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ORIGINAL ARTICLE

Do audited firms have a lower cost of debt?

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Abstract
The purpose of this study is to investigate if audited financial statements add value for firms in the private debt market. Using an instrumental variable method, we find that firms with audited financial statements, on average, save 0.47 percentage points on the cost of debt compared to firms with unaudited financial statements. We also find that using the big, well-known auditing firms does not yield any additional cost of debt benefits. Lastly, we investigate if there are industries where alternative sources of information make auditing less valuable in reducing the cost of debt. Here, we find that auditing is less important in lowering cost in one industry, agriculture, where one lender has a 74% market share and a 100-year history of lending to firms within that industry. As such, it seems that lenders having high exposure to a certain industry might act as an alternative to auditing in reducing the information asymmetry between the firm and the lender.

Keywords External audit · Regulation · Agency theory · Audit reform · Audit complexity · Cost of capital · Endogenous switching model · Private limited firms

JEL Classification D22 · D24 · M42 · M48

Introduction

The long-run economic progress of a country is, to a large extent, determined by the level of investments creating a productive stock of capital. Investments are, however, in most cases associated with risk; having access to high-quality information regarding the well-being of firms trying to raise external capital for investment purposes can be of vital importance for financiers. This creates an incentive for well-managed firms to provide high-quality information to financiers so that they can access capital at a lower cost than less well-managed firms. The question is then how to ensure that financiers know that the information is of high quality, creating a potential motive for the firm to use external audits (Kueppers and Sullivan 2010). If audited information is considered to be of higher quality by financiers, this could then lead to a lower cost of debt (*CoD*) for firms with audited financial statements.

The main question we address is thus, "Do audited financial statements add value in the private debt market?" One way to answer this question is to look at the economic consequences, if any, of having audited financial statements. Do firms with audited financial statements have a lower *CoD* than firms with un-audited financial statements? We know little about such effects in general (Leuz and Wysock 2016). The prior literature on audit issues of private firms is quite limited (Hope et al. 2012), and research into auditing practices is largely understudied, especially for private firms (Vanstraelen and Schelleman 2017). Also, both the methods and the results from previous studies are mixed. For instance, Blackwell et al. (1998), Huguet and Gandía (2014), Kausar et al. (2016), Kim et al. (2011), and Minnis (2011) found that audit decreases *CoD*, while Koren et al. (2014) found the opposite. Allee and Yohn (2009) did not find any significant association between audits and *CoD* for private

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limited firms. Of these, Huguet and Gandía (2014) study how auditing, mandatory or not, affect *CoD* in the Spanish market where some, but not all, firms can opt out of auditing, Kausar et al. (2016) primarily studies the effect of voluntary audit choice, while Allee and Yohn (2009), Blackwell et al. (1998), Kim et al. (2011), Koren et al. (2014), and Minnis (2011), all restrict their analysis to those parts of the market where the choice to audit is voluntary for the firm.

Sweden provides an interesting setting for analyzing the value of audited financial statements. First, Sweden has a well-developed capital market (Bruns and Fletcher 2008), but the primary source of external financing for small firms comes from banks (Winborg and Landström 2000). Second, in 2010, there was a regulatory reform making audits voluntary for firms fulfilling certain requirements. This reform makes it possible to create a valid instrument addressing the apparent endogeneity problem that otherwise arises since at least some firms in the sample can self-select into or out of auditing. Third, the Swedish regulatory setting differs substantially from that in countries where this issue has previously been studied. In Sweden, all limited firms are required to make their annual reports public through the Swedish Companies Registration Office (SCRO), and failure to do so results in fines or even, in severe cases, imprisonment. Because they form the basis of taxation and Sweden is a high book-tax alignment country, these financial statements are also subject to scrutiny by tax authorities (Svanström 2013). Sweden was one of the last countries to adopt this EU-wide reform, and prior to the reform all firms were subject to mandatory audit. Sweden also has one of the lowest threshold levels in Europe for opting out of mandatory audit, second only to Finland and Malta. As such, the Swedish setting gives us an opportunity to investigate how audits affect *CoD* in a more strictly regulated market than in previous studies.

Methodologically, the identification of how auditing affects the firm's *CoD* is quite difficult to carry out since at least some firms can choose auditing to systematically reduce *CoD*, thus creating a selection bias. In such a situation, OLS estimation will be biased because the indicator variable for the firm being audited will be correlated with the error term of the regression.

This is, however, only part of the difficulty in measuring how *CoD* is affected by having audited financial statements. The literature suggests that there are several other factors that affect firm level *CoD*. As the Swedish reform was focused on microfirms, we need to ensure that firm size does not drive the results, and as such we control for firm size and several other variables that could affect firm-level *CoD* in the empirical section of the paper. An additional concern is that if lenders use firm information differently, depending on if the firms are audited or not, these variables will have a different impact on *CoD* for audited and un-audited firms. Failure to address this concern might bias the estimates of the impact of auditing on *CoD* (Minnis 2011).

To take these problems into consideration, we use an endogenous switching (ES) model to estimate the impact of having audited financial statements on *CoD*. The ES model is a two-equation instrumental variable estimator for situations where there is an endogenous binary treatment, audited or not, which, by using the above-mentioned Swedish reform to create our instrumental variable, makes it possible to identify how auditing affects firms' *CoD*. The only previous study to use an ES model when studying how auditing affects *CoD* is Minnis (2011). Thus, as a robustness test and to simplify comparisons to previous studies using other methods, we also estimate a less general variant of the ES model, a 2-stage least squares (2SLS) model, and a propensity score matching (PSM) model.

We will follow Huguet and Gandía (2014) and investigate if auditing reduces *CoD*, irrespective of if the auditing is voluntary or not. The reason for this is twofold. First, since Sweden has one of the lowest threshold levels for opting out of mandatory audits, restricting the sample to only those firms required to audit would exclude large parts (approximately 95% if measured as market share of sales) of the Swedish economy from the analysis. Second, since we use the regulatory thresholds for voluntary audits to create our instrument, it will be a valid instrument only if there are firms both below and above these thresholds in the data.

Our results show that auditing reduces *CoD* by, on average, 0.47 percentage points, indicating that audits are deemed to contain significant information by financiers. This can then be compared to Huguet and Gandía (2014) who report reductions of 0.18 percentage points in a setting similar to ours, while other studies report reductions in *CoD* due to voluntary auditing in the range 0.25 (Blackwell et al. 1998) to 1.24 (Kim et al. 2011) percentage points. In the voluntary auditing literature, one study that stands out in comparison is Koren et al. (2014) who report that voluntary audits increased *CoD* by 0.21 percentage points.

There has also been a discussion that audits made by the well-known BigN auditing firms are of special value for financiers as they have been deemed to be of higher quality than audits made by the average auditing firm.[1] We found that using a BigN auditing firm reduced *CoD* by 0.50 percentage points, and we thus conclude that using well-known auditing firms does not yield any substantial *CoD* benefits to the audited firms.

[1] The Big4 auditing firms consist of PricewaterhouseCoopers, Ernst and Young, Deloitte and KPMG. In Sweden, BDO and Grant Thornton are usually also included as big auditing firms that in addition to the Big4 make up the Big6 (Vourc'h and Morand 2011).

Lastly, we investigate if there are industries where alternative sources of information make auditing less valuable in reducing the cost of debt. First, based on the work of Dunn and Mayhew (2004), we investigate if the value of auditing is less in industries that are highly regulated and monitored by government agencies. We use utilities as our benchmark since this industry is the most heavily regulated industry in our sample. The results show that 12 out of 14 other industries saved more on *CoD* due to auditing compared to the utilities industry and 2 industries saved less. However, although 12 out of 14 industries had the predicted sign, all these effects are statistically insignificant.

Then, as an alternative to regulation and monitoring, we also investigate if auditing has less impact in industries where one lender has a high exposure to specific industries. The general idea is that lenders with experience of lending to firms within a certain industry should not need audited financial statements to the same extent as other lenders (Berger et al. 2017). In Sweden, agriculture is an industry where one of the main banks, Swedbank, has a 74% market share and over 100 years of experience in lending to that industry. Our results show that the reduction in *CoD* due to auditing is larger in all the other 14 industries compared to agriculture and that the effect is statistically significant at the 1% level for 6 industries and at the 10% level for 3 industries. As such, it seems that experience and exposure can act as an alternative to auditing.

Our study contributes to the literature in several ways: Firstly, ours is the first study of how auditing affects *CoD* in a strict regulatory environment. Previous studies have all used data from markets with more generous regulations regarding auditing and financial reporting than Sweden. Secondly, we provide unbiased estimates in a European private firm setting of how a financial statement audit, mandatory or not, affects *CoD*. With the exception of Huguet and Gandía (2014), ours is the first study to investigate the value of auditing, rather than voluntary auditing, on *CoD*. Thirdly, we contribute to the "if lenders look at audit choice or auditor choice or both" discussion, finding that audit choice is more important than auditor choice. Lastly, we provide some of the earliest evidence to show that for some industries there are alternative sources of information that make auditing less valuable in reducing firm level *CoD*.

The remainder of the paper is organized as follows. Section The swedish setting presents the Swedish setting, while Section Hypotheses development discusses prior literature and presents our hypotheses. Section Empirical analysis presents the data, the empirical models and our estimation results. Section Summary and discussion summarizes and discusses our results.

The swedish setting

All Swedish limited firms, irrespective of their size, have to produce and submit a financial statement to the SCRO (*Bokföringslagen 1999:1078, Chapter 6 § 1; Årsredovisningslagen 1995:1554, Chapter 8, § 1)*. These statements form the basis of taxation and are made available to the public by the same authority. The statement must, according to these laws, contain a balance sheet, an income statement, a management report, as well as notes explaining the accounting methods used and how important valuations have been computed. For larger firms,[2] the annual report must also contain a cash flow analysis. Failure to produce and submit the annual report within seven months after the end of the fiscal year will result in an initial fine of 5000 SEK (515 EUR). After an additional two months another fine of 5000 SEK will be issued, and after a further two months the fine increases to 10,000 SEK (1030 EUR).[3] If the annual report is still not produced and submitted after these three warnings and fines, the case can be handed over to a prosecutor where the violation then carries a maximum sentence of 2 years imprisonment.

This system can be compared to the USA, where the financial reporting of small, privately held businesses[4] is not regulated by the Securities and Exchange Commission (SEC), although small limited liability firms have been found to be more likely to produce financial statements for other reasons than regulation (Allee and Yohn 2009). Also, in the USA, privately held firms without audit requirements generally do not disclose their financial statements (Minnis 2011), something that is a requirement for all limited firms in Sweden, irrespective of size or audit requirements.

In the EU, the EU Fourth Company Law Directive (78/660/EEC) provides member states with the option to exempt SMEs from mandatory audits (European Economic Community 1978), an option which most countries took up. Sweden was for a long time a rare exception, with audits being a requirement for all Swedish limited firms, even the smallest ones. The Swedish legislation can be dated back to 1895 when the Companies Act was re-written, making

[2] Here, a firm is defined as "large" when the same two or three of the following criteria have been exceeded for two or more consecutive years: more than 50 employees, more than 40 million SEK (4.1 million EUR) in assets, more than 80 million SEK (8.2 million EUR) in net sales.

