Tax Audits and their Distortionary Effects*

David Henning (UCLA)   Joseph Okello (URA)

First version: October 28, 2024
This version: November 7, 2024

Please click here for the latest version

Abstract

Tax audits are essential for governments to raise revenue, but can create economic distortions. To avoid the financial burden of an audit, firms could remain small, move to the informal sector, or shutdown. Leveraging detailed administrative tax data from the Ugandan Revenue Authority, a novel linked survey, and a regression discontinuity, we show that audits have two negative effects in our context: they reduce the tax revenue collected among audited firms, *and* impose large economic distortions. Audited firms are 11 percentage point more likely to shutdown, and those that remain operational are induced to reduce their output. The shutdown results are driven by firms who have to pay back substantial amounts of taxes. The output results are driven by firms who believe they are likely to be audited again soon. Back-of-the-envelope calculations indicate that comprehensive audits lead to a revenue loss of 3.6 million USD and, to a first order, an aggregate output loss of 10.9 million USD. Overall, our results demonstrate that comprehensive audits impose large costs on audited firms in our context and ultimately hurt both revenue collection efforts and the real economy.

*Contact information: djhenning@ucla.edu, jookello@ura.go.ug. David is heavily indebted to Natalie Bau, Adriana Lleras-Muney, Juliana Londoño-Vélez, and Manisha Shah for providing invaluable guidance and support throughout the project. We are grateful to staff at the Ugandan Revenue Authority for facilitating the project and providing data access. Special thanks to the Central Operations Office for their comments and insights throughout. We are indebted to Allan Gitta and Apollo Kamwebaze for helping us understand the audit selection process. We are also grateful to staff at the Ugandan Registry Services Bureau for providing registry information. The tracking and survey was done with the help of International Growth Research and Evaluation Center (IGREC). We also benefited from many insightful discussions with Jonas Hjort. We gratefully acknowledge funding from the International Growth Center (IGC), Private Enterprise Development in Low-Income Countries (PEDL), Center for Effective Global Action (CEGA), and Structural Transformation for Economic Growth (STEG). All errors are our own.

1 Introduction

A core challenge for countries is raising tax revenue while maintaining economic growth (Besley and Persson, 2013). Tax evasion is widespread (Alstadsaeter et al., 2023; Slemrod, 2019), suggesting one could raise tax revenue through raising tax enforcement. However, tax enforcement measures may – at least in theory – induce distortions that have negative effects on the economy. This is particularly a concern when conducting enforcement on firms, which are key drivers of economic growth (Aghion et al., 2014). To avoid enforcement measures – such as tax audits – firms may to reduce their output, move to the informal sector, or in the extreme, shut down altogether. These responses cause firms to stay inefficiently small, potentially creating misallocation, reducing aggregate output, and reducing economic growth (Bachas et al., 2019; Hsieh and Klenow, 2009). Despite the potential negative impact of tax enforcement on firm output and economic development, it has received little attention in the literature (Jensen et al., 2024).

This is especially a concern in developing countries. While the need for revenue is greater and evasion is more widespread (Besley and Persson, 2014), the distortionary effects are also likely to be more pronounced, due to structural features of the economy. First, formal firms are on average smaller than their counterparts in rich countries (Bento and Restuccia, 2021). More limited accounting and auditing capacity due to being smaller could make them more vulnerable to intense enforcement measures. Second, the informal sector tends to be significantly larger, offering firms an alternative operating environment where they avoid all taxes and regulatory costs (Ulyssea, 2018). Finally, statutory tax rates are higher (Enache, 2023). Higher statutory tax rates create a larger incentive to adjust output in the hope of avoiding scrutiny from the tax authority.

In this paper, we investigate how receiving a comprehensive audit from the tax authority affects ex-ante formal firms in a developing country, Uganda. Comprehensive audits are the most intense audit interventions. The median audit takes three months, and can investigate any part of a firms' accounts covering the past five years. We combine detailed administrative data with a novel linked survey and a regression discontinuity to document three new findings. First, leveraging the administrative tax data, we show that the potential revenue collected from the audited firms declines post-audit.[1] This is driven by firms exiting the tax system and remaining firms reporting lower tax liabilities. A marginal value of public fund calculation for audits in the style of Boning et al. (2023), yields a negative value. Second, we track down firms two years after the audit and find that firms receiving comprehensive audits do not only change their tax filing behavior, but are also more likely to shut down. Third, using our survey we find that firms that remained operational – in either the formal or informal sector – reduce their output after the audit.

Receiving a tax audit can affect firm operations in several ways. First, the audit induces an imme-

[1] In our data we never observe how much taxes a firm paid, we only observe how much they owe the tax authority. As such, our results indicate the "potential revenue collected" or the "tax liability" of the firm. We use both terms throughout the paper.

diate cost to the firm due to the hassle cost of dealing with the audit (such as time spent on the audit, accounting costs, etc.) and – if found liable – paying back taxes and associated penalties. If these costs are substantial, liquidity constrained firms could be pushed toward insolvency. Second, an audit may induce changes in perceptions about the enforcement environment. If an audit raises the belief that the authority is able to detect evasion, while considered "successful deterrence", the expected effective tax rate of the firm increases. Under the higher expected effective tax rate, firms on the margin may be unprofitable. Finally, an audit may also cause firms to change their beliefs about the extent to which audit rates depend on firm output. An increase in that belief incentivizes firms to lower their output to avoid future audits.

Descriptive evidence from our survey supports the notion that taxation and tax enforcement imposes a large economic burden for firms. Total costs of taxation range from 26-36% of *sales*, with firms in the lowest quintile of sales reporting the highest shares. This is not because small firms pay more taxes. Instead, the enforcement costs represent a larger share of sales, suggesting that the relative cost of tax enforcement is particularly high for small firms. An example of an enforcement cost would be the cost of hiring an accountant to handle the audit. Furthermore, 63% of firms mention high taxes, tax administration costs or tax corrections as one of the three main obstacles to their performance. This is the most frequently mentioned reasons, with the next most important reason – competition – mentioned by only 32% of firms.

Estimating the impact of a comprehensive tax audit is notoriously difficult because firms selected for such interventions are different from other firms in both observable and unobservable ways. We overcome this challenge by leveraging discontinuities in the audit selection process of the Ugandan Revenue Authority (the Ugandan IRS, hereafter referred to as the URA). Firms are selected into comprehensive audits based on risk scores that are calculated for the universe of tax filing firms annually. The risk scores are calculated based on information submitted by firms in previous tax returns. The firms with the highest risk scores are assigned to comprehensive audits, creating a discontinuity in the intensity of enforcement, with similar firms just above and below the threshold. Importantly, the firms below the comprehensive audit threshold receive lighter-touch audits (not no enforcement). Our results should thus be interpreted as the effect of changing the audit assignment of one more firm from light to comprehensive.

Combining information on tax audits and audit selection criteria with the universe of Corporate Income Tax (CIT) and Value Added Tax (VAT) filings, we find consistent negative effects on potential tax revenue collected. Firms receiving a comprehensive audit are 30 percentage points (p.p.) less likely to file a CIT return in the year after the audit, and the effect persists into – at least – the second post-audit year. The effect is not driven by "fictitious" firms that only exist to create and sell fake invoices.[2] The reduction in filing leads to an overall decrease in CIT revenue collected of 5,463 USD per firm audited. We find similar, though less precise, results for the VAT, likely because the VAT data only covers a short time period after the audit. While there is an increase in revenue

[2]See Carrillo et al. (2023) and Waseem (2023) for recent papers exploring this phenomenon in developing countries.

through correcting firms' past discrepancies – highlighting one reason why these audits might still be conducted – when considering post-audit reporting behavior in the year after the audit, the overall effect of the marginal audit on revenue is negative. Specifically, our preferred estimates indicate the URA is losing 7,400 USD per audit conducted. A back-of-the-envelope calculation suggests that the effect is not isolated to the marginal audit. Across all comprehensive audits conducted in our year of analysis, the URA is losing 3.6 million USD.

Characterizing the firms that exit the formal sector through a compliers analysis, reveals that they tend to be relatively productive *marginally formal* firms. The firms that exit are smaller, have limited capital and are either in the service or wholesale sector. However, they submitted substantial tax liabilities prior to the audit and have a high sales-to-cost ratio suggesting they are important contributors to revenue collection and relatively productive. Lack of capital investments suggests that it is easier for these types of firms to relocate or shut down when under pressure from tax authorities. While this explains why they respond by leaving despite being productive, these small and productive firms are exactly the type of firms needed to improve employment opportunities and growth (Quinn and Woodruff, 2019).

While the administrative data is rich in detail, it is challenging to measure changes in firm output using administrative tax data. Information in tax returns submitted post-audit are influenced both by real production decisions and changes in reporting behavior. We overcome this challenge by designing and implementing a firm survey for the firms on the margin of receiving a comprehensive audit. The survey attempts to track down all the 858 firms in the regression discontinuity sample. We implement the survey approximately two years after the audit and are able to verify whether a firm closed or remained open for 91% of firms.

Leveraging information from our novel survey, we find strong evidence that comprehensive audits cause firms to shut down. Our estimates show that firms on the margin of receiving a comprehensive tax audit are 11 p.p. more likely to shut down compared to firms receiving a light audit intervention. Dividing the coefficient by the change in the probability of filing taxes in the two years after the audit suggests that slightly more than half of the firms that stop filing taxes (54%) shut down post-audit. The remaining 46% kept operating in the informal sector. 43% of closed firms mention that they closed because of challenges with high taxes and/or the URA. Crucially, less than half of respondents that shut down opened another firm, underscoring that the exit is long-lasting. The effect is entirely driven by firms that had to pay back substantial amount of taxes. Based on our calculations, for the median firm receiving a tax correction, the total amount it had to pay back represented 86% of baseline gross profits.

We also find evidence that the firms that remain operational reduce their output after receiving a comprehensive audit. To improve our power for the sample where we have survey data, we move away from the firms immediately around the discontinuity. Combining the survey and administrative data in a difference-in-difference strategy, we find that comprehensive audits cause operational firms to reduce their sales by 56%. This intensive distortion effect would not have been possible to detect from the administrative tax data alone because the firms either do not file a tax return or report no sales in their tax return. We corroborate that the result is qualitatively similar when using the regression discontinuity design. Taken together, this demonstrates that comprehensive audits both reduce the number of firms in the economy and reduce the output of operational firms. A back-of-the-envelop aggregation exercise suggests this leads to a reduction in aggregate output for formal firms of 0.18 – 0.5%.

This paper contributes to several strands of the literature. First, it contributes to the literature on the economic (efficiency) costs of taxation and tax enforcement. Previous literature has documented that the VAT distorts firm-to-firm trade (Gadenne et al., 2022) and that VAT rebates affect the export performance of firms (Chandra and Long, 2013). More broadly, firms actively respond to thresholds that change their tax liability (Best et al., 2015; Harju et al., 2019; Liu et al., 2021) or enforcement levels (Almunia et al., 2022) and respond to enforcement interventions by appearing small (Carrillo et al., 2017) or not filing taxes (Belnap et al., 2024). A common theme in this literature is that it relies on administrative tax data. While rich in detail and ideal for studying compliance responses, it is unclear how much of a firms response is driven by changes in reporting (evasion) or changes in firm sales (output). We overcome this challenge by linking administrative tax data with a survey on firms. The survey is novel in that it allows us to measure the output of firms while accounting for changes in evasion behavior. The closest paper to ours is Harju et al. (2024) who document that bankruptcy rates increase after a tax audit in Finland. Unlike them, we also document that firms respond in less extreme ways, such as reducing output, which is crucial to understand the overall implication on aggregate output. We are also the first to study this question in a low-income context, where the economic costs might be particularly severe. Indeed, to the extent that our measure of firm exit is comparable to the one in Harju et al. (2024), our estimates are 9 times larger.

Second, we contribute to the literature on the revenue and welfare impacts of tax audits. Previous literature tends to find that tax audits increase revenue collected by the government (Advani et al., 2021; Beer et al., 2020; Best et al., 2021; Boning et al., 2023; Christiansen, 2024; DeBacker et al., 2015; DeBacker et al., 2018; Gemmell and Ratto, 2012; Harju et al., 2024; Kleven et al., 2011; Kotsogiannis et al., 2024; Li et al., 2018; Løyland et al., 2019).[3] Leveraging a marginal value of public funds approach suggests that audits have large positive welfare effects, at least in the US (Boning et al., 2023). We are one of the first to document that tax audits can have a negative effect on the potential revenue collected by the tax authority.[4] This might be due to two key features

[3] A large literature has investigated the effect of deterrence communication on tax evasion among firms (Almunia et al., 2022; Bergolo et al., 2023; Blumenthal et al., 2001; Boning et al., 2020; Brockmeyer et al., 2019; Carmen et al., 2022; Carrillo et al., 2017; Holz et al., 2023; Pomeranz, 2015; Slemrod et al., 2001), and tends to find positive or nil effects. Also see Slemrod (2019), Mascagni (2018) and Alm (2019) for recent surveys of the literature. Our work differs from that in that we consider the impact of *receiving* an enforcement action, which is distinct from receiving information.

[4] A noteable exception is DeBacker et al. (2015) who document that firms in the US evade more after an audit.

of our study: we focus on a developing country, Uganda, and on the most intense enforcement intervention, comprehensive tax audits. Other work on tax audits in developing countries finds zero (Best et al., 2021) or positive (Kotsogiannis et al., 2024) effects on revenue collection. In the case of the former, it is because tax audits are simple cross checks, and in the case of the latter they focus on a balanced panel of firms shutting down extensive margin responses. As such, our work adds to previous literature in similar contexts by drawing attention to the fact that a key margin of response from firms is to stop filing taxes altogether.

Third, this paper speaks to a broad literature on one of the key questions in development economics: why are there so few large firms? There is an extensive literature in microeconomics investigating different reasons why firms may not grow. Possible explanations include credit constraints (de Mel et al., 2008; McKenzie and Woodruff, 2008), barriers to hiring (Bassi and Nansamba, 2022; Carranza et al., 2022), limited business training (Blattman et al., 2016; de Mel et al., 2014; Field et al., 2010), delegation and organization of firms (Akcigit et al., 2021; Bassi et al., 2023; Bloom et al., 2012) poor contract enforcement (Boehm and Oberfield, 2020; Iyer and Schoar, 2015), high cost of formalization (McKenzie and Seynabou Sakho, 2010), and constraints on the demand side (Bold et al., 2022; Hjort et al., 2020; Vitali, 2023). However, one of the defining features of developing countries is that they have high statutory tax rates and high enforcement rates relative to firm size, potentially making it a key obstacle for firm growth. Our results on shutdown are driven by firms that have to pay back substantial amounts of taxes, suggesting that the tax rates may be too high for these firms to operate. To the extent that one would want such firms to exist, it suggests that Uganda might be on the wrong side of the laffer curve. Tax enforcement has been recognized as a reason for misallocation in the structural and macroeconomics literature, reducing aggregate output (Bachas et al., 2019; Corbellini, 2024; Di Nola et al., 2021; Leal Ordóñez, 2014).[5][6] However, to the best of our knowledge, there has been limited causal evidence on the extent to which tax audits – and by extension enforcement – induces firms to reduce their output. Our paper adds to this literature by highlighting that tax audits can distort the output of firms. Indeed a back-of-the-envelope calculation suggests that comprehensive audits reduced GDP in Uganda by 0.05%–0.24%.

The rest of the paper is structured as follows. In section 2 we describe the Ugandan economic context, tax system. Section 3 discusses comprehensive tax audits and the selection process. In section 4 we present the administrative data used in our analysis and provide descriptives for our sample. Section 5 describes our estimation strategy, and ascertains its validity. In section 6

A theoretical and experimental literature also documents the possibility that firms and individuals evade more in the immediate aftermath of the audit (Alm and Malézieux, 2021; Kasper and Alm, 2022; Kasper and Rablen, 2023). However, empirical evidence for this phenomenon remains limited.

[5] This is part of a broader literature documenting that firms below regulatory thresholds are over-represented, leading to misallocation (Boeri and Jimeno, 2005; Evans, 1986; Garicano et al., 2016; Gourio and Roys, 2014; Schivardi and Torrini, 2008).

[6] Micro-evidence of the distortionary effect of audits in public procurement has been documented by Gerardino et al. (2024).

we present results for the administrative tax data. Section 7 introduces the survey and presents results from the survey data. Section 8 discusses our results, which includes a back-of-the-envelope calculation, while section 9 concludes.

2 Context: the Ugandan economy and tax system

2.1 Ugandan economy

Uganda is a low-income country with a low tax-to-GDP ratio. Uganda's per-capita income was \$3,040 in purchasing power parity (World Bank, 2023b). Its tax-to-GDP ratio – 12.2% in 2020/21 (OECD et al., 2023) – is slightly below the 15.6% average in Sub-Saharan Africa (OECD et al., 2023) and is substantially lower than the 34.1% average in OECD countries (OECD et al., 2023). However, similar to other developing countries, the low tax-to-gdp ratio is not explained by low tax rates. The statutory Corporate Income Tax (CIT) rate is 30%, making it the 21st highest in the world (Enache, 2023).

The informal sector in Uganda accounts for a large share of economic activity. In 2017, 84% and 87% of the male and female workforce outside agriculture was estimated to work in the informal sector (World Bank, 2017).[7] 72% of businesses are estimated to operate informally, and the informal sector accounts for 51% of GDP (Sanday, 2023). These are large estimates by any measure, but are similar to other countries in the region. Across Sub-Saharan Africa, 90% of employment is estimated to be in the informal sector (World Bank, 2019a), 86% of enterprises in Africa are informal (OECD and ILO, 2019), and informality accounts for around 40% of GDP (World Bank, 2019a).

These two facts may be related. High tax rates, if enforced, may encourage formal firms to leave the formal sector and prevent informal ones from becoming formal. Conversely, high levels of informality push the government to enforce taxes more heavily, retrieving as much tax revenue as possible from the small pool of firms that are formal.

2.2 Ugandan tax system

Uganda uses a modern electronic tax system. Since 2013, firms in Uganda have been required to file their taxes electronically, creating a large repository of digital information on firms' tax filings. The three main tax bases in Uganda are VAT, CIT, and Pay As You Earn (PAYE). Firms with sales above 150 million UGX ($\sim$ 40,000 USD) are required to register for the VAT and CIT. Below this threshold a simple sales tax replaces both tax heads.[8] In contrast to VAT and CIT, PAYE is supposed to be paid for any employee with a monthly salary above 235 thousands UGX ($\sim$ 60 USD), regardless of the firms' sales.

[7] If we include employment in agriculture this rises to an average of 95% across both genders (ILO, 2021b).

[8] This automatically excludes many of the smallest taxpayers in Uganda, however, based on conversations with officers at the URA it is clear that those taxpayers rarely experience any form of compliance intervention. Firms can choose to register for CIT and VAT even if they are below the threshold. The reason they might do so is because of wanting to deal with VAT firms, or bid for government procurement.

The CIT is filed annually and is due 6 months after the close of the financial year. Firms are required to file a detailed balance sheet and profit/loss statement denoting each of their costs and income sources in their CIT return. The Ugandan fiscal year goes from the 1st of July to the 30th June, but firms can apply to change their accounting year to suit their operations. The CIT rate in Uganda is 30% and is calculated on the net income reported by companies, with few exceptions.[9] Once the return has been filed, firms can amend it – re-file the return with updated information – indefinitely. Not filing a tax return on time costs 200,000 UGX (55 USD) or 2% of the tax liability for the period, whichever is higher. It also gives the URA permission to determine how much the taxpayer owes in taxes.

VAT-registered firms have to submit monthly VAT declarations to the URA for the domestic part of their business due within 15 days of the end of the month. Similar to the CIT, firms may file amendments for past returns indefinitely. Negative liabilities can be carried over to subsequent months when less than 5 million UGX ($\sim$ 1,400 usd). Higher amounts can be claimed as a refund, but that triggers a refund audit. The VAT rate in Uganda is 18% and is calculated on the difference between total sales and inputs from VAT registered firms.

Similar to VAT, firms liable for PAYE have to submit monthly PAYE returns for their employees within 15 days of the end of the month. The PAYE tax in Uganda is based on the total income of an employee, including any non-salary payments, and the rates vary from 10% to 40% depending on the employee's income.[10] The PAYE tax is withheld by the firm and remitted directly to the URA, similar to payroll taxes in other countries.

2.3 Descriptive evidence on taxation and enforcement as an obstacle to firm growth

To explore to what extent high tax rates and tax enforcement impose an obstacle to firm growth in Uganda, we draw on our survey of formal firms (for more details on sample selection and implementation, see section 7), and a survey of informal firms conducted by the World Bank (for details on sample selection and methodology, please see World Bank (2019b)).

Information from our survey suggests that high statutory tax rates translate to high tax-induced operation costs in Uganda. This is not a given. In countries with limited enforcement capacity and/or extensive tax incentives, the effective tax rate faced by firms may be significantly lower than the statutory tax rates (Bachas et al., 2023).[11] Using the results from our survey (details in section 7), we document relevant facts about the cost of filing taxes in Uganda, as reported by the firms.

The results from the survey are shown in Figure 1, which reports the cost of taxes as a share of

[9] Repatriated branch profits are taxed at 15%, income from non-residents providing shipping services is taxed at 2%, and income from residents providing telecom services is taxed at 5%. In the period 2013 – 2022, this is relevant for 0.1% of all the CIT returns filed by taxpayers in Uganda.

[10] In Appendix Figure B1 we show how the tax rate varies with income. It quickly rises to 25%.

[11] The effective corporate income tax rate in Uganda is 15.1%, half of the statutory rate (Bachas et al., 2023).

sales, grouping firms into five quintiles based on their sales. There are three key takeaways. First, the costs of taxation as a share of sales are substantial, ranging from 26-36%. Second, the overall costs of taxation (as a share of sales) are substantially higher for the smallest firms in our survey, and relatively stable over the remaining quintiles, despite tax payments being lower for the lowest quintile. Third, this is because the enforcement costs to the firm – interactions with the URA, and tax corrections – represent a large share of sales for the small firms in our survey. Overall, our survey indicates that taxation imposes a significant economic burden on firms and that the burden of tax enforcement – measured as a share of sales – disproportionately affects the smaller firms in the formal sector. Note however, that this does not mean these firms are small in general. The median firm in the smallest quintile reports sales of 24 thousand USD and has 4 permanent employees. That puts it above the 40th percentile of the sales distribution among formal firms in Uganda in 2019.

Not only are the taxation-induced operation costs high, firms also say that taxation and its associated costs are one of the main obstacles to their growth. In the survey, we asked firms for the three main obstacles to their performance. Panel (a) of Figure 2 shows that 63% of firms mention high taxes, tax administration costs or tax corrections as one of their three main obstacles. This is the most frequently mentioned reason, with the second most important reason – competition – mentioned by only 32% of firms. Furthermore, firms do not seem to have strong intrinsic motivations to pay taxes. In Panel (b) of Figure 2 we show the reply from firms when asked about the three main reasons for paying taxes. Only 39% mention it contributes to economic development, and only 20% mention it helps their business. Together this demonstrates that formal firms in Uganda consider taxes to be a major obstacle to their performance, and do not see many benefits from paying them.

In addition to presenting a major obstacle for formal firms, high perceived costs of being formal discourage informal firms from formalizing. We draw on a survey of the informal sector in the greater Kampala area conducted by the World Bank in 2016 (for details on sample selection and methodology, please see World Bank (2019b)). In the questionnaire, non-registered firms are asked why they are not registered. In Panel (a) of Figure 3, we show the distribution of their answers. 36% of firms say they are not formalized because it is too expensive or it "could be bad for business". Together, these are the two most frequently mentioned reasons. Bear in mind, the question is about registering with the local administrative authorities *not even* about registering with the tax authority. The same set of informal firms are asked what would encourage them to formalize, and the number one reason mentioned by 42% of firms are low fees, as shown in Panel (b). This underlines that formalization is seen as prohibitively costly or as damaging to business operations by informal firms.

3 Intervention: audit process and selection

The URA conducts three types of audits, ranked from most to least intense: comprehensive, issue and desk. In this section, we both introduce each audit intervention, and describe the selection process. The focus of this study is the relative effect of a comprehensive audit compared to a desk audit.

Comprehensive and desk audits vary starkly in their intensity. In a comprehensive audit, auditors can investigate any tax base up to 5 years in the past, and the median comprehensive audit takes 93 days.[12] They always involve direct interaction with taxpayers and often include a physical visit to the firm. The average size of the auditing team for a comprehensive audit is 3 officers. In stark contrast, during a desk audit, the taxpayer receives a letter from the URA detailing what discrepancy in their tax return has been detected. The taxpayer is then asked to either amend the return to correct the discrepancy, or provide documentation explaining the discrepancy. A draft letter of the desk audit intervention can be found in Appendix Figure B2. Issue audits are a mix of the two audit interventions. Depending on the case, the audit may look more like a comprehensive audit or a desk audit, which is why we focus on the comparison between comprehensive and desk audits.

A data driven risk-calculation and the capacity of a taxpayer office together determine which taxpayers receive each audit type. Each year the Central Operations Office (COO) at the URA picks risk categories to focus on. In our year of analysis, they picked 35 risks. They then take the tax returns from two years prior and determine whether the risk applies to each taxpayer. If it does, they calculate a *risk score* for that taxpayer-risk category combination and estimate the *expected potential revenue* from resolving the risk for that taxpayer. If a risk category does not apply, the implied risk score and expected potential revenue is 0. Finally, for every taxpayer they aggregate the risk score and expected potential revenue of each underlying risk category to compute a taxpayer specific *total risk score* and *total expected potential revenue.* While the URA avoids auditing the same taxpayer for two years after an audit, tax return submitted in the immediate aftermath of an audit will be used in future risk calculations.

Once it has computed the risk scores, the COO determines how many comprehensive audits the central audit centre – the office responsible for conducting comprehensive audits – can do. Equipped with these numbers, it creates an ordinal ranking of firms. The ordinal rank prioritizes the total risk score over the total expected potential revenue. That is, firms with the highest risk score are always ranked higher. It is only for firms with the same risk score that the expected potential revenue determines their rank. The firms with the highest ordinal rank are assigned a comprehensive audit until the capacity of the central audit centre is reached.

Having concluded the comprehensive audit selection, it calculates the number of issue and desk

[12]While this is the official guidance, during qualitative interviews with firms, we heard stories of auditors raising tax issues that were more than 15 years old.

audits each *local* taxpayer station can do. Taxpayers are generally registered at the local taxpayer station that is closest to their premises.[13] For each taxpayer station, it ranks the remaining taxpayers – those not selected for comprehensive audits – by their expected potential revenue. The highest ranked firms within a local station are assigned to issue audits and the next set of firms are assigned to desk audits, until the capacity of the station is reached for each audit type. The remaining firms do not receive any compliance intervention that year. The details of the selection process vary annually. Our analysis exclusively focuses on financial year 2021, which is the earliest year for which we were able to pin it down.

Due to prioritizing different risk scores for different audits, the composition of audit assignment around the comprehensive audit threshold is not obvious. Figure 4 provides a scatter plot of firms along the ordinal ranking of the firm (horizontal axis) and their normalized total expected potential revenue (vertical axis). The total expected revenue is normalized to the station-specific cut-off that determines whether a firm receives issue or desk audits. There are two takeaways from Figure 4. First, as mentioned before, the comprehensive audit selection is entirely driven by the ordinal ranking of the firm. The highest ranked firms always receive comprehensive audits regardless of their normalized total expected potential revenue. Second, for firms close but below the comprehensive audit threshold, whether they receives issue or desk audits is driven by the normalized expected potential revenue of the firm. Because issue audits are hard to interpret and there are three times more desk audits around the threshold than issue audits, we compare comprehensive audits against desk audits. As a robustness check we compare comprehensive audits against the whole sample. The results are consistent.

Upon selection, all audit cases are sent to the relevant taxpayer station, where the supervisor distributes the cases among their officers. The officer who receives the case verifies that the data-driven risks are still present in their tax return, before moving forward with the audit.

For comprehensive audits, once the risk is verified, a notification is sent to the taxpayer informing them that they have been selected for an audit and asking them to prepare their records. The auditor either visits the taxpayer's premises and looks through the records, or the taxpayer comes to the URA offices with records in hand. If the auditor finds that the taxpayer is compliant, the audit is concluded. If the auditor determines that the taxpayer evaded taxes they can issue an assessment. An assessment is effectively a re-filing of a taxpayer's tax returns with revised amounts.[14] The taxpayer then has 45 days to object to the assessment. If they object, the case gets sent to a new department at the URA that will go through the audit and check that everything was done correctly.[15] If the taxpayer accepts the assessment, the audit is concluded and the higher

[13] The exceptions are: Large taxpayers, Medium taxpayers, government entities, and oil and gas companies. These have their own specialized taxpayer office. Taxpayers from the medium taxpayer office form part of our analysis, but the rest do not. The specialized taxpayer stations – apart from the Medium taxpayer office – do not follow the general compliance plan that the rest of the URA utilizes.

[14] For example, if a taxpayer was found to have underreported their sales by 10 million UGX in the VAT return for the month of June 2018, the assessment would override the original return and specify the higher amount.

[15] If the taxpayer is unhappy with the outcome of the objection they can file a legal complaint. Theoretically, this

tax liability is added to the taxpayer's ledger. If a firm was found to have underdeclared their tax liability, the auditor is legally empowered to add interest to the outstanding liability and levy fines on the taxpayer. In practice, fines are not frequently used to reduce the economic burden on the taxpayer. Appendix Figure B3 graphically illustrates the audit process.

The process for desk audits is much shorter. Upon verification of the discrepancy by the auditor, a letter is sent to the taxpayer informing them about the exact discrepancy that was discovered. The letter requests the taxpayer to either amend their tax returns eliminating the discrepancy, or provide documentation for why the discrepancy is correct. Taxpayer are given 7 days to respond to the request, after which the officer can issue an assessment. Similar to issue and comprehensive audits, the taxpayer can object to the assessment, otherwise the audit is concluded.

4 Administrative data & descriptives

We combine detailed administrative data on the universe of firms' tax filings with audit records, risk scores, and a survey, to evaluate the impact of receiving a comprehensive tax audit. In this section we describe each dataset, discuss how we construct our main sample of analysis, and present descriptive statistics.

4.1 Administrative data

The primary administrative dataset leveraged in this paper is a panel that combines the universe of CIT and VAT tax returns with audit records and risk scores, all provided by the URA. We also introduce the PAYE data from the URA and Census data from the Ugandan Bureau of Statistics (UBOS), because we use it to contextualize and describe our sample.

4.1.1 Sources

Administrative tax (CIT) The CIT dataset contains the universe of tax filings from all CIT registered firms for financial years 2013 to 2022. The CIT tax returns includes a full balance sheet and profit-loss statement of the firm. It contains information on total sales and a detailed breakdown of each type of cost. Our analysis focuses on CIT returns for the years 2019 – 2022. Baseline characteristics are drawn from the last unaffected year before the audit, which is financial year 2019.[16]

Administrative tax (VAT) The VAT data contains the universe of monthly returns filed between January 2013 and November 2022. The VAT data compliments the CIT data in two key ways. First, it is filed monthly, providing us with information at higher frequency. Second, it is an important tax for revenue collection in Uganda, comprising 30% of all revenue collected (OECD

can be escalated all the way up to the Ugandan supreme court. For a recent example see (The Independent, 2023).

[16]We exclude financial year 2020 because depending on when a firm files a tax return and when it was audited, the 2020 return may be affected.

et al., 2023). We focus on VAT returns filed between June 2019 and November 2022. Baseline characteristics are based on the returns filed between June 2019 and July 2020. This is the financial year the URA considers in their risk calculations and corresponds with the baseline data from the CIT.

