# *Real effects of the audit choice*



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# Real Effects of the Audit Choice

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**Abstract**

We hypothesize that the choice to obtain a financial statement audit provides external financiers with incremental information about the firm, which helps reduce information asymmetry and financing frictions. Using a natural experiment, we show that when external financiers observe a firm's choice to voluntarily obtain an audit, the firms obtaining an audit significantly increase their debt, investment, and operating performance, and become more responsive to their investment opportunities. Further, we find that these effects are stronger for firms that are financially constrained and weaker for firms with other means to reduce financing frictions. Overall, our evidence suggests that the audit *choice* conveys information to capital providers, which reduces financing frictions and improves performance.

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## 1. Introduction

Prior research examines and finds that financial statement audits help reduce the cost of debt capital for small, private firms (e.g., Blackwell et al., 1998; Minnis, 2011). The premise underlying this relation is that an audit helps increase the quality and reliability of financial statement disclosures, which reduces information asymmetry and thus the cost of capital. We argue that, in addition to the value provided by the audit with respect to the quality of the financial disclosures, the choice to obtain an audit itself can provide incremental information to creditors, which can reduce financing frictions. In particular, we argue that the *observable choice* to get an audit can convey information about the firm's future prospects because audits are costly in terms of audit fees paid as well as management time and effort spent preparing for, and engaging in, the audit. Thus, firms will voluntarily subject themselves to an audit to obtain financing if they foresee themselves generating sufficient future profits from their investment opportunities to recover the cost of the audit. In this paper, we examine whether the choice to obtain an audit eases financing frictions by conveying information about firms' future prospects that is independent of the information generated from the audit itself.

Understanding whether the decision to voluntarily get an audit contains information that reduces financing frictions is important from a regulatory point of view because audits are mandated for private firms in many countries and the vast majority of public firms across the world. If the choice to obtain an audit does indeed contain information that relaxes financing frictions, such information would be hidden by the audit mandate. Thus it is important to know the economic significance (if any) of information revealed from observing the audit choice.

Broadly, the intuition for our research question follows from two distinct (but related) theories. First, the audit choice can be thought of as a costly signal initiated by low risk firms that allows them to separate themselves from high risk firms (e.g., Spence, 1973; Jensen and Meckling, 1976; Melumad and Thoman, 1990). For example, Jensen and Meckling (1976) discuss that firms can incur "bonding costs," such as an audit, to create a separating equilibrium where high quality firms commit to getting an audit that is too costly for low quality firms to mimic. Here the ability to incur the audit cost informs financiers that the firm is likely to generate sufficient future profits to recoup the audit related costs. Second, the audit choice can be characterized as a screening mechanism used by external financiers to separate the good credit risk firms from the bad credit risk firms (Guasch and Weiss, 1981). The idea is that firms that are willing to incur the cost of an audit to obtain external financing are likely to have more profitable investment opportunities, and thus lower credit risk, than those unwilling to pay for an audit. The above theories differ in terms of *why* the audit choice conveys information and who initiates the audit, but they both suggest that the *choice to incur the cost* of an audit conveys information about the firm's future prospects because firms with better prospects are more willing to incur this cost ex ante, as they are more likely to recoup this cost in the future.

Empirically testing whether the audit choice conveys any information that relaxes financing frictions is challenging because the economic effects of the audit choice is confounded by the economic effects of the audited financial information. In other words, since firms that choose to obtain an audit will have better quality and more credible financial statements than firms that do not receive audits, it is difficult to empirically separate the economic effects of the *audit choice* from that of the *audited reports*. Further, the choice to get an audit is likely to be confounded by changes in growth opportunities because firms are more likely to get voluntarily audits when they require external funds to finance those opportunities.

We overcome these challenges by using a natural experiment involving U.K. private firms. External audits were mandatory for U.K. private firms with sales above £1 million or assets above £1.4 million until 2004, but firms below these size thresholds were exempt from the audit requirement. In 2004, this audit exemption was extended to private firms with sales (assets) in between £1 – 5.6 million (£1.4 – 2.8 million), i.e., audits became voluntary for additional firms within the prescribed size threshold. Using this audit regime shift as an exogenous shock to the *observability* of the audit choice, we examine the change in the investment and financing behavior of firms that mandatorily receive audits before 2004, but voluntarily receive audits post-2004 (henceforth referred to as treatment firms). That is, our treatment firms are firms that have always received an audit, but their audit choice is observed by external investors only after 2004 when they qualify for an audit exemption but choose to continue receiving audits. Consequently, the economic effects of supplying audited reports to investors, such as those documented by Minnis (2011), remain unchanged pre- and post-2004. This enables us to empirically isolate the economic effects of the audit choice from those of the audited reports.

To mitigate the concern that the treatment firms' investment/financing behavior changes as a result of changes in their growth opportunities (or other confounding changes) rather than a relaxation in their financing frictions (via the audit regulation), we construct two samples of benchmark firms and use a difference-in-differences estimator. Our first control sample consists of firms that were exempt from the audit requirement even before 2004, but choose to voluntarily receive audits despite the exemption. Similar to the treatment firms, these control firms also choose to voluntarily get audits, but their audit choice is observable both before and after 2004. Our second control sample consists of firms that are subject to the audit mandate both before and after 2004.[1] That is, we construct three samples of firms whose audit choice is (i) observable pre- and post-2004, (ii) unobservable pre-2004, but observable post-2004, and (iii) unobservable pre- and post-2004. Sample (ii) serves as our treatment firm sample, and samples (i) and (iii) serve as our control samples (see Figure 1 for a diagrammatic representation of our design). To ensure that the treatment and control firms have similar investment and financing opportunities, we match these firms on the determinants of investment and financing documented in prior research (i.e., debt, growth, performance, and liquidity). Importantly, all firm-years in our data (treatment

[1] Note that we do not use firms that opt out of receiving audits as control firms in any of our tests because these firms lose any economic benefit that comes with having financial statements audited. Thus, these firms are not comparable to our treatment firms. Nevertheless, for completeness, we examine firms that choose to stop receiving audits in Section 6.4.

and control) always receive audits and *only the observability of the audit choice varies over time* for our treatment firms. We also require all firms in our sample to have at least one observation in the pre- and post-treatment period to mitigate the concern that changes in sample composition affect our results. In sum, our research design parses out economic effects of the information in the audit choice from all the other effects of the audit because (i) our entire sample of treatment and control firms obtain audits over the entire sample period, thereby holding the economic effects of the audits constant, and (ii) the regulatory change allows us to use the firm as its own control and thus the reasons why the firm chose to obtain an audit will not affect our results unless the firm changed at the same time as the audit regulation.

We find that the treatment firms significantly increase their total debt and investment following the regulation as compared to both sets of control firms. Our coefficients imply that treatment firms increase debt by approximately 4 to 7%, and investment by approximately 7 to 12% following the regulation, depending on the control sample used. We also find that our treatment firms observe a 4 to 9% reduction in the cost of debt after the regulation. This evidence is consistent with the audit *choice* conveying important information to capital providers (independent to that conveyed by audit itself) and thus relaxing financing constraints.

We then examine whether the treatment firms improve their investment efficiency and operating performance following the audit regime shift, as the information in the audit choice reduces financing constraints. Consistent with our expectation, we find that the treatment firms become significantly more responsiveness to their investment opportunities and observe a 6 to 12% increase in their operating income after the regulation relative to that of the control firms. These results provide additional evidence that the choice to obtain an audit contains information that relaxes financing frictions, leading to more efficient investment and better firm performance.

We conduct numerous additional tests to further validate our inferences and mitigate endogeneity concerns. For example, we empirically show that the parallel trends assumption, which is the central assumption of our difference-in-differences estimator, is satisfied in the pre-treatment years spanning 2001 to 2003, and take a number of measures to ensure this assumption holds post-treatment as well (which we discuss in detail in Section 5.2.). We conduct cross-sectional tests using on variation in the treatment firms' financing constraints and access to finance to help reinforce our main inference. We use dynamic regression models to show that the economic effects we document are absent the year before the regulatory change and take effect only in the years following the regulation. We show that neither audit quality nor earnings quality change around the regulation. We also conduct tests to show that the information revealed from observing the audit choice is indeed hidden in the mandatory audit regime. Collectively, these tests significantly narrow down the probability that any alternative hypothesis explains our findings. We discuss these tests in detail in Sections 5 and 6.

Finally, a potential alternative hypothesis is that our treatment firms choose to obtain an audit post-2004 because they have had a shock to growth opportunities that (i) coincides with the 2004 regulation and (ii) does not affect the control firms (even though they are observably similar and get audits). To address this concern, we first directly examine whether the treatment firms have an increase in growth opportunities from 2003 to 2004 relative to that for the control firms. Consistent with our expectation, we find that there is no increase in treatment firm's growth opportunities at the time of the regulation. Second, we examine the sub-sample of our treatment firms that move back to a mandatory audit regime because they grow and surpass the £5.6 (£2.8) million sales (asset) threshold necessary to qualify for the audit exemption, thereby making their audit choice unobservable again. Essentially, these firms' audit choice is unobservable pre-2004, becomes observable for one to three years post-2004, and then becomes unobservable again. We find that the treatment firms that move back to a mandatory audit regime see a reduction in debt and an increase in their cost of debt once their audit choice becomes unobservable again compared to those treatment firms whose audit choice continues to be observable. We also observe a marginal reduction in investment that is significant at the one-tailed 10% level. These results are especially helpful in mitigating alternative explanations because they show that firms' investing/financing behavior shifts in a predictable manner that is contingent on the observability of the audit choice.

Our paper contributes to the growing research on the role of audits in a private firm setting. For example, Minnis (2011) finds that by verifying financial statements, audits increase earnings quality and lower the cost of debt for U.S. private firms. Lisowsky et al. (2015) find that the verification role of audits helps increase the portfolio quality in the commercial loan market.[2] These prior studies focus on the verification or assurance role of an audit, and try to control for selection effects, such as the information in the audit choice. Our findings extend this literature by showing that aside from the verification benefits of an audit, information embedded in the audit *choice* is incrementally informative to external investors. This incremental information in the audit choice (that is orthogonal to information in the audited reports) helps reduce financing frictions, leading to increases in debt, investment and overall firm performance.

Our findings, and the distinction between the audit choice and the verification benefit of audits, are particularly important because they have different regulatory implications. Specifically, the verification benefit of audits is often used to justify mandatory audits for firms. However, our results suggest that regulation that mandates firms to receive audits conceals the information contained in a firm's endogenous choice to receive the audit. We find that the information in the audit choice is distinct from the information conveyed by audited reports and, as such, is lost when audits are mandatory. It is worth noting that financial statement audits are mandatory for many private firms across the world (e.g., Australia, Brazil, India, and Russia, among others) and almost universally mandated for public companies with dispersed ownership.

[2] Also see Allee and Yohn (2009), Lisowsky and Minnis (2013), and Minnis and Sutherland (2014).

Although policy implications cannot be made without more analysis, our results indeed suggest that the audit mandate has some economically significant drawbacks.

Finally, our paper builds on recent work by Lennox and Pittman (2011), who find that firms that choose to obtain audits receive higher credit scores than those that choose to be unaudited. Our paper differs from Lennox and Pittman (2011) in two key respects. First, our analyses focus on real decisions made by firms – i.e., financing and investment, whereas Lennox and Pittman (2011) examine a credit score supplied by Qui Credit Assessment, a small regional firm. Second, Lennox and Pittman (2011) draw their inferences by comparing firms that choose to obtain audits to those that choose to be unaudited. As a result, they turn two dials in their analyses – (i) the observability of the audit choice and (ii) the verification of financial statements, thereby making it unclear whether their results are driven by (i) information in the audit choice, (ii) a drop in the control firms' reporting quality/credibility from being unaudited. Our analyses contribute by parsing out these competing explanations.

The rest of the paper proceeds as follows. Section 2 develops our hypotheses. Sections 3 and 4 discuss our setting and data. Sections 5 and 6 present our results, and Section 7 concludes.

## 2. Hypothesis development

A primary role of an audit is to verify the accuracy of financial statement disclosures and thereby assure users that financial statements are reliable (DeFond and Zhang, 2014). Theory suggests this verification benefit of an audit reduces financing frictions, such as adverse selection and moral hazard between managers and capital providers, which improves resource allocation and contracting efficiency (Jensen and Meckling, 1976; Watts and Zimmerman, 1983). For example, audits can reduce information problems ex ante by increasing the credibility of financial statements (Minnis, 2011; Lisowsky et al., 2015). Ex post, audits can assure contracting parties that the financial statements have been prepared in accordance with the provisions of the contract (Watts and Zimmerman, 1978, 1983). See Armstrong et al. (2010) and DeFond and

Zhang (2014) for surveys of the contracting and auditing literatures, respectively.

A relatively underexplored aspect of obtaining audits is whether the choice to obtain an audit conveys any incremental information to external stakeholders, distinct from the information in the audited reports. We posit that an audit can provide information to external investors about a firm's future prospects when the choice to obtain the audit is publicly observable, because the audit is costly for firms in terms of money paid to the auditors and considerable management time/effort necessary to prepare for, and oversee, the audit process. Thus, the willingness of management to voluntarily incur these audit costs informs external parties that the firm expects to make sufficient future profits to recoup the cost of the audit.[3]

Our intuition draws primarily from two theories. First, our intuition can be derived from the 'signaling' literature (e.g., Spence, 1973; Jensen and Meckling, 1976; Melumad and Thoman, 1990), where high quality economic agents/firms undertake costly actions to separate themselves from low quality agents/firms. For example, Jensen and Meckling (1976, p. 325) discuss that firms can incur "bonding costs" such as a contractual guarantee to have the financial statements audited to protect creditors against malfeasance on the part of the manager. Here the audit serves to create a separating equilibrium where high quality firms commit to getting an audit that is too costly (i.e., not net beneficial) for low quality firms to mimic. Similarly, Melumad and Thoman (1990, p. 79) show analytically, "a firm would rather hire an uninformative auditor [i.e., an audit that provides no information] and be known as an average risk firm than not hire an auditor and be thought of as a high risk by lenders." In our setting, firms with the most profitable investment opportunities are more willing to incur the costs associated with audits, as they are in the best position to reap benefits from those costs.

Second, our intuition also comes from the 'screening' literature, and in particular the theory developed in Guasch and Weiss (1981), who develop a labor market model, where an

[3] "Sufficient future profits" is a function of both the amount and probabilities/uncertainty associated with the investment payoffs – i.e., the quality of future prospects is a function of the first and second moments of the payoffs.

employer is screening applicants for potential employment. Guasch and Weiss (1981) show that aside from requiring applicants to take a pass-fail test to screen for better candidates, having applicants pay a fee to take the test conveys incremental information about the quality of the applicant, as test results can be imperfect. Their intuition is that the fee deters low ability applicants from applying for the job because they know that they are more likely to fail the test. Applying their intuition to our setting, the firm undergoing an audit to acquire capital is analogous to the applicant paying to take the test. The decision to undergo an audit (that is costly for the firm) conveys incremental information to external financiers about the quality of the firm and its prospects, over and above the information provided by the audited financial statements.[4]

Note that the two theories are very similar. In particular, in the Gausch and Weiss (1981) setting, it is the signal receiver (employer/bank) requiring the signal rather than the signal sender (applicant/firm) initiating the signal. In other words, when banks require firms to obtain audits as a pre-requisite for lending, the act of getting the audit can be viewed as a 'screen' rather than a 'signal.' However, as Spence (1976, p. 592) indicates, "We can refer to the subject as signaling or screening interchangeably, bearing in mind that they are opposite sides of the same coin." Thus, our intuition applies irrespective of which economic agent initiates the signal; in either case, firms must choose whether they want to undertake the cost of the audit to obtain financing. Our interest lies in determining whether that choice conveys information to lenders, irrespective of which party initiated the signal. The above discussion leads to our main hypothesis:

*H: A firm's choice to voluntarily obtain an audit provides information about the firm's future prospects that reduces financing frictions, leading to an increase in debt and investment.*

[4] Our intuition is also related more broadly to 'revealed preferences' theory, which examines an agent's choices and tries to infer the implications regarding the agent's preferences assuming the agent acts optimally (Samuelson, 1948). Although much of this literature focuses on consumer decision-making, we apply similar intuition for our setting that examines managerial decisions. In particular, we argue that the manager's decision to incur the cost of an audit 'reveals' that the audit is net beneficial for the firm. That is, the revealed preference implies that, at a minimum, the firm expects to recoup all audit-related costs from the future cash flows related to the investment payoffs facilitated by the audit. As such, this observable audit choice provides external financiers with incremental information regarding firms' future cash flows, and thus their ability to repay their loans, i.e., their credit risk.

