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ABSTRACT

This study examines the relation between voluntary audit and the cost of debt in private firms. We use a sample of 4,058 small private firms operating in the period 2006-2017 that are *not* subject to mandatory audits. Firms decide for a voluntary audit of financial statements either because the economic setting in which they operate effectively forces them to do so (e.g., ownership complexity, export-oriented supply chain, subsidiary status) or because firm fundamentals and/or financial reporting practices limit their access to financial debt, both reflected in earnings quality. We use these factors to model the decision for voluntary audit. In the outcome analyses, we find robust evidence that voluntary audits are associated with higher, rather than lower, interest rate by up to 3.0 percentage points. This effect is present regardless of the perceived audit quality (Big-4 vs. non-Big-4), but is stronger for non-Big-4 audits where auditees have a stronger position relative to auditors. Audited firms' earnings are less informative about future operating performance relative to unaudited counterparts. We conclude that voluntary audits facilitate access to financial debt for firms with higher risk that may otherwise have no access to this form of financing. The price paid is reflected in higher interest rates charged to firms with voluntary audits – firms with higher information and/or fundamental risk.

Keywords: private firms, voluntary audit, cost of debt, self-selection bias, risk

JEL Classification: M4, M42, M49

1) Introduction

The importance of financial statement audit for cost of debt has been largely studied in public-firm settings, but it is also gaining a momentum in the context of private firms. Financial statements' certification by an outside party is generally seen as providing additional assurance about the quality of financial statements and improves their credibility to outside users (Cassar et al., 2011; Dedman & Kausar, 2012; Kausar et al., 2016). Auditing acts as a guarantee that the statements have been prepared in accordance with existing regulations and accounting principles, and are adequately reflecting firms' underlying economic performance. Therefore, audit verification should result in reduced cost of debt relative to an otherwise equal, but unaudited firm. However, private firms are more likely to use the financial reporting process for objectives other than reporting true firm performance (Ball & Shivakumar, 2005). Examples include tax savings (Garrod et al., 2008), and non-tax benefits such as preserving future debt capacity (Szczesny & Valentincic, 2013), dividend payouts (Ball & Shivakumar, 2005; Burgstahler et al., 2006), employee relations (Brown et al., 1992). Consequently, the role of (voluntary) audit[1] in this process is not necessarily equal to that in public firms, although we stress this does not, in turn, imply that there are no economic incentives to engage in voluntary audit (Cassar et al., 2011).

In a public-firm setting, where audit is mandatory, researchers cannot observe its effects on cost of debt *per se*, but only observe which characteristics of the audit are associated with the cost of debt (Kausar et al., 2016). A setting where audit is voluntary, however, allows researchers to observe directly the effects of audit on the cost of debt as an important economic outcome. Traditionally, audit is valued by the capital market for its role as an information intermediary and for its assurance role (Mansi et al., 2004). Audit helps alleviating information asymmetries by independently verifying the reliability and integrity of the financial reporting process to the users of financial statements. Auditors also offer a risk

[1] We use the term voluntary audit to cover all audit decisions that are not legally prescribed (mandatory). This would for example include instances where a bank requires an audit as condition to provide financing or where a parent company requires a subsidiary to be audited for group reporting purposes.

guarantee because of the responsibility that they normally carry in the case of audit failures. In the public-firm setting, Mansi et al. (2004) find creditors valuing the assurance role by requiring a lower return on corporate debt.

These roles might be different for private firms (and their creditors). First, financial statements are less likely to be used to alleviate information asymmetries between owners and managers (Ball & Shivakumar, 2005; Garrod et al., 2008; Kosi & Valentincic, 2013). Second, creditors may rely more on other information sources, e.g., (qualitative) private information gathered from relationships with a firm, its owners and local community (Berger & Udell, 2006; Campbell et al., 2019). Third, the effectiveness of audit in private firms depends on the litigation risk and a country's tax environment (Vander Bauwhede et al., 2003; Van Tendeloo & Vanstraelen, 2008; Cano-Rodríguez, 2010). Due to lower level of litigation risk in the private-firm setting, and in Europe in general compared to the US (Francis, 2004; Szczesny & Valentincic, 2013), the assurance role of auditors may be relatively less important than for the larger and/or public firms.

Despite these possible influences and limitations to the value of audit in private firms the following motivations for private firms engaging in voluntary audits are documented in existing literature. First, audit may still mitigate the agency issues between the owners and/or managers and creditors, thereby lowering the monitoring costs for the creditors (Collis, 2010). This may be particularly the case in (smaller) private firms with simpler governance structures and absence of other control mechanisms. Similarly, the owner/manager may demand an audit as compensation for the loss of control because of a firm's hierarchical structure (Abdel-Khalik, 1993). Second, a voluntary audit could be used as commitment to higher reporting transparency that allows more reliable contracting in supply chain relationships. Since information asymmetry is a key factor in customer-supplier contracting, a major customer would require information of appropriate quality and quantity from their suppliers (Costello, 2013). Third, creditors base their lending decisions on information comprised of: (i) hard information such as (un-)audited financial

statements (and ratios calculated from them) (Berger & Udell, 2006); and (ii) (qualitative) private information gathered from relationships with firms (Campbell et al., 2019). Creditors use this (past) information to predict firms' future cash flows needed for repayment of debt. Unsurprisingly, higher earnings quality increases private firms' access to debt financing and lowers their cost of debt (Ding et al., 2016). In situations of high information and/or fundamental risk (see e.g., Tang, 2011), where as a consequence firms' reported earnings numbers relatively poorly predict future cash flows, audit of financial statement may be required as an additional means of creditors' 'self-insurance'. If the firm later defaults, the creditor can more easily claim to have done everything possible to ascertain the viability of the borrower.

Obtaining external financing for investment projects is very important for (small) private firms. The main providers of such financing are banks (Santikian, 2014), although many firms face severe constraints in accessing external financing. For example, descriptive statistics in Garrod et al. (2008) reveal that over 50% of small private firms in their sample are financed by equity investors and suppliers while having no financial debt. Szczesny & Valentincic (2013) show similar figures in a setting of relatively large private firms in Germany. Tillväxtverket (2017) reports that in recent years smaller Swedish firms apply for fewer loans and face higher rejection rates by creditors. Therefore, access to bank loans and cost of debt considerations are central to private-firm financing, and we investigate the effect of voluntary audit on the cost of debt.

We hypothesize that voluntary audit significantly affects the cost of debt. However, the direction of this relation depends on firm's reporting incentives, including the underlying reasons for the decision to have an audit. Firms may be aware of the effects of financial statement audit on the cost of debt, as discussed above. If they have positive NPV investments to execute and rely heavily on bank financing, they may have a true, firm-value maximizing incentive to have a voluntary audit and signal commitment to higher financial reporting transparency. Creditors may perceive firms in which reported numbers

relatively poorly predict future cash flows as carrying higher information and/or fundamental risk. As a result, these firms may have stronger incentives for financial statement audit to improve access to bank financing and obtain the lowest possible cost of debt given the underlying risks. Private firms have been shown to manage earnings to signal better quality to lenders (Mafrolla & D'Amico, 2017). On the other hand, firms with reported numbers closer to the underlying cash flows involve less information and/or fundamental risk for creditors. These are the less likely to require self-insurance in form of audited financial statements to provide firms with the lowest possible cost of debt given the underlying risks. Thus, it is an open empirical question what is the effect of voluntary audit on the cost of debt. We also investigate whether the effect of audit differs across Big-4 and non-Big-4 auditors.

To unravel the association between voluntary audits and the cost of debt in a private-firm setting, we employ a large sample of 4,058 distinct small private firms (15,962 observations) operating in the period from 2006 to 2017 in Slovenia. A notable advantage of our empirical setting is access to a detailed domestic database with financial statement information on all firms operating in Slovenia, containing standardized reports for both public and private firms.[2] Financial statements of private firms are often not publicly available and often lack the level of detail that would allow researchers to construct a precise measure of the cost of debt.

Contrary to several existing studies and after controlling for determinants of voluntary audit (ownership and legal complexity, supply chain relationship, bank demand), we find a robust central result that voluntary audits imply higher, rather than lower, cost of debt. Voluntary audit by a non-Big-4 auditor on average increases the interest rate by 3.0 percentage points. If voluntary audit is performed by a Big-4 auditor the cost of debt incrementally decreases but remains higher than for the unaudited firms (average increase by 2.2 percentage points). These findings imply that financial statement audit in a private-firm (voluntary-audit) setting reflects higher information and/or fundamental risk of borrowers

[2] The system of data collection is similar to the Belgian system in Europe and to the South Korean system in Asia.

and consequently results in higher cost of debt. Our study underlines and correctly addresses the self-selection issues present in the setting – firms are not randomly assigned to audited and unaudited groups. The main findings are thus based on Heckman's (1979) two-stage approach as one of the three ways to address selection bias (next to propensity score matching and instrumental variable approach which we describe further in section 5.2). We believe that the Heckman's assumptions are the most appropriate for our setting because an audit is an endogenous firm choice, which is related to factors observable as well as unobservable to the researcher.

After finding the relation between voluntary audit and cost of debt, we perform a validation analysis of the underlying mechanism. We compare the relative ability of audited (Big-4 and non-Big-4 separately) and unaudited financial statements to predict future cash flows. With one exception, results are as expected and show lower (higher) predictive ability (level of earnings management) for audited (in particular non-Big-4) compared to unaudited firms. To assess the robustness of our primary finding, we perform a series of sensitivity test, which consistently corroborate our main finding. In particular, we address the potential effect of the financial crisis, subsidiary status of some firms and inclusion of additional risk controls (Altman Z-score and operational risk). We descriptively confirm the relation between audit and cost of debt using observations that start or cease an audit, or switch the auditor. We also extend the sample and add firms with a mandatory audit. Finally, we use two alternative cost of debt measures based on financial statements and one based on data from individual loan contacts provided by the central bank (Bank of Slovenia, Eurozone). The latter measure yields a positive coefficient on voluntary audit albeit not significant at conventional levels.

We contribute to the existing literature in at least four important ways. First, contrary to some relatively recent studies (Kim et al., 2011; Minnis, 2011; Kausar et al., 2016; Huq et al., 2018), we show that creditors do not necessarily view voluntary audits as mere signal of increased financial reporting transparency, and that voluntary audits do not necessarily reduce the cost of debt. Estimation of future

cash flows based on borrower's (past) information is an integral part of creditors' lending decisions (i.e., access to debt and its terms) but firms vary in the ability of their financial statements to predict future cash flows. Our findings indicate that reported numbers of audited private firms relatively poorly predict future cash flows, and these firms consequently pay higher cost of debt. In this setting voluntary audit is thus viewed as a reflection of information and/or fundamental risk. Second, we highlight that cost of debt effects are subject to severe endogeneity issues. As there is market demand for voluntary audits, we first model the audit decision as a function of these economic determinants, including information and/or fundamental risk. We then study the outcome variable (cost of debt) while controlling for the selection determinants. Our main analysis involves controls for endogeneity and clearly underlines that this issue significantly affects the results if left uncontrolled for (also see Bar-Yosef et al., 2019). Third, we carefully control for self-selection into voluntary audits under different assumptions. We assume that observable variables cause self-selection, or alternatively that there are other unobservable factors affecting the audit decision and the cost of debt (more likely in a private firm setting with a poorer information environment) (Tucker, 2010). This step is often missing in existing literature with the exception for Downing & Langli (2019). Fourth, while our data source contains very detailed financial statement data, and is an advantage in itself in this setting, we additionally combine it with interest rate data from individual loan contracts compiled by the central bank. Employment of both data sources thus enables us to reduce the (non-random) measurement error in the dependent variable (interest rate), in a relatively precise manner compared to prior studies.

The remainder of the paper is structured as follows. Section 2 describes previous research in the field and develops the hypotheses. Section 3 presents our research design while section 4 describes the sample and its selection process. Section 5 presents the main results, section 6 continues with additional analyses and section 7 concludes.

2) Prior research and hypotheses development

2.1) Determinants of cost of debt – the outcome

Existing literature investigates various determinants of the cost of debt including its association to the attributes of financial reporting. A common finding is that the cost of debt is reduced with higher financial reporting transparency (e.g., Francis et al., 2005; Bharath et al., 2008; Bharath et al., 2011; Florou & Kosi, 2013; Kausar et al., 2016). Moreover, research on the effect of audit on cost of debt has mainly focused on the role of audit in improving the credibility and reliability of financial reporting in a public-firm setting with mandatory rather than voluntary audit requirements. For instance, Chu et al. (2009) find that various audit-related information is important for banks' loan-pricing decisions.

Prior literature relates cost of debt to various characteristics of the auditor that encompasses the quality of audit provided. These characteristics include, for instance, auditor size (Mansi et al., 2004; Pittman & Fortin, 2004; Cano-Rodríguez, 2010; Karjalainen, 2011), audit or non-audit fees (Brandon et al., 2004; Dhaliwal et al., 2008), and audit tenure (Mansi et al., 2004; Chu et al., 2009; Kim et al., 2013). In addition, studies find that the cost of debt is reduced if audit is performed by more than one auditor, whereas modified audit reports are associated with higher cost of debt (Karjalainen, 2011). Conversely, research on the effect of the audit *per se* is relatively scarce. Prior research in voluntary-audit setting finds that private firms that choose financial statement audit incur lower cost of debt (e.g., Kim et al., 2011; Minnis, 2011). Similarly, Spanish SMEs with mandatory and voluntary audit incur lower cost of debt (Huguet & Gandía, 2012). In a setting of small firms with mandatory audit, Big-4 auditors also decrease the cost of debt (Karjalainen, 2011). Big-4 auditors are associated with higher audit quality due to reputational attributes, better professional skills and bigger independence (Langli & Svanstrom, 2014; Downing & Langli, 2016). Pittman & Fortin (2004) document lower cost of debt for young public firms that have retained their Big-4 auditor from the pre-public period. Similarly, Kim et al. (2013) find lower cost of

debt for both Big-4 audits and long-tenure audits. Firms engaging in audit are also less likely to be denied loans (Allee & Yohn, 2009).

However, not all research documents positive effects of audit and/or Big-4 auditor. For example, Allee and Yohn (2009) find no effect of audit on the cost of debt, even after differentiating between limited and unlimited liability firms. Cassar et al. (2011) find no influence of audit on lending decisions. Huguet & Gandía (2012), Kim et al. (2011), and Langli (2015) do not find lower cost of debt for private firms audited by Big-4 auditors compared to non-Big4 auditees. Most studies attribute the absence of significant effect of audit to differences in the institutional settings. For example, Piot & Missonier-Piera (2009) argue that French banks use accounting information as a monitoring mechanism less extensively and prefer contractual guarantees. Moreover, Fortin & Pittman (2007) attribute the results to differences in information used by creditors when lending to public or private firms.

Motivated by the findings discussed above, we formulate the following two hypotheses. First, we test whether small private firms with a voluntary audit face different cost of debt than unaudited firms. Firms may be aware of the effects of financial statement audit on the cost of debt, as discussed above. If they can invest in positive NPV investment projects and rely heavily on banks to finance these projects, they may have a true, firm-value maximizing incentive to certify the financial statements and signal higher financial reporting transparency. On the other hand, firms with reported numbers which relatively poorly predict future cash flows may be perceived by creditors as carrying more information and/or fundamental risk (see Tang, 2011). Consequently, the firms may face higher constraints to external financing and thus choose an audit to improve access to bank financing and obtain the lowest cost given the underlying risks. If creditors are aware of and able to differentiate between both types of firms, we should observe differential effect on the cost of debt between audited and unaudited firms. If voluntary audit is chosen by firms with more underlying information and/or fundamental risk, we expect this to result in higher cost of debt compared to unaudited firms.