Administrative tax (PAYE) The PAYE data contains the universe of monthly returns filed between January 2013 and June 2022. The PAYE complements the other administrative datasets by providing us with the number of employees at the firm. We do not have any information on PAYE returns post-audit, so we use this dataset exclusively to describe the firms in our sample.

Audits and risk scores Data on comprehensive and issue audits come from the URA's online database, which contains the universe of all audits that were *assigned* from January 2013 to January 2024. This is important because even if an audit was assigned and sent to a taxpayer station it was not necessarily executed. The data contains information on the type of audit (issue/comprehensive), when the audit was assigned, started, and completed, and what tax bases the audit was conducted on. If an audit does not have a start date and there is no record of an audit outcome, we assume it did not happen.[17] The data also records which supervisor was in charge of the audit and the audits' outcome. For our analysis we focus on audits that were assigned in financial year 2021/22.

Desk audit information is not included in the URA's online database, because they are considered "compliance advisory" interventions rather than audits by the URA. We received information on desk audit allocations for financial year 2021/22 from the URA's annual compliance report. While the report does not include the start date of the desk audit, it does include information on when the desk audit finished. Given the timeline of a desk audit, we assume that the desk audit started 7 days before its completion date. Furthermore, we assume the desk audit did not happen if there is no completion date.[18]

Risk score information was shared with us by the URA officer in charge of audit selection during financial year 2021/22. In that financial year the risk scores and expected potential revenue calculations were based on a taxpayers' discrepancy across 35 different risk categories. We do not only observe a firms total risk score and expected potential revenue, but also their risk score and expected potential revenue for each individual risk category. The aggregate risk score and expected potential revenue are an unweighted aggregate of the scores and expected potential revenues of each of the underlying risks.

It is possible for a firm to receive several audits in a given year. In particular, after receiving a desk audit, the auditor may trigger an issue or comprehensive audit because they believe there are more compliance risks to investigate. For the purpose of our analysis, we consider the first

[17] Based on conversations with auditors at the URA that seems a reasonable assumption. The number of executed audits form part of the auditors annual evaluations, hence they have every incentive to add it to the online system.

[18] Desk audits are short interventions, so they are unlikely to lead to long-standing disputes which could cause an audit not to be completed.

audit intervention that a firm receives to be the assigned one. Anything happening afterwards is endogenous to the outcome of the first audit intervention.

Census The Census of Business Establishments (CoBE) allows us to compare our sample of firms to the universe of all firms in Uganda. The CoBE was conducted by the Ugandan Bureau of Statistics (UBOS) in financial year 2020/21, and contains information on the geographic location, sector, number of employees and turnover of all firms in Uganda. We cannot match firms one-to-one so we construct size-categories and compare the number of firms in the size categories.

Figure A1 provides a visualization of the time period covered by each of the datasets used in the analysis.

4.1.2 Sample selection criteria

To construct our main sample we merge the administrative tax data with the risk scores from the audit selection process and audit information. We impose a number of restrictions to facilitate our analysis. Table A1 in the Appendix highlights how the sample changes when imposing each restriction.

We start with the universe of firms that were flagged for at least one risk in financial year 2021.[19] We proceed to remove firms registered at specialized taxpayer stations. This excludes firms in the Large Taxpayer Office, Oil and Gas office and the Public Sector Office. These offices do not follow the compliance improvement plan of the rest of the URA, instead conducting their own compliance strategies. Furthermore, the administrative capacity at these stations is different from non-specialized offices, they typically have a higher officer-to-taxpayer ratio. We also remove firms in the mining sector, because 40% of firms in the mining sector are registered with the Oil and Gas office. The remainder would be a heavily selected sample.[20]

We remove firms that are audited the year before our year of analysis. Taxpayers are typically only audited two years in a row if the URA needs to follow-up on a prior issue. Including these set of audits could confound our estimates for two reasons. First, the firm is selected into the audit for reasons not related to the risk scores, hence the audit is not induced by the instrument. Second, the audit in the previous year could already have changed the firm's behaviour, leading us to identify a different treatment effect.

We restrict to "active" firms, which we define as any firm that filed either a CIT or VAT return at some point prior to the audit. Finally, we only consider stations where at least one of the firms received a comprehensive audit.

[19] Firms are defined as taxpayers who are registered as non-individuals and private entities. This excludes entities registered as clubs, estates or trusts, government bodies, international organizations, and NGOs.

[20] The highest percentage of firms registered with one of the specialized offices in any of the other sectors is manufacturing at 8%.

After imposing each step, we remain with 20,838 firms, of which 231 were selected for comprehensive audits, 1,041 for issue audits, 12,783 for desk audits, and 6,783 received no intervention. The final sample used to identify our treatment effect is restricted to the 13,014 firms assigned to comprehensive and desk audits.

4.1.3 Descriptives

In Table 1 we show that our sample of firms is skewed toward the upper tail of the firm size distribution, both among formal firms and the universe of firms in Uganda. We leverage information from tax filing records to assess how our sample compares to the universe of tax filing firms. We leverage the Census of business establishments from UBOS to determine how our sample compares to the universe of all firms in Uganda.

Table 1 presents summary statistics for our samples, and p-values for whether the means across samples are significantly different. In Column (1) we present averages for all firms that filed CIT or VAT taxes at some point prior to the audit year. This also includes firms that did not receive a risk score. Column (2) restricts to the full comprehensive sample, whereas Column (4) restricts to the part of the comprehensive sample that are used to identify our treatment effects, namely the set of firms that tend to fall within the optimal bandwidth.[21]

Firms in the comprehensive sample – shown in Column (2) – are on average larger than the universe of firms filing CIT or VAT. They report higher sales and purchases, higher VAT, but lower VAT liabilities. Similarly, they report higher sales and costs in the CIT data, higher profits and in this case also higher tax liabilities. One caveat is that they tend to report roughly half the number of employees. Note that firms in the comprehensive sample are far more likely to file tax returns across each of the three tax bases presented here. This is consistent with the URA leveraging information from a firms' tax return when calculating the risk scores. Finally, we consider the sector decomposition in the two samples. Column (3) presents the p-values for whether the averages of the full comprehensive sample are significantly different from the universe of firms. 24 of the 26 variables are significantly different at the 5% significance level.[22]

These differences become more pronounced when we consider the subset of firms in the comprehensive sample that tend to fall within the optimal bandwidth. In this case, firms are larger across all measures we have access to, even number of employees. Despite being larger, they report lower VAT liabilities and similar CIT liabilities compared to the universe of firms. This is in line with the objectives of the URA. Comprehensive audits are the heaviest tax compliance intervention the URA has at its disposal. These interventions are targeted towards large taxpayers that are not contributing sufficiently (or at all) towards revenue collection. Overall, Table 1 demonstrates that the subset of firms we study are large formal firms in Uganda. The median firm within the optimal

[21]The optimal bandwidth changes across outcome. We restrict to firms that fall within the median optimal bandwidth across all baseline covariates used in the balance test, shown in Table A2.

[22]The p-value of a joint f-test on all variables is 0.000 as shown in the second to last line.

bandwidth is at the 80th percentile of the formal firm-size distribution.

To compare our sample of firms to the universe of all firms in Uganda, we turn to the 2020 Census of Business Establishments. The number of employees in the census is measured in bins. To facilitate the comparison we construct equivalent bins using the information in the PAYE returns, and divide the number of firms in our samples of interest for each category by the number of all firms reported in the census. This allows us to indirectly assess the share of firms in each size category.[23] In Appendix Figure A2 we show the share of firms filing taxes and the share of firms in the comprehensive sample for each size category. As expected, both the set of firms filing taxes and the set of firms in the comprehensive sample are heavily skewed towards the upper tail of the size distribution. The number of firms that file PAYE taxes add up to 95% percent of all firms that employ more than 100 employees (19% for firms in the comprehensive sample). In stark contrast, the share of firms filing taxes among firms with one employee is merely 2% percent of all firms in the census (0.5% for firms in the comprehensive sample). Recall that the firms that tend to fall within the optimal bandwidth are, if anything, even larger than the firms in the comprehensive sample, underlining that we are investigating the impact of heavy enforcement interventions on firms that are relatively large in the Ugandan context.

5 Empirical strategy

We exploit discontinuities created by the audit selection process to estimate the local average treatment effect of a comprehensive tax audit. Our estimation strategy is designed to mimic the audit selection process. The validity of our design relies on three key assumptions: 1) No manipulation of audit assignment, 2) Observed differences around the discontinuity are only due to the difference in audit assignment, 3) Changes in audit assignment has large effects on the likelihood of receiving an audit. We provide evidence that each assumption holds.

Our first-stage equation – capturing the increase in the probability of receiving a comprehensive audit above the threshold – is written formally in equation 1.

$$D_f = \alpha\, \mathbb{1}\{z_f \geq l\} + \underbrace{\lambda_1\, \mathbb{1}\{z_f \geq l\}(z_f - l + 0.5) + \psi_1 z_f}_{\text{linear slopes in ordinal rank}} + \underbrace{\Gamma_1 \mathbf{X}_f}_{\text{Controls}} + \varepsilon_{1f} \tag{1}$$

Where D_f is an indicator for whether firm f received a comprehensive audit, z_f is the ordinal rank of firm f used for selecting comprehensive audits, and $\mathbf{X}_f$ is a vector of controls. Our estimation model allows for separate slopes on each side of the discontinuity. l is the threshold for the assignment of comprehensive audits; the ordinal rank of the last firm that was *not* assigned to a comprehensive audit. We recentre the threshold by $l+0.5$, such that the discontinuity is measured midway between the last firm not assigned to a comprehensive audit and the first firm assigned to a comprehensive audit.

[23] There is no unique identifier that matches across the tax data and the census.

Our vector of controls, $\mathbf{X}_f$, includes station fixed effects and separate indicators for whether a firm is in the VAT or CIT sample. Both set of controls are included to mimic the selection procedure at the URA. We include station fixed effects because the counterfactual – desk audits – is conducted and selected by officers at the local stations, not the central audit centre. While the central operations office assigns an initial set of firms for desk audits, officers at the stations have some leeway to adjust the selection. Station fixed effects control for any differential selection into desk audits.[24] We include two indicators for whether the firm is in the VAT or CIT sample because the URA targets comprehensive audits at firms that are in more than one tax base.

Thresholds: While we do know how firms are selected for audits we do not know exactly where the cut-off was set. The information could not be retrieved, probably because the URA did not keep a record of it.[25] We therefore employ a data-driven method to identify the universal threshold for comprehensive audit assignment. We take the universe of firms (N = 20,838) that remain after imposing each cleaning step and rank the firms from highest to lowest based on their risk score (first) and expected potential revenue (second). That is, we rank firm by their risk score, and for firms that share the same risk score we rank them by their expected potential revenue. Once ranked, we conduct a structural break test to find the single largest change in the likelihood of being *assigned* a comprehensive audit (which is distinct from receiving an audit). Because we know the initial number of firms assigned to comprehensive audits, an alternative approach would be to assume that the 289 firms with the highest rank were assigned a comprehensive audit. Both approaches yield similar results, and we conduct robustness checks using the alternative threshold.

Running variable: Our running variable is the ordinal ranking of the firm among the 13,014 that were assigned to comprehensive and desk audits. Once we derive the threshold, we rank firms again according to their risk score (first) and potential expected revenue (second). We then calculate the ordinal rank of the firm relative to the firm just below the threshold. We leverage the ordinal rank because it drives selection into comprehensive audits as illustrated in Figure 4.[26] By calculating it on the sample of firms assigned to desk and comprehensive audits we avoid having gaps in the data from not including firms assigned to issue audits or that did not receive any intervention. As a robustness, we include everyone assigned to any enforcement measure.

Counterfactual: As discussed extensively in section 3, we compare comprehensive audits against desk audits. This is both because issue audits are hard to interpret and there are three times as many desk audits around the comprehensive audit threshold as illustrated in Figure 4.

[24] There is evidence that the assignment of desk audits from the central audit centre was not always binding. Some stations decided to give all firms that had a risk score a desk audit.

[25] We received the risk scores from the supervisor in charge of audit selection during our year of analysis. The risk scores were stored on his work laptop and that was the only existing copy. He did not have a record of where the threshold was set, making it unlikely it was recorded.

[26] Other studies with similar set-ups have used a score standardized around the cut-off (Busso et al., 2023). We prefer the ordinal ranking because it is the driving force behind selection into comprehensive audits.

To capture the reduced form effect of being above the comprehensive audit threshold on an outcome of interest, we run the following reduced form regression discontinuity equation:

$$Y_f = \beta_{RF}\, \mathbb{1}\{z_f \geq l\} + \underbrace{\lambda\, \mathbb{1}\{z_f \geq l\}(z_f - l + 0.5) + \psi z_f}_{\text{linear slopes in ordinal rank}} + \underbrace{\Gamma \mathbf{X}_f}_{\text{Controls}} + \epsilon_f \tag{2}$$

The only difference with equation 1 is that we replace D_f with an outcome of interest Y_f.

Finally, we are also interested in understanding the implied effect of *receiving* a comprehensive audit on the outcome of interest, not just the effect of being above the threshold. To derive the effect of receiving a comprehensive audit we run a two-stage least-squares (2SLS) regression, where the first stage is given by equation 1 and the second stage is written below:

$$Y_f = \beta_{2SLS} D_f + \underbrace{\lambda_2\, \mathbb{1}\{z_f \geq l\}(z_f - l + 0.5) + \psi_2 z_f}_{\text{linear slopes in ordinal rank}} + \underbrace{\Gamma_2 \mathbf{X}_f}_{\text{Controls}} + \varepsilon_{2f} \tag{3}$$

Where $\hat{D}_f$ is the predicted increase in the probability of receiving a comprehensive audit from the first stage. Throughout our results we present both reduced form RD estimates and 2SLS estimates. β_{RF} indicates the former whereas β_{2SLS} indicates the latter.

Bandwidth: The bandwidths are selected using a data-driven approach implemented through the "rdrobust" package by Calonico et al. (2014). We use the universal optimal bandwidth, but also show that the main results are robust to changes in the bandwidth.

5.1 Increase in the likelihood of receiving a comprehensive audit

The discontinuity created by the audit selection process creates large differences in the likelihood of receiving a comprehensive audit, the most intense tax enforcement intervention at the URA. As outlined in more detail in section 3, these audits last for 3 months on average and can involve examination of all records in relation to all tax bases up to 5 years in the past. As such, they represent in-depth examination of a firms' accounts.

Figure 5 shows that crossing the threshold increases the likelihood of receiving a comprehensive audit by 69 p.p., which is highly statistically significant. The F-stat for a two-sided test of the coefficient being greater than 0 is 107. We plot the residualized probability of receiving a comprehensive audit along the ordinal ranking of firms and the share of firms receiving a desk audit in each bin. We construct residualized outcomes to match the variation shown graphically to the regression results.[27]

Figure 5 highlights three important aspects of the audit selection process. First, the probability of receiving a comprehensive audit and the share of firms receiving a desk audit do not add up

[27] We run a regression of the outcome variable against a constant, α, station fixed effects, and two indicators for whether the firm was in the CIT or VAT sample, captured by $\mathbf{X}_f$. Then we add back the average of the outcome for scaling purposes. Formally, the residualized outcome is given by: $\tilde{Y}_f = Y_f - (\hat{\alpha} + \hat{\mathbf{X}}_f) + \bar{Y}_f$.

to 1 because not all the firms assigned a desk audit received one, underlining the importance of controlling for station fixed effects. Second, no firms above the threshold were assigned a desk audit because *all* of them were assigned comprehensive audits.[28] If the comprehensive audit was not executed, the firm was not downgraded to a desk audit. Instead the audit would be carried over into the next financial year or dropped. Finally, the probability of receiving a comprehensive audit decreases in the ordinal ranking above the threshold. This is driven by auditors choosing "easier" audit cases first. Firms with a higher risk score have more risk factors to investigate, implying more challenging audits. The performance of auditors is measured in the number of audits they execute and the amount of revenue they collect, incentivizing them to avoid difficult and long-lasting audit.

5.2 Instrument validity

We have shown that the discontinuity causes large changes in the likelihood of receiving a comprehensive audit. The two remaining assumptions for the validity of our RD design are: 1) No manipulation of audit assignment, and 2) Observed differences around the discontinuity are only due to the difference in audit assignment. We perform a series of balance and validation tests to verify that both assumptions hold.

Manipulation by firms: The first concern is that firms might manipulate their risk score to not be assigned a comprehensive audit. To successfully manipulate selection into comprehensive audits, a firm would have to do the following. First, find out which risk parameters are used to conduct the risk analysis in a given year. This is hard to do. The group in charge of conducting the risk analysis is small, there were 35 risk parameters involved in the calculation, and both the officers in charge of risk calculations and the risk parameters change every year. Furthermore, the risk score calculations are divided up across officers within the risk score team so that no single officer knows how every risk parameter is computed. Second, find out how the risk profile of the firm compares to that of other firms. What matters for selection is not just a firms' risk score, but its risk score relative to the risk score of other firms. Not only would the firm have to manipulate its own risk score, but ensure that it is low relative to that of other firms. Finally, the firm would have to adjust its tax returns from two years prior. Risk calculations are done retrospectively. Even if a firm gained access to someone that knew how each parameter was calculated in the current year, it would have to file an amendment for its old tax return *before* the risk calculations were completed. Since risk selection and calculations go hand-in-hand, it will be extremely difficult for a firm to adjust its past tax return in time. The assignment process is therefore unlikely to be affected by corruption. Corruption may influence whether a firm *receives* an audit, but that does not affect the validity of the instrument remains.

A density test would not be appropriate in this context because the running variable is the ordinal rank of firms. To alleviate concerns that the ordinal ranking is hiding attempts at manipulation we

[28] In Appendix Figure A3 we show this more clearly by plotting the residualized probability of being assigned to a comprehensive audit. Literally every firm above the threshold was assigned a comprehensive audit.

conduct two exercises. First, we show in Appendix Figure A4 the mass of firms at each discrete risk score. There is no evidence of differential mass on each side of the threshold. Second, we conduct the test for density manipulation suggested and developed by Cattaneo et al. (2018); Cattaneo et al. (2020) for an alternative running variable designed to capture the relative importance of risk score and expected potential revenue in selection. The alternative running variable is defined as the risk score value plus the expected potential revenue of the firm relative to the maximum revenue of all firms with the same risk score.[29] The density is highly non-normal towards the lower tail of the distribution, as seen in Figure A4. To ensure that the density test can capture differences in mass around the discontinuity, we restrict the test to run within the optimal bandwidth. There is no evidence of density manipulation around the threshold. The resulting p-value is 0.232. A histogram with the density plot and function estimated using the "rddensity" command is shown in Appendix Figure A5.

Balance: One might be worried that the selection procedure by the URA causes firms above and below the threshold to be systematically different. To assess whether this is a concern we conduct a series of balance checks. First, we run equation 1 for a large set of baseline covariates, where the baseline covariates replaces the indicator for whether a firm was audited in the regression. The results can be seen in Appendix Table A2. We reject the null hypothesis of no difference for none of the 26 baseline covariates. Second, we conduct a test for the joint significance of all baseline covariates combined, and do not reject the joint null hypothesis of balance in all baseline covariates across various specifications. We conduct this test varying both the bandwidth and whether we impute 0's for missing values. As seen in the last rows of Appendix Table A2, the p-value of an f-test of joint significance is 0.151, 0.531 when using the minimum and maximum optimal bandwidth, respectively. As robustness, we also conduct a dimension reduction exercise discussed in more detail in section 6.3.

Manipulation by URA: One might be concerned that officers at the URA change the threshold. It could be the case that the central audit centre changes the threshold to include or exclude firms with certain characteristics. We show that this is unlikely to be the case. First, if firms right below the threshold are systematically different from the ones right above, this should show up as violation in the balance tests. As shown in Table A2, we did not find evidence of such violations. Second, the difference in the threshold from the data-driven method and what would be implied from the initial assignment of comprehensive audits is minimal. We discuss robustness to using the assigned threshold in section 6.3.

[29] Formally, let zn_f be the new running variable for firm f, $risk_{j(f)}$ be the risk score of the group of firms j that f belongs to, and r_f be the expected potential revenue of firm f. Then, formally $zn_f = risk_{j(f)} + \frac{r_f}{\max(r_{j(f)})}$.

6 Results using administrative tax data

The purpose of an audit for the URA is to raise revenue through correcting past evasion and in the post-audit periods through deterrence. In this section, we show that while audits do detect evasion, post-audit compliance drastically reduces both because firms exit the tax system and because the remaining firms report lower tax liabilities.

6.1 Tax corrections

We first investigate whether comprehensive audits differentially increase revenue potential through detecting and correcting past evasion. This is looking at the outcome of the audit itself and not considering filing responses from firms in the aftermath of the audit. From the audit data, we know what corrections were filed by the URA for previous tax returns as a result of the audit. Merging the audit corrections with the original tax returns, we can discern to what extent the corrections increased firms tax liability. Unlike comprehensive audits, desk audits do typically not lead to corrections, but instead ask the taxpayer to self-correct their past tax returns. To measure the differential increase in tax liability from a tax correction we therefore compare the increase from corrections (among firms receiving comprehensive audits) against the increase from self-corrections (among firms receiving desk audits).

Tax liabilities and corrections are notoriously noisy, making it often hard to detect average effects even in experiments with a large sample size (Pomeranz, 2015). We present three transformations of the amount variable when discussing impacts on tax liabilities and corrections. First, we winsorize the top and bottom 1% of the amount variable (Advani et al., 2021). Second, we scale the amount variable by firms' sales at baseline and winsorize the scaled variable at the top and bottom 1%, following Kennedy et al. (2024). Third, because we do have a significant number of zeros, we also use the inverse hyperbolic sine transformation (IHS) of the outcome variable. All three variations are standard in the literature.

Figure 6 shows that comprehensive audits induce differential changes in firms tax liabilities through tax corrections. In Panel (a) we show that firms above the discontinuity are 32 p.p. more likely to experience an increase in their tax liability from corrections. The implied effect of receiving an audit using the 2SLS is an increase of 46 p.p. In Panel (b) we show that the corrections lead to an increase in the tax liability of 29 thousand USD, significant at the 1% level. Scaling the increase in the tax liability by average baseline sales (shown in Panel (c)) suggests that the correction amount is approximately equal to 7.3% of baseline annual sales.

Our estimate do not imply that the URA actually collected 29 thousand USD from the marginal comprehensive audit. We do not have information on the actual payment made by each firm, hence the number should be interpreted as the upper bound on the potential revenue collected by the URA through correction of past evasion. We discuss robustness in section 6.3.

6.2 Post-audit compliance behavior

Having established that comprehensive tax audits increase firms tax liability through correcting past evasion, we next investigate the tax filing behavior of firms in the financial years after the audit. We primarily focus on CIT because we have information on filing behavior from two years post-audit for the CIT and only 5 months post-audit for the VAT.

6.2.1 Post-audit tax filing

To measure whether firms stop filing taxes we focus on the CIT tax base, which we can follow for two years post-audit.

In the period immediately after the audit – June 2022 to June 2023 – firms are less likely to file a CIT tax return. Figure 7(a) shows that firms above the discontinuity are 20 p.p. (35% compared to the control mean) less likely to file a CIT return in the year after the audit. The implied effect of receiving an audit, given by the 2SLS, is a decrease of 30 p.p. Both the reduced form and two stage-least-squares effects are statistically significant at the 1% significance level. The result is driven by firms that were marginally selected for a comprehensive audit.

The effect persists and becomes slightly larger in the second year post-audit. Panel (b) of Figure 7 shows that firms receiving comprehensive audits are 34 p.p. (51%) less likely to file a CIT return in the second year post-audit, compared to 30 p.p. one year post-audit. This suggests that the results are not driven by aggressive tax planning in the immediate aftermath of the audit. If firms believe they are not likely to be audited again for some time following an audit, the best time to evade taxes may be in the immediate aftermath of one. In the literature this has been colloquially referred to as the "bomb-crater" effect (Alm and Malézieux, 2021; DeBacker et al., 2015; Kasper and Alm, 2022; Kasper and Rablen, 2023). This does not appear to be driver in our case. We interpret our results as being driven by firms leaving the formal sector in the aftermath of an audit.

We do two additional checks to verify that the results can be interpreted as real firms exiting the formal sector. In Appendix Figure A6, we show that the result also holds when we use an indicator for whether a firm either filed a VAT or CIT return. The reduced form coefficient suggests a reduction of 16 p.p. and remains statistically significant at the 1% level. Firms do not just stop filing the CIT, but also other taxes such as the VAT. The results are also not driven by fictitious firms. It could be the case that the "firms" leaving are fictitious firms that only exist to create and sell fake invoices (see Carrillo et al. (2023) and Waseem (2023) for two recent papers exploring this phenomenon in developing countries). To verify this is not the case, in Column (4) of Table A5, we drop firms that have been identified as fictitious by the URA.[30] The result does not change. Our survey results, presented in section 7 further support the interpretation that these are not "fictitious"

[30] We received information on all firms identified as being "suspicious" and all the firms "confirmed" to be fictitious by the URA up until November 2023. We take a conservative approach and include an indicator for all firms labeled as "suspicious".

firms.

We discuss further robustness checks in section 6.3. Taken together we interpret our results as strong evidence that audits are inducing real firms to exit the tax system.

6.2.2 Post-audit tax liability

The effects on filing taxes are strikingly large, but may not have implications for the tax liabilities submitted by firms in the aftermath of the audit. If the firms that stop filing taxes are small and did not pay substantial taxes at baseline, removing them from the tax system would have little revenue implications for the tax authority. Furthermore, if the firms that kept filing taxes submitted substantially higher liabilities post-audit, tax liability may actually increase despite some firms exiting the tax system. This is the case in Harju et al. (2024). We again focus primarily on CIT taxes, but show that the results hold for VAT as well. Because we are interested in the overall change in tax liability we impute zero's for firms that did not submit a tax return.

When we focus on CIT returns that pertain to the year after the audit (but filed two years after) – effectively allowing business operations to be affected by the audit – we find strong negative effects on CIT liability. Note this does not include the increase in tax liability triggered by the tax corrections, such increases are paid separately and would not be reflected on a tax return. As shown in Column (1) of Table 2 we find that the probability of reporting a positive tax liability for firms that received a comprehensive audit falls by 27 p.p. (53%), which is marginally statistically significant. We present three variations of the CIT liability amount variable, all of which point to a negative effect. The winsorized CIT liability submitted to the URA decreases by 5,463 USD (shown in Column (2)), this represents a decrease of 2 p.p. relative to baseline sales (shown in Column (3)). Finally, the inverse hyperbolic sine transformation also points towards a large negative effect. RD figures with the equivalent outcomes can be found in Appendix Figure A7. Overall we take this as strong evidence that comprehensive audits have a negative impact on post-audit CIT liabilities.

Interestingly, when we focus on tax returns filed in the immediate aftermath of the audit, but that pertain to the year of the audit, we find no effect on CIT liabilities as shown in Appendix Table A3. Throughout the coefficients are small and statistically insignificant. This is probably because most comprehensive audits occur towards the end of the financial year, hence it is unlikely to affect the operations of the firm. Any differences would therefore have been driven by post-operation adjustments in what is reported to the URA. This provides the first indication that the effects we observe are driven by adjustments in firm operations.

In Appendix Table A4 we break down the overall decrease in tax liability into extensive and intensive filing margins. We leverage reduced form coefficients for this exercise. As shown in Column (2) firms are 23 p.p. less likely to file a tax return in the second year post-audit. Conditional on filing, firms are 19 p.p. less likely to file a tax return with a positive liability, albeit this is only marginally significant (Column (3)). Conditional on filing a return with a positive liability, firms report liabilities that are approximately 15% smaller, but this is statistically insignificant (Column (4)). We conclude that our results are primarily driven by firms not filing taxes, with the reduction in the probability of filing a positive CIT liability also playing a minor role.

As a final check, we verify that our results are not only driven by the CIT tax base. Leveraging information on 5 months post-audit from the VAT data, we investigate whether the VAT liability reduces. We use the same outcomes as for the CIT analysis and take the cumulative sum across the five months.[31] The results are shown in Table 3. RD figures with the equivalent outcomes can be found in Appendix Figure A8. While all variations of the outcome variable point towards a negative effect, the coefficient is only significant in Column (2) where the outcome is the winsorized amount of VAT liability submitted. The coefficient suggests that the total VAT liability submitted in the first five-months post-audit reduces by 30,833 USD. While this is a far larger magnitude than what we found in the CIT, given that only one of the specifications is significant, we interpret this as suggestive evidence of a negative effect.

Altogether we conclude that comprehensive audits have a negative effect on post-audit tax liabilities for the marginally audited firms through two channels: 1) causing firms to leave the tax system, and 2) reducing the probability of reporting positive liabilities. However, it is the former that is driving the aggregate decrease in revenue. We discuss robustness in the section below, and then investigate which firms stop filing taxes in the aftermath of the audit.

6.3 Robustness

We conduct a series of robustness checks to verify that our results are not sensitive to key adjustments.

Alternative bandwidths. We first present estimates using alternative bandwidths for our main outcomes of interest, shown in Appendix Figure A9. Our estimates are robust to even large changes in the bandwidths. Furthermore, as expected, the estimates become larger the closer we are to the threshold. This is consistent with the results shown earlier: the firms closer to the threshold are driving the results.

Alternative specifications. Next we show that our results are robust to using alternative controls. We fix the bandwidth to be the bandwidth used in our main specification and change the controls we include. In Table A5 we focus on whether a firm received a comprehensive audit and whether they filed tax returns. In Table A6 we focus on firms tax liabilities. In column (1) we include no controls, in column (2) we add station fixed effects, column (3) is our baseline specification, and column (4) removes firms that are fictitious. In column (5) we use the controls from our baseline specification and add sector fixed effects, and in column (6) we include controls for each of the underlying risk categories. This adds 70 separate controls which control for the risk score

[31] We will extend the time-frame of the analysis once more recent data becomes available.

and expected revenue of each of the underlying 35 risk parameters. Finally, in Column (7) we run a lasso specification to select the controls. Both in Table A5 and A6 effect sizes are stable across a large number of specifications and almost always statistically significant.

Alternative threshold. As discussed in section 5, there are two ways of identifying the threshold. Our baseline approach is conducting a structural break test to find the largest change in the probability of being assigned a comprehensive audit. Since we know the initial number of firms assigned to comprehensive audits, we can also identify the threshold by assuming the the highest ranked 289 firms were assigned comprehensive audits. In Table A7 we show that our main results are generally robust to using this alternative threshold.

Alternative sample. In our main specification, we restrict to firms that are assigned to comprehensive or desk audits. We then calculate the ordinal ranking of firms based on the restricted sample. One may be worried that by moving some firms closer to the threshold on the ordinal ranking our results are relying on assigning different weights to some firms. Alternatively, one may be worried that by comparing firms with different normalized expected potential revenue (see Figure 4) we are introducing imbalances to our estimation (though we do not find any evidence of that when we conduct our balance tests). To address both concerns we run a specification where we include all firms assigned to any intervention. That is, we run an RD on all the firms that are shown in Figure 4. In Table A8 we show that our main results are generally robust to using this alternative threshold.

Dimension reduction exercise. We conduct a dimension reduction exercise where we predict our outcome variables using all our 26 baseline covariates. We then run the RD on the predicted outcome. The results are displayed in A9 and confirm that imbalances in baseline covariates do not explain the large impacts we find.

Having verified the robustness of our results we next investigate which firms stop filing taxes in the aftermath of the audit.