## 3. Institutional setting

We use a setting in the U.K. that provides a unique opportunity to test our hypothesis. In the U.K., the Companies Act of 1967 (now part of the Companies Act 1985) required all limited liability companies, private and public, to file their financial statements annually with the Registrar.[5] Further, all financial statements must be prepared in accordance with U.K. accounting standards and must be audited by a registered auditor. Failure to comply with these rules is a criminal offense. Critics of the Companies Act argued that the imposition of universal regulatory standards results in a disproportionately high cost for small companies. Specifically, prior studies and industry groups argued that complying with regulation has large fixed cost elements and small companies are generally unable to take advantage of the economies of scale (see e.g., DTI, 1985; Rutteman, 1985). In an effort to reduce the burden of regulation, the Companies Act 1985 relaxed the above reporting requirements, including the audit requirement. Audit exemptions were first granted to the very smallest private firms in the U.K. in 1994, because the regulation was viewed as disproportionately expensive and of limited benefit for small firms (Keasey et al., 1988). Post-1994, companies with sales not exceeding £90,000 and assets not exceeding £1.4 million were exempt from the audit requirement via Section 249A of the Companies Act 1985 (SI 1994/1935) even though their financial statements were required to be filed in a public repository (the Companies House). Subsequently, the size thresholds to qualify for the audit exemption were progressively relaxed in 1997, 2000, 2004, 2008, and most recently in 2012.

In this study, we focus on the 2004 regulatory change, which allowed companies with fiscal years ending after March 30, 2004, to opt-out of an audit if their sales did not exceed £5.6 million and total assets did not exceed £2.8 million. Prior to 2004, only firms with sales less than

[5] Limited liability companies in the U.K. are formed by incorporation with the Companies House, the government agency that administers them. Companies House is an executive agency of the U.K. Department of Trade and Industry (DTI). The main functions of Companies House are to incorporate and dissolve limited companies, examine and store company information delivered under the Companies Act and related legislation, and make this information available to the public. For more information about Companies House, see http://www.companieshouse.gov.uk/about/functionsHistory.shtml.

£1 million and total assets less than £1.4 million were exempt from the audit requirement.[6] This regulatory change allows us to construct a sample of firms that went from being audited under a mandatory audit regime pre-2004 to being voluntarily audited post-2004. We focus on the 2004 regulation because: (i) it is the largest increase in the threshold limits, thereby allowing a larger number of companies to qualify for the exemption and thus increasing our sample size, and (ii) it provides us with a panel dataset of companies with at least three years of data both before and after the regulatory change. Since our dataset (discussed later) covers firms with fiscal years ending between 2000 and 2010, we do not have pre- and/or post-regulation data for the audit exemptions in 1994, 1997, 2000, and 2012. Although the 2008 exemption allows us to construct a dataset of firms with pre- and post-regulation data, the size thresholds increased by just 16% for both sales and assets, i.e., from £5.6 to £6.5 million in sales and £2.8 to £3.3 million in assets, compared to a 560% (100%) increase in the sales (assets) threshold in 2004. Thus, the 2008 regulatory change is simply not as powerful as the 2004 change.[7]

The U.K. setting is almost ideal to test our hypotheses for three reasons. First, our sample firms are small, privately owned firms with highly concentrated ownership. As a result, financing frictions are likely to be a first order concern for them, and any relaxation of financing

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[6] More precisely, to qualify for an audit exemption, companies are required to stay below at least two of three size thresholds. The third size threshold (besides sales and assets) is based on the number of employees; a company can qualify for the audit exemption if it has less than 50 employees in addition to meeting either the sales or asset threshold. The 2004 regulation did not change the 50 employee threshold limit. The database we use for this study does not provide coverage of the number of employees. As a result, some of our treatment and control firms might be misclassified. However, we note that any such classification error biases against our hypothesis. These amendments are contained in the Companies Act 1985 (Accounts of Small and Medium-Sized Enterprises and Audit Exemption) (Amendment) Regulations 2004, which was laid before Parliament on January 9, 2004. This amendment is Statutory Instrument 2004 No.16 and can be downloaded from the HMSO website at http://www.legislation.hmso.gov.uk/si/si2004/20040016.htm. Note that the audit exemption comes into force two months after the other provisions in the amendment became effective (on January 30, 2004).

[7] The size classification (based on assets, sales and number of employees) dictates not only the audit requirement but also the public disclosure requirements. As a result, the audit exemption we examine overlaps with a relaxation in the disclosure requirement. Specifically, firms newly exempt from the audit requirement are also exempt from the previous requirement to file an income statement with the Companies House. However, we note that our sample firms, by construction, continue to disclose their income statement. Moreover, Bernard et al. (2014) find that, unlike the audit requirement, the disclosure requirement is not perceived to be costly by private firms in the U.K. Thus, the relaxation of the disclosure requirement should not affect our inferences. See Bernard et al. (2014) for additional details about the exact disclosure and audit requirements for small private firms in the U.K.

constraints via the information in the audit choice is likely to have a significant effect on the investment and financing decisions of these firms. Second, audits are especially costly for these firms (as indicated by the intent of the regulation to ease the burden of audits on these small firms), thereby satisfying the condition that audits have to be costly.[8] Third, this setting allows us to identify a sample of firms that switched from obtaining mandatory audits to voluntary audits due to the 2004 regulatory change, and as a result, hold constant the verification role of the audit to isolate the incremental effect of the *audit choice*. Importantly, since the audit exemptions were granted to firms in a staggered manner from 1994 to 2012, we can construct control groups that are similar in almost every respect to the treatment firms except that they are unaffected by the 2004 regulation. We provide a detailed discussion of our research design strengths below.

**4. Data sources and sample selection**

Financial statement data for private firms are obtained from the "Financial Analysis Made Easy" (FAME) database supplied by Bureau Van Dijk. FAME contains data from income statements and balance sheets along with basic information, such as SIC industry codes, on over two million public and private British companies with up to ten years of data for each company.[9] We obtain access to data for companies with fiscal years ending between December 2000 and 2010. To construct our sample, we begin by identifying companies that qualify for the 2004 audit exemption (i.e., private firms with sales less than £5.6 million and assets less than £2.8 million) as of 2003 and 2004. Further, we require these companies to have at least £1 million in sales or £1.4 million in assets to ensure that they were not exempt from the audit requirement prior to 2004. As a result, these companies were required to obtain audits pre-2004, but could opt-out of

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[8] Although we cannot observe the time and effort incurred by management related to the audit, we do provide some empirical evidence in section 5.4. on (i) the relative size of audit fees, and (ii) attempts by firms to avoid mandatory audit size thresholds to support the notion that audits are costly for these firms.

[9] As described above, under current legislation in the U.K., private and public companies must file their financial statements with Companies House, where they are processed and checked, and subsequently made available to the public. Jordans, a provider of legal information in the U.K., collects data from Companies House daily and transfers it to its own database. Bureau Van Dijk collects these data from Jordans to compile the FAME database.

the audit requirement post-2004. These firms serve as our treatment sample. We require treatment firms to: (i) have a matched control firm (described below), (ii) operate in a non-financial industry, (iii) have non-missing data for all the variables used in our analyses, and (iv) have both assets and sales greater than £15,000. In addition, all firms are required to have at least one observation both before and after the regulatory change.

We match each treatment firm with at least one of two control firms that have non-missing data for all variables used in our main analyses. The first control sample includes firms that voluntarily obtain audits both before and after the regulatory change (henceforth referred to as the "voluntary audit" control sample). These firms have sales less than £1 million *and* assets less than £1.4 million prior to 2004 and obtain an audit (voluntarily) throughout our sample period.[10] The second control sample includes firms required to obtain audits both before and after the regulatory change (henceforth referred to as the "mandatory audit" control sample). This control sample is comprised of firms with sales greater than £5.6 million *and/or* assets greater than £2.8 million during our sample period. Figure 1 provides a diagram of our research design. Our sample period runs from 2001 to 2006, giving us three years before and after the regulatory change in March 2004. Table 1 describes our sample selection procedure in detail.

## 5. Research design, validating key assumptions, and results

### *5.1. Research design*

We estimate the following difference-in-differences regression to test our predictions:

$$y_{i,t} = \alpha_i + \alpha_t \times \alpha_{ind} + \beta\, TREATMENT\_FIRM \times POST\_REG_{i,t} + \gamma' \boldsymbol{X} + \varepsilon_{i,t} \qquad (1)$$

where *i* (*t*; *ind*) indexes firms (years; industry), $y_{i,t}$ is investment (*INVESTMENT*) or total debt (*DEBT*), $\alpha_i$, $\alpha_t$, and $\alpha_{ind}$ are firm, year, and industry fixed effects, *TREATMENT_FIRM* is an

[10] An added benefit of using the "voluntary audit" control sample is that, similar to our treatment firms, these firms chose to receive an audit, except they were able to do so both before and after the regulation. An implicit assumption is that the audit choice is effective in relaxing financing frictions for these firms as well. To provide empirical support for this assumption, we find (in untabulated analyses) that these firms experience growth in debt and investment over the sample period, as compared to similar size firms that opted out of getting an audit.

indicator variable that equals one (zero) for treatment (control) firms, *POST_REG* is an indicator variable that equals one for fiscal years ending after March 30, 2004, and ***X*** is a vector of controls that includes sales growth, firm size, profitability, liquidity, and audit fees.[11] Sales growth proxies for growth opportunities (Shin and Stulz 1998; Whited 2006; Badertscher et al., 2013); size, profitability, and liquidity proxy for the availability of financing to engage in investment (Kaplan and Zingales, 1997; Hadlock and Pierce, 2010). Audit fees captures differences in the verification value of an audit (Simunic, 1980; Hay et al., 2006).

The identifying assumption in our difference-in-differences regression is that the treatment and control firms have parallel trends in investment/debt. To satisfy this assumption, we match the treatment firms to control firms based their growth opportunities and access to finance. Specifically, we match on the following variables within each industry and year before the regulatory change (i.e., March 2004): sales growth (*SALES_GR*); debt (*DEBT*), return on assets (*ROA*), and liquidity (*LIQUIDITY*). We use nearest neighbor matching within caliper, which is set at 0.5 times the standard deviation of the variable (Rosenbaum and Rubin, 1985).[12]

Although firm size is a commonly used proxy for financing constraints, we do not match on size because the regulation partitions firms based on size thresholds and thus the treatment and control firms do not overlap along this dimension. Nevertheless, we conduct all our tests using two control samples: (i) smaller firms that were exempt from the audit requirement even before 2004 (i.e., the voluntary audit control sample) and (ii) larger firms that are required to obtain audits mandatorily even after 2004 (i.e., the mandatory audit control sample). Since the mandatory (voluntary) audit control firms are systematically larger (smaller) than the treatment firms, any monotonic relation between firm size and investment cannot explain our findings.

[11] The main effects of *TREATMENT_FIRM* and *POST_REG* are absorbed by the firm and industry × year fixed effects, and thus not identified in the equation above.

[12] We verify the robustness of our inferences to using two additional matching approaches (untabulated). We match firms based on: (i) their propensity to obtain voluntary audits using estimated values of their voluntary audit propensities, and (ii) variables that include total assets, sales, and, auditor. Our inferences are unchanged in all of the above specifications (i.e., our coefficients of interest are significant at the two-tailed 10% level or better).

Table 2, Panel A compares the mean values of the matching variables for our treatment sample with those for the two control samples, each year before the regulation. The table indicates that our matching procedure results in no statistically significant difference between our treatment firms and the two sets of control firms with respect to the matched variables in each of the pre-treatment years. Therefore, our control firms are observably similar to the treatment firms before the regulatory change in terms of their investment opportunities and access to finance.

### *5.2. Parallel trends assumption and research design strengths*

The identifying assumption central to the interpretation of our difference-in-differences estimates is that treatment and control firms share parallel trends in debt and investment. Table 2, Panel B shows that the pre-treatment trends in both debt and investment are indeed indistinguishable. The question then (for any difference-in-differences design) is whether the post-treatment trends would have continued to be parallel had it not been for the audit regime change. Our empirical design takes several steps to mitigate the concern that the treatment firm trend would have changed even in the absence of the regulation. First, we include industry-year fixed effects in all our regressions to difference away unobserved *time varying or dynamic* post-treatment trends at the industry level in investment and debt. Second, we include firm-fixed effects in our regressions to difference away unobserved firm-specific trends in investment and debt levels. Third, we control for standard firm-level characteristics (such as size, growth, and profitability) that could cause trends to diverge post-treatment for reasons unrelated to the audit regime change. We find that adding these controls has very little effect on our treatment effect, implying that the audit regime change is most likely exogenous. Notwithstanding the above, we conduct two cross-sectional tests that exploit differences in firms' ex ante characteristics to further mitigate identification concerns.

A few observations about our research design are in order. First, we require control and treatment firms to have at least one observation in both the pre- and post-treatment periods.

Thus, changes in sample composition do not affect our results. Second, we require both sets of control firms and the treatment firms to have audits over the pre- and post-regulation periods. This ensures that any differences across the treatment and control samples are not due to differences in the existence of an audit. Importantly, firms comprising the voluntary control sample also *choose to obtain an audit* every year during our sample period and essentially differ from our treatment firms only because their choice to obtain an audit is observable both before and after 2004, but our treatment firms' choice to obtain an audit is observable only post 2004. As a result, the only change from the pre- to post-regulation audit regimes is that our treatment firms' decision to obtain an audit becomes observable by external investors, but not for the control firms even though every firm-year in our sample obtains an audit.

Second, it is possible that the treatment firms systematically have an increase in investment opportunities at the time of the regulation that is not shared by the control firms (which would be a violation of the parallel trends assumption). It is precisely to address this concern that we use control samples matched on the determinants of investment and financing. Further, our treatment firms are sandwiched between two sets of control firms that are slightly larger in size (mandatory audit control group) and slightly smaller in size (voluntary audit control group) than the treatment sample. Given the similarities in all the observable characteristics, the indistinguishable pre-treatment trends in investment/debt, and the fact that the voluntary control firms also choose to obtain audits, it is unlikely that any changes in investment opportunities in 2004 will only be localized to our treatment firms but not control firms. Nevertheless, we conduct two additional tests to address this potential concern in section 6.1.

### *5.3. Descriptive statistics*

Table 3 presents summary statistics for our variables of interest. Panel A (B) reports the statistics for the treatment sample and its matched voluntary (mandatory) audit control sample both before and after the regulation. For brevity, we discuss the summary statistics for only our key variables of interest. Panel A shows that treatment firms and the voluntary audit control firms have similar *DEBT* before the regulation, but the treatment firms observe a relative increase in *DEBT* after the regulation. The average *INVESTMENT* is 4.1% of total assets before 2004 and increases to 4.3% following the regulation for our treatment sample. In contrast, the voluntary audit control sample's average *INVESTMENT* reduces from 3.9% to 3.6% over the same period. In Panel B, which presents the summary statistics for the treatment sample and the matched mandatory audit control sample, *DEBT* is statistically indistinguishable for the treatment and control samples before 2004, but the treatment firms have significantly higher *DEBT* after the regulation. Further, treatment (control) firms increase (decrease) *INVESTMENT* from 4.1% to 4.3% (4.4% to 3.9%) after the regulation. These patterns are consistent with that observed in Panel A for the voluntary audit control sample.

