Tax Enforcement Externalities and the Banking Sector

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April 2018

Preliminary – Please do not circulate without permission

*Corresponding author. We thank Kathleen Andries, John Barrios, Merle Erickson, Jochen Hundsdoerfer, Max Müller, Anya Kleymenova, and participants at Erasmus University Rotterdam, Tilburg University, WHU, and the 2018 German Academic Association for Business Research Tax Section Annual Meeting for helpful comments. We are grateful to Andrew Belnap for providing data on corporate headquarters. John Gallemore gratefully acknowledges the support of the Charles E. Merrill Faculty Research Fund and the Accounting Research Center at the University of Chicago Booth School of Business.

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Abstract

Governments around the world are considering increasing corporate tax enforcement efforts to mitigate base erosion and improve revenue. Whether such enforcement efforts have externalities is not well known. In this study, we examine whether corporate tax enforcement can affect banks via their corporate lending practices. Specifically, we hypothesize that tax enforcement efforts aimed at small-and-medium sized enterprises (SME) can improve the information environment of these firms, which in turn could lead to better commercial lending decisions and greater loan growth. Exploiting the regional structure employed by the IRS between 1992 and 2000, we find that the corporate tax audit probability for SMEs is associated with greater performance, loan quality, and commercial lending growth for regionally-focused banks. We find similar evidence when exploiting the IRS reorganization from a regional to federal-based system in 2000 as an exogenous change to tax enforcement at the region level, and our findings are robust to various specifications and placebo analyses. Overall, our findings show that the tax authority's mandate has important externalities on the banking sector via the latter's commercial lending practices, and suggest that the benefits to tax enforcement go beyond simply improving tax collections.

Keywords: tax authority, tax enforcement, bank performance, bank lending

JEL Classification Codes: G21, G28, H23, H25, M41

1. Introduction

Faced with slow growth and increasing budget deficits in recent years, countries have increasingly focused on reducing tax avoidance by corporations (OECD 2015). While policymakers have considered multiple remedies, such as withholding rules and information sharing, direct enforcement efforts such as auditing remain "crucial" in combating aggressive tax avoidance (International Monetary Fund 2015). The direct benefit of corporate tax enforcement is improved compliance and greater revenues, which comes both directly from those audited and indirectly from non-audited tax payers. Policymakers generally weigh these benefits against the costs of increased tax enforcement, which includes additional manpower for the taxing authority and compliance burden on the part of the taxpayers (International Monetary Fund 2015). Whether tax enforcement efforts could have other, unintended consequences on real activities is not well known, but understanding them is critical in assessing the overall net benefit of tax enforcement.

In this study, we examine whether tax enforcement via auditing has externalities on banks, a sector which is likely to be of interest to policymakers and academics given its role in promoting economic growth through the provision of capital to businesses (e.g., Demirgüç-Kunt and Maksimovic (1998, 1999); Levine and Zervos (1998); Levine (2003)). In particular, the banking sector is a primary source of capital for small-and-medium sized corporations for which other capital markets (e.g., public bond or equity markets) are too costly (Berger, Klapper, and Udell 2001).

We hypothesize that corporate tax enforcement efforts can have positive effects on banks' commercial lending activities. Specifically, we build on the theory in Desai, Dyck, and Zingales (2007) and Guedhami and Pittman (2008), which suggest that the tax authority is a source of corporate governance, especially as it pertains to the firm's information environment. Banks rely on both hard and soft information in assessing a borrower's creditworthiness (e.g., Berger, Miller, Petersen, Rajan, and Stein (2005); Liberti and Petersen (2017)). Important sources of hard information used in the lending process include corporate tax returns (Berger, Minnis, and Sutherland 2017). Furthermore, the information in tax returns can be used to validate information provided via other sources (e.g., financial

statements). Consistent with this notion, prior research finds that greater tax enforcement leads to overall improvements in the corporate information environment, e.g., less negative information hoarding (Bauer, Fang, and Pittman 2017) and greater financial reporting quality (Hanlon, Hoopes, and Shroff 2014). Therefore, tax enforcement can lead to improved quality of the information used by banks in assessing creditworthiness of existing and potential borrowers, which in turn should lead to better lending decisions. Therefore, we predict that banks whose existing and potential borrowers are subject to greater tax enforcement should exhibit greater profitability and loan quality.

We predict that tax enforcement can also affect the overall amount of bank lending to commercial borrowers. As discussed above, tax enforcement can lead to increased quality of information used in the lending process. Furthermore, this increase in information quality may allow banks to reallocate resources from information verification to collecting new information on additional potential borrowers. Finally, the overall quality of the potential borrower set could be increased by the corporate governance role of the tax authority. Desai and Dharmapala (2006) suggest that tax avoidance and managerial diversion may be complementary activities, and Desai et al. (2007) and Mironov (2013) find that the tax enforcer can improve corporate governance, and therefore firm performance and value, by reducing tax avoidance. Since resource diversion represents a transfer of value from outsiders (such as lenders) to insiders, mitigating such behavior can improve the quality of potential borrowers. In summary, greater tax enforcement could induce greater bank lending to corporate borrowers by improving both the information inputs into the lending process and the quality of the potential borrower set.

Alternatively, there are reasons to expect that corporate tax enforcement may have no effect or even a negative effect on banks' commercial lending. Tax enforcement is likely less effective in improving corporate information environments relative to other sources of oversight, such as auditing provided by financial accounting firms. This is because the purpose of tax enforcement is to ensure compliance with the tax code, not the credibility of financial information. This suggests that tax enforcement may have little effect on the quality of information used in the bank lending process. Furthermore, greater tax enforcement leads to less tax planning and greater payments to the tax authorities

(Hoopes, Mescall, and Pittman 2012), therefore reducing the after-tax cash flows and equity of the bank's existing and potential borrowers. If these reductions affect borrowers' ability to repay loans, stricter tax enforcement could lead to worse bank performance. Therefore, the effect of corporate tax enforcement on bank lending is an empirical question.

To study the effect of tax authorities on the banking sector, we exploit the fact that in the United States from 1992 to 2000, the IRS employed a district-based structure for tax enforcement purposes. We collect district-level corporate tax return audit probabilities, obtained from Syracuse University's Transactional Records Access Clearinghouse (TRAC). These audit probabilities vary both across IRS districts and within an IRS district across time and size classes. Exploiting this within-country variation mitigates many of the endogeneity concerns that come with cross-countries studies, such as country-level tax enforcement being correlated with bank regulation or tax law provisions. Using bank regulatory filings, we are able to isolate banks that have all of their operations (e.g., branches) within a given IRS district. By examining these regionally-focused banks, we restrict our sample to banks whose corporate clients are primarily local businesses, allowing us to identify the IRS audit probabilities that are likely to affect the bank's existing and potential borrowers.[1]

Our identification strategy is based on the assumption that district-level IRS probabilities are not under the purview of individual banks or borrowers. However, the IRS may target certain regions due to economic conditions or corporate characteristics (e.g., performance or tax avoidance), which could also affect bank performance. To address these concerns, we take several steps. First, we include bank-level fixed effects that account for time-invariant latent characteristics of the bank or IRS district. Second, we include various time-varying controls that capture time-varying state-level macroeconomic conditions and

[1] We assume that banks with concentrated operations are more likely to focus their commercial lending to local businesses. This assumption is seemingly corroborated by prior research that shows that smaller banks (e.g., those with geographically-concentrated operations) are better suited to lend to local businesses (Berger et al. 2001; Berger et al. 2005). An alternative way to link banks' lending to regions is through the Community Reinvestment Act data. However, this is only available for banks with $250 million or greater in assets during our sample period. Approximately 88 percent of our bank-quarters fall below this asset threshold. Furthermore, this data is only available after 1996; our sample begins in 1992. Therefore, we use the branch data to determine regionally-focused banks.

the performance and tax planning of the publicly traded corporations in those states, certain state-level fixed effects that account for slower moving state-level latent characteristics (e.g., regulation), and year-quarter fixed effects to capture U.S.-wide macroeconomic conditions. In additional specifications, we include either state-year fixed effects or IRS-district year fixed effects (when examining within-district variation in the effect of tax enforcement on banks). Finally, we conduct several placebo tests and exploit the IRS Restructuring and Reform Act of 1998 as a plausibly exogenous shock to district-level tax enforcement efforts.

In our tests, we focus on the tax enforcement efforts aimed at small-and-mid-sized corporations (between $10 and $100 million in assets) for several reasons. First, small-and-mid-sized firms are more likely to be privately held and exhibit less coverage from analysts and other possibly monitors. In contrast, larger corporations generally have multiple sources of corporate governance, suggesting that the incremental effects of governance sourced from the tax authority are likely to be lower. Second, small-and-medium sized enterprises are more likely to be funded by bank debt from local banks (Berger et al. 2001; Berger et al. 2005). In addition to being better able to access equity and public bond markets, larger firms are able to source bank capital from a larger set of financial institutions, suggesting that they are not constrained to using local banks. Third, during our sample period, the tax enforcement aimed at small-and-medium-sized corporations exhibits meaningful variation across districts. The average audit percentage of firms with assets between $10 million and $100 million is about 18 percent, but varies widely: from 3.6 percent (Houston 2000) to 55.9 percent (Michigan 1992). Furthermore, this audit percentage changes frequently over time. In contrast, the largest corporations (e.g., those with more than $250 million in assets) exhibit audit percentages often close to 100 percent, whereas micro-sized corporations (e.g., those with fewer than $1 million in assets) are essentially never audited.

First, we explore how tax enforcement affects bank performance, which we measure using interest income minus the loan loss provision. We find a strong positive association between IRS audit probabilities and bank performance in that district. Our result is also economically significant; an increase in the audit probability from the 25^{th} to the 75^{th} percentile is associated with an increase in bank

performance by 1 percent of the sample average. Considering that commercial lending only makes up 17 percent of loans for the average bank, the effect is sizable.[2] To further connect tax enforcement and improved lending choices, we examine loan loss provisioning, which is the bank's estimate of loan quality. We find that IRS enforcement leads to lower loan loss provisions, consistent with tax enforcement leading to improved bank performance via improved commercial lending decisions.

Second, we examine whether greater tax enforcement leads to increased commercial lending. We find empirical evidence for this prediction: an increase in the audit probability from the 25th to the 75th percentile is associated with an increase in commercial lending growth by 10 percent of the sample average growth rate in commercial lending. This finding is consistent with tax enforcement efforts improving the quality of the information environment quality of the potential borrower set. We further show that the effect of tax enforcement on commercial loan growth in concentrated in banks with lower leverage; that is, in banks that have the necessary capital capacity for additional commercial loans.

While IRS tax enforcement efforts are out of the purview of individual banks and borrowers, it could be that the IRS specifically targeted certain districts exhibiting strong economic growth. Therefore, our findings could be driven by local economic conditions rather than the tax enforcement efforts themselves. We conduct several additional tests to address this concern. First, we find no association between corporate tax enforcement and performance, provisioning, or commercial loan growth in banks with little to no exposure to commercial lending. Second, we show that corporate tax enforcement is not associated with growth in consumer lending (e.g., non-mortgage lending to individuals). Third, while we find a strong association between audit efforts aimed at small-and-medium-sized firms and various bank outcomes, we find no association between audit efforts aimed at either micro-firms or large firms.[3] The overall findings from these placebo analyses are inconsistent with local economic conditions driving the

[2] Ideally, we would isolate the interest income, interest expense, and loan loss provision related only to commercial loans. However, this is not possible during our sample period due to data constraints.

[3] While micro-firms are also likely to source capital from local banks, the audit percentages are very low for these banks (often essentially zero); therefore, the threat of audit may not be sufficiently large enough to provide any governance benefits. The non-finding for large firms is consistent with these firms being less likely to source capital from local banks and/or tax authority-sourced governance being less important for these firms.

association between audit rates and bank outcomes documented in our primary analyses, providing additional assurance that we are capturing the causal effect of tax enforcement efforts aimed at local corporations on bank lending decisions.