6.4 Characterizing compliers: Who are the firms that stop filing taxes?

To explore which firms stop filing taxes post-audit, we estimate the average characteristics of the compliers (Abadie, 2002; Imbens and Rubin, 1997), following the methodology by Pinotti (2017). We conduct the analysis with two outcomes: the probability of receiving a comprehensive audit, and the probability of not filing CIT taxes for two years following the audit (our proxy for exiting the tax system). This analysis relies on the monotonicity assumption. That is, being above the threshold does not decrease the probability of receiving an audit, and it does not increase the probability of filing taxes. Given our analysis up until this stage, both assumptions seem plausible on average. However, we recognize that it is possible that some firms are more likely to file taxes after receiving an audit because they believe they are under greater scrutiny from the tax authority (there may be some defiers). To the extent that this is the case, our estimates should be interpreted as suggestive.

We run a two-stage least squares regression whose first and second stages are given by:

$$h_f = \alpha\, \mathbb{1}\{z_f \geq l\} + \mathbb{1}\{z_f \geq l\}(z_f - l + 0.5) + z_f + \Gamma \mathbf{X}_f + \varepsilon_f \tag{4}$$

$$h_f \times k_f = \theta h_f + \mathbb{1}\{z_f \geq l\}(z_f - l + 0.5) + z_f + \Gamma \mathbf{X}_f + \varepsilon_f \tag{5}$$

Where h_f changes depending on which compliers we are looking at and k_f is the baseline characteristic of interest. Specifically, h_f is an indicator for either 1) whether the firm received a comprehensive audit, or 2) the probability of not filing CIT taxes for two years following the audit. The characteristics of the compliers are given by θ. Similar to equation 1, $\Gamma \mathbf{X}_f$ is a vector of controls including station fixed effects and separate indicators for whether the firm is in the VAT and CIT sample, l is the threshold for comprehensive audits, and z_f is the ordinal ranking of firms.

Figure 8 presents results on key variables of interest. The first column ("Sample") in each Figure presents the baseline average for the sample that falls within the optimal bandwidth for that variable. The second column ("Audited") is the baseline average of the firms that were induced into comprehensive audits (the comprehensive audit complier mean). Finally, the third column ("Not filing") is the baseline average for the firms that were induced to stop filing taxes (the non-filing compliers mean). A Table with results for all baseline variables, and p-values for the difference between the complier means and the sample averages, can be found in Appendix Table A10.

We first focus on the set of firms induced into being audited. Firms induced into comprehensive audits have slightly higher sales at baseline (Panel (a)), lower value of plants and machines (Panel (b)), have a lower sales to cost ratio (Panel (c)), and report higher VAT and CIT liability (Panel (d) and (e)). They are also less likely to be in the service sector (Panel (f)), and, as can been seen in Table A10, significantly more likely to be in the manufacturing sector. This is consistent with the URA targeting larger firms that pay several tax heads, which are typically firms not in the service sector.

Interestingly, the pattern changes significantly when focusing on the firms that are induced into not filing taxes. They report significantly lower sales, use less machines and plants and are more likely to be in the service sector. This helps explain why they are able to disappear, they do not have significant amounts of capital that would be foregone if they close shop. However, crucially, they have a high sales to cost ratio, especially compared to the set of firms induced into audits, and report significantly higher VAT liabilities at baseline. The firms induced into not filing taxes are relatively profitable and important contributors to revenue collection. Again, it is important to highlight that these are not small firms as such. Inverse hyperbolic sine sales of 5.40 puts a firm around the 45th percentile of the formal firm sales distribution in 2019.

Overall our evidence suggests that comprehensive audits induce the *marginal formal* firm – smaller formal firms that do not require a lot of capital – to exit. They appear to be profitable firms that are important contributors to revenue collection at baseline. This is exactly the type of firms that drive growth (Quinn and Woodruff, 2019), and tax authorities should want to keep in the tax system.

6.5 Discussion: Net Revenue and MVPF

Results from the administrative tax data clearly indicate that comprehensive audits can "backfire": they cause firms to exit the formal sector and reduce revenue potential. However, our results also help rationalize why the URA still audits these firms. We find that comprehensive audits lead to an increase in tax liability of around 29,000 USD from correcting evasion among marginally audited firms. That does not necessarily imply that the URA collects 29,000 USD from the marginal comprehensive audits. We do not have information on what each individual firm paid, but leveraging information from audit reports for our year of analysis we know that the URA collected 60% of all the tax corrections it issued. Under the assumption that the URA collects the same share from each firm and that conducting the audit was costless, the marginal audit raised roughly 17,400 USD.

However, post-audit reporting behavior demonstrates that this comes at the cost of a reduction in potential revenue collection post-audit. Our estimates suggests the URA is losing 31,000 thousand USD from firms reducing their VAT liabilities in the first five months for the year after the audit, and 5,400 USD from a reduction in CIT liabilities. This highlights a trade-off. Clamping down on evasion does raise revenue in the short-term, but at the cost of reducing revenue in the following year(s).

The net effect of the marginal audit on revenue once compliance behavior in the following year is considered is unambiguously negative. Assuming the URA will eventually collect the full amount of tax corrections, that the cost of the audit is zero and that the treatment effects we identify only persist for the period in our analysis the net effect is: $29,000 - 31,000 - 5,400 = -7,400$ USD. That is, even the *upper bound* on revenue collected from the marginal audit is negative. Under the more realistic assumptions that the URA collects 60% of tax corrections and that the treatment effects persists for the entire year post-audit, the net effect is: $17,400 - 74,400 - 5,400 = -62,400$ USD.

Conducting a marginal value of public funds approach following Boning et al. (2023) would suggest that the welfare effect of the marginal tax audit is negative. The welfare effect of a tax audit in this framework is the ratio between the willingness to pay (WTP) to avoid an audit by the taxpayer and the net revenue gain from the audit. Formally,

$$MVPF^{\text{audit}} = \frac{\text{WTP to avoid audit}}{\text{Net government revenue raised}}$$

As discussed above, the revenue raised for the government from the marginal audit is negative (even before considering the cost to the government of doing the audit). Because the WTP to avoid an audit is always positive, it is immediately clear that the marginal value of the audit is negative.

This calculation speaks to the effect of the marginal comprehensive audit and does not consider general deterrence – the idea that auditing one taxpayer may affect other taxpayers' behavior – or the overall effect of all comprehensive audits. We return to this discussion in section 8.

7 Tracking marginal firms through a survey

We have shown that firms exit the tax system post-audit and that tax liability decreases substantially in the post-audit period. While the effect on tax liability is unambiguously negative, this does not imply that the output of firms changed. It could be the case that firms move to the informal sector or maintain the same levels of production, but evade more. Changes in output are hard to measure with administrative tax data because information in tax returns will be influenced both by real production decisions and changes in reporting behavior. This is especially the case for the group of firms we study: designated to be likely tax evaders by the URA.

To overcome this challenge we design and implement a novel survey that tracks down firms on the margin of receiving a comprehensive tax audit. Our survey was designed to determine whether each firm existed, and if it did, what its financial situation looked like. We divided our data collection into two parts. The first, intended to determine whether firms exist, was an extensive tracking exercise where we tracked down every firm that was on the margin of receiving a comprehensive audit (that is, the firm falls within the optimal bandwidth). The second component was the survey itself. The survey included detailed questions about each firms' financial situation, such as: sales, costs, profits, number of employees, and prices and quantities of goods sold. Because we were interested in the effect of tax enforcement measures, we also included an extensive module on the costs of filing taxes. Here we took a broad view on what such costs might include. We asked firms about: 1) The administrative filing costs, 2) The direct cost of receiving compliance interventions, and 3) The cost of interacting with the tax authority (through audits or otherwise). Together this was designed to capture the full monetary burden imposed on firms by taxation. Due to our close collaboration with the URA we are able to link the survey with the administrative tax data.

The survey compliments the administrative tax data in two ways. First, it provides us with a measure of firm sales that is not affected by tax evasion decisions. We investigate whether audits affect firm sales through both firm shut downs, and reduction in output conditional on remaining operational. Second, the survey provides us with an in-depth view into the cost of audits and tax compliance in Uganda. To the best of our knowledge, there is little extensive information on the total costs of filing taxes, including administrative costs of interacting with the tax authority and the cost of tax correction in developing countries.[32]

[32] The most recent version of the World Bank (2023a) includes information on hours spent on tax compliance, but it is hard to assess what that implies in terms of costs to the firm. Furthermore, there is no information on tax corrections which we find to be important.

7.1 Data collection

We implemented the survey in the period November 2023 to July 2024.

Selection: We select firms for the survey to align with our identification strategy. Initially we focused on all firms in the Kampala Metropolitan Area that were close to the regression discontinuity cut-off for comprehensive audits *and* the various station-specific cut-offs for issue audits. Later on, given budgetary constraints, we redirected our effort to focus on firms that were within the optimal bandwidth for our measure of exiting the tax system; the probability of filing a CIT tax return in the two years after the audit. For this part of the sample, we also included firms outside Kampala Metropolitan Area. The narrow sample thus contains all the firms that identify our main treatment effect. To gain power, we use the broader sample to understand whether firms reduced their scale and for descriptives and context in section 2. A total of 858 and 3,323 were reached out to in the narrow sample and total sample, respectively. Of the firms reached out to 35% and 26% were surveyed in the narrow and broad sample, respectively.

Tracking: To maximize our chances of finding firms two years after they were audited, we merge contact information from four sources: 1) Online information ("Googling"), 2) The Ugandan Registry Services Bureau (URSB), 3) The 2020 Census of Business Establishments, and 4) Contact information registered with the URA.[33] The URSB is the official registration agency for all firms in Uganda, and firms are supposed to renew their registration every year. Around December 2022, the URSB switched to an online database, facilitating access to the registry information.[34] The contact information from the URSB and the URA contains information on both firms' addresses and phone numbers. The 2020 Census of Business Establishments only contains information on the geographic location of the firms.

Once we combine all sources of information, we proceeded in three steps. First, we called all the phone numbers we had on file that were not registered with the URA. We avoid the phone number registered with the URA because we knew from a pilot conducted the year prior that some firms know which numbers are registered with the URA. To avoid biasing respondents, we only used these phone numbers as a last resort. Second, if we did not reach the firm through a phone number, we sent enumerators to each location on file that was within the Kampala Metropolitan Area.[35] Finally, if we did not find the firm at any of the locations – and the neighbors did not know the firm or what happened to it – we called firms on the phone numbers they registered with the URA.

[33] There are no unique identifiers across these datasets. We conduct a fuzzy merge based on names and restrict to cases where the names are extremely similar.

[34] One important limitation is that the online database only holds information for firms that updated their registry after the switch to the online system. Due to limited resources it was not possible to look through the manual information on firms for years prior to the switch to the online system.

[35] This includes firms that were located in Mukono and Entebbe, but excludes firms that were registered in Jinja. For firms within our narrow sample we also sent an enumerator to Jinja. We did not send enumerator to any of the other cities around Uganda.

7.2 Descriptive statics

Table 4 presents summary statistics for the tracking exercise, and survey completion rates. We focus on the narrow sample. Of the 858 firms that we attempted to track down, we found evidence on whether a firm existed for 91.0% of them. That is to say, we either reached the firm or we learnt from people familiar with the firm – such as neighbors or previous employees – that the firm closed. The remaining 9.0% we label "vanished", and consider them closed in the analysis.[36] To our surprise, 17 firms on our list turned out to be non-governmental organisations (NGOs). Because they registered as a business with the URA, it is unclear to what extent they behave as a NGO or enterprise. We therefore include them in our analysis.

We completed interviews with 35% of all firms in the sample (N = 858) and 44% of firms that exist at the time of the survey (N = 683). This is slightly below the response rates in the World Bank Enterprise Surveys for Uganda, Kenya and Rwanda, which focus on similar sized firms (World Bank, 2023a). This could be explained by our sample being harder to reach due to recent (probably negative) experiences with the tax authority, which would make them more suspicious. They also tend to be larger than the firms in the World Bank Enterprise Surveys and therefore more busy. Importantly, we do not find differential survey response rates – defined as whether a firm agrees to be interviewed – above and below the discontinuity. We show in Appendix Figure A10 that the coefficient on probability of completing a survey is small (-0.018) and statistically insignificant.

To check whether firms believed we were from the URA – despite our best efforts – we asked enumerators to indicate whether the respondent did in the survey. Based on qualitative work and our pilot, it was apparent if the respondent thought we were affiliated with the URA. Of the 308 firms we interviewed from the narrow sample 25 (8%) thought we were from the URA. For the broader sample, the equivalent numbers are 43 (6%). We therefore think that most firms did not have an incentive to misreport their information in the survey.

7.3 Results on firm shutdown

Our first key outcome is firm shutdown. We define a firm as shutting down if it does not file CIT taxes for two years post-audit *and*, either someone informs us that they closed or they "vanish". This is important because firms might be "dormant", shutting down production and awaiting a profitable large contract – for example from the government – to restart. In such cases, firms keep filing taxes despite not operating. Combining the administrative tax data and the survey allows us to identify real shutdown. We define a firm as being informal if it does not file taxes post-audit, but we find evidence that it still exists. We choose this definition – as opposed to whether a firm is tax registered – because firms do not have an incentive to change their tax registration status when

[36]To verify that labelling vanished firms as closed is a reasonable assumption we compute the tax filing rates for each of our categories. In Appendix Table A11 we show that filing rates between closed and vanished firms are remarkably similar. 39% of closed firms file a CIT in the latest financial year and 39% of vanished firms do. The p-value for a t-test on whether the two averages are different the same is 0.992.

they leave the formal sector. As such, non-filing is the definition that most closely resembles what is used in the informality literature (Ulyssea, 2020).

Figure 9 suggests that the majority of firms that stop filing taxes close. In Panel (a) we show that firms above the threshold are 22 p.p. more likely to have shutdown when we track them down approximately two years after the audit. The implied effect of receiving an audit is 27 p.p. (395%). Both the reduced form and two stage least squares estimates are significant at the 5% significance level. In Panel (b) we show the effect on informality. The coefficient on the probability of being informal is also large, suggesting an increase in informality for firms above the threshold of 7 p.p., but this is only marginally significant. The coefficient on the probability of filing a CIT return during the two years post-audit is -20 p.p. Dividing the coefficient on shutdown and informal by the coefficient of not filing suggests that slightly more than half of the firms that stop filing taxes (54%) shutdown, whereas the remaining 46% of firms keep operating informally.

Robustness. We conduct the same robustness checks for the survey data as we did when using administrative tax data. In Appendix Table A12 we show that the point estimates are similar when using different controls and in Appendix Figure A11 we show that the results are stable over narrow bandwidths.[37]

7.4 Causes of shutdown and subsequent occupations

Firm closures were identified in various ways. Sometimes the neighbors of the firms' former premises told us the firm shutdown, in which cases retrieving information on why the firm shutdown was difficult. However, for the cases where we reached the former owner or a former employee we asked the respondent for the cause of the shut-down. We display the reasons mentioned in Panel (a) of Figure 10. While the number of observations is not high − and hence this should be interpreted with caution − note that the number of answers represents 63% of the firms we confirmed closed. The most commonly cited reason for shutdown was taxes or troubles with the URA. 43% of firms mentioned high taxes or issues with the URA as the reason for why the firm closed. The next highest category was low demand at 16%.

Finally, we also asked the respondent what they were doing after the firm closed (or if we did not reach the firm, if they knew what the owners of the firm were doing now). We find that 39% of respondents reported being an employee in another firm. Crucially, less than half of respondent (40%) said they started a new firm, suggesting that the majority of firms that closed do not open again. A significant percent of respondent also report being unemployed (14%), indicating that the firm closure has devastating consequences for some people. We view this as suggestive evidence the audits are not only leading firms to close, but also discouraging the owner from starting a new firm in its aftermath.

[37] The only time the coefficient changes by more than 20% is in Column (6), where we add sector fixed effects and a full set of risk controls (70 total). This is an extremely demanding specification leading us to conclude that the results are robust.

7.5 Results on reduction in output

Despite no differential response rates, we have limited statistical power to assess whether firms reduce their sales post-audit because we only interview 44% of the firms in the narrow sample. To gain power we move away from firms in the vicinity of the discontinuity and include all firms from the 13,014 where we can infer their sales in the survey. In other words, we include all firms who reported their sales in the survey or firms that closed or "vanished" and did not report any sales in their tax returns. This includes "dormant" companies as well.[38]

In moving away from the discontinuity, we lose the possibility to use the regression discontinuity as an identification strategy. We therefore leverage a difference-in-difference (DD) specification, combining the administrative and survey data. Specifically, we use the the administrative data to test for pre-trends and the survey data as the outcome in the post-audit period. We run two regressions: one where we only use administrative tax data and one where we substitute for the administrative tax data with the survey data in the last financial year. The DD specification is formally written below,

$$y_{ft} = \beta_{DD}\, \mathbb{1}\{z_f \geq l\} \times \mathbb{1}\{t = 2022\} + \alpha_f + \delta_t + \varepsilon_{ft} \tag{6}$$

where y_{ft} is the outcome of interest of firm f measured at time t. z_f is the ranking of firm f used for selecting comprehensive audits, and l is the threshold for the assignment of comprehensive audits. α_f and δ_t are firm and year fixed effects, respectively.

If the parallel trends assumption holds – that is, in the absence of the comprehensive audit, the outcomes of firms would have evolved on parallel trends – β_{DD} captures the average treatment effect on the treated. To test whether the parallel trends assumption holds, we run a two-way fixed effect version of equation 6, which is formally written below

$$y_{ft} = \beta_k \sum_{\substack{2013 \\ k \neq 2019}}^{2022} \mathbb{1}\{z_f^c \geq l^c\} \times \mathbb{1}\{t = k\} + \alpha_f + \delta_t + \varepsilon_{ft} \tag{7}$$

β_k captures the dynamic treatment effect of being above the threshold on the outcome of interest. All event-study results are shown in Figure 11. We do not find evidence of pre-trends for any of the specifications. The red squares are coefficient based on administrative data, whereas the blue circle is based on survey data. We discuss why the results diverge between the two data sources below.

A separate concern is what constitutes the post-period. As discussed in section 6, firms file CIT returns retrospectively. The first year in which all firms are unambiguously filing taxes for a period after the audit is therefore 2022. We therefore exclude 2020 and 2021 from the analysis altogether. This is because these years may be affected by the audit, but to a lesser extent, mechanically driving

[38] This removes 133 (14%) of the sample. These were firms where the respondent was afraid or not allowed to disclose financial information (100) or firms that were mistakenly labeled as closed (33).

the treatment effect towards zero.

Table 5 presents results on sales using the DD specification. In Column (1) – (5) we show results using only the administrative data, whereas in Column (6) – (10) we show results where we replace the post-audit administrative information with the survey data. Throughout we use the same set of firms.

Focusing on the results using only the administrative tax data, we find a 20.8 p.p. drop in the likelihood of reporting any sales to the tax authority. Note that this is broadly in line with the reduced form coefficient on the probability of filing a tax return in Figure 7(b) (-0.23). For the firms that do submit sales information to the tax authority, we observe a drop in the probability of reporting positive sales of 19.3 p.p. (Column (2)), in line with the reduction in the probability of having a positive tax liability, discussed in section 6 (-0.18). Using the logarithmic transformation of sales, effectively conditioning on firms reporting sales in their tax return in 2022, we find no effect on the amount of sales reported (Column (3)). The coefficient is small (0.017) and statistically insignificant. However, if we instead impute zeros for firms not filing taxes, we find large and significant reductions in total sales (Column (5)). The coefficient in Column (5) suggests a decrease in the average sales of 259 thousand USD. Note that this is largely driven by the firms not filing taxes. If we exclude firms that did not file taxes the coefficient reduces to 118 thousands USD and becomes statistically insignificant (Column (4)).

The story changes significantly when we leverage information from the survey data for the same set of firms. Because we restricted to firms where we can infer sales throughout the analysis, there is no significant differences in the likelihood of reporting sales (Column (6)). More interestingly, the reduction in the probability of reporting positive sales attenuates from 19.3 p.p. (Column (2)) to 6.2 p.p. (Column (7)) and is only marginally significant. In other words, while audited firms are less likely to file taxes or report positive sales in their tax return (conditional on filing taxes), they are only marginally less likely to report positive sales in the survey, highlighting the importance of combining administrative and survey data.

We next turn to the amount of sales reported. Using the logarithmic transformation of sales (Column (8)) we find that firms above the discontinuity reduce their sales by 0.842 log points (56%). This is entirely based on survey data and hence should not be affected by evasion. The results are significant at the 1% level. This suggests that firms reduce their output after an audit, but instead of reporting a reduction in output in their tax returns, firms stop filing taxes or reporting sales altogether. Finally, we include firms that exited and estimate the overall effect on sales. Interestingly, our effect on overall sales using the survey data is similar to what we find when using the administrative data. Using the survey data we find a reduction in overall sales of 267 thousand USD on average (Column (10)), while we find a reduction in sales of 258 thousand USD using the administrative data (Column (5)). It suggests that the extensive margin identified in the administrative data closely resembles the intensive margin in the survey data in terms of overall magnitude on sales. Mechanically, this need not be true.

In Appendix table, A13 we show results for sales leveraging the RD specification. Like in Table 5, we include exiters and dormant firms imputing zero's, and leverage both the winsorized and log transformation of the amounts. Standard errors are large. The reduced form coefficient on the logarithmic transformation of sales (Column (3)) suggest a reduction of 49%, which is similar to what we found in the DD specification (57%), though the estimate is imprecise. We also included other measures of firm size, such as costs and total number of employees in our analysis, shown in Column (4) – (6). Our reduced form estimates suggest a reduction in costs of 49% and a reduction in the number of employees of 5.7 workers. However, none of these are statistically significant. Overall, the RD analysis corroborates what we found in the DD analysis: firms that remain operational are reducing their size.

7.6 Mechanisms

We have documented that comprehensive tax audits have a negative effect on tax liabilities and firm output. A key remaining question is what causes firms to respond in such a drastic manner? In this section we explore potential mechanisms behind firms' responses.

7.6.1 A simple firm problem and potential mechanisms

There are several potential mechanisms that could be driving firm behavior, each of which would have different policy implications. To guide our discussion we introduce a simple firm optimization problem. The model adds audits to a simplified version of the model used in Best et al. (2015).

Assume there is a output tax τ, that firms produce output y at a strictly convex and differentiable cost $c(y)$.[39] They can choose to engage in evasion by underdeclaring their output $e = y - z$, where z is the amount of output declared to the tax authority. Firms face a convex and differentiable cost $g(e)$ of evading that increase in e. If they get audited they have to pay back the tax liability of everything they evaded at interest rate r. Firms also face a fixed cost of being audited c_a that is independent of the amount of evaded and represents the hassle cost of the audit on the firm. The firm chooses y and e to maximize after-tax profits taking into account the perceived probability of being audited $\hat{p}(y)$, which depends on firm output. Firms have idiosyncratic believes both about the audit rate and the extent to which it varies with firm output. Formally,

$$\max_{y,e} \pi_f = (1-\tau)y - c(y) + \tau e - g(e) - \hat{p}(y)\left[(1+r)\tau e + c_a\right] \tag{8}$$

Yielding the following optimal evasion and output decision,

$$g'(e) = \tau\left[1 - \hat{p}(y)(1+r)\right] \tag{9}$$

[39]This is a simplification, but the framework can be easily modified to include a parameter for what portion of expenses are deductible. The logic presented here still holds. See Best et al. (2015) and Basri et al. (2021) for recent papers including the additional parameter.

$$c'(y) = 1 - \underbrace{\left[\tau + \hat{p}'(y)\left[(1+r)\tau e + c_a\right]\right]}_{\text{tax and audit induced distortion}} \tag{10}$$

Intuitively, firms will set marginal cost of evasion, $g'(e)$, equal to the expected marginal gain from evasion (equation 9). In the absence of audits the gain from evasion would be τ. With audits, the expected gain from evasion decreases in the idiosyncratic belief $\hat{p}(y)$ and interest rate r. In other words, the expected gain from evasion is lower the higher the perceived probability of being audited and the higher the interest rate.

The intuition behind equation 10 is the usual profit maximization condition, setting marginal cost equal to marginal revenue. In the absence of taxes or audits the optimal input choice would set $c'(y) = 1$. In this firm problem marginal cost are lower than optimal because of two mechanisms. First, the output tax reduces input purchases. This is analogous to the classic result that tax rates distort labor supply (Mirrlees, 1971), but applied to firms.[40] The higher the tax rate the further the input choice is away from optimality. Second, there is an additional incentive to lower input purchases because audit rates depend on firm output. The more the firm believes that audit rates depend on firm size, the more it will reduce output to avoid audits.

How may an audit affect the firms decision? The theoretical framework highlights two broad channels. First, if a firm gets audited, the cost of the audit could drive profits into negative territory. That is, while in expectation $\pi_f > 0$ if a firm gets audited ($p(y) = 1$), $\pi_f < 0$. There are two parts of the cost, one is the taxes the firm has to pay back ($(1+r)\tau e$) and one is the cost of the audit c_a.

The second mechanism points to the central role of firms' beliefs about the probability of being audited and how it varies with firm output. Receiving an audit could alter $\hat{p}$. An upward revision of $\hat{p}$, according to equation 9, would reduce evasion, increasing the effective tax rates. Under the new effective tax rate the firm may no longer expect to be profitable. Receiving an audit could also alter $\hat{p}'(y)$. Firms may adjust their beliefs about the extent to which audit rates vary with firm output. If $\hat{p}'(y)$ is revised upwards following an audit, following equation 10, firms will reduce their output to avoid future audits.

While tax morale influence firms compliance (Luttmer and Singhal, 2014), we do not think it plays a major role in Uganda. It has been documented that tax morale in Uganda is low (Kakumba, 2024). 83% of Ugandans say they find it hard to know how the government uses its tax revenue, and less than half (46%) believe that the government uses taxes for the wellbeing of its citizens. Six out of ten (59%) say that most or all tax officials are corrupt and only 35% say the trust the URA somewhat or a lot. Finally, one-third of citizens say that other people in the country "often" or "always" avoid paying taxes. Given the overall low levels of tax morale in the country, changes in tax morale induced by audits can explain the results we find.

[40] Note that under a pure profit tax τ would not distort the firms input decision.

7.6.2 Empirical evidence

Having discussed the various channels, we combine descriptive and causal evidence to understand which ones may be driving the results.

If the key mechanism is the amount of taxes the firm has to pay back, we should observe a larger increase in shutdowns among firms with higher relative tax corrections. To test for this, we divide the sample of firms above the threshold into two groups: above and below the median ratio of tax corrections over baseline sales. Some firms filed sales of 0 in their tax returns at baseline, we include them in the category of firms above the median. If a firm above the threshold did not file a tax return at baseline we remove them from the analysis. We run a separate regression for each group, while keeping the firms below the threshold constant.

Figure 12 presents results for firm shutdown. In Panel (a) we present results for firms above the median and in Panel (b) we present results for firms below the median. We observe that the effect is *entirely* driven by firms above the median.[41] The reduced form coefficient suggests that firms with a tax correction share above median are 21 p.p. more likely to shutdown in the aftermath of the audit. This is statistically significant at the 5% level. For firms below the median the same coefficient suggests a 0.5 p.p. *decrease* in the probability of shutting down and is statistically insignificant. A key driver of firm shutdown appears to be that firms cannot afford to pay back the taxes they owe. If a firm is not paying their tax corrections, the URA typically does not issue a tax clearance certificate, which is essential to import goods and bid for government contracts. Not paying a tax correction can therefore have significant negative effects on business operations.

While the share of tax correction is a key driver of firm shutdown that does not explain whether the firms' response is "rational". That is, based on the firms past profits, could it reasonably expect to pay back the amount over time? Leveraging information from firms tax returns prior to the audit we document that for the median firm above the threshold that received a tax correction, the correction is worth 20% of annual sales and 86% of gross profit. If we reduce the importance of outliers by winsorizing at the 5% level, the average is 99% of annual sales and 3.7 times gross profits. These are large economic shocks for firms to absorb, making it plausible that a rational response would be to close shop. Even if the firm is productive at baseline.

Next, we investigate whether perceptions about the probability of being audited could explain why some firms are reducing their output. In our survey we asked firms what percent of firms similar to theirs they believe will be audited in the next financial year.[42] We use regression 6 and split the firms above the threshold into two groups. One with above median belief about the probability of being audited and one with below median belief.[43] The results are presented in Figure 13. Firms

[41] The median was 0. However, for the firms above the median the median penalty as a share of sales is 13%. A substantial amount.

[42] The exact phrasing of the question was: "Finally, imagine firms similar to yours in size and sector. In your opinion, what percentage of these firms do you think will be audited in the upcoming financial year?"

[43] The median was 50%. Significantly above true audit rates

with high belief about the probability of being audited are driving the reduction in log sales (as seen in Panel (a)). The coefficient on log sales for firms with low belief is statistically insignificant (panel (b)). In other words, the firms that got audited two years ago and now believe there is a high likelihood that firms similar to theirs are going to be audited are the ones driving the reduction in sales post-audit. This suggests that changes beliefs play a role in determining output among operational firms.

The mechanism discussion has implications for tax enforcement policy. First, we show that firms are not able to pay back the taxes they owe. From a policy perspective this suggests that tax rates are too high for these firms. Under a scenario with no evasion these firms would not survive. Second, firms overestimate the probability of receiving a comprehensive audit (the median firm believes that 50% of firms similar to theirs will be audited in the next financial year). We also observe that the firms with the highest beliefs are the ones reducing their output. This suggests that the URA could provide guidance about true audit rates, alleviating some of the fear among firms.

8 Discussion

This paper has shown that audits not only reduce the tax liability of firms in the post-audit period, but also affect the output reported by firms. One central question remains, what is the effect of all comprehensive audits on the revenue collected by the URA, as opposed to the marginal audit?

8.1 Back of the envelope calculation for overall revenue collected

To understand the impact of all comprehensive audits on revenue collection we conduct two back-of-the-envelope calculations. In the first, we include all comprehensive audits that were assigned and started in 2021/22 (our year of analysis). As such, the only audits that are excluded are comprehensive audits that were assigned in the year prior, or started after the financial year closed. In the second, we restrict to the subset of comprehensive audits that were conducted on firms that are registered at regular taxpayer stations. The firms in the specialized taxpayer offices are the largest firms in Uganda. Our analysis thus far has excluded them, because tax enforcement strategies differ for these firms.[44]

Firm receiving comprehensive audits are together liable for 42.2 million USD in tax payments because of tax corrections. While we do not know whether each individual firm paid their liability, leveraging information from audit reports for our year of analysis, we know that the URA collected 60% of the tax corrections. The revenue collected from comprehensive audits is therefore approximately 25.3 million USD.

However, we know that the marginally audited firms reduce their tax liability in the aftermath of the audit. To account for this, we aggregate the CIT liability submitted by audited firms at

[44]See section 4 for a discussion about the differences between firm in regular and specialized taxpayer offices.

baseline, 2019/20, and compare it to the amount submitted in 2022/23. On aggregate, these firms reduced their combined CIT liability by 7.3 million USD. Conducting a similar exercise for the VAT, comparing the average amount submitted in 2019/20 against the average amount submitted during the first five months of 2022/23, we derive a reduction in average monthly VAT liability of 1.8 million USD. Given this is based on estimates over 5 months, the total reduction in aggregate VAT liability is: $1.8 \times 5 = 9$ million USD.

