Audit Office Labor Market Proximity and Audit Quality

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Abstract

Using a unique set of hand-collected data, we examine whether audit office's proximity to its labor market of universities affects audit quality. We analyze the effect of proximity by the number of universities, and the number of accredited business universities, within a 25-mile radius from an audit office. Our findings suggest that audit offices that are closer to more universities, or to more accredited business universities, are associated with higher audit quality, as observed by a lower likelihood of financial accounting misstatements. Our results are robust across alternative measures of labor market proximity and audit quality, and to a battery of sensitivity tests, including controlling for client firm's proximity to universities. Overall, our results suggest that it is advantageous for audit offices to be proximate to one of their key labor source.

Keywords: Labor market; audit quality, restatements, audit offices, proximity
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1. Introduction

This paper investigates how audit quality is affected by an audit office's proximity to its labor market of universities. A growing literature in accounting and finance focuses on how geographic proximity of firms to banks and regulatory bodies affects corporate outcomes such as accessibility and terms of bank financing (see meta-analysis by Agarwal and Hauswald, 2010; Butler, 2008; Degryse and Ongena, 2005; Hollander and Verriest, 2016; Petersen and Rajan, 2002) and financial reporting quality (Kedia and Rajgopal, 2011). Other studies show how such proximity of investors and external stakeholders (e.g., analysts and fund managers) to firms produce more accurate earnings forecasts (Malloy, 2005) and greater returns on investments (Bodnaruk, 2009; Coval and Moskowitz, 2001).

Choi et al. (2012) and Francis et al. (2017) extend the notion of geographic proximity to an auditing setting by examining how audit quality is affected by the proximity of audit offices and audit partners to clients. However, largely overlooked in this discussion, be it in the field of auditing, accounting or any other business discipline, is whether proximity to an entity's most important production factor – labor – has economic consequences for the entity.[1] Labor market outcomes are of importance to accounting practitioners and researchers because labor is the main input in most accounting processes, and deficiencies in accounting labor quality can have adverse consequences for firms (e.g., Acito et al., 2009; Ge and McVay, 2005). Our study aims to extend the stream of research on proximity by focusing on a new factor that may play an important role in the link between accounting labor market and audit quality, namely, proximity of audit offices to their labor market of universities.

The audit profession is a particularly attractive setting for understanding the economic

[1] Prior research on labor market proximity has mainly focused on issues such as the impact of agglomeration and proximity to human capital on *employee* outcomes, such as wage effects (e.g. Rosenthal and Strange, 2008).

role of labor-market proximity to universities due to the critical role that universities play in screening out graduates that fill staff auditor positions in accounting firms. While prior studies in auditing provide ample evidence on various factors (e.g., industry expertise) that can enhance the effectiveness of audits at the audit office and audit partner levels, it is also important to consider factors that influence the currency of staff auditor competencies in audit offices. As discussed in more detail later, staff auditors play a pivotal role in the audit production process through conducting audit fieldwork (e.g., substantive tests, tests of details, analytical reviews, etc.). Consequently, the work that staff auditors do significantly influences the audit fees charged and the audit quality delivered to clients (Willett and Page, 1996).

Since its inception in 1997, the AICPA's biennial CPA Firm Top Issues Survey ranks the search for qualified talent, including staff auditors, as the top concern of audit firms (Drew, 2015). This concern has intensified in recent years as a result of factors such as workload compression and increased responsibilities, which have contributed to early-career CPAs leaving public accounting at an increasing rate (Hermanson et al., 2016). The role of labor market of university graduates in restituting such losses and addressing the demand for staff auditors has long been addressed in the AICPA's Trends in the Supply of Accounting Graduates and the Demand for Public Accounting Recruits report (Vien, 2015), and is reflected in the demographics of new hires at audit firms. For example, Deloitte, Ernst & Young (EY) and PricewaterhouseCoopers (PwC) make up the top three recruiters of university graduates in the U.S. in 2016 (Nieh, 2016), representing around 75 percent of the total hiring in these audit firms (PwC, 2013). The autonomous hiring practices of individual audit offices suggest that the proximity of audit offices to more universities should allow them to apply higher standards for recruiting and attract top talent to fill staff auditor positions, which should promote their audit quality.

However, we are also unable to discount the possibility of proximity of audit offices to

fewer universities benefiting audit quality through staff auditors possessing more homogeneous knowledge. Further, it is also possible that the labor market for audit offices has sprawling geographic boundaries and/or is influenced by the emergence of online accounting degree programs, which may bias against us documenting any result for audit office proximity to more universities. Thus, whether audit quality varies positively or insignificantly with an audit office's proximity to its labor market of universities is ultimately an empirical question.

We investigate our research question by examining the relation between audit quality and the number of universities within a 25-mile radius to an audit office. Following recent studies (Francis and Michas, 2013; Francis et al., 2013; Kohlbeck and Mayhew, 2017), we use reported financial accounting misstatements to measure audit quality, while our choice of a 25-mile radius to measure our test variable is supported by prior literature in urban economics (Rosenthal and Strange, 2008). Given that accredited business schools are expected to provide better career pathways to accounting positions for graduates, we also consider the number of universities with an AACSB accredited business school within the 25-mile concentric area.[2] Based on a sample of 37,916 observations during the period 1999 through 2016, we document strong positive associations between audit quality and our labor market proximity measures. Our results suggest that an interquartile shift in the number of universities (accredited universities) within 25 miles of an audit office reduces the likelihood of a client misstatement by around 10.74 (10.06) percent. These results support the view that audit offices provide better audit quality when they are proximate to more universities.

Our results are robust to using a two-stage least squares approach, which uses the

[2] The accreditation standards of AACSB require business schools to design accounting courses with a focus on a range of skills such as information technology that improve the knowledge and skill set of graduates for working in audit firms. For example, accreditation standard A7 of AACSB suggests that "accounting degree programs include learning experiences that develop skills and knowledge related to the integration of information technology in accounting and business" (http://bit.ly/2pd1Yyx). Specifically, these skills and knowledge relate should include "data creation, data sharing, data analytics, data mining, data reporting, and storage within and across organizations."

amount of railroad tracks in a Metropolitan Statistical Area (MSA) in 1898 to instrument for the proximity of an audit office to universities. We also use an exogenous shock setting to corroborate our main findings. Specifically, we assess how the audit quality of audit offices based in New Orleans was affected by the displacement of New Orleans-based university students by Hurricane Katrina in 2005.[3] Consistent with our main results and the view that Katrina depleted the labor supply of university graduates in New Orleans, we find that the audit quality of audit offices in New Orleans deteriorated relative to all other offices in the U.S. in the four-year period following Hurricane Katrina. We complement this finding by documenting an improvement in the audit quality of audit offices in the city of Shreveport in Louisiana and those located in Texas and Georgia, which stood to attract most of the displaced residents from Louisiana (LAGIC, 2005; Sastry and Gregory, 2014). We further support our main inferences by using change regression specifications to show a lower (higher) likelihood of misstatements in audit clients that switch to auditors with offices that are in proximity to a larger (smaller) labor market (pool) of universities.

Our main findings are also robust to using other proxies of audit quality such as auditors' propensity to issue going concern audit opinions to financially distressed clients, unsigned discretionary accruals of clients, and deficiencies in an area likely to be more salient to staff auditors, namely over-relying on internal controls when they are likely to have material weaknesses that are not reported. We further show that proximity to universities reduces the probability of the audit office experiencing a regulatory enforcement action for poor audit practices, which more directly captures audit quality at the audit office level. Our findings are also insensitive to defining the labor supply of universities based on different radii ranging

[3] Scott Cowen, Tulane University's president during Hurricane Katrina, stated in an interview, "it took us almost a decade to recover [from Hurricane Katrina]... a lot of students didn't return after Katrina, so we were under enrolled", his views on enrolments were similarly echoed by Dillard University's President Walter Kimbrough (Arnett, 2017).

from 10 miles to 60 miles from audit offices,[4] considering only universities that are within the same state as the audit office, and defining labor market as the total university population and the population of accounting university graduates.

To rule out the threat of client proximity to universities confounding our results, we show that our results are insensitive to controlling for client-firm proximity to universities and prevail within subsamples of audit clients that are either more geographically dispersed or headquartered in a foreign country, which allows us to achieve a greater degree of separation between the labor markets of audit offices and their clients. Our results remain robust when we control for a wide range of client management and board governance attributes previously linked to higher financial reporting quality and when we undertake other tests, including the impact threshold for a confounding variable (ITCV) procedure, to address any potential impact of other omitted variables. To further elucidate that our results are attributable to audit offices, we document a positive effect of labor supply proximity on audit quality in two settings where greater proximity to universities is expected to be more salient, namely, when there is a greater concentration of audit offices in a city (which signifies greater competition for a similar pool of potential labor supply) and in larger offices (which require a larger labor supply).

Our findings allow us to complement and extend the extant literature on geographic proximity and audit quality. While Choi et al. (2012) and Francis et al. (2017) show proximity to clients allows audit offices to provide higher audit quality, our study shows how audit quality is influenced by the proximity of audit offices to an important input represented by its labor market of university graduates. It is important to note that our findings cannot be inferred from prior studies that link financial reporting and audit quality to factors such as the concentration of human capital in a city (Beck et al., 2017; Call et al., 2017), which we control for in our

[4] Consistent with the viewpoint that audit offices are likely to benefit more from closer proximity to universities, our results suggest a monotonically increasing effect on audit quality when we consider smaller concentric areas around audit offices in defining labor market proximity.

analyses.[5] This is because a higher concentration of human capital in cities may not always reflect better proximity of audit offices to universities. Indeed, we find that there are numerous cities such as Las Vegas, Phoenix and San Diego (Boulder and Greensboro) that rank high (low) based on the population of educated adults (i.e., those with a Bachelor's or higher degree) but rank low (high) based on the proximity of the audit offices in these cities to universities.

Our study also contributes to the rich stream of research on audit offices that has predominantly focused on how the audit quality of audit offices is affected by various audit office- and partner-level factors (Francis et al., 2005; Reichelt and Wang, 2010). Our study complements and extends this field of research by showing that audit quality is also positively related to an audit office's proximity to the labor market that fuels its demand for talented staff auditors. Collectively, our findings should be of interest to AICPA, audit offices and their clients. While there has been long-standing interest in how university labor markets address the demand for staff auditors in audit firms, our study provides initial insights into how the proximity of audit offices to their labor market of universities can affect audit quality.

2. Background and research question/hypothesis

2.1 Staff auditors and audit quality

A considerable amount of research has been directed towards demonstrating how audit quality is enhanced by supply-side factors such as auditor brand name (Becker et al., 1998; DeAngelo, 1981; Francis and Krishnan, 1999) and industry expertise (Balsam et al., 2003; Lim and Tan, 2008). Around the turn of the century, this research paradigm shifted to include an audit office-level perspective based on the premise that the administration of audits is handled by the engagement office (Ferguson et al., 2003; Reynolds and Francis, 2000). These studies

[5] Beck et al. (2017) find that non-Big 4 audit offices are associated with better audits when they are located in cities with high human capital. They document relatively weaker results for Big 4 audit offices, which they attribute to the national network of these audit firms allowing easier transferability of skills and knowledge. However, they acknowledge that "…much of Big 4 entry level recruiting on college campuses remains 'local' in nature." As such, our study extends Beck et al. (2017) by directly addressing how audit quality is influenced by audit office proximity to its labor market of universities.

generally show that audit offices are able to deliver higher audit quality when they specialize in industries at the city-level (Francis et al., 2005; Reichelt and Wang, 2010), when clients are less economically significant to the audit office (Li, 2009) and when they are large in size (Choi et al., 2010; Francis et al., 2013; Francis and Yu, 2009).

The literature on audit quality has further evolved through recent studies exploiting the disclosure of audit partner information in non-U.S. jurisdictions to evaluate how audit quality is affected by the attributes of partners who are ultimately responsible for key audit decisions, such as whether or not to negotiate matters potentially requiring adjustments with client management (Beattie et al., 2000; Gibbins et al., 2001).[6] A number of such studies have mirrored the findings of office-level studies by linking higher audit quality to the industry expertise of the audit engagement partner (Chi and Chin, 2011; Chin and Chi, 2009; Zerni, 2012).[7] A limitation of these partner-level studies, however, is that they do not shed any insights into how audit quality is affected by factors reflecting the quality of junior staff auditors within audit offices. This is an important extension to consider because, analogous to the arguments employed in recent studies that highlight the salient roles of audit office and partners in audit production, the foundation of the judgments and audit opinion expressed by the auditor is largely based on the work performed by staff auditors (Willett and Page, 1996).[8]

Audit teams have hierarchical structures (Rich et al., 1997; Rudolph and Welker, 1998) composed of sub-teams of partners, managers, seniors and junior staff (Cameran et al., 2017). Ultimately, the product being supplied by these audit teams is a skilled service that is also labor intensive (Beck et al., 2017; McNair, 1991). While audit partners play an important role in

[6] The requirement of disclosing the identification of the audit engagement partner has been recently adopted by the PCAOB and SEC for U.S. firms. This disclosure rule is effective for audit reports issued on or after January 31, 2017 (PCAOB, 2015).

[7] Other studies in this domain have documented associations between audit quality and the gender (Cameran et al., 2016; Ittonen et al., 2013), age (Goodwin and Wu, 2016; Sundgren and Svanström, 2014), compensation (Knechel et al., 2013), and workload (Goodwin and Wu, 2016; Sundgren and Svanström, 2014) of audit engagement partners. See Lennox and Wu (2017) for a review of the literature on audit partners.

[8] Indeed, a significant portion of the audit fees charged to audit clients is based on the time cost of staff auditors undertaking fieldwork and other tasks (Willett and Page, 1996).

client relations and other significant audit decisions, the supervision and coordination of the audit are handled by managers, while the senior and staff auditors are mainly responsible for the technical tasks that the audit field work necessitates (Cameran et al., 2017; Maister, 1982). The tasks performed by staff auditors include substantive tests, tests of details and other tests involving inspection of records and procedures (Willett and Page, 1996). However, recent changes in the audit landscape to shift from compliance testing to increased analytical procedures have resulted in staff auditors being increasingly delegated analytical review procedures (Yen, 2012). This "pushing down" of work has intensified in the wake of higher employee turnover[9] and tight client deadlines (Kelly et al., 2011; Pierce and Sweeney, 2004; Sweeney and Pierce, 2004).