*5.4. Validating necessary conditions for the audit choice to provide information to investors*

The theoretical framework we rely on has two necessary conditions for the audit choice to provide information to external investors that relaxes financing frictions. First, we require that external financiers cannot ex ante distinguish between firms that would obtain audits voluntarily from those that would not do so in the mandatory audit regime. Only to the extent that external investors fail to separate these firms can the audit choice provide incremental information in the voluntary audit regime. Second, the audit has to be sufficiently costly that firms with weak or doubtful future prospects do not find it worthwhile to voluntarily get and pay for the audit.

To validate the first assumption, we compute the probability of type I (false positive) and type II (false negative) classification errors for the entire sample of firms that *qualify* to opt out from the audit mandate in 2004. Specifically, we estimate a logistic regression where the dependent variable equals one for firms that choose to obtain audits post-2004 and zero for firms that choose to opt out of the audit requirement post-2004. The independent variables include the following firm characteristics that are likely to be associated with the probability of obtaining a voluntary audit: (i) investment growth, sales growth, ROA growth, and debt growth, which proxy for investment opportunities and growth, (ii) liquidity and ROA, which proxy for external financing needs, (iii) firm size and the number of directors on the board, which proxy for monitoring difficulties and agency costs, and (iv) audit fees and the use of a big-4 auditor, which proxy for firms' desire to obtain higher quality audits, beyond the minimum requirement imposed by the audit mandate.[13]

Table 4, Panel A presents the results from the logistic regression. We find that although a number of variables in our model are significantly related to the probability of getting a voluntary audit, the model has limited explanatory power (pseudo r-squared=10%). Panel B presents the classification errors from using the model to predict the probability that a firm obtains an audit after 2004. We find that the model leads to large type I and type II errors and, as such, is unlikely to be very useful in separating firms that are likely to obtain voluntary audits ex ante. For example, using firms above the median predicted probability of obtaining a voluntary audit as the cut-off, we find that 27.6% of the firms that do not obtain audits are classified as obtaining audits, and 42.5% of the firms that obtain audits are classified as not obtaining an audit by the model. These results suggest that, ex ante, external financiers are unlikely to have been able to accurately distinguish between firms that would and would not obtain audits voluntarily.

To validate the second assumption (i.e., audits are costly), we examine the audit fees paid by the typical firm. Table 3, Panel A, shows that the average treatment firm in our sample pays £5,316 (£6,017) for an audit before (after) the regulation. While some may argue the absolute magnitude of the fee is small, it is important to consider that it represents approximately 6% of the average firm's earnings. Further, the above cost does not include non-monetary costs incurred during an audit related to significant managerial time and effort devoted to getting

[13] In untabulated tests, we estimate many different versions of the above logit model that include/exclude a number of additional explanatory variables. In all our iterations, we find that our main inference is unaffected.

through the process. In fact, the large burden of audit and other regulatory costs is precisely what led to the relaxation of the audit requirement for small firms (DTI, 1985).

To further examine whether the audit does indeed represent a non-trivial cost for our sample firms, we follow Bernard et al. (2014) and exploit the fact that the audit mandate for U.K. private firms is contingent on firms exceeding "bright-line" size thresholds. If the audit represents a significant cost, then at least some firms near the size thresholds are likely to manipulate their size (i.e., assets and/or sales) downwards to obtain an exemption from the audit requirement. To test this prediction, we follow the approach in Burgstahler and Dichev (1997) and examine the frequency distributions of firms along the continuum of size (sales and assets) bins. Consistent with our expectations and the evidence in Bernard et al. (2014), Figure 2, Panels A and B show that from 2001 to 2003 there is an unusually higher (lower) frequency of firms in the size bins immediately below (above) the threshold to qualify for an audit exemption (with the discontinuity significant at the 1% level). Further, this discontinuity in the frequency of firms around the size bins near the pre-2004 regulatory threshold disappears when the size thresholds to qualify for the audit exemption change in 2004. These results suggest that firms manage their size to qualify for the audit exemption and thus indicate that audits represent a non-trivial cost for small private firms in the U.K. It is also noteworthy that the discontinuity in the distribution of sales is especially stark near the regulatory threshold (see Figure 2, Panel A). This is consistent with our expectation because the sales threshold was lower than the assets threshold pre-2004 (£1 million vs. £1.4 million) and thus was more likely to be the binding constraint.[14]

*5.5. Tests of the main hypothesis: The effect of the audit choice on debt and investment*

Table 5 presents our main results. In Panels A and B, we tabulate the results concerning the effect of the audit choice on debt level and the cost of debt, and Panel C presents the

[14] As indicated earlier, firms have to be below two of the three thresholds (sales, assets, and employees) to be exempt from the audit requirement. We do not analyze the distribution of employees due to data limitations.

investment results. In each panel, we present three sets of regression results that correspond to using (i) a baseline specification without any control sample, (ii) firms obtaining *voluntary* audits before and after the regulation as the primary control sample, and (iii) firms obtaining *mandatory* audits before and after the regulation as an alternative control sample. The coefficient of interest in the regressions without a control sample is that for *POST_REG*, and the coefficient of interest in regressions with control samples is that for *POST_REG* × *TREATMENT_FIRM*.[15]

Table 5, Panel A shows that our treatment firms significantly increase their debt levels across all specifications. In particular, our baseline specification without any control sample suggests that the treatment firms increase debt by 0.9 percentage points following the regulation (i.e., coefficient on *POST_REG* = 0.009; t-statistic = 4.37) and the difference-in-differences specifications suggest that our treatment firms increase debt by 1.6 (1.0) percentage points relative to the voluntary audit control sample (mandatory audit control sample). To provide some sense for the economic significance, the coefficient in the voluntary (mandatory) audit control sample regression suggests that debt increases by £8,546, or 6.7% (£5,578, or 4.8%).

Next, we examine whether the audit choice signal leads to a reduction in the cost of debt. To the extent the audit choice conveys incremental information about the quality of the borrowers' future prospects (and thus their credit risk) to creditors, we should observe a reduction in the cost of the borrower's debt. Following Minnis (2011), we proxy for the cost of debt using interest expense scaled by debt because we do not have data on actual interest rates charged to the firm. Further, to determine whether there is a change in the cost of debt from the pre- to the post-regulation regime, this analysis is conducted only on firms that have non-zero debt both before and after the regulation. Table 5, Panel B shows that our treatment firms have a lower cost of debt after the audit regime change, across all specifications. The coefficients for *POST_REG* (in column 1) and *POST_REG* × *TREATMENT_FIRM* (in columns 2 and 3) are negative and statistically significant at the 10% level or better. The coefficients imply that cost of

[15] To estimate the coefficient for *POST_REG*, we do not include year indicators in the baseline specification.

debt reduces between 3.8% (or £617) and 9.3% (or £1,093). These results provide further support for our inference that the audit choice conveys information that reduces financing frictions.

The examination of the cost of debt helps us calibrate our results with that documented in prior research. For example, Minnis (2011) finds that interest rates decrease by 69 basis points (ranging between 25 to 105 basis points) for his sample of U.S. private firms because of the verification role of an audit. Our analysis indicates that the audit choice signal leads to a 30 to 80 basis points reduction in interest rates, depending on the control sample used. Thus, our results suggest that the audit choice signal has an economic benefit that is similar to that derived from the verification role of an audit, which is arguably the primary role of an audit.

Finally, Table 5, Panel C presents the results examining the effect of the audit choice signal on investment. Column 1 shows that the coefficient for *POST_REG* is 0.002, and it is statistically significant at the 1% level (t-stat. = 2.68). This coefficient suggests that firms increase their investment by 0.2 percentage points following the regulatory change that allows them to signal their future prospects by obtaining a financial statement audit. In economic terms, this coefficient represents a £9,719 increase in investment for the average treatment firm and corresponds to a 7% increase in investment from its conditional mean. Columns 2 and 3 present the regression results using firms obtaining voluntary and mandatory audits as the control sample, respectively. We find the coefficient for *POST_REG* × *TREATMENT_FIRM* is 0.009 (0.005) when the control sample comprises of firms obtaining voluntary (mandatory) audits and the coefficient is statistically significant at the 1% level. These coefficients indicate that the treatment firms increase investment by 0.9 (0.5) percentage points more than the voluntary (mandatory) control firms do, on average. In terms of economic magnitude, these coefficients suggest that investment increased by £12,279 (£14,278), representing approximately a 12% (9%) increase relative to the voluntary (mandatory) audit control sample.

Table 5 also shows that the coefficients for the control variables are consistent with prior research. For example, in Panel C, we find that the coefficients for *SALES_GR*, *ROA* and *LIQUIDITY* are positive and statistically significant at the 5% level or better in all regressions, suggesting the firms with greater investment opportunities, profitable firms and firms with greater liquidity tend to invest more. Similarly, the coefficient for *LSIZE* is negative and statistically significant at the 1% level, suggesting that larger firms tend to invest less.

To further corroborate our inferences above, we examine the dynamic effects of the audit regime shift on the debt levels, the cost of debt and investment of our treatment firms. Specifically, we replace the *POST_REG* indicator variable with the following four indicator variables: *POST_REG [-1]*, *POST_REG [0]*, *POST_REG [1]*, and *POST_REG [2]*, where *POST_REG [-1]* is an event time indicator that equals one for the year immediately preceding the audit regime shift, *POST_REG [0]* is an indicator that equals one for the year of the audit regime shift, and *POST_REG [1]* and *POST_REG [2]* are indicators that equal one for the year immediately following the audit regime shift and two years after the audit regime shift, respectively. These indicator variables enter our regressions as interactions with the *TREATMENT_FIRM* indicator, and their main effects are absorbed by the inclusion of year × industry fixed effects. To the extent the audit regime shift was a relatively exogenous event and not part of any pre-existing trend, we should find that our treatment firms increase their debt and investment levels only when the regulatory change became effective.

Table 6 presents the results. For brevity, we only tabulate the results for the voluntary audit control sample; however, our inferences are similar using the mandatory audit control sample to benchmark treatment effects. We find that the coefficient for *POST_REG [-1]* × *TREATMENT_FIRM* is statistically insignificant in all three regressions: debt, cost of debt, and investment. Further, the coefficients for *POST_REG [0]* × *TREATMENT_FIRM*, *POST_REG [1]* × *TREATMENT_FIRM*, and *POST_REG [2]* × *TREATMENT_FIRM* are all statistically significant at the 1% level (10% level or better) in the investment and debt (cost of debt) regressions. These results significantly strengthen our inference by mitigating endogeneity concerns related to the existence of a pre-existing trend in our variables of interest.

### *5.6. The effect of the audit choice information on investment efficiency and firm performance*

We next examine whether the information in the audit choice allows firms to respond more quickly to their growth opportunities and improve their overall operating performance. Our hypothesis is that the information in the audit choice conveys incremental information to external financiers about the firms' future prospects, thereby increasing the firm's access to finance (i.e., debt capacity) and their financial flexibility. Prior research suggests that financial flexibility enables firms to avoid financial distress in the face of negative shocks, and to readily fund investment when profitable opportunities arise (Gamba and Triantis, 2008). Specifically, financial flexibility facilitates easier and cheaper access to financing, thereby allowing firms to rapidly increase investment in response to growth opportunities. Further, financial flexibility also allows firms to decrease and/or abandon investment during down turns because their financing terms are less restrictive (e.g., fewer/less restrictive covenants, no/fewer restrictions on asset sales, etc.) and their assets are less likely to be tied up as collateral (Bradley and Roberts, 2004).

To test whether the information in the audit choice makes firms more responsive to their growth opportunities, we augment equation 1 by interacting sales growth (*SALES_GR*) – our proxy for growth opportunities – with *TREATMENT_FIRM* and *POST_REG*. As before, we present three sets of results that correspond to the baseline specification without any control sample and two difference-in-differences regressions using firms obtaining voluntary audits before and after the regulation or firms obtaining mandatory audits before and after the regulation as control firms. The variable of interest in our baseline specification is *SALES_GR* × *POST_REG* and the variable of interest in the difference-in-differences specifications is

*SALES_GR* × *POST_REG* × *TREATMENT_FIRM*. The coefficients for these variables capture the incremental investment-growth sensitivity following the regulatory change in 2004.

Table 7, Panel A presents the regression results. Consistent with our prediction, we find that the coefficient for *SALES_GR* × *POST_REG* is positive and statistically significant at the 1% level (coef. = 0.008; t-stat. = 2.99) in column 1. This coefficient suggests that our treatment firms become more responsive to their growth opportunities following the regulation. Similarly, columns 2 and 3 show that the coefficient for *SALES_GR* × *POST_REG* × *TREATMENT_FIRM* is also positive and statistically significant at the 10% level. In economic terms, our regressions suggest that prior to the regulation a 1% increase in sales growth leads to a 0.3% increase in investment pre-regulation, and a 1.1 to 1.2% increase in investment post-regulation.

In Panel B, we examine whether the information in the audit choice leads to an increase in the operating performance of our treatment firms. We measure operating performance as earnings before extraordinary items (scaled by average assets in the pre-regulation period). To test our prediction, we modify equation 1 by changing the dependent variable to operating performance. Consistent with our prediction, we find that the coefficient for *POST_REG* is positive and statistically significant at the 1% level (coef. = 0.034; t-stat. = 8.80) in column 1. This coefficient suggests that our treatment firms increase their operating performance following the regulation by 12.2%, amounting to approximately £1,687 per year. Similarly, columns 2 and 3 show that the coefficient for *POST_REG* × *TREATMENT_FIRM* is also positive and statistically significant at the 5% level (or better), suggesting that the audit choice signal leads to an increase in operating performance. These results support our hypothesis that the information conveyed by the audit choice following the removal of the audit requirement reduces financing frictions and consequently leads to an improvement in firm performance.

For brevity, we tabulate only the difference-in-differences specification using firms obtaining voluntary audits as the control sample for the remainder of our analyses. Nevertheless, we find that our inferences from the remaining tests are robust to estimating regressions without any control sample and using firms obtaining mandatory audits as the control sample. In addition, we do not tabulate additional results using the cost of debt as the dependent variable given the measurement error inherent in our interest expense proxy.

### *5.7. Heterogeneity in the treatment effect*

We next examine whether the value of the information in the audit choice is (i) greater for firms that are ex ante financially constrained and (ii) weaker for firms that use other means to reduce information asymmetry with external financiers. We begin by examining the role of financing constraints. Since our main hypothesis is that information in the audit choice reduces financing frictions, we argue that the information in the audit choice should be more valuable for firms that are ex ante more financially constrained. To test this prediction, we augment equation 1 by including additional covariates based on the interaction between our proxy for financing constraints and the *TREATMENT_FIRM* and *POST_REG* indicator variables. We classify firms in the bottom tercile of the age distribution of our sample (before 2004) as relatively more financially constrained. The intuition for our proxy follows from the evidence in Hadlock and Pierce (2010), who show that firm size and age are the best predictors of financing constraints.[16]

Table 8, Panel A shows that the coefficient for *POST_REG* × *TREATMENT_FIRM* × *FIN_CONSTRAINED* is positive and statistically significant at the 10% level both when debt and investment are the dependent variables (coef.=0.016 and 0.007, t-stat.=1.74 and 1.76). The table also shows that the coefficient for *POST_REG* × *TREATMENT_FIRM* is positive and statistically significant at the 5% level in both regressions. These coefficients indicate that both

[16] We do not use firm size to partition firms because the audit exemptions granted via the regulation is based on firm size. And we do not directly use the financing constraints index developed by Hadlock and Pierce because their index is calibrated for the sample of U.S. Compustat firms and the index parameters are unlikely to apply for our sample of small, private U.K. firms. Nevertheless, we note that our results are robust to measuring financing constraints using annual tercile cutoffs of the Hadlock and Pierce (2010) index, the Kaplan and Zingales (1997) index and the Whited and Wu (2006) index.

groups of treatment firms increase their debt and investment following the regulation, consistent with financial constraints on these firms. However, the more financially constrained firms increase debt and investment by a significantly larger magnitude than the less constrained firms. Specifically, the coefficients suggest that the more (less) financially constrained treatment firms increase their debt and investment by 2.7 and 1.4 (1.1 and 0.7) percentage points, respectively following the regulatory change. In economic terms, these coefficients represent a £13,379 increase in debt and £20,533 increase in investment from its conditional mean, which corresponds to an 11% (20%) increase in debt (investment) for the average more-constrained treatment firm. The less-constrained firms, on the other hand, increase debt by £6,322 and investment by £8,213 from its conditional mean, which corresponds to a 5% (8%) increase in debt (investment).