As an alternative identification strategy, we exploit the Internal Revenue Service Restructuring and Reform Act (IRSRRA) of 1998 that replaced the district organization with a federal, size-based tax audit assignment starting in late 2000. Using the pre-IRSRRA district-level audit probabilities and the post-IRSRRA size-based audit probabilities, we create an audit probability change measure at the district level. We then examine whether the shift in audit probabilities around this reorganization is associated with bank loan quality and commercial loan growth. The identification assumption is that the reorganization and subsequent federal-level asset-based audit probabilities are not driven by the banks or borrowers in any particular IRS district, and therefore these district-level changes in audit probability are plausibly exogenous.[4] Using a difference-in-difference design, we find that the shift in audit probability around the IRS reorganization is strongly predictive of bank performance and loan growth. Specifically, banks in districts that experienced a small reduction in audit probabilities exhibit greater performance, better loan quality, and higher growth in commercial lending in the post-period relative to banks in districts experiencing large reductions in audit probabilities. Importantly, none of these outcomes differ between treatment and control banks in the pre-period, supporting the parallel trends assumption. Overall, these findings confirm those from our primarily analyses that tax enforcement is positively associated with bank performance, loan quality, and growth in commercial lending.

Finally, we conduct several additional tests to support and extend our primary findings. First, to further support the information environment channel, we examine whether tax enforcement allows banks to better predict future loan losses. Employing a variant of the design from Beck and Narayanamoorthy (2013), we find that loan loss provisions are indeed more informative of future loan losses (i.e., charge-offs) when tax enforcement is greater. Second, we fail to find an association between aggregate bank

[4] Public discontent with individuals' treatment by IRS employees led to the IRSRRA, not corporate tax outcomes or auditing in certain districts, supporting our assumption (Hoffman 2013).

performance, size, loan quality, or commercial lending in an IRS district with the current or future level and change in audit percentages in that district. This non-finding is inconsistent with the alternative explanation that the corporate tax enforcement efforts are being driven by the observable bank outcomes which we examine in this paper. Finally, we fail to find a significant association between enforcement and bank effective tax rates, suggesting that tax enforcement only has an indirect impact via corporate lending for our regionally-focused banks.[5]

Our study is related to several broad strands of literature. First, we contribute to recent research on the effects of tax enforcement, which has as primarily focused on the direct effect of tax enforcement on the firm itself, either through its tax avoidance (Gupta and Lynch 2016; Hoopes et al. 2012; Mironov 2013), performance and output (Almunia and Lopez-Rodriguez 2018; Mironov 2013), or the information environment (Guedhami and Pittman 2008; El Ghoul, Guedhami, and Pittman 2011; Hanlon et al. 2014; Bauer et al. 2017; Haw, Hu, Hwang, and Wu 2004). We add to this literature by documenting the spillover effects of corporate tax enforcement onto the banking sector, a key source of corporate capital.

While banks benefit from other sources of improvements to borrower information environment (e.g., auditing), we are the first to document the tax authority as a potential benefit. In contrast to auditors or bank regulators, whose mandates are to improve the corporate information environment and bank stability, respectively, the objective of the tax authority is to collect tax revenues and to ensure compliance with the tax code. Our findings suggest that any assessment of the benefits and costs of corporate tax enforcement should include the positive externalities it has on banks via commercial lending. While our setting can be viewed as a laboratory for exploring tax enforcement externalities, the banking sector itself is an important because of banks' critical role in providing capital for individuals and corporations, and because prior research suggests that bank stability affects the overall economy (Bernanke 1983; Keeley 1990; Calomiris and Mason 2003a, 2003b, 1997).

[5] One potential explanation for this non-finding is that our sample banks, which are operating locally, have limited tax avoidance opportunities that can be challenged by the IRS.

We also contribute to the literature on financial institutions by providing new empirical evidence on how taxation affects banks. We believe this to be a critical question, as the performance and stability of the banking sector has important implications for the overall economy. Prior research documents how taxation can negatively impact the banking sector by directly encouraging higher leverage through interest deductibility (Keen and de Mooij 2012; de Mooij, Keen, and Orihara 2013). In contrast, we examine the indirect effects of tax enforcement on the banks' existing and potential borrowers, and find that this enforcement has a positive effect on bank lending decisions. Furthermore, prior research has documented how various bank regulators with similar mandates can differentially affect banks (Agarwal, Lucca, Seru, and Trebbi 2014; Costello, Granja, and Weber 2016). In contrast, our findings suggest that government agencies with very different mandates can have important effects on the banking sector.

Finally, our findings speak to the literature on bank financial reporting, and loan loss provisions in particular. Prior studies in this area document the role of auditors (Nicoletti 2017), regulators (Costello et al. 2016; Beck and Narayanamoorthy 2013), and credit registries (Balakrishnan and Ertan 2017) in determining loan loss provision quality. Most closely related to our study is Andries, Gallemore, and Jacob (2017), who show that bank loan loss provision choices are shaped by the design of the corporate tax system. In contrast, we show that provisions can also be shaped by tax enforcement through improved borrower information environments. Given that prior research suggests that bank financial reporting and provisioning in particular shapes regulatory (Gallemore 2017) and non-regulatory stakeholder (Bushman and Williams 2012) oversight of banks, our findings suggest that tax enforcement can have important externalities on the monitoring and risk discipline of banks.

2. Conceptual framework

2.1. Prior research on the effect of tax enforcement on firms

In contrast to the personal income tax on wages, interest income, and dividends, which is typically subject to third-party reporting, the true income of businesses can only be observed through costly tax audits. This makes corporate tax compliance and enforcement more complex (Kleven, Knudsen, Kreiner, Pedersen, and Saez 2011). The tax authority's enforcement mechanisms include actual

on-site tax audits, increased use of withholding taxes, or third party reporting (International Monetary Fund 2015). Prior research suggests that such enforcement efforts are effective at improving tax revenues (Gupta and Lynch 2016) and reducing corporate tax avoidance (Hoopes et al. 2012).

Furthermore, Desai et al. (2007) posit that the government, through its tax-related claim on profits, plays an important governance role in corporations. They argue that because there is overlap between the actions that divert resources to controlling shareholders and those that reduce tax liabilities, the actions of the tax authorities (i.e., the rules or enforcement of those rules) impacts the relationship between insiders and outsiders, which can in turn affect corporate value and performance. Building on this framework, Mironov (2013) documents that income diversion is associated with lower firm performance, and that greater tax enforcement leads to improved firm performance by reducing income diversion. Relatedly, Desai and Dharmapala (2009) find that tax avoidance is only value increasing when coupled with greater corporate governance, consistent with complementarities between tax avoidance and resource diversion in poorly governed firms. Other studies show that the monitoring role of the tax authority affects firms' cost of accessing the public debt and equity markets (Guedhami and Pittman 2008; El Ghoul et al. 2011).

One particular channel through which the tax authority's governance role can affect firms is through the latter's information environment. Managers may attempt to mask aggressive tax avoidance through obfuscating financial reports. Although the tax authority is not directly interested in improving the quality of the corporate information environment, it may do so indirectly by attempting to reduce the incidence and amount of aggressive tax avoidance. In this vein, Hanlon et al. (2014) show that as tax enforcement is negatively associated with discretionary accruals, a measure of financial reporting opacity. Furthermore, Bauer et al. (2017) finds that stricter tax enforcement reduce negative-news hoarding by firms, reducing their stock price crash risk. The findings from both studies are consistent with tax enforcement being positively associated with the quality of the corporate information environment.

2.2. How tax enforcement can affect banks via lending to corporate borrowers

Based on the preceding discussion, we hypothesize that tax enforcement can positively affect banks through the latter's commercial lending activities. Banks rely critically on high quality information to assess the creditworthiness of potential borrowers. Potential sources of that information include tax returns, the quality of which can be directly affected by corporate tax enforcement efforts. Furthermore, tax returns can be used to validate alternative sources of information, such as financial statements. Therefore, corporate tax enforcement can lead to reduced information asymmetry between the bank and the borrower, which in turns can lead to improved lending decisions and bank profitability.[6] Tax enforcement can also improve the creditworthiness of the borrowers themselves by improving their financial performance (Mironov 2013).

In addition to improving lending decisions, tax enforcement may encourage greater lending to commercial borrowers through several channels. First, improved lending decisions should lead to greater bank profitability and equity. Due to regulatory capital requirements, bank lending requires sufficient equity. Therefore, the increases in bank profitability due to improved lending decisions will increase the bank's lending capacity. Second, by acting as a form of assurance for various sources of hard information (e.g., directly affecting tax returns, indirectly affecting financial reports), tax enforcement efforts potentially allow banks to reallocate resources towards acquiring information (either hard or soft) on additional potential borrowers. Third, by improving the creditworthiness of borrowers, greater tax enforcement efforts can increase the set of potential borrowers that meet the bank's required risk profile. Therefore, tax enforcement may be positively associated with lending to corporate borrowers.

Alternatively, tax enforcement may have no effect or even a negative effect on banks. First, there are other forms of monitoring that may be superior to corporate tax authorities, and therefore the incremental monitoring provided by the tax authority may have little effect. In particular, audits

[6] It is important to note that this can occur even though the IRS is primarily concerned about understated income, whereas the bank may be primarily concerned with overstated income. By reducing the amount of noise and/or bias in the tax return, tax enforcement can improve the usefulness of the tax return as a direct signal of performance or its ability to verify other performance measures. Finally, tax enforcement lowers the likelihood and amount of tunneling (diverting income from stakeholders via tax avoidance), which will lead to reported performance being a more accurate measure of economic performance and increase the amount of cash available to repay bank loans in the future.

performed by financial accounting firms are specifically designed to improve the corporate information environment. In contrast to the positive assurance provided by accounting firms, tax authorities are only tasked with ensuring that the tax returns comply with the code. Second, firms' ability to obfuscate aggressive tax avoidance may outstrip the ability of the tax authority to uncover the tax avoidance, rendering the latter's monitoring efforts ineffective. Third, by affecting firms' ability to engage in tax planning, tax enforcement can reduce the after-tax cash flows of existing and potential borrowers, which can negatively impact their ability to repay bank debt, leading to lower bank performance. Taken together, it is ultimately an empirical question whether tax enforcement has a positive or negative effect on banks via their corporate lending practices.

3. Institutional Background, Research Design, and Data

3.1. Setting: IRS district-based auditing from 1992 to 2000 aimed at small-and-medium sized firms

From 1992 to 2000, the IRS employed a regional district structure (see Table 1 for a list of these districts).[7] Some of these districts spanned either a single state or multiple states, whereas four states (California, Florida, New York, and Texas) contained multiple districts. Each district office is responsible for directing the corporate tax return auditing efforts for the firms headquartered in their region. The result of this decentralized approach to corporate tax enforcement is that there is substantial variation in the audit percentages across districts, within a district across size categories, and within a district-size category across time. We exploit this variation in our empirical analyses.[8]

We focus on the probability of a face-to-face IRS audit as our measure of tax enforcement. These audits are conducted either at the taxpayer's headquarters or at an IRS office (Guedhami and Pittman 2008). We believe that this measure closely captures the construct of the enforcement of existing tax laws. A higher probability of a face-to-face audit with IRS officials is likely to have two effects on tax payers.

[7] Initially, there were 63 separate districts, which were combined into 33 districts starting in 1995. The TRAC data only covers the 33 districts from 1992 to 2000, combining the audit rates under the pre-1995 district structure into the 33-district structure. We therefore use the 33-district structure in our analyses.

[8] The IRS reports its auditing efforts in eight different size categories: (1) less than $250,000 assets, (2) $250,000 to $1 million assets, (3) $1 million to $5 million assets, (4) $5 million to $10 million assets, (5) $10 million to $50 million assets, (6) $50 million to $100 million assets, (7) $100 million to $250 million assets, and (8) $250 million assets or more.

