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The Joint Effect of Investor Protection Regimes and Big 4/non-Big 4 Auditors on Audit Quality: Evidence from Initial Public Offerings in the U.S. Market.

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Introduction

In this article, we investigate whether audit quality is jointly affected by both the investor protection regime and the use of a Big 4/non-Big 4 auditor. We first examine whether there are differences in audit quality of initial public offering (IPO) firms before their offering (pre-IPO registration statement) and immediately after their offering (post-IPO financial statements). We then examine whether there are differences in audit quality between the pre-IPO and post-IPO period that vary with the Big 4/non-Big 4 auditor characteristic.

To better understand whether audit quality is jointly affected by the level of investor protection and the firm's choice of auditor size, we use financial restatements as a measure of audit quality in a within-country setting of U.S. firms before and after their IPO. Our examination of the same firm's pre-IPO and post-IPO financial statements utilizes a setting that naturally creates two different investor protection regimes (Venkataraman, Weber, and Willenborg, 2008). Our focus on pre-IPO and post-IPO financial statements allows us to isolate the effects of investor protection regimes within the same country. Additionally, our comparison of Big 4 versus non-Big 4 audit quality in the U.S. market should be of interest to the PCAOB whose inspection cycle depends on auditor size, and to the SEC whose inspection efforts are indirectly influenced by auditor size.

Our article makes two primary contributions to the accounting literature. First, we provide evidence that audit quality is lower in the pre-IPO period, based on finding a higher likelihood of restatement pre-IPO. This result is in contrast to Venkataraman et al. (2008) who conclude audit quality is higher in the pre-IPO period, based on higher audit fees and lower discretionary accruals pre-IPO. However, the results of Venkataraman et al. (2008) may reflect increased pre-IPO auditor conservatism from higher auditor insurance costs, and from auditors systematically decreasing client accrual magnitude. Although the 1933 Act imposes additional auditor legal liability in the pre-IPO period, our results suggest that restatements are more likely under the 1933 Act, and consequently audit quality is also lower.

Second, we further our understanding of the joint effect of Big 4/non-Big4 auditors and investor protection regimes on audit quality. We find that clients of Big 4 auditors are less likely to have a restatement relative to clients of non-Big 4 auditors in the pre-IPO year (when investor protection is stronger). We also find that clients of non-Big 4 auditors are more likely to have a restatement in the pre-IPO year than the post-IPO year. In contrast, for clients of the Big 4 there is no difference in the likelihood of restatement between the pre-IPO year and the post-IPO year. We do not find evidence of a joint effect of Big4/non-Big 4 auditors and investor protection regimes on audit quality.

The remainder of the article is organized as follows: section 2 discusses the background and hypothesis development; section 3 covers the research design; the samples and descriptive statistics are presented in section 4; regression results are reported in section 5; and section 6 concludes.

Background

Auditor Quality and the IPO Setting

High audit quality can be thought of as providing greater assurance of high financial reporting quality. However, measuring audit quality empirically is challenging. Empirical research on the subject of audit quality has commonly used

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auditor tenure, auditor litigation, auditor fees, discretionary accruals, and auditor restatements to proxy for audit quality (Chan, Mo, and Zhang, 2020; Boskou, Kirkos, and Spathis, 2019; Chen, Ke, Wu, and Yang, 2018).

Research by Rezaee et al. (2016) indicates that audit quality is higher the second year of an engagement, relative to the first year, and that audit quality continues to increase in the third year before eventually reversing and resulting in lower audit quality. DeFond and Zhang (2014) note that contrary to the belief of regulators, longer auditor tenure can actually lead to higher audit quality.

Auditor litigation is a second method of measuring audit quality, as research by Feng and Fuerman (2019) defines low quality audits as those conducted by CPA firms that have been named defendants in securities class action litigation, with firms facing governmental prosecution related to the audit being the lowest ranked in terms of audit quality.

Audit fees are another commonly used measure of audit quality, with researchers arguing that audit fees proxy for audit quality because there is an expectation that the fee is correlated with audit effort. As well, it is not likely that the auditor can impose a high audit fee without there being a client demand for a high-quality audit. While excellent audit fee models exist, the weakness of the audit fee model is that the auditor also may impound a risk premium into the fee that does not necessarily correlate with audit quality, leading to the possibility that audit fees are a noisy measure of audit quality.

Discretionary accruals also have been used in the audit literature to proxy for audit quality, but these measures are plagued by issues stemming from auditors having less influence over accruals. As well, discretionary accrual measures are generally less powerful than proxies such as restatements, given that accrual measures do not capture GAAP violations and can be less serious audit issues, relative to restatements.

Restatements are discussed by DeFond and Zhang (2014) as having lower measurement error, compared to discretionary accruals. When companies have a subsequent restatement of originally audited financial statements, it is a strong indication that the original audit of those financial statements was low quality (Palmrose and Scholz, 2004; Kinney, Palmrose, and Scholz, 2004). Furthermore, DeFond and Zhang (2014) argue that restatements suggest that auditors mistakenly issued unqualified opinions on materially misstated financial statements. Francis and Michas (2013) discuss that a restatement is a strong indication of audit failure because the auditors did not enforce the correct application of GAAP at the time the financial statements were originally issued. Given the reduced measurement error of restatements, we focus our testing of audit quality using restatements as our empirical proxy.

The IPO Setting

Venkataraman et al. (2008) compares the audit quality of pre-IPO firms to the audit quality of the same firm post-IPO. This research design allows them to test for differences in audit quality likely caused only by differences in the investor protection regimes the auditor is subject to pre-IPO and post-IPO.

The potentially increased litigation risk to the auditor in the pre-IPO period is theoretically a result of additional liability that the 1933 Act imposes on auditors. The 1933 Act was enacted by the United States Congress for the primary purpose of increasing disclosure to prospective investors. In accordance with the 1933 Act, pre-IPO firms must provide a registration statement which includes a detailed prospectus and audited financial statements, and the audit firm has the responsibility of auditing up to three years of financial statements.

However, it is uncertain if there is any practical, actual increased auditor litigation risk associated with the 1933 Act. This uncertainty is partly because of the judicially created tracing requirement. Shareholders who purchase shares in an IPO must prove—and they sometimes find this impossible—that they bought their shares in the IPO and not in the subsequent secondary trading market (Sale, 2000).