The second hypothesis distinguishes Big-4 and non-Big-4 audits. Several studies in the public-firm domain show that Big-4 auditors are associated with higher quality audits (see e.g., Francis, 2004, for a review).[3] We reformulate these findings. We point out that both the Big-4 and non-Big-4 auditors incur similar losses in the event of a low-quality audit (e.g., reputational loss and potential exit), but that the gain from obtaining an additional auditee as a client is much larger in relative terms for a non-Big-4 auditor than for a Big-4 auditor. More intuitively, it is less reasonable for Big-4 firms to risk potentially large losses to gain a relatively small audit fee compared to the non-Big-4 auditor, where the audit fee from a private firm might represent a significant part of its sales. Big-4 auditors might thus be less willing to accept clients carrying higher risk (firms with reported numbers which poorly predict future cash flows). Conversely, non-Big-4 auditors might be more willing to accept audit clients with higher information and/or fundamental risk, as they gain a relatively large audit fee. Our second hypothesis tests whether perceived higher quality audit, proxied by a Big-4 auditor, incrementally reduces the cost of debt for small private firms with a voluntary audit compared to non-Big-4 auditees.

To substantiate the relative position of auditors and their auditees we plot their annual median sales in Figure 1.[4] While the Big-4 auditors' median sales exceeds the Big-4 auditees' median sales by a significant margin (i.e., the ratio is well above 100% in all years but 2010, Fig.1a), the ratio of median non-Big-4 auditor sales relative to median non-Big-4 auditee only averages at about 5% (Fig.1b). The relative power of a non-Big-4 auditor versus its auditee is thus relatively minuscule (Fig.1b right hand-side axis).

[INSERT Figure 1 HERE]

[3] We acknowledge that not all of these effects are due to higher-quality audits provided by Big-4 auditor per se, but may only be correlated with high audit quality (see e.g., Khurana & Raman, 2004). The Big-4 vs. non-Big-4 might also reflect client characteristics rather than Big-4 audits being of higher quality (e.g., Lawrence et al., 2011).

[4] We explicitly acknowledge that auditor sales is only a proxy for total audit fee collected from the clients. Auditors' sales may include other sources of sales (e.g., non-audit service fees). The audit fee collected is thus less or equal to total sales of an auditor.

The relatively weak position of non-Big-4 auditors is reinforced by their confinement to the domestic audit market. In Figure 2, we show the proportion of foreign-sourced sales for Big-4 and non-Big-4 auditors. A minor (and decreasing) proportion of non-Big-4 auditors' sales earned in non-domestic markets indicates the lack of possibilities to diversify to foreign markets, which would arguably reduce part of the pressure from relatively stronger clients. All these factors are in line with non-Big-4 auditors' limited ability to decline an audit to a small private firm even if the firm carries more risk.

[INSERT Figure 2 HERE]

2.2) Determinants of audit decision – the selection process

We are aware of selection issues in our setting because firms are not randomly allocated into audited and unaudited group. We therefore use prior literature and new features to model a firm's decision to have an audit. The usefulness of audit in private firms is likely to be different compared to public firms as they are very different in terms of agency conflicts and costs. Agency theory posits lower agency costs in the single owner-manager case with the costs increasing in divergence from this state (Jensen & Meckling, 1976; Ang et al., 2000; Garrod et al., 2008; Kosi & Valentincic, 2013). A private firm's governance structure and presence of various corporate governance tools acting as control mechanisms (Collis, 2010) is largely determined by the firm's legal form. Earnings properties depend upon the legal form of the firm (Bigus et al., 2016). The owner/manager may also demand an audit to compensate for the loss of control because of the firm's hierarchical structure (Abdel-Khalik, 1993). In a private-family-firm setting, Niskanen et al. (2010) on the one hand show that higher ownership dispersion increases agency costs and leads to greater probability of hiring a higher-quality (Big-4) auditor. On the other hand, increased managerial ownership results in lower probability of hiring a Big-4 auditor. Hence, we include measures of legal and ownership complexity into the model of voluntary audit decision. These measures

also control for cases where private firms are subsidiaries of parent firms, which require the audit of subsidiaries as a part of their own audit process.

Second, a small private firm may choose an audit to signal higher financial reporting transparency because supply chain relationships depend on exchange of reliable information (Baiman & Rajan, 2002). To overcome information asymmetry problems and enable reliable contracting, major customers require information of appropriate quality and quantity from their suppliers (Costello, 2013; Bauer et al., 2018).

Third, an important difference between private and public firms is their perceived financial reporting transparency. Prior research has documented that earnings of private firms are of lower quality compared to public firms' reported earnings (e.g., Ball & Shivakumar, 2005; Burgstahler et al., 2006). As a response, creditors may choose to put more weight on (qualitative) private information gathered through the relationships with their borrowers (Chu et al., 2009; Campbell et al., 2019) relative to accounting information. We thus control for common measures of close firm-bank relationships: the proportion of bank-loan financing in total external financing (subdivided in long-term and short-term loans) and the number of bank relationships. In general, small private firms have less need for many bank relationships as one bank is usually capable of supporting their operations. In line with this argument, Petersen & Rajan (1994) find that borrowing from multiple creditors increases the cost of debt and reduces its availability. Multiple-source borrowing weakens each individual firm-bank relationship as each bank tries to be the only creditor to a firm, thus increasing its ability to control the firm's activities. As another response, banks may require financial statement audit as an additional means of self-insurance in cases where borrowers carry high information risk. This may particularly be true for firms where lower quality of earnings is reflected in poor predictability of future cash flows.

3) Research design

3.1) Methodological approach to self-selection

Selection issues occur because observations are not randomly distributed into groups (audited and unaudited firms). Firms that self-select into voluntary audits may differ in some (known or unknown) aspects from unaudited firms. Being aware of the presence of self-selection and consequently selection bias in our setting, we adopt Heckman's (1979) two-stage approach. In specifying the Heckman model, we acknowledge that there may be other, potentially important but unobserved factors next to the ones we discuss in section 2.2 and we are able to measure.[5] Cassar (2011) indicates several potential factors, such as the use of accrual accounting rather than cash accounting, and the mere preparation of (accrual-based) financial statements that might affect the reported cost-of-debt findings (here, specifically the findings of Minnis (2011)).

In our private-firm setting, similar unobservable factors might potentially affect the voluntary audit decision and the cost of debt. We are able to observe the cost of debt for firms that chose a voluntary audit, and the cost of debt for those firms that chose not to be audited. What we cannot observe are the two counterfactual outcomes: the cost of debt for audited firms had they not chosen to be audited, and the cost of debt for unaudited firms had they chosen to be audited. Consequently, researchers are unable to directly estimate the effect of audit on the cost of debt. The observed results have to be used as a proxy, but if this proxy is not close to the counterfactual outcomes, this leads to selection bias. In formal terms, we are interested in the following relationship:

$$Y_i = \alpha + X_i\beta + A_i\gamma + \varepsilon_i \quad (1)$$

[5] In order to cause estimation bias, these unobservable factors must affect both the selection variable (voluntary audit decision) and our outcome variable (cost of debt). If a factor was to affect only one of the two, it would not be a source of such a bias (Tucker, 2010).

where Y_i is a cost of debt for firm i, X_i is a vector of determinants of cost of debt for firm i (β is a vector of coefficients), and A_i is an indicator variable for voluntary audit. However, as described above, we can only observe firms in Eq. (2) and (3):

$$Y_{1,i} = \alpha_1 + X_i\beta + \varepsilon_{1,i} \quad (\text{if } A_i = 1) \tag{2}$$

$$Y_{0,i} = \alpha_0 + X_i\beta + \varepsilon_{0,i} \quad (\text{if } A_i = 0) \tag{3}$$

$$A_i^* = Z_i\beta^* + \upsilon_i \tag{4}$$

where A_i = 1 if $A_i^* \geq 0$, A_i = 0 if $A_i^* < 0$, A_i^* is a continuous latent variable not observable to the researcher (we only observe the values of A_i as its consequence) and Z_i is a vector of variables affecting audit choice.

Heckman's approach first estimates a selection model for the probability to undergo a 'treatment' (a voluntary audit) and then calculates the inverse Mills ratio (henceforth IMR) as a bias correction term. As firms' decision to be audited is assumed to be endogenous, simple OLS regression of audit on the cost of debt, i.e., Eq. (1), would yield inconsistent results. That is, the audit indicator (A_i) would be correlated with the error term (ε_i), because of the correlation between the error term in the selection equation and second-stage equation. Including the IMR correction term as an additional variable in the second-stage regression controls for such correlation and produces consistent estimates of the audit dummy coefficient.[6] Following extant literature (Chaney et al., 2004; Tucker, 2010; Lennox et al., 2012) we calculate IMR separately for treated (audited) and untreated (unaudited) observations as:

$$IMR_i = \begin{cases} \varphi\left(Z_i\widehat{\beta^*}\right)/\phi\left(Z_i\widehat{\beta^*}\right) & (\text{if } A_i = 1) \\ -\varphi\left(Z_i\widehat{\beta^*}\right)/\left(1-\phi\left(Z_i\widehat{\beta^*}\right)\right) & (\text{if } A_i = 0) \end{cases} \tag{5}$$

[6] Estimation using IMR is subject to the following conditions: first stage regression has to be modelled as a probit, second stage regression has to be modelled as a linear regression and the unobservable terms in the two stages have to follow binomial distribution (Tucker, 2010).

where $\varphi(\cdot)$ is the probability density function of a standard normal distribution, $\phi(\cdot)$ is the cumulative density function of a standard normal distribution, and $Z_i\widehat{\beta^*}$ is the fitted value from the probit model of Eq. (4). Our final second-stage model therefore includes the computed IMR to correct for the self-selection, where its potential statistical significance is an indication of the presence of self-selection:

$$Y_i = \alpha + X_i\beta + A_i\gamma + IMR_i\delta + w_i \tag{6}$$

3.2) Empirical specification of audit decision

To test our hypotheses, we first estimate a probit selection model for voluntary audit decision:

$$V\_AUDIT_{i,t} = \alpha_0 + \alpha_1 JSC_{i,t} + \alpha_2 L\_LP_{i,t} + \alpha_3 L\_NP_{i,t} + \alpha_4 B\_ACC_{i,t} + \alpha_5 ST\_LOAN_{i,t} + \alpha_6 LT\_LOAN_{i,t} + \alpha_7 EXPORT_{i,t} + \alpha_8 EM1_{i,t} + \varepsilon_{i,t} \tag{7}$$

Based on section 2.2, we include variables that affect firms' decision to have an audit (selection), but expect them to have little or no impact on the cost of debt (the outcome).[7] *JSC* identifies firms that are joint stock companies, whereas the great majority of the sample firms are limited liability companies.[8] We expect *JSC* firms to choose an audit more often as their legal status, similarly as in public firms, allows for more complex corporate governance mechanisms to tackle agency conflicts on a larger scale. *L_LP* and *L_NP* (natural logarithm of the number of legal and natural person owners, respectively) control for

[7] Justifiable exclusion of variables in the first-stage equation is recognised as a common problem in selection model application (Lennox et al., 2012).

[8] There are 220 observations of joint stock companies in our sample, and 15,742 are limited liability companies.

ownership complexity.[9] While a natural person is a physical owner of a firm, a legal person is a firm that has an ownership stake in the observed firm. In particular, if *LP* equals at least one we assume that the firm is a subsidiary.[10] In subsidiaries, general accounting and financing policies as well as audit decision are likely to depend on the parent firm, so we control for this. Additionally, existence of a *LP* owner indicates more complex ownership structure and potentially larger agency issues. Similarly, the higher the number of owners (both *NP* and *LP*) the more severe are the associated agency conflicts. As control is more difficult and costly for each individual owner, an audit can provide an effective control mechanism. We expect the positive effect to be stronger for *LP* ownership than *NP* ownership, since the former is usually more detached from day-to-day management issues in small private firms (the inverse case appears in large public firms, where (larger) *LP* owners would be more involved in firms' governance than individual *NP* shareholders).

Next, we include variable *EXPORT* (the ratio of foreign sales to total sales) to control for the effects of supply chain relationships. Small private firms participating in (complex) international supply chains may use audited financial statements to communicate information reliability and commitment to high transparency (Baiman & Rajan, 2002; Costello, 2013). In line with Cheng et al. (2020), we expect a positive relationship between *EXPORT* and voluntary audit.

Finally, we control for bank-related demand for audit. We employ the total number of bank accounts[11] a firm has at the end of the fiscal year (*B_ACC)* as a proxy for the closeness of firm-bank

[9] Because of their skewed discrete distributions, we are using natural logarithms of *NP* (*L_NP*) and *LP* (*L_LP*) throughout the analysis. Main analysis results with non-logarithmic values, i.e., *NP* and *LP*, provide comparable significant results, but perform somewhat poorer in sensitivity tests. Nevertheless, we find it more informative to report descriptive statistics for the non-logarithmic values in Table 3.

[10] We do not have the percentage of ownership, and hence use the term 'subsidiary' for any firm owned by another firm regardless of the percentage of ownership.

[11] We stress that, we do not only see whether a bank name is related to a firm, but also the number of the firm's transaction accounts with each bank. Compared to Bigus & Hillebrand (2017), our *B_ACC* variable is an improved and refined measure of the '*number of bank relationships*'. They use the Amadeus database to determine the '*number of banks financing a firm*'. They observe the names of the banks related to a firm and count them, but not the total number of actual bank accounts reflecting the complexity of the firm's relations with a bank(s).

relationship (Bigus & Hillebrand, 2017) or, alternatively, multiple borrowing options.[12] The set of borrowing options might be associated with a decision for voluntary audit, but its effect is ambiguous. On the one hand, a small number of bank accounts could indicate that a firm does not have many borrowing alternatives and sees the audit as a way to improve its access to external financing. On the other hand, firms with a large number of bank accounts can be those that instantly look for new loan options if existing credit lines shrink, again increasing the probability of audit.[13] We capture a firm's reliance on (short- and long-term) bank financing with its proportion in total assets, *ST_LOAN* and *LT_LOAN*, respectively, as in De Meyere et al. (2018). A higher ratio of bank-loan financing, particularly long-term, may imply that banks are more interested in the borrower's activities as relatively more funds are committed to it, and an audit serves as a verification channel. However, banks also have superior information gathering and processing capabilities (Diamond, 1991; Bharath et al., 2008; Campbell et al., 2019). A bank's private gathering of firm information can offer substitute and potentially better information than the insights from an audit report, especially if the relationship is long lasting and the bank is the only creditor to a firm (a common situation in small private firms). If banks put more emphasis on private information compared to financial statements, we would expect audit to be negatively related to *ST_LOAN* and *LT_LOAN*. Because creditors lending decisions rely on prediction of future cash flows needed for debt repayment, audited financial statements may serve as an additional means of creditors' self-insurance in cases where firms' reported numbers relatively poorly predict future cash flows (i.e., more information risk). We expect that firms with a higher ratio of absolute total accruals to operating cash flows (*EM*1) would more often choose an audit to ameliorate banks' distrust in a firm's financial reporting process and outcomes.

[12]A register of bank accounts was set by the Bank of Slovenia based on the Payment Transactions Act. On July 1st, 2010, the management of the register of bank accounts was taken over by the Agency of the Republic of Slovenia for Public Legal Records and Related Services (AJPES) based on article 10 of the Payment Services and Systems Act.
[13] The number of bank accounts can be affected by a firm's size (*SIZE*) acting as a proxy for the scope and complexity of the firm's operations (Spearman correlation coefficient between *B_ACC* and *SIZE* is 0.225; see Table 4).

