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MANDATORY AUDITOR CHOICE AND SMALL FIRM FINANCE: EVIDENCE FROM FINLAND

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ABSTRACT: We study the capital market implications of mandatory auditor choice. This regulatory intervention provides us with an instrument that can be used to examine the role of asymmetric information in the market for small business finance. We propose, in particular, a new exogenous measure of information based on a legal requirement that makes it mandatory for firms above a certain size to use certified auditors. Using a large panel on Finnish (mostly) closely held small businesses we find that despite significant market-driven demand for auditing, the legal requirement forces some firms to use a certified auditor. The cost of debt capital for a small business that is forced to use a certified auditor goes down and its creditworthiness, as measured by a commercial credit rating, improves. These results are consistent with the view that asymmetric information and, in particular, borrower-lender conflict are empirically relevant in the market for small business finance.

JEL: G14, G31, G32.

KEYWORDS: auditing, small business finance, cost of debt capital, regression discontinuity.

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TIIVISTELMÄ: Tässä tutkimuksessa kartoitetaan pk-yritysten rahoitusta mahdollisesti hankaloittavia informaatio-ongelmia tutkimalla hyväksytyn tilintarkastajan pakollisen käytön vaikutuksia pk-yritysten rahoituskustannuksiin ja luottokelpoisuuteen. Tutkimuksessa hyödynnetään laajaa paneeliaineistoa suomalaisista pk-yrityksistä. Aineisto sisältää 78 505 vuosi-yritys -havaintoa vuosilta 1999-2002.

Käytämme pakollista hyväksytyn tilintarkastajan valintaa identifioimaan informaatio-ympäristön vaikutuksia yritysrahoitusmarkkinoilla. Lainsäädännössä määritellään tietyt kokokriteerit, jotka ylittäviltä yrityksiltä vaaditaan joko Keskuskauppakamarin tai kauppakamarin hyväksymän tilintarkastajan käyttöä. Tämä sääntely tuo ns. eksogeenista vaihtelua pk-yritysten ”informaatio-ympäristöön”, minkä ansiosta voidaan tutkia sitä, kuinka pakollinen hyväksytyn tilintarkastajan käyttö ja siten vaihtelu pk-yritysten ”läpinäkyvyydessä” vaikuttaa niiden luottokelpoisuuteen ja ulkoisen rahoituksen kustannuksiin. Tutkimuksessa sovellettava estimointi-menetelmä (regression discontinuity design) hyödyntää nimenomaan tilintarkastuslainsäädännöstä löytyvää ”kynnystä”, joka määrittelee sen, minkälaisen pk-yrityksen on pakko käyttää hyväksyttyä tilintarkastajaa. Vaikka useat pk-yritykset valitsevat hyväksytyn tilintarkastajan jo ennen kuin niiden koko ylittää laissa määritellyn kynnyksen, löydämme, että lakikynnys aiheuttaa ”hypyn” todennäköisyydessä, että pk-yritys käyttää hyväksyttyä tilintarkastajaa. Tämän voidaan tulkita tarkoittavan, että laki ”pakottaa” osan pk-yrityksistä valitsemaan hyväksytyn tilintarkastajan. Löydämme myös, että kun pk-yritys pakotetaan käyttämään hyväksyttyä tilintarkastajaa, yrityksen rahoituskustannukset alenevat ja sen luottokelpoisuus paranee. Tulokselle on monia mahdollisia tulkintoja, mutta yksi raportin keskeisistä johtopäätöksistä on, että empiiriset tulokset ovat yhdenmukaisia sen kanssa, että epäsymmetrinen informaatio vaikeuttaa yritysrahoitusmarkkinoiden toimintaa.

JEL-luokittelu: G14, G31, G32.

AVAINSANAT: tilintarkastus, pk-yritysrahoitus, vieraan pääoman kustannukset.

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1 Introduction

There is a significant market-driven demand for auditing services, because having the accounts voluntarily audited by a reputable auditor lowers the cost of capital.[1] Not *all* firms choose a high-quality auditor voluntarily, however. Especially in the small business sector, some would choose *no* external auditor were they not forced to do so by regulation. In this paper, we study empirically the capital market implications of mandatory auditor choices.

We study the mandatory auditor choices, because this regulatory intervention provides us with an instrument that can be used to examine the role of asymmetric information in the capital markets. We propose, in particular, a new exogenous measure of information based on a legal requirement that makes it mandatory for firms above a certain size to use certified auditors. We examine the effect of this legal requirement on both the financing costs of small businesses as well as their creditworthiness. Focusing on this kind of exogenous variation in information structure is important, because the effect of information environment on the ability of firms to raise external finance cannot be identified if the proxy used to measure the environment is partly endogenous. The often employed proxies for information structure, such as analyst coverage, bid-ask spread and voluntary auditor choice, are obviously subject to this criticism (Garmaise and Moskowitz, 2004a).

While compelling empirical evidence on the significance of asymmetric information on capital markets has been hard to come by, the theoretical corporate finance literature suggests that information problems are pervasive. Incomplete and asymmetric information creates scope for various agency problems, which stem both from the manager- shareholder conflict (i.e. separation of ownership and control) and from the lender-borrower conflict (Jensen and Meckling 1976; for a review, see Berger and Udell 1998). Our sample mainly consists of closely held micro-firms, in which the separation of ownership and control is rare, if non-existent. If agency considerations are empirically relevant in our data, we take that the lender-borrower conflict is their primary driver. We predict that the mandatory auditor choice alleviates the lender-borrower conflict, because certified auditors,

[1] See, for example, Titman and Trueman (1986) for a theoretical model, and Datar, Feltham and Hughes (1991), Pittman and Fortin (2004) and Hay and Davis (2004) for empirical evidence.

if they do their job, provide valuable information about borrowers (Watts and Zimmerman, 1986), improve the precision and credibility of financial statements (DeAngelo 1981), and make it more difficult to violate accounting-based covenants (Sweeney 1994, Pittman and Fortin 2004).

Using a large panel data on Finnish small businesses we find that while the voluntary use of certified auditors is common, the legal requirement does have a role in forcing some firms to engage them. There is a clear discontinuity in the proportion of firms with certified auditors, when looked at below and above the size threshold specified in the law. This empirical consequence of the legal threshold allows us to apply *regression discontinuity (RD) design* (see, e.g., Hahn, Todd and van der Klaauw 2001, and van der Klaauw 2002) to identify the effect of engaging a certified auditor on the cost of debt capital and creditworthiness.

To apply the RD design to our data, we follow van der Klaauw (2002) and pursue a two-step procedure: In the first stage we regress auditor choice on a dummy variable indicating whether the firm's size exceeds the threshold specified in the legislation and a control function of various measures of firm size. The first-stage results show that the legal requirement does have a significant positive effect on the choice of a certified auditor, even after controlling for the size of the firm in various ways. In the second stage we regress, together with controls, the cost of capital and creditworthiness on the instrumented auditor choice from the first stage. The second stage results show that the cost of debt capital for a small business that is forced to use a certified auditor goes down. Its creditworthiness, as measured by a commercial credit rating, improves too. These results are robust to controlling for unobservable firm heterogeneity.

