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Citation

Spillover Effects of Clients' Tax Enforcement on Financial Statement Auditors: Evidence from a Discontinuity Design[1]

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Abstract: We examine the impact of clients' tax enforcement on financial statement auditors. In a regression discontinuity design, we exploit the firm-registration-date-based application of a new rule that assigns firms to two different tax enforcement regimes. Our analysis implies that auditors exert less effort—evident in lower audit fees and shorter audit report lags—when their clients are monitored by the more stringent tax authority. In results supporting that audit quality improves in this situation despite the fall in auditor effort, we report that clients subject to tougher tax enforcement exhibit a lower incidence of accounting restatements and tax-related restatements. Additionally, we find no evidence of impaired auditor independence evident in the informativeness of auditors' modified opinions. Finally, we document that clients undergoing stricter tax enforcement are assigned less-experienced partners, suggesting that tax enforcement enables audit firms to optimize client-partner matching. Collectively, our research suggests that tax authority oversight engenders a positive externality by improving external audit efficiency.

Keywords: tax enforcement, audit fees, audit effort, auditor-client matching.

JEL codes: H25; H26; M42.

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1. Introduction

We examine the importance of corporate tax enforcement to the behavior of financial statement auditors. Research on tax enforcement has long been focused on its impacts on tax compliance and revenue collection (Slemrod 2019). More recently, the effects that tax enforcement may have on taxpayers' behaviors *other than tax compliance* have attracted considerable interest in research and policy circles (e.g., Guedhami and Pittman 2008; Hanlon et al. 2014; Gallemore and Jacob 2019). In this study, we consider the audit market, which is often neglected in the formation of economic and tax policies, to be a potentially important factor in gauging the benefits and costs of tax enforcement (Slemrod and Blumenthal 1996; Nessa et al. 2019).[3] Although the purpose of tax enforcement is not to improve accounting transparency, tax authority monitoring of firms' tax returns may have spillover benefits to the financial reporting numbers given the considerable overlap between the book and tax systems. Indeed, corporate financial statements are the starting point in reconciling book income to taxable income. Despite the close relationship between the two systems, prior research does not explicitly evaluate the role that monitoring by tax authorities plays. From a policy perspective, shedding light on the impact of tax enforcement on external auditors may enhance our understanding of the net benefits of tax enforcement.

The link between tax enforcement and auditor behavior remains unclear *ex ante*. On one hand, clients' tax positions are naturally subject to closer scrutiny when tax enforcement is stricter, increasing the likelihood that the tax authority will challenge a client's tax position. From the external auditor's standpoint, this higher likelihood engenders a higher risk of

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[3] In evaluating tax enforcement activities, policy makers weigh the benefits of increased tax revenue against the costs, which typically include the direct expenditures on tax authority personnel and the tax compliance cost borne by taxpayers (Slemrod and Blumenthal 1996; Hoopes et al. 2012; Nessa et al. 2019). We broaden this focus by analyzing whether strict tax authority monitoring engenders a positive externality in the form of improving external audit efficiency.

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financial restatement (Hanlon et al. 2012; Donohoe and Knechel 2014).[4] For example, in its 2016 tax audit of Tibet Huayu Mining Company, China's State Administration of Taxation (SAT) detected that the firm under-reported its financial income by almost 40 million Chinese Yuan (equivalent to US$6 million). This reinforces that strong tax enforcement exposes the auditor to higher risk of audit deficiency and reputation damage.[5] In addition to tax positions, stricter tax enforcement will also induce auditors to focus intently on constraining upward earnings management since large book-tax gaps can attract tax authority scrutiny (Mills 1998).[6] Consequently, the auditor will respond by charging a higher fee given that she must spend more time auditing the client's tax positions as well as its financial reporting. Moreover, it is important to stress that the higher fee may also reflect a risk premium since the additional audit risk arising from stricter tax enforcement cannot be completely mitigated by working harder on the engagement. In fact, income tax accounts are frequently considered the most complicated to audit (Badertscher et al. 2009; Graham et al. 2012). Nonetheless, despite auditors expending more effort, income tax-related issues still account for 12.8% of all restatements, ranking as the third most commonly listed reason for restatements according to Audit Analytics (2016). It follows that audit fees are higher when clients are monitored by a tougher tax authority.

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[4] In the U.S., tax-related issues are among the most frequently raised issues in Public Company Accounting Oversight Board (PCAOB) inspections (Drake et al. 2016; Acito et al. 2018).

[5] Reflecting audit failure, restatements are costly to both firms and their auditors (e.g., Cao et al. 2012; Mande and Son 2013; Hennes et al. 2014; Irani et al. 2015; Christensen et al. 2016; Aobdia 2019).

[6] Supporting both tax advisors' (Cloyd 1995) and corporate managers' (Cloyd et al. 1996) perceptions that tax audit costs fall with book-tax conformity, prior research implies that narrowing the difference between their financial reporting and taxable incomes enables firms to deflect the attention of tax authorities (e.g., Mills 1998; Mills and Sansing 2000), including in China (e.g., Li et al. 2019). It is also important to stress that although some prior research implies that auditors are more concerned about preventing clients from managing their book earnings upward (Nelson et al., 2002; Lennox et al., 2014), other evidence suggests that they strive to constrain both forms (e.g., Myers et al., 2003; Francis and Yu, 2009), reinforcing that both external auditors and tax authorities are eager to curb attempts to manipulate earnings downward..

On the other hand, firms' tax aggressiveness and financial reporting risks may subside under stricter tax enforcement that deters firms from pursuing tax aggressive strategies (Hoopes et al. 2012; Hanlon et al. 2014). This, in turn, results in fewer expected tax-related restatements, alleviating auditor concerns over this type of audit failure and deficiency. Alternatively, holding their strategies constant, firms in this situation have stronger incentives to diligently prepare for potential tax audits by committing more resources to researching and documenting their tax positions. Auditors, anticipating the lower tax aggressiveness and financial reporting risk that accompanies closer monitoring by the tax authority, will be less concerned about the quality of the client's tax positions. This implies that auditors will exert less effort evident in charging lower fees when their clients experience stricter tax enforcement.

Moreover, corporate tax enforcement may be irrelevant to auditor behavior. Set against prior research implying that tax authority resources or tax audit certainty do not perceptibly impact the tax positions that firms undertake (Ayers et al. 2019; Nessa et al. 2019), auditors may conclude that differential tax enforcement intensity does not materially impact detection risk and their clients' behavior. Given the tension stemming from the competing incentives at work, it is difficult to form a directional prediction on the role that corporate tax enforcement plays in shaping audit fees.

Our empirical analysis takes advantage of a quasi-experimental setting afforded by an unanticipated tax collection arrangement. On January 16, 2002, the SAT, China's national tax authority, announced a change in the tax collection process such that firms registered after January 1, 2002 (hereafter, the cut-off date) would become subject to SAT monitoring, whereas firms registered before the cut-off date remain under the administration of the Local Taxation Bureau (LTB). This arrangement was retroactively applied to all firms registered on or after January 1, 2002. Generally, the SAT is perceived to enforce higher levels of tax compliance than

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the LTB because the SAT is under the direct administration of the central government, whereas the LTB falls under the management of a local or provincial government.[7] Consequently, this policy created a discontinuity in tax enforcement by the date of new firm registration. Given that the new tax collector arrangement was unanticipated and that the firm registration date was not subject to manipulation, this setting allows us to examine the impact of corporate tax enforcement severity on external audits using a regression discontinuity (RD) framework. We focus on a group of newly registered firms established one year before and after the policy cut-off date. Diverging from the general pre-post (e.g., difference-in-differences) designs, the RD design establishes causality by comparing firms that are just above the eligibility threshold for a certain policy (treatment) with those that are just below it (control). Lee and Lemieux (2010) suggest that casual inferences from the RD design are more credible than and superior to other identification strategies.[8] Helping to justify our empirical strategy, extensive research in economics relies on date-based RD designs (Lee and Lemieux 2010).

Although the RD design has its merits, a crucial validity assumption underlying this technique is that the running variable is not subject to manipulation (Gow et al. 2016). In our setting, this assumption may be violated if firms anticipated the policy and responded by advancing their firm registration date ahead of the cut-off date. To help dispel the concern that this assumption is violated, we conduct two analyses. First, we check whether firms may have anticipated the new tax collector arrangement prior to the official announcement date by searching for press releases and news articles on this issue. We do not find any indications of potential leakage or anticipation of the arrangement. Second, after plotting the bi-monthly

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[7] We explain later in the paper how we follow prior research to empirically validate this assumption (Lin et al. 2014; Lin et al. 2018; Li et al. 2019).
[8] Gow et al. (2016) stress that difference-in-differences designs based on exogenous shocks often suffer from problems such as non-random assignment of treatments and correlation with unobserved factors.

number of new firms registered from January 2000 to December 2003, we observe no visible

discontinuity in the number of new firms registered just before and just after the cut-off date.

Reassuringly, this evidence is inconsistent with the possibility that firms strategically selected

their registration dates in response to the new tax collector arrangement, which is constructive

for validating that the RD design assumption is defensible in our setting.

To analyze our research question, we begin by comparing the audit fees of firms

administered by the SAT (i.e., firms that registered on or after the cut-off date) to those

administered by the LTB (i.e., firms that registered before the cut-off date). To allow sufficient

time for firms registered in the two years around the cut-off date (i.e., 2001 and 2002) to become

publicly listed firms, our sample period starts in 2008 and ends in 2015; in this timeframe, the

tax collectors of publicly listed firms remain unchanged since their establishment.[9] Our results

suggest that auditors charge lower fees to clients administered by the SAT than they charge

clients administered by the LTB, consistent with the narrative that auditors perceive that clients'

accounting transparency improves under the stricter tax enforcement imposed by the SAT. This

evidence is robust to controlling for a registration date trend (and its higher order polynomials),

to restricting the sample to a narrower window around the cut-off date to potentially improve

identification (Saez et al. 2012), and to using matched-sample designs.

After establishing the casual effect of tax enforcement on audit fees, we examine its

impact on the length of the audit report lag (or audit delay) to help empirically clarify whether

the audit fee results stem from reduced audit effort or a lower risk premium. We find that firms

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[9] In China, a newly registered firm is required to operate for at least three years before becoming eligible
to undergo an initial public offering. We choose to begin our sample in 2008 because of the change in
corporate tax rates and the adoption of a new accounting standard in 2007. The results are similar if we
use an extended sample beginning in 2005, the earliest year possible for the newly registered firms in
2002 to become publicly listed companies.

under the SAT have significantly shorter audit report lags, implying that stricter tax enforcement induces auditors to expend less effort. This result also suggests that the decrease in audit fees is not solely driven by risk premium compensation.

We also examine whether the lower auditor effort comes at the expense of their audit quality. Our results imply that tougher tax enforcement leads to fewer accounting restatements and tax-related restatements. Collectively, our evidence suggests that tax enforcement reduces audit effort (evident in audit fees and the audit report lag) and improves audit quality, supporting the intuition that audits become more efficient when tax enforcement is stricter.[10]

To further validate that tax enforcement intensity is the channel underlying the observed results, we identify two settings where the spillover effects of tax enforcement on auditors are more salient: (i) when SAT enforcement is stronger than LTB's at the local office level; and (ii) when the auditor has industry expertise. We expect that the spillover effects are stronger when auditors have a better understanding of the tax monitoring strength and its implications for their clients.[11] Consistent with this conjecture, we find that the impact of tax enforcement on audit fees, audit report lag, and restatements are all concentrated when tax enforcement is stricter at the local office level or the auditor has industry expertise.

We conduct two placebo tests to provide additional assurance that our observed results reflect the differences in tax enforcement levels between the two tax collectors. First, we hypothetically move the policy cut-off time backward (forward) by one year to January 2001 (January 2003) and re-estimate the empirical analysis between firms registered in 2000 and 2001

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[10] Additionally, we find that audit reports remain equally informative for both types of clients, helping to dispel the alternative explanation that impaired auditor independence is behind our evidence on this front (e.g., Geiger and Rama 2003).
[11] Both SAT and LTB operate local offices across provinces in China. Accordingly, LTB is not more geographically proximate to firms than the SAT is (Kubick et al. 2017).

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(2002 and 2003). Reassuringly, we do not find any perceptible differences in the outcome variables between the two groups, which are both administered by the LTB or SAT. Second, we re-run our tests after isolating Shanghai firms and central state-owned enterprises (SOEs), a group of firms not affected by the new tax collector arrangement because the income tax for Shanghai firms is collected by the Shanghai Municipal Tax Service and central SOEs are always subject to SAT monitoring. Consistent with expectations, the results suggest that auditor behavior relating to Shanghai firms or central SOEs registered in 2001 and 2002 do not exhibit discernible differences during our estimation period. Evidence from these placebo tests is difficult to reconcile with the competing explanation that other economic conditions surrounding the policy cut-off date are spuriously responsible for our core results.

To shed light on the impact on audit firm level resource allocation, we analyze audit partner assignment decisions. Our results suggest that audit firms assign more experienced partners to clients monitored by the less stringent authority, implying that audit firms perceive those clients as requiring tougher monitoring of the financial reporting process. Evidence from examining client acceptance decisions and auditor switches suggests that Big Four auditors consider the tax authority identity in relying on a portfolio approach to managing audit risk.

We make several contributions to prior evidence by analyzing auditors' strategic responses to variation in corporate tax enforcement levels. First, we extend research examining economic outcomes stemming from corporate tax enforcement to include an important third-party: financial statement auditors. Prior work mainly focuses on the role that corporate tax enforcement plays in shaping taxpayers' behaviors (e.g., Hoopes et al. 2012; Hanlon et al. 2014; Lin et al. 2018). In contrast, we find that the impact of tax enforcement can extend to other market participants. Our evidence implies that audit quality improves under stricter tax authority monitoring in that clients have fewer accounting and tax-related restatements despite

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that both audit fees and the audit report lag fall in this situation. Altogether, these results suggest that audit efficiency rises with tax enforcement severity. At the audit firm level, we document that corporate tax enforcement enables audit firms to better allocate resources by optimizing the matching of clients and partners. Accordingly, we respond to calls for research on the determinants of partner assignment policies, including the role that clients' propensity to manipulate their earnings plays (Lennox and Wu 2018). In short, our results suggest that corporate tax enforcement generates a positive externality for the audit market. Consequently, omitting the spillover benefits from tax authority cross-monitoring would underestimate the wider impact of strict tax enforcement. Our results have major implications for practitioners, standard-setters, and regulators striving to deepen their understanding of the forces that affect accounting transparency and audit quality, as well as for regulators and policy makers in the evaluation of the costs and benefits of tax enforcement (Mills 2019).

