

Auditor Choice and the Pricing of Initial Public Debt Issues

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Abstract: We examine the importance of auditor choice to the pricing of debt initial public offerings. These securities raise information asymmetry by expanding firms' debt ownership structures to include public bondholders and by extending debt maturity. Prior research on initial equity issues has had difficulty discriminating between two motives for choosing a Big Five auditor: reducing the uncertainty about firms' financial statements and providing greater implicit insurance coverage in the event of audit failure. Results from separately estimating the impact of auditor choice on the perceptions of credit rating agencies and bondholders suggest that initial bond pricing is driven by both information and insurance incentives, with each responsible for lowering at-issue yield spreads by about 19 basis points, on average. We also report evidence consistent with the prediction that firms intending to replace bank debt with public debt, which reduces cross-monitoring among lenders and increases default risk according to extant research, benefit more from hiring a Big Five auditor.

JEL Classification : D80; L14; G32; M40

Key words: audit quality; firm-lender relationships; asymmetric information; contracts and reputation

* Contact author. We appreciate helpful comments on earlier drafts from Neil Fargher, Pelham Gore, Jane Hamilton, Jack Hughes, Clive Lennox, Michel Magnan, and participants at the 2004 International Symposium on Audit Research and the 2004 Canadian Academic Accounting Association Annual Conference. We also thank the Social Sciences and Humanities Research Council for generous financial support.

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1. Introduction

Extant research on the importance of auditor selection to the pricing of equity initial public offerings has had difficulty distinguishing the information rationale, which stems from high-quality auditors leading to more precise financial reporting, from the insurance rationale, which stems from Big Five auditors appearing to better indemnify investors against future losses. We contribute to the understanding of this issue by estimating the impact of choosing a Big Five auditor on the credit ratings and yield spreads on firms' initial straight debt offers. By separately examining the perceptions of both ratings agencies and bondholders, our tests discriminate between the information and insurance roles of auditor choice, respectively, in the pricing of these securities.¹ In addition, we provide evidence on whether firms refinancing their bank loans, which reduces cross-monitoring among lenders and increases default risk, have more incentive to hire a Big Five auditor. Accordingly, this paper responds to calls from Kinney (1987) and Healy and Palepu (2001) for research on the economic implications of audits to financial statement users.

We investigate whether auditor choice matters to the cost of firms' initial public bond offerings for several reasons. First, the significant financing changes that accompany

¹ An initial public offering (IPO) of debt is defined as the first public offering of debt after the initial public offering of equity. Other studies examine the debt market implications of auditor choice by public firms. Pittman and Fortin (2004) provide evidence consistent with their prediction that selecting a high-quality auditor affects firms' interest rates less over time as informational asymmetry gradually subsides with age. They also review the extensive literature on the role of auditor choice in firms going public. Our research design for isolating the information and insurance benefits of choosing a Big Five auditor is similar to Mansi et al.'s (2004) study of seasoned bonds in the secondary market. For expositional convenience, we refer to the set of Big Five public accounting firms and their predecessors as the Big Five auditors.

these issues provide an opportune setting to examine the influence of retaining a Big Five auditor on debt pricing. Specifically, entering the public bond market usually results in a major shift in firms' capital structure policy by expanding the debt ownership structure to include public bondholders and by extending debt maturity.

Second, the relatively young, small firms that typically conduct first public debt offers suffer from greater informational asymmetry since they have short histories (Diamond, 1989 and Lang, 1991). Adverse selection and moral hazard problems may affect these firms more, which suggests that they would particularly benefit from the presence of a high-quality auditor. Third, examining initial public bond issues avoids confounding from monitoring by existing bondholders, enabling our design to isolate the link between bank cross-monitoring and auditor choice in the debt pricing under study. Fourth, identifying the determinants of these borrowing costs is important since young firms demand relatively more external financing (Rajan and Zingales, 1998). Finally, there is more variability in the choice between Big Five and non-Big Five auditors in firms' early public years that coincide with their first bond offers (Pittman and Fortin, 2004).

Consistent with the information and insurance motives for auditor choice, we find that credit ratings are higher and at-issue yield spreads lower for initial bond issuers' with Big Five auditors. After controlling for other firm and security characteristics, we estimate that the separate economic impact of engaging a Big Five auditor on the perceptions of credit rating agencies and bondholders is each equivalent to about 19 basis points, on average. We also report evidence supporting the predictions that auditor choice has a stronger impact on the ratings and yield spreads on the initial public debt of firms intending to refinance their bank loans. This implies that choosing a Big Five auditor

renders financial statements even more informative when there is less cross-monitoring among lenders and provides greater insurance protection when default risk rises because firms are less apt to restructure privately when bank debt subsides.

The rest of this paper is organized as follows. Section 2 further develops the motivation for the empirical tests, while Section 3 outlines the sample selection. Section 4 specifies and reports descriptive statistics on the regression variables. Section 5 reviews the primary estimation design and presents results for the debt pricing tests. The conclusions in Section 6 discuss implications for future research.

2. Motivation

Debt initial public offerings engender important changes to firms' financial policies that may affect the impact of auditor choice on the pricing of these securities.

Introducing publicly placed debt into firms' capital structures may lessen monitoring relative to that which ordinarily occurs on private borrowing. Most private lenders are institutions such as commercial banks and life insurance companies that specialize in conducting comprehensive credit evaluations before a debt issue and monitoring firm performance afterward. Financial intermediation theory suggests that these lenders as inside claimants have access to better information than outside claimants such as bondholders, who must rely on publicly available sources.

Chan (1983), Diamond (1984) and others argue that this information superiority enables private lenders to exploit their comparatively lower costs in monitoring loan agreements and enforcing restrictive covenants, which in turn reduces the adverse

selection and moral hazard costs of new debt financing.² Preece and Mullineaux's (1996) evidence on lending syndicates indicates that the efficient monitoring of these private debt contracts lowers firms' borrowing costs. In contrast, the free-rider problem arising from dispersed public debt ownership deters individual bondholders from undertaking costly information production and monitoring (Grossman and Hart, 1980). Accordingly, firms appear to endure higher agency costs on public debt issues relative to private loans.

Apart from changing the composition of private and public debt in firms' capital structures, James (1987) and Datta et al. (2000) report that bond issues substantially extend average debt maturity. Flannery (1986) and Kale and Noe (1990) model that firm quality is decreasing in debt maturity. Similarly, Easterbrook (1984) argues that high quality firms constrain the agency cost of monitoring by issuing shorter maturity debt, which commits them to the scrutiny of periodic reviews. In Myers (1977), only firms with no growth options issue long maturity debt since these firms are not concerned with having to eventually share the proceeds of exercising such options with bondholders.

Consistent with these theories of debt ownership structure and maturity, Datta et al. (2000) find that bond IPO announcements generate a negative stock price reaction.³ This evidence differs from previous research that detects positive reactions to announcements of seasoned straight public debt offers (Eckbo, 1986 and Shyam-Sunder, 1991) as well as

² Fama (1985) points out that banks often negotiate representation on the board of directors to monitor the impact of borrowers' decisions on default risks, which supports the argument that institutional lenders obtain proprietary firm-specific information. Sharpe (1990) suggests that long-term lending relationships permit banks to learn more than others about borrowers, while Carey et al. (1993) provide evidence that insurance companies have strong credit monitoring units.

³ Further, Datta et al. (2002) and Patel (2002) observe severe stock price and operating underperformance after firms' initial debt issues, respectively, which they interpret as also supporting these theories of debt ownership structure and maturity.

bank loan agreements (James, 1987) and renewals (Lummer and McConnell, 1989). Gande et al. (1997) and Fenn (2000) report that bondholders insist on higher yields on firms' first debt issues to reflect the greater uncertainty about these securities.

Since initial bond issues worsen informational asymmetry, higher borrowing costs will result unless the firm pursues ways to mitigate debt-related monitoring costs. Ball (2001) and Healy and Palepu (2001) argue that audited financial statements enable firms to reduce information problems when raising capital.⁴ Statement of Financial Accounting Concepts (SFAC) No. 1 describes providing information that is relevant to investors and creditors as the primary purpose of financial statements; practitioners contend that the primary purpose of auditing is to improve the reliability of this information; e.g., Elliott and Jacobson (1998). In fact, the importance of firms' financial reporting – and, consequently, the role of external auditors as information intermediaries – to the debt contracting process is evident from covenants that typically depend on accounting numbers (Smith and Warner, 1979 and Dichev and Skinner, 2002).

Easley and O'Hara (2004) show that greater precision in firms' financial reporting lowers their cost of capital. However, managers responsible for preparing financial statements have some discretion in their choice of accounting policies and estimates, which can undermine the value of firms' disclosures for monitoring (Watts and Zimmerman, 1986). Credit rating agencies will naturally protect against excessively opportunistic reporting decisions by assigning lower ratings to firms not retaining a Big

⁴ Smith and Warner (1979) and Leftwich (1983) report evidence implying that audited financial statements affect debt contracting.

