propensity to be subject to an audit is reduced when fellow taxpayers are observed to be subject to an audit. If that held true, we would, however, expect to see negative spillovers of audits on the tax reporting of neighboring firms, which we do not.

¹⁵For 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 connected within production chains, cost shocks may also negatively impact on firm profits (and hence tax payments), making the overall effect ambiguous. Note, however, that even for firms operating in the same industry (which are hence most likely competitors), we observe a zero effect of the number of neighbor audits on tax reporting (suggesting that effects related to cost shocks and business relations are indeed negligible).

skewed towards audit cases where the audited firm is found to be evading income. This would again suggest that spillover effects are stronger for audit cases where audited taxpayers face upward adjustments in their tax due. As this contrasts our empirical result pattern, the data speaks against the importance of this transmission channel.

Concluding, we presented evidence for statistically significant spatial spillover effects of business taxpayer audits on non-audited taxpayers. Note that the results also turn out to be quantitatively relevant. The findings suggest that one additional audited firm within a 100 meter distance radius raises the reported tax payments by 20.83 South African Rand and one additional taxpayers within a 100-500 meter distance band raises tax reporting by 17.93 South African Rand (cf. Table 3, Panel A, Column (3)). Evaluated at the sample mean (where firms have 131 and 291 neighboring firms in the respective distance bands), simple back-of-the-envelope calculation suggests that the average spillover-related revenue gain per audit amounts to 7946 South African Rand. For the 28,588 audits within our sample frame, audit spillovers are hence suggested to have yielded 222.5 Million South African Rand in additional tax revenues (which corresponds to around 0.4% of the around 56.8 billion South African Rand in additional tax revenue collected through direct audit adjustments within our sample frame).

5 Summary and Conclusion

In this paper, we provided evidence that business taxpayer audits exert a positive and significant effect on the post-audit tax reporting of spatial neighbors. The empirical analysis relied on business tax return and audit data from South Africa. Quantitatively, the results suggest that the audit of a close geographic neighbor raises corporate tax reporting by around 1%, on average. This translates in 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 businesses' tax due, 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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6 Figures and Tables

Table 1: Descriptive Statistics					
Variable	No. of Obs.	Mean	Std. Dev.	Min	Max
Log (Tax Payments)	3,171,900.	. 2.3444	4.5969	0	21.0120.
Tax Payments, Outlier Corr.	3,003,391.	4857.41	17,042.08	0	129,003.3
Taxable Income, Outlier Corr.	2,844,350	-26,025.11	166,358	-871,445	509,729
Audited, Year1, 100m	3,171,900	0.7400	2.3861	0	45
Audited, Year1, 500m	3,171,900	1.2660	3.2865	0	72
Audited, Year1, 1km	3,171,900	2.6071	5.5618	0	114
Audited, Year2, 100m	3,171,900	0.6614	2.1900	0	45
Audited, Year2, 500m	3,171,900	1.1603	3.0268	0	70
Audited, Year2, 1km	3,171,900	2.3769	5.0606	0	104
Audited, Year3, 100m	3,171,900	0.4896	1.6727	0	32
Audited, Year3, 500m	3,171,900	0.8617	2.3051	0	43
Audited, Year3, 1km	3,171,900	1.7539	3.7427	0	53
Audited, Year1, 100m, Positive	3,171,900	0.3010	1.1087	0	23
Audited, Year1, 500m, Positive	3,171,900	0.4794	1.2810	0	28
Audited, Year1, 1km , Positive	3,171,900	0.9948	2.0360	0	35
Not Audited 100m	3,171,900	130.6857	318.7944	0	5543
Not Audited 500m	3,171,900	290.6024	522.601	0	6649
Not Audited 1km	3,171,900	603.9591	795.2031	0	8711
Log Total Assets	3,171,900	6.9618	6.9871	0	27.52488

