Does tax enforcement affect the cost of bank loans? Evidence from the United States

Abstract

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We examine the relationship between tax enforcement and the cost of bank loans in the US syndicated market. We measure tax enforcement by the rate of IRS audits and find that it decreases bank loan spreads. This finding holds in a series of sensitivity tests, including alternative IRS tax enforcement measures, instrumental variable regressions, panel data estimations and a quasi- experimental framework of the Section 404b of the Sarbanes-Oxley Act (SOX). We also find that the negative effect of IRS tax enforcement on loan spreads strengthens for smaller corporations. In addition, we show that stringent IRS tax enforcement decreases the probability that loan contracts will contain covenants. Taken together, these findings suggest that banks acknowledge on tax enforcement its informational role in the debt market.

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1. Introduction

A stream of studies provides evidence that tax enforcement acts as a monitoring mechanism that plays an informational role for participants in the financial markets. These studies find that stringent tax enforcement helps in alleviating information asymmetries between firms and investors and therefore contributes to lower cost of equity (El Ghoul et al., 2011), lower cost of bonds (Guedhami and Pittman, 2008) and a reduction in the probability of a stock price crash (Bauer el., 2017). In this paper, we contribute to this stream of the literature by examining the relationship between Internal Revenue Service (IRS) tax enforcement and the cost of bank loans in the US syndicated loan market. In particular, we attempt to provide an answer to the following question: *Does IRS monitoring reduces the cost of bank loans for US firms?*

The theoretical premise of the ability of tax enforcement to alleviate information asymmetries between firms and investors or creditors rests on its function as an external corporate governance mechanism that limits the potential for managerial rent seeking behaviour (Desai et al., 2007). Stricter tax enforcement reduces the ability of firms' managers to engage into risky tax positions that are a source of information asymmetries. In order to prevent detection from the tax agency, risky tax positions entail complexity and information obfuscation. The complex and opaque environment that stems from risky tax positions increases information asymmetry and facilitates managerial rent seeking (Kim et al., 2011). Therefore, strict tax enforcement, by reducing the ability of managers to engage in risky tax positions and their ability for rent seeking behaviour (Desai et al., 2007; Desai and Dharmapala, 2009), improves the information available on the firm to outside investors or creditors (Desai et al., 2007; Hanlon et al. 2014). The empirical evidence that stricter tax enforcement reduces the cost of equity (El Ghoul et al., 2011) and the cost of bonds (Guedhami and Pittman, 2008) is consistent with this notion.

On the other hand, government agencies such as the IRS have been ineffective in identifying financial and accounting fraud scandals (e.g. the Enron case). This casts some doubt on the ability of such agencies to play an informational role for participants in the capital and debt markets (Erickson et al., 2004; Dyck et al., 2010). Furthermore, government agencies often lack appropriate resources and staffing levels that may diminish their ability as a monitoring mechanism (Jackson and Roe, 2009; Hanlon and Heitzman, 2010). For these reasons, Hanlon and Heitzman (2010) and Hanlon et al. (2014) call for more research about the extent to which tax enforcement could act as an external corporate governance mechanism that reduces information asymmetries issues. We follow their suggestion and investigate the unexplored yet effect of IRS tax enforcement on the cost of bank loans in the US syndicated loan market.

Focusing this study on the effect of IRS tax enforcement on the cost of bank loans is important for several reasons. The first is that debt is the most important source of new financing for corporations in the US. Around 75% of new corporate financing in the US comes in the form of debt (Contessi et al., 2013). The majority of this new corporate debt financing comes in the form of bank loans even for large public corporations (Barath et al., 2008; Hasan et al., 2014). It is therefore important from a managerial and public policy standpoint to examine the relationship between IRS tax enforcement and the cost of bank loans for US corporations.

Secondly, bank loans provide a challenging setting in which to examine the ability of tax enforcement to act as an external form of corporate governance that alleviates information asymmetries in debt markets. The empirical evidence of such an ability so far comes from the bond market (Guedhami and Pittman, 2008). However, banks, in comparison with bond investors, are more efficient in solving informational asymmetry issues through screening and monitoring (De Fiore and Uhlig, 2011). This is because banks enjoy scale economies and comparative advantages in the production of information and in monitoring debt (Diamond 1984). Banks also have stronger incentives to invest on the acquisition of information on borrowers because of their comparatively large stake in borrower funding (Boot and Thakor, 2009). Since holders of corporate bonds are more fluid and dispersed, their incentive to engage in monitoring is smaller than in the case of loans (Amihud et al., 1999). Additionally, banks have access to inside (private) information on the borrower firm that bond investors do not have (Fama, 1985; Dass and Massa, 2011). The above discussion renders bank loans a hard testing ground for the ability of strict tax enforcement to act as an external corporate governance mechanism that could reduce information asymmetry issues and thus lead to lower cost of financing for corporations.

Thirdly, the IRS has faced lately severe cuts in terms of its financing and its resources. According to the Center on Budget and Policy Priorities (CBPP) the IRS has experienced a 17% cut of its federal funding over the 2010-2016 period[3]. This has forced the IRS to cut substantially its workforce, employee training, and the upgrade of its IT systems over the same period. For example, over the 2010-2016 period the IRS has experienced a 17% reduction of its enforcement staff from 50,400 agents in 2010 to 38,800 in 2016 (Marr and Murray, 2016). The new US administration plans to continue the defunding of the IRS as President Trump's first budget blueprint calls for a further reduction of $239m in the budget of the agency (The

[3] https://www.cbpp.org/research/federal-tax/irs-funding-cuts-compromise-taxpayer-service-and-weaken-enforcement

Hill, 2017). Recent empirical evidence from Nessa et al. (2016) shows that fewer IRS resources weaken the tax enforcement process. In particular, the authors find that cuts in the IRS budget and in the number of IRS enforcement agents leads to a decrease in the rate of tax audits. By examining the effect, if any, of IRS tax enforcement on the cost of banks loans in the US we contribute to the public policy debate about the future of the IRS and its usefulness to the US economy.

To examine the effect of IRS monitoring on the cost of bank loans we use a large sample of 9,971 syndicated loan facilities initiated for US public firms over the 1992-2016 period. We follow the literature and measure the cost of bank loans as the *"all-in-spread"* which is the interest payment in basis points above the London Interbank Offered Rate (LIBOR) plus the annual fee (in basis points) for a loan facility that a firm obtains (Hasan et al., 2014; Delis et al., 2017). To capture IRS tax enforcement we employ the yearly rate of face-to-face IRS corporate audits. These audit rates utilise information from IRS records about the corporate tax returns received and audits completed by firm size class (there are eight classes based on the size of firms' assets) each year. Thus, this measure of IRS tax enforcement varies with firm size class and the calendar year. We source the IRS audit rate data from the Transactional Records Access Clearing House (TRAC) of Syracuse University. TRAC obtains its data directly from the IRS and in particular, from the same system that the agency is using for its own internal use and for its communication of information to the public and the US congress (Hanlon et al., 2014). This measure of IRS tax enforcement has been widely used in the literature (Guedhami and Pittman, 2008; El Ghoul et al., 2011; Hoopes et al., 2012; Hanlon et al., 2014; Bauer et al., 2017) to gauge the levels of tax enforcement in the US. In addition, we use an array of alternative IRS tax enforcement proxies, that we also source from TRAC, to supplement the analysis. These measures include yearly data on IRS staffing levels, IRS criminal penalties as well as IRS civil and criminal litigations.

After controlling for several firm and loan characteristics, the main finding of this study is that IRS tax enforcement exerts a negative effect on the cost of bank loans. In particular, our estimates show that an increase of the IRS audit probability from 24.7% (25^{th} percentile) to 37.3% (75^{th} percentile) results into an around 10 basis points reduction of bank loan spreads. Taking into account the average loan size ($433 million) and the average loan maturity (4 years) in our sample, this reduction in bank loan spreads translates into $1.8m of interest savings on average. This finding provides empirical support to the role of stringent tax enforcement as an external corporate governance mechanism that reduces information asymmetries between firms and outsiders (in our case banks). We also find that the beneficial

effect of stringent IRS monitoring on the cost of banks loans is more important for comparatively smaller corporations (firms with assets less than $250m). Smaller corporations have a poorer information environment and weaker corporate governance and internal control mechanisms (Beck and Demirguc-Kunt, 2006; Doyle et al., 2007; Boone and White, 2015). Therefore, the role of IRS monitoring in alleviating information asymmetries between firms and banks could be more important for smaller corporations. Besides the cost of bank loans, we also provide evidence that tax enforcement affects the non-price terms of bank loans. In particular, we find that stringent IRS monitoring reduces the likelihood of the presence of covenants in bank loan contracts.

The main finding of this study that stringent IRS monitoring reduces the cost of bank loans is robust in a number of alternative specifications and tests. These include the use of alternative firm size measures[4], alternative IRS tax enforcement variables, a cross-sectional identification strategy based on relationship lending, instrumental variable estimations that address potential endogeneity issues and fixed and random effects panel estimations that address unobserved firm heterogeneity. In addition, we take advantage of the recent findings of Bozanic et al. (2017) that the introduction of legislation that improves public corporate disclosure facilitates IRS monitoring and provide some evidence from a quasi-experimental setting. We examine whether banks reduce loan spreads after the introduction of the Section 404b of Sarbanes-Oxley (SOX) Act. This section of the SOX act, which was enacted in 2002, requires firms to obtain an independent auditor attestation on the management evaluation of the efficacy of the company's internal controls over financial reporting. Using a difference-in-difference analysis we find that firms (accelerated filler firms) that comply with the Section 404b of the SOX Act report a significant decrease in loans spreads after its implementation in comparison with firms (non-accelerated filler firms) that are exempted from this regulation. This finding is consistent with the notion that banks reduce the cost of the bank loans after the introduction of legislation that facilitates IRS monitoring.

Altogether, our study shows that IRS tax enforcement has an important relationship with bank loan cost, which translates into lower borrowing costs. This study contributes to the literature in two ways. The first is that we add to the extant literature that examines the external corporate governance role of tax enforcement (Desai and Dharmapala, 2009; Hoopes et al., 2012; Hanlon

[4] Audit rates are based on IRS records on the corporate tax returns received and audits completed by firm size class in terms of asset each year. This allows the IRS audit rate measure to vary with firm size class and the calendar year. Thus, we assess whether asset level is spuriously responsible for the relationship between tax enforcement and the cost of bank loans by using alternative measures to gauge firm's size as in Bauer et al. (2017).

et al., 2014; Kubick et al., 2017). In particular, we add to the stream of this literature that investigates the role of tax enforcement as a corporate governance mechanism that reduces information asymmetries and thus the financing costs for firms in the bond and equity markets in a US setting (Guedhami and Pittman, 2008; El Ghoul et al., 2011; Bauer el., 2017). We complement these studies by finding that IRS tax monitoring reduces cost of bank loans. This finding is important in the context of this literature because it shows that even banks, that are more efficient than other debt-holders in solving informational asymmetry problems through screening and monitoring, value the informational role of the IRS. The second contribution of this study is that it adds to the burgeoning literature that investigates the determinants of the cost of bank loans. In particular, several studies find that internal corporate governance mechanisms matter for the cost of bank loans (see e.g. Fields et al., 2012; Francis et al., 2012; Francis et al., 2013). This study provides evidence that external corporate governance mechanisms, such as tax enforcement, are also important for the pricing of bank loans. Furthermore, this study contributes also to the limited literature that examines the effect of taxation related issues to the cost of bank loans (see e.g. Hasan et al., 2014). Finally, this study adds to the public policy debate about the importance of the IRS for the US economy. Our findings show that the IRS exerts a positive spillover to the US economy by lowering the cost of bank financing for US corporations.