In addition, in almost all empirical research analyzing which private securities class actions included auditor defendants or which SEC Accounting and Auditing Enforcement Releases included auditor defendants, bankruptcy of the auditee is significant and positively associated with the inclusion of the auditor as a defendant (Fuerman, 2006, 41). This result demonstrates that when an auditee lacks assets to satisfy the plaintiffs, the plaintiffs more aggressively target additional defendants. This action is often called the "deep pockets theory." In an IPO, there exist two additional defendants beyond those normally available: the law firm that drafted the IPO documents and the underwriter. Both are typically asset-rich defendants. Their presence reduces the motivation to name the auditor a defendant. Thus, in all of the empirical research

focusing on the IPO market, the IPO variable has not been found to be significantly associated with the inclusion of the auditor as a defendant (Fuerman, 2006).

In addition, the auditor is required under the 1933 Act to provide a comfort letter to the underwriter which provides assurance to investors that the financial statements are fairly stated and are prepared in accordance with prevailing accounting standards. Legal exposure to the auditor is also higher under the 1933 Act because the auditor is required to demonstrate that they exercised due diligence as opposed to the lower threshold of good faith under the 1934 Act. In other words, in the course of litigation brought under the 1933 Act, the auditor is liable for ordinary negligence. In contrast, litigants must demonstrate that the auditor has demonstrated grossly negligent behavior when bringing suit under the 1934 Act.

Venkataraman et al. (2008) conclude that audit quality is higher in the pre-IPO period based on their finding that audit fees are higher and signed discretionary accruals are negative in the pre-IPO period. They conclude that the stricter investor protection regime that pre-IPO firms are subject to reduces the discretion an auditor will cede to managers in this setting. They articulate that when firms register to go public, they file a registration statement and prospectus under the 1933 Act, which provides more legal protection to investors than when firms file financial statements under the 1934 Act after they have gone public.

However, there are a large number of studies that argue that management's incentive to report strong financial performance in the pre-IPO period outweighs auditor demand for conservative reporting. Friedlan (1994) provides evidence that IPO firms make income-increasing discretionary accruals in their financial statements in the pre-IPO period because reporting strong financial performance should lead to a better IPO offering price. Friedlan (1994) discusses the unique nature of the IPO, which differs from seasoned equity offerings where investors are likely able to have an assortment of information available to come to an investment decision. During the IPO however, the main source of information comes from the financial statements presented in the prospectus.

Teoh, Wong, and Rao (1998) argue that the incentives to manage earnings are strong when the firm is planning to sell shares to the market as an IPO because doing so can lead to higher valuation and increased proceeds from the offering. Furthermore, there is often a high degree of information asymmetry between the issuer and the investors prior to the IPO which creates an opportunity to inflate performance on financials.

Efendi, Srivastava, and Swanson (2007) find that in a sample of firms raising additional equity capital, firms that raised more equity capital were also more likely to subsequently restate their financial statements. This result is consistent with the argument that the amount of capital a firm is able to raise is largely dependent on the firm's financial performance (e.g., accounting numbers). As such, the incentive to report strong financial performance to increase proceeds is further evident.

Fedyk, Singer, and Soliman (2017) argue that science, technology, engineering, and math (STEM) IPO firms are likely more incentivized to manage revenues relative to non-STEM IPO firms who are likely more incentivized to manage earnings. Consistent with their predictions, they find evidence of STEM IPO firms managing revenues and non-STEM IPO firms managing earnings.

Because of the competing managerial incentives for aggressive financial reporting, and the increased litigation risk auditors face, the IPO setting provides an interesting setting to test which effect dominates. Auditors are of paramount importance in the IPO process because information asymmetry is high (Leland and Pyle, 1977). This need should lead to higher demand for audit quality, in order to improve contract efficiency and resource allocation (DeFond and Zhang, 2014). However, prior studies find mixed results when comparing audit quality (earnings quality and audit fees) between the pre-IPO and post-IPO periods (e.g., Friedlan, 1994; Venkataraman et al., 2008; Fedyk et al., 2017). Additionally, whether Big 4 auditors provide a higher quality audit in the pre-IPO period is unclear. While auditors are akin to gatekeepers in the IPO setting (Leone, Rice, Weber, and Willenborg, 2012), Big 4 auditors may provide a higher quality audit pre-IPO because they have deeper pockets and greater reputational capital at risk.

Hypotheses Development

Prior studies provide mixed results about differences in audit quality in the pre-IPO and post-IPO period. Venkataraman et al. (2008) conclude that auditors provide higher quality audits of pre-IPO financial statements, finding that audit fees are higher and signed discretionary accruals are negative in pre-IPO period. They argue that increased

litigation exposure to an auditor in the pre-IPO period leads the auditor to allow less discretion to managers than in the post-IPO period. However, Venkataraman et al. (2008) examine firms that went public between January 1, 2000, and December 31, 2002. This period overlaps with the dotcom bust which began in March of 2000 and ended in October of 2002. Their results therefore might be driven by auditors demanding more conservative reporting due to the dotcom bust and not the increased litigation regime of the 1933 Securities Act.

Alternatively, Friedlan (1994) provides evidence of income-increasing discretionary accruals in the financial statements of pre-IPO firms. He argues that issuers have incentives to report opportunistically due to the importance of the financial statement information in setting IPO offering prices. Relative to firms that have already gone public, little may be known about pre-IPO firms which leads to investors having to rely more on the financial statements of pre-IPO firms. Furthermore, Efendi et al. (2007) find a positive association between the amount of capital raised in equity offerings and the likelihood of subsequently restating financial statements. Fedyk et al. (2017) find evidence of STEM IPO firms managing revenues while non-STEM IPO firms manage earnings, consistent with their respective incentives.

Based on the prior literature, we argue that there is a higher likelihood of restating client financial statements in the pre-IPO period because the issuer's incentive to report better performance appears to outweigh the investor protections in place. While evidence to the contrary exists in Venkataraman et al. (2008), concerns about the period and sample firms used lead us to believe that investor protections in the pre-IPO period may not be sufficient to increase audit quality and that issuer incentives dominate. As such, we test the following hypothesis presented in alternative form:

H1*. Client restatements are more likely in the pre-IPO period compared to the post-IPO period, ceteris paribus.*

We next examine whether the likelihood of client restatement differs between Big 4 and non-Big 4 auditors, irrespective of the investor protection regime. Auditor incentives may vary between Big 4 and non-Big 4 auditors, with Big 4 auditors being likely more sensitive to the cost of client misreporting as they have deeper pockets and greater reputational capital at risk. That is, they are less likely to acquiesce to managerial opportunism and are therefore more likely to provide higher audit quality irrespective of the investor protection regime.