Next, we examine whether firms that resort to other avenues to convey information about their future prospects during the mandatory audit regime receive lower benefits from the information in the audit choice. That is, to the extent firms use alternative means to reduce information asymmetry with external financiers, the firm's audit choice is less likely to provide incremental information to external financiers. As a result, the regulation should have a smaller impact on such firms. One potential mechanism through which private firms can reduce information asymmetry about their prospects is by hiring a high quality auditor, as these audits are typically more costly. Therefore, unless the firm is confident about its future prospects, it is unlikely to incur the additional costs necessary to hire a high quality auditor. We construct an indicator variable that equals one if the firm hires a Big 4 firm (*BIG4*). To test our prediction, we augment equation 1 by including additional covariates based on the interaction between *BIG4*, *TREATMENT_FIRM* and *POST_REG*.

Table 8, Panel B presents the results. We find that the coefficient for *POST_REG* × *TREATMENT_FIRM* × *BIG4* is negative in both the debt and investment regressions, but statistically significant only in the investment regression. The coefficient for *POST_REG* × *TREATMENT_FIRM* is positive and statistically significant at the 1% level in the debt and investment regressions. Our results in Panel B indicate that treatment firms increase their debt and investment following the regulation. However, those firms that hire a Big 4 auditor before the regulation increase investment by a significantly smaller magnitude than firms that did not do so. While the investment effect is consistent with our prediction, the debt result is not. We do not offer any ex post explanation for this result.

## 6. Additional analyses

### *6.1. Is the audit choice a proxy for growth opportunities?*

An important alternative interpretation for our results is that the audit choice is correlated with a firm's growth opportunities, and thus firms that choose to receive audits tend to increase investment and financing as a result of these growth opportunities. In other words, we find that our treatment firms increase debt and investment after the regulation, and we attribute this change to the observability of the audit choice, which relaxes financing constraints. However, a potential alternative hypothesis is that our treatment firms experience a shock to growth opportunities in 2004 that did not affect the control firms (despite our matching approach).

To address this concern, we conduct two additional tests. First, we directly test whether the treatment firms observe an increase in growth opportunities relative to the control firms in 2004. Again, our assumption is that the treatment firms have high growth opportunities pre-2004, but are unable to fund and exploit these opportunities because of financing constraints. Thus, our hypothesis is that the observed increase in debt and investment is the result of a relaxation in financing constraints and we do not expect to observe an increase in growth opportunities post-2004. We find (in an untabulated analysis) no evidence that treatment firms have an increase in growth opportunities in 2004. This finding supports our intuition and helps partially mitigate the concern that firms are simply responding to an increase in growth opportunities in 2004.

Second, we split our treatment firms into two groups: (A) those that eventually grow larger than the audit exemption threshold—£5.6 (£2.8) million sales (asset)—sometime after 2004 and thus move back to a mandatory audit regime, and (B) those that remain under the audit exemption threshold after 2004, i.e., remain in the voluntary audit regime. We then compare investment/financing for these two sets of firms. The intuition is that for both sets of treatment firms, the audit choice is unobservable pre-2004 and becomes observable post-2004. However, the audit choice for treatment firms in group (A) becomes unobservable again, as their assets/sales exceed the exemption thresholds. Thus, the information in the audit choice is no longer observed by external investors. If our inferences are correct, we should see a reduction in their investment/financing, as information in the audit choice is lost again. However, we should not observe such a reduction in investment/financing for treatment firms in group (B) since their audit choice continues to be observable. In contrast, if the audit choice is 'irrelevant,' or the audit choice is sorting firms based on their growth opportunities, there is no reason to expect the treatment firms in groups (A) and (B) to behave any differently after the group (A) firms reach the audit exemption threshold, particularly since all firms still get an audit but just differ on whether the audit is mandatory or not. Note that the group (A) treatment firms exceed the exemption thresholds faster than the group (B) treatment firms primarily because these firms are larger to begin with and thus closer to the threshold limits. Further, we match group (A) and group (B) firms on growth, profitability and industry in 2004 to ensure that differences in firm characteristics do not affect our results.

Table 9 presents the results from our analyses. Panel A presents the descriptive statistics for the group A and group B firms as of 2004. The table shows that these firms are statistically indistinguishable in terms of their investment opportunities (i.e., *SALES_GR*) and ROA, indicating that our matching procedure is effective. Further, the table also shows that both groups of firms have similar debt and investment levels in 2004. Finally, Table 9, Panel A shows that the group A firms that cross the sales/asset threshold limits to qualify for the audit exemption are significantly larger than the group B firms that do not cross these thresholds (during our sample period). Thus the data suggest that the primary reason why some of our treatment firms (i.e., group A firms) exceed the threshold limits to qualify for the audit exemption while others do not (i.e., group B firms) is because their initial size is closer to the thresholds.

Table 9, Panels B presents the regression results. Consistent with our expectation, we find that the subset of treatment firms that move back to a mandatory audit regime (i.e., group A firms) indeed see a reduction in their debt and an increase in their cost of debt once their audit choice again becomes unobservable compared to that of the sub-sample of treatment firms whose audit choice continues being observable (i.e., group B firms). The table also shows that there is a marginal reduction in investment, but the investment effect is significant only at the one-tailed 10% level.[17] Overall, these results help mitigate alternative explanations because we find that a firm's investment/financing behavior shifts in a predictable manner that is contingent on the *observability of the audit choice*.

*6.2. Did audit assurance or audit quality change after the regulation?*

Throughout our analyses thus far, we assume that the nature of the audit and the quality of audited reports do not change following the audit regime shift in 2004. If our assumption is misplaced and the transition from a mandatory to voluntary audit regime is associated with a change in the amount of time and effort devoted to audit a client's financial statements, then our results could be picking up the verification benefit of the audit in addition to (or rather than) the effect of the information in the audit choice. To examine the validity of our assumption that audit effort/quality does not change around the 2004 audit regime shift, we examine whether (i) audit fees (our proxy for audit effort) paid by client firms changes following the regime shift, and (ii)

[17] Note that since the post periods are firm specific, as different firms surpass the exemption thresholds in different years, the indicator variable for *POST* is not absorbed by year × industry fixed effects.

the mapping of accruals into future cash flows (our proxy for audit or earnings quality, following Minnis (2011)) changes following the regime shift.

Table 10 presents the results. Supporting our assumption, Table 10, Panel A shows that there is no significant change in the audit fees paid by our treatment firms following the 2004 regulation. Similarly, we find no evidence that the mapping of accruals into future cash flows changes following the 2004 regulation (Table 10, Panel B). These results help mitigate the concern that the audit regime shift is confounded by changes in audit effort and assurance. Notwithstanding the above test, we also note that any general change in audit effort in our main tests is likely to affect both treatment and control firms, and thus is filtered out by our difference-in-differences specification. Finally, we control for audit fees in all our regressions to further mitigate the effect of any changes in audit assurance on debt and investment.

### *6.3. The effect of firm size on debt and investment*

To qualify for the audit exemption, firms must fall within certain size thresholds. As a result, the treatment and control firms differ in terms of total assets and sales. Therefore, a potential concern with our tests is that differences in firm size between the treatment and control samples could be affecting our inferences. In our main tests, we address this concern by comparing our treatment firms to two sets of control firms – (i) voluntary audit control firms that are smaller than the treatment firms and (ii) mandatory audit control firms that are larger than the treatment firms. Since our treatment firms are sandwiched between two sets of control firms that are slightly larger in size (mandatory audit control group) and slightly smaller in size (voluntary audit control group) than the treatment sample, a monotonic relation between firm size and debt/investment cannot explain our findings.

To further mitigate the concern that differences in firm size affect our inferences, we conduct three additional tests. First, we verify the robustness of our inferences using a regression discontinuity design where the effect of the audit choice is identified only through the discontinuous change in audit regimes. So long as any omitted determinants of debt and investment (including factors related to size) do not exhibit a similar discontinuity at the time of the audit regime change, this analysis helps mitigate endogeneity concerns. Second, we interact firm size in 2004 with the indicator variables for each year to allow heterogeneous time trends for firms of different sizes in the base year. We find that our main results are unaffected by this set of additional controls. Finally, we devise a placebo test that compares the change in the investment behavior of large firms with that of small firms *within our mandatory control sample* following the audit regime shift. The intuition for this test is that if firm size explains the changes in investment behavior around the audit regime shift, then we should observe similar differences in investment and debt across the large and small firms in the mandatory control sample. Accordingly, we repeat our tests using the "small" mandatory controls firms as the treatment sample and the "larger" mandatory control firms as the control sample. In untabulated analyses, we find that the difference-in-difference coefficient is statistically insignificant, suggesting that there is no difference in investment changes or debt changes across the different size firms within the mandatory control sample. These results further support our contention that the regulatory size partition is unlikely to be the driver of our results.

### *6.4. Analyses of firms that opt out of receiving audits after 2004*

Finally, for completeness, we examine the effect of the regime shift on firms that choose to stop (i.e., opt out of) receiving audits after 2004. Similar to other economic decisions, the decision of whether to obtain an audit requires a cost-benefit analysis for each firm. As we discussed earlier, audits are costly in terms of time, money and managerial effort and these costs are disproportionately burdensome for small firms such as those in our sample. The primary benefit of an audit is to help verify the accuracy of financial statements, which provides a number of economic benefits including cheaper access to external finance (Blackwell et al., 1998; Minnis, 2011) and lower moral hazard costs (Jensen and Meckling, 1976). We predict that firms that choose to stop receiving an audit (presumably because the costs exceed the benefits) do not increase debt and investment, which is in contrast to the firms that choose to voluntarily receive audits. However, it is ex ante unclear whether firms opting out of the audit requirement would (i) *decrease* debt and/or investment or (ii) simply *not increase* debt and/or investment once they stop receiving audits. If these firms did not rely on external funds to finance investment in the pre-regulation regime (perhaps because they did not have high quality investment opportunities or because they were rationed from obtaining credit [Stiglitz and Weiss, 1981]), then we would observe no change in investment and debt in the post-regulation regime. However, if these firms were "pooled" with other firms that have better quality investment opportunities and thus gained access to low cost external finance, we would observe a decrease in debt and investment because the audit regime change creates a separating equilibrium.

We test the above predictions using equation 1. We re-match the opt-out firms to the two control samples used before as well as a third control sample. Specifically, our first two control samples are comprised of firms that obtain voluntary or mandatory audits before and after 2004 (similar to those used in our previous analyses). These firms differ from the opt-out firms in the post-2004 period because they receive audits while the opt-out firms do not receive audits. Our third control sample is comprised of firms that are exempt from the audit mandate even before 2004 and choose to opt out of the audit before *and* after 2004. These firms qualified for the audit exemption granted in 2000. This control sample is comprised of firms that are smaller than the opt-out sample (i.e., the treatment sample for our current analyses) and they differ from the opt-out firms in the pre-2004 period because they did not receive audits while the opt-out firms receive audits (due to the mandate). As before, we match the opt-out firms to these three control samples by sales growth, ROA, debt, liquidity, industry and year.

Table 11, Panel A (B) presents the results for our debt (investment) regression. We present four sets of results that correspond to using (i) a baseline specification without any control sample, (ii) firms obtaining voluntary audits before and after the regulation as the first control sample, (iii) firms obtaining mandatory audits before and after the regulation as the second control sample, and (iv) firms not obtaining an audit before nor after the regulation as the final control sample. Panel A presents the results when *DEBT* is the dependent variable. We find that the coefficient for *POST_REG* is negative and statistically significant at the 1% level in column 1. However, the coefficient for *POST_REG* × *OPT_OUT_FIRM* is statistically insignificant in the remaining three regressions with control samples. These results suggest that the opt-out firms decrease their debt levels after 2004, but this decrease is in line with the trends in the economy for observably similar firms not affected by the audit regime shift. Panel B shows that the coefficient for *POST_REG* (*POST_REG* × *OPT_OUT_FIRM*) in column 1 (columns 2 to 4) is statistically insignificant. These coefficients suggest that the opt-out firms do not change their investment after the audit regime shift in 2004.

## 7. Conclusion

In this paper, we examine whether the endogenous choice to obtain an audit conveys new information (independent to that conveyed in audited reports) that helps reduce financing frictions and information asymmetry. To identify the economic effects of the information in the audit choice, we use a difference-in-differences design that exploits the staggered relaxation of audit requirements for private firms in the U.K. Our natural experiment allows us to construct a sample of treatment firms whose audit choice goes from being unobservable pre-regulation to becoming observable post-regulation. Further, the staggered application of audit exemptions allows us to construct two matched control samples where all firms are audited, but the firms' audit choice is (i) observable both pre- and post-regulation, and (ii) unobservable both pre- and post-regulation. This setting enables us to empirically isolate the economic effects of the audit choice from that of the audited reports as well as control for confounding factors related to the choice to obtain an audit (e.g., changes in growth opportunities).

We find robust evidence that our treatment firms significantly increase their debt, investment, operating performance, and investment efficiency following the regulation. These economic effects are stronger for ex ante more financially constrained firms, and weaker for firms using other means reduce information asymmetry. Further, we find that the treatment firms that surpass the audit exemption thresholds and get pushed back into a mandatory audit regime witness a decrease in debt and increase their cost of debt once their audit choice becomes unobservable again. Combined, these results are consistent with the audit *choice* conveying information to capital providers (different from that conveyed by the audit itself), which reduces information asymmetry and financing frictions.

This paper contributes to the auditing literature by showing that, aside from the verification related benefits of an audit, observing a firm's decision to subject itself to the audit can be incrementally informative to external investors. This finding is important from a regulatory point of view because audits are mandatory for many firms across the world, and our evidence suggests that the audit mandate conceals the information in firms' audit choices.

Finally, a few caveats are in order. First, although our evidence supports the removal of an audit mandate, we cannot make regulatory recommendations without analyzing the cost of removing mandatory audits. Second, while our evidence points to a potential downside of regulation, this implication could be specific to the context of the audit mandate and does not necessarily apply to other regulation. In particular, regulation can be helpful in other cases, especially where there are externalities. For example, Bushee and Leuz (2005) show that disclosure regulation has positive externalities with respect to stock liquidity and Badertscher et al. (2013) and Shroff et al. (2014) show that disclosure regulation has positive externalities with respect of peer firms' investment decisions.

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**Figure 1**
Diagrammatic Representation of the Research Design

In the figure below, the x-axis represents time and the y-axis represents firm size. The dashed lines represent firms voluntarily obtaining a financial statement audit when the audit is not required by law and the solid line represents firms obtaining a financial statement audit when the audit is required by law. Our research design uses a difference-in-difference matching estimator where we match each treatment firm with at least one of two control firms. Our treatment sample comprises of firms that were required to obtain audits before 2004 but were exempt from this requirement after 2004. However, they continue receiving audits voluntarily after 2004. These firms are represented by the middle line in the diagram below. They have sales between £1 million and £5.6 million and assets between £1.4 million and £2.8 million. The first control sample includes firms that voluntarily obtain audits both before and after the regulatory change. These firms are represented at the bottom of the diagram. They have sales less than £1 million and assets less than £1.4 million prior to 2004 and obtain an audit (voluntarily) throughout our sample period. The second control sample includes firms required to obtain audits both before and after the regulatory change. This control sample is represented at the top of the diagram, and is comprised of firms with sales greater than £5.6 million and/or assets greater than £2.8 million during our sample period. Our sample period runs from 2001 to 2006, giving us three years before and after the regulatory change in March 2004.

