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Regulatory Spillovers in Common Audit Markets*

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Regulatory Spillovers in Common Audit Markets

Abstract

We find that Sarbanes-Oxley (SOX) had two significant effects on the audit market for nonpublic entities. The first short-run effect stems from inelastic labor supply coupled with an audit demand shock from public companies. As a result, private companies reduced their use of attested financial reports in bank financing by 12%, and audit fee increases for nonprofit organizations (NPOs) more than doubled. The second long-run effect was a transformation in the audit supply structure. After SOX, NPOs were less likely to match with auditors most exposed to public companies, while auditors increasingly specialized their offices based on client type. Audit market concentration for NPOs dropped by more than half within five years of SOX and remained at this level through the end of our sample in 2013, while the number of suppliers increased by 26%. Our results demonstrate how regulation directed at public companies generates economically important spillovers for nonpublic entities.

Keywords: Sarbanes-Oxley, securities regulation, auditing, market structure, accounting, private firms, non-profits, labor economics.
JEL Codes: H83, M12, M21, M41, M48, M49

1. Introduction

The market for financial statement attestation services has attracted considerable attention from academics, regulators, and practitioners, especially following the passage of the Sarbanes-Oxley Act in 2002 (SOX). Congress directed SOX at improving the audit quality of publicly held companies through enhanced oversight of auditors and additional testing of companies' financial reporting processes. As such, almost all of the discussion and analysis related to the effects of SOX focuses on public companies. Yet many other organizations—including private companies, nonprofits, and governmental entities—also purchase services from public accounting firms for managerial and external financing purposes.[1] If public regulation affects unregulated entities, then analyses focusing exclusively on regulated companies do not measure the full effect of the regulation. In this paper, we study the passage of SOX to examine how and why regulatory shocks to the public company audit market affect nonpublic entities.

SOX changed the public audit market in two primary ways. First, Section 404 of SOX required public company auditors to provide assurance on their clients' internal controls (IC). This created a substantial demand shock for accounting services because public companies needed accounting labor to document and test their internal controls, and auditors had to exert additional effort to attest to the effectiveness of those controls (Iliev 2010; Ge, Koester, and McVay 2017). Second, Title I of SOX altered the structure of the public audit market by establishing an audit regulator—the Public Company Accounting Oversight Board (PCAOB)—with two mandates: (1) to register and periodically inspect accounting firms conducting public company audits; and, (2) to establish auditing standards specifically for public companies, thus distinguishing public company and nonpublic entity audits.

Whether the audit market changes induced by SOX have spillover effects for nonpublic entities is

[1] Examples of papers investigating the effects of SOX on public companies include Engel, Hayes, and Wang (2007); Leuz (2007); Zhang (2007); Ashbaugh-Skaife, Collins, Kinney, and LaFond (2008); Cohen, Dey, and Lys (2008); Piotroski and Srinivasan (2008); Brochet (2010); DeFond and Lennox (2011); Ettredge, Sherwood, and Sun (2017); Albuquerque and Zhu (2018). For examples of papers investigating financial reporting in private companies and nonprofits, see Blackwell, Noland, and Winters (1998); Core, Guay, and Verdi (2006); Allee and Yohn (2009); Kitching (2009); Minnis (2011); Petrovits, Shakespeare, and Shih (2011); Yetman and Yetman (2013); DeFond and Zhang (2014); Cassar, Ittner, and Cavalluzzo (2015); Harris, Petrovits, and Yetman (2015); Badertscher, Kim, Kinney, and Owens (2017); Lisowsky and Minnis (2018); Lisowsky, Minnis, and Sutherland (2017).

not clear a priori. On one hand, if public and nonpublic entities purchase attestation services from a common pool of suppliers—i.e., there is a "common market" for accountants' services—then the audit market changes induced by SOX could have both short- and long-run effects on nonpublic entities. In the short-run, the public company demand shock for accounting services caused by the IC attestation requirements of SOX Section 404 could have increased prices and reduced quantities of attestation services purchased by nonpublic clients if the labor supply was sufficiently constrained and inelastic. In addition, Title I of SOX created the PCAOB with its focus on public companies. This new and distinct regulatory regime between public and nonpublic entities could have split a previously common market into distinct markets for each entity type. This suggests that SOX could have longer-run implications for nonpublic entities if auditors chose to specialize as a result of the distinct auditing regulations.

On the other hand, multiple features of the audit market could limit spillovers from regulation onto nonpublic entities. First, researchers argue that auditors specialize by client size, ownership, and location (e.g., Doogar and Easley 1998; Ferguson, Pinnuck, and Skinner 2017). If this specialization reduces the substitutability of labor across public and nonpublic clients, then the scope for spillovers from the creation of the PCAOB (Title I) or the internal control attestation requirements (Section 404) is limited. Moreover, firms can respond to the changes induced by SOX in a number of ways. For example, audit firms could address the demand shock by outsourcing, investing in technology, or hiring foreign workers or retired accountants. In addition, while the creation of the PCAOB may have segmented the attestation markets for public and nonpublic clients, such segmentation could have *increased* audit supply for nonpublic clients. For example, DeFond and Lennox (2011) find that many auditors exited the public company audit market following SOX and these auditors may have shifted their capacity to the nonpublic market.

Because prior research typically focuses on public companies, the extent to which the collective provisions of SOX affected nonpublic entities is an open question. Moreover, understanding whether and how public market regulatory shocks affect unregulated entities is important for several reasons. First, like public companies, nonpublic entities engage independent accountants to attest to the company's financial statements to access financing, contract with external parties, improve information quality, and constrain

agency problems.[2] Therefore, regulation directed at public companies can impose unintended harm on unregulated entities if it increases the cost of financial statement attestation services. Second, while there has been continued debate surrounding the overall effects of SOX and securities regulation generally (e.g., Leuz 2007, Coates and Srinivasan 2014, Leuz and Wysocki 2016), the discussion primarily focuses on public companies. However, the extent to which—and why—unregulated entities are affected by securities regulation is important to the debate, especially if the effects are large and persistent.

To investigate the spillover effects of SOX, we examine two types of nonpublic entities: private companies and nonprofit organizations (NPOs), including universities, hospital systems, and charities. Together, these entities control over $14 trillion in assets, and financial statement attestation is an important contracting and monitoring mechanism in the oversight of the majority of these assets.[3] The settings complement one another in the nature of financial reporting demand and data availability, which facilitates the generalization of our findings. Private companies do not face an attestation mandate, and we can observe changes in their quantity (but not price) of attested financial statements. By contrast, NPOs spending federal grants face mandatory audit requirements and disclose their external accounting fees and auditor identity, allowing us to observe changes in price and auditor.[4] We use these settings to examine first the short-run quantity and price responses to SOX, and then the longer run changes and segmentation in audit supply.

We acquire private company data from the Risk Management Association (RMA), which reports the

[2] Unlike public companies, nonpublic entities are not required to hire an independent accountant to perform an examination of their financial statements. Moreover, instead of being limited to an audit, nonpublic entities have a menu of services that accountants can provide, including "compilations" (providing no assurance that the statements are materially correct), "reviews" (which provide negative assurance), and "audits" (which provide positive—i.e., most intensive—assurance that the financial statements are not materially misstated). All of these services are collectively referred to as "attestation services." We provide additional definitions in Appendix A. For discussions and analyses of the use of the different types of attested financial statements in U.S. private companies, see for example: Botosan et al. (2006); Allee and Yohn (2009); Bradshaw et al. (2014); Berger et al. (2017); Lisowsky et al. (2017).

[3] Lisowsky and Minnis (2018) examine a comprehensive panel dataset of private U.S. companies and find over $9 trillion in assets in 2010 (excluding real estate and financial entities)—approximately $6 trillion of which is controlled by companies with audited GAAP financial statements. Nonprofits controlled over $5 trillion in assets in 2013 (Urban Institute 2015) and a large share of NPOs obtain financial statement audits either voluntarily or to comply with regulation.

[4] Following prior work, we study the line item on the Form 990 where NPOs report the fees for external accounting and audit services (Pearson, Brooks, and Neidermeyer 1998; Tate 2007; Webb and Waymire 2016). For parsimony, we refer to this line item as "accounting fees", "audit fees", and "audit prices" interchangeably throughout the paper.

propensity of private companies to provide banks with attested financial statements (i.e., unqualified audits, reviews, and compilations). We find that private companies significantly reduced their use of CPA attestation upon the implementation of Section 404 of SOX (i.e., years 2004 to 2009). For example, the propensity for private companies to report attested financial statements dropped by 6.3 percentage points, or approximately 12% of the pre-SOX sample attestation rate between 2003 and 2005. The reduction is particularly acute in reviews and compilations, which recent evidence suggests are useful forms of attestation for smaller companies (e.g., Badertscher et al. 2017). Instead, these borrowers were more likely to provide either tax returns or company-prepared financial statements. Thus these initial results suggest that public company audit regulation reduced the proportion of private companies accessing higher quality financial reporting, which has been shown to be important for private company access to capital.

To examine the effects on nonprofit entities, we then construct a panel dataset of NPOs, including auditor names and accounting fees using public filings of NPOs spending over $500,000 of federal grant money. Our specification exploits the fact that Section 404 of SOX affected auditors differently (Iliev 2010, Albuquerque and Zhu 2018). Some auditors became involved with IC attestation of public companies immediately upon the effective date of Section 404, while other auditors did not engage with IC attestation until one or more years after the effective date (e.g., because their clients were sufficiently small to avoid IC attestation), and still other auditors never had public clients with mandated IC attestation. To estimate the effects of SOX, we employ a generalized difference-in-differences design comparing treated NPOs engaging auditors providing Section 404 attestations to a control set of NPOs engaging auditors which do not provide Section 404 attestations. Not only does this research design exploit auditors' heterogeneous and staggered involvement with IC attestations, it also includes state-year, operating activity-year, and NPO-auditor pair fixed effects, reducing concerns that our findings reflect regional or nonprofit segment-specific shocks to the audit market, auditor-client matching, or time-invariant NPO characteristics, respectively. If accounting labor supply is sufficiently inelastic and talent is substitutable across client types, NPO clients with audit firms most affected by the Section 404 demand shock should in turn be most affected.

We first confirm that prior to the passage of Section 404, audit fees for both the treated and control

NPOs move in parallel. We then find the pattern changes. Once an auditor begins issuing IC attestations for its public company clients, its nonprofit clients face an incremental fee increase of 5.4%—twice the fee increase of nonprofit clients of auditors who do not produce IC attestations. We then show that the likelihood of NPOs switching auditors also more than doubles in the year an NPO's auditor begins performing IC attestations for public companies. Our results are not limited to clients of Big 4 auditors, do not depend on whether we define auditor IC involvement at the firm or office level, and remain after matching treated and control observations on observable characteristics.

From this set of results, we infer that a public company demand shock coupled with inelastic accounting labor supply caused negative spillover effects for private companies and NPOs.[5] The fixed effect structure we use and the fact that the pre-treatment parallel trend assumption holds for both the private company and NPO analyses make it unlikely that time-invariant characteristics or sample composition issues are driving these results. However, a time-varying attestation demand shock from nonpublic entities or unspecified sectoral trends would threaten this inference. For example, if the NPOs engaging the auditors who began providing IC attestations post-SOX Section 404 (i.e., the treated observations) simultaneously demanded additional audit procedures that other NPOs did not simultaneously demand (Iyer and Watkins 2008), this would result in differential price increases like the ones documented in our results.

