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"Does tax enforcement matter for the cost of bank loans? Evidence from the United States"

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

Abstract

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We examine the relationship between the tax enforcement effort of the internal revenue service (IRS) 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 robustness and sensitivity tests such as the use of alternative IRS tax enforcement measures, instrumental variable regressions, panel data estimations and a quasi-experimental framework of the Section 404b of the Sarbanes-Oxley (SOX) Act. 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. Overall, these findings suggest that banks acknowledge the informational and monitoring role of tax enforcement in the private debt market.

JEL classification: G21, H25, H26, M40

Keywords: tax enforcement; cost of bank loans; agency costs; IRS audit rates.

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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 tax enforcement alleviates the asymmetry of information between firms and other financial participants and decreases the cost of equity (El Ghoul, Guedhami & Pittman, 2011), the cost of bonds (Guedhami & Pittman, 2008) and the probability of a stock price crash (Bauer, Fang & Pittman, 2017). In this study, we contribute to this stream of the literature by examining the relationship between the tax enforcement effort of the Internal Revenue Service (IRS) and the cost of syndicated loans in the United States. In particular, we attempt to provide an answer to the following question: *Does IRS tax enforcement decreases the cost of syndicated bank loans for US corporations?*

The theoretical premise of the ability of tax enforcement to alleviate information asymmetries between firms and investors or creditors rests on its function as a mechanism of external corporate governance that limits the potential for managerial rent-seeking behaviour (Desai, Dyck & Zingales, 2007). Tax enforcement reduces the ability of firms' managers to engage in risky tax positions that are a source of information asymmetries. To prevent detection from the tax agency, risky tax positions entail complexity and information obfuscation. The complex and opaque environment that stems from aggressive tax positions increases information asymmetry and facilitates managerial rent-seeking (Kim, Li & Zhang, 2011). Stringent tax enforcement discourages managers from taking risky tax positions and deters their rent-seeking behaviour (Desai, Dyck & Zingales, 2007; Desai & Dharmapala, 2009). This, in turn, improves the information available on the firm to outside investors or creditors (Desai, Dyck & Zingales, 2007; Hanlon, Hoopes and Shroff, 2014). The empirical findings that tax enforcement reduces the cost of equity (El Ghoul, Guedhami & Pittman, 2011) and the cost of bonds (Guedhami & 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 in the capital and debt markets (Erickson, Hanlon & Maydew, 2004; Dyck, Morse & Zingales, 2010). Furthermore, government agencies often lack appropriate resources and staffing levels that may diminish their monitoring ability (Jackson & Roe, 2009; Hanlon & Heitzman, 2010). For these reasons, Hanlon & Heitzman (2010) and Hanlon, Hoopes & Shroff (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 significant funding source for corporations in the US. Around 75% of new corporate financing in the US comes in the form of debt (Contessi, Li & Russ, 2013). The majority of this new debt funding comes in the form of bank loans even for large public corporations (Barath, Dahiya, Saunders, & Srinivasan, 2008; Hasan, Hoi, Wu & Zhang, 2014; Khang, King & Nguyen, 2016[3]). It is, therefore, essential from a managerial and public policy standpoint to investigate the effect of IRS tax enforcement on the cost of bank loans for US corporations.

Secondly, bank loans provide a challenging area in which to investigate the ability of tax enforcement to act as an external form of corporate governance that alleviates information asymmetries in the debt markets. The empirical evidence of such ability so far comes from the bond market (Guedhami & Pittman, 2008). However, banks, in comparison with bond investors, are more efficient in solving informational asymmetry issues through screening and monitoring (De Fiore & Uhlig, 2011). This is because banks enjoy comparative advantages and scale economies in information production and debt monitoring (Diamond 1984). Banks also have stronger incentives to invest in the acquisition of knowledge on borrowers due to their comparatively large stake in the funding of the latter (Boot & Thakor, 2009). Holders of corporate bonds are more fluid and dispersed. Thus, their incentive to engage in borrower monitoring is smaller in comparison with banks (Amihud, Garbade & Kahan, 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 tax enforcement to act as an external corporate governance mechanism that could decrease information asymmetry issues and thus the cost of corporate financing. In other words, it is interesting to investigate if banks, who themselves screen and monitor borrowers more closely than other types of debt-holders, value the informational and monitoring role of tax enforcement.