The binary selection process modelled in Eq. (7) above may not be the only selection process that underlies the effects of audit on the cost of debt. A more general view is that a firm has a choice with three possible outcomes (no audit, Big-4 audit, non-Big 4 audit) or, alternatively, that there is a second selection process involved – that between a Big-4 and a non-Big-4 auditor. Absent clear guidance from the literature on how to account for multiple options in self-selection models, we opted to model a second binary selection process. Because of the underlying demand for audit, we grouped together observations that have no need for audit and those that choose a Big-4 auditor. Both these groups are more likely to be of similar quality/lower risk. The non-Big-4 is the other distinct group. The first-stage probit and the resulting IMR produce statistically significant effect on the outcome variable over and above the IMR from Eq. (7) in a predictable way – the selection modelled in this way is positively associated with the cost of debt. Because the total effects remain qualitatively the same and because of scant relevance from econometric literature on how to deal with these choices, we omit this second selection model.[14]

3.3) Empirical specification of cost of debt

After estimating the selection model, we calculate the inverse Mills ratios (*IMR*) as stated in Eq. (5) and we include them as an additional explanatory variable in our second-stage model to control for self-selection of firms into voluntary audit. Our second-stage model is presented in Eq. (8). The dependent variable, *IRATE*, is an estimated interest rate on debt for a firm *i* in year *t*+1.[15] It is measured as the sum of bank- and bond-related financial expenses in year *t*+1, divided by the average of short- and long-term financial obligations towards banks (interest bearing debt) in years *t* and *t*+1. This construct is more precise than the interest rate measures employed in prior studies (e.g., Pittman & Fortin, 2004; Karjalainen, 2011; Minnis, 2011), because both the income statement item (interest expense) and the

[14] Results are available from the authors.
[15] Similar to Minnis (2011) we are using the future (*t*+1) interest rate as we consider audit effects to be fully revealed in at least the following year or, conversely, because its time-(*t*)-form is likely to contain stale information.

balance sheet items (debt) are more finely grained: the latter into bank loans, bonds, loans from members of the group, and other.

$$IRATE_{i,t+1} = \alpha_0 + \alpha_1 V\_AUDIT_{i,t} + \alpha_2 BIG\_4_{i,t} + \alpha_3 SIZE_{i,t} + \alpha_4 PROF_{i,t} + \alpha_5 DAYS\_BLOCK_{i,t} + \alpha_6 SALESG_{i,t} + \alpha_7 ICOV_{i,t} + \alpha_8 PPE_{i,t} + \alpha_9 OP\_LEV_{i,t} + \alpha_{10} WC_{i,t} + \alpha_{11} N\_EQ_{i,t} + \alpha_{12} N\_EQ\_BL_{i,t} + \alpha_{13} TAX_{i,t} + \alpha_{14} IMR_{i,t} + \varepsilon_{i,t} \quad (8)$$

Our main test variables indicate whether a firm has a voluntary audit and whether the auditor is a Big-4 firm. *V_AUDIT* is a dummy variable that equals 1 for firms that have been audited, and 0 otherwise.[16] *BIG_4* is a dummy variable that equals 1 for firms that have been audited by a Big-4 auditor, and 0 otherwise. The *BIG_4* variable is effectively a multiplicative dummy with the *V_AUDIT* variable (i.e., the *BIG_4* can only equal 1 if *V_AUDIT* equals 1).

We include a set of controls identified by existing research (e.g., Blackwell et al., 1998; Mansi et al., 2004; Pittman & Fortin, 2004; Karjalainen, 2011; Minnis, 2011) as determinants of the cost of debt. The effect of *SIZE* (natural logarithm of total assets) is predicted to be negative as larger firms are found to be less risky, due to their higher visibility and reputation. Profitability (*PROF*) is measured as the sum of EBIT and asset write-offs to total assets. Firms with higher *PROF* are more capable to service their debt, so we expect a negative effect on the interest rate. We expand the set of control variables reported in the literature with a measure that reflects a particular aspect of firm failure: if a firm has its transaction (bank) account(s) blocked by the tax authorities due to suspected insufficient funds to cover its potential tax obligations (Mramor & Valentincic, 2003). The dummy variable *DAYS_BLOCK* equals 1 if a firm has its

[16] Although all audits in our main sample are voluntary, we specifically denote it as such so to be able to compare it to mandatory audit in the supplementary analysis.

transaction (bank) account(s) blocked by the tax authorities for at least one day.[17] We expect firms with blocked transaction account(s) to be penalized with higher cost of debt. To control for predicted negative effect of firms' growth options, we use *SALESG* (annual change in sales to lagged sales). Interest coverage (*ICOV*, sum of bank- and bond-related financial expenses to EBIDTA) controls for a firm's capability of repaying its debt obligations and is expected to positively affect the cost of debt. *PPE* (property, plant and equipment to total assets) represents possible collateral in debt contracts, thus reducing riskiness of the debt; we expect it to be negatively related to the cost of debt. Higher operating leverage (*OP_LEV*) increases the probability of financial distress and should increase the cost of debt. However, the existence of trade debtors may also replace financial debt, particularly for financially constrained firms or during periods when bank loans may be scarce, such as during financial crises (e.g., Nam & Uchida, 2019). Also note that the level (proportion) of financial debt has already been used as the decision variable, and hence it cannot appear again in the outcome – the cost of debt – regression. As higher working capital (*WC*, difference between current assets and current liabilities to total assets) is indicative of higher liquidity and lower risk, we expect it to have a negative effect on the cost of debt. *N_EQ* is a dummy variable that equals 1 if a firm reports negative book value of equity, and 0 otherwise. It indicates riskier firms in financial distress, which are expected to incur higher cost of debt. Another dummy variable, *N_EQ_BL*, indicates firms with a negative book value of equity that have existing bank loans as these may be treated differently by the banks. In this situation banks are also more likely to take an active role in restructuring the firm (e.g., banks may appoint a representative in the management, extend the loan maturity and/or reduce its interest rate). In addition, the *TAX* variable indicates firms with zero income tax and zero deferred tax. It captures tax avoidance, which creditors view negatively because of its relation to higher uncertainty of firms' future cash flows and decreased financial reporting transparency (Shevlin et al., 2019). Finally, we

[17] Bank account blockages are defined under the Claim Enforcement and Security Act published in the Official Gazette of Republic of Slovenia no. 3/07, 93/07, 37/08. This data was provided to us by Bisnode d.o.o., a private business information provider. Source: https://www.bisnode.si/globalassets/slovenia/produkti/faq_slo_si.pdf .

include industry- and year-fixed effects to control for correlated omitted variables that are unobservable, but firm-specific and time-invariant. Detailed definitions of all variables are presented in Table 1.

[INSERT Table 1 HERE]

3.4) Supplementary analyses

We perform several supplementary analyses using subgroups of our main sample, its extensions, and different cost of debt (interest rate measure) definitions. We first follow Minnis (2011) to estimate how informative current-period earnings are for future cash flows and then, as in Kosi & Valentincic (2013), we look at two earnings management proxies, for both audited vs. unaudited observations, and Big-4-audited vs. non-Big4-audited subsamples. If audited financial statements are a signal of higher reporting transparency they should better predict future cash flows than unaudited financial statements, and thus be more useful for assessing a firm's ability to repay its debt (Minnis, 2011; Kosi & Valentincic, 2013). To do so, we estimate the following equation:[18]

$$CFO_{i,t+1} = \alpha_0 + \alpha_1 NI_{i,t} + \varepsilon_i \quad (9)$$

where *CFO* is operating cash flow for a firm *i* in year *t*+1, and *NI* is net income for a firm *i* in year *t*. If earnings informativeness for future cash flows is greater in one subsample compared to another, we should observe a bigger α_1 coefficient in that group. Similarly, if net income is decomposed into operating cash flow (*CFO*) and accruals (*ACC*), we should observe higher α_2 and α_3 coefficients from Eq. 10 (Valentincic et al., 2017):

$$CFO_{i,t+1} = \alpha_0 + \alpha_2 CFO_{i,t} + \alpha_3 ACC_{i,t} + \varepsilon_i \quad (10)$$

[18] We include industry- and year-fixed effects in both equations (9) and (10). We additionally estimate them with the inclusion of IMR as a selection bias correcting term and the results remain unchanged.

Next, we calculate two measures designed to capture the extent of earnings management. *EM*1 is the ratio of absolute total accruals to operating cash flows: $|ACC_{i,t}|/CFO_{i,t}$, and *EM*2 is the Spearman correlation between total accruals and operating cash flows, multiplied by (-1): $-\rho(CFO_{i,t}, ACC_{i,t})$. Higher values of *EM*1, the median for each group, are indicative of higher values of total accruals compared to operating cash flows, and thus higher earnings management and lower earnings quality. Similarly, higher values of *EM*2 indicate more earnings smoothing, and thus lower earnings quality as total accruals and operating cash flows move more closely together (in opposite directions).

4) Sample selection and description

We obtain data from the Agency of the Republic of Slovenia for Public Legal Records and Related Services (henceforth AJPES). All firms, regardless of size, operating in Slovenia are required by law to submit their financial statements to AJPES and this data is made available for research purposes. The basic financial statements under the Slovenian Accounting Standards are separate (legal-entity level) financial statements, not consolidated (group) statements. Separate statements also represent the basis for tax reports. Our analysis includes small private firms operating from year 2006 to year 2017. During these years, the Slovenian Accounting Standards can be described as 'IFRS-like' (Valentincic et al., 2017; Novak & Valentincic, 2017). We choose the year 2006 as the starting point because of the change in accounting standards that came into effect in 2006, making observations from prior years less comparable.[19] Our data ends in 2017 as this is the last year of full data availability for all the variables in use (and we require the year 2018 for the *t*+1 analyses).

[19] More precisely, some balance sheet and income statement items are reported in more detail since 2006 and others have been reclassified. Many detailed items are from the financial obligations section and directly concern our dependent variable calculation making longer time series incomparable. As firms had to additionally report their 2005 statements 'meaningfully' resembling the new standards in 2006, we can use that data for calculations requiring annual changes and thus keep 2006 as our starting year despite longer data requirements. For time-series perspective of the development of Slovenian accounting standards, see Valentincic et al. (2017) and Novak & Valentincic (2017).

From the population of Slovenian firms in the sample period we first exclude publicly listed firms and then distribute the remaining private firms into four size groups: large, medium, small and micro firms. The criteria that denote a particular size of the firm is defined by the Slovenian Companies Act (2006, and subsequent amendments as described in Footnote 20) in terms of the number of employees, net sales and total assets. It follows the quantitative criteria set by the EU regulations (Fourth Council Directive 78/660/EEC and its amendments up to Directive 2006/46/EC). Size groups are especially important for our analysis, as the Companies Act further defines that (next to all publicly listed firms) large and medium private firms must have their annual financial statements audited, whereas this is optional for the small and micro firms.[20] However, we exclude micro firms from our analysis as they are very unlikely to have an audit. We therefore use small private firms with a voluntary choice to have an audit as our initial sample. The financial data of these firms is complemented with data on bank accounts (provided by AJPES) as well as ownership and audit data (both provided separately by Bisnode, d.o.o.).

[INSERT Table 2 HERE]

The sample selection process is presented in Table 2. We first exclude observations with fiscal year shorter than 12 months and those with changes in their legal status as they are not representative of a normal small private firm. We then remove firms from financial sector and utility services and firms with any missing data. Lastly, we exclude observations with variable values below the 1st percentile and/or

[20] The criteria that denote firm size are defined in article 55 of the Companies Act (2006), whereas article 57 defines the auditing requirements. In particular, private firms are not required to undergo mandatory audit if they fulfil at least two of the following three criteria: average number of employees must not exceed 50, sales revenues must not exceed €7,300,000 and total assets must not exceed €3,650,000. The latter two conditions were amended to €8,800,000 and €4,400,000 respectively by the amendment of the Companies Act in 2008 following the European Directive 2006/46/EC. For the third time the latter two conditions were amended by the amendment of the Companies Act in 2015 to €8,000,000 and €4,000,000 respectively. Consolidated fact-sheet is available on: https://www.accountancyeurope.eu/wp-content/uploads/1605_Audit_exemption_thresholds_update.pdf.

above 99th percentile.[21] We end up with 17,742 firm-year observations for small private firms. Out of these, 1,780 observations have a calculated interest rate of zero, meaning that they did not report any bank- and/or bond-related financial expenses during the year. The remaining 15,962 observations (4,058 distinct firms) have a positive value of *IRATE*. We use the latter sample for our main analysis, as we are primarily interested in the relation between voluntary audit and the interest rate.

[INSERT Table 3 HERE]

Table 3 presents descriptive statistics of our sample. Panels A and B report them for audited and unaudited observations, respectively. Out of 15,962 firm-years 5.0% (800) are audited. There is no statistically significant bivariate difference between the interest rate of the two groups.[22] For the variables in the voluntary audit decision model, we see that more audited firms are registered as joint stock companies, they are, on average, owned by a larger number of *LP* and *NP* owners. On average, audited firms have significantly less export and long-term bank loans but more bank accounts and short-term loans. For the determinants of cost of debt, we find that, on average, audited firms are significantly larger but less profitable. These firms have their transaction account(s) blocked more often and more often report negative equity. In addition, they have slower sales growth, less operating leverage and working capital but more often report no tax. As discussed above, more dispersed ownership connected with joint stock legal status and bigger size is in line with audited firms more likely being subject to agency problems and more complex corporate governance structures. More bank accounts and less long-term bank loans is in line with use of voluntary audit to secure multiple borrowing options. Contrary to our expectations,

[21] Unbounded continuous variables (*SIZE*, *PROF, SALESG*, *ICOV* and *WC*) have outliers removed at the 1st and 99th percentile, while continuous variables with a natural border (*IRATE*, *EXPORT, ST_LOAN, LT_LOAN, PPE* and *OP_LEV*) have outliers removed only on one side, at the unlimited (upper) side.

[22] The average calculated interest rate of 4.8% and 4.5% for the groups respectively, is somewhat smaller than in U.S. studies (Allee & Yohn, 2009; Minnis, 2011), but larger than in Finland (Karjalainen, 2011).

EXPORT is lower for audited firms and *EM*1 is not significantly different. Panel C describes our observations with selected financial statement numbers presented in monetary terms (EUR).

Correlation coefficients presented in Table 4 do not indicate severe multicollinearity problems between the regressors. Among the main test variables, the highest correlation (0.46) is between *BIG_4* and *V_AUDIT* dummies which are correlated by construction. The remaining correlations between the variables are all smaller (in absolute terms), with the exception of the Spearman coefficient between *ST_LOAN* and *ICOV* (0.46) but the two variables are not used simultaneously as the former is used in the first step and the latter in the second step of Heckman estimation.

[INSERT Table 4 HERE]

5) Results

5.1) Main results for determinants of cost of debt

We begin our analysis by estimating a probit selection model for voluntary audit decision and present the results in Table 5. Next, we use predictions from the model to calculate IMR. Joint stock companies (*JSC*) more often choose an audit than their limited liability counterparts as the legal status of the former is suitable for more complex corporate managing. In line with the agency theory (e.g., Jensen & Meckling, 1976), we find that the probability of an audit increases with *LP* ownership (also our proxy for subsidiary status). An increase in *L_LP* means more dispersed ownership, hence the resulting agency frictions increase the probability to choose an audit. In contrast, natural person ownership (*L_NP*), *EXPORT, B_ACC* and *ST_LOAN* variables are not significantly associated with the audit decision. *LT_LOAN*, which measures the exposure to long-term bank loans, is significantly negatively associated with the audit decision. This is consistent with Diamond (1991) and Bharath et al. (2008) who conclude that a bank's direct involvement in gathering firm information can offer substitute and potentially better information

than an audit report. Finally, significantly positive coefficient on *EM*1 implies that firms with greater disparity between accruals and cash flows have higher probability for an audit. These firms are likely to represent higher information risk for creditors, which are in turn more likely to demand an audit.