To mitigate this concern, we conduct cross-sectional tests based on differences in labor elasticity plausibly exogenous to audit demand. Because of the preponderance of entities with December fiscal year-ends in the U.S. economy, accounting labor capacity is seasonal resulting in a "busy season" for auditors immediately following December (Hay, Knechel, and Wong 2006; Lopez and Peters 2011). Importantly, despite this difference in accounting labor capacity, the attestation service they purchase is the same. Therefore, we argue that an entity's fiscal year-end is related to audit labor inelasticity, but is not related to any change in attestation demand. In the quantity (price) response analyses for private companies (NPOs), we find that while the effects are still positive for non-December fiscal year-end entities, they are significantly

[5] In the supplemental appendix to this paper, we discuss the evidence of both the demand shock and labor supply in more detail.

stronger for entities with December year-ends. Moreover, consistent with accounting labor entering the market in the years following the enactment of SOX Section 404 (i.e., a reduction in labor inelasticity), the difference in treatment effect between December and non-December entities diminishes over time. Thus, alternative explanations, such as time-varying demand shocks, have to describe not only why December year-end entities experience a stronger initial demand shock coinciding precisely with Section 404, but also why that demand shock dissipates over time vis-à-vis non-December entities. We conclude that the evidence is consistent with a public company demand shock coupled with labor inelasticity causing short-run spillovers.

We then investigate whether SOX had longer run effects caused by separating the audit markets. One view suggests that if a public company demand shock coupled with labor inelasticity caused the results documented above, then the pre-SOX equilibrium will simply return as more accounting labor enters the market. Under an alternative view, the creation of the PCAOB with its separate auditing standards and auditor monitoring features established under Title I of SOX created differential costs required to service public and nonpublic clients. This could cause auditors to further specialize between public and nonpublic clients to manage these differential costs. Moreover, because the IC testing newly required by Section 404 can be conducted throughout the fiscal year (rather than concentrated right after the fiscal year-end), public company audit labor can be spread more evenly throughout a given year. This makes NPO audit engagements, typically conducted during off-peak months, less appealing for public company auditors. Collectively, SOX could have sufficiently shifted the features of the audit market to make specialization by client type more appealing, thus separating attestation supply for public and nonpublic entities.

We test for market separation two ways. First, we assess whether NPOs were significantly less likely to have an auditor that focuses on public companies following SOX, which we refer to as "across-firm" separation. To test this, we first model the likelihood that an NPO engages an auditor that specializes in public companies in the year 2000 (the first year of our sample). We then use the model's parameters to predict the likelihood that NPOs match to public company auditors in each subsequent year. In the years prior to SOX, the model's aggregate predictions are not significantly different from the actual matches, however the predictions significantly worsen after 2004. Specifically, far fewer NPOs actually match with public

company auditors compared to the model's predictions. The poor performance of the predictive model continues to the end of our sample in 2013, far beyond when we expect any labor constraints to have subsided. The decline in the actual market share of public company auditors is economically significant: in 2013 the model predicts a market share of 16.4% but the actual NPO market share for public company auditors is only 6.1%. We conduct several robustness tests mitigating concerns that these results are solely a function of switching costs, sample composition changes, or auditors exiting the public company market. Our results suggest that auditors more intensively specialize by client type after SOX was enacted.

Second, we examine whether offices *within* audit firms having both public and nonpublic engagements increased their concentration of client types (i.e., public versus nonpublic) in specific offices, which we refer to as "within-firm" separation. To test this, we focus on auditors serving both public and nonpublic clients, and measure whether the distribution of client engagements across the auditor's offices changes post-SOX. Using two measures of client type concentration, we find that offices within the same audit firm were more likely to focus on *either* NPOs *or* public companies. These results persist through the end of our sample in 2013. This evidence documents a second channel through which SOX contributed to the separation of public and nonpublic audit markets.

Given the sizable, persistent changes in the audit market we document, we conclude our study by descriptively assessing how these changes relate to the aggregate audit supply concentration of the NPO market. Analyses using the Herfindahl-Hirschman Index (HHI) reveal a sharp *decline* in audit market concentration starting in 2004, driven by NPOs previously concentrating in larger auditors shifting to a broader set of auditors. By 2009, the HHI is less than half the value it was in 2003 and this level persists to the end of our analysis in 2013. Complementing this evidence, we find the number of auditors serving at least five NPO clients increased by 26% over this same period, from 54 to 68. While we view these results as descriptive, they stand in stark contrast with the literature investigating the public company audit market, in which both academic and practitioner literatures highlight increased audit market concentration (e.g., Gerakos and Syverson 2015; Ferguson et al. 2017) and decreased number of suppliers (DeFond and Lennox 2011).

Our results are relevant to both researchers and regulators for three reasons. First, we identify large,

persistent effects of SOX on the audit market for unregulated entities. Our findings thus indicate that analyses of the costs and benefits of public company regulation (e.g., Coates and Srinivasan 2014) should not be limited to public companies. Second, recent research highlights that as large customers have moved toward large public audit firms, the audit supply structure has become highly concentrated (Gerakos and Syverson 2015; Ferguson et al. 2017). Our findings reveal that the audit supply market is not only affected by long-run changes in client characteristics, but also affected by regulation. Specifically, following SOX audit firms specialized in either regulated or unregulated clients, resulting in *less concentrated* attestation markets for nonpublic entities. Finally, we document a new determinant of the equilibrium financial report attestation for nonpublic entities. While extant research investigates the demand drivers and consequences of attested financial reports to private companies (e.g., Blackwell et al. 1998; Allee and Yohn 2009; Minnis 2011) and nonprofits (Tate 2007, Kitching 2009, Petrovits et al. 2011), we know relatively little about the determinants of audit supply or spillovers from public entity regulation.

2. Motivation and Prior Literature

A significant body of literature investigates the effects of SOX; however, most prior literature focuses on measuring the effects of SOX on public companies (Engel et al. 2007; Zhang 2007; Cohen et al. 2008; Linck et al. 2008). For example, prior research finds a sharp increase in public company audit fees upon the enactment of Section 404 (e.g., Ettredge et al. 2007; Griffin and Lont 2007; SEC 2009; Iliev 2010; Ettredge et al. 2017). Another stream of literature investigates changes in auditors' portfolios of public companies in conjunction with the demise of Arthur Andersen and the passage of SOX. Moreover, several papers find an increase in the frequency of auditor switches among public companies, especially departures from the Big N, during the 2000s (Rama and Read 2006; Doogar, Fargher, and Hong 2007; Ettredge et al. 2007; Hogan and Martin 2009; Landsman, Nelson, and Rountree 2009; Chang et al. 2010; Aobdia, Enache, and Srivastava 2019). These papers ascribe these Big N portfolio changes to a number of factors such as SOX, post-Enron risk adjustments, or simply a continuation of longer run trends. Prior work also documents how the costs of complying with SOX affect entry in the public company market. For example, Piotroski and Srinivasan (2008) find that small foreign companies are less likely to choose to list in the U.S. compared to

the U.K. Bova, Minutti-Meza, Richardson, and Vyas (2014) show that private companies seeking public equity financing are more likely to be acquired by an existing public company rather than pursue an initial public offering.

While most literature does not investigate whether audit regulation of public companies has spillover effects on nonpublic entities, an understanding of any such spillovers is important for several reasons. Private companies and NPOs rely on CPA attestation to mitigate information and agency problems just as public companies do. For example, the literature investigating private companies shows that high quality reporting affects these companies' ability to access credit markets (e.g., Blackwell et al. 1998; Allee and Yohn 2009; Minnis 2011; DeFond and Zhang 2014). If CPA-prepared statements are more costly for borrowers to obtain, banks will have to rely on other information sources, including unverified management-prepared reports, soft information, and credit reports (Cassar et al. 2015; Minnis and Sutherland 2017; Sutherland 2018). This could increase interest rates or collateral requirements, or decrease the amount of credit available (Berger and Udell 2006; Minnis 2011; Badertscher et al. 2017). Thus, private companies may face a higher cost of capital. For NPOs, both Kitching (2009) and Harris et al. (2015) find that audit quality is related to donor decisions, thus the ability of NPOs to access high quality auditors affects the nearly $400 billion annual market for contributions (Charity Navigator 2016). More broadly, examining spillover effects of public entity regulation on nonpublic entities is informative about the full costs of the regulation.

Recent literature has begun exploring a link between SOX and the nonpublic audit market. For example, Feng and Elder (2017) examine the nonprofit clients of the Big 4 and find their propensity to switch to smaller auditors increases starting in 2002. They further find that downward switching is concentrated in hospitals and universities, and is associated with subsequent internal control weaknesses but not donor contributions. However, because their study focuses exclusively on the auditor switching of individual Big 4 clients over a relatively short period, it does not investigate pricing implications, or more importantly, any aggregate or long-run consequences for the NPO audit market.

One limitation of the extant SOX literature relates to the challenges associated with drawing causal inferences surrounding the effects of SOX. As Coates and Srinivasan (2014) note, "To date, however, most

studies of SOX…have not used research designs well adapted for (causal inference), and instead use simple before-and-after comparisons that fail to control for contemporaneous changes in the objects of study" (pp. 660). Related to our research question, prior research has struggled with identifying the causes of auditor switches and audit fee increases around SOX separately from other contemporaneous developments such as responses to corporate scandals, the fall of Arthur Andersen, and prevailing economic conditions. Disentangling these channels is crucial, however, if the goal is to document a regulatory spillover, rather than simply show greater prevalence of switches and fee increases after 2002. As we detail in Section 3, an attractive feature of our setting for studying the connectedness of public and nonpublic markets is the across-time and auditor variation in the involvement of IC attestation. We thus contribute not only by assessing whether public company regulation had spillover effects on nonpublic entities, but also by identifying causal effects of SOX.

3. Short-run Effects: Audit Quantities and Prices

3.1 Audit quantities of private companies

3.1.1 Private companies: Data and summary statistics

We collect private company data from the RMA's Annual Statement Studies ("Studies" hereafter). RMA is an industry association of banks that collects and publishes aggregate statistics about the type of financial statements gathered by its members each year. The Studies tabulates the types of financial reports submitted by commercial loan applicants and borrowers to member banks each year into one of five mutually exclusive and collectively exhaustive categories: unqualified audits, reviews, compilations, tax returns, and other (which are typically company prepared statements). The first three report types are collectively referred to as "attestation" reports (i.e., a CPA is involved in the report preparation and provides an attestation report in conjunction with the financial statements) and the latter two do not involve CPA attestation. We provide additional definitions of these reports in Appendix A.[6]

[6] While the American Institute of Certified Public Accountants refers to audits, reviews, and compilations as attestation services (requiring a CPA) (AICPA 2016), only audits and reviews are assurance engagements in which the CPA provides assurance with respect to the veracity of the figures reported in the financial statements.

The Studies does not report company level data for confidentiality purposes, but instead reports the distribution of statement types by six-digit NAICS code, region (Northeast, Southeast, Central, South Central, North Central, and West), and borrower size category (under $1M of revenue, $1M-$3M, $3M-$5M, $5M-$10M, $10M-$25M, and greater than $25M). Additional details of the RMA dataset are provided in Appendix A of Berger, Minnis, and Sutherland (2017) and Lisowsky et al. (2017). The version of the dataset used in our study differs slightly from the version in those papers. Because our tests do not require bank-level data, we can expand the sample back to 1995, instead of 2002 which is the first year bank-level detail is available. Accessing earlier data from RMA allows us to observe a longer time series of financial reporting practices during the pre-SOX period to better account for any secular trend.[7] An appealing feature of the RMA dataset is that it contains the reports collected by the banks responsible for the majority of commercial lending activity in the U.S., facilitating the generalizability of our analyses.