In contrast, non-Big 4 auditors are likely to be less sensitive to client misreporting and are more likely to cede to management demand to inflate financial performance because they have less reputation capital at risk and seek to avoid being dismissed by the client. As such we test the following hypothesis in alternative form:

H2. *Irrespective of the investor protection regime, the likelihood of a restatement is lower when IPO clients have a Big 4 auditor versus a non-Big 4 auditor, ceteris paribus.*

In our third hypothesis we examine whether the likelihood of client restatement differs between the pre-IPO year and the post IPO-year and between Big 4 and non-Big 4 auditors. On the one hand, it may be that audit quality is higher when the country's investor protection regime is stronger but only when client firms have a Big 4 auditor. Differences in the level of investor protection regimes may not affect the audit quality of firms with a non-Big 4 auditor because non-Big 4 auditors have less reputation capital at risk compared to Big 4 auditors.

However, the Big 4 fee premium may decrease as the investor protection regime increases. While non-Big 4 auditors generally have a higher audit failure rate than Big 4 auditors, non-Big 4 auditors may charge larger fees and exert more effort in a stronger legal regime to compensate for the higher expected legal liability costs.

Because of the offsetting possibilities, our prediction for the joint effect of investor protection regime and the choice of Big 4/non-Big 4 auditor on audit quality is non-directional. We test the following hypothesis in null form:

H3. *Between Big 4 and non-Big 4 auditors, there is no difference in the likelihood of restatement among IPO firms when the investor protection regime shifts from a strong regime to a weak regime, ceteris paribus.*

Method

Sample

We use a sample of companies going public from January 1, 2002, to December 31, 2012. We focus our analysis on IPO firms between the years 2002-2012 because numerous titles of the Jumpstart Our Business Act (JOBS Act) went into effect after 2012, which drastically changed the litigation environment for IPO firms. For example, the JOBS Act relieves companies with less than $1 billion in revenue (emerging growth companies) from certain key regulatory and

disclosure requirements in the registration statement. To focus our analysis on a single country with a differing level of litigation risk before and after the IPO, we therefore conduct our testing solely on IPO firms from 2002–2012.

We identify 1,584 firm-commitment IPOs on the SDC database with valid CUSIPs. Table 1 displays our sample selection. Auditor information is identified using the Audit Analytics opinion database. Financial statement information is obtained from COMPUSTAT. We eliminate 230 ADRs and unit IPOs following other empirical IPO studies (e.g., Li, Lin, and Robinson, 2016; Loughran and Ritter, 2004) and 353 IPO firms with missing COMPUSTAT data. The sample selection criteria yields 1,001 unique IPO firms. We match each firm-commitment IPO observation (year *t*) to the pre-IPO period (year *t*-1) to compare audit quality between the pre-IPO year and the post-IPO year of the same firm. Our final sample for the restatement analysis has 2,002 firm-year observations. [See Table 1, pg. xx]

Research design

Our research design exploits the different investor protection regimes inherent in the pre-IPO year (*t*-1) when auditors are subject to litigation under the 1933 Act and the post-IPO year (*t*) when they are subject to the 1934 Act. Note that the post-IPO year includes the IPO. Our pre and post research design matches firms on relevant observable dimensions except for the different investor protection regimes of the pre-IPO year under the 1933 Act and the post-IPO year under the 1934 Act.

In addition, we also empirically test for possible endogeneity of the client choice of Big 4 auditor by using a two-stage model based on the Heckman (1979) two-stage approach to estimate the selection adjustment variable (inverse Mill's ratio). We model our first stage probit regression for the client choice of auditor similar to the model used in Eshleman and Guo (2014) as follows:

$$\text{PROBIT}[BIGN = 1] = \beta_0 + \beta_1 CURR + \beta_2 ATURN + \beta_3 LEV + \sum CONTROLS + \varepsilon \quad (1),$$

Where *BIGN* is equal to a value of 1 if a client's auditor is one of the Big N auditors, otherwise 0.[1] *CURR* is the current ratio. *ATURN* is the ratio of sales to average total assets. *LEV* is firm leverage. All the control variables in the second stage model are included in the model. We calculate the inverse Mill's ratio (*IMR*) using the parameters from the first stage equation (1). In the second stage, we estimate our restatement model for the sample by including *IMR*. A significant *IMR* coefficient in the second stage would indicate that selection bias exists (Wooldridge 2016, 556). To test hypotheses 1 and 2, we estimate the second stage, by using the following probit regression model, similar to Francis, Michas, and Yu (2013):

$$\begin{aligned}\text{PROBIT}[RESTATE = 1] &= \beta_0 + \beta_1 PREIPO + \beta_2 BIGN + \beta_3 SIZE + \beta_4 ROA + \beta_5 LOSS + \beta_6 OFFICESIZE \\ &+ \beta_7 SECREGFAR + \beta_8 LNPOPULATION + \beta_9 SECONDTIER + \beta_{10} AUDITOR\Delta \\ &+ \beta_{11} LNAUDITFEE + \beta_{12} LNNONAUDITFEE + \beta_{13} INFLUENCE + \beta_{14} IMR \\ &+ \text{Year fixed effects} + \text{Industry fixed effects} \\ &+ \varepsilon \qquad (2),\end{aligned}$$

where the dependent variable, *RESTATE* is equal to 1 if a firm-year is subsequently restated and 0 otherwise. This variable measures a misstatement as the fiscal year that is subsequently restated, and not the fiscal year when the restatement was announced; thus, it measures audit quality of the restated fiscal year. The variable *PREIPO* is an indicator variable that takes a value of 1 if the period of the observation is the pre-IPO year (*t*-1), and 0 for the year of the IPO (*t* - the post-IPO year). The coefficient on *PREIPO* (β_1) is used to test hypothesis 1. We expect that β_1 is positive because hypothesis 1 predicts that the likelihood of a restatement is greater in the pre-IPO year than the post-IPO year. *BIGN* takes a value of 1 if a client's auditor is a Big N audit firm, and otherwise 0. The coefficient on *BIGN* (β_2) is used to test hypothesis 2. We expect that β_2 is negative because hypothesis 2 predicts that the likelihood of a restatement is lower for a Big N client than a non-Big N client.