A unique feature of the auditing profession is that it is syncretic in nature in that it is concerned with both matters of fact and matters of value (Malhotra and Morris, 2009). This means that in addition to being proficient at the technical aspects of their tasks (i.e., matters of fact), staff auditors are expected to exercise professional judgment in interpreting audit evidence and reasonableness of clients' statements (i.e., matters of value). In line with this view, Bennett and Hatfield (2013) find that around 86 (37) percent of recently graduated staff-level auditors interact with client management at least three days (every day) during a typical fieldwork week as part of their work to understand the clients" business processes and obtain clarification and explanations for audit test work findings. In fact, around 30 percent of these meetings are unsupervised and more than two-thirds of these meetings involve interactions with higher-level management and directors of clients. This highlights the importance of staff auditors possessing personality traits such as higher conscientiousness and lower openness, which can enhance their assessment of management accounting estimates (Fitzgerald et al.,

[9] For example, the global recession period of 2007-2009 caused large accounting firms to lay off thousands of staff, many of whom left the profession entirely (Drew, 2015).

2015). Evidence suggests that the coal-face audit work undertaken by staff auditors can allow them to become more adept than senior auditors at identifying mechanical errors (e.g., incorrect computations or transfer of numbers in audit files) in the carrying out audit procedures (Harding and Trotman, 1999) and exercise greater skepticism towards assessing audit evidence and conditions that may indicate possible misstatements (Montgomery et al., 2002; Pany and Whittington, 2001; Payne and Ramsay, 2005).

It is important to note that the intent of the above discussion is not to advocate the contributions of staff auditors relative to seniors, managers, and partners in the audit production process, but rather to stress the positive contributions of staff auditors to this process. The hierarchical structure of audit teams implies that audit partners and managers rely heavily on the work of juniors, implying that the quality of work performed by staff auditors can affect audit quality. The value of staff auditors can be further appraised from the wide range of recent initiatives that accounting firms have employed to curb the growing number of Millennials leaving the profession amidst the increasing demands that are being placed on staff auditors (Crowe Horwath, 2016; Moritz, 2014). For example, in 2014, the Global Chairman of PwC, Bob Moritz, indicated that PwC was striving to engage staff auditors on issues such as investments in human capital and the compensation structure of staff auditors (Moritz, 2014). Such initiatives further underline how highly staff auditors are valued in audit firms.

2.2 Proximity of audit offices to universities and audit quality

As discussed earlier, universities play a foundational role in the recruitment of staff auditors in audit firms (Vien, 2015) through providing accounting education that meet the needs of the profession. For example, based on the survey findings of Bonner and Pennington (1991), audit partners and managers estimate that over a third of knowledge required for performing analytical review tasks is obtained from formal university instruction. The magnitude of university graduate recruiting in large accounting firms is not trivial. According

to its 2013 U.S. Corporate Responsibility Report, PwC hired around 9,000 university graduates, which amounted to almost 75 percent of their hiring in that year (PwC, 2013)[10] and is commensurate with the historical hiring practices of PwC (Moritz, 2014). The fact that Deloitte and Ernst & Young were the only U.S. companies to recruit more graduates than PwC in 2016 suggests that the reliance on a strikingly young workforce is prevalent in most audit firms.

Prior studies find that prestigious banking, consulting and law firms, including those that require extensive travel, rely heavily on closely located universities to fill entry-level positions (Oyer and Schaefer, 2016; Weinstein, 2017). This could be due to employer search costs increasing in distance and/or student migration frictions preventing individuals from applying to more distant positions. The latter explanation has been supported in other studies (e.g., Manning and Petrongolo, 2017) and is consistent with the significant decline in the internal migration of university-educated individuals in the U.S. over the past three decades (Molloy et al., 2011).

Audit offices are operated by local managing partners, who have autonomy in their hiring decisions and who are also likely to be attracted to local labor markets (Beck et al., 2017). In fact, the reliance on proximate universities for recruiting is perhaps even more likely in audit offices due to the fact that audit professionals are impeded from relocating across state lines by heterogeneous state laws, which increase transaction costs in licensing and thus create job search frictions (Donabedian, 1991; Henry and Hicks, 2015; Rimerman and Solomon, 1991).[11] Indeed, discussions on best practices for recruitment in audit firms guide audit offices

[10] This imbalance instigated a class and collective action filed against PwC in 2016 alleging violations of the Age Discrimination in Employment Act of 1967 (see Plaintiffs vs. PricewaterhouseCoopers LLP available at https://www.pwcagecase.com/sites/default/files/documents/amended-complaint.pdf).

[11] There are several factors that are likely to constrain cross-state mobility after graduation. First, CPA exam sitting requirements vary from state to state, creating high switching costs for graduates. For instance, the neighbouring states of Arizona, California, Nevada, and Oregon have different education requirements (e.g. hours of accounting credits; mix of subjects); therefore, students seeking a job in a cross-state labor market may not have the prerequisites to sit the exam. Furthermore, as evidenced by university curriculums (http://bit.ly/2sCgean), schools design their accounting programs to match their state's CPA sitting requirements. Second, for graduates who sit the exam in one state and seek to transfer their license to another state (i.e., reciprocity rule), the transfer process can be tedious and lengthy. For example, some states require fingerprint and background checks, while

to direct their recruitment efforts toward proximate university campuses (Drew, 2015). The nurturing of long-term relationships with proximate universities can provide audit offices with an edge in recruiting the best graduates through implementing a plethora of initiatives ranging from on-campus networking and internship programs to "externship" programs that provide students an opportunity to work while also receiving course credits (Busta et al., 2007). Publications in regional media outlets provide strong support for the view that the Big 4 audit firms work closely with local universities to fill entry-level positions (Blumenthal, 2017; Lee, 2008). Our searches for student placement reports by universities and our consultations with academic advisors at several large universities also revealed that the majority of the graduates from these universities sought and secured positions locally.

The above observations underline the importance of proximate universities to audit offices for recruitment. To begin, the large pool of accounting graduates afforded by such proximity will naturally lead to audit offices applying higher standards to screen applicants for not only technical skills but for personal dimensions (e.g., professional conduct, interpersonal skills, commitment to organization and proclivity to act ethically) that have become critical for staff auditor roles in recent times. While audit offices that are proximate to more universities are not precluded from appointing graduates from more distant universities, these applicants will arguably have to be of exceptional quality (Weinstein, 2017) to not only exceed the higher screening thresholds of these audit offices, but overcome the lack of familiarity of these audit offices with these graduates. The latter is an important factor given the emphasis audit offices place on establishing relationships with early-year local university students for assessing

other states, such as California, do not recognize reciprocity. Third, the cost to attend out-of-state universities can be significant. Out-of-state tuition fees are on average 2-3 times higher (http://bit.ly/2Et92CP); this amounts to an additional $20,000-35,000 annually at larger schools. For those students that do complete their accounting education in another state, we find anecdotal evidence that suggests they tend to remain in that state (http://bit.ly/2BYuaPG). Finally, although we cannot assess (due to privacy issues and data limitations) the proportion of staff auditors at Big 4 firms that are hired locally, we visit the websites of non-Big 4 firms and find (via their staff bios) anecdotal evidence that most junior staff are recruited from local institutions.

knowledgeability and compatibility with their audit office culture (Jeffords et al., 2000; MPACE, 2017). Cameran et al. (2017) find that the audit quality of Italian audit firms is positively affected by audit partners and managers possessing diverse educational backgrounds. This suggests that the proximity of audit offices to more universities can also facilitate greater diversity in the competencies of staff auditors and further enhance audit quality. Collectively, these arguments suggest that the proximity of audit offices to more universities allows audit offices to recruit higher quality staff auditors who can positively influence audit quality.[12]

However, it is not obvious that proximity advantages will unequivocally lead to better audit quality in our setting. To begin, while we expect the labor market to be narrowly confined in our setting, with few exceptions (e.g., Stigler, 1962), prior studies consider labor markets to exist across space (e.g., a city, or a county, see Beggs and Villemez, 2001; Berry and Lobley, 1973; Tolbert and Killian, 1987), which may bias against finding a positive relation between audit quality and the proximity of an audit office to more universities. The recent growth in online accounting degree programs is another factor that may extinguish the benefits of audit office physical proximity to more universities. However, we believe this concern is mitigated through studies showing that CPA exam candidates from accredited and non-accredited universities outperform those from institutions that offer online accounting programs (Bunker and Harris, 2014) and that accounting firms prefer to recruit candidates with traditional education rather than online education (Tabatabaei et al., 2014). Another point to note is that the proximity of audit offices to fewer universities can result in staff auditors who are from the same university and, consequently, staff auditors with more homogeneous knowledge. This can positively affect audit team dynamics through minimising the risk of misunderstanding and

[12] Stice et al. (2017) find that firms with higher quality senior auditors (those with higher performance evaluation ratings) are more likely to issue going concern modifications to their clients. However, these findings are based on a unique dataset of internal performance evaluations conducted by the Deloitte in 2005 only.

facilitating more effective decisions, which can lead to higher audit quality.[13] As result of the contrasting views on whether and how an audit office's proximity to its labor market of universities may influence audit quality, we examine this relation as a research question without offering a directional hypothesis.

3. Research design

We examine the effect of audit office proximity to universities on audit quality by estimating the following logistic regression model based on prior literature (e.g., Francis et al., 2013; Kohlbeck and Mayhew, 2017):

$$MISSTATE = f\{PROXIMITY, Control\ variables\} \quad (1)$$

Consistent with prior studies (Francis and Michas, 2013; Francis et al., 2013; Kohlbeck and Mayhew, 2017; Paterson and Valencia, 2011), we employ reported financial accounting misstatements as the proxy for audit quality in our main analysis. As such, our dependent variable, *MISSTATE,* is an indicator variable coded one if the financial statements for a given firm-year were misstated and were subsequently restated, and zero otherwise.[14]

Our test variable (*PROXIMITY*) captures the labor supply of universities in proximity to an audit office. There are no established guidelines or rules to select a radius for calculating the proximity between an audit office and university. Rosenthal and Strange (2008) find that wage premiums associated with spatial concentration of employment are driven by college educated workers who are within a 25-mile radius of their place of employment. Accordingly, using the zip code of audit offices as their geographic centroid, we capture proximity of audit offices to universities based on a 25-mile radius. We employ two measures of labor supply

[13] Prior literature in social psychology finds instances when team homogeneity is preferred and leads to better outcomes (see meta analysis by Bowers et al., 2000). While Cameran et al. (2017) propose this possibility to examine how audit quality is affected when audit partners in managers in Italy attend the same university, consistent with audit firms benefiting from greater educational diversity, they find that common educational background of partners and managers has a negative effect on audit quality.

[14] We use all types of misstatements in our analysis because "auditors' opinions regarding the material accuracy of financial statements should be unaffected by the reason for a misstatement" (Newton et al., 2013). Our results (untabulated) are unaffected when we restrict our misstatements measure to non-clerical errors or those that adversely affect financial statements (Kohlbeck and Mayhew, 2017).

proximity. Our first measure captures the number of universities with schools that offer accounting programs within a 25-mile radius to an audit office (*UNI*). The second measure focuses on accredited business schools that are expected to provide career pathways to accounting positions for graduates and is measured as the number of universities with an AACSB accredited business school within a 25-mile radius to an audit office (*ACCUNI*). While we consider alternative proximity proxies later, we focus on number of universities in our main tests as it is highly correlated with number of university students (correlation coefficients are around 0.80) and accounts for other unobservable factors (e.g., benefits from heterogeneity in school programs and benefits from competition between schools to attract better students, build better programs, attract better faculty). In relation to universities with multiple campuses, we treat each university campus with an accounting program as a separate unit. Further, we do not focus solely on universities that offer a Master's degree in accounting. While there was an initial increase in students pursuing a Master's degree to fulfill the 150 hours of education for obtaining a CPA license when this requirement was introduced around the turn of the century, the ratio of Master's to Bachelor's Accounting students has slightly decreased since then (AICPA, 2017). This is consistent with survey evidence indicating that students are hesitant to undertake a Master's degree in recent times because it may not result in higher salaries (Larkin, 2014) and because they can fulfill the education hours requirement through other means such as taking non-degree courses or enrolling in a Bachelor's degree with an accelerated program covering 150 credit hours (http://ipassthecpaexam.com/cpa-exam-150-credit-hours/). Hence, enrollment in any accounting program seems an appropriate indicator of universities that educate and qualify graduates for staff auditor positions. Appendix A provides a detailed description of our proximity measures and sources of data.

Our first set of controls represent client-firm characteristics that are likely to affect the probability of misstatements (Francis et al., 2013; Kohlbeck and Mayhew, 2017). We control

for firm size because larger firms (*LNTA*) are likely to have a lower likelihood of misstatements due to better internal controls. We include return on assets (*ROA*), financial losses (*LOSS*), and variance in ROA (*ROAVOL*) as firms with lower and greater variance in profitability have an increased risk of misstatements. We control for firms that are growing (*CHGTA*, *MTB*), that engage in mergers and acquisitions (*MNA*), and that have higher leverage (*LEV*) because such firms are more highly exposed to misstatements. The likelihood of misstatements is also expected to be higher for firms that operate in high litigation risk industries (*LIT*) and firms that have recently switched auditors (*SWITCH*).

Our next set of control variables capture audit office characteristics that can affect the probability of misstatements. We expect fewer misstatements when audit offices have greater expertise, evident through generation of more audit fees in an industry (*AUDSIZE*) and market share leadership in an industry (*LEADER*). Economic bonding, as captured by fees paid to the auditor (*LNFEES*) and provision of non-audit services (*FEERATIO*), can lead to more misstatements (Kohlbeck and Mayhew, 2017; Newton et al., 2013). Conversely, as shown by Reynolds and Francis (2000) and Francis et al. (2013), client importance (*CLIENTIMP*) is likely to reduce misstatements, consistent with audit offices being more conservative in their treatment of larger clients. Auditors that are more proximate to their client firms (*CLIENTPROX*) also benefit from greater informational advantages, thereby enhancing audit quality (Choi et al., 2012). Further, proximity to regulators affects the probability of enforcement and could thus impact financial reporting and audit quality (Kedia and Rajgopal, 2011). As such, we control for a client firm's proximity to SEC offices (*SECPROX*) as well as the audit office's proximity to PCAOB offices (*PCAOBPROX*).

