Financial Statement Audits as Costly Signals: Evidence from Corporate Investment Decisions

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ABSTRACT

We hypothesize that a regulatory requirement to obtain financial statement audits conceals information about a firm's prospects that can be gleaned from observing the firm's *choice* to obtain an audit. We examine whether the information revealed from observing a firm's audit choice reduces financing frictions, thereby increasing investment and debt. We use a natural experiment, where a regulatory change relaxed the audit mandate for a subset of private firms, to isolate and examine the change in investment and financing behavior of these firms. Using a difference-in-difference matching estimator, we find that the firms that switch from obtaining audits under a mandatory audit regime to doing so under a voluntary audit regime significantly increase their investment, leverage, and operating performance, and become more responsive to their investment opportunities following the regulation. Further, we find that these effects are stronger for firms that are ex ante financially constrained and weaker for firms that had recent banking interactions and/or engaged in other means to signal their prospects before the regulation. Overall, our evidence suggests that the audit *choice* conveys information to capital providers (above that conveyed by the audit itself) about the quality of firms and their prospects, which reduces financing frictions and improves performance.

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I. INTRODUCTION

A healthy economy is characterized by a financial market that moves capital to those who have the most productive investment opportunities (Mishkin 1992). However, one of the most pervasive factors reducing corporate investment efficiency is information asymmetry between capital suppliers and users, which leads to financing frictions and slower economic growth (Rajan and Zingales 1998; Stein 2003). To mitigate financing frictions, policy makers across the world design various corporate governance regulations. Arguably the most notable of these regulations is the mandate for firms to disclose financial information on a periodic basis and have an independent outside party audit or certify those disclosures. Although the audit is valuable in that it provides credibility for those disclosures, the *mandating* of audits potentially removes valuable information from the market (Watts 1977; Chow 1982; Benston 1985; Sunder 2003). In particular, the mandatory audit requirement conceals information about a firm's incentive to conduct an audit, which can be particularly useful for capital suppliers assessing the quality of a firm and its future prospects.

This paper examines whether an audit mandate inhibits corporate investment by removing an important information signal contained in the 'audit choice,' thereby increasing financing frictions. To illustrate, consider a regime where firms are allowed to contract with auditors on a voluntary basis. This contracting decision involves a cost-benefit analysis by the firm. That is, although audits can provide credibility to the financial statements and have been shown to increase firms' debt capacity as a result (Blackwell, Noland, and Winters 1998; Allee and Yohn 2009; Minnis 2011), there are also nontrivial costs associated with undergoing an audit, such as the price paid to auditors, managerial time and effort, risk of report modification, etc. Accordingly, only those firms for which the benefits of an audit (e.g., increased access to finance) outweigh the costs undergo an audit in a voluntary audit regime. This decision therefore signals important information, beyond that provided by the audit itself, to capital suppliers regarding the quality of a firm and its prospects. Namely, those that choose to undergo an audit voluntarily are more likely to be the firms with the most productive uses of capital.[1]

This signaling intuition is similar to that of Guasch and Weiss (1981), who develop a labor market model, where an employer is screening applicants for potential employment. In an attempt to identify the high quality workers, the employer requires applicants to take a pass-fail test, where the test result is a function of the applicant's productivity, measured with error. To better identify the high quality workers, the employer also requires the applicants to pay a fee to take the test, thereby making the test costly for the applicant. As a result, only the most productive workers are willing to take the test, since the least productive workers do not want to incur the cost of taking the test given their higher probability of failing. In this setting, the decision to take the test conveys incremental information to the employer about the applicant's ability, over and above the information provided by the test result. In our setting, the firm undergoing an audit to acquire capital is analogous to the applicant paying to take the test. That is, 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.[2]

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[1] We acknowledge that firms can choose to undergo an audit for reasons other than obtaining external financing, and we acknowledge that firms can finance their investments through internally generated funds or the personal funds of the manager (rather than external financing). We control for the alternative benefits of an audit and the alternative sources of financing with our research setting and design, which we discuss below.

[2] We note that in some cases, banks require financial statement audits as a condition for lending, thereby making the audit de facto mandatory. Even then our setting closely parallels the Gausch and Weiss (1981) setting in that it is the signal receiver (employer/bank) requiring the signal rather than the signal sender (applicant/firm) initiating the signal. In the former case, 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; firms must choose whether they want to undertake the cost of the audit to obtain financing.

Empirically testing whether the audit choice can serve as a costly signal that affects corporate investment decisions is very challenging because an audit can serve at least two roles, both of which can reduce adverse selection concerns: (i) signaling role, which reduces adverse selection concerns by sending a positive signal about the firm's quality via the audit choice, and (ii) verification or assurance role, which reduces adverse selection concerns by increasing the reliability of financial statement numbers. Thus, the challenge lies in identifying the appropriate cause of the observed investment effects.

We overcome this challenge by using a unique, quasi-experimental setting where private firms are initially required to obtain audits, but a regulatory change relaxed the mandate for a subset of firms. Specifically, external audits were mandatory for almost all U.K. private firms until 2004. However, in 2004, a subset of private firms was allowed to opt out of an audit, i.e., audits became voluntary for this subset of firms. We exploit this audit regime shift to compare the change in investment behavior of firms that had the option to opt out of the audit requirement but continue to receive audits voluntarily with that of firms that continue to undergo audits but are unaffected by the regulation. That is, we examine the investment of firms that mandatorily receive audits before the regulation, but voluntarily receive audits afterward and benchmark any changes in investment behavior for these firms with that of two control samples: (i) firms that voluntarily receive audits both before and after the regulation (i.e., these firms had the option to opt-out of an audit even before 2004), and (ii) firms that mandatorily receive audits both before and after the regulation.[3] Therefore, both treatment and control firms obtain financial statement

[3] Firms that obtained mandatory audits prior to 2004 but exercised the option to opt-out of the audit requirement after 2004 could potentially serve as another control sample. However, a significant drawback of using this sample as a control group is that firms choosing to opt out of an audit lose the verification value of the audit (since their financial statements are no longer audited) following the regulation. Thus, disentangling the signaling effect of the audit choice from the verification effect of the audit becomes difficult with this alternate control group. By using control firms that receive audits both before and after the regulation, but do not have a shift in their signaling ability,

audits throughout our sample period but they differ in their ability to signal their quality via the audit choice following the regulatory change in 2004 (see Figure 1).

Our difference-in-difference research design allows us to parse out the signaling value of an audit from its verification value. By requiring both treatment and control firms to have audits (i.e., holding the verification role of the audit constant) over the sample period, we can isolate the signaling effect of the audit by examining the change in investment after the regulation.[4] Moreover, the regulatory change allows us to control for firm-specific factors that affect investment, while the matched control samples allow us to control for concurrent industry and/or market changes in investment opportunities (demand for funds) and the availability of credit (supply of funds) unrelated to the audit signal. The power of our tests is further strengthened by examining private firms, which face significantly more severe financing frictions than larger public firms do.

Using a novel database of private U.K. firms supplied by *Bureau Van Dijk* and the research design discussed above, we find that the treatment firms significantly increase their investment following the regulation as compared to the investment of two sets of control firms. In economic terms, our coefficients imply that treatment firms increase investment by approximately 29 to 98% following the regulation, depending on the control sample used.[5] This evidence is consistent with the audit *choice* conveying important information to capital providers (above that conveyed by audit assurance) about the quality of firms and their future prospects.

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we can better identify the impact of the signal on firm investment. For completeness, we also examine these opt out firms in additional analyses in Section VI.B.

[4] In Section VI.D., we provide evidence that audit assurance does not change following the regulation.

[5] Specifically, our coefficients suggest that the treatment firms increase their investment by approximately 38%, 98% and 29% when we (i) do not use a control sample, (ii) use firms obtaining voluntary audits as control firms, and (iii) use firms obtaining mandatory audits as control firms, respectively. These large percentage increases in investment are because (i) our sample is comprised of small private firm that are severely capital constrained and (ii) our sample firms have low investment levels pre-regulation, thereby creating a small denominator effect.

To provide further support for our hypothesis and better identify the mechanism through which additional investment occurs, we also examine changes in debt financing around the regulation. We find that our treatment firms increase their debt by approximately 4 to 7% (depending on the control sample used) and the magnitude of the increase in debt (in pounds) closely parallels the increase in investment. Further, we find that the increase in debt is driven by increases in long-term debt. Prior research finds financing frictions affect loan maturity and that lenders use shorter term loan contracts to force more frequent renegotiation with borrowers known to be risky ex ante (Myers 1977; Barclay and Smith 1995; Ortiz-Molina and Penas 2008). To the extent the audit signal provides incremental information to lenders about borrower type and the borrower's future prospects, we should observe that the increase in debt occurs via increases in long-term rather than short-term debt, which is consistent with our findings.