First, the actual occurrence an audit is likely to be associated with greater detection and remedying of aggressive avoidance and accompanying obfuscation of financial reports designed to hide such avoidance. Second, there could be a deterrence effect, as managers rationally adjust their level of tax aggressiveness and financial reporting obfuscation in accordance with the expected likelihood of a face-to-face audit. This assumption is supported by the fact that the corporate tax enforcement data we employ is well-known by accounting and finance professionals, and therefore internal firm tax professionals are likely to be aware of the likelihood of IRS audit probabilities associated with their district and size class. This assumption is further grounded in the findings of a contemporaneous association of observed audit percentages and corporate tax avoidance (Hoopes et al. 2012) or information environment (Guedhami and Pittman 2008; El Ghoul et al. 2011; Hanlon et al. 2014; Bauer et al. 2017; Haw et al. 2004).

In contrast, the interpretation of other possible measures of tax enforcement is not as clear. For example, a large amount of penalties in a particular district could indicate that enforcement is greater, or even that firms perceive the enforcement to be sufficiently weak that they engage in greater tax avoidance (and therefore on average incur more penalties).[9] Furthermore, the amount of penalties may be driven by a few large cases, rather than being representative of the average firm in that district.

In our analyses, we focus on the tax enforcement efforts aimed at small-and-medium-sized corporations (those between $10 and $100 million in assets), for several reasons. First, the smallest size groups (e.g., those with fewer than $10 million in assets) generally exhibit very low audit probabilities (around 4 percent on average), suggesting that the threat of a tax audit is unlikely to generate any improvements in firm governance or information environments. On the other hand, the larger size groups exhibit more substantial audit rates. For tax enforcement to generate substantially improved corporate information environments, the threat of an audit needs to be material. Second, relative to the largest firms, small-and-medium-sized firms are likely to be affected by the incremental corporate governance supplied by the tax authority. The largest firms already have multiple non-tax authority monitors (e.g., accounting

[9] Consistent with higher audit percentages being associated with lower average penalties (because firms are more compliant to begin with), we find that the correlation between audit rate and average penalty is -0.068 (but insignificant) for small-and-medium-sized firms during our sample period.

firms, analysts, pension funds). Hence, tax enforcement is likely to have less of an impact on the information environment of the largest firms. Finally, small-and-medium sized firms are more likely to use bank debt as a primary source of capital. Larger firms are better able to access alternative sources of capital, such as public bond and equity markets. Furthermore, larger firms are able to source bank debt from banks that are geographically distant (Berger et al. 2005). For these reasons, we employ the audit percentages for small-and-medium-sized firms as our primary measure of tax enforcement.

Furthermore, we focus on a subset of U.S. commercial banks that operate only in one IRS district during our sample period. By restricting the sample to banks that operate only in a single IRS district, we are better able to measure the IRS audit probability that affects the bank's existing and potential local borrowers. This is because the corporate borrowers of our smaller, regionally-focused banks are likely to be local in nature (Berger et al. 2001; Berger et al. 2005). We measure geographic focus using the bank's branching operations, which is motivated by the fact that bank deposit-taking and lending often overlap the same geographic areas.

3.2. Identification strategy overview

Our identification strategy relies on the assumption that the IRS audit percentages in each district-year are exogenous to the banks in those districts. Given that these audit probabilities affect all corporations in a district, it seems reasonable to assume that banks are not directly responsible for the level of enforcement chosen. However, there could be other factors that jointly affect the IRS tax enforcement choice and bank performance in the district. Prior research offers conflicting reasons for the district-level variation in IRS audit rates. Some suggest that is the variation across districts is due to inefficient allocation of resources (Beron, Tauchen, and Witte 1992), while others suggest that it is efficiently allocated based on predicted regional variation in tax evasion (Slemrod and Yitzhaki 2002). Neither of these reasons suggests that bank-level outcomes (e.g., performance, loan quality, or loan growth) are directly responsible for district-level variation in audit rates. However, if greater firm performance is associated with greater corporate tax enforcement, than the IRS may choose to target areas where economic conditions are favorable, assuming that firms in these areas will be more apt to engage in

tax avoidance. Where economic conditions are generally more favorable, banks are likely to perform better, to have improved loan quality, and to lend more. In our regression design, we include a multitude of different region-level control variables and fixed effects to account for such factors, which we explain in the following section. Furthermore, we exploit the IRS reorganization in the year 2000 as a plausible exogenous shock to tax enforcement at the district level. Finally, we explore potential determinants of IRS district-level audit rates in section 5.

3.3. Regression design

To test whether tax enforcement affects various bank outcomes, we estimate the following model:

$$\begin{aligned} \textit{Bank Outcome}_{i,q} = {} & \beta_0 + \beta_1 \textit{Percent Audited}_{d,t} + \Sigma_j \beta_j \textit{Bank Controls}_{i,q-4} + \Sigma_k \beta_k \textit{State Controls}_{s,q-4} \\ & + \textit{Bank Fixed Effects} + \textit{Time Fixed Effects} + \varepsilon_{i,t} \end{aligned} \quad (1)$$

Our dependent variable is *Bank Outcome*, which is one of three variables (all variables are defined in Appendix A). *Interest Income*, captures overall bank performance related to loans, and is defined as the interest income minus loan loss provisions, scaled by lagged total assets of bank *i* in quarter *q* of year *t*. We use the bank's loan loss provision as a measure of loan portfolio quality. *LLP* is defined as the ratio of current loan loss provisions to lagged gross loans. Higher (lower) loan loss provisions are consistent with a lower (higher) quality loan portfolio and therefore worse (better) lending decisions. Finally, we use the annual growth in commercial loans (*Commercial Growth*) defined as the change in commercial loans from year *t* to *t+1*, scaled by current commercial loans. The unit of observation is bank-quarter when employing *Interest Income* or *LLP* as the dependent variable, and bank-year when employing *Commercial Growth*.[10]

Our main independent variable is *Percent Audited Medium*, defined as the corporate tax return audit probability in district *d* conducted in federal fiscal year *t* for returns for the calendar year *t-1*. We define *Percent Audited Medium* as the audit percentage of corporations with assets between \$10m and \$100m, our definition of small-and-medium sized firms. If a higher audit probability leads to better

[10] We use bank–year instead of bank–quarter observations when examining commercial loan growth to mitigate quarter-to-quarter fluctuations in commercial lending that might not capture a bank's strategic choice to grow or shrink its commercial lending portfolio.

lending decisions and greater loan growth, we expect that β_1 will be positive (negative) when *Interest Income* and *Commercial Growth* (*LLP*) are the dependent variables.

We include two bank level control variables: *Size* is defined as the natural logarithm of total assets (in million USD), and *Leverage Ratio* is defined as the ratio of total debt over total assets (as a proxy for regulatory capital). Both of these variables are measured with a one year lag relative to *Bank Outcome*. When examining commercial lending growth, we also control for growth in interest revenues from *t-1* to *t* to account for a bank's growth opportunities.

Since audit probability may not be randomly assigned across IRS districts, it is important to account for regional factors that may be responsible for variation in enforcement. We thus include eight state-level characteristics (*State Controls*) to control for the economic environment in the respective state. As firm-level controls, state controls are lagged by one year. First, we include four variables based on the observable characteristics of the publicly traded non-bank corporations that are headquartered in that state. Specifically, we control for the ratio of total profits to sum of total assets, sum of cash holdings to total assets, the sum of capital expenditures to total assets as well as the average one-year cash effective tax rate in state *s* in a given year. Second, we include four state-level macroeconomic control variables: the unemployment rate in quarter *q*, the change in the unemployment rate from *q-1* to *q*, the housing price index in quarter *q*, and the change in the housing price index from *q-1* to *q*.

Furthermore, we include different types of fixed effects. To account for the latent time-invariant characteristics at the bank and IRS district level that may drive either bank profitability or tax enforcement, we include bank fixed effects. We include year-quarter fixed effects that account for any federal-level factors (e.g., macroeconomic conditions) affecting bank performance or tax enforcement. Finally, we account for state-level latent factors using two different fixed effect approaches. First, we include state-cycle fixed effects, where each state receives a different fixed effect for each two-year period (e.g., 1992-93, 1994-95, etc.). These state-cycle fixed effects account for slower moving state-level latent characteristics such as banking regulation and other institutional characteristics. The purpose behind using state-cycle, rather than state-year fixed effects is to allow all districts, several of which span

either a state or multiple states, to contribute to the identification of the *Percent Audited Medium* coefficient. Alternatively, we include state-year-quarter fixed effects, which control for the economic conditions and institutional characteristics (e.g., bank regulation) that impact all banks in a state-year-quarter equally. In these specifications, the coefficient of *Percent Audited Medium* is only identified by the states that have multiple IRS districts (California, Florida, New York, and Texas). Given the inclusion of these control variables and fixed effects, for an alternative factor to explain our findings, it would need to be correlated with IRS audit probabilities and bank outcomes within a district-year, but not be correlated with state-level macroeconomic conditions or the performance or tax planning of the publicly-traded corporations within that state.

3.4. Data sources, sample, and descriptive statistics

We employ corporate tax return audit rate information from the Transactional Records Access Clearinghouse (TRAC), a non-partisan research institute associated with Syracuse University. TRAC collects this data directly from the IRS as the result of a FOIA request and resulting court order. The validity of the TRAC data is supported by its use by the IRS itself (through its Oversight Board), important media agencies, and in prior research (e.g., Guedhami and Pittman (2008); El Ghoul et al. (2011); Hanlon et al. (2014); Bauer et al. (2017)).

In Figure 1, we present a histogram of the frequency of substantial increases and decreases in the audit percentages of small-and-medium-sized firms. We define a change as substantial if the audit probability changes by two (five) percentage points in Panel A (Panel B) of Figure 1. In each year, more than half of the IRS districts experience a change in the audit probability of at least two percentage points. Figure 1 also indicates that audit rates exhibit both increases and decreases, although we observe more decreases than increases in later years. Taken together, Figure 1 indicates that audit rates change frequently in both directions and these changes are not clustered in time.

We obtain bank regulatory filing data from the Federal Reserve Bank of Chicago. Banks are required to report income statements, balance sheets, and other data with the Federal Reserve each quarter via call reports. We obtain branch-level data from the FDIC's Summary of Deposits dataset. We obtain

state-level housing price data from the Federal Housing Finance Agency, and unemployment data from the Bureau of Labor Statistics. To create our state-level corporate control variables, we employ data from Compustat and SEC EDGAR.

For bank-quarter observations to be included in our final sample, we require that they report non-zero, non-negative assets, total capital, and total loans. Further, we only include banks for which we have information on the geographic location of their branches. Our final sample comprises 331,030 quarterly observations from 11,763 banks. Table 1 presents the number of unique banks and bank-quarter observations by district.

Table 2 presents summary statistics for our main sample. Continuous variables are winsorized at the 1st and 99th percentiles; effective tax rates are winsorized at 0 and 1.[11] The average bank has quarterly has an interest income of 1.02 percent relative to lagged total assets. The average bank size in our sample is USD 228 million with a debt to total assets ratio of about 90 percent. About 1.2 percent of all loans are non-performing loans in a given quarter. In each quarter, the average bank reports loan loss provisions of 0.1 percent of total gross loans. Commercial loans grow, on average, by 13.9 percent annually. Similarly, real estate and consumer lending (*Other Loans*) grow by about 14.5 percent. As mentioned above, the average audit probability for small-and-medium-sized firms is about 18 percent.

4. Main results

4.1. Effect of tax enforcement on overall performance

Table 3 presents regression results from estimating equation (1) using the quarterly call report data.[12] In Column (1), we include state-level control variables (not reported), bank fixed effects, year-quarter fixed effects, and state-cycle fixed effects. Column (2) replaces the state-cycle fixed effects with state-year-quarter fixed effects; the identification of the coefficient on *Percent Audited Medium* in this column comes from the four states with multiple districts. In both columns, we find empirical support for

[11] The exception is *Commercial Growth*, which we winsorized at the bottom 1% level (=-71 percent growth) as well as at 100 percent growth rate at the top.