[INSERT Table 5 HERE]

Our main results are shown in Table 6, which presents two OLS models and compares them to the Heckman estimation.[23] Propensity score matching approach and two-stage least squares estimation are explained in section 5.2. The OLS extended model (1) incorporates all the variables from the first and second stage together, while the OLS base model (2) only includes variables used in the second stage. The estimation of Heckman 2nd stage model (3) includes the suggested determinants of cost of debt and the *IMR* calculated form the 1st stage probit estimation to correct for the self-selection bias into voluntary audit. Moreover, it accounts for any other unobservable factors affecting the audit decision and the *IRATE*. Robust standard errors clustered on firm level are used to mitigate serial correlation and heteroscedasticity concerns in our setting (Petersen, 2009).[24]

As a central finding, the coefficient on *V_AUDIT* is positive and statistically significant in all three models, even gaining in magnitude and significance in the Heckman 2nd stage model (0.030). We interpret this result as a strong evidence that voluntary audits performed by a non-Big-4 auditor, after controlling for other factors, are on average, associated with *higher* private firms' cost of debt, rather than the conventional finding of lower cost of debt. We view this finding as a reflection of borrowers' higher risk. In particular, a bank requires audited financial statements as a means of additional self-insurance in cases

[23] For all the following analyses we only report the Heckman 2nd stage model although we also estimate the two OLS models, hereafter not tabulated. Results are available from the authors.

[24]Literature (Gow et al., 2010; Thompson, 2011) suggests clustering on firm and time, but simulations suggest that this works well only with at least 25 observations in both dimensions making it more applicable to monthly or daily data used in finance research. Hence, following Petersen (2009), we include time dummies and cluster on firm level.

where earnings quality is low (reported numbers poorly predict future operating cash flows). In this way, a firm with higher information risk is able to gain access to bank financing, but gets higher interest rate than unaudited firms.

If a small private firm is audited by a Big-4 auditor, this has a negative and statistically significant incremental effect on the interest rate compared to non-Big-4 auditees. It is worth noting that the coefficient on *BIG_4* in the two OLS specifications outweighs the positive effect of *V_AUDIT* and decreases the overall cost of debt for Big-4 auditees. This finding is consistent with most prior research. However, this is not the case in the Heckman 2nd stage model. The crucial difference is that OLS does not correctly control for self-selection into voluntary audits, while Heckman's procedure does. The total effect of Big-4 audits (*V_AUDIT* + *BIG_4*) on the cost of debt is significantly positive (0.022). This means that even Big-4 auditees still pay higher cost of debt compared to unaudited firms and implies that also Big-4 audits reflect higher risk and consequently result in higher cost of debt. The analyses in the sensitivity sections only focus on our main model (Heckman 2nd stage), as this is the correct specification given the details of the selection and outcome processes.

[INSERT Table 6 HERE]

The results for the control variables are as follows. In line with prior studies and our expectations, *SIZE* and *PROF* are inversely related to the interest rate (-0.005 and -0.016, respectively). This confirms that bigger and more profitable firms can get more favorable cost of debt. As expected, the positive coefficient on *DAYS_BLOCK* (0.009) suggests that firms with blocked transaction account(s) are penalized with a higher cost of debt. The significantly positive coefficient on *ICOV* (0.012) is as expected (note that the ratio is financial expense to EBITDA). The coefficient on *PPE* is significantly negative (-0.007) as more assets could be used as collateral which lowers the interest rate. *OP_LEV* is positively related to cost of

debt (0.019) as it indicates higher exposure to total obligations due to higher operating obligations (on top of financial obligations, resulting in higher total leverage), and *WC* displays a negative coefficient (-0.005) as it signals higher liquidity. The statistically significant coefficient on *N_EQ* (0.070) points to the expected positive direction – firms with negative equity face financial distress costs and this is reflected in the higher interest rate. The coefficient on *N_EQ_BL* (-0.074) appears puzzling, but it can be explained as the firms characterized by this dummy are probably facing adverse financial conditions and are having debts reprogrammed by their banks. This process would often include a reduction of the interest rate on existing debts (and greater control by the bank(s)).[25] The proxy for tax avoidance (*TAX*) is positive as expected (0.002): firms which report small earnings are more likely to use the financial reporting process to minimize the present value of taxes paid, and are thus of higher information risk. Lastly, the coefficient on *IMR* is statistically significant and negative (-0.012) implying that the error terms in the selection and the second-step equations are negatively correlated and that the self-selection correction is required.

5.2) Selection bias and mitigation of econometric consequences

We base our main results on Heckman's two-stage approach which assumes that there are other, potentially important factors over and above those that we identify and collect in this study and that affect the audit decision and the cost of debt. There are, in principle, three possible approaches to address selection bias: Heckman's two-stage procedure, propensity score matching, and instrumental variable approach – although they are not direct substitutes as they address different grounds for selection bias (Tucker, 2010). Existing literature also warns about the sensitivity of the Heckman model to sample

[25] Unreported result shows that due to their construction, the *N_EQ* and *N_EQ_BL* dummies are highly correlated which results in their high variance inflation factors (VIFs) compared to the next VIF for the *WC* (VIFs for other variables are lower with the exception of *IMR* and *V_AUDIT*, the treatment indicator variable, which is, according to Tucker (2010), not a weakness of the selection model but a consequence of *IMR*'s construction). Not including both simultaneously would results in loss of explanatory properties as the included variable possibly incorporates the effect of both, offering a less in-depth view of interest rate dynamics for the concerned firms.

composition and model specification issues that can critically affect the findings (Stolzenberg & Relles, 1997 [26]; Clatworthy et al. 2009; Lennox et al., 2012). To address these concerns, we study the cost of debt using the two alternative approaches and implicitly accepting that their underlying assumptions about the selection process are correct.

First, we employ the propensity score matching (PSM) approach to estimating causal treatment effects stemming from the work of Rosenbaum & Rubin (1983). Using a module in Stata authored by Leuven & Sianesi (2003), we match observations on all the covariates in our set (case 1) and then once more, excluding selection variables used in the 1st stage Heckman approach (case 2). Probit-based propensity scores represent the probability of 'treatment' (in our case a firm's decision to have a voluntary audit) given the observed characteristics. Then, for each treated unit, we find an untreated observation that is most similar to it according to the covariates, i.e., the propensity score (Wooldridge, 2010). Matching is performed on nearest neighbor without replacement on common support (within the minimum and maximum of control firms' propensity scores) as we want to achieve a balanced sample consisting of half audited and half unaudited observations. Under the key assumption that differs from our Heckman analysis, namely, that selection bias can be explained only by the observables defined as in case 1 or 2, we inspect the average treatment effect on the treated group (ATT) in terms of *IRATE* as well as estimate OLS regressions (with industry- and year-fixed effects and clustered robust standards errors) on the matched sample analogous to estimation of OLS extended and OLS base model in Table 6. At the same time, we check the goodness of match variable-wise with a *t*-test of equality of means both before and after matching.[27] To improve the match (i.e., to avoid 'bad' matches of observations with substantially

[26] Using simulations, these authors even find that Heckman's two-stage estimator can worsen estimates unless selection bias is severe and samples are big enough although sample selection is known to exist and none of the model's assumptions are violated.

[27] As this is not our central analysis, the results are not tabulated for brevity but are available from the authors.

different propensity scores), we employ calipers [28] (Caliendo & Kopeinig, 2008). This does result in a minor loss of observations as we require the matches to be in a pre-specified range but it substantially improves the quality of matching on the other hand.

The estimation of case 2 is presented in column (4) of Table 6. The results are aligned with our main results in column (3), although a few control variables lose statistical significance due to a substantial reduction in sample size. The coefficient on *V_AUDIT* is still significantly positive (0.011). This again indicates that voluntary audit is not associated with a lower cost of debt of small private firms, but rather with an increased cost of debt. The cost of debt incrementally decreases for Big-4 auditees (-0.006) but the total effect remains positive. As expected, the cost of debt decreases with *SIZE* and *PPE*. The cost of debt increases if a firm has its transaction account(s) blocked, has higher operating leverage, and reports negative equity. To sum up, the propensity score matching analysis corroborates our main findings, although we caution that the PSM approach assumes selection on observables only.

In our second alternative approach we employ the two-stage least square (2SLS) instrumental variable approach following Huq et al. (2018). The instrumental variable is constructed using observations for the year 2006 – i.e., before the Companies Acts was enforced. Since the firms are perceived to be unaware of the Companies Acts and its requirements at the time we measured the instrumental variable, there is no reason to believe that it is correlated with the error term of the regression. Hence, although not ideal, we have an instrumental variable that is assumed not to be affected by self-selection into treatment. More illustratively, we instrument the endogenous variable *V_AUDIT* using a variable which for navigating purposes we call '*iv_audit*'. The indicator variable equals 1 for the firms being audited based on information from year 2006, and 0 otherwise. Results in Table 6 (column 5) show significantly positive

[28] We use various calipers (1, 0.01, 0.005, 0.001, 0.0005, and 0.0001) however, we report results for caliper equal to 1. Decreasing the caliper (to 0.01, 0.005, 0.001 etc.) provides even more significant results, accompanied by an increasing number of observations, however these observations fall outside the set range, thus reducing our matched sample size.

coefficient on *V_AUDIT* (0.027). This again indicates that voluntary audit increases the cost of debt of small private firms. The cost of debt significantly incrementally decreases for Big-4 auditees (-0.024), but the total overall effect still indicates higher cost of debt for audited firms relative to unaudited counterparts. The control variables show robust inference in line with our expectations.

5.3) Validation analyses

Table 7 displays results of earnings' informativeness and earnings management analyses for audited vs. unaudited observations, and Big-4 audited vs. non-Big-4 audited observations. These analyses serve as validation tests for the information risk carried by small private firms. We expect firms with higher information risk to face greater constraints to external financing and consequently pay higher cost of debt. The ability of current net income to forecast future operating cash flow (earnings quality) is a particularly important indicator of information risk for creditors.

Panel A of Table 7 shows higher, both in magnitude and statistical significance, forecast ability of current net income for future operating cash flow (α_1) for unaudited firms than for audited firms. Among the latter, forecast relevance is higher for Big-4 auditees, although earnings quality for both audit groups is lower than in the unaudited group. This supports our reasoning that creditors are more likely to require audited financial statements from firms with higher information risk as a means of self-insurance against poor predictability of future cash flows for debt repayment. In addition, Big-4 auditors seem to be able to choose less risky firms, while most risky firms are left to non-Big-4 auditors which have relatively low power against the clients. These findings remain qualitatively equal even when current net income is split into the operating cash flows and accruals components and regressed on future operating cash flow. The coefficients are larger and statistically significant for unaudited compared to audited firms, and for Big-4 auditees compared to non-Big-4 auditees.

[INSERT Table 7 HERE]

Panel B presents two earnings management measures but with mixed results. On the one hand, lower values of *EM*1 (absolute total accruals to operating cash flows) for unaudited firms suggests that these firms are associated with weaker earnings management and hence higher earnings quality. On the other hand, the contemporaneous correlation between accruals and operating cash flows (*EM*2) is higher for unaudited firms associating them with more earnings management compared to audited firms. While this finding is anomalous, we note that it might be at least in part due to changing role of accruals and operating cash flows during the financial crisis (Valentincic et al., 2017) despite year controls. Alternatively, direct shareholders' loans may reduce the need for earnings smoothing (Bigus & Hafele, 2018). Unfortunately, the data on these loans does not exist in our empirical setting. Nevertheless, within the audit subsample, higher values of both measures (stronger earnings management) are found for non-Big-4 auditees compared to Big-4 auditees.

6) Additional tests

6.1) Sensitivity analyses of the main results

We perform a number of additional analyses and sensitivity tests. First, we consider the potential effect of the financial crisis starting in 2008, during and after which banks have become more cautious (imposing tighter lending terms) in lending decisions as the operating and financial conditions under which firms operate have worsened considerably. In financial crises, audits might be more valuable as external financing is more constrained, and hence voluntary audits might be viewed as an element of competition among private firms for scarce bank financing. Because banks are more cautious, it is less likely that firms' audited financial statements would be a sufficient self-insurance for the banks resulting in more audits, but equal interest rate all other things equal. Because of the increased uncertainty in the environment,

they are also more likely to charge a higher interest rate. A positive effect of *V_AUDIT* would thus be stronger in crisis period compared to the earlier years.

We split our sample into pre-2008 and 2008-and-post subsamples, and estimate the main model again. Columns (1) and (2) in Table 8 support our expectations. The results are robust, with minor quantitative changes in effects of some control variables, indicating a shift of focus related to lending decisions, but our main results are corroborated for the crisis period (2). *V_AUDIT* is significantly positive (0.032), *BIG_4* is negative (-0.006), and *IMR* is also negative (-0.013) indicating the need to control for selection bias.

Second, we address concerns that some of the small firms in our sample are subsidiaries and hence their audit decision only appears as 'voluntary', but it is in fact required by their parent firm. While we already include potential effects of this situation in our main results, we explore the issue in more detail. Since we cannot identify subsidiaries directly we use a proxy and split the sample at zero *LP* ownership. About half of our observations are owned by *NP* only (no-LP group) and are not subsidiaries in any sense of the word, and the other half has at least one *LP* owner (LP group). Some of the latter firms are subsidiaries, but not all (e.g., a firm may have two *LP* co-owners, each with a 2%-investment). We nevertheless assume that this last group are subsidiaries of other firms. The latent construct 'subsidiary' is thus measured with an error and requires more attention (following from Bar-Yosef et al., 2019).

Estimation of the main model for both subsamples is presented in columns (3) and (4) of Table 8. While there is a large positive effect of *V_AUDIT* for no-LP group, the effect persists in the subsidiary group as well. However, the coefficient on *V_AUDIT* is almost 1.5-times as high for the no-LP group, suggesting that the stand-alone status of these firms brings higher risks and is then reflected in the higher interest rate charged to this group of firms. Interestingly, the negative effect of *BIG_4* on *IRATE* is insignificant for no-LP firms, suggesting that neither type of audit is beneficial for their cost of debt. Effects of some control variables also differ among the groups. Specifically *N_EQ* and *N_EQ_BL* are important for the cost of debt

of no-LP firms – in line with any potential restructuring issues being more acute for stand-alone firms than for firms that are members of a group where any restructuring issues are more likely to be overseen and supported by the parent firm. Yet again, *IMR* is significantly negative indicating the need to control for selection bias.

[INSERT Table 8 HERE]

Third, we include Altman Z-score as an additional control variable in column (5). We expect the Z-score to at least partially identify firms in financial distress, and hence with higher risk, and isolate the effect that financial distress has on the cost of debt from other explanatory variables. The Z-score formulas (Altman, 1968; Altman et al., 1977, and subsequent modifications) use a combination of financial ratios to arrive at a score illustrative of a firm's financial health. Regardless of the specification, a higher score represents a sounder financial position and a lower score indicates a higher likelihood of bankruptcy in the near future. We use the following specification applicable to non-listed firms (Altman, 2000):[29] $Z = 0.717*WC + 0.847*RE + 3.107*EBIT + 0.420*BV + 0.998*SALES$, where *WC* is working capital scaled by total assets, *RE* is retained earnings scaled by total assets, *EBIT* is earnings before interest and taxes scaled by total assets, *BV* is book value of equity scaled by total liabilities, and *SALES* is sales scaled by total assets. The estimated coefficient of the *Z-SCORE* is positively related to *IRATE* (although of negligible magnitude) but statistically insignificant, hence its inclusion does not change the inferences of the main model. The rest of the controls show robust inference in line with our previous results.