Five auditor, which has a brand name reputation for providing a higher-quality audit according to extensive analytical and empirical research.⁵

As the credibility of firm disclosures affect debt pricing (Sengupta, 1998), hiring a Big Five auditor may help resolve costly contracting problems by producing valuable information about borrowers (Dopuch and Siminuc, 1982 and Watts and Zimmerman, 1986). Prior evidence suggests that external monitoring by a Big Five auditor constrains managerial discretion in the financial reporting of firms experiencing serious information problems; e.g., Becker et al. (1998) and Francis et al. (1999). Francis and Wilson (1988) and DeFond (1992) report that the demand for high-quality audits is increasing in firms' agency costs. Collectively, extant research implies that financial statements are more informative when the firm has a Big Five auditor. This is an important issue since debt contracts contain incentives for firms to avoid violating covenants by increasing earnings through accounting changes (Sweeney, 1994) and accruals (DeFond and Jiambalvo, 1994) and by reclassifying liabilities on their balance sheets (Gramlich et al., 2001).

Our inquiry begins with tests that estimate the influence of auditor choice on moderating costly information problems in initial straight bond offerings. Specifically, we measure the impact of retaining a Big Five auditor on the credit ratings on these securities. We predict that ratings will impound that the presence of a high-quality

⁵ Among others, Balvers et al. (1988) and Beatty (1989) find that high-reputation auditors enable entrepreneurs to reduce the extent of *ex ante* uncertainty in new equity issues. Besides research on new share issues, other studies argue that the larger public accounting firms are brand name suppliers of higher-quality audits (see summary in Weber and Willenborg, 2003). Finally, extant evidence that Big Five auditors obtain price premia also suggests the presence of differentiation on quality among public accounting firms; e.g. Palmrose (1986) and Craswell et al. (1995). Craswell et al. explain that although all auditors must comply with minimum professional standards, the Big Five firms voluntarily invest in higher levels of expertise and have incentives to conduct higher quality audits to protect their brand name reputations.

auditor enhances the credibility of firms' financial statements, which leads to the following hypothesis (all hypotheses are stated in alternate form):⁶

H₁: Selecting a Big Five auditor will improve the credit ratings on firms' initial straight debt offers.

Beyond its role in reducing uncertainty about firms' financial statements, lenders may perceive that a Big Five auditor provides more implicit insurance coverage in the event of audit failure (Dye, 1993). Menon and Williams (1994) and Baber et al. (1995) attribute the impact of the Laventhol & Horwath bankruptcy on the share prices of their clients to investors relying on the auditor to recover potential losses in the form of damages. However, both sets of authors concede that examining these security price reactions prevents cleanly separating whether information or insurance explanations are responsible for this evidence. The same identification problem affects research on the importance of auditor selection to the pricing of initial equity issues.⁷

We extend prior research that focuses strictly on initial equity prices by exploiting the structure of the market for initial public debt to help unravel the extent that auditor choice is driven by information and insurance incentives. Similar to Mansi et al. (2004), we assume that bond ratings fully reflect the information value in auditor identity since credit

⁶ Moody's (2003) explain that its credit rating amounts to an "opinion of the future ability, legal obligation, and willingness of a bond issuer or other obligor to make full and timely payments on principal and interest due investors." Mansi et al. (2004) cite other Moody's comments indicating that their ratings represent estimated default probabilities to justify assuming that examining ratings captures perceptions about whether the hiring of a Big Five auditor lends credibility to firms' financial statements, rather than any insurance implications. Further, credit rating agencies have persuaded the courts that they are not subject to a negligence standard liability for their ratings because of the protection afforded by the First Amendment. In *County of Orange vs. McGraw Hill* 245 B.R. 151 (C.D. Cal. 1999), the court ruled that the county had to demonstrate malice by Standard & Poor's to successfully sue over flawed municipal bond ratings.

⁷ In a notable exception to other studies on equity initial public offerings, Willenborg (1999) cleverly avoids this problem by isolating start-up firms that have hardly any revenues or audit-intensive assets.

analysts at rating agencies are privy to substantial inside information on firms, including the role of their auditors in the financial reporting process. For example, Graham et al. (2004) report that CFOs provide much more detailed disclosures, such as internal EPS projections, to credit analysts than stock analysts or institutional investors.⁸ Accordingly, after controlling for the information intermediation in ratings, we isolate the insurance motivation for choosing a Big Five auditor by measuring its incremental impact on firms' borrowing costs. In the absence of a Big Five auditor with supposedly "deep pockets", bondholders will require higher yields to compensate for the lower insurance coverage:⁹

H₂: Selecting a Big Five auditor will lower the at-issue yield spread on initial straight debt offers.

Our third prediction examines whether external monitoring by a Big Five auditor becomes more valuable when the purpose of the initial bond issue is to refinance bank debt. Among others, Larcker and Richardson (2004) and Pittman and Fortin (2004) stress the importance of factoring other monitoring mechanisms for mitigating agency problems into research designs that explore audit quality. Borrowing from banks, which have access to better information about firms through their loan monitoring and customer

Given that greater precision in these firms' financial statements is almost certainly immaterial to investors, this design enables Willenborg to more conclusively measure the insurance value of auditor choice.

⁸ Although Regulation Fair Disclosure (FD) prohibits public firms after October 23, 2000 from making selective disclosures to favored equity analysts, credit rating agencies have been granted an exemption under FD that ensures that they will continue to have access to confidential information.

⁹ Major rating agencies specifically identify the quality of information disclosure as a determinant of their ratings. However, to the extent that the assumption that ratings perfectly incorporate the information value in auditor choice is not valid, its impact on at-issue yield spreads represents further resolution of uncertainty about firms' financial statements. We note that studying newly issued rather than seasoned bonds avoids the potential problem of delays in updating the credit ratings contaminating our evidence on the information value of a Big Five auditor. In a particularly prominent example, both Moody's and S&P continued to assign an investment grade rating on Enron until four days before its bankruptcy despite earlier clear indications about the company's deteriorating financial condition. In academic research, Pinches and Singleton (1978) report that stock prices impound the information in bond rating changes up to a year ahead of the change. Finally, given the serious information problems that accompany security issuances, offering ratings should be more sensitive to perceived accounting transparency.

relationship activities, lowers the contracting costs of other lenders (Diamond, 1984 and Fama, 1985). Consistent with predictions from financial intermediation theory, Gande et al. (1997) and Datta et al. (1999) find that bond financing is cheaper for firms with bank loans outstanding. Choi (2004) interprets evidence that discretionary accruals are decreasing in both the strength and duration of bank-borrower relationships as supporting that banks reduce information risk in financial reporting. Overall, prior research implies that auditor identity will matter more when the firm intends to replace private debt with public debt since these are more information-sensitive securities. We examine the role of auditor choice in the pricing of initial bond issues involving a reduction in bank cross-monitoring with the following hypothesis:

H_{3A}: Selecting a Big Five auditor will improve the credit ratings on initial straight debt offers more for firms refinancing bank loans with the issue proceeds.

Gilson et al. (1990) document that firms are less likely to restructure privately as the fraction of public debt to total liabilities increases. Pittman and Klassen (2001) review other theory and evidence that private lenders are more apt to renegotiate debt contracts and to selectively relax loan covenants before disputes reach the bankruptcy stage. The link between bank debt and default probability suggests that bondholders will perceive that the value of the auditor's implicit insurance coverage depends on the purpose of the bond issue, which motivates the following hypothesis:

H_{3B}: Selecting a Big Five auditor will lower the at-issue yield spread on initial straight debt offers more for firms refinancing bank loans with the issue proceeds.

3. Sample Formation

We began compiling our sample by extracting from Thomson Financial's SDC database all domestic initial straight public bond issues that firms conducted between

January 1970 and December 2001. Thomson Financial collects this data from prospectuses, regulatory filings, company press releases, and news services. We follow Datta et al.'s (1999) procedure for distinguishing initial from seasoned issues. This involves meticulously tracing each of the issuers to various *Moody's Manuals* to determine if the firm had previously sold debt securities to the public.

This initial screening identifies 346 issuers (398 straight-debt securities) making their first public debt issue, with some firms issuing more than one category of debt to the public on their IPO date. Forty-two issuers (48 securities) are not listed in the *CRSP* stock database, which is essential for computing some of the regression variables. Another 62 issuers (65 securities) are not listed on *Compustat*, or data that we require for some regression variables is missing. Similarly, we could not retrieve other data for 22 issuers (24 securities) from various *Moody's Manuals*. Finally, we remove from the sample six securities with implausible data in SDC.¹⁰

Table 1 summarizes the selection criteria that were applied to assemble the sample, which consists of 215 first issuers that sold 255 different straight debt securities to the public. Although this relatively small sample means that our tests suffer from low power that works against rejecting the null hypotheses, this setting also provides distinct advantages. For example, we can examine the link between auditor choice and bank cross-monitoring in public debt pricing without confounding from the presence of existing bondholders. The considerable prior finance research on initial bond issues

¹⁰ The results on H_1 to H_{3B} persist after dropping securities with high Cook's distance values to ensure that our evidence represents pervasive economic phenomena, rather than influential observations dominating the data.

reflects that this is a unique testing ground despite the resulting small samples; e.g., Datta et al. (1999, 2000) and Patel (2002).

All regression results reported in this paper are weighted by the number of securities issued on the straight debt IPO date to control for firms that issued more than one security that day. The sample's calendar year and industry distributions (see Table 2) closely resemble the population's distributions according to our check of the data. There is ample dispersion in the sample with no single 2-digit SIC code (not reported) or year contributing more than 10 percent of the observations.