[3] Throughout the paper, all exchange rates used are from 2017–10-29.

[4] The SEC requires firms with total assets in excess of 10 million USD (8.6 million EUR) or having more than 500 shareholders to produce financial statements (Allee and Yohn 2009).

it mandatory for all limited firms to appoint independent auditors (Öhman and Wallerstedt 2012).[5]

In 2006, a center-right government was elected in Sweden and the newly elected government submitted the Bill, "A Voluntary Audit" (Prop. 2009/10:204), to the Swedish parliament (Sveriges Riksdag) on April 14, 2010, proposing that small firms should be allowed to choose whether they would be audited or not. The reform was justified on the grounds that the government wanted to reduce the regulatory burden on small firms, which was in line with the European Commission's plan[6] to reduce the administrative burden of SMEs by 25%. The bill was passed by the Swedish Parliament on June 21, 2010 (SFS 2010:834), and the new legislation allowed firms not exceeding certain thresholds to be exempted from mandatory audits.

Formally, the Swedish Companies Act (*Aktiebolagslagen* 2005:551) Chapter 9 § 1 still stipulates that—as a starting point—all Swedish limited firms are required to have an auditor audit their financial statements. The articles of association of a privately owned limited firm may, however, from November 1, 2010, specify that the firm need not have an auditor if at least two of the following conditions are met: (i) the average number of employees for each of the last two consecutive fiscal years amounts to no more than 3; (ii) reported total assets for each of the last two consecutive fiscal years amounts to no more than 1.5 million SEK (0.15 million EUR); or (iii) reported net sales for each of the last two consecutive fiscal years amounts to no more than 3 million SEK (0.3 million EUR).

The thresholds values in Sweden are considerably lower than in other EU countries (with Finland and Malta as the only exceptions). The corresponding median threshold values among the majority of the EU countries are 50 employees (3 in Sweden), total assets amounting to 2.5 million EUR (0.15 million Euros in Sweden) and net sales amounting to 3.5 million EUR (0.3 million Euros in Sweden).[7] For Korea, Kim et al. (2011) report that privately held firms are not required to have audited financial statements unless they have total assets in excess of 7 billion Korean Won (5.6 million EUR), while in the USA, according to Minnis (2011), auditing is voluntary for privately held firms regardless of employee numbers, assets and sales.

Hypotheses development

One of the best ways to describe the role of audit, and how it affects the *CoD* of borrowers, is through the lens of agency theory (Jensen and Meckling 1976; Watts and Zimmerman 1983).Even though agency problems are often more pronounced in larger firms, such problems may still persist in smaller firms which are also complex and diverse (Ang 1992; Eisenhardt 1989; Hope et al. 2012). SMEs also have high information asymmetries (Fenn 2000, Hope et al. 2012, Santos 2006), especially those that look to raise funds through external debt financing. As such, there exists a principal–agent relationship between the firm and the lender (Eisenhardt 1989; Pentland 1993; Power 1999); firms that look to raise funds through external debt will actively seek ways to improve the quality of their accounting information (Burgstahler et al. 2006), thus reducing the information asymmetry between the firm and the lender (Jensen and Meckling 1976).

Empirically, Clatworthy and Peel (2013) found that unaudited financial statements of private firms were twice as likely to contain accounting errors compared to audited financial statements. Indeed, high-quality accounting information has been shown to be a more important factor than stipulations in debt contracts in the determination of *CoD* (Spiceland et al., 2016), and variation in the quality of accounting information is significantly captured in private debt pricing (Bharath, Sunder, and Sunder, 2008; Baylis et al., 2017). Furthermore, there is some empirical evidence that auditing reduces private firms' *CoD* (Blackwell et al. 1998; Huguet and Gandía 2014; Kausar et al. 2016; Kim et al. 2011; Minnis 2011). These findings are usually motivated by the argument that auditing increases the information quality of the financial statements issued by the firms, for example, by increasing the ability of the financial statements to predict future cash flow and thus the ability to repay loans (Minnis 2011). The size of the effects differ in these studies. Blackwell et al. (1998) report a 25-basis point reduction in *CoD* due to auditing, Kim et al. (2011) report reductions of between 0.55 and 1.24 percentage points, while Minnis (2011) reports reductions of between 0.25 to 1.05 percentage points, with a reduction of 0.69 percentage points in the main model used. Finally, Huguet and Gandía (2014) report a reduction in *CoD* of 0.18 percentage points, while Kausar et al. (2016) report reductions between 0.30 and 0.80 percentage points.

From an agency theory point of view, audited financial statements should reduce firms' *CoD* because they reduce the information asymmetry between the firm and the lender (Jensen and Meckling 1976). With the few empirical papers examining the effect of audited financial statements on *CoD* identifying a negative relationship (Blackwell et al. 1998;

[5] Voluntary audits can be dated back to the 1650 s in Sweden, and official, but still voluntary, audits are also mentioned in the Companies Act of 1848.

[6] As per the EU Fourth Company Law Directive (78/660/EEC). Reports from the European Commission (2011) highlighted the importance of SMEs for the European economy and called for a more business-friendly environment for SMEs, including microfirms, so that they could become more competitive in the global economy.

[7] See Appendix B, Table 9, for further information.

Huguet and Gandía 2014; Kausar et al. 2016; Kim et al. 2011; Minnis 2011), our first hypothesis becomes:

H1 Firms that are audited will have a lower cost of debt compared to firms that are not audited, all else being equal.

Because information about which firms audit their financial statements and which do not is generally scarce, the most common approach in previous literature has been to use data where all firms are required to audit their financial statements, and to focus instead on the characteristics of the auditors (Minnis 2011). One prominent line of research has then been to focus on the quality of audits, often proxied by the size of the auditor. The theoretical background on why larger auditing firms should produce higher quality audits dates back to DeAngelo (1981), who suggested that large auditing firms have more to lose in case of misreporting, forcing them to pay more attention and conduct higher quality audits than smaller auditors do.

Prior literature testing this theory suggests that using well-known BigN auditors[8] does improve the quality of financial statements (Becker et al. 1998; DeFond and Jiambalvo 1991; Teoh and Wong 1993), and that better accounting quality also results in lower *CoD*, at least for publicly traded companies (Mansi et al. 2004; Pittman and Fortin 2004). Causholli and Knechel (2012) also report that firms going public for the first time enjoy lower *CoD* when employing BigN auditors.

There is not much evidence to show if private firms that largely depend on private debt for external funding benefit from an audit by BigN auditors or not, except for some recent studies. Fortin and Pittman (2007) conclude that private firms do not benefit from better yield spreads or credit rating of public debt from the retention of a BigN auditor, and both Kim et al. (2011) and Huguet and Gandía (2014) report that the appointment of a BigN auditor does not reduce *CoD* for these audited firms.

On the other hand, Karjalainen (2011), studying how auditing quality affected private firms' *CoD* in Finland, found that audits by BigN auditors tended to decrease *CoD*. Finland shares several characteristics with the Swedish market being studied in this paper, both with regards to history and regulation (Hyytinen and Pajarinen 2001). Both countries have bank-centered financial systems and, before the 1980s, both countries had heavily regulated financial markets with authorities regulating both quantities and rates of lending from the banks. In the mid-1980s, the banking sector in both Finland and Sweden were deregulated, which lead to rapid credit expansion and a banking crisis in the beginning of the 1990s, forcing a consolidation of the banking sector in both counties. After this consolidation, the ratio of bank lending to GDP has also followed similar patterns in Finland and Sweden (Hyytinen and Pajarinen 2001). With respect to regulation, both countries have similar legal protection for minority shareholders and creditors and a high quality of law enforcement regarding these issues (Hyytinen and Pajarinen 2001; La Porta et al. 1998). Also, Finland and Sweden have very similar rules allowing a firm to opt out of auditing (see Table 9, Appendix B), and the option to opt out of auditing was implemented within a four year period in both countries (2007 in Finland and 2010 in Sweden).

Although the empirical evidence regarding the impact of auditing by a BigN firm on *CoD* is mixed, the similarities between the Swedish and Finnish private firm-settings in our opinion makes it likely that the impact of BigN auditing in Sweden will be similar to that in Finland, and as such, we hypothesize:

H2 Firms that chose to be audited by BigN auditing firms have a lower cost of debt compared to firms that audit in general, all else being equal.

Until now, we have assumed that the option to opt out of auditing is of similar importance across all affected industries and that it thus will have similar effects in different industries. However, in practice, both lenders and firms in the affected industries might have characteristics that act as alternatives to auditing with respect to information gathering about the financial well-being of firms, meaning that the ability to opt out of auditing will have less of an impact on *CoD* in these industries. In this study, we will analyze two possible alternative pathways to gathering information, making auditing less important: a high level of industry regulation and oversight, and lenders having high exposure to a specific industry.

One possible cause of industry differences in how auditing affects *CoD* is that the value of audited information should be lower in industries where there are alternative sources of information, thus reducing the information asymmetry between the firm and the lender. Dunn and Mayhew (2004) investigate how having financial statements audited by an industry specialist auditor affects disclosure quality and finds that having an industry specialist auditor increases the disclosure quality of the financial statements issued by the firms, except in the case of regulated industries. In regulated industries, regulation often requires high levels of disclosure (financial, but sometimes also related to environmental, safety and health regulations), and there is also often a high level of monitoring by government agencies of adherence to these regulations. As such, Dunn and Mayhew (2004) find that the value of auditing is diminished by regulation and monitoring by government agencies.

[8] Since some of these studies use Big4 and others Big6 or Big8, we use BigN.

Thus, from an agency theory point of view, audited financial statements should be of less value to the lenders in regulated industries since monitoring and regulatory oversight provides an alternative source of financial (and other) information regarding firms in that industry. As such, our next hypothesis becomes:

H3a: Auditing will have less impact on cost of debt in heavily regulated industries than in less regulated industries, all else being equal.