The rest of the paper is organised as follows. Section 2 discusses some theoretical considerations and develops the hypotheses. Section 3 presents the data, research design and descriptive statistics. Section 4 presents the main findings and the sensitivity analysis, whilst Section 5 concludes.

2. Theoretical considerations and hypotheses development

In this section, we motivate our research hypotheses by focusing on the role of the tax enforcement as an external corporate governance and monitoring mechanism that could alleviate information asymmetries between borrowers and lenders in the US syndicated loan market. In brief, we use theory and previous empirical evidence to build on the notion that firms' borrowing costs (loan spreads) are lower under more stringent tax enforcement. We also foresee that the negative relationship between IRS monitoring and the cost of bank loans is stronger for smaller corporations.

Information asymmetries between borrowers and lenders is an important factor for the pricing and the contract terms of loans. Banks demand a higher interest and impose tighter contract terms, such as covenants, for the loans they supply to firms with poorer information quality (Graham and Qiu, 2008; Hollander and Verriest 2016; Houston et al., 2017; Prilmeier, 2017).

A source of information asymmetries between firm insiders and outsiders is the rent seeking behaviour of the former. A firm's insiders, such as managers and controlling shareholders, may use the company's resources for their own benefit. Therefore, they have incentives to distort the information on the real financial performance of the company, such as earnings, in order conceal their rent seeking activities from the firm's outsiders (Leuz et al., 2003).

The engagement of a firm in aggressive tax strategies and risky tax positions could be complementary with the ability of managers to distort the information that outsiders have on the firm because it facilitates their rent-seeking behaviour. Desai and Dharmapala (2006) posit that engaging into aggressive tax strategies requires complex structures and an opaque information environment in order to conceal the tax aggressiveness intention and to reduce the possibility of detection from the tax enforcement agency. Therefore, tax aggressive strategies and risky tax positions provide firm managers with the justification and the tools to distort the information environment of the firm (Kim et al., 2011). These tools comprise *inter alia* earnings manipulations, related party transactions and the hoarding of information (Desai and Dharmapala, 2006; Chen et al., 2010, Kim et al., 2011). Consistent with this argument, Wilson (2009) finds that tax shelter participant firms employ aggressive financial reporting practices. Taking the above into consideration, risky tax positions could facilitate managerial rent-seeking behaviour and thus result into higher information asymmetry issues between borrowing firms and lenders. Furthermore, if the engagement of a firm into risky tax positions facilitates managerial rent seeking then it could also lead to a decrease in firm value (Desai et al., 2007; Desai and Dharmapala, 2009)[5]. This in turn could hamper the ability of a firm to repay loans. Such information uncertainty and the need for intense monitoring may induce banks to charge a higher loan price to firms that engage in aggressive tax strategies (Hasan et al., 2014).

Tax enforcement could act as an external corporate governance mechanism that could lead to a reduction of information asymmetry issues that stem from aggressive tax strategies and risky tax positions. Desai et al. (2007), in their theoretical model, posit that outside stakeholders and the tax agency have the same interests in monitoring firm insiders. This, in turn, implies the presence of an informal bilateral agreement between firm outsiders and the tax authority. Intense monitoring from the tax agency could discourage firms from engaging into tax

[5] To be clearer, aggressive tax practices and risky tax positions could have a positive or a negative effect on firm value. This depends on whether savings from tax liabilities are put to more productive use or are diverted to firm managers in the form of rent extraction (Goh, 2016). In case the second effect prevails, then risky tax positions could reduce firm value as Desai et al. (2007) posit.

aggressive strategies and taking risky tax positions (Hoopes et al., 2012). Therefore, stringent tax enforcement could reduce the opportunity for managerial diversion of the firms' resources because risky tax positions empower the tools that managers could use to engage into rent-seeking activities (Kim et al., 2011). In support of this argument, Dyck and Zingales (2004) stress that since the government and its public agencies (tax authority) have a direct economic benefit in firms' tax returns, stringent tax enforcement may limit the opportunistic behaviour of insiders. Desai and Dharmapala (2007, 2009) also suggest that stricter tax enforcement could prevent firm insiders from misusing corporate income. This chilling effect of tax enforcement on the ability of managers to engage into rent-seeking actions could improve the information environment of a firm since the managers' justification for information obfuscation subsides. Therefore, the quality of information available to a firm's outsiders, such as the creditors, could improve when tax enforcement is strict (Desai et al., 2007; Hanlon et al. 2014). Moreover, if tax enforcement exerts a chilling effect on the diversion instincts of the managers then the value of the firm, and therefore its ability to repay the lenders, could improve. The disciplinary effect that stringent tax enforcement could exert on firm managers stems from the ability of the tax authority to impose huge direct material penalties to firms (Wilson, 2009)[6]. It also stems from the indirect reputational and political costs that firms could incur due to tax aggressive behaviour (Hanlon and Slemrod, 2009, Mills et al., 2010; Dyreng et al., 2016). The above discussion suggest that banks could value the informational and monitoring role of tax enforcement and reduce the cost of bank loans.

In the US context, the empirical evidence about the ability of the tax enforcement agency, the IRS, to alleviate information asymmetries issues in the financial markets is inconclusive. The studies of Gedhami and Pittman (2008) and of El Ghoul et al. (2011) provide evidence that stringent IRS monitoring has a negative association with the cost of bonds and the cost of equity respectively. These findings are consistent with the informational and monitoring role of the IRS. However, the other side of the coin suggests that firm outsiders such as creditors and financial markets participants may find it difficult to accept the view that tax enforcement agencies could reduce information asymmetry issues. This view stems from the fact that the IRS has failed to detect serious accounting and financial misreporting scandals (Erickson et al., 2004; Dyck et al., 2010). Furthermore, public enforcement agencies, such as the IRS, may

[6] For example, in 2007 the company Merck agreed to pay to $2.3 billion in back taxes, penalties and interest in order to settle a tax dispute with the IRS. Other famous tax disputes that resulted in firms paying huge fines to the IRS include the cases of GlaxoSmithKline in 2006 ($3.4 billion) and of Astrazeneca in 2011 ($1.1 billion). https://www.irs.gov/newsroom/merck-agrees-to-pay-irs-23-billion

possess limited firm-specific knowledge and lack of auditing resources/expertise that casts doubt on their monitoring capabilities (Jackson and Roe, 2009; Hanlon and Heitzman, 2010). Moreover, the policymakers who are in charge of public enforcement agencies, such as the IRS, maybe reluctant to impose serious penalties, both financial and reputational, that aim to discourage tax aggressive strategies and risky tax positions. Accounting firms also perceive valuable any information that could obtain by employing former IRS commissioners[7] and government tax experts in helping firms to avoid the possibility that the IRS will later challenge their tax returns (Larsen et al., 2007; Jiang et al., 2016).

The mixed previous evidence on the ability of the IRS to alleviate information asymmetries issues motivates us to investigate if firms subject to tougher tax enforcement, i.e., higher IRS audit rates, enjoy lower borrowing costs in the US syndicated loan market. We conjecture that the IRS plays an important informational and monitoring role that improves the information that banks have on borrowers and leads to a reduction in the cost of bank loans.

H1. US firms' bank loan spreads, ceteris paribus, will decrease when the probability of an IRS audit is higher.

We also develop a secondary hypothesis and posit that the negative effect of IRS monitoring on bank loan spreads strengthens for smaller corporations. The theoretical model of Diamond (1985) postulates that large firms have stronger incentives for information production because they enjoy a higher marginal benefit of disclosure in comparison with smaller firms. Several studies show that smaller firms have poor information environments and face high information asymmetry issues (e.g.; Bhattacharya et al., 2013, Muslu et al., 2014; Lambert and Verrechia, 2015). Recent empirical evidence from the US suggests that when banks face information problems with a specific corporate borrower, e.g. when they decide to price a loan for a smaller firm, they complement the information of the firm's financial statements with the firm's tax returns (Minnis and Sutherland, 2017). We conjecture that banks would perceive that the credibility of the information available on the tax returns is higher when there is a higher probability of an IRS audit.

Furthermore, several studies point out that smaller firms, because of limited resources, suffer from weak internal corporate governance structures and controls with regards to their financial reporting (Ge and McVay, 2005; Doyle et al., 2007; Ge et al., 2017). This may aggravate managerial rent-seeking behaviour and lead to a further deterioration of the information

[7] For instance, Kevin Brown was a former IRS commissioner who joined PricewaterhouseCoopers in 2008 as a principal and co-leader of PwC's Tax Controversy and Regulatory Services practice.

environment of the firm. This could be the case because, as discussed previously, the distortion of the true financial position of the firm is a way through which managers would try to conceal their rent-seeking behaviour (Leuz et al., 2003). Thus, more intensive IRS monitoring could be particularly important for the integrity of the information that smaller corporations produce.

Finally, the IRS has shown over the years a shift in auditing smaller corporations as the Transactional Records Access Clearinghouse (TRAC) states in 2008[8] *'...By ordering its revenue agents to concentrate on the smaller corporations that normally take a lot less time to audit, the agency was able to push up the overall number of corporate audits...'* Scholz and Wood (1998) in their theoretical model explain the notion of the IRS allocating resources based on some criteria, one of which is efficiency. The authors suggest that the IRS would prefer to shift resources towards a group of firms that for a given enforcement expenditure the IRS will earn easier returns simply because it can detect with less effort cases of non-compliance. Smaller firms are simpler to audit due to smaller scale and less complex operations.

Based on the above discussion, stringent IRS monitoring could be particularly important for alleviating information asymmetry issues between banks and smaller firms. Therefore, we state our second hypothesis as follows:

H2. US firms' bank loan spreads, ceteris paribus, will decrease more for smaller corporations when the probability of an IRS audit is higher.