[12] Statistical inferences are based on standard errors clustered at the IRS district level. In Table A.1, we document the robustness of our inferences to alternative clustering choices.

the prediction that stricter tax enforcement of small-and-medium-sized corporations is positively associated with bank performance. In economic terms, an interquartile increase in the audit probability from the 25th to the 75th percentile—an increase by about 8 percentage points—is associated with an increase in the quarterly interest income by 0.01 percent, or 1 percent of the sample average interest income, when using the coefficient estimate from Column (2). Such an increase in the interest income increases the average (median) bank's net income by $0.022 million ($0.054 million). Translated into return on equity, an interquartile increase in the audit probability increases by the return on equity of banks by about 0.1 percentage points (=$0.022 million increase in profits / average equity of $24 million). For a bank with median assets ($6.3 million), this is equivalent to an increase in the return on assets by 0.1 percentage points (=$0.054 million / $6.3 million).

4.2. Effect of tax enforcement on loan quality

Next, we examine whether increased tax enforcement affects bank performance via better lending decisions more directly by loan loss provisions. We re-estimate equation (1) using *LLP* as the dependent variable. Table 4 presents regression results for two different specifications: column (1) includes the full model with state-cycle fixed effects, and column (2) replaces the state-cycle fixed effects with state-year-quarter fixed effects. Results indicate that a higher audit probability is associated with lower loan loss provisions. In economic terms, an interquartile increase in the audit percentage by 7.8 percentage points, decreases loan loss provisions by 0.064 percent of total gross loans. Relative to the sample average of 0.1 percent, this is a decrease of 6.4 percent of the sample average loan loss provisions. These results thus suggest that stricter tax enforcement can increase loan portfolio quality.

4.3. Effect of tax enforcement on commercial loan growth

Next, we examine how tax enforcement affects growth in commercial lending. If tax enforcement leads to more efficient information collection and processing and improved borrower quality, we expect that stricter tax enforcement may lead banks to expand their lending activities to corporations. Panel A of Table 5 presents regression results from estimating equation (1) with *Commercial Growth* as the dependent variable. In Columns (1) to (3), we include state-level variables, year fixed effects, and state-

cycle fixed effects. We additionally include state-year fixed effects in Columns (4) to (6). Further, we restrict the sample to bank-years in which commercial loans comprise at least 5, 10, and 20 percent of a bank's loan portfolio, respectively. We use these cutoffs to focus our analyses on a sample of banks that actively engage in commercial lending and to avoid the possibility that small increases from very small absolute amounts of commercial lending will drive our results. The results in Panel A indicate that an increase in the audit percentage for small-and-medium-sized corporations increases commercial loan growth (results statistically significant in 4 of 6 models). In economic terms, the results in Column (2) indicate that an increase in the audit percentage from the 25^{th} to the 75^{th} percentile (7.8 percentage points) leads to an increase in commercial lending by 1.41 percentage points (=0.1852 × 0.078). Relative to the sample average commercial lending growth of 13.94 percent, an interquartile increase in the audit percentage increases commercial lending by about 10 percent. This effect becomes larger once we control for state-year fixed effects.

As with the previous analyses, there is a potential concern about correlated omitted variables as audit percentages are not randomly assigned to districts. We can exploit cross-sectional variation in the effect of tax enforcement on commercial loan growth. Specifically, due to regulatory capital requirements, growth in bank lending effectively requires capital. A bank with a high leverage ratio (i.e., low capital) may be unable to grow lending due to regulatory capital constraints. In contrast, a bank with regulatory capital capacity (i.e., lower leverage ratios) can issue new loans without coming close to its capital constraints. To test this prediction, we extend equation (1) by including a dummy variable *Low Leverage* as well as the interaction of this dummy with *Percent Audited Medium*. We set *Low Leverage* equal to one if a bank's leverage ratio is below the median leverage ratio in the respective year. Panel B of Table 6 presents the regression results from this cross-sectional test. We find that commercial loan growth of banks without additional capital capacities (highly levered banks) is not associated with tax enforcement. However, for banks with lower leverage, we find a significantly higher response to tax enforcement as indicated by the significant interaction *Percent Audited Medium* × *Low Leverage*. That is, commercial lending of banks with capital capacity grows at a faster pace when tax enforcement becomes

stricter. One advantage of exploiting a cross-section prediction is that we can include IRS-district–year fixed effects to account for all observable and unobservable time-varying characteristics at the district level that are potentially correlated with commercial lending growth and/or tax enforcement. The coefficient estimates for the interaction of *Low Leverage* and *Percent Audited Medium* remain very similar and statistically significant (see Columns (4) to (6) of Panel B).

In Panel C, we take an alternative approach to address the concern that a district-year specific unobserved factor is driving the commercial lending result. Specifically, we calculate the growth in consumer loans (*Consumer Growth*) over the same period and examine whether the growth in consumer loans responds in the same way as commercial loans. We choose consumer lending for this placebo test for two reasons. First, we expect it to be less related to corporate-level factors that could drive IRS enforcement efforts relative to commercial lending. Second, the average bank's level of consumer lending (16% of total loans) is close to the average level of commercial lending (17% of total loans), suggesting that this is close to an apples-to-apples comparison.[13] In Columns (1) to (3), we report results of estimating equation (1) but where we use *Consumer Growth* as the dependent variable. We find for all three cutoffs (5, 10, or 20 percent of consumer loans), that there is no statistically significant relation between tax enforcement and growth in consumer loans. In addition to the statistical insignificance, the coefficient estimates are very close to zero or even slightly negative. In the next step, we compare whether the growth in commercial loans is significantly different from the growth in consumer loans. We thus use two observations for each bank-year and include an indicator variable *Commercial* equal to one (zero) if the dependent variable *Growth* is set equal to *Commercial Growth* (*Consumer Growth*). In the regression, we include this dummy variable as well as interactions of this dummy with all control variables and fixed effects. Results are reported in Columns (4) to (6). The estimates for growth in consumer loans and commercial loans, respectively are the same as in the main analysis (Columns (1) to

[13] Alternatively, we could examine real estate landing. However, we chose not to do this for two reasons. First, real estate lending is quite different from commercial lending for the average sample bank; the former makes up 56% of total loans on average. Furthermore, total real estate loans also includes commercial real estate lending, and therefore could be affected by the same factors (e.g., performance and tax avoidance of local corporations) which potentially drives IRS enforcement choices.

(3) of Panel A and C). Importantly, the coefficient estimates between consumer loans and commercial loans are statistically significantly different from each other in all three cases. That is, the effect of tax enforcement on growth in commercial lending is significantly higher than on growth in consumer lending, and there is no significant relation with the latter.

4.4. Placebo analysis

Next, we conduct several placebo analyses to further increase confidence in our primary findings. First, we re-estimate our analyses within a sample of banks with loan portfolios predominantly comprising real estate and consumer loans. Since these banks engage in little commercial lending, they should not affected by a change in the audit percentage of mid-sized corporate clients. However, if our variable of interest proxies for local economic conditions, we should still find a positive association between it and our three bank outcome variables. Table 6 presents regression results from estimating equation (1), with the dependent variable being *Interest Income* (columns (1) and (2)), *LLP* (columns (3) and (4)), or *Commercial Growth* (columns (5) and (6)). In each pair of columns, the first column contains state-cycle fixed effects, whereas the second column contains the state-year-quarter fixed effects. All columns contain the full set of time-varying controls and bank fixed effects. Furthermore, panel A employs a sample of bank-observations with at least 90% consumer or real estate lending of the bank's loan portfolio, whereas panel B further restricts the sample to bank-observations with at least 95% consumer or real estate lending.[14] Across all 12 analyses, the coefficient on *Percent Audited Medium* is statistically insignificant; furthermore, when our main results are positive (*Interest Income* and *Commercial Growth* as dependent variable), the coefficient estimates are negative in five of eight cases, and when the coefficient is positive, the magnitude of the insignificant coefficient is close to zero. In case of *LLP*, the results always have the opposite sign as in our main tests and, again, the magnitudes are very small These non-results are not consistent with our variable of interest capturing local economic conditions, and are consistent with tax enforcement affecting bank lending decisions.

[14] These cutoffs are close to the top quintile (90.0 percent) and top decile (95.5 percent). When using the top quartile of the other loans to total loans distribution (87.7 percent), results (not reported) are very similar.

Second, we conduct an additional placebo test where we re-estimate the main analyses from Tables 3 through 5, now including the audit probabilities for micro firms (those with fewer than $10 million in assets) and large firms (those with greater than $100 million in assets) along with our primary variable of interest (*Percent Audited Medium*). We present the results of this analysis in Table 7. In summary, we find little evidence of an association between bank interest income, loan loss provisioning, and commercial loan growth and either the audit rates of micro firms (*Percent Audited Micro*) or large firms (*Percent Audited Large*). This non-result is consistent with our story that the small-and-medium sized firms are more likely to benefit from tax authority-sourced oversight than large firms. Furthermore, they are consistent with the lack of a credible audit threat for the smallest firms rendering the gains from tax authority governance non-existent. More importantly, these findings are inconsistent with the audit rates simply being a proxy for local economic conditions. We note that the coefficient on *Percent Audited Micro* is positive in columns 5 and 6 (with *Commercial Growth* as the dependent variable), but given the small value of a one-standard deviation move in audit rates for these firms (~2 percent), the economic magnitude of this finding is smaller than that of audit efforts aimed at small-and-medium sized firms.

4.5. Alternative identification strategy: IRS Restructuring and Reform Act of 1998

To further address the concern that changes in audit probabilities might be correlated with other unobservable characteristics that also affect bank performance, such as local economic conditions or corporate performance, we examine plausibly exogenous variation in audit probabilities around the Internal Revenue Service Restructuring and Reform Act of 1998 (IRSRRA). This act changes the IRS' organizational structure from a district based organizational structure to a size based organizational structure at the federal level. The first full year of the federal audit structure is 2001.After the enactment of IRSRRA, all firms in a given size category face the same audit probability, regardless of their location across the U.S. We examine how changes in tax enforcement around the IRSRRA affect bank performance. The identification assumption in this test is that new asset-based rates are set at the federal level, and therefore are not driven by district-level characteristics that may affect bank lending.

We use a difference-in-differences design to examine how tax enforcement affects bank performance. We collect additional call report data for the years 2001-2003. In this test, we limit the difference-in-differences analysis to three years before the change (1998-2000) and three years after the change (2001-2003). In this approach, the first difference compares bank outcomes before and after the IRS reorganization. The second difference compares the change in the audit probability in districts with large changes in the audit probability to districts with a smaller change In other words, we exploit the treatment intensity. For each district, we take the average audit probability of small-and-medium-sized firms over 1996-1999, and compare it the audit probability in the district after the reorganization. In Figure 2, we present a histogram of the change in the *Percent Audited Medium* distribution in our sample, which shows that while almost all districts experience a decrease in audit rates around the reorganization, there is substantial variation in the change in audit probabilities across districts.[15]

To examine how changes in audit rates around the IRSRRA affected banks, we estimate the following equation:

$$Bank\ Outcome_{i,t} = \beta_0 + \beta_1 Low\ Decrease_d \times Post_t + \Sigma_j \beta_j Controls_{i,t} + Fixed\ Effects \quad (2).$$

The dependent variable *Bank Outcome* one of the following bank outcomes in year t: (1) *Interest Income*, (2) *LLP*, and (3) *Commercial Growth* (measured from year t to t+1). The main independent variable is the difference-in-difference coefficient *Low Decrease* × *Post*. We define *Low Decrease* as an indicator variable equal to one if the IRS district experienced a decrease in the audit percentage that is below the 25th percentile of the change in audit probability distribution.[16] Districts sorted in the *Low Decrease* (*High Decrease*) group, on average, experience a decrease in the audit percentage of small-and-medium-sized corporations of 2.6 (9.8) percent.

Consistent with our previous results, we would expect a positive β_1 coefficient when using *Interest Income* or *Commercial Growth* as dependent variables. When using loan loss provisions (*LLP*) as the dependent variable, we expect β_1 to be negative. Our difference-in-differences approach controls for

[15] Since almost all districts experience a decline in the audit rate around the IRSRRA, we cannot test whether audit rates increases and decreases have differing effects.