Fourth, we consider initiations and discontinuations of audits, and switches of auditors (e.g., Schwartz & Menon, 1985). We analyze whether there is an effect on *IRATE* if: i) a firm ceases to be audited,

[29] The model for private firms is not identical to the model for public firms. It is re-estimated after assuming that market value of equity equals book value of equity (i.e., that the market-to-book ratio equals one).

ii) if it is audited for the first time, or iii) if it changes its auditor. Firms having an audit for the first time could see their cost of debt decrease if audit increases financial reporting transparency. Alternatively, cost of debt might reflect higher information and/or fundamental risk and hence a higher interest rate as documented in our main findings. Reversing this argument, firms ceasing audits of their financial statements because they become higher quality/lower risk are likely to experience lower cost of debt. A decision to change an auditor could be driven by borrower's risk because Big-4 auditors could decline a risky client, and more risk is related to higher cost of debt. In our sample there are very few such incidences: 146 observations switched from audit to no audit, 141 observations switched from no audit to audit, and 57 switched their auditor. We present the results in Figure 3. The nature of the analyses is rather descriptive although we formally test for the differences in relative interest rates and earnings quality measures across adjacent time periods.

For each group (discontinuators, initiators, and switchers) we aggregate the interest rate in technical time: the benchmark for the event of interest is in time *t*. The decision regarding the audit status must have been made during *t*+1, but the reference financial statements and the reference interest rate is still for time *t* (middle column) – i.e., a firm observes that it is of different risk/quality based on financial statements for time *t*, but as these are revealed only some time after the financial year end. The effect of the decision on the cost of debt is presented in the RHS column denoted as time *t*+1. For example, if a firm's financial statements in 2010 are audited, but are not audited in 2009, this observation is an initiator, *t* = 0 for 2009 and the differential effect is from *t* (2009) to *t*+1 (2010). To give a sense of time-series changes and serving as a control for the differential effect of *t*+1, the LHS column also shows the cost of debt in the year preceding the decision, *t*-1. Because technical time aggregates interest rates from periods of varying general economic conditions, we control for this by dividing the *IRATE* by the average interest rate for corporate loans in a specific year as reported by the central bank (the Bank of Slovenia, Eurozone).

The columns are thus standardized and comparable. The broken lines in each graph summarize the *EM*1 measure for each group, lower *EM*1 indicate higher earnings quality.

[INSERT FIGURE 3 HERE]

Fig.3a reveals that for firms discontinuing audits the relative interest rate statistically significantly decreases, while earnings quality increases (earnings management *EM*1 decreases). Both these changes are expected: if a firm discontinues audits it is likely that it infers, by comparing across time and across firms, that it is of lower risk/higher quality. The status is revealed by discontinuing the audits. Fig.3b shows the opposite. Firms that are of higher risk/lower quality as evidenced by financial statements at time *t*, have their financial statements for *t*+1 audited. Consistent with their status, the average relative interest rate for *t*+1 is higher, but most of the increase happens already from *t*-1 to *t*. However, earnings quality is statistically significantly lower (*EM*1 higher) in *t*+1, as expected. Finally, and perhaps most revealing of the underlying processes, Fig.3c shows the results for switchers. These are arguably firms with the strongest incentive to be audited – they are not of high quality enough to be unaudited and are, in one aspect or another, not satisfied with the audit process. The switch is associated with a statistically significant decrease in earnings quality (higher *EM*1) and an increase in the average relative interest rate, although most of the increase is already from *t*-1 to *t*. To sum up, these analyses corroborate our main results in a descriptive, but informative way.

Finally, we include a measure of firm's operational risk measured as standard deviation of deflated operating income (σ_{PROF}) over the last three years, as is standard in the literature (see e.g., Österholm, 2010). This causes some sample attrition, so we also compute the standard deviation across the last two years. Untabulated results show changed level of statistical significance for the main variables of interest

(*V_AUDIT*, *BIG_4*), but the results are qualitatively unchanged. We infer that operational risk, incrementally to the effects captured by other variables, is not the main cause of the results we observe.[30]

6.2) Inclusion of private firms that are subject to mandatory audits

We expand the sample of small private firms with voluntary audit to medium and large private firms above the mandatory audit threshold (denoted as mandatory firms). We first analyze the combined sample and then limit the sample to mandatory audit firms only. The results Table 9 (1) show significantly positive coefficient of *V_AUDIT* (0.023). Similarly to our previous observations, *BIG_4* appears to yield incrementally negative and statistically significant effect upon the cost of debt (Karjalainen, 2011). The new variable *M_AUDIT*, which indicates observations of mandatory firms,[31] is significantly negative (-0.001) in the combined sample. In contrast, we find that Big-4 auditor is negatively associated to the cost of debt when audits falls in the mandatory category (column (2)), but showing no statistical significance (*BIG_4_M_A* coefficient of -0.000). The results regarding the effects of control variables mainly remain unchanged direction- and statistical significance-wise.

6.3) Sensitivity analyses with alternative cost of debt measures

In the next set of analyses, we test alternative definitions of the cost of debt, though still constructed from financial statements. We present two alternative definitions in columns (3) and (4) of Table 9 and compare the results with prior literature (e.g., Pittman & Fortin, 2004; Karjalainen, 2011; Kim et al., 2011; Minnis, 2011). They are wider in the numerator and/or denominator compared to our primary

[30] We stress that several proxies for risk have already been included in the model. Adding additional aspects of the same underlying construct is thus unlikely to bring incremental information.

[31] When combining our private firm database with audit data some observations in the mandatory-audit-threshold group (medium and large private firms) were left unmatched. We excluded those observations although they had all the necessary data to compute our variables of interest except the one for audit. While we could code them as *M_AUDIT* based on legal threshold definition, we could not identify whether they were audited by a Big-4 auditor or not and therefore decided to eliminate them from the sample in question.

measure. *IRATE_*2 is defined as a ratio of financial expenses from financial obligations in year *t+*1 to the average of short- and long-term financial obligations in year *t* and *t+*1 (i.e., including financial sources other than banks, loans from other firms, parent firm). *IRATE_*3 is calculated as the sum of bank-, bond- and leasing-related financial expenses in year *t+*1, dividend by the average of short- and long-term financial obligations towards banks in year *t* and *t+*1 (the number of observations per analysis varies due to different data inputs required). The coefficient estimate of *V_AUDIT* in (3) and (4) is statistically significant and of similar magnitude than in the main model, suggesting rather robust results. The incremental coefficients on *BIG_4* are negative but statistically insignificant. Control variables mostly follow the effects presented in the main model.

[INSERT Table 9 HERE]

6.4) Data from individual loan contracts

In the sensitivity analyses above, we point out that the interest rate variable can be computed in various ways. However, in each case the resulting variable is still an estimate of the true cost of debt construct. The most obvious source for the difference between the theoretical and the empirical construct is the loan period and the corresponding annualization: a firm enters a loan contract at any point during the fiscal year and may exit the contract at any point during the year. The corresponding interest expense in the income statement may thus understate the 'true' interest expense as would be reported had the loan been outstanding for the entire fiscal year. The corresponding balance sheet amount reflecting the outstanding financial debt balance may also be affected for the same reason.

We attempt to address this problem in the following manner. We requested the Slovenian central bank (Bank of Slovenia, Eurozone) to calculate a value-weighted interest rate from individual loan contracts collected on a monthly basis made to each sample firm in each sample year. The central bank's

general procedure to calculate and annualize interest rates is as follows: '*the monthly base interest rate for a certain month is calculated to one decimal place as the arithmetic mean of the last twelve monthly rates of growth of consumer prices. February's monthly base interest rate equals to January's. The annual base interest rate for a certain month is calculated from the monthly base interest rate to two decimal places in a similar way taking into account the actual number of days*'.[32] A similar calculation was used to aggregate the individual loan-month data to a yearly basis.

Due to strict data anonymity rules, we had to supply our sample to the central bank, which ran our main models and communicated the statistical output to us. The central bank matched 10,256 observations (out of 15,962) with the average interest rate of 5.014% (s.d. = 1.831%).[33] We conjecture this is due to some firms taking on financial loans from sources other than banks (e.g., from other non-financial firms). These loans would not be reported to the central bank, but would be captured in our financial reporting-based sample data. While these transactions may be completely economically legitimate, it is conceivable that at least part of these loan arrangements belong to the 'grey' market (i.e., are taken by firms who would otherwise not be able to obtain a loan under normal circumstances at a bank).

This possibility leads to at least the following four important consequences: i) unmatched firms are more likely firms with highest constraints to external financing; ii) the resulting measure of cost of debt – while sourced directly from individual loan contracts is subject to sub-selection bias: economically 'normal' firms obtaining their loans at banks will more likely appear in the dataset; iii) the direction of bias is at least party predictable: missing (unobservable) firms are more likely paying higher interest rates than firms that are matched to our financial reporting-sourced sample;[34] and iv) it is less clear whether firms borrowing from the 'grey market' would have an incentive for a voluntary audit or not, as collateralization

[32] See source: https://www.bsi.si/en/statistics/interest-rates/ecb-interest-rates, and https://www.stat.si/StatWeb/en/News/Index/8210.
[33] We stress that this interest rate is not directly comparable to interest rates reported in Table 3.
[34] Alternatively, it is also conceivable that the best performing firms will avoid the normal bank channel obtaining loans under better conditions in 'grey' markets. We surmise that this is the case in our sample.

may be preferred on these markets (e.g., Berger et al., 2016). As a result, we anticipate that the effects reported in our main analyses are less likely to be discovered in this sub-sample.

The findings still indicate that firms' choice of an audit, *V_AUDIT,* increases rather than decrease the cost of debt, however the coefficient is, predictably, insignificant. Similarly, the regression coefficient on *BIG_4* is negative, which is consistent with our previous conclusions, but yet again (and predictably) insignificant. Estimated regression coefficients on control variables are generally statistically significant with their signs as reported in the main analyses in Table 6.

At the very least, the conservative conclusion from this particular analysis is that voluntary audit does *not decrease* the cost of debt of private firms as is commonly argued and reported in existing literature. Taking in consideration all constraints and limitations from above, in particular the non-random nature of unmatched firms, we interpret the results from this sub-section as being consistent with our main findings, although we conservatively clearly note that our main variable of interest (*V_AUDIT*) is statistically insignificant.

7) Conclusions and limitations

We analyze the effect of voluntary audit on the cost of debt of small private firms. Our tests show that voluntary external verification of financial statements is generally associated with higher, rather than lower, cost of debt financing. In the outcome analyses, we find robust evidence that voluntary audits increase the interest rates by up to 3.0 percentage points. This effect is present regardless of the perceived audit quality (Big-4 vs. non-Big-4), but is stronger by about 0.8 percentage points for non-Big-4 audits where auditees have a stronger position relative to auditors. Firms choose a voluntary audit of their financial statements either because the economic setting in which they operate effectively forces them to do so (e.g., ownership complexity, export-oriented supply chain, subsidiary status) or because firm fundamentals and/or financial reporting practices limit their access to financial debt. We document that

these factors are, overall, important in the voluntary audit decision. We conclude that voluntary audits facilitate access to financial debt for firms with higher information and/or fundamental risk that may otherwise have no access to this form of financing. The price these firms pay is reflected in higher interest rates being charged to voluntary auditees – firms with higher information and/or fundamental risk. Consistent with these findings, we study and find that audited firms' reported earnings are less informative about future operating performance relative to unaudited counterparts with the effect being concentrated in non-Big-4 auditees. These firms represent higher information and/or fundamental risk for creditors. The additional analyses we perform, e.g., controlling for selection bias using propensity score matching and 2SLS approaches, and employing Bank of Slovenia's value-weighted interest rate from individual loan contracts, further corroborate the robustness of our main results.

Our findings are subject to some limitations, too. Although a part of the common EU accounting regulatory framework, this is a single-county study carrying all the country-specific factors that may be different from other EU member states. This makes the results less comparable and generalizable. Nevertheless, other findings are consistent with extant research, while those that deviate from those reported elsewhere in the literature point to areas that need additional scrutiny in the literature. Examples include technical issues such as the question of how does the issue of measurement of the cost of debt affects research outcomes, as well as substance issues such as the differences in term structure of debt (we find that existence of long-term, but not short-term, loans affects the decision to audit). Another limitation is that the number of audit initiations, discontinuations and switches preclude us from a detailed analysis of these cases. Other than executing this study in an empirical setting with more data for these cases, an alternative way of shedding light on this issue would be to conduct a three-side survey of managers, auditors and banks.

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TABLES AND FIGURES

Table 1

Variable Descriptions

Variable	Description
Dependent variable	
$IRATE_{t+1}$	Interest rate for year *t+*1, measured as the sum of bank- and bond-related financial expense in year *t+*1, divided by the average of short- and long-term financial obligations towards banks (interest bearing debt) in years *t* and *t+*1.
Independent variables	
V_AUDIT_t	An indicator variable equaling 1 if a firm has been audited but it is not required to do so by law, and 0 otherwise.
M_AUDIT_t	An indicator variable equaling 1 if a firm has been audited because it is required to do so by law, and 0 otherwise.
BIG_4_t	An indicator variable equaling 1 if a firm has been audited by KPMG, PwC, E&Y or D&T, and 0 otherwise.
JSC_t	An indicator variable equaling 1 if a firm's legal status is a joint stock company, and 0 otherwise.
LP_t	Number of legal person owners of a firm.
L_LP_t	Natural logarithm of the number of legal persons owning a firm.
NP_t	Number of natural person owners of a firm.
L_NP_t	Natural logarithm of the number of natural persons owning the firm.
$EXPORT_t$	Exports as a share of total sales
B_ACC_t	Number of bank accounts open at the end of the year.
ST_LOAN_t	Short-term bank loans scaled by total assets.
LT_LOAN_t	Long-term bank loans scaled by total assets.
$EM1_t$	A measure of a firm's earnings quality calculated as a ratio of absolute total accruals in year *t* scaled by operating cash flows in year *t*.
$SIZE_t$	Natural logarithm of total assets.
$PROF_t$	Profitability, measured as the sum of EBIT and asset write-offs scaled by total assets.
$DAYS\_BLOCK_t$	Indicator variable equaling 1 if the number of days that the firm has its transaction account(s) blocked by the tax authorities is greater than zero, and 0 otherwise.
$SALESG_t$	Sales growth, measured as the ration of change in sales to previous year's sales.
$ICOV_t$	Interest coverage, measured as the ratio of the sum of bank- and bond-related financial expense to EBITDA.
PPE_t	Property, plant, and equipment scaled by total assets.
OP_LEV_t	Operating leverage, measured as the long-and-short term operating liabilities + short-term accrued costs and deferred revenues, scaled by total assets.
WC_t	Working capital, measured as the difference between current assets and current liabilities, scaled by total assets.
N_EQ_t	An indicator variable equaling 1 if the firm is reporting negative equity, and 0 otherwise.
$N\_EQ\_BL_t$	An interaction variable between firm reporting negative equity and firm bank loans scaled by total assets.
TAX_t	An indicator variable equaling 1 if the sum of firm's income and deferred taxes is equal to 0, and 0 otherwise.
IMR_t	Inverse Mills Ratio obtained from the 1st stage Heckman probit regression.
$Z\text{-}SCORE_t$	Altman Z-Score for each firm.