Table 1 reports descriptive statistics for the variables used in our private company analyses, measured between 1995 and 2011. The entire dataset includes 2,004,650 statements after eliminating financial services borrowers (two-digit sectors 52 and 55), and observations with fewer than five statements. We collapse the statements to a three-digit industry by region by company size category by year unit of observation. In the mean (median) industry-region-company size-year, 53.9% (56.0%) of the statements are unqualified audits, reviews, or compilations (collectively referred to as "*% CPA*"). Unqualified audits (reviews, compilations) comprise 20.5% (15.2%, 18.2%) of this 53.9% share. The remaining 46.1% of statements are split between tax returns and other statements. To study changes in reporting practices, we employ time indicators for periods within our sample marking distinct milestones associated with SOX (*Year 02-03*; *Year 04-09*). We chose our 2004-2009 post-enactment period to immediately follow the enactment of Section 404 and span the expected period of audit labor shortage, given licensing and education requirements. In the supplemental appendix, we show that focusing only on the period immediately following the enactment of Section 404

[7] Nevertheless, in untabulated robustness analyses we repeat our main tests using the bank-level data for the 2002-2013 period with bank fixed effects, and find similar results, mitigating concerns that bank-level participation decisions over time are responsible for our results. Our results are also the same if we restrict the sample to banks participating in RMA every year from 2002 to 2011.

(e.g., 2004-2005) strengthens our inferences. Last, we report summary statistics for the cross-sectional variable used in our tests. *NonCalendarFYEShare* measures the percent of private companies in the industry with a fiscal year-end in months other than in December. We measure this variable in 2003 using the Sageworks data described in Minnis (2011). The average value of *NonCalendarFYEShare* is 18.2%.[8]

3.1.2 Private companies: Results

Our first set of tests uses the RMA data to study the reporting practices of private companies as a function of time, using the following weighted least squares specification:

$$\%\ CPA_{r,i,s,t} = \alpha_r + \alpha_i + \alpha_s + \beta_1 \times Year_t + \beta_2 \times Trend_t + \varepsilon_{r,i,s,t} \quad (1)$$

The unit of observation is industry-region-company size category-year. We weight each observation by the number of statements collected within the unit of observation, but our results are not sensitive to this choice. Our baseline specification includes region (α_r), industry (α_i), and company size category (α_s) fixed effects. The coefficient of interest, β_1, measures how the quantity of financial report attestation differs in the years 2004-2009 relative to other years in the sample. Our prediction is that *% CPA* is lower after Section 404 implementation (i.e., after 2004), but before the supply of auditors has had time to adjust (i.e., before 2009). We include a linear trend term, $Trend$, increasing by one each year following 1995 to account for a potential secular trend. We cluster our standard errors by industry. Because our sample only spans 1995-2011 and contains only six regions and six size categories, clustering by year, region, or size category is inappropriate (Angrist and Pischke 2009). However, our results are robust to clustering on multiple dimensions (e.g., by region x size category or region x size category and industry, see Table A1 in the supplemental appendix).

Table 2, Column 1 presents the results of estimating (1). It shows that in the six years after SOX was enacted, bank collection of CPA attested statements declined by 2.2% beyond the long-term trend (i.e., *Year 04-09* is negative and significant). We then subject this result to a series of robustness tests. First, we include

[8] Sageworks is a panel data set of privately held companies that reports industry and fiscal year-end information. Because the RMA data is tabulated at the industry level, we aggregate the Sageworks fiscal year-end data by industry for the year 2003 and merge this into the RMA data. Prior literature discusses the factors driving across-industry variation in fiscal year-ends, such as inventory flows (see, for example, Huberman and Kandel 1989).

indicators for each industry-region-size category combination, to account for time-invariant reporting practices within an industry, across regions and company sizes. Second, we assess whether developments unrelated to SOX could be generating our findings by including either higher order trend terms or industry-specific trends in our specification. In each test, the economic and statistical significance of *Year 04-09* remains similar to column 1.[9]

Then, to reinforce that labor market developments contribute to variation in *% CPA* and to ensure the finding is not simply the result of an unmodeled secular trend, we study a second time period with regulatory-driven changes in audit supply for private companies. We focus on the period after SOX was passed (2002) but before the November 15th, 2004 mandatory compliance date for management reports on internal controls. Motivated by DeFond and Lennox (2011), we test whether *% CPA* increases for 2002-2003, as auditors exiting the public company market increase accountant supply for private companies. Column 2 finds that private companies were *more* likely to provide CPA-prepared reports to their bank in 2002 and 2003. The coefficient on *Year 02-03* is positive and significant at the 1% level.

We also examine changes in attestation by year, instead of grouping years. Figure 1 plots the coefficients of year fixed effects, thus allowing the attestation rate to vary by year. Note that points before 2002 are not significantly different from zero, thus establishing that no years had significant deviations from the overall trend during this time. Year 2003 is significantly positive, consistent with the results in column 2 of Table 2. More importantly, the decrease in attestation between 2003 and 2005 is large—a 6.3 percentage point decrease (or 12% of the pre-SOX rate) in just two years—and consistent with the timing of the majority of the public company demand shock. To gauge the economic significance of this decrease, we compare it to the difference in companies' propensity to provide attested statements to large versus small banks, which has received considerable attention in the banking literature (e.g., Berger, Miller, Petersen, Rajan, and Stein 2005). The difference in *% CPA* between large and small banks is only 3.4 percentage points (Berger et al.

[9] To further ensure entry by small companies into the sample between 2004 and 2009 does not drive the decline in *% CPA* (e.g. see Section 4.1 and Figure 4 of Lisowsky et al. 2017), we repeat our tests after omitting companies with less than $10M of revenue, and arrive at the same results. Our results are also the same if we allow for company size-specific trends.

2017)—approximately half of the magnitude of the SOX spillover. The remaining years of the figure show that *% CPA* slowly reverts to the trend rate by 2010 as accounting labor returns to the attestation market.

The results thus far identify the effects of SOX by comparison to the overall trend in changes in attestation. We next conduct cross-sectional tests to tie these findings more directly to labor constraints. Recall the spillover mechanism argument: audit firms did not have sufficient labor to supply all consumers, and private companies have elastic demand because they do not face a reporting mandate, and may be able to provide alternative information (e.g., tax returns or credit scores) to their contracting counterparties. If labor inelasticity is the mechanism, then the post-SOX decline in CPA statement reporting should be more apparent in settings where labor is more constrained. We exploit plausibly exogenous within-year variation in the demand for accountants to identify labor constraints while holding private company audit demand constant. The preponderance of December fiscal year-end companies in the U.S. economy has traditionally created a CPA "busy season" during the first few months of the year, while the summer months typically have accountant supply slack because of reduced demand. If labor constraints induce spillover, then private companies with December year-ends will experience more negative spillover effects from SOX than companies with fiscal year-ends outside of the busy season.

Column 3 of Table 2 presents the results of regressions that add interactions between *Non Calendar FYE Share* and our time variable of interest, *Year 04-09*, to equation (1). We present a deciled version of *Non Calendar FYE Share* to facilitate interpretation; alternative specifications and the raw continuous measure produce the same inferences. Consistent with our predictions, Column 3 shows a positive and significant coefficient on *Year 04-09 * Non Calendar FYE Share*. Moving from the industries with the lowest (1st) to highest (10th) decile of slack substantially reverses the post-SOX decline in *% CPA* (0.003 x 9 = 0.027, offsetting much of the -0.038 coefficient on *Year 04-09*). Thus, the increase in public company accountant demand most affected December year-end companies.[10]

[10] One concern about the fiscal year-end cross-sectional variable is that companies could simply switch their fiscal year-end to avoid the effect of SOX. However, switching fiscal year-ends is difficult. The majority of companies in the U.S. economy are passthrough entities, such as partnerships and subchapter S corporations, which have "required tax years" per the Internal Revenue Service. Required tax years generally force the entity to have the same fiscal year-end as the

Performing cross-sectional tests also allows us to augment our specification with sector and time controls, to account for developments concurrent with the passage of SOX. By including sector-by-time fixed effects, we can control for macroeconomic and governance developments affecting reporting within a class of similar industries, while exploiting differences in fiscal year-ends across these industries. The identifying assumptions behind these tests are: (1) reporting for the three-digit industries within a two-digit sector responds similarly to the state of the economy and governance reform during the sample period; and (2) within-sector, across industry variation in fiscal year-ends is unrelated to sector-level developments during our sample.[11] Column 4 presents the results using fully saturated specifications. We find that controlling for sector-by-year effects does not affect our original inferences. Companies belonging to industries with more off cycle reporting are less exposed to the accountant supply inelasticity.

The results to this point suggest that the SOX demand shock affected the reporting of CPA-attested statements by private companies. We exploit this shock to investigate which types of statements (audits, reviews, or compilations) are responsible for the decrease in *% CPA* between 2004 and 2009, and whether labor constraints mediate the decrease. Doing so also allows us to offer evidence on the elasticity of demand for different CPA statements. One possibility is that companies choose less expensive report options in the face of price increases. An alternative conjecture is that demand elasticity for attestation services is negatively related to attestation quality because higher quality financial statements could be contractually required by banks, suppliers, or shareholders. We acknowledge that theory can support still other predictions, so we consider our results exploratory.

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majority owner(s)—which is typically an individual with a calendar tax year. While the IRS allows for exceptions to the required tax year (providing the variation we exploit with the cross-sectional variable, *Non Calendar FYE*), it is not surprising that the majority of private companies have December year-ends and that this rarely changes in the Sageworks data (see also IRS Publication 538). Moreover, for those companies with a possibility of switching, they must first apply to the IRS to have this granted and then file a partial year return, which imposes further costs for the company.

[11] We uncover significant variation in fiscal year-ends across the industries belonging to the same sector. The standard deviation in *Non Calendar FYE Share within* sector (8.2%) is comparable to the standard deviation *across* sectors (8.1%). To illustrate the variation, consider two-digit sector 62 (health care and social assistance). Its three-digit industries (ambulatory health care services, hospitals, nursing and residential care facilities, and social assistance; industries 621, 622, 623, and 624) are comparably exposed to the broader economy, but have different fiscal year-end concentrations (*Non Calendar FYE Share* of 9.8%, 33.3%, 14.9%, and 17.6% respectively). Note that the sector-by-year fixed effects absorb the *Year04-09* variable, and thus that coefficient is not reported in column 4.

Columns 1-3 of Table 3 present the results. We find that the coefficients for *Year 04-09* monotonically decline in statement quality. That is, unqualified audits see no statistical change in reporting frequency, while reviews decline slightly and compilations decline more (statistically and economically). Moreover, as further evidence that labor constraints contribute to the quantity reduction following SOX Section 404, we find that the effects are weaker in industries with companies with a higher preponderance of non-December fiscal year-ends (*Year 04-09 * Non Calendar FYE Share* is significantly positive). To assess the economic magnitudes of the results, the coefficients imply a 4.7% (8.4%) decline in reporting rates for reviews (compilations) for the pooled sample of companies following SOX Section 404.[12] Note, however, that our evidence does not imply that private companies purchasing audit services were not affected by SOX. Instead, we suggest that SOX affected all attestation levels but that audit services—which likely have lower demand elasticity because of contractual requirements—experienced effects through price (which is unobservable to us) rather than quantity. Thus, we consider these results intriguing evidence of differential demand elasticity across statement types which can be explored in future research.

To summarize the private company results, we find that a public company audit shock affected the quantity of attestation services consumed by private companies. We conjecture these changes in quantities are a response to changes in attestation prices; however, we are unable to directly measure this because we do not have access to audit price data for private companies. Therefore, we examine the nonprofit setting.

3.2 Audit price and supply in NPOs

3.2.1 NPOs: Data and summary statistics

NPOs and governmental entities who expend total federal grants of $500,000 or more are required to obtain an attestation referred to as a "Single Audit."[13] An independent CPA must conduct the audit and

[12] To calculate these figures for the representative company, we assume a fifth decile of *Non-Calendar FYE Share*, and compare the change in reporting to the pre-SOX average. Specifically, the 8.4% decline for compilations is calculated as (-0.029 + 5 x 0.002)/0.225, where -0.029 and 0.002 are the column 3 coefficients for *Year 04-09* and *Year 04-09* x *Non-Calendar FYE Share*, respectively, and 0.225 is the average for *% Compilations* before 2004.