We then examine whether firms become more responsive to their investment opportunities and improve their operating performance following the regime shift, as the information in the audit choice signal reduces financing constraints and increases financial flexibility. Using a similar difference-in-difference design to the one described above, we find that the treatment firms observe significant increases in both their responsiveness to investment opportunities and their overall operating performance after the regulation relative to that of the control firms. These results provide additional evidence that the audit choice signal conveys useful information to external financiers, which relaxes financing frictions and facilitates more efficient investment.

Next, we examine whether there is cross-sectional variation in the benefit of the audit signal. We predict that the audit choice signal is more valuable for firms that are ex ante financially constrained and less valuable for firms that were able to raise additional debt and/or used alternative means to signal their quality (i.e., by appointing a higher quality auditor) before the regulation. Consistent with our prediction, we find that the audit choice signal leads to a larger increase in investment and debt in firms that are ex ante financially constrained and leads to a smaller increase in investment in firms with recent banking interactions and with high quality auditors. These results help further support our main hypothesis.

Finally, we examine the investment and financing behavior of firms that opt to stop receiving audits after the regulatory change. We find that, unlike our treatment firms, the opt out firms do not change their investment and financing behavior relative to a matched sample of firms that (i) obtained mandatory/voluntary audits before and after 2004, and (ii) firms that did not receive audits before and after 2004.

Our paper contributes to the literature by documenting a potential downside to regulation. In particular, policy makers generally pair disclosure requirements with audit requirements to increase the credibility of the disclosures, and thus mitigate the impact of information asymmetry on firms' financing capacity and investment. However, our findings suggest a potential drawback of an audit mandate is that information about firms' prospects may be hidden by removing the *observable* audit choice absent the regulation. Although policy implications cannot be made without more extensive discussion and analysis, this study provides initial fodder for this debate.

Second, our paper contributes to the large literature that documents a relation between corporate financing and investment. For example, Whited (1992) and Hennessy (2004) use structural econometric approaches to examine the impact of financing frictions on investment. However, the mechanisms through which this relation manifests are less clear (Stein 2003). Along this dimension, Chava and Roberts (2008) provide evidence that the transfer of control rights via debt covenant violations act as a mechanism through which financing frictions impact investment. Nini, Sufi, and Smith (2009) show that debt contracts contain explicit capital expenditure covenants that serve as a control mechanism, which reduces investment. We extend this literature by examining a mechanism – i.e., sending a costly signal by choosing to obtain an audit – that can mitigate the effect of financing constraints on investment.

Finally, this paper builds on related work by Lennox and Pittman (2011) who show that firms voluntarily obtaining audits receive higher credit ratings (by Qui Credit Assessment) relative to firms that choose to be unaudited. While these findings are informative as an initial step in empirically documenting the value of an audit, it is unclear whether changes in the credit rating from this small, regional rating agency translates into economically significant changes in corporate investment, particularly given known biases in credit rating agencies (e.g., Bolton, Freixas, and Shapiro 2009).[6] Further, since Lennox and Pittman (2011) use firms that do not receive audits as their benchmark sample, it is unclear whether the change in credit ratings is due to a signaling effect or a change in the control firms' financial statement verifiability from opting out of audits. Our paper contributes to the literature by showing that the audit choice serves as a costly signal and that this signal has economically significant *real* effects.

The rest of the paper proceeds as follows. Section II provides the theoretical development. Section III discusses our institutional setting. Section IV explains data. Section V presents our main results. Section VI presents robustness tests, and Section VII concludes.

II. THEORETICAL DEVELOPMENT

A well-functioning economy relies on capital markets for growth via efficient capital allocation (Rajan and Zingales 1998, Wurgler 2000). However, information asymmetry between

[6] In addition, any changes in credit ratings could be due to an idiosyncratic feature of the statistical model used by Qui Credit Assessment with little real economic effect (e.g., changes in firms' debt holdings and/or investment).

capital providers and capital suppliers exist in financial markets. In particular, borrowers and entrepreneurs know their abilities and future prospects better than lenders and investors. Although capital suppliers would benefit from knowing this information about borrowers and entrepreneurs, moral hazard prevents the direct transfer of this information between market participants (Leland and Pyle 1977). This information asymmetry induces market frictions (via financing frictions) by introducing adverse selection concerns on the part of capital suppliers, which results in reduced investment in the economy.[7]

One direct mechanism typically used to combat information asymmetry in the capital markets is corporate disclosure. In fact, after the 1929 stock market crash, which has been widely argued to be a result of moral hazard and adverse selection, Congress enacted the 1933 Securities Act and 1934 Securities Exchange Act, which require firms to provide much more information than they did previously and to do so in a standardized format on a periodic basis. By providing disclosure, firms can convey their private information to lenders and investors to indicate their type, thereby reducing market frictions and facilitating corporate investment.

Disclosure alone, however, is insufficient to address a market with asymmetric information. Although high quality firms can provide information to convey they are indeed high quality firms, low quality firms can generally mimic these disclosures, particularly if the disclosures are about items that lenders and investors cannot easily verify (e.g., future prospects). Thus, the low quality firms can erode the information content of other firms' disclosures, thereby mitigating the ability of capital suppliers to identify the high quality investments (Akerlof 1970;

[7] See Stein (2003) for a survey of both theoretical and empirical research on the influence of asymmetric information and agency problems on investment behavior.

Easterbrook and Fischel 1984). Thus, capital allocation becomes less efficient.[8] High quality firms must take action beyond disclosure to convince lenders and investors that they are indeed high quality.

Prior literature indicates that one approach economic agents can take to convey their type is to provide an independent signal. This signaling literature suggests that a signal must contain two key features to be effective. First, the signaling action must be observable, so that the information can be conveyed to the appropriate party. Second, the costs associated with the signal must be negatively correlated with the signal factor (e.g., productivity, quality or ability), such that economic agents of low quality do not find a net benefit to duplicating it (Spence 1976). Otherwise, every economic agent will invest in the signal in the same way, so that others cannot distinguish between the agents on the basis of the signal. For example, Spence (1973) describes a labor market where employee applicants with high ability are seeking employment and want to distinguish themselves as such to employers. To do so, these applicants invest in education to signal their type, where the cost of the education, which includes monetary and psychic costs, is decreasing in the applicants' ability.

More related to our setting, subsequent work by Guasch and Weiss (1981) describes a labor market setting where, as a condition for employment, applicants must pay to take a pass-fail test, where the test result measures the employee's productivity with error. They show that the act of paying to take the test sends a signal to employers that allows them to separate the high

[8] As Leland and Pyle (1977, 371) state, "Without information transfer, markets may perform poorly. Consider the financing of projects whose quality is highly variable. While entrepreneurs know the quality of their own projects, lenders cannot distinguish among them. Market value, therefore, must reflect average project quality. If the market were to place an average value greater than average cost on projects, the potential supply of low quality projects may be very large, since entrepreneurs could foist these upon an uninformed market (retaining little or no equity) and make a sure profit. But this argues that the average quality is likely to be low, with the consequence that even projects which are known (by the entrepreneur) to merit financing cannot be undertaken because of the high cost of capital resulting from low average project quality. Thus, where substantial information asymmetries exist and where the supply of poor projects is large relative to the supply of good projects, venture capital markets may fail to exist."

ability applicants from the low ability applicants over and above the information conveyed by the test. In both scenarios, the signal is observable and the signaling costs are negatively correlated with the signal factor (i.e., the signaling costs are ex ante cheaper for the high quality applicants).

With respect to our setting in which firms are interested in obtaining external capital to fund investment, firms can use a similar signaling approach by hiring an auditor to evaluate the firms' disclosures and certify their accuracy. Auditors who audit many firms have a significant reputational interest, and thus a possible loss, much larger than any potential profits to be made from fraudulent or careless activity related to a particular firm (Easterbrook and Fischel 1984), which disciplines the auditor. The audit then serves two important roles with respect to mitigating adverse selection concerns. First, the audit serves an assurance or verification role, which reduces uncertainty about the accuracy of the reports. Second, the audit *choice* provides information to external stakeholders, such as capital providers, that allows them to distinguish high quality firms from low quality firms, as only those firms with the better prospects will incur the costs to undergo the audit; that is, the audit choice serves as a useful signal of firm prospects.

