

# **Regulatory Spillovers in Common Audit Markets\***

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# Regulatory Spillovers in Common Audit Markets

## Abstract

We find that Sarbanes-Oxley (SOX) had large, persistent effects on how nonpublic entities access the audit market. Private companies reduced their use of attested financial reports in bank financing by 12%. For nonprofit organizations (NPOs), audit fee increases and the rate of switching to smaller audit firms more than doubled. We trace these effects to a shortage of audit labor by studying exogenous variation in audit labor availability across otherwise similar clients. Moreover, we find the audit supply structure changed. The audit supply concentration of the NPO market dropped in half within five years of SOX and remained at this level through the end of our sample in 2014. Our results demonstrate how public company regulation causes spillovers for nonpublic entities, and identifies significant consequences of regulations that expand beyond audit and disclosure requirements for public firms.

**Keywords:** Sarbanes-Oxley, securities regulation, auditing, market structure, accounting, private firms, non-profits, labor economics.

**JEL Codes:** H83, M12, M21, M41, M42, M48, M50

## 1. Introduction

The market for audit services has attracted considerable attention from academics, regulators, and practitioners, especially following the enactment of the Sarbanes-Oxley Act in 2002 (SOX). Most of the discussion and analysis has focused on the publicly held company clients in the audit market; yet, many other organizations—including private companies, nonprofits, and governmental entities—purchase financial statement attestation from auditors. If all consumers of attestation services purchase from a common pool of attestation suppliers, this raises the possibility that shocks to one set of consumers significantly affect the equilibrium quantity and prices of others. In this paper, we use the passage of SOX to examine how regulatory shifts in the public company audit market affect attestation consumption of nonpublic entities.

There are several motivations for investigating how changes in public company audit regulation affect other markets. First, like public companies, nonpublic entities use audits to access financing, contract with external parties, and constrain agency problems.<sup>1</sup> Therefore, regulation that increases the cost and reduces the availability of attestation for unregulated parties can impose unintended harm. Second, while there has been much interest in 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 firms. However, the extent to which unregulated entities are also affected by SOX are important to the debate, especially if the effects are large and persistent.

SOX caused at least two major changes in the audit market. First, it required public company auditors to provide assurance on their clients' internal controls. This created a substantial demand shock for accounting services by public companies because the auditees needed accounting labor to enhance their internal controls (Bird et al. 2017b) and the auditors had to exert addi-

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<sup>1</sup> For example, see Core et al. 2006, DeFond and Zhang 2014, Minnis 2011, and Lisowsky and Minnis 2017.

tional effort to attest to the effectiveness of those controls (Iliev 2010). Second, it altered the structure of the audit market by establishing an audit regulator—the Public Company Accounting Oversight Board (PCAOB)—with two mandates: (1) to register and periodically inspect public company audit firms; and, (2) to establish auditing standards specifically for public companies. With these new regulations, SOX not only shifted the demand and supply of auditors, but also created a potential fissure between public and nonpublic auditing by adding fixed costs to the audit market of public firms (Ferguson, Pinnuck, and Skinner 2016).

Because prior research on regulation focuses on public companies, how shocks in the public market affect nonpublic entities is an open empirical question. One hypothesis is that public company shocks such as SOX have little effect because the accountant labor supply is separate between public and nonpublic attestation consumers. For example, researchers argue that specialization by client size, ownership, and location contributes to a separation in the audit markets (e.g., Doogar and Easley 1998; Ferguson et al. 2016). If this specialization reduces the potential productivity of nonpublic audit labor on public audit work, then the scope for spillovers is limited. An alternative hypothesis is that the attestation market was sufficiently common across all consumers at the time of SOX that the public company demand shock reduced quantities and increased prices for nonpublic entities. Under this “common market hypothesis,” audit firms with limited resources will shift labor to the higher return public company customers. Nonpublic entities will face price increases or quantity reductions. Our paper investigates the extent to which public company financial reporting regulation affects nonpublic entities.

To investigate our research question, we examine two U.S. nonpublic settings: private companies and nonprofit organizations (NPOs), including charities and governmental entities. The settings are similar in that both sets of entities purchase financial statement attestation ser-

vices from accounting firms. However, the settings also differ in both their reporting mandates and data availability, allowing us to not only use alternative identification strategies, but also examine different responses to SOX. Private companies are not required to purchase attestation services, permitting us to examine whether the companies engage CPA firms for attestation services. However, we do not have access to fee data, restricting us from examining price responses. The NPOs we examine, by contrast, are required to purchase an audit because they receive federal grants. Therefore, we cannot observe changes in the decision to purchase an audit, but we can observe changes in the price paid for an audit and the choice of auditor by the NPO.

We use aggregate private company data covering the years 1995 to 2011 reporting the propensity of private firms to provide their banks with financial statements attested by an outside accountant. Following the public firm auditor registration period—i.e., 2002 to 2003 in which DeFond and Lennox (2011) identify a significant exodus of accounting firms from the public company market—we find private companies *increased* their propensity to provide CPA attested financial reports (i.e., audits, reviews, or compilations) to banks.

We then find these companies significantly reduced their reporting of attested financial statements (opting instead to report tax returns, for example) subsequent to the implementation of Section 404 (i.e., years 2004 to 2009). These results control for secular trends, firm size, geography, and industry effects, and are economically significant. For example, in just the two years 2003 to 2005, the propensity for private companies to have their financial statements attested by an accounting firm dropped by 6.3 percentage points, or approximately 12% of the pre-SOX sample attestation rate. By comparison, the oft-mentioned bank size effect—the difference in firms’ propensity to report attested statements to large versus small banks—is 3.4 percentage points (Berger et al. 2017). These results are important because the literature investigating pri-

vate firms shows that transparent reporting affects these firms' ability to access credit markets (e.g., Blackwell et al. 1998; Allee and Yohn 2009; Minnis 2011; Defond and Zhang 2014; Cassar et al. 2015). Our results suggest one consequence of public firm audit regulation was to affect the ability of private firms to access transparent financial reporting.

Our private company analysis lacks audit-level data, so we also examine the audit market for NPOs. Recipients of large federal grants, including charities and governmental entities such as municipalities, are required to purchase a "Single Audit" every year. A Single Audit includes a standard financial audit, plus special procedures related to the use of federal grant funds. We merge the GuideStar record of IRS Forms 990 containing financial results and audit fees with the Audit Analytics record of Single Audits to create an NPO-auditor panel dataset.

While Section 404 became effective at the same time for many *public companies* (Iliev 2010), our identification strategy for NPOs exploits the fact that Section 404 affected *auditors* differently. Some auditors became busy with internal control (IC) attestation of public companies immediately upon the effective date of Section 404, while other auditors were not engaged 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 use a difference-in-differences design by including NPOs with auditors not providing Section 404 attestations as a control group. 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 will reflect regional or nonprofit segment-specific shocks to the audit market, or auditor-client matching, respectively. If accounting labor supply is sufficiently inelastic and talent is substitutable across client types, NPO clients with audit firms

most affected by the SOX demand shock should in turn be most affected.

We first confirm that prior to the passage of Section 404, audit fees and the propensity to switch auditor move in parallel for NPOs whose auditors are affected by SOX relative to NPOs whose auditors are not.<sup>2</sup> We then find the pattern changes. Once an auditor is involved with issuing IC attestations to its public firm clients, its nonprofit clients experience an increase in fees and are more likely to switch auditors, compared to the nonprofit clients of auditors who do not produce IC attestations. Specifically, we find a 5.4% increase in fees and a 7.2 percentage point increase in the probability of switching auditor. These effects are economically important given the pre-SOX unconditional mean fee increase (auditor switch rate) was 2.7% (6.2%). Our results are not limited to clients of Big 4 auditors, and do not depend on whether we define auditor business based on the audit firm or the more restrictive audit office providing service to the NPO.

A potential concern with our results is that nonpublic entities' attestation demand may have changed in our event window or the nature of the audit product could have changed in conjunction with SOX.<sup>3</sup> To strengthen identification, we conduct cross sectional tests based on entities' fiscal year-ends. Because most companies have fiscal years ending in December and because much of the input related to an audit engagement is required immediately following a company's fiscal year-end, accounting labor market capacity varies within a calendar year (Hay et al. 2006; Chang et al. 2010; Lopez and Peters 2011). Exploiting this within-year variation, which should be unrelated to changes in audit demand caused by SOX, we find the price and quantity responses from our main tests are muted for entities with non-December fiscal year-ends. In other words, when audit firms are less labor constrained at the time of the public com-

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<sup>2</sup> Our results do not manifest *until* the onset of Section 404—at which point public firm audit demand shifted—and not before. This mitigates concerns that the demise of Arthur Andersen is contaminating our treatment effects.

<sup>3</sup> For example, the Public Company Accounting Oversight Board (PCAOB) began inspecting audit firms in 2003 (e.g., Gipper et al. 2016; Aobdia and Shroff 2017; Shroff 2017). NPOs may have viewed auditors who received these inspections as superior, thus enhancing the audit product relative to the pre-SOX period.

pany demand shock, effects in the nonpublic audit market are much less severe. This reinforces that our main findings are the result of a public company demand shock and inelastic labor supply, rather than nonpublic entity demand shocks. This cross-sectional approach also allows us to add sector-year fixed effects to our tests, alleviating concerns that sector-level responses to either economic conditions or contemporaneous scandals explains our cross sectional findings.

Having established that a public company audit demand shock affected nonpublic entities, we next examine how much the shock changed the aggregate audit supply, and whether the new equilibrium persisted. For these analyses, we focus on the NPO setting because we observe the identities of nearly every audit consumer and supplier. We first examine changes in which auditors serviced the NPO market. Recall from the main analysis that we identified a significant increase in auditor switches upon the enactment of IC attestation. Examining this further, we find most auditor changes by NPOs *before* the public company demand shock occur *within* an auditor type with some overall net movement from Big 4 to mid-tier auditors, consistent with prior literature investigating NPO audits prior to SOX (Tate 2007).