The industry most commonly mentioned in the literature as being heavily regulated is the utilities sector (Carcello et al. 2002; Casterella et al. 2004; Palmrose 1986; Simunic 1980). In Sweden, there are several government agencies monitoring regulation in the utilities industry, the largest and most important agency being the Swedish Energy Markets Inspectorate (SEMI). They provide oversight of the Swedish markets for electricity, gas and district heating. Firms active in these industries are required to hand in their annual reports to SEMI, as well as to SCRO, and SEMI then performs audits on some of the submitted annual reports. However, in addition to the information in the annual reports, SEMI also requires that firms submit price information regarding different types of contracts for heating or electricity and then makes this information publicly available, for example, through the electricity price comparison website Elpriskollen (www.elpriskollen.se). As such, the level of financial statement and pricing transparency is higher for utilities than other industries in Sweden, which means that we will use utilities as our benchmark industry regarding high regulation and government agency monitoring in the empirical section below.[9]

A second possible cause of industry differences in how auditing affects *CoD* is when lenders already have high exposure to an industry, the need for audited financial statements goes down (Berger et al. 2017). This is because the degree of exposure to an industry affects how banks interact with firms in that industry (Dell'Ariccia et al. 1999, Winton 1999). Banks with less exposure to a particular industry might demand more verified information before contracting with borrowers in that industry, suggesting a negative relationship between exposure and the need for verified financial information (Berger et al. 2017). Also, if high exposure allows a bank to gather superior information about an industry over time, this negative relationship will strengthen as the bank accumulates information about the specific industry (Berger et al. 2017). This is also verified empirically by Berger et al. (2017), who found that a one standard deviation increase in a bank's exposure to an industry reduced the collection of audited financial statements by 2.4 percentage points and that there was a negative relationship between a bank's experience in contracting with a certain industry and the collection of audited financial statements.

The Swedish credit market is dominated by four large banks.[10] Based on the loan exposure disclosed in the annual reports of these banks, we found that the agriculture industry is dominated by Swedbank, one of the four large banks, which has a 74% market share. Swedbank's dominance in agriculture is due to its merger in 1992 with Föreningsbanken, established in 1915 as a special interest bank for farmers and forest owners in Sweden. When the banks merged, Swedbank took over much of Föreningsbanken's existing business. As such, agriculture is an industry where we would expect the need for audited financial statements to be lower than in other industries and where firms would not have to pay a large *CoD* penalty if opting out of auditing.

It should also be noted that the agricultural industry is not heavily regulated compared to utilities and that lending to the utilities industry is quite evenly distributed among the four large banks according to their annual reports (all having market shares between 18 and 32%), thus making it possible to disentangle the effects of regulation from that of bank exposure to an industry. As such, we focus on agriculture as one of our two benchmark industries and so hypothesis 3b can be written as:

H3b The magnitude of the effect of audits on cost of debt will vary across industries due to differences in the level of exposure to an industry by the lenders, with less impact of audited financial statements on cost of debt in industries where one lender has a large market share.

Empirical analysis

Data

The data for this study are collected from the Retriever database, containing corporate information of all registered limited liability (both listed and non-listed) firms in Sweden. It should be noted that while having access to annual report data for the years 2007 to 2014, we only have information about firms that were audited for one year, 2013, and that

[9] As their benchmarks Butterworth and Houghton (1995) use mining, while Chen and Elder (2001) use banks and oil and natural gas. We chose to use utilities because both the mining and oil and natural gas industries have very few active firms, while banks are guided by special regulations regarding auditing in Sweden.

[10] According to the balance sheet total, the four largest banks in Sweden are SEB, Svenska Handelsbanken, Nordea Bank, and Swedbank, respectively. Total combined lending to public and corporate borrowers as of December 31, 2013, is 74.1% of the total market, which is divided as 32%, 21.7%, 9.7%, and 10.9%, respectively (Sveriges Riksbank 2016, Swedish Banker's Association 2014).

Table 1 Sample selection process

Total no. of firm year information retrieved from the database	489 131
Total no. of firms registered before 2006–12-31 and active until 2013–12-31	202 883
Excluding listed firms	(484)
Excluding firm with no industry classification	(15 120)
Excluding finance and insurance firms & government organizations	(8 757)
Excluding firms with less than 500 observation within an industry	(319)
Excluding firms whose PPE exceeds total assets	(957)
Excluding firms with missing observation on interest expense and debt	(2 460)
Excluding firms with missing observation on audit	(201)
Excluding firms with missing observations for some independent variables	(19 325)
Final sample of unique firms	155 260

this is thus a cross-sectional study.[11] Our estimations are run on a sample of 155 260 surviving firms in 2014 which were registered prior to December 31, 2006. Our study is thus based on surviving firms over that period, and inference cannot be drawn for firms that made an entry or exit during the study period. We arrived at the dataset used in the regression analysis in the following manner (Table 1):

The database contains historical financial information on 489 131 firms, including many inactive firms that were eliminated from our sample. We eliminated firms registered after December 31, 2006, since our calculation of the instrument used to address issues of self-selection and endogeneity requires data for the number of employees, total assets and total sales for the financial years 2007 and 2008 to calculate the instrument for the fiscal year 2009, one year before the actual implementation of the reform. Next, we eliminated firms that were not active after December 31, 2014, by only including firms that had submitted a financial report for the year 2014. Then, we excluded firms in the finance and insurance industry and in public administration since they are subject to different rules and regulations regarding auditing compared to firms in other industries. Furthermore, we eliminated listed firms since they are still subject to mandatory audit in Sweden, and thus not affected by the 2010 audit reform. Finally, we removed firm observations where calculated *CoD* was missing and/or where the total reported value of plant, property and equipment was more than the reported value of total assets. These actions resulted in a final dataset containing 155 260 observations.

Empirical models and descriptive statistics

The identification of how auditing affects a firm's *CoD* is quite difficult because there exists a simultaneous relationship between the firm's *CoD* and its decision to audit their financial statements (Huguet and Gandía 2014; Koren et al. 2014; Minnis 2011). Due to this endogeneity problem, OLS estimation will be biased because the indicator variable for being audited will be correlated with the error term. To take this problem into consideration, we will estimate endogenous switching (ES) regression models to investigate the impact of having audited financial statements on *CoD*, while using two-stage least squares (2SLS) regression models and propensity score matching (PSM) models to confirm that our results are robust with respect to the estimation method used.

In this section, we will assume that having the option to opt out of auditing is of similar importance for firms in all affected industries and that we have one binary treatment given by the legal requirements for opting out of auditing.[12] Since the ES model is based on having a binary endogenous treatment, our main model will be the ES model. The ES model is a two-equation regression model, composed of one equation for the outcome, *CoD*, and one equation for the binary endogenous treatment, *Audited*. While Minnis (2011) uses a three-equation variant of the endogenous switching model, we opt for a two-equation version and estimate this using the *etregress* command in STATA 15. This model addresses the endogeneity issue while also allowing the parameter estimates of the covariates in the outcome equation to differ between audited and unaudited firms, just as in the Minnis (2011) model though less burdensome to estimate. More specifically, we have the following equations;

[11] We selected 2013 because of data availability. Data on the firms that chose to audit were originally collected for another project; incorporating information on additional years in which firms chose to audit was found to be prohibitively expensive. We therefore decided to carry out a cross-section analysis using the readily available data for 2013.

[12] In later sections, we will lift this restriction and instead assume that there might be industry differences in the decision to gather high-quality financial information and estimate one treatment effect for each industry compared to the baseline industries, utilities and agriculture, discussed above.

$$
\begin{aligned}
CoD_i = {} & \beta_0 + \beta_1 Audited_i + \beta_2 ICR_i + \beta_3 Audited_i \\
& \times ICR_i + \beta_4 LR_i + \beta_5 Audited_i \times LR_i \\
& + \beta_6 PPEshare_i + \beta_7 Audited_i \times PPEshare_i \\
& + \beta_8 Leverage_i + \beta_9 Audited_i \times Leverage_i \\
& + \beta_{10} Ln\_TA_i + \beta_{11} Audited_i \times Ln\_TA_i \\
& + \beta_{12} Growth_i + \beta_{13} Audited_i \times Growth_i \\
& + \beta_{14} ROA_i + \beta_{15} Audited_i \times ROA_i \\
& + \beta_{16} Neg.Equity_i + \beta_{17} Audited_i \\
& \times Neg.Equity_i + \beta_{Industry} + u_i
\end{aligned} \tag{1}
$$

and

$$
\begin{aligned}
Audited_i = {} & \pi_0 + \pi_1 Reform2010_i + \pi_2 AudBig6_i \\
& + \pi_3 ICR_i + \pi_4 LR_i + \pi_5 PPEshare_i \\
& + \pi_6 Leverage_i + \pi_7 Ln\_TA_i \\
& + \pi_8 Growth_i + \pi_9 ROA_i + \pi_{10} Neg.Equity_i \\
& + \pi_{Industry} + \varepsilon_i
\end{aligned} \tag{2}
$$

where Eq. (2) also includes our instrument, *Reform 2010,* that should be strongly associated to the choice to audit, but not directly associated to the outcome, firm *CoD*. In the model, β and π are vectors of parameters to be estimated, and the ES model simultaneously estimates Eqs. (1) and (2) under the assumption that the errors u_i and ε_i are bivariate normal with zero mean. The variables used in the estimation, including the instrument, are described in detail below. To limit the influence of extreme values, *CoD* and all other variables are winsorized at 1% and 99%.[13]

Since there are interactions between the treatment indicator variable and the covariates, the estimated coefficient for *Audited* is not the treatment effect. Instead, the *margins* command in STATA 15 is used to calculate the difference in *CoD* between audited and unaudited firms in the full sample, thus testing hypothesis 1.

To test hypothesis 2, we exclude firms that are being audited by non-BigN audit firms from the sample and re-estimate the model described above. If hypothesis 2 is correct, we expect a larger negative impact of auditing on *CoD* in this sub-sample than in the original sample.

To test hypotheses 3a and 3b, we continue to use the model described by Eqs. (1) and (2), but now we also interact the industry indicator variables with the *Audited* variable to create an audit indicator variable for each industry in the dataset. Assuming that the choice of industry by the firms does not depend on the auditing reform, additional instruments were created by interacting the instrumental variable *Reform 2010* with the industry indicator variables, creating one instrument for each industry. Since we now have more than one endogenous treatment, we use a 2SLS model instead of the ES model to estimate Eqs. (3) and (4), and this gives us a parameter estimate of how auditing affects *CoD* for each industry, which we can then compare to any benchmark industry we choose. By setting up the model in this manner, we are able to test if there are statistically significant differences between our benchmark industries and others in the size of the effect of auditing on *CoD*.

Cost of Debt (CoD): The dependent variable *CoD* is calculated following Minnis (2011) and Huguet and Gandía (2014). In our study, it is calculated as reported external interest expenses divided by debt. The debt variable is calculated as the average total of bonds, long-term loans from credit institutions, other long-term liabilities, short-term liabilities from credit institutions and other short-term liabilities at the beginning and end of 2013, all of which are supposed to be interest bearing.[14] The average debt for the full sample is 5.8 million SEK (0.6 million EUR), while the median is 0.7 million SEK (0.07 million EUR). The average debt for audited firms is 7.7 million SEK (0.8 million EUR) and for unaudited firms is 0.5 million SEK (0.05 million EUR), while the medians are 1.2 million SEK (0.1 million EUR) and 0.2 million SEK (0.02 million EUR), respectively.[15]

The average *CoD* for the full sample is 2.48% with a median of 1.24%, while the mean (median) *CoD* for audited and unaudited firms is 2.54% (1.51%) and 2.32% (0.60%), respectively. The mean (median) across the various industries for both audited and unaudited firms ranges between 1.64% and 3.27% (0.22% and 2.82%). The mean (median) for audited firms across the various industries ranges between 1.51% and 3.51% (0.28% and 2.83%) and for unaudited firms across the various industries ranges between 1.77% and 3.01% (0.00% and 2.75%).