According to a report from the Center for Audit Quality on financial restatement trends in the United States over the years 2003-2012, companies restating their financial statements tend to be smaller and less profitable (Scholz, 2014). To control for these characteristics of restatement companies, we include *SIZE*, *ROA*, and *LOSS* variables. *SIZE* is the

[1] At the beginning of our sample time period (January 1, 2002), Arthur Andersen still existed and the Big 4 at that time was the Big 5.

natural log of total assets. *ROA* is calculated as income before extraordinary items divided by average total assets. *LOSS* is an indicator variable that takes a value of 1 if net income is negative, and otherwise 0.

Following Francis et al. (2013), we measure *OFFICESIZE* as the number of U.S. SEC registrants audited by each accounting firm's engagement office for a given fiscal year. Francis et al. (2013) find that larger Big N offices provide higher quality audits relative to smaller Big N offices. However, López and Peters (2012) provide evidence of a positive association between abnormal accruals and the level of auditor workload compression. This result suggests that when a large number of IPO audits are conducted in a local office with a large number of clients it may impair audit quality and increase management's ability to manipulate reported earnings. As such our expectation for office size is non-directional.

To control for the possibility that distance from an SEC office is associated with the likelihood of restatement, we use an indicator variable, *SECREGFAR*, if an auditor office is located in the same city as one of the SEC regional offices, and 0 otherwise.[2] *LNPOPULATION* is the natural log of MSA size to control for the likelihood that firms in large cities possess higher accounting expertise. We predict a negative association between *LNPOPULATION* and *RESTATE*. *SECONDTIER* is an indicator variable that takes the value of 1 if a client's auditor is either BDO Seidman LLP or Grant Thornton LLP, 0 otherwise. We expect that *SECONDTIER* is negatively related to restatements. *AUDITORΔ* is an indicator variable that is equal to 1 if an auditor changes from the previous year, and 0 otherwise. This variable controls for the potential effect of a new auditor requiring a client restate their financials.

LNAUDITFEE is the natural log of a client's audit fees and *LNNONAUDITFEE* is the natural log of a client's non-audit fees. These two variables control for the economic bond between clients and auditors which may reduce auditor's objectivity and professional skepticism (Kinney et al., 2004). *INFLUENCE* measures a client's importance to an office, and controls for the size of a client relative to the size of an audit office. Following Reynolds and Francis (2000), we use the ratio of a client's total fees relative to aggregate client fees in an audit office.

Lastly, year and industry fixed effects control for the systematic effects of time period and industry characteristics on the probability of a restatement. We also use a two-way clustering approach in which standard errors are clustered by audit firm and two-digit SIC industry code to address correlation among errors within audit firms and within industry (Petersen 2009).

To test hypothesis 3, we estimate equation (3):

$$\begin{aligned}\text{PROBIT}[RESTATE = 1] &= \beta_0 + \beta_1 PREIPO + \beta_2 BIGN + \beta_3 PREIPO * BIGN + \beta_4 SIZE + \beta_5 ROA \\ &+ \beta_6 LOSS + \beta_7 OFFICESIZE + \beta_8 SECREGFAR + \beta_9 LNPOPULATION + \beta_{10} SECONDTIER \\ &+ \beta_{11} AUDITOR\Delta + \beta_{12} LNAUDITFEE + \beta_{13} LNNONAUDITFEE + \beta_{14} INFLUENCE \\ &+ \beta_{15} IMR + \text{Year fixed effects} + \text{Industry fixed effects} \\ &+ \varepsilon \qquad (3),\end{aligned}$$

where we add the interaction term, *PREIPO*BIGN*, to measure the joint effect of investor protection regime (pre-IPO vs. post-IPO year) and auditor size (Big 4 vs. non-Big 4 audit firms).

Hypothesis 3 predicts no difference in restatement likelihood jointly between the pre-IPO and post-IPO year, and Big 4 and non-Big 4 clients. A positive (negative) sign of the coefficient on *PREIPO*BIGN* (β_3) jointly tests whether there is a greater (lesser) likelihood of a restatement for a Big 4 client than a non-Big 4 client, and between the pre-IPO year and the post-IPO year. We make no prediction of the sign of β_3, following hypothesis 3.

To better understand the results from testing hypothesis 3, we partition the comparison of restatement likelihood between the pre-IPO year to post-IPO year (hypothesis 1) into Big 4 clients and non-Big 4 clients. For Big 4 clients, a positive (negative) sign of the sum of the coefficients (β_{1+} β_3) on *PREIPO* and *PREIPO*BIGN* tests whether the restatement likelihood is greater (lesser) in the pre-IPO year than the post-IPO year. For non-Big 4 clients, a positive (negative) sign of the coefficient on *PREIPO* (β_1) tests whether the restatement likelihood is greater (lesser) in the pre-IPO year than the post-IPO year.

[2] The SEC regional offices are: Atlanta, Boston, Chicago, Dallas-Fort Worth, Denver, Los Angeles, Miami, New York, Philadelphia, Salt Lake City, San Francisco, and Washington, DC.

As well, we partition the comparison of restatement likelihood between Big 4 clients and non-Big 4 clients (hypothesis 2) into the pre-IPO year and the post-IPO year. For the pre-IPO year, a positive (negative) sign of the sum of the coefficients (β_2+ β_3) on *BIGN* and *PREIPO*BIGN* tests whether the restatement likelihood is greater (lesser) for a Big 4 client than a non-Big 4 client. For the post-IPO year, a negative sign of the coefficient on *BIGN* (β_2) tests whether the restatement likelihood is greater (lesser) for a Big 4 client than a non-Big 4 client.

Results

Descriptive Statistics

Table 2, Panel A reports the distribution of 1,001 firm-commitment IPOs by issue year for the period from 2002 to 2012. This distribution highlights that fewer companies went public in 2002 and 2003 after the dot com bust. In contrast, there was a large increase in the number of companies going public from 2004 to 2007 followed by a sharp decline in 2008 due to the financial crisis. The number of IPO's then gradually increases from 2008 to 2012.