Second, we advance our understanding of how auditors respond to tax enforcement intensity and to clients' tax risk in general. Although prior work analyzes the association between audit fees and clients' tax aggressiveness (Hanlon et al. 2012; Donohoe and Knechel 2014), audit research remains silent on the role that tax enforcement plays. We triangulate our audit fee results with evidence on various facets of audit inputs and outcomes, including the audit report lag, accounting and tax restatements, the accuracy of modified audit opinions, and audit firms' decisions on the matching of clients with partners. Our results on audit firms' use of resources also enrich our understanding of the real effects of the interplay between financial reporting and tax enforcement, a non-reporting institution on auditors (Leuz and Wysocki 2016). Collectively, our findings paint a fairly comprehensive picture of the causal effect of corporate tax enforcement on external auditors.

Finally, we report causal evidence on the importance of corporate tax enforcement to auditor behavior using a novel setting involving a new rule that exogenously assigns firms into one of two different tax enforcement regimes. Leuz and Wysocki (2016) advise researchers to examine non-traditional settings that permit tighter research designs that are not feasible using U.S. data. We initiate research on this regime shift in the tax collector arrangement in China, which represents an opportune testing ground that facilitates drawing causal inferences on the impact of tax enforcement. Importantly, examining the role that tax enforcement plays in shaping auditor behavior is particularly advantageous in this setting because auditor-provided tax services (APTS) are prohibited in China. [12] The provision of APTS complicates the interpretation of results because it is an endogenous choice that has been shown to affect various dimensions of audit as well as tax outcomes.[13] In another upside, auditor discipline stemming from litigation institutions is minimal in China such that we circumvent its nuisance impact on audit pricing (e.g., He et al. 2016), which facilitates isolating whether auditor effort is sensitive to tax enforcement. Our focus on non-state-run firms alleviates external validity concerns that our findings are less relevant to settings where the government plays a smaller role in the economy. Finally, additional analysis implies that the advantages of studying our

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[12] More generally, Chinese firms purchase hardly any non-audit services from their auditors.

[13] In terms of tax outcomes, prior research has documented that APTS are related to increased earnings management via tax accounts (Cook et al. 2008), improved estimation of tax reserves (Gleason and Mills 2011), and more tax avoidance (McGuire et al. 2012; Klassen et al. 2016). Another stream of empirical research generally finds that APTS are associated with better audit quality stemming from knowledge spillovers, including improved accuracy of going-concern opinions and internal control (Robinson 2008; De Simone et al. 2015), as well as a reduced likelihood of restatements and loss avoidance (Kinney et al. 2004; Krishnan and Visvanathan 2011). The prohibition of APTS in China significantly alleviates concerns that the results are contaminated by any of those effects.

research question using the China setting do not come at the expense of external validity, which

provides some assurance that our results are generalizable to environments outside of China.[14]

The rest of this paper is organized as follows. Section 2 summarizes the evolution of the

institutions governing corporate tax enforcement in China and develops the hypotheses. Section

3 describes the empirical design and the data. Section 4 outlines the main evidence, while

Section 5 covers the results from additional analyses. Section 6 concludes the paper.

2. Background and hypothesis development

2.1 Tax collection in China, before and after 2002

In China, the collection of corporate income taxes is administered by two tax authorities:

The State Administration of Taxation (SAT) and the Local Taxation Bureau (LTB). The SAT is

responsible for central state-owned firms (central SOEs), while the LTB collects income taxes

from local state-owned firms (local SOEs) and non-state-owned firms in their jurisdictions (SAT

1995). Originating with the fiscal reform in 1994, this tax collection arrangement was designed

to establish an efficient fiscal allocation mechanism enhancing the tax revenue collection and

power distribution of the central government. At the local level, this collection arrangement also

allows local governments to match their expenditures with their local tax revenue (Qian and

Roland 1998). Officials at the SAT are appointed by the central government to handle its tax

enforcement and tax collection, whereas LTB officials are appointed and supervised by the local

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[14] In untabulated tests using a large sample of Chinese listed firms between 2002 and 2007 where tax audit rate data are available, we find strong evidence that tax audit likelihood is associated with lower tax avoidance (Hoopes et al. 2012) and that tax aggressiveness is associated with higher audit fees (Donohoe and Knechel 2014). This associative evidence reinforces prior U.S.-based research and suggests that both the deterrence effects of tax enforcement and auditor behavior exhibit a large degree of similarity between the U.S. and China, lending some support for the external validity of our results. Nonetheless, it is important to exercise caution when interpreting the empirical relevance of results from any single-country study for other jurisdictions.

government for tax collection at the local level (Tang et al. 2017).[15] The SAT also provides guidance and coordination on taxation issues with the LTB and monitors the LTB as necessary.[16]

In 2002, China adopted a new arrangement for corporate income tax collection such that all firms registered on or after the cut-off date of January 1, 2002 became subject to the SAT's administration, whereas firms registered by December 31, 2001 follow the old regime by remaining under the administration of the LTB (SAT 2002). The rationale behind this policy shift was the central government's interest in enhancing its ability to fund national programs, ranging from infrastructure projects to initiatives intended to improve the financial situation of low-income provinces (Zhang and Martinez-Vazquez 2003). This new arrangement for administering corporate income tax collection allows us to compare firms that differ in tax enforcement due to their assigned tax collector (SAT or LTB) and share similar characteristics in other dimensions while holding constant other aspects of tax laws, including tax bases and statutory tax rates. Accordingly, we exploit this strong quasi-experimental setting to identify the impact of tax enforcement by comparing firms subject to the administration of the SAT (i.e., those that registered on or after 2002 under the new arrangement) and firms under the administration of the LTB (i.e., those registered as of 2001 under the former arrangement).

2.2 Prior research on the role of clients' tax aggressiveness in audit risk model

In their comprehensive review of empirical audit research, DeFond and Zhang (2014) consider four strategies that auditors apply in response to client risk and client complexity: (1) exert greater audit effort and increase audit quality to reduce risk under the production view of

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[15] Tang et al. (2017) find that local government ownership creates a conflict for the local tax authority that leads to local government-owned firms enjoying lax tax enforcement, resulting in local SOEs avoiding more taxes. We explain later that we exclude these firms from our analysis to improve identification.

[16] In China, all firms have the same December 31 fiscal year end and firms are required to issue their annual reports before April 30. The deadline for filing corporate tax returns is May 31 (or April 30 before 2008) of the same year.

the audit process (e.g., Simunic 1980); (2) charge a fee premium to compensate for bearing the risk (e.g., Bell et al. 2008); (3) manage the client portfolio to control overall risk exposure (e.g., Krishnan and Krishnan 1997; Johnstone and Bedard 2004); and (4) lobby for litigation relief (Geiger and Raghunandan 2001).[17] For tax enforcement to affect auditor behavior, it should shape the inherent risk of the audit engagement. However, prior research on the role that tax enforcement plays in the audit risk model remains scarce.

The complexity and judgment involved in the estimation of tax expenses and the difficulties in understanding the tax positions of firms render tax accounts a major source of financial reporting risk (Badertscher et al. 2009; Graham et al. 2012). Issues surrounding accounting for income taxes are often cited as the cause of financial restatements (Ernst and Young 2016). Hanlon et al. (2012) find a significant positive association between book-tax differences and audit fees, and the results are stronger for firms that might have managed earnings as compared to tax-avoiding firms. Donohoe and Knechel (2014) find that auditors charge higher fees to more tax-aggressive clients, which they attribute to auditors incorporating a risk premium into audit pricing, even with additional effort considered, as tax-aggressive clients expose the auditor to heightened litigation, regulatory, and reputational costs. Overall, prior evidence lends support to the notion that auditors impose a fee premium on more tax-aggressive clients to reflect their greater risk and complexity.

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[17] Extensive prior research implies that strict investor protection institutions motivate auditors to constrain firms from excessively distorting their reported earnings (Guedhami and Pittman 2006; Francis and Wang 2008). However, it is important to stress that auditor discipline stemming from civil litigation forces is minimal in China. He et al. (2016) explain that although investors in China are entitled to recover losses incurred when auditors issue an unqualified opinion on materially deficient financial statements, lawsuits against auditors almost never succeed there; i.e., in our setting, litigation incentives play almost no role in inducing auditors to closely monitor the financial reporting process.

2.3 The impact of tax enforcement on auditor behavior

At this stage, the importance of tax enforcement to auditor behavior remains unclear because extant audit research seldom considers the role that tax authority monitoring plays. In analytical research, Ewert and Wagenhofer (2019) model the impact of tougher regulatory enforcement (e.g., SEC oversight) on financial reporting quality and audit quality. Ewert and Wagenhofer (2019) demonstrate that the link between enforcement and audit effort hinges on enforcement intensity. Audit effort subsides in a strong enforcement regime since this lowers anticipated earnings management, which, in turn, engenders lower audit effort. However, if the enforcement regime is lax, auditors work harder to protect audit quality. It remains an empirical question whether external auditors are sensitive to variation in enforcement intensity.

In the presence of stricter tax enforcement, clients' tax positions will attract more attention from the tax authority, increasing the risk of financial restatement stemming from tax adjustments or disallowed tax claims (Hanlon et al. 2012; Donohoe and Knechel 2014). The elevated risk of restatement could lead to costly reputational damage to the auditor (e.g., Cao et al. 2012; Hennes et al. 2014; Irani et al. 2015; Christensen et al. 2016; Aobdia 2019). Reflecting the major disciplinary role that reputation protection incentives play, individual auditors not complicit in audit failures in China still suffer severe reputational damage (He et al. 2016; Su and Wu 2019). Consequently, auditors will expend more effort in auditing the client's tax positions when tax enforcement is tougher. It is important to stress that the increased audit effort focuses on not only clients' tax positions, but their overall financial reporting. Prior research suggests that tax authority assesses firms' financial statements (e.g., book-tax gap) in corporate tax examinations (Mills 1998; Bozanic et al. 2016; Ayers et al. 2019). As a result, we expect that auditors will charge higher fees to compensate for the incremental effort on both tax

and non-tax aspects of clients' financial reporting. It follows that audit fees will rise with tax enforcement severity.

However, there are reasons to suspect that the impact of tax enforcement on audit fees will run in the other direction. For starters, the auditor may anticipate that the client's tax aggressiveness and financial reporting risk will fall when tax enforcement is stricter. This expectation reconciles with prior research implying that closer monitoring by the tax authority constrains corporate tax avoidance (Mills 1998; Hoopes et al. 2012) and earnings management (Hanlon et al. 2014). Additionally, when tax enforcement is stricter, firms are naturally eager to better prepare for potential tax authority challenges by more extensively researching and documenting evidence that helps justify their tax positions. This enhanced effort in the research and documentation of tax positions will be revealed to the auditor during the engagement, providing additional assurance on the veracity of these amounts. Collectively, these arguments suggest that stricter tax enforcement under SAT can relieve the auditor's concerns about the quality of the client's tax positions and reported earnings, translating into auditors expending less effort and charging lower fees in this situation. Given the competing forces in play, the role that tax enforcement plays in shaping auditor behavior distils to an empirical question.

So far, we consider the implications for external auditors of their clients' tax aggressiveness and financial reporting risk (i.e., tax-related restatements) moving in the same direction.[18] However, it is important to stress that firms may treat tax aggressiveness and financial reporting aggressiveness as separate channels for increasing reported after-tax earnings (Graham et al. 2014). Motivated by financial reporting incentives, firms may elect to manage earnings upward (unrelated to the tax accounts) when improving the bottom line via

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[18] Frank et al. (2009) find that firms that are aggressive in tax reporting are also more likely to be aggressive in their financial reporting practices.

tax avoidance becomes more costly under a strong tax enforcement regime. Dhaliwal et al. (2004) report evidence suggesting that firms resort to relying on tax accounts in managing earnings when they exhaust other methods, consistent with the intuition that firms weigh various earnings management approaches based on their relative costs. Lennox et al. (2013) find that tax-aggressive firms are less likely to commit accounting fraud. Additionally, prior work implies that the tax authority may fail to play an effective monitoring role in constraining firms from orchestrating financial reporting fraud (Erickson et al. 2004; Dyck et al. 2010). Accordingly, to the extent that firms are more likely to manipulate accounting numbers when their tax avoidance practices are constrained under tougher tax enforcement, auditors may charge higher fees to clients monitored by the SAT.

Finally, despite the above discussion, auditors may conclude that tax enforcement severity has no material impact on either detection risk or clients' behavior. Reinforcing this perspective, Nessa et al. (2019) find no evidence that taxpayers are less likely to claim questionable or aggressive positions when IRS resources are higher. This evidence likely reflects the allocation of additional resources to audits of tax positions supported by stronger taxpayer facts, resulting in fewer collections of proposed tax deficiencies (Nessa et al. 2019). In analyzing the impact of IRS in-depth tax audits under the Coordinated Industry Cases (CIC) program, Ayers et al. (2019) find that assigning public companies to the CIC program is irrelevant to firms' tax aggressiveness. It follows that if the impact of tax enforcement on corporate tax behavior is trivial, we may fail to observe that tax enforcement shapes auditor behavior.

Consequently, given the tension underlying our research question, we formally state our hypothesis in null form, rather than making a directional prediction:

H1: There is no difference in audit fees between firms administered by the SAT and those administered by the LTB, all else equal.

3. Data and research design

In analyzing the impact of tax enforcement on auditor behavior, our identification strategy relies on the variation in tax collector regime (the SAT versus the LTB) stemming from the new tax collector arrangement that assigned firms registered on or after the cut-off date (January 1, 2002) to the SAT, China's national tax authority. Firms registered prior to the cut-off date continue to follow the former arrangement by remaining under the administration of the LTB (SAT 2002). As mentioned earlier, this new tax collector arrangement was announced by the government on January 16, 2002 and was unanticipated prior to the official announcement. Given that the new tax collector arrangement was unanticipated and that the assignment variable (i.e., the firm registration date) is unlikely to be manipulated, the tax collector identity (the SAT or the LTB) of firms registered near the cut-off date is randomized, resembling a randomized experiment (Lee and Lemieux 2010). Consequently, this regime change allows us to implement a sharp RD design to isolate the treatment variation of the tax collector identity, which we use as a measure of tax enforcement severity.

3.1 Sample and data

Our initial sample includes all China A-share listed companies from 2008 to 2015. We then impose our first sample screen backward by including only firms established between 2001 and 2002, one year before and after the policy cut-off date.[19] Our choice of a one-year pre- and post-policy cut-off window ensures that we have a reasonable number of sample firms for the analysis. The China Securities Regulatory Commission (CSRC) requires three consecutive years of financial statements in IPO filings. Accordingly, it takes at least three years for newly registered firms to become listed public companies. For firms established around the year of the

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[19] As a robustness test of the RD design, we specify a narrower window (e.g., six months or nine months before and after the cut-off date) and find that our core results persist.