[3] As an example, the study of Khang, King & Nguyen (2016) shows that 56% of the total debt of a typical US corporation consists of bank loans.

Thirdly, the IRS has lately faced severe cuts regarding its financing and its resources. Marr & Murray (2016) report that the IRS has experienced a 17% cut in its federal funding over the 2010-2016 period. 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 (Marr & 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 IRS (The Hill, 2017). Recent empirical evidence from Nessa, Schwab, Stomberg & Towery (2016) shows that fewer IRS resources weaken the tax enforcement process. The authors find that cuts in the IRS budget and the number of IRS enforcement agents lead to a decrease in the rate of tax audits. By investigating the impact, if any, of IRS tax enforcement on the cost of banks loans in the US we add to the public policy discussion about the future of the IRS and its usefulness to the US economy.

To investigate the effect of IRS tax enforcement on the cost of bank loans we use a sample of 9,971 syndicated loan facilities initiated for US public firms over the 1992-2016 period. We measure the cost of bank loans as the *"all-in-spread"* which is the interest payment in basis points above LIBOR plus the annual fee (in basis points) for each loan facility a firm attains (see, e.g., Hasan, Hoi, Wu & Zhang, 2014; Delis, Hasan & Mylonidis, 2017). To capture IRS tax enforcement, we employ the yearly rate of face-to-face IRS corporate audits. These audit rates use information from the 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 exhibits variability according to firm size class and the calendar year. We obtain information on the rate of IRS audits from the Transactional Records Access Clearing House (TRAC). TRAC, which is based in Syracuse University, sources its data from the IRS system that the agency uses for its internal use and its communication of information to the public and the US Congress (Hanlon, Hoopes & Shroff, 2014). This measure of IRS tax enforcement has been widely used in the literature (Guedhami & Pittman, 2008; El Ghoul, Guedhami & Pittman, 2011; Hanlon, Hoopes & Shroff, 2014; Bauer, Fang & Pittman, 2017) to gauge the levels of tax enforcement in the US. Also, we use an array of alternative IRS tax enforcement proxies, which 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, our main finding is that IRS tax enforcement exerts a negative effect on the cost of bank loans. We find that an increase of the

IRS audit probability from 24.7% (25th percentile) to 37.3% (75th percentile) results in an approximately 10 basis points reduction in bank loan spreads. Taking into account the average loan size ($433 million) and 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 view that tax enforcement could act as an external mechanism of corporate governance that reduces information asymmetry issues between firms and outsiders (in our case banks). Our analysis also shows that the negative effect of IRS tax enforcement on the cost of banks loans strengthens for comparatively smaller corporations (firms with assets less than $250m). Small companies have a weaker information environment and weaker corporate governance and internal control mechanisms in comparison with larger firms (Beck & Demirguc-Kunt, 2006; Doyle, Ge & McVay, 2007; Boone & White, 2015). Therefore, the role of IRS tax enforcement in alleviating information asymmetries between firms and banks could be more critical for smaller corporations. Besides the cost of bank loans, we also provide evidence that tax enforcement affects the non-price terms of loans. In particular, we find that stringent IRS tax enforcement reduces the likelihood of the presence of covenants in bank loan contracts.

The main finding of this study that stringent IRS tax enforcement reduces the cost of bank loans holds 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, Hoopes, Thornock & Williams (2017) that the introduction of legislation that improves public corporate disclosure facilitates IRS tax enforcement and provide some evidence from a quasi-experimental setting. We investigate if 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. By using a difference-in-difference analysis, we find that after the application of the Section 404b of the SOX Act firms that must comply with this regulation (accelerated

[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, Fang & Pittman (2017).

filler firms) report a significant decrease in their loan spreads when compared with exempted match firms (non-accelerated filler firms). This result conforms to the notion that banks reduce the cost of loans after the introduction of legislation that facilitates IRS tax enforcement.