3. Data, Research Design and Descriptive Statistics

3. 1 Sample Selection

We source data on syndicated loans from the Thomson One Banker database. This database provides a wide coverage of syndicated loans in the US since 1985. It includes comprehensive information on the characteristics of each loan facility (borrowing loan spread, amount, maturity, covenants etc.) and identifies the firm that receives each loan. This allows matching the firms' identities from Thomson One Banker to Compustat to obtain firms' accounting and financial information. A firm could obtain multiple loan facilities in the same year and we treat each loan facility as an individual observation[9]. We also eliminate from the sample all financial services firms (SIC codes 6000-6999) because they are subject to heavy regulation and their terms of borrowing may differ significantly from the rest of the firms in the sample. This

[8] http://trac.syr.edu/tracirs/newfindings/v13/

[9] We follow previous studies and perform our analysis at the loan facility level and not at the loan package deal level (e.g. Hasan et al. 2014; Hasan et al. 2017). Each loan package could contain more than one loan facility. Two loan facilities, even in the case they are part of the same loan package deal, could have different characteristics such as size, maturity and loan type. Therefore, ignoring the differences between loan facilities, even when they are part of the same loan package deal, could introduce estimation bias.

matching process yields 15,858 loan facilities for 2,448 unique borrowing firms for the 1985-2016 period. At the firm-year level, in combination with the availability of the IRS tax enforcement data that only commences in 1992, the sample comprises up to 9,971 firm-year observations for the 1992-2016 period. Table 1 provides the definitions and the calculation details of the variables that we use in the analysis.

Table 1

3. 2 Measures of IRS Tax Enforcement

The primary IRS tax enforcement measure we use in this study relies on data that we obtain from the Transactional Records Access Clearing House (TRAC). TRAC is a non-profit research institute associated with Syracuse University that collects data directly from the IRS. We source TRAC data on yearly IRS face-to-face corporate audit rates to use them as the main measure of IRS tax enforcement. These audit rates make use of information from the IRS about the corporate tax returns received and audits completed by eight firm size classes, in terms of total assets, each year[10]. Thus, the IRS audit rates exhibit variability both in terms of firm size class and in terms of calendar year. In particular, the variable *Audit rate* stands for the number of corporate tax return audits completed in year *t* for a given IRS firm size class, divided by the number of the received corporate tax returns in the previous year (*t-1*) for the same firm size class. Therefore, the IRS audit rate captures the probability of firm belonging to a specific firm size group to experience a face-to-face IRS audit in a given year. Our identification strategy includes contemporaneous IRS audit rates on the assumption that firm and bank managers predict future IRS audit rates on rational expectations and they do not make systematic errors.[11] We focus on the IRS *Audit rate* measure of tax enforcement because we are interested in capturing the managers' view that a firm will experience an IRS audit in a given year. Thus, even in case the IRS *Audit rate* does not capture accurately the probability of a firm audit, if it relates to the bank and firm managers' perception of the likelihood that a firm will experience an IRS audit, our analysis should be able to observe the relationship between IRS tax enforcement and the cost of bank loans[12].

[10] There are eight firm size classes based on the level of firms' assets. These are the following: Asset size of more than $250 million, $100-$250 million, $50-$100 million, $10-$50 million, $5-$10 million, $1-$5 million, $0.25-$1 million and $0-$0.25 million. Note that our sample includes observations from the seven biggest categories. This is because we did not find any firm in the smallest size class of $0-$0.25 million that has obtained a syndicated loan in the period under study.

[11] In robustness analysis, we have included lagged IRS audit rates as the actual IRS audit rates become available to the public with a delay. Reason being that there is a lag between the time that a firm reports its tax returns to the IRS and the time that the IRS completes its investigations (Graham and Tucker, 2006).

[12] Hanlon et al. (2014) posit that managers obtain information from several sources (channels) in order to develop rational expectations about the level of tax enforcement (i.e. the probability of an IRS audit). Briefly, these include

By covering the longest period (1992-2016) for which corporate IRS audit rates are available, we aim to enhance our inferences given the increased variability of audit rates during this timeframe. Reinforcing the reliability of this tax enforcement measure, the IRS Oversight annual reports submitted to the US Congress refer regularly to TRAC's corporate audit rate statistics. Furthermore, the IRS audit rates apply only to the US. This eliminates issues stemming from institutional differences that plague cross-country tax enforcement data (Hanlon et al., 2014). The credibility of the IRS audit rates as measure of tax enforcement is evident in its wide use by the government and several previous academic studies (Guedhami and Pittman, 2008; El Ghoul et al., 2011; Hoopes et al., 2012; Hanlon et al., 2014).

Yet, we complement our main tax enforcement variable with a number of federal-level proxies that capture IRS staffing levels, given that the IRS admits that its tax enforcement capacity diminishes when staffing levels are lower (Weisman, 2004; Rapperort, 2017). We source yearly data on the number of IRS employees, IRS revenue agents and IRS criminal investigators. Furthermore, some studies acknowledge the governance role of civil and criminal litigations (La Porta et al., 2006) and the reputational damage of financial penalties on firms (Armour et al., 2017). Therefore, we supplement our analysis with four additional IRS tax enforcement proxies. We use the number of IRS criminal referrals, IRS criminal prosecutions and IRS civil penalties for tax fraud and negligence against corporations. Following previous studies that employ these alternative IRS tax enforcement measures (e.g. Guedhami and Pittman, 2008) we normalise them by the number of yearly corporate tax returns, apart from the IRS criminal tax prosecutions measure that we normalise it by the number of yearly criminal referrals. Information on these alternative IRS tax enforcement variables are available for the 1992-2003 period. This reduces our sample to 3,312 observations. Nevertheless, we conjecture that additional estimations with these alterative measures enhance the credibility of the analysis.

3. 3 Baseline Regression Model

We test the prediction that stringent tax enforcement (higher IRS audit rates) decreases the cost of bank loans using the following equation:

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the following: information on proposed IRS budgets that are on public record, news about structural and leadership changes in the IRS, hiring former IRS employees, formal or informal meetings with IRS officials, IRS statements that imply more stringent tax enforcement, trends in government revenue, historical audit rates released directly from the IRS and agencies (TRAC) that monitor and publish data on IRS activities. Furthermore, Hoopes et al. (2012) provide evidence from interviews with managers that the probability of an IRS audit influences a firm's tax related strategies.

$$Log(loan\ spread)_t = f(audit\ rate_t, firm\ correlates_{t-1}, loan\ characteristics_t, industry\ dummies\ and\ yeardummies),$$

Where $Log(loan\ spread)_t$ is the natural logarithm of the *"all-in-spread drawn"* (AISD), which is the loan interest payment in basis points over the LIBOR plus the annual fee for a loan facility that a firm obtains in year t. $Audit\ rate_t$ is the probability that a firm in a given firm size class will experience a face-to-face IRS audit in year t. We control for several firm characteristics including size, leverage, profitability, tangibility, liquidity, and the cash effective tax rate following previous empirical studies (Graham et al., 2008; Kim et al., 2013; Hasan et al., 2014; Lian, 2017)[13]. We use lagged information on the firm correlates from one year before the initiation of the loan to somewhat ease possible endogeneity problems. Furthermore, we include loan-level attributes to control for loan size, loan maturity and a series of dummies designating loan type and loan purpose. We also use dummies to control for year effects and industry effects following the 2-digit Standard Industrial Classification (SIC).

3. 4 Descriptive Statistics and Correlations

In Panel A of Table 2, we present the IRS audit rates by firm size class and time as obtained from the TRAC database. We observe that there is considerable variation both across time and across the different firm size classes in the same calendar year. For example, the IRS audit rate for firms that fall within the largest size class (i.e. firms with an asset size larger than $250 million) in 2005 is 42.5%, while in 2016 it plummets to a record low of 17.8%. In Panel B we show the number of firm-year observations in each of the IRS firm size class/year group. It is clear that the majority of our sample (84.2%) comprises large firms with assets beyond $250 million as in previous studies (Guedhami and Pittman, 2008; Hoopes et al., 2012). Furthermore, our sample exhibits a steady distribution over time, as each year contributes no more than 8.3% of the total number of observations.

Table 2

Table 3 reports the summary statistics of the main explanatory variables that we include in the empirical specifications. Loan characteristics show the average loan size and loan spread to be $433 million and 200 basis points respectively. Furthermore, the mean loan maturity is around 4 years. These descriptive statistics are in line with the studies of Graham et al. (2008) and Hasan et al. (2014). Table 4 reports the Pearson correlations coefficients among the main variables in our analysis. This preliminary evidence suggests that *Audit rate* and *Loan spread*

[13]Table 1 provides detailed definitions of the control variables that we employ in the econometric specifications.

are negatively associated at the 1% significance level consistent with our prediction in hypothesis *H1*. Altogether, we observe that *Audit rate* and the rest of the explanatory variables exhibit a low correlation. This attenuates collinearity concerns that could influence our estimations. Finally, the control variables correlate significantly with bank loan spreads and these correlations are in line with those available in previous empirical work (Graham et al., 2008; Bharath et al., 2009; Hasan et al., 2014).

Table 3 and Table 4

4. The Relationship between IRS Tax Enforcement and the Cost of Bank Loans

4.1 Baseline Regressions

Table 5 reports the baseline estimations with regards to our main hypothesis *H.1* that the US firms enjoy lower bank loan costs when tax enforcement is more stringent. We use ordinary least square regressions (OLS) with robust standard errors and within-firm clustering.[14] Our models show a good fit with 50% adjusted R^2 on average. In model 1 of Table 5 we control for firm characteristics while in model 2 of Table 5 we control for both firm and loan characteristics. The coefficients on *Audit rate* $_t$ are significant at the 1% level and negative (-0.00709, -0.00796) in model 1 and model 2 of Table 5 respectively. We report similar findings when we use the extrapolation method to obtain IRS audit rate values for the 1985-1991 period (model 3 of Table 5) as the IRS audit rate data commence in 1992, and when we replace *Audit rate* $_t$ with its one-year lag *Audit rate* $_{t-1}$ in a successive regression (model 4 of Table 5).

Table 5

Overall, these results lend support to our main *H.1* hypothesis that firms' bank loan spreads decrease when IRS tax enforcement is more stringent. Based on the estimations of model 3 in Table 5, economically, our findings imply that raising the probability of IRS audit from 24.7% (25th percentile) to 37.3% (75th percentile) leads to a 10 basis points decrease of bank loan spreads. Another way to assess the relevance of these findings is by looking at the mean borrowing firm annual interest savings based on the average loan size of our sample, which is $433 million and the mean time to maturity that is around 4 years. As per our estimates, a shift of the IRS *Audit rate* $_t$ from the 25th percentile to the 75th percentile gives around $1.8 million in savings (1.74=433*0.00100296*4). Our findings are generally comparable with other studies that stress the importance of information asymmetries between borrowing firms and lenders for the cost of bank loans (Bharath et al, 2009; Costello et al., 2011; Francis et al.,

[14] We aim to avoid spurious correlation for firms that may obtain more than one loan facility in the same year.