[13] At the beginning of our sample period, the threshold is $300,000. Organizations expending total grants between $300,000 and $500,000 are therefore not observable throughout our sample period. We exclude these organizations to ensure that our results are not confounded by a change in sample composition. Because we do not have fees or necessary control variables (e.g., assets) for governmental entities, we do not examine them in our price and switching tests.

submit the audit report to the Federal Audit Clearinghouse, which in turn makes the report publicly accessible. Audit Analytics collects data filed by NPOs subject to a Single Audit, allowing us to observe the auditor identity for each NPO-year. By tracing that audit firm in both Audit Analytics' Opinions and SOX 404 databases, we can determine whether it also serves public companies, and when it begins issuing IC attestations.

The filing requirements for NPOs subject to a Single Audit are minimal. The auditees must file their audit report, but not their actual financial statements. We access GuideStar for detailed NPO-level data. GuideStar extracts the information reported on the Form 990 that NPOs file with the Internal Revenue Service. Certain types of organizations represented in Audit Analytics are not Form 990 filers (e.g., states, local governments, and Indian tribal governments). Similarly, not all NPOs receive grants from the federal government. Our sample is therefore at the intersection of Audit Analytics and GuideStar.

There are two selection issues related to smaller NPOs in the Form 990 data. First, GuideStar's coverage of NPOs is haphazard during the years 2004 to 2007, especially those with less than $750,000 in receipts.[14] Second, the IRS's revenue threshold exempting NPOs from filing an annual Form 990 varies over time. This causes small NPOs to leave and re-enter the sample. To ensure various sample selection issues do not induce our results, we impose a balanced panel on GuideStar's dataset prior to merging it with Audit Analytics. Of course, doing so introduces a survivorship bias concern; therefore, we replicate all of our analyses without this requirement. We also re-estimate our tests only on NPOs with receipts above $750,000 which do not suffer from these concerns, and our results are statistically and economically similar.

Table 4 describes the sample selection procedures. Our sample begins in 2000 because it is the first year Audit Analytics is adequately populated.[15] We begin with the 67,886 NPO-year observations at the intersection of the Audit Analytics nonprofit database and GuideStar. We exclude NPOs expending less than

[14] Based on our discussion with GuideStar, they experienced funding issues during 2004-2007 and as a cost savings measure, reduced coverage of some NPOs in an ad hoc manner, mainly affecting those with less than $750,000 in receipts. Since the vast majority of GuideStar's customers demand contemporaneous rather than archival data, GuideStar has decided not to backfill the missing observations.

[15] We end the sample in 2009 for our short-run price and switching tests to maintain a relatively tight window around the IC shock (nearly all audit firms who issue IC attestations begin doing so prior to 2009). Moreover, given the reduction in the demand shock from AS5 (Schroder and Hogan 2013) and the steady increase in accounting labor after 2004

$500,000 in federal grants because they are not subject to the audit requirement throughout the sample period. We also exclude observations where any of the following fields are missing: total assets at year-end, fees for audit and accounting services, date of the fiscal year-end, or activity type. Consistent with prior work, we find that the number of observations with missing fields is not trivial (Krishnan, Yetman, and Yetman 2006; Yetman and Yetman 2012; Burks 2015). One explanation is that errors that occur while filling out the Form 990 can go unnoticed because the financial statements are audited, not the Form 990. This leaves us with 41,605 NPO-year observations representing 8,123 distinct NPOs. For the specification where the outcome variable is the likelihood of switching auditor, we drop observations without prior-year data. Finally, we winsorize all continuous variables at the 1st and 99th percentile.

We then allocate these observations to treatment and control groups. In the audit fee specification, we assign an NPO to the treatment group in year *t* if its auditor issues IC attestations to public companies in year *t*. In the auditor switch specification, we assign an NPO to the treatment group in year *t* if its year *t-1* auditor issues IC attestations to public companies in year *t*, regardless of whether the NPO stays with that auditor. All NPO-year observations not meeting the treatment definition are in the control group.

In Panels A and B of Table 5, we present the summary statistics for the variables used in the tests of the effect of SOX on audit price and the probability of switching auditor, respectively. *NonCalendarFYE* takes a value of one when an NPO's fiscal year-end falls in a month other than December and zero if the NPO has a December fiscal year-end. Seventy-nine percent of organizations in the treatment group (72% in the control group) have a year-end that falls outside of the auditor busy period. By definition, NPOs in the control group have auditors which never audit public companies and are, therefore, smaller. As such, the NPOs in the control group are also smaller on average. Note that while the descriptive statistics reveal differences between treatment and control NPOs on average, we evaluate pre-treatment trends and conduct several matching analyses to mitigate the threat of such differences generating our results.

3.2.2 NPOs: Results

discussed in Section 2, much of the initial labor inelasticity had been resolved by 2009. We continue our long-run tests to the end of our data in 2013.

To identify the effect of public company demand shocks on NPOs, we use a generalized difference-in-differences design. We compare changes in fees and auditor switch rates of NPOs whose auditors become busy with 404 IC attestation for public company clients to NPOs whose auditors are not busy with IC attestations. Our identifying assumption behind this approach is that changes in fees and switch rates would be similar between these two groups of NPOs *except for* the fact that their auditors became busy with other clients in their portfolios.[16]

One advantage of our approach is that NPOs in the treatment group do not all experience the treatment simultaneously because not all audit firms begin issuing IC attestations in the same year. Section 404 of SOX becomes effective in 2004, but covers only public companies with a public float above a certain threshold (Albuquerque and Zhu 2018). Larger auditors with large public company clients perform their first IC attestation in 2004. However, many smaller auditors begin issuing IC attestations at different points in time over the subsequent years, depending on when the public companies in their client portfolio become subject to the mandate.[17] Consequently, the year in which treated NPOs first experience the treatment depends on their auditor and when that auditor begins issuing IC attestations to public companies.[18] Our difference-in-differences specification to examine audit fees is:

$$LnFees_{NPO,t} = \alpha_{NPO\ x\ auditor} + \alpha_t + \beta_1 \times AuditorBusy404_{NPO,t} + \beta_2 \times LnAssets_{NPO,t} + \varepsilon_{NPO,t} \quad (2)$$

The main regression includes NPO-auditor and year fixed effects. By including NPO-auditor fixed effects,

[16] Note that NPOs with auditors not issuing IC attestations (i.e., the control group) likely experience some effects of the SOX shock as well. For example, their auditor might lose staff to audit firms who issue IC attestations. Our design therefore measures the difference in the intensity of the spillover, which likely results in a lower bound of the treatment effect of SOX in the nonprofit sector.

[17] We identify 138 auditors who both cater to NPOs and issue IC attestations during our sample period. Fifty-seven of the auditors perform their first IC attestation in 2004, while the remainder do so over the subsequent years. Refer to Iliev (2010) for a year-by-year discussion of the specific criteria that determine whether a public company is subject to the mandate.

[18] We perform a robustness test where we exclude NPOs who first experience the treatment through a Big 4 auditor. The results are weaker because of sample attrition, but remain statistically significant. We also argue that the 2002 failure of Arthur Andersen is unlikely to confound our analysis for several reasons. First, Andersen, which had only 60 NPO clients in our sample, was not a major player in the NPO segment. By comparison, the other Big 4 auditors collectively had 779 nonprofit clients. Second, we replicate all our analyses after excluding former Andersen clients, and arrive at nearly identical results. Third, the timing of Andersen's failure works against us finding results because all of Andersen's clients switched to other auditors before 2002.

we estimate the model within auditee-auditor relationship and therefore capture the change in fees for NPOs who stay with their auditor. The year fixed effects reduce the likelihood that year-specific factors such as changes in economic conditions or other regulatory developments drive our results. We then run a version of regression (2) in which we include activity type-year and state-year fixed effects. We base the 26 activity types on the National Taxonomy of Exempt Entities, which is the standard classification system for NPOs. *AuditorBusy404* is the treatment variable of interest. The main effects are absorbed by the fixed effect structure. In addition to our fixed effects, we also control for NPO size using *LnAssets*, equal to the natural logarithm of assets reported on the NPO's Form 990, but our inferences are similar if we use revenue instead of assets. We cluster the standard errors by auditor.

We next investigate the effect of SOX on the likelihood of switching auditor. We use a similar difference-in-differences specification to the fees regression above:

$$SwitchAuditor_{NPO,t} = \alpha_{NPO\ x\ auditor,t-1} + \alpha_t + \beta_1 \times Auditor_{t-1}Busy404_{NPO,t} + \beta_2 \times LnAssets_{NPO,t-1} + \varepsilon_{NPO,t} \quad (3)$$

SwitchAuditor is an indicator variable that takes a value of 1 if the NPO has a different auditor in year *t* relative to *t-1*.[19] $Auditor_{t-1}Busy404_{NPO,t}$ is the treatment variable of interest and equals 1 when the NPO's auditor (identified in year *t-1*) has a client requiring IC testing in year *t*.[20]

Our design mitigates issues of omitted variables. An omitted variable would have to affect NPOs in the treatment group (i.e., NPOs with auditors who issue IC attestations at some point in time) differently from NPOs in the control group *and* this differential effect would have to occur at the time auditors issue IC attestations (and not before if the parallel trend assumption holds). Furthermore, the omitted variable would

[19] To ensure *SwitchAuditor* excludes mergers and acquisitions activities between audit firms, we manually inspect the names of all audit firms and track systematic movements of clients across auditors for every year in our sample. We adjust our code so as not to count these movements as switches; however, our results are economically and statistically similar if we count mergers and acquisitions of audit firms as auditor switches.

[20] We measure the auditor-client match in year *t-1* and auditor busyness in year *t* to allow for NPOs to switch auditors simultaneously with the busyness of the auditor. This definition assumes that auditors anticipate being busy in year *t* with IC attestation. However, if we redefine the variable to measure switching starting the year *after* an auditor engages in IC attestation (i.e., $Auditor_{t-1}Busy404_{NPO,t-1}$), we continue to find a significant increase in switches (untabulated). This result can also be observed by examining Figure 2b and noting the continued increase after year *t*=0.

also have to explain why the outcome reverses for NPOs who withdraw from the treatment by switching auditor.

We first examine pricing effects. Figure 2A plots the results of estimating equation 2 in event time. Point t=0 is the first year an NPO's auditor issues IC attestations to public companies and period t-1 serves as the benchmark period. The figure shows the coefficient estimates and their 95% confidence intervals for each period. Importantly, note that prior to t=0, the coefficients are very close to zero, providing evidence supporting the parallel trends assumption necessary for our identification strategy—i.e., audit fee changes for treatment and control NPOs did not change differentially *prior to* Section 404 implementation. NPOs in the treatment group experience an increase in audit fees relative to the control group as soon as their auditor begins issuing IC attestations (i.e., at t=0 and thereafter). Our point estimates of the increase in audit fees range from 4.0% to 9.5%, depending on the year. Since we perform the estimation within NPO-auditor relationship (i.e., we include NPO-auditor fixed effects), we capture the effect on audit fees conditional on the NPO not having switched auditor.[21]

Table 6, column 1 tabulates the results of estimating equation (2). We find that NPOs experience an incremental 6.5% increase in audit fees when their auditor becomes busy with IC attestations. We include activity type-year and state-year fixed effects in column 2, and find the magnitude of the effect attenuates slightly to 5.4%. This specification alleviates the concern that different time trends across activity types or states influences our results. While the use of fixed effects and the pre-treatment parallel trends substantially mitigate time-invariant omitted variable concerns, one may still be concerned that differences in the treatment and control NPOs revealed in Table 5 could be associated with different responses to SOX Section 404. A particular threat to our inference is that the treated firms are larger and that larger NPOs experienced an audit demand shock in conjunction with SOX. To mitigate this concern, in column 3 we re-estimate equation (2) after matching treated and control observations based on size, activity type and year. The results only slightly

[21] Recall, in our private company analysis, we find evidence of a positive attestation supply shock in 2002 to 2003, consistent with the outflow of auditors from the public company accounting market identified in DeFond and Lennox (2011). We test for a 2002-2003 response in the NPO market as well, but do not find evidence of one. We speculate the reason for this is that the human capital exiting the public market specialized in for-profit companies.

attenuate and remain significant.[22] In column 4, we assume that audit offices are independent within an audit firm and redefine the busyness treatment variable based on the audit office serving the NPO. We consider the office-level specification more restrictive because it suggests NPOs audited by auditors who become busy in other offices are not affected by personnel shuffling within an audit firm across offices—even though audit staff often travel for a given auditor. Again, we find similar results under this more restrictive specification.