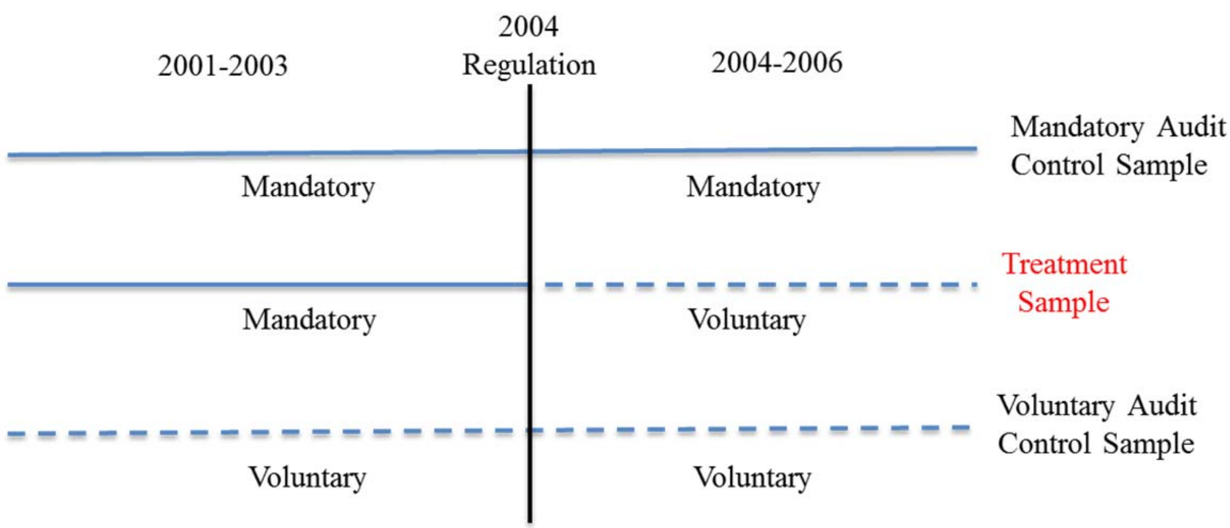
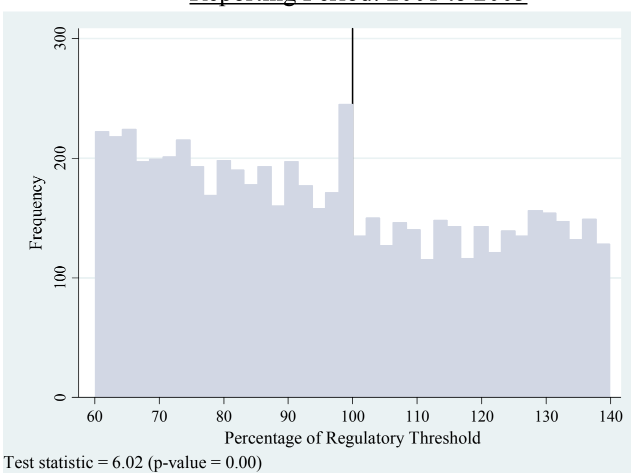


**Figure 2**
Distributional discontinuities in sales and assets near the pre-2004 regulatory threshold for audit exemptions

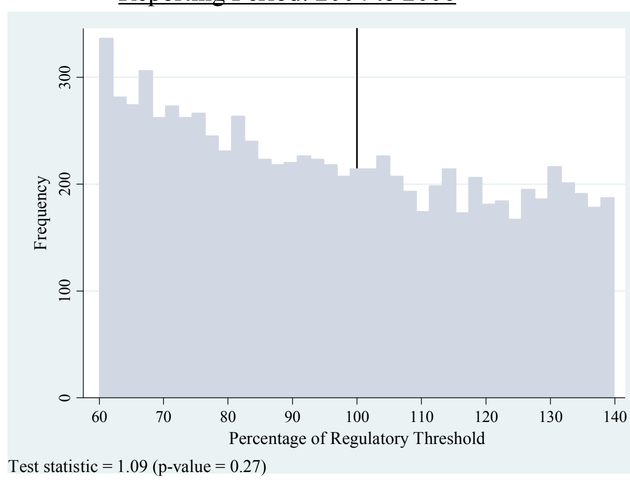
This figure presents the frequency distribution of sales and assets scaled by the pre-2004 regulatory threshold to be exempt from the audit requirement. The vertical axis labeled "Frequency" represents the number of observations in each sales or asset bin and the horizontal axis labeled "Percentage of Regulatory Threshold" is sales/assets range in each bin as a percentage of the respective regulatory threshold (£1 million for sales and £1.4 million for assets). The distribution interval widths are two percent of the threshold variable. We then plot the frequency of firms in each bin where bins range from 60 percent to 140 percent of the audit threshold. To illustrate how observations are aggregated in respective bins consider the following example. Suppose a firm reports sales of £950,000 for its fiscal year end in 2003. Its sales value is 5% below the audit threshold of £1 million. Thus, this firm-year observation would fall in the bin representing sales from 94 percent to 96 percent of the sales audit threshold. To test the significance of each discontinuity we use the standardized difference statistic from Burgstahler and Dichev (1997) as modified by Burgstahler and Chuk (2015). In particular, we calculate the test statistic as the difference between the actual number of observations in the bin and the expected number of observations in that bin, deflated by the estimated standard deviation of the difference. The expected number of observations in the bin is the average of the number of observations in the two immediately adjacent bins.

*Panel A: Distribution of sales*

Reporting Period: 2001 to 2003

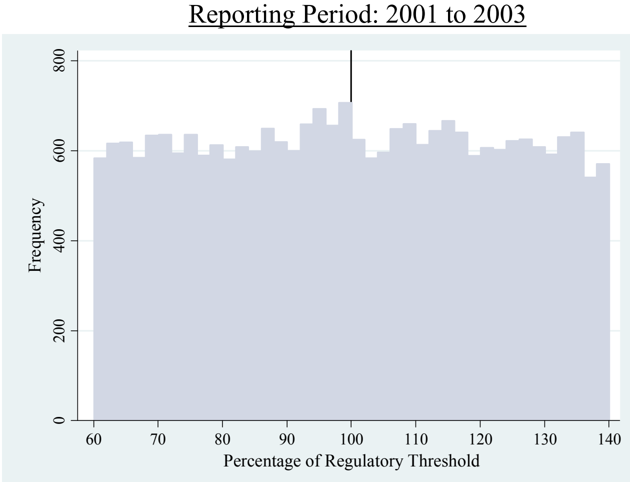



Reporting Period: 2004 to 2006




*Panel B: Distribution of assets*

Reporting Period: 2001 to 2003




Reporting Period: 2004 to 2006

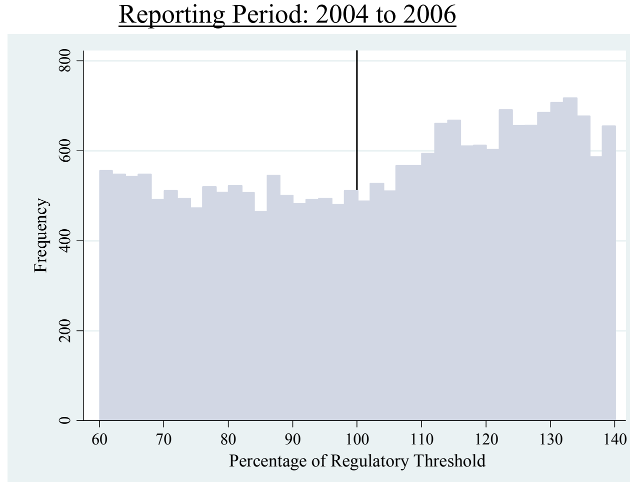


**Table 1**
Sample Selection

| Sample Selection (2001 - 2006) | Observations Dropped | Number of Observations |
|---|---|---|
| ***1) Sample selection when control sample comprises of firms obtaining voluntary audits both pre- and post-regulation*** | | |
| Firm-year observations in FAME meeting the following criteria: (i) £1 < sales < £5.6, (ii) £1.4 < total assets < £2.8 | | 21,666 |
| *Less:* Financial firms | 1,748 | 19,918 |
| *Less:* Firms with missing data on investment and control variables | 5,751 | 14,167 |
| *Less:* Firms without at least one observation in both periods (i.e., pre- and post-regulation) | 213 | 13,954 |
| *Less:* Firms with assets or sales less than £15,000 | 79 | 13,875 |
| Full treatment sample available for analyses | | 13,875 |
| *Less:* Firms with no matching control firm that obtains an audit and has sales < £1 and assets < £1.4 in the periods prior to January 2004 | 4,380 | 9,495 |
| Treatment sample plus matched control sample | | 18,990 |
| ***2) Sample selection when control sample comprises of firms obtaining mandatory audits both pre- and post-regulation*** | | |
| Firm-year observations in FAME meeting the following criteria: (i) £1 < sales < £5.6, (ii) £1.4 < total assets < £2.8 | | 21,666 |
| *Less:* Financial firms | 1,748 | 19,918 |
| *Less:* Firms with missing data on investment or control variables | 5,751 | 14,167 |
| *Less:* Firms without at least one observation in both periods (i.e., pre- and post-regulation) | 213 | 13,954 |
| *Less:* Firms with assets or sales less than £15,000 | 79 | 13,875 |
| Full treatment sample available for analyses | | 13,875 |
| *Less:* Firms with no matching control firm that obtains an audit and has sales > £5.6 and/or assets > £2.8 throughout the sample period | 1,797 | 12,078 |
| Treatment sample plus matched control sample | | 24,156 |

**Table 2**
Results from Matching Procedure

This table presents the descriptive statistics for our matching variables for our treatment and control samples before and after the regulatory change in March 30, 2004. Panel A compares the mean values of the matching variables for the treatment sample and our two control samples in the pre-regulation period by year, and Panel B presents the mean difference in *INVESTMENT* changes and *DEBT* changes between treatment and control firms in each of the pre-treatment years. The '***Voluntary Audit Control Sample***' comprises of firms with sales less than £1 million and assets less than £1.4 million prior to 2004 and they obtain an audit (voluntarily) throughout our sample period. The '***Mandatory Audit Control Sample***' includes firms required to obtain audits both before and after the regulatory change. This control sample is comprised of firms with sales greater than £5.6 million and/or assets greater than £2.8 million during our sample period. In the tables below, ***DEBT*** is the total debt outstanding scaled by total assets; ***SALES_GR*** is the percentage change in sales; ***ROA*** is return on assets computed as the income before extraordinary items scaled by total assets; ***LIQUIDITY*** is the ratio of current assets to current liabilities; ***INVESTMENT*** is the change in fixed assets scaled by lag total assets. In panel B, **Δ *DEBT*** is the change in *DEBT* and **Δ *INVESTMENT*** is the change in *INVESTMENT*.

*Panel A: Comparison of Treatment Sample with Matched Control Sample by Pre-Regulation Years*

| Matching Variables | Treatment Sample | Control Sample | Difference | *t*-Statistic | N | Year |
|---|---|---|---|---|---|---|
| **A. Using Firms Obtaining Voluntary Audits as Control Sample** | | | | | | |
| *DEBT* | 0.123 | 0.129 | -0.006 | -0.72 | 1,324 | 2001 |
| *SALES_GR* | 0.150 | 0.160 | -0.010 | -0.63 | 1,324 | 2001 |
| *ROA* | 0.194 | 0.188 | 0.006 | 0.42 | 1,324 | 2001 |
| *LIQUIDITY* | 2.013 | 2.058 | -0.046 | -0.52 | 1,324 | 2001 |
| *DEBT* | 0.120 | 0.125 | -0.005 | -0.71 | 1,768 | 2002 |
| *SALES_GR* | 0.117 | 0.110 | 0.008 | 0.67 | 1,768 | 2002 |
| *ROA* | 0.170 | 0.185 | -0.015 | -1.19 | 1,768 | 2002 |
| *LIQUIDITY* | 2.046 | 2.028 | 0.018 | 0.24 | 1,768 | 2002 |
| *DEBT* | 0.122 | 0.123 | -0.001 | -0.22 | 2,044 | 2003 |
| *SALES_GR* | 0.094 | 0.087 | 0.007 | 0.65 | 2,044 | 2003 |
| *ROA* | 0.176 | 0.177 | -0.001 | -0.09 | 2,044 | 2003 |
| *LIQUIDITY* | 2.109 | 2.064 | 0.045 | 0.62 | 2,044 | 2003 |
| **B. Using Firms Obtaining Mandatory Audits as Control Sample** | | | | | | |
| *DEBT* | 0.117 | 0.120 | -0.003 | -0.43 | 1,658 | 2001 |
| *SALES_GR* | 0.083 | 0.094 | -0.011 | -1.22 | 1,658 | 2001 |
| *ROA* | 0.164 | 0.165 | -0.001 | -0.40 | 1,658 | 2001 |
| *LIQUIDITY* | 1.862 | 1.927 | -0.065 | -1.00 | 1,658 | 2001 |
| *DEBT* | 0.112 | 0.116 | -0.003 | -0.58 | 2,275 | 2002 |
| *SALES_GR* | 0.053 | 0.062 | -0.009 | -1.18 | 2,275 | 2002 |
| *ROA* | 0.156 | 0.160 | -0.004 | -0.54 | 2,275 | 2002 |
| *LIQUIDITY* | 1.896 | 1.936 | -0.039 | -0.70 | 2,275 | 2002 |
| *DEBT* | 0.115 | 0.117 | -0.002 | -0.43 | 2,736 | 2003 |
| *SALES_GR* | 0.062 | 0.062 | 0.000 | 0.06 | 2,736 | 2003 |
| *ROA* | 0.159 | 0.156 | 0.003 | 0.40 | 2,736 | 2003 |
| *LIQUIDITY* | 1.994 | 2.032 | -0.038 | -0.70 | 2,736 | 2003 |

**Table 2 (continued)**

*Panel B: Parallel Trends in Investment and Debt in Pre-Regulation Years*

| Main Dependent Variables | Treatment Sample | Control Sample | Difference | *t*-Statistic | N | Year |
|---|---|---|---|---|---|---|
| ***A. Using Firms Obtaining Voluntary Audits as Control Sample*** | | | | | | |
| Δ *DEBT* | -0.002 | -0.004 | 0.002 | 0.33 | 1,108 | 2002 |
| Δ *INVESTMENT* | -0.008 | -0.007 | -0.001 | -0.27 | 1,108 | 2002 |
| Δ *DEBT* | -0.001 | -0.001 | 0.000 | -0.03 | 1,580 | 2003 |
| Δ *INVESTMENT* | -0.004 | -0.005 | 0.002 | 0.58 | 1,580 | 2003 |
| ***B. Using Firms Obtaining Mandatory Audits as Control Sample*** | | | | | | |
| Δ *DEBT* | -0.004 | -0.003 | -0.001 | -0.47 | 1,498 | 2002 |
| Δ *INVESTMENT* | -0.007 | -0.005 | -0.002 | -0.62 | 1,498 | 2002 |
| Δ *DEBT* | -0.003 | -0.006 | 0.004 | 0.98 | 2,220 | 2003 |
| Δ *INVESTMENT* | -0.002 | -0.004 | 0.002 | 0.66 | 2,220 | 2003 |

**Table 3**
Descriptive Statistics for the Treatment and Control Samples

Panel A (B) presents the descriptive statistics for all our variables of interest for our treatment sample and matched voluntary (mandatory) audit control sample. The '***Voluntary Audit Control Sample***' comprises of firms with sales less than £1 million and assets less than £1.4 million prior to 2004 and they obtain an audit voluntarily throughout our sample period. The '***Mandatory Audit Control Sample***' includes firms required to obtain audits both before and after the regulatory change. This control sample is comprised of firms with sales greater than £5.6 million and/or assets greater than £2.8 million during our sample period. In the tables below, ***DEBT*** is the total debt outstanding scaled by total assets; ***SALES_GR*** is the percentage change in sales; ***ROA*** is return on assets computed as the income before extraordinary items scaled by total assets; ***LIQUIDITY*** is the ratio of current assets to current liabilities; ***INVESTMENT*** is the change in fixed assets scaled by lag total assets; ***TOTAL_ASSETS*** is the total assets of the firm in thousands of pounds; ***LSIZE*** is the natural log of total assets; ***SALES*** is the total sales of the firm in thousands of pounds; ***ACC_PAYABLE*** is the firm's accounts payable scaled by total assets; ***AGE*** is the natural log of the firm's age; ***LAUDIT_FEE*** is the natural log of audit fees incurred by the firm; ***BIG4*** is an indicator variable that equals one for firms using one of the big four audit firms.