2012). Additionally, our results about the firm control variables show that larger firms, with more tangible assets, higher profitability and less leverage enjoy lower borrowing cost in line with previous studies (Graham et al., 2008; Bharath et al, 2009; Hasan et al., 2014). Furthermore, large loans with shorter time to maturity enjoy lower bank loan spreads. These findings are consistent with Chava et al. (2008) who suggest that banks exposed at the risk of a longer period to maturity charge higher loans spreads as a compensation.

Overall, the findings from the baseline models in Table 5 show that IRS audit rates and bank loan costs are negatively related. These results, in line with our main hypothesis *H.1*, suggest that banks perceive IRS tax enforcement as an effective external corporate governance mechanism that could exert a chilling effect on the rent-seeking instincts of firm managers and reduce information asymmetries issues. Thus, banks charge lower prices for the loans they provide to firms when tax enforcement is more stringent. This finding provides evidence from the bank loan market that IRS tax enforcement plays an important monitoring role that reduces the cost of financing for US firms. Therefore, it complements the studies that provide such evidence from the US bond and equity markets (Guedhami and Pittman, 2008; El Ghoul et al., 2011).

In Table 6, we provide estimations that focus on smaller corporations. This means that we exclude from these estimations observations from firms that fall within the largest IRS defined firm size group (i.e. firms with assets of more than $250 million). The aim of this exercise is to investigate if the negative relationship between the IRS audit rates and the cost of bank loans persists and whether it is indeed stronger for smaller corporations as we conjecture in our secondary *H.2* hypothesis[15]. We provide estimations for models that focus on firms that fall within the IRS firm size groups, in terms of assets, of $250-$100 million (models 1 and 2 of Table 6) and of $100-0.25 million (models 3 and 4 of Table 6). The findings from the models that control for both firm and loan characteristics (model 2 and model 4 of Table 6) show that for firms in the $250-100 and $100-0,25 million asset size groups the coefficients of the *Audit rate* $_t$ variable are negative (--0.0385, -0.228) and significant at the 1% level respectively. Comparing the findings of Table 6 with the ones in Table 5 that also include the larger firms suggest, from an economical perspective, that the negative relationship between the IRS audit

[15] This exercise is also useful for identification purposes. Providing estimations from models that exclude the firms that fall within the largest IRS defined size class means that we also exclude the very large corporations for which the IRS audit is almost certain (El Ghoul et al., 2011). Such firms for example could be the very large corporations (asset value of higher than $5 billion) that belong to the IRS's Coordinated Industry Case (CIC) and are subject to constant IRS monitoring (El Ghoul et al., 2011).

rates and the cost of bank loans is stronger for smaller corporations. Thus, the results from the estimations in Table 6 provide empirical support to our secondary *H.2* hypothesis that stringent IRS tax enforcement is particularly important for the reduction of the bank loan spreads for smaller corporations. These results suggest that IRS monitoring becomes vitally important, in terms of the cost of bank loans, for smaller firms that exhibit a poorer information environment and weaker internal corporate governance structures in comparison with larger corporations (Bhattacharya et al., 2013; Ge et al., 2017).

4.2 Sensitivity Tests

4.2.1 Controlling for Alternative Firm Size Measures

The measure of IRS tax enforcement we employ, i.e. the IRS audit rates by firm size group, hinges on firm size (total assets). In the baseline estimations, we have controlled for firm size by including in the models the natural logarithm of total assets. In this section, we further examine whether our baseline findings that support our main *H.1* hypothesis are sensitive when we control for alternative measures of firm size (Bauer et al., 2017). Hence, in models 1 and 2 of Table 7 we replace the natural logarithm of total assets as measure of firm size with the natural logarithm of total equity and sales respectively. We find that both of these alternative firm size variables exert a negative and significant at 1% effect on bank loan spreads consistent with our baseline estimates. More importantly, the estimations in Table 7 continue to lend support to our previous findings that US firms benefit from lower bank loan spreads when IRS tax enforcement is more stringent. The coefficients of *Audit rate* $_t$ are significant at the 1% level and negative (-0.0105, -0.00791) in models 1 and 2 of Table 7 respectively. These findings are similar with our baseline results both in terms of statistical and economic significance. Consequently, our estimations show that the negative relationship between *Audit rate* $_t$ and firms' borrowing cost persists after controlling for alternative firm size measures.

Table 7

4.2.2 Alternative IRS Tax Enforcement Measures

Our research focuses on the effect of the IRS *Audit rate* $_t$, as of a proxy for IRS monitoring, on the cost of bank loans. Nevertheless, we supplement our analysis with a number of alternative IRS tax enforcement proxies that capture further aspects of IRS monitoring. The first set of these alternative IRS tax enforcement measures reflects the yearly levels of IRS staffing

(normalised by the yearly corporate tax returns). Since these alternative IRS tax enforcement measures do not hinge on firm size, as the IRS audit rates do, they provide further evidence about the effect of IRS monitoring on the cost of bank loans. The estimations that examine the effect of the IRS staffing variables on the cost of bank loans are available in models 1-3 in Panel A of Table 8. Note that in these estimations we do not include time effects. This is because time effects would exhibit perfect collinearity with the IRS staffing measures that display only yearly variation. In Panel A of Table 8 we show that the coefficients of IRS employees (model 1), IRS revenue agents (model 2) and IRS criminal investigators (model 3) enter the regressions negatively and significantly at the 1% level. These findings suggest that tax enforcement gauged by the IRS staffing levels decreases the cost of bank loans. Therefore, they are consistent with our main *H.1* hypothesis that banks value IRS tax monitoring when they price loans to borrower firms. Additionally, these findings align with the government acknowledgement that staffing levels play a key role in strengthening tax enforcement (Weisman, 2004; Rapperort, 2017).

Table 8

The second set of alternative IRS tax enforcement variables comprises the normalised number of IRS criminal referrals, IRS criminal prosecutions and IRS civil penalties for tax fraud and negligence against corporations. The estimations that employ this second set of alternative IRS tax enforcement variables are available in models 4-7 in Panel A of Table 8. We find a negative relationship between IRS civil penalties enacted against corporations and bank loan spreads. The coefficients of IRS fraud penalties and IRS negligence penalties are negative and significant at the 1% level in models 4 and 5 in Panel A of Table 8 respectively. These results show that IRS tax enforcement in the form of civil penalty proxies decreases firms' borrowing cost supporting our earlier evidence. In models 6 and 7 in Panel A of Table 8 we also observe that the IRS criminal referrals and the IRS criminal prosecutions variables exert a negative and significant at the 1% level effect on the cost of bank loans. All in all, the findings in Panel A of Table 8 lend further support to the key role of IRS monitoring in diminishing information asymmetries between banks and borrowers.

One weakness of the estimations in Panel A of Table 8 is that we do not include time effects. As discussed, this is because these alternative measures of IRS tax enforcement exhibit only yearly variation. However, not including time effect might bias these estimates. Therefore, we proceed with a cross-sectional identification strategy that allows the inclusion of time effects in the models that employ these alternative IRS tax enforcement proxies. The extant literature supports that previous lending experience of a bank with a borrower reduces information

asymmetries (Ioannidou and Ongena, 2010; Schenone, 2010; Hu and Mao, 2017; Prilmeier, 2017). Hence, we posit that banks would perceive IRS tax enforcement especially important as an external corporate governance and monitoring mechanism for the pricing of loans to firms for which they do not exhibit a previous relationship, i.e. *"relationship lending"*. To this end, we create a non-lending relationship dummy *(NLR)* that takes the value of 1 in the case that a lead bank[16] of a syndicate has not provided a loan to the same borrower in the past five years before the current loan and zero otherwise[17]. As a next step, we employ models that include the non-relationship lending dummy *(NLR)* and its interactions with the alternative IRS tax enforcement measures. In these models, we include time effects and we drop the alternative IRS tax enforcement measures because they cannot be identified. However, the interactions of the alternative IRS tax monitoring variables and the non-relationship lending dummy *(NLR)* can be identified in the presence of time effects. We present the estimations of these models in Panel B of Table 8. There we show that the coefficients of the interactions between the alternative IRS tax enforcement measures and the *NLR dummy* $_t$ *are* negative and significant at the 5% expect for the interactions of IRS Fraud penalties $_t$ * NLR-dummy $_t$ and IRS Negligence penalties $_t$* NLR-dummy $_t$. These findings suggest that IRS tax monitoring is especially important for lowering the bank loans spreads when the information asymmetry between banks and borrowers is at higher levels (i.e. in the absence of a previous lending relationship).

4.2.3 Endogenous IRS Audit Rate

An assumption of the baseline OLS models in Table 5 is that the audit rate variable is exogenous to the cost of bank loans measure, which is the dependent variable. Hoopes et al. (2012) and Bauer et al. (2017) outline two reasons of why this is plausible. The first reason is that firm managers could exert limited influence on the audit probability rates since these depend on firm size class, time and their interaction (Guedhami and Pittman, 2008; El Ghoul el., 2011). This implies that the potential of a feedback effect, i.e. reverse causality, between the IRS audit rate and the cost of loans is constrained. Such a feedback effect would require a single firm to be able to affect the IRS auditing decisions with respect to the entire firm population in each firm size class (Hoopes et al., 2012). The second reason is that the intensity of IRS monitoring is highly dependent on the governmental political ideology, which could be

[16] In syndicated lending, the lead bank is the intermediary between the corporation that obtains the loan and the other participant lenders. The lead bank is the institution responsible to monitor the borrower (Ivashina, 2009; Prilmeier, 2017).

[17] Based on the study of Prilmeier (2017) we assume that a relationship ends if a borrower and a lead bank have not had a loan contact in the last five years because the maximum maturity of the loans in our sample is five years.

exogenous to firm outcomes such as the cost of the bank loans. Bagchi (2016) for example show that audit probabilities are significantly higher under Democratic administrations. Nevertheless, Hoopes et al. (2012) and Bauer et al. (2017) posit that one cannot exclude with certainty such reverse causality issue. For example, firm managers could engage into firm strategies, such as divestments or acquisitions, which could affect firm size and therefore the audit probability that their firms could face[18]. Furthermore, some firms might have very strong political influence and could affect, at least in theory, the IRS audit rates. To ease these concerns we relax the exogeneity assumption between the IRS audit rate and the cost of bank loans and proceed with a two-stage instrumental variable estimation. In the first stage, we obtain predicted values of *Audit rate* $_t$ using an OLS regression that comprises two instruments and all the control variables that we employ in our baseline analysis. In the second stage, we replace in our baseline model the IRS audit rate variable with its predicted values that we obtain from the first stage. For the first instrument we follow Guedhami and Pittman (2008) and employ lagged values of the IRS audit rates due to their explanatory power on *Audit rate* $_t$. A firm's probability of facing an IRS audit this year could also depend on the probability of an IRS audit in the previous years. More specifically, we opt for the four-year lagged values of the IRS audit rates as our first instrument because they satisfy the exclusion restriction[19] while they exert a strong explanatory power on the contemporaneous IRS audit rates. Furthermore, another reason for opting for the fourth lag of the audit rate variable is that the statute of limitations is applicable three after a corporation has filed its tax return (El Ghoul et al., 2011). For the second instrument of the IRS audit rates we follow Hoopes et al. (2012) and use the natural logarithm of the number of corporate tax returns in each asset size class filed in the previous year. The IRS audit rates are defined as the number of actual corporate returns audited by the IRS in each firm size class in a given year divided by the number of corporate tax returns filed in each firm size class in the previous year. Consequently, audit rates depend on fluctuations of tax returns filed across time in the same firm size group[20].