*Audited*An indicator variable equal to one if the firm was audited in 2013 and zero otherwise is our endogenous binary treatment variable to be instrumented. In our sample, approximately 72.7% of the firms audited their financial statements in 2013, while 26.3% did not. Our endogenous variable *Audited* will be instrumented using the variable *Reform 2010,* which is an indicator variable equal to one for firms that fulfilled

[13] Winsorization is one of the most common methods used to deal with outliers in accounting (Leone et al. 2017) and finance (Adams et al. 2018) studies.

[14] Interest can in some cases be capitalized in Sweden, but if the interest expense is capitalized, it will later be depreciated/amortized, so the expense will eventually find its way to the income statement (through accruals). s this applies to both audited and un-audited firms alike, it should not cause any bias in the estimations of the value of auditing on *CoD*.

[15] The mean (median) debt changes in percentages for the full sample from 2011 to 2012, 2012 to 2013, and 2013 to 2014 are –0.04 (–0.02), –0.06 (–0.03), and –0.06 (–0.03), respectively. The same figures for audited and unaudited firms are −0.02 (–0.02), –0.05 (–0.02), and –0.04 (–0.02); and –0.11 (–0.05), –0.11 (–0.05), and –0.10 (–0.04), respectively. There has been a general downward trend in debt levels during these years.

the requirements of the 2010 audit reform in the fiscal year 2009 based on information regarding the average number of employees, total assets and net sales during the two preceding fiscal years, 2007 and 2008, and equal to zero otherwise. As such, the instrument is equal to one for firms where at least two of the following conditions are met: (i) the average number of employees for either 2007 or 2008 amounts to no more than 3; (ii) reported total assets in neither 2007 nor 2008 amounts to no more than 1.5 million SEK (0.15 million EUR); or (iii) reported net sales in neither 2007 nor 2008 amounts to no more than 3 million SEK (0.3 million EUR).

Fulfilling the requirements of the audit reform clearly influences the likelihood that the firm will be audited, while being below or above the mandatory audit threshold level of the reform in 2009 should not in itself influence the firm's cost of debt in 2013 after controlling for firm size and other relevant exogenous variables.

Note also that the reform was passed in the Swedish parliament on June 21, 2010, and became effective on November 1, 2010. Since the firms were unaware of the reform and its requirements at the time we measured the instrumental variable, there is no reason to believe that firms could have adapted their behavior to the reform, and therefore, it is unlikely that our instrument is correlated with the error term of the ES regression. In our sample, 51.7% of the firms would have been below the mandatory audit threshold level had the reform been introduced in 2009, while 48.3% of the firms were below the mandatory audit threshold levels in 2013.

Following Minnis (2011), Kim, et al. (2011), and Koren et al. (2014), we also include control variables, such as liquidity ratio (*LR*), interest coverage ratio (*ICR*), plant, property and equipment share (*PPE_share*), debt by equity (*Leverage*), log of total assets (*Ln_TA*), sales growth (*Growth*), return on assets (*ROA*), and an indicator variable equal to one for firms with negative equity (*Neg. Equity*). To reduce the risk of these variables being correlated with the error term of the regression equation, we use one-year lagged values to measure these variables. Lastly, to control for possible industry differences in the average *CoD*, we also include industry indicator variables.

*Interest coverage ratio (ICR)*The variable was reported in the database and is calculated as earnings before interest, taxes, depreciation and amortization divided by interest expense. In 2012, the average ICR for audited firms was 74.73, while for unaudited firms it was 18.30.

*Liquidity ratio (LR)*The variable was reported in the database as the ratio of current assets over current liabilities. In 2012, the average LR for audited firms was 2.84, and for unaudited firms it was 4.88.

*Plant, property and equipment share (PPE_share)*This variable is calculated by adding firms' holdings of land and buildings, machinery and equipment and dividing by total assets. Observations for which the reported plant, property and equipment value were higher than the reported value of total assets were excluded from the analysis. In 2012, audited firms, on average, had 23% of their total assets as PPE, while unaudited firms had a PPE share of 17% of the total assets.[16]

*Leverage*This variable is calculated as total debt divided by total assets. The average leverage in 2012 of an audited firm was 0.56, while for an unaudited firm it was 0.61.

*Total Assets (Ln_TA)*This variable was reported in the database, and following previous studies (Karjalainen 2011; Minnis 2011), we included the natural log of (1 + total assets) in the model. For the year 2012, the natural log of assets for audited and unaudited firms was 15.41 and 13.47, respectively.

*Growth*This variable is calculated as yearly relative sales growth from year t-1 to year t. Audited firms had on average growth rates of -0.02%, while unaudited firms had growth rates of –0.13% during the period from 2011 to 2012. Regarding firm growth it has been shown previously that most Swedish firms, on average, do not grow (Bornhäll et al. 2014). On some occasions negative average firm growth has also been reported for Swedish annual report data such as ours (Daunfeldt et al. 2013).

*Return on Assets (ROA)*Return on assets was calculated as reported net income over reported total assets. In 2012, audited firms, on average, had a ROA of 4.06%, while it was 0.90% for unaudited firms.

*Negative Equity (Neg. Equity)*This is an indicator variable equal to one for firms that have negative equity, and zero otherwise. In 2012, 2.5% of the audited and 7% of the unaudited firms had negative equity.

Table 2 presents descriptive statistics and variable descriptions for all variables in the analysis, while Table 3 reports descriptive statistics of the two sub-groups: audited and unaudited firms.[17]

*Industry classification*To capture industry level heterogeneity, indicator variables for different types of industry have been created according to the first two digits of the

[16] As an alternative measure, we in some regressions use the natural log of collateral since this variable was readily available in our dataset. The results from these estimations are similar to those reported below.

[17] To investigate if multicollinearity might be a problem in our estimations, we also present statistics on how high the correlations between the variables are. See Table 10 in Appendix C. The correlations are, in most cases, low enough to make it unlikely that our results are affected by multicollinearity. One high correlation stands out, and that is between ROA and Leverage. However, removing these potentially collinear variables one by one does not alter the results regarding how being audited affects *CoD*, and thus, we choose to include both these variables in the estimations presented in this paper.

Table 2 Descriptive statistics for the full sample (155 260 unique firms)

Variable	Mean	Median	Max value	Min. value	SD	Description
CoD	2.4789	1.2435	37.378	0.0000	4.1731	Cost of debt is the dependent variable reported in percentage for the year 2013 and calculated as explained in Sect. Empirical models and descriptive statistics
Audited	0.7263	1.0000	1.0000	0.0000	0.4459	This is an indicator variable equal to 1, if the firm was audited in the financial year 2013, and zero otherwise
Reform2010	0.5177	1.0000	1.0000	0.0000	0.5000	This is an indicator variable used to instrument the variable of interest "Audited." The value equals zero if a firm was below the statutory audit threshold and equals one if a firm is above the statutory audit threshold in 2009. Thus, a value of zero means a firm was not subject to statutory audit in 2009, and a value of one means the firm was subject to statutory audit in 2009
ICR	59.285	2.8700	1946.0	–207.00	233.20	Interest coverage ratio calculated on EBIDTA for the year 2012. Control variable for the firm's ability to pay future interest expenses
LR	3.4005	1.5420	66.667	0.0020	6.7251	Liquidity ratio for the year 2012. Control variable for the liquidity of a firm
PPE share	0.2132	0.0526	0.9795	0.0000	0.2905	Share of total plant, property & equipment of the total assets for the year 2012. Control variable for tangibility of the firm
Leverage	0.5698	0.5262	7.6047	0.0103	0.5504	Leverage calculated as total debt divided by total assets for the year 2012. Control variable for the financial risk of the firm
Ln_TA	14.878	14.767	20.391	10.204	1.7638	Natural log of (1 + total assets) for the year 2012. Control variable for firm size
Growth	-0.0475	0.0000	2.3590	–3.1604	0.5512	Growth is measured as year-over-year sales growth between 2011 and 2012. Control variable for the business risk of a firm
ROA	3.1918	3.0509	86.058	–166.67	20.269	Return on assets calculated as net income divided by total assets for the year 2012. Control variable for the profitability of a firm
Neg. Equity	0.0375	0.0000	1.0000	0.0000	0.1901	An indicator variable equal to 1, if a firm has negative equity and, zero otherwise in the year 2012. Control variable for financial distress
Debt (Interest bearing)	5768.7	729.00	193,762	1.0000	21,345	Reported in thousands of Swedish krona. Is calculated as the average of bonds, long-term loans from credit institutions, other long-term liabilities, short-term liabilities from credit institutions and other short-term liabilities at the beginning and end of 2013

Dependent variable *CoD* is winsorized at 1% and 99%. Accounting and financial control variables: *ICR, LR, PPE share, Leverage, Ln_TA, Growth, ROA,* and *Neg. Equity* are lagged by one year to avoid a potential endogeneity problem. Furthermore, continuous control variables *ICR, LR, PPE share, Leverage, Ln_TA, Growth,* and *ROA* are winsorized at 1% and 99% to alleviate any possible effect of extreme outliers. Interest bearing debt is also winsorized at 1% and 99%

firms' NACE classification code.[18] Some summary statistics by industry are presented in Table 4:

Firms from industries such as manufacturing, construction, retail and wholesale, real estate, and professional services account for about 71% of the total sample. The benchmark industries used to test hypotheses H3a and H3b, utilities and agriculture, constitute 0.8% and 3.1% of the total sample, respectively. Other major industries include transportation (5.2%), information and communication (5.7%), renting real estate (3.5%) and hotel and restaurants (3.1%).

Robustness tests using constrained ES, 2SLS and PSM

As alternatives to the ES model presented in Eqs. (1) and (2) above, we also estimate an ES model where we constrain the parameter estimates of the covariates in the outcome equation to be similar for audited and unaudited firms, a 2SLS regression model, and a propensity score matching (PSM) model. The constrained ES and 2SLS models are based on the following two equations:

[18] The NACE code system is used by the EU for industry classification and has a similar function as the United Nations' International Standard Industrial Classification of all Economic Activities or the North American Industry Classification System.