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tax reform (2001-2002), the earliest year for them to become listed would be 2005. We choose to begin the sample period in 2008 to avoid potential confounding factors related to the change in corporate tax rates and the adoption of a new accounting standard in 2007.[20, 21] As discussed in more detail later, our core results hold if we extend the sample year to 2005, the earliest year that firms registered in 2002 can become publicly listed.

We impose additional criteria to exclude firms not affected by the new tax collector arrangement and firms whose characteristics may affect the interpretation of the results. First, we exclude central SOEs because they are always administered by the SAT; i.e., they are not affected by the new tax collector arrangement. Second, we exclude local SOEs because these firms' tax avoidance behavior may have been altered by the heightened intergovernmental agency conflicts around the same period (Tang et al. 2017).[22] We also exclude firms registered in Shanghai and Tibet because these firms are specially administered by separate tax authorities that are not directly comparable to the SAT or the LTB.[23] In short, our main sample consists of non-state-owned firms. After applying these screening criteria, we are left with a final sample of 1,233 publicly listed firm-years between 2008 and 2015 registered in 2001 and 2002. In Table 1, we summarize the sample selection process.

[Please insert Table 1 about here]

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[20] In 2008, the statutory tax rate rose from 15% to 25% for foreign firms and fell from 33% to 25% for domestic firms in China. Since our sample is comprised of strictly domestic firms, we do not expect the corporate tax rate change to have differential effects on LTB and SAT firms. We also control, as discussed in Appendix B, for any statutory rate differences stemming from industry-specific reductions in our ETR regressions.

[21] The accounting rule changes restricted tax reporting to the tax effect method that allows for deferred taxes, which may affect the measurement of ETR during this period (Chan et al. 2013). Accordingly, in our ETR regressions (Table B1 and Figure B1 in Appendix B), we specify current ETR as our proxy for tax avoidance, although the results persist under total ETR.

[22] In another upside, excluding SOEs is constructive from an external validity standpoint. Nevertheless, we verify that our core results are materially insensitive to including local SOEs in the sample.

[23] There are no observations for Tibet, so this constraint is not binding. We use Shanghai firms and central state-owned firms in our placebo tests.

3.2 Research design

We analyze the impact of the tax authority policy shift on auditor behavior using a pre-post shock framework given by the following specification:

$$Y = a + \beta SAT(t \geq 0) + \delta Z + \varepsilon \qquad (1)$$

where the dependent variable Y represents (the natural logarithm of) audit fees for our sample firms from 2008 to 2015. t is the registration date normalized to $t=0$ at the cut-off line of January 1, 2002. SAT is an indicator equal to one for firms that registered after the cut-off date such that their income taxes are collected and administered by the SAT, and zero otherwise. Z denotes a vector of control variables as well as industry and year fixed effects. Our coefficient of interest is β, which captures the effect of tax enforcement $SAT(t \geq 0)$ on audit fees. We follow prior work by controlling for variables that have been shown to explain audit fees (Wang et al. 2008; DeFond and Zhang 2014; Guan et al. 2016; Lennox and Kausar 2017).

Larger and more complex clients require more audit effort. Accordingly, we include firm size measured with the natural logarithm of total assets (*SIZE*), the square root of employees (*EMPLOYEE*), the natural logarithm of the number of segments (*SEGMENT*), and listing age (*AGE*). We also control for audit risk related to clients' leverage (*LEV*) and operational factors, such as receivable intensity (*RECEIVABLE*), inventory intensity (*INVENTORY*), and capital asset intensity (*PPE*). We control for clients' profitability with net income over total assets (*ROA*), the incidence of negative earnings (*LOSS*), and firm risk measured by annual market-adjusted stock returns for the fiscal year (*RETURN*). The estimations also include *BIGFOUR*, an indicator variable that identifies Big Four audit firms given that these major international audit firms eager to protect their valuable reputations and avoid costly litigation are known to supply higher quality audits that warrant higher fees. Modified audit opinion (*MAO*) is also used to control for greater audit effort. Finally, we

include institutional factors: whether the client also issues shares in Hong Kong (*HSHARE*). Please refer to Appendix A for detailed variable definitions.

4. Empirical analysis

4.1 Identification checks

In this empirical setup, a key requirement for identification is that the new firms registering shortly before and shortly after the cut-off date are comparable. This identification requirement could be invalidated if some firms had strategically selected their registration date in anticipation of the new tax collector arrangement. To help clarify whether the RD design assumption is violated, we plot the number of new firms registered around the policy cut-off date at a bi-monthly frequency. In Figure C1 of Appendix C, we plot the sample of all new firms registered, regardless of eventual listing status, between January 2000 and December 2003, covering two years immediately before and one year immediately after the policy cut-off date. This analysis illustrates a new firm registration pattern that solely reflects registration date selection and is not affected by subsequent decisions and the ability to become a publicly listed firm. There is no visible discontinuity in the number of new firms registered around the cut-off date. Additionally, we observe substantial bi-monthly variation in the number of new firms registered due to seasonality effects within a calendar year. In unreported analysis, we adjust for the bi-monthly seasonality in the data and obtain an even smoother series.

Besides the graphical analysis, we provide a more formal identification check by regressing the monthly new firm registration number on our variable of interest, *SAT*. We conduct this test over three sample windows (24-month, 18-month, and 12-month) surrounding the policy cut-off date. As reported in Table C2 of Appendix C, the coefficients on *SAT* across the three sample windows are positive (although statistically indistinguishable from zero in two of the three estimations), supporting that the pattern of monthly new firm registration did not

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suggest that firms were gaming the system by delaying registration to take advantage of the assignment to a less stringent tax authority (the LTB). In particular, the coefficient on *SAT* enters positively (albeit at only the 10% level in a two-tailed test) when the sample window is set to 24 months; in contrast, it fails to load when we narrow the sample window to 18 or 12 months. These results suggest that more firms were registered during the early period of the 24-month window from January to March 2003. Altogether, the evidence runs against firms strategically selecting their registration dates in response to the new arrangement, providing some validation for the assumption underlying the RD design.

The absence of gaming effects may stem from the unanticipated nature of this policy initiative. The new tax collector arrangement was announced by the government on January 16, 2002 and was applied retrospectively for firms registered on or after January 1, 2002. Accordingly, entrepreneurs could only time the policy cut-off date by rushing to register a new firm to benefit from the old regime if leakage of the arrangement occurred prior to its official announcement (or by delaying registration if they perceived the new regime to be more beneficial). To explore whether this explanation is viable, we search for press releases and news articles pertaining to the arrangement. We do not find any articles suggestive of a potential leakage or anticipation of the arrangement prior to its official announcement.

However, prospective entrepreneurs may still have been informed about the new arrangement through channels we are unable to identify. If we assume that prospective entrepreneurs were informed about the new arrangement, the absence of gaming effects could be due to the lack of perceived differences in tax enforcement strength between the SAT and the

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LTB in the eyes of the prospective new firm owners.[24] In the next section, we compare proxies of tax enforcement strength using available data to deepen our understanding of differences in tax enforcement between the SAT and LTB.

4.2 Tax enforcement strength: SAT versus LTB

In our RD approach, the assignment to the SAT and LTB based on the policy cut-off date can be interpreted as an instrumental variable affecting the tax enforcement severity imposed on firms. Prior research finds that tax avoidance is lower when tax authority personnel in China have more education or expertise, have larger budgets, and conduct more audits (Lin et al. 2018). We follow Lin et al. (2018) by hand-collecting data from hard copies of the China Tax Audit Yearbook on corporate tax enforcement intensity (i.e., the tax audit rate and penalty rate) and the resources allocated to corporate tax audits and inspections (i.e., the number of tax inspectors per tax audit unit, and inspectors' education and experience). We then compare these measures of corporate tax enforcement and tax authority resources, aggregated at the country level, between the SAT and LTB over time, to explore whether the SAT's corporate tax enforcement is indeed stricter than the LTB's.

Table 2 reports the mean and median values of the above measures over three periods: before the tax collector arrangement (1999-2001, Panel A), after the implementation of the arrangement and before our estimation period (2002-2007, Panel B), and during our sample period (2008-2015, Panel C). Despite concerted data collection efforts, the availability of certain information varies over time; therefore, not all proxies are available during the entire period.

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[24] Given that users tax enforcement perceptions are naturally unobservable, we follow prior research by appealing to rational expectations by assuming that actual enforcement levels constitute unbiased estimates of their expectations (e.g., Guedhami and Pittman 2008; Hoopes et al. 2012).

In Panel A of Table 2, we compare the mean and median differences in both tax audit rate over the number of tax returns (*TAX AUDIT*) and penalty rate over settled deficiencies (*TAX PENALTY*) between the SAT and LTB three years immediately before the implementation of the new tax collector arrangement. The statistics suggest that the tax enforcement efforts, evident in both the tax audit rate and the penalty rate, of the SAT are significantly stronger than those of the LTB during this pre-event period. In Panel B, we move our timeline to the six-year period after the policy and immediately before our sample period (2002-2007). For this timeframe, besides *TAX AUDIT* and *TAX PENALTY*, we obtain information on the number of tax inspectors per tax inspection unit (*TAX INSPECTOR/UNIT*), which is another way to gauge tax enforcement severity. Moreover, the education (*DEGREE INSPECTOR*) and experience (*EXPERIENCED INSPECTOR*) of tax inspectors are available during this period. These additional measures allow us to compare the resources allocated to tax audits and inspections in the SAT and LTB. As reported in Panel B, the mean and median values of these variables are generally consistent with the conjecture that the SAT imposes stricter tax enforcement relative to the LTB; *DEGREE INSPECTOR* is the lone exception in these pairwise comparisons. In Panel C, we compare the mean and median values of tax enforcement proxies during our sample period (2008-2015). *TAX INSPECTOR/UNIT*, *DEGREE INSPECTOR*, and *EXPERIENCED INSPECTOR* are the only variables with available data during this period. Despite the relatively poor data availability, the results reinforce that the SAT has a more educated and experienced workforce than the LTB. Overall, the evidence reinforces that the SAT subjects firms to tougher tax enforcement. Effectively, assignment to the SAT and LTB around the policy cut-off date operates as an instrumental variable. Validating that the SAT applies stricter tax enforcement than the LTB provides some assurance that the relevance condition of an instrument is satisfied.

4.3 Descriptive statistics

In Table 3, we provide some descriptive statistics. For the full sample, we report in Panel A that the mean (median) audit fee is approximately 652,000 (600,000) Chinese Yuan, which is equivalent to US$100,500 (US$92,500). This amount is slightly larger than that of Guan et al. (2016), who analyze a slightly earlier period. Our sample firms have a mean audit lag of 92 days. Approximately 13.1% (5.7%) of the firm-year observations experience an accounting restatement (tax-related restatement). The mean ETR for the full sample is 19.9%.

In Panel B of Table 3, we report that both the mean and median audit fee (*FEE*) of firms under SAT administration are significantly lower than those of firms under LTB administration, providing some preliminary evidence that auditors charge firms administered by the SAT lower audit fees. Firms under SAT monitoring also exhibit shorter audit reporting lags, a higher incidence of modified audit opinions, and higher effective tax rates than firms overseen by the LTB. For the vast majority of firm-level characteristics, there are no perceptible differences between the two groups. In the only exceptions, SAT firms are smaller (in terms of assets and employees) and younger (*AGE*).[25] To further neutralize the potential effects of these differences on our results, we employ two matching methodologies (propensity score matching and entropy balance matching) in sensitivity analysis to construct treatment (SAT) and control (LTB) samples that are comparable on all firm-level control variables (see Table 8, Panel C).

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[25] Firm age (*AGE*) is defined as the natural logarithm of the number of years since the firm went public. The mean difference in the unlogged value of *AGE* between SAT and LTB firms is less than one year. The results are similar when we define firm age as the natural logarithm of the duration since incorporation.

4.4 Tax enforcement and corporate tax aggressiveness

Initially, we analyze the impact of tax enforcement on the level of corporate tax avoidance. Although this result has been established in prior work (e.g., Hoopes et al. 2012; Lin et al. 2018), we examine this issue for two reasons: (i) to verify that the tax behavior that our sample firms exhibit is similar to the firms studied in other U.S. or China settings; and (ii) to provide within-sample empirical evidence given that the effect of tax enforcement on tax aggressiveness is a major channel through which tax enforcement may affect audit fees. Our proxy for tax avoidance is current effective tax rates (*Current_ETR*) (e.g., Rego 2003; Tang et al. 2017; Lin et al. 2018). We first plot the discontinuity in *Current_ETR* around the threshold or policy cut-off date (January 1, 2002) in Figure B1 of Appendix B. Firms just to the left (right) of the threshold are administered by the LTB (SAT). Both the linear and quadratic plots under Figure 1 present a jump in *Current_ETR* around the threshold as a result of tax enforcement.[26]

Next, we formally evaluate the impact of tax enforcement on *Current_ETR* using a regression approach. We follow prior research by including several firm characteristics associated with tax planning opportunities or tax outcomes. We include firm size (*SIZE*), firm age (*AGE*), and pre-tax performance (*PT_ROA*) to control for tax planning incentives (Mills et al. 1998; Rego 2003; Frank et al. 2009). We include leverage (*LEV*), property, plant and equipment (*PPE*) and intangible assets (*INTANG*) to control for debt tax shields and investment related to tax incentives, such as depreciation and amortization (Graham and Tucker 2006). We include inventory intensity (*INVENTORY*) and the number of geographic segments (*SEGMENT*) to control for tax planning opportunities (Mills et al. 1998; Gallemore and Labro 2015).

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[26] In untabulated regressions, we find consistent results using firms' book value of assets (Mills et al. 1998) and market value of assets (Henry and Sansing 2018) as an alternative denominator of ETR to retain the loss firms.

Additionally, we control for firms' financial reporting aggressiveness, proxied by the absolute values of abnormal accruals (*ABSDA* (Dechow et al. 1995), to mitigate concerns over the influence of firms' financial reporting aggressiveness on their corporate tax policies (Frank et al. 2009). Finally, we control for applicable statutory tax rates (*STAT_TAX_RATE*) since beneficial tax rates may apply to firms in different industry sectors and/or provinces (Tang et al. 2017).

In Column (1) of Table B1, the results indicate that the coefficient on *SAT* is positively related to *Current_ETR* (0.031, *t*=3.02) using a specification including only industry and year dummies. The results in Column (2) show that the coefficient on *SAT* remains significantly positively related to *Current_ETR* (0.022, *t*=2.04) after including the full set of control variables as well as industry and year fixed effects. To further mitigate the inherent differences of firm size and industry embedded in *Current_ETR*, we employ the industry- and size-adjusted measure (*Ind_Size_ETR*) developed in Balakrishnan et al. (2019). In Column (3), we continue to find that *SAT* enters positively under *Ind_Size_ETR* (0.022, *t*=2.14).[27] In short, this evidence implies that the SAT imposes stricter tax enforcement than the LTB.