One concern with the results in the first four columns of Table 6 is that features of the audit product changed in conjunction with SOX. For example, the PCAOB began inspections of audit firms starting in 2003. The fee increases could therefore be the result of a different audit product for NPOs with inspected auditors, rather than a result of a constrained supply (e.g., Gipper, Leuz, and Maffett 2017; Aobdia 2017; Aobdia and Shroff 2017; DeFond and Lennox 2017; Shroff 2017; Aobdia 2018) and even the matching specification is unable to identify these differences. To mitigate this concern, we exploit NPO fiscal year-ends, similar to our private company analysis. In column 5 of Table 6, we interact the treatment variable *AuditorBusy404* with *Non Calendar FYE* and find a negative and statistically significant coefficient on the interaction term. These findings suggest that an NPO whose fiscal year-end falls outside of its auditor's busy season experiences a smaller (though significant) increase in audit fees (an F-test of the sum of *AuditorBusy404* and *AuditorBusy404*NonCalendarFYE* produces a p-value of 0.0301).[23] Figure A4 in the supplemental appendix plots these results and further shows that the pre-treatment parallel trends assumption holds. We find the same results (untabulated) when we conduct the analysis after matching December and non-December fiscal year-end NPOs, thus ensuring that observable differences between NPOs based on the timing of the fiscal year-end are not driving the results. Analogous to our results for private company audit decisions, this cross-

[22] While the matching specification mitigates concerns that size differences between treatment and control NPOs are creating the results, this approach comes at a cost: it eliminates the larger NPOs from the analysis and, because they are more frequently using larger auditors, these are the NPOs which are most affected by the public company demand shock caused by SOX. Therefore, it is not surprising that the coefficient in column 3 slightly attenuates.

[23] Similar to the private company analysis, one concern is that NPOs could simply switch their fiscal year-end. This is also costly for NPOs. They often establish fiscal year-ends to comport with funding cycles of their major contributors. Moreover, switching a fiscal year-end requires two audits within the same year (shortened fiscal year and new fiscal year), minimizing the savings. Therefore, it is not surprising that we see very few fiscal year-end switches for NPOs in our sample.

sectional finding supports the inference that the increase in audit fees is related to labor inelasticity.

To reinforce this inference, we examine the magnitude of the interaction coefficient by year. If this coefficient is identifying differences in short-run labor inelasticity and labor is relatively elastic in the long-run, then we should find the magnitude of the cross-sectional coefficient attenuates as time moves further from the SOX shock. In column 6 of Table 6, we show the coefficients of the interactions between *Auditor-Busy404* and *Non Calendar FYE*, estimated each year. We find that the magnitude of the coefficient decreases after 2006 and becomes insignificant by 2009—coinciding almost exactly with the same time new accounting labor flows into accounting firms per Figure A2. Under the mild assumption that non-December fiscal year NPOs do not have a differential change in audit demand relative to December fiscal year NPOs, these cross-sectional findings support the inference that a public company demand shock coupled with an auditor supply inelasticity accounts for our fee results. Alternative demand side explanations have to describe not only a specific demand shock for December year-end private companies and NPOs, but also why that demand shock dissipates over time vis-à-vis non-December entities.

We now examine changes in NPO auditor relationships using equation (3). Figure 2B plots the likelihood of switching auditor in event time and finds no difference between treatment and control firms before IC attestations, and a significant increase once an NPO's auditor is required to conduct IC attestations for public companies in its portfolio. Table 7 tabulates the results. In column 1, we find a 6.7 percentage point increase in the probability of an NPO switching auditor at the time their auditor becomes busy with IC attestation for public companies. In column 2, we include activity type-year and state-year fixed effects. In column 3 we match treated and control NPOs based on observable characteristics.[24] In column 4 we change the specification of the busyness variable based on office-level (rather than auditor-level) involvement in Section 404 attestation. In all cases, our results are very similar to column 1, though the results are attenuated after matching, consistent with the treatment effect being stronger for larger NPOs.[25] In untabulated analysis, we repeat

[24] This matching approach provides us with a more balanced sample with respect to both size and number of observations.

[25] In untabulated analyses, we do not find that the switching results are stronger in December fiscal year-end entities, in contrast to the pricing results. We note that this lack of a result is consistent with December year-end entities having

our tests on the subset of NPOs engaging auditors outside the Big 4 and the results remain significant, indicating that switching after SOX is not limited to clients of Big 4 auditors (e.g., Feng and Elder 2017).

Collectively, our results suggest that the demand shock for public company auditors caused by SOX, combined with a labor inelasticity, significantly affected both private companies and NPOs. Specifically, private companies reduced their production of attested financial reports, while NPOs whose auditors became busy with SOX either paid higher audit fees or switched auditors (or both). These findings are consistent with the supply of auditors being sufficiently common that one segment of audit consumers is highly sensitive to the demand shocks experienced by other segments of audit consumers.

4. Long-run Effects: Market Separation and Aggregate Market Consequences

4.1 Market separation

The consequences documented in the prior section could be temporary because they stem from a demand shock and a short-run labor inelasticity. However, as noted in Section 1, SOX Title I altered the nature of auditing regulation, potentially creating longer-run implications. We test for two changes to the audit market: across- and within- audit firm specialization. To test for across-firm separation, we first model the propensity of NPOs to have public company specialist auditors in the first year of our sample (2000) and then use the parameters of this model to predict matches in future years. The idea behind this test is that if the market structure did not change over time, then the predictive ability of the model should perform similarly each year.[26]

To operationalize our tests, we exploit differential regulatory effects of SOX across auditors. As part of SOX, the PCAOB established an inspection regime which affected auditors differently according to their

more difficulty switching auditors unconditionally because other audit firms—who are also busy with their December year-end clients—do not want to accept them (Lopez and Peters 2011). These switching frictions for December FYE NPOs are consistent with our pricing tests showing that audit price increases are higher when capacity to take them on is most constrained (i.e., December FYE entities face the greatest price increases, which could reflect their diminished ability to switch).

[26] Our approach directly follows Fama and French (2001), who develop a parsimonious model to estimate the propensity of companies to pay dividends over time. The key appeal of their approach is that it decomposes the aggregate trend in company behavior into two types of changes: (1) changes because the composition of companies evolved in the economy (e.g., more high growth companies); and (2) changes because companies altered their behavior *conditional on their characteristics* (e.g., large companies became less likely to pay dividends).

exposure to public companies. Auditors with more than 100 public clients faced annual (as opposed to triennial) PCAOB inspections and greater overall scrutiny (Lennox and Pittman 2010).[27] These are the audit firms we expect were most affected by the regulatory regime shift and resulting fixed cost change. We estimate a base model using the following probit specification for the year 2000 data:

$$Auditor100PublicClients_{NPO} = \beta_1 \times LnAssets_{NPO} + \alpha_{State} + \alpha_{Activity\ Type} + \varepsilon_{NPO} \quad (4)$$

The dependent variable, *Auditor100PublicClients*, is an indicator equal to one if the NPO uses an auditor with 100 public clients in year *t*. We model this variable as a function of NPO size (adjusted for inflation), and state and activity type fixed effects.[28] We use the estimated parameters from (4) to predict the auditor type for each NPO-year subsequent to the year 2000.

We tabulate the analysis in Table A3 of the supplemental appendix but Figure 3 summarizes the main result. Figure 3 plots the difference in the predicted percentage of NPOs matching to public company auditors (based on the model in (4)) relative to the actual percentage of NPOs matching to public company auditors each year. The figure reveals that in the years leading up to 2004, the difference between the predicted and actual likelihood of matching with a public company specialist audit firm is negligible. After 2004, the model begins performing significantly worse. Specifically, the actual number of NPOs matching to public company auditors is significantly lower than that predicted by the model, consistent with a structural change in the audit market. To ensure robustness of this result, Figure A5 of the supplemental appendix summarizes two alternative specifications. The first excludes smaller NPOs, which are less likely to match with a large auditor in the first place. The second restricts the sample to new NPOs entering the sample. In both cases, the model's predictions are not significantly different from the actual results until 2004. However, after 2004, the predicted percentage of NPOs that match to public company auditors is systematically higher than the actual percentage of NPOs that do so, and this difference persists through 2013. These results provide one

[27] For example, Lennox and Pittman (2010) report that the median inspection length for audit firms with more than 100 clients is five months, compared to just four days for audit firms with fewer than 100 clients.

[28] By having a parsimonious model, we are trading off a reduction in prediction noise in the model with restricting the sample size by imposing variable requirements. NPO size is the most important characteristic, and the predictive ability of our model as measured by the R-squared is similar to that of other auditor matching models in the nonprofit literature (Tate 2007, Feng and Elder 2017, Harris, Tate, and Zimmerman 2017).

piece of evidence that SOX caused public company auditors to specialize in public companies and reduce their work on NPOs.[29]

We next examine the second potential implication of greater market segmentation and specialization: whether auditors serving both public companies and NPOs shifted the distribution of audit work *within* the audit firm to foster specialist offices based on client type. While our results to this point highlight that public company auditors reduced their exposure to NPOs to focus on public companies, they did not completely exit the NPO market for a variety of reasons.[30] One prediction consistent with greater segmentation is that because SOX created rules further distinguishing audits of public companies from nonpublic entities, offices of a given auditor were more likely to focus on either public companies or NPOs, rather than diversify across client types in each office. Such a shift would arise if distinct audit rules create economies of scale in focusing audit labor on single client groups. In other words, the client type concentration for individual offices should increase with the difference between audit standards for each client type.

To test this prediction, we develop two proxies for client concentration across offices. The first step in deriving the two proxies is to compute the client mix of each office, equal to the ratio of the total assets of NPO clients to the total assets of NPO plus public company clients.[31] We then measure the dispersion in client mix across the offices of a given audit firm in a given state. For the first measure, we use the standard deviation of the client mix across offices. For the second measure, we identify the office with the highest NPO client mix and the office with the lowest within an auditor-state and calculate the difference. When all offices within an auditor-state have the same mix of NPO to public client assets, both measures equal zero (i.e., there is no variation in client mix across offices within a state). The measures take a maximum value if

[29] We note that these results do not fully identify why this shift occurs. Based on discussions with audit partners, NPOs are less profitable than public companies because auditors bill equivalent labor at lower hourly rates.

[30] Based on our conversations with audit partners of both Big 4 and mid-tier firms, this occurs for multiple reasons including NPO size (like public companies, some NPOs are simply too big to be audited by smaller audit firms), personal relationships (audit partners doing "favors" for board members of nonprofits who are also executives at public companies), prestige (audit firms gain prestige from auditing certain institutions, and covenant requirements (e.g., some nonprofits have debt with specific auditor requirements). Regardless of the reason, our prediction is that conditional on having NPO clients, audit firms will concentrate clients in offices based on client type (i.e., public versus nonpublic).