Table 3 Descriptive statistics for the split samples: Unaudited versus audited firms

Variables	Unaudited firms in 2013 (42 495 unique firms)					Audited firms in 2013 (112 765 unique firms)				
	Mean	Median	Max value	Min. value	SD	Mean	Median	Max value	Min value	SD
Audited	0.0000	0.0000	0.0000	0.0000	0.0000	1.0000	1.0000	1.0000	1.0000	0.0000
CoD	2.3206	0.5970	36.378	0.0000	4.3675	2.5386	1.5075	36.378	0.0000	4.0959
Reform2010	0.8825	1.0000	1.0000	0.0000	0.3219	0.3806	0.0000	1.0000	0.0000	0.4854
ICR	18.303	0.0000	444.00	–123.00	69.825	74.729	4.0900	1946.0	–207.00	268.64
LR	4.8789	2.0660	66.667	0.0020	9.4447	2.8445	1.4120	39.036	0.0240	5.2448
PPE share	0.1657	0.0207	0.9684	0.0000	0.2671	0.2311	0.0686	0.9795	0.0000	0.2969
Leverage	0.6084	0.4323	7.6047	0.0103	0.9357	0.5552	0.5575	1.5876	0.0191	0.2940
Ln_TA	13.468	13.473	16.350	10.204	1.1816	15.410	15.277	20.391	11.824	1.6508
Growth	–0.1277	–0.0401	2.3590	–3.1604	0.7550	–0.0173	0.0050	1.6749	–2.0564	0.4474
ROA	0.8990	2.7006	86.058	–166.67	30.200	4.0558	3.1451	50.694	–67.256	14.807
Neg. Equity	0.0707	0.0000	1.0000	0.0000	0.2564	0.0250	0.0000	1.0000	0.0000	0.1562
Debt (Interest bearing)	526.13	201.00	6796.0	1.0000	1015.5	7744.3	1256.0	193,762	14.000	24,752

Dependent variable *CoD* is winsorized at 1% and 99%. Accounting and financial control variables: *ICR, LR, PPE share, Leverage, Ln_TA, Growth, ROA,* and *Neg. Equity* are lagged by one year to avoid a potential endogeneity problem. Furthermore, continuous control variables *ICR, LR, PPE share, Leverage, Ln_TA, Growth,* and *ROA* are winsorized at 1% and 99% to alleviate any possible effect of extreme outliers. Interest bearing debt is also winsorized at 1% and 99%

$$CoD_i = \beta_0 + \beta_1 Audited_i + \beta_2 AudBig6_i + \beta_3 ICR_i + \beta_4 LR_i + \beta_5 PPEshare_i + \beta_6 Leverage_i + \beta_7 Ln\_TA_i + \beta_8 Growth_i + \beta_9 ROA_i + \beta_{10} Neg.Equity_i + \beta_{Industry} + u_i \quad (3)$$

and

$$Audited_i = \pi_0 + \pi_1 Reform2010_i + \pi_2 AudBig6_i + \pi_3 ICR_i + \pi_4 LR_i + \pi_5 PPEshare_i + \pi_6 Leverage_i + \pi_7 Ln\_TA_i + \pi_8 Growth_i + \pi_9 ROA_i + \pi_{10} Neg.Equity_i + \pi_{Industry} + \varepsilon_i \quad (4)$$

where all variables are identical to the ones used in the estimations of the unconstrained ES model presented above. The main differences between the constrained ES model and the 2SLS model are that the 2SLS does not estimate both regressions simultaneously and that the estimation of the treatment equation in the 2SLS is made using a linear probability model rather than a probit regression.

As an additional robustness test, we also use propensity score matching (Rosenbaum and Rubin 1983). In PSM models, the focus is to match firms in the treated group, that is, those that are audited, with similar firms in the control group based on observables. Since there are in most cases multiple dimensions across which treated and control group firms can differ, Rosenbaum and Rubin (1983) suggest that the matching be done using the probability of treatment conditional on the variables determining treatment, that is, the "propensity score." As such, regression-based models and PSM both address potential bias by controlling for observables relating to outcome and treatment, but the PSM has the advantage of comparing firms that are similar across the independent variables, thereby relaxing assumptions about functional form (Shipman et al. 2017).

Following Minnis (2011), we start by creating the propensity score for each firm in the treatment and control groups using the conditional probability of being audited from a logit regression model,[19] excluding the exogenous instrument but otherwise identical to Eq. (4). Then, to increase efficiency in the treatment effect estimate, we match each treated firm with three firms from the control group, with replacement, using a caliper of 0.2. Finally, the average difference in *CoD* for audited and unaudited firms is calculated along with its p-value.

Estimation results

Results from the ES and 2SLS estimations of Eq. (1), using *Reform2010* as an instrument for *Audited,* are presented in Table 5, columns 1 through 5, while the estimated ATE from the PSM method is presented in column 6. More precisely, columns 1 through 3 in Table 5 contain the results from our main model, the un-constrained ES model, while columns 4 through 6 in Table 5 present the results from the robustness tests using the constrained ES model, the 2SLS model and the PSM model.[20] The estimation results for the binary

[19] The PSM model has also been estimated using a probit rather than logit model with similar results.

[20] In addition to the robustness checks presented in Table 5, we have also performed several other tests to verify the results presented in this section. We have, for example, used cut-off points of *CoD* instead of winsorizing to reduce the impact of influential observations, and

Table 4 Sample distribution and descriptive statistics of dependent variable *CoD* by industry

Industry	No. of firms	Percentage of total sample	Audited firms in 2013 (%)	CoD (Mean), Full sample	CoD (Median), Full sample	CoD (Mean), Unaudited firms	CoD (Median), Unaudited firms	CoD (Mean), Audited firms	CoD (Median), Audited firms
Agriculture	4.780	3.08	70.5	2.8161	2.5136	2.5010	1.5855	2.9477	2.7249
Manufacturing	15.365	9.90	80.4	2.8006	1.8717	2.7309	1.0639	2.8176	2.0290
Utilities	1.162	0.75	86.6	3.3698	2.3737	2.5380	2.7520	3.5045	2.3076
Construction	18.222	11.70	73.4	2.2344	1.2853	2.4647	1.1364	2.1509	1.3464
Retail and wholesale	30.580	19.70	77.6	2.9206	1.6496	3.0049	0.9917	2.8966	1.8007
Transportation	8.103	5.22	77.0	2.8367	2.5527	2.9380	2.2966	2.8064	2.5910
Hotel & restaurants	4.833	3.11	79.5	2.5433	1.2552	2.7364	1.0050	2.4933	1.3197
Information and communication	8.838	5.69	66.0	1.7664	0.2174	1.7716	0.0000	1.7637	0.2826
Real estate	15.912	10.25	82.3	3.2662	2.8065	2.8408	2.6264	3.3579	2.8337
Professional services	29.863	19.23	60.6	1.9018	0.3145	1.7860	0.0000	1.9770	0.4301
Renting and leasing	5.446	3.51	74.4	2.1672	0.6728	2.2435	0.5566	2.1409	0.7175
Training	2.283	1.47	68.1	1.6362	0.3197	1.8982	0.1634	1.5135	0.3453
Health care and social services	4.688	3.02	65.3	1.7806	0.4253	1.7709	0.0000	1.7858	0.5442
Culture and recreation	3.139	2.02	60.1	2.1295	0.4894	1.8052	0.0000	2.3446	0.7813
Other service activities	2.046	1.32	59.3	2.0630	0.6591	2.3274	0.5249	1.8814	0.7117
Total	155.260	100	72.7	2.4789	1.2435	2.3206	0.5970	2.5386	1.5075

treatment regressions, i.e., Eq. (2), are presented in Table 8 in Appendix A.

We favor the unconstrained ES model over the others since this model has the most general specification. The estimation results clearly show that the parameter estimates of the covariates are statistically different depending on if the firms are audited or not. The results from the unconstrained ES regression indicate that firms with audited financial statements on average have 0.47 percentage points lower *CoD* than firms with unaudited statements and that this difference is statistically significant at the 1% level. As such, we find that our first hypothesis is supported.

The sign of the effect of audit on *CoD* is consistent across all robustness checks (that is, constrained ES, 2SLS and PSM). What varies is the size of the effect and that the PSM result is not statistically significant at conventional levels. Our results are also largely in line with previous studies. The point estimates presented in Table 5 for our different models range from -0.28 (PSM, not significant) to -0.83 percentage points (constrained ES, significant at the 1% level). Results from previous studies range from –0.18 (Huguet and Gandía 2014) to –1.24 percentage points (Kim et al. 2011), and the results most similar to ours are those from Minnis (2011) who reports reductions of *CoD* due to auditing in the range –0.55–1.05 percentage points, with a reduction of 0.69 percentage points in the main model used; and Kausar et al. (2016) who report reductions of *CoD* due to auditing

Footnote 20 (continued)

this has been done in 6 steps: 15, 20, 30, 50, 100, and up to 150% *CoD*. We have also used a logarithmic transformation of the dependent variable *CoD* since it has a somewhat skewed distribution. In all of these estimations, available from the authors on request, the results show that auditing reduces *CoD*, but also that the size of the effect differs between models.

Table 5 Estimation results, dependent variable *CoD*, all industries, full sample.

Independent variables with expected sign in parenthesis	Endogenous switching, unconstrained model Coefficients			Endogenous switching, constrained model Coefficients	2SLS IV Coefficients	PSM Coefficients
	Audited (1)	Unaudited (2)	Difference and p–value of difference (Audited –Unaudited) (3)	(4)	(5)	(6)
Audited (–)	–3.7850***		–	–0.8309***	–0.8217***	–0.2832
	(0.0000)			(0.0000)	(0.0000)	(0.4715)
ICR (–)	–0.0015***	–0.0029***	0.0014***	–0.0017***	–0.0017***	–
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	
LR (–)	0.0416***	0.0029***	0.0387***	0.0054*	0.0052*	–
	(0.0000)	(0.0000)	(0.0000)	(0.0990)	(0.0940)	
PPE_share (–)	1.2606***	1.8377***	–0.5771***	1.5227***	1.5444***	–
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	
Leverage (+)	2.0032***	–0.1653***	2.1685***	0.2450***	0.2428***	–
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	
Ln_TA (–)	0.1904***	0.0523	0.1381***	0.2059***	0.2039***	–
	(0.0000)	(0.1830)	(0.0000)	(0.0000)	(0.0000)	
Growth (–)	–0.0729**	0.0193	–0.0922*	0.0088	0.0084	–
	(0.0410)	(0.5630)	(0.0571)	(0.7210)	(0.7320)	
ROA (–)	–0.0122***	–0.0030***	–0.0092***	–0.0088***	–0.0088***	–
	(0.0000)	(0.0010)	(0.0000)	(0.0000)	(0.0000)	
Neg. Equity (+)	0.0366	1.1516***	–1.1150***	0.8166***	0.8172***	–
	(0.7140)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	
Constant	1.8253***			–0.4294	–0.4733**	–
	(0.0000)			(0.1180)	(0.0390)	
ATE	–0.4672***			–0.8309***	–0.8217***	–0.2832
	(0.0010)			(0.0000)	(0.0000)	(0.4715)
Industry dummy	Yes			Yes	Yes	Yes
Observations	155 260			155 260	155 260	142 288
AIC	992,121.6			993,701.2	N/A	N/A
R–sq. second stage reg	N/A			N/A	0.0382	N/A
Pseudo–R–sq./adjusted R–sq. first stage reg.[1]	0.3604			0.3604	0.2994	0.2938
Partial R–sq. first stage reg. *Reform 2010*	N/A			N/A	0.0457	N/A
F–statistic	N/A			N/A	7090.5	N/A

Both voluntary and regulated auditing as comparison group and unaudited firms in control group

P-values in parenthesis for endogenous switching models, 2SLS IV, and PSM. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively. Coefficients of the Industry dummy are omitted. [1]Pseudo-R-sq. for endogenous switching models and PSM, and adjusted R-sq. for 2SLS IV

in the range 0.30 to 0.80 percentage points. One study that stands out compared to these is Koren et al. (2014) who, for a sample of Slovenian firms, found that voluntary audits increased *CoD* by 0.21 percentage points.