III. INSTITUTIONAL SETTING

In the U.K., all limited liability companies (both private and public) are formed by incorporation with the Companies House, the government agency that administers them.[9] Prior to 1967, only public companies were required to file their financial statements with the Registrar of Companies House. However, there were a substantial number of firms incorporated as private companies that were subsequently liquidated or struck off the Companies Register, which sparked political fears of abuse and creditor protection. This led to the Companies Act of 1967

[9] Companies House is an executive agency of the U.K. Department of Trade and Industry. 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.

(now part of the Companies Act 1985) requiring all companies, private and public, to file their financial statements annually with the Registrar. Failure to file is a criminal offense. Further, all financial statements must be prepared in accordance with U.K. accounting standards and must be audited by a registered auditor.

Critics of the Companies Act argue that the imposition of universal regulatory standards results in a disproportionately high cost for small companies. Specifically, prior studies and industry groups argue 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., Department of Trade and Industry 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, since the regulation was viewed as disproportionately expensive and of limited benefit for small firms (Keasey, Watson, and Wynarczyk 1988). Specifically, 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). That is, these firms were given the option to opt-out of the audit requirement even though their financial statements were required to be filed in a public repository (i.e., 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 examine the relaxation of the audit requirement in 2004, which allowed companies with fiscal years ending after January 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 £1 million and total assets less than £1.4 million were exempt from the audit requirement.[10] 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 of the exemption and thus increase our sample size, and (ii) it provides us with a large 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 £3.26 million in assets, compared to a 560% (100%) increase in the sales (assets) threshold in 2004. Thus, the 2008 regulation is simply not as powerful as the 2004 regulatory change.

The regulatory change in 2004 provides us with a number of opportunities to isolate the signaling value of financial statement audits. First, the regulation enables us to compile a sample of firms that switched from obtaining mandatory audits to voluntary audits. Second, the firms below the size thresholds that obtain voluntary audits before 2004 serve as a natural control group because even though they voluntarily obtain audits, the 2004 regulation did not affect the signaling value of their audit (because their audit choice was observable throughout our sample period). Finally, the firms immediately above the size thresholds serve as an alternative control group because they continue to receive audits, but these audits are mandatory in nature and thus do not offer any signaling value (because their audit *choice* is unobservable throughout our sample period). As a result, both control samples and the treatment sample are comprised

[10] 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.

exclusively of firms that receive audits; however, they vary in that the treatment firms' audits have a shift in their potential to serve as a 'costly signal' after January 2004.

IV. DATA AND METHODOLOGY

IV.A. 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*, one of Europe's leading electronic publishers of business information. 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.[11] 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. We obtain access to data for companies with fiscal years ending between December 2000 and 2010. Each firm in the FAME database has a unique identifier allowing us to construct a panel.

To construct our sample, we begin by identifying private companies below the size thresholds prescribed to qualify for the 2004 audit exemption (i.e., less than £5.6 million in sales and £2.8 million in assets) 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 prior

[11] Examples of prior studies that use the FAME database are Ball and Shivakumar (2005), Brav (2009), and Lennox and Pittman (2011).

to 2004, but had the option to opt-out of the audit requirement following the regulation. These firms serve as our treatment sample.[12] 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.

Figure 1 provides a diagram of our research design. 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 are represented at the bottom of the diagram, and 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 (henceforth referred to as the "mandatory audit" control sample). 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 January 2004. Table I describes our sample selection procedure in detail.

One advantage of our setting is that it allows us to hold constant the verification role of the audit and isolate the incremental effect of the *audit choice signal* by requiring the treatment firms to have audits in the pre-regulation and post-regulation periods, where the regulation serves

[12] To qualify for an audit exemption companies are also required to have less than 50 employees. However, the FAME database does not provide coverage of this variable. As a result, some of our treatment and control firms might be misclassified. However, we note that any such classification error bias against our hypothesis.

as the shock that endogenizes the audit (i.e., switches the audit from mandatory to voluntary). Thus, the audit in the post-regulation (voluntary) regime not only verifies the accuracy of the financial statements, as in the pre-regulation (mandatory) regime, but also allows lenders to observe the firm's choice to undertake an audit.

A secondary, and perhaps the most unique, advantage of our setting is the staggered implementation of the regulation, which provides us two distinct control groups (i.e., voluntary and mandatory audit control firms) that differ from one another with respect to size and audit option, but remain constant in these regards across the regime change of the treatment firms. These firms serve as valuable benchmark firms to mitigate any effects from market-wide or industry-wide shocks to the demand for funds or supply of funds. This strengthens our identification, as alternative explanations must address not only the change in our outcome variables (i.e., investment, performance and debt) for treatment firms, but also the lack of a related change for the larger, mandatory audit control firms and the smaller, voluntary audit control firms, given all firms undertake audits across the sample period.

IV.B. Empirical Methodology

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

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

where *i* indexes firms, *t* indexes years, $y_{i,t}$ is investment measured as the change in net fixed assets (*INVESTMENT*), α_i and α_t are firm and year fixed effects, *TREATMENT_FIRM* is an indicator variable that equals one for companies with sales (assets) less than £5.6 (£2.8) million, and sales greater than £1 million *and/or* assets greater than £1.4 million in all years covered in our sample, *POST_REG* is an indicator variable that equals one for fiscal years ending after January 30, 2004, and $\boldsymbol{X}$ is a vector of control variables that includes sales growth, firm size, profitability, liquidity, and audit fees. Sale growth proxies for growth opportunities (Shin and Stulz 1998; Whited 2006; Bloom, Bond, and Van Reenen 2007; Badertscher, Shroff and White 2013); firm 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, Knechel and Wong 2006). Note that the main effects of *TREATMENT_FIRM* and *POST_REG* are absorbed by the firm and year fixed effects, and thus not identified in the equation above. We cluster standard errors at the firm level, thereby accounting for any serial correlation of the regression error terms within firms (Bertrand, Duflo, and Mullainathan 2004).

We use nearest neighbor matching within caliper (set at 0.5 times the standard deviation) to construct a sample of control firms that are observably similar to the treatment firms in terms of their investment opportunities and access to finance (Rosenbaum and Rubin 1985).[13] We match on the following variables within each industry and year before the regulatory change (i.e., January 2004): (i) leverage (*DEBT*), (ii) sales growth (*SALES_GR*), (iii) return on assets (*ROA*), and (iv) liquidity (*LIQUIDITY*) measured as the ratio of current assets to current liabilities. We do not match on firm size because the regulation partitions firms based on size thresholds and thus the treatment and control firms do not overlap along this dimension. To mitigate the concern that differences in firm size between the treatment and control firms affects our inferences, we conduct all our tests using both control samples: (i) smaller firms that were exempt from the audit requirement even before 2004 (i.e., the voluntary audit control sample)

[13] In untabulated analyses, we examine the robustness of our inferences to using two additional matching approaches. Specifically, we try the following: (i) we match firms based on their propensity to obtain voluntary audits based on estimated values of their voluntary audit propensities, and (ii) we match firms on additional variables that include total assets, sales, and, auditor. We find that our inferences are unchanged in all of the above specifications (i.e., our coefficients of interest always remain significant at the one-tail 5% level or better).

and (ii) larger firms that are required to obtain audits mandatorily even after 2004 (i.e., the mandatory audit control sample). Since the mandatory audit control firms are systematically larger than the treatment firms and the voluntary audit control firms are systematically smaller than the treatment firms, any monotonic relation between firm size and investment cannot explain our findings. We also conduct additional robustness tests in Section VI.E.

Table II, Panel A compares the mean values of the matching variables for our treatment sample with those for the two control samples in 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 all 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. Importantly, these results suggest that the key identifying assumption in the difference-in-different specification – i.e., the parallel trends assumption – holds in the pre-treatment years. Further, any residual differences between the treatment and control firms are likely to be filtered out by the inclusion of firm fixed effects and the use of a difference-in-difference design.

IV.C. Descriptive Statistics

Table II, Panels B and C presents summary statistics for our variables of interest. Panel B (C) reports the statistics for the treatment sample and its matched voluntary (mandatory) audit control sample both before and after the regulation. Panel B shows that the average change in net fixed assets (*INVESTMENT*) is 0.4% of total assets before 2004 and increases to 1% following the regulation for our treatment sample. In contrast, the voluntary audit control sample's average *INVESTMENT* remains constant at 0.7% both before and after the regulation. The voluntary audit control sample is comprised of significantly smaller firms (assets = £0.7 million, sales =

£0.6 million) relative to the treatment sample (assets = £0.8 million, sales = £1.7 million), which is by construction since the regulation partitions firms based on size. Further, we note that the difference in firm size persists after the regulation (difference significant at 1% level). We find that the 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. Further, we note that the average treatment firm finances only 13% of its assets through debt (approximately), but finances 37% of its assets via trade credit (*ACC_PAYABLE*). This pattern of financing among small firms is consistent with that documented in prior research (e.g., Rajan and Zingales 1995; Petersen and Rajan 1997; Nilsen 2002). We also find that *SALES_GR*, *ROA*, and *LIQUIDITY* are comparable for the treatment and control samples before 2004, but they are significantly higher for the voluntary audit control sample after 2004. Finally, we find that the treatment firms are marginally younger than the control firms.