Putting it together, the URA gains $25.3 - 7.3 - 9 = 9$ million USD. However, bear in mind that the VAT liability is based on estimates from 5 months post-audit. If we assume that the average reduction in VAT liability stays the same over the entire financial year, the URA gain reduces to $25.3 - 7.3 - 21.6 = -3.6$ million USD. That is, accounting for firms behavioral response post-audit, and under reasonable assumptions, the URA is losing 3.6 million USD from comprehensive audits conducted in 2021/22. These calculations do not account for the cost to the URA of conducting a comprehensive audit, and only considers firms' behavioral response one year post-audit. If we assume that the behavioral responses of firms persist for a second year post-audit, the loss increases to 32.5 million USD.

Restricting our calculations to firms that are registered at the regular taxpayer stations, alters the analysis slightly. Together, this subset of audited firms, are liable for 18.7 million USD. Assuming that 60% of their liability was collected suggests the URA gains 11.2 million USD from conducting comprehensive audits on these firms. On aggregate, these firms reduce their CIT liability by 0.6 million USD, and their VAT liability by an average of 0.7 million USD. Assuming these effects hold for the entire financial year, suggests the URA gains $11.2 - 0.6 - 0.7 \times 12 = 2.2$ million USD. Unlike the calculations with all firms, the URA does gain revenue from conducting audits on these firms. However, if we assume the behavioral responses of firms persist into the second year post-audit, the URA would make a loss of 6.8 million USD.

Overall, we conclude that while comprehensive audits do raise money through tax corrections, under realistic assumptions, the URA is experiencing an aggregate revenue loss from conducting comprehensive audits.

8.2 Back of the envelope aggregation

The results from our survey demonstrates that comprehensive audits do not only reduce the tax liability of firms, but also cause them to reduce output. A central remaining question is whether this has implications for aggregate output.

If we assume that 1) production is constant returns to scale, 2) all firms sell to final consumers or firms not in group g, 3) inputs are perfect complements, and 4) the economy was at a constrained efficient equilibrium before the intervention, the effect of an intervention on the changes in aggregate output for a group of firms g is, to a first order, approximated by the following

$$\Delta Y_g \approx \sum_{i \in g} [s_i - c_i] \times \%\Delta l_i \tag{11}$$

where s_i are total sales of firm i at baseline, c_i are total costs of firm i at baseline, and $\%\Delta l_i$ is the percentage change of labor induced by the intervention (in this case, being above the threshold). Full derivation in Appendix C. We do not observe quantities, instead we use changes in the wage bill. By doing so, we impose the additional assumptions that labor markets are competitive.

Note that the approximation assumes homogeneous effects across all firms. That is a strong assumption in our context since we know some firms exited, indicating quite different responses. Closure of a firm has, naturally, a distinct and much stronger response on the amount of inputs used. To account for the heterogeneity in firms' responses, note that $\%\Delta l_i = (1 - \Delta p_e)\%\Delta l_i + \Delta p_e \%\Delta l_i$, where p_e is the increase in the probability of a firm exiting induced by the treatment. Firms that exit do not use any labor, as such $\Delta l_i = -1$ for firms that exit. Plugging this into equation 11, we derive a first order approximation that allows for separate estimates for firms that exit and firms that remain operational.

$$\Delta Y_g = \sum_{i \in g} \Delta \pi_i \approx \sum_{i \in g | operating} [s_i - c_i] \times \Delta \log l_i - \sum_{i \in g} [s_i - c_i] \, \Delta p_e \tag{12}$$

because we are conditioning on firms existing $\%\Delta l_i \approx \Delta \log l_i$. Full derivation in Appendix C.

To estimate equation 12, note that all components are either readily identifiable in the data or can be estimated using the DD regression from equation 6. We observe sales (s_i) and cost (c_i) of firms at baseline (financial year 2019) in the CIT data. Under the standard DD assumptions that firms outside the treatment group (firms below the threshold) are unaffected by the audit, $\Delta \log l_i$ and Δp_e can be predicted from a DD regression. We restrict to the set of firms we have survey data on (same sample used in Table 5 and Figure 11). This is because we only know whether the firm shut down for this sample. The outcome variable we use to estimate Δp_e is the probability of reporting sales. Given the restrictions imposed on the sample, the primary reason a firm did not report sales is that they closed.

We present results from the DD regression in Figure 14. Panel (a) presents results for the probability of reporting sales, whereas Panel (b) report results for the logarithmic transformation of the wage bill. We estimate that audits increase the probability of exiting by 21.9 p.p., and that they induce a reduction in the wage bill of 0.606 log points. Both estimates are statistically significant at the 1% level.

With the estimates derived from the DD regression and the data points from the CIT data, we have all the components to calculate equation 12. Note that our estimates represent the average treatment effect on the treated, which is why we estimate equation 12 for the firms above the threshold; the treated group. Multiplying the estimate for $\Delta \log l_i$, 0.606, with the aggregate output for the firms above the threshold that remained operational, suggests that comprehensive audits induced a 7.4 million USD decline in aggregate output. A further 3.5 million USD in aggregate output was lost from firms exiting for a total aggregate output loss of 10.9 million USD. If we assume that the treatment effects we estimate hold for *all* firms above the threshold – not just the ones we managed to collect survey data on – the effect increases to 19 million USD for the firms that remain operational and 11.2 million for exiting firms. That would make the total aggregate output loss 30.2 million USD.[45]

To get a sense of the magnitude of the reduction, we compare it to the pre-audit aggregate output for different samples. Starting with all the 13,014 firms in the comprehensive sample, our estimates suggest a reduction in aggregate output of 1.4% – 4.0%, depending on whether we assume the treatment effect applies to all firms above the threshold. Repeating the same exercise for all formal sector firms gives us a decline of 0.18% – 0.50% reduction in aggregate output for all formal firms. Finally, our own calculation suggests that formal firms accounted for 30% of GPD in Uganda. However, estimates from the Bank of Uganda and the Ugandan Bureau of Statistics, suggest the formal sector represented 49% of GDP (ILO, 2021a). Scaling our results by the share of GDP represented by formal firms, we calculate a range of 0.05% – 0.09% for surveyed firms, and 0.15% – 0.24% for all firms above the threshold.

Given the small number of firms affected by the audit this is substantial. While the assumptions imposed are strong, it underscores that audits are inducing economic distortions that are sufficiently large to be relevant for aggregate output in the economy.

9 Conclusion

In this paper we investigate the impact of comprehensive tax audits on firms' tax liability and output. We show that the marginal comprehensive audit backfires for the revenue authority. The upper bound on the revenue collected from the marginal audit – when considering post-audit compliance behavior – is $-7,400$ USD. A marginal value of public fund analysis of the marginal audit would undoubtedly be negative. This is in stark contrast to the recent literature on the impact of tax audits in rich countries, which finds large positive effects for each dollar spent on audits (Boning et al., 2023). A back of the envelope calculations suggests that across all comprehensive audits, assuming the audit is costless to the government and under realistic assumptions of taxpayer behavior one year after the audit, the URA is losing 3.6 million USD.

However, comprehensive audits do not only reduce the potential tax revenue collected, but also firm output. Firms are more likely to shut down after the audit and firms that remain operational reduce their sales. This underscores that the reduction in revenue is not only driven by changes in evasion, but also by adjustments in firms operational decisions. A back-of-the-envelope aggregation

[45] Since we do not have survey data for all firms, we assume a firms that stop filing taxes is exiting in this exercise.

exercise suggests that the comprehensive audits in our year of reduced aggregate output by 10.9 million USD for firms that are affected by audit and we could survey. This represents a reduction in aggregate output for all firms in the formal sector of 1.4%.

One thing we have not considered in the analysis is the general deterrence effect of audits. Indeed, one of the reasons audits are done is to encourage non-audited firms to be compliant. While it is tricky to estimate the general deterrence effect directly, we can bound the increase in compliance necessary across the non-audited firms for the audits to be worth it. If we only care about whether the URA breaks even, using the estimates for all comprehensive audits and assuming our treatment effects persists for one year, compliance across the remaining firms would have to increase by 1.3%, on average, across *all* formal firms. However, the audits also cause declines in aggregate output of 10.9 million USD. To cover the additional loss in output, compliance would have to increase by 5.3% across all formal firms. We deem it unlikely that comprehensive audit would have such a large general deterrence effect.

We conclude that comprehensive audits in Uganda – at least in the way they are practiced now – ultimately harm both revenue collection efforts and the real economy. Less comprehensive enforcement actions may be better suited for an economic environment like Uganda, something we will explore in future work.

References

Abadie, Alberto (2002). "Bootstrap Tests for Distributional Treatment Effects in Instrumental Variable Models". In: *Journal of the American Statistical Association* 97.457, pp. 284–292. ISSN: 0162-1459. JSTOR: 3085782. (Visited on 07/16/2024).

Advani, Arun, William Elming, and Jonathan Shaw (2021). "The Dynamic Effects of Tax Audits". In: *The Review of Economics and Statistics*, pp. 1–45. ISSN: 0034-6535. DOI: 10.1162/rest_a_01101. (Visited on 03/28/2022).

Aghion, Philippe, Ufuk Akcigit, and Peter Howitt (2014). "What Do We Learn From Schumpeterian Growth Theory?" In: *Handbook of Economic Growth*. Ed. by Philippe Aghion and Steven N. Durlauf. Vol. 2. Handbook of Economic Growth. Elsevier, pp. 515–563. DOI: 10.1016/B978-0-444-53540-5.00001-X. (Visited on 11/06/2024).

Akcigit, Ufuk, Harun Alp, and Michael Peters (2021). "Lack of Selection and Limits to Delegation: Firm Dynamics in Developing Countries". In: *American Economic Review* 111.1, pp. 231–275. ISSN: 0002-8282. DOI: 10.1257/aer.20180555. (Visited on 10/18/2024).

Alm, James (2019). "What Motivates Tax Compliance?" In: *Journal of Economic Surveys* 33.2, pp. 353–388. ISSN: 1467-6419. DOI: 10.1111/joes.12272. (Visited on 09/27/2024).

Alm, James and Antoine Malézieux (2021). "40 Years of Tax Evasion Games: A Meta-Analysis". In: *Experimental Economics* 24.3, pp. 699–750. ISSN: 1573-6938. DOI: 10.1007/s10683-020-09679-3. (Visited on 07/14/2024).

Almunia, Miguel, Justine Knebelmann, Lin Tian, David Henning, and Dorothy Nakyambadda (2022). *Information, Fiscal Capacity, and Tax Enforcement: An Experimental Evaluation*. Tech. rep.

Alstadsaeter, Annette, Sarah Godar, Panayiotis Nicolaides, and Gabriel Zucman (2023). "Global Tax Evasion Report 2024". In: *PSE Working Papers* halshs-04563948. (Visited on 11/06/2024).

Bachas, Pierre, Anne Brockmeyer, Roel Dom, and Camille Semelet (2023). *Effective Tax Rates and Firm Size*. Tech. rep. The World Bank. DOI: 10.1596/1813-9450-10312. (Visited on 08/18/2024).

Bachas, Pierre, Roberto N. Fattal Jaef, and Anders Jensen (2019). "Size-Dependent Tax Enforcement and Compliance: Global Evidence and Aggregate Implications". In: *Journal of Development Economics* 140, pp. 203–222. ISSN: 0304-3878. DOI: 10.1016/j.jdeveco.2019.05.001. (Visited on 09/06/2024).

Basri, M. Chatib, Mayara Felix, Rema Hanna, and Benjamin A. Olken (2021). "Tax Administration versus Tax Rates: Evidence from Corporate Taxation in Indonesia". In: *American Economic Review* 111.12, pp. 3827–3871. ISSN: 0002-8282. DOI: 10.1257/aer.20201237. (Visited on 03/28/2022).

Bassi, Vittorio, Jung Hyuk Lee, Alessandra Peter, Tommaso Porzio, Ritwika Sen, and Esau Tugume (2023). *Self-Employment Within the Firm*. Working Paper. DOI: 10.3386/w31740. National Bureau of Economic Research: 31740. (Visited on 10/18/2024).

Bassi, Vittorio and Aisha Nansamba (2022). "Screening and Signalling Non-Cognitive Skills: Experimental Evidence from Uganda". In: *The Economic Journal* 132.642, pp. 471–511. ISSN: 0013-0133. DOI: 10.1093/ej/ueab071. (Visited on 09/29/2024).

Beer, Sebastian, Matthias Kasper, Erich Kirchler, and Brian Erard (2020). "Do Audits Deter or Provoke Future Tax Noncompliance? Evidence on Self-Employed Taxpayers". In: *CESifo Economic Studies* 66.3, pp. 248–264. ISSN: 1610-241X. DOI: 10.1093/cesifo/ifz018. (Visited on 03/28/2022).

Belnap, Andrew, Jeffrey L. Hoopes, Edward L. Maydew, and Alex Turk (2024). "Real Effects of Tax Audits". In: *Review of Accounting Studies* 29.1, pp. 665–700. ISSN: 1573-7136. DOI: 10.1007/s11142-022-09717-w. (Visited on 03/08/2024).

Bento, Pedro and Diego Restuccia (2021). "On Average Establishment Size across Sectors and Countries". In: *Journal of Monetary Economics* 117, pp. 220–242. ISSN: 0304-3932. DOI: 10.1016/j.jmoneco.2020.01.001. (Visited on 09/08/2024).

Bergolo, Marcelo, Rodrigo Ceni, Guillermo Cruces, Matias Giaccobasso, and Ricardo Perez-Truglia (2023). "Tax Audits as Scarecrows: Evidence from a Large-Scale Field Experiment". In: *American Economic Journal: Economic Policy* 15.1, pp. 110–153. ISSN: 1945-7731. DOI: 10.1257/pol.20200321. (Visited on 10/21/2024).

Besley, Timothy and Torsten Persson (2013). "Chapter 2 - Taxation and Development". In: *Handbook of Public Economics*. Ed. by Alan J. Auerbach, Raj Chetty, Martin Feldstein, and Emmanuel Saez. Vol. 5. Handbook of Public Economics, Vol. 5. Elsevier, pp. 51–110. DOI: 10.1016/B978-0-444-53759-1.00002-9. (Visited on 09/11/2022).

– (2014). "Why Do Developing Countries Tax So Little?" In: *Journal of Economic Perspectives* 28.4, pp. 99–120. ISSN: 0895-3309. DOI: 10.1257/jep.28.4.99. (Visited on 10/17/2024).

Best, Michael Carlos, Anne Brockmeyer, Henrik Jacobsen Kleven, Johannes Spinnewijn, and Mazhar Waseem (2015). "Production versus Revenue Efficiency with Limited Tax Capacity: Theory and Evidence from Pakistan". In: *Journal of Political Economy* 123.6, pp. 1311–1355. ISSN: 0022-3808. DOI: 10.1086/683849. (Visited on 01/04/2024).

Best, Michael Carlos, Jawad Shah, and Mazhar Waseem (2021). *Detection Without Deterrence: Long-Run Effects of Tax Audit on Firm Behavior*. Tech. rep., p. 91.

Blattman, Christopher, Eric P. Green, Julian Jamison, M. Christian Lehmann, and Jeannie Annan (2016). "The Returns to Microenterprise Support among the Ultrapoor: A Field Experiment in Postwar Uganda". In: *American Economic Journal: Applied Economics* 8.2, pp. 35–64. ISSN: 1945-7782. DOI: 10.1257/app.20150023. (Visited on 09/29/2024).

Bloom, Nicholas, Raffaella Sadun, and John Van Reenen (2012). "The Organization of Firms Across Countries*". In: *The Quarterly Journal of Economics* 127.4, pp. 1663–1705. ISSN: 0033-5533. DOI: 10.1093/qje/qje029. (Visited on 10/18/2024).

Blumenthal, Marsha, Charles Christian, and Joel Slemrod (2001). "Do Normative Appeals Affect Tax Compliance? Evidence from a Controlled Experiment in Minnesota". In: *National Tax Journal* 54.1, pp. 125–138. ISSN: 0028-0283. DOI: 10.17310/ntj.2001.1.06. (Visited on 09/27/2024).

Boehm, Johannes and Ezra Oberfield (2020). "Misallocation in the Market for Inputs: Enforcement and the Organization of Production*". In: *The Quarterly Journal of Economics* 135.4, pp. 2007–2058. ISSN: 0033-5533. DOI: 10.1093/qje/qjaa020. (Visited on 09/29/2024).

Boeri, Tito and Juan F. Jimeno (2005). "The Effects of Employment Protection: Learning from Variable Enforcement". In: *European Economic Review* 49.8, pp. 2057–2077. ISSN: 0014-2921. DOI: 10.1016/j.euroecorev.2004.09.013. (Visited on 10/18/2024).

Bold, Tessa, Selene Ghisolfi, Frances Nsonzi, and Jakob Svensson (2022). "Market Access and Quality Upgrading: Evidence from Four Field Experiments". In: *American Economic Review* 112.8, pp. 2518–2552. ISSN: 0002-8282. DOI: 10.1257/aer.20210122. (Visited on 09/29/2024).

Boning, William C., John Guyton, Ronald Hodge, and Joel Slemrod (2020). "Heard It through the Grapevine: The Direct and Network Effects of a Tax Enforcement Field Experiment on Firms". In: *Journal of Public Economics* 190, p. 104261. ISSN: 0047-2727. DOI: 10.1016/j.jpubeco.2020.104261. (Visited on 09/27/2022).

Boning, William C., Nathaniel Hendren, Ben Sprung-Keyser, and Ellen Stuart (2023). *A Welfare Analysis of Tax Audits Across the Income Distribution*. Working Paper. DOI: 10.3386/w31376. National Bureau of Economic Research: 31376. (Visited on 07/11/2023).

Brockmeyer, Anne, Spencer Smith, Marco Hernandez, and Stewart Kettle (2019). "Casting a Wider Tax Net: Experimental Evidence from Costa Rica". In: *American Economic Journal: Economic Policy* 11.3, pp. 55–87. ISSN: 1945-7731. DOI: 10.1257/pol.20160589. (Visited on 11/07/2024).

Busso, Matias, Sebastián Montaño, and Juan Muñoz-Morales (2023). *Signaling Specific Skills and the Labor Market of College Graduates*. Discussion Paper Series 16449. IZA Discussion Papers.

Calonico, Sebastian, Matias D. Cattaneo, and Rocio Titiunik (2014). "Robust Nonparametric Confidence Intervals for Regression-Discontinuity Designs". In: *Econometrica* 82.6, pp. 2295–2326. ISSN: 1468-0262. DOI: 10.3982/ECTA11757. (Visited on 07/18/2023).

Carmen, Giselle Del, Edgardo Enrique Espinal Hernandez, and Thiago Scot (2022). *Targeting in Tax Compliance Interventions: Experimental Evidence from Honduras*. Tech. rep. 9967. Washington, DC: World Bank.

Carranza, Eliana, Robert Garlick, Kate Orkin, and Neil Rankin (2022). "Job Search and Hiring with Limited Information about Workseekers' Skills". In: *American Economic Review* 112.11, pp. 3547–3583. ISSN: 0002-8282. DOI: 10.1257/aer.20200961. (Visited on 09/29/2024).

Carrillo, Paul, Dave Donaldson, Dina Pomeranz, and Monica Singhal (2023). "Ghosting the Tax Authority: Fake Firms and Tax Fraud in Ecuador". In: *American Economic Review: Insights* 5.4, pp. 427–444. ISSN: 2640-205X. DOI: 10.1257/aeri.20220321. (Visited on 08/20/2024).

Carrillo, Paul, Dina Pomeranz, and Monica Singhal (2017). "Dodging the Taxman: Firm Misreporting and Limits to Tax Enforcement". In: *American Economic Journal: Applied Economics* 9.2, pp. 144–164. ISSN: 1945-7782. DOI: 10.1257/app.20140495. (Visited on 03/28/2022).

Cattaneo, Matias D., Michael Jansson, and Xinwei Ma (2018). "Manipulation Testing Based on Density Discontinuity". In: *The Stata Journal* 18.1, pp. 234–261. ISSN: 1536-867X. DOI: 10.1177/1536867X1801800115. (Visited on 07/18/2023).

Cattaneo, Matias D., Michael Jansson, and Xinwei Ma (2020). "Simple Local Polynomial Density Estimators". In: *Journal of the American Statistical Association* 115.531, pp. 1449–1455. ISSN: 0162-1459. DOI: 10.1080/01621459.2019.1635480. (Visited on 10/11/2022).

Chandra, Piyush and Cheryl Long (2013). "VAT Rebates and Export Performance in China: Firm-level Evidence". In: *Journal of Public Economics* 102, pp. 13–22. ISSN: 0047-2727. DOI: 10.1016/j.jpubeco.2013.03.005. (Visited on 10/17/2024).

Christiansen, Tobias Gabel (2024). "Dynamic Effects of Tax Audits and the Role of Intentions". In: *Journal of Public Economics* 234, p. 105121. ISSN: 0047-2727. DOI: 10.1016/j.jpubeco.2024.105121. (Visited on 09/13/2024).

Corbellini, Nicola (2024). "The Effects of Tax Enforcement on the Firm Size Distribution and Aggregate Productivity".

de Mel, Suresh, David McKenzie, and Christopher Woodruff (2008). "Returns to Capital in Microenterprises: Evidence from a Field Experiment*". In: *The Quarterly Journal of Economics* 123.4, pp. 1329–1372. ISSN: 0033-5533. DOI: 10.1162/qjec.2008.123.4.1329. (Visited on 09/29/2024).

– (2014). "Business Training and Female Enterprise Start-up, Growth, and Dynamics: Experimental Evidence from Sri Lanka". In: *Journal of Development Economics* 106, pp. 199–210. ISSN: 0304-3878. DOI: 10.1016/j.jdeveco.2013.09.005. (Visited on 09/29/2024).

DeBacker, Jason, Bradley T. Heim, Anh Tran, and Alexander Yuskavage (2015). "Legal Enforcement and Corporate Behavior: An Analysis of Tax Aggressiveness after an Audit". In: *The Journal of Law and Economics* 58.2, pp. 291–324. ISSN: 0022-2186. DOI: 10.1086/684037. (Visited on 03/28/2022).

– (2018). "Once Bitten, Twice Shy? The Lasting Impact of Enforcement on Tax Compliance". In: *The Journal of Law and Economics* 61.1, pp. 1–35. ISSN: 0022-2186. DOI: 10.1086/697683. (Visited on 03/28/2022).

Di Nola, Alessandro, Georgi Kocharkov, Almuth Scholl, and Anna-Mariia Tkhir (2021). "The Aggregate Consequences of Tax Evasion". In: *Review of Economic Dynamics* 40, pp. 198–227. ISSN: 1094-2025. DOI: 10.1016/j.red.2020.09.009. (Visited on 09/29/2024).

Enache, Cristina (2023). *Corporate Tax Rates around the World, 2023.* https://taxfoundation.org/data/all/global/co
tax-rates-by-country-2023/. (Visited on 07/14/2024).

Evans, David S. (1986). "The Differential Effect of Regulation across Plant Size: Comment on Pashigian". In: *The Journal of Law & Economics* 29.1, pp. 187–200. ISSN: 0022-2186. JSTOR: 725408. (Visited on 10/18/2024).

Field, Erica, Seema Jayachandran, and Rohini Pande (2010). "Do Traditional Institutions Constrain Female Entrepreneurship? A Field Experiment on Business Training in India". In: *American Economic Review* 100.2, pp. 125–129. ISSN: 0002-8282. DOI: 10.1257/aer.100.2.125. (Visited on 09/29/2024).

Gadenne, Lucie, Tushar K. Nandi, and Roland Rathelot (2022). *Taxation and Supplier Networks: Evidence from India.* Tech. rep.

Garicano, Luis, Claire Lelarge, and John Van Reenen (2016). "Firm Size Distortions and the Productivity Distribution: Evidence from France". In: *American Economic Review* 106.11, pp. 3439–3479. ISSN: 0002-8282. DOI: 10.1257/aer.20130232. (Visited on 10/18/2024).

Gemmell, Norman and Marisa Ratto (2012). "Behavioral Responses to Taxpayer Audits: Evidence from Random Taxpayer Inquiries". In: *National Tax Journal* 65.1, pp. 33–57. ISSN: 0028-0283. DOI: 10.17310/ntj.2012.1.02. (Visited on 03/28/2022).

Gerardino, Maria Paula, Stephan Litschig, and Dina Pomeranz (2024). "Distortion by Audit: Evidence from Public Procurement". In: *American Economic Journal: Applied Economics* 16.4, pp. 71–108. ISSN: 1945-7782. DOI: 10.1257/app.20220512. (Visited on 10/18/2024).

Gourio, François and Nicolas Roys (2014). "Size-Dependent Regulations, Firm Size Distribution, and Reallocation". In: *Quantitative Economics* 5.2, pp. 377–416. ISSN: 1759-7331. DOI: 10.3982/QE338. (Visited on 10/18/2024).

Harju, Jarkko, Kaisa Kotakorpi, Tuomas Matikka, and Annika Nivala (2024). "How Do Firms Respond to Risk-based Tax Audits?" In.

Harju, Jarkko, Tuomas Matikka, and Timo Rauhanen (2019). "Compliance Costs vs. Tax Incentives: Why Do Entrepreneurs Respond to Size-Based Regulations?" In: *Journal of Public Economics* 173, pp. 139–164. ISSN: 0047-2727. DOI: 10.1016/j.jpubeco.2019.02.003. (Visited on 10/17/2024).

Hjort, Jonas, Golvine de Rochambeau, Vinayak Iyer, and Fei Ao (2020). *Informational Barriers to Market Access: Experimental Evidence from Liberian Firms*. Working Paper. DOI: 10.3386/w27662. National Bureau of Economic Research: 27662. (Visited on 09/29/2024).

Holz, Justin E., John A. List, Alejandro Zentner, Marvin Cardoza, and Joaquin E. Zentner (2023). "The $100 Million Nudge: Increasing Tax Compliance of Firms Using a Natural Field Experiment". In: *Journal of Public Economics* 218, p. 104779. ISSN: 0047-2727. DOI: 10.1016/j.jpubeco.2022.104779. (Visited on 04/11/2023).

Hsieh, Chang-Tai and Peter J. Klenow (2009). "Misallocation and Manufacturing TFP in China and India*". In: *The Quarterly Journal of Economics* 124.4, pp. 1403–1448. ISSN: 0033-5533. DOI: 10.1162/qjec.2009.124.4.1403. (Visited on 10/15/2024).

ILO (2021a). *Diagnosis on Informality in Targeted Intervention Areas of the PROSPECTS Programme in Uganda*. News. (Visited on 04/11/2023).

– (2021b). *Informal Employment Rate by Sex (%)*. https://rshiny.ilo.org/dataexplorer31/?lang=en&id=EMP_TEM (Visited on 08/13/2024).

Imbens, Guido W. and Donald B. Rubin (1997). "Estimating Outcome Distributions for Compliers in Instrumental Variables Models". In: *The Review of Economic Studies* 64.4, pp. 555–574. ISSN: 0034-6527. DOI: 10.2307/2971731. (Visited on 07/15/2024).

Iyer, Rajkamal and Antoinette Schoar (2015). "Ex Post (In) Efficient Negotiation and Breakdown of Trade". In: *American Economic Review* 105.5, pp. 291–294. ISSN: 0002-8282. DOI: 10.1257/aer.p20151077. (Visited on 09/29/2024).

Jensen, Anders, Anne Brockmeyer, and Lucie Gadenne (2024). *Taxation and Development*. Tech. rep. 12.

Kakumba, Makanga Ronald (2024). *Most Ugandans Struggle to Access Information on Taxes They Owe and How Revenues Are Used*. Tech. rep. 886.

Kasper, Matthias and James Alm (2022). "Audits, Audit Effectiveness, and Post-Audit Tax Compliance". In: *Journal of Economic Behavior & Organization* 195, pp. 87–102. ISSN: 0167-2681. DOI: 10.1016/j.jebo.2022.01.003. (Visited on 07/09/2024).

Kasper, Matthias and Matthew D. Rablen (2023). "Tax Compliance after an Audit: Higher or Lower?" In: *Journal of Economic Behavior & Organization* 207, pp. 157–171. ISSN: 0167-2681. DOI: 10.1016/j.jebo.2023.01.013. (Visited on 07/09/2024).

Kennedy, Patrick J, Paul Landefeld, Christine L Dobridge, and Jacob Mortenson (2024). *The Efficiency-Equity Tradeoff of the Corporate Income Tax: Evidence from the Tax Cuts and Jobs Act*. Tech. rep.

Kleven, Henrik Jacobsen, Martin B. Knudsen, Claus Thustrup Kreiner, Søren Pedersen, and Emmanuel Saez (2011). "Unwilling or Unable to Cheat? Evidence From a Tax Audit Experiment in Denmark". In: *Econometrica* 79.3, pp. 651–692. ISSN: 1468-0262. DOI: 10.3982/ECTA9113. (Visited on 03/28/2022).

Kotsogiannis, Christos, Luca Salvadori, John Karangwa, and Theonille Mukamana (2024). "Do Tax Audits Have a Dynamic Impact? Evidence from Corporate Income Tax Administrative Data". In: *Journal of Development Economics* 170, p. 103292. ISSN: 0304-3878. DOI: 10.1016/j.jdeveco.2024.103292. (Visited on 07/07/2024).

Leal Ordóñez, Julio César (2014). "Tax Collection, the Informal Sector, and Productivity". In: *Review of Economic Dynamics* 17.2, pp. 262–286. ISSN: 1094-2025. DOI: 10.1016/j.red.2013.07.004. (Visited on 09/29/2024).

Li, Wanfu, Jeffrey A. Pittman, and Zi-Tian Wang (2018). "The Determinants and Consequences of Tax Audits: Some Evidence from China". In: *Journal of the American Taxation Association* 41.1, pp. 91–122. ISSN: 0198-9073. DOI: 10.2308/atax-52136. (Visited on 03/28/2022).

Liu, Li, Ben Lockwood, Miguel Almunia, and Eddy H. F. Tam (2021). "VAT Notches, Voluntary Registration, and Bunching: Theory and U.K. Evidence". In: *The Review of Economics and Statistics* 103.1, pp. 151–164. ISSN: 0034-6535. DOI: 10.1162/rest_a_00884. (Visited on 10/17/2024).

Løyland, Knut, Oddbjørn Raaum, Gaute Torsvik, and Arnstein Øvrum (2019). *Compliance Effects of Risk-Based Tax Audits*. Tech. rep. 7616. CESifo. (Visited on 03/28/2022).

Luttmer, Erzo F. P. and Monica Singhal (2014). "Tax Morale". In: *Journal of Economic Perspectives* 28.4, pp. 149–168. ISSN: 0895-3309. DOI: 10.1257/jep.28.4.149. (Visited on 10/21/2024).

Mascagni, Giulia (2018). "From the Lab to the Field: A Review of Tax Experiments". In: *Journal of Economic Surveys* 32.2, pp. 273–301. ISSN: 1467-6419. DOI: 10.1111/joes.12201. (Visited on 03/28/2022).

McKenzie, David and Yaye Seynabou Sakho (2010). "Does It Pay Firms to Register for Taxes? The Impact of Formality on Firm Profitability". In: *Journal of Development Economics* 91.1, pp. 15–24. ISSN: 0304-3878. DOI: 10.1016/j.jdeveco.2009.02.003. (Visited on 09/29/2024).

McKenzie, David and Christopher Woodruff (2008). "Experimental Evidence on Returns to Capital and Access to Finance in Mexico". In: *The World Bank Economic Review* 22.3, pp. 457–482. ISSN: 0258-6770. DOI: 10.1093/wber/lhn017. (Visited on 09/29/2024).

Mirrlees, J. A. (1971). "An Exploration in the Theory of Optimum Income Taxation12". In: *The Review of Economic Studies* 38.2, pp. 175–208. ISSN: 0034-6527. DOI: 10.2307/2296779. (Visited on 10/24/2024).