4. Research Design – Debt Pricing Tests

In this section, we focus on developing the empirical methodology for isolating the information and insurance motives for auditor choice by separately measuring its impact on the perceptions of credit rating agencies and bondholders, respectively. We begin by examining the prediction in H₁ that choosing a Big Five auditor leads to higher ratings by lending more credibility to initial bond issuers' financial statements. Given credit rating agencies' role as information intermediaries, we expect that their ratings will impound that the presence of a low-quality auditor renders financial statements less precise. We estimate the following model using ordinary least squares to test this cross-sectional relation by regressing bond ratings on auditor quality and control variables:¹¹

$$\begin{aligned}
 \text{Rating} = & \alpha_0 + \alpha_1 \text{Big Five} + \beta_1 \text{Purpose} + \beta_2 \text{Underwriter} + \beta_3 \text{Callable} + \beta_4 \text{Sinking} \\
 & + \beta_5 \text{Dividend} + \beta_6 \text{Coupon} + \beta_7 \text{Firm Size} + \beta_8 \text{Firm Age} + \beta_9 \text{Leverage} + \beta_{10} \\
 & \text{Volatility} + \beta_{11} \text{Industrial} + \gamma_{1970, \dots, 2000} \text{Year} + \varepsilon
 \end{aligned} \tag{1}$$

¹¹ Estimating an ordered probit model – given that bond ratings are ordinal – using a seven-way ratings classification that represents unrated to the highest rated issues (bonds rated Aaa by Moody's and AAA by Standard & Poor's) does not qualitatively affect our evidence on H₁.

Next, we examine the importance of retaining a Big Five auditor to at-issue yield spreads after controlling for ratings and other determinants of initial bond pricing. In this specification, we follow Mansi et al. (2004) by relying on credit ratings to capture the information value of auditor choice, enabling us to cleanly distinguish its insurance value. Accordingly, the following model suits testing the prediction in H₂ that hiring a Big Five auditor will lower borrowing costs to reflect the greater implicit insurance protection:

$$\begin{aligned}
 \text{Yield Spread} = & \alpha_0 + \alpha_1 \text{Big Five} + \beta_1 \text{Rating} + \beta_2 \text{Unrated} + \beta_3 \text{Purpose} + \beta_4 \\
 & \text{Underwriter} + \beta_5 \text{Callable} + \beta_6 \text{Sinking} + \beta_7 \text{Dividend} + \beta_8 \text{Coupon} + \\
 & \beta_9 \text{Firm Size} + \beta_{10} \text{Firm Age} + \beta_{11} \text{Leverage} + \beta_{12} \text{Volatility} + \beta_{13} \\
 & \text{Industrial} + \gamma_{1970, \dots, 2000} \text{Year} + \varepsilon
 \end{aligned} \tag{2}$$

We then measure the joint influence of the information and insurance aspects of auditor choice on at-issue yield spreads. This requires re-estimating equation (2) after removing the auditors' information impact from the other explanatory variables. Specifically, we replace the raw credit rating with the orthogonalized transformation of this rating, which is the residual from regressing auditor choice and the control variables on credit ratings. Since this residual is orthogonal to each of the right hand side variables, we can interpret the transformed rating as representing the portion of the raw rating that is not driven by the test and other control variables. By subtracting the coefficient estimate on *Big Five* in equation (2), which identifies the insurance value of auditor choice, from the corresponding coefficient in the following model, we can gauge its incremental information value to initial public debt pricing:¹²

$$\begin{aligned}
 \text{Yield Spread} = & \alpha_0 + \alpha_1 \text{Big Five} + \beta_1 \text{Orthogonalized Rating} + \beta_2 \text{Unrated} + \beta_3 \\
 & \text{Purpose} + \beta_4 \text{Underwriter} + \beta_5 \text{Callable} + \beta_6 \text{Sinking} + \beta_7 \text{Dividend} + \\
 & \beta_8 \text{Coupon} + \beta_9 \text{Firm Size} + \beta_{10} \text{Firm Age} + \beta_{11} \text{Leverage} + \beta_{12} \\
 & \text{Volatility} + \beta_{13} \text{Industrial} + \gamma_{1970, \dots, 2000} \text{Year} + \varepsilon
 \end{aligned} \tag{3}$$

¹² Results are nearly identical when we re-specify the orthogonalized rating as the residual from regressing just the raw credit rating on *Big Five*.

Finally, we investigate whether auditor choice affects initial bond pricing more for firms intending to repay their bank loans. We test the predictions in H_{3A} and H_{3B} that choosing a Big Five auditor will have a stronger impact on credit ratings and at-issue yield spreads in these situations by re-estimating the equations after bisecting the main sample according to whether the firm will refinance private debt with public debt.

We specify *Rating*, which is the dependent variable in equation (1) and an explanatory variable in equation (2), as Moody's bond rating converted to an ordinal scale by assigning a value of 19 to those rated Aaa, 18 to those rated Aa1, and so forth. We reserve the lowest value for issues that are unrated.¹³ The rating is taken as the average of Moody's and Standard & Poor's ratings whenever both are available because Elton et al. (2002) report that rating differences convey additional information that affects bond pricing.¹⁴ This numeric rating incorporates information about minor rating levels while conserving degrees of freedom that are precious since we have a fairly small data set. In the yield-spread regressions, we also specify a dummy variable, *Unrated*, one when neither credit rating agency rates the bond issue and zero otherwise.

The dependent variable, *Yield Spread*, in equations (2) and (3) is the difference in basis points between the at-issue yield for the initial public straight debt offer and the yield of a Treasury bond with similar maturity and coupon rate on the same day (Datta et al.,

¹³ Although inspecting average yield spreads for each minor rating class indicates that the unrated issues are not the lowest quality bonds in the sample, our evidence is virtually identical when we assign these issues the same numeric rating as the minor rating class with the nearest average yield spread.

¹⁴ Since we later compare the magnitude of the coefficient estimates on *Big Five* from equations (1) to (3) and the regressions are connected through the orthogonalization, we consistently measure *Rating* as the average of Moody's and S&P's minor ratings classes. This specification imposes linearity on the relation between bond ratings and yields spreads. However, our evidence on the prediction in H₂ on the insurance value of auditor choice is very similar when we re-specify this variable as the natural logarithm of one plus the numeric rating to reflect that its influence on the yield spread may decline as bond quality increases

1999).¹⁵ Extensive finance research links information asymmetry to this spread (Smith, 1986), which represents the risk premium required by bondholders.¹⁶

We assign the primary test variable, *Big Five*, a value of one when the firm retains a Big Five auditor and zero otherwise. Our models control for other potential determinants of debt pricing, including issue and firm characteristics. The choice and specification of these variables generally follows prior research; e.g. Gande et al. (1997), Datta et al. (1999) and Mansi et al. (2004). For simplicity, we outline the predicted impact of these controls on yield spreads; the predictions for ratings usually run in the opposite direction.

4.1 Issue Characteristics

The dummy variable, *Purpose*, has the value of one if the purpose of the issue is to refinance bank debt and zero otherwise (Gande et al., 1997). Private lenders that have superior information on borrowers can provide efficient monitoring that, in turn, can reduce the contracting costs on initial bond issues. We predict a positive relation between *Purpose* and interest rates driven by the reduction in bank cross-monitoring that accompanies replacing private debt with public debt.

(Datta et al., 1999 and Fenn, 2000) or simply control for this determinant with dummy variables for Moody's major ratings classes (we use the equivalent S&P rating when Moody's does not rate the issue).

¹⁵ The fixed-income literature frequently uses this measure to determine the debt risk premium (Duffie, 1998). In situations in which there is no equivalent Treasury maturity, we rely on interpolation to determine yields (Anderson et al., 2003); our results concerning auditor choice are virtually identical when we exclude these issues. We restrict our sample to straight debt to avoid the difficulty of measuring yields on convertible issues and preferred stock. Since we match our sample of corporate bonds to a government bond have similar maturity when estimating yields spreads, we avoid having to specify a separate control variable for bond maturity.

¹⁶ At-issue yield spreads are more directly observable than alternate information asymmetry measures that are more difficult to quantify. For example, this avoids the complications of estimating firms' interest rates using financial statement data (Pittman and Fortin, 2004).

Carter and Manaster (1990) and Carter et al. (1998) attribute evidence that there is less underpricing in equity IPOs taken public by prestigious underwriters to their superior certification of these firms. In our regressions, we control for this predicted inverse relation with *Underwriter*, which we specify as the prestige ranking according to the 10-point scale in Carter et al. updated by Loughan and Ritter (2004).¹⁷ We expect that callable issues, which carry prepayment risk detrimental to bondholders, will have higher yield spreads (Datta et al., 1999). *Callable* takes the value of one if the security has a call provision and zero otherwise.

It is difficult to make a directional prediction about the impact that the presence of a sinking fund will have on a bond's yield. Although a sinking fund protects bondholders by increasing the probability that they will collect the principal portion of the debt, riskier issues tend to have a sinking fund provision (Smith and Warner, 1979). Consequently, we make no prediction about the sign of the coefficient on the indicator variable, *Sinking*, which is equal to one when there is a sinking fund provision and zero otherwise. Similarly, the predicted sign is left indeterminate on *Dividend*, which we assign one if the issue has a covenant restricting the firm from paying dividends and zero otherwise.

Elton et al. (2001) provide evidence that tax premiums are partly responsible for the rate spread between corporate and government bonds. This premium stems from corporate bonds requiring a higher pre-tax return to compensate for the taxes incurred by investors relative to the tax-free payments on government bonds (Scholes and Wolfson,

¹⁷ Loughran and Ritter's (2004) prestige ratings, which are based on the pecking order in "tombstone" advertisements, only cover 1980 to 2000, although descriptive statistics strongly suggest that underwriter reputations are stable over time. Accordingly, we rely on the earliest available rating for the underwriter variable for the 1970-1979 bond issues in our sample. Our results are not sensitive to dropping *Underwriter* from the regressions.