Table II, Panel C presents the summary statistics for the treatment sample and the matched mandatory audit control sample both before and after the regulation. Note that the mandatory audit control sample is comprised of relatively larger firms than the alternate (voluntary audit) control sample because of the higher size thresholds imposed by the regulation. Panel C shows that the treatment firms increase *INVESTMENT* from 0.6% to 1.1% after the regulation, whereas the control firms increase *INVESTMENT* from 0.6% to 0.8% after the regulation. Next, we find that *DEBT*, *SALES_GR*, *ROA*, and *LIQUIDITY* are statistically indistinguishable for the treatment and control samples before 2004, but the treatment firms have significantly higher *DEBT* and *ROA* after the regulation. Finally, we find that the treatment firms are marginally younger than the control sample.

Table II also presents the descriptive statistics for the audit fees (*LAUDIT_FEE*) incurred by the firms in our sample and the proportion of our sample employing one of the big four auditing firms (*BIG4*).[14] In Panel B, we find that the average treatment firm in our sample pays £5,316 (£6,017) for an audit before (after) the regulation. At first glance, these numbers do not appear to be very large in magnitude. However, we note they represent 6.0% (5.8%) of the average firm's earnings and 0.7% (0.7%) of the average firm's assets before (after) the regulation. In addition, we note that the cost listed above does not include potentially significant non-monetary costs incurred during an audit due to the managerial time and effort devoted to getting through the process. Thus, the audit imposes a non-trivial cost on firms and is likely to deter at least some firms from undergoing an audit. Consistent with this expectation, we find that 32.3% of the firms in our database opt-out of obtaining an audit following regulation in 2004. Table II also shows that only about 8% of our treatment firms employ a *BIG4* auditor. This is not surprising given that *BIG4* auditors charge higher fees for their service, which presumably exceeds the auditing budgets of most firms in our sample. For example, average treatment firm employing a *BIG4* auditor pays £7,372 for an audit, which is approximately 24% higher than that paid for a non-*BIG4* auditor.

V. RESULTS

V.A. The Effect of Signaling on Investment Levels

Table III presents our main results. We present three 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, and (iii) firms obtaining *mandatory* audits before and after the regulation as an alternative control sample. The first specification

[14] The *BIG4* auditors include KPMG, Pricewaterhouse Coopers (PwC), Deloitte, and Ernst & Young (E&Y). These are the largest accounting firms in the world and are widely considered to provide the highest quality audits.

includes firm fixed effects, so the main effect of *TREATMENT_FIRM* is not identified. Similarly, in the latter two specifications, we include firm and year fixed effects and thus the main effects of *TREATMENT_FIRM* and *POST_REG* are not identified. Column 1 presents results from the baseline specification without any control sample. The variable of interest in this regression is *POST_REG*.[15] The coefficient for *POST_REG* is 0.005, and it is statistically significant at the 1% level (t-stat. = 6.38). This coefficient suggests that firms increase their investment by 0.5 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 £5,201 increase in investment for the average treatment firm and corresponds to a 38% increase in investment from its conditional mean.

A drawback of the baseline specification is that the changes in investment following the regulation could be due to concurrent changes in growth opportunities and/or the availability of financing not caused by the audit choice signal. To mitigate any effects of confounding factors, we benchmark the changes in investment for our treatment firms with that of a matched control sample using a difference-in-difference specification. Columns 2 and 3 present the regression results using firms obtaining voluntary and mandatory audits as the control sample, respectively. The variable of interest in these regressions is *POST_REG* × *TREATMENT_FIRM*, and it captures the incremental change in investment for our treatment firms following the regulation relative to that for our control samples. We find the coefficient for *POST_REG* × *TREATMENT_FIRM* is 0.008 (0.004) when the control sample comprises of firms obtaining voluntary (mandatory) audits and the coefficient is statistically significant at the 1% (5%) level. These coefficients indicate that the treatment firms increase investment by 0.8 (0.4) percentage

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

points more than the voluntary (mandatory) control firms do, on average. In terms of economic magnitude, these coefficients suggest that investment increased by £8,320 (£3,976), representing approximately a 98% (29%) increase relative to the voluntary (mandatory) audit control sample. The large variation in the economic magnitudes is partially because our sample firms (particularly the voluntary audit control firms) have low investment levels to begin with, thereby creating a small denominator effect. In addition, our sample firms are fairly small and thus likely to be severely financially constrained.

Table III also shows that the coefficients for the control variables are consistent with our expectations and prior research. Specifically, we find that the coefficients for *SALES_GR*, *ROA* and *LIQUIDITY* are positive and statistically significant at the 10% 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.

V.B. The Effect of Signaling on Debt

To further corroborate our hypothesis that the audit choice signal increases investment by increasing debt capacity, we examine whether firms obtaining voluntary audits take on additional debt following the regulation. An increase in debt capacity can lead to an increase in investment either by directly leading to an increase in debt levels or by giving firms the option to increase debt in the future in case they need additional financing during the life of the project. As a result, ex ante, it is unclear whether the audit choice signal will lead to an increase in debt levels for the treatment firms.

To test the relation between the audit choice signal and debt levels, we modify equation 1 by changing the dependent variable to total debt scaled by assets (*DEBT*). We continue to include the same control variables as before with one modification – we control for tangible assets rather than total assets following prior research (Harris and Raviv 1991; Berger and Udell 1994). Table IV, Panel A presents the results. We find 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., coef. = 0.009, t-stat. = 4.37) and the difference-in-difference specifications suggest that our treatment firms increase debt by 1.8 (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 £9,367, or 7.3% (£5,204, or 4.5%).

To refine our investigation, we then examine whether the increase in debt is driven by changes in long-term or short-term debt. Prior research finds that information asymmetry between companies and private debt markets affects the maturity of loans and that lenders use shorter term loan contracts to force more frequent renegotiation with borrowers known to be risky ex ante (Myers 1977; Barclay and Smith 1995; Ortiz-Molina and Penas 2008). Therefore, to the extent the audit choice signal provides incremental information to lenders about borrower type and the borrower's future prospects, we should observe that the increase in debt primarily occurs via increases in long-term debt rather than short-term debt.

Table IV, Panels B and C present the results for changes in long-term and short-term debt, respectively. Consistent with our expectations, we find that the entire increase in debt for our treatment firms is driven by changes in long-term debt. The coefficients for *POST_REG* (in column 1) and *POST_REG* × *TREATMENT_FIRM* (in columns 2 and 3) are similar to those in Panel A when total debt is the dependent variable. In contrast, we observe no change in the short-term debt of our treatment firms in any of the specifications. These results provide further evidence supporting our hypothesis that financial statement audits can serve as a signal about the quality of a firm's future prospects.

V.C. The Effect of Signaling on Investment-Growth Opportunity Sensitivity and Performance

We next examine whether the audit choice signal allows firms to respond more quickly to their growth opportunities and improve their overall operating performance. Our hypothesis is that the audit signal 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 audit signal 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-difference 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-difference 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 V, 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. = 0.006; t-stat. = 2.75) 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 5% level (or better). In economic terms, our regressions suggest that prior to the regulation a 1% increase in sales growth leads to a 0.4% increase in investment, and following the regulation a 1% increase in sales growth leads to a 1% increase in investment for the treatment firms. Thereby, the investment responsiveness increases by 0.6 percentage points following the regulation for the average treatment firm.

In Panel B, we examine whether the audit choice signal 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 increases the investment performance of firms that choose to voluntarily obtain financial statement audits.

For the remainder of our analyses, we tabulate only the difference-in-difference specification using firms obtaining voluntary audits as the control sample (in the interest of brevity). Nevertheless, we note that our inferences are robust to estimating regressions without any control sample and using firms obtaining mandatory audits as the control sample unless indicated otherwise.

V.D. Heterogeneity in the Treatment Effect

Having documented that the audit choice signal leads to an increase in investment and debt, we next examine whether the value of the signal is (i) greater for firms that are ex ante financially constrained, (ii) weaker for firms that increased their debt levels immediately before the regulation and (iii) weaker for firms that use alternative means to signal their future prospects. In the regressions below, we label the partitioning variables used in our analyses, *CX_VAR*, for expositional ease.