[16] Our results are robust to using other cutoffs, such as a tercile split (see Table A.2 of the Online Appendix).

size, leverage, revenue growth, and the level of tax enforcement. It also includes bank fixed effects as well as state-year fixed effects. Standard errors are again clustered at the IRS district level.

One key assumption of the difference-in-difference design is that there is a parallel trend prior to the event. To provide evidence that this condition is satisfied, we follow the test in Patel and Seegert (2017) and examine whether there is a difference between treatment (*Low Decrease Districts*) and control group (*High Decrease Districts*) in any of the three pre-reform years for our dependent variables. Figure 3 plots the respective differences and the 95 percent confidence bounds of the difference between treatment and control group. All coefficient estimates are close to zero and statistically indistinguishable from zero. Further, we fail to reject the null that the sum of coefficients is statistically different from zero. These findings support the parallel trends assumption.

Table 8 presents regression results from estimating equation (2) using our three dependent variables. The results in Column (1) suggest that relative to districts with a high decrease in the audit percentage, banks in *Low Decrease* districts increase their interest income. The difference-in-difference coefficient on *Low Decrease*× *Post* is positive and statistically significant at the 5 percent level. The coefficient estimate of 0.0023 in Column (1) indicates that relative to banks with a high decrease in the audit probability, banks facing a low decrease in the audit probability points increase their ratio of interest income to total assets by about 0.2 percentage points. Given that the average decrease in the audit percentage in *High Decrease* (*Low Decrease*) districts is 9.8 percent (2.6 percent) and, thus, that the difference between treated and control group is comparable to the interquartile range in our baseline test, the effect on profitability we find in the difference-in-differences setting is similar to the economic magnitude in Column (2) of Table 3 that also includes state-year-quarter fixed effects.[17]

In Column (2), we employ *LLP* as our dependent variable. Relative to banks in districts with a high decrease in the audit percentage, banks located in districts with a low decrease have fewer loan loss provisions. Finally, in Columns (3) and (4) of Table 8, we present regression results on commercial loan

[17] When using the estimate in Column (2) of Table 3, a one standard deviation increase in the audit percentage (0.0782) is associated with increase in the ratio of interest income to lagged total assets of 0.023 percentage points (= 0.0030 × 0.0782).

growth. We find that banks located in districts with a low decrease have higher loan growth relative to banks experiencing a large decrease in tax enforcement. In Column (4) of Table 8, we also repeat the cross-sectional test from Panel B of Table 6. Again, we find a higher growth in commercial lending following a change in the audit percentage for firms with lower leverage. Overall, these difference-in-differences results are similar to our baseline findings. They address important concerns about potential omitted correlated variables. For such a variable to explain our results, it needs to be correlated with the change in the audit percentage around the IRS reorganization to a federal-level organization as well as with bank performance, loan quality, and commercial lending growth at the same time.

5. Additional Tests

We conduct several sets of tests to support our primary results. First, we examine whether tax enforcement is associated with greater loan loss provision informativeness, which would be consistent with enforcement leading to more informed lending decisions. Second, we examine what factors explain variation in IRS audit levels and whether reverse causality could be responsible for our findings. Third, we explore whether our supposition about the stronger effects of tax enforcement for small-and-medium sized firms seems to be substantiated. Finally, we explore whether greater tax enforcement affects banks direct via their own tax planning.

5.1. Loan loss provision informativeness

In this section, we provide empirical support for our hypothesis that tax enforcement leads to improved borrower information environments by exploring whether tax enforcement is associated with the ability of loan loss provisions to predict future loan losses. A stronger mapping of current provisions into future loan losses is consistent with an improved ability to forecast loan losses. We therefore examine how current loan loss provisions map into future net charge-offs (Beck and Narayanamoorthy 2013; Andries et al. 2017), and how this mapping varies across IRS districts with weaker and stronger tax enforcement. We therefore split the sample into two groups where the weak tax enforcement group comprises IRS districts with the lowest audit probability in a given year. We define IRS districts with a

low audit percentage as those districts whose audit percentage is below the bottom quartile of the audit percentage distribution, with the other districts forming the group of stricter tax enforcement districts.[18]

Table 9 presents these results. The dependent variable is the average net charge-off over the next four quarters relative to total gross loans (*NCO*). The coefficient of interest is the coefficient on LLP. A higher coefficient indicates a higher informativeness of current loan loss provisions in prediction future net charge-offs. All regressions include control variables and fixed effects. In all regressions, we find that current loan loss provisions are significantly and positively associated with future net charge-offs. Importantly, we find in districts where IRS enforcement is stricter, provisions are more predictive of future charge-offs, consistent with banks' ability to forecast borrower defaults being improved via tax enforcement's effect on the borrower's information environment. The *LLP* coefficient estimates for the stricter tax enforcement sample is more than twice the magnitude as in the low audit percentage districts. In Columns (3) and (6), we use the full sample of banks to test whether the informativeness of current loan loss provisions significantly differs between low and high/medium audit percentage districts. In both columns, we find a statistically significantly stronger relation between current loan loss provisions and future net charge-offs for the banks located in districts with higher audit percentages.

5.2. Reverse causality

One potential concern about using district-level audit probabilities is that bank outcomes drive a district's decision to change the audit frequency. To mitigate reverse causality concerns, we examine whether observable bank characteristics, such as performance, bank leverage, size, provisioning, lending growth or banks' effective tax rates, are correlated with current or future levels of and changes in audit probabilities. We further include characteristics of listed industrial firms to control for the non-banking corporate sector in a district. Results are reported in Panel A of Table A.3 in the Online Appendix. While not definitive, these findings suggest that reverse causality is not an issue in our study. With one exception (the average effective tax rate of banks in a district), none of the bank level characteristics are correlated with current or future levels of and changes in audit probabilities. This is perhaps not

[18] We note that we obtain similar results when using a tercile split (results not reported).

surprising, since these audit rates are for all corporations, not just the commercial banks we examine. We find that the state-level characteristics unemployment and house price index as well as some industrial firm characteristics are correlated with audit percentages in a district. In Panel B, we further show that our main results—profitability, loan quality, and commercial loan growth—are robust to using the residual audit probability from determinants regression that controls for state-level characteristics well as some industrial firm characteristics. In fact, Panel B of Table A.3 shows that the results are very similar to our baseline findings in Tables 3, 4, and 5. Taken together, the results in Table A.3 indicate that reverse causality, i.e., the fact that banking outcomes or bank lending affect audit percentage of small-and-medium-sized firms, is unlikely to explain our findings.

5.3. Effects of tax enforcement on public corporations

Next, we address concerns about one specific research design choice, namely our choice of small-and-medium-sized firms to define the relevant asset class. As noted before, smaller firms are only rarely subject to IRS audits; it is unlikely that the threat of an IRS audit would be effective in improving corporate governance in these firms. However, larger firms with assets above $100 million are more frequently subject to corporate audits. Hence, also larger corporations could experience an improvement in the information environment. In Section 3, we argue that relative to large firms, small-and-medium-sized firms' internal and external information environment is more likely to be responsive to tax enforcement, relative to large firms. While we believe this assumption to be justified, prior literature on the effect of tax enforcement on financial reporting quality (Hanlon et al. 2014) does not provide empirical tests to support or reject this assumption.

We extend prior literature and examine whether the positive effect on the information environment differs between small-and-medium-sized and large firms. We thus build on equation (1) in Hanlon et al. (2014) and their main findings from Table 3. We then examine whether the effect of tax enforcement on financial reporting quality is indeed stronger for public small-and-medium-sized firms relative to large public firms. We use all industrial firms with assets above $10 million available in Compustat for our sample period 1992–2000. We exclude utility and financial firms. We calculate the

proxies for financial reporting quality, discretionary accruals as well as of accrual quality exactly as described in Appendix B of Hanlon et al. (2014). In these regressions, we use the actual audit percentage faced by the firm. Table A.4 of the Online Appendix presents the regression results for discretionary accruals (Columns (1) to (3)) and for accrual quality (Columns (4) to (6)). The results in Columns (1) and (4) are consistent with the findings prior literature: A higher audit percentage is associated with a better external information environment (less discretionary accruals and higher accrual quality). In Columns (2) and (5), we restrict the sample to small-and-medium-sized industrial firms. We present regressions for large firms in Columns (3) and (6). The results in these columns support our assumption behind our baseline approach. For small-and-medium-sized firms, we find a strong and significant relation of tax enforcement with financial reporting quality. This association is weaker for large firms. Consistent with our argument that a small-and-medium-sized firms' internal and external information environment is more affected by tax enforcement, we find that the coefficient estimates between small-and-medium-sized and large firms differ significantly from each other using either measure of financial reporting quality.

5.4. Bank tax payments

In Table A.5 of the Online Appendix, we examine whether banks' current or future effective tax rates (ETR) are related to tax enforcement actions. Previous literature shows that industrial firm's tax avoidance responds negatively to increases in tax enforcement (Gupta and Lynch 2016; Hoopes et al. 2012; Mironov 2013). However, it is unclear whether a similar result can be obtained in case of banks and, in particular, in our sample of locally operating banks. These banks have limited tax avoidance opportunities that can be challenged by the tax enforcer. Hence, tax enforcement may not lead to changes in actual tax avoidance behavior of our sample banks. To test this prediction, we use the banks ETR in year *t* (Columns (1) and (2)) or in year *t+1* (Columns (3) and (4)) as dependent variable. The regression follows equation (1) but we use annual observations and the asset class specific audit percentage (*Percent Audited Size*) since we test for a direct effect on banks. The results show that audit percentages are not significantly related to bank ETRs consistent with the explanation above. This result further indicates that banks are not affected directly by tax enforcement actions through increased tax collections.

6. Conclusion

We provide the first empirical evidence of the effect of tax enforcement on the banking sector. Employing a sample of U.S. commercial banks and exploiting the IRS district structure from 1992 to 2000 as well as the IRS reorganization around 2000, we document that greater corporate tax return audit probabilities are positively associated with bank profitability, loan portfolio quality, and commercial loan growth. Furthermore, we document that banks' estimates of loan loss provisioning are more accurate when tax enforcement is greater. Overall, our findings point to an unintended consequence of tax enforcement: a more profitable and more transparent banking system that encourages greater commercial lending. Our findings should be of interest to tax authorities, bank regulators, and general policymakers, especially given the recent emphasis to increase tax enforcement. The primary motivation for greater tax enforcement to date has been to increase revenues and close the tax gap. While tax enforcement is not costless and any benefits must be weighed against those costs, our results suggest that there are previously undocumented benefits to tax enforcement.