Turning to estimates of the control variables using the unconstrained ES model, the signs for *ICR, ROA,* and *Neg. Equity* are as expected from prior literature for both the audited and unaudited groups, and the signs for *Leverage and Growth* for the audited sample are also in line with the expectation from prior literature. However, the signs for *LR, PPE_share, Ln_TA* for both groups, and the signs for *Leverage* and *Growth* for the unaudited sample are not in line with prior literature. The difference of the control variables' coefficients between the audited and unaudited group is all

Table 6 Estimation results, dependent variable *CoD*, all industries, firms audited by BigN auditor versus unaudited firms. Both voluntary and regulated auditing as comparison group and unaudited firms in control group

Independent variables with expected sign in parenthesis	Endogenous switching, unconstrained model Coefficients			Endogenous switching Coefficients	2SLS IV Coefficients	PSM Coefficients
	Audited (1)	Unaudited (2)	Difference and p–value of difference (Audited – Unaudited) (3)	(4)	(5)	(6)
Audited (–)	–6.8375***		–	–0.2195	–0.4120***	0.1203
	(0.0000)			(0.1200)	(0.0030)	(0.4654)
ICR (–)	–0.0015***	–0.0030***	0.0015***	–0.0019***	–0.0018***	–
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	
LR (–)	0.0521***	0.0075***	0.0446***	0.0090**	0.0073*	–
	(0.0000)	(0.0600)	(0.0020)	(0.0190)	(0.0610)	
PPE_share (–)	1.1289***	1.8326***	–0.7037***	1.5276***	1.4912***	–
	(0.0000)	(0.0000)	(0.0001)	(0.0000)	(0.0000)	
Leverage (+)	1.2441***	–0.1447***	1.3888***	–0.0430	–0.0319	–
	(0.0000)	(0.0000)	(0.0000)	(0.1360)	(0.2800)	
Ln_TA (–)	0.3460***	–0.0318	0.3778***	0.1535***	0.1832***	–
	(0.0000)	(0.2820)	(0.0000)	(0.0000)	(0.0000)	
Growth (–)	–0.1648	0.0045	–0.1693	–0.0172	–0.0148	–
	(0.1980)	(0.8910)	(0.1998)	(0.6000)	(0.6510)	
ROA (–)	–0.0201***	–0.0029***	–0.0172***	–0.0060***	–0.0061***	–
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	
Neg. Equity (+)	0.2875	1.0711***	–0.7836*	1.0606	1.0460	–
	(0.4540)	(0.0000)	(0.0508)	(0.2810)	(0.4670)	
Constant	2.4406***	–		–0.2193	–0.9338***	–
	(0.0000)			(0.5870)	(0.0000)	
ATE	–0.5027***			–0.2195	–0.4120***	0.1203
	(0.0020)			(0.1200)	(0.0030)	(0.4654)
Industry dummy	Yes			Yes	Yes	Yes
Observations	58 966			58 966	58 966	51 423
AIC	373,475.8			373,795.7	N/A	N/A
R–sq. second stage reg	N/A			N/A	0.0296	N/A
Pseudo–R–sq./adjusted R–sq. first stage reg.[1]	0.6232			0.6119	0.5904	0.5694
Partial R–sq. first stage reg. *Reform 2010*	N/A			N/A	0.1084	N/A
F–statistic	N/A			N/A	3821.9	N/A

P-values in parenthesis for endogenous switching models, 2SLS IV, and PSM. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively. Coefficients of the Industry dummy are omitted. [1]Pseudo-R-sq. for endogenous switching models and PSM, and adjusted R-sq. for 2SLS IV

significant at 1% except for the *Growth* variable, which is significant at 10%.[21]

[21] Since ICR and Leverage are both proxies of financial risk, there could be issues of multicollinearity, even though the correlations between these variables (reported in Table 10 in Appendix C) are not that severe. As such, we have re-run our estimations removing these potentially collinear variables one by one, as we did with ROA and Leverage above. However, doing this does not alter the results regarding how being audited affects *CoD* in any significant manner. We chose to include both these variables in the estimations presented in this paper.

To test hypothesis 2, we created a sub-sample containing firms audited by BigN auditors and unaudited firms. For our main model, the unconstrained ES model, the results from these estimations are reported in Table 6, columns 1 through 3. The size of the parameter estimate indicates that having a BigN auditing firm decreases the *CoD* by 0.50 percentage points as compared to 0.47 percentage points in the original sample. However, the difference is quite small (0.04 percentage points), and 95% confidence intervals from the two

estimations clearly overlap. As such, we find that hypothesis 2 is not supported.

Our findings are thus consistent with most previous literature that report small and often statistically insignificant advantages of using BigN auditors as compared to auditing in general. For publicly traded companies, there is some evidence of advantage when using a BigN auditor. Both Mansi et al. (2004) and Pittman and Fortin (2004) report that using BigN auditors resulted in a lower *CoD* compared to auditing in general. Also, Causholli and Knechel (2012) found that firms going public for the first time had a lower *CoD* when using BigN auditors. However, when studying private firms as we do in this study, Fortin and Pittman (2007) report that using a BigN auditor does not result in better yield spreads or credit ratings, and both Huguet and Gandía (2014) and Kim et al. (2011) report that the appointment of a BigN auditor does not reduce the *CoD* of the audited firms more than auditing in general does. The two studies that stand out are Karjalainen (2011) and Koren et al. (2014), who both report that using BigN auditing firms reduces *CoD* more than auditing in general.

We also compare the audit effect on *CoD* for our benchmark industries, utilities (H3a) and agriculture (H3b), to that of all the other industries in our sample. First, based on the work of Dunn and Mayhew (2004), H3a states that the value of auditing should be less in industries that are highly regulated and monitored by government agencies. We use utilities as our benchmark since this industry is the most heavily regulated industry in our sample. The results presented in Table 7, first column, show that 12 out of 14 industries save more on *CoD* due to auditing compared to the utilities industry, 2 industries save less and all of these effects are statistically insignificant at conventional levels. As such, hypothesis 3a is not supported.

Second, based on the work by (Berger et al. 2017), H3b states that auditing should have less impact on industries where one lender has high exposure to that industry. In our sample, agriculture is an industry where one of the main banks, Swedbank, has a 74% market share. As a comparison, lending to the utilities industry is quite evenly distributed among the four large banks. Unlike utilities, the agricultural industry is not heavily regulated, making it possible to disentangle the effects of regulation from that of bank exposure to an industry. The results are presented in column 2, Table 7, and they provide strong support for hypothesis 3b, that when a lender has a high exposure in an industry, the impact of having audited financial statements is reduced. We find that the point estimates of the reduction in *CoD* due to auditing are larger in all of the 14 industries that are compared to agriculture and that the effect is statistically significant at the 1% level for 6 industries and at the 10% level for 3 industries.

Finally, turning to the average effect of auditing on *CoD* in specific industries, these are presented in the last column of Table 7. While the sign of the audited coefficient is consistent across all industries except for culture and recreation, the effect in the manufacturing, construction, retail and wholesale, transportation, hotel & restaurant, rental and leasing, training, and other service activities industries was also significant at the 1% level. The effect in the information and communication and health care and social services industries was significant at the 10% level, while the effect in the utilities, agriculture, real estate, professional services, and culture and recreation industries was insignificant. Taken together, these results lend further support to our claim that auditing in most cases reduces *CoD* for the audited firms. However, it should also be noted that the effects for utilities and agriculture were found to be insignificant, lending further support to the claim that auditing is of less importance in industries which are subject to high regulation and monitoring by government agencies and when a lender has a significant exposure in a particular industry.

Summary and discussion

Financial statement verification does indeed add value to a firm, at least in terms of reducing the firm's *CoD*. Our findings suggest firms with audited financial statements, on average, save 0.47 percentage points (or 47 basis points) on interest for debts.[22] In the US private-firm setting, Minnis (2011) found that audits reduce *CoD* on average by 0.69 percentage points, equivalent to 25 000 USD (21 535 EUR) in annual interest charge savings by an average firm. In the Korean private firm-setting, Kim et al. (2011) found that voluntary audits reduce *CoD* by 0.56 to 1.24 percentage points, depending on model specifications. In the Spanish setting Huguet and Gandía (2014) found that audits, voluntary or not, reduce *CoD* by on average 0.18 percentage points. In a sample of Slovenian firms, Koren et al. (2014) found that voluntary audits increase *CoD* by 0.21 percentage points, while Kausar et al. (2016), using a sample of U.K. private firms, found that voluntary audits decrease *CoD* by 0.30 to 0.80 percentage points, equivalent to savings of 617 GBP (697 EUR) to 1 093 GBP (1 235 EUR). As such, our results are in line with these studies. With the Swedish sample, we too have found a reduction in the *CoD* of being audited and also in terms of percentage points saved.[23]

[22] There have not been any changes in the thresholds for voluntary auditing or any other changes that have altered the relationship between audited and unaudited firms between 2009 and 2013, a necessary condition for such changes to have biased the estimations of the effects of auditing on *CoD*.

[23] There are some differences between our dependent variable, *CoD,* and the interest rate spreads used by Blackwell et al. (1998) and Kim et al. (2011). However, Allee and Yohn (2009), Minnis (2011), Huguet and Gandía (2014), Koren et al. (2014) and Kausar et al. (2016) all use *CoD* measures similar to ours. The reported savings on *CoD* due to auditing should thus be comparable.