We begin by examining the role of financing constraints. Since our main hypothesis is that information in the audit choice increases access to finance, we argue that the audit signal should be more valuable for firms that are ex ante 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 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.

We do not use firm size to partition firms into constrained and unconstrained groups 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. public firms (in Compustat) and the index parameters are unlikely to apply for our sample of 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.

Table VI presents the investment results using firms obtaining voluntary audit as the control sample. Consistent with our prediction, the first column in the table shows that the coefficient for *POST_REG* × *TREATMENT_FIRM* × *CX_VAR* is positive and statistically significant at the 5% level (coef.=0.007, t-stat.=1.76). Further, we find that the coefficient for *POST_REG* × *TREATMENT_FIRM* is also positive and statistically significant at the 1% level (coef.=0.006, t-stat.=2.78). These coefficients indicate that both financially constrained and unconstrained treatment firms increase their investment following the regulation. However, financially constrained firm increase investment by a significantly larger magnitude than unconstrained firms. Specifically, the coefficients suggest that financially constrained (unconstrained) treatment firms increase their investment by 1.3 (0.6) percentage points following the regulatory change. In economic terms, these coefficients represent a £13,015 (£6,007) increase in investment from its conditional mean, which corresponds to a 153% (71%) increase in investment for the average financially constrained (unconstrained) treatment firm.

Next, we examine whether firms that increased their debt levels immediately before the regulation are relatively less affected by the regulatory change in 2004. Our intuition is that firms that are able to raise additional external financing before the regulation have either relationships with banks/external financiers or some other means to reduce information asymmetry and financing frictions. In addition, the creditworthiness and future prospects of these firms are likely to have been vetted by external financiers immediately before the regulation as part of the process of increasing debt. As a result, we expect firms that increased their debt levels immediately before the regulation to benefit less from the audit choice signal, on the margin. To test our prediction, we create an indicator variable that takes on the value of one (zero) if the firm increased (did not increase) its debt levels in the years two year before the regulation. We then augment equation 1 by including additional covariates based on the interaction between the above indicator variable and the *TREATMENT_FIRM* and *POST_REG* indicator variables.

Column 2 in Table VI presents the results from our analysis. Consistent with our prediction, we find that the coefficient for *POST_REG* × *TREATMENT_FIRM* × *CX_VAR* is negative and statistically significant at the 5% level (coef.=-0.006, t-stat.=-1.70). Further, we find that the coefficient for *POST_REG* × *TREATMENT_FIRM* is positive and statistically significant at the 1% level (coef.=0.010, t-stat.=4.50). These coefficients indicate that all our treatment firms increase their investment following the regulation. However, those firms that were able to access external financing immediately before the regulation increase investment by a significantly smaller magnitude than firms that did not do so.

Finally, we examine whether firms that resort to other avenues to signal their future prospects during the mandatory audit regime receive lower benefits from the audit choice signal. That is, to the extent firms use alternative means to signal their prospects, 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 the investment behavior of such firms. Private firms typically have fewer means to signal their prospects to external financiers than public firms (e.g., they cannot use dividends or repurchases as signaling mechanisms). Nevertheless, one potential mechanism through which private firms can signal their prospects is by hiring a high quality auditor (DeAngelo 1981). We construct an indicator variable that equals one if the firm hires one of the four biggest auditors in the world (*BIG4*) – KPMG, PwC, Deloitte, E&Y. These auditing firms are widely considered to provide the highest quality audits. To test our prediction, we augment equation 1 by including additional covariates based on the interaction between *BIG4*, *TREATMENT_FIRM* and *POST_REG*.

Column 3 in Table VI presents the results. Consistent with our prediction, we find that the coefficient for *POST_REG* × *TREATMENT_FIRM* × *CX_VAR* is negative and statistically significant at the 5% level (coef. = -0.009, t-stat. = -1.71) and the coefficient for *POST_REG* × *TREATMENT_FIRM* is positive and statistically significant at the 1% level (coef. = 0.009, t-stat. = 4.37). These coefficients indicate that the signaling benefit from obtaining a voluntary audit is lower for firms that hire high quality auditors and thus signaled their type via their auditor choice. However, we note that only about 8% of our sample firms hire a *BIG4* auditor, which suggests that this mechanism of signaling is prohibitively costly for most firms in our sample.

Finally, in Table VII, we reexamine the above heterogeneous treatment effects of the audit signal on debt rather than investment. Our dependent variable in these regressions is total debt, and the independent variables are as described earlier. Our coefficient of interest is the triple interaction term (i.e., *POST_REG* × *TREATMENT_FIRM* × *CX_VAR*) that captures any incremental effect of the regulation on the debt levels of treatment firms that (i) are financially constrained, (ii) increased their debt level immediately before the regulation, and (iii) employ a Big Four auditor. Column 1 in Table VII show that the coefficient for *POST_REG* × *TREATMENT_FIRM* × *CX_VAR* is positive and statistically significant at the 5% level (coef.=0.014, t-stat.=1.78) when the cross-sectional partition is financing constraints. This coefficient suggests that ex ante financially constrained firms increase their debt by a significantly larger magnitude than firms that are relatively less financially constrained. In economic terms, the coefficients suggest that financially constrained (unconstrained) firms that receive voluntary audits increase their debt by 2.7 (1.3) percentage points following the regulatory change that allows them to signal their future prospects via the audit. This coefficient corresponds to a £14,353 (or 11%) increase in debt for the average financially constrained treatment firm compared to a £6,910 (or 5.4%) increase for the average unconstrained firm.

Columns 2 and 3 show that the coefficient for *POST_REG* × *TREATMENT_FIRM* × *CX_VAR* is negative (as predicted), but statistically insignificant. These coefficients suggest that the treatment firms that increase leverage before the regulation and those that hire a Big Four auditor do not increase their debt levels any differently than firms that did not increase leverage before the regulation and those that do no hire a Big Four auditor, respectively. However, the coefficient for *POST_REG* × *TREATMENT_FIRM* is positive and statistically significant in all three regressions in Table VII, which suggests that the average treatment firm does indeed increase their debt levels after the audit regime shift in 2004.

VI. ADDITIONAL ANALYSES

VI.A. Dynamic Effect of the Audit Signal

To further corroborate our inferences, we examine the dynamic effects of the audit regime shift on the investment and debt levels of our treatment and control 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 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 investment and debt levels only when the regulatory change became effective (Bertrand and Mullainathan 2003).

Table VIII presents the results from both the investment and debt regressions. We find that the coefficient for *POST_REG [-1]* × *TREATMENT_FIRM* is statistically insignificant in both the investment and debt regressions (t-stat. = 0.50 and 0.82, respectively). However, 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 in both the investment and debt regressions. These results significantly strengthen our inference that the audit signal enables firms to increase investment by increasing debt capacity and reducing financing frictions.

VI.B. Analyses of Firms that Opt Out of Receiving Audits after 2004

Having documented that the audit regime shift facilitates the investment of firms that choose to obtain audits, we next 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. The costs of an audit are often non-trivial in terms of both the price paid to auditors and managerial time/effort devoted to engaging in and completing the audit. In fact, the burden of these costs led to the relaxation of the audit requirement for smaller private firms in the U.K. (Department of Trade and Industry 1985). Notwithstanding these costs, audits also provide important benefits. One of the primary benefits of an audit is greater access to, and a lower cost of, external finance (Minnis 2011). Accordingly, only those firms for which the benefits of an audit outweigh the costs undergo an audit in a voluntary audit regime. As a result, we predict that firms that opt out of the audit do not increase investment and debt. However, it is ex ante unclear whether these firms would decrease investment and/or debt once they stop receiving audits.

We test the above predictions using equation 1. However, we do not control for audit fees in these regressions because these "opt-out" firms do not obtain audits in the post-2004 regime. In addition, we re-match the opt-out firms to 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 IX, Panel A (B) presents the results for our investment (debt) 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 and after the regulation as the final control sample. Panel A shows that the coefficient for *POST_REG* (*POST_REG* × *TREATMENT_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. Panel B 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* × *TREATMENT_FIRM* is statistically insignificant in the remaining three regressions with control samples. These results suggest that while the opt-out firms decrease their debt levels after 2004, this decrease is in line with the trends in the economy for observably similar firms not affected by the audit regime shift.

VI.C. Can the Audit Choice Serve as a Costly Signal in the U.K. Private Firm Setting?