OECD, African Union Commission, and African Tax Administration Forum (2023). *Revenue Statistics in Africa 2023*, p. 388. DOI: https://doi.org/10.1787/15bc5bc6-en-fr.

OECD and ILO (2019). *Tackling Vulnerability in the Informal Economy*.

Pinotti, Paolo (2017). "Clicking on Heaven's Door: The Effect of Immigrant Legalization on Crime". In: *American Economic Review* 107.1, pp. 138–168. ISSN: 0002-8282. DOI: 10.1257/aer.20150355. (Visited on 07/08/2024).

Pomeranz, Dina (2015). "No Taxation without Information: Deterrence and Self-Enforcement in the Value Added Tax". In: *American Economic Review* 105.8, pp. 2539–2569. ISSN: 0002-8282. DOI: 10.1257/aer.20130393. (Visited on 03/28/2022).

Quinn, Simon and Christopher Woodruff (2019). "Experiments and Entrepreneurship in Developing Countries". In: *Annual Review of Economics* 11.Volume 11, 2019, pp. 225–248. ISSN: 1941-1383, 1941-1391. DOI: 10.1146/annurev-economics-080218-030246. (Visited on 09/28/2024).

Sanday, Amos (2023). *Unlocking the Potential of the Informal Sector in Uganda through Business Formalisation*. (Visited on 08/13/2024).

Schivardi, Fabiano and Roberto Torrini (2008). "Identifying the Effects of Firing Restrictions through Size-Contingent Differences in Regulation". In: *Labour Economics* 15.3, pp. 482–511. ISSN: 0927-5371. DOI: 10.1016/j.labeco.2007.03.003. (Visited on 10/18/2024).

Slemrod, Joel (2019). "Tax Compliance and Enforcement". In: *Journal of Economic Literature* 57.4, pp. 904–954. ISSN: 0022-0515. DOI: 10.1257/jel.20181437. (Visited on 04/01/2022).

Slemrod, Joel, Marsha Blumenthal, and Charles Christian (2001). "Taxpayer Response to an Increased Probability of Audit: Evidence from a Controlled Experiment in Minnesota". In: *Journal of Public Economics* 79.3, pp. 455–483. ISSN: 0047-2727. DOI: 10.1016/S0047-2727(99)00107-3. (Visited on 09/27/2024).

The Independent (2023). *Supreme Court Dismisses Heritage Appeal in $435m Oil Tax Row*. (Visited on 06/21/2024).

Ulyssea, Gabriel (2018). "Firms, Informality, and Development: Theory and Evidence from Brazil". In: *American Economic Review* 108.8, pp. 2015–2047. ISSN: 0002-8282. DOI: 10.1257/aer.20141745. (Visited on 10/06/2022).

– (2020). "Informality: Causes and Consequences for Development". In: *Annual Review of Economics* 12.1, pp. 525–546. DOI: 10.1146/annurev-economics-082119-121914. (Visited on 04/16/2023).

Vitali, Anna (2023). *Consumer Search and Firm Location: Theory and Evidence from the Garment Sector in Uganda*. Tech. rep.

Waseem, Mazhar (2023). “Overclaimed Refunds, Undeclared Sales, and Invoice Mills: Nature and Extent of Noncompliance in a Value-Added Tax”. In: *Journal of Public Economics* 218, p. 104783. ISSN: 0047-2727. DOI: 10.1016/j.jpubeco.2022.104783. (Visited on 08/20/2024).

World Bank (2017). *Informal Employment by Gender (% of Total Non-Agricultural Employment)*. https://databank.worldbank.org/source/gender-statistics/Series/SL.ISV.IFRM.FE.ZS#advancedDownloadOpti (Visited on 08/13/2024).

– (2019a). *Global Economic Prospects, January 2019: Darkening Skies*. Washington, DC: World Bank. ISBN: 978-1-4648-1343-6 978-1-4648-1386-3. DOI: 10.1596/978-1-4648-1343-6. (Visited on 08/13/2024).

– (2019b). *National Manpower Survey 2016, Kampala Informal Sector Survey*. DOI: 10.48529/8H55-GF47.

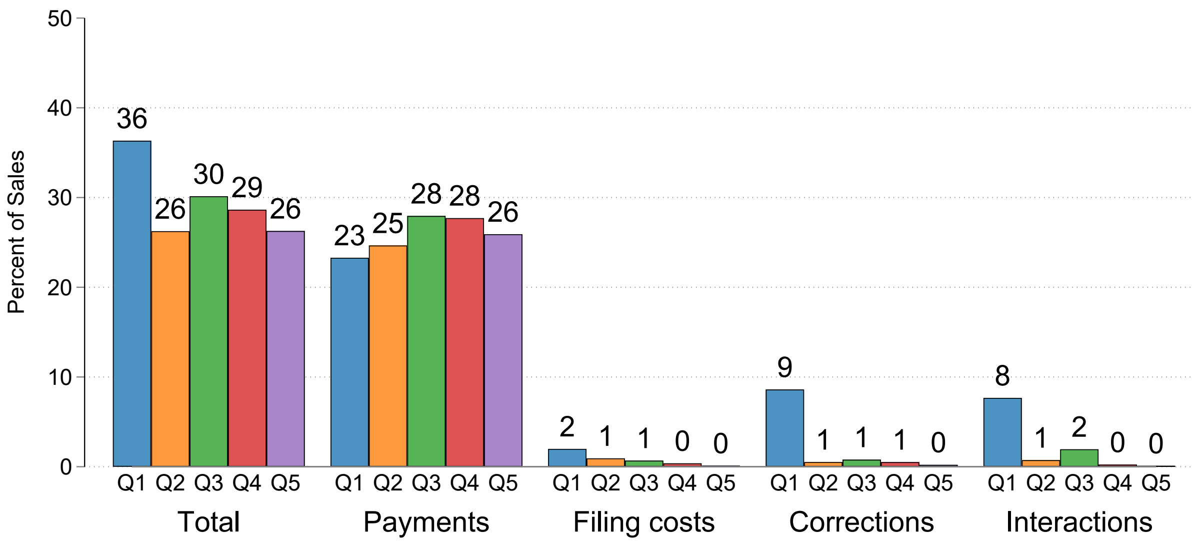
– (2023a). *Enterprise Surveys*.

– (2023b). *GNI per Capita in PPP (Current International Dollars)*. https://data.worldbank.org. (Visited on 07/08/2024).

Tables & Figures

Figures

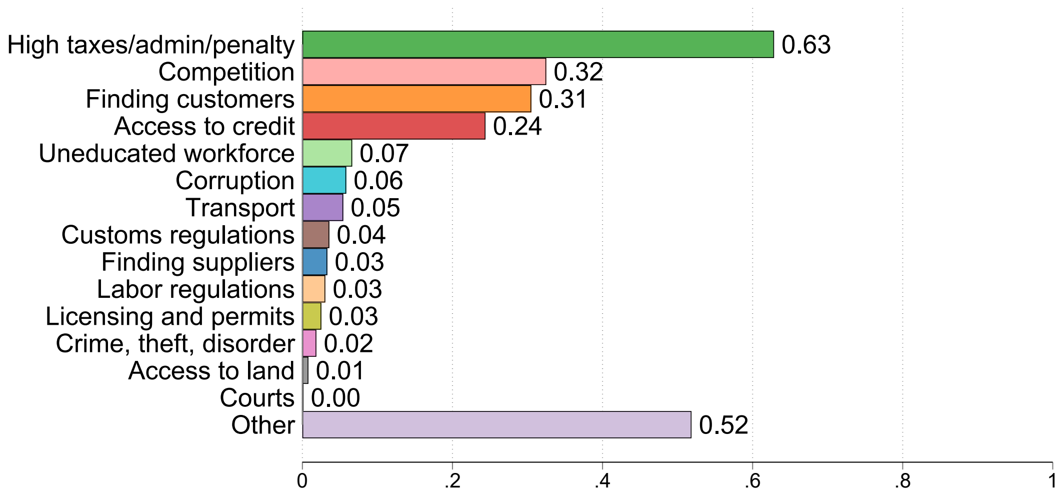
Figure 1
Costs of filing taxes as a share of sales



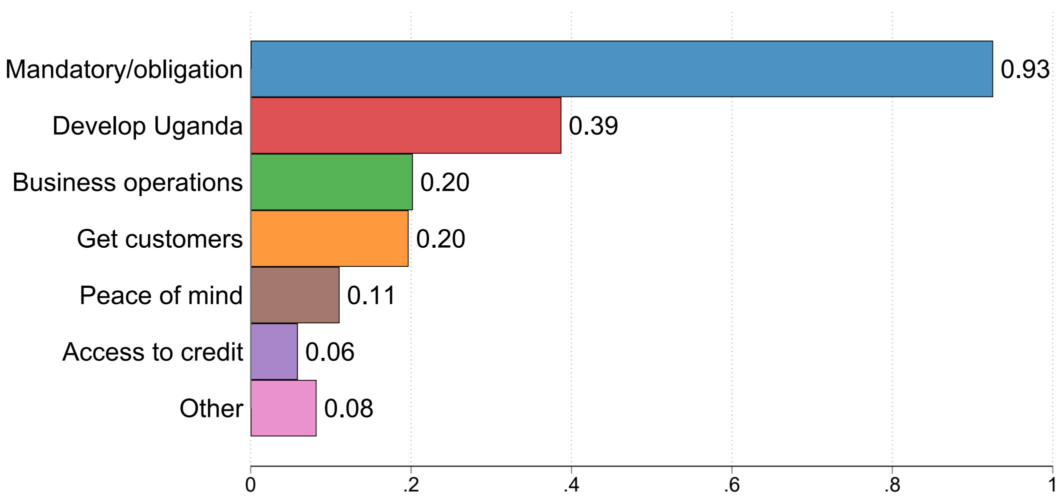
Notes: This figure displays average costs associated with filing taxes as a share of sales across the quintiles of firm sales. "Payments" are total tax payments across all tax heads. "Filing costs" are the total administrative costs (internal and outsourced) of filing taxes. "Corrections" is the average value of the tax corrections filed by the URA over the last two accounting years. "Interactions" are all the costs of interacting with the URA, including audits. "Total" is the total costs across all the categories. Each term is presented as a share of the firms sales. We restrict to firms that reported sales, tax payments, and either reported the correction value or did not report receiving a correction. Sample consists of 528 firms. Source: Authors' survey and computation.

Figure 2
Obstacles to firm performance and reasons for paying taxes

(a) Top three obstacles to firm growth



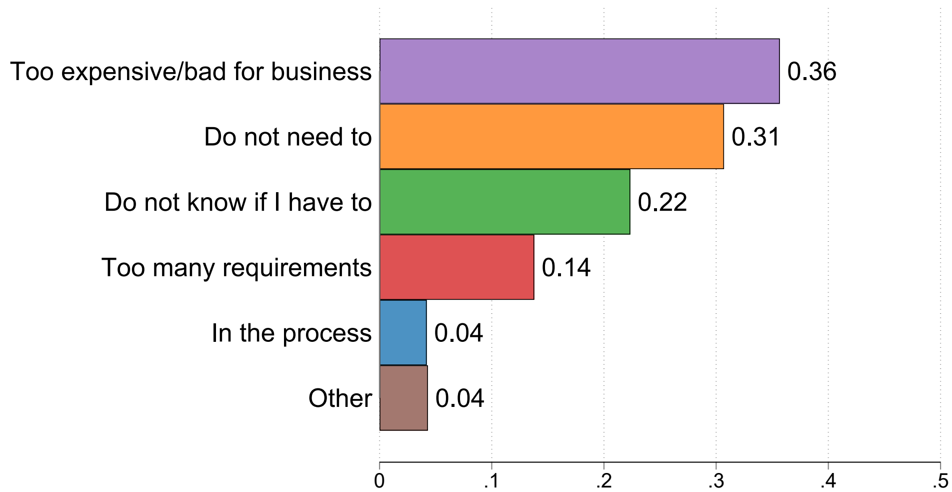
(b) Top three reasons for paying taxes



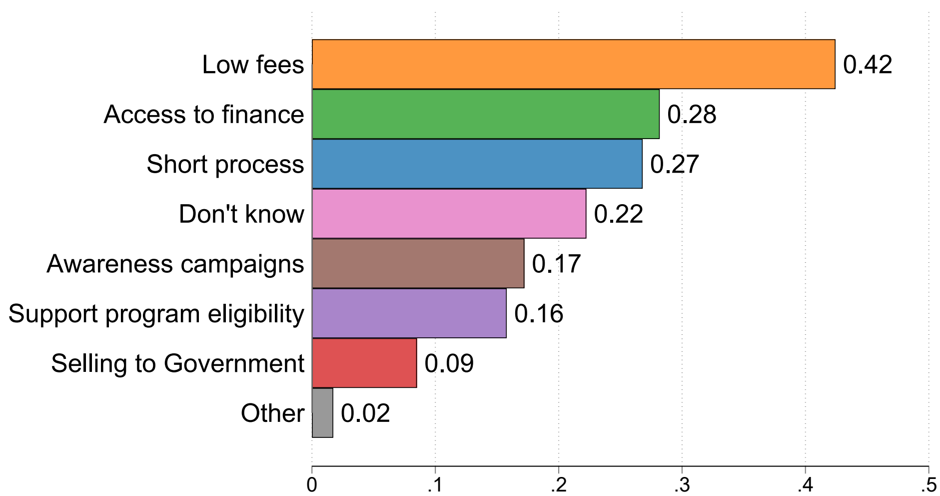
Notes: This figure displays the top three obstacles for firm performance (Panel (a)) and the top three reasons for paying taxes (Panel (b)). In *Panel (a)* the question was a multiple select question with an option of "other, specify" in which case the enumerator typed in the answer of the respondent.. The exact phrasing of the question was: "we will now ask you about the three most important obstacles to the firms' performance in the last accounting year". The question was asked before discussing the financials of the firms and any questions related to taxes. We aggregate across three answer options that relate to taxes and the URA, namely: "High taxes", "High administrative costs of taxes", and "Tax assessments". N = 754. In *Panel (b)* the question was a multiple select question with an option of "other, specify" in which case the enumerator typed in the answer of the respondent. The exact phrasing of the question was: "What is the most/second most/third most important reason you pay taxes?" N = 767. Source: Authors' survey and computation.

Figure 3
Reasons for not formalizing among informal firms

(a) Why is the firm not registered?

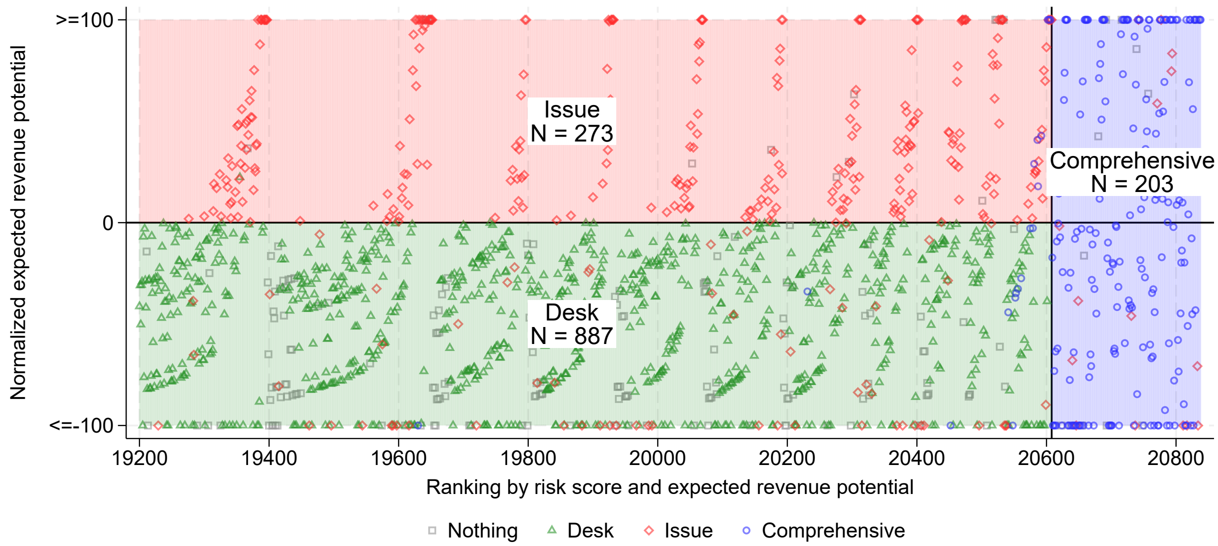


(b) What would encourage firm registration?



Notes: This figure displays the main reasons for why informal firms are not registered (Panel (a)) and what would encourage them to register (Panel (b)). The question was about registration with the Ugandan Registry Services Bureau, not with the Ugandan Revenue Authority. In both Panel (a) and (b) reasons were not mutually exclusive, respondents could choose more than one option. In Panel (a) we combine "too expensive" and "bad for business" to capture the costs of becoming formal. N = 1951. Source: World Bank (2019b), authors' calculations.

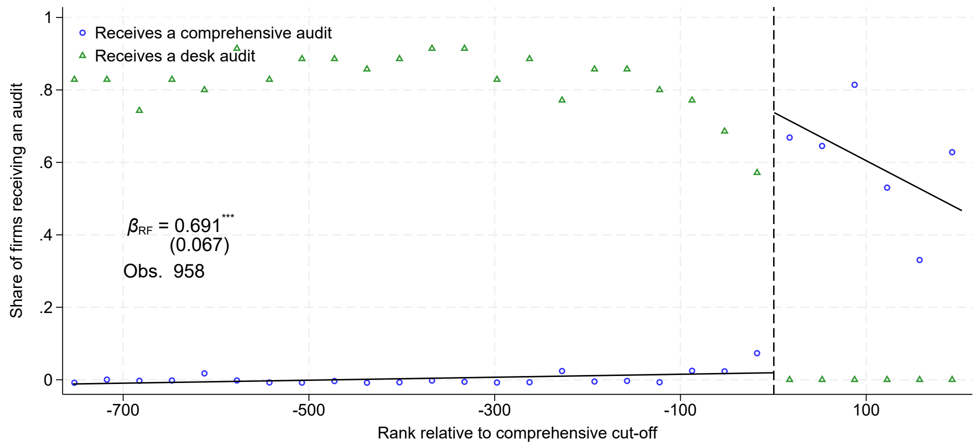
Figure 4
Relationship between the ordinal rank of the firms, its normalized expected potential revenue, and audit assignment



Notes: This figure presents the discontinuity leveraged for identification. On the horizontal axis is the ordinal rank of the firm which depends on the discrete risk score (first) and the expected potential revenue (second). On the y-axis is the expected potential revenue of the firm normalized to the expected potential revenue of the last firm receiving a desk audit within the firms local jurisdiction. Each point represents a firm. We stop the distribution at ordinal rank 19,200 to make it readable. In the main specification we compare firms assigned to comprehensive audits against firms assigned to desk audits. Source: Authors' calculations using URA tax data.

Figure 5

The effect of a firms' relative rank on the probability of receiving a comprehensive audit

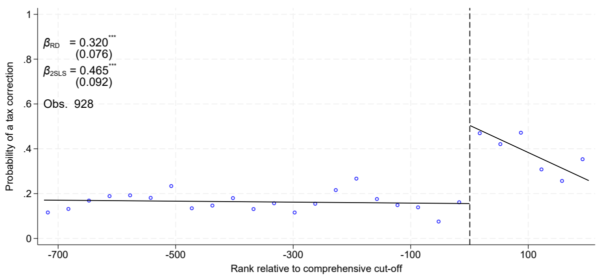


Notes: This figure presents the discontinuity leveraged for identification. The probability of comprehensive audit is residualized. The plots are manually constructed, each point on the graph includes approximately 35 firms. The blue circles show the share of firms receiving a comprehensive audit and the green triangles show the share of firms receiving a desk audit within each of the manually defined bins. The black lines show the linear best fit for the residualized data. The black dashed line presents the threshold. Bandwidths are the optimal bandwidths calculated using the "rdrobust" package by Calonico et al. (2014). We present the reduced form coefficient we retrieve from running equation 1, with heteroskedastic robust standard errors presented in parenthesis below the coefficient. "Obs." refers to the number of firms used in the regression. Source: Authors' calculations using URA tax data.

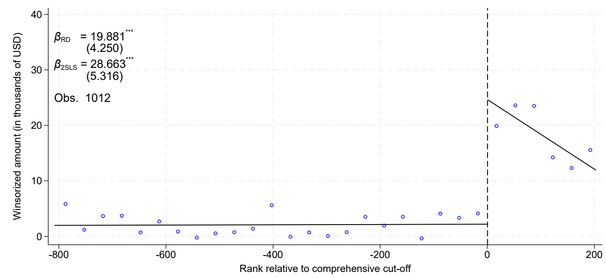
Figure 6

The effect of comprehensive audits on increased tax liability due to tax corrections

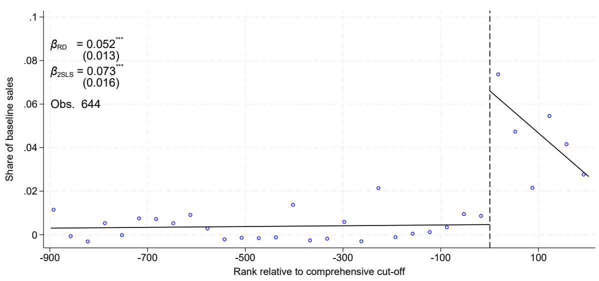
(a) Probability of an increase in tax liability



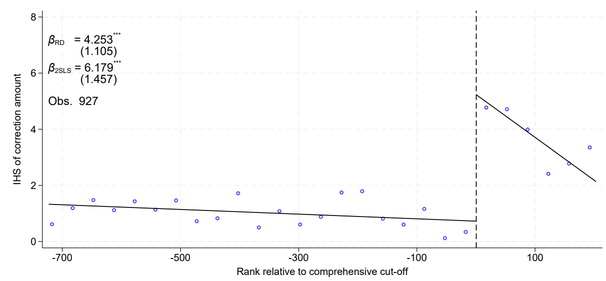
(b) Winsorized increase in tax liability



(c) Increase in tax liability as a share of baseline sales



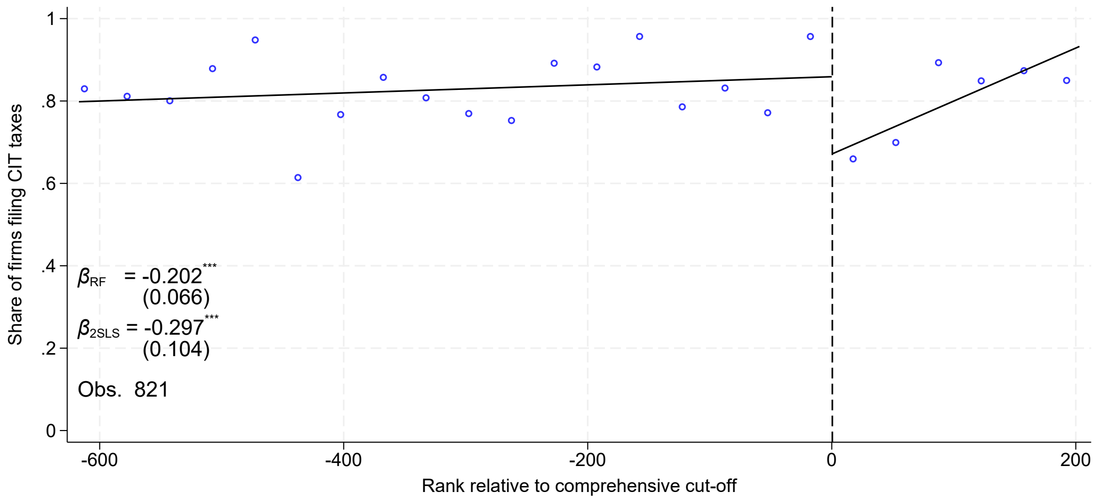
(d) IHS transformation of increase in tax liability



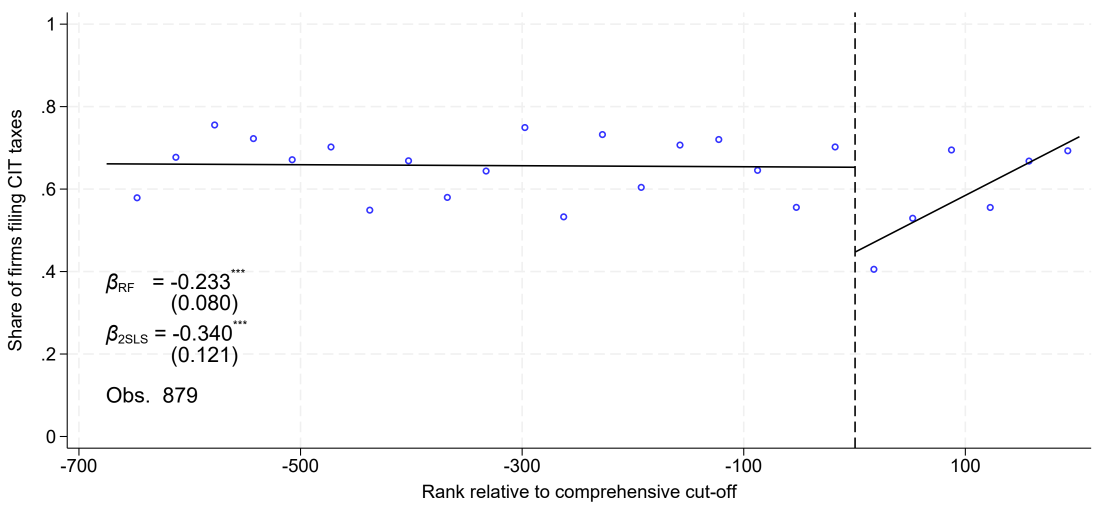
Notes: This figure presents results for the differential increases in tax liability due to tax corrections issued as a consequence of the audit. We compare the increase in tax liability from tax corrections (among firms receiving comprehensive audits) against the increase in tax liability from self-corrections (among firms receiving desk audits). In Panel (a) we report the probability of the tax liability increasing. In Panel (b) we report the increase in tax liability in thousands of USD, winsorized at the 1%. In Panel (c) we present the increase in tax liability as a share of baseline sales. In Panel (d) we present the inverse hyperbolic sine transformation of the amount of tax liability. The outcome variables shown in the figures are residualized. The plots are manually constructed, each point on the graph contains roughly 35 firms. The black lines show the linear best fit for the residualized data. The black dashed line indicates the threshold. Bandwidths are the optimal bandwidths calculated using the "rdrobust" package by Calonico et al. (2014). β_{RF} shows the reduced form effects – running equation 2 – and β_{2SLS} shows the 2SLS effect – running equation 1 and 3. Heteroskedastic robust standard errors are shown in parentheses under the coefficients. "Obs." refers to the number of firms used in the regression. Source: Authors' calculations using URA tax data.

Figure 7
The effect of comprehensive audits on the probability of filing CIT returns

(a) Filed CIT return one year post-audit



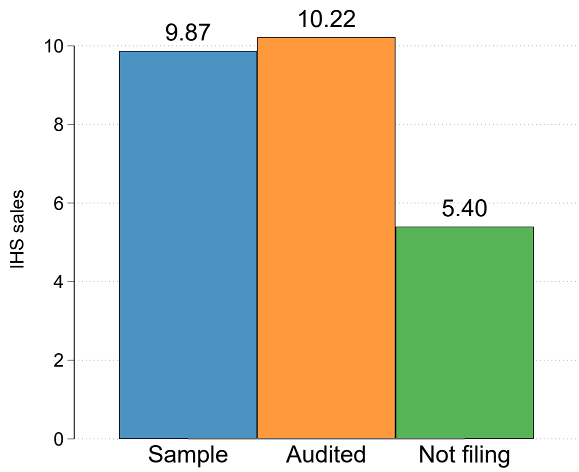
(b) Filed CIT return two years post-audit



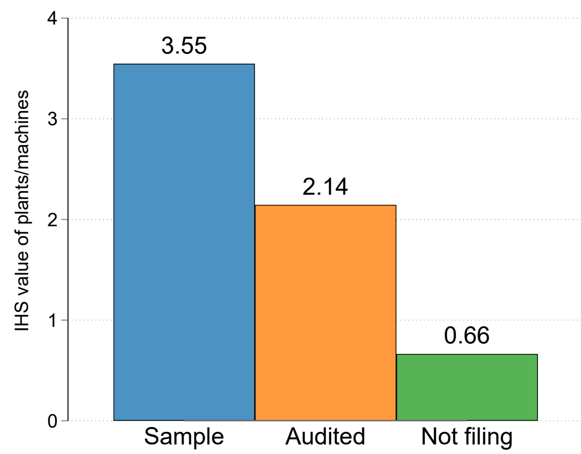
Notes: This figure presents results on the probability of filing CIT returns after the audit. In Panel (a) we report the probability of filing a CIT return in the year after the audit (July 2022 to June 2023). In Panel (b) we report the probability of filing a CIT return in the second year post-audit (July 2023 to June 2024). The outcome variables shown in the figures are residualized. The plots are manually constructed, each point on the graph contains roughly 35 firms. The black lines show the linear best fit for the residualized data. The black dashed line indicates the threshold. Bandwidths are the optimal bandwidths calculated using the "rdrobust" package by Calonico et al. (2014). β_{RF} shows the reduced form effects – running equation 2 – and β_{2SLS} shows the 2SLS effect – running equation 1 and 3. Heteroskedastic robust standard errors are shown in parentheses under the coefficients. "Obs." refers to the number of firms used in the regression. Source: Authors' calculations using URA tax data.