We thus find strong evidence that variation in the information environment matters. Taken together, these results are consistent with the view that asymmetric information and the borrower-lender conflict are empirically relevant in the market for small business debt. Our findings thus echo those of Garmaise and Moskowitz (2004a), who report that information concerns are real in commercial estate markets.

The rest of the paper is organized as follows: In the next section we briefly outline the legal and theoretical framework of our empirical analysis. The empirical framework is described in section 3, and section 4 discusses the data. In section 5 we present the results of our empirical analysis, and consider and try to rule

out a set of alternative explanations for our results. Section 6 contains a brief summary.

2 Legal and theoretical framework

2.1 Finnish auditing law

The Finnish auditing legislation was revised in the mid 1990s. The new Auditing Act was introduced at the beginning of 1995, replacing the old auditing legislation that had been introduced in the early 1980s. Besides incorporating the latest European developments into Finnish legislation, the Act increased both the qualification requirements for auditors, and their reporting and monitoring duties, as well as emphasized auditors' independence.

Neither the old nor the new auditing law exempts small firms from auditing. As explained for example in Sundgren (1998), very small firms can have auditors without any professional qualifications, as long as the auditor possesses sufficient knowledge of accounting, financial, and legal issues as well as knowledge and experience of auditing. The quality of these audits has been questioned repeatedly (see, e.g., Sundgren 1998), which is one of the reasons why this part of the law will, in all likelihood, be changed some time soon. For firms above a certain size, the law sets a legal requirement to use a certified auditor. In Finland there is a two-tier system of audit qualifications, consisting of a lower HTM qualification (in Finnish: "Kauppakamarin hyväksymä tilintarkastaja"), and a higher KHT qualification (in Finnish: "Keskuskauppakamarin hyväksymä tilintarkastaja"). Auditors with either of these professional qualifications will be referred to as certified auditors in this paper.

The new Auditing Act that was introduced at the beginning of 1995 set new size thresholds, which when exceeded, make it obligatory for the firm's accounts to be audited by a person with a certain professional qualification in auditing. These new limits became effective at the beginning of 1996. The first size threshold in the legislation specifies that *at least one* certified auditor must be chosen when a firm meets at least two of the following three criteria: employs more than 10 people, has a turnover of more than 680 000 euros, or has total assets of more than 340 000 euros. The second size threshold states that *only* a certified accountant can audit the books of firms meeting at least two of three of the following cri-

teria: employs more than 50 people, has a turnover of more than 4.2 million euros, or has total assets of more than 2.1 million. The third threshold applies to very large (public) firms, and is thus not relevant to this study.

A large part of the administrative audit in Finland has traditionally been related to the protection of creditors (Sundgren 1998, p. 443, and Hyytinen, Kuosa and Takalo 2003). A primary purpose of the audit is to examine whether the members of the company board and its managing director have behaved diligently and according to the rules in Company Law. Another primary purpose of the audit is to examine whether liquidation rules have been compromised. In closely held firms, these checks translate into the monitoring of the lender-borrower conflict. Creative withdrawals from the firm or cumulative losses that deteriorate the solvency of the firm could, for example, result in an auditing note or trigger an initiation of liquidation proceedings.

2.2 Economics of auditor choice

Voluntary versus mandated auditor choice

In the literature on accounting and economics, it is often argued that it pays for a firm to hire a reputable auditor, as having the accounts audited by such an auditor enhances capital market access. As we mentioned above, the access is potentially enhanced for many reasons: First, such auditors provide valuable information about borrowers (Watts and Zimmerman, 1986); second, they improve the precision and credibility of financial statements (DeAngelo 1981); and third, they make it more difficult to violate accounting-based covenants (Sweeney 1994, Pittman and Fortin 2004). Dye (1993) argues, moreover, that the wealthier (larger) the auditor, the more it risks if the quality of its auditing does not reflect the best practice. Larger auditors may also be more likely to detect mistakes in a firm's financial reporting because they are more independent than local firms (Mitton 2002).[2]

[2] A recent empirical analysis that is consistent with these views is for example Pittman and Fortin (2004). They show that engaging a high-quality auditor lowers a firm's cost of debt over time, especially if the firm has a short track record. Hyytinen and Pajarinen (2004) study the relation between the use of Big 5 auditors and the availability of external finance firms using a cross section of relatively large Finnish firms. They show that the excess growth made possible by external finance is associated with the use of Big 5 auditors and that at least a part of the association arises because firms (with an expected need for external finance) self-select. No instruments are used to identify and estimate the causal effect of a voluntary auditor choice.

If having accounts audited by a certified or prestigious auditor enhances the availability of external finance, why do not all firms engage such an auditor?[3] Obvious explanations suggest themselves: Hiring a reputable auditor involves i) direct monetary and non-monetary costs, ii) opportunity costs in terms of lost private benefits, and iii) less opportunities for tax evasion.

First, engaging an auditor involves (immediate) monetary outlays, and the better the quality of the auditor, the higher the auditing fee. Non-monetary search costs that arise from finding a suitable auditor matter, too. These direct costs need to be weighed against the expected benefits.[4] If a firm has only limited or no need for external finance, the benefit of the enhanced access to capital markets is negligible (see, e.g., Titman and Trueman 1986). Further, if there is a fixed component to these direct costs, small businesses with moderate needs for external finance probably do not have an incentive to pay them.

Second, engaging a certified auditor may involve an opportunity cost that arises from the peculiar incentives of people to establish and run entrepreneurial ventures. Many small businesses are run by entrepreneurs who like being their "own bosses" (see, e.g., Blanchflower and Oswald 1998 and Hamilton 2000). Because an important part of this liberty is to manage the firm and its accounts creatively, engaging an external monitor (which an auditor is) reduces the entrepreneur's privacy and her ability to withdraw funds from the firm without bureaucracy and frictions. These lost private benefits from running the firm reduce the probability that a firm engages a certified or prestigious auditor.[5]

Third, the decision not to have one's accounts audited by a certified auditor may be related to taxes. Possibilities for tax evasion apparently motivate some people to start running their own business (see Parker 2004, for a review of this

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[3] This question is, of course, familiar to many: The early literature on mandatory versus voluntary disclosure predicts that lack of disclosure can be taken to be bad news (Ross 1979, Grossmann 1981). Voluntary disclosure should therefore be forthcoming and no mandatory disclosure rules are needed (Viscusi 1978, Grossman and Hart 1980). Indeed, "if disclosure is good, why don't firms do it voluntarily?" (Admati and Pfleiderer 2000, pp. 479). Stocken (2000) shows, moreover, that if a manager has private information, she almost always disclose it to investors in a repeated game, provided that the manager is able to develop reporting creditability (i.e. she is not too short-sighted). A nice summary of the older and more recent literature on mandatory versus voluntary disclosure can be found in Fishman and Hagerty (2003).

[4] If hiring a reputable auditor is costly, we might expect that firms' decisions to hire them are socially optimal given the costs; see e.g. Viscusi (1978) and Jovanovic (1982);

[5] They reduce it, because the lost benefits are a cost that is weighed against the benefit of having better access to external finance.

literature). Because hiring a rigorous auditor reduces opportunities for tax evasion, some entrepreneurs may avoid doing so voluntarily.