Altogether, our study shows that IRS tax enforcement has an important relationship with the cost of bank loans, which translates into lower borrowing costs. This study contributes to the literature in two ways. The first is that we add to the extant research that examines the external corporate governance role of tax enforcement (Hoopes, Mescall & Pittman, 2012; Hanlon, Hoopes & Shroff, 2014; Kubick, Lockhart, Mills & Robinson, 2017). In particular, we add to the stream of this literature that investigates the role of tax enforcement as an external mechanism of corporate governance that reduces information asymmetries and thus the financing costs for firms in the bond and equity markets in the US environment (Guedhami & Pittman, 2008; El Ghoul, Guedhami & Pittman, 2011; Bauer, Fang & Pittman, 2017). We complement these studies by finding that IRS tax enforcement reduces the cost of bank loans. This finding is significant in the context of this literature because it shows that even banks, who are more efficient than other debt-holders in solving informational asymmetry problems through screening and monitoring, value the informational and monitoring role of the IRS. The second contribution is that we add to the burgeoning literature that investigates the determinants of the cost of loans. Several studies find that internal corporate governance mechanisms matter for the price of bank loans (see, e.g., Fields, Fraser & Subrahmanyam, 2012; Francis, Hasan, Koetter & Wu, 2012; Francis, Hasan & Wu 2013). This study provides evidence that external corporate governance mechanisms, such as tax enforcement, are also of importance when banks set the price of the loans they supply to corporations. Furthermore, we also add to the limited literature that examines the effect of tax-related issues on the cost of bank loans (see, e.g., Hasan, Hoi, Wu & Zhang, 2014). Finally, this study could further stimulate 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 reducing the cost of bank financing for US corporations.

The rest of the paper is organised in the following way. Section 2 discusses some theoretical considerations and formulates the hypotheses. Section 3 presents the data, research design and descriptive statistics. Section 4 discusses the main empirical findings together with some robustness and sensitivity tests, whilst Section 5 offers some concluding remarks.

2. Theoretical considerations and development of hypotheses

In this section, we motivate our research hypotheses by focusing on the role of the tax enforcement as an external mechanism of monitoring and corporate governance that could alleviate asymmetry of information issues between banks and borrowers in the market for syndicated loans. 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 tax enforcement and the cost of bank loan strengthens for smaller corporations.

Information asymmetry between banks and borrowers is an important factor for the pricing and the contract terms of loans. Banks demand a higher interest and set stringent contract terms, such as covenants, for the loans they supply to firms with poorer information quality (Graham, Li & Qiu, 2008; Hollander & Verriest 2016; Houston, Itzkowitz & Naranjo, 2017; Prilmeier, 2017). A source of the asymmetry of information between the insiders of a firm 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 benefit. Therefore, they have incentives to distort the information on firm performance, such as earnings, to conceal their rent-seeking activities from the firm's outsiders (Leuz, Nanda & Wysocki, 2003).

The engagement of a firm in aggressive tax strategies and precarious 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 & Dharmapala (2006) posit that engaging into aggressive tax strategies requires complex structures and an opaque information environment that would enable 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, Li & Zhang, 2011).

These tools comprise *inter alia* earnings manipulations, related party transactions and the hoarding of information (Desai & Dharmapala, 2006; Kim, Li & Zhang, 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 in 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 decrease firm value

(Desai, Dyck, & Zingales, 2007; Desai & 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 with aggressive tax strategies (Hasan, Hoi, Wu & Zhang, 2014).

Tax enforcement could act as an external corporate governance mechanism that reduces the asymmetry of information issues that stem from aggressive tax strategies and risky tax positions. Desai, Dyck & Zingales (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 settlement between firm outsiders and the tax agency. Intense monitoring from the tax agency could discourage firms from engaging in tax aggressive strategies and taking risky tax positions (Hoopes, Mescall & Pittman, 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, Li & Zhang, 2011). In support of this argument, Dyck & 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 & Dharmapala (2007, 2009) also suggest that strict tax enforcement discourages firm insiders from misusing corporate income. This chilling effect of tax enforcement on the ability of managers to engage in rent-seeking actions could improve the information available on a firm since the managers' justification for obfuscation of information subsides. Therefore, the quality of information available to a firm's outsiders, such as the creditors, could improve when tax enforcement is strict (Desai, Dyck & Zingales, 2007; Hanlon, Hoopes & Shroff 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 impact that stringent tax enforcement could exert on firm managers stems from the ability of the tax authority to impose substantial direct material penalties on firms (Wilson, 2009)[6]. It also stems from the indirect reputational and political costs that firms could incur due to

[5] To be clearer, aggressive tax practices and risky tax positions could have a negative or a positive 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, Lee, Lim & Shevlin, 2016). In case the second effect prevails, then risky tax positions could reduce firm value as Desai, Dyck & Zingales (2007) posit.