[31] One limitation of our approach is that our ratio cannot consider NPOs that do not appear in our dataset or other types of non-public clients.

all offices within an auditor-state exclusively serve one client type.[32] Both measures require an audit firm to have at least two offices in a given state. We further restrict the sample to auditors with both public company and NPO clients in that year. We aggregate the offices at the auditor-state-year level because audit clients and audit staff typically have more mobility within a state than across states. For example, CPA licensing is administered at the state level, which can constrain employee mobility (Cascino, Tamayo, and Vetter 2018). Our results are similar if we do not include the geographic restriction. Our inferences also remain if we more tightly restrict the geographic dimension to the auditor-county-year level, however this version of the test reduces our sample thus raising generalizability issues (i.e., we require two offices for the same auditor within a county).

We estimate the following generalized difference-in-differences model:

$$Y_{auditor,state,t} = \alpha_{auditor\ x\ state} + \alpha_t + \beta_1 \times AuditorState404_{auditor,state,t} + \varepsilon_{auditor,state,t} \quad (5)$$

The unit of observation is auditor-state-year. *AuditorState404* is the treatment variable. It is equal to one if an auditor issues internal control attestations to public companies in a given state-year, and zero otherwise.

Table 8 presents the results of modeling our concentration measures as a function of *AuditorState404* between 2000 and 2013. We find a positive and significant coefficient on *AuditorState404* for both of our office concentration measures. Once the auditor becomes involved with IC attestations, the across-office dispersion in their client focus increases by 0.115 (0.181), an economically significant margin above the 0.257 (0.437) pre-SOX mean of our standard deviation (high minus low) variable.

Taken together, our results from Figure 3 and Table 8 document two distinct aspects of segmentation driven by SOX. Starting in 2004, fewer NPOs match with auditors serving the public company market. The auditors choosing to serve both public companies and NPOs are more likely to concentrate their individual offices on a single client type.

4.2 Aggregate market consequences

[32] Our measurement approach ensures a systematic decline in NPO market share cannot generate an increase in client type dispersion. For example, if an auditor dropped 25% of its NPO clients in all of its offices, our dispersion measures would decrease, rather than increase.

Having confirmed the significant audit supply composition change, we conclude our study by providing descriptive evidence of the aggregate effects of these shifts on NPO audit market concentration. Much attention has been paid to the concentration of audit supply for public companies because of the very high concentration of the Big 4 auditors, but evidence on the supply of auditors in other markets is much more limited (GAO 2003, 2008). To examine the audit market concentration, we follow the literature and calculate the Herfindahl-Hirschman Index (HHI) for the audit supply in the NPO market. Specifically, we use the square of the market share, based on NPO revenue.[33] In Figure 4, we plot the HHI for both the public company audit market and the NPO audit market and find opposite trends. As has been well documented, the public market HHI increases with the demise of Arthur Andersen. By comparison, the concentration in the NPO market does not respond to this event. Beginning with the onset of Section 404 attestation, however, we find the HHI for NPOs substantially *decreases* and, by 2009 is *less than half* its 2003 level.[34] This concentration level continues to decline through 2013. We also note that the long-run decline in market concentration is not driven solely by the decrease in market share of Big 4 auditors. In untabulated results, we find that the number of auditors serving at least five NPOs grew by 26% from 2003 to 2013 (from 54 to 68).

Thus, our results suggest that the public company demand shock caused auditors to reassign constrained labor away from NPOs to public company engagements, creating a large shift in the suppliers of attestation. This, in turn, dramatically changed the concentration of supply.

5. Conclusion

We examine how auditing regulation targeted at public companies affects the attestation market for nonpublic entities. If the attestation market is sufficiently common across consumer types, a demand shock for one set of consumers will affect other consumers. We test this idea in two nonpublic entity settings: private companies and nonprofits. Upon the enactment of Section 404 of SOX, we find that private companies

[33] To provide an HHI that reflects the degree of competition in the overall audit market, we omit the largest 1% of NPOs, which skew the data and are generally too big to be feasible clients of auditors other than the Big 4.

[34] This trend in market share change occurred despite the PCAOB easing much of the public company demand shock for fiscal years ending after November 15, 2007 by passing Auditing Standard No. 5 (see Schroeder and Hogan 2013 for discussion and analysis of AS5 in the public company setting).

reduced their consumption of attestation services. For nonprofit entities required to purchase an annual audit, we find the audit fees of nonprofits whose auditors conducted public company audits increased by 6% more than nonprofits whose auditors were not occupied with SOX-related engagements. These results—highlighting significant spillover effects to the attestation market for unregulated entities—are important because nonpublic entities also rely on attestation services for management and accessing capital markets.

Moreover, we find a significant transformation in the market for NPO audits. NPOs were less likely to match with an auditor specializing in public company audits. Those auditors continuing to serve both public companies and nonpublic entities increased their concentration of client types in individual offices. Starting in 2004, the concentration of audit supply to NPOs decreased by half as NPOs turned away from public company auditors. This finding points researchers to a potentially powerful setting in which to examine the link between concentration and competition in the audit market—an area of current interest to both regulators and academics. This supply transformation also appears to be an acceleration of the separation in the audit markets. That is, while the audit market was sufficiently common for a shock to one set of audit consumers to affect another prior to the regulation, the longer run trend of audit suppliers focusing on specific consumers (e.g., Doogar and Easley 1998; Ferguson et al. 2017) advanced because of public company regulation.

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Appendix A: Private Company Variable Definitions

Variable	Definition
Unqualified Audits	A financial statement audit provides positive assurance that the financial statements are reported in accordance with Generally Accepted Accounting Principles. An unqualified audit opinion indicates that the auditor believes that the financial statements are materially in accordance with GAAP. Unqualified audited financial statements are accompanied by complete footnote disclosure, providing the most complete set of information of all of the reports along with the highest level of assurance and no detected material deviations from GAAP.
Reviews	Financial statement reviews provide negative assurance. An independent accountant performs analytical procedures (e.g., ratio analyses) and interviews management to assess whether the financial statements are misstated; however, the accountant does not perform substantive procedures to obtain positive evidence of an account balance. Reviews are generally accompanied by complete footnote disclosure; therefore, reviewed financial statements provide a similar information set to unqualified audits, but the information has a significantly lower level of assurance, reporting quality, and cost.
Compilations	A compilation provides no assurance about the financial statement balances reported in the financial statements. An accountant puts the company's financial information in the form of financial statements but performs no procedures and provides no assurance as to the reporting quality. Compilations include all three standard financial statements, but are not required to report (and generally omit) footnote disclosures. Therefore, compilations provide substantially less assurance and information than either audits or reviews.
% CPA	The percent of financial statements collected that are unqualified audits, reviews, or compilations. The unit of observation is industry-region-company size category-year.
% Unqualified	The percent of financial statements collected that are unqualified audits. The unit of observation is industry-region-company size category-year.
% Reviews	The percent of financial statements collected that are reviews. The unit of observation is industry-region-company size category-year.
% Compilations	The percent of financial statements collected that are compilations. The unit of observation is industry-region-company size category-year.
Year 04-09	An indicator equal to one between 2004 and 2009.
Year 02-03	An indicator equal to one in 2002 and 2003.
Non Calendar FYE Share	The percent of private companies within an industry with non-December fiscal year-ends, measured in 2003 using the company-level Sageworks dataset described in Minnis (2011). The unit of observation is industry.

Appendix B: NPO Variable Definitions

Variable	Definition
Fees	The fees for external accounting and audit services reported on the NPO's Form 990 in a given year.
SwitchAuditor	An indicator variable equal to one if the NPO's auditor differs from that in the prior year.
Auditor100Public-Clients	An indicator variable equal to one if the NPO uses an auditor with 100 public clients in a given year.
Big4Auditor	An indicator variable equal to one if the NPO uses a Big 4 auditor in a given year.
ClientMixDispersion	We use two versions of the variable: *ClientMixDispersion* (Standard Deviation): The standard deviation of the client mix across the offices of a given auditor, in a given state, for a given year. Client mix is measured as the total assets of NPO clients over the sum of total assets of NPO and public company clients for a given office, for a given year. *ClientMixDispersion* (Highest minus Lowest): The difference between the highest and lowest client mix across the offices of a given auditor, in a given state, for a given year. Client mix is measured as the total assets of NPO clients over the sum of total assets of NPO and public company clients for a given office, for a given year.
AuditorBusy404	We use two versions of the variable: *AuditorBusy404* (used in the *Fees* regression): An indicator variable equal to one if, in a given year, the NPO's auditor issues internal control attestations to public companies. If the NPO's auditor never issues internal control attestations, then *AuditorBusy404* is always zero. $Auditor_{t-1}Busy404_t$ (used in the *SwitchAuditor* regression): An indicator variable equal to one if, in a given year, the NPO's *prior year* auditor issues internal control attestations to public companies in the *current year*. If the NPO's prior year auditor never issues internal control attestations, then $Auditor_{t-1}Busy404_t$ is always zero. In robustness tests, we redefine both of these variables by identifying the specific audit office in charge of the NPO audit. We measure *AuditorOfficeBusy404* as equal to one if the *specific office* of their auditor issues internal control attestations to public companies.
Assets	The assets reported on the NPO's Form 990 in a given year.
Non Calendar FYE	An indicator variable equal to one if the fiscal year-end of the NPO does not end in December.
AuditorState404	An indicator variable equal to one if an auditor issues internal control attestations to public companies in a given state, in a given year.

Figure 1: Bank Statement Collection from Private Companies by Year
Figure 1 is a graphical representation of the effect of SOX on private company reporting to banks. The figure assesses the parallel trends assumption. We estimate a version of equation (1) where we model *% CPA*, the proportion of financial reports which are either unqualified audits, reviews, or compilations. Year 2004 has a coefficient of zero and no confidence interval because it serves as the benchmark period. We plot the coefficients and their 95% confidence interval.

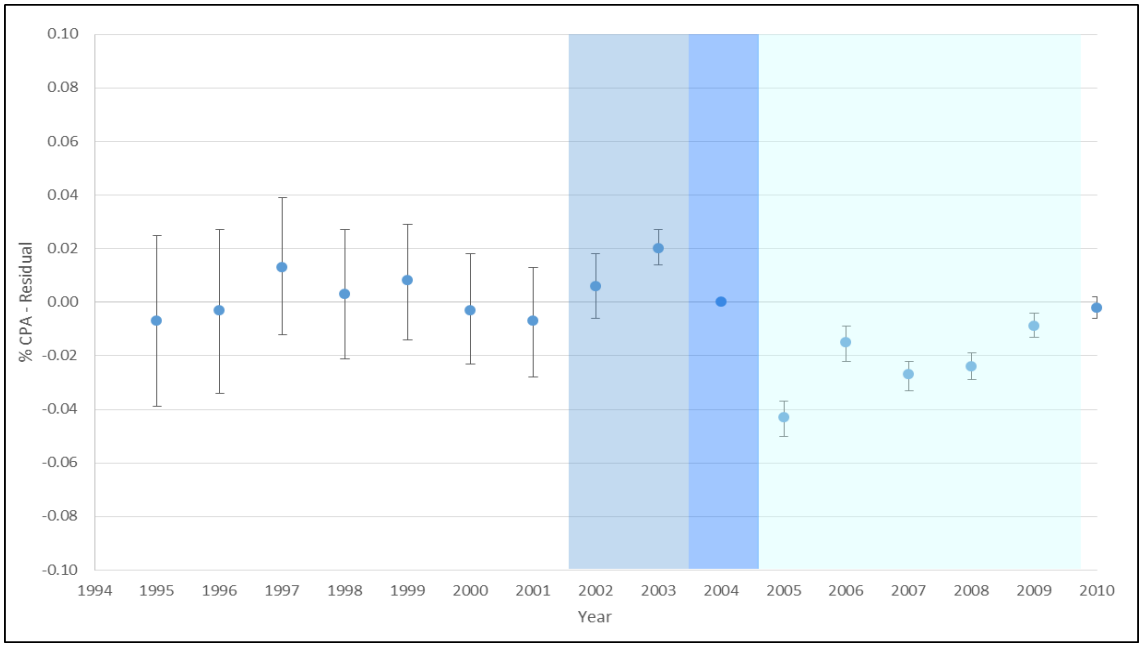

Figure 2: Audit Price Treatment Effect in Event Time

Figure 2A

Figure 2A is a graphical representation of the difference-in-differences analysis of the effect of SOX on audit price in the nonprofit sector. We estimate equation (2) but replace the AuditorBusy404 indicator variable with separate indicator variables, each marking one year over the t−4 to t+5 period relative to t=0, which is the first year an NPO's auditor issues internal control attestations to public companies. Period t-1 has a coefficient of zero and no confidence interval because it serves as the benchmark period. We plot the coefficients and their 95% confidence interval. These coefficients capture the difference of the realized outcome between the treatment and control groups, in event time.