The other side of the foregoing motives not to engage a reputable auditor is, of course, that they may be positively correlated with the magnitude of the borrower-lender conflict. Having flexibility to choose what kinds of projects to pursue, obtaining various private benefits from running the firm, or being able to hide some income are gray-area activities that an economist would regard as an expression of interim and ex post moral hazard. Most of these activities need not be outright illegal, but just something that, in the absence of a mandatory use of a certified auditor, would remain subject to an entrepreneur's discretion.

In addition to these moral hazard considerations, adverse selection may explain why some firms do *not* hire a reputable auditor: Consider a small business that has a high default probability and that would therefore pay a high risk premium for the external finance it needs, *if* financiers were able to tell it apart from small businesses with a lower default probability. Under asymmetric information, they cannot do so, and the more creditworthy firms bring down the cost of finance to the less creditworthy firms (de Meza and Webb 1987, de Meza 2002). If a reputable auditor can identify a firm's quality and if the choice of having such an auditor is endogenous, some good firms can ensure a lower cost of capital by separating themselves from the less creditworthy (as in Titman and Trueman 1986). However, in a semi-separating equilibrium, which often exists in signaling models, only a fraction of good firms choose a reputable auditor. In this case, the less creditworthy firms also opt out, as they benefit from pooling with the good firms and from obtaining debt at the cross-subsidized interest rate at which this group of observationally identical firms is offered credit.[6]

If not all firms use a certified auditor voluntarily, we expect that the legal requirement that makes it mandatory for firms above a certain size to use certified auditors, binds for some firms:

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[6] In a semi-separating equilibrium a good firm randomly chooses between separating by the choice of a reputable auditor and pooling with the less creditworthy by not choosing such an auditor. Unlike in a fully separating equilibrium, financiers cannot in this case distinguish between the different types of firms who belong to the subpopulation that does not choose a reputable auditor. In a pooling equilibrium, the choice of auditor is uninformative.

Prediction 1. *The legal requirement to engage a certified auditor forces some small businesses to choose such an auditor.*

While this prediction may sound trivial, there is surprisingly little evidence that legal requirements of this type actually bind. Both the significant market demand for reputable auditors (documented in the available literature; see e.g. Sundgren 1998 and Pittman and Fortin 2004, and the references therein) and non-compliance (which we also observe in our data) might imply that the legal requirement is redundant. The cautious null hypothesis we test in the empirical part is that the legal requirement has no effect whatsoever.

The capital market implications of mandatory auditor choice

If we find evidence that some firms choose a certified auditor only because they are required by the law to do so, we can examine the capital market implications of the mandatory auditor choice. When studying these implications it is important to note that the treatment effect of using a certified auditor may vary across firms. The effect is potentially different for firms that voluntarily choose a certified auditor from those that do so only because of the law. The treatment effect identified by the RD design (that we will use) is the local average treatment effect at the cutoff point determined by the law, for the subgroup of individuals for whom treatment changes discontinuously at the cutoff point (Van der Klaauw 2002, p.1262). It is, thus, *not* necessarily same as the effect of certified auditing when chosen voluntarily.

Based on the reasoning why some firms do not voluntarily choose a certified auditor, we hypothesize that the mandatory use of certified auditors reduces primarily the scope for borrower moral hazard and thus mitigates the lender-borrower conflict. For brevity, this borrower moral hazard effect is, in what follows, called the “discipline-effect”. We expect that the discipline effect increases the creditworthiness of small businesses and reduces their cost of debt capital. There is no reason to expect that a firm would not obtain these gains even if the act of engaging a certified auditor has *not* been privately optimal for the firm earlier.

The mandatory use of certified auditors may also bring about a second effect, for it may break the semi-separating equilibrium we discussed above: When

firms are required to engage a rigorous auditor, some of the firms who comply with the regulation might turn out to have a relatively high risk of default. In such a case, the effect of the forced use of a certified auditor on the creditworthiness of small businesses and their cost of debt capital reflects the *average* quality of the firms who hit the legal threshold (and comply with the regulation): If the average quality of these firms is relatively high, we expect to obtain the same effect as that implied by the discipline effect. If the average quality is low, the effect would be opposite.[7]

Based on the discussion above, we put forward the following prediction:

Prediction 2. *If asymmetric information is empirically important in the market for small business debt, the mandatory use of a certified auditor enhances the creditworthiness of small businesses and reduces their cost of debt capital.*

The cautious hypothesis that we test is that there are no effects whatsoever of the mandatory use of a certified auditor on the creditworthiness of small businesses and their cost of debt capital. This null hypothesis is consistent with the view that the informational considerations put forward by the modern corporate finance theory are not empirically important.

It is of course possible that both the moral hazard and adverse selection considerations that we discussed above are empirically relevant. In this case, the discipline effect could be exactly cancelled out, and possibly reversed, by the opposite effect that would follow if the forced use of certified auditors breaks the semi-separating equilibrium and if the average quality of the firms that in this equilibrium pool (i.e., do not choose a certified auditor voluntarily) is very low. We have, however, two reasons to think that our overall conclusions are not biased because of this potential bundling of the two phenomena: First, the possibility of finding two opposite effects reduces the likelihood that we find evidence for prediction 2.[8] Hence the bias, if any, is in the direction of us not rejecting the null

[7] If some firms that hit the legal threshold are "rotten apples", they probably do not comply with the regulation. In our data, we observe some non-compliance. The implications of these observations for our empirics are discussed in section 5.3.

[8] On the other hand, if the forced use of certified auditors breaks the semi-separating equilibrium and if the average quality of the firms that in this equilibrium pool is high, the likelihood that we find support for prediction 2 is larger. This is not a problem, for the finding supports the theoreti-

hypothesis (and thus finding no role for asymmetric information in the market for small business finance). Second, adverse selection is about hidden types, and hence refers to a permanent characteristic of firms. Because we also run fixed-effects estimations in which unobserved heterogeneity is allowed for, we can control for the cross-sectional variation induced by the hidden types. Potential non-compliance with the regulation makes this interpretation a bit more subtle, however. We therefore return to it in section 5.3., where we discuss the robustness of our results.

Before proceeding to the empirics, we describe our empirical approach and the data.

3 Empirical framework

3.1 Regression discontinuity -model

Our aim is to estimate the "treatment" effect of mandatory auditor choice on the cost of capital of small businesses and their creditworthiness. A basic linear representation of the problem is

$$Y_{it} = \alpha + \beta \cdot AUDITOR_{it} + u_{it} \qquad (1)$$

where Y_{it} is either the cost of debt capital or the creditworthiness score, AUDITOR is the dummy indicating the use of a certified auditor, and β is the treatment effect we are interested in.

The selection to using a certified auditor is non-random across firms. The OLS estimation of β would therefore be biased because of the dependence between the error term and the choice of auditor. The typical solution to this endogeneity problem is to find a suitable instrument for the choice of auditor. However, such instruments are hard to come by. Many if not most of the factors that affect the choice of auditor by a firm are also correlated with the factors that determine the firm's cost of capital and its creditworthiness.

cal prediction that asymmetric information matter in the market for small business finance. We cannot, of course, identify the exact driver of the finding.