Our final set of control variables account for the effect of important city-level factors. We control for the population of adults holding a Bachelor's or higher degree (*LNEDU*) as a more educated population leads to knowledge sharing, which can enhance audit quality (Beck

et al., 2017).[15] We also control for unemployment rate (*UNEMPLOY*), the annual change in population in a city (*CHGPOP*) and annual change in per capital income in a city (*CHGINC*) because they affect the change in supply and demand of labor in a city (Beck et al., 2017). Finally, we include state-level income tax (*TAX*) for the average salary of an accountant working in the state because taxation considerations may affect the supply of labor in a city. A detailed description of our variables is provided in Appendix B.[16]

We estimate equation (1) after including year and industry (defined using SIC 2-digit codes) fixed-effects in our model to control for the idiosyncratic effects of time and industry on misstatements. Standard errors are clustered by firm and year to control for potential cross-sectional correlation and correct heteroskedasticity.[17] We report all *p*-values conservatively at the two-tailed level, even when we have made a directional prediction.

3.1 Sample

We begin with a sample of 137,497 observations with coverage on the AuditAnalytics and Compustat databases during the period 1999 through 2016. Next, we eliminate 48,087 firm-years audited by non-Big 4 firms to account for innate differences between offices of Big 4 and non-Big 4 auditors. Such differences could arise through Big 4 firms (1) maintaining greater presence in larger cities that allow them to be centered around more universities, (2) proving more attractive to graduates, and (3) possessing greater resources and expertise to

[15] Consistent with Beck et al. (2017), city-level educational level is defined using Core Based Statistical Area (CBSA) and data is obtained from the American Community Survey (ACS). For each year from 2009, ACS releases a rolling 5-year estimate (i.e., the first available 5-year estimate is for the period 2005-2009). As such, we code values prior to year 2009 with the 5-year estimate for the period 2005-2009, and code years after 2015 with the 5-year estimate for the period 2011-2015. Our results are not sensitive to coding all years using the 5-year estimate for the period 2005-2009, as done by Beck et al. (2017).

[16] Our analysis includes all city-level factors that controlled for in Beck at al. (2017) except for commuting time in cities (*COMMUTE*), which we exclude because *COMMUTE* is highly correlated with several variables in our model (e.g., 0.63 correlation with *LNEDU* and 0.43 with *PCAOBPROX*). We find that our main results remain robust and *COMMUTE* is insignificantly associated with misstatements when we repeat our analysis after including *COMMUTE.* Consistent with Beck et al. (2017), data on commuting is defined using CBSA and obtained from the ACS. As ACS data on commuting is available only from years 2010 to 2015, we code values prior to year 2010 with 2010 values, and code years after 2015 with 2015 values. Our results are not sensitive to coding all years using the 5-year estimate value at year 2010, as done by Beck et al. (2017).

[17] Our results are not sensitive to clustering standard errors by CBSA area code (Beck et al., 2017) or audit office (Francis et al., 2013).

conduct better quality audits, relative to non-Big 4 firms. We also exclude 12,981 observations with non-U.S. audit offices as the proximity and quality of universities may vary across these countries, which can adversely affect comparability. Next, we discard 11,631 observations with missing data for computing our city-level control variables relating to educated population, population growth, and change in per-capital income. An additional 26,882 observations are dropped due to insufficient data to construct our remaining control variables. Our final sample consists of 37,916 observations, corresponding to 5,715 unique firms.

4. Empirical results

4.1 Descriptive statistics

Table 1, Panel A, presents descriptive statistics for the sample. The mean value of *MISSTATE* indicates that around 16.7 percent of the firm-year observations are associated with misstatements. This is consistent with the misstatement rates documented in prior studies (Kohlbeck and Mayhew, 2017; Paterson and Valencia, 2011). The summary statistics on *UNI* (*ACCUNI*) indicate that, on average, there are around 15 (5.506) universities (accredited universities) within 25 miles of an audit office. In relation to our controls, we find that the market value of the sample firms is almost three times the book value of assets (mean *MTB* = 2.903), while 15.7 percent of the firms seek further growth through mergers and acquisitions (*MNA*). However, almost a third of the firms report losses (*LOSS*) with the mean return on assets amounting to around -5.5 percent (*ROA*). These statistics as well as the high level of liabilities relative to assets (mean *LEV* = 55.9 percent) are similar to summary statistics reported in prior studies (Francis et al., 2013; Kohlbeck and Mayhew, 2017; Newton et al., 2013).

Also in line with prior studies, around 59.6 percent of the sample firms are clients of industry-leading auditors at the city-level (*LEADER*) and around 22.4 percent of the fees that audit firms receive from their clients are for the provision of non-audit services (*FEERATIO*). The mean annual growth of 0.7 percent in city population (*CHGPOP*) closely tracks the mean

annual U.S. population growth of 0.8 percent documented by the World Bank during our sample period (https://data.worldbank.org/), while the mean annual growth of 3.3 percent in per capita income in cities (*CHGINC*) exceeds the mean annual U.S. inflation rate of 2.1 percent reported by the Bureau of Labor Statistics (https://www.bls.gov/bls/inflation.htm). The descriptive statistics for the remaining control variables are similar to those reported in prior studies (e.g., Francis et al., 2013; Kohlbeck and Mayhew, 2017).[18]

Summary statistics presented in Panels B and C of Table 1 show that the rate of misstatements decreases monotonically moving from observations with the lowest to the highest quartile of audit office proximity to more universities (based on the values of our test variables *UNI* and *ACCUNI*). Results from two sample *t*-tests reveal that the misstatement rates of the lowest and highest quartile subsamples are significantly different ($p < 0.01$).

4.2 Main results

Table 2 presents the results from estimating equation (1).[19] The results reveal negative and significant ($p < 0.01$) relations between misstatements and *UNI* (Column 1) and *ACCUNI* (Column 2), supporting the view that proximity to more universities enhances the quality of audits provided by audit offices. The coefficient size of *UNI* (*ACCUNI*) infers that an interquartile shift in the number of universities (accredited universities) within 25 miles of an audit office reduces the likelihood of a client misstatement by around 10.74 (10.06) percent.[20]

[18] Consistent with prior studies, untabulated Pearson correlation among the explanatory variables reveal that the highest correlation coefficient (0.766) is between the variables capturing firm size (*LNTA*) and audit fees (*LNFEES*). Our study's conclusions are unaffected when we exclude either *LNTA* or *LNFEES* from our analyses. The highest variance inflation factor (4.11 for *LNFEES*) in our models is less than 10, indicating no severe multicollinearity issues among the predictor variables (Kennedy, 2003).

[19] We find that the area under the under the Receiver Operator Curve (ROC) is around 0.66, suggesting that our model has modest discriminatory power (Hosmer and Lemeshow, 2000). When we follow the approach used by Francis et al. (2013) to assess the predictive power of the models by comparing predicted misstatements with actual misstatements and non-misstatements, we find that our model correctly predicts around 83% on the misstatements/non-misstatements.

[20] The marginal likelihood effect of *UNI* at the 25th (75th) percentile value of 7 (21) equals 0.1787 (0.1595), implying that the likelihood of a misstatement decreases by 10.74 percent ([0.1595/0.1787] – 1) when moving from the 25th to the 75th percentile of *UNI*, holding all other variables at their mean values. Similarly, the marginal likelihood effect at the 25th (75th) percentile value of 2 (8) equals 0.1779 (0.1600), signalling a 10.06 percent decrease ([0.1600/0.1779] – 1) in the likelihood of misstatements from an interquartile shift in *ACCUNI*, holding all other variables at their mean values.

The results for most of our control variables are consistent with our expectations and the findings of prior studies (Francis et al., 2013; Kohlbeck and Mayhew, 2017; Newton et al., 2013). While our results for market-to-book (*MTB*), return on assets (*ROA*), and industry-leading auditor (*LEADER*) are contrary to our expectations, they are consistent with other recent studies showing higher rates of misstatements for low growth firms (Francis et al., 2013), firms reporting high levels of profits (Newton et al., 2013), and clients of industry specialist auditors (Francis et al., 2013; Kohlbeck and Mayhew, 2017; Newton et al., 2013).

5. Additional results and robustness tests

5.1 Instrumental variable approach

A potential endogeneity concern in our analyses is that there are unobservable variables that affect both audit offices' proximity to universities and misstatements. We address this concern using a two-stage least-squares (2SLS) approach, employing the amount of railroad tracks in a MSA in 1898 (*RAIL*)[21] as the instrumental variable. Duranton and Turner (2012) find that the construction of early railroad tracks paved the way for highways that instigated significant population and employment growth in U.S. cities. In fact, Atack et al. (2010) find that at least 50 percent of the growth in urbanization in the Midwest may be attributable to the spread of rail networks. Given that the increase in transportation and business infrastructure causes complementary changes in the demand for educational institutions (Duranton and Turner, 2012) and that audit offices are more likely to operate where there are more client firms, the coverage levels of MSA railroad tracks in 1898 is likely to be an indicator of audit offices that are in proximity to more universities. Moreover, the 1898 MSA railroad tracks also should not directly affect misstatements (i.e., the dependent variable in the second stage

[21] Details on the construction of the *RAIL* variable can be found in Duranton and Turner (2012). We thank Gilles Duranton and Matthew A. Turner for providing the railroad tracks data.

regression).[22] In support, we find a high correlation between our two proximity measures (*UNI* and *ACCUNI*) and *RAIL* (correlation coefficients = 0.68 and 0.51), while repeating our main misstatement analysis in Table 2 after including *RAIL* yields an insignificant coefficient on *RAIL* (untabulated).

Columns (1) and (3) of Table 3 present the results from the first stage of our 2SLS analysis. In line with our expectation, we find a positive and highly significant ($p < 0.01$) relation between *RAIL* and *UNI* (*ACCUNI*) after controlling for the exogenous variables from equation (1). Further, untabulated results from the Montiel Olea and Pflueger (2013) weak instrument tests reject the null hypothesis of weak instruments (*F*-statistics = 688.250 and 82.498). The results from the second stage of our 2SLS analysis, which reestimates equation (1) after replacing our test variables with their predicted values from the first stage regressions, are reported in Columns (2) and (4) of Table 3. We find negative and significant ($p < 0.05$) associations between misstatements and the predicted values of *UNI* and *ACCUNI*. Thus, our main inferences are robust to using a 2SLS approach to correct for any endogeneity bias.

5.2 Quasi-natural experiment

We next conduct a quasi-natural experiment based on the unprecedented devastation caused by Hurricane Katrina to New Orleans in 2005 to identify an exogenous shock to the proximity of audit offices located in New Orleans to their labor market of local university graduates.[23] The damage caused by Katrina displaced many residents and led to the closure of universities in New Orleans for an extended period. This resulted in "the most serious disruption of American higher education in the nation's history… depleting severely the

[22] The instrumental variables approach is only appropriate if the instrumental variables are correlated with the endogenous independent variable, but unrelated to the error term of the second-stage regression (Larcker and Rusticus, 2010).

[23] Drew (2015) provides examples of the strategies that Louisiana-based accounting firms use to attract talent from local universities. These recruitment activities include internships, externships with course credit hours, bi-annual campus visits to conduct interviews, presenting at university career fairs, participating in school networking events, holding meetings for Beta Alpha Psi chapters, and sponsoring scholarships.

student population" (AAUP, 2007). While universities in New Orleans re-opened in 2006, enrolments of new and existing students were below expected figures (AAUP, 2007) and universities faced a long recovery process.[24] Indeed, untabulated tests reveal that the mean number of accounting graduates from universities in New Orleans decreased significantly ($p < 0.01$) by around 19 percent in the four-year period following Hurricane Katrina, in comparison to the preceding four-year period. The number of accounting graduates in all other U.S. cities increased by around 58.5 percent, on average, over the same period.

To assess the impact of the diminished labor supply of accounting graduates on the audit quality delivered by New Orleans audit offices, we use a difference-in-differences (DiD) research design based on a nine-year sample period (2001 to 2009) surrounding the year of Katrina's landfall (2005). Specifically, we regress a proxy for audit quality on a dichotomous variable capturing clients of audit offices located in New Orleans (*NEWORL*),[25] an indicator variable representing the year of and the four years following Hurricane Katrina (*POSTKATRINA*), the interaction of *NEWORL* and *POSTKATRINA*, and the controls.

Also, as a result of insufficient variation in misstatements in clients of audit offices located in New Orleans (the audit quality proxy used in or main tests), we execute our DiD analyses using absolute discretionary accruals, which numerous other studies (e.g., Chen et al., 2008; Chi et al., 2012; Choi et al., 2010; Lim and Tan, 2008) have relied on to make inferences about audit quality. Following prior literature, we estimate absolute discretionary accruals using the cross-sectional performance-adjusted modified Jones (1991) model (Kothari et al., 2005). The control variables include those employed in prior studies as well as the audit office-level and city-level controls from our main analysis in Table 2. We also include year and industry fixed effects and cluster standard errors by firm and year. All variables used in this

[24] Congressional records state that the student population of New Orleans colleges and universities was still almost 30 percent lower in 2008, three years after Katrina's landfall (USGPO, 2008).
[25] This treatment sample consists of 113 firm-year observations audited by an audit office located in New Orleans between years 2001 to 2009.

model are defined and described in Appendix B.

The results from the estimation of the DiD analysis are reported in column (1) of Table 4.[26] The insignificant coefficient on *POSTKATRINA* implies that the level of unsigned abnormal accruals did not significantly change for other U.S. firms in the period following Hurricane Katrina. However, the positive and significant ($p < 0.05$) coefficient on the interaction term *NEWORL* x *POSTKATRINA* indicates relatively higher abnormal accruals in the post-Katrina period for clients of audit offices based in New Orleans. This result is consistent with the view that the audit quality of New Orleans audit offices is negatively affected by the depletion of the labor market of university graduates in New Orleans.[27]

It is possible that the reduction in the audit quality of New Orleans' audit offices is partially explained by the poorer performance of the audit office employees due to stress from the aftermath of Hurricane Katrina (DeSalvo et al., 2007). We attempt to account for this effect by repeating our DiD analysis after redefining our treatment group as audit offices located in Shreveport (*SRVPRT*). Assessments conducted by the Louisiana Geographic Information Center reveal that Shreveport was the only city in Louisiana that was not impacted by Hurricane Katrina (LAGIC, 2005). This allowed the city to accommodate an estimated 25,000 people from the hurricane-affected areas of southern Louisiana (USDHUD, 2008), which would have

[26] We follow the approach used by Lennox (2016) to formally test whether the treatment and control firms exhibit parallel trends in the unsigned discretionary accruals in the period prior to Katrina's landfall (2001 to 2004). This approach entails regressing discretionary accruals on our indicator variable for audit offices located in New Orleans (*NEWORL*), a time trend variable (*TREND*), and a variable capturing the interaction between *NEWORL* and *TREND*, whereby *TREND* equals 1, 2, 3, and 4 in the year 2001, 2002, 2003, and 2004, respectively. Consistent with the parallel trend assumption, we find that the interaction term yields an insignificant coefficient in this estimation, confirming that there were no significant differences in the trends of discretionary accruals in our treatment and control firms.