Table 2: Baseline Results - Audit Spillovers on Neighboring Firms			
(Dependent Variable: Log Tax Payments)			
	(1)	(2)	(3)
Audited, Year1, 100m	0.0138*** (0.0049)	0.0117*** (0.0046)	0.0111*** (0.0045)
Audited, Year1, 500m		0.0078*** (0.0014)	0.0054*** (0.0013)
Audited, Year1, 1km		.	0.0031*** (0.0009)
Observations	3,171,900	3,171,900	3,171,900
Base Controls	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes
Audited, Year1, 100m	0.0146*** (0.0043)	0.0128*** (0.0040)	0.0123*** (0.0039)
Audited, Year1, 500m		0.0069*** (0.0013)	0.0048*** (0.0011)
Audited, Year1, 1km		.	0.0032*** (0.0008)
Observations	3,107,484	3,107,484	3,107,484
Base Controls	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes
Mun.-Year Fixed Effects	Yes	Yes	Yes
Ind.-Year Fixed Effects	Yes	Yes	Yes
Audited, Year1, 100m	0.0145*** (0.0044)	0.0126*** (0.0041)	0.0121*** (0.0039)
Audited, Year1, 500m		0.0075*** (0.0012)	0.0051*** (0.0011)
Audited, Year1, 1km		.	0.0038*** (0.0007)
Observations	3,100,002	3,100,002	3,100,002
Base Controls	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes
Mun.-Ind.-Year Fixed Effects	Yes	Yes	Yes
Audited, Year1, 100m	0.0148*** (0.0048)	0.0132*** (0.0046)	0.0125*** (0.0043)
Audited, Year1, 500m		0.0093*** (0.0019)	0.0063*** (0.0013)
Audited, Year1, 1km		.	0.0058*** (0.0009)
Observations	3,097,029	3,097,029	3,097,029
Base Controls	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes
Mun.-Ind.-Year Fixed Effects	Yes	Yes	Yes

Notes: Robust standard errors that account for clustering at the municipality level in parentheses. ***, **, * indicate statistical significance at the 1%, 5% and 10% level respectively. The baseline controls comprise a size control (the log of firms' total assets), a full set of fixed effects indicating the year when the tax return was submitted, a full set of fixed effects indicating the tax year of the return and control variables for the log of the number of firms in 100m, 500m and 1km radius around the considered taxpayer as well as dummies indicating the deciles of the average taxable income submitted by firms within a 100m/500m/1km radius of the considered taxpayer. Specifications (1) includes the described control variables for the 100m radius, Specifications (2) for the 100m and 500m radius and Specifications (3) for the 100m, 500m and 1km radius.

Table 3: Alternative Dependent Variables

	Tax Payments			Taxable Income		
	(1)	(2)	(3)	(4)	(5)	(6)
Audited, Year1, 100m	28.67** (12.12)	21.42* (11.99)	20.83* (11.84)	502.92*** (108.56).	434.96*** (102.21)	422.05** (101.45)
Audited, Year1, 500m		21.04*** (7.86)	17.93** (8.77)		200.29** (91.48)	174.14 (106.86)
Audited, Year1, 1km			3.77 (5.44)			59.56* (35.55)
Observations	2,993,308	2,993,308	2,993,308	2,823,440	2,823,440	2,823,440
Base Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Audited, Year1, 100m	31.39*** (10.83)	25.10** (11.12)	24.36** (11.18)	359.80*** (116.90)	306.84*** (110.80)	299.54*** (109.38)
Audited, Year1, 500m		17.15** (7.66)	14.13 (8.77)		141.89 (87.54)	115.28 (99.08)
Audited, Year1, 1km			4.82 (4.85)			62.20* (32.72)
Observations	2,930,177	2,930,177	2,930,177	2,762,064	2,762,064	2,762,064
Base Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Mun.-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Ind.-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Audited, Year1, 100m	30.66*** (11.46)	24.19** (11.76)	23.09** (11.74)	341.75*** (113.24)	292.53*** (108.00)	286.76*** (106.44)
Audited, Year1, 500m		18.62*** (7.25)	14.18 (8.73)		116.18 (88.28)	88.66 (95.96)
Audited, Year1, 1km			7.01 (4.56)			64.97** (30.84)
Observations	2,922,575	2,922,575	2,922,575	2,754,329	2,754,329	2,754,329
Base Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Munc.-Ind.-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Audited, Year1, 100m	37.72*** (11.58)	31.67*** (11.99)	30.10*** (11.70)	260.88* (138.93)	221.53 (138.76)	215.93 (135.18)
Audited, Year1, 500m		30.44*** (6.26)	24.14*** (8.85)		193.06** (87.09)	160.52 (100.22)
Audited, Year1, 1km			12.27* (7.38)			81.17** (40.92)
Observations	2,919,682	2,919,682	2,919,682	2,751,591	2,751,591	2,751,591
Base Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Suburb-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Ind.-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors that account for clustering at the municipality level in parentheses. ***, **, * indicate statistical significance at the 1%, 5% and 10% level respectively. The baseline controls comprise a size control (the log of firms' total assets), a full set of fixed effects indicating the year when the tax return was submitted, a full set of fixed effects indicating the tax year of the return and control variables for the log of the number of firms in 100m, 500m and 1km radius around the considered taxpayer as well as dummies indicating the deciles of the average taxable income submitted by firms within a 100m/500m/1km radius of the considered taxpayer. Specifications (1) includes the described control variables for the 100m radius, Specifications (2) for the 100m and 500m radius and Specifications (3)