1992). As investors are liable for state and local taxes on the entire coupon of corporate bonds, rather than just the premium over treasuries, we predict that the yield spread will widen with the *Coupon* rate (Elton et al., 2002 and Campbell and Taksler, 2003).¹⁸

4.2 Firm Characteristics

The following variables control for variation in the cost of initial public debt financing attributable to firm characteristics apart from auditor choice. Several of these resemble accounting-based covenants that are prevalent in private debt contracts according to descriptive statistics in Dichev and Skinner (2002).

We predict an inverse relation between yield spreads and firm size since lenders perceive larger firms as less risky (Sinkey, 1998) and there are economies of scale in the cost of issuing public bonds (Carey et al., 1993). We measure *Firm Size* as the natural logarithm of total assets normalized using the Consumer Price Index (Sengupta, 1998).

Datta et al. (1999) detect a negative relation between borrower reputation, which they estimate with firm age to comply with Diamond's (1989) theory that young firms can alleviate asset substitution and moral hazard problems by compiling a track record for servicing their debts, and the at-issue yield spread on first straight bond offers. Our proxy for *Firm Age* is the duration between the firm's founding and its debt initial public offering since Petersen and Rajan (1994) and Pittman and Fortin (2004) report evidence that firms begin forming their reputations with lenders at this early date rather than when

¹⁸ All results are very similar when we re-specify this variable as the coupon rate on the bond minus the mean coupon rate for all issues in the sample to avoid transferring the average tax impact to the intercept.

they go public.¹⁹ This specification is also consistent with Diamond's (1989) model, which starts with an observationally identical set of borrowers that have no credit history.

Higher agency costs ensue when firms increase their leverage. We predict a positive relation between leverage in the year of the initial bond issue and its yield spread. We specify *Leverage* as total debt scaled by firm market value (Datta et al., 1999).

Elton et al. (2001) find that expected default losses are insufficient to explain the yield spread over treasuries on corporate bonds. In fact, they provide evidence that a portion of the unexplained spread constitutes a risk premium to compensate bondholders for bearing non-diversifiable corporate bond risk. Since Elton et al. report that the same influences that affect systematic risks in the stock market affect risk premiums in the corporate bond market, we specify an additional control variable, *Volatility*, as the standard deviation of daily stock returns over the preceding year (Sengupta, 1998 and Campbell and Taksler, 2003). This may be particularly important in our setting since relatively low-grade bonds are more sensitive to equity volatility (Collin-Dufresne et al., 2001).

In addition, we include an industry dummy variable, *Industrial*, with a value of one identifying industrial firms and zero identifying financial firms (Datta et al., 1999). Since Elton et al. (2001) and Campbell and Taksler (2003) document that the yield spread on bonds within a given rating category is higher for financials than it is for industrials, we predict that *Industrial* will have a negative coefficient.²⁰ We control for changing

¹⁹ Since Loughran and Ritter (2004), among others, find that SDC's coverage of firms' founding dates is unreliable, we hand-collect this data from several Moody's Manuals. Re-specifying the firm age variable as the natural logarithm of one plus age does not materially affect any of our results.

²⁰ Elton et al. (2001) attribute their evidence to differences between financial and industrial firms in their sensitivity to systematic influences and to idiosyncratic shocks occurring over time that affect bond returns.

macroeconomic conditions with bond issue year dummies and in the specification of one of our dependent variables as the spread over treasury bond yields.

Table 3 provides descriptive statistics that suggest that there is considerable variation across all continuous variables. The low cross-correlations between *Big Five* and the control variables reduces the possibility that other determinants of credit ratings and yield spreads could be spuriously responsible for any evidence consistent with our predictions.

Table 4 reports univariate statistics for the sample split by firms' auditor choice. These results imply that the 242 firms of the 255 firms in the sample that chose a Big Five auditor are slightly larger, younger, less levered, and, importantly, have higher credit ratings and lower yield spreads than those with a non-Big Five auditor.²¹ Although this provides some preliminary support for the predictions in H₁ and H₂ that information and insurance incentives affect auditor choice in the initial public debt market, this analysis ignores the potential influence of correlations among the explanatory variables. We consider all of these determinants of bond pricing in the multivariate tests in the next section.

²¹ These descriptive statistics indicate that the sub-sample of firms with non-Big Five auditors are significantly smaller, younger, and more levered, which are all characteristics that have been used in prior research to represent default risk; e.g., Petersen and Rajan (1994). This raises the concern that our auditor choice variable may be capturing an omitted default risk factor, rather than the economic demand for a high-quality auditor stemming from the lower information risk that results when financial statements are more credible. We initially protect against this possibility by including additional proxies for default risk – Altman's Z-score, Ohlson's O-score, the standard deviation in the issuer's earnings over the past five years, and whether shareholders' equity is negative – in our cross-sectional regressions and find that our results are qualitatively identical. Although this provides some support that our auditor choice variable reflects limited information as the source of risk in our tests, we later return to the econometric issue that borrowers with Big Five auditors may be a non-random sample arising from, for example, these public accounting firms deliberately restricting their clientele to less risky firms.

5. Empirical Results

5.1 Primary Specifications

Our empirical tests exploit the structure of the initial public debt market to isolate the information and insurance roles of auditor choice. In column (1) of Table 5, we provide the least-squares estimation results of the model in equation (1) that examines whether credit rating agencies consider that the presence of a Big Five auditor enhances the credibility of issuers' financial statements. The coefficient on *Big Five* is positive and statistically different from zero at the 1 percent level, supporting the prediction in H₁ that choosing a brand name auditor improves bond ratings. This evidence suggests that credit rating agencies rationally anticipate that managerial opportunism in financial reporting will be worse for firms without Big Five auditors.

After controlling for firm fundamentals and issue characteristics, the *Big Five* estimate implies that monitoring by a high-quality auditor raises ratings, on average, by 1.67 minor classes. Consequently, auditor choice is an economically important determinant of credit ratings given that, for example, firms retaining a Big Five auditor receive a Ba1 rating from Moody's while equivalent firms only receive about a Ba3 rating. Putting this result in more perspective, Table 3 reports that the mean (median) linear rating is 12.37 (12.50). Our estimate of the impact of choosing a Big Five auditor on credit ratings is almost double that reported by Mansi et al. (2004) in their study of seasoned bonds in the secondary market. This might reflect that more precise financial statements are more valuable for initial bond issues that suffer from severe information asymmetry.

In equation (2), we separate the insurance motive for auditor choice from its information motive by controlling for credit rating when examining the influence of hiring a Big Five auditor on initial public debt pricing. Assuming that *Rating* perfectly incorporates the primitive relation between auditor identity and uncertainty about firms' financial statements, this regression cleanly measures the value to bondholders of auditors' implicit insurance coverage against future losses. Column (2) of Table 5 presents the results of estimating this model, which include evidence at the 5 percent level consistent with the prediction in H₂ that choosing a Big Five auditor narrows at-issue yield spreads.

The *Big Five* estimate suggests that the perceived insurance protection lowers, on average, these borrowing costs by about 19 basis points. In comparison, Table 3 reports that the mean (median) yield spread is 145 (129) basis points. In fact, coarsely calibrating this evidence by comparing the *Big Five* coefficient estimate to the one on *Rating* reveals that its economic importance amounts to about 1.7 minor rating classes.

Equation (3) is identical to equation (2) except that we replace the raw numeric credit rating with the orthogonalized rating, which is the residual from the regression in equation (1). Since the information content of a Big Five audit is extracted from the credit rating, this specification enables us to measure the combined information and insurance impacts of auditor choice on initial public debt pricing. Column (3) of Table 5 summarizes the results of estimating the model in equation (3) using ordinary least squares. All controls have the predicted signs, although some are not statistically significant. We also find that the coefficient on *Big Five* is negative and strongly significant.

Importantly, relative to the regression in equation (2) that only measures the insurance role of auditor choice, the magnitude of the coefficient on *Big Five* rises from about 19 to 38 basis points. Given that equation (2) captures only insurance and equation (3) captures both information and insurance, we attribute the 19 basis point difference in the coefficient estimates on *Big Five* between these regressions to the information role of auditor choice. Collectively, our evidence indicates that the information and insurance incentives for choosing a Big Five auditor equally affect at-issue yield spreads.

Next, after splitting the main sample according to bond issue purpose, we re-estimate equations (1) to (3) to examine the predictions that the impact of auditor choice on initial bond pricing will be stronger when firms plan to refinance their bank loans.²² Consistent with the intuition in H_{3A} that the presence of a high-quality auditor becomes more valuable when firms cannot rely as heavily on monitoring delegated to private lenders to moderate information asymmetry, the results in columns (1) and (4) of Table 6 suggest that engaging a Big Five auditor only leads to higher ratings for firms intending to replace bank debt with public debt. Similarly, the evidence in columns (2) and (5) that the importance of auditor choice to yield spreads is concentrated in firms planning to reduce their bank loans supports the prediction in H_{3B} that bondholders perceive that the implicit insurance from hiring a Big Five auditor is more valuable when default risk is higher.