[6] For example, in 2007 the company Merck agreed to pay to $2.3 billion in interest, back taxes, and penalties 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 and Astrazeneca, $3.4 and $1.1 billion respectively. https://www.irs.gov/newsroom/merck-agrees-to-pay-irs-23-billion

aggressive tax behaviour (Hanlon and Slemrod, 2009; Mills, Robinson and Sansing, 2010; Dyreng, Hoopes and Wilde, 2016). The above discussion suggests that banks could value the informational and monitoring role of tax enforcement and reduce bank loan spreads.

In the US context, the empirical evidence with regards to the capacity of the tax enforcement effort of the IRS to alleviate information asymmetries issues in the financial markets is inconclusive. The studies of Gedhami & Pittman (2008) and of El Ghoul, Guedhami & Pittman (2011) provide evidence that stringent IRS tax enforcement has a negative association with the cost of bonds and the cost of equity respectively. These findings provide empirical support to the view that the IRS plays an informational and monitoring role in the financial markets of the United States. 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 notion that tax enforcement agencies could reduce information asymmetry issues. The above stems from the fact that the IRS has failed to detect serious accounting and financial scandals (Erickson, Hanlon & Maydew, 2004; Dyck, Morse & Zingales, 2010). Furthermore, public enforcement agencies, such as the IRS, may possess limited firm-specific knowledge and lack of auditing resources/expertise that casts doubt on their monitoring capabilities (Jackson & Roe, 2009; Hanlon and Heitzman, 2010). Moreover, the policymakers who are in charge of public enforcement agencies, such as the IRS, may be 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, Beran, D'Avino and Hawkins, 2007; Jiang, Robinson and Wang 2016).

The previous mixed evidence with the regards to the ability of the IRS to alleviate information asymmetries issues motivates us to investigate if firms subject to stricter 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 in the private debt market that could reduce bank loan spreads. Thus, we formulate our main hypothesis *(H1)* as follows:

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

[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.

We also develop a secondary hypothesis and posit that the negative association of IRS tax enforcement on bank loan spreads strengthens for smaller corporations. The theoretical model of Diamond (1985) postulates that large firms have stronger incentives for information production. This is 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 exhibit high information asymmetry issues (e.g., Bhattacharya, Desai & Venkataraman, 2013; Muslu, Radhakrishnan & Subramanyam, 2014; Lambert & 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 & Sutherland, 2017). We conjecture that banks would perceive that the credibility of the information available on the tax returns is higher when the probability of an audit by the IRS is higher.

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 & McVay, 2005; Doyle, Ge & McVay, 2007; Ge, Koester & McVay 2017). This may aggravate managerial rent-seeking and lead to a further deterioration of the quality of the information available on small firms. This could be the case because, as discussed previously, the distortion of the actual financial position of the firm is a way through which managers would try to conceal their rent-seeking behaviour (Leuz, Nanda and Wysocki, 2003). Thus, more intensive IRS tax enforcement 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. The Transactional Records Access Clearinghouse (TRAC) states in 2008[8] that the IRS orders its revenue agents to focus on smaller firms, who are easier to audit, to increase the efficiency of its operations. 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 just because it can detect with fewer effort cases of non-compliance. Smaller firms are more straightforward to audit due to a smaller scale and less complex operations.

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

Based on the above discussion, stringent IRS tax enforcement could be particularly important for alleviating information asymmetry issues between banks and smaller firms. Therefore, our second hypothesis *(H2)* is the following:

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

3. Data, Research Design and Descriptive Statistics

3. 1 Sample Selection

We source data on syndicated loans from Thomson One Banker. This database covers extensively the US syndicated loan market 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 several loans in a given year. Therefore, 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 matching process yields 15,858 loan facilities for 2,448 unique firms over the 1985-2016 period. The IRS tax enforcement data are available for the 1992-2016 period. Therefore, when we merge the data on loan facilities and firm characteristics with the IRS tax enforcement data we obtain our final sample that comprises up to 9,971 observations at the firm-year level 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 employ 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 from the IRS. We source TRAC data on yearly face-to-face corporate audit rates by the IRS to use them as

[9] We follow previous studies and perform our estimations at the loan facility level and not at the loan package deal level (e.g. Hasan, Hoi, Wu & Zhang, 2014; 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.