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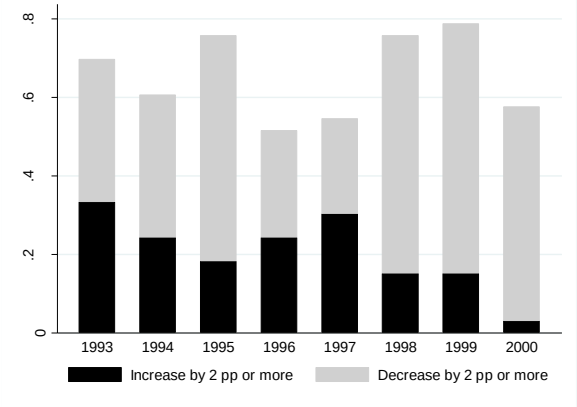
Appendix A: Variable Definitions

Variable	Description
Panel A: Bank variables	
Interest Income	*Interest Income* is defined as interest income minus loan loss provisions in quarter *q* over lagged total assets.
Commercial Growth	*Commercial Growth* is defined as the change in commercial loans from *q* to *q+4* relative to commercial loans from *q*.
Consumer Growth	*Consumer Growth* is defined as the change in consumer loans from *q* to *q+4* relative to consumer loans from *q*.
ETR	*ETR* is the effective tax rate defined as tax expenses over earning before tax.
Assets	*Assets* is defined as the natural logarithm of total assets in million USD.
Leverage Ratio	*Leverage Ratio* is the ratio of total debt to total assets.
Revenue Growth	*Revenue Growth* is defined as the change in interest revenues from *q-4* to *q* relative to interest revenues from *q-4*
LLP	*LLP* is the ratio of loan loss provisions in quarter q to total gross loans.
NCO	*NCO* is the ratio of average net charge-offs from *q+1* to *q+4* to total gross loans in quarter *q*.
Panel B: State variables	
Unemployment	*Unemployment* is the seasonally-adjusted unemployment rate in state *s* in quarter *q*.
ΔUnemployment	*ΔUnemployment* is the change in the seasonally-adjusted unemployment rate in state *s* from quarter *q-1* to *q*.
HPI	*HPI* is the seasonally-adjusted house price index in state *s* in quarter *q*.
ΔHPI	*ΔHPI* is the change in the seasonally-adjusted house price index in state *s* from quarter *q-1* to *q*.
$\text{Profits}_{\text{State}}$	The variable $Profits_{State}$ is defined as the sum of pre-tax income of non-bank firms over the sum of total assets of non-bank firms in state *s* in year *t*.
$\text{Cash}_{\text{State}}$	The variable $Cash_{State}$ is defined as the sum of cash holdings of non-bank firms over the sum of total assets of non-bank firms in state *s* in year *t*.
$\text{CapEx}_{\text{State}}$	The variable $CapEx_{State}$ is defined as the sum of capital expenditures of non-bank firms over the sum of total assets of non-bank firms in state *s* in year *t*.
$\text{Mean(ETR)}_{\text{State}}$	The variable $Mean(ETR)_{State}$ is defined as the average one-year *Cash ETR* of non-bank firms in state *s* in year *t*. Before calculating the average ETR in a state-year, we winsorize the *Cash ETR* at the 1% and 99% levels.
Panel C: District Level Variables	
Percent Audited Micro	*Percent Audited Micro* is the percentage of audited corporate tax returns to total corporate tax returns in an IRS district in year *t* for firms with assets between $250,000 to $10 million USD.
Percent Audited Medium	*Percent Audited Medium* is the percentage of audited corporate tax returns to total corporate tax returns in an IRS district in year *t* for firms with assets between 10 and 100 million USD..
Percent Audited Large	*Percent Audited Large* is the percentage of audited corporate tax returns to total corporate tax returns in an IRS district in year *t* for firms with assets over 100 million USD.
Percent Audited Size	*Percent Audited Size* is the audit percentage in the bank's asset size class in an IRS district in year *t*.

Figure 1: Changes in Audit Probabilities 1992-2000

This figure presents the frequency of changes in the audit probability in the 33 IRS districts over 1992-2000. Panel A presents changes in the audit percentage in the 10-100 million USD asset size class by at least 2 percentage points. Panel B uses a cutoff of at least 5 percentage points.

Panel A: Changes by 2 Percentage Points



Panel B: Changes by 5 Percentage Points

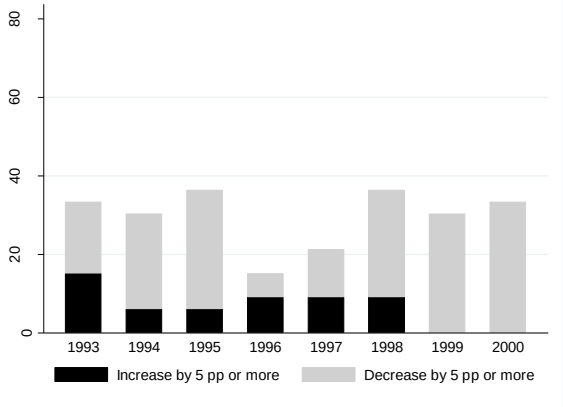


Figure 2: Changes in Audit Probabilities around the IRS Reorganization of 2000

This figure presents a histogram of the changes in the audit percentage in the 10-100 million USD asset size class from before to after 2001. We use the sample of firms used in our difference-in-differences tests.

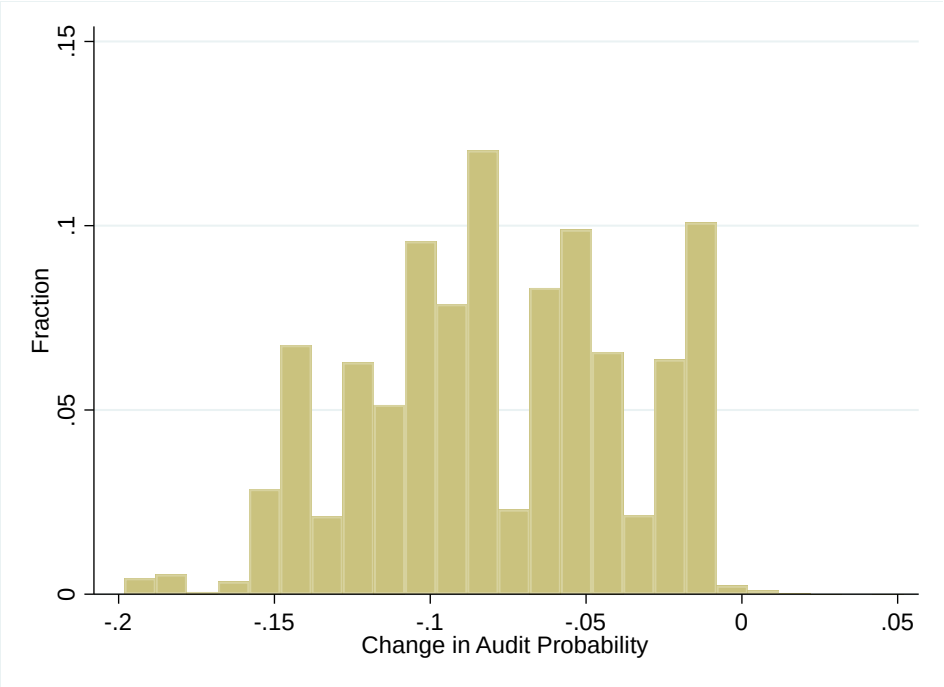


Figure 3: IRS Reorganization and Bank Performance: Testing for Parallel Trends

This figure plots the coefficient of *Low Decrease* which is a dummy variable equal to one if the difference between the audit probability in an IRS district before 2000 and the audit probability in the district after the reorganization is above the 25th percentile. Following Patel and Seegert (2017), we run the following regression for each dependent variable: $y_{i,t} = Low\ Decrease_d \times YearDum_t + YearDum_t + \epsilon$, where $YearDum_t$ are indicator variables for 1998, 1999, and 2000, respectively. We also present the 95 percent confidence intervals based on standard errors clustered at the IRS district level. When presenting the estimates and confidence bounds for *Commercial Growth*, we divide coefficients and standard errors by 10 to have comparable scales.

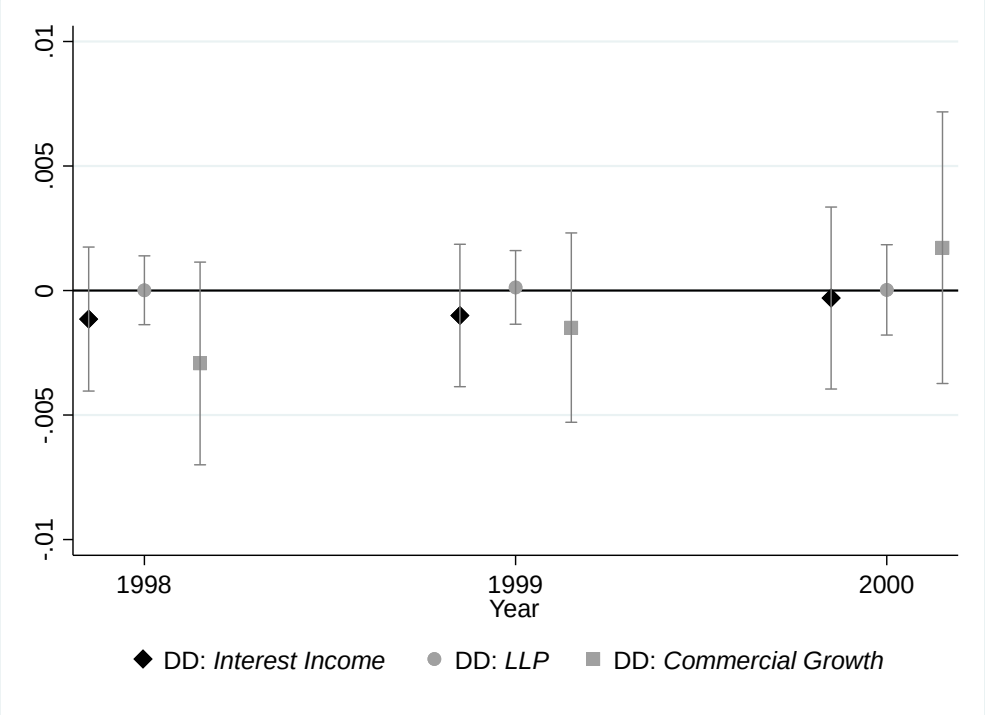


Table 1: Sample Selection and District Overview

Panel A: Sample Selection

	Screen	Observations
	All bank-quarters with call reports from 1992–2000	392,303
–	Less bank-quarters with negative assets, negative loans, negative total capital and missing state information	-459
	Less bank-quarters with operations in multiple districts and banks changing locations	-37,455
–	Less bank-quarters with missing information on dependent variables and control variables	-7,076
–	Drop banks with very low importance of commercial lending	-15,739
	Final Sample	**331,574**

Panel B: District Overview

IRS District	Banks	Observations
Arkansas-Oklahoma	637	19,791
Brooklyn	17	401
Central California	81	2,003
Connecticut-Rhode Island	107	2,518
Delaware-Maryland	144	3,772
Georgia	447	12,378
Gulf Coast	586	16,428
Houston	146	3,982
Illinois	1027	29,424
Indiana	246	6,964
Kansas-Missouri	995	30,176
Kentucky-Tennessee	603	17,221
Los Angeles	83	1,984
Manhattan	53	1,356
Michigan	239	6,299
Midwest	1373	42,021
New England	311	7,910
New Jersey	131	2,751
North Central	854	27,154
North Florida	230	5,348
North Texas	544	16,069
North-South Carolina	229	4,984
Northern California	115	2,934
Ohio	283	8,047
Pacific-Northwest	195	4,681
Pennsylvania	307	8,121
Rocky Mountain	570	14,664
South Florida	192	4,353
South Texas	327	9,861
Southern California	76	1,858
Southwest	173	3,972
Upstate New York	110	3,307
Virginia-West Virginia	341	8,842
Full Sample	**11772**	**331,574**

Table 2: Summary Statistics

This table presents descriptive statistics of our main variables for 331,030 observations from 11,763 banks over 1992–2000. The unit of observations is bank-year-quarter. *Interest Income* is defined as interest income in quarter *q* over lagged total assets. *Commercial Growth* (*Consumer Growth*) is defined as the change in commercial loans (consumer loans) from *q-4* to *q* relative to commercial loans (consumer loans) from *q-4*. *ETR* is the effective tax rate defined as tax expenses over earning before tax. *Percent Audited Micro* is the percentage of audited corporate tax returns to total corporate tax returns in an IRS district in year *t* for firms with assets up to 10 million USD. *Percent Audited Medium* is the percentage of audited corporate tax returns to total corporate tax returns in an IRS district in year *t* for firms with assets between 10 and 100 million USD. *Percent Audited Large* is the percentage of audited corporate tax returns to total corporate tax returns in an IRS district in year *t* for firms with assets over 100 million USD. *Percent Audited Size* is the audit percentage in the bank's asset size class in an IRS district in year *t*. *Assets* is defined as the natural logarithm of total assets in million USD. *Leverage Ratio* is the ratio of total debt to total assets. *Revenue Growth* is defined as the change in interest revenues from *q-4* to *q* relative to interest revenues from *q-4*. *LLP* is the ratio of loan loss provisions in quarter q to lagged total gross loans. *NCO* is the ratio of average net charge-offs from *q+1* to *q+4* to total gross loans in quarter *q*.