**Table 3 (continued)**

*Panel A: Descriptive Statistics of the Treatment Sample Compared to the Voluntary Audit Control Sample*

| Variables | Mean | SD | P25 | P50 | P75 | Mean | SD | P25 | P50 | P75 | N |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | ***Treatment Sample (2001 - 2003)*** | | | | | ***Voluntary Audit Control Sample (2001 - 2003)*** | | | |
| *DEBT* | 0.122 | 0.208 | 0.000 | 0.003 | 0.156 | 0.125 | 0.218 | 0.000 | 0.000 | 0.182 | 5,136 |
| *INVESTMENT* | 0.041 | 0.071 | 0.000 | 0.013 | 0.050 | 0.039 | 0.084 | 0.000 | 0.003 | 0.038 | 5,136 |
| *TOTAL_ASSETS* | 804 | 486 | 466 | 685 | 1,021 | 673 | 401 | 374 | 651 | 981 | 5,136 |
| *LSIZE* | 6.504 | 0.645 | 6.144 | 6.529 | 6.929 | 6.143 | 1.135 | 5.923 | 6.479 | 6.889 | 5,136 |
| *SALES* | 1,691 | 950 | 1,116 | 1,501 | 2,137 | 437 | 334 | 106 | 393 | 728 | 5,136 |
| *ACC_PAYABLE* | 0.386 | 0.590 | 0.079 | 0.239 | 0.483 | 0.355 | 0.636 | 0.019 | 0.142 | 0.448 | 5,136 |
| *SALES_GR* | 0.116 | 0.415 | -0.069 | 0.052 | 0.203 | 0.114 | 0.424 | -0.120 | 0.022 | 0.188 | 5,136 |
| *ROA* | 0.179 | 0.310 | 0.032 | 0.119 | 0.265 | 0.183 | 0.397 | 0.019 | 0.088 | 0.219 | 5,136 |
| *LIQUIDITY* | 2.062 | 2.166 | 1.000 | 1.345 | 2.140 | 2.050 | 2.368 | 0.676 | 1.216 | 2.365 | 5,136 |
| *AGE* | 8.415 | 0.789 | 7.826 | 8.423 | 8.930 | 8.519 | 0.842 | 7.870 | 8.520 | 9.124 | 5,136 |
| *LAUDIT_FEE* | 1.684 | 0.558 | 1.386 | 1.609 | 2.079 | 1.287 | 0.606 | 0.693 | 1.238 | 1.609 | 5,136 |
| *BIG4* | 0.088 | 0.285 | 0.000 | 0.000 | 0.000 | 0.058 | 0.234 | 0.000 | 0.000 | 0.000 | 5,136 |
| | | | ***Treatment Sample (2004 - 2006)*** | | | | | ***Voluntary Audit Control Sample (2004 - 2006)*** | | | |
| *DEBT* | 0.129 | 0.216 | 0.000 | 0.005 | 0.170 | 0.121 | 0.218 | 0.000 | 0.000 | 0.138 | 4,359 |
| *INVESTMENT* | 0.043 | 0.070 | 0.000 | 0.014 | 0.051 | 0.036 | 0.082 | 0.000 | 0.002 | 0.029 | 4,359 |
| *TOTAL_ASSETS* | 1,013 | 578 | 573 | 900 | 1,359 | 750 | 612 | 293 | 631 | 1,065 | 4,359 |
| *LSIZE* | 6.737 | 0.657 | 6.351 | 6.802 | 7.215 | 6.132 | 1.226 | 5.680 | 6.448 | 6.971 | 4,359 |
| *SALES* | 1,983 | 1,180 | 1,182 | 1,790 | 2,625 | 546 | 657 | 104 | 377 | 777 | 4,359 |
| *ACC_PAYABLE* | 0.370 | 0.550 | 0.077 | 0.238 | 0.456 | 0.350 | 0.687 | 0.011 | 0.111 | 0.389 | 4,359 |
| *SALES_GR* | 0.082 | 0.329 | -0.062 | 0.047 | 0.179 | 0.125 | 0.425 | -0.088 | 0.036 | 0.216 | 4,359 |
| *ROA* | 0.178 | 0.298 | 0.035 | 0.121 | 0.257 | 0.195 | 0.409 | 0.021 | 0.088 | 0.233 | 4,359 |
| *LIQUIDITY* | 2.145 | 2.180 | 1.030 | 1.435 | 2.258 | 2.411 | 2.710 | 0.742 | 1.351 | 2.905 | 4,359 |
| *AGE* | 8.677 | 0.688 | 8.154 | 8.675 | 9.118 | 8.715 | 0.705 | 8.148 | 8.677 | 9.207 | 4,359 |
| *LAUDIT_FEE* | 1.760 | 0.575 | 1.386 | 1.792 | 2.122 | 1.369 | 0.635 | 0.898 | 1.386 | 1.792 | 4,359 |
| *BIG4* | 0.084 | 0.279 | 0.000 | 0.000 | 0.000 | 0.067 | 0.249 | 0.000 | 0.000 | 0.000 | 4,359 |

**Table 3 (continued)**

*Panel B: Descriptive Statistics of the Treatment Sample Compared to the Mandatory Audit Control Sample*

| Variables | Mean | SD | P25 | P50 | P75 | Mean | SD | P25 | P50 | P75 | N |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | ***Treatment Sample (2001 - 2003)*** | | | | | ***Mandatory Audit Control Sample (2001 - 2003)*** | | | | | |
| *DEBT* | 0.115 | 0.196 | 0.000 | 0.005 | 0.152 | 0.117 | 0.192 | 0.000 | 0.012 | 0.161 | 6,669 |
| *INVESTMENT* | 0.041 | 0.072 | 0.000 | 0.013 | 0.050 | 0.044 | 0.074 | 0.002 | 0.017 | 0.051 | 6,669 |
| *TOTAL_ASSETS* | 986 | 566 | 567 | 885 | 1,273 | 4,832 | 4,198 | 2,185 | 3,469 | 6,025 | 6,669 |
| *LSIZE* | 6.714 | 0.644 | 6.340 | 6.786 | 7.149 | 8.221 | 0.709 | 7.690 | 8.152 | 8.704 | 6,669 |
| *SALES* | 1,726 | 1,075 | 1,060 | 1,542 | 2,313 | 6,386 | 4,774 | 2,940 | 5,221 | 8,705 | 6,669 |
| *ACC_PAYABLE* | 0.352 | 0.405 | 0.081 | 0.244 | 0.481 | 0.301 | 0.328 | 0.080 | 0.209 | 0.414 | 6,669 |
| *SALES_GR* | 0.064 | 0.260 | -0.069 | 0.044 | 0.174 | 0.070 | 0.260 | -0.065 | 0.050 | 0.180 | 6,669 |
| *ROA* | 0.159 | 0.256 | 0.036 | 0.110 | 0.239 | 0.160 | 0.243 | 0.047 | 0.116 | 0.225 | 6,669 |
| *LIQUIDITY* | 1.928 | 1.984 | 0.977 | 1.325 | 2.068 | 1.973 | 1.899 | 1.010 | 1.374 | 2.224 | 6,669 |
| *AGE* | 8.513 | 0.797 | 7.929 | 8.512 | 9.051 | 8.674 | 0.811 | 8.124 | 8.678 | 9.239 | 6,669 |
| *LAUDIT_FEE* | 1.654 | 0.562 | 1.386 | 1.609 | 1.946 | 2.267 | 0.664 | 1.792 | 2.303 | 2.708 | 6,669 |
| *BIG4* | 0.086 | 0.282 | 0.000 | 0.000 | 0.000 | 0.156 | 0.364 | 0.000 | 0.000 | 0.000 | 6,669 |
| | ***Treatment Sample (2004 - 2006)*** | | | | | ***Mandatory Audit Control Sample (2004 - 2006)*** | | | | | |
| *DEBT* | 0.126 | 0.209 | 0.000 | 0.006 | 0.172 | 0.118 | 0.190 | 0.000 | 0.011 | 0.170 | 5,493 |
| *INVESTMENT* | 0.043 | 0.071 | 0.000 | 0.013 | 0.051 | 0.039 | 0.070 | 0.002 | 0.014 | 0.045 | 5,493 |
| *TOTAL_ASSETS* | 1,119 | 610 | 637 | 1,029 | 1,490 | 6,332 | 3,759 | 3,508 | 5,128 | 8,101 | 5,493 |
| *LSIZE* | 6.846 | 0.642 | 6.457 | 6.936 | 7.307 | 8.594 | 0.561 | 8.163 | 8.542 | 9.000 | 5,493 |
| *SALES* | 1,974 | 1,237 | 1,128 | 1,799 | 2,713 | 8,125 | 4,485 | 4,747 | 7,605 | 11,197 | 5,493 |
| *ACC_PAYABLE* | 0.334 | 0.397 | 0.071 | 0.231 | 0.440 | 0.286 | 0.322 | 0.071 | 0.190 | 0.396 | 5,493 |
| *SALES_GR* | 0.094 | 0.330 | -0.048 | 0.050 | 0.182 | 0.085 | 0.287 | -0.047 | 0.052 | 0.165 | 5,493 |
| *ROA* | 0.172 | 0.285 | 0.034 | 0.113 | 0.245 | 0.145 | 0.217 | 0.046 | 0.107 | 0.198 | 5,493 |
| *LIQUIDITY* | 2.113 | 2.134 | 1.024 | 1.438 | 2.258 | 2.115 | 2.039 | 1.042 | 1.433 | 2.375 | 5,493 |
| *AGE* | 8.712 | 0.694 | 8.204 | 8.692 | 9.174 | 8.878 | 0.700 | 8.379 | 8.840 | 9.366 | 5,493 |
| *LAUDIT_FEE* | 1.723 | 0.565 | 1.386 | 1.758 | 2.079 | 2.445 | 0.650 | 2.056 | 2.398 | 2.862 | 5,493 |
| *BIG4* | 0.080 | 0.272 | 0.000 | 0.000 | 0.000 | 0.163 | 0.370 | 0.000 | 0.000 | 0.000 | 5,493 |

**Table 4**
Classification Errors from an Audit Prediction Model

Panel A presents the results from a logistic regression where the dependent variable is an indicator variable that equals one for firms that obtained mandatory audits before March 30, 2004 (i.e., before the regulation) and choose to continue receiving audits (voluntarily) after the regulation. Panel B presents the type I and type II classification errors when we use the model in Panel A to predict whether a firm is likely to obtain a voluntary audit after March, 2004. The classification errors are presented using different values as cut-offs along the distribution of the predicted audit probability from the Panel A regression. In the table below ***INVESTMENT_GR*** is the percentage change in investment; ***DEBT_GR*** is the percentage change in debt; ***ROA_GR*** is the percentage change in return on assets; ***SALES_GR*** is the percentage change in sales; ***ROA*** is return on assets computed as the income before extraordinary items scaled by total assets; ***LIQUIDITY*** is the ratio of current assets to current liabilities; ***DEBT*** is the total debt outstanding scaled by total assets; ***LSIZE*** is the natural log of total assets; ***N_DIRECTORS*** is the number of directors on the company's board; ***LAUDIT_FEE*** is the natural log of audit fees incurred by the firm; ***BIG4*** is an indicator variable that equals one for firms using one of the big four audit firms. *,**,*** indicate statistical significance at the 10%, 5%, and 1% level, respectively, using a two-tailed *t*-test.

*Panel A: Logistic Regression Predicting the Probability of Obtaining an Audit Voluntarily after 2004*

| **Dependent Variable:** | ***Indicator Variable for Firms that Obtain a Voluntary Audit After 2004*** | |
|---|---|---|
| | Coefficient | *z*-Statistic |
| *INVESTMENT_GR* | 1.062 ** | 2.24 |
| *DEBT_GR* | 0.579 ** | 2.38 |
| *ROA_GR* | 0.001 | 0.54 |
| *SALES_GR* | -0.183 | -1.26 |
| *ROA* | -0.079 | -0.66 |
| *LIQUIDITY* | 0.019 | 0.96 |
| *LSIZE* | 0.342 *** | 5.82 |
| *N_DIRECTORS* | 0.336 *** | 11.63 |
| *LAUDIT_FEE* | 0.726 *** | 10.39 |
| *BIG4* | 1.677 *** | 6.25 |
| *INTERCEPT* | -3.757 *** | -8.88 |
| Pseudo R-Squared | 10.0% | |
| No. of Observations | 3,977 | |

*Panel B: Classification Errors of the Logistic Model*

| **Distribution of Audit Probabilities (used as Cut-Offs)** | | **Probability of a Type I Error (i.e., False Positive) if Firms above Cut-Off are Classified as Obtaining an Audit** | **Probability of a Type II Error (i.e., False Negative) if Firms above Cut-Off are Classified as Obtaining an Audit** |
|---|---|---|---|
| *5th Percentile* | 0.450 | 89.0% | 2.8% |
| *25th Percentile* | 0.620 | 55.2% | 17.2% |
| *Median/Mean* | 0.730 | 27.6% | 42.5% |
| *75th Percentile* | 0.830 | 8.6% | 68.4% |
| *95th Percentile* | 0.950 | 1.2% | 93.4% |
| *N* | 3,977 | 1,117 | 2,860 |

**Table 5**
Effect of the Information in the Audit Choice on Debt, Cost of Debt, and Investment

Panels A, B, and C present the results from regressing debt, cost of debt, and investment, respectively, on indicator variables for the post-regulation period, treatment firm, an interaction between the two indicators, and control variables. The '***Voluntary Audit Control Sample***' comprises of firms with sales less than £1 million and assets less than £1.4 million prior to 2004 and they obtain an audit (voluntarily) throughout our sample period. The '***Mandatory Audit Control Sample***' includes firms required to obtain audits both before and after the regulatory change. This control sample is comprised of firms with sales greater than £5.6 million and/or assets greater than £2.8 million during our sample period. The dependent variables are measured as follows: ***DEBT (INVESTMENT)*** is the total debt (change in fixed assets) scaled by lag total assets, and ***INTEREST_EXP*** is the interest expense scaled by lagged total debt conditional on the firm financing at least 1% of its assets with debt. The independent variables are defined as follows: ***POST_REG*** is an indicator variable that equals one for fiscal years ending after March 30, 2004; ***TREATMENT_FIRM*** is an indicator variable that equals one for our treatment firms (i.e., firms that obtain an audit throughout our sample period and have sales between £1 million and £5.6 million and assets between £1.4 million and £2.8 million); ***SALES_GR*** is the percentage change in sales; ***LTANGIBLE_ASSETS*** is the natural log of tangible assets; ***LSIZE*** is the natural log of total assets; ***ROA*** is return on assets computed as the income before extraordinary items scaled by total assets; ***LIQUIDITY*** is the ratio of current assets to current liabilities; ***LAUDIT_FEE*** is the natural log of audit fees incurred by the firm scaled by total assets. The *t*-statistics are clustered at the firm-level to control for residual correlation in firms' investment. *,**,*** indicate statistical significance at the 10%, 5%, and 1% level, respectively, using a two-tailed *t*-test.