Finally, the results in columns (3) and (6) of Table 6 that measure the combined influence of the information and insurance motives for auditor choice also imply that

²² Extant research on debt pricing also sorts firms according to within-sample characteristics, including on whether the purpose of the bond issue is to refinance existing bank loans (Gande et al., 1997).

choosing a Big Five auditor only affects the initial public debt pricing of firms refinancing their bank debt. In addition, an F -test reveals that the coefficient estimate on *Big Five* is larger for the refinancing firms than for the non-refinancing firms.

5.2 Alternative Specifications

Our relatively small data set, together with the relatively small fraction of firms choosing a non-Big Five auditor, prevents us from extensively examining the sensitivity of our results. For example, partitioning the data often leaves only three or four firms using a non-Big Five auditor in a particular sub-sample, making interpretation of results difficult since the tests suffer from low power. Still, we consider the impact on our evidence of re-specifications concerning the endogeneity of auditor choice, industry specialization among the large public accounting firms, bond ratings, and issue years.

5.2.1 Endogeneity of Auditor Choice

In the standard linear regressions reported so far in this paper, we ignore self-selection by implicitly assuming that *Big Five* is an exogenous variable. However, the observed auditor choice in our sample is probably endogenous given that firms weigh the costs and benefits of retaining a Big Five auditor relative to a non-Big Five auditor. In fact, several descriptive statistics in Table 4 imply the presence of endogeneity in the decision to hire a Big Five auditor – specifically, their clients are significantly larger, less levered, and older. Since auditor clienteles are not randomly assigned, this may lead to inconsistent OLS coefficient estimates unless we explicitly control for any selectivity bias (Heckman, 1979). Considerable prior research relies on the following two-stage “treatment effects” model to mitigate this concern about self-selection (Greene, 1997).

In the first stage, we estimate a probit regression of the likelihood that a Big Five auditor is chosen:²³

$$Big\ Five = \phi_0 + \phi_1 Firm\ Size + \phi_2 Subsidiaries + \phi_3 Ohlson's\ O-score + \phi_4 Leverage + \phi_5 Volatility + \phi_6 Speculative + \phi_7 Underwriter + \eta \quad (4)$$

The determinants of auditor choice in this regression include *Firm Size* and *Subsidiaries* (the number of subsidiaries), both reflecting that Big Five auditors can more efficiently handle large, complex clients; e.g., Feltham et al. (1991). We include four risk proxies – *Ohlson's O-score*, *Leverage*, *Volatility*, and *Speculative* (a dummy variable identifying speculative grade bonds) – since greater agency costs may induce riskier firms to prefer a Big Five auditor to improve the credibility of their financial reporting and to provide more insurance coverage.²⁴ Finally, we include *Underwriter* since higher-quality underwriters may insist that firms issuing their initial public debt retain a Big Five auditor (Balvers et al., 1988).

In the second-stage regression, our specification yields consistent coefficient estimates by correcting for any selectivity bias. This involves adding another variable, the inverse Mills ratio derived from the first-stage probit, to the set of explanatory variables in our original equation (1) and re-estimating using ordinary least squares. Results from the two-stage estimation include that the coefficient on *Big Five* remains negative and strongly significant. Applying this correction procedure to the rest of our regressions

²³Weber and Willenborg (2003) and Mansi et al. (2004) provide more detailed descriptions of this selection correction procedure in auditor choice settings as well as additional citations justifying the choice and specification of independent variables in the first-stage regression.

²⁴The causality may run in the opposite direction with Big Five auditors deliberately restricting their clienteles to less risky firms. This screening would likely result in their clients having, for example, higher bond ratings, spuriously inducing evidence supporting our prediction that users perceive Big Five-audited financial statements as more credible unless we consider the potential impact of selectivity bias.

indicates that all reported results are virtually identical after conditioning for the apparent selection bias in *Big Five*. The inverse Mills ratio is significant in most of these regressions, reinforcing the importance of controlling for the endogeneity of auditor choice, although this does not affect inferences in our case.

5.2.2 *Industry-specialist Auditors*

In this section, we explore whether our results are robust to replacing *Big Five* with a variable, *Specialist*, which identifies industry-specialist auditors. These tests may provide interesting triangulation of our main evidence that the presence of a Big Five auditor lowers the yield spreads on initial bond offers, especially for firms refinancing their bank loans. Both DeAngelo (1981) and Titman and Trueman (1986) argue that industry knowledge is an important element of auditor expertise that could affect the pricing of new equity issues. Similarly, some empirical research suggests that audit quality is higher for Big Five auditors with specialized industry experience; e.g., DeFond (1992), Craswell et al. (1995), and Krishnan (2003).

By restricting these tests to the 95 percent of firms in our full sample that rely on a Big Five auditor, we avoid potential confounding of brand name reputation and industry specialization. We designate auditors as industry specialists if they are one of the top three auditing firms in terms on client sales within this two-digit SIC code for that year or their share of client sales is within 15 percent of the leading industry specialist (DeFond et al., 2000). This results in 62 percent of the observations in our sample being audited by industry specialists.

As this reduced sample controls for brand name reputation, we simply code *Specialist* one when the firm chooses an industry specialist auditor and zero otherwise. Table 7 reports the results from re-estimating equations (1) to (3) to examine whether perceived information asymmetry and insurance protection varies systematically with the presence of an industry specialist auditor. In column (1), there is no evidence consistent with the prediction in H₁ that these auditors lend more credibility to initial public debt issuers' financial statements. Altogether, our research implies that the bond market in pricing these securities distinguishes audit quality *between* Big Five and non-Big Five firms, but not by industry specialization *within* the Big Five firms.

According to column (2) of Table 7, the data supports the prediction in H₂ that an industry specialist auditor provides greater implicit insurance coverage to bondholders. However, given that this result is very sensitive to the specification of *Specialist* and that we are not aware of any research indicating differential wealth or insurance protection among Big Five auditors, no inferences are drawn from this evidence.²⁵

5.2.3 Bond Ratings

Slightly surprisingly, some bond issues in the sample have a maximum safety rating (Aaa by Moody's). Identifying with certainty whether an issue constitutes an initial

²⁵ Any delineation between specialist and non-specialist Big Five auditors is somewhat arbitrary. In fact, prior research contributes several proxies for identifying Big Five industry-specialist auditors. These range from strict definitions that generally result in only one or two Big Five auditors being considered industry specialists; e.g., Palmrose (1986) to more lenient ones that generally result in three or four Big Five auditors being considered industry specialists; e.g., Craswell et al. (1995). Accordingly, we re-estimate the regressions after applying other *ad hoc* cut-offs. Specifically, we find no evidence consistent with H₁ to H_{3B} when we (i) determine market share based on number of clients rather than client sales; (ii) classify the largest Big Five auditor according to client sales revenue in each two-digit SIC code as the only industry specialist; e.g., Chung and Kallapur (2003) (this reduces the portion of the sub-sample with industry specialist auditors to 32 percent from 62 percent to ensure that our measure genuinely reflects industry specialization); and (iii) identify as industry specialists all Big Five auditors with at least 10 percent of total

public offering is occasionally difficult, especially for firms that have been operating prior to 1970 when Thomson Financial begins its coverage. Firms with a maximum safety rating may not belong in the sample; i.e., we may have inadvertently included some seasoned issues. However, all results are nearly identical when we discard the seven securities that are rated Aaa.

5.2.4 Issue Years

In the 1980s and 1990s, there was considerable consolidation within the auditing profession, with already large public accounting firms becoming even larger through mergers and acquisitions.²⁶ This trend has been accompanied by more public firms hiring a Big Five auditor (Shu, 2000 and Mansi et al., 2004). In our sample, a higher fraction of firms have a Big Five auditor in the 1970s than in the 1980s and, especially, the 1990s. We re-ran our regressions on sub-samples covering the years from 1970 to 1985, 1970 to 1989, and 1970 to 1995. The results of these tests consistently suggest that auditor choice leads to higher bond ratings and lower at-issue yield spreads on initial public debt, although the magnitude of the *Big Five* coefficients subside as we add calendar years. Accordingly, our evidence is not period-specific; i.e., removing the more recent years in which firms rely less on Big Five auditors strengthens both the statistical and economic significance of our results.

client sales (or number of clients) in the two-digit SIC code of the firm under study; e.g., DeFond (1992) and Craswell et al. (1995).

²⁶ The “Big Eight” public accounting firms became the “Big Six” after Ernst & Whinney and Arthur Young merged on May 5, 1989 to form Ernst & Young and Deloitte, Haskins, and Sells and Touche Ross merged on August 11, 1989 to form Deloitte & Touche. In 1998, these “Big Six” firms became the “Big Five” when Coopers & Lybrand and Price Waterhouse merged to form PricewaterhouseCoopers. The “Big Five” became the “Big Four” with the demise of Arthur Andersen, although this follows our sample period.

6. Conclusions

The extensive research on the link between auditor choice and the pricing of *equity* initial public offerings has had difficulty distinguishing between two motives for choosing a Big Five auditor: lending more credibility to firms' financial statements and affording investors greater implicit insurance coverage in the event of audit failure. We help empirically resolve this issue by providing the first evidence on the importance of auditor choice to the pricing of *debt* initial public offerings, which induce considerable information asymmetry by expanding firms' debt ownership structures to include public bondholders and by extending debt maturity.