Table 7 Estimation results of 2SLS IV, dependent variable *CoD*, by industries. Industries utilities and agriculture as benchmarks for H3a and H3b, respectively

Industry	Utilities as benchmark (H3a) (1)	Agriculture as benchmark (H3b) (2)	Manufacturing as benchmark (3)	Construction as benchmark (4)	Retail and wholesale as benchmark (5)	Transportation & Storage as benchmark (6)	Hotel & Restaurants as benchmark (7)	Information & communication as benchmark (8)	Real estate as benchmark (9)	Professional services as benchmark (10)	Renting and leasing as benchmark (11)	Training as benchmark (12)	Health care and social as benchmark (13)	Culture and recreation as benchmark (14)	Other service activities as benchmark (15)	Effect of audit on specific industry (16)
Utilities	–	–1.0944 (0.2830)	–0.6016 (0.5490)	–0.0508 (0.9600)	–0.6083 (0.5430)	–0.1702 (0.8660)	–0.0381 (0.9710)	–0.8703 (0.3900)	–0.7625 (0.4750)	–1.1198 (0.2630)	–0.1961 (0.8500)	0.4980 (0.6320)	–0.8627 (0.3990)	–1.9547* (0.0670)	0.4344 (0.6770)	–1.3204 (0.1920)
Agriculture	1.9044 (0.2830)	–	0.4927* (0.0690)	1.0436*** (0.0000)	0.4860* (0.0510)	0.9241*** (0.0010)	1.0563*** (0.0010)	0.2241 (0.4680)	0.3319 (0.5080)	–0.0254 (0.9230)	0.8982** (0.0150)	1.5923*** (0.0000)	0.2317 (0.4740)	–0.8603* (0.0540)	1.5287*** (0.0000)	–0.2260 (0.3260)
Manufacturing	0.6016 (0.5490)	–0.4927* (0.0690)	–	0.5509*** (0.0060)	–0.0067 (0.9740)	0.4314* (0.0800)	0.5635 (0.1430)	–0.2686 (0.3190)	–0.1608 (0.7270)	–0.5182** (0.0180)	0.4055 (0.2300)	1.0996*** (0.0010)	–0.2611 (0.3770)	–1.3531*** (0.0010)	1.0360*** (0.0040)	–0.7187*** (0.0010)
Construction	0.0508 (0.9600)	–1.0436*** (0.0000)	–0.5509*** (0.0060)	–	–0.5576*** (0.0010)	–0.1194 (0.5820)	0.0127 (0.9720)	–0.8195*** (0.0010)	–0.7117 (0.1230)	–1.0690*** (0.0000)	–0.1454 (0.6500)	0.5487* (0.0850)	–0.8119*** (0.0030)	–1.9039*** (0.0000)	0.4852 (0.1520)	–1.2696*** (0.0000)
Retail and wholesale	0.6083 (0.5430)	–0.4860* (0.0510)	0.0067 (0.9740)	0.5576*** (0.0010)	–	0.4381** (0.0490)	0.5703 (0.1230)	–0.2620 (0.2940)	–0.1541 (0.7340)	–0.5115*** (0.0080)	0.4122 (0.2000)	1.1063*** (0.0010)	–0.2543 (0.3560)	–1.3464*** (0.0010)	1.0427*** (0.0020)	–0.7120*** (0.0000)
Transportation	0.1702 (0.8660)	–0.9242*** (0.0010)	–0.4314* (0.0800)	0.1194 (0.5820)	–0.4381** (0.0490)	–	0.1321 (0.7370)	–0.7001*** (0.0140)	–0.5923 (0.2190)	–0.9496*** (0.0000)	–0.0259 (0.9410)	0.6682* (0.0550)	–0.6925** (0.0230)	–1.7845*** (0.0000)	0.6046* (0.0990)	–1.1501*** (0.0000)
Hotel & restaurants	0.0381 (0.9710)	–1.0563*** (0.0100)	–0.5636 (0.1430)	–0.0127 (0.9720)	–0.5703 (0.1230)	–0.1321 (0.7370)	–	–0.8321** (0.0430)	–0.7244 (0.2020)	–1.0817*** (0.0040)	–0.1581 (0.7300)	0.5360 (0.2400)	–0.8246* (0.0520)	–1.9166*** (0.0000)	0.4724 (0.3160)	–1.2823*** (0.0000)
Information and communication	0.8703 (0.3900)	–0.2241 (0.4680)	0.2686 (0.3190)	0.8195*** (0.0010)	0.2619 (0.2940)	0.7001*** (0.0140)	0.8322** (0.0430)	–	0.1078 (0.8220)	–0.2495 (0.3370)	0.6741* (0.0660)	1.3682*** (0.0000)	0.0076 (0.9820)	–1.0844** (0.0150)	1.3046*** (0.0010)	–0.4501* (0.0880)
Real estate	0.7625 (0.4750)	–0.3319 (0.5080)	0.1608 (0.7270)	0.7117 (0.1230)	0.1541 (0.7340)	0.5923 (0.2190)	0.7244 (0.2020)	–0.1078 (0.8220)	–	–0.3573 (0.4310)	0.5663 (0.2800)	1.2604** (0.0200)	–0.1002 (0.2000)	–1.1922** (0.0410)	1.1969** (0.0300)	–0.5579 (0.2730)
Professional services	1.1198 (0.2630)	0.0254 (0.9230)	0.5182** (0.0180)	1.0690*** (0.0000)	0.5115* (0.0080)	0.9496*** (0.0000)	1.0817*** (0.0040)	0.2495 (0.3370)	0.3573 (0.4310)	–	0.9237*** (0.0050)	1.6178*** (0.0000)	0.2571 (0.3720)	–0.8349** (0.0460)	1.5542*** (0.0000)	–0.2005 (0.3400)
Renting and leasing	0.1961 (0.8500)	–0.8982** (0.0150)	–0.4055 (0.2300)	0.1454 (0.6500)	–0.4122 (0.2000)	0.0259 (0.9410)	0.1581 (0.7300)	–0.6741* (0.0660)	–0.5663 (0.2800)	–0.9237*** (0.0050)	–	0.6941* (0.0990)	–0.6665* (0.0840)	–1.7586*** (0.0000)	0.6305 (0.1490)	–1.1242*** (0.0010)
Training	–0.4980 (0.6320)	–1.5923*** (0.0000)	–1.0996*** (0.0010)	–0.5487* (0.0850)	–1.1063*** (0.0010)	–0.6682* (0.0550)	–0.5360 (0.2400)	–1.3682*** (0.0000)	–1.2604** (0.0200)	–1.6178*** (0.0000)	–0.6941* (0.0990)	–	–1.3607*** (0.0000)	–2.4527*** (0.0000)	–0.0636 (0.8840)	–1.8183*** (0.0000)
Health care and social services	0.8627 (0.3990)	–0.2317 (0.4740)	0.2611 (0.3770)	0.8119*** (0.0030)	0.2543 (0.3560)	0.6925** (0.0230)	0.8246* (0.0520)	–0.0076 (0.9820)	0.1002 (0.2000)	–0.2571 (0.3720)	0.6665* (0.0840)	1.3607*** (0.0000)	–	–1.0920** (0.0180)	1.2971*** (0.0010)	–0.4577* (0.0860)
Culture and recreation	1.9547* (0.0670)	0.8603* (0.0540)	1.3531*** (0.0010)	1.9039*** (0.0000)	1.3464*** (0.0010)	1.7845*** (0.0000)	1.9166*** (0.0000)	1.0844** (0.0150)	1.1922** (0.0410)	0.8349** (0.0460)	1.7586*** (0.0000)	2.4527*** (0.0000)	1.0920** (0.0180)	–	2.3891*** (0.0000)	0.6344 (0.1250)

Table 7 (continued)

Industry	Utilities as benchmark (H3a) (1)	Agriculture as benchmark (H3b) (2)	Manufacturing as benchmark (3)	Construction as benchmark (4)	Retail and wholesale as benchmark (5)	Transportation & Storage as benchmark (6)	Hotel & Restaurants as benchmark (7)	Information & communication as benchmark (8)	Real estate as benchmark (9)	Professional services as benchmark (10)	Renting and leasing as benchmark (11)	Training as benchmark (12)	Health care and social as benchmark (13)	Culture and recreation as benchmark (14)	Other service activities as benchmark (15)	Effect of audit on specific industry (16)
Other service activities	−0.4344 (0.6770)	−1.5287*** (0.0000)	−1.0360*** (0.0040)	−0.4852 (0.1520)	−1.0427*** (0.0020)	−0.6046* (0.0990)	−0.4724 (0.3160)	−1.3046*** (0.0010)	−1.1969** (0.0300)	−1.5542*** (0.0000)	−0.6305 (0.1490)	0.0636 (0.8840)	−1.2971*** (0.0010)	−2.3891*** (0.0000)	–	−1.7547*** (0.0000)

P-values in parenthesis. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively. Coefficient results presented for independent variable *Audited* only

Given that the debt of an average unaudited firm in Sweden is 526 130 SEK (54 184 EUR), these firms could save about 526 130 SEK × 0.47% = 2 473 SEK (250 EUR) on annual interest charges, if they chose to be audited. Since the thresholds for voluntary auditing are low in Sweden, firms below these thresholds are small and with low levels of debt, making the potential savings due to auditing in absolute terms lower in Sweden than in most other countries. The 250 EUR savings in Sweden can, for example, be compared to Kausar et al. (2016) who reports savings in the range of 697 to 1 235 EUR in Great Britain, or Minnis (2011) reporting savings equivalent to 21 535 EUR in the USA.

Informal inquiries with business owners in Sweden suggest that audit costs for small and microfirms average somewhere between 15 000 SEK (1 544 EUR) to 20 000 SEK (2 060 EUR), excluding the costs of any additional internal staff time for the preparation and carrying out of the audit. This cost is even higher for more complex firms. Thus, for a typical unaudited firm in our sample the potential savings due to auditing is well below the cost of auditing, and these firms will not have any short-run savings from auditing their financial statements.

Also, most previous studies have noted that firms that can opt out of auditing do so. Minnis (2011) reports that 77% of firms who could opted out of auditing. For Kim et al. (2011), 96% of firm-year observations were for firms opting out, while Blackwell (1998) reports that 63% opted out of auditing. Huguet and Gandía (2014) had a final sample of 15 423 firms, of which

3 133 firms qualified for voluntary audit, but only 776 chose to proceed, giving an opt out rate of 75%. In our data, only 47 percent of firms with the opportunity to opt out did so, but it should be noted that the low thresholds for voluntary auditing in Sweden could make firms more likely to choose voluntary audits to make sure they are in compliance with the rules and regulations. In addition, Ojala et al. (2016), using Finish data, found that choosing voluntary audit was a strong indication of the firm having growth ambitions, making it likely for these firms to exceed the mandatory auditing thresholds in the near future. Our finding, that the benefits of auditing do not cover the costs, and that a large proportion of the firms who have the opportunity to opt out also do so raises the question to what extent auditing is actually worth the costs it incurs. This could be an interesting question for future research.

We also find that there is no substantial additional benefit to firms in terms of reduced *CoD* from employing BigN audit firms in the Swedish private firm setting. Our results are similar to the findings of Huguet and Gandía (2014) and Kim et al. (2011), but contradict the findings of Karjalainen (2011) and Koren et al. (2014). All of these studies investigate the effect of BigN auditors in a private firm setting. Thus, the results are mixed across different institutional

settings, which warrants more research in the private firm setting. This differs from previous studies regarding the public firm setting, where most studies find there are apparent benefits of having a BigN auditor in terms of reduced *CoD* (Causholli and Knechel 2012; Mansi et al. 2004; Pittman and Fortin 2004).