Prior research suggests that tax audits in China reduce firms' income-decreasing accruals, reflecting that tax authority monitoring deters firms from underreporting their taxable income (Lin et al. 2014; Li et al. 2019). In another approach to validating our tax monitoring proxy, we evaluate whether the SAT is more effective than the LTB in constraining firms' discretionary accruals that can potentially lead to lower taxable income. Using three standard estimation models for discretionary accruals (Dechow et al. 1995; Dechow and Dichev 2002; Kothari et al. 2005) in unreported regressions, we find that the SAT has a significantly negative impact on downward earnings management, ranging from -11.1% to -34.8%, depending on the

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[27] The results are qualitatively similar using total ETR as well as ETR measured over multiple years (3-year or 5-year measure).

model. These results reconcile with Lin et al. (2014) and Li et al. (2019), lending additional support that the SAT imposes tougher tax enforcement than the LTB.

4.5 Tax enforcement and audit fees: Test of H1

Figure 1 displays the plots of the discontinuity in audit fees around the policy cut-off date. Again, firms just to the left of the threshold are under the LTB and firms just to the right of the threshold are under the SAT. Both the linear and quadratic plots in Figure 1 show a sharp drop in audit fees around the threshold as a result of tax enforcement.

[Please insert Figure 1 about here]

Before proceeding to estimate the effect of tax enforcement on audit fees using regressions, we validate that the control variables we use are balanced across the threshold by regressing each of the control variables on *SAT* along with industry and year fixed effects (Lin et al. 2018). As reported in Panel A of Table 4, the results indicate almost no statistical evidence for a discontinuity of any control variable around the threshold.[28] It follows that discontinuous control variables are unlikely to bias our findings.

[Please insert Table 4 Panel A about here]

In Table 4, we report in Panel B the regression results for the prediction in H1, which focuses on the importance of tax enforcement to audit fees. In Column (1), the coefficient on *SAT* is significantly negative (-0.084, *t*=-2.45) after we control for industry and year fixed effects. In Column (2), the coefficient on *SAT* continues to enter negatively (-0.051, *t*=-2.13) after we control for other determinants of audit fees according to prior research. Next, we follow Lee and

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[28] Predictably given the mechanical relation, the lone exception is *AGE*. However, as discussed earlier, the mean difference is less than 1 year (Table 1). Results in regression analysis controlling for *AGE* suggest that it is not a significant factor in our tests. In matched sample analysis, the differences in *AGE*, among other variables, are fully mitigated to ensure the full comparability between the SAT and LTB samples.

Lemieux (2010) and Saez et al. (2012) by evaluating whether our RD results persist after adding a linear registration date trend variable and its interaction with *SAT* to the estimation. In Column (3), we find that our evidence is robust to this approach.[29] Reflecting its first-order economic materiality, the coefficient estimate on *SAT* implies that, on average, the stricter tax enforcement under the SAT translates into audit fees falling by 5% to 15.9%, depending on the specification. Shifting gears to the control variables, we find that almost all are statistically significant in the predicted directions (e.g., Wang et al. 2008; Hanlon et al. 2012; Guan et al. 2016). Collectively, our results imply that stricter tax enforcement leads to lower audit fees, lending support to rejecting the prediction in H1 that no difference exists in audit fees between firms administered by the SAT and those administered by the LTB.

[Please insert Table 4 Panel B about here]

4.6 Tax enforcement and audit report lag

Since audit fees reflect both effort and risk premia, we examine the impact of tax enforcement on the audit reporting lag, specified as the duration between the balance sheet date and the audit report date to provide triangulating evidence (e.g., Bamber et al. 1993; Tanyi et al. 2010). We expect that the stricter tax enforcement under the SAT will translate into auditors expending less effort evident in a shorter audit report lag. Table 5 reports the results for the audit report lag regressions. In Column (1), the coefficient on *SAT* is significantly negative (-0.008, *t*=-2.21), suggesting that tougher tax enforcement results in shorter audit delays. After controlling for the linear date trend and its interaction with *SAT* in Column (2), we find that the coefficient of *SAT* remains significantly negative, albeit at only the 10% level (-0.014, *t*=-1.73).

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[29] In additional robustness checks (untabulated), we continue to find supportive evidence when the regressions include higher polynomial terms of trends and the corresponding interaction term with *SAT*.

Collectively, our evidence implies that tighter tax monitoring leads to lower audit fees and shorter audit report lags, consistent with tax enforcement improving audit efficiency.

[Please insert Table 5 about here]

4.7 Tax enforcement and restatements

To explore whether auditors devoting less effort comes at the expense of their audit quality, we examine the relation between tax enforcement and the likelihood of financial restatements, a signal of audit failure (e.g., Cao et al. 2012; Christensen et al. 2016; Aobdia 2019), including in China (e.g., He et al. 2017; Li et al. 2017; Su and Wu 2019). In Column (1) of Table 6, we find that stricter tax enforcement (*SAT*) leads to a lower likelihood of accounting restatement (-0.077, *t*=-4.09). This implies that the lower audit effort—evident in cheaper audit pricing and a shorter audit report lag—in the presence of the SAT documented earlier does not come at the cost of sacrificing audit quality evident in the incidence of egregious financial reporting failures. This evidence becomes stronger (-0.219, *t*=-6.88) when we control for both the linear date trend and its interaction with *SAT* in Column (2). Additionally, in Columns (3) and (4), we find supportive evidence that tougher tax enforcement leads to a lower likelihood of tax-related restatements, irrespective of whether we control for the date trend and its interaction with *SAT*.[30]

[Please insert Table 6 about here]

4.8 Cross-sectional analysis: Tax enforcement differences at the local office level

Next, we isolate settings in which the spillover effects of tax enforcement may play a larger role in shaping auditor behavior. Like the IRS in the U.S., the SAT operates regional

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[30] In Table 6, we report results using a linear probability model, although the results (untabulated) are both quantitatively and qualitatively similar using logit as the estimation technique.

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offices to facilitate tax enforcement and collection from taxpayers. Although the central SAT sets the tone and determines the overall resources committed to corporate tax enforcement at the national level, enforcement intensity may vary across local offices. Accordingly, the tax enforcement at a regional SAT office may be more lenient in comparison to the LTB office in certain locations. To deepen our analysis by exploring the channel potentially underlying the observed results, we exploit the cross-sectional variation in tax audit rates and penalty rates across local offices of the SAT and LTB. Using hand-collected data on tax enforcement measures at the local office levels, we specify an indicator equal to one for firms located in provinces where the median tax audit rate and penalty rate of the provincial SAT office are higher than those of the provincial/local LTB office. This variable captures the relative strength of tax enforcement between the SAT and LTB at the local office level. We label this variable *HIGH_SAT_ENFORCE* and interact it with our SAT indicator. We report the results in Table 7.

Consistent with expectations, we find that the impact of tax enforcement is stronger for firms in provinces where the local SAT office exerts observably stricter tax enforcement than the local LTB office. More specifically, in Panel A, the coefficient on the interaction term (*SAT×HIGH_SAT_ENFORCE*) is significantly negative for audit fees (-0.105, *t*=-2.11), audit report lag (-0.012, *t*=-1.71), accounting restatements (-0.090, *t*=-2.28) and tax-related restatements (-0.150, *t*=-4.15), corroborating our earlier evidence. For audit fees, accounting restatements and tax-related restatements, the results are virtually identical when we control for the linear date trend and its interaction with *SAT*. However, in the audit report lag regression, the coefficient on the interaction term becomes statistically indistinguishable from zero when we add the linear date trend and its interaction with *SAT* to the model (-0.011, *t*=-1.48). Overall, in exploiting the cross-sectional variation in tax audit rates and penalty rates at the local office

level of the SAT and LTB, we provide evidence in seven of the eight estimations implying that tax enforcement constitutes the channel underlying the main results reported earlier.

4.9 Cross-sectional analysis: industry expert auditor

In our second set of cross-sectional analyses, we examine whether the spillover effect of tax enforcement on auditor behavior varies systematically with the presence of an industry specialist auditor. We expect that auditors are in a better position to evaluate the implications of tax enforcement to the client when they have expertise in the client's industry since, for example, complex tax planning strategies are highly industry-specific (Balakrishnan et al. 2019).

To examine this conjecture, we follow prior research in specifying industry specialist auditors (*INDUSTRY_EXPERT*) (e.g., Audousset-Coulier et al. 2015) and interact it with our SAT indicator in Equation (1). In the results reported in Panel B of Table 7, we find that, under the stronger tax enforcement of the SAT, the results of lower audit fees, shorter audit report lag, and fewer restatements are all concentrated in firms that appoint industry specialist auditors.

[Please insert Table 7 about here]

4.10 Robustness checks

We conduct two placebo tests to provide additional assurance that our observed results reflect the differences in tax enforcement severity between the tax collectors. First, we hypothetically move the policy cut-off time backward (and forward) by one year to January 2001 (January 2003) and conduct the same empirical analysis between firms registered in 2000 and 2001 (2002 and 2003). In Panels A1 and A2 of Table 8, we report the results from examining the impact of the falsified policy dates on audit outcomes, including audit fees, audit delay, financial restatements and tax-related restatements. Consistent with expectations, none of the coefficients on *SAT_PLACEBO1* or *SAT_PLACEBO2* are significant at conventional levels.

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Second, we re-run our tests on a group of firms for which the new tax collector arrangement is irrelevant: Shanghai firms and central state-owned firms. The collection and administration of income tax for Shanghai firms are handled by the Shanghai Municipal Tax Service, which acts as a combination of the SAT and LTB. Central state-owned firms are always subject to the SAT's monitoring. Consistent with expectations, the results in Panel A3 of Table 8 indicate that, for Shanghai firms and central state-owned firms registered in 2001 and 2002, their audit fees, audit report lags, and the propensity for financial restatements are not perceptibly affected during our estimation period. The results of these placebo tests help dispel the competing explanation that other economic conditions surrounding the policy cut-off time are spuriously responsible for our core results.

Third, we restrict the sample to firms that were newly registered closer to the policy cut-off date. Recall that in the main analysis, we set a 24-month window around the policy cut-off date and include new firms that registered during the window (and those became public between 2008 and 2015) as our sample firms. Although including firms that registered further away from the threshold has the benefit of yielding a larger sample, this may come at the expense of admitting bias because such firms are further away from the threshold. Additionally, including firms that registered further away from the two sides of the threshold will likely engender a larger difference in firm age, as well as other covariates that may grow with age such as firm size and the number of employees as reported in Panel B of Table 8. Using a narrower window will lower the probability of bias (Saez et al. 2012) and reduce the differences in firm-level characteristics that are associated with firm age between the two groups. We present the results in Panel B of Table 8 using (i) an 18-month window; and (ii) a 12-month window. Our results imply that the effects of more stringent tax enforcement on our main outcomes are highly robust, evident in the results improving in terms of both statistical

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significance and economic magnitude as the time window narrows. The results (untabulated) are similar after controlling for the date trend.

As another empirical strategy to confront this issue, we employ two matching methodologies to construct treatment (SAT) and control (LTB) samples that are comparable. First, we create a matched sample using propensity score matching (PSM). We begin by estimating the probability of being a SAT firm using a logit framework with our set of covariates and both year and industry fixed effects included. We follow Shipman et al. (2017) by compiling the matched sample using the 1:1 nearest neighbor matching technique without replacement and a caliper set at 0.03. The matched sample consists of 830 firms. In Table 8 Panel C1, we reassuringly report that the SAT firms and LTB firms are comparable across all covariates. The regression results using this PSM matched sample reinforce our earlier findings.

Second, we assemble another matched sample using the entropy balancing method. Consistent with Hainmueller (2012), we reweight the covariates of the LTB firms to reach covariant balance using the first moment adjustment. As shown in Panel C2 of Table 8, no perceptible differences in the means of the control variables remain between the two groups after applying the reweighting procedure. The regression results based on this matched sample are nearly identical to the evidence using the full sample and the PSM matched sample.

Finally, we extend the sample to 2005, the earliest year possible for the newly registered firms in 2002 to become publicly listed companies in China. We report the results in Table 8 Panel D. The coefficient on *SAT* enters negatively in the audit pricing (-0.048, *t*=-2.21), audit report delay (-0.008, *t*=-2.30), accounting restatements (-0.052, *t*=-2.59), and tax-related restatements (-0.058, *t*=-3.14) regressions, reflecting that our main evidence is materially insensitive to the sample construction. The results (untabulated) are almost identical when we add the date trend variables. We also find that our core results persist when we exclude firms

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that are cross-listed in Hong Kong given that its legal and regulatory institutions, including those governing auditor discipline (e.g., Ke et al. 2015), are far stronger than those in China.

[Please insert Table 8 about here]

4.11 Additional analysis: Audit report informativeness

In this section, we examine the importance of tax enforcement to the informativeness of auditor reporting decisions to provide triangulating evidence on our earlier results on audit quality using accounting and tax-related restatements. We focus on the informativeness of modified audit opinions in terms of the accuracy of the audit opinion in reflecting the client's financial condition given that deterioration in the informativeness of audit reports may indicate impaired auditor independence (e.g., Geiger and Rama 2003). We follow prior research by measuring the informativeness or accuracy of auditor reporting from an *ex post* perspective (e.g., Holder-Webb and Wilkins 2000; Carcello et al. 2009; Guan et al. 2016). This involves coding *INACCUR_MAO* equal to one if a modified audit opinion is issued but financial distress is not realized *ex post* or a modified audit opinion is not issued but financial distress is realized *ex post*, and zero otherwise. We identify the *ex post* realization of financial distress with whether a firm received a "Special Treatment" warning of delisting risk from the stock exchange or its stock was delisted two years after receiving the modified audit opinion (Guan et al. 2016).

The results from analyzing the role that tax enforcement plays in shaping the informativeness of auditor reporting are provided in Table 9. In Column (1), the coefficient on *SAT* is significantly negative (-0.949, *z*=-1.92), suggesting that auditor reporting becomes more informative when tax enforcement is stricter. In Column (2), however, this result becomes insignificant after we control for the date trend. Altogether, this evidence implies that the lower

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auditor effort expended under the tougher tax enforcement regime does not undermine the information value of audit reporting.