Table 2

Sample Construction Procedure

Firm-year observations of small private firms in the period 2006 - 2017	32,003
- observations of firms not operating for 12 consecutive months	114
- observations with legal structure change within a given year	197
- observations of financial and utility firms	1,550
- observations with incomplete data required for the analysis	10,311
- outlier observations	2,089
Final sample firm-year observations	17,742
- observations with IRATE = 0	1,780
- observations with IRATE > 0 (4,058 distinct firms)	15,962

Notes: Table 2 presents the starting sample of small private firms obtained from AJPES database and identified as firms that do not have to undergo a mandatory audit following the Slovenian Companies Act (2006 and subsequent amendments). All financial industry related firms (i.e., financial and insurance activities, real estate) as well as utilities are excluded due to their distinct balance sheet structure and operating properties. Lastly, we exclude (outlier) observations with values lying below the 1st percentile and/or above the 99th percentile.

Table 3

Descriptive Statistics

Variable	Mean	Min	25%	Median	75%	Max	SD	N
Panel A: Audited observations (voluntary audit)								
IRATE	0.048	0.000	0.027	0.045	0.060	0.322	0.033	800
BIG_4	**0.220**	0.000	0.000	**0.000**	0.000	1.000	0.415	800
JSC	**0.058**	0.000	0.000	**0.000**	0.000	1.000	0.233	800
LP	**1.830**	0.000	1.000	**1.000**	2.000	35.000	2.725	800
NP	**6.649**	0.000	0.000	**1.000**	3.000	256.000	22.776	800
EXPORT	**0.191**	0.000	0.000	**0.016**	0.295	0.998	0.297	800
B_ACC	**2.451**	1.000	2.000	**2.000**	3.000	7.000	1.154	800
ST_LOAN	**0.100**	0.000	0.023	**0.062**	0.147	0.917	0.111	800
LT_LOAN	**0.108**	0.000	0.000	**0.050**	0.160	0.692	0.141	800
EM1	-0.185	-177.223	0.194	**0.684**	0.945	48.555	9.919	800
SIZE	**15.311**	13.511	14.840	**15.255**	15.743	16.790	0.642	800
PROF	**0.043**	-0.168	0.010	**0.038**	0.074	0.336	0.067	800
DAYS_BLOCK	**0.075**	0.000	0.000	**0.000**	0.000	1.000	0.263	800
SALESG	**0.027**	-0.636	-0.056	**0.000**	0.105	1.135	0.213	800
ICOV	0.132	-1.131	0.014	0.071	0.187	1.501	0.247	800
PPE	0.389	0.000	0.196	**0.369**	0.576	0.933	0.248	800
OP_LEV	**0.259**	0.006	0.128	**0.217**	0.349	0.969	0.175	800
WC	**0.054**	-0.550	-0.087	**0.045**	0.187	0.665	0.226	800
N_EQ	**0.014**	0.000	0.000	**0.000**	0.000	1.000	0.117	800
N_EQ_BL	**0.013**	0.000	0.000	**0.000**	0.000	1.000	0.116	800
TAX	**0.265**	0.000	0.000	**0.000**	1.000	1.000	0.441	800

Variable	Mean	Min	25%	Median	75%	Max	SD	N
Panel B: Unaudited observations								
IRATE	0.045	0.000	0.024	0.039	0.058	0.340	0.034	15,162
BIG_4	**0.000**	0.000	0.000	**0.000**	0.000	0.000	0.000	15,162
JSC	**0.011**	0.000	0.000	**0.000**	0.000	1.000	0.107	15,162
LP	**0.892**	0.000	0.000	**0.000**	1.000	357.000	5.228	15,162
NP	**3.012**	0.000	1.000	**2.000**	3.000	256.000	8.777	15,162
EXPORT	**0.226**	0.000	0.001	**0.065**	0.377	1.000	0.299	15,162
B_ACC	**2.347**	0.000	2.000	**2.000**	3.000	20.000	1.185	15,162
ST_LOAN	**0.091**	0.000	0.147	**0.057**	0.128	0.900	0.106	15,162
LT_LOAN	**0.130**	0.000	0.005	**0.086**	0.204	0.992	0.143	15,162
EM1	10.507	-578.664	0.171	**0.623**	0.889	14.386	11.748	15,162
SIZE	**14.754**	13.346	14.341	**14.749**	15.141	16.796	0.611	15,162

PROF	**0.063**	-0.169	0.024	**0.049**	0.090	0.339	0.063	15,162
								Continued…
DAYS_BLOCK	**0.057**	0.000	0.000	**0.000**	0.000	1.000	0.232	15,162
SALESG	**0.053**	-0.706	-0.062	**0.030**	0.157	1.200	0.235	15,162
ICOV	0.124	-1.147	0.018	0.065	0.173	1.520	0.204	15,162
PPE	0.401	0.000	0.218	**0.399**	0.577	0.934	0.231	15,162
OP_LEV	**0.311**	0.000	0.173	**0.278**	0.418	1.269	0.180	15,162
WC	**0.114**	-0.556	-0.019	**0.104**	0.246	0.672	0.204	15,162
N_EQ	**0.007**	0.000	0.000	**0.000**	0.000	1.000	0.082	15,162
N_EQ_BL	**0.006**	0.000	0.000	**0.000**	0.000	1.000	0.080	15,162
TAX	**0.230**	0.000	0.000	**0.000**	0.000	1.000	0.421	15,162

Variable	**Mean**	**Min**	**25%**	**Median**	**75%**	**Max**	**SD**	**N**
Panel C: Selected financial statement components (EUR, audited and unaudited observations)								
Audited observations (voluntary audit)								
Total assets	5,487,850	737,543	2,786,117	4,220,093	6,869,695	19,600,000	3,734,279	800
Equity	2,211,591	-1,195,982	753,757	1,478,031	2,903,040	12,900,000	2,216,837	800
Financial debt (s/t + l/t)	1,278,799	0	214,960	666,609	1,607,961	9,392,464	1,635,189	800
Sales	4,738,016	53,521	2,679,362	4,140,933	6,236,486	27,300,000	2,936,481	800
EBIT	198,694	-4,644,731	15,751	125,076	322,289	2,756,532	442,583	800
Taxes	27,877	-424,121	0	5,731	34,591	468,234	59,938	800
Net income	130,001	-10,600,000	6,534	68,771	253,173	2,135,483	533,718	800
Unaudited observations								
Total assets	3,109,439	625,175	1,691,170	2,544,446	3,765,431	19,700,000	2,256,137	15,162
Equity	1,139,773	-1,230,272	453,117	826,872	1,443,795	13,400,000	1,158,727	15,162
Financial debt (s/t + l/t)	762,198	0	165,819	426,501	970,808	17,200,000	1,026,582	15,162
Sales	3,672,497	74,216	2,028,448	3,016,150	4,635,584	19,100,000	2,732,557	15,162
EBIT	163,114	-5,608,718	43,582	108,342	223,904	3,791,187	241,508	15,162
Taxes	22,659	-306,223	149	8,532	28,224	701,258	40,689	15,162
Net income	110,242	-6,535,492	17,325	62,047	156,881	5,455,119	220,422	15,162

Notes: Panels A and B present descriptive statistics for audited and unaudited observations, respectively. Observations are partitioned with respect to *V_AUDIT*, i.e., had firm-year observation chosen a voluntary audited or not. In Panels A and B, means of variables with differences significant at 5% level or better (two-tailed t-test) are presented in bold as are medians with 5% significance of the Wilcoxon rank-sum test. Panel C shows descriptive statistics of selected financial statement components which are presented in EUR denomination. *Equity* is firm's book value of equity, *Financial debt (s/t + l/t)* is the sum of firm's short-and-long term bank loans, and *Taxes* is the sum of firm's income and deferred taxes.

Table 4

Correlations

	IRATE	*V_AUDIT*	*BIG_4*	*JSC*	*LP*	*NP*	*EXPORT*	*B_ACC*	*ST_LOAN*	*LT_LOAN*	*EM1*	*SIZE*	*PROF*	*DAYS_BLOCK*	*SALESG*	*ICOV*	*PPE*	*OP_ LEV*	*WC*	*N_EQ*	*TAX*
IRATE		**0.030**	0.012	**0.075**	**0.113**	**-0.095**	**-0.102**	**0.040**	**0.242**	**-0.153**	**0.058**	**0.016**	**-0.129**	**0.126**	**-0.088**	**0.341**	**-0.131**	**0.225**	**-0.176**	**0.030**	**0.049**
V_AUDIT	0.015		**0.460**	**0.086**	**0.172**	**-0.068**	**-0.051**	**0.022**	**0.028**	**-0.042**	**0.030**	**0.175**	**-0.066**	**0.017**	**-0.031**	0.002	-0.015	**-0.071**	**-0.061**	**0.018**	**0.017**
BIG_4	-0.003	**0.460**		**0.018**	**0.115**	**-0.089**	-0.004	0.010	0.002	**-0.023**	0.004	**0.085**	**-0.031**	-0.003	**-0.020**	-0.011	-0.014	**-0.039**	**-0.018**	**0.020**	**0.007**
JSC	**0.049**	**0.086**	**0.018**		**0.116**	**-0.056**	**-0.054**	-0.015	0.014	-0.007	0.011	**0.086**	**-0.056**	**0.021**	**-0.020**	0.016	**0.032**	**-0.028**	**-0.050**	0.003	**0.048**
LP	**0.021**	**0.040**	**0.027**	**0.058**		**0.075**	**-0.060**	**0.056**	**0.056**	**-0.080**	-0.000	**0.171**	**-0.026**	-0.014	**-0.023**	0.014	**-0.078**	**-0.027**	0.008	-0.009	-0.003
NP	-0.000	**0.080**	**-0.020**	**0.041**	**-0.160**		**0.081**	**0.063**	**-0.027**	**0.040**	0.010	**-0.054**	**-0.016**	-0.013	**-0.059**	-0.002	-0.005	**-0.022**	**0.068**	**-0.021**	**0.023**
EXPORT	**-0.058**	**-0.025**	0.006	**-0.038**	**-0.027**	**0.021**		**-0.012**	**-0.023**	**-0.021**	**-0.030**	**0.063**	**0.104**	**-0.025**	**0.098**	**-0.092**	0.007	0.014	**0.076**	-0.002	**0.039**
B_ACC	0.014	**0.019**	0.003	-0.009	0.011	**0.022**	**0.024**		**0.176**	**0.126**	0.013	**0.225**	**-0.040**	**0.085**	**-0.030**	**0.169**	**-0.037**	**-0.021**	**-0.028**	**-0.019**	0.011
ST_LOAN	**0.097**	**0.019**	0.009	0.013	0.002	-0.005	-0.000	**0.136**		**-0.034**	-0.004	**0.091**	**-0.103**	**0.096**	**-0.102**	**0.464**	**-0.144**	**0.016**	**-0.297**	**0.031**	**0.052**
LT_LOAN	**-0.150**	**-0.032**	**-0.023**	-0.008	**-0.020**	**-0.032**	0.004	**0.104**	**-0.088**		**0.060**	**0.183**	**-0.047**	**0.086**	**0.004**	**0.375**	**0.379**	**-0.277**	**-0.144**	**0.012**	**0.074**
EM1	0.001	-0.002	-0.001	-0.001	-0.000	-0.000	-0.006	-0.001	0.000	-0.003		0.009	**-0.247**	**0.057**	**-0.075**	**0.093**	**0.160**	**0.029**	**-0.188**	**0.026**	**0.178**
SIZE	**-0.016**	**0.195**	**0.105**	**0.093**	**0.035**	**0.018**	**0.040**	**0.223**	**0.098**	**0.182**	0.006		**-0.051**	**0.084**	**0.029**	**0.168**	**0.098**	**-0.227**	**-0.102**	-0.008	**-0.020**
PROF	**-0.087**	**-0.069**	**-0.036**	**-0.046**	**-0.031**	**-0.060**	**0.099**	**-0.045**	**-0.100**	**-0.082**	0.014	**-0.060**		**-0.118**	**0.248**	**-0.170**	**-0.070**	**-0.065**	**0.237**	**-0.085**	**-0.367**
D_BLOCK	**0.099**	**0.016**	-0.003	**0.021**	-0.003	-0.002	-0.013	**0.080**	**0.090**	**0.100**	-0.002	**0.085**	**-0.121**		**-0.124**	**0.137**	**0.034**	**0.024**	**-0.136**	**0.108**	**0.131**
SALESG	**-0.034**	**-0.024**	-0.013	-0.016	-0.006	**-0.024**	**0.089**	**-0.028**	**-0.087**	-0.012	-0.002	**0.026**	**0.288**	**-0.122**		**-0.133**	0.008	**0.076**	**0.029**	**-0.035**	**-0.114**
ICOV	**0.140**	0.008	-0.009	0.005	0.004	-0.010	**-0.078**	**0.126**	**0.353**	**0.274**	0.000	**0.145**	**-0.315**	**0.134**	**-0.086**		**-0.020**	**-0.029**	**-0.252**	-0.014	**0.071**
PPE	**-0.100**	-0.012	-0.014	**0.036**	0.003	**0.028**	**0.021**	**-0.042**	**-0.198**	**0.351**	-0.001	**0.099**	**-0.051**	**0.036**	-0.009	**-0.045**		**-0.019**	**-0.419**	-0.016	**0.177**
OP_LEV	**0.204**	**-0.063**	**-0.035**	**-0.023**	-0.015	**-0.055**	**0.015**	**-0.035**	**-0.050**	**-0.303**	-0.004	**-0.221**	**-0.041**	**0.026**	**0.095**	**-0.053**	**-0.361**		**-0.162**	**0.062**	**-0.032**
WC	**-0.109**	**-0.064**	**-0.023**	**-0.052**	0.003	**0.023**	**0.042**	**-0.022**	**-0.277**	**-0.120**	0.007	**-0.097**	**0.211**	**-0.141**	**0.020**	**-0.153**	**-0.417**	**-0.190**		**-0.091**	**-0.194**
N_EQ	**0.033**	**0.018**	**0.020**	0.003	-0.004	-0.009	0.004	**-0.019**	**0.039**	**0.039**	-0.001	-0.004	**-0.105**	**0.106**	**-0.029**	-0.010	**-0.016**	**0.092**	**-0.111**		**0.110**
TAX	**0.043**	**0.017**	0.007	**0.048**	**0.020**	**0.071**	**0.047**	0.003	**0.033**	**0.089**	-0.003	**-0.019**	**-0.339**	**0.131**	**-0.101**	**0.065**	**0.179**	**-0.021**	**-0.195**	**0.110**	

Notes: The table presents Spearman (pairwise Pearson) correlation coefficients above (below) the diagonal for the whole sample of 15,962 observations in the period from year 2006 to 2017. Coefficients with a significance level of 5% or better (two-tailed t-test) are presented in bold. The interaction term *N_EQ_BL* is excluded from the correlation table.

Table 5

The Selection Model: Probit Regression

Dependent variable: *V_AUDIT*

Independent variables	Coefficient
JSC	**0.413****
	(2.53)
L_LP	**0.480*****
	(8.64)
L_NP	-0.041
	(-0.71)
EXPORT	0.045
	(0.40)
B_ACC	0.033
	(1.18)
ST_LOAN	0.288
	(1.11)
LT_LOAN	**-0.840*****
	(-3.33)
*EM*1	**0.050****
	(2.12)
Constant	**-1.832*****
	(-2.64)
Robust standard errors	Yes
Clustered by firm	Yes
Industry controls	Yes
Year controls	Yes
Observations	15,962
Pseudo R^2	0.174

Notes: The table presents the selection model (1st stage Heckman Probit regression) results, on the conditional probability of a private firm choosing a voluntary audit. The model is estimated accounting for industry and year controls. Coefficient estimates are reported in the first row whereas the two-tailed z-statistics of significance are reported in parentheses. Asterisks **, and *** denote significance at 5%, and 1% levels, respectively.