*Panel A: Debt Level Regressions*

| **Dependent Variable:** | | ***DEBT*** | | | | | |
|---|---|---|---|---|---|---|---|
| **Control Sample:** | | ***No Control Sample*** | | ***Voluntary Audit Control Sample*** | | ***Mandatory Audit Control Sample*** | |
| | Pr. Sign | Coefficient | *t*-Statistic | Coefficient | *t*-Statistic | Coefficient | *t*-Statistic |
| *POST_REG* | + | 0.009 *** | 4.37 | | | | |
| *POST_REG × TREATMENT_FIRM* | + | | | 0.016 *** | 3.67 | 0.010 *** | 2.83 |
| *SALES_GR* | | -0.004 * | -1.88 | -0.008 | -1.24 | -0.005 | -1.59 |
| *LTANGIBLE_ASSETS* | | 0.001 | 0.59 | 0.004 ** | 2.34 | 0.003 ** | 2.05 |
| *ROA* | | -0.036 *** | -5.53 | -0.031 *** | -6.73 | -0.040 *** | -6.74 |
| *LIQUIDITY* | | -0.001 | -0.92 | 0.000 | -0.48 | 0.000 | -0.19 |
| *LAUDIT_FEE* | | -0.004 | -0.67 | 0.001 | 0.26 | 0.002 | 0.40 |
| *Year × Industry Indicators* | | Not Included | | Included | | Included | |
| *Firm Indicators* | | Included | | Included | | Included | |
| R-Squared | | 6.3% | | 7.2% | | 7.5% | |
| No. of Observations | | 13,875 | | 18,990 | | 24,156 | |

**Table 5 (continued)**

*Panel B: Cost of Debt Regressions*

| Dependent Variable: | | *INTEREST_EXP* | | | | | |
|---|---|---|---|---|---|---|---|
| Control Sample: | | *No Control Sample* | | *Voluntary Audit Control Sample* | | *Mandatory Audit Control Sample* | |
| | Pr. Sign | Coefficient | *t*-Statistic | Coefficient | *t*-Statistic | Coefficient | *t*-Statistic |
| *POST_REG* | - | -0.005 *** | -3.18 | | | | |
| *POST_REG × TREATMENT_FIRM* | - | | | -0.008 *** | -2.89 | -0.003 * | -1.67 |
| *SALES_GR* | | -0.003 ** | -2.07 | -0.002 | -1.41 | -0.001 | -0.50 |
| *LTANGIBLE_ASSETS* | | -0.003 ** | -2.32 | -0.002 * | -1.64 | -0.002 * | -1.90 |
| *ROA* | | 0.005 | 1.16 | 0.006 | 1.60 | 0.009 ** | 2.02 |
| *LIQUIDITY* | | -0.003 *** | -4.14 | -0.002 *** | -3.63 | -0.003 *** | -4.84 |
| *LAUDIT_FEE* | | 0.004 | 1.10 | 0.002 | 0.74 | 0.004 | 1.43 |
| *Year × Industry Indicators* | | Not Included | | Included | | Included | |
| *Firm Indicators* | | Included | | Included | | Included | |
| R-Squared | | 1.0% | | 1.4% | | 1.2% | |
| No. of Observations | | 6,602 | | 8,392 | | 11,976 | |

*Panel C: Investment Regressions*

| Dependent Variable: | | *INVESTMENT* | | | | | |
|---|---|---|---|---|---|---|---|
| Control Sample: | | *No Control Sample* | | *Voluntary Audit Control Sample* | | *Mandatory Audit Control Sample* | |
| | Pr. Sign | Coefficient | *t*-Statistic | Coefficient | *t*-Statistic | Coefficient | *t*-Statistic |
| *POST_REG* | + | 0.002 *** | 2.68 | | | | |
| *POST_REG × TREATMENT_FIRM* | + | | | 0.009 *** | 4.09 | 0.005 *** | 2.97 |
| *SALES_GR* | | 0.005 *** | 4.27 | 0.003 ** | 2.21 | 0.004 *** | 3.36 |
| *LSIZE* | | -0.030 *** | -13.65 | -0.024 *** | -12.55 | -0.035 *** | -18.59 |
| *ROA* | | 0.012 *** | 3.24 | 0.010 *** | 3.16 | 0.010 *** | 3.06 |
| *LIQUIDITY* | | 0.003 *** | 5.30 | 0.003 *** | 4.92 | 0.003 *** | 6.02 |
| *LAUDIT_FEE* | | 0.004 | 1.60 | 0.014 *** | 4.94 | 0.008 *** | 4.03 |
| *Year × Industry Indicators* | | Not Included | | Included | | Included | |
| *Firm Indicators* | | Included | | Included | | Included | |
| R-Squared | | 4.6% | | 6.2% | | 6.6% | |
| No. of Observations | | 13,875 | | 18,990 | | 24,156 | |

**Table 6**
Dynamic Effect of the Information in the Audit Choice

This table presents the results from regressing firm debt, cost of debt, and investment on indicator variables for the year immediately before the regulation and each of the three years following the enactment of the regulation, an indicator for treatment firm, interactions between these variables and control variables. The dependent variables are measured as follows: ***DEBT (INVESTMENT)*** is the total debt (change in fixed assets) scaled by lag total assets and ***INTEREST_EXP*** is the interest expense scaled by lagged total debt conditional on the firm financing at least 1% of its assets with debt. The independent variables are defined as follows: ***POST_REG [-1]*** is an indicator variable that equals one for fiscal years ending between March 30, 2003 and March 30, 2004; ***POST_REG [0]*** is an indicator variable that equals one for fiscal years ending between March 30, 2004 and March 30, 2005; ***POST_REG [1]*** is an indicator variable that equals one for fiscal years ending between March 30, 2005 and March 30, 2006; ***POST_REG [2]*** is an indicator variable that equals one for fiscal years ending between March 30, 2006 and December 31, 2006; ***TREATMENT_FIRM*** is an indicator variable that equals one for our treatment firms (i.e., firms that obtain an audit throughout our sample period and have sales between £1 million and £5.6 million and assets between £1.4 million and £2.8 million); ***SALES_GR*** is the percentage change in sales; ***LSIZE*** is the natural log of total assets; ***LTANGIBLE_ASSETS*** is the natural log of tangible assets; ***ROA*** is return on assets computed as the income before extraordinary items scaled by total assets; ***LIQUIDITY*** is the ratio of current assets to current liabilities; ***LAUDIT_FEE*** is the natural log of audit fees incurred by the firm scaled by total assets. The control sample in the regressions below is the *Voluntary Audit Control Sample*, which is comprised of firms with sales less than £1 million and assets less than £1.4 million prior to 2004 that voluntarily obtain an audit throughout our sample period. The *t*-statistics are clustered at the firm-level to control for residual correlation in firms' investment. *,**,*** indicate statistical significance at the 10%, 5%, and 1% level, respectively, using a two-tailed *t*-test.

| **Dependent Variable:** | | ***DEBT*** | | ***INTEREST_EXP*** | | ***INVESTMENT*** | |
|---|---|---|---|---|---|---|---|
| | Pr. Sign | Coefficient | *t*-Statistic | Coefficient | *t*-Statistic | Coefficient | *t*-Statistic |
| *POST_REG [-1] × TREATMENT_FIRM* | | 0.005 | 1.16 | 0.000 | -0.05 | 0.001 | 0.42 |
| *POST_REG [0] × TREATMENT_FIRM* | +, - ,+ | 0.019 *** | 3.78 | -0.005 | -1.55 | 0.010 *** | 3.19 |
| *POST_REG [1] × TREATMENT_FIRM* | +, - ,+ | 0.019 *** | 3.09 | -0.008 ** | -2.12 | 0.008 *** | 2.60 |
| *POST_REG [2] × TREATMENT_FIRM* | +, - ,+ | 0.024 *** | 2.76 | -0.015 *** | -2.68 | 0.009 ** | 2.35 |
| *SALES_GR* | | -0.008 | -1.22 | -0.002 | -1.44 | 0.003 ** | 2.20 |
| *LTANGIBLE_ASSETS / LSIZE* | | 0.004 ** | 2.31 | -0.002 * | -1.65 | -0.024 *** | -12.52 |
| *ROA* | | -0.031 *** | -6.78 | 0.006 | 1.63 | 0.010 *** | 3.15 |
| *LIQUIDITY* | | 0.000 | -0.47 | -0.002 *** | -3.63 | 0.003 *** | 4.92 |
| *LAUDIT_FEE* | | 0.001 | 0.21 | 0.002 | 0.67 | 0.014 *** | 4.94 |
| *Year × Industry Indicators* | | Included | | Included | | Included | |
| *Firm Indicators* | | Included | | Included | | Included | |
| R-Squared | | 7.2% | | 7.7% | | 6.2% | |
| No. of Observations | | 18,990 | | 8,392 | | 18,990 | |

**Table 7**
Effect of the Audit Choice on Investment Efficiency and Firm Performance

Panel A (B) in this table presents the results from regressing firm investment (operating performance) on indicator variables for the post-regulation period, treatment firm, sales growth, interaction terms between these three variables and control variables. The '***Voluntary Audit Control Sample***' is comprised of firms with sales less than £1 million and assets less than £1.4 million prior to 2004 and they obtain an audit (voluntarily) throughout our sample period. The '***Mandatory Audit Control Sample***' includes firms required to obtain audits both before and after the regulatory change. This control sample is comprised of firms with sales greater than £5.6 million and/or assets greater than £2.8 million during our sample period. The dependent variable in Panel A, ***INVESTMENT***, is measured as the change in fixed assets scaled by lag total assets. The dependent variable in Panel B, ***OPERATING PERFORMANCE***, is measured as net income before extraordinary items scaled by average total assets in the pre-regulation period. The independent variables are defined as follows: ***POST_REG*** is an indicator variable that equals one for fiscal years ending after March 30, 2004; ***TREATMENT_FIRM*** is an indicator variable that equals one for our treatment firms (i.e., firms that obtain an audit throughout our sample period and have sales between £1 million and £5.6 million and assets between £1.4 million and £2.8 million); ***SALES_GR*** is the percentage change in sales; ***LSIZE*** is the natural log of total assets; ***ROA*** is return on assets computed as the income before extraordinary items scaled by total assets; ***LIQUIDITY*** is the ratio of current assets to current liabilities; ***LAUDIT_FEE*** is the natural log of audit fees incurred by the firm scaled by total assets. The *t*-statistics are clustered at the firm-level to control for residual correlation in firms' investment. *,**,*** indicate statistical significance at the 10%, 5%, and 1% level, respectively, using a two-tailed *t*-test.

*Panel A: Sensitivity of Investment to Investment Opportunities*

| **Dependent Variable:** | | ***INVESTMENT*** | | | | | |
|---|---|---|---|---|---|---|---|
| **Control Sample:** | | ***No Control Sample*** | | ***Voluntary Audit Control Sample*** | | ***Mandatory Audit Control Sample*** | |
| | Pr. Sign | Coef. | *t*-Stat. | Coef. | *t*-Stat. | Coef. | *t*-Stat. |
| *POST_REG × TREATMENT_FIRM* | | | | 0.008 *** | 3.55 | 0.005 *** | 2.69 |
| *SALES_GR × TREATMENT_FIRM* | | | | -0.001 | -0.45 | 0.003 | 1.06 |
| *SALES_GR × POST_REG* | + | 0.008 *** | 2.99 | 0.000 | 0.11 | 0.001 | 0.32 |
| *SALES_GR × POST_REG × TREATMENT_FIRM* | + | | | 0.009 * | 1.72 | 0.008 * | 1.79 |
| *SALES_GR* | | 0.003 ** | 2.25 | 0.003 * | 1.65 | 0.003 * | 1.67 |
| *LSIZE* | | -0.031 *** | -13.25 | -0.024 *** | -12.49 | -0.036 *** | -18.47 |
| *ROA* | | 0.012 *** | 3.18 | 0.010 *** | 3.12 | 0.010 *** | 3.01 |
| *LIQUIDITY* | | 0.004 *** | 5.68 | 0.003 *** | 4.99 | 0.003 *** | 6.09 |
| *LAUDIT_FEE* | | 0.004 * | 1.74 | 0.014 *** | 4.88 | 0.008 *** | 4.04 |
| *Year × Industry Indicators* | | Included | | Included | | Included | |
| *Firm Indicators* | | Included | | Included | | Included | |
| R-Squared | | 7.5% | | 6.2% | | 6.7% | |
| No. of Observations | | 13,875 | | 18,990 | | 24,156 | |

**Table 7 (continued)**

*Panel B: Operating Performance Following the Regulation*

| **Dependent Variable:** | | ***OPERATING PERFORMANCE*** | | | | | |
|---|---|---|---|---|---|---|---|
| **Control Sample:** | | ***No Control Sample*** | | ***Voluntary Audit Control Sample*** | | ***Mandatory Audit Control Sample*** | |
| | Pr. Sign | Coefficient | *t*-Statistic | Coefficient | *t*-Statistic | Coefficient | *t*-Statistic |
| *POST_REG* | + | 0.034 *** | 8.80 | | | | |
| *POST_REG × TREATMENT_FIRM* | + | | | 0.017 ** | 2.01 | 0.022 *** | 3.96 |
| *SALES_GR* | | -0.002 | -0.41 | 0.048 *** | 3.68 | 0.008 * | 1.68 |
| *LSIZE* | | 0.017 * | 1.77 | 0.017 * | 1.89 | 0.020 ** | 2.51 |
| *LIQUIDITY* | | -0.011 *** | -4.61 | -0.013 *** | -6.06 | -0.010 *** | -5.96 |
| *LAUDIT_FEE* | | 0.004 | 0.39 | -0.005 | -0.52 | 0.003 | 0.41 |
| *Year × Industry Indicators* | | Not Included | | Included | | Included | |
| *Firm Indicators* | | Included | | Included | | Included | |
| R-Squared | | 3.3% | | 5.5% | | 4.6% | |
| No. of Observations | | 13,875 | | 18,990 | | 24,156 | |

**Table 8**
Heterogeneous Treatment Effects of the Information in the Audit Choice

This table presents the results from regressing firm debt and investment on indicator variables for the post-regulation period, treatment firm, one of three indicator variables representing cross-sectional partitions of the data, interaction terms between these three variables and control variables. The dependent variable, ***DEBT*** (***INVESTMENT***), is measured as the total debt (change in fixed assets) scaled by lag total assets. The independent variables are defined as follows: ***POST_REG*** is an indicator variable that equals one for fiscal years ending after March 30, 2004; ***TREATMENT_FIRM*** is an indicator variable that equals one for our treatment firms (i.e., firms that obtain an audit throughout our sample period and have sales between £1 million and £5.6 million and assets between £1.4 million and £2.8 million); ***FIN_CONSTRAINED*** is an indicator variable that equals one for firms in the bottom tercile of the age distribution of our sample firms; ***BIG4*** is an indicator variable that equals one for firms that hire one of the big-four auditors; ***SALES_GR*** is the percentage change in sales; ***LTANGIBLE_ASSETS*** is the natural log of tangible assets; ***LSIZE*** is the natural log of total assets; ***ROA*** is return on assets computed as the income before extraordinary items scaled by total assets; ***LIQUIDITY*** is the ratio of current assets to current liabilities; ***LAUDIT_FEE*** is the natural log of audit fees incurred by the firm scaled by total assets. The control sample in the regressions below is the *Voluntary Audit Control Sample*, which comprises of firms with sales less than £1 million and assets less than £1.4 million prior to 2004 and they obtain an audit (voluntarily) throughout our sample period. The *t*-statistics are clustered at the firm-level to control for residual correlation in firms' investment. *,**,*** indicate statistical significance at the 10%, 5%, and 1% level, respectively, using a two-tailed *t*-test.