for the 100m, 500m and 1km radius.

Table 4: Dynamic Effects						
	(1)	(2)	(3)	(4)	(5)	(6)
Audited, Year1, 100m	0.0130** (0.0053)	0.0115*** (0.0048)	0.0107*** (0.0045)	0.0114* (0.0064).	0.0095* (0.0057)	0.0088 (0.0054)
Audited, Year2, 100m	0.0044 (0.0043)	0.0048 (0.0042)	0.0035 (0.0054)	0.0052 (0.0042)	0.0054 (0.0041)	0.0059 (0.0043)
Audited, Year3, 100m				-0.0033 (0.0074).	-0.0034 (0.0074)	-0.0030 (0.0073)
Audited, Year1, 500m		0.0078*** (0.0014)	0.0049*** (0.0016)		0.0092*** (0.0018)	0.0062*** (0.0019)
Audited, Year2, 500m		-0.0008 (0.0015)	0.0035 (0.0024)		0.0016 (0.0019)	0.0029 (0.0026)
Audited, Year3, 500m					0.0018 (0.0033)	0.0019 (0.0037)
Audited, Year1, 1km			0.0042*** (0.0007)			0.0041*** (0.0006)
Audited, Year2, 1km			0.0015 (0.0012)			-0.0018 (0.0013)
Audited, Year3, 1km						-0.0006* (0.0017)
Observations	2,534,204	2,534,204	2,534,204	1,913,063	1,913,063	1,913,063
Base Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Munc.-Ind.-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors that account for clustering at the municipality level in parentheses. ***, **, * indicate statistical significance at the 1%, 5% and 10% level respectively. The baseline controls comprise a size control (the log of firms' total assets), a full set of fixed effects indicating the year when the tax return was submitted, a full set of fixed effects indicating the tax year of the return and control variables for the log of the number of firms in 100m, 500m and 1km radius around the considered taxpayer as well as dummies indicating the deciles of the average taxable income submitted by firms within a 100m/500m/1km radius of the considered taxpayer. Specifications (1) includes the described control variables for the 100m radius, Specifications (2) for the 100m and 500m radius and Specifications (3) for the 100m, 500m and 1km radius.

Table 5: Firm Density

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Audits, Year 1, 100m	0.0145*** (0.0044).	0.0220*** (0.0058).	0.0311*** (0.0110)	0.0126*** (0.0041).	0.0207*** (0.0056).	0.0304*** (0.0104).	0.0121*** (0.0039).	0.0201*** (0.0056).	0.0302*** (0.0104)
Audits, Year 1, 500m				0.0075*** (0.0012).	0.0078** (0.0036).	0.0055 (0.0086).	0.0051*** (0.0011).	0.0064* (0.0036)	.0040 (0.0090)
Audits, Year 1, 1km							0.0038*** (0.0008).	0.0054** (0.0026)	0.0071 (0.0060)
Observations	3,100,002	2,257,800	1,412,219.	3,100,002	2,257,800	1,412,219	3,100,002.	2,257,800	1,412,219
Sample	All	< 700	< 300.	. All	< 700.	< 300	All	< 700.	< 300
Base Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Munic.-Ind.-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors that account for clustering at the municipality level in parentheses. ***, **, * indicate statistical significance at the 1%, 5% and 10% level respectively. See the notes to Table 1 for a definition of variables. In all specifications, we include a full set of municipality fixed effects and instrument for the firm size measures using the 'industry-shock'-instruments constructed in Section 4. 'IV (country-sets)' indicates country-sets of firms used to construct $\tilde{g}_{k,i,t}$.