We propose that the threshold(s) in the auditing legislation that trigger the use of a certified auditor provides us with a unique variable that is by design directly related to the auditor choice but has no direct relation to Y_{it}. The threshold can thus serve as an instrument and makes the application of the RD design possible. The key to the RD approach is the existence of a selection variable in which the probability of treatment assignment is a discontinuous function at some point. We take that here the selection variable is firm size. There is, moreover, a cutoff point at which the probability of hiring a certified auditor is a discontinuous function of size. Such a discontinuity arises, if for some firms, the legal requirement is what forces them to choose a certified auditor. The legal requirement is based on a threshold of firm size, specified by a combination of three size variables: employment, turnover and assets. The randomization at the margin identifies the treatment effect (Angrist and Lavy 1999, and Jaffe 2002).

Firm size and the cutoff point are not, however, the only variables affecting treatment assignment. There are likely to be various unobservable variables that influence the probability of assignment. This type of RD design is referred to as fuzzy RD design, where treatment assignment is not a deterministic function of the selection variables (here firm size) and the related cutoff, but is based on both observables and unobservables (see, e.g., van der Klaauw 2002).

The key benefit of the RD design is that by only exploiting its relation with a single observable variable, one does not have to choose a functional form for the way in which other (possibly unobservable) variables affect the outcome equation (van der Klaauw 2002). In our case, the legal threshold creates an exclusion restriction based on functional form: The selection process generates this assumption, and the identification does not thus rely on an assumption imposed on the distribution of an unobserved latent variable. However, special care must be taken in modeling the dependence between the selection variable and the treatment variable, and between the selection variable and the outcome variable (van der Klaauw 2002) to identify the treatment effect around the threshold.

3.2 Estimation procedure

Inspired by the method of Van der Klaauw (2002), we estimate the treatment effect by a two-step procedure. In the first stage, we specify that the expected probability of choosing a certified auditor is

$$E\left[AUDITOR_{it} \mid s_{it}\right] = f(s_{it}) + \gamma \cdot LAW_{it} \qquad (2)$$

where s_{it} is a vector of the three size variables specified in the law, $f(s_{it})$ is a (possibly non-linear) function of each of the three variables, and the indicator variable LAW specifies the discontinuity point in the treatment assignment. This first stage allows us to identify the size of the discontinuity (γ) in the propensity score function at the cutoff, which gives us the (marginal) effect of the legal requirement on the choice of a certified auditor. We initially specify the functional form of $f(s_{it})$ as a cubic function in each of the three size variables, but demonstrate later that our results are not driven by this specific choice of $f(s_{it})$.

In the second stage, the outcome equation is estimated using the first stage estimates in place of AUDITOR:

$$Y_{it} = \alpha + \beta \cdot E\left[AUDITOR_{it} \mid s_{it}\right] + k(s_{it}) + \varepsilon_{it} \qquad (3)$$

The estimated propensity score is discontinuous at the cutoff point, because of the effect of the threshold variable. We also include a control function $k(s_{it})$ on the right hand side. We specify the control function in the same way as in the first stage, i.e., as a cubic function of each of the three size variables and show later that this specific choice is not driving our results.

Our estimation procedure is equivalent to two-stage least squares using LAW and $f(s_{it})$ as the instruments. The treatment effect, β, is the local average treatment effect at the cutoff point.

4 Data

4.1 Sample description

The data come from a database compiled by Asiakastieto Ltd, a commercial vendor of financial data and a credit information company. The raw dataset contains financial data on tens of thousands Finnish SMEs over the years 1999-2002. In addition to financial data, the dataset contains information on the firms' auditors and auditing reports. It also contains a credit score, which is a commercial credit information product of Asiakastieto Ltd and which we use as an indicator of creditworthiness. The dataset is an unbalanced panel, not containing data for all the years for all of the firms.

For the purposes of this study, we only keep firms that are limited liability companies. We also exclude firms in the primary sectors, such as agriculture, forestry, and mining. Further, we use two criteria to limit our study to closely held micro-firms. First, we only keep firms that are SMEs (according to size criteria from the EU definition): employing less than 250 people, having a turnover of at most 40 million Euros, and total assets of at most 27 million. Second, we use the size criteria as implied by the second threshold in the Finnish Auditing Act to limit the sample to firms strictly below that. This restriction means that we focus on the effect of the first threshold. We thus only keep firms meeting at least two of the following three criteria: employs 50 people or less, has a turnover of 4.2 million euros or less, and has total assets of 2.1 million or less. A firm remains in the data set if it never breaks these thresholds. The thresholds are measured using values lagged by one year, which is consistent with how the law specifies these triggers. Because we need data on the lagged values of turnover, assets, and number of employees, we loose the first year of the data as well as observations for which there is no data on two consecutive years. This procedure results in an initial raw dataset of 141 112 firm-year observations.

Unfortunately, we lose a number of additional observations for various reasons: First, we drop firms with negative turnover or assets, and firms for which there is no data on auditors. Second, we remove from the data firms that are clearly in bankruptcy. To this end, we drop firms for which the equity ratio, i.e. the ratio of shareholders' capital to total assets, is negative or missing. Third, dropping firms or observations is a means to deal with outliers that are the result

from using generated accounting ratios with very small denominators (see, e.g., Dechow, 1994 and Pittman and Fortin 2004). To limit the effect of such outliers, we drop 5 percent of the observations in the upper tail of our dependent variable (see below). Fourth, we lose observations due to missing values in some of the variables that we need to construct our dependent variables. We trim, moreover, the sample to only include small businesses for whom the cost of debt capital can be meaningfully measured. We describe this measurement problem in more detail below. Our final (estimating) sample contains 78 505 firm-year observations.

4.2 Definition of variables

Dependent variables

There are two main dependent variables in the second stage of our two-step estimation procedure. The first one is the cost of debt capital. As we do not observe the cost directly, we have to estimate it. To this end, we use (scaled) financial expenses. In our data, reported financial expenses can consist of interest and other financial expenses and of foreign exchange losses. However, we know that for all practical purposes, these expenses relate to the costs of debt capital. The reason for this is that all firms in our sample are small or micro-sized: The turnover (sales) of the median firm is only 350303 euros. Such micro-firms issue external equity only very rarely and seldom have foreign-currency dominated debt or other such exposures on financial markets.

There is no agreed way of measuring the costs of debt finance when only accounting data are available. Following Pittman and Fortin (2004), we settle to the version of the dependent variable that is defined as the ratio of interest costs (as proxied by financial expenses) to average debt during the year.[9] We call this variable INTEREST.[10]

Measuring INTEREST and the effect of auditors on it is problematic for firms that have *no* debt finance and/or *no* financial expenses. An obvious reason for having no debt finance and/or no financial expenses is that a firm simply has

[9] This average debt is computed as the sum of total interest-paying debt at the end of previous year and total interest-paying debt at the end of current year, divided by two.