However, we identify a stark change precisely when auditors become busy with Section 404 implementation: the rate of auditor switching jumps sharply in 2004 and much of this increase is attributable to switches *across* auditor types. In line with contemporaneous work (e.g., Feng and Elder 2017) we confirm the ratio of switches *out of* Big 4 auditors to switches *into* Big 4 auditors, which averaged 5.2 between 2001 and 2003, jumped to 18.0 between 2004 and 2006. Moreover, we find between 2003 and 2009, over 1,500 NPO organizations (including governmental entities) left the Big 4, resulting in the Big 4 size-weighted market share declining from 71% to 58%. We then connect the shock to public company audit demand caused by regulation to client switches away from the largest public company auditors by measuring the SOX-related

audit demand shock for each U.S. state (as proxied by the change in audit fees for public companies at the time of IC attestation) and the Big 4 auditor NPO market share. We find states experiencing the most severe public company audit demand shock had the largest drop in Big 4 NPO market share. Thus, our analyses link SOX mandated audit demand to NPO auditor choice.

We then examine longer run aggregate market outcomes. If SOX effects were only driven by a temporary audit demand shock and accounting labor is elastic in the long run, then we would not expect to see longer run consequences. On the other hand, if changes in audit regulations (such as new audit rules and a new regulator) made it more costly to serve both public and nonpublic clients, then the treatment effects we identify might be persistent. Using data extending to 2014, we first confirm that NPOs did not return to their previous auditors and that *new* NPOs more likely selected smaller auditors as they entered the sample. Moreover, analyses using a size-weighted Herfindahl-Hirschman Index (HHI) are even more revealing about the potential long run aggregate implications of the audit supply market shift. While both academic and practitioner literatures raise potential concerns about increased audit market concentration for public companies (e.g., Gerakos and Syverson 2015), we find a sharp *decline* in audit market concentration for NPOs starting precisely in 2004, driven by a shift of NPOs concentrated in the largest auditors to a broader dispersion of NPOs across all auditor types. By 2009, the HHI is half the value it was in 2003 and this level persists to the end of our GuideStar sample in 2011.

Collectively, although these longer run tests are not causal, they are consistent with SOX further separating the market into two tiers: large public and nonpublic audit firms. While we are cautious about implications, if dispersed audit supply enhances competition, then one potential longer run benefit of public company regulation is a more competitive audit market for NPOs.

Our paper contributes to research examining the effects of securities regulation, and SOX

in particular. Prior literature focuses on measuring effects of SOX on public companies (Engel et al. 2007; Zhang 2007; Cohen, Dey, and Lys 2008; Linck et al. 2008; Iliev 2010; Ettredge et al. 2017). Several related papers investigate auditor portfolios and client switches by public companies in conjunction with the demise of Arthur Andersen and the passage of SOX (e.g., Ettredge et al. 2007; Landsman et al. 2009; Hogan and Martin 2009). This work finds that the Big 4 shifted away from small public clients to large public clients between 2002 and 2004. Our paper contributes by investigating how public company regulation affects *nonpublic* entities and identifying a specific channel through which the regulation affected nonpublic entities: a demand shock for public firms coupled with inelastic labor supply. Moreover, we find that demand shocks in the market for public company auditing have important effects on audit supply for private companies. Therefore, our findings are relevant to the growing body of research studying the reporting practices of unregulated entities.<sup>4</sup>

Second, we demonstrate how audit regulation in one market can affect the overall audit supply structure. In particular, we find that SOX accelerated the separation of the public and nonpublic entity markets, and significantly reduced the concentration of the nonpublic entity market. Therefore, our results are relevant to the line of work concerned with the emergence of Big N auditors (Ferguson et al. 2016), as well as work relating audit consumer surplus to the structure of the audit market (Gerakos and Syverson 2015). Moreover, by documenting the role of labor supply inelasticity in these audit market developments, our work contributes to research studying flows of accounting labor and frictions constraining these flows (Kleiner 2000; Barrios 2017; Bird et al. 2017a; Bloomfield et al. 2017).

## **2. Setting and Motivation**

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<sup>4</sup> See, for example: Core et al. 2006; Tate 2007; Allee and Yohn 2009; Stefaniak 2009; Vermeer et al. 2009; Kitching 2009; Minnis 2011; Petrovits et al. 2011; Burks 2015; Minnis and Sutherland 2017.

## *2.1 Prior literature investigating SOX*

SOX made at least two major changes to the public company audit market. First, the reporting and verification rules of Section 404 required an independent auditor to attest to the internal controls of a public company. This attestation mandate increased both the external and internal audit demand by public companies. Specifically, 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).

Second, SOX established the PCAOB, which in turn required auditors of public companies to register and submit to periodic inspections. DeFond and Lennox (2011) find this requirement caused many smaller auditors to exit the public company audit market, especially before 2004. Moreover, several papers find an increase in the frequency of auditor switches, including departures from the Big N, during the 2000s (Rama and Read 2006; Doogar et al. 2007; Ettredge et al. 2007; Hogan and Martin 2009; Landsman et al. 2009; Chang et al. 2010). In a contemporaneous study, Feng and Elder (2017) focus exclusively on NPO clients of Big 4 firms, and find their propensity to switch to lower tier 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.

While SOX created several substantial changes to the audit market, empirical evidence has yet to explicitly link client switches or audit fee increases to the extent and timing of specific public auditor involvement in Section 404. The difficulty in disentangling the causes of auditor switches and audit fee increases happening around SOX is the number of other contemporaneous changes such as responses to corporate scandals, the fall of Arthur Andersen, or prevailing economic conditions. These identification challenges have also prevented researchers from docu-

menting how shocks in the public audit market reverberate in nonpublic markets, such as private firms, NPOs, and governmental entities. As we detail in Section 3, an appeal of our setting for investigating the connectedness of public and nonpublic markets is the across-time and auditor variation in the involvement of IC attestation. We also benefit from a longer time series of auditor-consumer relationships than earlier work, which permits us to investigate long run consequences (if any) of the SOX shock.

## *2.2 Audit demand shock and labor supply around the passage of SOX*

We argue that there are two necessary conditions for changes in the public audit market caused by SOX to affect the audit market for nonpublic entities: 1) the regulation caused a demand shock to public firms; and 2) accounting labor was sufficiently inelastic causing audit firms to reallocate resources to address mandated public firm demand. We begin by presenting evidence supporting both of these conditions. Figure 1 plots the annual residuals from a regression of log public firm audit fees on a linear trend, firm size, and industry fixed effects. Consistent with prior work, this plot shows a sharp upward shift in 2004—the first year firms had to comply with the Section 404 provision of SOX (Griffin and Lont 2007; Ettredge et al. 2007; Iliev 2010, Badertscher et al. 2014).<sup>5</sup> Moreover, the increase is large: total audit fees for public firms increased by nearly \$4 billion, or 70% from 2003 to 2004. While per-unit price increases likely account for some portion of the fee spike in Figure 1, an increase in the volume of auditing was undoubtedly an important factor given the considerable amount of effort entailed in Section 404 IC attestation.

We next present descriptive evidence of accounting labor supply around the passage of

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<sup>5</sup> Prior studies such as Badertscher et al. (2014, Figure 2) produce charts similar to our Figure 1. However, in contrast to our Figure 1, the graph in Badertscher et al. increases sharply in 2004, but remains elevated rather than peaking and declining. The difference is caused by our inclusion of a linear trend, which is important for our purposes to eliminate any secular changes in audit fees.

SOX. The ability of accounting firms to satisfy demand is constrained by a number of related factors. Auditing work can only be attested to by Certified Public Accountants (“CPAs”). CPAs receive their license from their respective state Boards of Accountancy, after completing the necessary education requirements, passing the Uniform CPA Exam, and fulfilling the state minimum work experience. In light of these requirements, it typically takes more than six years for a high school graduate to become a CPA. The lag with which accountant supply responds to changes in demand is evident in the pool of CPA candidates sitting for the exam.<sup>6</sup> The number of candidates taking the exam rose by only 2.7% between 2001 and 2002 and 0.8% between 2002 and 2003 (AICPA 2015). This suggests the accounting labor market experienced limited growth prior to SOX coming into effect.

Figure 2A plots the number of individuals working in offices of Certified Public Accountants. Notably, the number of individuals working in CPA offices *declined* at the same time as the sharp increase in audit fees shown in Figure 1, with employment reaching a local low in 2005. We suggest two explanations for this. First, following the passage of SOX, public firms improved their internal controls. To do so, they hired many accountants, including individuals from CPA firms. Figure 2B plots the number of individuals working as “Accountants and Auditors” regardless of what type of entity employs them. In contrast to the decline shown in Figure 2A, Figure 2B shows a steady increase, concentrated between 2002 and 2008.<sup>7</sup> Thus, non-CPA firms such as industrial, financial, and service companies were employing significantly more ac-

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<sup>6</sup> Hiring from outside the pool of U.S. CPAs can help mitigate the shortage of accountants, but is unlikely to allow auditors to fully meet the heightened demand for their services. For example, visa restrictions prevent significant hiring of foreign accountants. Likewise, employing more untrained or unlicensed accountants will expose the auditor to client retention problem, reputation loss, litigation, and high training costs in the face of new regulation. Moreover, other countries—Canada, for example—had their own version of SOX during a similar timeframe, so it is not certain how much labor would be available from outside the U.S.

<sup>7</sup> In untabulated analysis, we compare the number of “Lawyers” to the number of “Accountants and Auditors” in the Bureau of Labor Statistics database. Employment changes in these two professions are almost identical until 2004, when employment of “Accountants and Auditors” jumps.

countants. Second, accounting education and licensure requirements produce a short run inelasticity in supply. Figure 2C plots the number college graduates produced each year in the U.S. It is striking that the number of graduates *fell* around the passage of SOX. The graduation rate recovers by 2009, approximately five years after the audit shock in 2004 and consistent with the five years of formal college education to produce an accountant. 2009 is also the same year the audit fee shock documented in Figure 1 subsides.

Collectively, audit fees for public firms increased substantively in 2004 with the enactment of Section 404 IC testing, while at the same time employment in accounting firms experiences a local low point. These short run attributes were coupled with potentially longer run changes in the form of new audit standards and regulation of public audit firms. To examine the extent to which these audit market features allowed SOX to have spillover effects on entities other than public companies, we examine the audit market for privately held firms and NPOs in both the short- and long run.

### *2.3 Motivation*

Understanding whether SOX had spillover effects on nonpublic entities is important for several reasons. Private companies and NPOs rely on CPA attestation to mitigate information and agency problems just as public companies do. If CPA prepared statements are more difficult for borrowers to obtain, for example, then 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 2017). Moreover, when banks prefer to screen and contract on CPA-prepared financial statements but borrowers cannot obtain them, banks may raise interest rates or collateral requirements, or decrease the amount of credit available (Berger and Udell 2006; Minnis 2011). Thus, private companies may face a higher cost of

capital. For NPOs, Kitching (2010) finds that audit quality affects 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 regulation on nonpublic entities is informative about the full costs of the regulation.