Panel B of Table 2 details the number of restatements of pre-IPO and post-IPO financial statements by year. This tabulation demonstrates that there were more restatements of pre-IPO financial statements (n=114) than post-IPO financials (n=87). This univariate result is consistent with our prediction of more restatements in the pre-IPO year because issuer incentives to report strong financial performance outweigh the increased investor protections present in the pre-IPO year.

Panel C details the industry composition of the 1,001 firm-commitment IPOs in our sample. There are 16 industries represented in our sample as well as firms in industries that are "non-classifiable" or "other." The most common industries are business services and chemicals and allied products, which comprise 16.4 and 13.5 percent of the sample, respectively. Our sample shows a wider variety of industries compared to the sample in Venkataraman et al. (2008), which contained IPO firms from eight distinct industries. [See Table 2, pg. xx]

Table 3, Panel A, presents descriptive statistics for the dependent and independent variables used in our multivariate model estimations. We present our descriptive statistics by partitioning the sample into these two different regimes (pre-IPO year and post-IPO year). The univariate statistics provide some evidence that the probability of restatement is greater in the pre-IPO year. The test of differences in means is a paired t-test. The test of differences in medians is a Wilcoxon signed rank test. The means of *RESTATE* differ significantly at the five percent level. We find that 78 percent of IPO clients have a Big 4 auditor in the pre-IPO year compared to 77 percent in the post-IPO year; and the difference in means and medians are not statistically significant. Other control variables including *SIZE, ROA, LOSS, AUDITORΔ, LNAUDITFEE, LNNONAUDITFEE,* and *INFLUENCE* differ significantly between the pre- and post-IPO year at the one percent level. These results indicate the importance of including these variables as controls in our multivariate analyses.

Untabulated descriptive statistics of our sample demonstrate that median assets increase from the pre-IPO year (*t*-1) to the post-IPO year (*t*) from $121.4 million to $239.0 million. Similarly, revenues increase from $91.4 million in the pre-IPO year to $128.7 million in the post-IPO year. Venkataraman et al. (2008) show that median assets for their sample increase from $36.1 million in the pre-IPO period to $128.7 million post-IPO, and revenues increase from $21.7 million in the pre-IPO period to $49.6 million post-IPO. Interestingly, the median ROA is positive (0.005 in pre-IPO) and (0.014 in post-IPO) for our sample, while the median ROA is negative in the sample used in Venkataraman et al. (2008). These descriptive statistics demonstrate that firms in our sample are more profitable and larger than the sample used in Venkataraman et al. (2008). [See Table 3, pg. xx]

Table 4 reports the Pearson correlation matrix for all of the variables used in our multivariate analysis. Coefficients with a "*" are significant at the five percent level. As predicted, *RESTATE* is positively correlated with *PREIPO* (at the five percent level), which provides initial support for our first hypothesis. We calculate variance inflation factors (VIFs) for all models used in our study. The average VIF is 2.21 and none of the VIFs on any of the variables exceed the threshold of 10 recommended by Kennedy (1992). The VIF statistics indicate that collinearity across independent variables is not present at the level to impair the interpretation of our regression results. [See Table 4, pg. xx]

Multivariate Results

Table 5 reports the results of estimating equation (2). The Wald chi-squared test is significant at one percent and the Pseudo R-squared is 10.8 percent, comparable with other restatement studies (Francis et al., 2013). The control variable coefficients for *SIZE* and *OFFICESIZE* differ from Francis et al. (2013), likely because IPO clients are not representative

of all publicly traded clients (smaller and less profitable). The coefficient on *PREIPO* is positive and statistically significant (0.202, $p<0.01$). This result suggests that the likelihood of financial restatement is higher in the pre-IPO year than the post-IPO year and supports hypothesis 1. The marginal effect of *PREIPO* is 2.99, implying that pre-IPO firms are 2.99 percent more likely to subsequently restate their financial statements compared to post-IPO firms. This effect is quite large considering that the restatement rate for our full sample of 2,002 firm-year observations is 10.5 percent.

The coefficient on *BIGN* is negative and statistically significant (-0.678, $p<0.05$). This result suggests that Big 4 clients are less likely to restate their financial statements, which supports hypothesis 2. The marginal effect of *BIGN* is -12.97 percent, suggesting that the probability of a restatement is 12.97 percent lower if an IPO firm engages a Big 4 audit firm.

The insignificant coefficient on *IMR* in the model indicates that self-selection is not a serious issue in our sample (Wooldridge, 2016, 556). In a probit model, marginal effects are more informative than coefficients because they provide good approximation of the change in probability in the dependent variable that will be produced by a one-unit change in an independent variable (Williams, 2015).

Overall, the results presented in Table 5 provide strong support for the first and second hypotheses. [See Table 5, pg. xx]

Having established that the probability of financial restatement is lower in the pre-IPO year, and that IPO firms with a Big 4 auditor are less likely to restate their financial statements, we next examine whether the restatement likelihood jointly differs between the pre-IPO year and the post-IPO year, and between Big 4 and non-Big 4 clients. Table 6 reports the results of estimating equation (3). The Wald Chi-squared test and the Pseudo R-squared are similar to results reported in Table 5. All control variables are similar to results reported in Table 5. The coefficient on *PREIPO*BIGN* is negative (-0.318, $p<0.10$), but is not statistically significant at conventional levels, suggesting that we cannot reject the null H3.

To further examine these results, we partition the comparison of pre-IPO and post-IPO years into Big 4 and non-Big 4 clients (hypothesis 1). The F-test of the sum of the coefficients (*PREIPO + PREIPO*BIGN*) is positive and not statistically significant at conventional levels (0.137, $p<0.10$, average marginal effect 2.28%). This result suggests that Big 4 clients are no more likely to restate for the pre-IPO year than the post-IPO year, suggesting that audit quality is consistent for Big 4 clients across investor protection regimes. The coefficient on *PREIPO* is positive and statistically significant (0.456, $p<0.01$, average marginal effect 6.74%). This result implies that non-Big 4 clients are more likely to restate for the pre-IPO year than the post-IPO year. The implication is that support for hypothesis 1 is driven by non-Big 4 clients, while audit quality is not statistically different for the Big 4 audit firm clients between the pre-IPO and post-IPO periods.