[18] We posit that this is not an issue of concern for this study because the number of observations of firms that change firm size class over the period of the study is limited. Moreover, in another robustness test that we present later in this study we find that our results hold when we exclude these observations.

[19] We run our baseline regression to observe the relationship between four-year lagged values of IRS audit rates and the cost of bank loans. We observe that there is no significant association between theses variables supporting in that way the suitability (in terms of satisfying the exclusion restriction) of the four-year lagged values of IRS audit rates as an instrument. These results are available upon request.

[20] Similarly with our first instrument, the four-year lagged values of the IRS audit rate, also in this case we run our baseline regression model including our second instrument; the natural logarithm of the one-year lagged values of tax returns by firm size class. We do not find a statistically significant association between this second instrument and the cost of bank loans. This finding supports the suitability (in terms of satisfying the exclusion restriction) of the natural logarithm of the one-year lagged values of filed tax returns by size class as an instrument

The numerator component (i.e. the number of corporate tax returns audited by the IRS) is contemporaneous with the dependent variable that captures the cost of bank loans and therefore more vulnerable to a feedback effect. The denominator component (i.e. the number of corporate tax returns filed in the previous year), which we use as the second instrument, reflects a lagged value with respect to the cost of bank loans and is therefore less susceptible to reverse causality issues. Furthermore, corporations are required to file their tax returns. This implies that it is not likely that a firm will try to influence its audit probability rate by not filing a tax return.

The findings from the two-stage instrumental variable estimation are available in model 1 of Table 9. The first stage results show that the two instrumental variables (the four-year lagged values of the IRS audit rate and the natural logarithm of the lagged number of corporate tax returns) have a significant at the 1% level effect on the IRS audit rates and exhibit the right coefficient signs (see lower part of model 1 in Table 8). Furthermore, in model 1 of Table 8 the validity of the instruments is supported by the under-identification LM test (UIT), the weak identification Wald F-Test (WIT)[21] and the Hansen over-identification test of Hansen (OIT). The second stage results (see upper part of model 1 in Table 8) show that the predicted values of the IRS audit rate that we obtain from the first stage exert a negative at the 1% level of significance effect on the cost of bank loans. We obtain similar results in models 2 and 3 of Table 9 when we perform two-stage instrumental estimations using only one of the two instrument in each model[22]. Thus, the findings from the two stage instrumental variable estimation in models 1 to 3 of Table 9 provide further support to our main *H.1* hypothesis that stringent IRS tax enforcement reduces the cost of bank loans[23].

Table 9

4.2.4 Other Sensitivity Analysis

The estimations so far show that US public firms incur lower bank lower spreads under higher level of IRS tax enforcement. In this section, we provide some additional tests in order to enhance the validity of this finding. Our sample includes firms that obtain more than one loan

of the IRS audit rates. Furthermore this result is compatible with Hoopes et al. (2012) who show that most of the variation of the audit rate variable comes from its numerator (i.e. the corporate tax returns audited by the IRS) rather than from its denominator (the number of corporate tax return filed in the previous year). The results from this estimation are available upon request.

[21] We use critical values for the weak identification test (WIT) from Stock and Yogo (2005).

[22] Note that the use of one instrument in models 2 and 3 of Table 8 means that we are not able to conduct over-identification tests.

[23] Authors of studies that have used instrumental variable analysis with regards to the IRS audit rate advise that we should be cautious and interpret findings from such analysis just as corroborating evidence to the baseline analysis (Guedhami and Pittman, 2008; Hoopes et al., 2012; Bauer et al., 2017). This is because of the difficulty of identifying 100% suitable instruments for the IRS audit rate.

facility in the same year. This could introduce a spurious correlation issue in our estimations. In the baseline estimations, we have tried to address this issue with firm level clustering. Here we further examine whether our findings hold when we keep only the observations with the largest loan-year facilities if a firm has more than one facility in a given year (Francis et al., 2012; Hasan et al., 2014). In model 1 of Table 10, the coefficient on *Audit rate* $_t$ is negative and significant at the 1% (-0.0137), lending support to our baseline findings. The previous exercise allows us also to exploit the panel nature of our dataset since we do not have repeated firm observations in a given year. Therefore, we proceed in performing firm fixed-effect regressions to control for unobserved firm heterogeneity. We present the findings of this estimation in model 2 of Table 10. The *Audit rate* $_t$ variable enters the regression negatively and is significant at the 1% level (-0.0197) corroborating our previous results. Even though the Hausman test rejects the null hypothesis that the random-effect model fits our panel data specification, we also tabulate in model 3 of Table 10 the findings from the random-effect estimation to show the consistency of our findings. Additionally, in model 4 of Table 10, we re-estimate our regression using lead-bank fixed effects as in Hasan et al. (2014). We show that *Audit rate* $_t$ is negative and significant at the 1% level consistent with our baseline regressions. Lastly, we control for potential bias stemming from potential inflation effects reflected into firms falling into higher IRS defined firm size classes and thus higher audit rates over the period of the study. To this end, we drop from our sample observations of firms that have changed IRS firm size class during the period of our study following Hoopes et al. (2012). Our results show that the coefficient on *Audit rate* $_t$ is still negative and significant at the 1% level supporting our previous findings.

Table 10

4.2.5 Evidence from Section 404b of the Sarbanes-Oxley (SOX) Act 2002

Recent empirical evidence from Bozanic et al. (2017) shows that regulations that increase public corporate disclosure facilitate IRS monitoring. The authors find that the IRS acquires information from the public financial disclosures of corporations in order to supplement and corroborate the private information, such as the corporate tax returns, it already possess on these firms.

A regulation that its introduction has significantly increased the public disclosure of financial information on corporations, and therefore could assist the monitoring function of the IRS, is the Sarbanes-Oxley (SOX) Act. The SOX Act that the Securities and Exchange Commission (SEC) administers was enacted in 2002 with the aim to improve the financial reporting of the US publicly listed firms. Specifically, the Section 404b of the SOX Act requires firms to obtain

an independent auditor attestation on the management evaluation of the efficacy of the company's internal controls over financial reporting (ICFR). These auditor attestations on the ICFRs could be favourable or adverse. Adverse auditor attestations denote the presence of internal control weaknesses (ICWs) in terms of financial reporting. A report from Audit Analytics in 2016 shows that each year at least 4% of the independent auditor attestations on the ICFRs of public corporations are adverse (Audit Analytics, 2016). Furthermore, the Public Company Accounting Oversight Board (PCAOB) places strong emphasis on auditors obtaining sufficient evidence to support their attestations (PCAOB, 2015).

In the light of the findings of Bozanic et al. (2017), it is plausible that the increased public financial reporting via the Section 404b of the SOX Act could facilitate IRS monitoring. The information available through the Section 404b of the SOX Act, such as the presence of ICWs in a firm, could be useful to the IRS. Firms with ICWs exhibit lower quality of financial reporting (Doyle et al., 2007, Ashbaugh-Skaife, 2008) and this could also be reflected to taxation related information. Furthermore, many ICWs under the Section 404b of the SOX act relate to the tax function of a corporation (Bauer, 2016). Gleason et al. (2017) find that in corporations with tax-related ICWs the implementation of aggressive financial reporting practices becomes easier. This could be of particular interest to the IRS since such practices is a common characteristic of tax shelter corporations (Wilson, 2009; Lisowsky 2010). All in all, the information available through the Section 404b of the SOX Act is likely to facilitate IRS monitoring and therefore enhance its importance as an external corporate governance mechanism that lowers information asymmetries and tax uncertainties. This could lead to lower cost of bank loans for the firms that have to comply with the Section 404b of the SOX Act.

Yet, the Section 404b of the SOX Act does not affect all firms as it provides an exemption to non-accelerated fillers, which are firms with public float of less than $75 million. Reason being that these firms face relatively higher burdens in terms of compliance costs. As a result, the introduction of the Section 404b of the SOX Act offers a quasi-experiment to explore the change in bank loan spreads across two group of firms; treated and control firms. The logic there is that the introduction of the Section 404b of the SOX Act facilitates IRS monitoring for the firms for which it applies to (i.e. treated firms) due to reasons not stemming from the cost of bank loans of these firms. Treated firms are the ones classified as accelerated fillers, i.e., typically firms with market capitalization above $75 million, following the SOX Act in 2002. Control firms are those that remain unaffected by the Section 404b of the SOX Act and are known as non-accelerated fillers. We source annual data observations from the Audit Analytics

database to identify which firms fall within the treated group (accelerated fillers firms) and which in the control group (non- accelerated filler group). All things being equal, if the information available in the Section 404b of the SOX Act facilitates IRS monitoring then one may expect that treated firms would enjoy lower borrowing costs in terms of bank loan spreads after its enforcement compared with the control firms.

We examine this prediction employing a sample covering two periods; one period before the implementation of the Section 404b of the SOX Act in 2002 (1990-2001) and one period after its adoption (2003-2016). As a first step, we use propensity score matching as a technique to identify one control firm for each treated. Treated firm stands for a dummy variable that is equal to 1 for an accelerated filler firm for the post-adoption period and 0 otherwise. Further, we employ this treated firm dummy variable as a dependent variable in a logistic regression to estimate the likelihood that a firm is compliant with the Section 404b of the SOX Act for the 2003-2016 periods.[24] We then use the propensity score estimated from the logistic regression to match each one of treated firms, which takes the value of 1, with one control firm (non-accelerated filler firms for the 2003-2016 period) with the closest propensity score that takes the value of 0. Then we obtain the matching loan-year facilities for these pairs. We need to have pairs in both periods prior and post the adoption of the Section 404b of the SOX Act to perform a difference-in-difference estimation. This method diminishes our sample to 105 loan-facilities initiated before the implementation of Section 404b and 180 after the implementation of Section 404b. We use the Post-404b loan dummy variable for loans originated between 2003-2016 period and 0 otherwise. Lastly, we use the interaction variable *Post-404b loan* Treated firm* to gauge the difference-in-difference estimate in loan spreads between treated and control firms for the two periods following the Section 404b of the SOX Act in 2002. Model 1 of Table 11 reports our finding. We find that the coefficient of the *Post-404b loan* Treated firm* interaction term is negative (-0.354) and significant at the 5% level. This result suggests that accelerated fillers firms that comply with the Section 404b of the SOX Act have significantly lower interest spreads for the loans obtained after its implementation compared with control firms that are non-accelerated fillers and were not affected by the Section 404b of the SOX Act.[25]

[24]The likelihood for a firm to comply with the 404b SOX section depends solely on its level of public float. As a result, the independent variable that we use to predict the likelihood of being in that group is the natural logarithm of market capitalization.