### **3. Effects in Private Firms**

#### *3.1 Private firms: Data and summary statistics*

To examine how changes to the audit market caused by SOX affected private firm attestation, we use data from the RMA's Annual Statement Studies. 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 tabulate the number of unqualified audits, reviews, compilations, tax returns, and other statements submitted by commercial loan applicants and borrowers to member banks. The statement categories are further categorized by six-digit NAICS code, region (Northeast, Southeast, Central, South Central, North Central, and West), and RMA's borrower size bracket (under \$1M of revenue, \$1M-\$3M, \$3M-\$5M, \$5M-\$10M, \$10M-\$25M, and greater than \$25M). The features of the RMA dataset are detailed in Appendix A of Berger et al. (2017) and Lisowsky et al. (2017). To summarize, 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., ensuring the generalizability of our study.

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 required in those papers, 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. Nevertheless, in untabulated robustness analyses we repeat our main tests using the bank-level data for the 2002-2011 period with bank fixed ef-

fects, and find similar results, mitigating concerns that bank-level participation decisions over time are responsible for our results.

Table 1, Panel A reports the number of statements collected by RMA member banks by type and year during our sample period. The unit of observation is three-digit industry-region-RMA firm size category-year. Prior to tabulating these figures, we eliminate statements collected from financial services borrowers (two-digit sectors 52 and 55), and observations with fewer than five statements. We note a prominent trend in the data. While the total statements gathered by RMA has grown over time, collection of CPA-prepared statements, defined as the number of unqualified audits, reviews, and compilations has grown at a much slower rate. The total number of statements has more than doubled from 1995 to 2011, while CPA-prepared statements have increased by only 34%. Our research design employs a variety of trend and time variables to help pinpoint the role of SOX in these developments.

Table 1, Panel B reports descriptive statistics for the variables used in our private firm analysis. In the mean (median) industry-region-firm size-year, 53.9% (56.0%) of the statements are unqualified audits, reviews, or compilations (“% CPA”). Unqualified audits (reviews, compilations) comprise 20.5% (15.2%, 18.2%) of this 53.9% share. 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*). Last, we report summary statistics for the cross-sectional variable used in our tests. *NonCalendarFYEShare* measures the percent of private firms 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 11.9%.

### *3.2 Private firms: Research design*

Our first set of tests uses the RMA data to study the reporting practices of private firms 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-firm 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 ( $\alpha_r$ ), industry ( $\alpha_i$ ), and firm size category ( $\alpha_s$ ) fixed effects. We conduct subsequent tests that control for industry-region-size category or sector-year effects. The coefficient of interest,  $\beta_1$ , measures how the quantity of financial report attestation differs in the years 2002-2003 and 2004-2009. Our prediction is that % *CPA* is lower after Section 404 implementation (i.e., between 2004-2009), but before the supply of auditors has had time to adjust. Following DeFond and Lennox (2011), we predict that % *CPA* increases for 2002-2003, as auditors exiting the public firm market increase accountant supply for private firms. We control for a linear trend term, *Trend*, increasing by one each year following 1995. We cluster our standard errors by industry. Because our sample only spans 1995-2011, we do not cluster by year, but note that our main results are robust to doing so.

### 3.3 Private firms: Results

Table 2 presents the results of estimating (1). Column 1 shows that in the six years after SOX became law, collection of CPA statements declined by 2.2% beyond the long-term trend (i.e., *Year 04-09* is negative and significant). We subject this result to a series of robustness tests. First, we take advantage of the dimensionality of our dataset and include a stricter fixed effect structure. Column 2 repeats our original tests but includes indicators for each industry-region-size category combination. This controls for time-invariant heterogeneity in reporting practices within an industry, across regions and firm size groups (e.g., Berger et al. 2017). Our results are very similar in this specification. Second, we add second and third order trend terms to equation

(1). Column 3 reports similar findings. Third, to address the concern that differences in long-term trends across industries cause our findings, we include industry-specific trends (i.e., Industry x Trend effects). Column 4 shows the coefficient of interest is of similar magnitude to the results in column 2.<sup>8</sup>

Last, we consider the period after SOX was passed but before the November 15<sup>th</sup>, 2004 mandatory compliance date for management reports on internal controls. Column 5 estimates a piecewise regression by adding an indicator for 2002-2003. Private firms were *more* likely to provide CPA-prepared reports to their bank. The coefficient on *Year 02-03* is positive and significant at the 1% level, while the coefficient on *Year 04-09* remains significantly negative. Together, these shifts in reporting practices appear uniquely related to changes in accountant supply driven by SOX. Specifically, the 2002-2003 rise in % CPA coincides with the exit of a large number of auditors from the public firm market, and the period before the remaining public firm auditors were required to attest to their clients' internal controls (DeFond and Lennox 2011). Such an increase, followed by a reversal after SOX enforcement, seems less likely to be explained by responses to economic conditions or early 2000s scandals alone.

To examine changes in attestation by year, instead of grouping treatment years 2002-2003 and 2004-2009, in Figure 3 we plot the coefficients of yearly 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. By contrast, 2003 is significantly positive. 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—

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<sup>8</sup> To ensure entry by small firms 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 firms with less than \$10M of revenue, and arrive at the same results. Our results are also the same if we include a firm size category-specific trend term.

and consistent with the timing of the majority of the public company demand shock. % CPA slowly reverts to the trend rate by 2010.

We next conduct cross sectional tests to tie these initial findings more directly to supply and demand factors in the market for accountant services. Recall the basic argument: audit firms did not have sufficient labor to supply all consumers, and private firms have more elastic demand than public firms and NPOs. 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 within-year variation in the demand for accountants to vary labor constraints while holding constant private company audit demand. The preponderance of December fiscal year-end firms 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. We suggest that private firms with December year-ends will therefore experience more negative spillover effects from SOX than firms with fiscal year-ends outside of the busy season.

Table 3 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* (transformed to the interval [0,1]) to facilitate interpretation of the economic importance of audit supply slack to responses to SOX. Consistent with our predictions, Column 1 shows a positive and significant coefficient on *Year 04-09 \* Non Calendar FYE Share*. Moving from the lowest to highest decile of slack substantially reverses the post-SOX decline in % CPA. Thus, firms with a December fiscal year-end are more affected by the increases in public firm accountant demand driven by SOX.<sup>9</sup>

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<sup>9</sup> The overwhelming majority of firms 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 gener-

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 pressure during the sample period; and 2) within-sector, across industry variation in fiscal year-ends is unrelated to sector-level developments during our sample.<sup>10</sup>

Columns 2 and 3 present the results using fully saturated specifications. We find that controlling for sector-year effects does not affect our original inferences. Firms belonging to industries with more off cycle reporting are less exposed to the accountant supply inelasticity. The magnitude of the interaction term in column 2 is slightly larger than that in column 1 (while the main effects are absorbed by the sector-year indicators). Column 3 includes sector-*region*-year effects to account for differences in sector conditions across different parts of the country. Our results remain.

So far, we find the quantity of attestation services consumed by private companies was significantly affected by a public company audit shock. However, we are unable to measure price

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ally require the entity to have the same fiscal year-end as the majority owner(s)—which is typically an individual with a calendar tax year (i.e., ending in December). 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 firms have December year-ends and that this rarely changes (see IRS Publication 538).

<sup>10</sup> We validate this approach in two ways. First, 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). Second, we find considerable stickiness in fiscal year-ends in our setting. Tax laws require many private firm owners to maintain a fiscal year tied to their tax reporting year, and we find few private firms change their fiscal year-ends in the Sagedworks data.

changes in response to this shock because we do not have audit price data for private firms. Therefore, we turn to tests in the nonprofit setting.

## **4. Effects in NPOs**

### *4.1 NPOs: Data and summary statistics*

NPOs and governmental entities who receive grants of \$500,000 or more from the federal government are required to obtain an attestation referred to as a “Single Audit.”<sup>11</sup> An independent CPA must conduct the audit and submit the audit report to the Federal Audit Clearinghouse, which in turn makes the report publicly accessible.<sup>12</sup> Audit Analytics collects data filed by nonprofits subject to a Single Audits, allowing us to observe the auditor identity for each NPO-year. By tracing that auditor in Audit Analytics’ Opinions and SOX404 databases, we can determine whether it also serves public companies, and issues internal control 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.<sup>13</sup> GuideStar extracts the information reported on the Form 990 that public charities 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 public charities 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 small charities in the Form 990 data. First, GuideStar’s coverage of small charities is haphazard during some of the years in our sample pe-

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<sup>11</sup> At the beginning of our sample period, the threshold is \$300,000. Organizations with 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.

<sup>12</sup> These audits are also referred to as “A-133” audits because the Office of Management and Budget (OMB) establishes the rules for these audits in OMB Circular A-133. The circular states that the audits must be conducted using Generally Accepted Government Auditing Standards, which are referred to as the “Yellow Book.”

<sup>13</sup> During our sample window, GuideStar provides the same or better coverage of Form 990 data than the NCCS dataset used in Core et al. (2006).

riod.<sup>14</sup> Second, the revenue threshold for exemption from filing a standard Form 990 varies over time. This causes small charities to leave and re-enter the sample. We address this issue by imposing a balanced panel on GuideStar’s dataset prior to merging it with Audit Analytics. Of course, doing so introduces a survivorship bias concern. To ensure that the balanced panel requirement does not drive our results, we replicate all of our analyses without this requirement, and on charities with receipts above \$750,000, the highest revenue threshold for exemption during our sample period. Our results are qualitatively similar.

Table 4 describes the sample selection procedures. Our sample begins in 2000 because it is the first year Audit Analytics is adequately populated. We begin with the 67,886 NPO-year observations at the intersection of the Audit Analytics nonprofit database and GuideStar. We exclude NPOs with less than \$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 (Krishanan et al 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 Form 990 per se is not audited, the financial statements are. 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 1<sup>st</sup> and 99<sup>th</sup> percentile.