Lastly, we investigated the benefit of audits in the presence of alternative ways to reduce the information asymmetry between lenders and borrowers in different industries. We did this by examining the two alternatives of high business regulation and significant market exposure by lender. Based on the work of Dunn and Mayhew (2004), we expected high regulation and monitoring by government agencies to reduce the value of audit. Our results show that firms in 12 out of 14 less regulated industries save more on *CoD* due to auditing compared to the highly regulated industry, utilities. However, the effects are not statistically significant at conventional levels in any of these estimations, and we thus conclude that high levels of regulation and government monitoring does not constitute an alternative to auditing.

On the other hand, when one lender has a historically significant market exposure in a particular industry, it affects how that lender interacts with and evaluates firms from that industry (Dell'Ariccia et al. 1999, Winton 1999) and also possess rich information from experience (Berger et al. 2017). Here, we found that firms from one such industry, agriculture, save less on *CoD* from financial statement audits compared to 14 other industries, the results being statistically significant at the 1% level for 6 industries and at the 10% level for 3 industries.

Taken together, our results also allow us to disentangle the effects of regulation from that of bank exposure to an industry since in the utilities industry all major banks had a market share between 18 and 32% in contrast to the agriculture industry where one major bank held a 74% market share. We do not have any prior results to compare our findings with as this is, to the best of our knowledge, the first study that investigates this issue empirically. However, there is a well-established literature that the value of auditing is diminished by high regulation and monitoring by government agencies (Dunn and Mayhew 2004), and when lenders already have high exposure to an industry (Berger et al. 2017). We further corroborated our results by investigating the effect of audit on *CoD* in the utilities and agriculture industries separately and found insignificant effects for both industries, while 8 out of the remaining 12 industries had negative and significant effects at the 1% level and 2 others at the 10% level. This lends further support to our claim that audits in most cases reduce *CoD,* but not necessarily so when the information asymmetry between lender and borrower is reduced as a result of one lender having high market exposure to one specific industry.

Even though our results from the Swedish setting are similar to those from the American, British, Korean and Spanish settings, findings from other settings within the EU, or outside the EU, may vary, since risk varies greatly between developed and developing countries, and there is a known disparity amongst EU countries too (Sbarcea, 2015). Thus, future studies with cross-country samples will help us to better understand if the institutional differences influence the relationship between audits and *CoD*. Future studies can also address some of the caveats of this present study, and of some past studies, in this line of work. For example, a more comprehensive measure of *CoD* could improve future studies. One could, for example, try to calculate the total cost of borrowing (TCB) measure suggested by Berg et al. (2016). Most studies, including ours, only have access to interest rate data; however, total cost of debts also includes other contract terms, such as maturity, collateral, and additional conditions, if any, stipulated by the lender. Calculating the TCB would require additional data gathering and is clearly outside the scope of the present paper but could be an interesting avenue for future research. Furthermore, factors, such as the firm-lender relationship, and the individual lender's risk appetite also influence debt pricing, and lenders tend to optimize their risk–return relationship with a balance between these factors (Bharath et al. 2008). Future studies should, if possible, also incorporate these variables in their analysis. The literature also lacks comprehensive knowledge about the net benefit of audits, meaning the benefits after deducting the direct and indirect costs of auditing, as well as how banks view and value audits. As such, researchers could interview loan officers and credit analysts to better understand how they value and/or evaluate a firm's audit and auditor choices. These are, however, suggestions for future studies and outside the scope of the present paper.

Appendix A

See Table 8.

Table 8 First stage probit estimations of endogenous switching models, first stage estimations of 2SLS IV, and first stage logistic estimations of PSM

Independent variables	Endogenous switching, unconstrained model All Coefficients (1)	Endogenous switching unconstrained model BigN Coefficients (2)	Endogenous switching, constrained model All Coefficients (3)	Endogenous switching constrained model BigN Coefficients (4)	2SLS IV, All Coefficients (5)	2SLS IV, BigN Coefficients (6)	PSM, All Coefficients (7)	PSM, BigN Coefficients (8)
Reform 2010	–1.2867*** (0.0000)	–1.4602*** (0.0000)	–1.2867*** (0.0000)	–1.5900*** (0.0000)	–0.2116*** (0.0000)	–0.2914*** (0.0000)	–	–
ICR	0.0002*** (0.0000)	0.0003*** (0.0000)	0.0002*** (0.0000)	0.0000*** (0.0140)	0.0000*** (0.0000)	0.0001*** (0.0000)	0.0007*** (0.0000)	0.0011*** (0.0000)
LR	–0.0188*** (0.0000)	–0.0163*** (0.0000)	–0.0188*** (0.0000)	–0.0005* (0.0750)	–0.0061*** (0.0000)	–0.0033*** (0.0000)	–0.0543*** (0.0000)	–0.0672*** (0.0000)
PPE_share	–0.2110*** (0.0000)	–0.3031*** (0.0000)	–0.2110*** (0.0000)	–0.1157*** (0.0000)	–0.0354*** (0.0000)	–0.0771*** (0.0000)	–0.9435*** (0.0000)	–1.5127*** (0.0000)
Leverage	–0.0804*** (0.0000)	0.1425*** (0.0000)	–0.0804*** (0.0000)	0.0021** (0.0210)	–0.0214*** (0.0000)	0.0386*** (0.0000)	0.1034 (0.4500)	0.5309*** (0.0000)
Ln_TA	0.4057*** (0.0000)	0.5873*** (0.0000)	0.4057*** (0.0000)	0.5239*** (0.0000)	0.0891*** (0.0000)	0.1153*** (0.0000)	1.1077*** (0.0000)	1.6514*** (0.0000)
Growth	0.1036*** (0.0000)	0.1333*** (0.0000)	0.1036*** (0.0000)	0.0989*** (0.0000)	0.0373*** (0.0000)	0.0205*** (0.0000)	0.1253*** (0.0000)	0.1487*** (0.0000)
ROA	–0.0011*** (0.0000)	–0.0023*** (0.0000)	–0.0011*** (0.0000)	0.0001 (0.7600)	–0.0001* (0.0600)	–0.0005*** (0.0000)	–0.0036*** (0.0000)	–0.0045*** (0.0000)
Neg. Equity	0.2152*** (0.0000)	–0.2305*** (0.0010)	0.2152*** (0.0000)	–0.0005 (0.9930)	0.0062 (0.3930)	–0.0419*** (0.0000)	0.3888*** (0.0000)	–0.7048*** (0.0000)
Constant	–4.1735*** (0.0000)	–8.1609*** (0.0000)	–4.1735*** (0.0000)	–7.1922*** (0.0000)	–0.5008*** (0.0000)	–1.2530*** (0.0000)	–15.068*** (0.0000)	–25.851*** (0.0000)
Industry dummy	YES	YES	YES	YES	YES	YES	YES	YES
Observations	155 260	58 966	155 260	58 966	155 260	58 966	155 260	58 966
Pseudo–R–sq./ adjusted R–sq. first stage reg.[1]	0.3604	0.6232	0.3604	0.6119	0.2994	0.5904	0.2938	0.5694

P-values in parenthesis. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively. [1]Pseudo-R-sq. for endogenous switching models and PSM, and adjusted R-sq. for 2SLS IV

Appendix B

See Table 9.

Table 9 Threshold values (in Euros) for mandatory audit in European countries as of May 2016 with corresponding increase from last ceiling

Country	Total assets	Increase	Net turnover	Increase	Employees	Increase
Austria	5,000,000	3%	10,000,000	3%	50	–
Belgium	4,500,000	23%	9,000,000	23%	50	–
Bulgaria	1,000,000	33%	2,000,000	60%	50	–
Croatia	2,000,000	–	4,000,000	–	25	–
Cyprus	3,400,000	–	7,000,00	–	50	–
Czech Republic	1,500,000	–	3,000,000	–	50	–
Denmark	4,837,000	–	9,674,000	–	50	–
Estonia	2,000,000	100%	4,000,00	100%	60	100%
Finland	100,000	–	200,000	–	3	–
France	1,550,000	–	3,100,000	–	50	–
Germany	6,000,000	24%	12,000,000	24%	50	–
Greece	4,000,000	60%	8,000,000	60%	50	–
Hungary	–	–	965,000	44%	50	–
Iceland	1,400,000	–	2,800,000	–	50	–
Ireland	4,400,000	–	8,800,000	–	50	–
Italy	4,400,000	–	8,800,000	–	50	–
Latvia	800,000	100%	1,600,000	100%	50	100%
Lithuania	1,800,000	–	3,500,000	–	50	–
Luxembourg	4,400,000	–	8,800,000	–	50	–
Malta	46,600	–	93,000	–	2	–
Netherlands	6,000,000	36%	12,000,000	36%	50	–
Norway	2,500,000	–	625,000	–	10	–
Poland	2,500,000	–	5,000,000	–	50	–
Portugal	1,500,000	–	3,000,000	–	50	–
Romania	3,650,000	–	7,300,000	–	50	–
Slovakia	1,000,000	–	2,000,000	–	30	–
Slovenia	4,000,000	–9%	8,000,000	–9%	50	–
Spain	2,850,000	–	5,700,000	–	50	–
Sweden	150,000	–	300,000	–	3	–
Switzerland	18,203,000	–	36,405,000	–	250	–
UK	6,541,000	56%	13,082,000	57%	50	–

Source: Federation of European Accountants, 2016

Appendix C

See Table 10.

Table 10 Pearson correlations

	CoD	Audited	Reform2010	ICR	LR	PPE_share	Leverage	Ln_TA	Growth	ROA	Neg. Equity
CoD	1.0000										
Audited	**0.0233**	1.0000									
Reform2010	**–0.0143**	**–0.4482**	1.0000								
ICR	**–0.1115**	**–0.1079**	**–0.1414**	1.0000							
LR	**0.0183**	**–0.1347**	**0.1386**	**–0.0054**	1.0000						
PPE_share	**0.1354**	**0.1005**	**-0.0409**	**–0.0813**	**–0.1372**	1.0000					
Leverage	**0.0826**	**–0.0431**	–0.0028	**–0.0780**	**–0.2417**	**0.1279**	1.0000				
Ln_TA	**0.0530**	**0.4909**	**–0.5532**	**0.1637**	**0.0320**	**0.2813**	**–0.1107**	1.0000			
Growth	**–0.0127**	**0.0893**	0.0025	**0.0642**	**–0.1100**	**0.0416**	0.0035	**0.0905**	1.0000		
ROA	**–0.0832**	**–0.0694**	**–0.0313**	**0.1665**	**0.0615**	**–0.0843**	**–0.2734**	**0.1170**	**0.2097**	1.0000	
Neg. Equity	**0.0595**	**–0.1073**	**0.0781**	**–0.0515**	**–0.0704**	**–0.0093**	**0.5844**	**–0.2265**	**–0.0437**	**–0.2311**	1.0000

Items in bold are significant at the 1% level. *ICR, LR, PPE share, Leverage, Ln_TA, Growth, ROA,* and *Neg. Equity* are lagged by one year. *CoD* is winsorized at 1% and 99%. Furthermore, continuous control variables *ICR, LR, PPE share, Leverage, Ln_TA, Growth,* and *ROA* are winsorized at 1% and 99% to alleviate any possible effect of extreme outliers

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