Table 6: Placebo Test, Dep. Var: 2-Year-Lag of Tax Payments			
	(1)	(2)	(3)
Audited, Year1, 100m	0.0022 (0.0024)	0.0020 (0.0027)	0.0018 (0.0028)
Audited, Year1, 500m		0.0012 (0.0016)	0.0007 (0.0014)
Audited, Year1, 1km			. 0.0008 (0.0010)
Observations	1,465,677	1,465,677	1,465,677
Base Controls	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes
Munc.-Ind.-Year Fixed Effects	Yes	Yes	Yes

Notes: Robust standard errors that account for clustering at the municipality level in parentheses. ***, **, * indicate statistical significance at the 1%, 5% and 10% level respectively. The baseline controls comprise a size control (the log of firms' total assets), a full set of fixed effects indicating the year when the tax return was submitted, a full set of fixed effects indicating the tax year of the return and control variables for the log of the number of firms in 100m, 500m and 1km radius around the considered taxpayer as well as dummies indicating the deciles of the average taxable income submitted by firms within a 100m/500m/1km radius of the considered taxpayer. Specifications (1) includes the described control variables for the 100m radius, Specifications (2) for the 100m and 500m radius and Specifications (3) for the 100m, 500m and 1km radius.

Table 7: Positive vs. Non-Positive Adjustments			
	(1)	(2)	(3)
Audited, Year1, 100m, Positive	0.0087 (0.0087)	0.0073 (0.0082)	0.0067 (0.0081)
Audited, Year1, 100m, Non-Positive	0.0171*** (0.0046)	0.0147*** (0.0041)	0.0144*** (0.0039)
Audited, Year1, 500m, Positive		0.0029 (0.0029)	0.0021 (0.0028)
Audited, Year1, 500m, Non-Positive		0.0084*** (0.0014)	0.0062*** (0.0013)
Audited, Year1, 500m, Positive			0.0047*** (0.0015)
Audited, Year1, 500m, Non-Positive			.0031*** (0.0009)
Observations	3,100,002	3,100,002	3,100,002
Base Controls	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes
Munic-Ind.-Year-FE	Yes	Yes	Yes

Note: Robust standard errors that account for clustering at the municipality level in parentheses. ***, **, * indicate statistical significance at the 1%, 5% and 10% level respectively. See the notes to Tables 1 and 3 for a definition of the (instrumental) variables. 'IV' indicates instrumental variable regressions (where the 'baseline' set of countries is used to construct $\tilde{g}_{k,i,t}$, cf. Section 4).

Table 8: Positive vs. Non-Positive Adjustments, Same Industry vs. Other Industry

	(1)	(2)	(3)
Same Industry, Audited, Year1, 100m, Positive	-0.0053 (0.0134)	-0.0067 (0.0122)	-0.0065 (0.0121)
Same Industry, Audited, Year1, 100m, Non-Positive	0.0146** (0.0060)	0.0112** (0.0057)	0.0104* (0.0058)
Other Industry, Audited, Year1, 100m, Positive	0.0115 (0.0096)	0.0101 (0.0092)	0.0093 (0.0091)
Other Industry, Audited, Year1, 100m, Non-Positive	0.0174*** (0.0058)	0.0153*** (0.0052)	0.0152*** (0.0049)
Same Industry, Audited, Year1, 500m, Positive		0.0025 (0.0056)	0.0032 (0.0055)
Same Industry, Audited, Year1, 500m, Non-Positive		0.0092*** (0.0036)	0.0066*** (0.0031)
Other Industry, Audited, Year1, 500m, Positive		0.0031 (0.0030)	0.0020 (0.0029)
Other Industry, Audited, Year1, 500m, Non-Positive		0.0082*** (0.0020)	0.0060*** (0.0014)
Same Industry, Audited, Year1, 500m, Positive			0.0003 (0.0033)
Same Industry, Audited, Year1, 500m, Non-Positive			0.0033* (0.0019)
Other Industry, Audited, Year1, 500m, Positive			0.0059*** (0.0019)
Other Industry, Audited, Year1, 500m, Non-Positive			0.0030** (0.0014)
Observations	3,100,002	3,100,002	3,100,002
Base Controls	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes
Munic-Ind-Year Fixed Effects	Yes	Yes	Yes

Note: Robust standard errors that account for clustering at the municipality level in parentheses. ***, **, * indicate statistical significance at the 1%, 5% and 10% level respectively. See the notes to Tables 1 and 3 for a definition of the (instrumental) variables. 'IV' indicates instrumental variable regressions (where the 'baseline' set of countries is used to construct $\tilde{g}_{k,i,t}$, cf. Section 4).