We then allocate these observations to the treatment and control groups. In the audit fee specification, an NPO is assigned to the treatment group in year  $t$  if its auditor issues IC attesta-

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<sup>14</sup> Based on our discussion with GuideStar, this was due to funding issues at the time. GuideStar is a nonprofit organization and relies on donations to help fund its operations. Since the vast majority of GuideStar’s customers demand contemporaneous rather than archival data, GuideStar has decided not to backfill the missing observations.

tions to public firms in year  $t$ . In the auditor switch specification, an NPO is assigned to the treatment group in year  $t$  if its year  $t-1$  auditor issues IC attestations to public firms 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. We proxy for audit fees using the line item on the Form 990 where NPOs report the fees for accounting and audit services. *Non Calendar FYE* 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-seven percent of organizations in the treatment group (68% 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 firms 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, this does not pose a threat to our identification as long as the parallel trend assumption holds, which we address below.<sup>15</sup>

#### 4.2 NPOs: Research design

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 firm clients to NPOs whose auditors are not busy with IC attestations. Our identifying assumption with this approach is that

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<sup>15</sup> We argue that the 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 1,071 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. By the end of 2002 (our pre-period), all of Andersen's audit clients had moved to other auditors, most of which were large auditors who cater to public firms. Furthermore, Figures 4A and 4B show that fees and switches for the control and treatment groups move in parallel during our pre-treatment period—i.e., the period during which Andersen clients moved to other firms.

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.<sup>16</sup>

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.<sup>17</sup> Larger auditors with large public firm 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 firms in their client portfolio become subject to the mandate.<sup>18</sup> 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 firms.<sup>19</sup>

Our difference-in-differences specification to examine audit fees is:

$$\begin{aligned} \ln Fees_{NPO,t} = & \alpha_{NPO \times auditor} + \alpha_t + \beta_1 \times AuditorBusy404_{NPO,t} \\ & + \beta_2 \times LnAssets_{NPO,t} + \varepsilon_{NPO,t} \end{aligned} \quad (2)$$

The main regression includes NPO-auditor and year fixed effects. By including NPO-auditor fixed effects, 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 year-specific factors such as changes in economic conditions drive our results. We then run a version of regression (2) in which we include activity type-year and state-year fixed effects. We

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<sup>16</sup> 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.

<sup>17</sup> 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.

<sup>18</sup> 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.

<sup>19</sup> 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.

base the 26 activity types on the National Taxonomy of Exempt Entities, which is the standard classification system for charities. *AuditorBusy* is the treatment variable of interest. The main effects are absorbed by the fixed effect structure. We cluster the standard errors by auditor, which is the observation level of the treatment variable.

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:

$$\begin{aligned} SwitchAuditor_{NPO,t} = & \alpha_{NPO \times auditor} + \alpha_t + \beta_1 \times Auditor_{t-1}Busy_{NPO,t} \\ & + \beta_2 \times LnAssets_{NPO,t-1} + \varepsilon_{NPO,t} \end{aligned} \quad (3)$$

*SwitchAuditor* is a binary variable that takes a value of 1 if the NPO's auditor differs from that in the prior period.<sup>20</sup> *Auditor<sub>t-1</sub>Busy<sub>NPO,t</sub>* is the treatment variable of interest. We lag the control variables and fixed effects by one year, allowing the auditor fixed effects to be based on the auditor through which the treatment flows.

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

### 4.3 NPOs: Results

Table 6, column 1 shows 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 at-

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<sup>20</sup> 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. Our results are qualitatively unchanged if we count mergers and acquisitions of audit firms as auditor switches.

tations. 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 alleviates the concern that different time trends across activity types or states influences our results. In column 3, we make the assumption that audit offices are independent within an audit firm and redefine the busyness treatment variable based on the audit office serving the NPO. Again, we find similar results under this more restrictive specification.<sup>21</sup> Figure 4A 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 firms 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.

One concern with the fee result 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

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<sup>21</sup> 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 within a region throughout the country for a given auditor. This specification also effectively reduces the number of “treated” NPOs and potentially misclassifies NPOs as control observations.

auditors, rather than a result of a constrained supply.<sup>22</sup> To mitigate this concern, we exploit NPO fiscal year-ends, similar to our Table 3 analysis of private companies. In column 4 of Table 6, we interact the treatment variable *AuditorBusy* with *Non Calendar FYE* and find a negative and statistically significant coefficient on the interaction term. We find the same results when we conduct the analysis at the office level (not tabulated for brevity). These findings suggest that an NPO whose fiscal year-end falls outside of its auditor's busy season experiences a smaller increase in audit fees. Analogous to our results for private firm audit decisions, this cross sectional finding supports the inference that the increase in audit fees is related to labor inelasticity.<sup>23</sup>

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 5, we show the coefficients of the interactions between *AuditorBusy* and *Non Calendar FYE*, estimated each year. We find that the coefficient decreases after 2006 and becomes insignificant by 2008—coinciding almost exactly with the same time new accounting labor becomes available per Figure 2C. 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, rather than an NPO demand-side driven argument.

We now examine NPO auditor choice. Table 7 shows the results from estimating equation (3). In column 1, we find a 6.7 percentage point increase in the probability of an NPO

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<sup>22</sup> Note that A-133 audits required internal control test reporting prior to SOX, so IC testing of NPOs itself does not change over this time.

<sup>23</sup> Note that the increase in fees for NPOs with a non-calendar fiscal year-end is smaller, but it remains statistically significant. The sum of the coefficients for *AuditorBusy* and *AuditorBusy\*Non Calendar FYE* is 0.038 and the p-value using an F-test is 0.029.

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 while in column 3 we change the specification of the busyness variable based on office-level (rather than auditor-level) involvement in Section 404 attestation. In both cases, our results are very similar to column 1. In untabulated analysis, we repeat our tests on the subset of NPOs engaging Tier 2 or below auditors and find similar results, indicating that switching after SOX is not limited to clients of Big 4 auditors (e.g., Feng and Elder 2017).

Figure 4B plots the treatment effect on the likelihood of switching auditor in event time. The first period where an NPO's *prior year* auditor issues IC attestations to public firms is  $t=0$  (i.e., regardless of whether the NPO stays with that auditor at  $t=0$ ). The difference in the pre-treatment trends relative to  $t-1$  are statistically indistinguishable from zero. Beginning at  $t=0$ , we estimate a positive and statistically significant increase in the probability of switching auditor for the treatment group relative to the control group (i.e., between 4 and 9 percentage points depending on the year). Given the specification includes NPO-auditor fixed effects, each period's estimate is conditional on not having switched auditor yet.

Collectively, our results suggest that the demand shock for public company firms caused by SOX, coupled with a labor inelasticity, significantly affected the NPO audit market. Specifically, 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. We now assess how the aggregate supply changed and whether the shock was temporary or persistent.

#### *4.4 NPOs: Aggregate Market Effects and Long Run Consequences*

To provide perspective on the aggregate impact of SOX on the composition of the non-profit audit market, we initially track the movements of NPOs across auditor tiers. We follow the literature studying auditor switches and classify auditors into three tiers, where the first tier is the Big 4 (Hogan and Martin 2009; Chang et al. 2010; Feng and Elder 2017). Our second tier is comprised of non-Big 4 auditors who issue IC attestations at some point during the sample period. Auditors who never issue IC attestations fall into the third tier. Given we do not require detail beyond the auditor identity, we can also study governmental entities such as municipalities which are also required to receive Single Audits if they receive federal grants.<sup>24</sup> Because we include these entities, we create a fourth category for state auditors (e.g., the Michigan Office of the Auditor General) since certain governmental entities are required by law to hire their state auditor.

In Table 8, we present a transition matrix to track auditor switches from 2003 to 2009. Most strikingly, the Big 4 audit firms lose 1,529 clients in the nonprofit sector. Six hundred and eighty seven organizations move to second tier auditors, 410 switch to third tier auditors, and 430 exit the sample. By contrast, very few nonprofit audit clients move from lower tier auditors to the Big 4, resulting in the Big 4's equal- (size-weighted) share of the market for Single Audits declining from 7% (71%) in 2003 to 3% (58%) in 2009.<sup>25</sup> To show how this phenomenon is related to the enactment of Section 404, in Figure 5 we plot the client retention rate of Big 4 and middle tier auditors by year.<sup>26</sup> The Big 4 audit retention rate significantly declines in 2004, but then returns to pre-SOX levels by 2009, consistent with the labor market hiring as plotted in Fig-

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<sup>24</sup> Governmental entities are not included in the regression analyses because they do not file a Form 990 and therefore do not report data such as audit fees. The pattern we identify by tracking movements across auditors holds whether we perform our analysis on charities and governmental entities together, charities alone, or governmental entities alone.

<sup>25</sup> Because we do not require entity size, the equal-weighted market share includes governmental entities, while size-weighted only includes charities.

<sup>26</sup> To calculate the retention rate, we divide the number of clients remaining with an audit firm from year t-1 to t by the total number of clients in year t-1.

ure 2C. Figure 6 plots the total number of Single Audit clients of Big 4 firms over time. The number sharply declines in 2004, before leveling off in 2009, consistent with those auditors losing many of their clients immediately after SOX before slowly steadying the exodus. In untabulated results, we find a very similar pattern as new NPOs enter the sample: the likelihood of a new NPO selecting a Big 4 auditor is substantially lower following the enactment of Section 404 (but not before). In sum, these results are consistent with audit supply for nonpublic entities changing when public entities experienced a significant demand shock mandated by regulation.

Having confirmed the significant audit supply composition change, we then examine the potential 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—and increasing—concentration of the Big 4 auditors, but evidence on the supply of auditors in other markets is much more limited (GAO 2003, 2008). Notably, in Figure 7 we find the opposite trend in audit supply concentration for NPOs as for public companies.<sup>27</sup> Beginning with the onset of Section 404 attestation, we find the HHI for NPOs substantially *decreases* and, by 2009 is *half* its 2003 level.<sup>28</sup> This concentration level continues to at least the end of our NPO sample period in 2011. 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. This result potentially suggests one longer-term outcome from the SOX shock is a more competitive auditing supply landscape for NPOs insofar as concentration is related to competition.

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<sup>27</sup> We measure the HHI based on market shares weighted by auditee revenue. To provide an HHI that reflects the degree of competition in the overall audit market, we omit the largest 1% of NPOs, which are too big to be feasible clients of smaller auditors.

<sup>28</sup> 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). This reinforces the inference that aspects of SOX other than the demand shock—such as separate auditor regulation and audit standards—caused spillover effects on nonpublic entities.