[10] As we already explained, we drop observations outside the upper 95th percentile of the distribution of INTEREST.

no need for external finance. A second and possibly more subtle reason for having no debt and no financial expenses is that a firm is completely excluded from the capital markets (e.g., it is rationed or redlined), or is discouraged from borrowing by prohibitively high interest rates. For such firms, the true cost of debt is clearly *not* zero. Treating these firms as if their cost of debt was zero would clearly be misleading. Because of the measurement and identification problems that these cases induce, we limit our sample to small businesses that have a positive amount of financial expenses.

The second dependent variable of ours is a proxy for creditworthiness. The proxy we use is a firm's credit score, called RATING. This variable ranges from 3 to 99, with a lower score indicating better rating and thus better creditworthiness. While we have no exact numbers, our understanding is that this score, or its derivatives, are rather widely used by Finnish credit institutions and firms that grant trade credit as an input in their creditworthiness analyses of small businesses.

The first stage of our two-step estimation procedure involves estimating a model for the choice of auditor. The dependent variable in this first-stage is AUDITOR, defined as a dummy variable equal to one if at least one of the firm's auditors is a certified auditor and equal to zero otherwise (i.e. if none of the auditors have any formal, i.e., either the lower HTM or the higher KHT, audit qualification).

Instrument and control variables

The main explanatory variable in the first stage, serving as our instrument, is the trigger specified in the Finnish Auditing Act. This variable, called LAW above, is a dummy equal to one if the firm exceeds in year t-1 the first size threshold that makes it obligatory for the firm to choose a certified accountant next year (i.e., year t). LAW is thus equal to one for firm i in year t if, in year t-1, at least two of the following three thresholds were crossed: it employed more than 10 people, had a turnover of more than 680 000 euros, or had total assets of more than 340 000 euros.

In both stages, we initially use cubic specifications of the control functions (i.e. $f(s_{it})$ and $k(s_{it})$). The included size variables are EMP, the number of employees; SALES, the firm's turnover; and ASSETS, firm's assets.

For some small firms, the number of employees is only available as a categorical variable. These categories are re-coded into a continuous variable using the average of each category (i.e., 0-4 = 2, 5-9 = 7, 10-19 = 14, 20-49 = 34, 50-99 = 74, 100-249 = 174). We acknowledge that using such a recoded variable is not entirely satisfactory, and may introduce measurement error. We therefore include into all regressions a dummy variable, EMPCAT, which is one if EMP is an outcome of this re-coding and zero otherwise. We trust that the dummy alleviates the problems that the recoding potentially creates.

5 Empirical results

5.1 Descriptive statistics

Table 1 reports descriptive statistics (mean, median, standard deviation, minimum and maximum). The mean INTEREST in the sample is about 0.05 (median = 0.055), and the mean RATING is 27 (median = 24). Roughly put, 29 percent of the firm-year observations are such that firms are large enough in size to exceed the legal threshold that triggers the use of a certified auditor. The higher mean of AUDIT indicates, therefore, that there is a lot of voluntary selection, as was expected.

[Insert Table 1 here]

Is there a discontinuity around the legal threshold in the use of a certified auditor? Table 2 addresses this question: It reports that below the threshold, AUDIT = 1 for 66% of the firm-year observations, while AUDIT = 1 for 95% of the firm-year observations above the threshold.[11] This difference could, however, be due to size factors even without the existence of a discontinuity. It is likely, for example, that only few of the smallest firms in the sample engage a certified auditor, because there is no net gain for them from doing so. We therefore tabulate the results for a

smaller sample limited to those firms that are in size near to the legal threshold: firms that have assets and turnover +/- 30% of the respective thresholds, and have employees +/-8 around the threshold. This restricted sample contains 18222 observations (about 22% of the original sample). There is evidence of a discontinuity around the threshold: above it, AUDITOR = 1 for 89% of firm-year observations but, below AUDITOR = 1 only for 76% of the observations. That is, there is a discontinuity in the proportion of firms with certified auditors, when looked at below and above the size threshold specified in the law.

[Insert Table 2 here]

In Table 2 we also present the means of INTEREST and RATING conditional on the legal threshold and on the type of auditor, both for the whole sample as well as for the sample restricted to the firms around the threshold. Firms above the threshold have a lower cost of debt and better rating than firms below it. As expected, these differences are less apparent than in the full sample. They are still visible, however.

5.2 Estimation results

Results from pooled regressions: Full sample

Table 3 presents the results from estimating the two-stage model using linear two-stage least squares (Panel A) and using a procedure in which we account for the binary nature of AUDITOR in the first stage (Panel B). In the latter procedure, the first stage is a Probit model and the predicted probabilities are used as an instrument in the second stage (see Wooldridge 2002). In the estimations of Table 3, the full sample of 78505 observations is used.

Column 1 of Panel A shows the results from the first stage estimation, in which the dependent variable is AUDIT. The threshold variable LAW is statistically significant and positive, implying that we can reject the null hypothesis that the legal requirement does not bind. This finding supports Prediction 1, and echoes our earlier univariate results.

[11] We thus observe some non-compliance.

The next two columns of the panel show the second stage estimations. They show that the effect of the mandatory use of a certified auditor on INTEREST and RATING is negative and statistically significant. The estimation results in Panel B echo this finding. These findings allow us to reject the null hypothesis that the mandated use of a certified auditor has no effect on the creditworthiness of small businesses and their cost of debt capital. Our empirical results are consistent with Prediction 2 and suggest, in particular, that asymmetric information is important in the market for small business finance.

[Insert Table 3 here]

Results from pooled regressions: Restricted sample

For Table 4 we consider the smaller sample that is restricted to firms that lie near to the legal threshold. The results of the table show that the effect of the mandated use of a certified auditor on INTEREST and RATING is, again, negative and statistically significant. There is some variation in the marginal effects, but nothing that would compromise our earlier findings.

[Insert Table 4 here]

The results in Table 4 are based on a simpler specification of the control functions than we used in the models of Table 3. We have, however, run the regressions on this smaller sample using the cubic specifications, too. The results are qualitatively similar to the ones we see in Table 4. It is comforting to report, furthermore, that when *no* control variables besides LAW (and a constant) are used, the regressions run on the restricted sample echo our basic findings.

Results from panel regressions: Full and restricted sample

Table 5 shows the results from a fixed-effects instrumental variables estimation. In these estimations, we allow for no controls expect for the fixed-effects and LAW, which serves as the instrument. The effect of AUDIT on INTEREST and RATING that we identify in these estimations arises thus solely from changes in within-firm dynamics induced by LAW.

The results show that LAW is a significant predictor of AUDIT even after fixed effects are controlled for. The effect of (instrumented) AUDIT on INTEREST and RATING is negative and statistically significant.

To put these panel data results into a proper perspective, we stress the following: First, the panel dimension of the data is very limited, as we only have three years of data. Second, our earlier estimations document that in the cross-section, small businesses that are forced to choose a certified auditor, face lower costs of debt finance and obtain a better creditworthiness rating (than their cross-sectional counterparts). Panel data estimations show, moreover, that when a given small business is forced to engage a certified auditor, the cost of debt financing to that small business goes down, and its creditworthiness improves. Within firm dynamics of INTEREST and RATING is, in other words, driven by the temporal variation in AUDIT that is due to firms hitting the legal threshold.