An important assumption in our theoretical framework is that in a mandatory audit regime, external financiers cannot ex ante distinguish between firms that would and would not obtain audits voluntarily. Only to the extent that external financiers fail to separate firms that would voluntarily obtain audits from firms that would not obtain audits absent the mandate, can the audit choice provide incremental information to external financiers and serve as a costly signal in the voluntary audit regime. We examine whether this condition is met in our setting by computing 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 takes on the value of one (zero) for firms that choose to obtain audits (opt out of the audit requirement) after 2004. The independent variables include the following firm characteristics that are likely to be associated with the probability of obtaining a voluntary audit: *ROA*, *LIQUIDITY*, *DEBT*, *SALES_GR*, *LSIZE*, the number of shareholder and directors on the board (*N_SHAREHOLDER*; *N_DIRECTORS*), *LAUDIT_FEE*, and *BIG4*.

Table X, 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 an audit, the model has limited explanatory power as observed by the pseudo R-squared (=5.7%). Table X, 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 34.8% of the firms that choose not to obtain audits are classified as obtaining an audit and 43.9% of the firms that voluntarily obtain audits are classified as not obtaining an audit by the model. Thus, these results suggest that external financiers are unlikely to have been able to ex ante distinguish between firms that would and would not obtain audits voluntarily.

VI.D. The Effect of the Regulation on Audit Assurance

A potential concern in our setting is that the audit regime shift could be correlated with changes in the effort exerted by auditors, and thus the assurance or verification value of the audit. In other words, it is plausible that 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, and as a result a change in the information obtained from an audit report. Any such changes in the assurance value of an audit could lead to more reliable financial statements, and thus greater access to credit. To investigate potential changes in audit assurance, we examine whether audit fees (our proxy for audit effort) paid by the treatment firms changed relative to that paid by our control firms following the regime shift. In untabulated results, we find no evidence of any change in audit fees following the regulation. Specifically, the difference-in-difference coefficient (*POST_REG* × *TREATMENT_FIRM*) is 0.001 (*t*-statistic = 0.06). This result helps 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 is likely to affect both treatment and control firms, and thus is filtered out by our difference-in-difference specification. Finally, we control for audit fees in all our regressions to further mitigate the effect of any changes in audit assurance on investment and debt.

VI.E. The Effect of Firm Size on Investment and Debt

To qualify for the audit exemption, firms have to 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 control firms that are smaller than the treatment firms and (ii) mandatory control firms that larger than the treatment firms. Given that the mandatory control sample is systematically larger than the treatment firms and the voluntary control sample is systematically smaller than the treatment firms, a monotonic relation between firm size and investment cannot explains our findings.

To further mitigate the concern that differences in firm size affect our inferences, we conduct two additional tests. First, 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. Second, 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.

VII. CONCLUSION

One of the most important factors in facilitating efficient investment is the flow of information between firms and capital suppliers. To mitigate financing frictions caused by information asymmetry, policy makers across the world have regulatory mandates for audited financial statements of public firms. Although the audit adds value to the disclosures, forcing firms to receive audits removes valuable information about the quality of a firm and its prospects that can be gleaned from observing a firm's choice to obtain an audit absent the mandate.

This paper uses a unique natural experiment to examine whether an audit mandate inhibits corporate investment by removing an important information signal in the audit choice. In particular, we examine a setting where private firms are initially required to obtain audits, but a regulatory change relaxed the mandate for a subset of firms. We exploit this audit regime shift to compare the change in investment behavior of firms that had the option to opt-out of the audit requirement but continue to receive audits voluntarily with the investment behavior of two control sample firms: (i) firms that voluntarily receive audits both before and after the regulation, and (ii) firms that mandatorily receive audits both before and after the regulation.

We find that our treatment firms (i.e., firms that switch from obtaining audits under a mandatory audit regime to doing so under a voluntary audit regime) significantly increase their investment, debt, the responsiveness of their investment to investment opportunities, and operating performance following the regulation. Combined, these results are consistent with the audit *choice* conveying information to capital providers (above that conveyed by the audit itself) about the quality of the firms and their prospects, which reduces financing frictions imposed by the audit requirement under the mandatory audit regime.

Our paper contributes to the literature by documenting a potential downside to regulation. In particular, policy makers generally pair disclosure requirements with an audit requirement to increase disclosure credibility and mitigate the impact of information asymmetry on firms' financing capacity and investment. However, a potential drawback of an audit mandate is that information about firms' prospects may be hidden by removing the observable audit choice absent the regulation. Although such a limitation of governance regulation is acknowledged in prior research, there is limited empirical evidence supporting such an argument. We show that regulation can have adverse effects by hiding valuable information relevant to creditors about the quality of firms.

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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 January 2004.

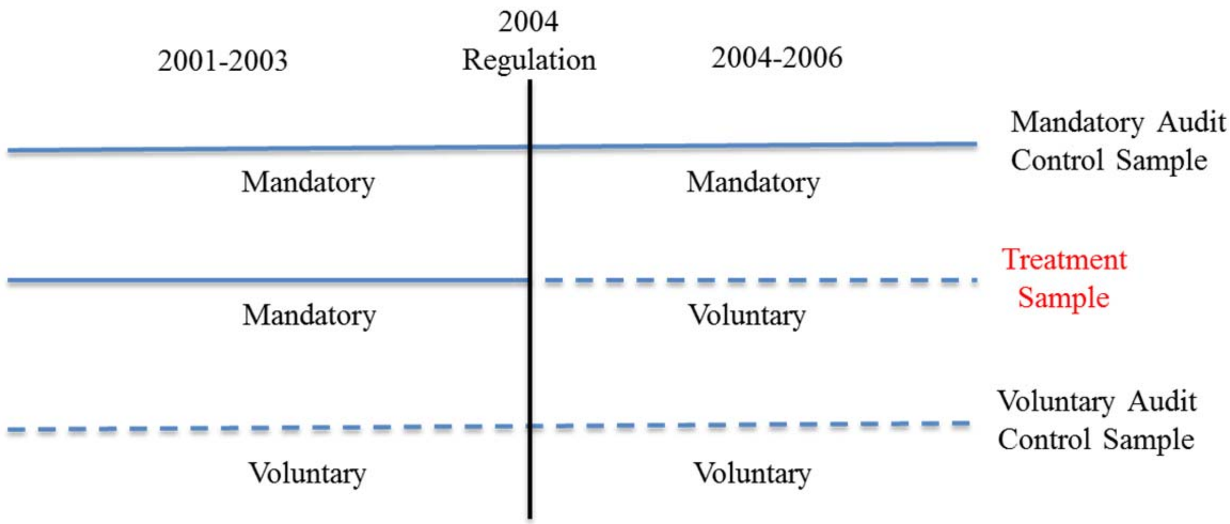

TABLE I
Sample Selection

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 II

Descriptive Statistics for the Treatment and Control Samples

This table presents the descriptive statistics for our matching variables for our treatment and control samples before and after the regulatory change in January 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. Panel B (C) 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 net 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.

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 II
(CONTINUED)

Panel B: 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)***					
INVESTMENT	0.004	0.056	-0.018	-0.001	0.009	0.007	0.072	-0.017	0.000	0.005	5,136
TOTAL_ASSETS	804	486	466	685	1,021	726	841	374	651	981	5,136
LSIZE	6.504	0.645	6.144	6.529	6.929	6.107	1.360	5.923	6.479	6.889	5,136
SALES	1,691	950	1,116	1,501	2,137	557	2,448	106	393	728	5,136
DEBT	0.122	0.208	0.000	0.003	0.156	0.125	0.218	0.000	0.000	0.182	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)***					
INVESTMENT	0.010	0.053	-0.010	0.000	0.011	0.007	0.070	-0.015	0.000	0.003	4,359
TOTAL_ASSETS	1,013	578	573	900	1,359	795	900	293	631	1,065	4,359
LSIZE	6.737	0.657	6.351	6.802	7.215	6.111	1.344	5.680	6.448	6.971	4,359
SALES	1,983	1,180	1,182	1,790	2,625	562	825	104	377	777	4,359
DEBT	0.129	0.216	0.000	0.005	0.170	0.121	0.218	0.000	0.000	0.138	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 II
(CONTINUED)

Panel C: 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)***			
INVESTMENT	0.006	0.059	-0.017	-0.001	0.010	0.006	0.062	-0.016	-0.002	0.011	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
DEBT	0.115	0.196	0.000	0.005	0.152	0.117	0.192	0.000	0.012	0.161	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)***			
INVESTMENT	0.011	0.055	-0.010	0.000	0.011	0.008	0.059	-0.013	-0.001	0.011	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
DEBT	0.126	0.209	0.000	0.006	0.172	0.118	0.190	0.000	0.011	0.170	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 III

Investment Regressions

This table presents the results from regressing firm investment on indicator variables for the post-regulation period, treatment firm, an interaction between the two and control variables. In the tables below, 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 variable, ***INVESTMENT***, is measured as the change in net 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 January 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 one-tailed *t*-test when a prediction is indicated and a two-tailed *t*-test otherwise.