Table 6

Main Results

Dependent variable: *IRATE*					
Independent variables	(1) OLS extended	(2) OLS base model	(3) Heckman 2nd stage	(4) Propensity score matching	(5) IV: *2SLS estimation*
V_AUDIT	**0.003****	**0.005*****	**0.030*****	**0.011*****	**0.027*****
	(1.97)	**(3.32)**	**(5.62)**	**(4.20)**	**(3.73)**
BIG_4	**-0.006*****	**-0.006****	**-0.008*****	**-0.006****	**-0.024*****
	(-2.67)	**(-2.46)**	**(-3.11)**	**(-2.43)**	**(-3.23)**
JSC	0.001				
	(0.42)				
L_LP	0.000				
	(0.35)				
L_NP	-0.000				
	(-0.78)				
EXPORT	0.000				
	(0.27)				
B_ACC	**0.002*****				
	(4.98)				
ST_LOAN	**-0.024*****				
	(-6.22)				
LT_LOAN	**-0.040*****				
	(-14.57)				
*EM*1	0.000				
	(0.12)				
SIZE	**-0.005*****	**-0.005*****	**-0.005*****	**-0.008*****	-0.001
	(-7.42)	**(-7.53)**	**(-7.69)**	**(-4.51)**	(-1.78)
PROF	**-0.019*****	**-0.016*****	**-0.016*****	-0.011	-0.008
	(-3.56)	**(-3.14)**	**(-2.99)**	(-0.76)	(-1.36)
DAYS_BLOCK	**0.010*****	**0.009*****	**0.009*****	**0.011*****	**0.010*****
	(7.25)	**(6.52)**	**(6.60)**	**(3.30)**	**(6.92)**
SALESG	0.001	0.000	0.001	-0.002	**-0.003****
	(0.55)	(0.19)	(0.15)	(-0.53)	**(-2.25)**
ICOV	**0.021*****	**0.012*****	**0.012*****	0.004	**0.020*****
	(11.81)	**(7.77)**	**(8.01)**	(1.00)	**(11.74)**
PPE	**-0.007****	**-0.008*****	**-0.007*****	**-0.013****	**-0.008*****
	(-2.46)	**(-4.02)**	**(-3.76)**	**(-2.22)**	**(-2.72)**
OP_LEV	**0.010*****	**0.020*****	**0.019*****	**0.023*****	**0.033*****
	(3.37)	**(7.27)**	**(7.23)**	**(3.31)**	**(11.64)**
WC	**-0.009*****	**-0.005*****	**-0.005****	-0.001	**-0.008*****
	(-4.11)	**(-4.02)**	**(-2.36)**	(-0.18)	**(-3.66)**
N_EQ	**0.068****	**0.070*****	**0.070****	**0.013****	**0.077*****
	(2.44)	**(2.55)**	**(2.52)**	**(2.22)**	**(2.62)**
N_EQ_BL	**-0.068****	**-0.074*****	**-0.074*****		**-0.080*****
	(-2.41)	**(-2.69)**	**(-2.65)**		**(-2.72)**
TAX	**0.003*****	**0.002*****	**0.002*****	0.004	**0.002****
	(3.44)	**(3.02)**	**(2.90)**	(1.86)	**(2.08)**
IMR			**-0.012*****		
			(-4.99)		
Constant	**0.126*****	**0.119*****	**0.115*****	**0.164*****	**0.153*****
	(12.75)	**(12.21)**	**(11.97)**	**(5.90)**	**(2.86)**

Robust standard errors	Yes	Yes	Yes	Yes	Yes
Clustered by firm	Yes	Yes	Yes	Yes	Yes
Industry controls	Yes	Yes	Yes	Yes	Yes
Year controls	Yes	Yes	Yes	Yes	No
Observations	15,962	15,962	15,962	15,962 (1,600 PSM)	15,962
R^2	0.216	0.195	0.196	0.224	0.171
Sum of coefficients (*V_AUDIT* + *BIG_4*)	**-0.003*** ** (F=7.06)	**-0.001*** ** (F=9.78)	**0.022*** ** (F=32.02)	**0.005*** ** (F=13.34)	**0.003*** ** (F=14.47)

Notes: The table presents regression results for the main sample. Extended OLS model is reported in column 1 and the base OLS model in column 2. Column 3 reports the Heckman 2nd stage estimation with included inverse Mills ratio from the 1st stage (estimation results in Table 5). Column 4 presents the propensity score matching (PSM) regression estimates which contribute to the resilience of the results. In column 4, interaction dummy *N_EQ_BL* was excluded to impede collinearity. Column 5 presents the instrumental variables (IV) approach. The instrumental variable in use, '*iv_audit'*, is an indicator variable equal to one, for firms that fulfilled the requirements for being audited, based on information from the year 2006, and equal to zero otherwise. Coefficient estimates are reported in the first row whereas the two-tailed z-statistics (F-statistics for sum of coefficients significance) of significance are reported in parentheses. Asterisks **, and *** denote significance at 5%, and 1% levels, respectively.

Table 7

Informativeness of Earnings and Earnings Management

Panel A: Forecast relevance of earnings and earnings' components

	NI regression	CFO and ACC regression	
	$\propto_1$	$\propto_2$	$\propto_3$
Unaudited observations (N=10,726)	**0.530*** **	**0.522*** **	**0.491*** **
Audited observations (N=507)	**0.417*** **	**0.353*** **	**0.365*** **
Audited by a Non-Big-4 auditor (N=385)	0.403	0.333	0.300
Audited by a Big-4 auditor (N=122)	**0.444** **	**0.406** **	**0.560*** **

Panel B: Earnings management measures

	*EM*1	*EM*2
Unaudited observations (N=15,162)	0.624	0.848
Audited observations (N=800)	0.684	0.780
Audited by a Non-Big-4 auditor (N=624)	0.693	0.792
Audited by a Big-4 auditor (N=176)	0.652	0.722

Notes: Panel A presents coefficient estimates of net income, cash flows and accruals, predicting future cash flows from the following equations (estimated with industry- and year-fixed effects, and robust standards errors clustered at firm level): $CFO_{t+1} = \propto_0 + \propto_1 NI_t + \in_t$ and $CFO_{t+1} = \propto_0 + \propto_2 CFO_t + \propto_3 ACC_t + \in_t$. The number of observations is reduced compared to Panel B because of one year ahead data requirements. Panel B presents earnings management proxies. *EM*1 is the median ratio of absolute total accruals scaled by operating cash flow: $|ACC_{it}|/CFO_{it}$, and *EM*2 is Spearman correlation coefficient between total accruals and operating cash flow, multiplied by -1: -ρ(*CFO, ACC*). *NI* is net income scaled by total assets, *ACC* is total accruals scaled by total assets calculated as: (Δ *current assets* – Δ *cash*) - (Δ *current liabilities* – Δ *short term debt*) - *depreciation*, and *CFO* is cash flow from operations calculated as net income minus total accruals. Both panels include two comparisons. First, they compare unaudited and audited observations, and second, audited observations are separated into audited by a non-Big4 auditor and audited by a Big-4 auditor. Asterisks **, and *** denote significance at 5%, and 1% levels, respectively.

Table 8

Additional Tests: Crisis Effect, Legal Person Ownership, and Z-Score

Dependent variable: *IRATE*					
	(1)	(2)	(3)	(4)	(5)
Independent variables	Pre-2008	2008-and- post	No LP ownership	LP ownership	Altman Z-score
V_AUDIT	0.017	**0.032*** **	**0.079*** **	**0.032*** **	**0.029*** **
	(1.36)	**(5.46)**	**(4.47)**	**(4.75)**	**(5.61)**
BIG_4	**-0.014** **	**-0.006** **	-0.005	**-0.008*** **	**-0.008*** **
	(-1.96)	**(-2.40)**	(-0.41)	**(-2.86)**	**(-3.11)**
SIZE	-0.003	**-0.005*** **	**-0.005*** **	**-0.004*** **	**-0.005*** **
	(-1.25)	**(-8.44)**	**(-6.00)**	**(-5.09)**	**(-7.70)**
PROF	0.005	**-0.018*** **	-0.013	**-0.021*** **	**-0.016*** **
	(0.29)	**(-3.41)**	(-1.77)	**(-2.66)**	**(-2.99)**
DAYS_BLOCK	**0.013*** **	**0.008*** **	**0.008*** **	**0.010*** **	**0.009*** **
	(2.80)	**(6.04)**	**(4.75)**	**(4.44)**	**(6.60)**
SALESG	0.001	-0.001	-0.002	0.002	-0.001
	(0.09)	(-0.11)	(-1.26)	(1.05)	(-0.16)
ICOV	**0.012*** **	**0.012*** **	**0.016*** **	**0.008*** **	**0.012*** **
	(2.76)	**(8.23)**	**(7.70)**	**(4.35)**	**(8.00)**
PPE	0.009	**-0.010*** **	**-0.005** **	**-0.007*** **	**-0.007*** **
	(1.57)	**(-5.08)**	**(-2.23)**	**(-2.55)**	**(-3.74)**
OP_LEV	**0.025*** **	**0.017*** **	**0.021*** **	**0.018*** **	**0.019*** **
	(3.67)	**(6.33)**	**(5.80)**	**(4.68)**	**(7.24)**
WC	0.004	**-0.007*** **	-0.005	-0.004	**-0.005** **
	(0.66)	**(-3.14)**	(-1.89)	(-1.32)	**(-2.34)**
N_EQ	**0.139*** **	0.045	**0.122*** **	0.017	**0.069** **
	(33.89)	(1.78)	**(9.65)**	(1.32)	**(2.52)**
N_EQ_BL	**-0.155*** **	-0.047	**-0.125*** **	-0.025	**-0.074*** **
	(-25.86)	(-1.78)	**(-8.08)**	(-1.76)	**(-2.65)**
TAX	0.004	**0.002** **	0.002	**0.003*** **	**0.002*** **
	(1.58)	**(2.49)**	(1.57)	**(2.61)**	**(2.87)**
IMR	-0.005	**-0.013*** **	**-0.032*** **	**-0.014*** **	**-0.012*** **
	(-0.84)	**(-4.90)**	**(-4.35)**	**(-4.03)**	**(-4.97)**
Z-SCORE					0.000
					(1.00)
Constant	**0.122*** **	**0.098*** **	**0.128*** **	**0.112*** **	**0.115*** **
	(3.26)	(10.53)	(10.08)	(7.40)	(11.97)
Robust standard errors	Yes	Yes	Yes	Yes	Yes
Clustered by firm	Yes	Yes	Yes	Yes	Yes
Industry controls	Yes	Yes	Yes	Yes	Yes
Year controls	Yes	Yes	Yes	Yes	Yes
Observations	2,037	13,925	8,743	7,219	15,962
R^2	0.059	0.155	0.188	0.204	0.196
Sum of coefficients (*V_AUDIT* + *BIG_4*)	0.003 (F=3.12)	**0.026*** ** (F=29.10)	**0.074*** ** (F=12.41)	**0.024*** ** (F=26.07)	**0.021*** ** (F=32.07)

Notes: The table presents various sensitivity tests for the estimation of the main Heckman 2nd stage model. Columns 1 and 2 use year 2008 as a cut-off to analyze the effect of financial crisis. Column 3 analyses small private firms that are not owned by a legal person (not subsidiaries), and column 4 analyses those firms owned by at least one legal person (subsidiaries). In column 5 Altman's Z-score is used as an additional control variable. We apply Altman's Z-score form for private firms: Z = 0.717 *WC* + 0.847 *RE* + 3.107 *EBIT* + 0.420 *BV* + 0.998 *SALES*, where *WC* is working

capital scaled by total assets, *RE* is retained earnings scaled by total assets, *EBIT* is earnings before interest and taxes scaled by total assets, *BV* is book value of equity scaled by total liabilities, and *SALES* is sales scaled by total assets. Coefficient estimates are reported in the first row whereas the two-tailed z-statistics (F-statistics for sum of coefficients significance) of significance are reported in parentheses. Asterisks **, and *** denote significance at 5%, and 1% levels, respectively.

Table 9

Additional Tests Results: Mandatory Audit Firms and Wider IRATE Specifications

Dependent variable: IRATE				
Independent variables	(1) Voluntary and mandatory firms	(2) Mandatory firms	(3) *IRATE_2*	(4) *IRATE_3*
V_AUDIT	**0.023*** **		**0.033*** **	**0.022*** **
	(3.59)		**(4.20)**	**(2.55)**
M_AUDIT	**-0.001*** **			
	(-1.96)			
BIG_4	**-0.006** **		-0.003	-0.004
	(-2.50)		(-0.92)	(-1.21)
BIG_4_M_A	0.001	-0.000		
	(0.62)	(-0.21)		
SIZE	**-0.005*** **	**-0.002*** **	**-0.003*** **	**-0.003*** **
	(-9.18)	**(-3.41)**	**(-4.23)**	**(-3.61)**
PROF	**-0.021*** **	-0.013	0.013	0.009
	(-5.26)	(-1.86)	(1.76)	(1.46)
DAYS_BLOCK	**0.008*** **	**0.010*** **	**0.009*** **	**0.012*** **
	(7.47)	**(4.14)**	**(4.11)**	**(5.40)**
SALESG	-0.000	-0.001	-0.004**	-0.003
	(-0.11)	(-0.57)	(-2.42)	(-1.86)
ICOV	**0.011*** **	**0.011*** **	**0.025*** **	**0.013*** **
	(9.86)	**(5.98)**	**(11.56)**	**(8.47)**
PPE	**-0.009*** **	**-0.012*** **	**-0.005** **	0.003
	(-5.85)	**(-3.66)**	**(-1.96)**	(1.40)
OP_LEV	**0.015*** **	0.007	**0.046*** **	**0.045*** **
	(7.00)	(1.85)	**(13.15)**	**(13.22)**
WC	**-0.009*** **	**-0.017*** **	**-0.008*** **	**-0.015*** **
	(-5.38)	**(-5.83)**	**(-3.05)**	**(-5.36)**
N_EQ	**0.072** **	0.008	-0.000	-0.001
	(2.52)	(1.06)	(-0.05)	(-0.11)
N_EQ_BL	**-0.073*** **		-0.015	-0.012
	(-2.53)		(-1.53)	(-0.86)
IMR	**-0.009*** **	-0.000	**-0.013*** **	-0.007
	(-3.21)	(-0.02)	**(-3.47)**	(-1.66)
Constant	**0.125*** **	**0.121*** **	**0.084*** **	**0.077*** **
	(16.11)	**(9.39)**	**(6.96)**	**(6.75)**
Robust standard errors	Yes	Yes	Yes	Yes
Clustered by firm	Yes	Yes	Yes	Yes
Industry controls	Yes	Yes	Yes	Yes
Year controls	Yes	Yes	Yes	Yes
Observations	21,432	5,446	10,801	20,014
R^2	0.212	0.255	0.205	0.113
Sum of coefficients (*V_AUDIT* + *BIG_4*)	**0.017*** ** (F=14.80)		**0.030*** ** (F=14.78)	**0.018*** ** (F=7.35)

Notes: The table presents various additional analyses. Columns 1 to 4 estimate the main Heckman 2nd stage model (initially presented in column 3 of Table 6) for different sensitivity tests. The sample in column 1 includes small and big private firms (the later have to undergo a mandatory audit), and column 2 includes only big private firms. Column 3 presents identical model as our main model in column 3 of Table 6, i.e., uses small private firms again, but defines *IRATE_2* as financial expenditures from financial obligations in year *t+1,* dividend by the average of short- and long-

term financial obligations in year *t* and *t*+1. Column 4 also presents identical model as our main model in column 3 of Table 6, but defines *IRATE_3* as bank-, bond-, and leasing-related financial expenditures in year *t*+1*,* dividend by the average of short- and long-term financial obligations towards banks in year *t* and *t*+1. All samples exclude observations with calculated *IRATE*=0. *M_AUDIT* is an indicator variable for mandatory audit, and *BIG_4_M_A* is an interaction variable between *BIG_4* and *M_AUDIT*, respectively. *BIG_4* and *N_EQ_BL* were excluded from column 2 due to collinearity. The *TAX* was excluded for the same reason too. Coefficient estimates are reported in the first row whereas the two-tailed z-statistics (F-statistics for sum of coefficients significance) of significance are reported in parentheses. Asterisks **, and *** denote significance at 5%, and 1% levels, respectively.