Next, we partition the comparison of restatement likelihood between Big 4 versus non-Big 4 clients into the pre-IPO and post-IPO year (hypothesis 2). The F-test of the sum of the coefficients (*BIGN+PREIPO*BIGN*) is negative and statistically significant (-0.869, $p<0.01$). This result suggests that in the pre-IPO year, the Big 4 clients are less likely to restate than non-Big 4 clients. The coefficient on *BIGN* is negative (-0.550, $p<0.10$, one tailed). This result suggests in the post-IPO year, Big 4 clients are marginally less likely to restate than non-Big 4 clients. [See Table 6, pg. xx]

Figure 1 illustrates our probit regression results from Table 6, displaying the probability of restatement of Big 4 and non-Big 4 clients between the pre-IPO year and the post-IPO year. The blue line represents the restatement likelihood of Big 4 clients between the pre-IPO year and the post-IPO year. The orange line represents the restatement likelihood of non-Big 4 clients between the pre-IPO year and the post-IPO year. In order to demonstrate the economic significance of our results, the probability of restatements from Table 6 also are displayed in Figure 1.

Figure 1 shows that in the pre-IPO year when investor protections are stronger (1933 Act) audit quality of the Big 4 is significantly higher than the non-Big 4. In the post-IPO year when investor protections are weaker (1934) audit quality is marginally higher for the Big 4 than the non-Big 4. There is no statistical difference in audit quality between the Big 4 and the non-Big 4 as the investor protection regime shifts from a high litigation regime to a low litigation regime. [See Figure 1, pg. xx]

Conclusion

In this study, we analyze a sample of IPO firm financial statements before and after their IPO, finding that pre-IPO financial statements are more likely to be restated compared to post-IPO financial statements. These results are driven by

non-Big 4 clients, and restatement likelihood does not differ for Big 4 clients between pre-IPO and post-IPO years. This result suggests that non-Big 4 auditors, having less reputation capital at stake, acquiesce to the management incentive to report strong financial performance in the pre-IPO period when the investor protection regime is stronger. On the other hand, the audit quality of Big 4 audit firms is consistent across investor protection regimes.

Further research is needed to determine why restatements occur more in the pre-IPO period than in the post-IPO period. For example, as discussed above, the practical increased auditor litigation risk (compared to the auditor litigation risk of the post-IPO period) of the pre-IPO period is less than what would be assumed theoretically from the different liability standards and the different burden of proof, but it is unclear exactly what this level of auditor litigation risk should be. As well, it is unclear to what extent, if any, the behavior of the auditor is motivated by auditor litigation risk versus the documented fact that the first-year audit tends to be of lower quality than subsequent years during the tenure of the auditor.

Probably there are more first-year audits during the pre-IPO period that during the post-IPO period because some companies are having their first audit performed. Finally, additional research is needed to determine whether there have been changes in recent years in the auditor litigation risk environment and in the IPO environment. Since 2002, the beginning of our sample period, the Sarbanes-Oxley legislation created the Public Company Accounting Oversight Board (PCAOB), the management report on internal control over financial reporting, and the audit by CPA firms of the internal control over financial reporting of large public companies. Restatement levels also have changed over time, with 700 restatements occurring in 2002, increasing to 1,900 in 2006, and dropping to 484 in 2019 (McKeon, 2020).

Appendix A: Variable Definitions

Variable		Definition
RESTATE	=	1 if the firm subsequently issues an accounting restatement, 0 otherwise.
PREIPO	=	1 if the pre-IPO year *t*-1 period, 0 if the post-IPO year *t* period.
BIGN	=	1 if the firm has a Big N auditor, 0 otherwise. At the beginning of our sample period (January 1, 2002), Arthur Andersen still existed and the Big N at that time was the Big 5.
CURR	=	Total current assets/total current liabilities.
ATURN	=	Total sales/average total assets.
LEV	=	Total liabilities/average total assets.
SIZE	=	The natural log of total assets.
ROA	=	Net income/average total assets.
LOSS	=	1 if net income is negative, 0 otherwise.
OFFICESIZE	=	The number of U.S. SEC registrants audited by each accounting firm's engagement office in the fiscal year.
SECREGFAR	=	1 if an auditor office is located in the same city as one of the SEC regional offices (Atlanta, Boston, Chicago, Dallas-Fort Worth, Denver, Los Angeles, Miami, New York, Philadelphia, Salt Lake City, San Francisco, and Washington, DC), 0 otherwise.
LNPOPULATION	=	The natural log of the population in the MSA.
SECONDTIER	=	1 if a client's auditor is either BDO Seidman LLP or Grant Thornton LLP, 0 otherwise.
AUDITORΔ	=	1 if an auditor change occurred during the year, 0 otherwise.
LNAUDITFEE	=	The natural log of a client's audit fee.
LNNONAUDITFEE	=	The natural log of a client's non-audit fee.
INFLUENCE	=	The ratio of a specific client's total fees (audit fees plus nonaudit fees) relative to annual fees generated by the practice office in the year.
IMR	=	Inverse Mills ratio (IMR) based on the first stage equation (1).
Year fixed effects	=	Indicator variables for each fiscal year represented in the sample.
Industry fixed effects	=	Indicator variables for each industry represented in the sample.

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Table 1: Sample Selection Criteria

Number of firm-commitment IPOs on the SDC database with valid CUSIPs for 2002-2012	1,584
Less: American Depository Receipts (ADRs) and unit IPOs	(230)
Less: firms with missing Compustat data	(353)
Number of firm-commitment IPOs (year *t*-1)	1,001
Number of firms in the post-IPO period (year *t*)	1,001
Final sample	2,002

Year *t* is the IPO issue year. Year *t* financial statements are subject to the 1934 Act. Year *t-1* financial statements are subject to the 1933 Act.

Table 2: Descriptive Statistics of IPO Sample

Panel A: Distribution of IPO firms by Year

Year	No. of Firms	% of Sample
2002	25	2.5
2003	60	6.0
2004	153	15.3
2005	132	13.2
2006	137	13.7
2007	146	14.6
2008	24	2.4
2009	39	3.9
2010	92	9.2
2011	84	8.4
2012	109	10.9
Total	1,001	100

The sample comprises 1,001 firm-commitment IPOs from January 1, 2002, to December 31, 2012.