the primary measure of 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, regarding total assets, each year[10]. Thus, the IRS audit rates exhibit variability both regarding firm size class and regarding the calendar year. In particular, the variable *Audit rate* stands for the number of completed IRS audits of corporate tax returns in a given year *t* for each IRS firm size class, divided by the number of corporate tax returns filed in the prior year (*t-1*) for the same firm size class. Therefore, the IRS audit rate captures the probability that a firm in each asset size group will experience an IRS audit in a given year. In our analysis, we include contemporaneous IRS audit rates on the assumption that managers predict future IRS audit rates on rational expectations and they do not make systematic errors.[11] We focus on the IRS *Audit rate* as a tax enforcement measure because we are interested in capturing the managers' view that a firm will experience an IRS audit in a given year. Thus, if the IRS *Audit rate* relates to the managers' perception of the possibility that a corporation will experience an audit by the IRS, our analysis should be able to observe the relationship between IRS tax enforcement and the cost of bank loans[12].

By covering the most prolonged 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 regularly refer 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, Hoopes & Shroff, 2014). The credibility of the IRS audit rates as a measure of tax enforcement is evident in its extensive use by the government and several previous academic

[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 to $250 million, $50 to $100 million, $10 to $50 million, $5 to $10 million, $1 to $5 million, $0.25 to $1 million and $0 to $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 to $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. The reason being that there is a delay between the time that a firm reports its tax returns to the IRS and the time that the IRS completes its investigations (Graham & Tucker, 2006).

[12] Hanlon, Hoopes & Shroff (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 the following: information on proposed IRS budgets that are on public record, news about leadership changes in the IRS, recruiting prior 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 (e.g. TRAC) that monitor and publish data on IRS activities. Furthermore, Hoopes, Mescall & Pittman (2012) provide evidence from interviews with managers that the probability of an IRS audit influences a firm's tax related strategies.

studies (Guedhami & Pittman, 2008; El Ghoul, Guedhami & Pittman, 2011; Hoopes, Mescall & Pittman, 2012; Hanlon, Hoopes & Shroff, 2014).

Yet, we complement our main tax enforcement variable with a number of federal-level proxies that capture IRS staffing levels. Reason being that the IRS admits that its tax enforcement capacity diminishes at lower staffing levels (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, Lopez-de-Silanes & Shleifer, 2006) and the reputational damage of financial penalties on firms (Armour, Mayer & Polo, 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 (see for example Guedhami & 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 annual criminal referrals. Information on these alternative IRS tax enforcement variables is available for the 1992-2003 period. This reduces our sample to 3,312 observations. Nevertheless, we conjecture that additional estimations with these alternative measures enhance 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:

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

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 each loan facility that a firm attains 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 also employ control variables for several firm characteristics. These comprise measures of profitability, leverage, size, liquidity, tangibility, and the cash effective tax rate following previous empirical research (Graham, Li & Qiu, 2008; Hasan, Hoi, Wu & Zhang, 2014; 2017). For these firm-level controls we use information from the year before the initiation of each loan facility to somewhat ease potential endogeneity concerns. Furthermore, we control for loan