[27] Interestingly, we find that there are no audit clients headquartered in New Orleans in the sample. Nonetheless, we examine the annual reports of clients of audit offices based in New Orleans and find a handful of clients that discuss the negative effect of Hurricane Katrina on their operations in 2005 and 2006, which could potentially impact our results (such firms adopting aggressive financial reporting practices). While the use of performance-adjusted discretionary accruals in our Table 4 analysis may alleviate this concern, we document similar results to those reported in Table 4 when we exclude the years 2005 and 2006 from our analysis in Table 4 (untabulated). The reduction in the audit quality of New Orleans audit offices may also be partially explained by an audit office's pre-Hurricane Katrina workforce relocating to other cities. While this is possible, anecdotal evidence suggests that audit employees working in New Orleans moved back following the disaster (PwC, 2005) The above-mentioned sensitivity analysis after the exclusion of 2005 and 2006 further alleviates this concern.

included displaced New Orleans graduates and students seeking to remain in Louisiana. To the extent that this enhances the labor market of audit offices based in Shreveport, we expect an improvement in the audit quality of these audit offices in the aftermath of Katrina. In line with this expectation, the results from this DiD estimation, reported in Column (2) of Table 4, reveal a negative and significant ($p < 0.01$) coefficient on the interaction of *SRVPRT* and *POSTKATRINA*.

We also repeat this analysis after defining our treatment audit offices as those located in Texas and Georgia (*TEXGEO*), which were the two outside states that attracted most of the displaced New Orleans population (Sastry and Gregory, 2014). The results from this analysis are reported in the last column of Table 4 also depict a negative and significant ($p < 0.05$) coefficient on the interaction of *TEXGEO* and *POSTKATRINA*. While this suggests an improvement in the audit quality of Texas and Georgia audit offices in the post-Katrina period, we interpret this result with caution. This is because this state-level analysis is unable to isolate cities within these states that may have accommodated most of the displaced population.

5.3 Change regression specification

We also implement a change regression specification by regressing the annual change in our audit quality proxy (*MISSTATE*) on the annual changes in our proximity measures (*UNI* and *ACCUNI*) and control variables. It is important to note that changes in our proximity measures are mostly driven by firms switching to audit offices that are in proximity to more or fewer universities as we find relatively few occurrences of new universities being established and existing universities closing during our sample period.

The results from these analyses, reported in the first two Columns of Table 5, indicate negative and significant ($p < 0.10$) associations between changes in misstatement rates and changes in our proximity proxies based on both all universities (Column 1) and accredited universities (Column 2). Further, results reported in the last two columns of Table 5 indicate

that large positive changes (changes in the highest quartile) in our proximity proxies reduce the likelihood of misstatements ($p < 0.05$) while large negative changes (changes in the lowest quartile) in our proximity measures increase the probability of misstatements ($p < 0.10$ or better). Collectively, the results from our change regressions suggest lower (higher) likelihood of misstatements in audit clients that switch to audit offices that are in proximity to a larger (smaller) labor market (pool) of universities. These results are consistent with our main results.

5.4 Accounting for client-universities proximity

We recognize that it is important to account for the fact that proximity to more universities could also allow clients to attract higher quality staff who positively affect financial reporting quality.[28] Thus, we commence by controlling for client firm proximity to universities in our main analysis. Similar to our audit office proximity measures, we capture client firm proximity to universities as the number of universities (accredited universities) within a 25-mile radius to the headquarters of the client firm. We orthogonalize the client and audit firm proximity to universities (and accredited universities) with respect to each other in this analysis because these measures are highly correlated (correlation coefficient = 0.70). The results reported in Columns (1) and (2) of Table 6 indicate that our orthogonalized measures of audit firm proximity to universities (*UNI_RES* and *ACCUNI_RES*) remain negatively related to misstatements ($p < 0.01$), after controlling for client firm proximity to universities. We also find that client firm proximity to universities (*CTUNI_RES* and *CTACCUNI_RES*) is negatively associated with misstatements ($p < 0.05$ or better).

Our next two tests entail estimating our main analysis based on a (1) sample of geographically diversified client firms (8,773 client firms with equal or higher than the median geographical segments) and, (2) a smaller sample of client firms that are headquartered in a

[28] Call et al. (2017a) provide support for this view by showing that financial reporting quality is positively related to the education level of the workforce in the Metropolitan Statistical Area in which the firm is headquartered.

foreign country (n = 642). We focus on these firms because these firms are likely to rely on a diversified and/or overseas-based labor market that does not overlap with the labor market of the engagement audit office. The results from these extensions, reported in Columns (3) through (6) of Table 6, indicate that the coefficients on our measures of audit firm proximity to universities remain negative and significant ($p < 0.05$ or better) within these subsamples. Collectively, these results continue to support our inferences on the positive impact of audit office proximity to more universities on audit quality.

5.5 Additional controls and the impact threshold for confounding variables

To further rule out any confounding effects relating to audit clients, we control for other audit client-level operational and governance characteristics that could affect audit quality. These variables include managerial ability (*MA*), complexity of operations (*LNSEG*, *FOREIGN*), effectiveness of internal controls (*ICOP*), absence of CEO duality (*DUALITY*), board independence (*INDDIR*), board members with qualifications (*BRDQUAL*) and board size (*BRDSIZE*). While these inclusions result in a significant attrition in sample (n = 13,612), the results from these analyses, reported in Table 7, do not affect our inferences. Further, none of the additional controls present statistically significant coefficients except for *ICOP*, which implies a lower likelihood of misstatements in firms with effective internal controls.

In untabulated analyses, we find that our results remain robust to controlling for factors such as population, total employment, unemployment rates, and per capita income at the zip-code-, CBSA-, and state-levels. We also include audit firm fixed effects to rule out that our results are driven by audit firm characteristics and employ a probit regression model to assess whether our results are sensitive to a change in estimation technique. Our results remain robust.

While we generally find that our test variables are moderately correlated with our state- or city-level controls (largest correlation coefficient = 0.248), we undertake several other untabulated analyses to further alleviate the concern that our results are driven by omitted state,

city or firm conditions. First, we orthogonalize *UNI* and *ACCUNI* with respect to all our state- and city-level control variables and find consistent results using the orthogonalized measures, which are not correlated with the state- and city-level controls. Second, we find that our results are robust to removing observations in the top or bottom decile of our state- and city-level controls reflecting state/city attractiveness (e.g., top decile of increase in per capita income in city, bottom decile of state income tax rate). Third, we follow Frank (2000) and compute the percentage bias to invalidate our inferences. We find that around 67.44% (56.94%) of the estimates for *UNI* (*ACCUNI*) would have to be due to bias to invalidate our inferences, suggesting our inferences are reasonably robust. Finally, we follow recent studies and estimate the impact threshold for a confounding variable (ITCV) for our test variables using a linear probability model (Call et al., 2017b; Christensen, 2016; Larcker and Rusticus, 2010). The computed ITCV for *UNI* (*ACCUNI*) is -0.0211 (-0.0135), indicating that an omitted variable would have to be correlated at 0.145 (=√0.0211) with *UNI* and *MISSTATE*, or 0.116 (=√0.0135) with *ACCUNI* and *MISSTATE*, to overturn our results. To assess the likelihood of such a variable existing, we examine the comparable impacts of our control variables. These values are considerably lower (ranging between 0.0091 and -0.0063) than the ITCVs for *UNI* and *ACCUNI*, suggesting that an omitted variable would need to have stronger correlations with our test and dependent variables, relative to our extensive collection of control variables, to invalidate our significant results for *UNI* and *ACCUNI.*

5.6 Coarsened exact matching and entropy balancing

To further underline the robustness of our main findings, we consider two other methods that have been increasingly applied in accounting studies to address endogeneity bias, namely coarsened exact matching (CEM) and entropy balancing.[29] We execute our CEM

[29] CEM is an application of conventional exact matching and seeks to improve causal inferences by improving covariate imbalance between clients of audit offices that are in proximity to more and fewer universities. King, Nielsen, Coberley, Pope, and Wells (2011) and DeFond, Erkens, and Zhang (2016) show that CEM outperforms

analysis by transforming our two continuous test variables (*UNI* and *ACCUNI*) into dichotomous measures (*UNID* and *ACCUNID*). We use Stata's default binning algorithm based on Sturge's (1926) rule to coarsen the continuous control variables into bins (subgroups), except that we use two bins to stratify our continuous state-level variables.[30] This results in considerably smaller subsamples for the CEM-based analyses on *UNID* (n = 6,671) and *ACCUNID* (n = 5,412). In contrast to the CEM method, entropy balancing allows us to retain our original sample (n = 37,916) but uses an algorithm to reweight the covariates such that there are no significant post-weighting differences in the mean values of these covariates across our treatment and control firms (Hainmueller, 2012). The results from our CEM-based (entropy balancing) analyses, reported in first (last) two columns of Table 8, continue to return negative and significant coefficients ($p < 0.10$ or better) on *UNID* and *ACCUNID*. These findings are again consistent with our main results in Table 2.

5.7 Additional audit office-based evidence

Next, we investigate whether the audit quality effect of audit offices' proximity to more universities is driven by two settings where greater proximity to universities is expected to be more salient. This approach can improve identification (He et al., 2017; Nunn, 2007; Rajan and Zingales, 1998) by making it challenging for an alternative argument to explain not only our the main effect but also how the main effect varies across proposed conditions.[31]

The first setting we consider is the competition for a given pool of labor suply from universities. We expect that audit offices will realize greater benefits from being proximate to more universties (i.e., larger labor supply) when they are exposed to greater competition for

propensity score matching by providing better covariate balancing between treatment and control firms. Our main results remain robust when we apply a propensity score matched approach in place of CEM.

[30] The use of the default binning option for our state-level variables does not allow Stata to estimate the CEM analysis as it produces only a handful of matched firms. This could be possibly due to the requirement of achieving balance across a large number of covariates in our analysis as well as the smaller range of possible values that the state-level variable can take in our sample.

[31] For example, Rajan and Zingales (1998) evaluate the effect of financial development on economic growth and state that "One way to make progress on causality is to focus on the details of theoretical mechanisms through which financial development affects economic growth, and document their working".

talent from other audit firms in the same city. To investigate this, we repeat our main analyses in Table 2 after partitioning our sample into two sub-samples reflecting cities with lower or higher concentration of audit firms (based on the median concentration value in our sample).[32] The second factor that can moderate the effect of our proximity measures on audit quality is office size. A larger audit office will naturally require a larger labor supply to maintain audit quality because audit quality can deteriorate when auditors face an increased workload (Bills et al., 2016; López and Peters, 2012). As such, proximity to universities should be more important to larger audit offices for maintaining a healthy supply of workforce.[33] To investigate, we reestimate our main analysis in Table 2 after splitting our main sample based on the median size of the audit offices in our sample.

The results based on the concentration (size) of audit offices analyses, reported in Panel A (B) of Table 9. These results indicate a negative and significant ($p < 0.05$ or better) relationship between client misstatements and the proximity of audit offices to more universities and more accredited universities, but only when there are more audit firms in cities and for larger audit offices (Columns (1) and (3) in Table 9). The corresponding results based on cities with a lower concentration of audit firms and smaller audit offices, reported in Columns (2) and (4) of Table 9, are insignificant. This could be due to audit offices in less concentrated cities attracting the best graduates by default or facing lower competition for

[32] We use this approach rather than using an interaction term approach because the use of interactions can be problematic in logistic regressions. Norton et al. (2004) state that "Unfortunately, the intuition from linear regression models does not extend to nonlinear models. The marginal effect of a change in both interacted variables is not equal to the marginal effect of changing just the interaction term. More surprisingly, the sign may be different for different observations. The statistical significance cannot be determined from the z-statistic reported in the regression output. The odds-ratio interpretation of logit coefficients cannot be used for interaction terms."

[33] We were unsuccessful in our attempts to identify data on staff auditor numbers across the audit offices in our sample, which would have allowed us to test whether our proximity measures are more influential in audit offices with a higher concentration of staff auditors. Specifically, while we were able to access datasets from the Book of Lists (which uses the American City Business Journals and Crain's Business Journals as its two sources) to identify the concentration of partners in approximately 15 cities in the U.S., these datasets cover staff in non-audit areas and do not provide specific data on staff auditor numbers. As a pilot exercise, we contacted three audit offices covered on these datasets to request this data on staff auditor numbers but did not receive a response, suggesting that undertaking such an exercise at a larger scale is unlikely to be fruitful.

talent and due to lower turnover of staff and/or lower demand for new staff in small offices.

5.8 Alternative measures of audit quality

Our main tests capture audit quality using misstatements. Next, we evaluate the robustness of our findings to other commonly used proxies of audit quality including (1) the propensity of auditors issuing a going concern modification to clients (e.g., DeFond et al., 2002; Krishnan and Wang, 2014; Li, 2009), (2) absolute discretionary accruals (e.g., Chen et al., 2008; Chi et al., 2012; Choi et al., 2010; Lim and Tan, 2008), and (3) enforcement actions against auditors for failing to meet professional standards for audit practices (e.g., Markelevich and Rosner, 2013; Sundgren and Svanström, 2013).

Following prior studies, our going concern analysis (*GC*) is estimated at the client-level based on a sample of financially distressed firms (i.e., firms with negative net income or negative operating cash flows in the current year). Our firm-level control variables include those from prior studies (e.g., Krishnan and Wang, 2014; Li, 2009) and our base control variables, which comprise of the auditor- and city-level variables as well as the industry and year fixed effects included in our main model. We employ a similar approach to execute our absolute discretionary accruals (*ABSDA*) analysis except that our firm-level control variables derive from prior studies that have relied on *ABSDA* as a proxy for audit quality (e.g., Chen et al., 2008; Chi et al., 2012; Choi et al., 2010; Lim and Tan, 2008).[34] In contrast to the above tests, we estimate our enforcement analysis at the audit-office level since these enforcement actions allow us to identify audit offices that have been linked to deficient audits. As such, our dependent variable in this analysis, *ENFORCE*, is coded one for an audit office in a year if SEC or PCAOB subsequently took action against at least one auditor from that audit office for failing to meet professional auditing standards, zero otherwise. Our controls include averaged

[34] We estimate absolute discretionary accruals (*ABSDA*) using the cross-sectional performance-adjusted modified Jones (1991) model (Kothari et al., 2005). Our results are robust to estimating discretionary accruals using the cross-sectional Jones (1991) model or the Dechow and Dichev (2002) model.

client firm and fee characteristics, audit office characteristics, city-level controls, year fixed effects, as well as state-level fixed effects to control for differences in regulatory enforcement across states.[35] All variables used in the additional analyses are defined in Appendix B.