If labor is elastic in the long run, then a natural question to ask is whether the supply shifts we have identified to this point were temporary. In Figure 6, we first plot the number of Big 4 NPO clients as far as we have available data (2014) and find that not only does the Big 4 not recover NPO clients, the number of clients actually continues to decline. To help attribute this result to the SOX generated public company demand shock, we conduct an additional test exploiting heterogeneous public company demand shocks at the state level. We expect that the more a state's audit supply available for NPOs was shocked (by demand from public companies), the more the Big 4 audit firms in that state shed their nonprofit clients. We proxy for the audit constraint for NPOs caused by Section 404 using the average log difference in audit fees paid by public companies from 2003 to 2005 in each state. We call this variable *Shock Public Market* and it is the treatment variable of interest. We then measure the change in the Big 4's share of the nonprofit audit market from 2003 to 2006 and then from 2003 to 2014 for each state to assess the shorter and longer run implications, respectively. Strikingly, no single state experiences an increase in the Big 4's NPO market share through 2014.

Table 9 presents the results of our regression. We find a negative and statistically significant association between *Shock Public Market* and  $\Delta$ *Big4 Share Nonprofit Market*, even after controlling for the number of public firms in a given state to help account for the variation in the Big 4's share of the nonprofit market that is driven by a change in the demand for consulting services rather than SOX. That is, the more the audit fees for clients in a given state increased *at the time of SOX*, the more clients the Big 4 audit firms cumulatively lose. Column 2 extends the window measuring the change in market share, but leaves the treatment effect window unchanged. This column shows the Big 4 client loss does not reverse in the long run, but in fact continues to grow over time. This finding is consistent with Section 404 causing a long run de-

cline in the Big 4's share of the nonprofit audit market.

## **5. Conclusion**

We examine how auditing regulation targeted at public companies affects the audit market for nonpublic entities. If the audit 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 firms and nonprofits. For private firms, which do not face an audit mandate, we find that the quantity of attestation services initially increased in 2002 and 2003, as smaller audit firms exited the public firm audit market and reallocated their capacity to private company engagements. Upon the enactment of Section 404 of SOX, we find that private firms 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.

Moreover, we find a significant transformation in the aggregate market for Single Audits. Starting in 2004, the concentration of audit supply to NPOs decreased by half through 2009 as NPOs dispersed away from larger 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., Ferguson et al. 2016) advanced because of public company regulation.

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## Appendix A: Private Firms – Variable Definitions

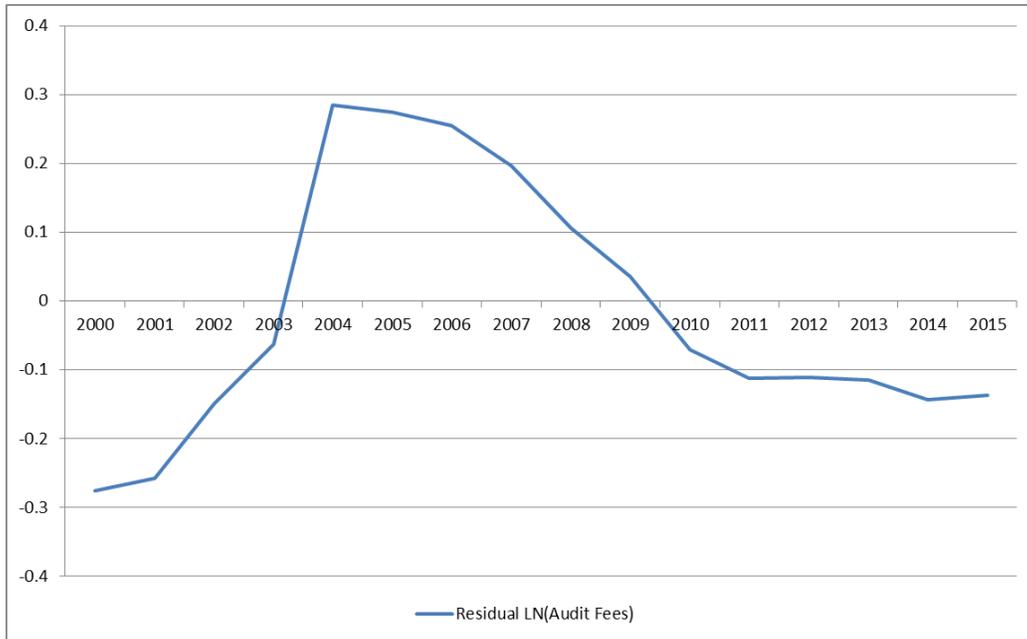
<b>Variable</b>	<b>Definition</b>
% CPA	The percent of financial statements collected that are Unqualified Audits, Reviews, or Compilations. The unit of observation is industry-region-firm size category-year.
% Unqualified	The percent of financial statements collected that are Unqualified Audits. The unit of observation is industry-region-firm size category-year.
% Reviews	The percent of financial statements collected that are Reviews. The unit of observation is industry-region-firm size category-year.
% Compilations	The percent of financial statements collected that are Compilations. The unit of observation is industry-region-firm size category-year.
Post	An indicator equal to one beginning in 2004.
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 firms with non-December fiscal year-ends, measured in 2003. The unit of observation is industry.

## Appendix B: NPOs and Governmental Entities – Variable Definitions

Variable	Definition
Assets	The assets reported on the NPO's Form 990 in a given year.
AuditorBusy404	<p>There are two versions of the variable:</p> <p><i>AuditorBusy404</i> (used in the <i>Fees</i> regression): A binary variable equal to one if, in a given year, the NPO's auditor issues internal control attestations to public firms, zero otherwise. If the NPO's auditor never issues internal control attestations, then <i>AuditorBusy404</i> is always zero.</p> <p><i>Auditor<sub>t-1</sub>Busy404<sub>t</sub></i> (used in the <i>SwitchAuditor</i> regression): A binary variable equal to one if, in a given year, the NPO's <i>prior year</i> auditor issues internal control attestations to public firms in the <i>current year</i>, zero otherwise. If the NPO's prior year auditor never issues internal control attestations, then <i>Auditor<sub>t-1</sub>Busy404<sub>t</sub></i> is always zero.</p> <p>In robustness tests, we redefine both of these variables by identifying the specific audit office in charge of the NPO audit. We measure <i>AuditorOfficeBusy404</i> as equal to one if the <i>specific office</i> of their auditor becomes busy with a 404 client, zero otherwise.</p>
$\Delta$ Big4ShareNonprofitMarket	The change in the Big 4's share of the nonprofit market at the state level. The market share is calculated as the number of nonprofit clients audited by the Big 4 divided by the total number of nonprofit clients. Depending on the analysis, the change in market share is measured from 2003 to 2006 or 2003 to 2014.
Fees	The fees for accounting and audit services reported on the NPO's Form 990 in a given year.
NumberPublicFirms	The number of public firms in a given state.
Non Calendar FYE	A binary variable equal to one if the fiscal year-end of the NPO does not end in December, zero otherwise.
ShockPublicMarket	The average log difference in audit fees paid by public companies from 2003 to 2005. The variable is measured at the state level.
SwitchAuditor	A binary variable equal to one if the NPO's auditor differs from that in the prior year, zero otherwise.

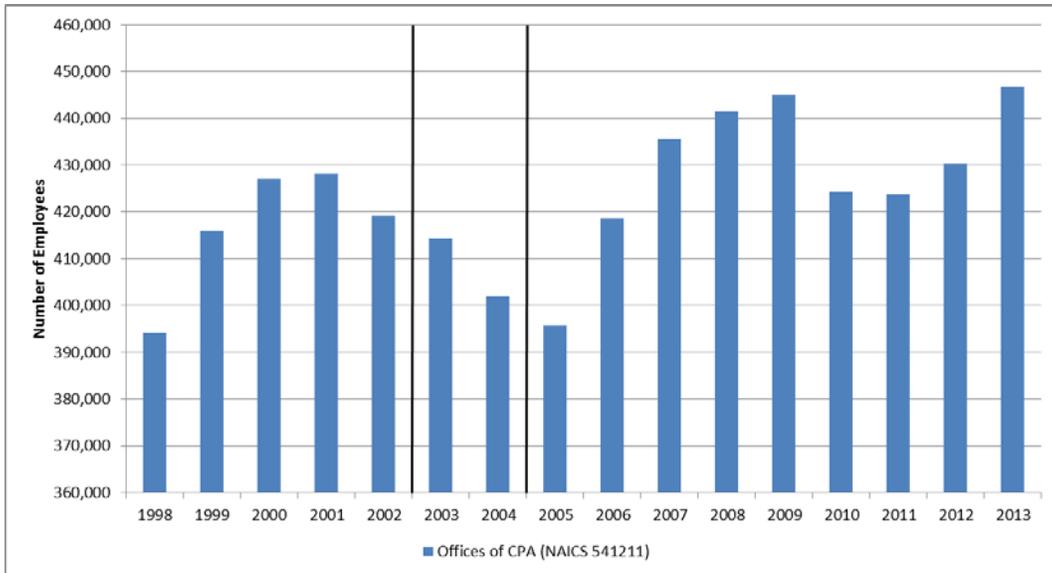
**Figure 1: Residual Public Firm Audit Fees**

This graph plots the sum of the residuals for each year from the regression:  $\ln(\text{audit fees})_{i,t} = \beta_1 \ln(\text{sales})_{i,t} + \beta_2 \text{trend}_i + \text{industry}_i + e_{i,t}$ . The data is for all firms included in Audit Analytics.