[Please insert Table 9 about here]

4.12 Additional analysis: Audit firm level efficiency—audit partner assignments

Our analysis so far implies that audit efficiency improves under stricter tax enforcement. However, another issue is whether the efficiency of resource allocation at the audit-firm level is sensitive to corporate tax enforcement. In providing initial evidence on this question, we focus on audit partners given that these highly-educated professionals are among audit firms' most valuable assets. Moreover, since the client-auditor assignment decision is determined by the audit firm (Ke et al. 2015), this assignment represents resource allocation decisions from the audit firm's perspective. We identify audit partners, manually collect their biographical information, and measure audit partner i's experience (*PARTNERS_EXPERIENCE*) relative to other partners within the audit firm using the approach taken by Ke et al. (2015).

We report in Table 10 the results from examining the role that tax enforcement plays in partner assignments. In Column (1), the coefficient on *SAT* is significantly negative (-2.705, *t*=-1.89), suggesting that client firms subject to SAT monitoring are assigned less-experienced audit partners. In Column (2), after we control for the date trend, *SAT* remains significantly negative (-6.738, *t*=-2.25). These results reconcile with our earlier evidence that tougher tax enforcement reduces audit fees because less-experienced audit partners charge lower per-hour rates (Lee et al. 2019). This evidence implies that, from the standpoint of the audit firm, the demand for auditing subsides when the client experiences stricter tax enforcement. Importantly, despite being assigned to less-experienced audit partners, clients administered by the SAT enjoy higher audit quality evident in the incidence of accounting misstatements and tax-related

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misstatements, as shown earlier. Collectively, our findings suggest that tax enforcement generates a positive externality to the audit market.

[Please insert Table 10 about here]

4.13 Additional analysis: Auditors' client portfolio management

As discussed earlier, DeFond and Zhang (2014) summarize four strategies auditors routinely apply in response to higher client risk and complexity: (i) exert more effort to improve audit quality; (ii) charge a higher fee premium; (3) manage their client portfolio; and (4) lobby for litigation relief. After analyzing the first two strategic responses, we next shed light on the third potential strategy; i.e., client portfolio management (e.g., Johnstone and Bedard 2004).[31]

Prior research suggests that auditors consider how prospective clients compare with each other and with the characteristics of their client portfolio in determining whether to accept a new client (Johnstone and Bedard 2004). Our earlier results suggest that auditors value cross-monitoring by the SAT. It follows that SAT firms are attractive clients according to auditors' screening process. We follow prior research on new client acceptance decisions by gauging whether SAT firms are more likely to become clients of Big Four auditors (Hsieh and Lin 2016). Empirically, we use continuing clients as the benchmark group because the potential new clients screened out by Big Four auditors are unobservable to researchers (Hsieh and Lin 2016). Specifically, we regress variables representing client and audit firms' characteristics (which include *BIGFOUR*, *SAT*, and their interaction, our variable of interest) on a variable indicating a newly accepted client firm-year. Our results (untabulated) show that the interaction term *SAT×BIGFOUR* is significantly positive (granted at only the 10% level), lending support that

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[31] As stressed earlier, the lax institutions governing investor protection in China suggest that litigation incentives play almost no role in disciplining auditors since lawsuits against auditors almost never succeed there (He et al. 2016).

SAT firms are more likely to become Big Four clients. This result holds when we re-specify large auditors as the domestic top ten according to market share, and control for clients' audit and financial risks (Hsieh and Lin 2016).[32] These findings imply that SAT firms are favored in the client-screening process for Big Four auditors, consistent with the Big Four weighing the tax authority identity as they actively manage their client portfolio (Johnstone and Bedard 2004).

Although the results from the new client acceptance analysis above are suggestive of auditors' client portfolio management, this evidence may be driven by SAT clients' selection of Big Four auditors (Lennox and Pittman 2010). To explore whether the results primarily reflect auditor screening of clients or client selection of their auditor, we examine in another test whether the frequencies of downgrades from a Big Four to a non-Big Four auditor (or upgrades from a non-Big Four to a Big Four) and lateral switches vary systematically between SAT and LTB firms (Lennox and Pittman 2010). This evidence (untabulated) implies that, compared to LTB firms, SAT firms are significantly less likely to downgrade from a Big Four to a non-Big Four auditor, implying that Big Four auditors possess superior screening and favor SAT clients. Concerning upgrades from non-Big Four to Big Four auditors, we do not find any perceptible difference between SAT and LTB firms, which is inconsistent with the argument that SAT clients select Big Four auditors.[33] Taken together, our results support that Big Four auditors consider the tax authority identity in relying on a portfolio approach to manage audit risk.

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[32] In a standard re-specification, we identify large auditors with whether they belong to the domestic top ten audit firms in China (e.g., Wang et al., 2008; Gul et al., 2013; He et al., 2016).

[33] Our sample includes 241 auditor switches, with 15 incidents of downgrades from a Big Four to a non-Big Four auditor, 168 incidents of lateral switches (i.e., from one Big Four auditor to another; or from one non-Big Four auditor to another), and 58 incidents of upgrades from non-Big Four to a Big Four. In our analysis, we compare upgrades and downgrades with lateral switches.

5. Conclusion

We examine the impact of stricter tax enforcement on auditors. Effective in 2002, China implemented a new tax collector arrangement such that firms registered in 2002 onwards are subject to the administration of the State Administration of Taxation (SAT), the national tax authority in China. Firms that registered before 2002 follow the old regime and remain under the administration of the Local Tax Bureau (LTB). This sharp regime shift offers a fertile quasi-experimental setting for identifying the effect of tax enforcement by comparing firms subject to the administration of the SAT (i.e., those that registered on or after 2002 under the new arrangement) and firms under the administration of the LTB (i.e., those that registered as of 2001 under the former arrangement).

We implement a research discontinuity design and compare auditor behavior for firms monitored by two tax authorities. Our results show that auditors exert less effort on clients monitored by the stricter tax authority (SAT) than they do for clients monitored by the more lenient one (LTB), as evident in lower audit fees and shorter audit report lags. Additionally, we document that firms monitored by the SAT are significantly less likely to experience financial or tax-related restatements, suggesting that the apparent decrease in effort does not come at the expense of audit quality. We also find that audit firms assign less-experienced partners to clients monitored by the tougher tax authority, implying that audit firms consider those clients as requiring less demand for auditing and optimize the matching of client and partner. Finally, we also find evidence suggestive of clients under SAT are viewed positively by Big Four auditors, consistent with Big Four auditors valuing the tax authority identity when implementing a portfolio approach to managing audit risk. Our paper offers novel and robust results on the spillover effects of tax enforcement on the audit market and audit efficiency.

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Appendix A: Variable Definitions

Variables	Definition	Sources
SAT	Indicator variable equal to one if the firm's income tax is administered by the State Administration of Taxation (i.e., firms registered in 2002) and zero if it is administered by the Local Taxation Bureau (i.e., registered in 2001).	CSMAR
Current_ETR	Current income tax expense divided by pre-tax income. Set as missing when the denominator is zero or negative; truncated at [0, 1].	CSMAR
Ind_Size_ETR	A firm's *Current_ETR* less the mean *Current_ETR* for the portfolio of firms in the same decile of total assets and the same industry.	CSMAR
FEE	Natural logarithm of total audit fees.	CSMAR
LAG	The number of days from a company's fiscal year-end to the date of the auditor's report, scaled by 365 days.	CSMAR
RESTATE_FINANCIAL	Indicator variable equal to one if the firm's earnings are restated or if the firm is sanctioned by regulatory bodies (e.g., China Securities Regulatory Commission, the Ministry of Finance, or their stock exchanges) due to accounting frauds or accounting irregularities during the sample period, and 0 otherwise (He et al. 2017).	CSMAR
RESTATE_TAX	Indicator variable equal to one if the firm's earnings are restated due to income tax issues.	CSMAR
SIZE	The natural logarithm of total assets.	CSMAR
LEV	Total liabilities divided by total assets.	CSMAR
ROA	Net income divided by total assets.	CSMAR
PT_ROA	Net income before income tax divided by total assets.	CSMAR
STAT_TAX_RATE	Statutory tax rate applied to the firm (may vary due to industry).	CSMAR
LOSS	Indicator variable equal to one if a firm's net income is below zero and zero otherwise.	CSMAR
PPE	Total fixed assets divided by total assets.	CSMAR
INTANG	Total intangible assets divided by total assets.	CSMAR
RECEIVABLE	Total receivables divided by total assets.	CSMAR

INVENTORY	Inventory divided by total assets.	CSMAR
R&D	Research and development expenditure divided by total assets.	CSMAR
BIGFOUR	Indicator variable equal to one if the audit firm is a "Big 4 firm" (i.e., one of the four major international audit firms) in the current year and zero otherwise.	CSMAR
SEGMENT	The natural logarithm of one plus the number of geographic segments.	CSMAR
RETURN	Annual market-adjusted stock returns during the fiscal year.	CSMAR
HSHARE	Indicator variable equal to one if the firm is also listed on the Hong Kong Stock Exchange and zero otherwise.	CSMAR
AGE	Natural logarithm of the difference between current year and IPO year plus one (+1).	CSMAR
EMPLOYEE	The square root of the number employees (in thousands).	CSMAR
ABSDA	Absolute discretionary accrual, discretionary accruals are obtained from cross-sectional estimations of the modified Jones model of accruals quality (Jones 1991; Dechow et al. 1995).	CSMAR
TAXAGG	Indicator variable equal to one if the firm's *Current_ETR* is in the lowest quintile and zero otherwise (Donohoe and Knechal 2014).	CSMAR
CFO	Net operation cash flow divided by total assets.	CSMAR
CURRENT_RATIO	Current asset divided by current liability.	CSMAR
SALES_GROWTH	$(Sales_t - Sales_{t-1})/ Sales_{t-1}$	CSMAR
ASSET_TO	Asset turnover, defined as total sales divided by total assets.	CSMAR
TACCRUAL	Total accruals, calculated as the difference between net income and net cash flow from operations scaled by sales.	CSMAR
MAO	Indicator variable equal to one if the auditor issues a modified report and zero otherwise.	CSMAR
INACCUR_MAO	Indicator variable equal to one if a modified audit opinion is issued but financial distress is not realized ex post, or a modified audit opinion is not issued but financial distress is realized ex post and zero otherwise. Ex post realization of financial distress is determined by whether a firm received a	CSMAR

	"Special Treatment" warning of delisting risk from the stock exchange or whether its stocks are delisted in year *t*+2 (e.g., Holder-Webb and Wilkins 2000; Carcello et al. 2009; Guan et al. 2016).	
PARTNERS_EXPERIENCE	Average relative experience (years) of the two partners who sign the audit report (Ke et al. 2015), defined as the mean value of *RELATIVE YEARS* for the two signing partners. (Note: Audit reports in China are signed by two audit partners.) *RELATIVE YEARS* is defined below.	CSMAR
RELATIVE YEARS	The experience (years) of audit partner *i* relative to all other partners in the same audit firm (Ke et al. 2015). For each partner, we count the number of years that the partner has audited at least one listed company up to the beginning of year t. The relative experience of audit partner *i* (*RELATIVE YEARS*) is defined as: $$100 - \frac{(-1 + RANK\ YEARS_{ijt})}{(-1 + N_{jt})} \times 100$$ where $RANK\ YEARS_{ijt}$ is partner *i*'s ranking in audit firm *j* in year *t*. N_{jt} is the total number of partners in audit firm *j* at year *t*. The value of $RANK\ YEARS_{ijt}$ is between 0 and 100, with higher values indicating that partner *i* has relatively more experience.	CSMAR
HIGH_SAT_ENFORCE	Indicator variable equal to one if the provincial SAT's median tax audit rate and penalty rate from 1999 to 2007 are greater than the LTB's and zero otherwise.	China Tax Audit Yearbook (Hand-collected)
INDUSTRY_EXPERT	Indicator variable equal to one if the audit firm is an expert in the client's industry, and zero otherwise. Following the approach of Audousset-Coulier et al. (2015), we identify an audit firm *i* as a specialist in industry *j* when audit firm *i*'s weighted market share is bigger than [(1/N)×1.2]×(1/K). Where weighted market share for audit firm *i* is equal to its audit fees in industry *j* divided by the sum of audit fees in industry *j* for all audit firms; N is the number of audit firms in industry *j*; K is the number of industries that audit firm *i* serves.	CSMAR
SAT_PLACEBO1	Indicator variable equal to one if the firm is	CSMAR

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	established in 2001 and zero if it is established in 2000.	
SAT_PLACEBO2	Indicator variable equal to one if the firm is established in 2003 and zero if it is established in 2002.	CSMAR
REG2002	Indicator variable equal to one if the firm is registered in Shanghai or central a SOE in 2002 and zero if it is registered in Shanghai or a central SOE in 2001.	CSMAR
TAX AUDIT	The number of tax audits conducted over the number of total tax returns filed.	China Tax Audit Yearbook (Hand-collected)
TAX PENALTY	The amount of penalties over the amount of tax deficiencies settled.	China Tax Audit Yearbook (Hand-collected)
TAX INSPECTOR/UNIT	The number of tax audit inspectors over the number of tax audit institutions.	China Tax Audit Yearbook (Hand-collected)
DEGREE INSPECTOR	The percentage of tax inspectors with a undergraduate degree or higher over the total number of tax inspectors.	China Tax Audit Yearbook (Hand-collected)
EXPERIENCED INSPECTOR	The number of experienced tax inspectors over the total number of tax inspectors. A tax inspector is considered experienced if she/he is older than 40 years old (Panel B) or 45 years old (Panel C).	China Tax Audit Yearbook (Hand-collected)

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Appendix B: Tax Enforcement and Corporate Tax Aggressiveness