The results from our alternative audit quality tests are reported in Panel A of Table 10. The results in Column (1) provide some evidence ($p < 0.10$) to suggest that audit offices that are in proximity to more universities are more likely to issue going concern modifications to financially distressed clients. However, this effect becomes insignificant when we consider proximity to accredited universities (Column 2). The results reported in the last four columns show that both of our proximity measures are negatively and significantly ($p < 0.10$ or better) associated with absolute discretionary accruals and enforcement actions. Thus, on balance, the results from the analyses reported in Panel A of Table 10 are consistent with our main finding of proximity to a larger labor supply of universities enhancing the audit quality of audit offices.

In addition to the above alternative audit quality proxies, we consider instances where auditors are likely to over-rely on internal controls, represented by clients that are likely to be exposed to material weaknesses in their internal controls over financial reporting but no weaknesses are not reported (Bhaskar et al., 2017). Our focus on internal controls is based on the substantial role that staff auditors play evaluating the internal controls of public clients (Abdolmohammadi and Wright, 1987; Abdolmohammadi, 1999; Brown et al., 2016; Trompeter and Wright, 2010).

We execute this analysis by estimating the material weaknesses in internal controls model employed in Bhaskar et al. (2017) to obtain the predicted probability of a material weakness in internal controls existing.[36] We then compute, by year, the cut-off probabilities

[35] We estimate our going concern and unsigned discretionary accruals analyses after clustering standard errors by firm and year, while the standard errors in enforcement analysis are clustered by audit office and year.

[36] Given that internal controls are more likely to be pertinent to an audit when financial statement audits are integrated with internal controls, we follow Bhaskar et al. (2017) and estimate the model based on accelerated filers (firms with more than $75 million in public float) for each year after 2006, which represents the period following the implementation of Auditing Standard No. 5 that requires the performance of integrated audits for

that maximize the overall rate of correct classification (Hosmer and Lemeshow, 2000). A firm-year predicted probability that is above the cut-off indicates the firm-year is likely to have at least one material weakness in internal controls (Bhaskar et al., 2017). We construct our measure of audit quality by coding an indicator variable, *MATWEAK*, as one if the firm-year was estimated to have a material weakness in internal controls (based on the material weakness likelihood model) but the firm did not report any material weaknesses, and zero otherwise. We also proxy for audit quality using another measure, *MISSTATE|MATWEAK*, coded one if the firm was estimated to have an existing material weakness that was not reported *and* financial statements were subsequently restated, and zero otherwise. The results reported in Panel B of Table 10 show that *MATWEAK* and *MISSTATE|MATWEAK* are both negatively and significantly ($p < 0.05$ or better) associated with our proximity measures. These results suggest that proximity to more universities reduces the likelihood of deficiencies in an area salient to staff auditors, namely over-relying on materially weak internal controls that are not reported (and in firms that misstate their financial statements).

5.9 Varying distances to universities

Our main analysis considers audit office labor market supply by the number of universities within 25 miles of an audit office (*UNI*). We next test the sensitivity of our findings to defining an audit office's labor market as the closest four, five and six universities and measuring our proximity variables as the natural logarithm of the average distance of an audit office to these universities (*AVEDISTUNI*). The results from this analysis, reported in Panel A of Table 11, depict positive and significant ($p < 0.05$ or better) coefficients on *AVEDISTUNI*, suggesting higher likelihood of misstatements in clients when audit offices are located further away from their labor market of universities.

accelerated filers. Specifically, we estimate the following model: $Pr(MWi,t) = \beta_0 + \beta_1 LNMVi,t + \beta_2 LNAGEi,t + \beta_3 LNSEGi,t + \beta_4 FOREIGNi,t + \beta_5 MNAi,t + \beta_6 RESTRUCTi,t + \beta_7 ARINVi,t + \beta_8 CHGTAi,t + \beta_9 CFOi,t + \beta_{10} LOSSi,t + \beta_{11} MTBi,t + \beta_{12} LITi,t + \beta_{13} INTGi,t + \beta_{14} AUDRESi,t + \beta_{15} RESANCi,t + \beta_{16} MWLAGi,t + INDUSTRY + \varepsilon i,$ All variables employed in this analysis are defined and described in Appendix B.

We also investigate the effects of our test variables, *UNI* and *ACCUNI*, after re-calculating them based on a 10-, 30-, 40-, 50- and 60-mile radius around audit offices.[37] The results from these analyses are reported in Panels B and C of Table 11. We find that our results for *UNI* and *ACCUNI* remain unchanged with the size of the effects (coefficients) increasing monotonically as we move towards smaller concentric circles (e.g., 10-mile) to define labor market proximity. We complement this analysis by simultaneously considering the number of proximate universities based on several bands of concentric rings: within 25 miles, between 25 to 50 miles and between 50 to 100 miles. The results from this extension, reported in Panel D of Table 11, indicate that audit quality is positively affected by the concentration of universities located within 25 miles of an audit office. In contrast, the concentration of universities located between 25 to 50 miles and between 50 to 100 miles of an audit office do not appear to affect audit quality. These findings indicate that our results from Panels B and C are largely driven universities that are located within 25 miles of an audit office.

5.10 Considering university attributes

We also consider two alternative university attributes in deriving our proximity measures: total population size of universities (regardless of disciplines/degrees) (*UNIPOP*) and total number of university accounting graduates in the prior year (*UNIGRAD*). For sake of consistency, these additional measures are also based on universities that are within a 25-mile radius to an audit office. The results from these analyses, reported in Table 12, continue to reveal a positive relationship between audit quality and these alternative proxies of proximity.

5.11 Other additional tests

We conduct several other untabulated tests for robustness. First, although the inclusion of year fixed effects in our analyses are expected to absorb the effects of macroeconomic

[37] We restrict this analysis to a 60-mile proximity radius because informational advantages arising from geographical proximity has a threshold of 100km, approximately 62 miles (Kedia and Rajgopal, 2011).

shocks such as the global financial crisis and the enactment of the Sarbanes Oxley Act of 2002 (SOX), we find that our findings remain robust after excluding years of the global financial crisis (2007 to 2009) or years prior to the introduction of SOX (pre-2001). Second, while we follow recent studies (Francis et al., 2013; Kohlbeck and Mayhew, 2017; Paterson and Valencia, 2011) and include firms in the financial services (SIC codes 6,000 to 6,999) and utilities (SIC codes 4,900 to 4,999) industries in our main analysis, our results are robust to the exclusion of these industries that are more heavily regulated. Third, while our approach of considering universities within a 25-mile radius of an audit office allows for appointment of graduates from neighboring states, to account for factors (e.g., differences in CPA sitting requirements) that may constrain cross state mobility, we find consistent results after repeating our analyses based on universities that are located in the same state as the audit office. Finally, we evaluate whether audit quality is affected by the proximity of audit offices to flagship universities, which may be more attractive to audit offices for recruiting. The results from this analysis reveal that audit quality is unaffected by the number of flagship universities (as identified in Rizzo and Ehrenberg, 2004) that are located within 25 miles of an audit office. The inclusion of this variable does not affect the results for our main proximity measures.

6. Conclusion

Spurred by the significant role that universities play in the educational requirements of staff auditors in audit firms, this study examines how audit quality is affected by the proximity of audit offices to more universities. Our main findings reveal a positive relation between number of universities (and also accredited universities) within 25 miles of an audit office and audit quality, proxied by misstatements. This finding is robust to an instrumental variable approach and a change regression specification. We further corroborate our main findings using an exogenous shock, namely the landfall of Hurricane Katrina, that displaced university students in New Orleans in 2005. Results from these analyses indicate a deterioration in the

audit quality of audit offices in New Orleans, which experienced a weakening in their local university labor market, and an improvement in the audit quality of audit offices in Shreveport, Texas and Georgia, which were best positioned to attract most of the displaced population of students. Our findings are robust to a battery of other sensitivity checks including other proxies of audit quality and alternative ways of defining the labor market of universities. Collectively, these findings extend prior studies on audit quality and geographical proximity.

Our analyses are subject to several caveats. First, while we closely follow prior studies in measuring audit quality and base our test variables on the view that the quality of staff recruiting in audit offices is enhanced by proximity of audit offices to more universities, our proxies may not perfectly capture audit quality and the labor market supply of audit offices. However, this should bias against us finding consistent results across all our analyses. Second, while our main findings are robust to a number of tests that seek to address various endogeneity problems, similar to all archival studies, our study documents association and not causation. Finally, due to the unavailability of qualitative data, we are unable to provide direct evidence on the specific channels (e.g., superior technical skills, desired personality traits, greater diversity in audit teams) through which the proximity of audit offices to more universities enhances audit quality. These limitations serve as fruitful avenues for future research.

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Appendix A: Construction of Proximity Measures

A.1 Data on audit office locations

We obtained a list of 390 unique Big 4 audit office locations at the city-level from AuditAnalytics. AuditAnalytics provides information on the city, state and region of the Big 4 audit office, but does not provide the exact address for audit offices.[38] Therefore, to construct our measure of proximity of audit offices to universities, we had to obtain the address and zip code for each audit office. This was primarily performed by hand collecting audit office addresses from the websites of the Big 4 audit firms.

We were unable to obtain addresses for 116 audit offices from the Big 4 audit firms' websites because of audit office closures. For these missing cases, we manually searched for the address and zip code using internet search engines and the historical websites of Big 4 audit firms on the Internet Archive, a non-profit library that archives the Internet (https://archive.org/). From this search, we obtained the addresses of another 94 audit offices. After removing observations with missing data needed to construct the variables in our sample, our final sample consists of 300 unique audit offices in 127 cities from 46 states. The distribution of the number of audit offices of the Big 4 firms in our sample are as follows:

Big 4 audit firm	Number of audit offices	Percentage
Deloitte & Touche LLP	71	23.67
Ernst & Young LLP	79	26.33
KPMG LLP	79	26.33
PricewaterhouseCoopers LLP	71	23.67
Total	300	100.00

A.2 Data on universities

We obtain a list of US universities from the National Center for Education Statistics (NCES) website (https://nces.ed.gov/collegenavigator/). As our test variables relate to the labor

[38] AuditAnalytics provides the address of an audit firm's headquarters, but not the addresses of individual audit offices.

supply potentially available to audit offices, we capture the pool of university students that are potentially likely to work for audit firms by restricting our list of universities to those that offer an accounting program/major using the filter feature that is available on the NCES website (filter: Accounting and Related Services). We obtain this list by developing a software program that captures the name, unique university identifier (hereafter, university ID) and address (including the zip code) of these universities from the NCES website.

Next, we obtain a list of universities with an accredited business school in the US from the Association to Advance Collegiate Schools of Business (AACSB) website (http://www.aacsb.edu/accreditation/accredited-members/global-listing). The list consists of 524 accredited business schools. We hand collect the name, unique university ID and address (including the zip code) of these universities from the AACSB website.

A.3 Construction of the main measures of audit office's proximity to universities

We treat the audit office as being situated at the geographic centroid of the audit office's zip code. There is no established guideline or rule in selecting a radius to calculating proximity between an audit office and a university. Our primary measures of proximity use a 25-mile radius because Rosenthal and Strange (2008) find that the benefits of spatial concentration of employment are driven by proximity to college educated workers up to a 25-mile radius. While our primary measures of proximity are constructed using a 25-mile radius, we explore the use of other distance thresholds in additional analyses. As such, in describing the construction of our proximity measures below, we specify our distance threshold as *N*th-mile.

We measure the proximity of audit offices to universities within an *N*th-mile radius extending out from the audit office's zip code centroid. To do so, we develop a software program that for each audit office's zip code from the dataset described in A.1, calculates the distance (in miles) between the geographic centroid of each audit office's zip code and the

geographic centroid of each university's zip code. We do this both for the list of universities with an accounting program, and the list of universities with an accredited business school (i.e., our two main measures of audit office's proximity to universities).

Next, we generate a unique identifier that identifies a combination of an audit office's zip code and a university ID because there could be several audit offices located within the same zip code. Using this unique identifier, we remove duplicate observations to avoid over-counting the number of universities that are proximate to the zip code of an audit office. Based on the geographic distance computed between the geographic centroid of an audit office's zip code and a university's zip code, we then construct our measures of proximity by summing the number of universities (*UNI*), and the number of accredited universities (*ACCUNI*), that are located within an *N*th-mile radius. Following this, we merge this dataset with the file that contains audit office location (as described in A.1) using the zip code of an audit office.

A.4 Construction of audit office's proximity to universities using concentric rings

As a further analysis, we create concentric ring variables that measure proximity of audit offices to universities at various distances from a given audit office's location. Following Rosenthal and Strange (2008), we create the following concentric rings: within 5 to 25 miles; between 25 to 50 miles and between 50 to 100 miles. Similar to as before, we treat the audit office as being situated at the geographic centroid of the audit office's zip code. We then draw concentric rings around the geographic concentric of each audit office zip code and measure the number of universities (and number of accredited universities) within each concentric ring that extends out from the geographic concentric of the audit office zip code.