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	+	0.005 ***	6.38				
POST_REG × TREATMENT_FIRM	+			0.008 ***	4.20	0.004 **	2.31
SALES_GR		0.005 ***	4.40	0.007 *	1.89	0.008 ***	4.41
LSIZE		-0.019 ***	-9.53	-0.015 ***	-8.46	-0.022 ***	-13.22
ROA		0.010 ***	2.86	0.008 ***	2.64	0.007 **	2.22
LIQUIDITY		0.004 ***	6.64	0.003 ***	6.16	0.004 ***	7.90
LAUDIT_FEE		-0.003	-1.31	-0.001	-0.46	-0.004	-1.37
Year Indicators		Not Included		Included		Included	
Firm Indicators		Included		Included		Included	
R-Squared		3.7%		2.6%		2.9%	
No. of Observations		13,875		18,990		24,156	

TABLE IV

Debt Regressions

Panel A (B; C) in this table presents the results from regressing firm total debt (long-term debt; short-term debt) on indicator variables for the post-regulation period, treatment firm, an interaction between the two and control variables. In the tables below, 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 variable, ***DEBT (LONG TERM DEBT; SHORT TERM DEBT)***, is measured as the total debt (long-term debt; short term debt) 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 January 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; ***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' debt levels. *,**,*** indicate statistical significance at the 10%, 5%, and 1% level, respectively, using a one-tailed *t*-test when a prediction is indicated and a two-tailed *t*-test otherwise.

Panel A: Total Debt

Dependent Variable:		***DEBT***					
Control Sample:		***No Control Sample***		***Voluntary Audit Sample***		***Mandatory Audit Sample***	
	Pr. Sign	Coefficient	*t*-Statistic	Coefficient	*t*-Statistic	Coefficient	*t*-Statistic
POST_REG	+	0.009 ***	4.37				
POST_REG × TREATMENT_FIRM	+			0.018 ***	4.15	0.010 ***	2.68
SALES_GR		-0.004 *	-1.88	-0.008	-1.36	-0.005	-1.59
LTANGIBLE_ASSETS		0.001	0.59	0.004 *	1.88	0.003 *	1.85
ROA		-0.036 ***	-5.53	-0.032 ***	-6.88	-0.039 ***	-6.65
LIQUIDITY		-0.001	-0.92	-0.001	-0.52	0.000	-0.26
LAUDIT_FEE		-0.004	-0.67	0.003	0.62	0.003	0.72
Year Indicators		Not Included		Included		Included	
Firm Indicators		Included		Included		Included	
R-Squared		6.3%		6.9%		7.2%	
No. of Observations		13,875		18,990		24,156	

TABLE IV
(CONTINUED)

Panel B: Long-term Debt

Dependent Variable:		***LONG TERM DEBT***					
Control Sample:		***No Control Sample***		***Voluntary Audit Sample***		***Mandatory Audit Sample***	
	Pr. Sign	Coefficient	*t*-Statistic	Coefficient	*t*-Statistic	Coefficient	*t*-Statistic
POST_REG	+	0.008 ***	5.57				
POST_REG × TREATMENT_FIRM	+			0.013 ***	4.10	0.007 ***	2.71
SALES_GR		0.000	0.24	0.001	0.12	0.000	-0.06
LTANGIBLE_ASSETS		0.001	0.81	0.003 *	1.76	0.003 **	2.44
ROA		-0.010 **	-2.42	-0.012 ***	-3.89	-0.013 ***	-3.07
LIQUIDITY		0.000	0.54	0.001	1.27	0.001	1.11
LAUDIT_FEE		-0.002	-0.45	0.005	1.40	0.001	0.33
Year Indicators		Not Included		Included		Included	
Firm Indicators		Included		Included		Included	
R-Squared		5.8%		6.5%		6.3%	
No. of Observations		13,875		18,990		24,156	

Panel C: Short-term Debt

Dependent Variable:		***SHORT TERM DEBT***					
Control Sample:		***No Control Sample***		***Voluntary Audit Sample***		***Mandatory Audit Sample***	
	Pr. Sign	Coefficient	*t*-Statistic	Coefficient	*t*-Statistic	Coefficient	*t*-Statistic
POST_REG	?	-0.001	-0.61				
POST_REG × TREATMENT_FIRM	?			0.002	1.28	0.000	-0.22
SALES_GR		-0.003 ***	-3.45	-0.003	-1.28	-0.003 *	-1.76
LTANGIBLE_ASSETS		0.000	-0.24	0.000	0.47	0.000	0.13
ROA		-0.016 ***	-5.72	-0.012 ***	-6.68	-0.017 ***	-6.98
LIQUIDITY		-0.001 ***	-2.86	-0.001 ***	-3.58	-0.001 ***	-2.58
LAUDIT_FEE		0.000	-0.15	-0.001	-0.72	0.002	1.25
Year Indicators		Not Included		Included		Included	
Firm Indicators		Included		Included		Included	
R-Squared		6.3%		5.9%		6.3%	
No. of Observations		13,875		18,990		24,156	

TABLE V

Investment-Growth Opportunity Sensitivity and Firm Performance Regressions

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. In the tables below, 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 variable in Panel A, ***INVESTMENT***, is measured as the change in net 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 January 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 one-tailed *t*-test when a prediction is indicated and a two-tailed *t*-test otherwise.

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 ***	3.70	0.004 **	2.19
SALES_GR × TREATMENT_FIRM				0.001	0.45	0.002	1.16
SALES_GR × POST_REG	+	0.006 ***	2.75	-0.001	-0.35	0.002	0.63
SALES_GR × POST_REG × TREATMENT_FIRM	+			0.010 ***	2.77	0.006 **	1.69
SALES_GR		0.004 ***	2.62	0.003 *	1.65	0.003 *	1.74
LSIZE		-0.019 ***	-9.60	-0.015 ***	-8.46	-0.023	-13.18
ROA		0.009 ***	2.78	0.007 ***	2.62	0.007 **	2.21
LIQUIDITY		0.004 ***	6.76	0.003 ***	6.24	0.004 ***	7.94
LAUDIT_FEE		-0.003	-1.29	-0.001	-0.51	-0.004	-1.42
Year Indicators		Included		Included		Included	
Firm Indicators		Included		Included		Included	
R-Squared		4.2%		2.8%		3.0%	
No. of Observations		13,875		18,990		24,156	

TABLE V
(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	+			0.012 *	1.71	0.018 ***	3.25
SALES_GR		-0.002	-0.41	0.048 ***	3.66	0.007	1.56
LSIZE		0.017 *	1.77	0.018 **	2.05	0.021 ***	2.72
LIQUIDITY		-0.011 ***	-4.61	-0.013 ***	-6.13	-0.010 ***	-6.07
LAUDIT_FEE		0.004	0.39	-0.004	-0.44	0.004	0.56
Year Indicators		Not Included		Included		Included	
Firm Indicators		Included		Included		Included	
R-Squared		3.3%		4.5%		2.8%	
No. of Observations		13,875		18,990		24,156	

TABLE VI

Heterogeneous Treatment Effects in the Investment Regressions

This table presents the results from regressing firm 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, ***INVESTMENT***, is measured as the change in net 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 January 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); ***CX_VAR*** represents one of the following variables: (i) ***FIN_CONSTRAINED***, which is an indicator variable that equals one for firms in the bottom tercile of the age distribution of our sample firms, (ii) ***PREREG_INC_DEBT***, which is an indicator variable that equals one for firms that increase their debt levels in the two years immediately before the regulation, or (iii) ***BIG4***, which 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; ***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 one-tailed *t*-test when a prediction is indicated and a two-tailed *t*-test otherwise.