Variable	Mean	Standard Deviation	25th Percentile	Median	75th Percentile
Interest Income	0.0102	0.0023	0.0090	0.0101	0.0114
Commercial Growth	0.1392	0.3307	-0.0639	0.0887	0.2796
Consumer Growth	0.0897	0.2729	-0.0613	0.0501	0.1806
ETR	0.2794	0.1136	0.2381	0.3050	0.3453
Percent Audited Micro	0.0420	0.0195	0.0273	0.0400	0.0532
Percent Audited Medium	0.1829	0.0679	0.1373	0.1763	0.2155
Percent Audited Large	0.3896	0.1332	0.2870	0.3699	0.4887
Percent Audited Size	0.2348	0.1495	0.1355	0.1955	0.2833
Size (in logarithm)	4.1805	1.1796	3.3975	4.0609	4.7926
Size (in $ million)	228.6787	1,808.6540	29.8880	58.0240	120.6170
Leverage Ratio	0.8977	0.0521	0.8879	0.9085	0.9220
Revenue Growth	0.0742	0.2509	-0.0379	0.0324	0.1111
LLP	0.0010	0.0019	0.0000	0.0005	0.0011
NCO	0.0007	0.0014	0.0000	0.0003	0.0009

Table 3: IRS Audits and Bank Profitability

This table presents the regression results on bank performance over 1992–2000. The dependent variable is *Interest Margin*. The independent variables are defined in Appendix A. In Column (1) we include year-quarter, bank, and state-cycle fixed effects. We further include state level control variables defined in Table 1. Column (2) includes bank fixed effects as well as state-year-quarter fixed effects. We report robust standard errors clustered at the IRS District level in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)
Percent Audited Medium	**0.0012*** **	**0.0030*** **
	(0.0004)	**(0.0010)**
$Size_{year-1}$	-0.0010***	-0.0010***
	(0.0001)	(0.0001)
Leverage $Ratio_{year-1}$	0.0024***	0.0024***
	(0.0003)	(0.0003)
State Controls	Yes	No
Bank FE	Yes	Yes
Year-Quarter FE	Yes	No
State-Cycle FE	Yes	No
State-Year-Quarter FE	No	Yes
Observations	331,574	331,574
Adj. R-squared	0.526	0.530

Table 4: Tax Enforcement and Loan Portfolio Quality

This table presents the regression results on bank behavior over 1992–2000. The dependent variable is current *LLP*. The independent variables are defined in Appendix A. In Column (1), we include year-quarter, bank, and state-cycle fixed effects. We further include state level control variables defined in Table 1. Column (2) includes bank fixed effects as well as state-year-quarter fixed effects. We report robust standard errors clustered at the IRS District level in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)
Percent Audited Medium	**-0.0008** **	**-0.0024*** **
	(0.0003)	**(0.0008)**
$Size_{year-1}$	0.0004***	0.0004***
	(0.0001)	(0.0001)
Leverage $Ratio_{year-1}$	-0.0026***	-0.0027***
	(0.0003)	(0.0003)
State Controls	Yes	No
Bank FE	Yes	Yes
Year-Quarter FE	Yes	No
State-Cycle FE	Yes	No
State-Year-Quarter FE	No	Yes
Observations	331,574	331,574
Adj. R-squared	0.226	0.230

Table 5: Tax Enforcement and Growth in Commercial Lending

This table presents the regression results on bank behavior over 1992–2000 using annual observations. The dependent variable is growth in commercial loans from year *t* to *t+1* in Panel A and B. We define a minimum level of commercial lending in year *t* in this test (*Cutoff*). This cutoff is 5% in Columns (1) and (4), 10% in Columns (2) and (5), and 20% in Columns (3) and (6). In Panel B, we additionally include an interaction of the audit percentage with a dummy *Low Leverage* which is equal to one if the bank is below the median *Leverage Ratio* in the respective year. Panels C and D present placebo analyses. In Panel C, we compare growth in consumer loans and commercial loans by using two observations per firm. The dependent variable is *Growth* which we set to growth in consumer loans for one bank-year observation. We set *Growth* to the growth in commercial loans for the other bank-year observation. We then repeat the regression from Panel A but include interactions of each independent variable and fixed effects with a dummy *Commercial*, which is equal to one for the one bank-year observation for we set *Growth* to the growth in commercial loans. We use the same cutoffs as describe above but apply the respective loan type. That is, when using consumer loans to define *Growth*, we require that banks' loan portfolio comprises at least 5%, 10%, and 20%, respectively of consumer loans. In Panel D, we repeat the analysis from Panel A and B but use a sample of bank-observations with at least 90%/95% of their loan portfolio comprising of real estate or consumer loans. We report robust standard errors clustered at the IRS District level in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Baseline Analysis

	(1)	(2)	(3)	(4)	(5)	(6)
Cutoff	5%	10%	20%	5%	10%	20%
Percent Audited	**0.1142**	**0.1811****	**0.1832****	**0.2268****	**0.2670*****	**0.1733**
Medium	**(0.0783)**	**(0.0801)**	**(0.0791)**	**(0.0938)**	**(0.0840)**	**(0.1124)**
Size	-0.1708***	-0.1742***	-0.1784***	-0.1701***	-0.1737***	-0.1776***
	(0.0104)	(0.0111)	(0.0159)	(0.0102)	(0.0109)	(0.0163)
Leverage Ratio	-0.5956***	-0.5293**	-0.8123**	-0.6045***	-0.5406***	-0.8186**
	(0.1437)	(0.1939)	(0.3005)	(0.1445)	(0.1953)	(0.3033)
Revenue Growth	0.0391***	0.0397***	0.0346***	0.0387***	0.0394***	0.0336***
	(0.0069)	(0.0078)	(0.0093)	(0.0069)	(0.0078)	(0.0096)
State Controls	Yes	Yes	Yes	No	No	No
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	No	No	No
State-Cycle FE	Yes	Yes	Yes	No	No	No
State-Year-FE	No	No	No	Yes	Yes	Yes
Observations	63,501	48,297	19,353	63,501	48,297	19,353
Adj. R-squared	0.069	0.099	0.163	0.069	0.100	0.163

Panel B: Cross-Sectional Analysis

	(1)	(2)	(3)	(4)	(5)	(6)
Cutoff	5%	10%	20%	5%	10%	20%
Percent Audited	0.0369	0.1340*	0.1127			
	(0.0775)	(0.0788)	(0.0834)			
Percent Audited ×	0.1728***	0.1101**	0.1739**	0.1836***	0.1189**	0.1847**
Low Leverage	(0.0524)	(0.0499)	(0.0835)	(0.0536)	(0.0499)	(0.0850)
Joint Effect Low	0.2096**	0.2441***	0.2866***			
Leverage Banks	(0.0893)	(0.0896)	(0.0978)			
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Cycle FE	Yes	Yes	Yes	Yes	Yes	Yes
IRS-Year FE	No	No	No	Yes	Yes	Yes
Observations	63,501	48,297	19,353	63,501	48,297	19,353
Adj. R-squared	0.069	0.099	0.163	0.069	0.100	0.165

Panel C: Placebo Analysis, Comparing Consumer and Commercial Loan Growth						
	(1)	(2)	(3)	(4)	(5)	(6)
Cutoff	5%	10%	20%	5%	10%	20%
Percent Audited	0.0036	-0.0049	-0.0292	0.0036	-0.0049	-0.0292
× Consumer	(0.0778)	(0.0789)	(0.0780)	(0.0778)	(0.0789)	(0.0780)
Percent Audited				0.1142	0.1811**	0.1832**
× Commercial				(0.0783)	(0.0801)	(0.0791)
Δ Commercial				**0.1106***	**0.1861****	**0.2124***
and Consumer				**(0.0587)**	**(0.0783)**	**(0.1161)**
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Cycle FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	61,093	44,749	18,542	124,594	93,046	37,895
Adj. R-squared	0.123	0.134	0.158	0.095	0.117	0.167

Table 6: Placebo Test, Effect of Corporate Tax Enforcement on Banks without Commercial Lending

This table presents the regression results on bank performance over 1992–2000. We replicate the main results from Tables 3, 4, and 5 but restrict the sample in Panel A (Panel B) to bank-quarters in which real estate loans and consumer loans account for at least 90% (95%) of total loans. The independent variables are defined in Table 2. In Columns (1), (3), and (5), we include year-quarter, bank, and state-cycle fixed effects. We further include state level control variables defined in Appendix A. Columns (2), (4), and (6) include bank fixed effects as well as state-year-quarter fixed effects. We report robust standard errors clustered at the IRS District level in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Real Estate and Consumer Loans Accounting for >90% of Total Loans						
	Interest Income		**LLP**		**Commercial Growth**	
	(1)	(2)	(3)	(4)	(5)	(6)
Percent Audited	0.0003	0.0003	0.0003	0.0006	-0.0346	0.1401
Medium	(0.0005)	(0.0012)	(0.0004)	(0.0007)	(0.1921)	(0.1919)
State Controls	Yes	No	Yes	No	Yes	No
Bank Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	No	Yes	No	Yes	No
State-Cycle FE	Yes	No	Yes	No	Yes	No
S-Y-Q FE	No	Yes	No	Yes	No	Yes
Observations	68,596	68,525	68,596	68,525	12,263	12,247
Adj. R-squared	0.637	0.644	0.568	0.574	0.047	0.050
Panel B: Real Estate and Consumer Loans Accounting for >95% of Total Loans						
	Interest Income		**LLP**		**Commercial Growth**	
	(1)	(2)	(3)	(4)	(5)	(6)
Percent Audited	-0.0005	-0.0016	0.0001	0.0011	-0.1580	-0.4734
Medium	(0.0008)	(0.0018)	(0.0005)	(0.0013)	(0.3812)	(0.5620)
State Controls	Yes	No	Yes	No	Yes	No
Bank Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	No	Yes	No	Yes	No
State-Cycle FE	Yes	No	Yes	No	Yes	No
S-Y-Q FE	No	Yes	No	Yes	No	Yes
Observations	36,521	36,379	36,521	36,379	5,675	5,649
Adj. R-squared	0.657	0.664	0.639	0.646	0.063	0.072

Table 7: Placebo Test, Effect of Corporate Tax Enforcement in other Asset Classes on Banks

This table presents the regression results on bank performance over 1992–2000. We replicate the main results from Tables 3, 4, and 5 but additionally account for corporate tax enforcement of very small firms (*Percent Audited Micro*) and large firms (Percent Audited Large). The independent variables are defined in Table 2. In Columns (1), (3), and (5), we include year-quarter, bank, and state-cycle fixed effects. We further include state level control variables defined in Appendix A. Columns (2), (4), and (6) include bank fixed effects as well as state-year-quarter fixed effects. We report robust standard errors clustered at the IRS District level in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Interest Income		**LLP**		**Commercial Growth**	
	(1)	(2)	(3)	(4)	(5)	(6)
Percent Audited	-0.0006	0.0024	-0.0006	-0.0054	0.3757	0.2991
Micro	(0.0016)	(0.0019)	(0.0020)	(0.0035)	(0.3007)	(0.4762)
Percent Audited	**0.0014*** **	**0.0031*** **	**-0.0007***	**-0.0021*** **	**0.1516***	**0.2461*** **
Medium	**(0.0004)**	**(0.0011)**	**(0.0004)**	**(0.0006)**	**(0.0842)**	**(0.0735)**
Percent Audited	-0.0003	-0.0005	-0.0000	0.0000	0.0137	0.0058
Large	(0.0002)	(0.0003)	(0.0002)	(0.0002)	(0.0278)	(0.0418)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
State Controls	Yes	No	Yes	No	Yes	No
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	No	Yes	No	Yes	No
State-Cycle FE	Yes	No	Yes	No	Yes	No
S-Y-Q FE	No	Yes	No	Yes	No	Yes
Observations	331,574	331,574	331,574	331,574	48,392	48,392
Adj. R-squared	0.526	0.530	0.226	0.231	0.099	0.100