Figure 8
Baseline characteristics of compliers

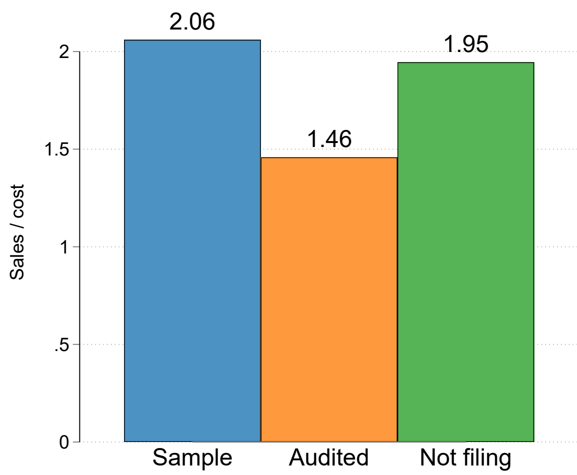
(a) Inverse hyperbolic sine of sales



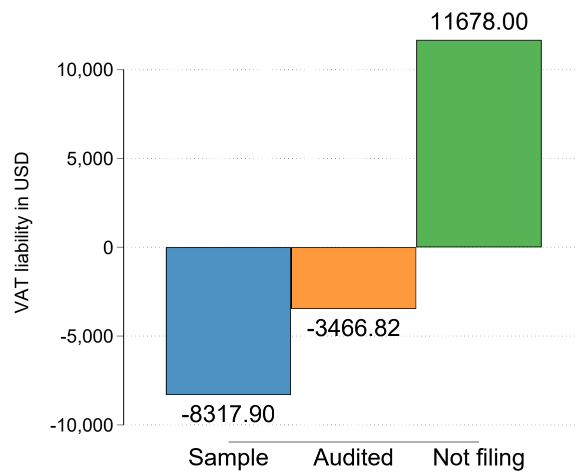
(b) Inverse hyperbolic sine of plants and machines



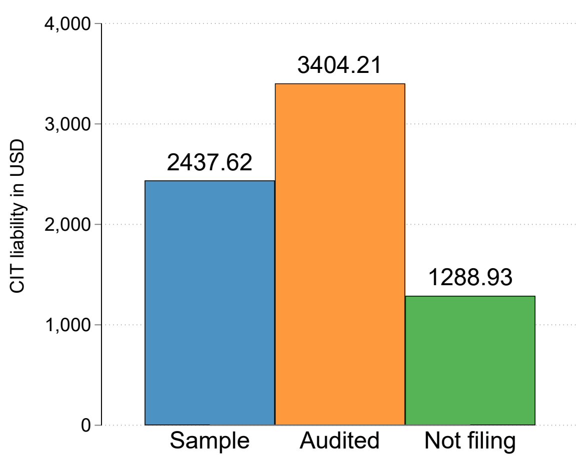
(c) Sales to cost ratio



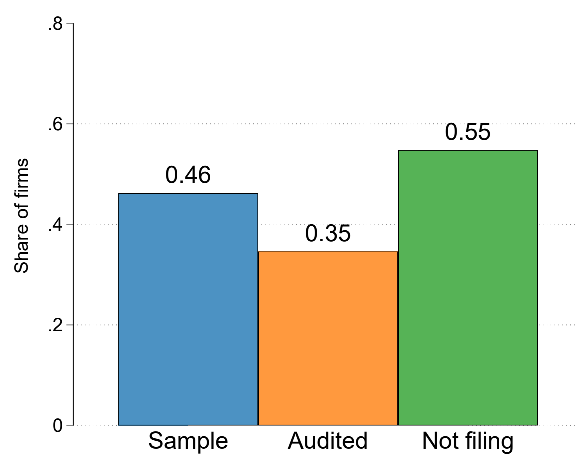
(d) VAT liability in USD



(e) CIT liability in USD



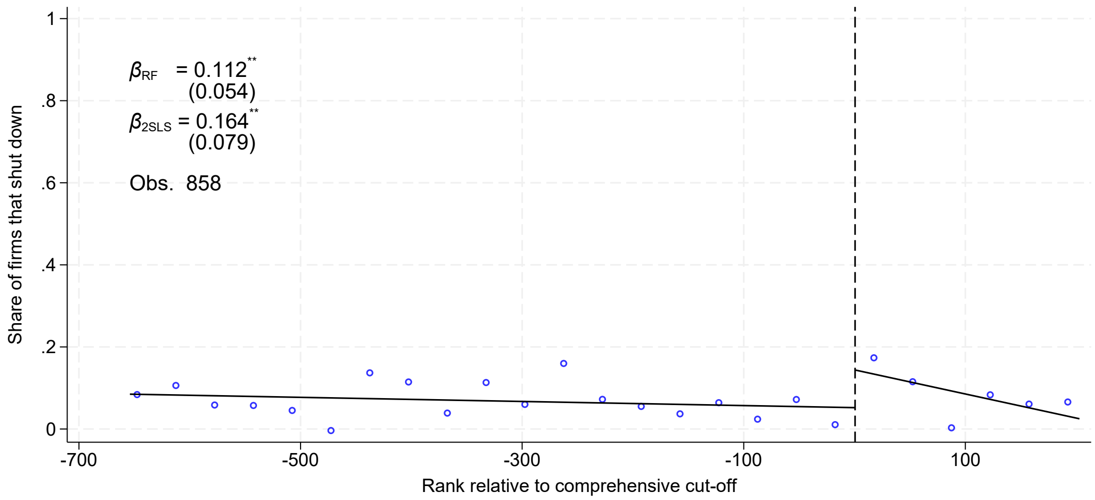
(f) Share of firms in the service sector



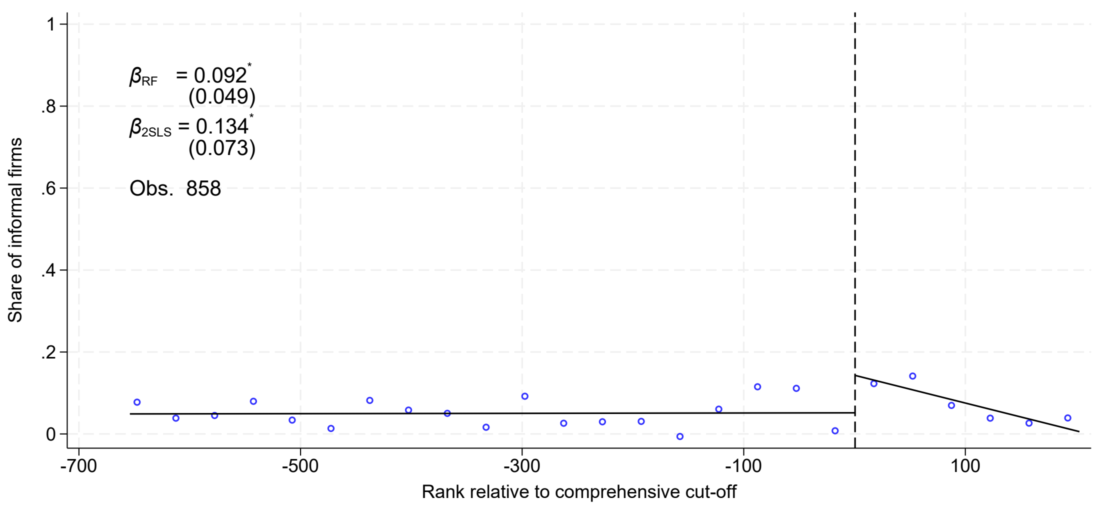
Notes: This figure presents the baseline characteristics of compliers. All variables are based on information submitted in the returns for financial year 2019/20. The first bar ("Sample") presents average baseline characteristics for the sample within the optimal bandwidth of the variable in question. The second bar ("Audited") presents the baseline characteristics for the firms that get induced into comprehensive audits. The third bar ("Not filing") presents baseline characteristics for firms that get induced into not filing for two years after the audit. The estimates in bar two and three are derived from running equation 4 and 5. Bandwidths used are the optimal bandwidths calculated using the "rdrobust" package by Calonico et al. (2014).

Figure 9
The effect of comprehensive audits on firm closure and informality

(a) Probability of being closed



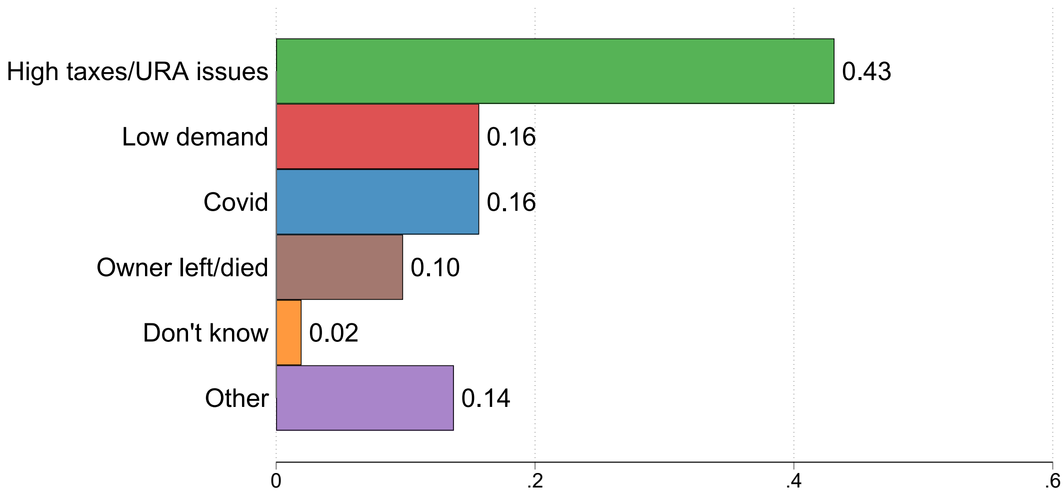
(b) Probability of being informal



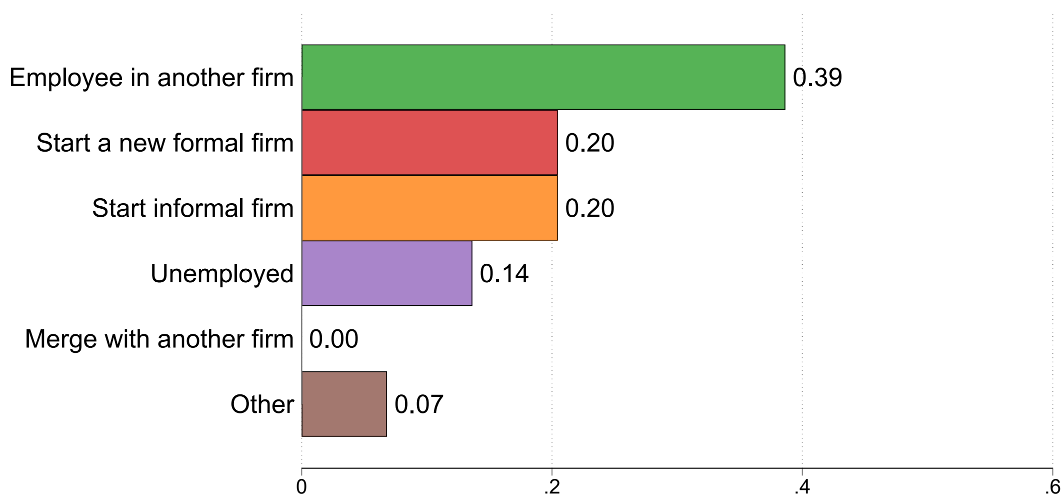
Notes: This figure present results on the probability of being closed (Panel (a)) or being informal (Panel (b)) roughly two years after the audit. "Probability of being closed" is an indicator for whether the firm closed or vanished *and* did not file a CIT return for two years after the audit. "Probability of being informal" is an indicator for whether we found the firm, but it did not file a CIT return for two years after the audit. The outcome variables shown in the figures are residualized. The plots are manually constructed, each point on the graph contains roughly 35 firms. The black lines show the linear best fit for the residualized data. The black dashed line indicates the threshold. Bandwidths are the optimal bandwidths calculated using the "rdrobust" package from Calonico et al. (2014) for the outcome variable whether a firm filed a CIT return in the two years after the audit. This is because we tracked down all the firms within the bandwidth for that variable (our closest measure to firm exit in the administrative tax data). β_{RF} shows the reduced form effects – running equation 2 – and β_{2SLS} shows the 2SLS effect – running equation 1 and 3. Heteroskedastic robust standard errors are shown in parentheses under the coefficients. "Obs." refers to the number of firms used in the regression. Source: URA administrative tax data and authors' survey. Authors' calculations.

Figure 10
Reasons for firm closure and subsequent occupation of respondents

(a) Why did the firm close?



(b) What is the respondent doing now?



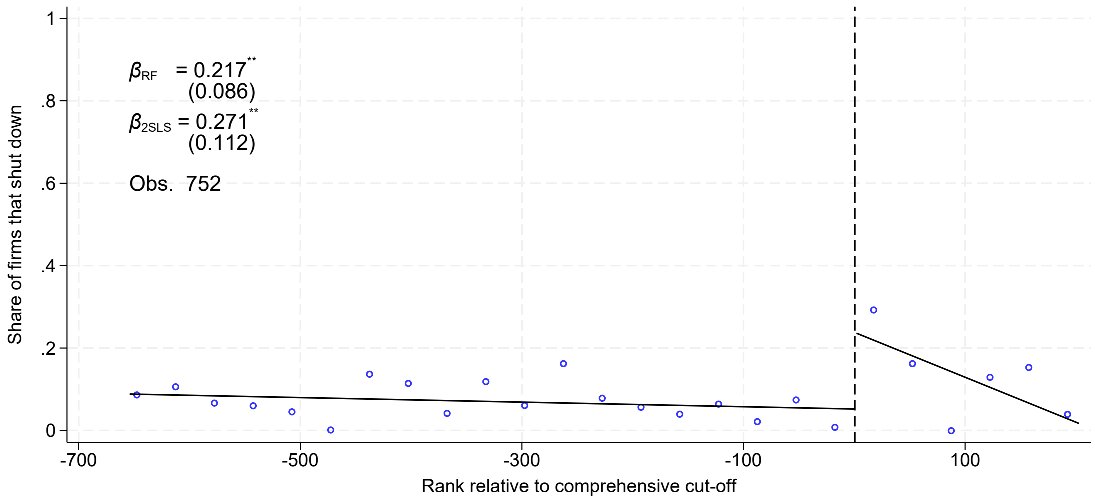
Notes: This figure presents reasons for closure (Panel (a)) and subsequent occupations of the respondent if the firm closed (Panel (b)). In *Panel (a)* the question was asked to respondent who could confirm that the firm had closed. The question was asked as soon as the respondent confirmed the firm had closed and it was an open ended question. We categorized question into the categories shown here. N = 52. In *Panel (b)* we show the current occupation of the respondent that confirmed that the firm had closed. This was a multiple select question, with an option of "Other, specify". If "Other, specify" was selected the enumerator typed out the answer. The question was asked immediately after the question on why the firm closed. Occupations are mutually exclusive. N = 44. Source: Authors' survey and calculations.

Figure 11
The effect of comprehensive audits on the sales of firms

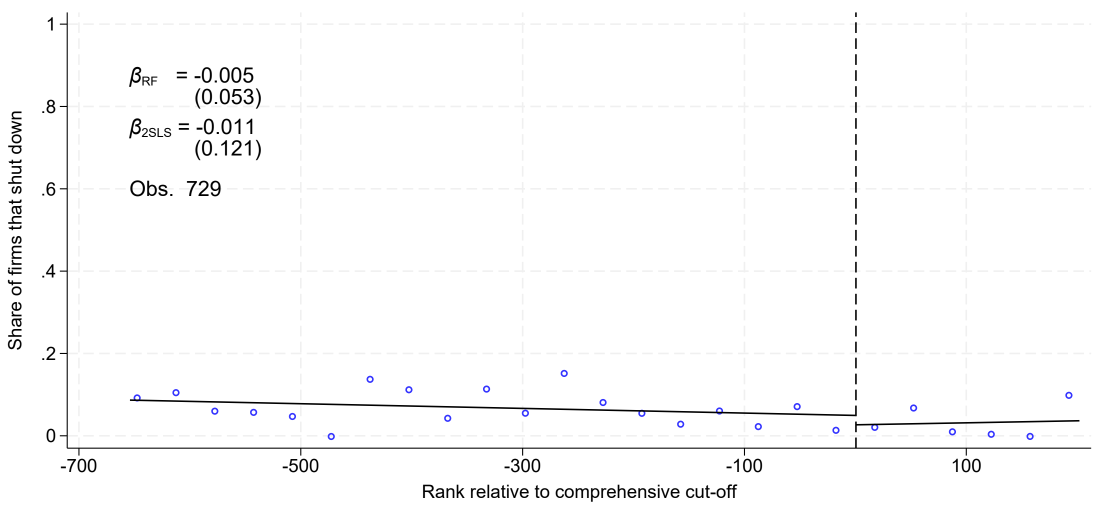
Notes: This figures presents the results from running equation 7 for the administrative data and a combination of the administrative and survey data for each of the outcomes shown in table 5. The blue circle indicate the coefficient when using survey data for the last year (2022), and the red squares represent coefficients using the administrative tax data. We restrict to firms that either: 1) reported sales in our survey, 2) reported that their last active accounting year was before 2022, so we can infer that their sales in 2022 were zero, or 3) we know that the firm closed down, hence we can also infer that their sales in 2022 were zero. In Panel (a) we present an indicator for whether any sales information was reported (i.e. it was not missing), Panel (b) is an indicator for whether the firm reported positive sales, conditional on submitting any sales information. Panel (c) is the amount of sales reported in thousands of USD, winsorized at the top and bottom 1 percentile and imputing zero's for firms that did not report sales. Panel (d) is the logarithmic transformation on the amount of sales, effectively conditioning on reporting sales in 2022. The shaded area indicates the two years we remove when running equation 6. Standard errors are robust to heteroskedasticity and clustered at the firm level.

Figure 12
The effect of comprehensive audits on firm shutdown separately by high and low correction share

(a) Correction share above median



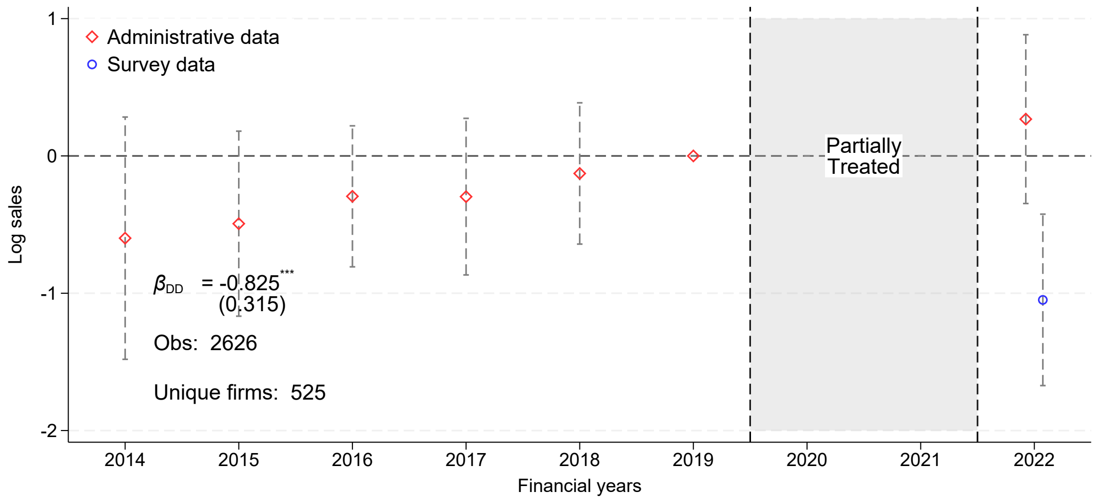
(b) Correction share below median



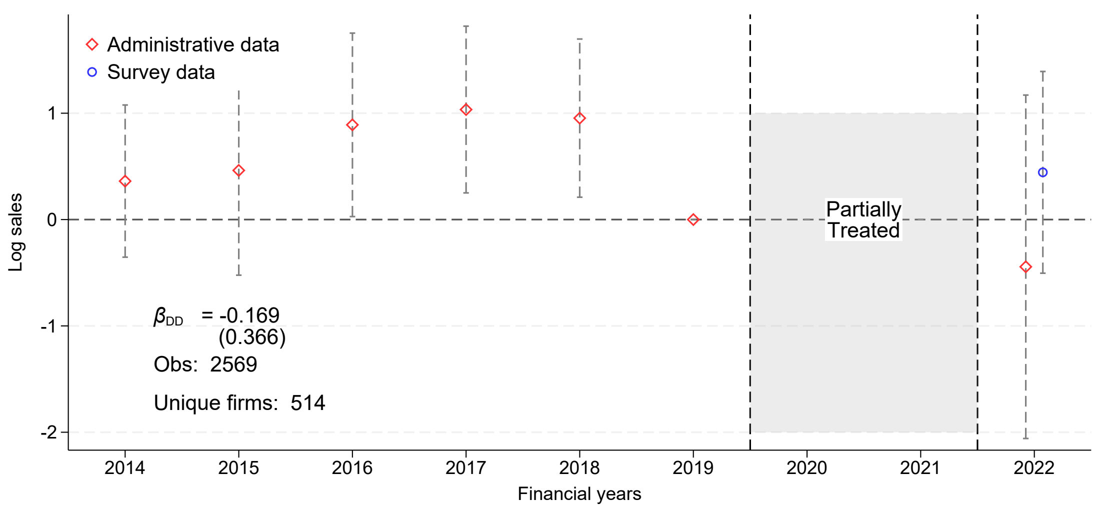
Notes: This figure present results on the probability of being closed roughly two years after the audit for firms with an above median (Panel (a)) and below median (Panel (b)) corrections as a share of baseline sales. If a firm reported zero sales at baseline we include them in Panel (a). "Probability of being closed" is an indicator for whether the firm closed or vanished *and* did not file a CIT return for two years after the audit. The outcome variables shown in the figures are residualized. The plots are manually constructed, each point on the graph contains roughly 35 firms. The black lines show the linear best fit for the residualized data. The black dashed line indicates the threshold. Bandwidths are the optimal bandwidths calculated using the "rdrobust" package from Calonico et al. (2014) for the outcome variable whether a firm filed a CIT return in the two years after the audit. This is because we tracked down all the firms within the bandwidth for that variable (our closest measure to firm exit in the administrative tax data). β_{RF} shows the reduced form effects – running equation 2 – and β_{2SLS} shows the 2SLS effect – running equation 1 and 3. Heteroskedastic robust standard errors are shown in parentheses under the coefficients. "Obs." refers to the number of firms used in the regression. Source: URA administrative tax data and authors' survey. Authors' calculations.

Figure 13
The effect of comprehensive audit on sales separately by high and low belief of audit probability

(a) Belief of audit probability above median



(b) Belief of audit probability below median

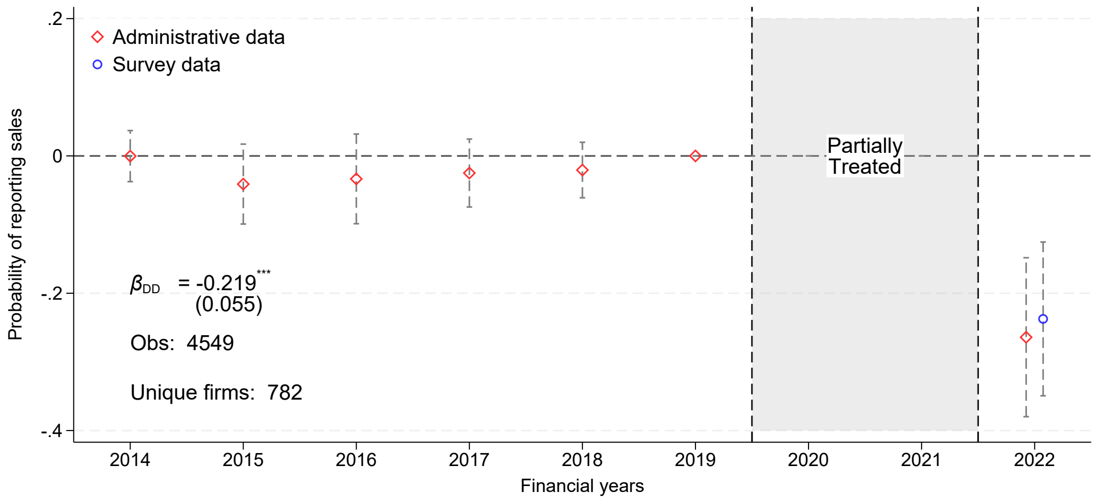


Notes: This figures presents the results from running equation 7 for the administrative data and a combination of the administrative and survey data for the logarithmic transformation of sales, separately be above and below median perceived audit rates. The blue circle indicate the coefficient when using survey data for the last year (2022), and the red squares represent coefficients using the administrative tax data. We restrict to firms that either: 1) reported sales in our survey, 2) reported that their last active accounting year was before 2022, so we can infer that their sales in 2022 were zero, or 3) we know that the firm closed down, hence we can also infer that their sales in 2022 were zero. We also restrict to firms that answered the question: "Finally, imagine firms similar to yours in size and sector. In your opinion, what percentage of these firms do you think will be audited in the upcoming financial year?". In Panel (a) we remove firms above the discontinuity that reported a below median percentage, and in Panel (b) we remove firms that reported an above median percentage. The shaded area indicates the two years we remove when running equation 6. β_{DD} shows the difference-in-difference coefficient from running equation 6. "Unique firms" is the number of unique firms used in the regression. Standard errors are robust to heteroskedasticity and clustered at the firm level.

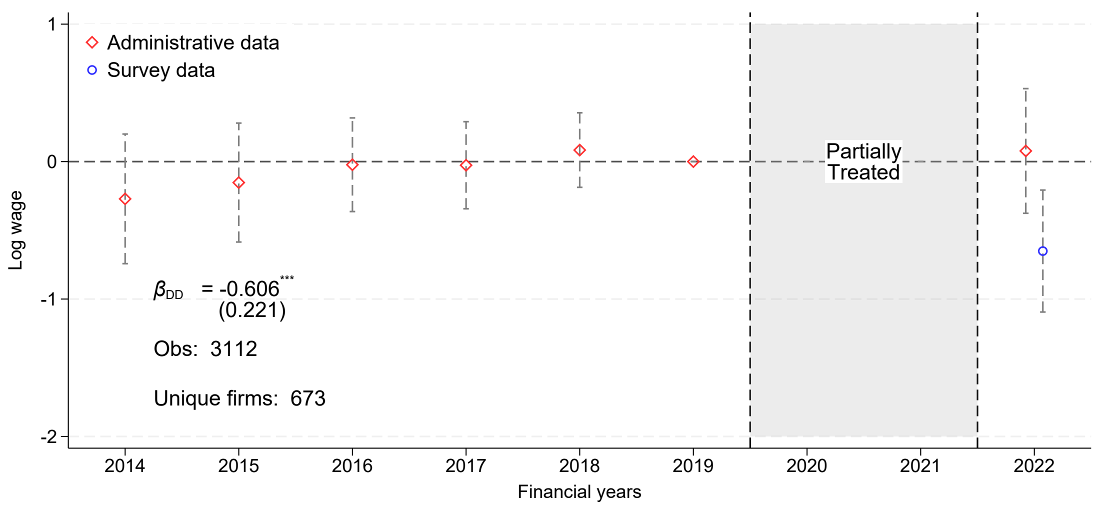
Figure 14

The effect of comprehensive audit on outcomes used in the aggregation exercise

(a) Probability of reporting sales



(b) Logarithmic transformation of wage bill



Notes: These figures presents the results from running equation 7 for the administrative data and a combination of the administrative and survey data for the wage bill. We restrict to firms that either reported sales in our survey or reported that their last active accounting year was before 2022, so we can infer that their sales in 2022 were 0. Panel (a) is the change in the percentage of the wage bill relative to the wage bill at baseline (in financial year 2019), winsorized at the top and bottom 1 percentile. Panel (b) is the logarithmic transformation of the wage bill. The shaded area indicates the two years we remove when running the DD-specification. Standard errors are robust to heteroskedasticity and clustered at the firm level.

Tables

Table 1
Summary statistics for different samples

	All	Comprehensive			
		Full		Optimal	
	Mean	Mean	P-value	Mean	P-value
	(1)	(2)	(3)	(4)	(5)
Registration					
Years since tin registration	5.95	6.46	0.000	7.67	0.000
VAT data					
Share of VAT returns filed	0.23	0.49	0.000	0.78	0.000
IHS Sales	10.44	10.95	0.000	12.04	0.000
IHS Purchases	9.44	9.98	0.000	11.18	0.000
Share with positive VAT	0.51	0.54	0.000	0.54	0.052
IHS VAT	4.49	4.74	0.013	4.77	0.277
Share with positive VAT due	0.44	0.45	0.021	0.44	0.783
IHS VAT Due	1.18	1.03	0.250	0.60	0.078
Number of suppliers	2.14	1.63	0.000	2.55	0.112
Number of buyers	1.17	1.12	0.008	1.58	0.000
CIT data					
Prob. of filing CIT	0.56	0.73	0.000	0.77	0.000
IHS Turnover	6.32	9.00	0.000	9.85	0.000
IHS Cost of sales	4.88	7.14	0.000	8.49	0.000
Prob. of positive pre-tax profits	0.34	0.46	0.000	0.38	0.010
IHS Profit pre tax	0.51	0.62	0.172	-0.43	0.000
Prob. of positive CIT liability	0.32	0.43	0.000	0.36	0.014
IHS CIT liability	2.25	3.22	0.000	3.22	0.000
PAYE data					
Share of PAYE returns filed	0.28	0.50	0.000	0.69	0.000
Number of employees	22.13	13.30	0.000	26.90	0.361
Sectors					
Agriculture	0.05	0.04	0.000	0.03	0.015
Construction	0.14	0.13	0.002	0.13	0.228
Manufacturing	0.05	0.06	0.000	0.11	0.000
Missing sector	0.01	0.01	0.002	0.01	0.579
Retail trade	0.10	0.11	0.512	0.10	0.602
Services	0.52	0.52	0.801	0.46	0.001
Wholesale trade	0.13	0.14	0.022	0.17	0.000
P-value of joint F-test			0.000		0.000
Number of firms	58,651	13,014		937	

Notes: All variables are based on information submitted in the returns for financial year 2019/20. Column (1) presents averages for the universe of firms that filed at least one tax return prior to the audit year. Column (2) and (4) present averages for the full comprehensive sample and the comprehensive sample that tend to fall within the optimal bandwidth. To derive at the latter, we select the firms within the median optimal bandwidth across all baseline covariates shown here. Column (3) shows the p-value of a t-test for whether the average of the comprehensive sample is significantly different from the average in the universe of firms. Column (4) repeats the same exercise, but comparing the average for firms in the optimal bandwidth to the average in the full comprehensive sample. Variables preceded by "IHS" are the inverse hyperbolic sine transformation of the variable in question. For the *VAT data* variables represent monthly averages over the 12 months from July 2019 to June 2020. There are two exceptions to this. "Prob. caught evading" is an indicator for whether any assessment was filed during the financial year that increased the tax liability of the firm. "Number of returns amended" is the total number of VAT amendments that were filed during the financial year, we count missings as 0's. Unless otherwise indicated, variables are conditional on filing at least one VAT return. For the *CIT data* variables show what is declared in the CIT return for financial year 2019/20. There is one exception, "Number of returns amended" is the total number of CIT amendments filed during the financial year. Unless otherwise indicated, variables are conditional on filing a CIT return. For the *PAYE data* "Number of employees" is the average number of employees reported conditional on filing at least one PAYE return. Sectors are aggregate versions of the sector classification used by the URA. Source: VAT, CIT, PAYE return from the URA. Authors' calculations.

Table 2
Effect of comprehensive tax audits on CIT liability in the year after the audit

	Probability of	Amount of CIT liability		
	positive CIT	Winsorized	Scaled	IHS
	(1)	(2)	(3)	(4)
RD coefficient	-0.184^{***}	-3.714^{**}	-0.015^{**}	-1.864^{***}
	(0.067)	(1.761)	(0.007)	(0.661)
2SLS coefficient	-0.269^{***}	-5.463^{**}	-0.022^{**}	-2.729^{***}
	(0.100)	(2.670)	(0.010)	(0.997)
N	870	816	497	861
Mean in control	0.33	3.98	0.02	3.01
Bandwidth	667	613	658	658

Notes: This table presents reduced form and two stage least squares estimates for the amount of CIT liability in the year after the audit. In Column (1) we present an indicator for the probability of filing a CIT return with a positive CIT liability. In Column (2) we present the amount of CIT liability submitted in thousands of USD winsorized at the top and bottom 1%. In Column (3) we present the amount of CIT liability submitted scaled by baseline sales. In Column (4) we present the inverse hyperbolic sine transformation of the CIT liability. The reduced form (RF) coefficient is estimated by running equation 2. The 2SLS coefficient is estimated by running equation 1 and 3. All regressions include station fixed effects and indicators for whether the firm was in the VAT or CIT sample at baseline. Standard errors are robust to heteroskedasticity.