*Panel A: Financially Constrained Firms*

| **Dependent Variable:** | | ***DEBT*** | | ***INVESTMENT*** | |
|---|---|---|---|---|---|
| | Pr. Sign | Coefficient | *t*-Statistic | Coefficient | *t*-Statistic |
| *POST_REG × TREATMENT_FIRM* | + | 0.011 ** | 2.17 | 0.007 *** | 2.68 |
| *POST_REG × FIN_CONSTRAINED* | | -0.007 | -0.92 | 0.000 | -0.11 |
| *POST_REG × TREATMENT_FIRM × FIN_CONSTRAINED* | + | 0.016 * | 1.74 | 0.007 * | 1.76 |
| *SALES_GR* | | -0.008 | -1.23 | 0.003 ** | 2.38 |
| *LTANGIBLE_ASSETS / LSIZE* | | 0.004 ** | 2.32 | -0.024 *** | -12.60 |
| *ROA* | | -0.031 *** | -6.74 | 0.010 *** | 3.14 |
| *LIQUIDITY* | | -0.001 | -0.52 | 0.003 *** | 4.90 |
| *LAUDIT_FEE* | | 0.001 | 0.21 | 0.014 *** | 4.92 |
| *Year × Industry Indicators* | | Included | | Included | |
| *Firm Indicators* | | Included | | Included | |
| R-Squared | | 7.2% | | 6.2% | |
| No. of Observations | | 18,990 | | 18,990 | |

**Table 8 (continued)**

*Panel B: Firms Using Big 4 Auditor*

| Dependent Variable: | | *DEBT* | | *INVESTMENT* | |
|---|---|---|---|---|---|
| | Pr. Sign | Coefficient | *t*-Statistic | Coefficient | *t*-Statistic |
| *POST_REG × TREATMENT_FIRM* | + | 0.017 *** | 4.30 | 0.010 *** | 4.36 |
| *POST_REG × BIG4* | | 0.000 | 0.02 | 0.008 | 1.23 |
| *POST_REG × TREATMENT_FIRM × BIG4* | - | -0.006 | -0.79 | -0.014 * | -1.86 |
| *SALES_GR* | | -0.008 | -1.25 | 0.003 ** | 2.20 |
| *LTANGIBLE_ASSETS / LSIZE* | | 0.004 ** | 2.38 | -0.024 *** | -12.56 |
| *ROA* | | -0.031 *** | -6.74 | 0.010 *** | 3.17 |
| *LIQUIDITY* | | -0.001 | -0.51 | 0.003 *** | 4.94 |
| *LAUDIT_FEE* | | 0.001 | 0.24 | 0.014 *** | 4.95 |
| *Year × Industry Indicators* | | Included | | Included | |
| *Firm Indicators* | | Included | | Included | |
| R-Squared | | 7.2% | | 6.2% | |
| No. of Observations | | 18,990 | | 18,990 | |

**Table 9**
Examination of Treatment Firms that Surpass the Audit Exemption Threshold Limits Post-2004

This table presents an analysis of firms that initially obtained mandatory audits (pre-2004), then switch to obtaining voluntary audits post-2004 and then again switch back to obtaining mandatory audits between the periods 2005 to 2007. The sample for this analysis is restricted to our treatment firms (i.e., firms that obtain a mandatory audit pre-2004 and voluntary audit post-2004) for the fiscal years ending between 2004 and 2007. We partition these treatment firms into two groups: (A) firms that surpass the sales/assets threshold to qualify for audit exemptions and (B) firms that do not surpass the sales/asset thresholds to qualify for the audit exemption during our sample period. Panel A presents descriptive statistics for the sample firms used in this analysis (as of the year 2004). Panel B present the results from regressing firm debt, cost of debt, and investment on (i) an indicator variable for treatment firms that switch back to getting mandatory audits because they surpass the sales/assets threshold to qualify for the audit exemption, (ii) an indicator variable for the years following the switch back to the mandatory audit regime, interactions between (i) and (ii) and control variables. The dependent variables are measured as follows: ***DEBT (INVESTMENT)*** is the total debt (change in fixed assets) scaled by lag total assets and ***INTEREST_EXP*** is the interest expense scaled by lagged total debt conditional lagged total debt being greater than zero. ***GROUP_A_TREATMENT*** is an indicator variable that equals one for the Group A treatment firms that switch from getting voluntary audits to mandatory audits because their sales/assets surpasses the regulatory thresholds that exempt firms from the audit requirement. The independent variables are defined as follows: ***POST*** is an indicator variable that equals one for fiscal years ending after the treatment firms (i.e., Group A firms) switch back to mandatory audits. Since Group B firms do not surpass the sales/asset thresholds to switch back to a mandatory audit regime, the *POST* variable equals one for these firms when their matched Group A firm switches to a mandatory audit regime; ***SALES_GR*** is the percentage change in sales; ***LSIZE*** is the natural log of total assets; ***LTANGIBLE_ASSETS*** is the natural log of tangible assets; ***ROA*** is return on assets computed as the income before extraordinary items scaled by total assets; ***LIQUIDITY*** is the ratio of current assets to current liabilities; ***LAUDIT_FEE*** is the natural log of audit fees incurred by the firm scaled by total assets. In Panels B and C, the *t*-statistics are clustered at the firm-level to control for residual correlation in firms' investment. $^{*,**,***}$ indicate statistical significance at the 10%, 5%, and 1% level, respectively, using two-tailed *t*-test.

*Panel A: Descriptive Statistics of Group A and Group B Firms as of 2004*

| Variables | Mean | SD | P25 | P50 | P75 | N |
|---|---|---|---|---|---|---|
| ***GROUP A:*** ***Treatment firms that exceed the sales/asset thresholds to qualify for the audit exemption after 2004*** | | | | | | |
| *SALES_GR* | 0.122 | 0.242 | -0.017 | 0.079 | 0.269 | 454 |
| *ROA* | 0.176 | 0.211 | 0.042 | 0.135 | 0.293 | 454 |
| *TOTAL_ASSETS* | 1,655 | 692 | 1,151 | 1,566 | 2,317 | 454 |
| *SALES* | 2,354 | 1,828 | 357 | 2,427 | 4,046 | 454 |
| *DEBT* | 0.107 | 0.194 | 0.000 | 0.000 | 0.075 | 454 |
| *INVESTMENT* | 0.035 | 0.055 | 0.000 | 0.013 | 0.043 | 454 |
| ***GROUP B:*** ***Treatment firms that do not exceed the sales/asset thresholds to qualify for the audit exemption after 2004*** | | | | | | |
| *SALES_GR* | 0.127 | 0.245 | -0.007 | 0.097 | 0.262 | 454 |
| *ROA* | 0.178 | 0.201 | 0.038 | 0.135 | 0.302 | 454 |
| *TOTAL_ASSETS* | 1,076 | 592 | 609 | 972 | 1,451 | 454 |
| *SALES* | 1,810 | 1,136 | 1,100 | 1,675 | 2,464 | 454 |
| *DEBT* | 0.101 | 0.191 | 0.000 | 0.000 | 0.078 | 454 |
| *INVESTMENT* | 0.036 | 0.062 | 0.000 | 0.012 | 0.039 | 454 |

**Table 9 (continued)**

*Panel B: Debt, Cost of Debt, and Investment Effects for Firms that Surpass the Audit Exemption Thresholds*

| **Dependent Variable:** | ***DEBT*** | | ***INTEREST_EXP*** | | ***INVESTMENT*** | |
|---|---|---|---|---|---|---|
| | Coefficient | *t*-Statistic | Coefficient | *t*-Statistic | Coefficient | *t*-Statistic |
| *GROUP_A_TREATMENT × POST* | -0.014 * | -1.83 | 0.007 * | 1.66 | -0.006 | -1.50 |
| *POST* | 0.016 * | 1.77 | 0.003 | 0.67 | 0.000 | -0.04 |
| *SALES_GR* | -0.015 | -1.53 | 0.001 | 0.23 | 0.012 *** | 2.76 |
| *LTANGIBLE_ASSETS / LSIZE* | 0.008 * | 1.80 | -0.002 * | -1.69 | -0.024 *** | -5.68 |
| *ROA* | -0.077 *** | -3.99 | 0.003 | 0.33 | 0.012 | 1.61 |
| *LIQUIDITY* | 0.002 | 0.67 | -0.001 | -0.84 | 0.000 | 0.50 |
| *LAUDIT_FEE* | -0.004 | -0.55 | 0.001 | 0.19 | 0.003 | 0.71 |
| *Year × Industry Indicators* | Included | | Included | | Included | |
| *Firm Indicators* | Included | | Included | | Included | |
| R-Squared | 9.1% | | 1.9% | | 9.6% | |
| No. of Observations | 3,252 | | 1,080 | | 3,252 | |

**Table 10**
Effect of 2004 Regulation on Audit Fees and Audit Quality

This table presents the results from regressing audit fees and future cash flows on indicator variables for the post-regulation period, treatment firm, interaction terms between these variables and control variables. ***LAUDIT_FEE*** is the natural log of audit fees incurred by the firm; ***CFO*** is cash flows from operations scaled by total assets; ***ACC*** is the accruals scaled by total assets; ***DEBT*** is the total debt outstanding scaled by total assets; ***LSIZE*** is the natural log of total assets; ***LSALES*** is the natural log of the total sales. ***ROA*** is return on assets computed as the income before extraordinary items scaled by total assets; ***LIQUIDITY*** is the ratio of current assets to current liabilities; The control sample in the regressions below is the *Voluntary Audit Control Sample*, which comprises of firms with sales less than £1 million and assets less than £1.4 million prior to 2004 and they obtain an audit (voluntarily) throughout our sample period. The *t*-statistics are clustered at the firm-level to control for residual correlation in firms' investment. *,**,*** indicate statistical significance at the 10%, 5%, and 1% level, respectively, using two-tailed *t*-test otherwise.

*Panel A: Audit Fees Regression*

| **Dependent Variable:** | ***AUDIT FEES*** | |
|---|---|---|
| | Coefficient | *t*-Statistic |
| *POST_REG × TREATMENT_FIRM* | 0.004 | 0.38 |
| *DEBT* | 0.021 | 0.74 |
| *LSIZE* | 0.043 ** | 5.02 |
| *LSALES* | 0.114 *** | 10.69 |
| *ROA* | -0.046 *** | -3.83 |
| *LIQUIDITY* | -0.004 * | -1.76 |
| *Year × Industry Indicators* | Included | |
| *Firm Indicators* | Included | |
| R-Squared | 28.2% | |
| No. of Observations | 18,990 | |

*Panel B: Audit Quality Regression*

| **Dependent Variable:** | ***CFO*** $_{t+1}$ | |
|---|---|---|
| | Coefficient | *t*-Statistic |
| *CFO × POST_REG × TREATMENT_FIRM* | 0.027 | 0.53 |
| *ACC × POST_REG × TREATMENT_FIRM* | 0.034 | 0.61 |
| *CFO × TREATMENT_FIRM* | -0.014 | -0.33 |
| *ACC × TREATMENT_FIRM* | 0.035 | 0.76 |
| *CFO × POST_REG* | -0.152 *** | -3.62 |
| *ACC × POST_REG* | -0.080 * | -1.74 |
| *CFO* | -0.128 *** | -3.63 |
| *ACC* | 0.123 *** | 3.23 |
| *POST_REG × TREATMENT_FIRM* | 0.003 | 0.53 |
| *Year × Industry Indicators* | Included | |
| *Firm Indicators* | Included | |
| R-Squared | 11.1% | |
| No. of Observations | 18,990 | |

**Table 11**
Analyses of Firms that Opt Out from Obtaining Audits

Panel A (B) in this table presents the results from regressing total debt (investment) on indicator variables for the post-regulation period, opt-out firm, interaction terms between these two variables and controls. The '***Voluntary Audit Control Sample***' is comprised of firms with sales less than £1 million and assets less than £1.4 million prior to 2004 and they obtain an audit (voluntarily) throughout our sample period. The '***Mandatory Audit Control Sample***' includes firms required to obtain audits both before and after the regulatory change. This control sample is comprised of firms with sales greater than £5.6 million and/or assets greater than £2.8 million during our sample period. The '***Voluntary Opt-Out Control Sample***' consists of firms with sales less than £1 million and assets less than £1.4 million prior to 2004 that opt out of the audit requirement throughout our sample period. See the notes in the previous tables for variable definitions. The *t*-statistics are clustered at the firm-level to control for residual correlation in firms' investment. *,**,*** indicate statistical significance at the 10%, 5%, and 1% level, respectively, using a two-tailed *t*-test.

*Panel A: Debt Regressions*

| **Dependent Variable:** | ***DEBT*** | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| **Control Sample:** | ***No Control Sample*** | | ***Voluntary Audit Control Sample*** | | ***Mandatory Audit Control Sample*** | | ***Voluntary Opt-Out Control Sample*** | |
| | Coef. | *t*-Stat. | Coef. | *t*-Stat. | Coef. | *t*-Stat. | Coef. | *t*-Stat. |
| *POST_REG* | -0.011 *** | -3.20 | | | | | | |
| *POST_REG × OPT_OUT_FIRM* | | | 0.002 | 0.31 | 0.007 | 1.32 | -0.007 | -1.24 |
| *SALES_GR* | 0.000 | -0.16 | -0.003 | -0.87 | 0.000 | -0.13 | -0.002 | -0.84 |
| *LTANGIBLE_ASSETS* | 0.007 ** | 2.39 | 0.006 ** | 2.45 | 0.006 *** | 3.20 | 0.003 | 1.33 |
| *ROA* | -0.056 *** | -6.08 | -0.056 *** | -6.60 | -0.051 *** | -6.63 | -0.052 *** | -6.67 |
| *LIQUIDITY* | 0.003 * | 1.67 | 0.000 | 0.26 | 0.002 | 1.16 | 0.000 | 0.08 |
| *Year × Industry Indicators* | Not Included | | Included | | Included | | Included | |
| *Firm Indicators* | Included | | Included | | Included | | Included | |
| R-Squared | 5.0% | | 6.8% | | 5.9% | | 7.2% | |
| No. of Observations | 5,756 | | 10,322 | | 10,724 | | 9,004 | |

*Panel B: Investment Regressions*

| **Dependent Variable:** | ***INVESTMENT*** | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| **Control Sample:** | ***No Control Sample*** | | ***Voluntary Audit Control Sample*** | | ***Mandatory Audit Control Sample*** | | ***Voluntary Opt-Out Control Sample*** | |
| | Coef. | *t*-Stat. | Coef. | *t*-Stat. | Coef. | *t*-Stat. | Coef. | *t*-Stat. |
| *POST_REG* | -0.002 | -0.89 | | | | | | |
| *POST_REG × OPT_OUT_FIRM* | | | 0.002 | 0.52 | 0.002 | 0.77 | -0.001 | -0.49 |
| *SALES_GR* | 0.004 * | 1.90 | 0.004 ** | 2.26 | 0.005 *** | 2.95 | 0.002 | 0.95 |
| *LSIZE* | -0.033 *** | -7.63 | -0.034 *** | -9.44 | -0.029 *** | -8.04 | -0.040 *** | -8.50 |
| *ROA* | 0.019 *** | 2.69 | 0.018 *** | 3.18 | 0.018 *** | 3.13 | 0.007 | 1.11 |
| *LIQUIDITY* | 0.007 *** | 5.26 | 0.004 *** | 4.91 | 0.006 *** | 5.92 | 0.007 *** | 7.08 |
| *Year × Industry Indicators* | Not Included | | Included | | Included | | Included | |
| *Firm Indicators* | Included | | Included | | Included | | Included | |
| R-Squared | 4.7% | | 8.7% | | 8.2% | | 10.2% | |
| No. of Observations | 5,756 | | 10,322 | | 10,724 | | 9,004 | |