Figure B1: Tax Aggressiveness around the Threshold
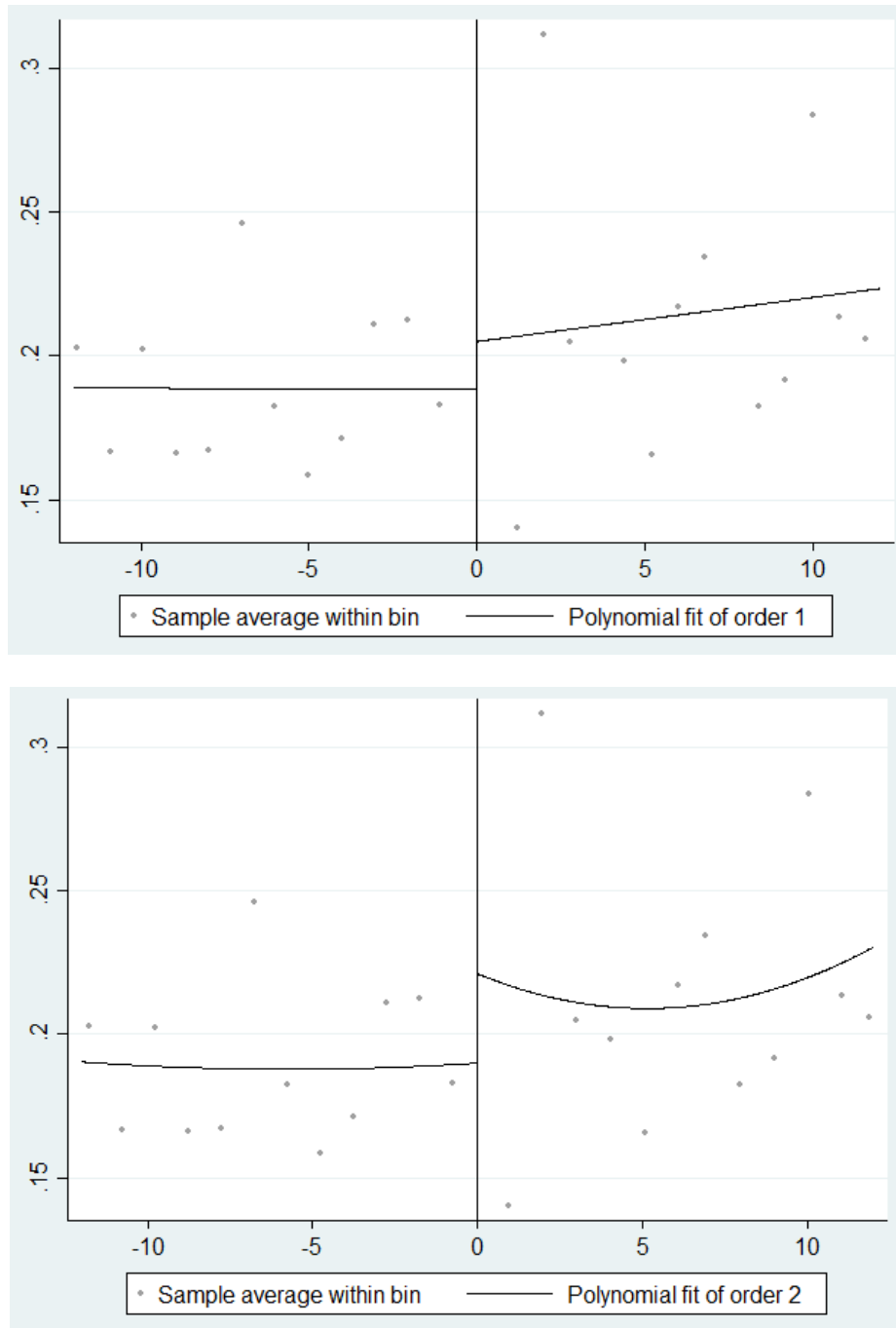
This figure shows regression discontinuity plots of firms' *Current_ETR* with linear (top graph) and quadratic (bottom graph) fits. The x-axis displays the month relative to the threshold date (January 1, 2002). Positive (negative) values indicate the firm's income tax is administered by SAT (LTB). The y-axis shows the mean effective tax rates in the respective month.

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APPENDIX B: Tax Enforcement and Corporate Tax Aggressiveness

Table B1: Tax Enforcement and Current Effective Tax Rates

	(1) *Current_ETR*	(2) *Current_ETR*	(3) *Ind_Size_ETR*
SAT	0.031***	0.022**	0.022**
	(3.02)	(2.04)	(2.14)
SIZE		0.007	0.007
		(0.93)	(0.97)
AGE		-0.004	-0.001
		(-0.39)	(-0.12)
PT_ROA		-0.850***	-0.711***
		(-6.50)	(-5.81)
LEV		0.052	0.051
		(1.32)	(1.46)
PPE		-0.083	-0.092*
		(-1.55)	(-1.94)
INTANG		-0.144	-0.137
		(-0.87)	(-0.88)
INVENTORY		0.036	0.029
		(0.57)	(0.50)
SEGMENT		0.009	0.012
		(1.12)	(1.52)
ABSDA		-0.160***	-0.164***
		(-2.60)	(-2.68)
STAT_TAX_RATE		0.485***	0.390***
		(3.53)	(3.13)
Fixed Effects	Industry + Year	Industry + Year	Industry + Year
N	1,157	1,067	1,067
Adjusted R²	0.037	0.136	0.086

This table reports OLS results of the effect of tax enforcement on corporate tax aggressiveness, measured by firms' current effective tax rates. *SAT* is an indicator equal to one if the firm's income tax is administered by the State Administration of Taxation (i.e., those registered in 2002) and zero if it is administered by the Local Taxation Bureau (i.e., registered in 2001). For brevity, the intercept and fixed effects are not tabulated. The *t*-statistics in parentheses are based on standard errors clustered at the firm-year level. The superscripts ***, **, and * indicate statistical significance at the 1%, 5% and 10% levels for two-tailed tests, respectively.

Appendix C: Identification Checks

Figure C1: Number of New Firm Registration around the Policy Cut-off Date (Bi-Monthly)
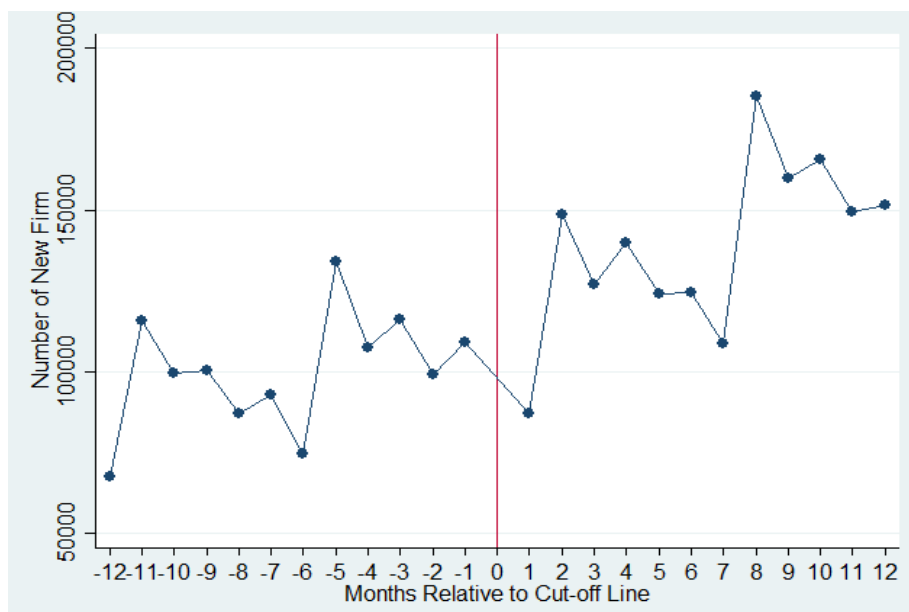
This figure displays the bi-monthly number of new firms registered by registration period in the sample of all Chinese firms (regardless of subsequent listing status) two years before and after the policy cut-off date (January 1, 2002) from January 1, 2000 to December 31, 2003. Note the large bi-monthly variations in the number of registrations due to seasonality within a calendar year across the 4-year period. The graph displays no sharp changes at the cut-off date, suggesting firms did not manipulate or control their registration date.

Table C1: Number of New Firm Registration around the Policy Cut-off Date (Bi-Monthly)

Sample Period	(1) 24-month Window 2001.01-2002.12	(2) 18-month Window 2001.04-2002.09	(3) 12-month Window 2001.07-2002.06
SAT	9199*	8283	6371
	(1.87)	(1.46)	(0.82)
Constant	53417***	55657***	54118***
	(15.39)	(13.89)	(9.82)
N (number of month)	24	18	12

This table displays the OLS coefficients from regressing the bi-monthly number of bi-monthly new firm registrations on our variable of interest, *SAT*, an indicator equal to one if the firm's income tax is administered by the State Administration of Taxation (i.e., those registered in 2002) and zero if it is administered by the Local Taxation Bureau (i.e., registered in 2001). Columns (1), (2), and (3) report the results using a 24-month, 18-month, and 12-month window, respectively, surrounding the policy cut-off date of 1/1/2012. The sample includes all Chinese firms, regardless of subsequent listing status.

Table 1: Sample Selection

Sample selection criteria for main analysis	Observations (Firm-year)
A-share listed companies established between 1/1/2001 and 31/12/2002 (one year before and after the policy cut-off date of 1/1/2002) that are covered in the China Stock Market and Accounting Research (CSMAR) database during our sample period from 2008 to 2015.	2,206
Less: State-owned enterprises (SOEs)	(556)
Less: Firms registered in Shanghai and Tibet	(99)
Less: Financial firms	(0)
Less: Firms with missing data for the main variables	(318)
Final sample	1,233

Table 2: Provincial-level Differences between the SAT and LTB

Panel A: Before the New Tax Collector Arrangement: 1999-2001

Variables	SAT			LTB			SAT - LTB	
	N	Mean	Median	N	Mean	Median	Mean Diff	Median Diff
TAX AUDIT (%)	72	16.773	12.000	69	10.894	7.000	5.878***	5.000**
TAX PENALTY (%)	68	15.382	12.000	68	10.426	11.000	4.956***	1.000**

TAX AUDIT is available in 2000 and 2001, and *PENALTY* is available in 1999 and 2001.

Panel B: After the New Tax Collector Arrangement and Before Our Sample Period: 2002-2007

Variables	SAT			LTB			SAT - LTB	
	N	Mean	Median	N	Mean	Median	Mean Diff	Median Diff
TAX AUDIT (%)	204	5.487	4.000	204	3.878	2.000	1.609**	2.000***
TAX PENALTY (%)	204	20.24	18.000	204	17.142	15.000	3.098***	3.000***
TAX INSPECTOR / UNIT	170	24.398	18.000	170	21.044	16.000	3.354*	2.000*
DEGREE INSPECTOR (%)	170	92.267	91.000	170	88.906	90.000	3.361	1.000
EXPERIENCED INSPECTOR (%)	95	45.083	45.000	92	38.141	38.000	6.942***	3.000***

TAX AUDIT and *PENALTY* are available from 2002 to 2007. *TAX INSPECTOR/UNIT* and *DEGREE INSPECTOR* are available from 2003 to 2007. *EXPERIENCED INSPECTOR* is available from 2005 to 2007.

Panel C: During Our Sample Period: 2008-2015

Variables	SAT			LTB			SAT - LTB	
	N	Mean	Median	N	Mean	Median	Mean Diff	Median Diff
TAX INSPECTOR / UNIT	272	22.718	16.000	272	19.867	16.000	2.851**	0.000
DEGREE INSPECTOR (%)	238	94.729	96.000	238	94.805	95.000	-0.076	1.000*
EXPERIENCED INSPECTOR (%)	269	47.743	47.000	268	39.336	40.000	8.407***	7.000***

All variables in Panel C are available from 2008 to 2015, except for *DEGREE INSPECTOR* (available until 2014). Other variables reported in Panels A and B (e.g., *TAX AUDIT*, *TAX PENALTY*) are not available from 2008.

Table 3: Descriptive Statistics

Panel A: Summary Statistics

Variable	N	Mean	Median	S.D.	P25	P75
FEE	1,233	13.388	13.305	0.605	13.017	13.71
LAG	1,233	0.252	0.252	0.059	0.304	0.219
RESTATE_FINANCIAL	1,233	0.131	0.000	0.338	0.000	0.000
RESTATE_TAX	1,233	0.054	0.000	0.369	0.000	0.000
INACCURATE_MAO	1,233	0.025	0.000	0.157	0.000	0.000
PARTNERS_EXPERIENCE	1,213	70.470	70.962	22.657	54.040	92.593
Current_ETR	1,157	0.199	0.167	0.151	0.132	0.233
Ind_Size_ETR	1,157	-0.013	-0.036	-0.083	0.019	0.142
SIZE	1,233	21.481	21.328	0.971	20.812	21.997
AGE (*logged*)	1,233	1.522	1.609	0.501	1.099	1.946
AGE (*year*)	1,233	5.173	5.000	2.530	3.000	7.000
LEV	1,233	0.354	0.352	0.176	0.205	0.492
ROA	1,233	0.047	0.046	0.048	0.020	0.075
LOSS	1,233	0.061	0.000	0.239	0.000	0.000
RECEIVABLE	1,233	0.149	0.128	0.097	0.076	0.205
INVENTORY	1,233	0.141	0.124	0.093	0.077	0.188
PPE	1,233	0.210	0.195	0.123	0.111	0.291
BIGFOUR	1,233	0.033	0.000	0.179	0.000	0.000
SEGMENT	1,233	1.378	1.099	0.610	1.099	1.946
RETURN	1,233	0.097	-0.035	0.547	-0.233	0.266
HSHARE	1,233	0.015	0.000	0.123	0.000	0.000
EMPLOYEE	1,233	1.511	1.275	1.008	0.928	1.785
MAO	1,233	0.022	0.000	0.146	0.000	0.000
ABSDA	1,233	0.075	0.058	0.066	0.026	0.103
TAXAGG	1,233	0.252	0.000	0.434	0.000	1.000
ASSET_TO	1,233	2.101	1.824	1.297	1.249	2.537
TACCRUAL	1,233	0.089	0.077	0.102	0.032	0.139

TABLE 3: Descriptive Statistics

Panel B: Differences between the State Authority of Taxation (SAT) and Local Tax Bureau (LTB)

Variables	SAT			LTB			SAT−LTB Differences in	
	N	Mean	Median	N	Mean	Median	Mean	Median
FEE	446	13.351	13.271	787	13.409	13.305	-0.058*	-0.034**
LAG	446	0.247	0.247	787	0.255	0.255	-0.007**	-0.008**
RESTATE_FINANCIAL	446	0.072	0.000	787	0.165	0.000	-0.093***	0.000***
RESTATE_TAX	446	0.013	0.000	787	0.078	0.000	-0.064***	0.000**
INACCURATE_MAO	446	0.018	0.000	787	0.029	0.000	-0.011	0.000
PARTNERS_EXPERIENCE	437	67.609	67.129	776	72.082	71.539	-4.473*	-4.410**
Current_ETR	422	0.216	0.170	735	0.189	0.165	0.028***	0.005**
Ind_Size_ETR	422	0.000	-0.026	735	-0.021	-0.036	0.021***	0.010*
SIZE	446	21.433	21.252	787	21.509	21.382	-0.076	-0.130**
AGE (*logged*)	446	1.418	1.386	787	1.581	1.609	-0.163***	-0.223***
AGE (*year*)	446	4.583	4.000	787	5.507	5.000	-0.924**	-1.000***
LEV	446	0.344	0.338	787	0.361	0.360	-0.017	-0.022
ROA	446	0.047	0.043	787	0.048	0.047	-0.001	-0.004
LOSS	446	0.054	0.000	787	0.065	0.000	-0.011	0.000
RECEIVABLE	446	0.157	0.132	787	0.144	0.125	0.014**	0.007
INVENTORY	446	0.142	0.122	787	0.141	0.125	0.002	-0.003
PPE	446	0.211	0.202	787	0.209	0.189	0.003	0.013
BIGFOUR	446	0.029	0.000	787	0.036	0.000	-0.006	0.000
SEGMENT	446	1.394	1.099	787	1.369	1.099	0.025	0.000
RETURN	446	0.097	-0.043	787	0.097	-0.034	0.000	-0.009
HSHARE	446	0.009	0.000	787	0.019	0.000	-0.010	0.000
EMPLOYEE	446	1.479	1.139	787	1.528	1.344	-0.049	-0.205***
MAO	446	0.011	0.000	787	0.028	0.000	-0.017*	0.000
ABSDA	446	0.078	0.064	787	0.072	0.055	0.006	0.009
TAXAGG	446	0.222	0.000	787	0.269	0.000	-0.047*	0.000
ASSET_TO	446	2.502	1.837	787	2.129	1.809	-0.077	-0.028
TACCRUAL	446	0.086	0.075	787	0.090	0.079	-0.004	-0.004