Appendix B: Variable definitions

Variable	Definition	Data source
ABSDA	Absolute value of discretionary accruals estimated as the residual from the Jones (1991) model, modified with return on assets as suggested by Kothari et al. (2005). The model is estimated by industry-year, where industries are defined using two digit SIC codes. We use all firms with sufficient data on Compustat and require at least 10 firms per industry-year to estimate this variable.	Compustat
ACCUNI	Number of accredited universities within a 25-mile radius of the audit office.	AACSB website
ACCUNI_RES	Residuals computed when *ACCUNI* is regressed on the number of accredited universities within a 25-mile radius of the client's headquarters.	AACSB website
ACCUNID	Indicator variable coded one if *ACCUNI* is equal or greater than the median, 0 otherwise.	
ARINV	Receivables and inventory divided by total assets.	Compustat
AUDCLIENT	Number of clients of the audit office.	Compustat
AUDRES	Indicator variable coded one if there was an auditor resignation in the prior year, zero otherwise.	AuditAnalytics
AUDSIZE	Natural logarithm of one plus total audit fees per audit office in each industry-year.	AuditAnalytics
BRDQUAL	Average number of qualifications of the board.	BoardEx
BRDSIZE	Natural logarithm of the number of directors on the board.	BoardEx
CFO	Cash flows from operations divided by total assets.	Compustat
CHGINC	One-year change in per capital income of a city.	US Bureau of Economic Analysis
CHGPOP	One-year change in population of a city.	US Bureau of Economic Analysis
CHGSALE	Change in sales divided by total assets.	Compustat
CHGTA	Change in total assets during the year.	Compustat
CLIENTIMP	Audit fees of the client firm divided by audit fees of all client firms in a given year.	AuditAnalytics
CLIENTPROX	Indicator variable coded one if audit office and client firm are located in the same city, zero otherwise.	AuditAnalytics
CTACCUNI_RES	Residuals computed when the number of accredited universities within a 25-mile radius of the client's headquarters is regressed on *ACCUNI*.	AACSB website
CTUNI_RES	Residuals computed when the number of universities within a 25-mile radius of the client's headquarters is regressed on *UNI*.	National Center for Education Statistics
CURR	Current assets divided by total current liabilities.	Compustat

Variable	Definition	Data source
DUALITY	Indicator variable coded one if the CEO and chairperson are the same person, zero otherwise.	BoardEx
ENFORCE	Indicator variable coded one for an audit city-level office in a year if SEC or PCAOB subsequently took action against auditor(s) from that audit city-level office for failing to meet professional auditing standards, zero otherwise.	SEC's AAER and PCAOB's enforcement database
FEERATIO	Ratio of non-audit fees to total audit fees.	AuditAnalytics
FOREIGN	Indicator variable coded one if the firm paid foreign taxes in the year, zero otherwise.	Compustat
GC	Indicator variable coded one if a going concern opinion was issued for the year, zero otherwise.	AuditAnalytics
ICOP	Indicator variable coded one if auditor's opinion is that internal controls are effective, zero otherwise.	AuditAnalytics
INDDIR	Percentage of independent board members.	BoardEx
INTG	Indicator variable coded one if the firm had an integrated audit, zero otherwise.	AuditAnalytics
ISSUE	Indicator variable equal to one if the sum of debt or equity issued during the past three years is greater than five percent of total assets, zero otherwise.	Compustat
LAGACCR	One-year lagged total accruals. Accruals are computed as income before extraordinary items minus operating cash flows, deflated by lagged total assets.	Compustat
LAGGC	Lagged one-year GC.	AuditAnalytics
LEADER	Indicator variable equal to one if the audit office has the highest market share in an industry for a given year, where industries are defined using two-digit SIC codes. Market share is calculated as total audit fees of an audit office divided by total audit fees in an industry for a given year.	AuditAnalytics
LEV	Total liabilities divided by total assets.	Compustat
LIT	Indicator variable equal to one if the firm operates in a high-litigation industry and zero otherwise. High litigation industries are those with SIC codes of 2833–2836, 3570–3577, 3600–3674, 5200–5961, and 7370.	Compustat
LNAGE	Natural logarithm of firm age. Firm age is calculated as year minus the year of IPO.	Compustat
LNEDU	Natural logarithm of the total number of individuals 18 years old and above holding a Bachelor's degree or higher qualification living in a CBSA. CBSA education data is available from years 2009 to 2015. We code firm-year observations prior to 2009 with 2009 values, and code firm-year observations in 2016 using 2015 values.	American Community Survey
LNFEES	Natural logarithm of one plus audit fees.	AuditAnalytics
LNMV	Natural logarithm of market capitalization.	Compustat

Variable	Definition	Data source
LNSEG	Natural logarithm of the number of business and geographical segments.	Compustat
LNTA	Natural logarithm of total assets.	Compustat
LNTOTFEES	Natural logarithm of one plus total audit fees per audit office.	AuditAnalytics
LOSS	Indicator variable coded one if the firm reports a loss for the year, zero otherwise.	Compustat
LTD	Long term debt divided by total assets.	Compustat
MA	Managerial ability score based on Demerjian et al. (2012).	Demerjian et al. (2012)
MW	Indicator variable coded one if a firm receives or discloses at least one material weakness, zero otherwise.	AuditAnalytics
MWLAG	Indicator variable coded one if a firm receives or discloses at least one material weakness in the prior year, zero otherwise.	AuditAnalytics
MATWEAK	Indicator variable coded one if the material weakness likelihood model estimates a material weakness but a material weakness was not received or disclosed, zero otherwise.	AuditAnalytics
MISSTATE	Indicator variable coded one if the financial statements of the year was subsequently restated, zero otherwise.	AuditAnalytics
MNA	Indicator variable coded one if the firm engaged in a merger or acquisition in the year, zero otherwise.	Compustat
MTB	Market to book ratio.	Compustat
NEWORL	Indicator variable coded one if the audit office is located in the city of New Orleans, zero otherwise.	AuditAnalytics
PCAOBPROX	Indicator variable coded one if the audit office is located in the same city as the PCAOB headquarters (Washington, DC) or its regional offices (Ashburn, VA; Atlanta, GA; Boston, MA; Charlotte, NC; Chicago, IL; Denver, CO; Fort Lauderdale, FL; Irvine, CA; Irving, TX; Houston, TX; Los Angeles, CA; New York City, NY; Philadelphia, PA; San Mateo, CA; and Tampa, FL), 0 otherwise.	Compustat
POSTKATRINA	Indicator variable coded one for years 2005 to 2009 and 0 for years 2001 to 2004.	
RAIL	Natural logarithm of the sum of 1898 railroad tracks (km.) in a metropolitan statistical area.	Duranton and Turner (2012)
RESANC	Indicator variable coded one if the firm announced a restatement during the year, zero otherwise.	AuditAnalytics
RESTRUCT	Indicator variable coded one if the firm recorded a restructuring charge in the year, zero otherwise.	Compustat
ROA	Return on assets calculated as net income divided by total assets.	Compustat
ROAVOL	Volatility of return on assets over the prior three years.	Compustat

Variable	Definition	Data source
SECPROX	Indicator variable coded one if the client firm's headquarters is located in the same city as the SEC headquarters (Washington, DC) or its regional offices (New York City, NY; Miami, FL; Chicago, IL; Denver, CO; Los Angeles, CA), 0 otherwise.	Compustat
SRVPRT	Indicator variable coded one if the audit office is located in the city of Shreveport, zero otherwise.	AuditAnalytics
SWITCH	Indicator variable coded one if a firm switched auditors during the year, zero otherwise.	Compustat
TAX	State-level income tax rate based on the average salary of an accountant working in the state for a year. The average salary of accountants for each state is obtained from the US Bureau of Labor Statistics.	US Bureau of Labor Statistics; Urban-Brookings Tax Policy Center
TEXGEO	Indicator variable coded one if the audit office is located in the state of Texas or Georgia, zero otherwise.	AuditAnalytics
UNEMPLOY	Unemployment rate in the CBSA in the year.	American Community Survey
UNI	Number of universities within a 25-mile radius of the audit office.	National Center for Education Statistics
UNI_RES	Residuals computed when *UNI* is regressed on the number of universities within a 25-mile radius of the client's headquarters.	National Center for Education Statistics
UNID	Indicator variable coded one if *UNI* is equal or greater than the median, 0 otherwise.	
UNIGRAD	Natural logarithm of one plus the number of accounting graduates of universities within a 25-mile radius of the audit office in the prior year.	National Center for Education Statistics
UNIPOP	Natural logarithm of the size of the population of universities within a 25-mile radius of the audit office.	National Center for Education Statistics
ZSCORE	Altman (1968) Z-score, calculated as: Z = 1.2 × working capital/total assets + 1.4 x retained earnings/total assets + 3.3 × earnings before interest and taxes/total assets + 0.6 × market value equity/book value of total debt + 0.999 × sales/total assets. ZSCORE is coded two if Z is less than 1.81; ZSCORE is coded one if Z is between 1.81 and 2.99; ZSCORE is coded zero if Z is greater than 3. A lower score indicates a higher probability of firm distress.	AuditAnalytics

Table 1: Descriptive Statistics

Panel A: Descriptive Statistics

	mean	p25	p50	p75	sd
MISSTATE	0.167	0.000	0.000	0.000	0.373
UNI	15.241	7.000	11.000	21.000	12.035
ACCUNI	5.506	2.000	4.000	8.000	4.655
LNTA	6.614	5.232	6.626	7.981	2.048
MTB	2.903	1.192	2.006	3.464	7.386
LEV	0.559	0.339	0.534	0.717	0.438
LOSS	0.317	0.000	0.000	1.000	0.465
CHGTA	0.121	-0.045	0.044	0.156	0.555
MNA	0.157	0.000	0.000	0.000	0.364
SWITCH	0.053	0.000	0.000	0.000	0.225
ROA	-0.055	-0.029	0.027	0.069	0.441
ROAVOL	0.104	0.011	0.030	0.089	0.315
LIT	0.267	0.000	0.000	1.000	0.442
AUDSIZE	14.767	13.648	14.728	15.917	1.592
LNFEES	13.686	12.806	13.707	14.529	1.273
LEADER	0.596	0.000	1.000	1.000	0.491
CLIENTIMP	0.078	0.009	0.026	0.078	0.135
FEERATIO	0.224	0.066	0.175	0.334	0.196
CLIENTPROX	0.339	0.000	0.000	1.000	0.473
SECPROX	0.076	0.000	0.000	0.000	0.264
PCAOBPROX	0.366	0.000	0.000	1.000	0.482
LNEDU	12.898	12.221	13.187	14.001	1.460
CHGPOP	0.007	0.000	0.005	0.013	0.013
CHGINC	0.033	0.006	0.034	0.063	0.050
UNEMPLOY	0.052	0.048	0.051	0.056	0.009
TAX	0.051	0.031	0.053	0.069	0.030

Panel B: Mean of Misstatement by Quartiles of *UNI*

	Quartile of *UNI*				Comparison between highest and lowest quartile
	Q1	Q2	Q3	Q4	
MISSTATE	0.1799	0.1664	0.1605	0.1602	*t* = 3.493***
n	9,855	10,221	9,701	8,139	

Panel C: Mean of Misstatement by Quartiles of *ACCUNI*

	Quartile of *ACCUNI*				Comparison between highest and lowest quartile
	Q1	Q2	Q3	Q4	
MISSTATE	0.1726	0.1684	0.1649	0.1602	*t* = 2.337***
n	10,258	13,467	4,907	9,284	

Panel A reports descriptive statistics for variables in the sample. Panels B and C presents the means of misstatement by quartile of *UNI* and *ACCUNI*, respectively. A higher quartile indicate proximity to more accredited universities. The sample period is between 1999 and 2016. ***, **, * represent significance at the 0.01, 0.05 and 0.10 levels (two-tailed), respectively. All variables are defined in Appendix B.

Table 2: Proximity and Misstatement

	(1) Number of Universities	(2) Number of Accredited Universities
UNI	-0.010***	
	(-3.781)	
ACCUNI		-0.022***
		(-3.036)
LNTA	-0.137***	-0.139***
	(-4.445)	(-4.462)
MTB	-0.008***	-0.008***
	(-2.645)	(-2.650)
LEV	0.061	0.055
	(1.001)	(0.910)
LOSS	0.240***	0.242***
	(5.187)	(5.232)
CHGTA	0.069***	0.069***
	(3.361)	(3.408)
MNA	0.106*	0.107*
	(1.659)	(1.686)
SWITCH	0.317***	0.317***
	(6.734)	(6.762)
ROA	0.191***	0.189***
	(3.391)	(3.367)
ROAVOL	-0.106	-0.104
	(-1.511)	(-1.496)
LIT	0.048	0.058
	(0.512)	(0.624)
AUDSIZE	-0.013	-0.010
	(-0.607)	(-0.461)
LNFEES	0.422***	0.421***
	(7.569)	(7.542)
LEADER	0.087*	0.089*
	(1.868)	(1.900)
CLIENTIMP	-0.718***	-0.689***
	(-2.817)	(-2.691)
FEERATIO	0.526***	0.516***
	(4.230)	(4.135)
CLIENTPROX	-0.128**	-0.124**
	(-2.463)	(-2.383)
SECPROX	0.260***	0.234**
	(2.750)	(2.437)
PCAOBPROX	0.004	0.010
	(0.061)	(0.126)
LNEDU	0.023	0.020
	(1.029)	(0.841)
CHGPOP	1.273	1.499
	(0.846)	(0.957)
CHGINC	-0.249	-0.232
	(-0.720)	(-0.666)

	(1) Number of Universities	(2) Number of Accredited Universities
UNEMPLOY	-6.600**	-5.234*
	(-2.322)	(-1.870)
TAX	0.537	0.675
	(0.584)	(0.723)
Constant	-8.443***	-8.554***
	(-12.464)	(-12.547)
Observations	37,916	37,916
Year dummies	Yes	Yes
Industry dummies	Yes	Yes
Pseudo R^2	0.054	0.053

This table reports the results of audit office proximity to universities (*UNI* or *ACCUNI*) on the likelihood of misstatement (*MISSTATE*). The sample period is between 1999 and 2016. *z*-statistics are reported in parentheses. Standard errors are clustered by firm and year. ***, **, * represent significance at the 0.01, 0.05 and 0.10 levels (two-tailed), respectively. All variables are defined in Appendix B.