Table 3
Effect of comprehensive tax audits on total VAT liability in the first 5 months after the audit

	Probability of	Amount of VAT liability		
	positive VAT	Winsorized	Scaled	IHS
	(1)	(2)	(3)	(4)
RD coefficient	-0.041	-21.327^{**}	-0.091	-1.371
	(0.062)	(10.159)	(0.073)	(1.526)
2SLS coefficient	-0.060	-30.833^{**}	-0.139	-1.991
	(0.089)	(14.464)	(0.109)	(2.171)
N	895	975	831	931
Mean in control	0.31	-7.29	-0.13	-0.07
Bandwidth	692	772	894	728

Notes: This table presents reduced form and two stage least squares estimates for the amount of VAT liability collected in the first five months of the financial year after the audit. In Column (1) we present an indicator for the probability of filing a VAT return with a positive VAT liability. In Column (2) we present the total amount of VAT liability submitted across the 5 months in thousands of USD, winsorized at the top and bottom 1%. In Column (3) we present the amount of VAT liability submitted scaled by baseline sales. In Column (4) we present the inverse hyperbolic sine transformation of the VAT liability. The reduced form (RF) coefficient is estimated by running equation 2. The 2SLS coefficient is estimated by running equation 1 and 3. All regressions include station fixed effects and indicators for whether the firm was in the VAT or CIT sample at baseline. Standard errors are robust to heteroskedasticity.

Table 4
Summary statistics on tracking and survey completion rates

	N	Percent
All firms	858	
Closed shop	82	10%
Cannot be found (vanished)	76	9%
Exist	683	80%
Completed survey	301	44%
Refused participation	221	32%
Other	161	24%
NGO	17	2%

Notes: This table presents summary statistics for the tracking exercise and the share of surveys done among the firms that were found to exist. We define a firm as "Closed shop" if we can confirm from neighbours of the former premises or former employees/owners that the firm closed. "Vanished" is defined as us not being able to reach the firm despite at least three phone call attempts for each number and a visit to each location we have on file. "Other" are firms that were found to exist and never officially gave us a refusal. However, they kept delaying and rescheduling the interview to the point where it was not completed. The sample is the set of firms that fall within the optimal bandwidth of the regression discontinuity for the probability of filing a CIT return during the two years after the audit.

Table 5
The effect of comprehensive tax audits on sales using administrative tax and survey data

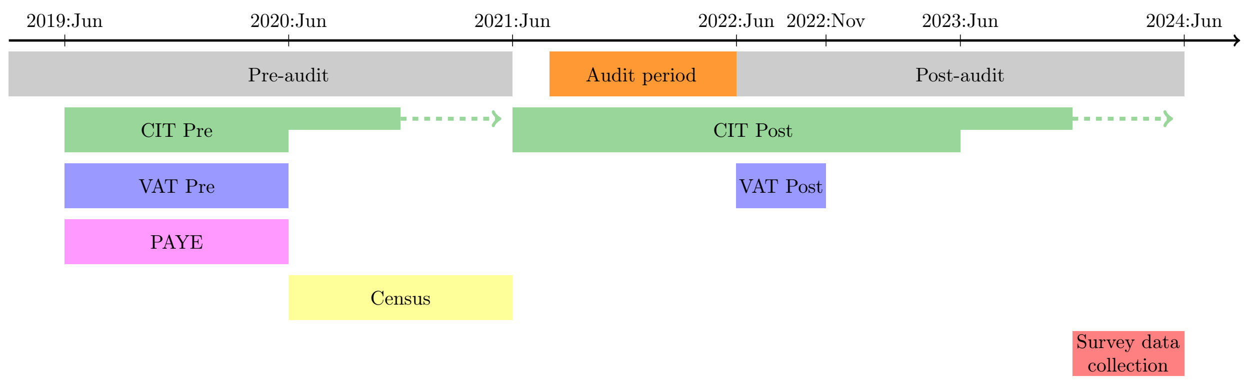
	Panel A: Admin					*Panel B: Admin & survey*				
	Probability of		Sales amount			Probability		Sales amount		
	Sales information	Positive sales	Log	Winsorized	Winsorized imputing zeros	Sales information	Positive sales	Log	Winsorized	Winsorized imputing zeros
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
DD coefficient	-0.208^{***}	-0.193^{**}	0.017	-118.171	-258.861^{**}	-0.022	-0.062^{*}	-0.842^{***}	-93.690	-265.958^{**}
	(0.060)	(0.078)	(0.337)	(211.747)	(131.683)	(0.018)	(0.033)	(0.270)	(144.726)	(127.295)
N	4549	2856	2342	2856	4549	4549	3686	3158	3686	4549
No. of firms	782	503	478	503	782	782	654	630	654	782
Mean of Dep. in Control	0.952	0.836	19.831	386.696	347.433	0.952	0.838	19.800	357.250	353.101

Notes: This table presents results from running equation 6 exclusively for the administrative data (Panel A) and a combination of the administrative and survey data (Panel B). In Panel B we replace the administrative with the survey data in the post-audit period (2022). Throughout we include years 2013-2019 for the pre-period and 2022 for the post-period. We restrict to firms that either: 1) reported sales in our survey, 2) reported that their last active accounting year was before 2022, so we can infer that their sales in 2022 were zero, or 3) we know that the firm closed down, hence we can also infer that their sales in 2022 were zero. In Column (1) and (6) the outcome is an indicator variable for whether any sales information was reported (i.e. it was not missing). In Column (2) and (7) the outcome is an indicator variable for whether the firm reported positive sales, conditional on submitting any sales information. In Column (3) and (8) the outcome variable is the logarithmic transformation of the amount of sales reported. In Column (4) and (9) we show the amount of sales reported, winsorized at the top and bottom 1 percentile. Column (5) and (10) use the same outcome variable, but impute 0's for firms not reporting sales. Recall that we only include firms where we can confidently infer that their sales were zero in 2022. Standard errors are robust to heteroskedasticity and clustered at the firm level. Event study-specifications are shown in Figure 11.

A Robustness

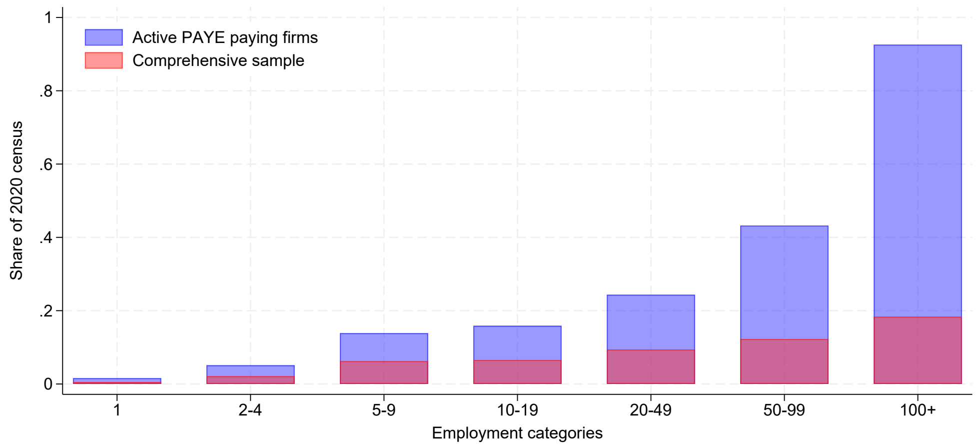
A.1 Figures

Figure A1
Time-period covered by each data source



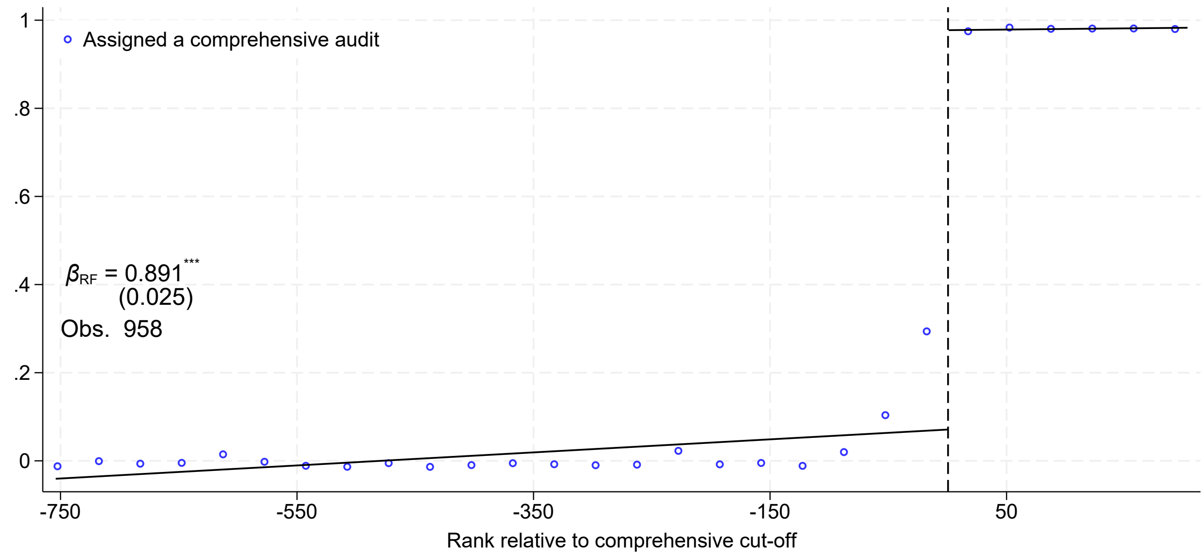
Notes: This figure displays the time period covered of each of the data sources. The CIT data is filed annually, and is due 6 months after the close of a firms' accounting year. 79% end their accounting year in June, 6% in December and the remainder are scattered across the year. The dashed green line illustrates the time period where the last set of firms are filing the return for the previous financial year. Given the delay in filing by firms, the last unaffected CIT return before the audit intervention is the CIT return for financial year 2019. VAT returns are filed monthly, but we use VAT information that coincides with the coverage of the CIT return. The census of business establishments was conducted in financial year 2020/21. The survey data was collected between January and July 2024.

Figure A2
Share of tax filing firms in each employment category



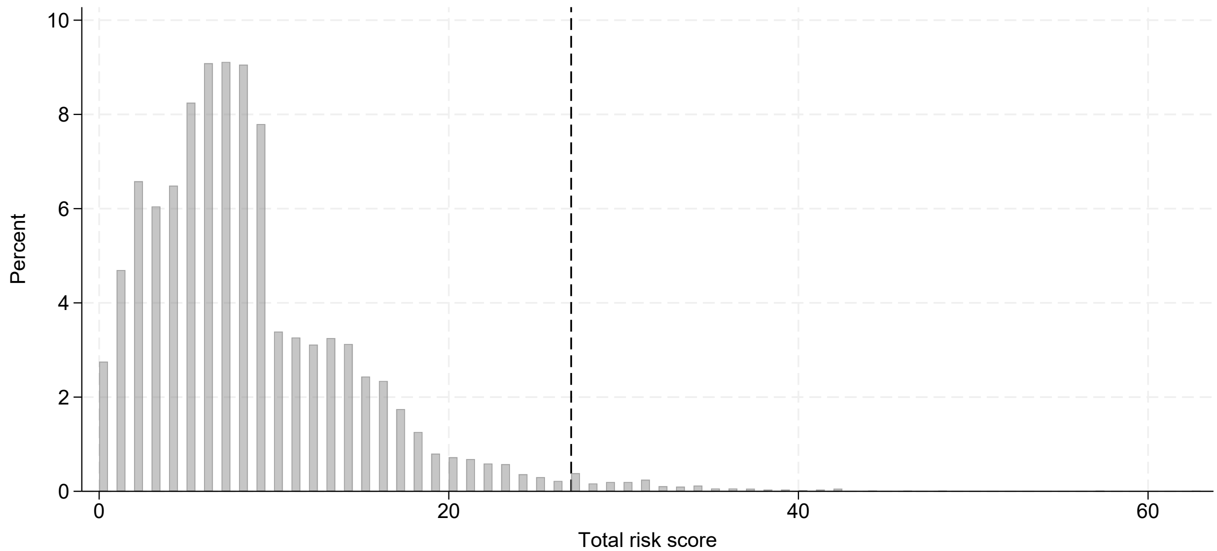
Notes: This figure presents the share of firms filing taxes and the share of firms in the comprehensive sample in each employment category. The employment categories are defined by the census, and we construct equivalent categories using information on number of employees submitted in the PAYE tax return. We restrict to firms that have filed at least one VAT or CIT before the audit year. We take a conservative approach and define the number of employees as the minimum number of employees reported in a PAYE return over the financial year 2020/21, which is the year the census was conducted. Thus, if anything, we are under-counting the number of tax-filing firms in the larger employment categories.

Figure A3
The effect of a firms' relative rank on the probability of being assigned a comprehensive audit



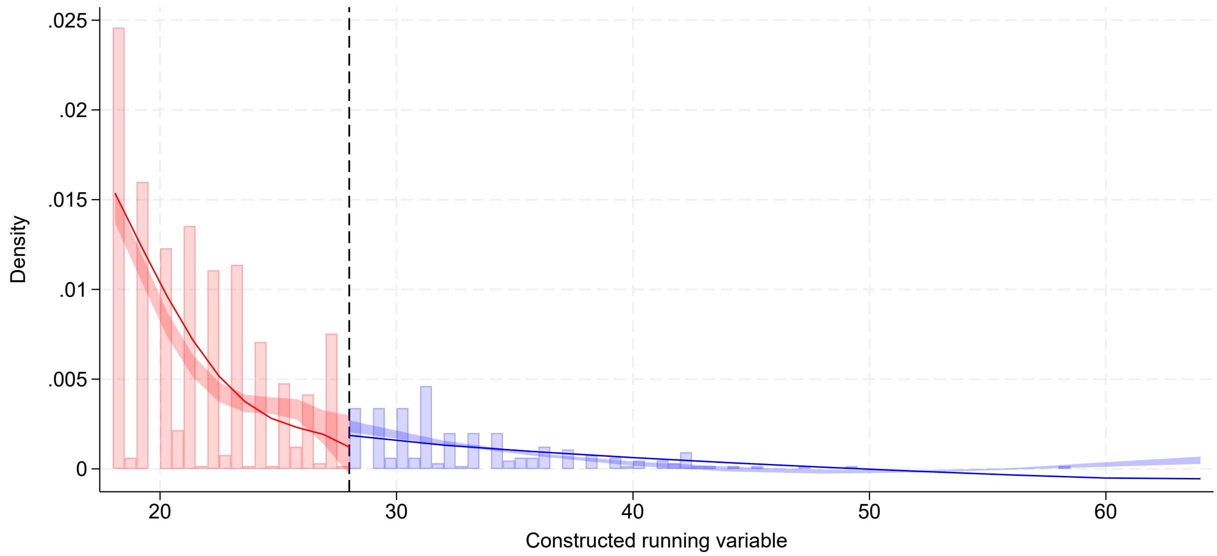
Notes: This figure present results on the probability that a comprehensive audit was assigned. The outcome variable shown in the figure is residualized. The plots are manually constructed, each point on the graph contains roughly 35 firms. The black lines show the linear best fit for the residualized data. The black dashed line indicates the threshold. Bandwidths are the optimal bandwidths calculated using the "rdrobust" package by Calonico et al. (2014). β_{RF} shows the reduced form effects – running equation 2. Heteroskedastic robust standard errors are shown in parentheses under the coefficients. "Obs." refers to the number of firms used in the regression. Source: URA administrative tax data. Authors' calculations.

Figure A4
Density distribution of total risk score



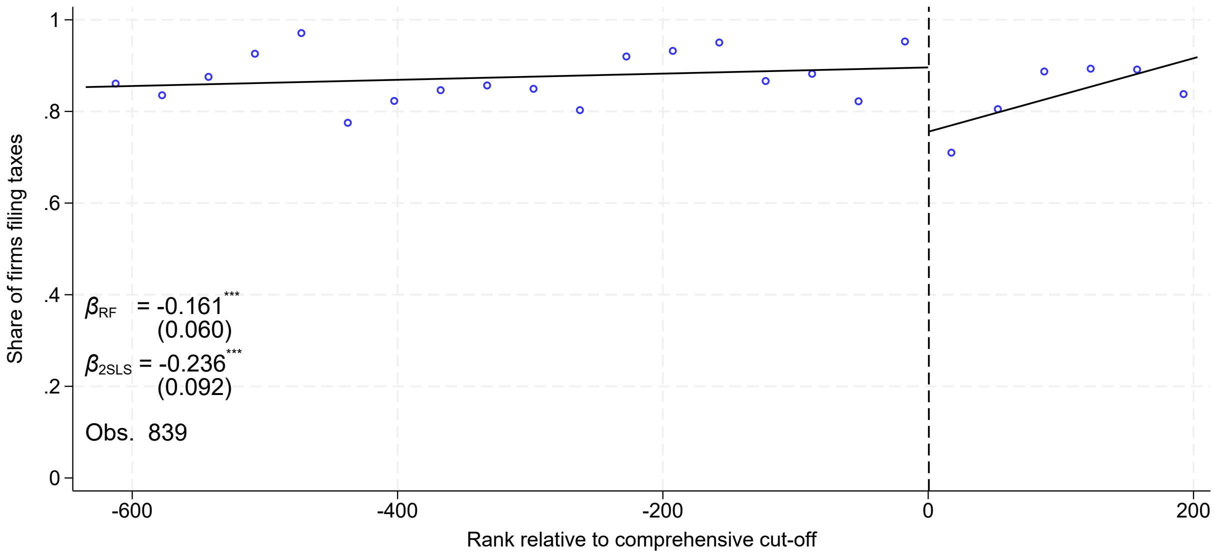
Notes: This figure presents the percent of firms at each discrete risk score in the comprehensive sample. Source: URA administrative tax data. Authors' calculations.

Figure A5
Density plot for alternative running variable



Notes: This figure presents the density plot for the alternative running variable discussed in section 5. The figure is produced using the "rddensity" package developed by Cattaneo et al. (2018); Cattaneo et al. (2020). Due to the left-skewed risk score distribution, shown in Figure A4 we restrict the rddensity command to run within the optimal bandwidth for one of our outcomes of interests; filing a CIT return in the two years following the audit. Source: URA administrative tax data. Authors' calculations.

Figure A6
The effect of comprehensive audits on the probability of filing a CIT or VAT return one year post-audit

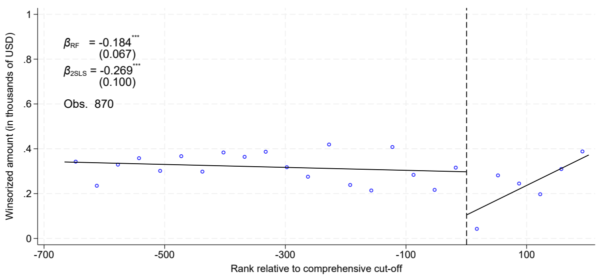


Notes: This figure present results on the probability of filing a CIT or VAT return in the year after the audit. The outcome variable shown in the figure is residualized. The plots are manually constructed, each point on the graph contains roughly 35 firms. The black lines show the linear best fit for the residualized data. The black dashed line indicates the threshold. Bandwidths are the optimal bandwidths calculated using the "rdrobust" package by Calonico et al. (2014). β_{RF} shows the reduced form effects – running equation 2 – and β_{2SLS} shows the 2SLS effect – running equation 1 and 3. Heteroskedastic robust standard errors are shown in parentheses under the coefficients. "Obs." refers to the number of firms used in the regression. Source: URA administrative tax data. Authors' calculations.

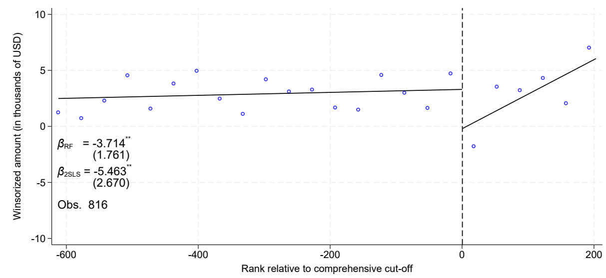
Figure A7

The effect of comprehensive audits on CIT liability in the year after the audit

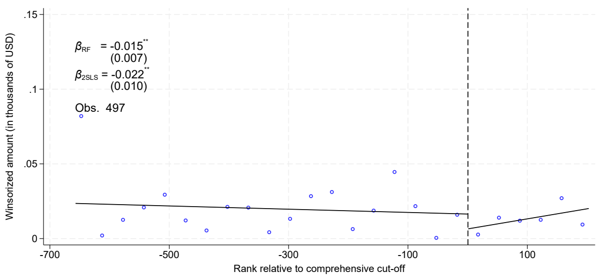
(a) Probability of reporting positive CIT liability



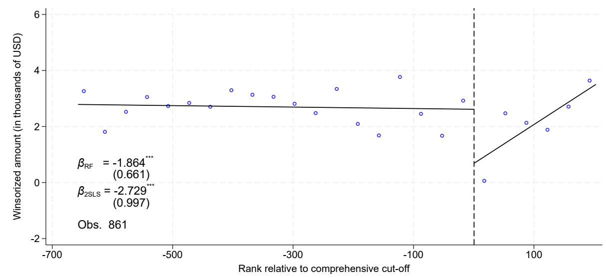
(b) Winsorized amount of CIT liability



(c) Scaled CIT liability



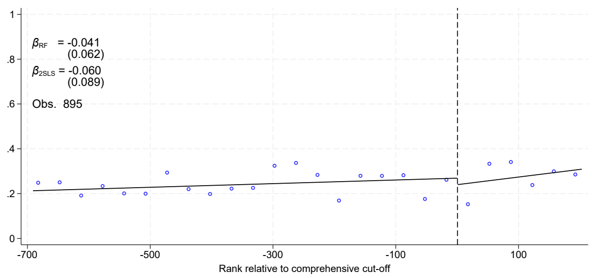
(d) IHS amount of CIT liability



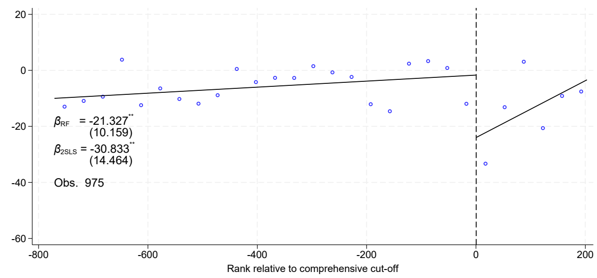
Notes: This figure presents the regression discontinuity graphs for each of the outcomes shown in Table 2. In Panel (a) we present an indicator for the probability of filing a CIT return with a positive CIT liability. In Panel (b) we present the amount of CIT liability submitted in thousands of USD winsorized at the top and bottom 1%. In Panel (c) we present the amount of CIT liability submitted scaled by baseline sales. In Panel (d) we present the inverse hyperbolic sine transformation of the CIT liability. The outcome variables shown in the figures are residualized. The plots are manually constructed, each point on the graph contains roughly 35 firms. The black lines show the linear best fit for the residualized data. The black dashed line indicates the threshold. Bandwidths are the optimal bandwidths calculated using the "rdrobust" package by Calonico et al. (2014). β_{RF} shows the reduced form effects – running equation 2 – and β_{2SLS} shows the 2SLS effect – running equation 1 and 3. Heteroskedastic robust standard errors are shown in parentheses under the coefficients. "Obs." refers to the number of firms used in the regression. Source: URA administrative tax data. Authors' calculations.

Figure A8
The effect of comprehensive audits on VAT liability in the first five months after the audit

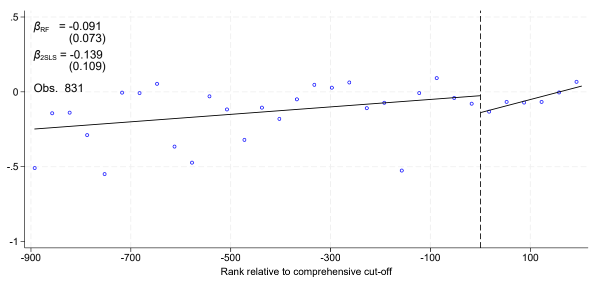
(a) Probability of reporting positive VAT liability



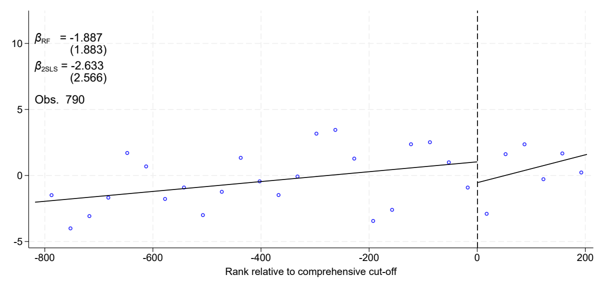
(b) Winsorized amount of VAT liability



(c) Scaled VAT liability



(d) IHS amount of VAT liability

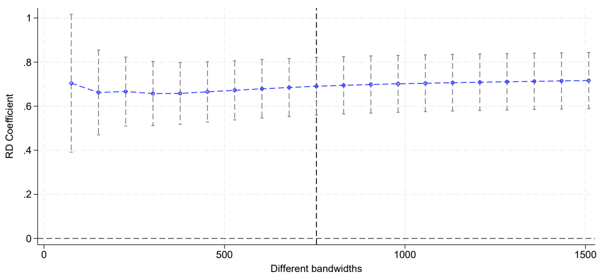


Notes: This figure presents the regression discontinuity graphs for each of the outcomes shown in Table 3. In Panel (a) we present an indicator for the probability of filing a VAT return with a positive VAT liability. In Panel (b) we present the total amount of VAT liability submitted in thousands of USD winsorized at the top and bottom 1%. In Panel (c) we present the amount of VAT liability submitted scaled by baseline sales. In Panel (d) we present the inverse hyperbolic sine transformation of the VAT liability. The outcome variables shown in the figures are residualized. The plots are manually constructed, each point on the graph contains roughly 35 firms. The black lines show the linear best fit for the residualized data. The black dashed line indicates the threshold. Bandwidths are the optimal bandwidths calculated using the "rdrobust" package by Calonico et al. (2014). β_{RF} shows the reduced form effects – running equation 2 – and β_{2SLS} shows the 2SLS effect – running equation 1 and 3. Heteroskedastic robust standard errors are shown in parentheses under the coefficients. "Obs." refers to the number of firms used in the regression. Source: URA administrative tax data. Authors' calculations.

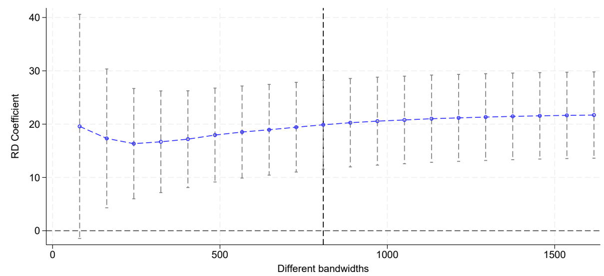
Figure A9

The effect of comprehensive audits on key outcomes using alternative bandwidths

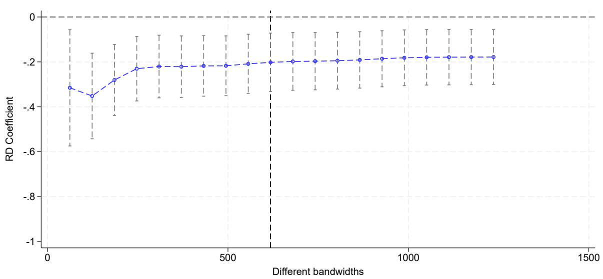
(a) Likelihood of receiving a comprehensive audit



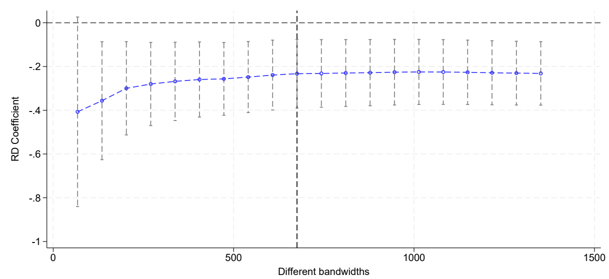
(b) Increase in tax liability from tax corrections



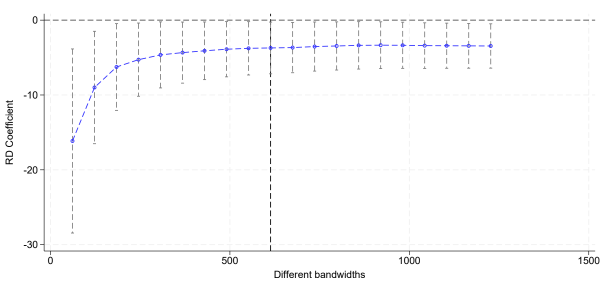
(c) Filing CIT tax one year post-audit



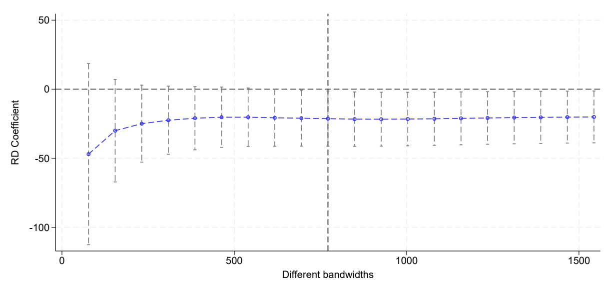
(d) Filing CIT tax two years post-audit



(e) CIT liability one year post-audit



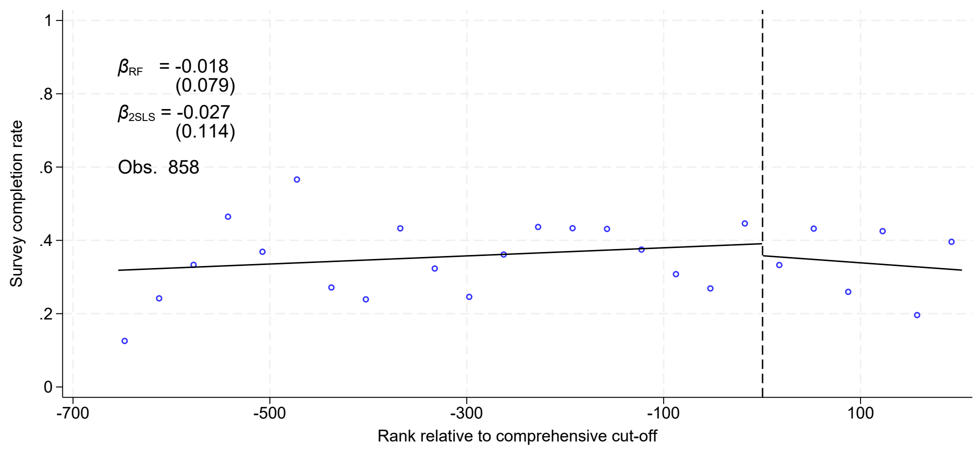
(f) Total VAT liability during 5 months post-audit



Notes: This figure presents the results from running equation 2 for some of our key outcomes of interest with different bandwidths. In Panel (a) the outcome is the probability of receiving a comprehensive audit, in Panel (b) the outcome is the increase in tax liability from tax corrections in thousands of USD, winsorized at the top and bottom 1%, in Panel (c) the outcome is the probability of filing a CIT return in the year after the audit, whereas in Panel (d) the outcome is the probability of filing a CIT return two years after the audit. In Panel (e) the outcome is the amount of CIT liability in thousands of USD, winsorized at the top and bottom 1%, and in Panel (f) the outcome is the total amount of VAT liability submitted during the first five months of the year after the audit, winsorized at the top and bottom 1%. The vertical dashed line represent the optimal bandwidth selected by Calonico et al. (2014). 95% confidence intervals are presented in the figure.