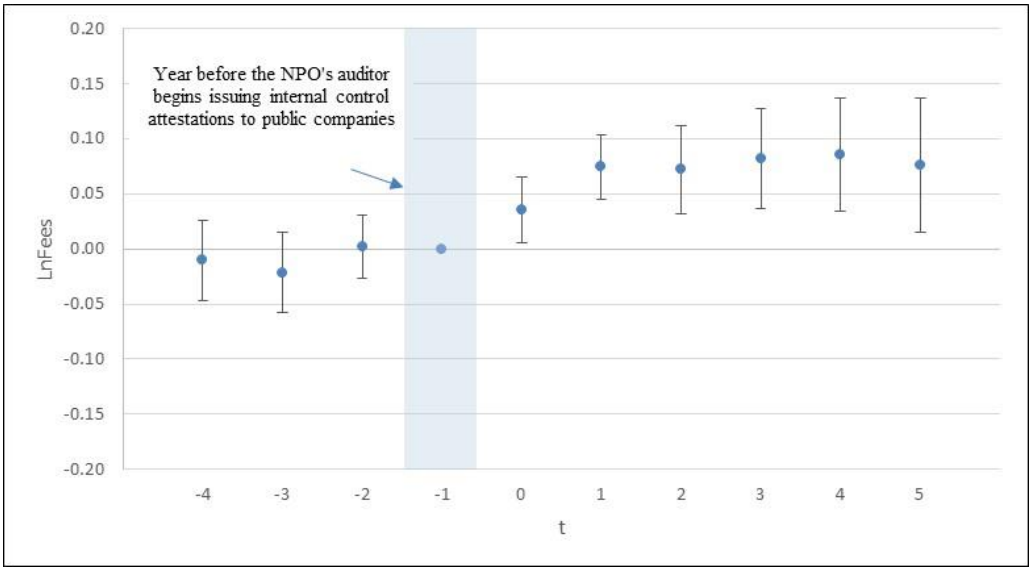

Figure 2B

Figure 2B is a graphical representation of the difference-in-differences analysis of the likelihood of switching auditor in the nonprofit sector. We estimate equation (3) but replace the $Auditor_{t-1}Busy404_{NPO,t}$ indicator variable with separate indicator variables, each marking one year over the t−3 to t+5 period relative to t=0, which is the first year an NPO's prior year auditor issues internal control attestations to public companies. Period t-1 has a coefficient of zero and no confidence interval because it serves as the benchmark period. We plot the coefficients and their 95% confidence interval. These coefficients capture the difference of the realized outcome between the treatment and control groups, in event time.

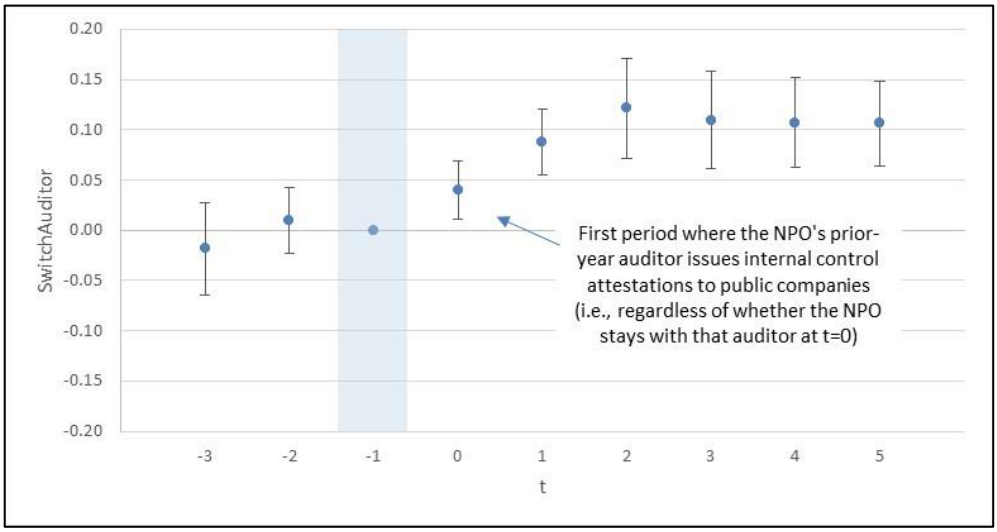

Figure 3: NPO Characteristics and the Propensity to use an Auditor with Public Clients
This figure presents the difference between the actual and predicted propensity of NPOs to match with large auditors of public companies. We estimated the predicted propensity using the probit model from eq. (4) estimated in 2000. The sample includes all NPOs in the GuideStar dataset. The bandwidths correspond to the 95% confidence interval based on standard errors clustered at the auditor level.

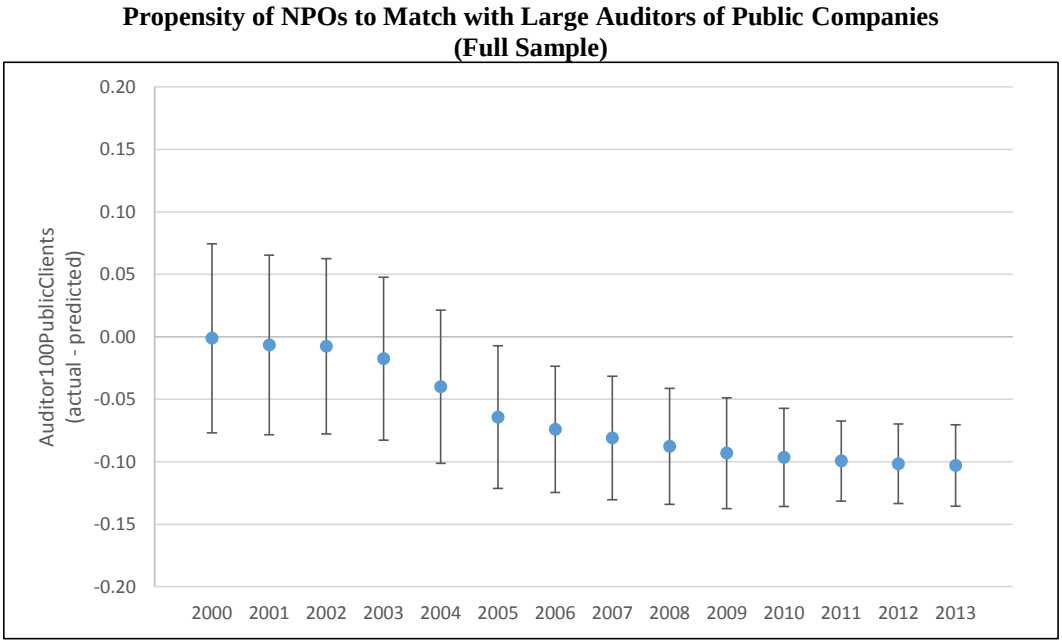

Figure 4: Audit Market Concentration
This figure plots the Herfindahl-Hirschman Index for the audit market of both public companies (line) and NPOs (bar). The Index is based on revenue-weighted market shares calculated for the two markets separately.

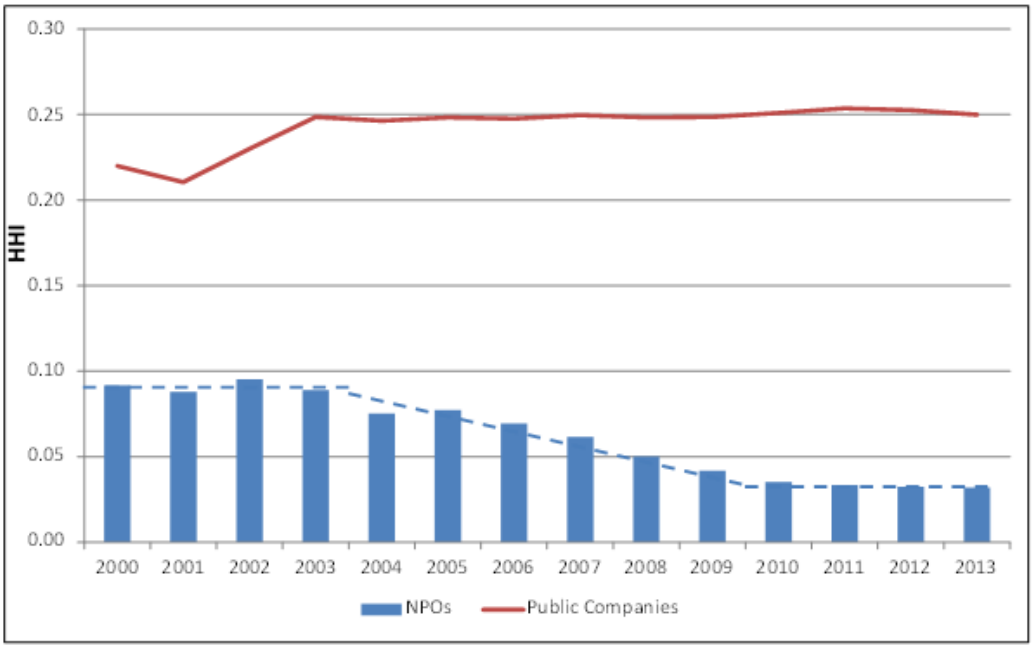

Table 1: Private Company Reporting to Banks – Summary Statistics

This table presents summary statistics for the industry-region-company size category-year observations in our Table 2 and 3 tests. The sample period is 1995-2011. The *NonCalendarFYEShare* sample is slightly less populated than the rest of the sample because the Sageworks dataset covers slightly fewer industries than the RMA dataset. See Appendix A for variables definitions.

	Mean	*Std Dev*	*25%*	*50%*	*75%*	*N*
% CPA	0.539	0.212	0.400	0.560	0.695	34,902
% Unqualified	0.205	0.232	0.011	0.111	0.333	34,902
% Reviews	0.152	0.149	0.024	0.118	0.231	34,902
% Compilations	0.182	0.153	0.056	0.157	0.278	34,902
Year 02-03	0.120	0.325	0.000	0.000	0.000	34,902
Year 04-09	0.372	0.483	0.000	0.000	1.000	34,902
NonCalendarFYEShare	0.182	0.077	0.133	0.180	0.228	34,105

Table 2: SOX and Reporting Practices of Private Companies

This table models financial statement collection by banks as a function of time. The dependent variable is *% CPA*, the proportion of financial reports which are either unqualified audits, reviews, or compilations. *Year 04-09* and *Year 02-03* are indicators for observations in those respective years. *Non Calendar FYE Share* is the industry's decile of percent of private companies with a non-December fiscal year-end. The sample spans 1995-2011. The unit of observation is industry-region-company size category-year. Reported below the coefficients are t-statistics calculated with standard errors clustered at the industry level. *, **, *** indicate significance at the two-tailed 10%, 5% and 1% levels, respectively. See Appendix A for variable definitions.