Figure 1

Median and Relative Sales

Fig.1a

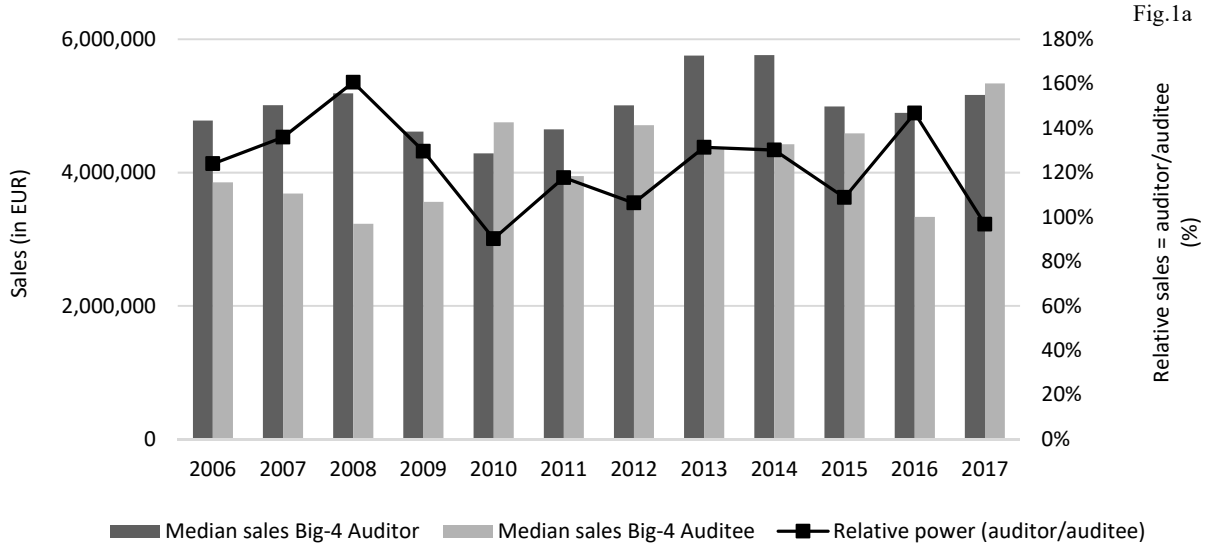
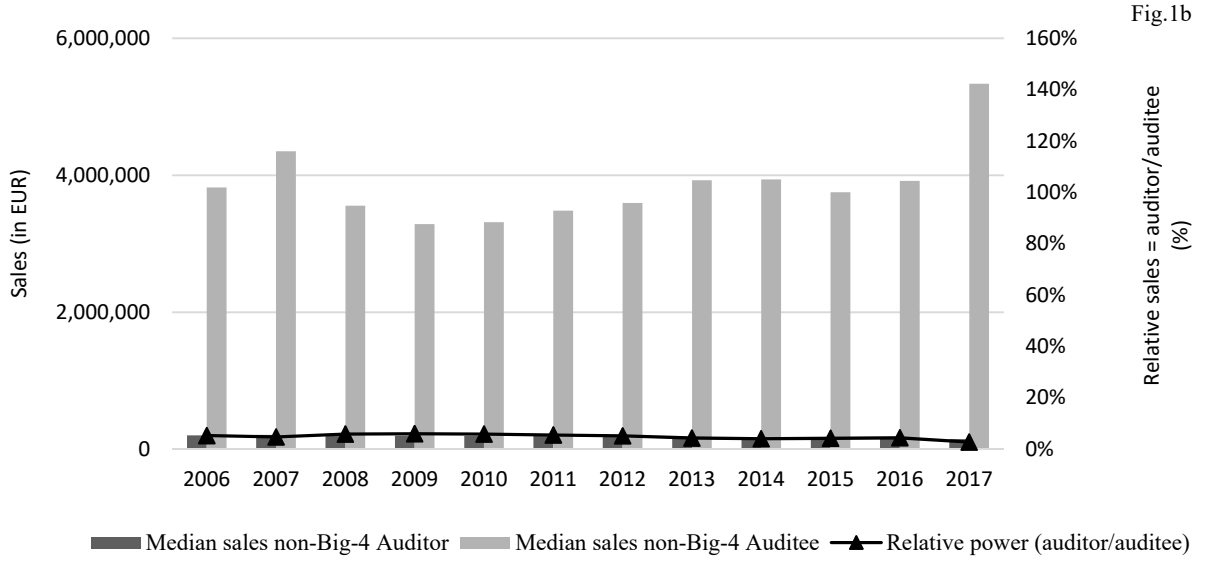


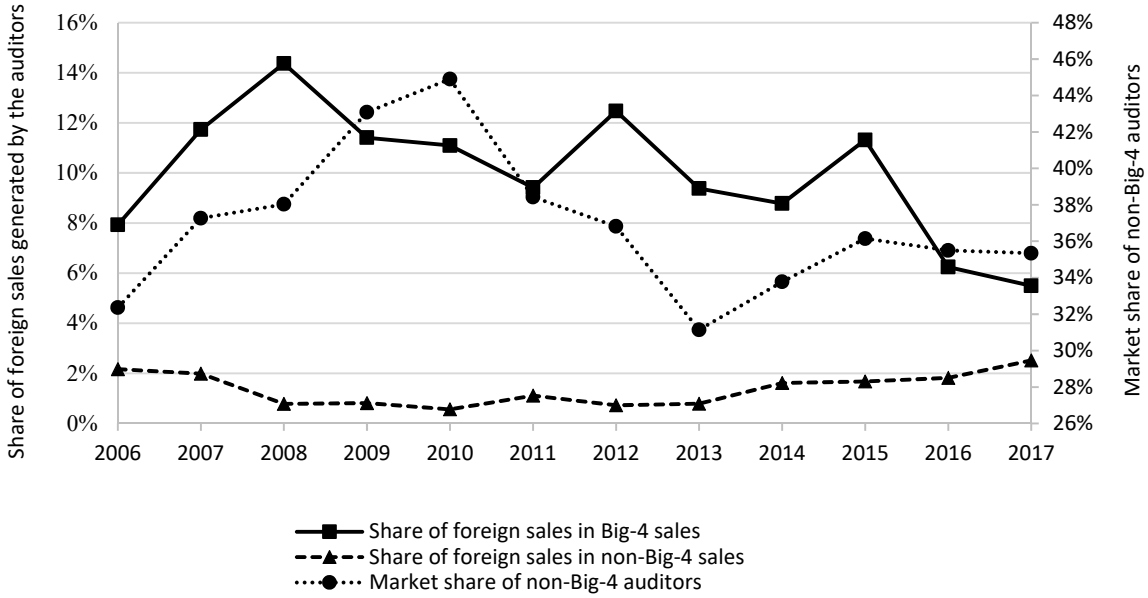
Fig.1b



*Notes: **Fig.1a presents median sales of Big-4 auditors versus their auditees. Fig.1b presents median sales of non-Big-4 auditors versus their auditees. The sales presented in EUR terms are scaled on the left hand-side axis, whereas the right hand-side axis scales percentage relative sales. The data spans from year 2006 to year 2017.***

Figure 2

Share of Foreign Sales



Notes: Fig.2 presents the share of foreign sales in total sales for both, Big-4 and non-Big-4, auditor groups scaled on the left hand-side axis. Market share of non-Big-4 auditors is scaled on the right hand-side axis. The market share is proxied by the share of non-Big-4 auditor income in total auditor income.

Figure 3

Mean Interest Rates

Fig. 3a

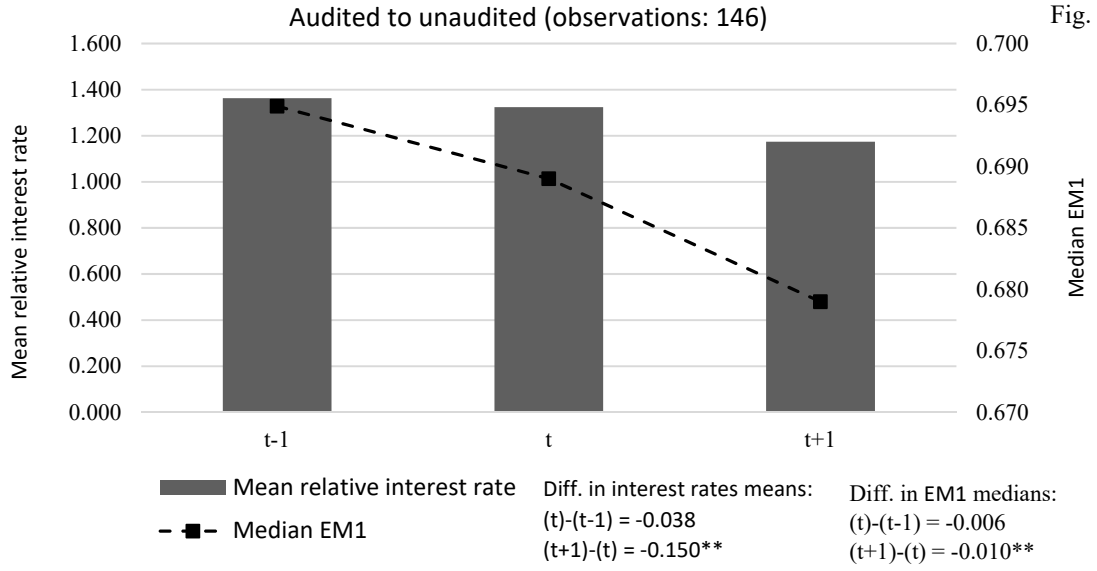


Fig. 3b

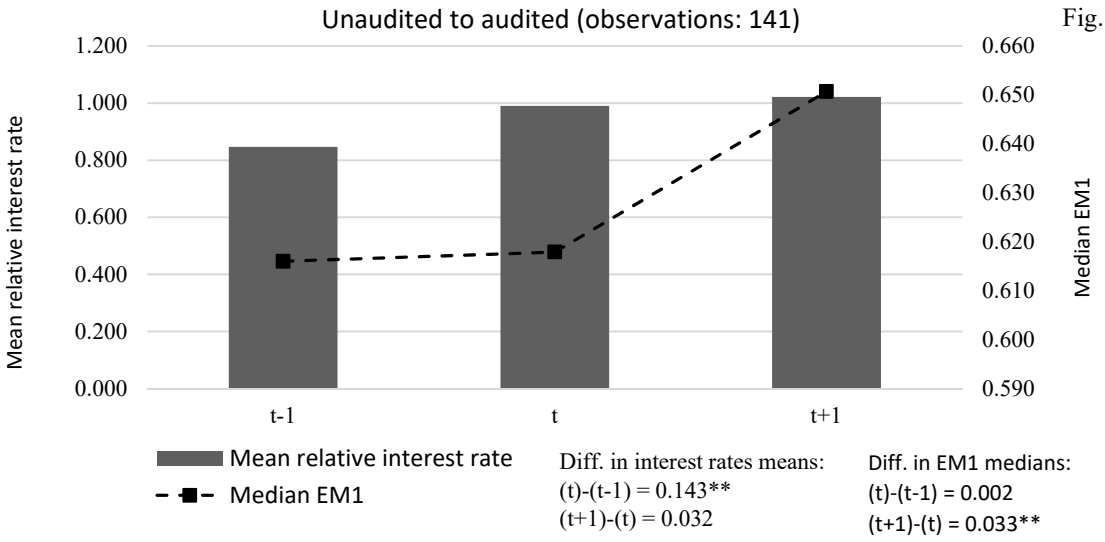
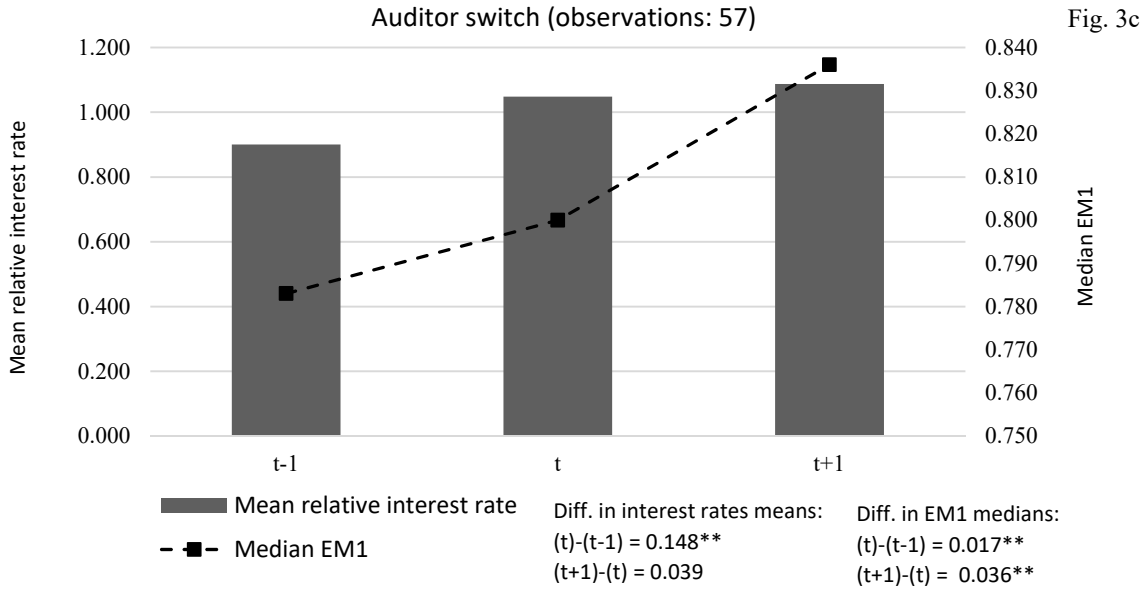


Fig. 3c



Notes: Fig.3a, Fig.3b, and Fig.3c present the observations that transited from being audited to unaudited, from unaudited to audited, and the ones that switched their auditor, respectively. The mean relative interest rate in time t-1, t, and t+1 is presented on the left hand-side axis, and the median EM1 (as calculated previously) is presented on the right hand-side axis. The mean relative interest rate in time t is calculated as the ratio between *IRATE* and Bank of Slovenia's 'interest rate of monetary financial institutions - new loans to non-financial corporations by original maturity in the domestic currency'. The figure covers data from year 2007 to 2017. Data for year 2006 is unavailable in Bank of Slovenia's database. Asterisks ** and *** denote significance at 5%, and 1% levels, respectively.

TRR 266 Accounting for Transparency

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