[25]These estimations do not include some loan characteristics, as there are missing observations on loan maturity and loan size variables, which diminishes our final sample importantly.

These findings are consistent with the notion that the information available through the implementation of the Section 404b of the SOX Act could facilitate IRS monitoring and therefore enhance its importance as an external corporate governance mechanism. This could lower information asymmetries between banks and the treated firms (i.e. the firms that have to comply with the Section 404b of the SOX Act) resulting into lower cost of bank loans for the latter group of firms in the period after the implementation of this legislation.

Table 11

4.2.6 IRS Tax Enforcement and Loan Covenants

Besides setting the price of a loan, banks could also set covenant requirements in loan contracts in order to reduce the risk associated with granting credit (Graham et al., 2008; Bharath et al, 2009; Hasan et al., 2014). We use logistic regression to observe the relationship between *Audit rate* $_t$ and a dummy variable that takes the value of 1 if a loan facility has covenant requirements and 0 otherwise. We present these findings in model 1 of Table 12. We find that the coefficient -0.0504 of *Audit rate* $_t$ is negative and significant at the 1% level. These findings show that stringent IRS tax enforcement reduces the probability of the presence of a covenant in a loan contract. This provides further evidence to the notion that banks view tax enforcement as an external corporate governance mechanism that could mitigate information asymmetries between them and corporate borrowers. Thus, stringent IRS monitoring decreases banks' incentive to set covenants when they provide credit to firms. We provide further support to this finding in model 2 of Table 12 when we re-estimate model 1 with an OLS regression. We also report similar results in model 3 of Table 12 when we perform a two-stage least squares instrumental variable estimation using the two instruments we employ in section 4.2.3 (four-years lagged values of IRS audit rates and the natural log of the number of corporate tax returns filed the year before).

Table 12

5. Conclusion

Previous empirical studies find that stringent tax enforcement plays an informational role in the financial markets and reduces the cost of bonds and the cost of equity for corporations. In this paper, we shed light on the relationship between tax enforcement and the cost of banks loans in the US syndicated loan market. We find that that tax enforcement, as measured by the IRS audit rates, reduces the bank loan spreads for US corporations. This result holds in a series of robustness checks that comprise the use of alternative IRS tax enforcement measures,

instrumental variable estimations to account for endogeneity issues, panel data estimations, and a quasi-experiment framework based on the Section 404b of the Sarbanes-Oxley (SOX) Act. We also find that the negative effect of IRS tax enforcement on bank loan spreads strengthens for smaller corporations. Furthermore, we provide evidence that stringent IRS monitoring reduces the probability that a loan will contain covenants.

All in all, these results suggest that banks value the role of tax enforcement as an external corporate governance mechanism that could reduce information asymmetries between them and corporate borrowers. The findings of this study are useful from a theoretical standpoint because they provide evidence that tax enforcement plays a valuable informational role even for banks, who in comparison with other types of debt-holders, such as the owners of corporate bonds, are better positioned and more likely to monitor corporate borrowers. From a public policy perspective, this study highlights the usefulness of the IRS to the US economy. Except its main function as a tax collection agency, the IRS exerts a positive spillover to the US economy in the form of lower costs of corporate financing. Therefore, this study informs from this perspective the political debate about the future of the IRS.

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List of Tables

Table 1. Variable definition and measurements
Panel A. Dependent variable and main explanatory variables

Variables	Definitions	Source
Audit rate t	Probability of a firm to face an IRS audit in year t	TRAC
Firm characteristics		
Firm size $_{t-1}$	The natural logarithm of total assets in year t-1	COMPUSTAT
ROA $_{t-1}$	Net operating income divided by total assets in year t-1	COMPUSTAT
Liquidity $_{t-1}$	Current assets divided by total assets in year t-1	COMPUSTAT
Leverage $_{t-1}$	Long term debt divided by total assets in year t-1	COMPUSTAT
Tangibility $_{t-1}$	Net property, plant and equity divided by total assets in year t-1	COMPUSTAT
Cash effective tax rate $_{t-1}$	Cash tax paid divided by pre-tax book income in year t-1	COMPUSTAT
Loan characteristics		
Loan spread $_t$ *(basis points)*	The "all-in-spread drawn" (AISD), which is the loan interest payment in basis points over the LIBOR plus the annual fee for a loan facility that a firm obtain in year t.	Thomson One Banker
Loan size $_t$ *($ Millions)*	Total amount of a loan facility obtained by a firm in year t	Thomson One Banker
Loan maturity $_t$ *(year)*	Number of years to a maturity of a loan facility obtained by a firm in year t	Thomson One Banker
Loan purpose $_t$	Dummy variables for loan purposes including corporate purposes, debt repayment, working capital, takeover, back up loans etc.	Thomson One Banker
Loan type $_t$	Dummy variables for loan types including term loan, revolver greater than one year, revolver less than 1 year and 364-day facility	Thomson One Banker
Panel B. Variables used in sensitivity analysis specifications		
Alternative IRS proxies		
IRS employees $_t$	Number of IRS employees divided by corporate tax returns in year t	TRAC

IRS revenue agents $_t$	Number of IRS revenue agents divided by corporate tax returns in year t	TRAC
IRS criminal investigators $_t$	Number of IRS criminal investigators divided by corporate tax returns in year t	TRAC
IRS fraud penalties $_t$	Number of IRS fraud penalties divided by corporate tax returns in year t	TRAC
IRS negligence penalties $_t$	Number of IRS negligence penalties divided by corporate tax returns in year t	TRAC
IRS criminal referrals $_t$	Number of IRS criminal tax referrals divided by corporate tax returns in year t	TRAC
IRS criminal prosecutions $_t$	Number of IRS criminal tax prosecutions divided by IRS criminal referrals in year t	TRAC
Alternative size proxies		
Equity $_{t-1}$	The natural logarithm of total equity in year t-1	COMPUSTAT
Sales $_{t-1}$	The natural logarithm of total sales in year t-1	COMPUSTAT
Loan characteristics		
Covenant t	Equals to one if a loan facility obtained by a firm in year t has covenants and zero otherwise	Thomson One Banker
NRL dummy t	Equals to one if a firm has been credited a loan in year t and has not obtained any other loan from the same lead bank during the last five years, while zero otherwise.	Thomson One Banker

Table 2. IRS face-to-face audit rates of corporate income tax returns and sample distribution

Panel A of this table reports the probability of an IRS audit for firms according to time and size. Panel B outlines the sample distribution (9,971 observations) by time and asset classification.

Panel A. IRS audit rates across time and size

Asset class	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016
>$250 Million	54.6	55.5	54.7	50.9	48.4	45.9	37.3	34.6	30.5	31.4	33.7	29	38.1	42.6	34.2	26.3	26.9	25	24.3	27	28.5	32.3	24.7	19.9	17.8
$100-250 Million	31.3	32.3	30.6	27.8	26.8	22.5	19	18.5	16.9	17.1	15.5	12.2	15.9	16.7	13.7	11.5	12.6	13.3	14.4	16.4	22.5	18.8	12.4	13.4	10.4
$50-100 Million	28.5	25.4	24.3	21.5	20.8	19.2	17.5	16	14.1	11.9	10.3	9.4	12.1	15.5	13.4	10.9	11.4	14	15.9	18.6	20.4	15	10.8	10.7	9.9
$10-50 Million	23.2	23.3	22.2	19.6	19.7	20	17.7	14.7	11.4	9.3	7.5	5.9	8.8	11.4	14	14.7	11.6	9.9	13.2	13.1	10.1	6.6	5.9	5.3	4.4
$5-10 Million	18.8	19.3	15.7	14.7	13.9	16	13.4	10.1	6.8	5.1	4.5	3.2	1.9	2.4	3.3	2.9	3	2.6	2.8	2.5	2.4	1.7	1.7	1.3	1.4
$1-5 Million	9.9	9.6	7	6	6.6	7.7	6.4	4.9	2.9	2	2	1.5	0.6	0.9	1.1	1.6	1.9	1.8	1.7	1.9	2	1.3	1.1	1	0.9
$0,25- 1 Million	4	4	2.4	2.1	2.7	3.5	2.5	1.7	1.1	0.8	0.7	0.6	0.3	0.9	0.9	1.3	1.3	1.3	1.3	1.6	1.6	1.2	1.2	1.1	1

Panel B. Sample Distribution

Asset class	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016	Total	%
>$250 Million	93	134	47	291	579	501	606	592	651	420	389	435	394	340	356	235	134	236	369	267	341	289	328	308	63	**8398**	**84.2%**
$100-250 Million	13	8	6	48	149	158	143	127	57	54	34	39	28	33	21	11	10	5	8	9	14	9	7	6	1	**998**	**10.0%**
$50-100 Million	7	8	10	40	40	71	37	30	13	4	3	6	10	5	11	5	3	9	1	2	9	6		2		**332**	**3.3%**
$10-50 Million		2	3	27	50	56	27	21	8	3	3	4				2	4	2				3				**215**	**2.2%**
$5-10 Million	1		3	2	4	4	4																			**18**	**0.2%**
$1-5 Million					4	2	1	1													1					**9**	**0.1%**
$0,25- 1 Million															1											**1**	**0.0%**
Total	**114**	**152**	**69**	**408**	**826**	**792**	**818**	**771**	**729**	**481**	**429**	**484**	**432**	**378**	**389**	**253**	**151**	**252**	**378**	**278**	**365**	**307**	**335**	**316**	**64**	**9971**	**100.0%**
%	**1.1%**	**1.5%**	**0.7%**	**4.1%**	**8.3%**	**7.9%**	**8.2%**	**7.7%**	**7.3%**	**4.8%**	**4.3%**	**4.9%**	**4.3%**	**3.8%**	**3.9%**	**2.5%**	**1.5%**	**2.5%**	**3.8%**	**2.8%**	**3.7%**	**3.1%**	**3.4%**	**3.2%**	**0.6%**	**100.0%**	

Table 3. Descriptive statistics for the dependent and the main regression variables.

Variable	Mean	Standard Deviation	25th percentile	50th percentile	75th percentile
Audit rate $_t$	30.541	9.581	24.7	30.5	37.3
Firm characteristics					
Firm size $_{t-1}$	7.2648	1.7394	6.0647	7.2308	8.4713
ROA $_{t-1}$	0.0223	0.0342	0.0112	0.0216	0.0341
Liquidity $_{t-1}$	1.8826	2.1252	1.0734	1.5299	2.2312
Leverage $_{t-1}$	0.5317	0.2500	0.3794	0.5066	0.6472
Tangibility $_{t-1}$	0.6431	0.4354	0.3080	0.5763	0.9188
Cash effective rate $_{t-1}$	0.2817	1.8268	0.2346	0.3511	0.3926
Loan characteristics					
Loan spread $_t$ *(basis points)*	200	151	98	175	275
Loan size $_t$ *($ Millions)*	433	889	75	197	450
Loan maturity $_t$ *(year)*	4	2	3	5	5

The table reports descriptive statistics for the 9,971 firm-year observations over the 1992-2016 period used in our regression analysis. Table 1 provides full details on the definitions and calculation for all variables.