Panel B: Distributions of Restatements in Pre- and Post-IPO Years

Year	Pre-IPO Year (*t*-1)	Post-IPO Year (*t*)
2002	5	6
2003	10	11
2004	13	10
2005	14	5
2006	9	9
2007	26	12
2008	4	4
2009	4	2
2010	10	10
2011	8	7
2012	11	11
Total	114	87

Financial restatements are identified from the Audit Analytics Restatements database as of December 31, 2014.

Table 2 (continued)

SIC	Industry	Total	% of Sample
13	Oil And Gas Extraction	28	2.8
28	Chemicals And Allied Products	135	13.5
35	Industrial Machinery and Equipment	37	3.7
36	Electronic & Other Electric Equipment	66	6.6
38	Scientific Instruments	62	6.2
48	Communications	23	2.3
50	Wholesale Trade-Durable Goods	18	1.8
58	Eating And Drinking Places	17	1.7
59	Miscellaneous Retail	23	2.3
60	Depository Institutions	64	6.4
61	Non depository Institutions	22	2.2
62	Security And Commodity Brokers	31	3.1
63	Insurance Carriers	26	2.6
67	Holding And Other Investment Offices	47	4.7
73	Business Services	164	16.4
87	Professional Services	16	1.6
99	Non classifiable Establishments	21	2.1
	Other	201	20.1
	Total	1,001	100.0

Industry classification is based on the firm's 2-digit SIC code. The sample comprises 1,001 firm-commitment IPOs from January 1, 2002, to December 31, 2012.

Panel C: Distribution of IPO Firms by SIC Industry

Table 3: Descriptive Statistics

Panel A: Descriptive statistics and univariate tests between pre-IPO year and post-IPO year (n =2,002)

	Pre-IPO Year (*t*-1) (n=1,001)			Post-IPO Year (*t*) (n=1,001)				
Variable	Mean	Median	Std. Dev	Mean	Median	Std. Dev	Paired t-test	Wilcoxon z-stat
RESTATE	0.11	0.00	0.32	0.09	0.00	0.28	**	**
BIGN	0.78	1.00	0.41	0.77	1.00	0.42		
SIZE	4.80	4.80	2.10	5.58	5.48	1.58	***	***
ROA	0.06	0.01	1.87	-0.21	0.01	1.54	***	
LOSS	0.47	0.00	0.50	0.41	0.00	0.49	***	***
OFFICESIZE	94.24	49.00	118.62	90.19	44.00	113.58		
SECREGFAR	0.49	0.00	0.50	0.48	0.00	0.50		
LNPOPULATION	15.21	15.28	0.96	15.21	15.28	0.95		
SECONDTIER	0.07	0.00	0.25	0.07	0.00	0.26		
AUDITOR∆	0.03	0.00	0.18	0.08	0.00	0.27	***	***
LNAUDITFEE	11.67	12.72	3.80	13.37	13.56	1.44	***	***
LNNONAUDITFEE	8.64	10.67	5.11	10.14	11.48	4.30	***	***
INFLUENCE	0.06	0.01	0.13	0.08	0.02	0.14	***	***

*, **, *** Indicates significance at the 10, 5, and 1 percent levels, respectively (two-sided tests). Test of differences in means is a paired t-test. Test of differences in medians is a Wilcoxon signed rank test. The sample size is 2,002 firm-year observations (1,001 firm-year observations in the pre-IPO period and 1,001 firm-year observations in the post-IPO period). Variables are defined in Appendix A.

Table 4: Pearson Correlation Matrix

		[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]	[12]	[13]	[14]
[1]	*RESTATE*	1.00													
[2]	*PREIPO*	0.04*	1.00												
[3]	*BIGN*	-0.01	0.01	1.00											
[4]	*SIZE*	0.04	-0.20*	0.26*	1.00										
[5]	*ROA*	0.02	0.07*	0.04*	0.16*	1.00									
[6]	*LOSS*	-0.02	0.06*	-0.03	-0.38*	-0.29*	1.00								
[7]	*OFFICESIZE*	0.03	0.02	0.28*	0.03	0.01	0.04*	1.00							
[8]	*SECREGFAR*	-0.01	0.01	0.06*	0.07*	-0.05*	-0.03	-0.45*	1.00						
[9]	*LNPOPULATION*	0.02	0.00	-0.07*	-0.08*	0.08*	0.03	0.51*	-0.72*	1.00					
[10]	*SECONDTIER*	0.01	-0.01	-0.51*	-0.02	0.03	-0.02	-0.08*	-0.05*	0.07*	1.00				
[11]	*AUDITOR∆*	0.01	-0.09*	-0.12*	-0.04	-0.01	0.00	-0.03	-0.04	0.02	0.05*	1.00			
[12]	*LNAUDITFEE*	0.00	-0.28*	0.18*	0.31*	-0.06*	-0.05*	0.04	0.04	-0.06*	-0.01	0.04	1.00		
[13]	*LNNONAUDITFEE*	0.03	-0.15*	0.20*	0.35*	-0.03	-0.14*	0.05*	0.06*	-0.09*	-0.09*	-0.02	0.47*	1.00	
[14]	*INFLUENCE*	0.03	-0.06*	-0.37*	0.12*	-0.02	-0.09*	-0.30*	0.25*	-0.35*	0.08*	0.05*	0.05*	0.07*	1.00

This table presents the Pearson correlation coefficients. Coefficients with a star are significant at the 5 percent level. The sample contains 2,002 firm-year observations. The sample size is 2,002 firm-year observations (1,001 firm-year observations in the pre-IPO period and 1,001 firm-year observations in the post-IPO period). Variables are as defined in Appendix A.