characteristics. In particular, we employ control variables that capture the size of a loan and its maturity as well as dummies that control for the type and the purpose of a loan. We also use dummies to control for time (i.e. year) 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 size class and year 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%. Panel B shows 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 research (Guedhami & Pittman, 2008; Hoopes, Mescall & Pittman, 2012). Furthermore, our sample exhibits an even 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 primary explanatory variables that we include in the empirical specifications. Regarding loan characteristics, the average loan size and loan spread are $433 million and 200 basis points respectively. Furthermore, the mean loan maturity is around 4 years. These descriptive statistics are in line with previous empirical studies (see, e.g. Graham, Li & Qiu, 2008; Hasan, Hoi, Wu & Zhang, 2014). In Table 4 we report the Pearson correlations coefficients for the main variables in our analysis. The preliminary evidence from the correlation analysis suggests that *Audit rate* and *Loan spread* exhibit a negative and signficant at the 1% level relationship. This negative association is 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 the spreads of bank loans and these associations are in line with those available in previous empirical work (Graham, Li & Qiu, 2008; Bharath, Dahiya & Saunders, 2009; Hasan, Hoi, Wu & Zhang, 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 *H1* that US firms enjoy lower bank loan costs when tax enforcement is more stringent. We use ordinary least square (OLS) models with robust standard errors and within-firm clustering.[13] Our models show a good fit with 50% adjusted R^2 on average. In the first model of Table 5, we control for corporate characteristics while in the second model of Table 5 we control for both corporate and loan characteristics. The coefficients on *Audit rate* $_t$ are significant at the 1% level and negative (-0.00709, -0.00796) in the first and second model 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 commenced in 1992. Analogous results we observe 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 findings provide empirical evidence in support of our main *H1* hypothesis that the spreads of bank loans decrease at higher tax enforcement levels. The estimations of the second model of Table 5 imply that increasing the probability of an audit by the IRS from 24.7% (25th percentile) to 37.3% (75th percentile) leads to a 10 basis points decrease of bank loan spreads. An alternative way to evaluate the relevance of these findings is by looking at the mean borrowing firm savings, in terms of annual interest, based on the average loan size, 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 comparable with other studies that stress the importance of the asymmetry of information between borrowing firms and banks for the price of bank loans (Bharath, Dahiya & Saunders 2009; Francis, Hasan, Huang & Sharma 2012). Additionally, our results for the firm control variables show that larger firms, with higher profitability, less leverage and more assets that are tangible enjoy lower borrowing costs in line with previous studies (Graham, Li & Qiu, 2008; Bharath, Dahiya & Saunders 2009; Hasan, Hoi, Wu & Zhang, 2014). Furthermore, large loans with shorter time to maturity enjoy lower bank loan spreads. These results conform to the ones of Chava, Livdan & Purnanandam (2008) who suggest that banks exposed at the risk of a long period to maturity charge higher loan spreads as a compensation.

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

Overall, the estimations we obtain from the baseline models in Table 5 show that IRS audit rates and bank loan costs are negatively related. These findings, in line with our main hypothesis *H1*, suggest that banks perceive IRS tax enforcement as an efficient 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 a key 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 & Pittman, 2008; El Ghoul, Guedhami & Pittman, 2011).

In Table 6, we provide estimations from models that focus on smaller corporations. This means that we exclude from these estimates observations from firms that fall within the largest IRS defined firm size group (i.e. corporations with asset size higher than $250 million). This exercise aims to investigate if the negative relationship between the IRS audit rates and bank loan spreads persists and whether it is indeed stronger for smaller corporations as we conjecture in our secondary *H2* hypothesis[14]. We provide estimations for models that focus on firms that fall within the IRS firm size groups, regarding assets, of $250-$100 million (models 1 and 2 of Table 6) and $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, which include the larger firms, we observe that the negative relationship between the IRS audit rates and the cost of bank loans is stronger, from an economic standpoint, for smaller corporations. Thus, the results from the estimations in Table 6 provide empirical support to our secondary *H2* 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 tax enforcement becomes vitally important, regarding the price of bank loans, for smaller firms that exhibit a poorer information environment and weaker internal

[14] 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, Guedhami & Pittman, 2011). Such firms for example could be the very large corporations (asset value of higher than $5 billion) that belong to the Coordinated Industry Case (CIC) of the IRS and experience the constant monitoring of the agency (El Ghoul, Guedhami & Pittman, 2011).

corporate governance structures in comparison with larger corporations (Bhattacharya, Desai & Venkataraman, 2013; Ge, Koester & McVay, 2017).

4.2 Robustness and Sensitivity Tests

4.2.1 Controlling for Alternative Firm Size Measures

The measure of IRS tax enforcement that 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 the size of firms 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 *H1* hypothesis are sensitive when we control for alternative measures of firm size (Bauer, Fang & Pittman, 2017). Hence, in models 1 and 2 of Table 7, we replace the natural logarithm of a firm's total assets as a variable that captures size with the natural logarithm of total equity and sales respectively. In these models, both of these alternative size variables exert a negative and significant at 1% level 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 results are comparable to 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 tax enforcement, on the cost of bank loans. Nevertheless, we complement this analysis with a series of alternative IRS tax enforcement proxies that capture further aspects of IRS tax enforcement. The first set of these alternative IRS tax enforcement measures reflects the yearly levels of IRS staffing (normalised by the annual corporate tax returns). Since these alternative IRS tax enforcement measures do not hinge on firm size, as the IRS audit rates do, they could supply further empirical evidence with regards to the effect of IRS tax enforcement on the cost of bank loans. The estimations that investigate the impact 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, the coefficients of IRS employees (model 1), IRS revenue agents (model 2) and IRS criminal investigators (model 3) enter the regressions with a negative sign and are significant 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 *H1* hypothesis that banks value IRS tax enforcement when they price loans to borrower firms. Additionally, these findings align with the government acknowledgment that staffing levels play a crucial 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 civil penalties imposed by the IRS for tax fraud and negligence against firms. 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 the spreads of bank loans. 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 provide additional empirical support to the critical role of tax enforcement in reducing the cost of bank loans.