7 Appendix

Table A1: Dynamic Effects, Appendix

	(1)	(2)	(3)	(4)	(5)	(6)
Audited, Year1, 100m	0.0117** (0.0060)	0.0100* (0.0054)	0.0092* (0.0052)	0.0093 (0.0067).	0.0072 (0.0059)	0.0064 (0.0057)
Audited, Year2, 100m	0.0040 (0.0055)	0.0037 (0.0053)	0.0052 (0.0044)	0.0052 (0.0053)	0.0042 (0.0049)	0.0039 (0.0050)
Audited, Year3, 100m				-0.0101 (0.0073).	-0.0098 (0.0072)	-0.0088 (0.0071)
Audited, Year1, 500m		0.0093*** (0.0018)	0.0059*** (0.0020)		0.0103*** (0.0019)	0.0071*** (0.0018)
Audited, Year2, 500m		0.0050*** (0.0015)	0.0002 (0.0022)		0.0069*** (.0017)	0.0059** (0.0026)
Audited, Year3, 500m					-0.0005 (0.0044)	0.0000 (0.0045)
Audited, Year1, 1km			0.0040*** (0.0007)			0.0038*** (0.0010)
Audited, Year2, 1km			-0.0016 (0.0013)			0.0009 (0.0013)
Audited, Year3, 1km						-0.0030* (0.0017)
Observations	2,586,859	2,586,859	2,586,859	1,913,063	1,913,063	1,913,063
Base Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Audited, Year1, 100m	0.0135*** (0.0055)	0.0124** (0.0051)	0.0107** (0.0048)	0.0122* (0.0068).	0.0110* (0.0063)	0.0099* (0.0059)
Audited, Year2, 100m	0.0047 (0.0043)	0.0054 (0.0043)	0.0058 (0.0043)	0.0066 (0.0050)	0.0072 (0.0051)	0.0075 (0.0051)
Audited, Year3, 100m				0.0005 (0.0071).	-0.0034 (0.0071)	0.0009 (0.0070)
Audited, Year1, 500m		0.0087*** (0.0016)	0.0053*** (0.0018)		0.0098*** (0.0020)	0.0063*** (0.0020)
Audited, Year2, 500m		-0.0027 (0.0021)	-0.0017 (0.0025)		-0.0008 (0.0027)	0.0006 (0.0029)
Audited, Year3, 500m					0.0003 (0.0033)	0.00051 (0.0034)
Audited, Year1, 1km			0.0061*** (0.0011)			0.0057*** (0.0007)
Audited, Year2, 1km			-0.0012 (0.0013)			-0.0020 (0.0014)
Audited, Year3, 1km						-0.0023 (0.0020)
Observations	2,531,416	2,531,416	2,534,204	1,874,072	1,874,072	1,874,072
Base Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Suburb-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Ind.-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors that account for clustering at the municipality level in parentheses. ***, **, * indicate statistical significance at the 1%, 5% and 10% level respectively. The baseline controls comprise a size control (the

log of firms' total assets), a full set of fixed effects indicating the year when the tax return was submitted, a full set of fixed effects indicating the tax year of the return and control variables for the log of the number of firms in 100m, 500m and 1km radius around the considered taxpayer as well as dummies indicating the deciles of the average taxable income submitted by firms within a 100m/500m/1km radius of the considered taxpayer. Specifications (1) includes the described control variables for the 100m radius, Specifications (2) for the 100m and 500m radius and Specifications (3) for the 100m, 500m and 1km radius.