Figure A10

The effect of comprehensive audits on the probability of completing the survey

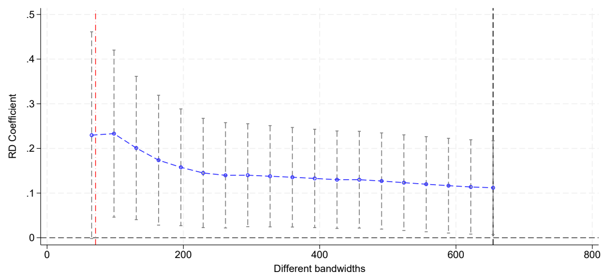


Notes: This figure present results on the probability of completing a survey. The outcome variable shown in the figure is residualized. The plots are manually constructed, each point on the graph contains roughly 35 firms. The black lines show the linear best fit for the residualized data. The black dashed line indicates the threshold. Bandwidths are the optimal bandwidths calculated using the "rdrobust" package by Calonico et al. (2014). β_{RF} shows the reduced form effects – running equation 2 – and β_{2SLS} shows the 2SLS effect – running equation 1 and 3. Heteroskedastic robust standard errors are shown in parentheses under the coefficients. "Obs." refers to the number of firms used in the regression. Source: URA administrative tax data and authors' survey. Authors' calculations.

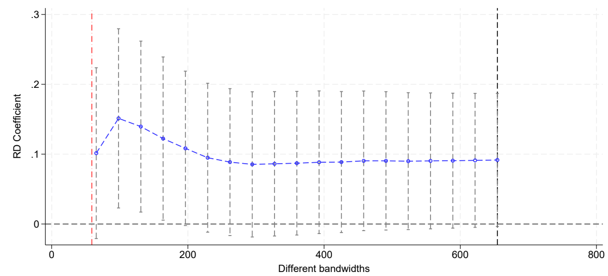
Figure A11

The effect of comprehensive audits on the probability of being closed or informal using alternative bandwidths

(a) Probability of being closed



(b) Probability of being informal



Notes: This figure presents the results from running equation 2 for whether a firm closed (Panel (a)) or informal (Panel (b)) with different bandwidths. The vertical dashed black line represent the optimal bandwidth selected by Calonico et al. (2014) for the outcome variable whether a firm filed a CIT return in the two years after the audit. This is because we tracked down all the firms within the bandwidth for that variable (our closest measure to firm exit in the administrative tax data). The dashed and dotted red line is the optimal bandwidth selected by Calonico et al. (2014) for the outcome of interest when the package is restricted to look within the sample we tracked. 95% confidence intervals are presented in the figure.

A.2 Tables

Table A1
Changes in the number of firms for each restriction imposed on the sample

	N	Comprehensive	Issue	Desk	Nothing
All firms with at least one risk	29,082	276	1,411	16,058	11,337
Removing specialized stations and mining sector	28,205	243	1,301	15,459	11,202
Removing taxpayers audited previous year	27,579	233	1,231	15,053	11,062
Ever filed a CIT or VAT return before audit period	23,592	231	1,217	14,528	7,616
Removing stations with no comprehensive audits	20,838	231	1,041	12,783	6,783
Restrict to firms assigned to comprehensive and desk audits	13,014	231		12,783	

Notes: This table presents the number of firms that remain after each sample restriction is imposed. In Column 1 we present the total number of firms in the sample, whereas in Column 2 - 4 we show how many firms were assigned to each of the enforcement interventions. Specialized stations include the Large Taxpayer Office, Oil and Gas office and the Public Sector Office. The mining sector is excluded because 40% of firms in the mining sector are registered with the Oil and Gas office.

Table A2
Baseline covariate balance test around the comprehensive audit selection threshold

	Comprehensive			
	(1)	(2)	(3)	(4)
	Mean	Coef	p-value	N
Years since tin registration	7.52	-0.73	0.15	813
Share of VAT returns filed	0.76	0.04	0.24	871
IHS Sales	11.91	-0.09	0.84	814
IHS Purchases	10.98	0.45	0.39	776
Prob of positive VAT Payable	0.54	0.00	0.94	820
IHS VAT Payable	4.64	-0.79	0.59	809
Prob of positive VAT Due	0.43	0.02	0.74	827
IHS VAT Due	0.56	1.03	0.56	815
Number of suppliers	2.49	-1.11	0.20	713
Number of buyers	1.48	0.11	0.69	833
Prob of filing CIT	0.75	0.04	0.49	886
IHS Turnover	9.84	-1.00	0.32	731
IHS Cost of sales	8.29	0.13	0.90	715
Prob of positive pre-tax profits	0.39	-0.13	0.13	716
IHS Pre-tax profits	-0.34	-2.24	0.22	720
Prob of positive tax liability	0.37	-0.04	0.61	660
IHS Tax liability	3.26	-0.56	0.51	624
Prob of filing PAYE	0.66	-0.07	0.32	817
Number of employees	24.46	-7.27	0.65	671
Agriculture	0.03	-0.01	0.73	954
Construction	0.13	-0.01	0.82	938
Manufacturing	0.09	0.05	0.42	963
Missing sector	0.01	0.03	0.36	831
Retail trade	0.10	-0.03	0.49	900
Services	0.48	-0.03	0.75	901
Wholesale trade	0.16	0.01	0.86	913
				999
P-value of joint f-test (min. bandwidth)			0.15	324
P-value of joint f-test (med. bandwidth)			0.44	360
P-value of joint f-test (max. bandwidth)			0.53	396
P-value of joint f-test (min. bandwidth) imputing 0			0.55	813
P-value of joint f-test (med. bandwidth) imputing 0			0.60	936
P-value of joint f-test (max. bandwidth) imputing 0			0.64	1023

Notes: In this table we present results from conducting balance tests on a wide range of covariates measured at baseline. We run equation 2 with the optimal bandwidth selected using the "rdrobust" package by Calonico et al. (2014). In the last 6 rows we conduct a test for whether the baseline covariates are jointly different from 0. We run the specification with 3 different bandwidths, the minimum, median and maximum bandwidth selected across all the baseline covariates and separately for when we impute 0's for missing values and when we do not. All variables are based on information submitted in the returns for financial year 2019/20. Variables preceded by "IHS" are the inverse hyperbolic sine transformation of the variable in question. For the *VAT data* variables represent monthly averages over the 12 months from July 2019 to June 2020. There are two exceptions to this. Variables are conditional on filing at least one VAT return. For the *CIT data* variables show what is declared in the CIT return for financial year 2019/20. Variables are conditional on filing a CIT return. For the *PAYE data* "Number of employees" is the average number of employees reported conditional on filing at least one PAYE return. Sectors are aggregate versions of the sector classification used by the URA. Source: VAT, CIT, PAYE return from the URA. Authors' calculations.

Table A3

Effect of comprehensive tax audits on CIT liability for year of the audit

	Probability of	Amount of CIT liability		
	positive CIT	Winsorized	Scaled	IHS
	(1)	(2)	(3)	(4)
RD coefficient	−0.012	−0.375	−0.007	−0.069
	(0.083)	(2.751)	(0.009)	(0.827)
2SLS coefficient	−0.018	−0.553	−0.010	−0.102
	(0.120)	(3.973)	(0.013)	(1.189)
N	859	794	543	832
Mean in control	0.42	4.97	0.02	3.78
Bandwidth	656	591	735	629

Notes: This table presents reduced form and two stage least squares estimates for the amount of CIT liability in the year of the audit (but filed after). In Column (1) we present an indicator for the probability of filing a CIT return with a positive CIT liability. In Column (2) we present the amount of CIT liability submitted in thousands of USD winsorized at the top and bottom 1%. In Column (3) we present the amount of CIT liability submitted scaled by baseline sales. In Column (4) we present the inverse hyperbolic sine transformation of the CIT liability. The reduced form (RF) coefficient is estimated by running equation 2. The 2SLS coefficient is estimated by running equation 1 and 3. All regressions include station fixed effects and indicators for whether the firm was in the VAT or CIT sample at baseline. Standard errors are robust to heteroskedasticity.

Table A4
The effect of comprehensive audits on different margins of the CIT returns

	CIT liability	Probability of		CIT liability
	Winsorized	filing a CIT return	positive CIT liability conditional on filing	Log
	(1)	(2)	(3)	(4)
RD coefficient	-3.528^{**}	-0.232^{***}	-0.186^{*}	-0.153
	(1.671)	(0.079)	(0.107)	(0.507)
N	939	939	616	302
Mean in control	4.11	0.67	0.50	8.40
Bandwidth	736	736	736	736

Notes: This table presents reduced form results for different margins of firms changes in CIT filings. In Column (1) the outcome is the amount of CIT liability in thousands of USD, winsorized at the top and bottom 1%. In Column (2) the outcome is the probability of filing a CIT return in the second year post-audit. In Column (3) the outcome is the probability of filing a CIT return with a positive CIT liability, conditional on filing a CIT return. In Column (4) the outcome is the logarithmic transformation of the CIT liability submitted. To make the results comparable we fix the bandwidth to be the optimal bandwidth for the outcome in Column (4). The coefficients are estimated by running equation 2. All regressions include station fixed effects and indicators for whether the firm was in the VAT or CIT sample at baseline. Standard errors are robust to heteroskedasticity.

Table A5

First stage and the effect on filing returns with different set of controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Panel A: Probability of receiving a comprehensive audit						
RD coefficient	0.719***	0.686***	0.691***	0.682***	0.673***	0.667***	0.662***
	(0.067)	(0.067)	(0.067)	(0.069)	(0.068)	(0.070)	(0.066)
N	958	958	958	928	933	933	933
Bandwidth	755	755	755	755	760	760	760
	Panel B: Probability of filing a CIT return one year post-audit						
RD coefficient	−0.192***	−0.210***	−0.202***	−0.191***	−0.161**	−0.157**	−0.165***
	(0.067)	(0.065)	(0.066)	(0.066)	(0.064)	(0.066)	(0.061)
N	821	821	821	795	831	831	831
Bandwidth	618	618	618	618	656	656	656
	Panel C: Probability of filing a return two years post-audit						
RD coefficient	−0.207**	−0.225***	−0.233***	−0.233***	−0.219***	−0.279***	−0.227***
	(0.081)	(0.080)	(0.080)	(0.082)	(0.080)	(0.078)	(0.076)
N	879	879	879	850	881	881	881
Bandwidth	676	676	676	676	707	707	707
Station FE	No	Yes	Yes	Yes	Yes	Yes	No
Sample FE	No	No	Yes	Yes	Yes	Yes	No
Removing fictitious firms	No	No	No	Yes	No	No	No
Sector FE	No	No	No	No	Yes	Yes	No
Risk controls	No	No	No	No	No	Yes	No
Lasso selected controls	No	No	No	No	No	No	Yes

Notes: This table presents reduced form results on the probability of receiving a comprehensive audit (Panel A), the probability of filing a CIT return the year after the audit (Panel B), and the probability of filing a CIT return in the second year post-audit (Panel C) with different empirical specifications. In Column (1) we do not use any controls, in Column (2) we only include station fixed effects, in Column (3) we add controls for whether the firm was in the VAT or CIT sample at baseline (our main specification), in Column (4) we remove "fictitious" firms defined as being labeled a suspicious firms by the URA, in Column (5) we add sector fixed effects, in Column (6) we add risk controls. The risk controls are 70 separate controls which control for the risk score and expected potential revenue of each of the underlying 35 risk parameters. Finally, in Column (7) we run a lasso specification to select the controls. In the specification we do not penalize station fixed effects because – based on conversations with the URA – they are an important part of selection. We fix the bandwidth to be the optimal bandwidth selected when running the specification in Column (3). The coefficient are estimated by running equation 2, and varying $\mathbf{X}_f$. Standard errors are robust to heteroskedasticity.

Table A6
The effect of comprehensive audits on changes in tax liabilities with different set of controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Panel A: Increase in tax liability from tax corrections						
RD coefficient	22.854***	20.146***	19.881***	19.589***	19.404***	17.823***	19.210***
	(4.401)	(4.253)	(4.250)	(4.295)	(4.299)	(4.095)	(4.149)
N	1012	1012	1012	982	982	982	982
Bandwidth	809	809	809	809	809	809	809
	Panel B: CIT liability one year post-audit						
RD coefficient	−2.457	−3.278*	−3.714**	−3.668**	−3.747**	−4.259**	−3.510**
	(1.698)	(1.727)	(1.761)	(1.826)	(1.802)	(1.869)	(1.627)
N	816	816	816	790	790	790	790
Bandwidth	613	613	613	613	613	613	613
	Panel C: Total VAT liability during 5 months post-audit						
RD coefficient	−22.685**	−22.090**	−21.327**	−23.184**	−22.437**	−14.125	−22.371**
	(10.015)	(10.156)	(10.159)	(10.586)	(10.324)	(9.534)	(9.457)
N	975	975	975	945	945	945	945
Bandwidth	772	772	772	772	772	772	772
Station FE	No	Yes	Yes	Yes	Yes	Yes	No
Sample FE	No	No	Yes	Yes	Yes	Yes	No
Removing fictitious firms	No	No	No	Yes	No	No	No
Sector FE	No	No	No	No	Yes	Yes	No
Risk controls	No	No	No	No	No	Yes	No
Lasso selected controls	No	No	No	No	No	No	Yes

Notes: **Notes:** This table presents reduced form results on the increase in tax liability from tax corrections (Panel A), CIT liability in the year after the audit (Panel B), and VAT liability in the first five months post-audit (Panel C) with different empirical specifications. In Column (1) we do not use any controls, in Column (2) we only include station fixed effects, in Column (3) we add controls for whether the firm was in the VAT or CIT sample at baseline (our main specification), in Column (4) we remove "fictitious" firms defined as being labeled a suspicious firms by the URA, in Column (5) we add sector fixed effects, in Column (6) we add risk controls. The risk controls are 70 separate controls which control for the risk score and expected potential revenue of each of the underlying 35 risk parameters. Finally, in Column (7) we run a lasso specification to select the controls. In the specification we do not penalize station fixed effects because – based on conversations with the URA – they are an important part of selection. We fix the bandwidth to be the optimal bandwidth selected when running the specification in Column (3). The coefficient are estimated by running equation 2, and varying $\mathbf{X}_f$. Standard errors are robust to heteroskedasticity.

Table A7

Alternative threshold: the effect of comprehensive audits when using the threshold from the initial assignment of audits

	Probability			Winsorized amount		
	receiving comprehensive audit	filing CIT return 21/22	filing CIT return 22/23	Tax correction	CIT liability	VAT liability
RD coefficient	0.611^{***}	-0.168^{***}	-0.190^{**}	16.709^{***}	-2.744	-19.867^{**}
	(0.068)	(0.062)	(0.078)	(4.050)	(1.781)	(9.764)
N	849	850	924	958	895	1022
Mean in control	0.01	0.85	0.67	2.77	4.05	-7.27
Bandwidth	641	642	716	750	687	814

Notes: This table presents results of a robustness check where we use the alternative (assigned) threshold. The purpose is to see whether results differ when using the alternative threshold. That is, we run equation 2 using the alternative cut-off. All regressions include station fixed effects and indicators for whether the firm was in the VAT or CIT sample at baseline. Standard errors are robust to heteroskedasticity.

Table A8
Alternative sample: the effect of comprehensive audits when using all firms

	Probability			Winsorized amount		
	receiving comprehensive audit	filing CIT return 21/22	filing CIT return 22/23	Tax correction	CIT liability	VAT liability
RD coefficient	0.639^{***}	-0.162^{***}	-0.192^{***}	20.633^{***}	-3.798^{**}	-13.522
	(0.065)	(0.057)	(0.072)	(5.168)	(1.771)	(9.387)
N	1421	1220	1277	1419	1288	1341
Mean in control	0.01	0.85	0.67	5.33	4.56	-9.27
Bandwidth	1191	990	1047	1189	1058	1111

Notes: This table presents results using the full sample of all firms. The purpose is to determine whether our results hinge on us comparing firms assigned to desk audits against firms assigned to comprehensive audits. That is, we run equation 2 using firms assigned to any enforcement intervention (comprehensive, issue, desk) or to no enforcement intervention. All regressions include station fixed effects and indicators for whether the firm was in the VAT or CIT sample at baseline. Standard errors are robust to heteroskedasticity.

Table A9
Dimension reduction exercise: the effect of comprehensive audits when using predicted outcomes

	Probability			Winsorized amount		
	receiving comprehensive audit	filing CIT return 21/22	filing CIT return 22/23	Tax correction	CIT liability	VAT liability
RD coefficient	−0.002	−0.028	−0.011	−0.327	−0.432	−0.396
	(0.005)	(0.029)	(0.026)	(0.402)	(0.805)	(1.745)
N	958	821	879	1012	816	975
Mean in control	0.01	0.86	0.67	2.97	3.98	-7.29
Bandwidth	755	618	676	809	613	772

Notes: This table presents results from our dimension reduction exercise. We predict each of our main outcomes using all baseline covariates and run an RD on the predicted outcome. The purpose is to see whether difference in baseline covariates can predict the treatment effects we find. That is, we run equation 2 on the predicted outcome. All regressions include station fixed effects and indicators for whether the firm was in the VAT or CIT sample at baseline. Standard errors are robust to heteroskedasticity.

Table A10
Baseline characteristics of the compliers

	All	Compliers			
		Audited		Not filing CIT	
	Mean	Mean	P-value	Mean	P-value
	(1)	(2)	(3)	(4)	(5)
Years since tin registration	7.68	7.63	0.93	6.25	0.19
Share of VAT returns filed	0.75	0.87	0.01	0.76	0.94
IHS Sales	12.02	12.94	0.07	11.38	0.42
IHS Purchases	11.19	12.98	0.00	10.11	0.40
Prob of positive VAT Payable	0.54	0.53	0.84	0.51	0.80
IHS VAT Payable	4.77	4.10	0.67	4.18	0.86
Prob of positive VAT Due	0.44	0.44	1.00	0.58	0.34
IHS VAT Due	0.47	0.52	0.98	5.47	0.25
VAT Due	-8317.90	-3466.82	0.67	11678.00	0.11
Number of suppliers	2.39	2.56	0.84	0.66	0.02
Number of buyers	1.59	2.52	0.00	0.64	0.01
Prob of filing CIT	0.77	0.80	0.64	1.03	0.00
IHS Turnover	9.87	10.22	0.75	5.40	0.04
IHS Cost of sales	8.48	10.47	0.07	5.03	0.09
Prob of positive pre-tax profits	0.39	0.25	0.10	0.16	0.09
IHS Pre-tax profits	-0.39	-3.89	0.06	-4.20	0.15
Prob of positive tax liability	0.36	0.33	0.69	0.26	0.51
IHS Tax liability	3.21	2.91	0.74	2.14	0.39
Tax liability	2437.62	3404.21	0.59	1288.92	0.42
IHS Value of plant/machines	3.55	2.15	0.20	0.66	0.01
Sales/Cost of sales	2.06	1.46	0.01	1.95	0.89
Agriculture	0.03	0.01	0.47	0.02	0.80
Construction	0.14	0.14	0.98	0.04	0.23
Manufacturing	0.07	0.24	0.03	-0.07	0.00
Missing sector	0.01	0.03	0.45	0.11	0.40
Retail trade	0.10	0.12	0.63	0.13	0.75
Services	0.46	0.35	0.20	0.55	0.63
Wholesale trade	0.17	0.15	0.76	0.25	0.58

Notes: All variables are based on information submitted in the returns for financial year 2019/20. Column (1) presents average baseline characteristics for the sample within the optimal bandwidth of the variable in question. In Column (2) we present the baseline characteristics for the firms that get induced into comprehensive audits (the comprehensive audit complier average). In Column (4) we present baseline characteristics for firms that get induced into not filing CIT taxes after the audit (the non-filing compliers mean). Column (5) and (6) present the p-value of a difference in means between the averages of the compliers and the sample. Variables preceded by "IHS" are the inverse hyperbolic sine transformation of the variable in question. We only impute 0's for "VAT Due" and "Tax liability". For the *VAT data* variables represent monthly averages over the 12 months from July 2019 to June 2020. For the *CIT data* variables show what is declared in the CIT return for financial year 2019/20. Sectors are aggregate versions of the sector classification used by the URA. Source: VAT, CIT, PAYE return from the URA. Authors' calculations.

Table A11
Filing rates across tracking categories

	File vat or cit	File cit 22/23
Closed	0.72	0.38
Vanished	0.64	0.38
Exist	0.95	0.72
NGO	0.94	0.72

Notes: This table presents the share of firms filing a vat or cit tax return in the immediate aftermath of the audit (Column (1)) and filing a CIT tax return in the second year post-audit (Column (2)) across each of the tracking categories.

Table A12
The effect of comprehensive audits on being closed or informal with different set of controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Panel A: Probability of being closed						
RD coefficient	0.108**	0.115**	0.112**	0.097*	0.098*	0.083*	0.112**
	(0.052)	(0.053)	(0.054)	(0.052)	(0.051)	(0.050)	(0.052)
N	858	858	858	830	858	858	858
Bandwidth	655	655	655	655	655	655	655
	Panel B: Probability of being informal						
RD coefficient	0.083*	0.096**	0.092*	0.098**	0.086*	0.095*	0.092*
	(0.049)	(0.048)	(0.049)	(0.049)	(0.049)	(0.052)	(0.048)
N	858	858	858	830	858	858	858
Bandwidth	655	655	655	655	655	655	655
Station FE	No	Yes	Yes	Yes	Yes	Yes	No
Sample FE	No	No	Yes	Yes	Yes	Yes	No
Fictitious FE	No	No	No	Yes	No	No	No
Sector FE	No	No	No	No	Yes	Yes	No
Risk controls	No	No	No	No	No	Yes	No
Lasso selected controls	No	No	No	No	No	No	Yes

Notes: This table presents reduced form results on the probability of being closed (Panel A), and informal (Panel B) with different empirical specifications. In Column (1) we do not use any controls, in Column (2) we only include station fixed effects, in Column (3) we add controls for whether the firm was in the VAT or CIT sample at baseline (our main specification), in Column (4) we remove "fictitious" firms defined as being labeled a suspicious firms by the URA, in Column (5) we add sector fixed effects, in Column (6) we add risk controls. The risk controls are 70 separate controls which control for the risk score and expected potential revenue of each of the underlying 35 risk parameters. Finally, in Column (7) we run a lasso specification to select the controls. In the specification we do not penalize station fixed effects because – based on conversations with the URA – they are an important part of selection. We fix the bandwidth to be the optimal bandwidth selected when running the specification in Column (3). The coefficient are estimated by running equation 2, and varying $\mathbf{X}_f$. Standard errors are robust to heteroskedasticity.

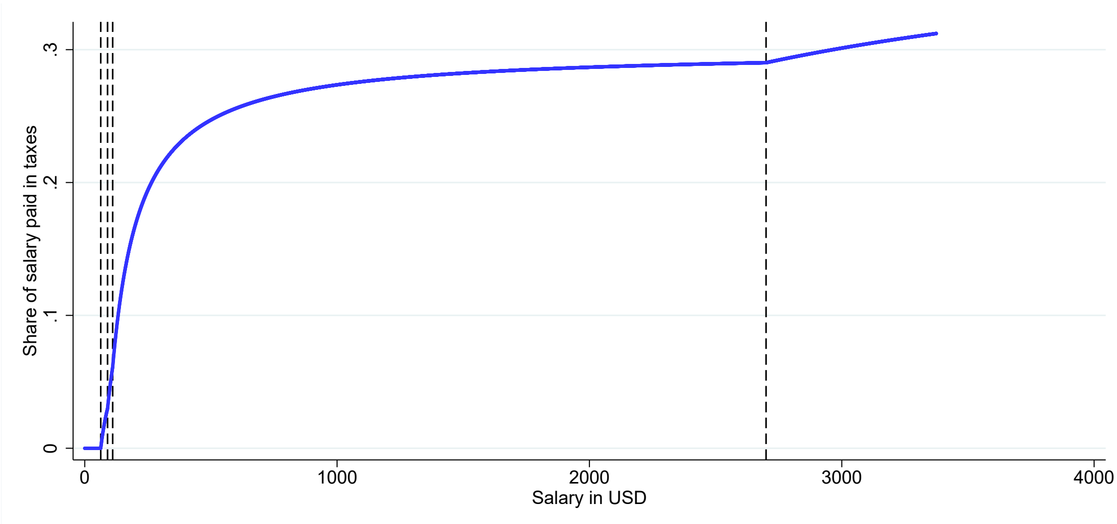
Table A13
The effect of comprehensive audits on sales and different measures of costs

	Sales			Cost		
	Probability	Amount		Amount		Number of
	Positive	Log	Winsorized	Log	Winsorized	Employees
	(1)	(2)	(3)	(4)	(5)	(6)
RD coefficient	−0.082	−0.687	−238.853	−0.684	−121.005	−5.776
	(0.119)	(0.650)	(376.564)	(0.645)	(254.942)	(20.709)
2SLS coefficient	−0.104	−0.828	−303.348	−0.876	−157.610	−7.316
	(0.145)	(0.793)	(459.692)	(0.847)	(319.344)	(25.136)
N	445	236	393	215	370	443
Mean in control	0.65	5.42	523.05	5.13	422.37	25.97
Bandwidth	636	636	636	636	636	636

Notes: This table presents reduced form and two stage least squares estimates for sales, costs and number of employees reported by firms. In Column (1) we present an indicator for whether the firm reported having positive sales. In Column (2) and (3) we present the amount of sales, winsorized at the top and bottom 1st percentile and the logarithmic transformation. In Column (4) and (5) we present the same transformations, but for total costs. Finally, in Column (6) we present results on the number of employees. Throughout we fix the bandwidth to be the optimal bandwidth for the probability of filing a CIT tax return in the two years after the audit. The reduced form (RF) coefficient is estimated by running equation 2. The 2SLS coefficient is estimated by running equation 1 and 3. All regressions include station fixed effects and indicators for whether the firm was in the VAT or CIT sample at baseline. Standard errors are robust to heteroskedasticity.

B Context

Figure B1
PAYE Tax schedule



Notes: This figure shows how the tax rate for PAYE evolves as the income of a person increases.

Figure B2
Desk audit notification letter

RE: TAX COMPLIANCE ADVISORY FOR THE PERIOD ENDED DECEMBER 31, 2020.

We appreciate your continued support and contribution to revenue mobilization for the development of Uganda.

We have reviewed ABC Limited's tax returns for the period ended December 31, 2020 and noted the following tax compliance issues in the company's declarations that require your immediate attention.

1. **Risk 1.**
2. **Risk 2.**

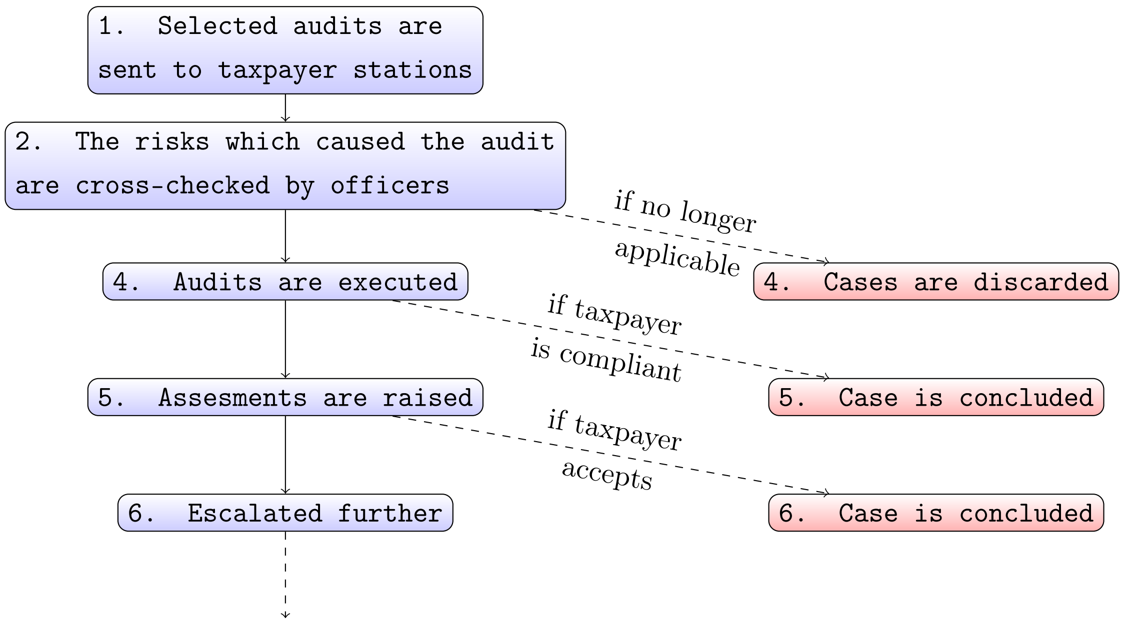
You are therefore advised to amend the company's tax returns to rectify the above issues and pay resultant tax liability.

Provide your response to the above issues by **October 20, 2022.** Note that failure to respond to this communication within the stated timeline may lead to raising of additional assessments and penalties thereof as **per Sec 23(2) and Sec 50 of the Tax Procedures Code Act**, **2014** respectively without further reference to you.

For further guidance and clarification, please do not hesitate to contact the undersigned on Tel; **0772-xxx-xxx** or email xxx@ura.go.ug

We thank you and look forward to your continued cooperation as We Develop Uganda together.

Figure B3
Graphic illustration of the audit process at the URA



Notes: This figure graphically presents the audit process after firms were selected for an audit.

C Math

In this section we drive the equation 11 and 12 used in the aggregation exercise in section 8.

Assume output y_i is produced using only labor with a constant returns to scale production function $f(l_i)$

$$y_i = A_i f(l_i)$$

Where A_i is the productivity of firm i. Profits of the firm are then given by

$$\pi_i = p_i A_i f(l_i) - w l_i$$

For a small change – prices and wages do not adjust – the total derivative of profits with respect to labor is given by

$$d\pi_i = p_i \left[dA_i f(l_i) + A_i \frac{\partial f}{\partial l_i} dl_i \right] - w dl_i$$

Changes in labor are unlikely to affect the productivity of the firm, hence we set $dA_i = 0$, and get

$$\begin{aligned} d\pi_i &= \left[p_i A_i \frac{\partial f}{\partial l_i} - w \right] dl_i \\ &= \left[p_i A_i \frac{\partial f}{\partial l_i} l_i - w l_i \right] \frac{dl_i}{l_i} \\ &= [s_i - c_i] \frac{dl_i}{l_i} \end{aligned}$$

Where the second equality follows by multiplying and dividing by l_i and the third equality follows by definition under constant returns to scale. Finally, we aggregate across all firms and consider discrete changes Δ instead infinitesimal changes d. As such, the first order approximation is given by

$$\Delta Y_g = \sum_{i \in g} \Delta \pi_i \approx \sum_{i \in g} [s_i - c_i] \times \% \Delta l_i \tag{13}$$

This holds for a constant returns to scale production function with one input. It also holds separately for each input, under multiple inputs. To hold for changes in labor if the firm uses multiple inputs we need to impose the additional assumption that inputs are perfect complements.

The derivation of equation 12 follows by substituting in $\% \Delta l_i = (1 - p_e) \% \Delta l_i + p_e \% \Delta l_i$

$$\Delta Y_g = \sum_{i \in g} \Delta \pi_i \approx \sum_{i \in g} [s_i - c_i] \times [(1 - p_e) \% \Delta l_i - p_e]$$

$$\approx \sum_{i \in g} [s_i - c_i] \times (1 - p_e)\%\Delta l_i - \sum_{i \in g} [s_i - c_i]\, p_e$$

$$\approx \sum_{i \in g | exist} [s_i - c_i] \times \Delta \log l_i - \sum_{i \in g} [s_i - c_i]\, p_e$$