One weakness of the estimations in Panel A of Table 8 is that we do not include time effects. As discussed, this is due to the fact 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 permits the presence of year 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 & Ongena, 2010; Schenone, 2010; Prilmeier, 2017). Hence, we posit that banks would perceive IRS tax enforcement especially important as an external monitoring and corporate governance mechanism with regards to 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[15] of a syndicate has not provided a loan to the same firm over the previous five years prior to the current loan and zero otherwise[16]. 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 enforcement 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. In these models, the coefficients of the interactions between the alternative IRS tax enforcement measures and the *NLR dummy* $_t$ are negative and exhibit at the 5% level of significance except 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 enforcement is especially vital 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, Mescall & Pittman (2012) and Bauer, Fang & Pittman (2017) outline two reasons why this is plausible. The first reason is that firm managers could exert limited influence on the audit probability rates since the latter depends on firm size class, time and their interaction (Guedhami & Pittman, 2008; El Ghoul, Guedhami & Pittman, 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 influence the auditing decisions of the IRS with regards to the entire business population in each firm size class (Hoopes, Mescall & Pittman, 2012). The second reason is that the intensity of IRS tax enforcement is dependent on the governmental political ideology, which could be exogenous to firm outcomes such as the cost of the bank loans. Bagchi (2016) for

[15] 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).

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

example show that audit rates of the IRS are significantly higher when the Democrats hold the executive branch of the US government.

Nevertheless, Hoopes, Mescall & Pittman (2012) and Bauer, Fang & Pittman (2017) posit that one cannot exclude with certainty such reverse causality issue. For example, firm managers could engage in firm strategies, such as divestments or acquisitions, which could affect the firm size and therefore the audit probability that their firms could face[17]. Furthermore, some firms might have powerful 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. As a first step, we obtain predicted values of *Audit rate* $_t$ by employing an OLS model that comprises two instruments and the control variables that we employ in our baseline analysis. As a second step, 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 & 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 depend on the likelihood of an audit by the IRS 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[18] 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 only for three years after a corporation has filed its tax return (El Ghoul, Guedhami & Pittman, 2011). For the second instrument of the IRS audit rates we follow Hoopes, Mescall & Pittman (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 that the IRS audits in each firm size class in a specific year divided by the number tax returns filed by corporations in each size class in the previous year. Consequently, audit rates depend on fluctuations of tax returns filed across time in the same firm size group[19]. The numerator component (i.e., the number of corporate tax returns that the

[17] 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.

[18]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.

[19]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

IRS audits) 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 tax returns that were filed by corporations in the previous year), which we use as the second instrument, reflects a lagged value concerning 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) exert 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)[20] and the Hansen over-identification test (OIT). The second stage findings (see the 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 and signficiant at the 1% level effect on the cost of bank loans. We achieve 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[21]. Thus, the findings from the two-stage instrumental variable estimation in models 1 to 3 of Table 9 provide further support to our main *H1* hypothesis that IRS tax enforcement reduces the cost of bank loans[22].

Table 9

4.2.4 Other Robustness and Sensitivity Tests

The estimations so far show that US public firms incur lower bank loan spreads under a higher level of IRS tax enforcement. This section includes some additional tests to provide

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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 of the IRS audit rates. Furthermore, this result conforms to the findings of Hoopes, Mescall & Pittman (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.

[20] We use the critical values for the weak identification test (WIT) as developed by Stock & Yogo (2005).

[21] 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.