Table A2: Firm Density, Appendix

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Audits, Year 1, 100m	0.0138*** (0.0049).	0.0246*** (0.0054).	0.0336*** (0.0101)	0.0117*** (0.0046).	0.0233*** (0.0053).	0.0329*** (0.0095).	0.0111*** (0.0045).	0.0228*** (0.0052).	0.0327*** (0.0096)
Audits, Year 1, 500m				0.0078*** (0.0014).	0.0067* (0.0035).	0.0041 (0.0087).	0.0054*** (0.0013).	0.0053 (0.0035).	0.0024 (0.0092)
Audits, Year 1, 1km							0.0031*** (0.0009).	0.0037 (0.0028).	0.0067 (0.0066)
Observations	3,171,900	2,311,819	1,450,749.	3,171,900	2,311,819	1,450,749.	3,171,900.	2,311,819	1,450,749
Sample	All	< 700	< 300.	. All	< 700.	< 300	All	< 700.	< 300
Audits, Year 1, 100m	0.0148*** (0.0048).	0.0214*** (0.0059).	0.0314*** (0.0114)	0.0132*** (0.0046).	0.0206*** (0.0056).	0.0314*** (0.0110).	0.0125*** (0.0043).	0.0202*** (0.0056).	0.0318*** (0.0108)
Audits, Year 1, 500m				0.0093*** (0.0019).	0.0070** (0.0036).	0.0093 (0.0093).	0.0063*** (0.0013).	0.0061 (0.0038).	0.0085 (0.0093)
Audits, Year 1, 1km							0.0058*** (0.0009).	0.0060** (0.0022).	0.0092* (0.0052)
Observations	3,097,029	2,254,935	1,409,501.	3,097,029	2,254,935	1,409,501	3,097,029.	2,254,935	1,409,501
Sample	All	< 700	< 300.	. All	< 700.	< 300	All	< 700.	< 300
Base Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Suburb-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ind-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors that account for clustering at the municipality level in parentheses. ***, **, * indicate statistical significance at the 1%, 5% and 10% level respectively. See the notes to Table 1 for a definition of variables. In all specifications, we include a full set of municipality fixed effects and instrument for the firm size measures using the 'industry-shock'-instruments constructed in Section 4. 'IV (country-sets)' indicates country-sets of firms used to construct $\hat{g}_{k,i,t}$.

Table A3: Placebo Test, Dep. Var: 2-Year-Lag of Tax Payments			
	(1)	(2)	(3)
Audited, Year1, 100m	0.0025 (0.0030)	0.0024 (0.0032)	0.0023 (0.0033)
Audited, Year1, 500m		0.0011 (0.0017)	0.0008 (0.0014)
Audited, Year1, 1km			. 0.0004 (0.0012)
Observations	1,496,944	1,496,944	1,496,944
Base Controls	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes
Audited, Year1, 100m	0.0025 (0.0025)	0.0023 (0.0027)	0.0019 (0.0028)
Audited, Year1, 500m		0.0016 (0.0015)	0.0005 (0.0014)
Audited, Year1, 1km			0.0018 (0.0011)
Observations	1,464,818	1,464,818	1,464,818
Base Controls	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes
Suburb-Year Fixed Effects	Yes	Yes	Yes
Ind.-Year Fixed Effects	Yes	Yes	Yes

Notes: Robust standard errors that account for clustering at the municipality level in parentheses. ***, **, * indicate statistical significance at the 1%, 5% and 10% level respectively. The baseline controls comprise a size control (the log of firms' total assets), a full set of fixed effects indicating the year when the tax return was submitted, a full set of fixed effects indicating the tax year of the return and control variables for the log of the number of firms in 100m, 500m and 1km radius around the considered taxpayer as well as dummies indicating the deciles of the average taxable income submitted by firms within a 100m/500m/1km radius of the considered taxpayer. Specifications (1) includes the described control variables for the 100m radius, Specifications (2) for the 100m and 500m radius and Specifications (3) for the 100m, 500m and 1km radius.

Table A4: Positive vs. Non-Positive Adjustments			
	(1)	(2)	(3)
Audited, Year1, 100m, Positive	0.0036 (0.0092)	0.0031 (0.0087)	0.0028 (0.0085)
Audited, Year1, 100m, Non-Positive	0.0184*** (0.0044)	0.0150*** (0.0040)	0.0145*** (0.0038)
Audited, Year1, 500m, Positive		-0.0014 (0.0035)	-0.0012 (0.0032)
Audited, Year1, 500m, Non-Positive		0.0097*** (0.0018)	0.0069*** (0.0015)
Audited, Year1, 500m, Positive			0.0004 (0.0020)
Audited, Year1, 500m, Non-Positive			.0030*** (0.0012)
Observations	3,171,900	3,171,900	3,171,900
Base Controls	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes
Audited, Year1, 100m, Positive	0.0071 (0.0086)	0.0055 (0.0081)	0.0049 (0.0081)
Audited, Year1, 100m, Non-Positive	0.0183*** (0.0054)	0.0164*** (0.0050)	0.0157*** (0.0046)
Audited, Year1, 500m, Positive		0.0027 (0.0029)	0.0022 (0.0028)
Audited, Year1, 500m, Non-Positive		0.0110*** (0.0019)	0.0072*** (0.0015)
Audited, Year1, 500m, Positive			0.0044* (0.0024)
Audited, Year1, 500m, Non-Positive			0.0058*** (0.0011)
Observations	3,097,029	3,097,029	3,097,029
Base Controls	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes
Suburb-Year Fixed Effects	Yes	Yes	Yes
Ind.-Year Fixed Effects	Yes	Yes	Yes