Table 8: Tax Enforcement and Banking Outcomes: Evidence from IRS Reorganization

This table presents the regression results on bank performance over 1998–2003 using annual data. The independent variables are defined in Appendix A. We use a difference-in-differences approach. We define the decrease in the audit probability in an IRS around the IRS reorganization in 2001, *Decrease Audit Prob*, as the decrease from the audit probability in an IRS district before 2000 to the audit probability in the district after the reorganization. The post-reorganization probability is obtained by using the federal level size-based audit probabilities weighted by the pre-2000 frequencies of audits in a respective asset-size class in a district. The variable *Post* is a dummy variable equal to one for years after 2000. We use a dummy variable *Low Decrease* equal to one if the change in the audit probability is in the top quartile of the *Decrease Audit Prob* distribution. We include bank fixed effects as well as state-year fixed effects in all columns. We include size, leverage, revenue growth, and the audit percentage as control variables. In Columns (3) and (4), we define a minimum level of commercial lending in year *t* of 10% that is based on the pre-reform average ratio of commercial loans to total loans. We report robust standard errors clustered at the IRS District level in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable	**Interest Income**	**LLP**	**Commercial Growth**	
	(1)	(3)	(3)	(4)
Low Decrease × Post	0.0023**	-0.0017***	0.0551**	0.0394
	(0.0010)	(0.0006)	(0.0239)	(0.0236)
Low Decrease × Post × Low Leverage				0.0337***
				(0.0099)
Controls & Bank FE	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes
Observations	34,476	34,476	24,341	24,341
Adjusted R-squared	0.649	0.463	0.246	0.247

Table 9: Tax Enforcement and Informativeness of Loan Loss Provisions

This table presents regression results on informativeness of loan loss provisions. The dependent variable is defined as the average net charge-offs in the next four quarters relative to gross loans. We split the sample into districts with low audit percentage and high & medium audit percentages. Specifically, we split the sample at the bottom quartile of the respective year's audit percentage distribution. In Columns (3) and (6), we use the full sample and include an interaction of *LLP* with *Medium & High Audit* which is a dummy variable equal to one if the district's audit percentage is above the bottom quartile of the audit percentage distribution. In Columns (3) and (6), we also interact control variables and fixed effects with *Medium & High Audit*. We report robust standard errors clustered at the IRS District level in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Sample	**Low Perc.**	**Medium & High Perc.**	**Full Sample**	**Low Perc.**	**Medium & High Perc.**	**Full Sample**
	(1)	(2)	(3)	(4)	(5)	(6)
Average Audit Percentage	12%	20%	18%	12%	20%	18%
LLP	0.0527*	0.1085***	0.0527**	0.0517*	0.1086***	0.0517**
	(0.0250)	(0.0054)	(0.0244)	(0.0256)	(0.0054)	(0.0250)
LLP × Medium & High Audit			0.0558**			0.0569**
			(0.0259)			(0.0265)
Controls & Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
State Controls	Yes	Yes	Yes	No	No	No
State-Cycle FE	Yes	Yes	Yes	No	No	No
State-Year-Quarter FE	No	No	No	Yes	Yes	Yes
Observations	60,208	224,517	284,725	60,208	224,517	284,725
Adjusted R-squared	0.545	0.439	0.459	0.546	0.440	0.460

ONLINE APPENDIX – NOT FOR PUBLICATION

Table A.1: Robustness to Alternative Clustering of Standard Errors

This table presents regression results of our main results from Tables 3, 4, and 5 to alternative clustering of standard errors. In Columns (1) to (3), we use robust standard errors clustered at the IRS District–year level in parentheses. Columns (4) to (6) reports robust standard errors with two-way clustered standard errors at the bank and at the IRS District level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	District-Year Cluster			**2-Way Cluster District-Bank**		
	Interest Income	**LLP**	**Commercial Growth**	**Interest Income**	**LLP**	**Commercial Growth**
	(1)	(2)	(3)	(4)	(5)	(6)
Percent Audited	0.0012***	-0.0007**	0.1847***	0.0012***	-0.0007**	0.1847**
Medium	(0.0003)	(0.0003)	(0.0650)	(0.0004)	(0.0004)	(0.0802)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank Fe	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Cycle FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	331,574	331,574	48,289	331,574	331,574	48,289
Adj. R-squared	0.527	0.226	0.099	0.526	0.226	0.098

Table A.2: Tax Enforcement and Banking Outcomes: Evidence from IRS Reorganization: Robustness to Using Alternative Cutoff

This table presents the regression results on bank performance over 1998–2003 using annual data. The independent variables are defined in Appendix A. We use a difference-in-differences approach. We define the decrease in the audit probability in an IRS around the IRS reorganization in 2001, *Decrease Audit Prob*, as the decrease from the audit probability in an IRS district before 2000 to the audit probability in the district after the reorganization. The post-reorganization probability is obtained by using the federal level size-based audit probabilities weighted by the pre-2000 frequencies of audits in a respective asset-size class in a district. The variable *Post* is a dummy variable equal to one for years after 2000. We use a dummy variable *Low Decrease* equal to one if the change in the audit probability is in the top tercile of the *Decrease Audit Prob* distribution. We include bank fixed effects as well as state-year fixed effects in all columns. We include size, leverage, revenue growth, and the audit percentage as control variables. We report robust standard errors clustered at the IRS District level in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable	**Interest Income**	**LLP**	**Commercial Growth**	
	(1)	(3)	(3)	(4)
Low Decrease × Post	0.0094***	-0.0015***	0.0631**	0.0538*
	(0.0033)	(0.0005)	(0.0274)	(0.0280)
Low Decrease × Post				0.0213*
× Low Leverage				(0.0107)
Controls & Bank FE	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes
Observations	34,465	34,465	24,332	24,332
Adjusted R-squared	0.901	0.504	0.246	0.247

Table A.3: Reverse Causality

Panel A of this table presents regression results of audit probabilities at the district level over 1992–2000. We use bank characteristics and industrial firm characteristics aggregated at the district level. Odd columns use concurrent audit probabilities (or changes) while even columns use one year ahead audit probabilities. We estimate the model in levels (Columns (1) and (2)) as well as in changes (Columns (3) and (4)). Panel B of this table replicates the main results from Tables 3, 4, and 5 but uses the residual from a regression following the specification from Column (1) but without bank variables. We report robust standard errors clustered at the IRS District level in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Explaining Audit Percentages

	Level Estimation		**Change Specification**	
	Percent Audited$_{t}$	Percent Audited$_{t+1}$	Percent Audited$_{t,t-1}$	Percent Audited$_{t+1,t}$
	(1)	(2)	(3)	(4)
Mean Interest Margin	-0.0824	-0.2236	0.3042	-0.5198
	(0.3293)	(0.4043)	(0.3719)	(0.5349)
Interest Income	0.0729	0.0593	-0.0036	-0.0129
	(0.0499)	(0.0458)	(0.0631)	(0.0535)
Bank Size	-0.6840	-0.4429	0.3441	0.9916
	(0.4192)	(0.4149)	(0.3725)	(0.6463)
Bank Liabilities	0.6335	0.3933	-0.3089	-0.9392
	(0.4005)	(0.4078)	(0.3648)	(0.6377)
Bank LLP	-0.0139	-0.0095	0.0107	-0.0018
	(0.0116)	(0.0102)	(0.0109)	(0.0092)
Bank NPL	0.0073	0.0083	-0.0166	-0.0335
	(0.0204)	(0.0222)	(0.0148)	(0.0204)
Growth in Commercial	-0.0108	0.0086	-0.0104	0.0116
	(0.0128)	(0.0197)	(0.0143)	(0.0122)
Bank ETR	0.2483*	0.2823**	-0.4562**	0.4223*
	(0.1341)	(0.1325)	(0.1848)	(0.2435)
Unemployment	0.0136**	0.0184**	-0.0069	0.0144
	(0.0067)	(0.0071)	(0.0118)	(0.0141)
HPI	-0.0012***	-0.0009*	-0.0012	0.0009
	(0.0004)	(0.0005)	(0.0012)	(0.0011)
PI (Industry)	-0.0040	0.0003	-0.0165*	0.0092
	(0.0120)	(0.0147)	(0.0084)	(0.0089)
Sales (Industry)	-0.0491*	-0.0371	0.0532	0.0165
	(0.0281)	(0.0310)	(0.0379)	(0.0371)
CapEx (Industry)	0.0162	-0.0136	0.0356	-0.0670**
	(0.0265)	(0.0313)	(0.0252)	(0.0266)
LT Debt (Industry)	0.0226	0.0365**	-0.0440	0.1019**
	(0.0143)	(0.0170)	(0.0295)	(0.0396)
ETR (Industry)	-0.2089	-0.1960	-0.0198	0.1537
	(0.1251)	(0.1300)	(0.2566)	(0.2607)
Year FE	Yes	Yes	Yes	Yes
Observations	254	223	221	191
Adj. R-squared	0.444	0.452	0.048	0.123

Panel B: Replicating Main Results using Residual Audit Percentage			
	(1) **Interest Income**	(2) **LLP**	(3) **Commercial Growth**
Residual Audit $Perc_t$	0.0012***	-0.0008**	0.1934**
	(0.0004)	(0.0003)	(0.0717)
Controls	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes
State-Cycle FE	Yes	Yes	Yes
Observations	331,574	331,574	48,384
Adj. R-squared	0.526	0.226	0.098

Table A.4: Tax Enforcement and Industrial Firms Outcomes

This table presents regression results on performance and financial reporting quality of industrial firms over 1992–2000 using annual data from Compustat. We use *Discretionary Accruals* and *Accrual Quality* as dependent variables following Hanlon et al. (2012). We use the modified Jones model to measure *Discretionary Accruals* (Jones 1991, Dechow et al. 1995). The measure of *Accrual Quality* is based on Dechow and Dichev (2002) with the modifications by McNichols (2002). The main independent variable is the audit percentage in the firm's asset size class. Control variables are the firm-level variables from Hanlon et al. (2012), Table 3. In Columns (2) and (5), we include firms with total assets between $10 million and $100 million. Columns (3) and (6) present results using observations with total assets above $100 million. We include IRS-District fixed effects and industry-year fixed effects. We require firms to have at least three observations of the respective dependent variable. We report robust standard errors clustered at the IRS District level in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Dependent Variable	**Discretionary Accruals**			**Accrual Quality**		
	(1)	(2)	(3)	(4)	(5)	(6)
	All Firms	Medium	Large	All Firms	Medium	Large
Audit Percentage	-0.0305***	-0.0491**	-0.0145***	0.6359**	1.8333**	0.1002
	(0.0048)	(0.0188)	(0.0043)	(0.2633)	(0.7319)	(0.2817)
Difference between		-0.0346*			1.7331**	
Medium and Large		[1.74]			[2.28]	
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry–Year FE	Yes	Yes	Yes	Yes	Yes	Yes
IRS District FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	24,879	10,997	13,880	9,621	4,603	5,016
Adj. R-squared	0.173	0.135	0.188	0.262	0.234	0.191

Table A.5: IRS Audits and Bank Tax Avoidance

This table presents the regression results on bank performance over 1992–2000. In Columns (1) and (2) (Columns (3) and (4)) we use the current (the future) ETR as dependent variable. The unit of observation in s the bank-year. Independent variables are defined in Appendix A. In Columns (1) and (3), we include year, bank, and state-cycle fixed effects. We further include state level control variables defined in Table 1. Columns (2) and (4) include bank fixed effects as well as state-year fixed effects. We report robust standard errors clustered at the IRS District level in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	ETR_{t+1}		**ETR_{t+1}**	
	(1)	(2)	(3)	(4)
Percent Audited Size	-0.0057	-0.0063	-0.0072	-0.0060
	(0.0064)	(0.0067)	(0.0052)	(0.0059)
Bank Controls	Yes	Yes	Yes	Yes
State Controls	Yes	No	No	No
Bank FE	Yes	Yes	Yes	Yes
Year FE	Yes	No	No	No
State-Cycle FE	Yes	No	No	No
State-Year-FE	No	Yes	Yes	Yes
Observations	78,581	78,581	75,246	75,246
Adj. R-squared	0.393	0.394	0.421	0.421