[22] 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 & Pittman, 2008; Hoopes, Mescall & Pittman, 2012). This is because of the difficulty of identifying 100% suitable instruments for the IRS audit rate.

further empirical proof in support of this finding. Our sample includes firms that obtain more than one loan 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 for firms that have obtained more than one loan facility in a given year (Francis, Hasan & Wu, 2013; Hasan, Hoi, Wu & Zhang, 2014). In the first model of Table 10, the coefficient on *Audit rate* $_t$ is negative and significant at the 1% level (-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 fails to accept the null hypothesis for random effects, we also tabulate in model 3 of Table 10 the findings from the random-effects estimation to show the consistency of our results. Additionally, in model 4 of Table 10, we re-estimate our regression using lead-bank fixed effects as in Hasan, Hoi, Wu & Zhang (2014). We show that *Audit rate* $_t$ is negative and significant at the 1% level as in our baseline regressions. Lastly, in model 5 of Table 10, we control for potential bias stemming from potential inflation effects reflected into firms falling into higher IRS defined firm size classes over the period of the study. To this end, we drop from our sample observations of firms that have moved to a different IRS firm size class during the period of our study following Hoopes, Mescall & Pittman (2012). Our results show that *Audit rate* $_t$ continues to exert a negative and significant at the 1% level effect on bank loan spreads in support of our previous findings.

Table 10

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

Recent empirical evidence from Bozanic, Hoopes, Thornock & Williams (2017) shows that regulations that increase public corporate disclosure facilitate IRS tax enforcement. The authors find that the IRS acquires information from the public financial disclosures of corporations to supplement and corroborate the private information, such as the corporate tax returns, it already possesses on these firms.

A regulation that its introduction has significantly increased the public disclosure of financial information on corporations, and therefore could facilitate 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 enhance the financial reporting quality of public US 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) regarding 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 a strong emphasis on auditors obtaining sufficient evidence to support their attestations (PCAOB, 2015).

In the light of the findings of Bozanic, Hoopes, Thornock & Williams (2017), it is plausible that the increased public financial reporting via the Section 404b of the SOX Act could facilitate IRS tax enforcement. 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 a lower quality of financial reporting (Doyle, Ge & McVay, 2007; Ashbaugh-Skaife, Collins, Kinney Jr. & LaFond, 2008) and this could also relate to taxation information. Furthermore, many ICWs under the Section 404b of the SOX act refer to the tax function of a corporation (Bauer, 2016). Gleason, Pincus & Rego (2017) find that in corporations with tax-related ICWs the implementation of aggressive financial reporting practices becomes easier. This could be of interest to the IRS since such methods 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 tax enforcement and therefore enhance its importance as an external corporate governance mechanism that lowers information asymmetries and tax uncertainties. This could lead to lower bank loan spreads 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 the firms that have a smaller than $75 million public float value. The reason being that these firms face relatively higher burdens regarding compliance costs. As a result, the introduction of the Section 404b of the SOX Act offers a quasi-experiment to explore any change in bank loan spreads between two groups of firms; treated and control firms. The logic there is that the introduction of the Section 404b of the SOX Act

facilitates IRS tax enforcement 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 the firms that their market capitalization is more than $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 filler. We source annual data observations from Audit Analytics to identify which firms fall within the treated group (accelerated filler 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 tax enforcement, then one may expect that treated firms would enjoy lower borrowing costs regarding bank loan spreads after its implementation compared with the control firms.

We examine this prediction employing a sample covering two periods; one period before the enactment 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 complies with the Section 404b of the SOX Act for the 2003-2016 periods.[23] Next, we employ 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 the 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 bank loan spreads among treated and control companies for the two different 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

[23]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.

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.[24]

These results are in line with the notion that the information available through the implementation of the Section 404b of the SOX Act could facilitate IRS tax enforcement. 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 bank loan spreads 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 to reduce the risk associated with granting credit (Graham, Li & Qiu, 2008; Bharath, Dahiya & Saunders, 2009; Hasan, Hoi, Wu & Zhang, 2014). We use logistic regression to observe the relationship between *Audit rate* $_t$ and a dichotomous 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 observe 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 mechanism of corporate governance that could mitigate information asymmetries between them and corporate borrowers. Thus, stringent IRS tax enforcement 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 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

[24]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.

5. Conclusion

Previous empirical studies find that tax enforcement plays an informational role in the financial markets and decreases 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 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 tax enforcement 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 mechanism of corporate governance 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 for 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 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 $_t$	The number of IRS employees divided by corporate tax returns in year t	TRAC
IRS revenue agents $_t$	The number of IRS revenue agents divided by corporate tax returns in year t	TRAC
IRS criminal investigators $_t$	The 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 $_t$	The number of IRS criminal tax referrals divided by corporate tax returns in year t	TRAC
IRS criminal prosecutions $_t$	The 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 of 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. 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	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	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 for year effects. Panel B shows 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. 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 robustness and 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	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 determined 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.