Figure 1: Audit Fees around the Threshold
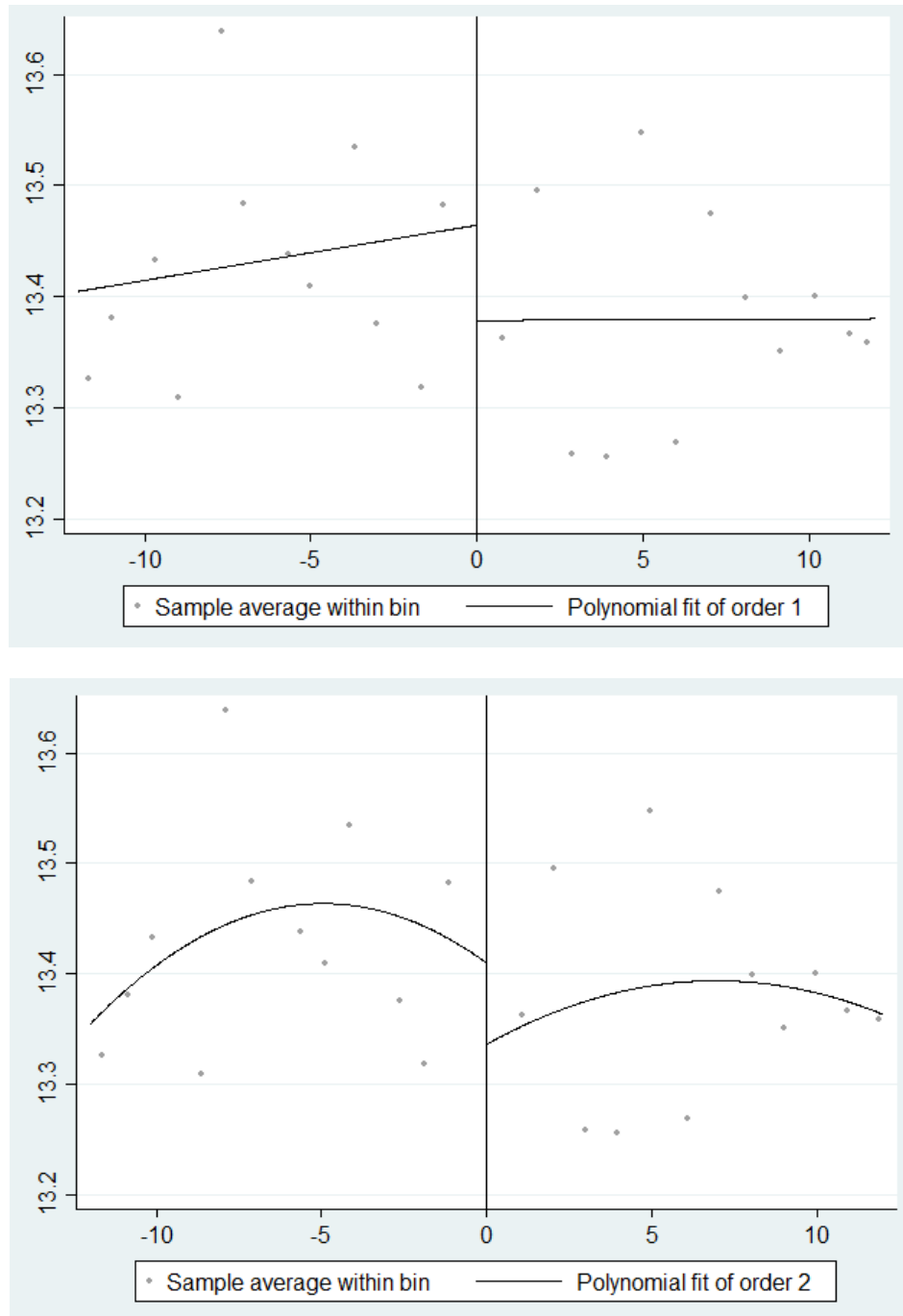
This figure shows regression discontinuity plots with linear (top graph) and quadratic (bottom graph) fits. The x-axis displays the month relative to the threshold date (January 1, 2002). Positive (negative) values indicate the firm's income tax is administered by SAT (LTB). The y-axis shows the mean effective tax rates in the respective month.

Table 4: Tax Enforcement and Audit Fees

Panel A: Covariate around Threshold

	(1) SIZE	(2) LEV	(3) ROA	(4) LOSS	(5) RECEIVABLE
SAT	-0.101	-0.015	-0.003	0.001	0.014
	(-0.84)	(-0.70)	(-0.50)	(0.05)	(1.24)
Fixed Effects			Industry + Year		
N	1,233	1,233	1,233	1,233	1,233
Adjusted R^2	0.155	0.093	0.040	0.018	0.125

	(6) INVENTORY	(7) PPE	(8) BIGFOUR	(9) SEGMENT
SAT	0.009	-0.005	-0.000	0.023
	(0.84)	(-0.40)	(-0.02)	(0.35)
Fixed Effects		Industry + Year		
N	1,233	1,233	1,233	1,233
Adjusted R^2	0.150	0.205	0.054	0.061

	(10) RETURN	(11) HSHARE	(12) AGE	(13) EMPLOYEE
SAT	0.001	-0.015	-0.199***	-0.016
	(0.02)	(-0.92)	(-3.80)	(-0.11)
Fixed Effects		Industry + Year		
N	1,233	1,233	1,233	1,233
Adjusted R^2	0.096	-0.004	0.196	0.059

This panel reports test results of whether the control variables used in the regression analysis are balanced around the threshold. For brevity, the intercept and fixed effects are not tabulated. The t-statistics in parentheses are based on standard errors clustered at the firm-year level. The superscripts ***, **, and * indicate statistical significance at the 1%, 5% and 10% levels for two-tailed tests, respectively.

Table 4: Tax Enforcement and Audit Fees

Panel B: Regression on Tax Enforcement on Audit Fees

	(1) FEE	(2) FEE	(3) FEE
SAT	-0.084**	-0.051**	-0.159***
	(-2.45)	(-2.13)	(-3.59)
SIZE		0.367***	0.372***
		(15.71)	(15.80)
AGE		-0.017	-0.016
		(-0.59)	(-0.54)
LEV		-0.122	-0.129
		(-1.17)	(-1.23)
ROA		0.422	0.440
		(1.17)	(1.21)
LOSS		0.574***	0.577***
		(5.82)	(5.76)
RECEIVABLE		-0.035	-0.017
		(-0.26)	(-0.13)
INVENTORY		0.283**	0.304**
		(2.16)	(2.32)
PPE		0.008	0.001
		(0.07)	(0.01)
BIGFOUR		0.692***	0.693***
		(9.46)	(9.59)
SEGMENT		0.022	0.027
		(1.13)	(1.39)
RETURN		0.040	0.039
		(1.57)	(1.54)
HSHARE		-0.354**	-0.356**
		(-2.35)	(-2.36)
EMPLOYEE		0.082***	0.082***
		(4.40)	(4.39)
MAO		0.015	0.030
		(0.29)	(0.56)
ABSDA		-0.148	-0.154
		(-0.83)	(-0.86)
TAXAGG		-0.017	-0.015
		(-0.64)	(-0.58)
Linear Date Trend	No	No	Yes
Fixed Effects	Industry + Year	Industry + Year	Industry + Year
N	1,233	1,153	1,153
Adjusted R^2	0.154	0.642	0.643

This panel reports results of the effect of tax enforcement on corporate audit fees using OLS. *SAT* is an indicator equal to one if the firm's income tax is administered by the State Administration of Taxation (i.e., those registered in 2002) and zero if it is administered by the Local Taxation Bureau (i.e., registered in 2001). For brevity, the intercept and fixed effects are not tabulated. The *t*-statistics in parentheses are based on standard errors clustered at the firm-year level. The superscripts ***, **, and * indicate statistical significance at the 1%, 5% and 10% levels for two-tailed tests, respectively.

Table 5: Tax Enforcement and Audit Report Lag

	(1) LAG	(2) LAG
SAT	-0.008**	-0.014*
	(-2.21)	(-1.73)
SIZE	0.005	0.008**
	(1.49)	(2.21)
LEV	0.008	-0.018
	(0.55)	(-1.13)
ROA	-0.142**	-0.145**
	(-2.54)	(-2.48)
LOSS	-0.003	0.002
	(-0.33)	(0.22)
RECEIVABLE	-0.015	0.036
	(-0.72)	(1.45)
INVENTORY	0.012	0.003
	(0.58)	(0.11)
PPE	-0.071***	-0.060***
	(-4.02)	(-3.10)
BIGFOUR	0.003	-0.013
	(0.38)	(-1.36)
SEGMENT	0.005*	0.005
	(1.91)	(1.63)
RETURN	-0.010***	-0.009***
	(-2.81)	(-2.62)
ANALYST	-0.005***	-0.004**
	(-2.73)	(-2.16)
EMPLOYEE	0.001	0.001
	(0.54)	(0.40)
CFO	-0.015	-0.017
	(-0.52)	(-0.57)
HSHARE	-0.033***	-0.033***
	(-3.78)	(-3.36)
MAO	0.009	0.012
	(0.67)	(0.91)
Linear Date Trend	No	Yes
Fixed Effects	Industry + Year	Industry + Year
N	1,233	1,233
Adjusted R^2	0.080	0.126

This table reports results of the effect of tax enforcement on audit report lag using OLS. *SAT* is an indicator equal to one if the firm's income tax is administered by the State Administration of Taxation (i.e., those registered in 2002) and zero if it is administered by the Local Taxation Bureau (i.e., registered in 2001). The *t*-statistics in parentheses are based on standard errors clustered at the firm-year level. The superscripts ***, **, and * indicate statistical significance at the 1%, 5% and 10% levels for two-tailed tests, respectively.

Table 6: Tax Enforcement and Restatements

	(1) RESTATE_ FINANCIAL	(2) RESTATE_ FINANCIAL	(3) RESTATE_ TAX	(4) RESTATE_ TAX
SAT	-0.077***	-0.219***	-0.057***	-0.128**
	(-4.09)	(-6.88)	(-3.11)	(-2.46)
SIZE	0.014	0.026	0.034*	0.037*
	(0.74)	(1.33)	(1.91)	(1.94)
AGE	0.034	0.022	0.012	0.009
	(1.58)	(1.02)	(0.53)	(0.40)
LEV	0.283	0.257	0.294*	0.293
	(1.39)	(1.31)	(1.66)	(1.64)
ROA	0.346	0.309	0.881	0.897
	(0.66)	(0.60)	(1.57)	(1.62)
LOSS	0.099*	0.102*	0.067	0.070
	(1.65)	(1.75)	(1.27)	(1.32)
RECEIVABLE	0.016	0.065	-0.544***	-0.540***
	(0.13)	(0.53)	(-3.44)	(-3.59)
INVENTORY	-0.238*	-0.171	-0.168	-0.162
	(-1.87)	(-1.35)	(-1.46)	(-1.47)
PPE	-0.002	-0.009	-0.455***	-0.461***
	(-0.02)	(-0.09)	(-4.07)	(-4.08)
SEGMENT	-0.003	0.004	-0.019	-0.017
	(-0.20)	(0.22)	(-1.28)	(-1.16)
RETURN	-0.001	-0.002	-0.023	-0.022
	(-0.06)	(-0.10)	(-1.55)	(-1.53)
HSHARE	0.108	0.093	0.529***	0.519**
	(0.74)	(0.71)	(2.59)	(2.51)
EMPLOYEE	-0.025*	-0.031**	-0.061***	-0.061***
	(-1.77)	(-2.28)	(-3.53)	(-3.45)
MKT	-0.002	-0.004**	-0.012***	-0.013***
	(-1.27)	(-2.00)	(-3.43)	(-3.28)
BIGFOUR	0.011	0.018	0.012	0.016
	(0.12)	(0.20)	(0.19)	(0.26)
CFO	-0.190	-0.185	0.298*	0.299*
	(-1.03)	(-0.99)	(1.75)	(1.75)
LAG	-0.017	-0.020	0.238	0.254
	(-0.11)	(-0.13)	(1.47)	(1.57)
ASSET_TO	-0.002	-0.005	-0.029***	-0.029***
	(-0.18)	(-0.41)	(-3.76)	(-3.61)
TACCRUAL	-0.356	-0.336	-0.174	-0.177
	(-1.35)	(-1.32)	(-0.96)	(-0.97)
Linear Date Trend	No	Yes	No	Yes
Fixed Effects	Industry + Year	Industry + Year	Industry + Year	Industry + Year
N	1,233	1,233	1,233	1,233

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Adjusted R²	0.054	0.077	0.108	0.109

This table reports results of the effect of tax enforcement on financial or tax-related restatements using a linear probability model. *SAT* is an indicator equal to one if the firm's income tax is administered by the State Administration of Taxation (i.e., those registered in 2002) and zero if it is administered by the Local Taxation Bureau (i.e., registered in 2001). For brevity, the intercept and fixed effects are not tabulated. The *t*-statistics in parentheses are based on standard errors clustered at the firm-year level. The superscripts ***, **, and * indicate statistical significance at the 1%, 5% and 10% levels for two-tailed tests, respectively.