Table 4. Correlation matrix of the dependent and the main explanatory variables.

	Loan spread $_t$	*Audit rate* $_t$	*Firm size* $_{t-1}$	*ROA* $_{t-1}$	*Liquidity* $_{t-1}$	*Leverage* $_{t-1}$	*Tangibility* $_{t-1}$	*Cash effective rate* $_{t-1}$	*Loan maturity* $_t$	*Loan size* $_t$
Loan spread $_t$	1									
Audit rate $_t$	**-0.363**	1								
Firm size $_{t-1}$	**-0.362**	**0.219**	1							
ROA $_{t-1}$	**-0.201**	0.075	0.035	1						
Liquidity $_{t-1}$	0.056	-0.076	**-0.188**	0.027	1					
Leverage $_{t-1}$	**0.23**	0.053	-0.031	-0.07	**-0.207**	1				
Tangibility $_{t-1}$	-0.056	0.022	**0.141**	**-0.121**	**-0.223**	0.028	1			
Cash effective rate $_{t-1}$	-0.012	-0.01	0.03	0.025	-0.005	-0.036	-0.019	1		
Loan maturity $_t$	**0.178**	-0.039	**-0.111**	0.021	0.066	0.012	-0.056	-0.002	1	
Loan size $_t$	**-0.325**	**0.187**	**0.696**	0.074	**-0.114**	-0.017	0.084	0.045	0.029	1

This table reports Pearson correlations coefficients for the main variables used in our regressions. Boldface denotes 1% statistical significance. Table 1 includes full details on the definitions and calculation for all variables.

Table 5. The effect of IRS audit rate on bank loan spreads

Variable	Model 1	Model 2	Model 3	Model 4
Audit rate $_t$	-0.00709***	-0.00796***		
	(0.00152)	(0.00171)		
Extr_Audit rate $_t$			-0.0140***	
			(0.00156)	
Audit rate t-1				-0.00853***
				(0.00211)
Firm Size $_{t-1}$	-0.228***	-0.162***	-0.150***	-0.159***
	(0.0140)	(0.0179)	(0.0175)	(0.0178)
ROA $_{t-1}$	-3.072***	-3.922***	-3.844***	-3.842***
	(0.583)	(0.810)	(0.796)	(0.781)
Liquidity $_{t-1}$	0.0197***	0.0108	0.0130	0.0104
	(0.00713)	(0.00722)	(0.00810)	(0.00684)
Tangibility $_{t-1}$	-0.188***	-0.156***	-0.146***	-0.157***
	(0.0408)	(0.0447)	(0.0438)	(0.0432)
Leverage $_{t-1}$	0.772***	0.744***	0.755***	0.726***
	(0.0976)	(0.120)	(0.118)	(0.113)
Cash effect. rate $_{t-1}$	0.00277	0.00382	0.00405	0.00173
	(0.00257)	(0.00266)	(0.00252)	(0.00337)
Loan size $_t$		-0.0821***	-0.0777***	-0.0819***
		(0.0145)	(0.0144)	(0.0143)
Loan maturity $_t$		0.143***	0.140***	0.146***
		(0.0225)	(0.0226)	(0.0224)
Constant	6.852***	6.742***	6.607***	6.766***
	(0.159)	(0.173)	(0.166)	(0.170)
Observations	9,971	7,054	7,286	7,146
R-squared	0.498	0.512	0.499	0.512
Loan type & purpose	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes

This table presents results from regressing bank loan spreads on Internal Revenue Service (IRS) audit rates after controlling for firm (all Models) and loan-level characteristics (Model 2, 3 & 4). Table 1 includes full details on the definitions and calculation for all variables. Regressions are based on OLS regressions with robust standard errors in parentheses and within firm clustering. Significance at the 10%, 5%, and the 1% level is represented by *, **, and *** respectively.

Table 6. The effect of IRS audit rate on the bank loan spreads of firms with less than $250 million of assets

VARIABLES	Model 1	Model 2	Model 3	Model 4
Audit rate $_t$	-0.0488***	-0.0385***	-0.0720*	-0.228***
	(0.00521)	(0.00958)	(0.0430)	(0.0826)
Firm Size $_{t-1}$	-0.257***	-0.102	0.0880	0.232
	(0.0826)	(0.108)	(0.126)	(0.263)
ROA $_{t-1}$	-1.984**	-1.045	-0.242	2.620
	(0.967)	(1.421)	(0.741)	(2.278)
Liquidity $_{t-1}$	0.0124	0.00649	-0.0300	0.168
	(0.00880)	(0.00742)	(0.0306)	(0.135)
Tangibility $_{t-1}$	-0.0809	-0.104*	0.791**	0.621
	(0.0524)	(0.0616)	(0.397)	(0.804)
Leverage $_{t-1}$	0.424***	0.626***	-0.111	1.623
	(0.102)	(0.151)	(0.121)	(1.307)
Cash effective rate $_{t-1}$	-0.00221	-0.000518	-0.0737**	1.234
	(0.00627)	(0.00518)	(0.0367)	(0.771)
Loan size $_t$		-0.113***		-0.00304
		(0.0297)		(0.0244)
Loan maturity $_t$		0.0579		0.0152
		(0.0565)		(0.0182)
Constant	7.653***	7.439***	5.923***	6.869***
	(0.488)	(0.638)	(0.805)	(1.212)
Observations	974	486	571	221
R-squared	0.359	0.409	0.965	0.980
Loan type & purpose	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes

This table presents results from regressing bank loan spreads on Internal Revenue Service (IRS) audit rates after controlling for firm (all Models) and loan-level characteristics (Model 2 & 4). Models 1 & 2 include loan-year facilities for firms of asset size between $100 and 250Millions, while Models 3 & 4 of asset size between $100-0.25Million. In models 1 and 3 we exclude loan size and loan maturity to allow running our regressions with more observations. Table 1 includes full details on the definitions and calculation for all variables. Regressions are based on OLS regressions with robust standard errors in parentheses and within firm clustering. Significance at the 10%, 5%, and the 1% level is represented by *, **, and *** respectively.

Table 7. The effect of IRS audit rate on bank loan spreads: controlling for alternative size measures

VARIABLES	(1) Model 1	(2) Model 2
Audit rate $_t$	-0.0105***	-0.00791***
	(0.00193)	(0.00277)
Equity $_{t-1}$	-0.175***	
	(0.0156)	
Sales $_{t-1}$		-0.146***
		(0.0137)
ROA $_{t-1}$	-4.017***	-3.215***
	(0.693)	(0.701)
Liquidity $_{t-1}$	0.0424***	0.00549
	(0.0142)	(0.00791)
Tangibility $_{t-1}$	-0.184***	-0.144***
	(0.0431)	(0.0444)
Leverage $_{t-1}$	0.643***	0.764***
	(0.118)	(0.124)
Cash effective rate $_{t-1}$	0.00975***	0.00191
	(0.00370)	(0.00269)
Loan size $_t$	-0.0658***	-0.0993***
	(0.0125)	(0.0130)
Loan maturity $_t$	0.152***	0.144***
	(0.0205)	(0.0227)
Constant	6.526***	6.713***
	(0.182)	(0.160)
Observations	6,630	6,871
R-squared	0.538	0.507
Loan type & purpose	Yes	Yes
Year FE	Yes	Yes
Industry FE	Yes	Yes

This table presents results from regressing bank loan spreads on IRS audit rate and alternative size measures after controlling for firm and loan-level characteristics. Model 1 includes the natural logarithm of total equity and Model 2 the natural logarithm of total sales. Regressions are based on OLS regressions with robust standard errors in parentheses and within firm clustering. Significance at the 10%, 5%, and the 1% level is represented by *, **, and *** respectively.

Table 8. Sensitivity analysis: The effect of alternative measures of IRS tax enforcement on bank loan spreads

VARIABLES	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
Panel A. Regressions of bank loan spreads on alternative IRS tax enforcement proxies and controls.							
IRS Employees $_t$	-0.0529***						
	(0.009)						
IRS Revenue agents $_t$		-0.494***					
		(0.088)					
IRS Criminal investigators $_t$			-1.894***				
			(0.329)				
IRS Fraud penalties $_t$				-20.98***			
				(3.776)			
IRS Negligence penalties $_t$					-5.403***		
					(1.424)		
IRS Criminal referrals $_t$						-2.399***	
						(0.457)	
IRS Criminal prosecutions $_t$							-1.227***
							(0.291)
All control variables except year effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,312	3,312	3,312	3,312	3,312	3,312	3,312
R-squared	0.469	0.474	0.470	0.496	0.480	0.484	0.486
Loan type & purpose FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel B. Alternative IRS proxies cross sectional identification strategy	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
NLR-dummy $_t$	0.0769	0.0731	0.0747	0.0426	0.0162	0.0747	0.00909
	(0.0777)	(0.0770)	(0.0775)	(0.0747)	(0.0710)	(0.0775)	(0.0705)
IRS Employees $_t$ ** NLR-dummy* $_t$	-0.0022**						
	(0.001)						
IRS Revenue agents $_t$ ** NLR-dummy* $_t$		-0.016**					
		(0.007)					
IRS Criminal investigators $_t$ ** NLR-dummy* $_t$			-0.079**				
			(0.039)				
IRS Fraud penalties $_t$ ** NLR-dummy* $_t$				-0.395			
				(0.246)			
IRS Negligence penalties $_t$ ** NLR-dummy* $_t$					-0.129		
					(0.087)		
IRS Criminal referrals $_t$ ** NLR-dummy* $_t$						-0.071**	
						(0.034)	
IRS Criminal prosecutions $_t$ ** NLR-dummy* $_t$							-0.521***
							(0.101)
All control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,312	3,312	3,312	3,312	3,312	3,312	3,312
R-squared	0.537	0.537	0.537	0.537	0.536	0.537	0.536
Loan type & purpose dummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year & Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel A of Table 8 presents the results from regressing bank loan spreads on alternative IRS tax enforcement measures after controlling for firm and loan-level characteristics except year effects. Panel B presents findings from regressing bank loan spreads on the interaction between the alternative IRS tax enforcement measures and the *NLR-dummy* $_t$ which that stands for a non-lending relationship dummy variable. This dummy takes the value of 1 when a firm has not obtained a loan by the same lead bank in the last 5 years before a loan facility initiation and 0 otherwise. Table 1 includes full details on the definitions and calculation for all variables. Regressions are based on OLS regressions with robust standard errors in parentheses and within firm clustering. Significance at the 10%, 5%, and the 1% level is represented by *, **, and *** respectively.