[Insert Table 5 here]

5.3 Discussion

In the following, we first consider and try to rule out certain alternative, data-related explanations for our empirical results. We then discuss what other theories, if any, could explain our findings.

Additional robustness tests

Robustness test 1: The inclusion of a set of new control variables does not alter our basic results. For Table 6, we have for example repeated the estimations of Table 3, but control, in addition to the size variables, for the industry sector of the firm, whether the firm is a subsidiary of a foreign company or not, whether the firm is government or municipally-owned, and firm age. The results echo our previous findings.

[Insert Table 6 here]

Robustness test 2: We have also used a more restricted sample in which we keep only firms that have assets and turnover +/- 20% of the respective thresholds,

and have employees +/-5 around the threshold. This restricted sample contains 10502 observations. Repeating the estimations of Table 4 on this sample confirms our basic finding, as the effect of AUDIT on INTEREST and RATING is negative and statistically significant. The effect of LAW on AUDIT is positive, as before.

Robustness test 3: We have also run the panel estimations (i.e. those reported in Table 5) using various combinations of control variables, but the estimations confirm our basic findings.

Alternative explanations

What alternative explanations are there for our findings besides asymmetric information? We consider three possibilities: (a) less-than fully rational behavior, (b) the role of informal financial networks, and (c), non-compliance (and adverse selection).

Less-than fully rational behavior: If we abandon the assumption of full rationality, the possibility opens that the managers of some small businesses are not behaving rationally to start with when it comes to hiring (or not) a reputable auditor. If this view is empirically valid, one might expect that when forced to use a certified auditor, such firms begin to make better (or somehow more reasonable) choices to secure their access to external finance. This is an interesting alternative explanation for our empirical findings. However, our fixed-effects estimations are robust to the criticism: Not being "rational" reflects, almost by definition, an unobserved ability of a small business owner. It can therefore be taken as a time-invariant characteristic of these firms. If that is the case, fixed firm-effects control for whatever effects variation in such unobserved ability might have both on the probability of engaging a certified auditor and on the relation between the mandatory auditor choice and the cost of capital (or creditworthiness).

Role of informal financial networks: Somewhat more compelling criticism arises from a recent analysis of Garmaise and Moskowitz (2004b), who study the effects of informal financial network on their clients' access to external finance. These authors argue convincingly that informal intermediaries, such as lawyers, accountants, consultants, property brokers, etc., facilitate the matching of borrowers with lenders. Their empirical analysis of the role of property brokers in the United States commercial real estate market provides support for the theory. Our results would be challenged, if certified auditors are non-financial intermediaries

who, like property brokers, participate in the capital market over the long-run and thus enhance matching. We respond to this challenge in two ways: First, to the extent that certified auditors *are* non-financial intermediaries who facilitate the matching of borrowers with lenders, our results are not challenged, if they do it, because auditing is a business that enables auditors to build a relationship with lenders due to their ability to alleviate the adverse effects of asymmetric information. Second, if a certified auditor cannot alleviate these adverse effects but nevertheless enhances matching due to its role as a long-run market participant, we might expect that the cost of debt for a small business goes down. However, it is not entirely clear why also the firm's creditworthiness would increase in this case.[12]

Non-compliance: Adverse selection is driven by the permanent quality differences between firms. If some firms are truly rotten apples that are not creditworthy *and* do not comply with the auditing regulation, AUDITOR might correlate negatively with INTEREST and RATING. The reason for this is that the legal threshold in the first stage regression identifies in this case the choices made by those firms that are not rotten apples (from the sub-population of firms who have not voluntarily chosen a certified auditor). Because these firms are, on average, of better quality, the negative and statistically significant effects that we identify in the second stage might follow. While this is a plausible piece of criticism, we have a mitigating response: In the fixed-effects estimations we control for the cross-sectional variation in AUDIT, INTEREST and RATING induced by the unobserved hidden types. In particular, the effect of LAW on AUDITOR is estimated *conditional* on the fixed effects being controlled for. The documented positive correlation between the threshold and AUDITOR and subsequently the negative correlation between the instrumented AUDITOR and the two outcome variables (INTEREST and RATING) should therefore reflect neither the cross-sectional variation in the propensity to engage a reputable auditor nor in the two outcome variables. It is the temporal variation in the data that allows us to separate the capital market effects of the mandated auditor choice from the joint effect of non-compliance and adverse selection.

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[12] The enhanced market access might, in itself, result indirectly in better creditworthiness, but it is not clear if such second-order feedback effect would increase the rating as much as we found. Hardly, we conjecture, but leave the question open for future research.

6 Conclusions

In this paper, we focus on the empirical implications of mandatory auditor choice. This regulatory intervention provides us with an instrument that can be used to examine the role of asymmetric information in the capital markets. The instrument is an exogenous measure of the information environment that is based on a legal requirement that makes it mandatory for firms above a certain size to use certified auditors.

The instrument makes it possible to apply a regression discontinuity design to our data. To implement the design, we follow a two-step procedure: In the first stage we regress auditor choice on a dummy variable indicating whether the firm's size exceeds the threshold specified in the legislation and a control function of various measures of firm size. The first-stage results show that the legal requirement does have a significant positive effect on the choice of a certified accountant. In the second stage we regress, together with controls, the cost of capital and creditworthiness on the instrumented auditor choice from the first stage. The second stage results show that the cost of debt capital for a small business that is forced to use a certified auditor goes down and its creditworthiness, as measured by a commercial credit rating, improves. These empirical results are robust to using different estimation techniques, sets of control variables, and variations in how we define the estimating sample.

Like Garmaise and Moskowitz (2004a) who (also) use an exogenous information measure, we find evidence that the variation in the information structure of a market matters. Our results suggest, in particular, that asymmetric information and the borrower-lender conflict are empirically relevant in the market for small business debt.

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Table 1. Descriptive statistics

	Mean	Median	S.D.	Min	Max	N
LAW	0.293	0	0.455	0	1	78505
AUDIT	0.743	1	0.437	0	1	78505
RATING	27	24	18.300	3	96	78505
INTEREST	0.055	0.050	0.039	1.05E-07	0.220	78505
EMP	7	3	9.936	0	235	78505
EMPCAT	0.335	0	0.472	0	1	78505
SALES (t€)	799.124	350.303	1214.535	0.001	30427	78505
ASSETS (t€)	598.458	206.871	1553.882	0.111	26992	78505

Notes: The table provides descriptive statistics for the full estimating sample of 78505 firm-year observations over the period 2000-2002.