Table 3: Two Stage Least Squares Approach

	(1) First stage DV – *UNI*	(2) Second stage DV – *MISSTATE*	(3) First stage DV – *ACCUNI*	(4) Second stage DV – *MISSTATE*
RAIL	3.867***		0.486***	
	(26.301)		(9.103)	
UNI_HAT		-0.015**		
		(-1.997)		
ACCUNI_HAT				-0.113**
				(-1.994)
LNTA	-0.383***	-0.096***	-0.125***	-0.103***
	(-4.611)	(-4.463)	(-3.456)	(-4.746)
MTB	0.006	-0.003	0.000	-0.003
	(1.036)	(-1.562)	(0.172)	(-1.564)
LEV	-0.643***	0.011	-0.164**	0.003
	(-3.865)	(0.223)	(-2.269)	(0.049)
LOSS	0.065	0.154***	-0.003	0.150***
	(0.491)	(3.899)	(-0.061)	(3.793)
CHGTA	0.055	0.027	-0.017	0.023
	(0.976)	(1.172)	(-0.659)	(1.035)
MNA	-0.029	0.073	0.007	0.072
	(-0.183)	(1.591)	(0.107)	(1.589)
SWITCH	-0.139	0.157***	-0.092	0.147***
	(-1.007)	(3.104)	(-1.433)	(2.911)
ROA	-0.104	0.132**	-0.031	0.128**
	(-0.735)	(2.515)	(-0.625)	(2.516)
ROAVOL	-0.056	0.009	-0.028	0.006
	(-0.427)	(0.162)	(-0.420)	(0.107)
LIT	0.075	-0.003	-0.095	-0.014
	(0.154)	(-0.036)	(-0.422)	(-0.178)
AUDSIZE	1.158***	-0.017	0.317***	0.002
	(11.469)	(-0.667)	(7.231)	(0.060)
LNFEES	0.485***	0.290***	0.300***	0.313***
	(2.885)	(7.172)	(4.150)	(7.520)
LEADER	-1.720***	0.036	-0.592***	-0.007
	(-9.954)	(0.852)	(-7.767)	(-0.120)
CLIENTIMP	-5.502***	-0.503***	-1.497***	-0.586***
	(-8.295)	(-3.084)	(-4.986)	(-3.286)
FEERATIO	0.179	0.505***	-0.228	0.471***
	(0.479)	(4.672)	(-1.398)	(4.295)
CLIENTPROX	-1.144***	-0.075	-0.462***	-0.109*
	(-5.286)	(-1.584)	(-4.964)	(-1.945)
SECPROX	0.143	0.234	0.690*	0.308*
	(0.157)	(1.633)	(1.899)	(1.907)
PCAOBPROX	1.528***	0.070	2.753***	0.355*
	(5.407)	(1.025)	(21.361)	(1.927)
LNEDU	0.830***	0.006	0.273***	0.024
	(11.367)	(0.252)	(9.083)	(0.907)
CHGPOP	-21.329***	0.470	-16.983***	-1.173

	(-3.623)	(0.492)	(-4.761)	(-0.783)
CHGINC	2.132**	0.268	0.469	0.277
	(2.349)	(0.834)	(1.105)	(0.863)
UNEMPLOY	-134.980***	-2.880	-48.996***	-6.410
	(-17.796)	(-1.042)	(-16.193)	(-1.643)
TAX	26.752***	0.312	0.167	-0.021
	(8.256)	(0.381)	(0.129)	(-0.028)
Constant	-32.440***	-4.781***	-6.860***	-5.026***
	(-13.787)	(-8.709)	(-6.516)	(-8.794)
Observations	17,795	17,795	17,795	17,795
Year dummies	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes

This table reports the results using a two-stage least squares approach. In the first stage, the dependent variable is our measure of proximity (*UNI* or *ACCUNI*) and the instrumental variable is the amount of 1898 railroad tracks (km.) in a metropolitan statistical area (*RAIL*). In the first stage, the dependent variable is the likelihood of misstatements (*MISSTATE*) The sample period is between 1999 and 2016. *z*-statistics are reported in parentheses. Standard errors are clustered by firm and year. ***, **, * represent significance at the 0.01, 0.05 and 0.10 levels (two-tailed), respectively. All variables are defined in Appendix B.

Table 4: Hurricane Katrina

	(1)	(2)	(3)
TREATED =	*NEWORL*	*SRVPRT*	*TEXGEO*
TREATED	-0.025***	0.004	0.014***
	(-2.943)	(0.289)	(2.628)
POSTKATRINA	0.004	0.004	0.006*
	(1.266)	(1.326)	(1.763)
TREATED ×	0.030**	-0.031***	-0.011**
POSTKATRINA	(2.442)	(-2.738)	(-2.094)
LNTA	-0.013***	-0.013***	-0.013***
	(-9.784)	(-9.783)	(-9.795)
MTB	0.001**	0.001**	0.001**
	(2.380)	(2.383)	(2.393)
LEV	0.013*	0.013*	0.013*
	(1.664)	(1.662)	(1.669)
CFO	-0.092***	-0.092***	-0.092***
	(-4.385)	(-4.386)	(-4.394)
CHGSALE	0.027***	0.027***	0.027***
	(6.649)	(6.649)	(6.657)
ISSUE	0.011***	0.011***	0.011***
	(5.308)	(5.326)	(5.321)
AUDCLIENT	0.000	0.000	0.000
	(0.675)	(0.678)	(1.013)
LOSS	-0.003	-0.003	-0.003
	(-0.727)	(-0.738)	(-0.728)
LAGACCR	-0.003	-0.003	-0.003
	(-0.412)	(-0.412)	(-0.408)
CLIENTIMP	0.004	0.004	0.005
	(0.487)	(0.508)	(0.654)
LNFEES	0.007***	0.007**	0.007**
	(2.577)	(2.566)	(2.553)
LEADER	-0.004**	-0.004**	-0.004*
	(-2.009)	(-1.976)	(-1.876)
AUDSIZE	0.001	0.002	0.001
	(1.321)	(1.329)	(1.234)
FEERATIO	0.018***	0.018***	0.018***
	(2.803)	(2.793)	(2.808)
CLIENTPROX	0.001	0.001	-0.000
	(0.347)	(0.361)	(-0.073)
SECPROX	0.003	0.003	0.004
	(0.534)	(0.536)	(0.890)
PCAOBPROX	-0.006*	-0.006*	-0.007**
	(-1.921)	(-1.904)	(-2.137)
LNEDU	-0.001	-0.001	-0.001
	(-0.465)	(-0.471)	(-0.769)
CHGPOP	0.100*	0.097*	0.089
	(1.823)	(1.796)	(1.642)
CHGINC	0.007	0.007	0.009
	(0.336)	(0.354)	(0.419)

UNEMPLOY	-0.030	-0.031	-0.046
	(-0.174)	(-0.175)	(-0.263)
TAX	-0.012	-0.013	0.017
	(-0.264)	(-0.270)	(0.351)
Constant	0.027	0.027	0.029
	(0.927)	(0.935)	(0.994)
Observations	21,741	21,741	21,741
Adjusted R-squared	0.160	0.160	0.160
Year dummies	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes

This table reports the results of the effect of Hurricane Katrina on affected audit offices' proximity to the labor supply from universities using absolute discretionary accruals as the proxy for audit quality (*ABSDA*). *POSTKATRINA* is an indicator variable coded one for the year of Hurricane Katrina and the four years following Hurricane Katrina, the four years pre-Hurricane Katrina are coded zero. The sample period is between 2001 and 2009. *t*-statistics are reported in parentheses. Standard errors are clustered by firm. ***, **, * represent significance at the 0.01, 0.05 and 0.10 levels (two-tailed), respectively. All variables are defined in Appendix B.

Table 5: Change in Proximity

	(1)	(2)	(3)	(4)
Δ*UNI*	-0.012*			
	(-1.763)			
Δ*ACCUNI*		-0.038*		
		(-1.928)		
Large positive Δ*UNI*			-0.586**	
			(-2.294)	
Large negative Δ*UNI*			0.374*	
			(1.934)	
Large positive Δ*ACCUNI*				-0.552**
				(-2.216)
Large negative Δ*ACCUNI*				0.367**
				(1.982)
Δ*LNTA*				-0.552**
	(-1.356)	(-1.362)	(-1.352)	(-1.364)
Δ*MTB*	-0.040	-0.039	-0.040	-0.038
	(-0.525)	(-0.522)	(-0.528)	(-0.507)
Δ*LEV*	0.002	0.002	0.002	0.002
	(1.352)	(1.351)	(1.389)	(1.364)
Δ*LOSS*	-0.096	-0.096	-0.095	-0.096
	(-1.356)	(-1.362)	(-1.352)	(-1.364)
Δ*CHGTA*	0.024	0.023	0.023	0.023
	(0.758)	(0.746)	(0.727)	(0.719)
Δ*MNA*	-0.046**	-0.047**	-0.046**	-0.047**
	(-2.001)	(-2.005)	(-1.994)	(-2.012)
Δ*SWITCH*	-0.046	-0.046	-0.046	-0.045
	(-1.377)	(-1.369)	(-1.358)	(-1.336)
Δ*ROA*	0.077	0.077	0.077	0.078*
	(1.628)	(1.642)	(1.639)	(1.662)
Δ*ROAVOL*	0.053	0.053	0.053	0.052
	(1.166)	(1.143)	(1.158)	(1.116)
Δ*LIT*	--	--	--	--
Δ*AUDSIZE*	0.055*	0.056*	0.059*	0.059**
	(1.848)	(1.892)	(1.959)	(1.998)
Δ*LNFEES*	0.542***	0.541***	0.541***	0.539***
	(11.263)	(11.250)	(11.225)	(11.194)
Δ*LEADER*	-0.091***	-0.091***	-0.093***	-0.091***
	(-2.652)	(-2.673)	(-2.724)	(-2.665)
Δ*CLIENTIMP*	-0.530**	-0.531**	-0.573**	-0.552**
	(-2.317)	(-2.325)	(-2.471)	(-2.413)
Δ*FEERATIO*	-0.072	-0.072	-0.072	-0.070
	(-0.720)	(-0.716)	(-0.721)	(-0.702)
Δ*CLIENTPROX*	-0.009	-0.019	-0.007	-0.042
	(-0.054)	(-0.109)	(-0.041)	(-0.243)
Δ*SECPROX*	--	--	--	--
Δ*PCAOBPROX*	-0.267	-0.227	-0.175	-0.180

	(1)	(2)	(3)	(4)
	(-1.628)	(-1.314)	(-1.059)	(-1.104)
Δ*LNEDU*	0.004	0.004	0.005	0.006
	(0.182)	(0.193)	(0.252)	(0.326)
Δ*CHGPOP*	2.554***	2.538***	2.453***	2.514***
	(4.500)	(4.486)	(4.388)	(4.452)
Δ*CHGINC*	0.966***	0.966***	0.965***	0.970***
	(4.707)	(4.709)	(4.706)	(4.727)
Δ*UNEMPLOY*	13.349***	13.444***	13.252***	13.526***
	(3.618)	(3.659)	(3.595)	(3.682)
Δ*TAX*	1.852	2.166	1.135	2.211
	(0.619)	(0.718)	(0.380)	(0.711)
Constant	-1.687***	-1.687***	-1.687***	-1.686***
	(-58.490)	(-58.501)	(-58.410)	(-58.370)
Observations	31,571	31,571	31,571	31,571
Pseudo R-squared	0.009	0.009	0.010	0.010

This table reports the results that examine the change in proximity (Δ*UNI* or Δ*ACCUNI*) on the probability of misstatement (*MISSTATE*). *Large positive* Δ*UNI* (Δ*ACCUNI*) refer sto the highest quartile for Δ*UNI* (Δ*ACCUNI*). *Large negative* Δ*UNI* (Δ*ACCUNI*) refers to the lowest quartile for Δ*UNI* (Δ*ACCUNI*). The sample period is between 1999 and 2016. *z*-statistics are reported in parentheses. Standard errors are clustered by firm. ***, **, * represent significance at the 0.01, 0.05 and 0.10 levels (two-tailed), respectively. All variables are defined in Appendix B.

Table 6: Controlling For Client Firm's Proximity To Universities

	(1)	(2)	(3)	(4)	(5)	(6)
	Inclusion of client's proximity to universities		Geographically dispersed client firms		Audit and client firms are located in different countries	
UNI_RES	-0.021***					
	(-3.711)					
CTUNI_RES	-0.015***					
	(-2.620)					
ACCUNI_RES		-0.045***				
		(-3.001)				
CTACCUNI_RES		-0.034**				
		(-2.213)				
UNI			-0.014**		-0.054***	
			(-2.521)		(-2.681)	
ACCUNI				-0.037**		-0.152***
				(-2.564)		(-2.764)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	37,149	37,149	8,773	8,773	642	642
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes	Yes	Yes
Pseudo R^2	0.054	0.054	0.056	0.056	0.179	0.180

This table reports the results of audit office proximity to universities (on the likelihood of misstatement (*MISSTATE*) in a sub-sample that includes client's proximity to universities within a 25-mile radius (*CTUNI_RES* or *CTACCUNI_RES*) in columns (1) and (2); in a sub-sample where client firms have equal or higher than the median geographical segments in columns (3) and (4); and a sub-sample where the client firm is located outside the US in columns (5) and (6). The control variables included follow model (1). The sample period is between 1999 and 2016. *z*-statistics are reported in parentheses. Standard errors are clustered by firm and year. ***, **, * represent significance at the 0.01, 0.05 and 0.10 levels (two-tailed), respectively. All variables are defined in Appendix B.

Table 7: Inclusion of Corporate Governance and Other Firm-Level Controls

	(1) Number of Universities	(2) Number of Accredited Universities
UNI	-0.018***	
	(-3.723)	
ACCUNI		-0.045***
		(-3.128)
MA	-0.176	-0.167
	(-0.510)	(-0.484)
LNSEG	0.030	0.033
	(0.659)	(0.733)
FOREIGN	-0.013	-0.013
	(-0.149)	(-0.146)
ICOP	-1.030***	-1.028***
	(-11.391)	(-11.349)
DUALITY	-0.017	-0.022
	(-0.238)	(-0.300)
INDDIR	-0.465	-0.492
	(-1.170)	(-1.238)
BRDSIZE	-0.078	-0.086
	(-0.821)	(-0.910)
BRDQUAL	-0.070	-0.056
	(-0.913)	(-0.743)
Other controls	Yes	Yes
Observations	13,612	13,612
Year dummies	Yes	Yes
Industry dummies	Yes	Yes
Pseudo R^2	0.066	0.065

This table reports the results of audit office proximity to universities (*UNI* or *ACCUNI*) on the likelihood of misstatement (*MISSTATE*) with the inclusion of corporate governance and other firm-level controls. Other control variables included follow model (1). The sample period is between 1999 and 2016. *z*-statistics are reported in parentheses. Standard errors are clustered by firm and year. ***, **, * represent significance at the 0.01, 0.05 and 0.10 levels (two-tailed), respectively. All variables are defined in Appendix B.