Note: Robust standard errors that account for clustering at the municipality level in parentheses. ***, **, * indicate statistical significance at the 1%, 5% and 10% level respectively. See the notes to Tables 1 and 3 for a definition of the (instrumental) variables. 'IV' indicates instrumental variable regressions (where the 'baseline' set of countries is used to construct $\tilde{g}_{k,i,t}$, cf. Section 4).

Table A5: Positive vs. Non-Positive Adjustments, Same Industry vs. Other Industry

	(1)	(2)	(3)
Same Industry, Audited, Year1, 100m, Positive	-0.0078 (0.0124)	-0.0097 (0.0113)	-0.0094 (0.0111)
Same Industry, Audited, Year1, 100m, Non-Positive	0.0160*** (0.0065)	0.0131** (0.0060)	0.0120** (0.0060)
Other Industry, Audited, Year1, 100m, Positive	0.0100 (0.0093)	0.0085 (0.0090)	0.0076 (0.0089)
Other Industry, Audited, Year1, 100m, Non-Positive	0.0186*** (0.0066)	0.0169*** (0.0061)	0.0163*** (0.0056)
Same Industry, Audited, Year1, 500m, Positive		0.0010 (0.0059)	0.0026 (0.0059)
Same Industry, Audited, Year1, 500m, Non-Positive		0.0117*** (0.0032)	0.0084*** (0.0026)
Other Industry, Audited, Year1, 500m, Positive		0.0022 (0.0029)	0.0020 (0.0029)
Other Industry, Audited, Year1, 500m, Non-Positive		0.0069*** (0.0018)	0.0060*** (0.0014)
Same Industry, Audited, Year1, 500m, Positive			-0.0003 (0.0036)
Same Industry, Audited, Year1, 500m, Non-Positive			0.0050*** (0.0018)
Other Industry, Audited, Year1, 500m, Positive			0.0056** (0.0028)
Other Industry, Audited, Year1, 500m, Non-Positive			0.0059*** (0.0016)
Observations	3,097,029	3,097,029	3,097,029
Base Controls	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes
Ind-Year Fixed Effects	Yes	Yes	Yes
Suburb-Year Fixed Effects	Yes	Yes	Yes

Note: Robust standard errors that account for clustering at the municipality level in parentheses. ***, **, * indicate statistical significance at the 1%, 5% and 10% level respectively. See the notes to Tables 1 and 3 for a definition of the (instrumental) variables. 'IV' indicates instrumental variable regressions (where the 'baseline' set of countries is used to construct $\tilde{g}_{k,i,t}$, cf. Section 4).

Table B1: Overview SBC Tax Schedule - 2009-2015

Financial year	Taxable income	Marginal tax rate
01/04/2009 - 31/03/2010	R1 - R54,200	0%
	R54,201 - R300,000	10%
	R300,001 and above	28%
01/04/2010 - 31/03/2011	R1 - R59,750	0%
	R59,751 - R300,000	10%
	R300,001 and above	28%
01/04/2011 - 31/03/2012	R1 - R59,750	0%
	R59,751 - R300,000	10%
	R300,001 and above	28%
01/04/2012 - 31/03/2013	R1 - R63,556	0%
	R63,557 - R350,000	7%
	R350,001 and above	28%
01/04/2013 - 31/03/2014	R1 - R67,111	0%
	R67,112 - R365,000	7%
	R365,001 - R550,000	21%
	R550,001 and above	28%
01/04/2014 - 31/03/2015	R1 - R70,700	0%
	R70,701 - R365,000	7%
	R365,001 - R550,000	21%
	R550,001 and above	28%
01/04/2015 - 31/03/2016	R1 - R73,650	0%
	R73,651 - R365,000	7%
	R365,001 - R550,000	21%
	R550,001 and above	28%

Notes: This table shows the tax schedules for Small Business Corporations for the tax years 2009/2010 to 2015/2016.