Dependent Variable:		*INVESTMENT*						
Cross-Sectional Partitioning Variable (CX_VAR):		***FIN_CONSTRAINED***			***PREREG_INC_DEBT***		***BIG4***	
	Pr. Sign	Coefficient	*t*-Statistic	Pr. Sign	Coefficient	*t*-Statistic	Coefficient	*t*-Statistic
POST_REG × TREATMENT_FIRM		0.006 ***	2.78		0.010 ***	4.50	0.009 ***	4.37
POST_REG × CX_VAR		0.001	0.32		-0.004	-1.43	0.008	1.16
POST_REG × TREATMENT_FIRM × CX_VAR	+	0.007 **	1.76	-	-0.006 **	-1.70	-0.009 **	-1.71
SALES_GR		0.007 **	2.04		0.007 *	1.89	0.007 *	1.89
LSIZE		-0.015 ***	-8.61		-0.015 ***	-8.46	-0.014 ***	-8.45
ROA		0.007 ***	2.60		0.008 ***	2.68	0.007 ***	2.64
LIQUIDITY		0.003 ***	6.14		0.003 ***	6.12	0.003 ***	6.16
LAUDIT_FEE		-0.001	-0.55		-0.001	-0.47	-0.001	-0.45
Year Indicators		Included			Included		Included	
Firm Indicators		Included			Included		Included	
R-Squared		2.6%			2.6%		2.6%	
No. of Observations		18,990			18,990		18,990	

TABLE VII

Heterogeneous Treatment Effects in the Debt Regressions

This table presents the results from regressing firm debt 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***, is measured as the total debt 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 January 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); ***CX_VAR*** represents one of the following variables: (i) ***FIN_CONSTRAINED***, which is an indicator variable that equals one for firms in the bottom tercile of the age distribution of our sample firms, (ii) ***PREREG_INC_DEBT***, which is an indicator variable that equals one for firms that increase their debt levels in the two years immediately before the regulation, or (iii) ***BIG4***, which 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; ***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 one-tailed *t*-test when a prediction is indicated and a two-tailed *t*-test otherwise.

Dependent Variable:		***DEBT***						
Cross-Sectional Partitioning Variable (CX_VAR):		***FIN_CONSTRAINED***			***PREREG_INC_DEBT***		***BIG4***	
	Pr. Sign	Coefficient	*t*-Statistic	Pr. Sign	Coefficient	*t*-Statistic	Coefficient	*t*-Statistic
POST_REG × TREATMENT_FIRM		0.013 ***	2.73		0.020 ***	4.69	0.018 ***	4.35
POST_REG × CX_VAR		-0.003	-0.38		0.003	0.24	0.004	0.21
POST_REG × TREATMENT_FIRM × CX_VAR	+	0.014 **	1.78	-	-0.006	-0.50	-0.006	-0.80
SALES_GR		-0.008	-1.29		-0.009	-1.36	-0.009	-1.36
LTANGIBLE_ASSETS		0.004 *	1.86		0.004 *	1.88	0.004 *	1.93
ROA		-0.032 ***	-6.88		-0.032 ***	-6.85	-0.032 ***	-6.89
LIQUIDITY		-0.001	-0.55		-0.001	-0.53	-0.001	-0.56
LAUDIT_FEE		0.003	0.54		0.003	0.62	0.003	0.58
Year Indicators		Included			Included		Included	
Firm Indicators		Included			Included		Included	
R-Squared		7.1%			6.9%		6.9%	
No. of Observations		18,990			18,990		18,990	

TABLE VIII

Dynamic Effect of the Audit Signal on Investment and Debt

This table presents the results from regressing firm investment or debt 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 variable, ***INVESTMENT*** (***DEBT***), is measured as the change in net fixed assets (debt) scaled by lag total assets. The independent variables are defined as follows: ***POST_REG [-1]*** is an indicator variable that equals one for fiscal years ending between January 30, 2003 and January 30, 2004; ***POST_REG [0]*** is an indicator variable that equals one for fiscal years ending between January 30, 2004 and January 30, 2005; ***POST_REG [1]*** is an indicator variable that equals one for fiscal years ending between January 30, 2005 and January 30, 2006; ***POST_REG [2]*** is an indicator variable that equals one for fiscal years ending between January 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 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 one-tailed *t*-test when a prediction is indicated and a two-tailed *t*-test otherwise.

Dependent Variable:		***INVESTMENT***		***DEBT***	
	Pr. Sign	Coefficient	*t*-Statistic	Coefficient	*t*-Statistic
POST_REG [-1] × TREATMENT_FIRM	*0*	0.001	0.50	0.003	0.82
POST_REG [0] × TREATMENT_FIRM	+	0.009 ***	3.37	0.019 ***	3.82
POST_REG [1] × TREATMENT_FIRM	+	0.008 ***	2.62	0.020 ***	3.25
POST_REG [2] × TREATMENT_FIRM	+	0.010 ***	3.01	0.027 ***	3.55
SALES_GR		0.007 *	1.89	-0.008	-1.32
LSIZE / LTANGIBLE_ASSETS		-0.015 ***	-8.46	0.004 *	1.87
ROA		0.007 ***	2.61	-0.032 ***	-6.88
LIQUIDITY		0.003 ***	6.16	-0.001	-0.51
LAUDIT_FEE		-0.001	-0.48	0.003	0.56
Year Indicators		Included		Included	
Firm Indicators		Included		Included	
R-Squared		2.6%		6.9%	
No. of Observations		18,990		18,990	

TABLE IX

Analyses of Firms that Opt Out from Obtaining Audits

Panel A (B) in this table presents the results from regressing firm investment (total debt) on indicator variables for the post-regulation period, opt-out firm, sales growth, interaction terms between these three variables 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 '***Voluntary Opt-Out Control Sample***' comprises of firms with sales less than £1 million and assets less than £1.4 million prior to 2004 and they opt out of the audit requirement throughout our sample period. The dependent variable in Panel A, ***INVESTMENT***, is measured as the change in net fixed assets scaled by lag total assets. The dependent variable in Panel B, ***DEBT***, is measured as total debt outstanding scaled by total assets. The independent variables are defined as follows: ***POST_REG*** is an indicator variable that equals one for fiscal years ending after January 30, 2004; ***OPT_OUT_FIRM*** is an indicator variable that equals one for firms that opted out from the audit requirement after January, 2004. These firms obtain a mandatory audit before the regulation and opt-out of the audit after the regulation. They 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; 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: 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	0.000	-0.13						
POST_REG × OPT_OUT_FIRM			0.000	0.12	0.001	0.26	0.002	0.89
SALES_GR	0.005 **	2.23	0.004 **	2.51	0.006 ***	3.51	0.003 *	1.78
LSIZE	-0.022 ***	-5.63	-0.023 ***	-6.99	-0.020 ***	-6.66	-0.029 ***	-7.19
ROA	0.015 **	2.41	0.012 **	2.46	0.013 **	2.52	0.002	0.40
LIQUIDITY	0.008 ***	5.88	0.005 ***	6.37	0.007 ***	6.83	0.008 ***	8.12
Year Indicators	Not Included		Included		Included		Included	
Firm Indicators	Included		Included		Included		Included	
R-Squared	3.3%		2.8%		2.9%		3.5%	
No. of Observations	5,756		10,322		10,724		9,004	

TABLE IX
(CONTINUED)

Panel B: 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	0.40	0.006	1.07	-0.006	-1.06
SALES_GR	0.000	-0.16	-0.001	-0.45	-0.001	-0.36	-0.003	-1.02
LTANGIBLE_ASSETS	0.007 **	2.39	0.007 **	2.54	0.006 ***	3.24	0.004	1.58
ROA	-0.056 ***	-6.08	-0.056 ***	-6.68	-0.051 ***	-7.08	-0.048 ***	-6.54
LIQUIDITY	0.003 *	1.67	0.000	0.09	0.002	1.04	0.000	0.22
Year Indicators	Not Included		Included		Included		Included	
Firm Indicators	Included		Included		Included		Included	
R-Squared	5.0%		4.8%		5.1%		5.3%	
No. of Observations	5,756		10,322		10,724		9,004	

TABLE X

Classification Errors from an Audit Prediction Model

Panel A in this table 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 January 30, 2004 (i.e., before the regulation) and choose to continue receiving audits (voluntarily) after the regulation. In the table below ***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; ***SALES_GR*** is the percentage change in sales; ***LSIZE*** is the natural log of total assets; ***N_SHAREHOLDERS*** is the number of shareholders owning the firm; ***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 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 January, 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.

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

Dependent Variable:	***Indicator Variable for Firms that Opt-Out of the Audit after 2004***	
	Coefficient	*z*-Statistic
ROA	0.105	0.78
LIQUIDITY	-0.008	-0.62
DEBT	0.136	0.92
SALES_GR	0.104 ***	2.81
LSIZE	0.337 ***	6.80
N_SHAREHOLDERS	0.041 ***	3.14
N_DIRECTORS	0.148 ***	5.47
LAUDIT_FEE	0.551 ***	8.40
BIG4	2.201 ***	5.24
INTERCEPT	-2.891 ***	-8.65
Pseudo R-Squared	5.7%	
No. of Observations	4,854	

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	0.525	91.3%	4.2%
25th Percentile	0.637	62.5%	20.8%
Median/Mean	0.710	34.8%	43.9%
75th Percentile	0.781	12.2%	69.6%
95th Percentile	0.916	0.5%	93.4%
N	4,854	1,405	3,449