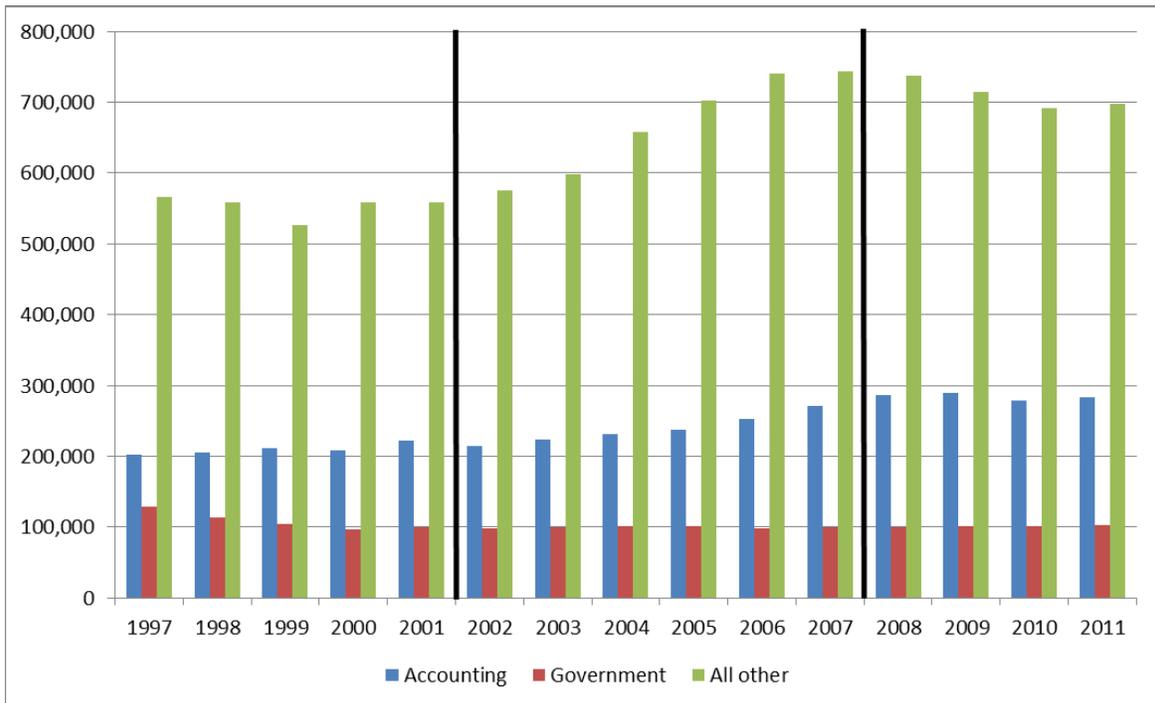
**Figure 2A: Employees at Offices of Certified Public Accountants**

This graph plots the number of employees at establishments of CPA firms by year. The data comes from Census Bureau’s County Business Patterns.



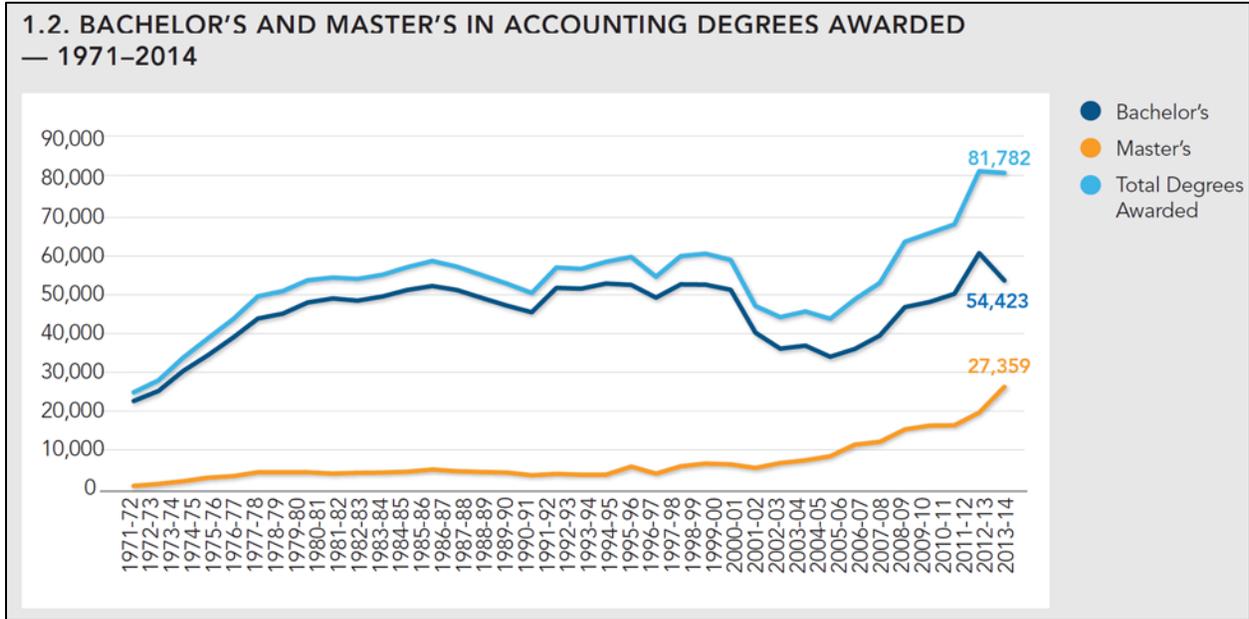
**Figure 2B: Number of Individuals Employed as Accountants and Auditors**

This Figure plots the number of individuals employed as “Accountants and Auditors” (BLS occupation code 13-2011). These are individuals employed in a variety of firms and industries with the description of: “Examine, analyze, and interpret accounting records to prepare financial statements, give advice, or audit and evaluate statement prepared by others. Install or advise on systems of recording costs or other financial and budgetary data” (BLS, Occupational Employment Statistics).



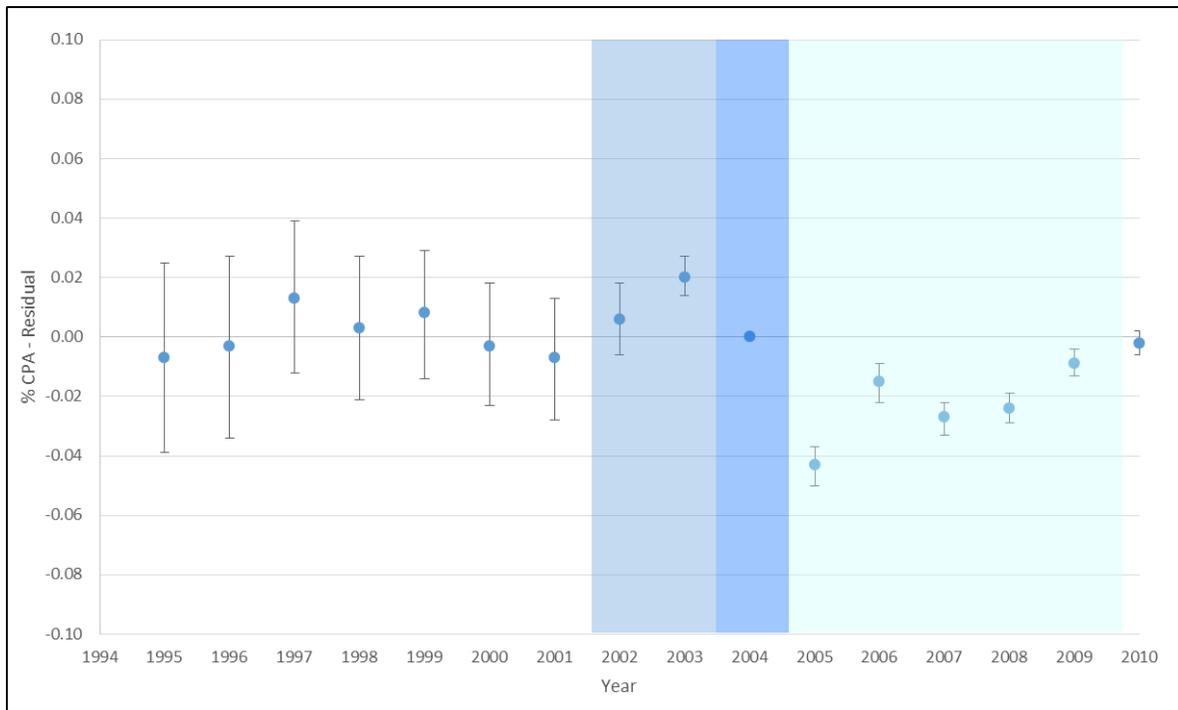
**Figure 2C: Number of Accounting Graduates in the U.S. by Year**

This graph plots the number of accounting degrees awarded in the U.S. for each year. The graph comes from the AICPA’s “2015 Trends in the Supply of Accounting Graduates and the Demand for Public Accounting Recruits,” annual report.



### Figure 3: Bank Statement Collection from Private Firms by Year

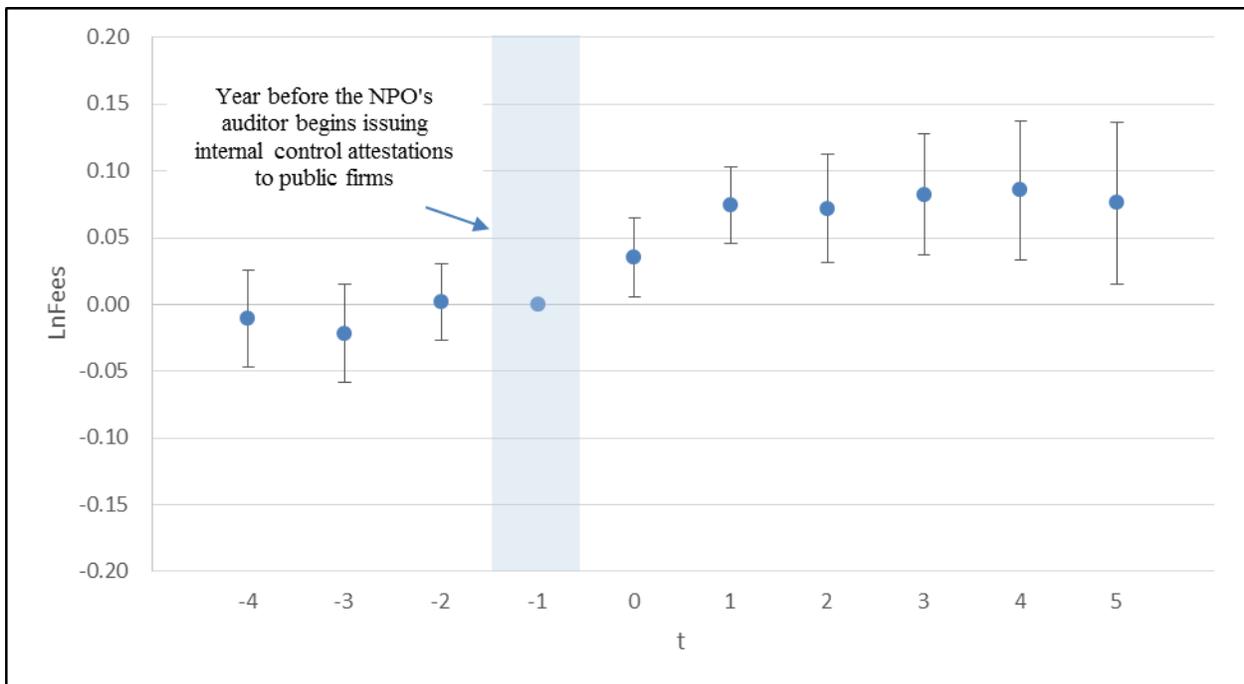
This Figure plots the estimated year fixed effect coefficient from a regression of % CPA on size, region, industry, and year fixed effects and a trend term. The solid black line is the overall sample linear trend.