Table 8: Coarsened Exact Matching and Entropy Balancing

	(1)	(2)	(3)	(4)
	Coarsened Exact Matching		Entropy Balancing	
UNI	-0.015***		-0.014***	
	(-3.085)		(-3.163)	
ACCUNI		-0.032**		-0.039*
		(-2.101)		(-1.760)
Controls	Yes	Yes	Yes	Yes
Observations	6,671	5,412	37,916	37,916
Year dummies	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes
Pseudo R-squared	0.073	0.071	0.098	0.106

This table reports the results of audit office proximity to universities (*UNI* or *ACCUNI*) on the likelihood of misstatement (*MISSTATE*) using coarsening exact matching and entropy balancing. The control variables included follow model (1). The sample period is between 1999 and 2016. *z*-statistics are reported in parentheses. Standard errors are clustered by firm. ***, **, * represent significance at the 0.01, 0.05 and 0.10 levels (two-tailed), respectively. All variables are defined in Appendix B.

Table 9: Concentration and Size of Audit Offices, Proximity and Misstatement

Panel A: Concentration of Audit Offices, Proximity and Misstatement

	(1) Greater concentration of audit offices	(2) Lower concentration of audit offices	(3) Greater concentration of audit offices	(4) Lower concentration of audit offices
UNI	-0.015***	-0.008		
	(-4.025)	(-1.390)		
ACCUNI			-0.033***	-0.018
			(-2.935)	(-1.384)
Controls	Yes	Yes	Yes	Yes
Observations	19,046	18,808	19,046	18,808
Year dummies	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes
Pseudo R^2	0.062	0.058	0.061	0.058

This panel report results of audit office proximity to universities (*UNI* or *ACCUNI*) on the likelihood of misstatement (*MISSTATE*) in two sub-samples based on concentration of audit offices equal to or above, and below the median. The concentration of audit offices is captured by the number of audit offices that are located in a city. The control variables included follow model (1). The sample period is between 1999 and 2016. *z*-statistics are reported in parentheses. Standard errors are clustered by firm and year. ***, **, * represent significance at the 0.01, 0.05 and 0.10 levels (two-tailed), respectively. All variables are defined in Appendix B.

Panel B: Size of Audit Offices, Proximity and Misstatement

	(1) Larger audit office size	(2) Smaller audit office size	(3) Larger audit office size	(4) Smaller audit office size
UNI	-0.011***	-0.008		
	(-3.008)	(-1.295)		
ACCUNI			-0.027**	-0.008
			(-2.495)	(-0.551)
Controls	Yes	Yes	Yes	Yes
Observations	18,909	18,955	18,909	18,955
Year dummies	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes
Pseudo R^2	0.059	0.060	0.058	0.060

This panel report results of audit office proximity to universities (*UNI* or *ACCUNI*) on the likelihood of misstatement (*MISSTATE*) in two sub-samples based on size of audit offices equal to or above, and below the median. The size of audit office is captured by the total fees earned by the audit office in the year. The control variables included follow model (1). The sample period is between 1999 and 2016. *z*-statistics are reported in parentheses. Standard errors are clustered by firm and year. ***, **, * represent significance at the 0.01, 0.05 and 0.10 levels (two-tailed), respectively. All variables are defined in Appendix B.

Table 10: Alternate Measures of Audit Quality

Panel A: Alternate Measures of Audit Quality

	(1) *GC*	(2) *GC*	(3) *ABSDA*	(4) *ABSDA*	(5) *ENFORCE*	(6) *ENFORCE*
UNI	0.009*		-0.000***		-0.231***	
	(1.834)		(-2.669)		(-3.232)	
ACCUNI		0.007		-0.001*		-0.440***
		(0.476)		(-1.853)		(-3.453)
LNTA	-0.392***	-0.392***	-0.011***	-0.012***	-0.021	-0.242
	(-4.486)	(-4.477)	(-6.169)	(-6.187)	(-0.032)	(-0.374)
LEV	0.375	0.373	0.007	0.007	2.552	1.336
	(1.139)	(1.130)	(1.252)	(1.237)	(1.077)	(0.443)
CFO	-0.891***	-0.888***	-0.090***	-0.090***	3.918	5.061
	(-4.520)	(-4.528)	(-5.515)	(-5.522)	(0.913)	(1.297)
CURR	-0.184***	-0.185***				
	(-4.446)	(-4.428)				
ROA	-0.189	-0.189			-3.141	-2.801
	(-1.187)	(-1.189)			(-1.433)	(-1.374)
LOSS	1.168***	1.168***	0.002	0.002	0.005	-0.122
	(4.975)	(5.001)	(0.317)	(0.327)	(0.003)	(-0.060)
ISSUE	0.034	0.033	0.010***	0.010***		
	(0.305)	(0.296)	(5.394)	(5.457)		
LTD	-1.341***	-1.337***				
	(-3.536)	(-3.524)				
ZSCORE	0.552***	0.552***				
	(6.085)	(6.048)				
LAGGC	2.344***	2.347***				
	(18.797)	(18.763)				
CLIENTIMP	0.809	0.804	-0.006	-0.006		
	(1.538)	(1.548)	(-1.005)	(-0.919)		
LNFEES	0.176	0.173	0.005	0.005		
	(1.125)	(1.093)	(1.401)	(1.394)		
LEADER	-0.061	-0.070	-0.004***	-0.004***		
	(-0.531)	(-0.602)	(-2.919)	(-2.930)		
AUDSIZE	0.037	0.040	0.002**	0.002**		
	(0.958)	(1.031)	(2.174)	(2.296)		
FEERATIO	-0.504	-0.509	0.024**	0.024**		
	(-1.356)	(-1.381)	(2.553)	(2.518)		
CLIENTPROX	0.112	0.104	0.001	0.001		
	(1.094)	(1.005)	(0.762)	(0.756)		
SECPROX	-0.185	-0.128	0.003	0.003		
	(-1.487)	(-0.976)	(0.813)	(0.768)		
PCAOBPROX	0.039	0.088	-0.008***	-0.007***		
	(0.589)	(1.094)	(-3.765)	(-3.131)		
LNEDU	-0.036	-0.017	0.001	0.001	0.326	0.229
	(-0.595)	(-0.283)	(1.321)	(1.305)	(1.119)	(0.789)
CHGPOP	0.692	-0.407	0.142**	0.142**	15.772	30.343
	(0.158)	(-0.089)	(2.058)	(2.148)	(0.548)	(0.989)
CHGINC	0.473	0.515	0.076*	0.077*	8.861***	8.489***

	(1)	(2)	(3)	(4)	(5)	(6)
	GC	*GC*	*ABSDA*	*ABSDA*	*ENFORCE*	*ENFORCE*
	(0.537)	(0.614)	(1.767)	(1.764)	(4.592)	(6.021)
UNEMPLOY	-13.913	-16.591**	0.064	0.091	-32.984	-55.970
	(-1.606)	(-1.992)	(0.739)	(0.997)	(-1.057)	(-1.558)
TAX	-0.927	-1.057	0.023	0.029	104.426	93.374
	(-0.549)	(-0.654)	(0.614)	(0.810)	(1.261)	(1.116)
MTB			0.001***	0.001***	0.094	0.049
			(3.492)	(3.499)	(1.023)	(0.511)
CHGSALE			0.030***	0.030***		
			(9.695)	(9.696)		
AUDCLIENT			0.000**	0.000*	0.002	0.002
			(1.989)	(1.848)	(1.277)	(1.340)
LAGACCR			-0.006	-0.006		
			(-0.749)	(-0.750)		
LNTOTFEES					1.731***	1.764***
					(3.814)	(4.088)
Constant	-2.872	-2.884	0.034	0.030	-28.363***	-27.924***
	(-1.462)	(-1.400)	(0.804)	(0.711)	(-3.209)	(-3.346)
Observations	8,733	8,733	35,897	35,897	1,553	1,553
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes	Yes	Yes
State dummies	No	No	No	No	Yes	Yes
Pseudo/Adjusted R^2	0.340	0.389	0.161	0.161	0.338	0.316

This table reports the results of alternative measures of audit quality. In columns (1) and (2), we use going concern (*GC*) to proxy for audit quality. The going concern analysis is conducted using a financially distressed firm sample, where financially distressed firms are defined as firms that report either negative net income or negative operating cash flows during the current fiscal year. In columns (3) and (4), we use absolute discretionary accruals (*ABSDA*), estimated using the cross-sectional performance-adjusted modified Jones model (Kothari et al., 2005), to proxy for audit quality. In columns (5) and (6), we use enforcement action against the auditor for poor audit practices (*ENFORCE*) to proxy for audit quality. For columns 5 and 6, the variables *LNTA*, *LEV*, *MTB*, *CFO*, *ROA*, *LOSS*, *FEERATIO*, *LNFEES* and *CLIENTIMP* are computed as the average for all clients of that audit office in the given year. The sample period is between 1999 and 2016. *z*- or *t*-statistics are reported in parentheses. Standard errors are clustered by firm and year. ***, **, * represent significance at the 0.01, 0.05 and 0.10 levels (two-tailed), respectively. All variables are defined in Appendix B.

Panel B: Audit Quality Proxy by Material Weaknesses

	(1) Pr(*MATWEAK*)	(2) Pr(*MATWEAK*)	(3) Pr(*MISSTATE* |*MATWEAK*)	(4) Pr(*MISSTATE* |*MATWEAK*)
UNI	-0.010**		-0.026***	
	(-2.517)		(-3.061)	
ACCUNI		-0.025***		-0.115***
		(-3.021)		(-4.143)
Controls	Yes	Yes	Yes	Yes
Observations	5,958	5,958	5,837	5,837
Year dummies	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes
Pseudo R^2	0.200	0.200	0.161	0.164

This table reports the results of using disclosure of material weaknesses as a proxy for audit quality. In columns (1) and (2), the dependent variable, *MATWEAK*, is an indicator variable coded one if the material weakness likelihood model estimates a material weakness but a material weakness was not received or disclosed, zero otherwise. In columns (3) and (4), the dependent variable is an indicator variable coded one if the material weakness likelihood model estimates a material weakness but a material weakness was not received or disclosed, and there was a subsequent restatement of the financials, and zero otherwise. The control variables included follow model (1). The sample period is between 2007 and 2016 and only includes accelerated filers (Bhaskar et al. 2017). *z*-statistics are reported in parentheses. Standard errors are clustered by firm and year. ***, **, * represent significance at the 0.01, 0.05 and 0.10 levels (two-tailed), respectively. All variables are defined in Appendix B.

Table 11: Varying Proximity on Restatement

Panel A: Average distance between audit office and Nth Universities

	(1) Four Universities	(2) Five Universities	(3) Six Universities
AVEDISTUNI	0.102***	0.097**	0.112***
	(2.704)	(2.500)	(2.677)
Controls	Yes	Yes	Yes
Observations	37,916	37,916	37,916
Year dummies	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes
Pseudo R^2	0.053	0.053	0.053

Panel A report the results by computing the average geographical distance (*AVEDISTUNI*) between the audit office and the closest 4, 5, and 6 universities. The sample period is between 1999 and 2016. *z*-statistics are reported in parentheses. Standard errors are clustered by firm and year. ***, **, * represent significance at the 0.01, 0.05 and 0.10 levels (two-tailed), respectively.

Panel B: Number of Universities within Nth proximity

Proximity within:	(1) 10 mile	(2) 30 mile	(3) 40 mile	(4) 50 mile	(5) 60 mile
UNI	-0.020***	-0.009***	-0.007***	-0.006***	-0.004***
	(-3.757)	(-3.683)	(-3.412)	(-3.380)	(-2.996)
Controls	Yes	Yes	Yes	Yes	Yes
Observations	37,916	37,916	37,916	37,916	37,916
Year dummies	Yes	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes	Yes
Pseudo R^2	0.053	0.053	0.053	0.053	0.053

Panel B report the results of alternative proximity distances for the *UNI* measure. The sample period is between 1999 and 2016. *z*-statistics are reported in parentheses. Standard errors are clustered by firm and year. ***, **, * represent significance at the 0.01, 0.05 and 0.10 levels (two-tailed), respectively.

Panel C: Number of Accredited Universities within Nth proximity

Proximity within:	(1) 10 mile	(2) 30 mile	(3) 40 mile	(4) 50 mile	(5) 60 mile
ACCUNI	-0.027	-0.020***	-0.012**	-0.011**	-0.010**
	(-1.473)	(-3.081)	(-2.396)	(-2.419)	(-2.521)
Controls	Yes	Yes	Yes	Yes	Yes
Observations	37,916	37,916	37,916	37,916	37,916
Year dummies	Yes	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes	Yes
Pseudo R^2	0.053	0.053	0.053	0.053	0.053

Panel C report the results of alternative proximity distances for the *ACCUNI* measure. The sample period is between 1999 and 2016. *z*-statistics are reported in parentheses. Standard errors are clustered by firm and year. ***, **, * represent significance at the 0.01, 0.05 and 0.10 levels (two-tailed), respectively.

Panel D: Proximity Using Concentric Rings

	(1) Number of Universities	(2) Number of Accredited Universities
Between 0 to 25 miles	-0.010***	-0.023**
	(-3.058)	(-2.145)
Between 25 to 50 miles	-0.001	0.001
	(-0.299)	(0.150)
Between 50 to 100 miles	0.000	-0.000
	(0.225)	(-0.045)
Controls	Yes	Yes
Observations	37,916	37,916
Year dummies	Yes	Yes
Industry dummies	Yes	Yes
Pseudo R2	0.054	0.053

Panel D reports the results of proximity distances using concentric rings. The sample period is between 1999 and 2016. *z*-statistics are reported in parentheses. Standard errors are clustered by firm and year. ***, **, * represent significance at the 0.01, 0.05 and 0.10 levels (two-tailed), respectively.

Table 12: Alternate Measures of Proximity

	(1) Population of universities	(2) Accounting graduates
UNIPOP	-0.130***	
	(-3.288)	
UNIGRAD		-0.028**
		(-2.057)
Controls	Yes	Yes
Observations	37,913	19,292
Year dummies	Yes	Yes
Industry dummies	Yes	Yes
Pseudo R^2	0.053	0.057

This table reports the results of alternative measures of proximity on the likelihood of misstatement (*MISSTATE*). In column (1) we capture the population of universities that are within a 25-mile radius to an audit office. In column (2) we capture the number of accounting graduates of universities that are within a 25-mile radius to an audit office. The control variables included follow model (1). The sample period is between 1999 and 2016. *z*-statistics are reported in parentheses. Standard errors are clustered by firm and year. ***, **, * represent significance at the 0.01, 0.05 and 0.10 levels (two-tailed), respectively. All variables are defined in Appendix B.