Table 9. Sensitivity analysis: The effect of endogenous IRS audit rate on bank loan spreads

Variables	IV I Estimation	IV II Estimation	IV III Estimation
Second stage			
Predicted Audit rate $_t$	-0.00990***	-0.0141***	-0.00896***
	(0.00188)	(0.00354)	(0.00212)
Firm Size $_{t-1}$	-0.153***	-0.146***	-0.154***
	(0.0116)	(0.0149)	(0.0108)
ROA $_{t-1}$	-3.754***	-3.850***	-3.762***
	(0.584)	(0.602)	(0.581)
Liquidity $_{t-1}$	0.00915**	0.00948**	0.00912**
	(0.00440)	(0.00442)	(0.00440)
Tangibility $_{t-1}$	-0.161***	-0.155***	-0.161***
	(0.0244)	(0.0248)	(0.0243)
Leverage $_{t-1}$	0.796***	0.785***	0.795***
	(0.120)	(0.116)	(0.120)
Cash effective rate $_{t-1}$	0.00260	0.00267	0.00264
	(0.00234)	(0.00236)	(0.00234)
Loan size $_t$	-0.0871***	-0.0883***	-0.0873***
	(0.0104)	(0.00973)	(0.0105)
Loan maturity $_t$	0.124***	0.118***	0.124***
	(0.0157)	(0.0166)	(0.0156)
Constant	6.590***	6.641***	6.574***
	(0.108)	(0.130)	(0.107)
First stage			
Corporate tax returns $_{t-1}$	-3.101 ***	-8.588***	-
	(0.247)	(0.349)	
Audit rate $_{t-4}$	0.557***	-	0.665***
	(0.014)		(0.009)
Observations	6,823	6,977	6,823
R-squared	0.492	0.496	0.492
LM test p-value (UIT)	0.000	0.000	0.000
Wald F-Test (WIT)	3352.85	604.28	5980.42
with critical value	19.93	16.38	16.38
Hansen *J* p-value (OIT)	0.3093	-	-
Loan type & purpose	Yes	Yes	Yes
Year & Industry FE	Yes	Yes	Yes

This table presents results from regressing bank loan spreads on IRS audit rates after controlling for firm and loan-level characteristics. Model IV I estimation shows the results from a two-stage instrumental regression procedure where we use two instruments; i) four-year lagged values of IRS audit rates and ii) natural logarithm of the number of corporate tax returns filed in the previous year. Model IV II & III shows estimations from a two stage instrumental procedure where we use as the instrumental variable the natural logarithm of the number of corporate tax returns filed in the previous year and the four-year lagged values of IRS audit rates respectively. UIT is the under-identification LM test by Kleibergen and Paap, WIT is the Wald F-statistic of the weak identification test, which must be higher than its critical value to reject the null. OIT is the over-identification test of Hansen. Table 1 includes full details on the definitions and calculation for all variables. Significance at the 10%, 5%, and the 1% level is represented by *, **, and *** respectively.

Table 10. Other sensitivity analysis

VARIABLES	Cross-sectional Model 1	Fixed-effects Model 2	Random-effects Model 3	Lead bank Model 4	Inflation accounted Model 5
Audit rate $_t$	-0.0137***	-0.0197***	-0.0187***	-0.00867***	-0.0134***
	(0.00179)	(0.00204)	(0.00154)	(0.00179)	(0.00209)
Firm Size $_{t-1}$	-0.141***	-0.0172	-0.109***	-0.160***	-0.165***
	(0.0204)	(0.0286)	(0.0162)	(0.0120)	(0.0174)
ROA $_{t-1}$	-3.518***	-2.405***	-2.492***	-3.825***	-4.798***
	(0.757)	(0.565)	(0.478)	(0.663)	(0.899)
Liquidity $_{t-1}$	0.00997	-0.000658	0.00282	0.0102*	0.0109
	(0.00880)	(0.00625)	(0.00607)	(0.00614)	(0.00789)
Tangibility $_{t-1}$	-0.116***	-0.142**	-0.161***	-0.152***	-0.145***
	(0.0429)	(0.0596)	(0.0420)	(0.0315)	(0.0493)
Leverage $_{t-1}$	0.708***	0.462***	0.541***	0.732***	0.852***
	(0.159)	(0.0888)	(0.126)	(0.113)	(0.0791)
Cash eff. rate $_{t-1}$	0.00207	0.00119	0.000688	0.00162	0.000316
	(0.00406)	(0.00390)	(0.00394)	(0.00321)	(0.00357)
Loan size $_t$	-0.106***	-0.0280**	-0.0589***	-0.0827***	-0.0759***
	(0.0193)	(0.0131)	(0.0127)	(0.0124)	(0.0125)
Loan maturity $_t$	0.160***	0.0707***	0.0961***	0.146***	0.148***
	(0.0247)	(0.0180)	(0.0182)	(0.0178)	(0.0234)
Constant	6.767***	6.004***	6.639***	6.750***	6.824***
	(0.182)	(0.266)	(0.184)	(0.139)	(0.187)
Observations	4,703	4,703	4,703	7,086	6,194
R-squared	0.523	0.422	0.562	0.513	0.529
Loan type & purpose	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	No	Yes	Yes	Yes
Lead bank FE	No	No	No	Yes	No

This table presents results from regressing bank loan spreads on Internal Revenue Service (IRS) audit rates after controlling for firm and loan characteristics. Model 1 re-estimates the cross -sectional regression of IRS audit rates on bank loan spreads keeping only the largest loan-facility per year (for the firms that have taken more than one loan facility in a given year). Regression is based on OLS estimation with robust standard errors in parentheses and within firm clustering. Models 2 & 3 make use of the panel nature of our dataset and tabulate fixed and random firm effects regressions with robust standard errors respectively. Model 4 re-estimates the cross-sectional regression of IRS audit rates on bank loan spreads after controlling for lead-bank fixed effects. Model 5 re-estimates the cross-sectional regression of IRS audit rates on bank loan spreads excluding observations from firms that have changed an IRS defined size class over the period under study. Significance at the 10%, 5%, and the 1% level is represented by *, **, and *** respectively.

Table 11. Evidence from a quasi-experiment. Section 404(b) of Sarbanes-Oxley Act of 2002.

Variables	Model (1)	Model (2)
Treated firm	-0.0113	
	(0.296)	
Post-404b loan	0.951***	
	(0.165)	
Post-404b loan Treated firm*	-0.354**	
	(0.158)	
Firm Size $_{t-1}$	-0.712***	-0.684***
	(0.0556)	(0.0547)
ROA $_{t-1}$	-12.49***	-13.97***
	(1.239)	(1.057)
Liquidity $_{t-1}$	-0.0176	-0.0470
	(0.0319)	(0.0297)
Tangibility $_{t-1}$	-1.914***	-1.611***
	(0.175)	(0.111)
Leverage $_{t-1}$	1.481***	1.538***
	(0.185)	(0.184)
Cash effective rate $_{t-1}$	0.159	0.0959
	(0.151)	(0.149)
Constant	10.15***	9.518***
	(0.630)	(0.491)
Observations	285	285
Loan type dummy	Yes	Yes
Year & Industry FE	Yes	Yes

Model 1 of this table presents the results of the difference-in-difference estimation using a sample of treated firms (firms that comply with the 404b Section of the SOX act and take the value 1) and their matched pairs (firms that are exempted from the 404b Section of the SOX act and take the value of 0). We use propensity score matching technique to identify the matched pairs of our sample. Our final sample includes 105 loan-facilities initiated before the 404b and 180 after the 404b. Post-404b loan dummy variable stands for loans originated between 2003 and 2016 and 0 otherwise. Lastly, the interaction variable Post-404b loan* Treated firm gauges the difference-in-difference estimate in loan spreads between treated and control firms for the two periods following the 404b SOX in 2002. Model 2 presents OLS regression results of the sample with robust standard errors and within-firm clustering including only firm-level characteristics.

Table 12. IRS audit rate and the presence of loan covenants

VARIABLES	Logistic model Estimation	OLS Estimation	IV Estimation
Audit rate $_t$	-0.0504***	-0.00520***	
	(0.0129)	(0.00120)	
Predicted Audit rate $_t$			-0.00381***
			(0.00122)
Firm Size $_{t-1}$	-0.0505	-0.0196**	-0.0191***
	(0.0739)	(0.00872)	(0.00515)
ROA $_{t-1}$	0.711	0.0473	0.0136
	(1.733)	(0.247)	(0.171)
Liquidity $_{t-1}$	0.0231	0.00272	0.00439*
	(0.0268)	(0.00425)	(0.00235)
Tangibility $_{t-1}$	-0.369**	-0.0583***	-0.0538***
	(0.151)	(0.0222)	(0.0144)
Leverage $_{t-1}$	-0.00871	-0.0203	0.00151
	(0.209)	(0.0307)	(0.0217)
Cash effective rate $_{t-1}$	0.000762	6.84e-05	0.00101
	(0.0210)	(0.00229)	(0.00163)
Loan size $_t$	0.0956	0.0189**	0.0169***
	(0.0788)	(0.00909)	(0.00542)
Loan maturity $_t$	0.371***	0.0560***	0.0587***
	(0.0735)	(0.0115)	(0.00807)
Constant	-2.580***	0.562***	0.0810*
	(0.442)	(0.126)	(0.0423)
First stage			
Corporate tax returns $_{t-1}$			-3.384***
			(0.280)
Audit rate $_{t-4}$			0.518***
			(0.016)
Observations	7,089	7,089	6,823
Pseudo R-squared	0.2775	-	-
R-squared	-	0.331	0.5207
LM test p-value (UIT)	-	-	0.000
Wald F-Test (WIT)	-	-	2862.197
with critical value			19.93
Hansen *J* p-value (OIT)	-	-	0.1389
Loan type & purpose dummy	Yes	Yes	Yes
Year & Industry FE	Yes	Yes	Yes

This table presents results from regressing the loan covenant dummy (equals to one if a loan facility obtained by a firm in year t has covenants and 0 otherwise) on Internal Revenue Service (IRS) audit rates after controlling for firm and loan-level characteristics. Model 1 is estimated using logistic regression, while Model 2 is estimated based on OLS. Model IV shows the results from a two-stage instrumental variable regression procedure where we use two instruments; i) four-year lagged values of IRS audit rates and ii) natural logarithm of the number of corporate tax returns filed in the previous year. UIT is the under-identification LM test by Kleibergen and Paap, WIT is the Wald F-statistic of the weak identification test, which must be higher than its critical value to reject the null. OIT is the over-identification test of Hansen. Table 1 includes full details on the definitions and calculation for all variables. Regressions are based on OLS regressions with robust standard errors in parentheses and within firm clustering. Significance at the 10%, 5%, and the 1% level is represented by *, **, and *** respectively.