Table 5: Regression results for the probability of financial restatement pre-IPO and between Big 4 and non-Big 4 auditors

$$\text{PROBIT}[RESTATE = 1] = \beta_0 + \beta_1 PREIPO + \beta_2 BIGN + \beta_3 SIZE + \beta_4 ROA + \beta_5 LOSS + \beta_6 OFFICESIZE + \beta_7 SECREGFAR + \beta_8 LNPOPULATION + \beta_9 SECONDTIER + \beta_{10} AUDITOR\Delta + \beta_{11} LNAUDITFEE + \beta_{12} LNNONAUDITFEE + \beta_{13} INFLUENCE + \beta_{14} IMR + \text{Year fixed effects} + \text{Industry fixed effects} + \varepsilon \quad (1)$$

Variable	Coefficient	Average Marginal Effect	p-value	
Intercept	-1.641		0.242	
PREIPO	0.202	2.99%	0.006	***
BIGN	-0.678	-12.97%	0.032	**
SIZE	0.073	1.08%	0.046	**
ROA	0.018	0.27%	0.555	
LOSS	0.171	2.56%	0.162	
OFFICESIZE	0.002	0.02%	0.027	**
SECREGFAR	0.089	1.31%	0.519	
LNPOPULATION	-0.012	-0.18%	0.886	
SECONDTIER	-0.171	-2.26%	0.394	
AUDITORΔ	-0.014	-0.20%	0.944	
LNAUDITFEE	-0.002	-0.02%	0.911	
LNNONAUDITFEE	0.014	0.20%	0.217	
INFLUENCE	-0.009	-0.13%	0.983	
IMR	0.189	2.78%	0.256	
Year Fixed Effects	Yes			
Industry Fixed Effects	Yes			
Standard errors clustered by industry and auditor	Yes			
Number of observations	2,002			
Wald chi2	221.96			
Prob > chi2	< 0.000			
Pseudo R2	0.1084			

*, **, *** Indicate significance at the 10, 5, and 1 percent levels, respectively. Bolded variables are the variables of interest. The dependent variable is $RESTATE$, which takes the value of 1 if a client subsequently restates its financial statements, and 0 otherwise. *PREIPO* takes a value of 1 for pre-IPO year observations, and 0 otherwise. *BIGN* takes a value of 1 if a client's auditor is one of the Big N auditors, otherwise 0. At the beginning of our sample time period (January 1, 2002), Arthur Andersen still existed and the Big N at that time was the Big 5. All other variables are defined in Appendix A. The sample size is 2,002 firm-year observations (1,001 firm-year observations in the pre-IPO period and 1,001 firm-year observations in the post-IPO period). Year and industry (based on 2-digit SIC codes) are included in the model. Standard errors are clustered by industry and auditor. *IMR* stands for the Heckman Inverse-Mills-ratio. The coefficient on *IMR* is statistically insignificant which suggests self-selection is not a serious issue in our sample (Wooldridge 2016, 556). Consequently, our test results (untabulated) without including *IMR* variable do not differ from the results in Table 5.

Table 6: Regression results for the joint effect of investor protection regime and Big 4/non-Big 4 auditor and the probability of financial restatement

$$\text{PROBIT}[RESTATE = 1] = \beta_0 + \beta_1 PREIPO + \beta_2 BIGN + \beta_3 PREIPO * BIGN + \beta_4 SIZE + \beta_5 ROA + \beta_6 LOSS + \beta_7 OFFICESIZE + \beta_8 SECREGFAR + \beta_9 LNPOPULATION + \beta_{10} SECONDTIER + \beta_{11} AUDITOR\Delta + \beta_{12} LNAUDITFEE + \beta_{13} LNNONAUDITFEE + \beta_{14} INFLUENCE + \beta_{15} IMR + \text{Year fixed effects} + \text{Industry fixed effects} + \varepsilon \quad (2)$$

Variable	Coefficient	Average Marginal Effect	p-value	
Intercept	-1.624		0.252	
PREIPO	**0.456**	**6.74%**	**0.007**	*******
BIGN	**-0.550**	**-10.01%**	**0.098**	*****
PREIPO* BIGN	**-0.318**	**-4.47%**	**0.072**	*****
SIZE	0.079	1.15%	0.034	**
ROA	0.017	0.24%	0.595	
LOSS	0.169	2.52%	0.167	
OFFICESIZE	0.002	0.02%	0.020	**
SECREGFAR	0.090	1.32%	0.514	
LNPOPULATION	-0.021	-0.31%	0.806	
SECONDTIER	-0.172	-2.27%	0.391	
AUDITORΔ	-0.003	-0.04%	0.990	
LNAUDITFEE	-0.002	-0.02%	0.916	
LNNONAUDITFEE	0.013	0.20%	0.226	
INFLUENCE	-0.077	-1.13%	0.868	
IMR	0.212	3.11%	0.202	
Year Fixed Effects	Yes			
Industry Fixed Effects	Yes			
Standard errors clustered by industry and auditor	Yes			
Number of observations	2,002			
Wald chi2	222.56			
Prob > chi2	< 0.000			
Pseudo R2	0.1104			
F-test	Coefficient	AME	p-value	
$\beta_2 BIGN + \beta_3 PREIPO * BIGN = 0$	-0.869	-14.48%	0.008	***
$\beta_1 PREIPO + \beta_3 PREIPO * BIGN = 0$	0.137	2.28%	0.069	*

*, **, *** Indicate significance at the 10 percent, 5 percent, and 1 percent levels, respectively. Bolded variables are the variables of interest. The dependent variable is $RESTATE$, which is an indicator variable that takes the value of 1 if a client subsequently restates its financial statements, and 0 otherwise. *PREIPO* is an indicator variable that takes a value of 1 for pre-IPO year observations, and 0 otherwise. *BIGN* is an indicator variable that takes a value of 1 if a client's auditor is one of the Big N auditors, otherwise 0. Because we interact *PREIPO* and *BIGN* (*PREIPO*BIGN*), the coefficient on *BIGN* (β_2) tests the difference in the likelihood of restatements in the post-IPO year between Big 4 and non-Big 4 auditors. The sum of the coefficients on $BIGN$ and $PREIPO * BIGN$ ($\beta_2 + \beta_3$) tests the difference in the likelihood of restatement in the pre-IPO period between Big 4 and non-Big 4 auditors. The coefficient on $PREIPO$ (β_1) tests the difference in the likelihood of restatement between the pre-IPO period and the post-IPO period for non-Big 4 auditors. The sum of the coefficients on $PREIPO$ and $PREIPO * BIGN$ ($\beta_1 + \beta_3$) tests the difference in the likelihood of restatement between the pre-IPO year and the post-IPO year for Big 4 auditors.

Figure 1: Probability of restatement of Big N and Non-Big N auditor clients between the pre-IPO year and the post-IPO year