In cross-sectional tests, we separately examine the impact of retaining a Big Five auditor on the perceptions of credit ratings agencies and bondholders to isolate the information and insurance roles of auditor choice, respectively, in the pricing of these securities. After controlling for other determinants of initial bond pricing, we estimate that each role lowers at-issue yield spreads by about 19 basis points, on average. This evidence implies that public debt market participants perceive that hiring a Big Five auditor both moderates debt-related monitoring costs by reducing uncertainty about firms' financial disclosures and provides greater insurance protection to lenders.

We also examine whether choosing a Big Five auditor particularly benefits firms planning to replace private debt with public debt, which reduces cross-monitoring among lenders and elevates default risk according to prior research. Consistent with our predictions, we find that auditor choice has a stronger impact on the credit ratings and yield spreads of initial bond issuers intending to refinance their bank loans.

Future research could explore how auditor choice affects the information content of announcements of initial bond issues. For example, does retaining a high-reputation auditor reduce the negative stock price response to debt initial public offering announcements detected in prior studies (Datta et al., 2000)? Our research could also be extended by documenting whether auditor choice provides other economic incentives to firms besides lowering their borrowing costs. For example, empirical research on the impact of auditor quality on credit rationing would complement our evidence on debt pricing; i.e., does choosing a Big Five auditor increase the quantity of credit available to firms issuing their first public debt?

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Table 1: Sample Selection Summary

	<u>Firms</u>	<u>Securities</u>
Initial public straight debt issues during 1970-2001 available on SDC database	346	398
Subtract: Number not followed by <i>CRSP</i> prior to their bond IPO	(42)	(48)
Subtract: Number not followed by or missing data on <i>Compustat</i>	(62)	(65)
Subtract: Number with incomplete data from various <i>Moody's Manuals</i>	(22)	(24)
Subtract: Number with implausible <i>SDC</i> data	(5)	(6)
Number in the sample	215	255

Table 2: Sample of Initial Straight Bond Issues – Calendar Year and Industry Distribution

<u>Calendar Year / 1-Digit SIC Code</u>	<u>1</u>	<u>2</u>	<u>3</u>	<u>4</u>	<u>5</u>	<u>6</u>	<u>7</u>	<u>8</u>	<u>Total</u>
1970	1	5	7	0	5	1	0	0	19
1971	4	4	4	0	1	1	0	0	14
1972	0	3	3	0	1	0	0	0	7
1973	0	0	1	0	0	1	0	0	2
1974	2	0	0	0	1	4	0	0	7
1975	2	4	4	1	1	2	0	0	14
1976	0	0	0	0	1	0	0	0	1
1977	0	0	0	0	0	0	0	2	2
1979	0	0	3	0	0	1	0	0	4
1980	0	5	1	0	0	2	0	0	8
1981	0	0	0	2	0	0	0	0	2
1982	0	1	2	1	1	5	0	0	10
1983	0	0	1	0	0	3	0	0	4
1984	0	1	0	0	2	0	0	0	3
1985	0	5	1	2	5	2	1	0	16
1986	0	0	0	3	1	2	0	0	6
1987	0	7	2	0	1	0	0	0	10
1988	0	1	1	1	0	2	0	0	5
1989	0	2	1	0	0	0	0	0	3
1991	0	2	0	0	0	0	0	0	2
1992	0	0	0	0	0	1	0	0	1
1993	0	1	0	3	0	0	0	0	4
1994	2	2	3	0	0	2	0	0	9
1995	1	7	4	0	2	4	0	1	19
1996	0	0	5	2	3	9	1	0	20
1997	4	0	7	1	0	8	0	0	20
1998	0	2	10	0	6	0	4	0	22
1999	1	4	1	2	0	4	3	0	15
2000	0	0	2	0	0	0	0	0	2
2001	0	2	0	0	0	1	1	0	4
Number of issues in the sample	17	58	63	18	31	55	10	3	255

Notes:

This table reports the number of observations in the sample by single-digit SIC code and by the calendar year of the initial straight bond issue. These SIC codes correspond to the following industries: 1 – Mining, 2 – Construction, 3 – Manufacturing, 4 – Transportation, 5 – Wholesale Trade, 6 – Retail Trade, 7 – Agricultural Services, and 8 – Forestry.

Table 3: Summary Statistics*Panel A – Correlations*

<u>Variable</u>	<u>Yield</u>	<u>Big Five</u>	<u>Rating</u>	<u>Underwriter</u>	<u>Purpose</u>	<u>Callable</u>	<u>Sinking</u>	<u>Dividend</u>	<u>Coupon</u>	<u>Firm size</u>	<u>Firm age</u>	<u>Leverage</u>	<u>Volatility</u>	<u>Industrial</u>
Yield spread		-0.057 (0.361)	-0.329 (0.000)	-0.247 (0.000)	0.096 (0.127)	0.229 (0.000)	0.068 (0.280)	0.300 (0.000)	0.216 (0.001)	-0.188 (0.003)	-0.090 (0.150)	0.233 (0.000)	0.277 (0.000)	0.029 (0.651)
Big Five	-0.096 (0.123)		0.110 (0.078)	0.116 (0.064)	-0.021 (0.741)	-0.021 (0.741)	0.060 (0.340)	-0.044 (0.482)	-0.126 (0.044)	0.140 (0.026)	0.079 (0.207)	-0.025 (0.686)	-0.011 (0.860)	0.139 (0.027)
Rating	-0.438 (0.000)	0.156 (0.013)		-0.229 (0.000)	0.018 (0.776)	0.015 (0.810)	0.109 (0.082)	-0.151 (0.016)	0.195 (0.002)	0.223 (0.000)	0.366 (0.000)	-0.307 (0.000)	-0.031 (0.614)	0.127 (0.043)
Underwriter	-0.371 (0.000)	0.138 (0.028)	0.150 (0.013)		-0.217 (0.001)	-0.175 (0.005)	-0.156 (0.013)	-0.298 (0.000)	-0.472 (0.000)	0.010 (0.873)	-0.232 (0.000)	-0.143 (0.022)	-0.084 (0.182)	0.005 (0.942)
Purpose	0.029 (0.647)	-0.021 (0.741)	0.032 (0.610)	0.032 (0.610)		0.066 (0.293)	-0.134 (0.033)	0.026 (0.676)	-0.036 (0.571)	-0.083 (0.189)	-0.022 (0.725)	0.029 (0.643)	-0.078 (0.214)	0.003 (0.968)
Callable	0.179 (0.004)	-0.021 (0.741)	-0.026 (0.677)	-0.026 (0.677)	0.066 (0.293)		0.121 (0.053)	0.170 (0.006)	0.125 (0.046)	0.015 (0.809)	-0.019 (0.762)	-0.047 (0.453)	0.125 (0.046)	0.088 (0.159)
Sinking	0.066 (0.300)	0.060 (0.340)	0.069 (0.272)	-0.033 (0.604)	-0.134 (0.033)	0.121 (0.053)		0.224 (0.000)	0.266 (0.000)	-0.006 (0.926)	0.031 (0.626)	0.055 (0.378)	-0.030 (0.635)	0.057 (0.364)
Dividend	0.333 (0.000)	-0.044 (0.482)	-0.256 (0.000)	-0.234 (0.000)	0.026 (0.676)	0.170 (0.006)	-0.025 (0.689)		0.224 (0.000)	-0.203 (0.001)	0.070 (0.268)	0.159 (0.011)	0.021 (0.743)	-0.004 (0.948)
Coupon	0.215 (0.000)	-0.100 (0.111)	0.130 (0.038)	-0.182 (0.004)	-0.014 (0.821)	0.010 (0.111)	0.232 (0.000)	0.191 (0.002)		-0.100 (0.111)	0.134 (0.032)	0.227 (0.000)	-0.041 (0.516)	-0.022 (0.732)
Firm size	-0.222 (0.000)	0.123 (0.051)	0.214 (0.001)	0.071 (0.258)	-0.093 (0.141)	0.019 (0.763)	-0.041 (0.518)	-0.178 (0.004)	-0.108 (0.085)		-0.011 (0.862)	0.177 (0.005)	-0.050 (0.427)	-0.122 (0.052)
Firm age	-0.117 (0.063)	0.069 (0.272)	0.321 (0.000)	0.006 (0.925)	(0.018) (0.770)	-0.057 (0.365)	0.011 (0.865)	0.027 (0.664)	0.040 (0.523)	-0.010 (0.869)		-0.241 (0.000)	-0.060 (0.339)	0.360 (0.000)
Leverage	0.100 (0.123)	-0.117 (0.063)	-0.313 (0.000)	-0.216 (0.001)	-0.002 (0.980)	-0.008 (0.902)	-0.051 (0.419)	(0.231) (0.000)	0.198 (0.002)	0.294 (0.000)	-0.211 (0.001)		-0.200 (0.001)	-0.461 (0.000)
Volatility	0.352 (0.000)	-0.054 (0.391)	-0.088 (0.160)	-0.092 (0.145)	-0.053 (0.399)	0.142 (0.024)	-0.046 (0.460)	0.030 (0.634)	-0.074 (0.237)	0.023 (0.001)	-0.101 (0.108)	-0.210 (0.001)		0.361 (0.000)
Industrial	0.002 (0.979)	0.139 (0.027)	0.159 (0.011)	0.117 (0.062)	0.003 (0.968)	0.088 (0.159)	0.057 (0.364)	-0.004 (0.948)	-0.059 (0.345)	-0.487 (0.000)	0.312 (0.000)	-0.487 (0.000)	0.288 (0.000)	