	(1)	(2)	(3)	(4)
	% CPA	% CPA	% CPA	% CPA
Year 04-09	-0.022***	-0.019***	-0.038***	
	[-7.15]	[-5.05]	[-7.49]	
Year 02-03		0.014***		
		[2.72]		
Year 04-09 * Non Calendar FYE Share			0.003***	0.003**
			[3.08]	[2.20]
Adj R-Sq.	0.781	0.781	0.782	0.792
N	34,902	34,902	34,105	34,105
Linear Trend	Yes	Yes	Yes	No
Industry FEs	Yes	Yes	Yes	Yes
Region FEs	Yes	Yes	No	Yes
Size Category FEs	Yes	Yes	No	Yes
Sector x Year FEs	No	No	No	Yes
Clustering	Industry	Industry	Industry	Industry

Table 3: SOX and Reporting Practices of Private Companies by Statement Type

This table models financial statement collection by banks as a function of time. The dependent variable in column 1 (2, 3) is *% Unqualified (% Reviews, % Compilations*). *Year 04-09* is an indicator for observations in those respective years. The sample spans 1995-2011. The unit of observation is industry-region-company size category-year. Reported below the coefficients are t-statistics calculated with standard errors clustered at the industry level. *, **, *** indicate significance at the two-tailed 10%, 5% and 1% levels, respectively. See Appendix A for variables definitions.

	(1)	(2)	(3)
	% Unqualified	% Reviews	% Compilations
Year 04-09	-0.001	-0.008***	-0.029***
	[-0.36]	[-2.90]	[-8.36]
Year 04-09 * Non Calendar FYE Share	0.000	0.000	0.002***
	[0.06]	[0.97]	[4.32]
Adj R-Sq.	0.826	0.673	0.604
N	34,105	34,105	34,105
Linear Trend	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes
Region FEs	Yes	Yes	Yes
Size Category FEs	Yes	Yes	Yes
Clustering	Industry	Industry	Industry

Table 4: Nonprofit Sector – Sample Selection

This table reports the sample selection for data used in the tests reported in Tables 5-7. We exclude NPO-years expending less than $500,000 in grants because these NPOs are not subject to the audit reporting requirement (and are therefore not observable) throughout the sample period. We also exclude observations where any of the following data points are missing: total assets at year-end, fees for audit and accounting services, fiscal year-end, or activity type. We drop additional observations in the second column because constructing the *SwitchAuditor* variable requires prior year data. We assign an NPO to the treatment group if its auditor issues internal control attestations to public companies at some point during the sample period. We use Audit Analytics' SOX 404 database to determine whether auditors issue internal control attestations.

	Table 6 (Fees)	Table 7 (SwitchAuditor)
Intersection of Audit Analytics nonprofit database and Guidestar database, 2000-2009	67,886	67,886
Less: observations with less than $500K in grants	(3,308)	(3,308)
Less: observations with missing data	(22,973)	(22,973)
Less: observations without prior year data	N/A	(11,583)
Number of NPO-year observations	41,605	30,022
Observations in treatment group	15,029	11,649
Observations in control group	26,576	18,373
Number of NPOs	8,123	6,025
NPOs in treatment group	2,432	2,070
NPOs in control group	5,691	3,970

Table 5: Nonprofit Sector – Summary Statistics

This table reports summary statistics for variables used in the tests reported in Tables 6 and 7. Panel A reports statistics for variables used in our difference-in-differences analysis of the effect of SOX on audit price in the nonprofit sector. We assign an NPO to the treatment group if its auditor issues internal control attestations to public companies at some point during the sample. Panel B reports statistics for variables used in the difference-in-differences analysis of the effect of SOX on the likelihood of switching auditor in the nonprofit sector. We assign an NPO to the treatment group if, at some point during the sample period, its *prior year* auditor issues internal control attestations to public companies in the *current year* (i.e., regardless of whether the NPO stays with that auditor). We winsorize the continuous variables at the 1st and 99th percentile. See Appendix B for variables definitions.

Panel A: Summary statistics for variables used in the test reported in Table 6

Variable	*Mean*	*Std Dev*	*25%*	*50%*	*75%*	*N*
			Treatment group			
Fees	98,459	125,157	25,544	54,785	113,602	15,029
Assets ($ thousands)	144,294	299,102	4,320	19,480	115,020	15,029
Non Calendar FYE	0.79	0.40	1	1	1	15,029
			Control group			
Fees	31,831	51,108	10,900	19,160	34,551	26,576
Assets ($ thousands)	12,143	53,412	1,248	3,122	7,745	26,576
Non Calendar FYE	0.72	0.45	0	1	1	26,576

Panel B: Summary statistics for variables used in the test reported in Table 7

Variable	*Mean*	*Std Dev*	*25%*	*50%*	*75%*	*N*
			Treatment group			
SwitchAuditor	0.09	0.29	0	0	0	11,649
Assets ($ thousands)	159,595	327,801	4,692	24,481	130,963	11,649
Non Calendar FYE	0.80	0.40	1	1	1	11,649
			Control group			
SwitchAuditor	0.05	0.23	0	0	0	18,373
Assets ($ thousands)	10,997	47,701	1,228	3,081	7,666	18,373
Non Calendar FYE	0.73	0.45	0	1	1	18,373

Table 6: The Effect of SOX on Audit Price in the Nonprofit Sector

This table presents the results of our difference-in-differences analysis of the effect of SOX on audit price in the nonprofit sector. *AuditorBusy404* is equal to one during the years where an NPO's auditor issues internal control attestations to public companies. If an NPO's auditor never issues internal control attestations, then *AuditorBusy404* is always zero. *AuditorOfficeBusy404* is a version of *AuditorBusy404* coded at the auditor office level. *Non Calendar FYE* is an indicator for NPOs with non-December fiscal year-ends. In column 3, we use coarsened exact matching to balance the treatment and control groups. Our column 6 tests include interactions between *AuditorBusy404* and each year, but we do not tabulate these interactions for brevity. We base activity types on the National Taxonomy of Exempt Entities. We winsorize the continuous variables at the 1st and 99th percentile. Reported below the coefficients are t-statistics calculated with standard errors clustered at the auditor level. *, **, *** indicate significance at the two-tailed 10%, 5% and 1% levels, respectively. See Appendix B for variables definitions.

	(1)	(2)	(3)	(4)	(5)	(6)
	LnFees	LnFees	LnFees	LnFees	LnFees	LnFees
AuditorBusy404	0.065***	0.054***	0.042**		0.115***	
	[3.69]	[3.17]	[2.21]		[3.90]	
AuditorOfficeBusy404				0.047**		
				[2.27]		
AuditorBusy404*NonCalendarFYE					-0.077***	
					[-2.74]	
AuditorBusy404*NonCalendarFYE*2004						-0.043
						[-0.86]
AuditorBusy404*NonCalendarFYE*2005						-0.093*
						[-1.79]
AuditorBusy404*NonCalendarFYE*2006						-0.110**
						[-2.22]
AuditorBusy404*NonCalendarFYE*2007						-0.101**
						[-2.21]
AuditorBusy404*NonCalendarFYE*2008						-0.081*
						[-1.86]
AuditorBusy404*NonCalendarFYE*2009						-0.020
						[-0.40]
LnAssets	0.151***	0.151***	0.131***	0.151***	0.151***	0.151***
	[5.68]	[5.43]	[5.24]	[5.43]	[5.43]	[4.88]
Adj R-Sq.	0.883	0.884	0.843	0.884	0.884	0.884
N	41,605	41,605	20,078	41,605	41,605	41,605
NPO x auditor FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	No	No	No	No	No
Activity type x Year FEs	No	Yes	Yes	Yes	Yes	Yes
State x Year FEs	No	Yes	Yes	Yes	Yes	Yes
AuditorBusy404* year interactions	No	No	No	No	No	Yes
Clustering	Auditor	Auditor	Auditor	Auditor	Auditor	Auditor

Table 7: The Effect of SOX on the Likelihood of Switching Auditor in the Nonprofit Sector
This table presents the results of our difference-in-differences analysis of the effect of SOX on the likelihood of switching auditor in the nonprofit sector. $Auditor_{t-1}Busy404_t$ is equal to one if an NPO's *prior year* auditor issues internal control attestations to public companies in the *current year* (i.e., regardless of whether the NPO stays with that auditor). If an NPO's prior year auditor never issues internal control attestations, then $Auditor_{t-1}Busy404_t$ is always zero. $AuditorOffice_{t-1}Busy404_t$ is a version of $Auditor_{t-1}Busy404_t$ coded at the auditor office level. The outcome variable, $SwitchAuditor_t$, is equal to one if the NPO switches to a new auditor. In column 3, we use coarsened exact matching to balance the treatment and control groups. We base activity types on the National Taxonomy of Exempt Entities. We winsorize the continuous variables at the 1st and 99th percentile. Reported below the coefficients are t-statistics calculated with standard errors clustered at the auditor level. *, **, *** indicate significance at the two-tailed 10%, 5% and 1% levels, respectively. See Appendix B for variables definitions.

	(1)	(2)	(3)	(4)
	$SwitchAuditor_t$	$SwitchAuditor_t$	$SwitchAuditor_t$	$SwitchAuditor_t$
$Auditor_{t-1}Busy404_t$	0.067***	0.072***	0.056***	
	[4.01]	[4.46]	[3.00]	
$AuditorOffice_{t-1}Busy404_t$				0.067***
				[3.48]
LnAssets	-0.006	-0.010*	-0.021**	-0.010
	[-0.03]	[-1.69]	[-2.10]	[-1.64]
Adj R-Sq.	0.123	0.123	0.099	0.118
N	30,022	30,022	14,382	30,022
NPO x auditor FEs	Yes	Yes	Yes	Yes
Year FEs	Yes	No	No	No
Activity type x Year FEs	No	Yes	Yes	Yes
State x Year FEs	No	Yes	Yes	Yes
Clustering	Auditor	Auditor	Auditor	Auditor

Table 8: The Effect of SOX on Client Mix Dispersion across Offices

This table presents our difference-in-differences analysis of the effect of SOX on the dispersion of the client mix across offices. For every office-year observation, we compute client mix as the total assets of NPO clients over the sum of total assets of NPO clients and public clients. In column 1 (2), we aggregate the observations at the auditor-state-year level by calculating the standard deviation (difference between the highest and lowest) of the offices' client mix for a given auditor-state-year. *AuditorState404* is equal to one during the years where an auditor issues internal control attestations to public clients in that state. The sample is restricted to auditors with both NPO and public clients. The unit of observation is auditor-state-year and we only include observations with at least two offices in a given auditor-state-year. Reported below the coefficients are t-statistics calculated with standard errors clustered at the auditor level. *, **, *** indicate significance at the two-tailed 10%, 5% and 1% levels, respectively. See Appendix B for variables definitions.

	Client Mix Dispersion	
	(1)	(2)
	Standard Deviation	Highest Minus Lowest
AuditorState404	0.115***	0.181***
	[5.86]	[5.78]
Adj R-Sq.	0.556	0.559
N	2,107	2,107
Auditor x state FEs	Yes	Yes
Year FEs	Yes	Yes
Clustering	Auditor	Auditor
Unit of observation	Auditor-state-year	Auditor-state-year