Table 7: Cross-sectional Analyses

Panel A: Local SAT Office Tax Enforcement Strength

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	FEE		LAG		RESTATE_FINANCIAL		RESTATE_TAX	
SAT	0.049	-0.080	-0.001	0.006	-0.012	-0.164***	0.046*	0.014
	(1.15)	(-1.37)	(-0.11)	(0.71)	(-0.41)	(-3.41)	(1.87)	(0.38)
HIGH_SAT_ENFORCE	-0.159***	-0.162***	0.004	0.004	0.039	0.029	0.113***	0.113***
	(-5.69)	(-5.83)	(0.95)	(0.91)	(1.36)	(1.05)	(3.35)	(3.45)
SAT×HIGH_SAT_ENFORCE	-0.105**	-0.101**	-0.012*	-0.011	-0.090**	-0.082**	-0.150***	-0.150***
	(-2.11)	(-2.08)	(-1.71)	(-1.48)	(-2.28)	(-2.14)	(-4.15)	(-4.22)
Control Variables				Included				
Linear Date Trend	No	Yes	No	Yes	No	Yes	No	Yes
Fixed Effects				Industry + Year				
N	1,233	1,233	1,233	1,233	1,233	1,233	1,233	1,233
Adjusted R²	0.658	0.661	0.080	0.082	0. 201	0.239	0. 091	0.091

This panel reports results of the interaction effect of tax enforcement at the local office level and SAT enforcement on audit fees, audit report lag, and tax-related restatements using OLS. *HIGH_SAT_ENFORCE* is an indicator equal to one if the local SAT office's median tax audit rate and penalty rate from 1999 to 2007 are greater than the local LTB office's and zero otherwise. *SAT* is an indicator equal to one if the firm's income tax is administered by the State Administration of Taxation (i.e., those registered in 2002) and zero if it is administered by the Local Taxation Bureau (i.e., registered in 2001). The *t*-statistics in parentheses are based on standard errors clustered at the firm-year level. The superscripts ***, **, and * indicate statistical significance at the 1%, 5% and 10% levels for two-tailed tests, respectively.

TABLE 7: Cross-sectional Analyses

Panel B: Industry Expert Auditors

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	FEE		*LAG*		*RESTATE_FINANCIAL*		*RESTATE_TAX*	
SAT	0.029	-0.081	0.004	0.009	0.031	-0.012	0.028	0.006
	(0.65)	(-1.58)	(0.62)	(1.01)	(1.23)	(-0.32)	(1.14)	(0.25)
INDUSTRY_EXPERT	0.096***	0.091***	0.009	0.009*	0.083***	0.081***	0.028	0.027
	(3.03)	(2.87)	(1.63)	(1.70)	(3.39)	(3.30)	(1.58)	(1.55)
SAT×INDUSTRY_EXPERT	-0.107**	-0.125**	-0.016**	-0.014*	-0.112***	-0.123***	-0.052**	-0.054**
	(-2.12)	(-2.40)	(-2.03)	(-1.73)	(-3.64)	(-3.73)	(-2.02)	(-2.01)
Control Variables				Included				
Linear Date Trend	No	Yes	No	Yes	No	Yes	No	Yes
Fixed Effects				Industry + Year				
N	1,233	1,233	1,233	1,233	1,233	1,233	1,233	1,233
Adjusted R²	0.638	0.640	0.082	0.083	0.092	0.092	0.012	0.013

This panel reports results of the interaction effect of audit firm and SAT enforcement on audit fees, audit report lag, and tax-related restatements using OLS. *INDUSTRY_EXPERT* is an indicator equal to one if the audit firm is an expert in the client's industry, and zero otherwise. We identify industry audit specialist following the approach of Audousset-Coulier et al. (2015). Please refer to Appendix A for detailed definitions. *SAT* is an indicator equal to one if the firm's income tax is administered by the State Administration of Taxation (i.e., those registered in 2002) and zero if it is administered by the Local Taxation Bureau (i.e., registered in 2001). The *t*-statistics in parentheses are based on standard errors clustered at the firm-year level. The superscripts ***, **, and * indicate statistical significance at the 1%, 5% and 10% levels for two-tailed tests, respectively.

Table 8: Robustness Checks

Panel A1: Placebo Test—Assumed Policy One Year Ahead

| | (1) | (2) | (3) | (4) |
	FEE	LAG	RESTATE_ FINANCIAL	RESTATE_ TAX
SAT_PLACEBO1	0.055	0.001	0.047	0.061
	(1.35)	(0.38)	(0.79)	(0.84)
Control Variables		Included		
Fixed Effects		Industry + Year		
N	1411	1411	1411	1411
Adjusted R²	0.615	0.090	0.068	0.094

This panel reports the placebo test results of the effect of tax enforcement on our interested variables using a hypothetical policy year of 2001, or one year before the actual policy year.

Panel A2: Placebo Test—Assumed Policy One Year After

| | (1) | (2) | (3) | (4) |
	FEE	LAG	RESTATE_ FINANCIAL	RESTATE_ TAX
SAT_PLACEBO2	0.012	0.004	0.104	-0.008
	(0.49)	(0.79)	(0.26)	(-1.34)
Control Variables		Included		
Fixed Effects		Industry + Year		
N	790	790	790	790
Adjusted R²	0.585	0.061	0.065	0.098

This panel reports the placebo test results of the effect of tax enforcement on our interested variables using a hypothetical policy year of 2003, or one year after the actual policy year.

Panel A3 Placebo Test—Unaffected Firms

| | (1) | (2) | (3) |
	FEE	LAG	RESTATE_ FINANCIAL
SAT	-0.096	0.003	-0.009
	(-0.46)	(0.34)	(-0.64)
Control Variables		Included	
Fixed Effects		Industry + Year	
N	281	281	281
Adjusted R²	0.815	0.144	0.359

This panel reports the placebo test results of the effect of tax enforcement on our interested variables using firms that were not affected by the policy (i.e., firms registered in Shanghai or central SOEs). Note that there is no tax-related restatement (RESTATE_TAX) in this unaffected firm sample.

Table 8: Robustness Checks

Panel B: Sensitivity Test—Narrowed Time Window around the Threshold

	(1)	(2)	(3)	(4)
		18-Month Window		
		(4/2001–9/2002)		
	FEE	*LAG*	*RESTATE_ FINANCIAL*	*RESTATE_ TAX*
SAT	-0.096***	-0.009**	-0.136***	-0.075***
	(-3.42)	(-2.02)	(-6.27)	(-2.71)
Control Variables		Included		
Fixed Effects		Industry + Year		
N	916	916	916	916
Adjusted R²	0.659	0.071	0.104	0.103

	(1)	(2)	(3)	(4)
		12-Month Window		
		(7/2001–6/2002)		
	FEE	*LAG*	*RESTATE_ FINANCIAL*	*RESTATE_ TAX*
SAT	-0.142***	-0.013**	-0.204***	-0.132***
	(-4.90)	(-2.36)	(-7.43)	(-2.76)
Control Variables		Included		
Fixed Effects		Industry + Year		
N	624	624	624	624
Adjusted R²	0.686	0.131	0.133	0.144

This panel reports the results of the effect of tax enforcement using narrower windows around the threshold, focusing on firms that were newly registered closer to the policy cut-off date. In the main analysis, a 24-month window (1/2001–12/2002) is used.

Table 8: Robustness Checks

Panel C1: Matched Sample using Propensity Score Matching (PSM)

Covariate balance after PSM

Control Variables	SAT		LTB		SAT – LTB
	N	Mean	N	Mean	Mean Diff
SIZE	415	21.394	415	21.433	-0.039
LEV	415	0.327	415	0.343	-0.017
ROA	415	0.050	415	0.045	0.005
LOSS	415	0.048	415	0.058	-0.010
RECEIVABLE	415	0.158	415	0.157	0.001
INVENTORY	415	0.136	415	0.140	-0.005
PPE	415	0.209	415	0.212	-0.003
BIGFOUR	415	0.022	415	0.022	0.000
SEGMENT	415	1.397	415	1.391	0.006
RETURN	415	0.105	415	0.096	0.009
HSHARE	415	0.005	415	0.010	-0.005
EMPLOYEE	415	1.409	415	1.469	-0.060
AGE	415	1.415	415	1.434	-0.020

Regression results

	(1) FEE	(2) LAG	(3) RESTATE_ FINANCIAL	(4) RESTATE_ TAX
SAT	-0.059**	-0.008**	-0.072***	-0.042**
	(-2.43)	(-2.04)	(-3.34)	(-2.23)
Control Variables		Included		
Fixed Effects		Industry + Year		
N	830	830	830	830
Adjusted R²	0.557	0.079	0.080	0.045

This panel reports the results of matched sample analysis for the effect of tax enforcement on our interested variables by using PSM. We first use a logit regression to estimate the probability of being a SAT firm. The variables we include in the logit regression are covariate below, year and industry fixed effects. We then create the matched sample using the 1:1 nearest neighbor matching technique without replacement and a caliper set at 0.03 following Shipman et al. (2017).

Table 8: Robustness Checks

Panel C2: Matched Sample using Entropy Balance Matching

Covariate balance after Entropy Balancing Matching

Control Variables	SAT		LTB		SAT – LTB
	Mean	Variance	Mean	Variance	Mean Diff
SIZE	21.430	0.845	21.430	0.867	0.000
LEV	0.344	0.032	0.344	0.029	0.000
ROA	0.047	0.002	0.047	0.002	0.000
LOSS	0.054	0.051	0.054	0.051	0.000
RECEIVABLE	0.157	0.010	0.157	0.010	0.000
INVENTORY	0.143	0.009	0.143	0.008	0.000
PPE	0.212	0.013	0.212	0.017	0.000
BIGFOUR	0.029	0.028	0.029	0.028	0.000
SEGMENT	1.394	0.331	1.394	0.404	0.000
RETURN	0.097	0.281	0.097	0.303	0.000
HSHARE	0.009	0.009	0.009	0.009	0.000
EMPLOYEE	1.479	1.364	1.479	0.771	0.000
AGE	1.418	0.214	1.418	0.268	0.000

Regression results

	(1)	(2)	(3)	(4)
	FEE	LAG	RESTATE_ FINANCIAL	RESTATE_ TAX
SAT	-0.058**	-0.009**	-0.068***	-0.057***
	(-2.51)	(-2.53)	(-3.60)	(-3.47)
Control Variables		Included		
Fixed Effects		Industry + Year		
N	1,233	1,233	1,233	1,233
R^2	0.627	0.104	0.098	0.098

This panel reports the results of Entropy Balancing matched sample analysis for the effect of tax enforcement on our interested variables. Control firms (LTB) are reweighted to achieve the covariant balance (the first moment adjustment) via Entropy Balancing following Hainmueller (2012).

Table 8: Robustness Checks

Panel D: Sensitivity Test—Extended Sample Period

	(1) *FEE*	(2) *LAG*	(3) *RESTATE_ FINANCIAL*	(4) *RESTATE_ TAX*
SAT	-0.048**	-0.008**	-0.052***	-0.058***
	(-2.11)	(-2.30)	(-2.59)	(-3.14)
Control Variables		Included		
Fixed Effects		Industry + Year		
N	1,309	1,309	1,309	1,309
Adjusted R²	0.650	0.085	0.057	0.106

This panel reports the results of the effect of tax enforcement on our interested variables using an extended sample that begins in 2005, the earliest year possible for the newly registered firms in 2002 to become publicly listed companies.

Table 9 Additional Analyses: Tax Enforcement and Audit Reporting

	(1) INACCUR_MAO	(2) INACCUR_MAO
SAT	-0.949*	0.508
	(-1.92)	(0.46)
SIZE	0.472	0.482
	(1.27)	(1.30)
LEV	-0.837	-0.503
	(-0.25)	(-0.14)
ROA	-7.926	-6.939
	(-1.07)	(-0.84)
LOSS	1.236	1.328
	(1.27)	(1.28)
RECEIVABLE	-10.752***	-11.552***
	(-3.55)	(-3.89)
INVENTORY	0.869	1.048
	(0.30)	(0.42)
PPE	-0.543	-0.060
	(-0.32)	(-0.04)
SEGMENT	-0.720*	-0.677*
	(-1.93)	(-1.89)
RETURN	0.588*	0.647*
	(1.73)	(1.90)
HSHARE	-0.992	0.161
	(-0.58)	(0.09)
ANALST	-0.843***	-0.868***
	(-3.61)	(-3.87)
EMPLOYEE	-0.683	-0.633
	(-0.89)	(-0.93)
MKT	-0.038	-0.004
	(-1.12)	(-0.10)
BIGFOUR	2.083	1.786
	(1.05)	(0.84)
CFO	-4.044	-3.734
	(-1.19)	(-1.17)
LAG	4.349	4.429
	(0.77)	(0.82)
Linear Date Trend	No	Yes
Fixed Effects	Industry + Year	Industry + Year
N	1,128	1,128
Pseudo R²	0.304	0.329

This table reports results of the effect of tax enforcement on the accuracy of audit opinion using logit estimation. *SAT* is an indicator equal to one if the firm's income tax is administered by the State Administration of Taxation (i.e., those registered in 2002) and zero if it is administered by the Local Taxation Bureau (i.e., registered in 2001). The *t*-statistics in parentheses are based on standard errors clustered at the firm-year level. The superscripts ***, **, and * indicate statistical significance at the 1%, 5% and 10% levels for two-tailed tests, respectively.

Table 10 Additional Analyses: Tax Enforcement and Audit Partners' Assignments

	(1) *PARTNERS_* *EXPERIENCE*	(2) *PARTNERS_* *EXPERIENCE*
SAT	-2.705*	-6.738**
	(-1.89)	(-2.25)
SIZE	1.777	1.885
	(1.35)	(1.43)
LEV	-7.669	-7.830
	(-1.38)	(-1.39)
ROA	66.337***	67.197***
	(3.28)	(3.31)
LOSS	5.629*	5.961*
	(1.80)	(1.90)
RECEIVABLE	4.186	3.268
	(0.50)	(0.39)
INVENTORY	7.088	6.029
	(0.87)	(0.73)
PPE	1.817	1.046
	(0.26)	(0.15)
BIG FOUR	-12.901***	-12.650***
	(-3.69)	(-3.64)
SEGMENT	-1.424	-1.290
	(-1.31)	(-1.18)
RETURN	0.413	0.401
	(0.34)	(0.33)
HSHARE	1.358	0.728
	(0.31)	(0.17)
ANALYST	-1.201	-1.292*
	(-1.56)	(-1.68)
EMPLOYEE	0.858	0.976
	(0.98)	(1.12)
Linear Date Trend	No	Yes
Fixed Effects	Industry + Year	Industry + Year
N	1,213	1,213
Adjusted R²	0.055	0.058

This table reports the results of the effect of tax enforcement on auditor partners' assignment using OLS. *SAT* is an indicator equal to one if the firm's income tax is administered by the State Administration of Taxation (i.e., those registered in 2002) and zero if it is administered by the Local Taxation Bureau (i.e., registered in 2001). The *t*-statistics in parentheses are based on standard errors clustered at the firm-year level. The superscripts ***, **, and * indicate statistical significance at the 1%, 5% and 10% levels for two-tailed tests, respectively.