## Figure 4: Audit Price Treatment Effect in Event Time

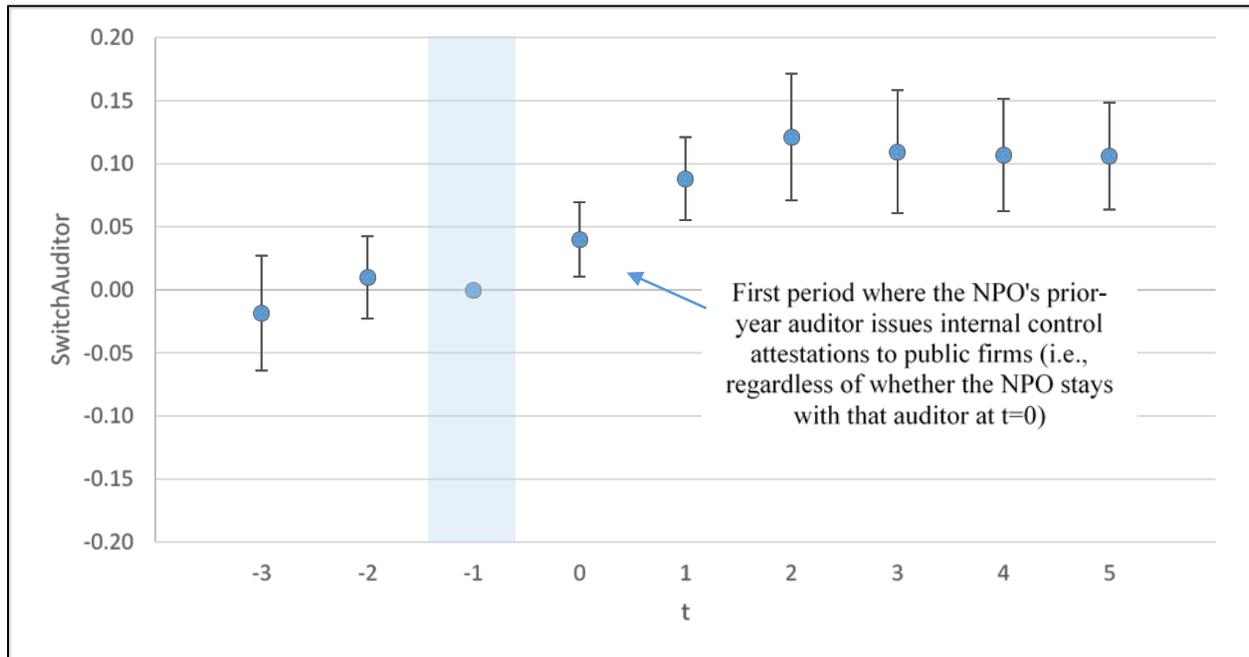
Figure 4A

Figure 4A is a graphical representation of the difference-in-differences analysis of the effect of SOX on audit price in the nonprofit sector. The figure assesses the validity of the parallel trends assumption. We estimate a version of (2) where we measure the difference in audit fees between NPOs in the treatment group and NPOs in the control group, relative to the difference at  $t-1$ . We assign an NPO to the treatment group if its auditor issues internal control attestations to public firms at some point during the sample period.  $t=0$  is the first year an NPO's auditor issues internal control attestations to public firms. 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.



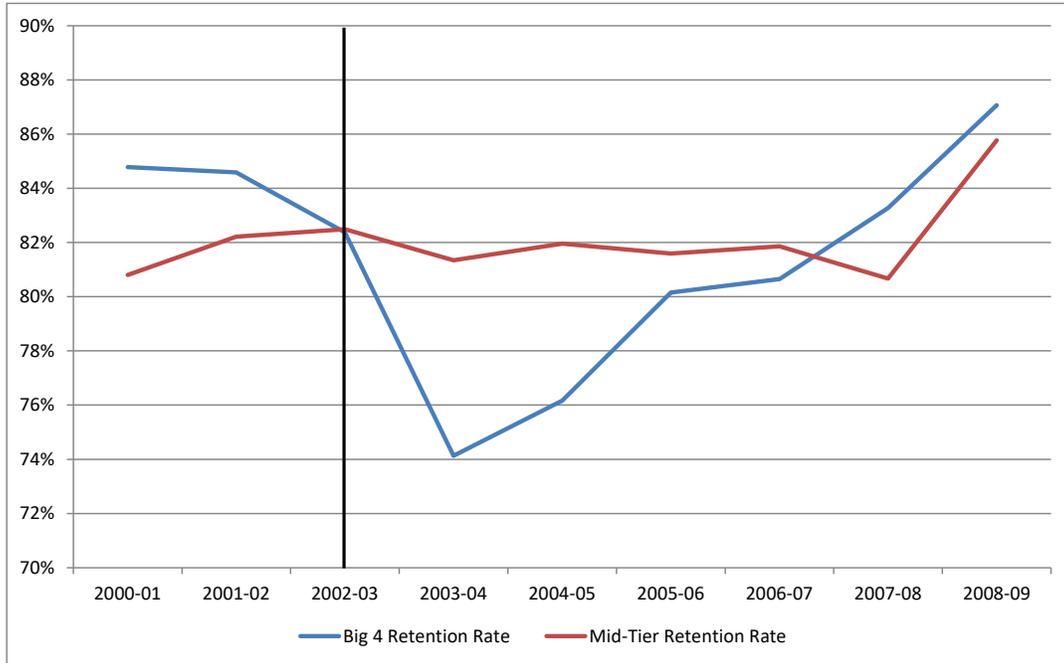
**Figure 4B**

Figure 4B is a graphical representation of the difference-in-differences analysis of the likelihood of switching auditor in the nonprofit sector. The figure assesses the validity of the parallel trends assumption. We estimate a version of (3) where we measure the difference in the probability of switching auditor between NPOs in the treatment group and NPOs in the control group, relative to the difference at t-1. 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 firms in the *current year* (i.e., regardless of whether the NPO stays with that auditor). t=0 is the first year an NPO's prior year auditor issues internal control attestations to public firms. 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.



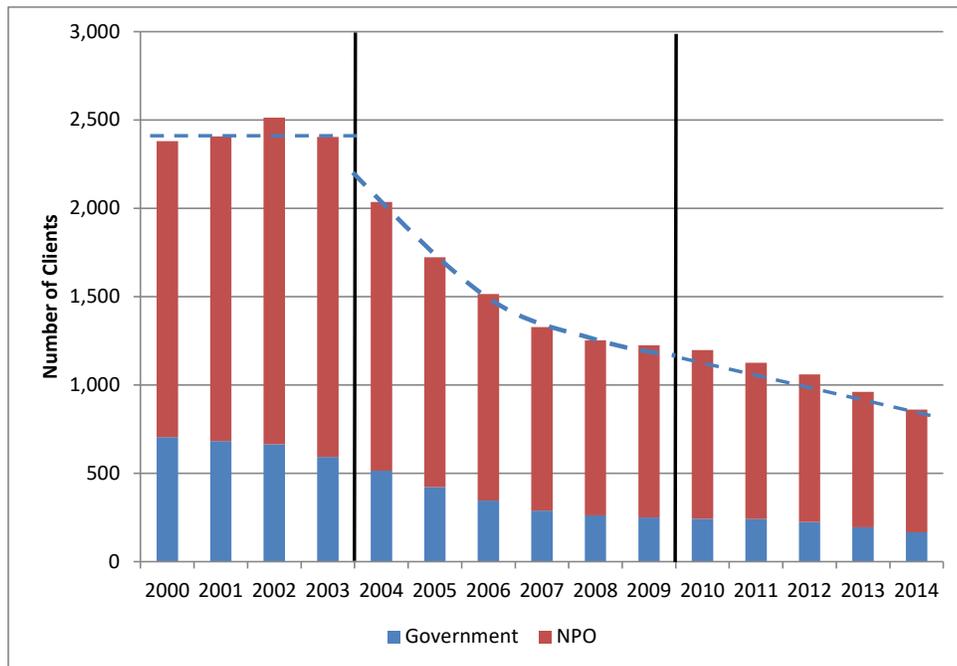
**Figure 5: Retention of Single Audit Clients for Big 4 and Middle Tier Audit Firms**

This graph shows the proportion of Single Audit clients who remain with their audit firm each year. We calculate the retention rates by dividing the number of nonprofit clients who remain with the audit firm in year t+1 by the number of clients in year t.



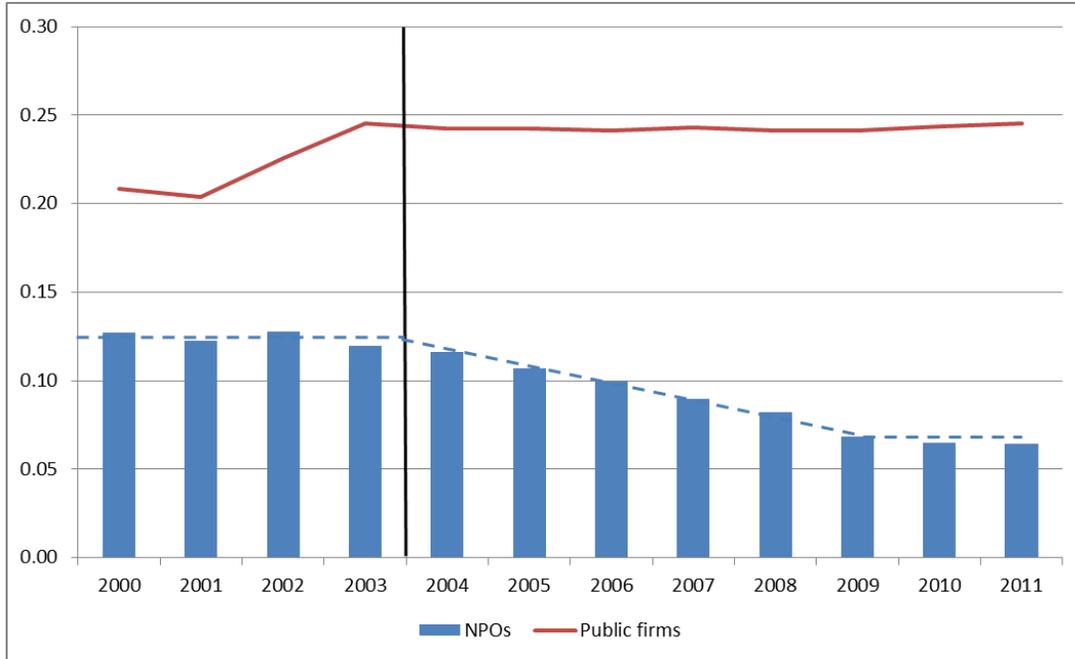
**Figure 6: Number of Single Audit Clients for Big 4 Firms**

This graph shows the total number of Single Audit clients of all Big 4 auditors by year.