Table 2. Conditional means

	FULL SAMPLE			RESTRICTED SAMPLE		
	OBS	MEAN	S.E	OBS	MEAN	S.E
AUDIT						
LAW=0	55,473	0.659	0.002	13,056	0.758	0.004
LAW=1	23,032	0.945	0.002	5,166	0.890	0.004
t-test	-87.590			-20.157		
p-value	0.000			0.000		
INTEREST						
LAW=0	55,473	0.055	1.65E-04	13,056	0.054	3.26E-04
LAW=1	23,032	0.053	2.49E-04	5,166	0.054	5.12E-04
t-test	8.506			1.362		
p-value	0.000			0.173		
AUDIT=0	20,200	0.056	2.76E-04	3,734	0.055	6.14E-04
AUDIT=1	58,305	0.054	1.59E-04	14,488	0.054	3.08E-04
t-test	6.179			2.277		
p-value	0.000			0.023		
RATING						
LAW=0	55,473	29.220	0.077	13,056	25.113	0.157
LAW=1	23,032	22.576	0.118	5,166	23.621	0.250
t-test	46.963			5.044		
p-value	0.000			0.000		
AUDIT=0	20,200	29.726	0.127	3,734	24.503	0.288
AUDIT=1	58,305	26.420	0.076	14,488	24.738	0.150
t-test	22.202			-0.710		
p-value	0.000			0.478		

Notes: The table provides conditional means for selected endogenous variables. These means are based on the full sample of 78505 firm-year observations over the period 2000-2002 and on a restricted sample that are used in the estimations. The restricted sample is limited to those firms that are in size near to the legal threshold (LAW) that triggers that mandatory use of a certified auditor: firms that have assets and turnover +/- 30% of the respective thresholds, and have employees +/-8 around the threshold.

Table 3. Full sample and non-linear size controls

	PANEL A: "LINEAR 2SLS"		
	1st stage (OLS)	2nd stage	
	AUDIT	INTEREST	RATING
AUDIT		-0.014 ***	-15.400 ***
		(0.005)	(2.540)
LAW	0.104 ***		
	(0.006)		
EMP	0.006 ***	-5.90E-05	0.094 ***
	(4.93E-04)	(5.99E-05)	(0.030)
EMP2	-1.05E-04 ***	-1.18E-06	0.001 **
	(1.07E-05)	(1.19E-06)	(6.26E-04)
EMP3	4.00E-07 ***	4.11E-09	-8.25E-06 ***
	(4.58E-08)	(4.69E-09)	(2.58E-06)
EMPCAT	-0.056 ***	-8.48E-04 **	1.670 ***
	(0.003)	(4.21E-04)	(0.206)
SALES	6.10E-05 ***	3.62E-06 ***	-0.003 ***
	(4.67E-06)	(6.80E-07)	(3.24E-04)
SALES2	-9.17E-09 ***	-3.77E-10 ***	4.81E-07 ***
	(7.32E-10)	(1.02E-10)	(4.89E-08)
SALES3	2.63E-13 ***	8.65E-15 ***	-1.39E-11 ***
	(2.36E-14)	(2.89E-15)	(1.52E-12)
ASSETS	1.15E-04 ***	-2.46E-06 ***	-6.01E-04 *
	(4.11E-06)	(7.21E-07)	(3.61E-04)
ASSETS2	-1.21E-08 ***	2.62E-10 ***	1.20E-07 ***
	(5.77E-10)	(8.09E-11)	(4.10E-08)
ASSETS3	3.22E-13 ***	-6.77E-15 ***	-3.83E-12 ***
	(1.89E-14)	(2.32E-15)	(1.18E-12)
Observations	78505	78505	78505
F-statistics	917.36		
df	11; 78493		
Significance	0.000		

	PANEL B: "PROBIT 2SLS"		
	1st stage (probit)	2nd stage	
	AUDIT	INTEREST	RATING
AUDIT		-0.019 ***	-27.700 ***
		(0.002)	(1.050)
LAW	0.228 ***		
	(0.024)		
EMP	0.029 ***	-2.31E-05	0.193 ***
	(0.002)	(4.62E-05)	(0.024)
EMP2	-4.88E-04 ***	-1.83E-06 *	-3.42E-04
	(7.03E-05)	(9.71E-07)	(5.11E-04)
EMP3	2.06E-06 ***	6.52E-09 *	-1.58E-06
	(4.07E-07)	(3.93E-09)	(2.10E-06)
EMPCAT	-0.172 ***	-0.001 ***	0.974 ***
	(0.011)	(3.24E-04)	(0.175)
SALES	2.99E-04 ***	4.08E-06 ***	-0.002 ***
	(2.21E-05)	(4.48E-07)	(2.09E-04)
SALES2	-3.96E-08 ***	-4.41E-10 ***	3.04E-07 ***
	(4.15E-09)	(7.27E-11)	(3.19E-08)
SALES3	1.17E-12 ***	1.04E-14 ***	-8.89E-12 ***
	(1.67E-13)	(2.14E-15)	(9.49E-13)
ASSETS	9.27E-04 ***	-1.90E-06 ***	9.62E-04 ***
	(2.82E-05)	(4.26E-07)	(2.17E-04)
ASSETS2	-1.22E-07 ***	2.03E-10 ***	-4.42E-08
	(6.28E-09)	(5.44E-11)	(2.76E-08)
ASSETS3	4.35E-12 ***	-5.20E-15 ***	5.43E-13
	(3.28E-13)	(1.70E-15)	(8.50E-13)
Observations	78505	78505	78505
LR statistics	12157.92		
df	11		
Significance	0.000		

Notes: The table provides two types of 2SLS estimates (linear 2SLS and Probit 2SLS) of van der Klaauw's (2002) two stage regression discontinuity model using 78505 firm-year observations over the period 2000-2002. Linear 2SLS is a standard 2SLS with first stage estimates obtained by from OLS; Probit 2SLS uses a Probit model to estimate the first stage and uses the predicted values as instruments in the second stage. *** indicates significance at 1 percent level, ** at 5 percent level and * at 10 percent level.

Table 4. Restricted sample and linear size controls

	PANEL A: "LINEAR 2SLS"		
	1st stage (OLS)	2nd stage	
	AUDIT	INTEREST	RATING
AUDIT		-0.020 **	-23.532 ***
		(0.009)	(4.491)
LAW	0.089 ***		
	(0.008)		
EMP	0.004 ***	-2.26E-05	0.274 ***
	(7.81E-04)	(9.67E-05)	(0.051)
EMPCAT	-0.057 ***	-0.002 **	2.480 ***
	(0.006)	(7.83E-04)	(0.413)
SALES	2.99E-05 ***	4.09E-06 ***	0.001 ***
	(7.12E-06)	(9.68E-07)	(4.21E-04)
ASSETS	2.23E-05 ***	1.62E-07	2.66E-04 *
	(2.42E-06)	(3.23E-07)	(1.47E-04)
Observations	18222	18222	18222
F-statistics	124.48		
df	5; 18216		
Significance	0.000		

	PANEL B: "PROBIT 2SLS"		
	1st stage (probit)	2nd stage	
	AUDIT	INTEREST	RATING
AUDIT		-0.036 ***	-21.351 ***
		(0.006)	(2.859)
LAW	0.277 ***		
	(0.033)		
EMP	0.019 ***	1.10E-04	0.257 ***
	(0.003)	(8.43E-05)	(0.042)
EMPCAT	-0.199 ***	-0.003 ***	2.605 ***
	(0.023)	(7.27E-04)	(0.357)
SALES	1.44E-04 ***	5.04E-06 ***	0.001 ***
	(3.07E-05)	(8.70E-07)	(3.67E-04)
ASSETS	4.94E-04 ***	6.02E-07 **	2.07E-04 *
	(4.73E-05)	(2.83E-07)	(1.11E-04)
Observations	18222		
LR statistics	804.11		
df	5		
Significance	0.000		

Notes: The table provides two types of 2SLS estimates (linear 2SLS and Probit 2SLS) of van der Klaauw's (2002) two stage regression discontinuity model using the restricted sample that is limited to those firms that are in size near to the legal threshold (LAW) that triggers that mandatory use of a certified auditor: firms that have assets and turnover +/- 30% of the respective thresholds, and have employees +/-8 around the threshold. Linear 2SLS is a standard 2SLS with first stage estimates obtained by from OLS; Probit 2SLS uses a Probit model to estimate the first stage and uses the predicted values as instruments in the second stage. *** indicates significance at 1 percent level, ** at 5 percent level and * at 10 percent level.