Table 3: Summary Statistics (Continued)*Panel B – Continuous variables*

<u>Variable</u>	<u>N</u>	<u>Mean</u>	<u>Std Dev</u>	<u>Median</u>	<u>Min</u>	<u>Max</u>
Yield spread	255	145.04	76.36	129.23	13.75	499.31
Rating	255	12.37	3.19	12.50	5.00	19.00
Underwriter	255	8.40	1.73	9.00	0.00	9.10
Coupon	255	8.58	1.93	8.00	5.90	15.50
Firm size	255	7.87	1.09	7.73	4.80	11.02
Firm age	255	34.9	28.8	26.0	0.00	128.00
Leverage	255	2.34	6.25	0.69	0.10	51.07
Volatility	255	0.32	0.13	0.30	0.02	0.89

Panel C – Discrete variables

<u>Variable</u>	<u>N</u>	<u>Percent</u>
Big Five	242	94.9%
Unrated	20	7.8%
Purpose	186	72.9%
Callable	186	72.9%
Sinking	16	6.3%
Dividend restriction	41	16.1%
Industrial	200	78.4%

Notes:

This table presents summary statistics for the 255 firm-year observations over the period 1970 to 2001 used in the hypotheses tests. Panel A reports correlations for the regression variables, with Pearson correlations presented below the diagonal and Spearman correlations presented above the diagonal; related probability values are presented in parentheses. Panel B presents the distributional statistics for the continuous variables and Panel C presents the distribution for the discrete variables. Table 2 provides descriptive statistics on the calendar year and industry distributions.

Continuous variables are as follows (all dollar-denominated variables are stated in millions). *Yield spread* is the difference in basis points between the at-issue yield for the initial public straight debt offer and the yield of a Treasury bond with similar maturity and coupon rate on the same day. *Rating* is the bond's credit rating converted to an ordinal scale by assigning a value of 19 to those rated Aaa by Moody's, 18 to those rated Aa1, etc. We measure the rating as the average of the Moody's and Standard & Poor's ratings whenever both are available. In certain regressions, we replace *Rating* with *Orthogonalized Rating*, which is the residual from regressing auditor choice and the control variables on credit ratings. *Underwriter* is the prestige ranking on the 10-point scale in Loughran and Ritter (2004). This variable is set to zero when an investment bank does not underwrite the bond issue. *Coupon* is the coupon rate on the bond issue. *Firm size* is the natural logarithm of total assets normalized using the Consumer Price Index. *Firm age* is the duration in years between the firm's founding and its debt initial public offering. *Leverage* is the total debt in the year of the bond issue scaled by firm market value. *Volatility* is the standard deviation of the firm's daily stock returns over the preceding year.

Indicator variables are as follows. *Big Five* auditor has a value of one if the firm retains a Big Five (Big Eight before 1989 and Big Six until 1998) auditor and zero otherwise. *Unrated* indicates bonds that are not rated by either Moody's or Standard & Poor's. *Purpose* has a value of one if the firm intends to refinance bank loans with the issue proceeds and zero otherwise. *Callable* indicates if the firm can force the repurchase of the issue prior to its maturity date. *Sinking* indicates the presence of a sinking fund provision. *Dividend restriction* has the value of one when the issue includes a covenant curtailing the rights of the firm to pay dividends and zero otherwise. *Industrial* has a value of one when identifying industrial firms and zero when identifying financial firms. The year dummy variables indicate the calendar year in which the firm had its debt initial public offering.

Table 4: Summary Statistics – Sample Split by Auditor Choice

<u>Variable</u>	<u>Big Five Audited Firms (n=242)</u>		<u>Non-Big Five Audited Firms (n=13)</u>		<u>Differences</u>		<u>p-value of Differences</u>	
	<u>Mean</u>	<u>Median</u>	<u>Mean</u>	<u>Median</u>	<u>Means</u>	<u>Medians</u>	<u>Means</u>	<u>Medians</u>
Yield spread	143.350	128.991	176.476	136.696	-33.126	-7.705	0.07	0.18
Rating	12.486	13.000	10.231	11.000	2.255	2.000	0.03	0.04
Unrated issues	0.066	0.000	0.308	0.000	-0.242	0.000	0.00	0.00
Underwriter	8.460	9.000	7.380	8.100	1.080	0.900	0.02	0.03
Purpose	0.727	1.000	0.769	1.000	-0.042	0.000	0.37	0.37
Callable issues	0.727	1.000	0.769	1.000	-0.042	0.000	0.37	0.37
Sinking fund provision	0.066	0.000	0.000	0.000	0.066	0.000	0.17	0.17
Coupon rate	8.535	7.913	9.410	9.250	-0.875	-1.337	0.06	0.02
Firm size	7.899	7.760	7.293	7.173	0.606	0.587	0.03	0.01
Firm age	35.400	26.500	26.384	21.000	9.016	5.500	0.14	0.10
Leverage	2.171	0.690	5.476	0.756	-3.305	-0.066	0.03	0.34
Volatility	0.318	0.301	0.350	0.308	-0.032	-0.007	0.20	0.43
Industrial firm dummy	0.798	1.000	0.538	1.000	0.260	0.000	0.02	0.01
Dividend restriction	0.157	0.000	0.231	0.000	-0.074	0.000	0.48	0.48

Notes:

This table presents summary statistics for the sample split into the 242 firms using Big Five auditors and the 13 firms using non-Big Five auditors. The variables are specified in Table 3. A *t*-test evaluates differences in the means; a Wilcoxon matched pair sign rank test evaluates differences in the median values.

Table 5: Debt Pricing OLS Regression Results – Full Sample

$$\text{Model 1: Rating} = \alpha_0 + \alpha_1 \text{ Big Five} + \beta_1 \text{ Purpose} + \beta_2 \text{ Underwriter} + \beta_3 \text{ Callable} + \beta_4 \text{ Sinking} + \beta_5 \text{ Dividend} + \beta_6 \text{ Coupon} + \beta_7 \text{ Firm Size} + \beta_8 \text{ Firm Age} + \beta_9 \text{ Leverage} + \beta_{10} \text{ Volatility} + \beta_{11} \text{ Industrial} + \gamma_{1970, \dots, 2000} \text{ Year} + \varepsilon$$

$$\text{Model 2: Yield Spread} = \alpha_0 + \alpha_1 \text{ Big Five} + \beta_1 \text{ Rating} + \beta_2 \text{ Unrated} + \beta_3 \text{ Purpose} + \beta_4 \text{ Underwriter} + \beta_5 \text{ Callable} + \beta_6 \text{ Sinking} + \beta_7 \text{ Dividend} + \beta_8 \text{ Coupon} + \beta_9 \text{ Firm Size} + \beta_{10} \text{ Firm Age} + \beta_{11} \text{ Leverage} + \beta_{12} \text{ Volatility} + \beta_{13} \text{ Industrial} + \gamma_{1970, \dots, 2000} \text{ Year} + \varepsilon$$

$$\text{Model 3: Yield Spread} = \alpha_0 + \alpha_1 \text{ Big Five} + \beta_1 \text{ Orthogonalized Rating} + \beta_2 \text{ Unrated} + \beta_3 \text{ Purpose} + \beta_4 \text{ Underwriter} + \beta_5 \text{ Callable} + \beta_6 \text{ Sinking} + \beta_7 \text{ Dividend} + \beta_8 \text{ Coupon} + \beta_9 \text{ Firm Size} + \beta_{10} \text{ Firm Age} + \beta_{11} \text{ Leverage} + \beta_{12} \text{ Volatility} + \beta_{13} \text{ Industrial} + \gamma_{1970, \dots, 2000} \text{ Year} + \varepsilon$$

Explanatory variable	Dependent variable = Rating	Dependent variable = Yield spread	Dependent variable = Yield spread
	<u>1</u>	<u>2</u>	<u>3</u>
Intercept	13.31 ^{***}	112.60 ^{**}	-36.53
Big Five auditor	1.67 ^{***}	-19.45 ^{**}	-38.12 ^{***}
Rating		-11.21 ^{***}	
Orthogonalized rating			-11.21 ^{***}
Unrated		-107.70 ^{***}	-107.70 ^{***}
Purpose	0.01	2.87	2.75
Underwriter	-0.03	-2.88 ^{***}	-2.50 [*]
Callable	-0.45	17.44 ^{***}	22.49 ^{***}
Sinking	-0.35	15.55	19.47 [*]
Dividend	-1.54 ^{***}	11.21	28.49 ^{***}
Coupon	-0.59 ^{**}	31.86 ^{***}	38.42 ^{***}
Firm size	0.60 ^{***}	-2.25	-8.94 ^{***}
Firm age	0.01 [*]	0.09	-0.02
Leverage	-0.23 ^{***}	2.16 ^{***}	4.76 ^{***}
Volatility	-2.42 [*]	101.85 ^{***}	128.99 ^{***}
Industrial	-0.20	-12.33 [*]	-10.03
Adjusted R ²	0.466	0.779	0.779
F-statistic	6.54 [°]	22.26 [°]	22.26 [°]
Observations	255	255	255

Notes:

This table presents regression results for the bond rating and yield spread models using ordinary least squares for the full sample. The dependent and explanatory variables are specified in Table 3. The regressions include dummy variables for calendar year of the initial straight debt issue; coefficient estimates for these are not reported. Regression equation *F*-tests significant at less than 0.001 are identified by a ° superscript. White's (1980) test consistently reveals no violation of the homoscedasticity assumption. The superscript asterisks indicate explanatory variable coefficient significance at *p*-values less than 0.10 (*), 0.05 (**), and 0.01 (***) in one-tailed tests where directional predictions are made and two-tailed tests otherwise.