**Figure 7: Audit Market Concentration**

This figure reports the Herfindahl-Hirschman Index for the audit market of both public firms (line) and NPOs (bar). The index is based on revenue-weighted market shares calculated for the two markets separately.



**Table 1: Private Firm Reporting to Banks – Summary Statistics**

This table reports the statements banks collected by type and year, as well as the summary statistics for variables used in the tests reported in Tables 2-3 and 8. See Appendix A for variables definitions.

Panel A: Statements by type and year

	Unqualified <u>Audits</u>	<u>Reviews</u>	<u>Compilations</u>	CPA Prepared <u>Statements</u>	<u>Tax &amp; Other</u>	Total <u>Statements</u>
1995	15,220	16,013	18,964	50,197	26,178	76,375
1996	16,105	16,339	19,318	51,762	28,767	80,529
1997	17,138	17,034	19,861	54,033	29,222	83,255
1998	19,335	19,033	21,481	59,849	36,425	96,274
1999	18,653	18,410	19,943	57,006	35,304	92,310
2000	14,809	16,359	21,363	52,531	35,797	88,328
2001	15,205	15,074	21,337	51,616	38,005	89,621
2002	20,211	19,372	18,660	58,243	44,647	102,890
2003	21,117	21,396	26,611	69,124	55,104	124,228
2004	21,997	20,886	19,860	62,743	58,087	120,830
2005	21,798	19,414	17,968	59,180	67,656	126,836
2006	25,278	21,358	19,687	66,323	72,785	139,108
2007	23,403	19,672	17,787	60,862	75,496	136,358
2008	25,111	21,373	18,322	64,806	86,918	151,724
2009	25,459	21,812	18,460	65,731	92,665	158,396
2010	25,137	21,957	18,518	65,612	96,525	162,137
<u>2011</u>	<u>25,549</u>	<u>22,158</u>	<u>19,705</u>	<u>67,412</u>	<u>108,039</u>	<u>175,451</u>
Total	351,525	327,660	337,845	1,017,030	987,620	2,004,650

Panel B: Summary statistics

	<i>Mean</i>	<i>Std Dev</i>	<i>25%</i>	<i>50%</i>	<i>75%</i>	<i>N</i>
% 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: Sarbanes-Oxley and Reporting Practices of Private Firms**

This table models financial statement collection by banks as a function of time. The dependent variable is % CPA. *Year 04-09* and *Year 02-03* are indicators for observations in those respective years. The sample spans 1995-2011. The unit of observation is industry-region-firm 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)	(4)	(5)
	% CPA				
Year 04-09	-0.022***	-0.021***	-0.020***	-0.023***	-0.019***
	[-7.15]	[-7.29]	[-6.45]	[-6.93]	[-5.05]
Year 02-03					0.014***
					[2.72]
Adj R-Sq.	0.781	0.846	0.781	0.794	0.781
N	34,902	34,902	34,902	34,902	34,902
Linear Trend	Yes	Yes	Yes	No	Yes
Second and Third Order Trend	No	No	Yes	No	No
Industry-Specific Trend	No	No	No	Yes	No
Industry FEs	Yes	No	Yes	Yes	Yes
Region FEs	Yes	No	Yes	Yes	Yes
Size Category FEs	Yes	No	Yes	Yes	Yes
Industry-Region-Size Category FEs	No	Yes	No	No	No
Clustering	Industry	Industry	Industry	Industry	Industry

**Table 3: Auditor Supply Slack, Sarbanes-Oxley, and Reporting Practices of Private Firms**

This table performs cross-sectional tests on our Table 2, Column 1 results according to the timing of private firms' fiscal year-ends in the industry. The dependent variable is % CPA. *Year 04-09* is an indicator for observations in those respective years. *Non Calendar FYE Share* is the percent of private firms in the industry with a non-December fiscal year-end. We assign industries to deciles of *Non Calendar FYE Share* to facilitate interpretation of our interaction term. The sample spans 1995-2011. The unit of observation is industry-region- firm 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)
	% CPA	% CPA	% CPA
Year 04-09	-0.038*** [-7.49]		
Year 04-09 * Non Calendar FYE Share	0.003*** [3.08]	0.003** [2.20]	0.003** [2.17]
Adj R-Sq.	0.782	0.792	0.811
N	34,105	34,105	34,105
Linear Trend	Yes	No	No
Industry FEs	Yes	Yes	Yes
Region FEs	Yes	Yes	No
Size Category FEs	Yes	Yes	Yes
Sector x Year FEs	No	Yes	No
Sector x Region x Year FEs	No	No	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 and 9. We exclude observations with less than \$500,000 in grants because such 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, date the fiscal year-ended, 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 firms at some point during the sample period. We use Audit Analytics' SOX 404 database to determine whether auditors issue internal control attestations.

	Table 6	Table 7
	(Fees)	(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 tests reported in Tables 6, 7 and 9. 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 firms 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 firms 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 Tables 6 and 9

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. 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 coded as one for NPOs with non-December fiscal year-ends. Activity types are based on the National Taxonomy of Exempt Entities. We Winsorize the continuous variables at the 1<sup>st</sup> and 99<sup>th</sup> 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)
	LnFees	LnFees	LnFees	LnFees	LnFees
AuditorBusy404	0.065*** [3.69]	0.054*** [3.17]		0.115*** [3.90]	0.115*** [3.38]
AuditorOfficeBusy404			0.047** [2.27]		
AuditorBusy404*NonCalendarFYE				-0.077*** [-2.74]	
AuditorBusy404*NonCalendarFYE*2004					-0.089** [-2.40]
AuditorBusy404*NonCalendarFYE*2005					-0.091** [-2.33]
AuditorBusy404*NonCalendarFYE*2006					-0.106*** [-2.69]
AuditorBusy404*NonCalendarFYE*2007					-0.073* [-1.95]
AuditorBusy404*NonCalendarFYE*2008					-0.042 [-1.16]
AuditorBusy404*NonCalendarFYE*2009					-0.028 [-0.67]
LnAssets	0.151*** [5.68]	0.151*** [5.43]	0.151*** [5.43]	0.151*** [5.43]	0.151*** [4.88]
Adj R-Sq.	0.883	0.884	0.884	0.884	0.884
N	41,605	41,605	41,605	41,605	41,605
NPO x auditor FEs	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	No	No	No	No
Activity type x Year FEs	No	Yes	Yes	Yes	Yes
State x Year FEs	No	Yes	Yes	Yes	Yes
Clustering	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 firms 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. The variables used in the controls and fixed effects are lagged (this allows the auditor fixed effects to be based on the auditor through which the treatment flows). Activity types are based on the National Taxonomy of Exempt Entities. We Winsorize the continuous variables at the 1<sup>st</sup> and 99<sup>th</sup> 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)
	SwitchAuditor <sub>t</sub>	SwitchAuditor <sub>t</sub>	SwitchAuditor <sub>t</sub>
$Auditor_{t-1}Busy404_t$	0.067*** [4.01]	0.072*** [4.46]	
$AuditorOffice_{t-1}Busy404_t$			0.067*** [3.48]
LnAssets	-0.006 [-0.03]	-0.010* [-1.69]	-0.010 [-1.64]
Adj R-Sq.	0.118	0.123	0.118
N	30,022	30,022	30,022
NPO x auditor FEs	Yes	Yes	Yes
Year FEs	Yes	No	No
Activity type x Year FEs	No	Yes	Yes
State x Year FEs	No	Yes	Yes
Clustering	Auditor	Auditor	Auditor

**Table 8: Movements of NPOs and Governmental Entities Across Auditors**

This table tabulates a transition matrix that tracks the movements of nonprofit clients across auditors from 2003 to 2009. The numbers along the diagonal represent organizations who remain in the same auditor tier. We construct three tiers: Big 4 auditors, non-Big 4 auditors who issue internal control attestations at some point, and other auditors. We also include a category for state auditors (e.g., the Michigan Office of the Auditor General).

		2009					Total
		Big4	Non-Big4 who IC attestations	Other auditors	State auditors	Client left sample since 2003	
2003	Big4	875	687	410	2	430	2,404
	Non-Big4 who IC attestations	19	2,475	1,120	4	1,073	4,044
	Other auditors	18	1,373	18,523	70	6,861	27,492
	State auditors	0	45	197	1,368	457	2,067
	Client to enter sample by 2009	312	2,089	11,431	769	0	14,483
	Total	1,224	5,894	32,456	2,213	8,703	

**Table 9: SOX Section 404 Demand Effect and Big 4’s Share of the Nonprofit Market—  
State Level Evidence**

This table presents a state-level test of the association between the extent to which Section 404 constrained the audit supply and the change in the Big 4’s share of the nonprofit market. *SupplyShockPublicMarket* is the average log difference in audit fees paid by public companies from 2003 to 2005. *ΔBig4ShareNonprofitMarket* is the change in the Big 4’s share of the nonprofit market from 2003 to 2006 (column 1) or 2003 to 2014 (column 2). *LnNumberPublicFirms* is the log of the number of public firms for the year 2003. Reported below the coefficient is the t-statistics \*, \*\*, \*\*\* indicate significance at the two-tailed 10%, 5% and 1% levels, respectively. See Appendix B for variables definitions.

	(1)	(2)
	Short window (2003-2006)	Long window (2003-2014)
	<i>ΔBig4ShareNonprofitMarket</i>	<i>ΔBig4ShareNonprofitMarket</i>
<i>ShockPublicMarket</i>	-0.038** [-2.61]	-0.065*** [-2.48]
<i>LnNumberPublicFirms</i>	-0.003** [-2.07]	-0.004 [-1.55]
Adj R-Sq.	0.205	0.157
N	51	51
Unit of observation	State	State