Table 5. Fixed-effects estimations (full and restricted sample)

	2 STAGE FIXED EFFECTS (WITHIN) ESTIMATION		
	1st stage	2nd stage	
	AUDIT	INTEREST	RATING
PANEL A: FULL SAMPLE			
AUDIT		-0.076 ***	-54.517 ***
		(0.024)	(11.289)
LAW	0.034 ***		
	(0.004)		
Observations	78505	78505	78505
F-statistics	68.68		
df	1; 36123		
Significance	0.000		
PANEL B: RESTRICTED SAMPLE			
AUDIT		-0.056 **	-38.165 ***
		(0.028)	(13.048)
LAW	0.035 ***		
	(0.006)		
Observations	18222	18222	18222
F-statistics	37.65		
df	1; 6679		
Significance	0.000		

Notes: The table provides fixed-effects 2SLS estimates of van der Klaauw's (2002) two stage regression discontinuity model using both the full sample (78505 firm-year observations over the period 2000-2002) and a restricted sample that is limited to those firms that are in size near to the legal threshold (LAW) that triggers that mandatory use of a certified auditor: firms that have assets and turnover +/- 30% of the respective thresholds, and have employees +/-8 around the threshold. *** indicates significance at 1 percent level, ** at 5 percent level and * at 10 percent level.

Table 6. Full sample, non-linear size and additional control variables

	PANEL A: "LINEAR 2SLS"		
	1st stage (OLS)	2nd stage	
	AUDIT	INTEREST	RATING
AUDIT		-0.011 **	-12.232 ***
		(0.005)	(2.493)
LAW	0.104 ***		
	(0.006)		
EMP	0.006 ***	-1.73E-04 ***	0.161 ***
	(5.17E-04)	(6.35E-05)	(0.031)
EMP2	-1.20E-04 ***	1.23E-06	4.58E-04
	(1.09E-05)	(1.25E-06)	(6.34E-04)
EMP3	4.57E-07 ***	-4.70E-09	-4.91E-06 *
	(4.63E-08)	(4.90E-09)	(2.55E-06)
EMPCAT	-0.058 ***	-3.68E-04	1.720 ***
	(0.003)	(4.25E-04)	(0.203)
SALES	7.42E-05 ***	2.30E-06 ***	-0.005 ***
	(4.90E-06)	(7.42E-07)	(3.62E-04)
SALES2	-1.07E-08 ***	-2.28E-10 **	6.18E-07 ***
	(7.47E-10)	(1.07E-10)	(5.46E-08)
SALES3	3.03E-13 ***	4.83E-15	-1.71E-11 ***
	(2.39E-14)	(2.99E-15)	(1.76E-12)
ASSETS	9.52E-05 ***	-8.08E-07	8.67E-04 ***
	(4.38E-06)	(6.44E-07)	(3.16E-04)
ASSETS2	-1.05E-08 ***	1.43E-10 *	-4.05E-08
	(5.92E-10)	(7.52E-11)	(3.69E-08)
ASSETS3	2.86E-13 ***	-4.20E-15 *	5.68E-13
	(1.92E-14)	(2.21E-15)	(1.07E-12)
AGE	2.91E-04 **	-1.10E-04 ***	-0.291 ***
	(1.46E-04)	(1.34E-05)	(0.007)
SUBSIDIARY	0.060 ***	-0.008 ***	4.399 ***
	(0.013)	(0.001)	(0.593)
PUBLIC	0.077 ***	-0.008 ***	2.518 ***
	(0.014)	(0.001)	(0.508)
INDUSTRY	Yes	Yes	Yes
Observations	78505	78505	78505
F-statistics	299.86		
df	36; 78468		
Significance	0.000		

	PANEL B: "PROBIT 2SLS"		
	1st stage (probit)	2nd stage	
	AUDIT	INTEREST	RATING
AUDIT		-0.014 ***	-25.058 ***
		(0.002)	(1.006)
LAW	0.232 ***		
	(0.025)		
EMP	0.030 ***	-1.39E-04 ***	0.272 ***
	(0.003)	(4.86E-05)	(0.025)
EMP2	-5.54E-04 ***	6.11E-07	-0.002 ***
	(7.21E-05)	(9.92E-07)	(5.15E-04)
EMP3	2.39E-06 ***	-2.38E-09	2.82E-06
	(4.16E-07)	(3.97E-09)	(2.09E-06)
EMPCAT	-0.177 ***	-5.93E-04 *	0.970 ***
	(0.011)	(3.22E-04)	(0.169)
SALES	3.65E-04 ***	2.75E-06 ***	-0.003 ***
	(2.34E-05)	(4.81E-07)	(2.26E-04)
SALES2	-4.82E-08 ***	-2.90E-10 ***	4.14E-07 ***
	(4.17E-09)	(7.40E-11)	(3.40E-08)
SALES3	1.43E-12 ***	6.55E-15 ***	-1.14E-11 ***
	(1.66E-13)	(2.11E-15)	(1.07E-12)
ASSETS	8.37E-04 ***	-3.94E-07	0.002 ***
	(2.98E-05)	(4.25E-07)	(2.11E-04)
ASSETS2	-1.10E-07 ***	9.79E-11 *	-1.91E-07 ***
	(6.39E-09)	(5.42E-11)	(2.62E-08)
ASSETS3	3.88E-12 ***	-2.97E-15 *	4.66E-12 ***
	(3.26E-13)	(1.71E-15)	(7.91E-13)
AGE	1.49E-05	-1.09E-04 ***	-0.286 ***
	(5.87E-04)	(1.33E-05)	(0.007)
SUBSIDIARY	1.026 ***	-0.007 ***	5.143 ***
	(0.127)	(0.001)	(0.586)
PUBLIC	0.429 ***	-0.007 ***	3.520 ***
	(0.075)	(9.60E-04)	(0.506)
INDUSTRY	Yes	Yes	Yes
Observations	78505	78505	78505
LR statistics	12817.70		
df	36		
Significance	0.000		

Notes: The table provides two types of 2SLS estimates (linear 2SLS and Probit 2SLS) of van der Klaauw's (2002) two stage regression discontinuity model using 78505 firm-year observations over the period 2000-2002. Linear 2SLS is a standard 2SLS with first stage estimates obtained by from OLS; Probit 2SLS uses a Probit model to estimate the first stage and uses the predicted values as instruments in the second stage. *** indicates significance at 1 percent level, ** at 5 percent level and * at 10 percent level.

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