Table 6: Debt Pricing OLS Regression Results – Sample Split by Bond Issue Purpose

Models 1 and 4: Rating = $\alpha_0 + \alpha_1$ Big Five + β_1 Underwriter + β_2 Callable + β_3 Sinking + β_4 Dividend + β_5 Coupon + β_6 Firm Size + β_7 Firm Age + β_8 Leverage + β_9 Volatility + β_{10} Industrial + $\gamma_{1970,\dots,2000}$ Year + ϵ

Models 2 and 5: Yield Spread = $\alpha_0 + \alpha_1$ Big Five + β_1 Rating + β_2 Unrated + β_3 Underwriter + β_4 Callable + β_5 Sinking + β_6 Dividend + β_7 Coupon + β_8 Firm Size + β_9 Firm Age + β_{10} Leverage + β_{11} Volatility + β_{12} Industrial + $\gamma_{1970,\dots,2000}$ Year + ϵ

Models 3 and 6: Yield Spread = $\alpha_0 + \alpha_1$ Big Five + β_1 Orthogonalized Rating + β_2 Unrated + β_3 Underwriter + β_4 Callable + β_5 Sinking + β_6 Dividend + β_7 Coupon + β_8 Firm Size + β_9 Firm Age + β_{10} Leverage + β_{11} Volatility + β_{12} Industrial + $\gamma_{1970,\dots,2000}$ Year + ϵ

<u>Dependent variable</u>	<u>Refinancing firms</u>			<u>Non-refinancing firms</u>		
	<u>Rating</u>	<u>Yield spread</u>	<u>Yield spread</u>	<u>Rating</u>	<u>Yield spread</u>	<u>Yield spread</u>
	<u>1</u>	<u>2</u>	<u>3</u>	<u>4</u>	<u>5</u>	<u>6</u>
<u>Explanatory variable</u>						
Intercept	13.65 ^{***}	162.83 ^{***}	21.01	15.43 ^{**}	239.91	10.99
Big Five auditor	1.66 ^{**}	-28.87 ^{**}	-46.13 ^{***}	0.65	3.69	-5.93
Rating		-10.39 ^{***}			-14.84 ^{***}	
Orthogonalized rating			-10.39 ^{***}			-14.84 ^{***}
Unrated		-85.17 ^{***}	-85.17 ^{***}		-162.63 ^{***}	-162.63 ^{***}
Underwriter	-0.00	-1.90	-1.88	-0.34	-11.74 [*]	-6.63
Callable	-0.65	17.10 ^{**}	23.90 ^{***}	0.58	27.68 ^{**}	19.09
Sinking	-0.79	18.36	26.59	-1.66	18.40	43.01 [*]
Dividend	-0.92	11.37	20.97 ^{**}	-2.97 ^{**}	33.61	77.74 ^{***}
Coupon	-0.71 ^{***}	20.67 ^{***}	28.06 ^{***}	0.14	26.36 ^{***}	24.25 ^{***}
Firm size	0.73 ^{***}	-3.50	-11.04 ^{***}	0.74 [*]	-0.03	-10.99
Firm age	0.01	-0.03	-0.12	-0.00	-0.03	0.01
Leverage	-0.24 ^{***}	1.01 [*]	3.55 ^{***}	-0.37 ^{***}	2.53	8.06 ^{***}
Volatility	-3.65 ^{**}	132.41 ^{***}	170.39 ^{***}	-7.08 [*]	18.86	123.96 [*]
Industrial	-0.22	-22.42 ^{**}	-20.17 ^{**}	1.11	-17.02	-33.47 [*]
Adjusted R^2	0.414	0.726	0.726	0.611	0.815	0.815
F -statistic	4.62 [°]	13.91 [°]	13.91 [°]	4.56 [°]	10.38 [°]	10.38 [°]
Observations	186	186	186	69	69	69

Notes:

This table presents regression results for the bond rating and yield spread models using ordinary least squares for the sample split by bond issue purpose. The refinancing sub-sample identifies firms intending to replace their bank debt with public debt. The non-refinancing sub-sample comprises firms disclosing an issue purpose besides refinancing their bank loans. The dependent and explanatory variables are specified in Table 3. The regressions include dummy variables for calendar year of the initial straight debt issue; coefficient estimates for these are not reported. Regression equation F -tests significant at less than 0.001 are identified by a [°] superscript. White's (1980) test consistently reveals no violation of the homoscedasticity assumption. The superscript asterisks indicate explanatory variable coefficient significance at p -values less than 0.10 (*), 0.05 (**), and 0.01 (***) in one-tailed tests where directional predictions are made and two-tailed tests otherwise.

Table 7: Debt Pricing OLS Regression Results – Industry Specialist Auditor

$$\text{Model 1: Rating} = \alpha_0 + \alpha_1 \text{ Specialist} + \beta_1 \text{ Purpose} + \beta_2 \text{ Underwriter} + \beta_3 \text{ Callable} + \beta_4 \text{ Sinking} + \beta_5 \text{ Dividend} + \beta_6 \text{ Coupon} + \beta_7 \text{ Firm Size} + \beta_8 \text{ Firm Age} + \beta_9 \text{ Leverage} + \beta_{10} \text{ Volatility} + \beta_{11} \text{ Industrial} + \gamma_{1970, \dots, 2000} \text{ Year} + \varepsilon$$

$$\text{Model 2: Yield Spread} = \alpha_0 + \alpha_1 \text{ Specialist} + \beta_1 \text{ Rating} + \beta_2 \text{ Unrated} + \beta_3 \text{ Purpose} + \beta_4 \text{ Underwriter} + \beta_5 \text{ Callable} + \beta_6 \text{ Sinking} + \beta_7 \text{ Dividend} + \beta_8 \text{ Coupon} + \beta_9 \text{ Firm Size} + \beta_{10} \text{ Firm Age} + \beta_{11} \text{ Leverage} + \beta_{12} \text{ Volatility} + \beta_{13} \text{ Industrial} + \gamma_{1970, \dots, 2000} \text{ Year} + \varepsilon$$

$$\text{Model 3: Yield Spread} = \alpha_0 + \alpha_1 \text{ Specialist} + \beta_1 \text{ Orthogonalized Rating} + \beta_2 \text{ Unrated} + \beta_3 \text{ Purpose} + \beta_4 \text{ Underwriter} + \beta_5 \text{ Callable} + \beta_6 \text{ Sinking} + \beta_7 \text{ Dividend} + \beta_8 \text{ Coupon} + \beta_9 \text{ Firm Size} + \beta_{10} \text{ Firm Age} + \beta_{11} \text{ Leverage} + \beta_{12} \text{ Volatility} + \beta_{13} \text{ Industrial} + \gamma_{1970, \dots, 2000} \text{ Year} + \varepsilon$$

<u>Explanatory Variable</u>	<u>Dependent variable = Rating</u>	<u>Dependent variable = Yield spread</u>	<u>Dependent variable = Yield spread</u>
	<u>1</u>	<u>2</u>	<u>3</u>
Intercept	7.46 ^{***}	209.88 ^{***}	124.70 ^{**}
Specialist	0.20	-8.90 ^{**}	-11.22 ^{**}
Rating		-11.41 ^{***}	
Orthogonalized rating			-11.41 ^{***}
Unrated		-25.55	-25.55
Purpose	-0.13	3.44	4.97
Underwriter	0.35 ^{***}	-4.86 ^{**}	-8.85 ^{***}
Callable	-0.75 ^{***}	16.67 ^{***}	25.27 ^{***}
Sinking	-0.23	18.84 ^{**}	21.48 ^{**}
Dividend	-0.78 [*]	11.64	20.49 ^{**}
Coupon	-0.54 ^{***}	24.53 ^{***}	30.69 ^{***}
Firm size	0.92 ^{***}	-5.18 ^{**}	-15.64 ^{***}
Firm age	0.01 ^{**}	0.09	-0.01
Leverage	-0.08 [*]	4.05 ^{***}	4.91 ^{***}
Volatility	-3.81 ^{***}	87.26 ^{***}	130.73 ^{***}
Industrial	-0.07	-9.18 [*]	-8.33
Adjusted R ²	0.628	0.784	0.784
F-statistic	10.93 [°]	21.20 [°]	21.20 [°]
Observations	207	207	207

Notes:

This table presents regression results for the bond rating and yield spread models using ordinary least squares. *Specialist* is a dummy variable coded one if the firm's auditor is among the top three public accounting firms in terms of sales audited within the company's 2-digit SIC code or their share of client sales is within 15 percent of the leading industry specialist; zero otherwise. The dependent and other explanatory variables are specified in Table 3. The sample spans the years from 1974 to 2001 since the Compustat data required to measure auditor industry specialization was not available for earlier years. The regressions include dummy variables for calendar year of the initial straight debt issue; coefficient estimates for these are not reported. Regression equation *F*-tests significant at less than 0.001 are identified by a ° superscript. White's (1980) test consistently reveals no violation of the homoscedasticity assumption. The superscript asterisks indicate explanatory variable coefficient significance at p-values less than 0.10 (*), 0.05 (**), and 0.01 (***) in one-tailed tests where directional predictions are made and two-tailed tests otherwise.